Peerage and Judgment

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How transdisciplinary collaborations recognize
contributions without a consensus of meaning

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Abstract

This thesis concerns judgments of quality and belonging in transdisciplinary research (TD). TD includes academics from various disciplines and is open to participation from non-academics. TD typically aims to address societal problems and is argued to produce knowledge that is more nuanced than traditional disciplinary research due to the plurality of perspectives included. The focus of this thesis is on the dynamics underlying judgments made by TD collaborations where members recognize each other as epistemic peers despite different conceptions of what it means for science to be good.

To investigate these dynamics, I adopt a middle perspective that connects theoretical and empirical investigations. The thesis is a compilation of two theoretical and two empirical papers, and the middle-level theory is applied in the synopsis. This middle-level theory illuminates two central issues surrounding epistemic peerage in TD.

The first issue concerns the coordination of the demarcation of a TD collaboration and the collaboration across boundaries within the collaboration. The investigated cases illustrate how boundaries are drawn towards an outside of non-peers while the peers within the collaboration maintain a multiplicity of understandings. The core issue is that those within the collaboration cannot have world-views that are so different as to prevent them from recognizing each other as peers, while also not so similar that there can be no substantial exchanges across borders. I show how the investigated cases use hub-and-spoke concepts to coordinate demarcation and collaboration.

The second issue concerns which issues are kept open and closed for discussion within a TD collaboration. The aims of TD of production of nuanced knowledge with societal relevance and inclusive practices require an openness to discuss matters that would in other circumstances be considered closed facts. At the same time a certain amount of closedness is required to stabilize the collaboration. The cases in this thesis show how the question of which issues are kept open and closed is affected by the institutional environment of TD collaborations.

This thesis contributes to our understanding of judgments of scientific quality in TD. The thesis also demonstrates the fruitfulness of a middle-level theoretical perspective in investigating TD.

Svenskspråkig sammanfattning

Avhandlingen behandlar problematik kring bedömning av kvalitet och tillhörighet inom transdisciplinär forskning (TD). TD är forskning som inkluderar akademiker från olika discipliner och fält, samt är öppen för medverkan från icke-akademiker. Denna forskning inriktar sig typiskt på att behandla konkreta samhällsproblem och gör anspråk på en mer nyanserad och anpassad kunskap än traditionell disciplinär forskning till följd av de olika perspektiv som inkluderas. Fokus i denna avhandling är på den dynamik som ligger till grund för bedömningar gjorda av TD-grupper där medlemmar ser varandra som epistemiskt jämbördiga (*epistemic peers*) trots olika uppfattningar om vad som utgör god vetenskap.

För att undersöka detta anläggs ett mellan-perspektiv som kopplar samman teoretiska och empiriska undersökningar. Avhandlingen är en sammanläggning av två teoretiska och två empiriska artiklar, och det mellan-teoretiska perspektivet anläggs i kappan. Detta mellan-teoretiska perspektiv belyser två centrala frågor kring epistemisk jämbördighet i TD.

Den första rör koordineringen av gränsdragning kring ett TD-samarbete och gränsöverskridande samarbete inom TD-samarbetet. Fallen som undersökts illustrerar hur gränser dras mot en utsida av icke-jämbördiga samtidigt som de jämbördiga inom samarbetet bibehåller en mångfald av förståelser av studieobjekt och kunskapskvalitet. Centralt är att de som ingår i samarbetet inte får ha så olika världsbilder att de inte kan erkänna varandra som jämbördiga, men samtidigt inte så lika världsbilder att det inte kan ske substantiella utbyten över gränser. I avhandlingen visas hur de studerade fallen använder begreppsliga nav (*hub-and-spoke concepts*) för att koordinera gränsdragning och samarbete.

Den andra frågan rör vilka frågor som är öppna och stängda för diskussion inom ramarna för ett TD-samarbete. TD:s målsättningar i form av nyanserad kunskap med samhällelig relevans och inkluderande förfarande kräver en öppenhet att diskutera sådant som i andra sammanhang ses som stängda fakta. Samtidigt krävs en viss stängdhet för att stabilisera samarbetet. TD-samarbeten behöver därför balansera mellan öppenhet och stängdhet. Fallen i avhandlingen visar hur vilka frågor som hålls öppna och stängda påverkas av TD-samarbetets institutionella omgivning.

Denna avhandling bidrar till förståelsen av bedömning av vetenskaplig kvalitet i TD. Avhandlingen visar också på fruktbarheten i ett mellan-teoretiskt perspektiv för att undersöka TD.

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Appended papers

Paper 1: Lundgren, J. (2018). No "Real" Experts: Unexpected Agreement Over Disagreement in STS and Philosophy of Science. *Perspectives on Science*, *26*(6), 722–735.

Paper 2: Lundgren, J. (2020). The Grand Concepts of Environmental Studies: Boundary objects between disciplines and policymakers. *Journal of Environmental Studies and Sciences, 11,* 93–100.

Paper 3: Lundgren, J. (2022). Unity through disunity: Strengths, values, and tensions in the disciplinary discourse of ecological economics. *Ecological Economics*, *191*, 107241.

Paper 4: Lundgren, J. (2022) Cutting across quality and relevance: reviewers' understanding of competence to assess societal relevance in transdisciplinary grant application peer-review. (Publishable manuscript)

Table 1: Overview of papers

	Paper 1	Paper 2	Paper 3	Paper 4
Type	Theoretical	Theoretical	Empirical	Empirical
Topic	Expertise	Boundary objects	Identity of a field	Assessment
To be understood	Different Understanding of expertise in controversy	Maintenance of vagueness or move to standardization	Inclusion or exclusion of mainstream frameworks	Understanding of competence and quality criteria
Suggested explanation	*A priori* or empirical disciplinary norms	Focus on large (political) or local scale	Emphasis on different values	Forms of inclusivity
Contribution to project	Delineating research question and object of study	Development of theoretical tools and concepts	Case of long-term collaboration in academic context	Case of short-term collaboration in policy context

Glossary

Boundary object – Object (not necessarily material) that is common to multiple social groups, with a vague understanding shared between groups yet different complex understandings in each group

Boundary work (of demarcation) – The rhetorical practice of establishing the autonomy and authority of one's own field through contrast with others

Discipline – Area within science incorporating both organizational and intellectual elements

Epistemic peerage – Mutual recognition of equal epistemic competence in relation to the same topic or question

Field – Cognitive dimension of areas in science, including theoretical structures and mode of being of practitioners

Institutional and intellectual environment – Actors, practices, or ideas that are taken to subsist outside of a collaboration

Meso-level – Level of analysis located above individual interaction, yet not constituting the whole of some larger system

Organizational form – The practices and setup of a collaboration in terms of explicit and implicit rules

Peer-review – The assessment of scientific contributions by the epistemic equals of its producers

Role – Beliefs about which actions are appropriate to take by certain designated groups, and how it is appropriate to act towards them in turn

Scientific values – Any normative beliefs about what is good or right about knowledge or the process of its production

Social world – A social group defined through interaction centered on some core activity, sharing a certain understanding of the world

Sustainability – The concern that future generations should not be worse off than the current, especially regarding ('natural') environmental risks and resources

Transdisciplinary research – Research that involves actors from multiple disciplines and/or from outside academia intended to produce knowledge better suited to addressing real-life problems

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STS: Science and Technology Studies
TD: Transdisciplinary/Transdisciplinarity

1. Introduction: The central problem

Current trends in research reflect an increased interest in questions concerning the grand challenges of society, such as health, security, equality, and sustainability among many others. Such problems are perceived to require collaborations across disciplines and sectors, such that different ways of understanding are taken into consideration. This would then, it is argued, allow for more nuanced descriptions of the problems and solutions that better fit the complex reality. However, if the collaborators have different ways of understanding a problem, they also may have different ways of understanding what kind of knowledge is required to take on that problem. What makes it possible for such collaborations to end up with a common judgment of new scientific contributions? How can scientists make collaborative judgments about new science if their conceptions about what it means for science to be good are substantially different? This is the question I explore in this thesis.

In this thesis, I investigate transdisciplinary (TD) collaborations that involve peer-review. TD research is research that involves not only scientists from various disciplines but also is open to stakeholders from outside of academia. When TD research is to be produced or recognized on the grounds of peer-review, it is (or should be) judged by a group that is the peers of the collaborating researchers. Therefore, I have in this thesis concretized the central problem as the *problem of epistemic peerage in TD*.

I approach this problem through theoretical investigation as well as two case studies of TD collaborations in the general area of sustainability. The first theoretical study concerns the notion of epistemic peerage and relates it to the notion of expertise as well as different norms in philosophy and Science and Technology Studies (STS). I find an unexpected continuity between a thinker in STS and a philosopher in the question of expertise in controversy. The second theoretical study concerns the concept of boundary objects and its use by actors in TD. I identify two ideal types of different conceptual boundary objects within TD that each follow different dynamics. The first case study concerns the field ecological economics, a methodologically open field that emerged in the late 1980's in opposition to the mainstream in economics and ecology. I interview editors and editorial board members of the flagship journal of the field in order to investigate the disciplinary discourse that informs judgments of belonging. The second case study concerns two review panels at the Swedish research council Formas, panels that dealt with two directed calls concerning specific issues within sustainability and that included reviewers from outside of academia.

I interview reviewers in order to investigate how the review panel as a whole understood the notion of being competent to assess research proposals, i.e., being an epistemic peer to the researchers and the panel.

The central problem in this thesis touches on the issue of the emergence and development of new fields in science. Many new areas of research have resulted from the crossing of disciplinary boundaries, such as molecular biology, evolutionary game theory, and STS. Although initially dependent on the disciplinary structures of their parent disciplines, these fields have gained an autonomous existence and thus develop their own disciplinary structures. Researchers in these fields thus over time form a common template for judgments of potential contributions to their emergent field, a template that can include various values; both academic and relating to societal relevance. They go from a period of being unsure how to interrelate their various judgments to a period of broad and detailed consensus over what it means to make a good contribution to their field. Between these two periods lies the problem I investigate.

1.1. The purpose and structure of this synopsis

The form of this thesis is a compilation of four peer-reviewed papers with a summarizing and synthesizing synopsis. The central role of the synopsis is the explication of the commonality and continuity in the compiled papers (Krumsvik, 2022). This task requires me to move beyond the theoretical resources in each paper. The first two papers focus on developing general theoretical concepts. Their results are thus abstract, and their validity stems only from their potential or conditional applicability. The second two papers are contrasting empirical case studies. The approach undertaken in these focuses on the particular and the aim is to describe the details of each case in isolation. Neither approach on its own creates a holistic synthesis. Thus, the synopsis must incorporate its own theoretical framework. More than just an explication, this synopsis therefore constitutes the commonality and continuity between papers.

I apply a middle-range or meso-level theoretical framework to the papers and their underlying data. There are various notions of what does or should constitute the middle or meso in the study of science (Wyatt & Balmer, 2007). I conceptualize the middle in three ways. First, as the object of study located on the meso-level, on the one hand 'above' the particular and on the other hand 'below' the general and abstract. Second, as the methodological concern of middle-range applicability of results, reaching beyond the single case yet not purporting to universal validity. Third, as being 'in the middle' between fields, objects, and discussions, linking the commonalities and explicating the differences between them.

To reflect the synthesizing purpose, the appended papers are introduced and summarized relatively early, in chapter 3, after a general discussion of the phenomena under study in chapter 2. Chapters 4 and 5 relate the phenomena and approaches in the singular papers to the middle level. Chapter 4 delineates the meso-level object. This is used to illustrate how the papers investigate some of the same underlying problems. Chapter 5 discusses epistemology, methodology, and the middle range. Here I argue that the approaches in the papers complement each other, and that results can be applicable beyond this thesis. Chapters 6 and 7 discuss the cross-cutting issues in the papers. Chapter 6 focuses on overarching ethical issues and chapter 7 explores theoretical problems that arc through the four papers. Chapter 8 is a summarizing conclusion.

1.2. The disciplinary context of the thesis

Because this thesis deals with the boundaries of disciplines and discipline-crossing, it is worth sparing a few words for the disciplinary context of the thesis itself. This thesis was written at the Department of Philosophy, Linguistics and Theory of Science at the University of Gothenburg. The field of theory of science at this department has boundaries with two neighboring fields: philosophy (of science) on the one hand and STS on the other. The history of the department shows how a historical tension with philosophy has been overcome, theory of science having split off from the then-prevailing logical positivism in Swedish philosophy by incorporating empirical studies and hermeneutical and dialectical elements into the investigation of science (Heidegren, 2016; Törnebohm, 1985). Since then, both theory of science and philosophy have developed in such a way that this historical tension has dissipated. I am myself a product of both *milieus*, having a bachelor's degree in the classics- and philosophy-oriented Liberal Arts as well as master's degrees in theoretical philosophy and theory of science from the department.

The relationship between theory of science and STS could be the subject of an entire study, and I will therefore have to be very brief in this explication. The fields share a history and an emphasis on the need for empirical studies and descriptive accuracy in the investigation of science. However, theory of science also has a clear historical link to German neo-Kantian tradition, a link that is not as pronounced in STS. Moreover, some influential schools of thought in STS are skeptical of generalizations beyond empirical cases as well as non-empirical conceptual work, which are important parts of my approach in this thesis. At the same time, STS is itself a broad field incorporating a variety of perspectives, meaning that my approach is at home in an extended STS-community. Theory of science, and this thesis, can thus be considered both a part of and apart from STS. In this thesis (primarily the synopsis), I have attempted to be sensitive to

the notion that many readers will be thinking in STS-terms, yet I consider myself to be primarily a theorist of science

2. Background: Transdisciplinarity, society, and peer-review

In this chapter, I explicate TD peer-review as the general phenomenon under investigation. First, I explore the way that TD, as a large-scale theoretical structure, is understood by actors and argue that this understanding of TD makes it a good object for investigating the central question in this thesis. Second, I explore how TD reflects large-scale issues in the relationship between science and society. Third, I discuss peer-review, focusing on discussions surrounding peer-review of research that does not conform to disciplinary norms.

2.1. Actors' understanding of TD

Before exploring understandings of how disciplines are crossed, I believe it to be useful to explore various ways of conceptualizing the notion of a 'discipline', or the related notion of a 'field'. Disciplines are seen as constituted by various organizational and intellectual forms, including on the one hand departments, journals, classification systems, curriculums, and professional associations, and on the other hand language or jargon, accumulated knowledge, a narrative about itself, and a normative discourse (D'Agostino, 2012). The notion of 'fields' focuses less on the formal institutional or organizational aspects of the organization of knowledge and knowers, instead emphasizing its cognitive dimension (Darden & Maull, 1977), or the mode of being of its practitioners (Bourdieu, 1988). Depending on the researcher's own epistemic norms, the study of disciplines can proceed from their cognitive (Darden & Maull, 1977), classificatory (Abramo et al., 2012), organizational (Whitley, 2000), interpersonal (Parker & Hackett, 2012), anthropological (Becher & Trowler, 2001), political (Kay, 1997), or any combination of aspects.

Although it is difficult to say when fields (or the intellectual aspects of disciplines) started emerging within science, the origin of the modern organizational unit of disciplines can be followed back to the 19th century[1]. However, by the 20th century already, arguments were made that some forms of knowledge, or the solution of certain problems, required crossing disciplinary lines. The creation of the new (inter-)discipline of molecular biology in the 1930's

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[1] Of course, earlier than that was the mediaeval organization of university education into schooling in the seven liberal arts, although little trace is left of this organizational convention today.

is an early example of deliberate discipline crossing to pursue specific research goals (Kay, 1997). Since then, the notion of 'interdisciplinarity' has gained increasing popularity. Today, discipline-crossing research is seen as the key to addressing humanity's grand challenges (Baptista et al., 2019; NAS et al., 2004). The crossing of disciplines is advocated in the medical sciences (Choi & Pak, 2007), education (Dillon, 2008), engineering (McNair et al., 2015), humanities and qualitative social sciences (Pedersen, 2016), and extensively in sustainability studies and sciences (Brandt et al., 2013; Felt et al., 2016; Jónsson et al., 2016).

Contemporary theorists view discipline-crossing as having three distinct forms: multidisciplinarity, interdisciplinarity, and TD (Klein, 2017). Most theorists who distinguish between these forms focus on the integration between the collaborating disciplines or fields, where multidisciplinarity contains little or no integration, interdisciplinarity integrates disciplines, and TD consists of a more fundamental integration than interdisciplinarity. The notion of 'integration' is vague (O'Rourke et al., 2016), but is commonly taken to involve the cognitive aspects of disciplines, such as theories (Darden & Maull, 1977), knowledge (Vilsmaier et al., 2015), or languages (Holbrook, 2013). In sections 4.1.2 and 4.2.1 I discuss how integration relates to two underlying theoretical issues: communication across borders and scientific values.

There have also been different conceptualizations of how TD differs from interdisciplinarity. Early theorists focused on the presumed universality of knowledge, e.g., mathematics as a universal metalanguage (Apostel et al., 1972) or overarching conceptual frameworks such as structuralism or general systems (Miller, 1982). Later theorists have focused on a critical interpretation of TD, aiming to question the disciplinary organization of knowledge production (Butler, 2009) or the epistemological foundations of science in general (Max-Neef, 2005).

In the 1990's, theoretical developments were made that would greatly influence the discourse surrounding TD. Proceeding from similar starting points and reaching similar conclusions, these developments were the ideas of Post-Normal Science (Funtowicz & Ravetz, 1993) and Mode-2 Production of Knowledge (Gibbons, 1994). If a distinction is to be made between the two, then post-normal science focuses more on the intellectual aspect and mode-2 more on the organizational aspect of science, although both perspectives discuss both aspects. The central idea of both perspectives, that also figures in the discourse advocating discipline crossing, is the inadequacy of traditional disciplinary science to address the problems seen as relevant to society or humanity. In post-normal science, these problems are seen as located in areas of high uncertainty and high concern. The high uncertainty means that it is difficult for researchers to close contested issues and thus stabilize facts, and yet the high concern means that issues need urgent closure. This closure must thus be achieved with appeal to heuristics such as the precautionary principle. This means that post-normal

production of knowledge cannot always uphold a clear distinction between fact and value. Seeing as science is perceived to be primarily oriented around fact rather than value, post-normal science must include actors outside of academia to assist in the production of knowledge through guiding the evaluative choices that need to be made. Mode-2 proceeds instead from the observation that problem formulation in science is increasingly steered by societal interest, both through instruments of funding and a sense of accountability to society among scientists. Thus, argues Nowotny (1999), both the questions asked and the answers provided need to be socially robust, in addition to scientifically robust.

The development of post-normal and mode-2 perspectives has led to an interpretation of TD as a way of conducting research that is problem-oriented and cuts across the boundaries between disciplines, and between academia and society (Gibbons & Nowotny, 2001). The problems that TD research should address need to be formulated from the life-world of those who have a stake in their solution (Hirsch Hadorn et al., 2008). As such, a core tenet of this interpretation of TD is the inclusion of perspectives from non-academics (Bergmann & Jahn, 2008). Central concerns regarding this are whether stakeholders are included in a way that is meaningful and not token (Elzinga, 2008), and the circumstances of their inclusion, i.e., whether they are seen as circumscribed consultants or equal participants (Mobjörk, 2010). This interpretation of TD is particularly common in the European Union (Klein, 2017).

Most forms of discipline-crossing research face problems of evaluation (Klein, 2008). Due to a lack of access to many of the organizational features of established disciplines, interdisciplinary and TD research often has problems being published (Kueffer et al., 2007; Pohl et al., 2015) or otherwise recognized, disadvantaging the careers of discipline-crossing researchers and steering them towards publishing their research in more disciplinary forms. This stems from the traditional evaluative criteria for recognizing science being largely tied to disciplines. At the same time, there are uncertainties about how to interpret the notion of societal relevance (Samuel & Derrick, 2015), and a discussion about whether it is at all appropriate to evaluate alongside scientific quality (Bozeman & Boardman, 2009; Holbrook & Frodeman, 2011), relevance being one of the central reasons given for the production of TD knowledge. This affects the perceived value of producing TD over regular science from the point of view of scientists. Thus, decision-making about both the production and recognition of TD research are understood as problematic by actors.

It is the presence of a multitude of perspectives with different outlooks and values and the perceived problematic nature of decision-making about

production and recognition that makes TD a good object of study for this investigation.

2.2. TD and the relationship between science and society

In this section, I explore some ways in which TD research relates to broader issues in the relationship between science and society. This mainly takes the form of interactions with research policy.

From its inception, the notion of TD has been associated with the organization of research. The canonized origin of the term is a report by the Organization for Economic Cooperation and Development aiming to explore ways of reorganizing universities in order to "meet the intellectual and social demands of the present time" (Apostel et al., 1972, abstract). At that point, TD did not yet have the connotations of close collaborations with stakeholders, but this development followed along the lines of developments in research policy. Indeed, analysts proposing a mode-2 understanding of science stress that it follows from a renegotiated relationship between science and society (Gibbons, 1994; Nowotny et al., 2011). The logics of neoliberalism and new public management are argued to have led to a regime of accountability in contemporary research policy (Elzinga, 2012), involving both increased evaluation of scientific quality and a push to include societal relevance as a quality criterion (Elzinga, 1997; Holbrook & Frodeman, 2011). One of the driving factors of this regime is the perceived link between research, innovation, and economic advantage. Significant changes to intellectual property laws occurred in the US in the 1980's, allowing publicly funded institutions to produce patents and thus generate economic growth (Coriat & Orsi, 2002). However, the idea of a link between research and national prosperity in US science policy can be traced as far back as Vannevar Bush (1945), who in a report to the President argued that even basic research leads to prosperity in the long run, and Alvin Weinberg (1963), who as science policy advisor argued the need to judge science by external criteria when determining funding, including social merit.

Parallel to the changes to intellectual property law in the 1980's, research budgets saw relative decreases, which led to a climate of competition for resources among researchers (Fang & Casadevall, 2015), leading in turn to a drive to find finances from alternative sources such as industry, deepening the bonds between academia and capital (Hackett, 2014; Slaughter & Leslie, 1997). Although a close relationship between science and industry is by no means a new phenomenon (Rip, 1997), recent developments have seen codes and practices from industry be adopted within the academy and vice versa, shaped on both

sides by the logic of profit (Kleinman & Vallas, 2006). At the same time, increasing demands for accountability coupled with the continued insistence on autonomy has led to an increase of evaluative work for scientists in the form of peer-review. Moreover, concerns about bias have led to an increase of quantitative evaluation methods and concerns about the evaluation of societal relevance has led to an inclusion of non-academics in peer-review (Langfeldt & Kyvik, 2011). Thus, the accountability regime leads to an inclusion of non-academics both in the production and evaluation of research.

While policy for science is one aspect of the accountability regime, the other is science for policy (Elzinga, 1997, 2012). This reflects the idea that science has an obligation to provide decisionmakers with the knowledgebase required to make informed decisions. This idea is not new either, reflecting the views of Max Weber, seen as one of the founders of sociology, John Desmond Bernal (1939), both a pioneer in X-ray crystallography and an influential thinker in science policy, and John Dewey (1946), one of the most prominent American pragmatist philosophers. Indeed, the TD view of the need to shape the understanding of problems in accordance with those whom they concern resembles the methodology for policy-relevant research advocated by Dewey. The idea of a science for policy has led to the emergence of TD boundary organizations where science and policy are being served in tandem (Guston, 1999, 2001). Such organizations include the Intergovernmental Panel on Climate Change and the Intergovernmental science-policy Platform on Biodiversity and Ecosystem Services (Löfmarck & Lidskog, 2017), whose reports are used to inform environmental policy on a global scale. The presence of these organizations as actors in global politics shows that science for policy is not limited to a subservient 'fact-finding' relationship in service of particular political interests, but also opens for science to influence the policy agenda itself. The accountability regime thus enables influence between science and policy to go in both directions.

The influence of policy on science has been met with protests from the scientific community. The requirement to pursue societal relevance in order to secure public funding has resulted in some scientists heralding the death of scientific autonomy (Bhattacharya, 2012). Again, the idea that political steering undermines scientific quality is not new, having been argued as early as Michael Polanyi (1962), the originator of the term 'the republic of science'. On the other hand, proponents of TD stress the inadequacy of traditional disciplinary science to address problems that are relevant to the life-world of ordinary people (Hirsch Hadorn et al., 2008). Thus, the rhetoric arguing for TD is not only oriented around societal relevance, but also invokes the notion of scientific quality by emphasizing the more nuanced knowledge produced through TD.

2.3. Peer-review beyond disciplinary borders

In this section, I discuss the notion of peer-review as the most prominent institutionalization of epistemic peerage, focusing on the issue of peer-reviewing research that does not clearly conform to particular disciplinary standards. Peer-review has come to be seen as the *sine qua non* of scientific quality. As such, it is both criticized for its opacity and lauded as a core characteristic of science. Its origins are intimately tied up with the emergence of the scientific journal, and the institution still to some degrees reflect the historical context of this emergence.

Peer-review is one of the primary institutions that is to guard the autonomy of science. It is based on the idea that only scientists are competent to assess the quality of science. Thus, the process is used to inform decisions about science funding, article publication, tenure, and almost any other form of adjudication in science (Chubin & Hackett, 1990). Because it is such a central institution, scientists have a complicated relationship with it. On the one hand, it is the mechanism behind nearly every rejected application in a scientist's career, and the opacity and perceived arbitrariness of the process (Cole et al., 1981) leads many scientists to criticize the institution as not fit for purpose. On the other hand, because of the same opacity and the trust in the institution provided by society outside of academia it is also taken as the strongest bastion against the complete politization of science. To many, it has become a necessary evil. Others seek to improve the process in a variety of ways, for example through a wider base of inclusion (Healy, 1999) or through using bibliometrics as a complementary tool (Langfeldt & Kyvik, 2011).

Peer-review was first instituted alongside the introduction of the *Philosophical Transactions of the Royal Society*, commonly seen as the first scientific journal, in the 17th century (Chubin & Hackett, 1990; Csiszar, 2018). In order to be published in that journal, texts would first have to be reviewed by members of the Society, taken to be the epistemic peers of scientist authors. At the time, reports about scientific experiments and accounts of theories could be published in any periodical or journal, as there was not yet an established scientific profession which would be the given 'target audience' (Csiszar, 2018). Instead, interested readers could be found anywhere in the literate classes. Thus, the *Philosophical Transactions*, as well as subsequent pioneering scientific journals, when first launched would be competing with ordinary periodicals of general interest. Therefore, for both externalistic and internalistic reasons, the publishers of these early scientific journals were concerned that the published texts be as scientifically accurate as possible. Internalistically because of concerns about non-scientists writing accounts of science sloppily and without properly understanding it. Externalistically as a way of establishing a unique selling point

over other periodicals: only in these journals can the accuracy of the scientific information be guaranteed.

The founding principles of peer-review reflect the self-understanding of the members of the Royal Society. Most of these would be part of the landed gentry, financially stable and to a large extent idle. Among these, there was a high sense of trust (Porter, 1996). A gentleman with no wants, so was the view, could be trusted to tell the truth in matters where there was no personal connection, as he would not be negatively impacted by any of the outcomes. As such, trust plays a crucial role in the institution of peer-review. Because review is most often anonymous (although exceptions to this are increasingly common), the one being reviewed must trust that the reviewer is in fact competent to review and has no ulterior motives. Decision-makers who use peer-review to guide their decisions also must trust the reviewers to be competent. Reviewers also must trust each other's competences to be complementary in cases when research crosses boundaries. Thus, the relation of epistemic peerage involves trust that the judgments of the other are competent and honest.

The institution of peer-review has become perhaps the most prolific rhetorical tool in demarcating science from non-science. The rhetorical practice of defending the autonomy and authority of science is captured by the concept 'boundary work' (Gieryn, 1983). Rhetorically limiting the competence to assess science to a select group of epistemic peers serves to exclude non-scientists from certain decisions about science, namely those concerned with which science to recognize as valid and of good quality. This secures the autonomy of science to decide its own criteria of quality. At the same time, the idea that the epistemic peers are precisely those that are the most competent to judge condemns the (contrary) judgments of non-peers as amateur, ripe to be dismissed. This secures the authority of scientific knowledge within its own domain of research problems.

Because of the institution's fundamental ties with disciplinarity, peer-review of research that is 'groundbreaking' or in various ways goes against current disciplinary norms is often seen as problematic. There are concerns that such research is disadvantaged by peer-review (Luukkonen, 2012; Travis & Collins, 1991). At the same time, policymakers often seek ways to promote such research specifically (Heinze, 2008).

One of the most influential investigations of the practice of interdisciplinary peer review is that of Lamont (2009). Her work focuses on the process of peer-review from a micro perspective, and she argues for the central role of a few customary rules in governing the dynamics at this level. I discuss this aspect of her work more in section 4.1.1. Alongside Mallard and Guetzkow, Lamont also shows how evaluators use different epistemological styles to argue the qualities of proposals (Mallard et al., 2009) and how their understanding of 'good'

interdisciplinary research involves a balance between disciplinary breadth and methodological rigor (Lamont et al., 2006).

Many other studies focus less on the practice of reviewing and more on the different organizational features of peer-review. In the Nordic countries, funding is normally awarded by centralized research councils that include multiple disciplines. As such, many empirical studies of peer-review of discipline-transcending research have been carried out by Nordic researchers. Langfeldt (2001) emphasizes the importance of rating scales and budget restrictions for the possibility of promoting research that challenges established disciplinary standards. Coarser rating scales allow for more room to consider 'non-scientific' aspects of proposals, such as societal relevance, even when review of such criteria is not explicitly called for. Dedicated budgets for discipline-crossing research are also a way of promoting such research. Moreover, some researchers see their role as peer-reviewers as a form of disciplinary gatekeeper and are hostile to non-conforming projects. Policymakers should thus choose which peers they select and how they design the reviewing procedure in accordance with their policy aims (Langfeldt, 2006).

Huutoniemi (2012) explores different ways of coordinating expertise among peers in four different grant application review panels at the Academy of Finland. Proceeding from Lamont (2009), she shows a diversity of customary rules in dealing with the competence of other reviewers that vary according to panel. The most technical panel would aggregate competence, understanding it as additive and in practice consisting in spotting flaws according to disciplinary understanding. Panels with a wider range of included perspectives instead put increasing weight on deliberation and reappraisal of projects in the light of the different understanding of others. Huutoniemi concludes that discipline-crossing research can more easily be funded if review panels contain generalists rather than specialists, and if there is a broad representation of disciplines on the panel.

As policymakers increasingly call for discipline-crossing research, there are parallel calls for ways to evaluate the quality of projects with regard to their discipline-crossing nature (Bergmann et al., 2005; Klein, 2008). It is in these circumstances in particular that the issue of typologies and classifications come to the fore. Huutoniemi et al. (2010) suggest a typology that for purposes of evaluation takes into consideration the scope, type, and goals of collaborations. Collaborating disciplines can be conceptually close or distant. Collaborations can be juxtaposing or integrative, and center on data, theory, or methods. The goals of collaborating can be epistemological or instrumental. Mobjörk (2010) focuses on TD in particular, distinguishing between projects that include non-academics as mere consultants and those that include them as equal participants in research.

One of the central issues of contention in the peer-review of TD is the issue of evaluating societal relevance. Langfeldt et al. (2020) argue that there exist multiple notions of research quality that emerge from different sites, broadly understandable as research field- or policy-oriented. Either can encapsulate some notion of 'usefulness'. Bozeman and Youtie (2017) observe that the study of impact-evaluative frameworks is reasonably new, identifying a number of concrete theoretical and methodological gaps. Although various frameworks have been suggested to implement peer evaluation of societal relevance (e.g., Buxton & Hanney, 2008; Spaapen & van Drooge, 2011), reviewers can have contrary conceptions of what constitutes relevance (Samuel & Derrick, 2015).

Underlying this issue is the question of whether societal relevance is appropriate to evaluate through peer-review at all. Bozeman and Boardman (2009) argue that the question of what is in the interest of society as a whole, or what the 'social good' is, should not be decided upon by a group of isolated elites. I argue in section 7.3. that setting the task of evaluating relevance in itself presupposes a certain understanding of the concept, and thus that the discussion of its meaning is not completely open in peer-review, i.e., not decided unilaterally by these 'isolated elites'. On the other hand, this is because the notion of the 'social good' is circumscribed by the institutional mandate of an external actor.

3. Summary of included papers

In this chapter, I summarize the papers appended to this thesis and relate them to the overall project.

3.1. Paper 1: No "Real" Experts

This paper represents the most general philosophical form of the central problem I investigate in this thesis. It also presents a theoretical and methodological parting of the ways, suggesting two different approaches to the problem. Although it opens for cross-pollination between the two, they are in practice very difficult to combine.

The paper sets out to outline a commonality between two theorists in differing fields. This commonality is exposed when the two discuss the notion of expertise, albeit in very different contexts. Adam Elga (2007) examines the notion according to the epistemic norms of analytical philosophy. He thus constructs cases *a priori*, only including detail to the extent deemed absolutely necessary to test our intuitions regarding a specifically formulated question. To Elga, 'expertise' is relatively uncomplicated: experts are those we justifiably believe to possess superior reasoning ability and superior information, and therefore their judgments concerning their topic of expertise should be deferred to. The interesting question occurs when disagreements happen between those who are epistemic equals, or epistemic peers. Elga concludes that in order to take the opinion of the other into consideration, we must be justified in believing them to be our epistemic peer. However, in some politically controversial or philosophically complex issues we are unable to find such justification. By extension, in some cases there can be no expertise.

Sheila Jasanoff (2003b) engages in a discussion about how theorists in STS should view expertise. She responds to Collins' and Evans' (2002) suggestion that expertise should be viewed as an analyst's concept. Objecting to this suggestion, Jasanoff stresses that STS has viewed expertise as contingent on the institutional and political frameworks in which questions are raised. The questions that are interesting to theorists in STS, according to Jasanoff, are how social processes shape which competences come to be seen as relevant to a question and what understanding of the world comes to be taken as 'real'. The parallel with Elga consists in the idea that when it comes to making judgments in areas where different political or social outlooks result in different

understandings of the world, expertise is not a purely epistemic matter, i.e., there is no 'real' expertise.

This parallel suggests the most general form of the problem that informs this thesis. In these situations, there are no purely epistemic hierarchies, and therefore no epistemically privileged solution. Yet judgments still have to be made. How? The notion of epistemic peers provides two ways of approaching this question. On the one hand, it is possible to investigate at which point questions become so politicized as to not admit epistemic peerage, showing which questions are not to be decided by 'experts'. This would be congruent with analytical philosophy. On the other hand, it is possible to study instances where actors in fact hold each other to be epistemic peers, regardless of their differing political or social outlooks, to investigate the dynamics governing their judgments. This would be more in line with STS. My institutional and intellectual environment, as well as my theoretical interest, has led me to take the latter approach.

Formulating the problem in this way allows me to home in on the kinds of cases that would likely provide the most valuable insights. In science, the notion of epistemic peers figures most prominently in the institution of peer-review, where reviewers are the peers both of each other and those under review. Instances within science where both political and social outlooks are radically different are likely to be found in TD research, where participants are included not only from widely different academic fields, but also potentially from fields outside of academia. Thus, I should investigate cases relating to peer-review in TD settings.

3.2. Paper 2: The Grand Concepts of Environmental Studies

In this paper, I explore actors' uses of a central concept, boundary objects, as a point of contact between my object of study and my theoretical framework. On the one hand, this paper represents a birds-eye look at TD through the theoretical lens of boundary objects, allowing me to distinguish two relevant kinds of cases. On the other hand, the paper both studies and exemplifies the relationship between my own field and the fields I study.

The paper is a review of the literature about the use of boundary objects in interdisciplinary and TD research, focusing especially on instances where concepts were considered to be boundary objects. Boundary objects are objects that figure in collaborations where there is a lack of consensus about meaning (Star & Griesemer, 1989). These objects have a vague meaning that is shared between social worlds, but also different complex meanings in each social world. They therefore facilitate collaboration without the need to enforce a common

understanding of the world. I conclude that the use of conceptual boundary objects in TD falls into two overlapping ideal types. Grand concepts are oriented towards policy, with the goal of effecting societal change. Examples include 'sustainability' and 'resilience'. Hubs-and-spokes figure in localized collaborations, their purpose being to stabilize these collaborations. The same concept can be a grand concept on the macro scale and a hub or spoke at the local scale.

The two uses of conceptual boundary objects alert me to two different kinds of aims in TD research, aims towards which judgments about production and recognition of science are directed. One is the realization of societal change, or the idea of 'impact' or 'societal relevance'. The other is the stabilization of one's own project or field. Accordingly, my two cases, in addition to focusing on either production or recognition, focus on either relevance or stabilization.

The paper is published in an environmental studies journal. It thus both investigates and consists in an overlap between the domain of the analyst and the actor. Because it conforms to the norms of writing that allow it to be published in that journal, the paper differs from what it would be if I had chosen to publish in a journal more clearly in my own field. This actualizes the question of for whom our research is conducted. It has been fruitfully argued that even symmetrical descriptions can be of use to actors (Vikkelsø, 2007), and thus that we need not necessarily choose between impact and symmetry. The wide proliferation of the concept 'boundary objects' is indicative of this. However, even if the quality of our analysis does not suffer, choosing to aim our research towards actors does alter the content of that research in some way. The significance of this paper for this thesis, and the significance of actor-oriented research in general for the theory of science, thus has to come from the wider theoretical implications that can be drawn in relation to other works.

Apart from the distinction between grand concepts and hubs-and-spokes, the central theoretical implication from this paper in relation to the overall project is the significance of the concept of thinness. During the literature review, it became clear that although much discussion has been held about the interpretive flexibility of boundary objects, the nature of the vague commonality that allows for collaboration has been little explored, except for cautions that outright ambiguity could be an obstacle to such collaborations. For that reason, I suggest the notion of thinness as a way of conceptualizing the kind of vagueness that helps facilitating collaboration. Thinness is to be understood as the opposite of 'thickness' as used by Geertz (1973). It consists in descriptions that strip away layers of cultural, social, and historical meaning. Thinness is thus a decontextualization of the object. It figures in the pidgin languages in trading zones (Galison, 1997), where the deeper meaning of objects needs not be agreed

upon, only the willingness to exchange them (Galison, 2010, p. 32). Thinness allows for a core of vague yet determinate meaning to remain and be shared between social groups, each of which have their own thick understanding of the object.

The concept proves useful in analyzing several features in both subsequent case studies, as seen in the discussion chapter.

3.3. Paper 3: Unity through disunity

This paper presents the first case study of this thesis. It concerns a case where the focus is on judgments about the recognition of works or approaches as belonging to one's (TD) field. The case also investigates the discursive elements involved in the continuous stabilization of a TD collaboration. I argue that values that are shared but differently emphasized among actors are not a source of tension in themselves, but that tensions arise when these are operationalized in the field's institutional and intellectual environment.

Ecological economics is a field that emerged in the late-1980's-early-1990's as a response to the perceived inadequacies of the mainstream in economics and ecology in dealing with the environmental crisis arising from humanity's economic activity. Central to the critique of these mainstream approaches is their reliance on a narrow set of formal methods and problem definitions, and thus ecological economics set out to remain methodologically pluralistic to its core. Mainstream economics in particular is perceived as problematic, and ecological economics joins numerous other heterodox economic approaches in critique of this mainstream (Lee, 2009). However, the mathematical formalism of mainstream economics rhymes with biophysical approaches to the problem, approaches that are recognized as relevant to ecological economics. Moreover, the language of mainstream economics is effective when communicating with societal decision-makers, making it easier to achieve societal relevance if that language is adopted.

I interviewed editors and editorial board members of the journal *Ecological Economics* in order to investigate the disciplinary discourse guiding the perception of what the field should be, and which works belong in the field. I found that actors hold the discipline-crossing character of the field to be central to its identity and use mainstream economics as a contrast to highlight the perceived strengths of their field. Actors hold that the strengths of ecological economics consist in its theoretical robustness, breadth, and open and dynamic nature, and embrace the scientific values of consistency, societal relevance, and novelty.

The agreement between actors about the strengths and values of the field concerns the thin notion of these concepts. As such, tensions can arise when the

differing complex understandings of these concepts suggest different ways of operationalizing them. Most prominently, there are strong disagreements about how to relate to approaches that incorporate elements of mainstream economic theory. Some hold that breadth and openness mean that no approach should be excluded *a priori*. Others hold that theoretical robustness must involve the exclusion of (perceived) poor theories. Some hold that societal relevance needs to be realized through concrete policy suggestions. Others hold that societal relevance consists of a fundamental critique of the existing social system, and thus that policy involvement helps perpetuate a system that rightly should be fundamentally challenged.

Such tensions, however, arise only when actors are faced with the concrete situations in which the differing conceptions matter. The dilemma over whether to include mainstream economics is actualized through on the one hand its close relationship with policy and on the other hand the incentive structure of academia. Mainstream economics is a populous and prestigious field. Much of that prestige comes from publication in highly rated journals. Seeing as *Ecological Economics* is highly rated, mainstream economists seek to interact with it and the field in order to gain the prestige of having published there. If mainstream economists weren't drawn to ecological economics by its prestige, and if most policy weren't intimately tied up with the principles of mainstream economics, the tension between the fields need not have actualized as the dilemma it has become for ecological economics.

Thus, tensions within ecological economics arise not from a difference in values, or solely through different emphasis on shared values, but through the interaction between these values and the intellectual and institutional environment of the field. Actors in this case saw this environment as static relative to their actions. Although they see their environmental conditions as problematic, they do not consider themselves able to affect these conditions through their roles as editors and editorial board members.

3.4. Paper 4: Cutting across quality and relevance

This paper presents the second case study of this thesis. Here, focus is on decisions concerning the future production of science, where the societal relevance of that science is at issue. The core of this paper concerns the question of whether and how one can be 'competent' to assess the societal relevance of proposed scientific research, comparing it with the notion of being competent to assess scientific quality. I argue that actors' understanding of what it means to be competent to assess, and subsequently also the meaning of 'societal relevance', follows in part from the institutional setup of the review panel.

I investigated two TD peer review panels at the Swedish research council Formas by interviewing panelists after the proposal reviews had concluded. These panels assessed proposals submitted to directed calls and included academics from a variety of fields as well as practitioners. Practitioners and academics were given the same set of instructions, and practitioners were thus included as participants rather than consultants (Mobjörk, 2010). Indeed, I found that the self-assessment of competence follows the same dynamics for both practitioners and academics and in two kinds of cases the competence of practitioners and academics was understood and recognized in the same way by the panel.

Panelists reported that self-assessment of competence consisted in them reading titles and abstracts of proposals and identifying the topics and approaches of those proposals. I conceptualized the topic of a proposal as a thin understanding of the subject of research and an approach as a way of doing things, such as a method or a theorization. When the panel assessed the perceived novelty of proposals, the topic functioned as a boundary object that facilitated a negotiation of meaning between social groups. In this instance, practitioners were treated as members of some group among others, in no particularly epistemically privileged or unprivileged position. Competence was understood as 'familiarity'. When the panel assessed the approaches of proposals, panelists would defer their judgment to the one perceived as most experienced with the approach. Practitioners could be deferred to concerning approaches to implement or communicate results. Competence was understood as 'expertise'. However, actors maintained a distinction between the content of the competence of practitioners and academics, mainly concerning a lack of knowledge of the scientific literature and methods among practitioners.

The concepts 'scientific quality' and 'societal relevance' follow along the same lines. When assessing novelty, they are not discussed separately, and it is unclear whether there is a practical distinction between the two. When relevance is taken as communication or implementation, it admits of expert judgments the same as the quality of scientific approaches.

This case alerted me to the significance of an institutional framework for demarcating TD collaborations. Although the review process is open to practitioners outside of academia, these must have a PhD or equivalent in order to be included. There are thus many with legitimate stakes in the issues that cannot be included in the reviewing process. Moreover, Formas had a hand in shaping the understanding of practitioners through their institutional setup, the instructions provided, and their collaboration with chairs. However, there were some indications that panelists could also give feedbacks.

4. Theory: Delineating the meso-level object

In sociology, the distinction and interaction between the micro- and macro-levels of analysis is well-established (Giddens & Sutton, 2017, pp. 24–25). Sociological macroanalysis concerns large-scale social structures and long-term processes, such as entire political and legal frameworks or nation- or worldwide economic or infrastructural changes. Microanalysis on the other hand deals with direct interactions between individuals, or individuals and objects. It is commonly recognized in sociology that everyday life is influenced by both levels, forming a middle, or meso-, level, and much applied sociological research utilizes meso-level frameworks. Early institutionalist sociologists of science, such as Robert Merton (1973), paid much attention to the macro-level structures of science. However, in STS there is a core of theorists and paradigmatic works that focus exclusively on the micro-level. Towards the end of my work on this thesis, I found that the thus-inspired micro-oriented approach that I applied in each empirical case study was insufficient to discuss the issues that emerged when I related my cases to the theoretical work I had done earlier. To discuss these issues, I need to sketch a meso-level where the conceptual and empirical results can meet.

Historically, STS has had an adversarial relationship with institutionalism. David Bloor, the founder of the Strong Program for the sociology of knowledge, defines his approach in explicit opposition to the institutional approach of Merton (Bloor, 1976). Bloor's criticisms focus on the idea that Merton seems to award science a special status among social phenomena, keeping the factors that drive scientists and scientific belief formation separate from ordinary life[2]. Instead, these factors should be examined through empirical case study, where we find the conduct of scientists to be influenced by a variety of competing systems of norms (Mitroff, 1974) and interests (Mulkay, 1976). Laudan (1982) analyses the dispute between Merton and early STS as one between different core theoretical assumptions and research orientations. To Mertonian institutionalists, science is analyzed as centering around broad consensus guided by overarching norms, and the core issue is how this consensus is maintained on the macro scale. This makes non-adherence to these norms a theoretically interesting problem.

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[2] Bloor's reading of earlier sociological work has itself been subject to criticism (e.g., Lynch, 1994, 2005; Pels, 1996). Merton's focus on the ambivalence of scientists in relation to fame and recognition (Hackett, 2005; Merton, 1973) shows that his analysis of scientists' motivations is not so universalistic as Bloor seems to suggest.

To early paradigmatic theorists in STS, science is instead analyzed through local controversy or uncertainty, and the central issue is how this controversy is closed on the micro scale (e.g., Collins, 1985; Latour & Woolgar, 1979). Non-adherence to overarching norms is in this perspective to be expected, and thus theoretically trivial, since actors are seen as driven by micro-level interests[3].

Although micro-oriented studies of science reveal important dynamics of how knowledge is produced, later theorists argue that this approach neglects the role of larger-scale political factors (Frickel & Moore, 2006; Gläser & Laudel, 2016). Frickel and Moore (2006) argue that a new political sociology of science is necessary to explain "why science works better or more often for some groups than for others" (p. 7). This school, they argue, should proceed from the insights made by micro-oriented STS to focus on structural dimensions of power in knowledge production in a reworked institutionalist framework. In this framework, institutions are understood as "relatively durable sets of practices and ideas that […] shape the contour and experience of daily life" (p. 8). A key element of the approach is the investigation of rules and rulemaking as fundamental to questions of inclusion and exclusion, as well as the dynamics of stability and conflict. In a similar vein, Panofsky (2010) attempts to reappraise Merton's approach in light of the theoretical progress made by constructivist STS, arguing that Merton's other theoretical interests show that his view of social structure admits of differences in interpretation and does not assume a uniformity of culture and function (Merton, 1968).

To investigate TD, I need to take into consideration the interaction between factors on both the micro- and macro-level. The dynamics underlying the functioning of TD collaborations need micro-level investigation, yet chapter 2 demonstrates that to understand why these collaborations exist and look the way they do one must attend to macro-level political and theoretical factors.

In this chapter, I delineate the meso-level at which my empirical and theoretical investigations meet. I do this in two steps: first by moving upwards from the object at the micro-level, i.e., finding the higher-level social structure in which individual interactions take place, yet not so high a level as the entire framework of science-society-relations (as introduced in section 2.2.). Second by moving downwards from general ideas about TD as a large-scale theoretical structure to the dynamics of how such ideas come to be implemented in practice in collaborations. Through this two-way delineation I find concepts that help me highlight the continuities between the four papers. Finally, I explore the theoretical issues that inspired the need for a meso-level analysis.

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[3] In the case of Latour and Woolgar, 'interests' (and 'actors') are not even considered to be pre-existing, emerging only as a consequence of the structure of a micro-level network.

4.1. Moving upwards from micro to meso

In this section I will show how I relate the classical micro-oriented STS approach to the meso-level. The first sub-section explicates three central micro-oriented perspectives on boundary-crossing collaborations that each highlight one theoretical issue. In the second sub-section I discuss how these issues recur on the meso-level as part of the investigation of various organizational forms and their intellectual and institutional environment. Thus, I take the organizational form as the meso-level corollary to the micro-level of the social world. The organizational form of a collaboration refers to the practices and setup of that collaboration in terms of explicit and implicit rules. The environment I take to be actors, practices, or ideas that actors in a collaboration understand as subsisting outside of their own collaboration.

4.1.1. Collaboration across cognitive borders

Collaborations across cognitive borders in knowledge production has long been an object for the empirical study of science. One of the most influential case studies is Star's and Griesemer's (1989) investigation of the Berkeley Museum of Vertebrate Zoology using the 'social worlds'-framework (see section 5.1.). The collaboration required that the understandings and interests of different groups be translated in such a way as to contribute to the overall collaboration. Star and Griesemer argue that this was done in two different ways: standardization of methods and mediation through boundary objects. On the one hand, methods of collecting and sending specimen had to be standardized such that non-biologists could collect biologically interesting specimen without themselves knowing what 'biologically interesting' entails. On the other hand, the biologists needed a way of communicating which specimen would be interesting, not only how to collect them. Here, the notion of 'species' functioned as a boundary object, being understood differently in different social worlds yet also having a vague understanding that was shared between worlds. In paper 2, I explore the variety of boundary objects in TD as conceptualized by actors themselves.

Star's and Griesemer's focus when studying their case of border-crossing collaboration was thus on coordination through standardization and objects. Galison (1997) also proceeds from material objects, and the accompanying material cultures, in his study of the development of physics laboratories from the late 19th to the middle 20th centuries. Researchers in these laboratories incorporated technologies that were developed elsewhere, making them their own and adapting them to their needs. However, as technologies grow more advanced, and their use becomes increasingly esoteric, laboratories also need to incorporate the competence to operate them, competence stemming from different social worlds. This led to a focus on the issue of communication. Taking

inspiration from anthropological linguistics, Galison observed how collaborators developed various 'trade languages', pidgins, in order to facilitate the exchanges. These pidgin languages exhibit the kind of thinness explicated in paper 2 and would thus not incorporate the evaluative connotations of collaborators' 'native' languages, letting them agree to a 'this for that'-trade without needing to understand or accept the value assigned to the objects of trade (material instruments or competence) by the other.

While Star and Griesemer study coordination, and Galison studies communication, consensus-making in border-crossing collaboration has been influentially studied by Lamont (2009). Lamont investigates the peer-review process at various interdisciplinary panels assessing tenure applications in the social sciences and humanities. As the calls were interdisciplinary, panelists were tasked with assessing applications that were not necessarily from their own discipline. Lamont observed that conduct on these panels was guided by unwritten customary rules. One of these rules was that when assessing applications from other disciplines, panelists would strive to maintain disciplinary sovereignty. The questions and approaches undertaken in applications should thus ideally be assessed according to the standards of the discipline in which the application was formulated. An application in history should be evaluated according to the norms of history. Panelists thus attempted to adopt different systems of norms to the extent possible. However, inevitably personal sensibilities and outlooks would play some part in some assessments. The dynamics identified by Lamont of recognizing assessment competence in various forms are central to paper 4.

The micro-level dynamics of TD collaborations thus involve various aspects of coordination, communication, and consensus-making.

4.1.2. Organizational forms of collaboration

In the US, the coordination of knowledge production and public policy have been extensively studied. One of the central theorists in this tradition is Jasanoff, who argues that the boundaries between 'science' and 'politics' are negotiated through organizational arrangements (Jasanoff, 1992, 2003b). The question of who is to be recognized as a scientific expert is resolved through the relevant public's expectation of what expertise and knowledge is to deliver: the civic epistemologies (Jasanoff, 2012). Thus, different organizational forms with different epistemologies can coordinate collaborations in different ways (Jasanoff, 1997, 2003a), depending also on their institutional and intellectual environment. In the same vein, Guston captures a class of organizational forms that have emerged to stabilize a porous boundary between science and politics in the concept of boundary organizations (Guston, 1999, 2001). These act as the agents of both science and politics, creating boundary objects that facilitate the

satisfaction of the interest of both sides. Such boundary objects can be patents in a commercial context (Guston, 1999) or reports that form the basis for policy (Keating, 2001). The key feature of this class of organizational forms is the dual accountability to both sides of the science-policy-divide. This is an intellectual backdrop to paper 2, where grand concepts can be argued to be part of opening the possibility of such collaborations.

In collaborations aimed at the production of knowledge, issues of communication go beyond interpersonal exchanges. The knowledge products themselves are ideally to embody a hybrid and mutual form of knowledge that is also communicable beyond just one side of the collaboration. Holbrook (2013) notes that the underlying philosophical problem of communication across cognitive borders in settings of interdisciplinary or TD knowledge production is discussed in the literature in terms of the integration of knowledge. When discussed as a large-scale theoretical phenomenon, the notion of integration remains general and vague (O'Rourke et al., 2016; Pohl et al., 2008; Repko, 2007). However, the discussion is informed by various case studies (e.g., Grüne-Yanoff, 2016; O'Malley, 2013; Vilsmaier et al., 2015; Zaucha et al., 2016). The different forms, goals, and properties of these cases allow for the construction of typologies of integration alongside typologies of discipline-crossing collaboration (Huutoniemi et al., 2010; Klein, 2012, 2017; Mobjörk, 2010). These typologies relate the conceptualization of integration to the organizational form of the collaboration. Thus, what is to count as successful integration—and successful communication—is dependent on the goals encapsulated in the organizational form. The goals and conceptualization of integration will of course depend on the collaborators' own situation in their intellectual environment as well as the institutional mandate for the collaboration. Such variations in the conceptualization of integration are an important part of the analysis in paper 3.

Although Lamont (2009) and colleagues (Lamont et al., 2006; Mallard et al., 2009) were among the earliest to investigate consensus-making on the micro-level of peer-review, there have both before and since been a host of studies investigating the effect of different organizational forms on the outcomes of peer-review. Langfeldt (2001), in a study of grant reviews at the Norwegian Research Council, found that budgets, rating scales, and ranking methods could affect the outcome of review. Policymakers can attend to these formal organizational factors in order to promote interdisciplinary or groundbreaking research (Heinze, 2008; Langfeldt, 2006). Huutoniemi (2012) shows how the disciplinary intellectual environment of reviewers interacts with the setup of review panels to create variations of Lamont's (2009) customary rules. In the same vein, Laudel (2006) argues that researchers studying peer-review should apply an institutionalist framework in order to study the processes by which

particular constellations of actors construct knowledge in each situation. These issues constitute the theoretical backdrop of paper 4.

Thus, the issue of coordination that at the micro-level is captured by the concept of boundary objects recurs at the meso-level in the form of boundary organizations. The issue of communication that is conceptualized as pidgin languages in micro returns in meso in the guise of integration. Consensus-making, finally, that at the micro-level is conceptualized as customary rules between peers instead is captured in the question of the formal organization of evaluative bodies. The theoretical focus on the organizational form is thus a move upwards that allows for the transfer of theoretical issues and insights from the micro-level of the social world to an organizational meso-level.

4.2. Moving downwards from idea to institution

In this section, I present the institutionalist thoughts I use to capture how ideas about large-scale theoretical structures, such as the notion of TD itself, give cognitive content and meaning to organized action on the meso-level. Although they are abstract, these ideas shape the experience of the everyday of collaborative work. The content of these ideas themselves is not immutable or universal, and the dynamics of changing collaborations is also the dynamics of the institutionalization of different understandings. In the first sub-section, I present some of the ideas about TD as a general macro-level phenomenon that emerged as relevant during my investigations. In the second sub-section, I discuss how such ideas have been argued to interact with micro-level dynamics through institutionalization.

4.2.1. General ideas behind TD

In this sub-section I explore some of the general ideas about TD and other discipline-crossing research that emerged as relevant during my investigations. These take the form of scientific values and different conceptions of roles. I take scientific values to be any normative beliefs about what is good or right about knowledge or the process of its production. Roles I take to be actors' beliefs about which actions are appropriate to take by certain designated groups, and how it is appropriate to act towards them in turn.

More than a problem of communication, integration is commonly described as one of the core values of TD and interdisciplinary research (Bammer, 2013; Hackett & Rhoten, 2009; Klein, 2012, 2017; Pohl et al., 2008). It is the integration of different forms of knowledge into new, more nuanced and apt forms that is taken to constitute the superiority of TD knowledge over its disciplinary counterpart. However, the vagueness of the term allows for a variety of interpretations (O'Rourke et al., 2016), some with far-reaching unifying

ambitions (e.g., Spash, 2012), others aimed at local problem solving (Bergmann & Jahn, 2008; Jahn et al., 2012). In paper 3, I discuss these different interpretations as reflecting the different scientific values of consistency on the one hand and novelty on the other (c.f. Weingart, 2000). Parallel to the idea of integration as a value of TD is the notion of societal relevance. However, this idea can also be interpreted in different ways. On the one hand, societal relevance can be seen as strict policy relevance, where the aim is to produce concrete policy suggestions or knowledgebases (e.g., Pohl, 2008). The case in paper 4 reflects this idea, as Formas seeks to fund research that pertains to concrete policy in the form of sustainable development goals. On the other hand, relevance can be interpreted as a more fundamental critique of the greater social system[4] (e.g., Butler, 2009). If understood this way, alignment with policy can be seen as undermining this deeper form of societal relevance, as argued by some ecological economists (e.g., Spash, 2013b) as discussed in paper 3. Integration and societal relevance thus represent values that proponents of TD argue make this form of research superior to conventional disciplinary research.

The inclusion of non-academics in the production of scientific knowledge invites questions of how their knowledge-claims should be related to those of scientists. Mobjörk (2010) discusses various ways of conceptualizing the role of non-academics in TD collaborations. On the one hand, they can be seen as equal participants or as consultants. This affects whether they are allowed to have a say in all aspects of knowledge production or have a circumscribed set of issues that 'concern' them, usually excluding questions of 'scientific quality'. On the other hand, they can be included based on their rights as stakeholders or based on their perceived complementary practical knowledge (c.f. Stirling, 2005). This relates to the question of whether the primary aim of inclusion is taken to be to make marginalized voices with legitimate stakes heard or to improve the quality of knowledge produced. This question of the circumscription or equal participation of non-academics is at the core of paper 4. TD research thus highlights questions about the roles of scientists and practitioners/stakeholders.

4.2.2. Institutionalized ideas on the meso-level

Although general ideas can be discussed as abstract, they are also implemented in various institutional structures. Individual interactions take place within these structures, and the actors within them deploy various general ideas to make their interactions meaningful and to further their interests. The implementation of general ideas thus depends on interpretation by actors.

Chubin and Hackett (1990) demonstrate how the institution of peer-review is differently conceptualized and rhetorically circumscribed in different contexts.

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[4] I.e., capitalism in this case.

Although they present a list of possible criteria for evaluating peer-review processes (pp. 43–48), they show how different criteria are emphasized in different contexts by different actors depending on their role. In the grant application context (pp.49–81), the stakes are the money needed to conduct research. Here, peer-review represents a struggle between the norms of researchers and policymakers, where the former seeks effectiveness and autonomy and the latter efficiency and accountability. In the context of manuscript peer-review (pp. 83–123), the stakes are not money but recognition. Here, there is a tension between the practical and symbolic functions of peer-review. While symbolically the process is to represent the abstract ethos of science and its pure interest in the production of knowledge, its practices reveal a negotiation between the interests of authors, reviewers, and editors, interests that do not necessarily align. The concern of authors is not with the autonomy of editors and reviewers, but the validity of their judgments (and often the lack of accompanying accountability). Thus, the same person can in the role of researcher as opposed to policymaker (designated as epistemically inferior) see the function of peer-review as autonomy, and at the same time in the role of author as opposed to editor (designated as an epistemic peer) hold autonomy to be a dysfunction of peer-review. Both paper 3 and paper 4 exemplify how actors in different roles and situations relate differently to peer-review, and to the understanding of their own role.

Panofsky (2010) makes four observations when interpreting the Mertonian question of ethos and autonomy in light of constructivist STS (pp. 155-156). First, both the ethos and autonomy of science do not exist only as abstract notions but are deployed by actors on the micro-level in a struggle for meaning and power. Second, following from the first, the characteristics of the ethos and autonomy of science thus vary according to institutional context. Third, roles and tendencies in relation to the ethos and autonomy cannot be assigned based on our prior assumptions on who is on the 'inside' and on the 'outside' of science. Finally, the struggle over science is not only about rewards and recognition but can also be about anything from the content of knowledge to the very social structures in which struggle takes place. The ethos of science is thus malleable on the institutional level, available as a rhetorical resource for actors on the micro-level. The perceived values of science are thus according to Panofsky used in a rhetorical struggle for power and stability. One conclusion to draw from paper 3 is that it is not only the ethos of scientists that is deployed this way, but also the values of different forms of knowledge and knowledge production. Thus, it is not only the virtues of scientists, such as universality and disinterestedness, but also the virtues of knowledge, such as integration or societal relevance, that are used as rhetorical tools for the safeguarding of autonomy (c.f. Gieryn, 1983).

Thus, abstract ideas get deployed by actors to shape the meso-level of meaningful and durable social interaction. In this investigation, the kind of ideas studied are on the one hand values, such as the novelty of knowledge or the ethos of science, and on the other hand the conceptualization of different roles in collaborations, such as experts, peers, practitioners, academics. It is through these ideas that collaboration and change is made meaningful. This form of institutionalism thus allows for a downward move from abstract and general ideas to a cognitive meso-level.

4.3. Central theoretical issues

In this section, I present the theoretical issues that emerged as I first attempted to explicate the commonalities across the four papers. The first issue is boundaries in TD; on the one hand the micro-level issue of collaboration across boundaries within a TD collaboration, on the other hand the macro-level issue of demarcating the boundary between TD and its counterparts. The second issue concerns the question of which particular issues are open to discussion within TD collaborations and which remain closed; a question some answers to which are to be found on levels beyond the micro-level.

4.3.1. Boundaries in TD

Because TD is my object of research, the notion of boundaries emerged as an important theoretical reference frame. Both analysts and actors use this notion to conceptualize disciplines and the practice of crossing them. The notion of boundaries is an evocative metaphor in the social sciences (Lamont & Molnár, 2002). Boundaries can denote both symbolic, conceptual separation and social, material separation. As soon as there is more than one entity, and an interaction between these entities, there is the boundary between them. Thus, analyses of different classes, ethnicities, social worlds, or thought-styles all suggest the existence of boundary phenomena happening at the points where they interact. This is also the case in the theory of science, and in this thesis.

In theory of science and STS, the study of boundaries focuses on the nexus of meaning, power, and diversity of knowledges. Generally speaking, there are two directions of research. The first follows from the classical macro-level interest in demarcation (e.g., Popper, 2002). The analyst's interest is however no longer in finding and enforcing some demarcation of science, but rather the fact that actors do establish and enforce such boundaries in practice themselves. This is captured in the notion of boundary work, the rhetorical practice of arguing the superiority of science (or one's field within science) over other belief systems (Gieryn, 1983). The other direction instead takes micro-level collaboration as its starting point, as discussed in section 4.1. This direction of boundary research

studies instances of successful collaboration in the absence of a consensus of meaning (Star & Griesemer, 1989). Here, the notion of boundary objects is a key analytical tool.

For TD, both directions need to be taken into consideration. TD is perceived as being collaborations between different ways of knowing yet at the same time strives to demarcate itself as superior to traditional research (Funtowicz & Ravetz, 1993; Hirsch Hadorn et al., 2008). This can be seen in both empirical cases. In ecological economics, researchers from a variety of fields attempt to cross-pollinate their research, but the identity that unites them is found through their insistence on their superiority to mainstream economics. At Formas, proposed research is ranked in order to prioritize which science is valuable enough to get made and which not, but this demarcating activity presupposes a recognition of epistemic peerage between members of different fields. The way that collaboration is coordinated, with communication and consensus-making, relates to the organizational form of collaboration. Demarcation, meanwhile, is the way that a collaboration distinguishes itself from its institutional and intellectual environment.

4.3.2. The 'factual' and the 'political'; or 'closing' and 'opening'

This sub-section follows up on a question that is stated in paper 1, and that is illuminated by the empirical cases. This question concerns which issues are understood as 'political', i.e., open to discussion, or 'factual', i.e., where the judgment of one or a few is taken to reflect a non-political 'fact of the matter', closing discussion. The closedness of an issue represents a fixed institutionalization of an idea.

When investigating the relationship between science and decision-making, it is important to pay attention to the dynamics of the opening and closing of issues (Stirling, 2005; Wynne, 2003). The closing down of an issue as natural or purely factual reinforces and justifies structures of power and domination through the disciplining of the mind (Foucault, 1977, 1982). Those things that are established as factual are not to be objected to, seen as part of a natural order. Thus, in democratic societies, facts are that which is seen as rightly outside the limits of deliberation. However, this means that they also constitute the common ground on whose basis other discussions can be held. Openness, a rhetorically powerful tool when professing adherence to democracy, can thus both expose the contingent status of perceived unjust structures and be used by powerful interests to undermine the factual resources of social groups (Proctor & Schiebinger, 2008).

In TD, one of the core assumptions is that in key stages of problem formulation and interpretation of results, 'political' values cannot be separated from the 'scientific' process (Funtowicz & Ravetz, 1993). In areas of high uncertainty and high stakes, it is argued, decisions cannot wait for the slow deliberations of normal science to establish facts in the conventional manner. In addition, the argument continues, the very formulation of research problems and solutions needs to take influence from the understanding of those whom the results and solutions concern (Bergmann & Jahn, 2008; Hirsch Hadorn et al., 2008). This results in a strive to keep open such things as would in other contexts be regarded as closed facts. The openness of the formulation of understandings to stakeholders' perspectives is seen as the precondition for their meaningful participation (Elzinga, 2008; Mobjörk, 2010). At the same time, perpetually open deliberations may delay the decision-making process (Collins & Evans, 2002), allowing powerful actors to use their influence to prolong discussions that would otherwise close against their interests (Michaels & Monforton, 2005; Oreskes & Conway, 2012). This shows that while the rhetoric of TD leans towards openness, closedness is often necessary to achieve the goals TD collaborations set for themselves. TD research must therefore be sensitive to the question of which issues to close and which issues to keep open.

5. Epistemology and methodology

In this chapter, I explicate the epistemological and methodological frameworks I use as a theorist of science pursuing middle range applicability of results. Reflecting the history of theory of science at the University of Gothenburg, these frameworks have influences from both neo-Kantianism and sociologically oriented STS. The frameworks also represent a blend of theoretical and empirical research orientations.

5.1. Hermeneutical-dialectical epistemology

In this section, I present the underlying epistemology that informs my investigations.

The confrontation with logical empiricists by early theorists of science at the University of Gothenburg came about because of the openness of theory of science to hermeneutical-dialectical approaches to the study of science (Heidegren, 2016; Törnebohm, 1985). In this view, science should not be studied as a purely logical structure. However, the view also holds that empirical data about science are not always simply given. Science, as a cultural and meaningful phenomenon, must be studied through interaction and interpretation. My own epistemological outlook arrives at this conclusion via two separate paths, one German and one American.

The German path starts with the philosophical problem of the object. Cassirer argues that the objects of consciousness, the objects about which we can have knowledge, are created through acts of signification by consciousness (Cassirer, 1971). The fixation and unity of objects is achieved when human consciousness assigns meaning to the sensuous substratum of objects in experience. The creative nature of this act, and the properties of the symbolic form being abstract and not corresponding to the sensuous substratum, means that different meaningful human activities create different systems of symbolic forms, such as different languages, myths, art, and various sciences. Thus, the structure of knowable reality follows not from the 'properties' some thing-in-itself but from the inner necessity of symbolic systems.

To Ludwik Fleck, these symbolic systems, or thought-styles, are realized through the socially organized act of cognition (Fleck, 2017). Our meaningful activities and their cognitive accompaniment are organized through various thought-collectives. Although collectives are formed anytime meaningful interaction takes place, e.g., in any conversation, some collectives are durable and

with elements of formal organization. As individuals, any act of cognition involving long established systems of symbolic forms means taking part in that system's accompanying thought-collective. This means that our cognitive acts are dependent on our entry into collective forms of life through schooling and initiation. These forms of life are the cultural spheres that assign meaning to the objects of experience. Studying science empirically as a cultural and meaningful phenomenon, then, means investigating the meanings that are being applied to reality by various cultural spheres, or thought-collectives, in science (Rickert, 1926).

Because I, the researcher studying constellations of actors in science from 'outside', am not part of the thought-collectives I study, my investigation must center around interpretation. My epistemological framework must therefore draw inspiration from hermeneutical tradition. Moreover, because my method centers around meeting and interacting with others who act within the cultural sphere I study, acts of interpretation go both ways. Before I interpret the other's response, they must interpret my elicitation, and if I then present my interpretation through a follow-up, then my interpretation is re-interpreted by the other. The outcome of investigation is thus dependent on the interaction between individuals from different thought-collectives, and because the object itself exists only through our culturally assigned meaning, the object itself is changed through our mutual re-interpretations. Thus, dialectical tradition becomes relevant to reflecting about the knowledge I produce through interviews. The knowledge I produce comes from a merging of cultural horizons, where the outlook of both researcher and researched have moved from their original understanding, changing the object itself (Gadamer, 2008).

If the German path is highly theoretical, then the American path is more empirical-methodological. This path starts with Geertz's view of ethnographic research (Geertz, 1973). In the face of a rising importance of quantitative methods in the American social sciences, Geertz attempted to find a role for qualitative studies in sociology. Rather than the demographic data produced by quantitative studies, the goal of qualitative ethnographic work should be to produce an understanding of the foreign actor; an understanding that is still a fair representation of the actor's own understanding. This can be achieved, according to Geertz, by using thick descriptions. Thick descriptions are descriptions that provide empirically rich accounts of the cultural and historic meaning of actions as understood by those that perform them. The understanding these descriptions provide is the end of research by itself.

Glaser and Strauss (2017) go further than the mere focus on description, holding that qualitative social science should also strive to generate empirically grounded theory. In contrast to earlier structuralist theory (e.g., Parsons, 1967), this theory should follow closely from empirical data. Thus, the basis for

theorizing should be thick descriptions. Theoretically significant categories should be derived from descriptions that are as close to the intention of actors as possible. However, in order to form theories, these categories have to be grouped and related by the researcher into second-order categories, meaning that even in this grounded theory there is an inevitable moment of interpretation. Thus, hermeneutical ideas can have a bearing on this type of theory.

Theoretically, actors are understood as participants in collectives that ascribe meaning to actions: social worlds (Strauss, 1978). Strauss sees his explication of social worlds as a synthesis and clarification of ideas that were prominent within the contemporary Chicago interactionist school of sociology and in Mead's theory of universes of discourse. Thus, communication is at the core of what defines a social world. Rather than formal membership, as in a union, or a membership based on empirical criteria, such as geographical proximity or ethnicity by itself, membership in social worlds is determined through interaction with other members. Moreover, this interaction is in the form of a certain discourse, a conceptual ordering of the world, that shapes the understanding of members of social worlds. The study of science should thus according to this epistemological view proceed through interpretation of thick descriptions of actions within, and sometimes by members of, the social worlds of science (Clarke & Star, 2007).

Epistemologically, the 'social worlds'-framework proceeds from the American Pragmatist school, primarily Dewey (e.g., 1939) and Mead (e.g., 1927). Central to this school of philosophy is that our understanding of the world proceeds not primarily through abstract reasoning but through practical problem solving. It is thus through goal-oriented engagements that we learn about the world. Our knowledge is thus shaped by the kinds of problems we perceive as relevant and possible to solve, and thus our understanding is fundamentally tied up with a pragmatically oriented perspective. This insight was developed into the sociological school of symbolic interactionism. In this school, humans are understood as living in a social reality, where objects and their properties are shaped by meaningful interaction based on this perspective. In the 'social worlds'-framework, then, objects and properties are taken to exist on the basis of the discursive universes of social worlds, and knowledge about them thus must be derived from the meaning ascribed through symbolic interaction by their members. It is thus the meaning-making of members of social worlds that is to be investigated.

Although there is much text produced by scientists, and oftentimes very detailed archives of scientific activity, if we want to study science as it is produced an invaluable resource is the scientists themselves. We can elicit thick descriptions directly from our actors by means of interviews. This means that we

need theory and methodology about interviews for qualitative research (Charmaz, 2014; Kvale & Brinkmann, 2014)[5]. One crucial insight is the special nature of the interviewing situation. By posing questions from an outsider's perspective, we as interviewers are sometimes able to turn actors' attention towards phenomena they have not previously reflected on. Their answers thus cannot be only knowledge that they already possessed. Moreover, when actors respond to our questions, they do so with the intention of making us understand what they mean[6]. Thus, they re-interpret their own experiences in a form they believe will get through to us. The knowledge produced through interviews is thus determined through the interviewing situation, where the understanding of interviewer and interviewed interact. It is thus fruitful to understand qualitative interviews from a dialectical perspective.

Interestingly, the idea of the object as socially determined and interpretively flexible that in the German path started as a philosophical problem emerges in the American path as an empirical result (Star & Griesemer, 1989)[7]. Thus, the German path that starts from a theoretical insight about the object ends up with a suggested methodology, while the American path that starts with a suggested methodology ends up with a theoretical insight about the object. Both suggest a hermeneutical-dialectical approach to the empirical study of science.

5.2. Methodology

Theory of science should strive to do more than just describe singular situations (Törnebohm, 1985). Although qualitative case studies are notorious for a lack of generalizability, and abstract theorizing for potential lack of concrete applicability, my aim is to produce insights of a middle range (Merton, 1968), balancing empirical detail and generalizing abstraction (Boudon, 1991) in such a way that they are transferrable to a host of similar contexts. Geels (2007) holds that a middle-range theory in science studies should be limited to particular themes and topics, use only a few interrelated concepts, and link these abstractions clearly to empirical cases. This requires a methodology that both takes the gathering of empirical data and the development of concepts into consideration.

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[5] Kvale is of course not an American theorist, but this methodological interpretation is very much in line with Charmaz's constructivist grounded theory.

[6] I am proceeding from the principle of charity here, assuming that my actors are not intentionally trying to deceive me. Naturally, this cannot always be assumed, e.g., in research where significant power is at stake.

[7] Note, however, that this particular study did not center around interactions with actors and is thus not an unambiguous example of a dialectical approach, although many subsequent studies utilizing the concept of boundary objects are.

5.2.1. Developing analytical concepts

In this sub-section, I present some of the methodological considerations that relate to my developing of analytical concepts. I make central conceptual developments for this thesis in papers 1 and 2. Although analytical philosophers have various methods that can be used for the engineering of concepts, my own process of developing them cannot be said to have adhered to any strict method or technique. However, there are issues concerning the process and validity of such conceptual development that I have considered.

Fleck (2017) accounts for the development of the concept of syphilis, parallel to the technical development of the Wassermann test. He shows how ideas from various areas influenced its development, from etiological ideas about disease vectors to folk-lore ideas about the significance of blood and religious notions of disease as punishment for sinful lifestyles. Yet although the history of the concept shows these influences, its meaning when used by practitioners of the Wassermann test is specific to their understanding of the phenomenon. The various ideas take on a specific significance in the thought-style surrounding the test. Fleck further argues that all communication between thought-styles entails that the receiving style alters the meaning of ideas to cohere with the existing mood of the style, although the style itself could in turn be altered by the incorporation of new concepts. The validity of new concepts (or developments of old concepts) within a thought-style is thus not reliant on staying true to a concepts original meaning, but on its specific adaption to the style.

The case of the concept of boundary objects as explicated in paper 2 reflects this insight. The survey of the many uses of the concept in various discipline-crossing contexts shows how different ideas linked to it are given different significance and prominence by different groups. Some emphasize the materiality of physical objects, others the immutability of data, yet others the malleability of models. The lesson here should not be that there is a 'correct' meaning of the concept, but rather that its meaning must cohere with the larger system of thought of each group that employs it.

In practice, my conceptual development proceeds through interpreting the meaning of concepts as either received from outside or currently applied within the analyst's thought-style—or cultural tradition to use Heinrich Rickert's (1926) term—and proposing and applying alterations to these meanings, i.e., showing how the concepts are understood and could be otherwise understood. Although analysts' concepts belong to our cultural tradition, our interactions with actors open for the possibility that actors learn of our understanding the way we learn of theirs. There can thus be mutual hermeneutical exchange between actors and analysts that can lead to a merging of cultural horizons (Gadamer, 2008).

In paper 1 I import the notion of an epistemic peer from analytical philosophy and strive to make it cohere with the general thought-style of the theory of science. I explicate the norms that inform the understandings of the concept in the cultural tradition of analytical social epistemology in order that these norms can be 'compensated for' when used in an STS or theory of science context. It is through compensating for the norms that do not cohere with the cultural tradition of STS that the concept can be adapted to the mood of that thought-style. In paper 2 I explore the uses of boundary objects in TD by means of a literature survey[8]. Because the concept this time originates in STS (or rather, this particular use of the concept does), focus is on the new uses of the term in this different context. My conceptual development takes the form of two ideal types that encompass characteristics of these new uses, feeding the new contextualization back into the originating thought-style.

Coherence with the cultural tradition of STS or theory of science entails that the validity of analyst's concepts stems from being potentially applicable to some empirical cases, although not necessarily through their being applicable to some *particular* case. Put another way, concepts must not contradict the norms of how empirical cases are to be understood. This check is to be performed by my own peers. One of the central norms in empirical science studies is the notion of symmetry. Although originating in Bloor's (1976) explication of the Strong Program for the sociology of scientific knowledge alongside a host of other methodological principles, the notion of symmetry has since come to stand for a general idea of epistemic even-handedness in regards to actors' contrary claims. We should not shape our investigation in such a way that the knowledge claims of one group of actors are privileged through empirical description alone. All actors should be subject to the same kinds of explanations, and all concepts should in principle be applicable to any actor at any point in analysis[9]. What symmetry prohibits, then, is the use of analytical concepts that because of their very structure can only ever be applied to one group of actors in each case. The prime example is the concept of 'truth'. If the worldview of one actor is 'true', then *a priori* the contrary worldview of another must be false[10]. Thus, concepts that rely on the 'truth' of some world-view are not coherent with the broader system of concepts in my cultural tradition.

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[8] The method and search terms used to identify the corpus are described in the paper.

[9] Of course, in practice not all concepts will apply, otherwise there would be no point to empirical investigation. Symmetry does not require that explanations be *the same* for all actors

[10] Analytically minded readers will notice that I've presupposed a unitary notion of truth. I hope that my exclusion of pluralistic understandings can be granted for the sake of argument.

5.2.2. Social worlds and grounded theory

In this sub-section, I explicate the methodological framework I use when theorizing based on interaction with actors. This framework is the 'social worlds'- framework (Clarke & Star, 2007) that is based on grounded theory and symbolic interactionism. I undertake this approach in papers 3 and 4. The epistemological considerations of this framework largely follow Strauss's (1978) explication (see section 5.1.).

Methodologically, the framework departs from Geertz's (1973) conception of ethnographic research. However, Geertz held that the aim of such ethnographic research should not be to generate any general theory about social phenomena. Strauss, alongside Glaser, thus developed the methodology of grounded theory as a way to theorize on the basis of actors' understanding (Glaser & Strauss, 2017). The core idea of grounded theory is the primacy of data collection. The researcher should first try to ascertain the meanings ascribed by actors, and only later try to figure out which parts of actors' understanding are 'significant' to the theoretical understanding of an issue. When theorizing, the categories used to capture significant phenomena should ideally be derived directly from actors' understanding. Only when grouping categories together into higher levels of categories can the researcher themselves judge which words to use. The methodology also advocates continuous re-evaluation of categories in light of new data, meaning that analysis and data gathering proceed in tandem. Although Glaser and Strauss are somewhat unclear about the nature of the knowledge produced through grounded theory, later theorists explicitly argue for a constructivist interpretation (Charmaz, 2014).

When I investigated my cases, I used interviews to elicit actors' descriptions of their own understandings of their fields and practices. I take actors' responses to be adapted to the interviewing situation, thus containing an important aspect of interpretation of (memories of) past experiences and events (Kvale & Brinkmann, 2014). This interpretation is shaped to fit the actors' purpose with the interview, which I, based on the principle of charity, take to be to achieve a mutual understanding with me of the phenomenon I investigate[11]. Because actors' own understandings of which aspects of cases are significant and which categories are related are prioritized, interviews cannot be overly structured. I thus in both case studies used four or five general lines of inquiry, deviating from these if actors brought up different topics. My analysis in each case proceeded in two steps. The first round of coding was used to identify generally significant categories. These codes were then used to identify possible lines of analysis, or

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[11] Indeed, many actors directly express the sentiment that they take interviews as an opportunity to reflect on the issues we discuss.

themes around which to group the next round of categories. The second round of coding then focused on only the parts of transcriptions that dealt with the themes I judged as relevant to my analysis. In the second round, I grouped categories together to interpret the relationship between them and form second-order categories[12].

I used the sensibilities of my responding actors to check the validity of my empirical descriptions. I would derive categories from the transcribed texts of earlier interviews and introduce these in subsequent ones if the flow of conversation permitted. This allowed me to check whether these categories, judged by me to be significant to some actors, were also significant to subsequent actors. I continued to check my descriptions against the sensibilities of actors throughout analysis by sending excerpts and summaries. If respondents are agreeable to my descriptions, then that strengthens their validity[13]. Submitting manuscripts to actors' journals is a continuation of this check of the validity of my empirical descriptions, as the reviewers for those journals are (or at least should be) part of the same social world yet not part of the data collection. This was particularly salient for paper 3, where reviewers (I assume) were the literal colleagues of my respondents.

5.2.3. Case selection and range

In this sub-section, I explore the range of my investigation. In order to clarify what the middle range consists of I discuss the kinds of cases where the results could be applicable. I do this by exploring the important commonalities and contrasting properties of the empirical cases I have investigated.

Both of my cases are from the general area of sustainability, an area that because of high stakes for both humanity in general and specific groups of actors, paired with frequent collaborations between a wide variety of disciplines and actors, is a paradigm case of TD. A paradigm case, according to Flyvbjerg (2006), is a case that highlights general features of the phenomenon under study. Although sustainability exhibits the above-mentioned general features of TD, it nevertheless has characteristics that set it apart from other TD fields. Although

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[12] There are more detailed descriptions of the design of my case studies, including selection of respondents, questions asked, and lines of analysis, in the respective appended papers.

[13] Of course, there are potential issues with actors' memory and availability. In some instances, actors were initially hesitant to comment on the validity of my interpretation of their words because they could not remember the context in which they said them, but in each of these instances some further contextualization was able to resolve the issue. I also could not reasonably expect all actors to comment on entire drafts of articles (even though I did make these available to them), so I also sent the excerpts that were relevant to their quotes specifically, and thus quicker and more relevant for them to respond to.

its implications are far-reaching and involve various societal factors, the area has since the outset had an emphasis on the natural sciences. This sets it apart from TD areas that have a higher emphasis on social sciences, such as security or equality, although it also has the emphasis on natural sciences in common with the TD field of health. Sustainability is also highly politicized, which is not uncommon among TD areas, although it is a less polarized issue in the EU than in the US.

One underlying theoretical problem at the intersection between policymaking and knowledge production is touched upon in paper 1. The core of the commonality between Jasanoff and Elga comes down to the entanglement of wider political outlook with knowledge claims. Boundary work as studied in Jasanoff's cases (e.g., Jasanoff, 1992, 1997) thus entails the stabilization of this environment through setting boundaries that close certain discussions such that collaboration can commence. However, in the cases in this thesis, actors from different social worlds are already ready to recognize each other as epistemic peers. I thus study collaborations where the intellectual and institutional environment has already been stabilized to some extent through the prior closing of certain issues. As my cases come from the general field of sustainability, this stabilization takes the form of a general common view that the current paradigm of environmental policymaking is insufficient and a conviction that a deeper collaboration between academic and non-academic spheres is needed to take on the issue. Different dynamics might be in place in cases where there is no such 'political' consensus.

Because my concern is with the underlying dynamics of judgments, I have chosen to focus on the phenomenon that is commonly held to be the most characteristic of the evaluative and decision-making processes of science: peer-review. However, some TD collaborations follow decision-making norms from outside of academia, and thus do not involve peer-review. It is not certain that my results would be relevant to those kinds of TD collaborations. On the other hand, it is also not certain that the dynamics I describe in relation to TD peer-review are unique to TD, and it is possible that my results could be relevant in other instances of boundary-crossing peer-review.

Ecological economics, the field in my first case study, is located at an intersection between political and academic norms. It is a long-lasting collaboration, being founded in the late 1980's and continuing today. Ecological economists define their field in clear opposition to a powerful mainstream discipline: mainstream economics. Its characteristics thus set it apart from localized, project-oriented instances of TD that are short-term and organizationally defined. There is also an ambition in the field to keep its

methodologically open, non-disciplinary character, setting it apart from TD fields that move towards more formalized and integrated organization.

Formas, the research council in my second case study, is located at the same intersection of science and policy as ecological economics. Its focus is on problem-oriented research, and my case consists of two directed calls for funding applications. The collaborations themselves are thus temporally bounded, but there is an organizational continuity between collaborations as the administrators and general reviewing procedures remain. This case sets itself apart from TD funding calls that have a general, researcher-defined focus.

As mentioned in section 3.2., paper 2 delineates a further distinction between cases of TD as aiming either towards stability or societal relevance. However, for my two cases this distinction is not entirely clear-cut. While ecological economics has some of the features of an ideal type loosely organized, policy-oriented exchange, other features run opposite to this ideal type. Although the field resists standardization as a traditional discipline, it nevertheless retains some organizational features of such disciplines, namely various professional societies and peer-reviewed journals. Moreover, while many participants in the field take an activistic approach, influential actors at the center of these core institutions disagree with this aim, holding the production of academic knowledge to be the field's ultimate value. Formas's review panels, meanwhile, are highly organized and aim at producing a concrete knowledgebase that underlies decision-making, features that resonate with a local-scale, stability-seeking collaboration. On the other hand, collaborations are short, and frameworks of communication are *ad-hoc* rather than standardized. Moreover, one of the core objectives of the review panels is the assessment of the societal relevance of proposals, meaning that fitness to policy is an explicit aim of the collaboration. Thus, rather than neatly conforming to the ideal types suggested in paper 2, the two empirical cases in this project cut across them, with the large-scale, loosely organized collaboration aiming at stability, and the local-scale, highly organized collaboration aiming at ensuring the policy-relevance of research.

My two cases thus complement each other in a number of aspects. Ecological economics is a long-term, organizationally loose collaboration, whereas Formas review panels are short term and highly organized. The Formas reviews focus on problem-oriented research whereas ecological economics has a more general outlook as a whole. Although they both navigate the norms of both academia and policy, ecological economics leans more towards academia whereas Formas is more closely linked to policy. The two are thus contrastive cases within the area of sustainability. Because they have these contrasting properties, there is some likelihood that the issues I identify as relevant to both of them will also be relevant beyond the two. Thus, my discussion can have a *prima facie* validity that ranges across other cases with commonalities.

As follows from the discussion in this section and the previous chapter, the commonality of cases that is the range of my investigation is therefore the general organizational form of TD peer-review and the institution of TD epistemic peerage in a natural-science-leaning context. The dual nature of this meso-level object reflects the duality of the perception of disciplines as on the one hand organizational and on the other hand cognitive entities (D'Agostino, 2012).

6. Ethical considerations

In this chapter I discuss the ethical considerations of my research. These can be separated into internal considerations relating to the conduct of research and external consideration relating to the role of the researcher (Vetenskapsrådet, 2017). In this project, internal considerations relate to the treatment of respondents. The external ethical considerations are partly an extension of the ethical considerations of my object of study: TD. These reflect the relationship between TD and broader society, as I discuss in section 2.2. As Vikkelsø (2007) convincingly argues, researchers in science studies cannot take for granted that work that is intended to be descriptive will not be picked up by actors in the fields we study and used to influence those fields. There is thus an ethical duty for researchers to make themselves aware of the ethical issues in their fields of study, and to reflect on how their own research might impact these issues. In the first section, I therefore explore the ethical issues in TD. In the second section, I discuss my own internal and external ethical issues.

6.1. The ethics of TD research

TD research by its nature touches on ethical issues in at least two regards. Firstly, while there is a default position that research should be relevant to solving societal problems, questions remain as to which solutions are to be considered appropriate, and how that decision is to be made. Secondly, the inclusion of stakeholders into the process of research presents a host of issues regarding how this inclusion is to be handled in order to be meaningful and not token (Elzinga, 2008).

Science is highly regarded in most policy discussions in contemporary democratic societies (Porter, 1996). It is thus taken as a given by researchers that science (i.e., scientific 'experts') should consult in some way on questions of public interest. This is the core backdrop of paper 1, the real-world stakes of the theoretical issue. Central to the question of science's role in policy is the issue of establishing the facts upon which public policy should be based. There are two ideal types of conceptualizations of the role of science in relation to this issue: a technocratic and a deliberative ideal. The ideal of technocracy points back to a positivistic conception of science (Hahn et al., 1929) that presupposes a strict separation of fact and value and a unity of knowledge. Objectivity is in this positivistic conception taken to be freedom from values. Because science in this view is fact-oriented, eliminating all forms of value from consideration, only science can form objective conclusions. The conclusions drawn by science are

thus not to be objected to by the laity. Moreover, public opinion is seen as easily manipulated by propaganda and steeped in self-interest (Lippmann, 1997). Thus, it is also the task of science to discern the facts upon which public opinion should be formed. Technocracy thus advocates that science tell the public the basis on which the public may formulate its problems, and then produce objectively valid solutions to those problems. Note however that this ideal type of technocracy does not hold that science should govern every aspect of public policy.

The ideal of a deliberative role for science can be understood as a response to the positivistic ideal. It proceeds from the idea that even scientific knowledge, especially scientific knowledge concerning society, is rooted in an ideological understanding of the world (Bloor, 1976; Mannheim, 1960). Areas within science are thus bound to different thought-styles (Fleck, 2017). Differences between thought-styles can be irreconcilable, and the choice between them is non-rational. Moreover, the deliberative ideal holds that there are ways of knowing that are non-scientific, yet still valid as knowledge (Epstein, 1995; Wynne, 1992b). Thus, the facts upon which to base public policy should be decided through deliberative processes that involve all affected parties. According to the deliberative ideal, science is not to have an all-encompassing authoritative role in these deliberations, the limits of its authority being one of the issues in contention.

The professed ideals of TD lean more towards the deliberative. However, there are factors that make the issue less clear-cut. Oftentimes, projects are time-limited, and results need to be achieved within the allotted time. Indeed, if problems cause continuous disadvantage (or even suffering), it is a clear ethical duty to provide a solution in a timely manner. But a truly deliberative process must give each perspective the appropriate time to consider and present their view of things, and objections must be taken into consideration. A maximally open and deliberative process could therefore undermine the possibility to provide timely and well-founded decisions (Collins & Evans, 2002). There is an ethical dilemma between the ideal of deliberative knowledge production and the duty to provide timely solutions.

Another aspect that needs to be taken into consideration is the issue of power in open deliberations of meaning. Researchers in TD projects often engage with concepts that cut across the border between research and politics, such as 'sustainability' or 'resilience'. While these concepts are almost universally held to be policy aims, they retain a vagueness that allows their interpretation to be molded in accordance with powerful interests (Gillard, 2016). If the meaning of such terms were instead kept more technical and stricter, it would be clearer what their implementation into policy would mean (Brand & Jax, 2007). However, enforcing a technical definition of these concepts would mean that scientists would take control over the negotiation of their public meaning (Wynne, 2003).

Here, the dilemma is between the ideal of deliberative knowledge production and resistance to interested co-opting of results by the already powerful.

When it comes to the inclusion of stakeholders, there are issues concerning the purpose of their inclusion and the interests that are served by their inclusion. Stirling (2005) observes that there are three ways of rhetorically motivating the inclusion of practitioners: either normatively, instrumentally, or substantively. A normative inclusion focuses on the right of stakeholders to be heard in issues of their concern. An instrumental inclusion focuses on the effective uptake or communication of the results of research. A substantial inclusion focuses on the improvement in the quality of research. However, Stirling argues that there is a more crucial dimension that cuts across the three rhetorics. This is the question of whether the inclusion of stakeholders aims to open up or close down discussion. According to Stirling, including stakeholders can either be a means to take more perspectives into consideration or to decide precisely which perspective to take into consideration, i.e., terminate the negotiations of meaning. This reflects the ethical dilemma between discursive openness and the need for timely decisions.

Elzinga (2008) notes that it is not to be taken for granted that the inclusion of stakeholders serves the interest of those stakeholders. Because of the perceived gap between the competence of stakeholders and scientists, it is easy to rhetorically justify sidelining the former such that their influence on the process of research becomes minimal. This would also be methodologically simpler, as this is more in line with traditional disciplinary research and familiar methods. Mobjörk (2010) argues that there is a difference between 'consulting' and 'participatory' inclusion of stakeholders. When stakeholders are consultants, their role can be circumscribed as giving advice to the scientists that do the actual researching. Although this allows for their concerns to be heard, the question of how to implement those concerns in practice will be left to scientists. On the other hand, if stakeholders are included as participants, they can contribute to shaping the practices that are to address their concerns.

Mobjörk further notes that our conceptualization of the non-academics to be included affects the conceived premises of their inclusion. The term 'stakeholder' suggests that they are to be included based on their stakes in the outcome of research, i.e., the motivation for their inclusion is normative (Stirling, 2005). Conceptualizing them as 'practitioners' instead suggests that they are to be included based on their special competence to contribute to research, i.e., a substantive motivation. Thus, the form of inclusion that aligns best with the proposed ideals of TD would be a participatory inclusion of stakeholders because this would both take democratic right into consideration and seek to improve

the quality of research. However, my own research suggests that there may be contradictions when this ideal is to be realized. I discuss this in the next section.

The issue of interest intersects the issue of inclusion. Stirling (2005), Elzinga (2008), and Mobjörk (2010) all stress that stakeholders are not one homogenous group. The ethical issues of involving stakeholders in research depend on who the stakeholders are and what is at stake. It may well be that the interest of one stakeholder goes against the interest of another, less powerful stakeholder. Thus, researchers in TD need to take into active consideration who they include, and who they exclude. This is reminiscent of Popper's famous paradox of tolerance (Popper, 2010). TD research that is inclusive to the interest of powerful actors in society risks eventually being dominated by those interests.

6.2. The ethics of researching TD

The Swedish Research Council separates two aspects of the ethics of science (Vetenskapsrådet, 2017). This is on the one hand research ethics, or internal ethical questions, concerning the way that research is conducted, and on the other hand researcher ethics, or external ethical questions, concerning the role of the researcher and their research in relation to others outside of the conduct of research. In this section I discuss both these aspects of my research, first the internal, then the external ethical aspects. Ethical conduct of research is a matter of balancing interests, where the value of the knowledge produced must be balanced against risk of harm (Vetenskapsrådet, 2017).

The internal ethical issues in this thesis concern papers 3 and 4. These are based on interviews, and thus share many ethical dilemmas with other qualitative work in the social sciences and humanities (Vetenskapsrådet, 2002). The researcher must be given the informed consent of respondents, which means that respondents must be informed of the purpose of the research and how interview material will be used, and agree to participate based on this information. Part of the question of how interview material will be used concerns the handling of information about respondents, including concerns about anonymity and respect for sensitive personal information (Vetenskapsrådet, 2017, sec. 4.4). The nature of my research meant that interview questions did not require addressing sensitive personal information, and because such information is analytically irrelevant, if it were to be mentioned it would not need to be included in the final transcription.

In both my empirical studies, I addressed internal ethical issues through continuous involvement of respondents throughout the investigation. I thus not only sought initial informed consent prior to interviews, but also sought consent before using excerpts from interviews, both in drafts shared with other respondents and manuscripts submitted (and re-submitted) for publication.

Respondents were also provided with transcriptions and offered to comment on analysis, allowing them to raise concerns if I were to misrepresent them. Given the explorative nature of these studies, the concrete questions that were addressed in each paper evolved over time and continuous involvement allowed me to keep respondents informed of new directions taken by the research.

Continuous involvement also allowed me to decide the level and manner of anonymization in consort with respondents. This decision followed the principle of the lowest common denominator regarding the amount of information revealed about respondents. For paper 3, respondents were asked if they would like complete anonymization (i.e., being referred to only as "a respondent"), to have me include reference to their role in relation to the journal (i.e., "editor," "board member" etc.), or if they were comfortable attaching their names to their quotes. At least one respondent was not comfortable attaching their name, whereas no respondents objected to having their role referenced. For paper 4, the analysis was based on the premise that respondents are individuals from a broad representation of groups within the Formas panels. Referring to respondents as numbered individuals belonging to specified groups provides a measure of transparency to this analysis. I therefore indicated this as my preferred level of anonymization to respondents, while still making clear their right to object. No respondent objected to this level of anonymization. I also consulted respondents regarding into which group to categorize them.

Even if respondents would not have been anonymous, while there would be some potential amount of harm to respondents connected to both paper 3 and 4, I judge this potential harm to be both unlikely and outweighed by the value of the knowledge produced. Respondents in paper 3 could as a direct consequence of participating at worst lose their positions as editors of *Ecological Economics*, positions that while prestigious are not necessary for their livelihood or continued careers. I judge this unlikely because of the journal's professed openness to meta-reflection. Respondents in paper 4 could at worst not be re-invited to review at Formas, an activity that does award some prestige but many view as primarily an ethical obligation (Lamont, 2009). I judge this unlikely based on my interactions with Formas staff and their insisting heavily that individual respondents not be 'singled out' in my analysis, signaling concern that respondents would react negatively to my investigation and thus conversely be less likely to respond to a re-invitation by Formas.

Aside from the internal ethical questions, my research also involves external ethical concerns. One central such concern is about me investigating actors that are in a position of power in relation to myself, the editors of *Ecological Economics* being able to influence whether my work is published in their (prestigious) journal and Formas being a potential future source of funding for myself.

Readers are asked to keep this tension in mind in the following discussion. Other external ethical concerns connect to the ethical issues in TD addressed in the previous section.

Paper 2 concerns the use of a concept originally from STS ('boundary objects') within the TD literature. This case clearly illustrates the blurred boundaries between description and intervention in STS. The concept itself was intended to be purely descriptive and aimed towards an STS audience[14]. However, it has since been used by actors not only to describe their own activities, but as a framework for designing the structures of their own collaborative projects. I thus have to be aware that my own work also could end up used in this way.

Paper 3 investigates a TD field: ecological economics. Ecological economics is a field of alternative economics in opposition to the mainstream neoclassical paradigm. Being pluralistic and reflexive, the field is home to rich discussions about the problems of inclusivity and interest. The core problem is whether to welcome mainstream approaches or to entirely exclude them. If they are excluded, the field risks losing potential fruitful contributions and impacts, if they are included, the field risks being coopted by the very forces they set out to oppose. My dilemma as a researcher here concerns the issue of taking sides. Qualitative researchers in STS often tend to want to side with the outsider or the underdog. However, Gouldner (1968) points out that the status of underdog is relative to one's point of view. If our sympathies lie naturally with the underdog, then the act of picking who to describe as underdog is a highly partisan act, promoting the interests of some actors over others.

When investigating ecological economics, the status of mainstream economics varies drastically according to context. When viewed from academia and society at large, mainstream economics is the clear top dog. The field commands much greater resources and has much larger policy influence. However, viewed from within some institutions within ecological economics, mainstream economics has more of an underdog status. Within the European Society for Ecological Economics, powerful and influential actors have pushed for the complete exclusion of all mainstream approaches, leading to some previously welcomed mainstream researchers leaving the field for good. At the same time, article submissions to *Ecological Economics* are disproportionally mainstream, fueling perceptions of a 'threat from outside'.

Thus, my dilemma was whether and how to present the power dimension of the cognitive differences within the field. I chose to not discuss issues of power, focusing instead of the different values that are shared, but differently

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[14] The original article was published in *Social Studies of Science*, a journal that is highly regarded in STS, but with a lower impact factor than top journals in other fields, indicating that it is not much read outside of STS.

emphasized between camps in the field. However, this is also not a neutral description, as it runs counter to the perception that there are such fundamental differences between camps that values are in fact not shared. I am not sure whether my description of the field aligns with the interests of mainstream economics or not. I did however align myself with the interest of those who promote pluralism within the field, as my description is consistent with the idea of one field with shared values but different approaches.

Paper 4 concerns the reviewing process at the Swedish research council Formas. My central dilemma in this case was the question of whether to focus my discussion on either the dynamics of actors' understanding or the broader implications of Formas's organizational setup. Formas subscribes to the view that non-academics should be included as participants (rather than consultants) in their process (Mobjörk, 2010). Academics and non-academics are given the same instructions and participate on equal terms in the final panel discussion. However, given that the task of peer-reviewers is to assess the quality of proposed scientific research, if non-academics are to participate on equal terms in peer-review, they have to be seen as competent to assess scientific quality (in some way). At Formas, this is realized through a requirement that the non-academic reviewers have PhDs. Therefore, the people who can potentially be included into Formas's peer-review process is limited to a small section of all the people who have some stake in the outcome of potential research—and a small section with an aspect of their perspective in alignment with academia no less! Moreover, the non-academics are not understood to be there to argue their own interest. This shows that the inclusion of non-academics is not primarily based on a normative motivation in consideration of stakeholders' rights, but a substantive one based on the perceived special competence of non-academics to improve the quality of the outcome of the process (Stirling, 2005), to the degree that some who would have stakes in the outcome are excluded from the process based on perceived lack of competence.

I could have centered the discussion in my article on the argument that this shows that in some instances the ideals of an inclusion based on rights and based on substantive contribution are contrary to each other. This would highlight an ethical problem in the work of including non-academics in peer-review. However, this would have taken space away from the micro-level dynamics that I had informed my respondents and Formas that I would be investigating. Moreover, the respondents I had interviewed and the supplementary sources I had investigated were not the best fit to mobilize such an argument. In the end, I chose not to discuss the ethical dimension in detail, mainly because I lacked the theoretical and empirical resources to make a point with strong scientific validity.

7. Discussion

This chapter highlights the common themes and continuous theoretical problems that run through all the compiled papers. The papers are published[15] in journals aiming at quite different audiences as well as applying different theoretical perspectives, meaning that these commonalities and continuities are not immediately obvious. However, viewed in the light of the theoretical explication above, these common themes and problems become clear. In the first section, I explicate four concepts that emerge from the theoretical explication, which illuminate the connections between the papers. In the two following sections, I analyze and discuss how my investigations illuminate the theoretical problems of demarcation and collaboration on the one hand and opening and closing on the other hand. I conclude with the insight that my understanding of TD peerage shares a fundamental commonality with my understanding of my own epistemology.

7.1. Theoretical concepts on the middle level

The central idea behind the synthesizing middle approach in this synopsis is for me to be located in the middle space between theories and objects, explicating differences and making connections between them (Wyatt & Balmer, 2007). One step in this process was explicating a meso-level between individual interaction and large-scale science-society-relations in chapter 4. This meso-level in turn has two aspects, an organizational and a cognitive, mirroring the two aspects of scientific disciplines (D'Agostino, 2012). The explication of the meso-level has highlighted four concepts that are both empirically and theoretically useful for understanding the commonalities and continuities in the papers and the following discussion. These are the organizational concepts of form and environment and the cognitive concepts of values and roles.

The influence on collaborations of organizational practices and setup in terms of rules is at its most clear when rules are explicit, as in the review panels at Formas or work-distribution in *Ecological Economics*, but the different dynamics of stability-seeking and policy-oriented forms are also hinted at in the empirical material underlying paper 2 and reflected in the distinction between grand concepts and hubs-and-spokes. Organizational forms represent the larger social context in which individual interaction takes place. While micro-oriented studies show the theoretical importance of these individual interactions (e.g., Galison,

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[15] In the case of paper 4, 'formulated as to be publishable in'.

1997; Lamont, 2009; Star & Griesemer, 1989), studies that focus on the organizational form itself shows how this form can affect the outcome of the process of knowledge production (e.g., Guston, 1999; Huutoniemi et al., 2010; Langfeldt, 2006). Focusing on this meso-level object, yet also being in the middle between micro-sociological and organizational theoretical approaches can give insight into how the levels are connected. The analysis in 7.3. shows how decisions made in ecological economics are influenced by the organizational setup of journal publishing and 7.2. shows how the demarcation of peerage in Formas's panels is facilitated by the setup of those panels.

The institutional and intellectual environment is the larger theoretical and political outlook that is crucial to whether actors can recognize each other as epistemic peers, as discussed in paper 1. It also influences the dynamics and discussions within collaborations where peerage has been established. Jasanoff's cases illustrate how collaborations are coordinated through the stabilization of their environment through the negotiation of formal institutional rules and practices that actors deploy and reinterpret to serve their interest (e.g., Jasanoff, 1992). Guston (1999, 2001) connects this stabilizing activity with the micro-level dynamics of collaboration discussed by Star (Bowker & Star, 1999; Star & Griesemer, 1989) in his description of boundary organizations that both maintain and facilitate the crossing of a science-policy-boundary. Section 7.2. shows how the intellectual and institutional environment plays a key role in both demarcation and the coordination of collaboration in ecological economics. In section 7.3., I show how the institutional environment of Formas contributes to closing the deliberative perimeter around the concept of societal relevance.

Scientific values are central to the discussion in paper 3. These are the normative conceptions of the virtues of knowledge that can both guide the choice of theory and approach and serve to stabilize collaborations. Values in science are usually discussed in two different ways. In the classical Mertonian discussion, values are taken to constitute an ethos that regulates the conduct of scientists in order to ensure the autonomy and stability of science (Merton, 1973; Panofsky, 2010), with questions about the variability and universality of this normative structure (Mitroff, 1974; Mulkay, 1976). In the more recent philosophical discussion, values are linked to the question of theory choice (Bueter, 2015; Douglas, 2013; Hicks, 2014; Kuhn, 1977), where the insight that the choice of how to interpret data to favor one theory over another is not reducible to 'pure' rationality has led to a discussion of which values do an should guide this choice. In the discourse surrounding TD, the notion of societal relevance takes a central place as a candidate for one such value, yet one whose meaning and evaluability is debated (Bozeman & Youtie, 2017; Langfeldt et al., 2020; Samuel & Derrick, 2015). In section 7.2. I discuss how the perceived values of ecological economics serve as hubs-and-spokes for ecological economists.

The concept of peerage relates clearly to the concept of roles. The mutual recognition of peerage relates to accepting a certain epistemic role in a knowledge-producing collaboration. The discussion surrounding TD also incorporates a larger question of the role of science in society in general, and the role of experts in policymaking in particular (see sections 2.2. and 6.1). The question at this level revolves around how to find the right persons to act as 'experts' (Collins & Evans, 2002; Goldman, 2001) and what role those not designated as 'experts' should have in the production of a knowledgebase for decision-making (Dewey, 1946; Lippmann, 1997; Wynne, 2003). The actors in this thesis are 'downstream' from this problem, having been designated by others or reasonably successfully designating themselves as experts. However, Lamont (2009) shows that within collaborations of experts, where they each take on the role of epistemic peer, there are still micro-level dynamics that shape the outcome of knowledge production. These dynamics in turn are affected by organizational form, institutional and intellectual environment, and different scientific values (c.f. Huutoniemi, 2012). In section 7.3., I show how actors can mobilize ideas to change the institutionalized conception of their roles. I explicate how panelists at Formas changed the perception of the role of peers and how editors at *Ecological Economics* did the same for the role of editors.

7.2. Coordination of demarcation and collaboration

The core theoretical problem that I explore in paper 1 concerns the notion of epistemic peerage. Elga and Jasanoff agree that opposing worldviews can justifiably hinder actors from recognizing each other as epistemic peers. The implication for TD, where mutual recognition of competence across different perspectives is central, is that there is a first hurdle to be overcome in the guise of the various worldviews of collaborators. These must not be opposite, such that peerage cannot be recognized, but they must also not simply be the same, such that there is no exchange between different perspectives. There must thus be a boundary around the collaboration that excludes those that cannot be recognized as peers, but also boundaries within the collaboration that allow for substantive exchange.

In paper 2, I introduce a distinction between two types of conceptual boundary objects in TD. On the one hand, the policy-oriented and discussion-opening grand concepts, on the other hand, the stabilizing hubs-and-spokes that tend towards formalization or standardization. Grand concepts are the concepts that afford TD its political relevance through its connection to concrete policy challenges. Hubs-and-spokes instead relate to the functioning of the collaboration itself. These concepts both delineate the boundaries of collaborations by excluding those without their own complex understanding of

the topic and facilitate communication within collaborations by providing the common ground for collaborators to exchange and learn from their different understandings. The hub-and-spoke concepts in TD collaborations can thus coordinate collaboration and demarcation.

The coordination of boundary activity simultaneously in both directions is a phenomenon that has been given little attention in the study of science. The main exception is Guston's (1999, 2001) work on the boundary organizations that stabilize the interactions between science and politics. These organizations internalize the negotiation—i.e., the demarcation—of the boundary between the two, allowing for the production of boundary objects useful to both sides. However, the emphasis is on the collaboration across the boundary being demarcated: boundary organizations and their professional mediators become the dual agents of both researchers and politicians, allowing collaborations to fulfill the goals of both sides while maintaining a stable (yet porous) boundary between science and politics. In this investigation, focus is on the collaboration that happens between epistemic peers within the perimeter that is being demarcated, a perimeter that is itself drawn in such a way as to include a multitude of potentially conflicting perspectives.

Both case studies in this thesis illuminate the functions of hub-and-spoke concepts. In addition to illustrating their demarcating function, the case of ecological economics also emphasizes their coordinating function within collaborations and the case of Formas's review panels also emphasizes their facilitation of communication. In ecological economics, it is not deemed necessary that all contributions be understandable to all actors in the field. Thus, the theoretically interesting problem is how substantially differing works are coordinated such as to be considered contributions to the same field. In Formas, the coordination of reviewers is expedited by explicit rules. The theoretically interesting problem in this case is therefore how reviewers with different thought-styles thus brought together are able to communicate their different understandings.

In ecological economics, the idea of a demarcation from mainstream economics is central to the identity of the field. The disciplinary discourse holds that ecological economics is theoretically robust, broad, and dynamic and open, as opposed to the theoretically inadequate, narrow, and static and closed mainstream economics. However, this demarcation only establishes that ecological economics is not mainstream economics and says very little about what the field is about. Instead, there is a central idea that the field is concerned with investigating the interconnection of economic, social, and natural systems. This idea is a positive designation of ecological economic research. Apart from this, there is a community, a label, a number of societies, and the journal *Ecological Economics*. Respondents in my investigation, however, raised concerns about the

functioning of both the label and the societies and journal. The core concern was that the dominance of mainstream economics in the intellectual environment of the field means that the openness of these institutions invites a strong influx of mainstream influence into the field, causing it to drift from its ideological core (Spash, 2013a, 2013b). Moreover, the community is loosely knit, with little interdependence in the way that research is produced and recognized (Røpke, 2005), and, according to one respondent, its focus is not on network-building, as contrasted with the Resilience Alliance (Parker & Hackett, 2012), a prominent neighboring research program. It thus seems that the central idea and the characteristics ascribed through the disciplinary discourse are the more robust coordinating elements in ecological economics.

These are the concepts that function as hubs-and-spokes, with a thin common understanding and different complex understandings in different social worlds. The question of how to understand or analyze the interconnection between systems is answered differently by different ecological economists depending on their disciplinary background and the scientific values it incorporates. This is reflected in different complex understandings of 'theoretical robustness'. Ecological economists agree that their field is more theoretically robust than mainstream economics. The economic models of mainstream economics are seen as inadequate to deal with the problems captured by the core idea of ecological economics. To some ecological economists from a biophysical or ecological background, their own superior robustness stems from an incorporation of variables from natural science into similar kinds of mathematical models. To others from a background in qualitative social sciences, the robustness instead stems from the abandoning of primarily mathematical modelling in favor of more qualitative approaches. The different understandings of 'theoretical robustness' thus open the field up to a variety of approaches, each seen as legitimately engaging with ecological economics.

The idea of 'breadth', another agreed-upon strength of the field, is also interpreted differently depending on which scientific value one prioritizes. If consistency is prioritized, the included approaches and perspectives should be at least fundamentally compatible with each other, and the field should ultimately strive towards an integrated worldview. Thus, because mainstream economics is perceived as having some fundamental inconsistencies with ecological economics, such approaches should be excluded from this breadth. Focusing instead on novelty means that the field should constantly reinvent itself, not pursue some specific consistent position. Rather than integration, innovation is seen as the goal of the TD collaboration. Thus, if unexpected and novel connections can be made between mainstream and ecological economics, then the former should not be excluded. Unlike the different understandings of 'theoretical robustness', which opens the field to the co-existence and

collaboration of a variety of approaches, the different interpretations of 'breadth' lead to tensions. These tensions are derived from the academically and politically privileged position of mainstream economics. If not for this, these tensions would not actualize. On the other hand, if not for the privileged position of mainstream economics, ecological economics might not exist as a field.

This shows the interplay of scientific values and institutional and intellectual environment in the coordination of ecological economics. In contrast to cases typical of Star (e.g., Bowker & Star, 1999; Star & Griesemer, 1989), it is neither material objects nor infrastructures (e.g., disciplinary methodologies) that do the coordinating work. The lack of common cognitive infrastructure and the abstract theoretical focus is part of the *raison d'être* of this kind of TD collaboration. Moreover, while the formal organizational factors (journals, societies, label) do some work in coordinating, actors hold them to be not unambiguously functional. In contrast to the cases of Guston (e.g., 1999, 2001), the central factors to understanding the stabilization of ecological economics as a field are not to be found in formal organizational factors. Instead, normative beliefs about what is good about the knowledge that is produced seem to be the central factors in coordinating the collaboration. Actors are thus rhetorically deploying a normative framework in order to stabilize their collaboration (c.f. Panofsky, 2010), yet this normative framework does not center on a Mertonian ethos of science but on the values that guide theory choice (c.f. Hicks, 2014). The values that thus coordinate the collaboration are the same values used to draw a rhetorical contrast to a neighboring field (c.f. Gieryn, 1983) to fellow actors within ecological economics (c.f. Amsterdamska, 2005). The content of these values thus relates inversely to this part of the intellectual environment. Understanding demarcation and collaboration in ecological economics requires taking into account not only the relation between theories within the field, but also the academically, politically, and financially dominant position of mainstream economics; not only the institutionalization of various organizational elements but also the values that actors pursue through their organizing and the tension that can arise between the two.

In the review panel at Formas, there is a clear formal demarcation from the outside. Those on the panel are there because they are invited by Formas, those outside are not. This entails a crucial difference to ecological economics because the autonomy and authority of the panel as a whole is guaranteed by the institutional framework of Formas. This means that the panel members need not involve themselves with the boundary work that establishes this autonomy and authority. Instead, Formas have a number of criteria and tacit practices that they apply when inviting reviewers, which serve as the boundary work of demarcation. One significant example is that practitioners must have a PhD or equivalent experience in order to be allowed to participate. The tacit practices and criteria

make it so that Formas can strive to select reviewers whose worldviews are not opposite. Panelists can thus assume a mutually recognized peerage to the extent that they trust the criteria and practices of Formas. I elaborate on this in section 7.3. Nevertheless, during the panel meeting, panelists had to demarcate which of them were competent to discuss each individual proposal. This demarcation proceeded differently depending on which aspect of the proposal was being discussed.

When the novelty of proposals was up for discussion, the demarcation of competence followed along the lines of panelists' self-assessment. Panelists read the titles and abstracts of proposals to assess their own competence in relation to each proposal. Here they come across the 'topic' of the proposal, a thin description of what the research is about, functioning as a hub-and-spoke concept. Panelists judged themselves as competent to review if they had their own complex understanding of the topic. This complex understanding would be different from other members of the panel. The thin understanding, however, is in common for all the self-assessed competent reviewers. Therefore, when panelists meet in the final panel discussion, members of different social worlds can agree that they are all competent to review the same topic, despite their complex understandings of the issue being different. It is this agreement that all reviewers are competent about the same thing—the recognition of epistemic peerage—that opens the panel for communication and a negotiation of meaning in the assessment of novelty. The hubs-and-spokes allow panelists to share their complex understanding of issues with others while also learning from those others.

When the approaches of proposals were being discussed, demarcation proceeded differently. Here, a status of expertise was awarded to one or a few panelists who could demonstrate extensive familiarity with the approach in question. This represents a respect for the sovereignty of the discipline from which the approach stems (see section 7.3). Arguably, this customary rule is an extension of the boundary work demarcating science from non-science (Gieryn, 1983), signaling that only scientists close to the frontier of a discipline have the competence to judge the quality of science at that frontier, and thus that policymakers themselves have not (Polanyi, 1962). Respondents' use of the word 'method' to denote scientific approaches but not approaches to achieve societal relevance is indicative of this.

The case of Formas illustrates the relation between the organizational form of TD collaborations and the understanding of the role of peers and experts within them. The organization of the panels coheres with Mobjörk's (2010) ideal type of participatory TD, because the practitioners were given the same instructions as academics and were not formally circumscribed in any other way. They were

thus formally recognized as the epistemic peers of academics. As illustrated by paper 1 and by Formas's website (Formas, 2021), this recognition of epistemic peerage is tantamount to being awarded the status of an 'expert' in relation to certain questions of academic and societal relevance. The question of how to relate to the notion of expertise within STS has been hotly debated (Collins & Evans, 2002; Jasanoff, 2003b; Wynne, 2003). Collins and Evans have argued that the notion can be reduced to experience, whereas Jasanoff stresses the importance of institutional contexts and Wynne the underlying question of framing of issues. Studies of different organizational forms of peer-review show that the dynamics of how the expertise of others is treated indeed are affected by such organizational circumstances (e.g., Heinze, 2008; Huutoniemi, 2012; Langfeldt, 2001). Yet the case of Formas also shows that the question of what is being assessed plays a part in how the role of an expert or peer is conceptualized. When approaches were assessed, actors conformed (without prior knowledge) to the deferential, experience-based definition of Collins and Evans (2002), showing that this conception is spontaneously adhered to in some institutional contexts. When assessing the relevance of topics, more open discussions were held, conforming more to the deliberative ideal of Wynne (2003). The question of how to understand expertise in the peer-review at Formas thus relates not only to the organization of the panel, but also to the understanding of competence in relation to what is in question. When understood as broad 'familiarity' in the case of topics, the reviewers partook in open discussion, whereas when competence was understood as 'expertise' in the case of approaches, the reviewers deferred to the panelists that best demonstrated experience.

Hackett and Rhoten (2009) observe in their Snowbird Charrette study that the more successful discipline-crossing groups of students were the ones that found some specific coordinating concept that related to each member's competence. In this section, my aim has been to show that TD collaborations that revolve around mutual recognition of epistemic peerage function in the same way. Ecological economists are those that concern themselves with the interconnection of economic, social, and natural systems, and reviewers at Formas are competent to assess novelty if they are familiar with the topic of a proposal. This marks a reasonably clear distinction between who is part and who is not part of the TD collaboration. At the same time, these common ideas are understood in different complex ways among different participants, facilitating an opening up of the surrounding issues to negotiation and mutual learning. It is these differing complex understandings that provide the substance for the boundary-crossing collaboration. The conceptual boundary objects in my cases are used both to construct an external boundary around the TD collaboration and to enable communication across the boundaries within the collaboration.

Cases of TD where practitioners are consulting (Mobjörk, 2010) or where contributions are not recognized through peer-review would not be expected to follow the same dynamics.

My two cases also illustrate the different dynamics of grand concepts and hubs-and-spokes. Ecological economics is known to extensively discuss and produce various grand concepts, having been monikered the "science of sustainability" (Costanza, 1992; Dodds, 1997) and arguably being the field to elevate the concept of ecosystem services into public discourse (Costanza et al., 1997; Toman, 1998). However, the discussion-generating function or policy orientation of these concepts does not contribute to the stabilization of the field (indeed some argue that they undermine its stability). The review panels at Formas are put together specifically to assess proposals relating to grand concepts. They thus exemplify some of the political functions of these concepts as steering the flow of research funding. However, when peerage is to be identified in the actual collaboration it is more narrow topics that serve as the hubs-and-spokes. Thus, the concepts that afford TD collaborations their political relevance are not necessarily the same concepts that provide stability to the collaborations.

7.3. Closing and opening by actors and environment

Paper 2 illustrates two different overarching aims of TD collaborations. On the one hand, the aim of opening up a deliberative discourse around issues intersecting the conventional boundaries between 'science' and 'politics', as discussed in STS by such theorists as Wynne (Leach et al., 2005; Wynne, 1992a, 1992b). On the other hand, the aim of creating a long-term, stable collaboration that coordinates understandings from different social worlds in the continuous production of new knowledge, in STS extensively discussed by Star and her various collaborators (Bowker & Star, 1999; Fujimura, 1988; Star & Griesemer, 1989). For collaborations that pursue both, there is a balance to be struck between keeping issues open in order to pursue deliberative goals and closing issues in order to stabilize. The previous section explicates how in both the empirical cases there are hub-and-spoke concepts that stabilize by coordinating demarcation and collaboration. In this section, I explore some issues that were both open and closed in the cases. The cases reveal that there are more dynamics at play than the decisions of the actors directly involved in the collaborations.

The tension between opening and closing in TD is clear in the discussion surrounding grand concepts. These have to fill the dual role of on the one hand being a means to facilitate a negotiation of meaning between stakeholders and

decisionmakers and on the other hand serving as a knowledgebase for decision-making (Steger et al., 2018). To serve the former role, a concept needs to maintain an open thinness that allows for such negotiation. To serve the latter, the concept needs to be standardized in such a way as to facilitate concrete plans of action. This reflects the different overarching aims of TD research. Thus, grand concepts must move between an open and closed status in the process of democratic deliberation and decision-making, tacking between the status of object and infrastructure (Bowker & Star, 1999; Star, 2010).

Traditional disciplinary structures, such as methods, can also move between an open and closed status. Hackett (2005) notes that research technologies (consisting of both concrete apparatuses and competences) are crucial to the maintenance of the identity of research groups. Methods and methodology can be key factors in the boundary work of demarcation (Beddoes, 2014). When employed during normal research, they are considered fixed and are not problematized. However, when the identity of a field is at stake, methods can become an object of deliberation. In TD collaborations, it cannot be assumed that methods and methodology are the same for all participants. It is therefore interesting to see in which situations this diversity is taken as a closed matter, and when it becomes the subject of negotiation. In both ecological economics and Formas the notion of methods exhibited states of both openness and closedness.

In ecological economics, methodological diversity is at the heart of the field's identity. It is this diversity that is at the core of the perceived superiority of ecological over mainstream economics. At the same time, the question remains whether and how much mainstream methodology to incorporate into the field. The journal *Ecological Economics* was founded on open negotiations of methodology, and space for it is still kept open. The decision to keep this space open rests with the International Society for Ecological Economics, the professional organization behind the journal. It is this body that decides on the formulation of the aims and scope of the journal, as well as the submission formats. However, this organization does not operate in a vacuum. While the international society mostly retains control of the direction of the journal, it is owned by Elsevier, and they have an expectation that the journal remains successful in terms of scientific prestige and monetary profit. Thus, some decisions are closed off, such as going full open access or reducing the number of issues.

Despite the open space in the journal for deliberation of which methods to employ within the field, when it comes to the process of reviewing submissions to the journal, the issue of methods is more clearly closed. Editors do not always (in fact they do rarely) see themselves as competent to assess the quality of the specific methods employed in the manuscripts whose reviews they coordinate. They instead rely on the judgment of the external reviewers they find. Even when

they do deem themselves competent, the assessment is understood as judging whether the method is appropriate to answer the question posed, and whether it has been carried out adequately, not whether the method as such belongs in ecological economics. Editors do not see themselves as being in a position, *qua* editors, to question who is and who isn't an ecological economist. Thus, the inclusivity of the field (or at least the journal) towards the mainstream is enacted through the closedness of the question of method in each single review. The issue of what kinds of questions (and consequently methods) belong in the journal are in practice deferred to the editors-in-chief. Moreover, according to some actors it is not unlikely that Elsevier would intervene if the editors-in-chief were to suddenly restrict the scope of acceptance in such a way that more mainstream—and thus potentially high-impact—papers are rejected.

At Formas, like in ecological economics, methods are closed when it comes to assessing singular proposals. The panel defers to the judgment of the member(s) who have experience with the method. Panelists are not instructed to do this, it rather seems to be a customary rule that panelists have been schooled into over a long time (Lamont, 2009). It represents a respect for disciplinary sovereignty, the idea that disciplines should be free to control which methods are appropriate to address the problems they themselves formulate. However, despite this customary rule, the issue of methods was not altogether closed. Outside the perimeters of the actual review of applications, panelists could argue that the distribution of competences on the panel was biased towards or against certain methodological views. Qualitatively oriented social scientists had noted that the dominance of technical and natural scientists on previous panels meant that proposals that employed qualitative methods were judged as inadequate because there were not enough reviewers on the panels with the competence to recognize the merits of these methods. I.e., these methods were not awarded the status of methods by technical scientists. The closedness here worked to exclude certain methods, because of the lack of a representative from their sovereign discipline. The social scientists who perceived this were able to indirectly open up the notion of methods by taking up these issues of diversity with Formas staff, as elaborated below.

Thus, in the case of ecological economics, the closedness of the notion of methods hinders the exclusion of mainstream economics, while in the panel reviews at Formas, the closedness of methods instead hindered the inclusion of qualitative social science. In ecological economics, even if most actors would have wanted to exclude mainstream economics, the fact that the issue of methodology is closed ensures that mainstream manuscripts are published in *Ecological Economics* based on the discretion of mainstream economists, not

ecological economists[16]. At Formas, closedness of methods means that panels are unable to recognize methods from fundamentally different fields as methods, meaning that if these methods were to be included the panels needed to be diversified. One lesson to be learned from these cases is thus that the relationship between openness and closedness, and inclusion and exclusion is not fixed, but dependent on context.

One more notion that could be both open and closed in the panel discussions at Formas was 'societal relevance'. Panelists could negotiate its meaning in discussions about the novelty of proposals. However, in some cases, Formas staff intervened to "clarify" what Formas meant by relevance. Thus, Formas can exert some control over the openness of the notion. Indeed, the guidelines about what to look for when assessing the relevance of proposals are written by Formas. Respondents represented the view that assessment must be made against the text of the specific call, and Formas staff is given interpretive priority of the text they have written. Moreover, the very task of assessing relevance presumes that relevance is something that is possible to assess. It thus seems that the notion of societal relevance was only open within a certain set of parameters. Relevance had to be assessed in relation to the various fields that panelists represented, and concerned the topics provided by proposals. Thus, panelists could not interpret the question of social relevance as the question of what in general is good for society as a whole (Bozeman & Boardman, 2009).

Thus, in both ecological economics and Formas's review panels, institutional factors affect the opening and closing of issues. Keeping *Ecological Economics* methodologically open reflects the founding ideals of the field and journal. At the same time, respect for the institution of peer-review requires that editors give much weight to the judgment of their reviewers, keeping the issue of methods closed for singular manuscripts. The idea that peer-review mandates judgments of method by disciplinary experts is fixed. This has led to a situation where despite the openness to discuss the appropriateness of methods, it is difficult to exclude any particular ones. This situation aligns with the institutional incentives of the academic publishing business as it allows for a broader selection of papers and increased exclusivity. Formas, moreover, is a government agency, and thus aims to fulfill the goals of Swedish research policy. The inclusion of relevance as an assessment criterion reflects this policy, and thus the presence of the concept that is to be opened or closed is due to an institutional mandate. The perimeter enforced by Formas within which the concept can remain open reflects the

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[16] Provided they are not rejected without review; although as elaborated above, Elsevier might oppose the notion that papers with high potential to achieve success in terms of metrics be rejected without review.

organizational interpretation of their policy mandate. Here, it is the idea that peer-review should relate to the intentions of the writers of call texts that is fixed.

Thus, some of the power to influence which issues are to be opened or closed, and to what extent, seems to stem from the institutional environment that fixes ideas. This affords some power to close or keep open issues to actors outside the collaborations themselves, in these cases Elsevier and the Swedish government respectively. The closing of issues that concern the process of research, such as the methods applied or values pursued, affects the character of the knowledge that is produced and recognized within the relevant area. There are thus some tentative clues to be found in these cases as to the question of how macro-level factors such as science policy or norms of business sectors can affect cognitive content in knowledge production (c.f. Gläser & Laudel, 2016) in these kinds of TD collaborations.

However, this institutional environment is itself not necessarily closed. Actors were in some instances able to provide feedbacks that altered subsequent institutionalized practices and ideas. In my cases, actors' reinterpretation of their roles could effect changes in the organizational form of collaborations.

As observed by Lamont (2009), research councils have an interest in cultivating and retaining good reviewers (however interpreted). Thus, many of my respondents in the second case had evaluated for Formas previously. These respondents could recount how the organizational form of Formas's review panels has changed over time, and their own role in effecting this change. One social scientist recalled their time on another of Formas's recurrent panels. When they first started reviewing, they were the only social scientist on a panel dominated by technical natural scientists and engineers. The respondent thus observed how proposals with a qualitative angle were rated poorly by reviewers who were unfamiliar with the approaches undertaken in those proposals. In Lamont's (2009) terms, the respondent witnessed violations of their own field's disciplinary sovereignty. After one such occasion, perceived as particularly egregious, the respondent decided to approach Formas staff after the fact to complain about the matter. The respondent lifted the perceived underrepresentation of their field, the unfairness of it, and the neglect of quality research proposals resulting from it. In subsequent panels, qualitative social science had more representation. The role of a reviewer on this panel was thus expanded to include assessment of qualitative approaches, and the question of which scientific values to endorse became open to discussion.

Another respondent, a veteran chair also from the social sciences, relayed a similar story. This respondent had worked with Formas for many years, first as a reviewer, and eventually chairing several panels. They recalled how in the early days the interactions between qualitatively and quantitatively oriented researchers

had been confrontational, and how they themselves adopted a stubborn attitude, butting heads with researchers with publications in *Nature*. Once again, this reflected the quantitatively oriented researchers' idea that qualitative methods were of inherently lower scientific quality than quantitative methods. Over time, the respondent reported, attitudes started to change and be more open, and once they themselves was made chair, they took active measures to steer their chaired panels towards this open attitude. Social scientists thus according to this respondent had to struggle for their equal recognition through a confrontational attitude, and once they had reached positions of power sought to institutionalize this recognition, behaving like a decentralized and loosely knit scientific/intellectual movement (Frickel & Gross, 2005). The role of quantitative reviewers viz. qualitative reviewers thus changed from 'defending' the scientific 'superiority' of quantitative approaches to one of mutual learning.

A third respondent who had participated on early instances of panels where practitioners were included recalled how in these early panels the review of quality by academics and the review of relevance by practitioners were kept separate from each other. However, this was not appreciated by panelists. The respondent framed the issue as academics also being able to assess relevance, and thus that their competence to do so was being wasted. On the other hand, other respondents who were practitioners insisted in interviews that because of their experience with research, they could also to some extent identify scientific quality, something that could also have been an argument in the issue. Thus, in subsequent panels, academics and practitioners were given the same instructions and their competence was in some cases treated the same. Practitioners were thus included as participants rather than as consultants (Mobjörk, 2010).

At Formas, the institution of peer-review was thus malleable to the actions of reviewers. The story by the first respondent shows how the notion was expanded to include a need for epistemological diversity and a role for reviewers that included assessment of qualitative proposals. The second respondent illustrated how the institution changed from eliciting a confrontational to a more consensus-seeking interaction between quantitative and qualitative researchers. The third story shows the institutionalization of a participatory ideal as a part of the institution of peer-review and the role of practitioners. However, while peer-review was malleable to reinterpretation in part, some of its features seem to have still been taken for granted. Although the idea of a 'peer' had been reinterpreted, the process was still understood as a qualitative assessment based on the judgment of people with special competence. Although it is difficult to find unambiguous indicators of this, it would seem that reviewers implicitly understand this to be necessary for there to be such a thing as a peer-review at all. This thin notion of peer-review is thus out of reach for actors, even though its thick interpretation is malleable.

In ecological economics, the institutions of peer-review and academic publishing seem to remain fixed to editors of *Ecological Economics*. However, there were some actions by respondents that effected changes in durable practices. Two respondent editors in particular reported how the manuscripts they had to process largely failed to reflect their research interests or their conception of the field. They had brought up this issue with the editors-in-chief, one of them going so far as to threaten to leave the position of editor. Subsequently, as reported by both respondents, the situation improved somewhat, as the editors-in-chief started taking care to apportion out manuscripts based on research interests. Thus, the role of editors was modified to emphasize the importance of research interests, and the role of chief editors was expanded to include higher sensitivity to the research interests of editors.

These examples show how dynamics on the micro-level can effect change on the higher meso-level in these forms of TD collaboration. Some of the institutionalized ideas that form part of this meso-level are open, and thus malleable to re-interpretation on the micro-level, a process that can lead to a re-institutionalization given sufficient momentum in the right circumstances. This changes the perceived meaning and content of action, changes that can be reflected in a change in organizational form, the other aspect of the meso-level. Panofsky (2010) argues that the Mertonian ethos of science, although abstract and universalistic when considered as a large-scale theoretical structure, is malleable on the institutional meso-level and deployed by actors in micro-level struggle. In the investigation of these cases, similarly malleable institutionalized ideas were the conceptualizations of the roles of various groups of actors. In paper 3, the scientific values of ecological economics are shown to be used in the same way. Other kinds of ideas could turn out to be malleable in the relevant way in other cases. These examples complement the theory of Scientific/Intellectual Movements (Frickel & Gross, 2005) dealing with larger scale political[17] change.

In summary, issues could be kept open or closed in these TD collaborations through affordances given through the institutional environment of the collaboration. These affordances could also extend beyond the boundaries of the collaboration. Thus, the institutional environment can set perimeters of the internal institutionalization of ideas. However, the institutional environment was also in part malleable to actions by actors and could therefore itself be an issue to be opened or closed. Reinterpreting the meaning of their roles allowed actors to affect the organizational form of their collaboration, changing the institutionalization of those roles. In a sense, the question of whether an issue is open or closed can be seen as the question of whether actors take the idea to be

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[17] 'Political' with a lower-case 'p', i.e., to do with the distribution of power.

part of the external environment or the internal institutional structure of their collaboration.

7.4. Hermeneutical-dialectical epistemology in common

The focus of this thesis has been on the notion of TD epistemic peerage. I have therefore investigated situations where actors are the epistemic peers of each other. In my empirical case studies, I interact with these actors directly. The hermeneutical-dialectical epistemology I explicate in section 5.1. shows that the knowledge produced through my investigations is influenced by both my own and my respondents' thought-styles, and that neither go unchanged through the exchange. This exchange between actors and myself is thus one of mutually recognized competence across disciplinary boundaries. In a sense, I and my actors are thus TD epistemic peers. Furthermore, the TD exchange within these collaborations themselves endorses a similar hermeneutical-dialectical epistemology.

In ecological economics, the characterized strength of the field that was the most uncontroversial is the notion that the field is open and dynamic. This is one of the perceived key scientific strengths over the traditional disciplinary mainstream economics. This openness is manifested through spaces open to discuss how to approach, and thus also how to understand, the central object of study. Here, actors can put forward their understanding of the notion of the interconnected natural, social, and economic system. To some, this system is best characterized as biophysical, rooted in the notion of entropy (Georgescu-Roegen, 1971; Melgar-Melgar & Hall, 2020). To others, it is best characterized as social, rooted in the notion of institutions (Vatn, 2020; Vatn & Bromley, 1994). This reflects the idea that the object of research is not given through some independent nature but shaped by the thought-styles of researchers through acts of apprehension and interpretation. This makes a hermeneutical epistemology useful in understanding how the object of research is fixed in ecological economics.

Actors in ecological economics are also in remarkable agreement about the importance of the TD nature of the field. There are three ways of characterizing the value of TD in ecological economics. It can bring societal relevance, consisting in a meeting between academia and society where knowledge can be useful to both sides. This can be achieved through mutual exchange, where both sides learn about the understanding of the other. The value of TD can also be characterized as consistent integration. Integration in this sense represents the inclusion of all perspectives into a consistent worldview. This means finding an

understanding that is mutual and common to everyone involved, requiring an exchange between all parties. Finally, the value of TD can be characterized as innovation; acts of creative destruction that find connections between positions that can seem radically different. This requires meetings and exchanges between perspectives that start far away from each other, but through interaction manage to find a common ground. Each of these characterizations involve reaching a common understanding through a dialectical exchange. Thus, dialectical schools of thought are relevant to understanding how the value of TD is achieved in ecological economics.

In the review panels at Formas, panelists were recognized as competent to assess novelty, both by themselves and mutually, through their familiarity with a thinly understood topic. This thin topic is in common to all the panelists, but not all panelists have a familiarity with it in the form of their own thick understanding of the topic. The demarcation of competence is done through the presence of such a thick understanding. However, this thick understanding is different for each panelist, reflecting the different perspectives of their varying backgrounds. Yet again, the characteristics of the topic are not simply given through independent nature, but the apprehension and interpretation by panelists, making hermeneutical epistemological theories relevant. In the actual discussions, the commonality of the topic facilitates open discussions of meaning. These occur in a collegial fashion, in a spirit of mutual learning. It is described as normally and ideally separate from concerns about power and securing resources for one's own field, instead focusing on achieving a mutual understanding based on an open exchange. This makes dialectical theory useful in understanding the process at Formas.

Thus, the knowledge I produce in this thesis is in part an outcome of the same kind of relation and interaction I investigate: a TD peerage relating to a hermeneutical-dialectical epistemology.

8. Conclusion

In this thesis, I investigate the problem of epistemic peerage in TD from different perspectives. This problem concerns the mutual recognition of competence despite substantially different conceptions of quality. Whereas in most areas of science there are strong disciplinary structures that inform judgments of quality, in TD there is an explicit aim to transcend these structures. Nevertheless, contributions need to be judged as belonging to the work of a TD collaboration, both in terms of their quality and topic. My investigations concern the dynamics and preconditions of such judgments.

The different approaches of the appended papers–general and theoretical on the one hand and particular and empirical on the other–necessitate a synthesizing middle-level approach. Doing middle-level theory can be conceptualized as being in the middle between different theoretical perspectives and phenomena. In this synopsis, I have placed myself in the middle between my compiled papers, between abstract and general theorizing and micro-level analysis. It is through this middle perspective that I can discuss the issues that cut across each paper, issues that are underlying parts of the phenomenon of TD epistemic peerage and thus have relevance beyond this thesis.

The first issue concerns boundaries in TD. TD collaborations must draw a boundary towards their environment yet also retain substantial boundaries within the collaboration. This involves both general ideas about the nature of knowledge and TD as well as micro-level social dynamics of collaboration. Crucially, the worldviews of collaborators must not be so opposite that they cannot recognize each other as peers but must also not be so similar that there can be no fruitful exchanges between different perspectives. In my empirical cases, I observe how conceptual boundary objects function as hubs-and-spokes that coordinate both demarcation and collaboration, where a thin common understanding draws an external boundary towards those who do not engage with the concept, yet different thick understandings among collaborators facilitate open deliberations and mutual learning.

The second issue concerns which issues are open to deliberations, and which are closed. TD collaborations can have the dual aims of stability and societal relevance. Long-term stability is facilitated by the closing of certain issues, whereas societal relevance is taken to relate to the opening of issues for deliberation. Moreover, the core ideas of inclusivity rely on an openness to multiple perspectives whereas the exclusion of opposite worldviews, i.e., those where there is no mutual recognition of peerage, requires that some fundamental

commonalities remain closed. My cases show that the question of which issues to close and which to open are not isolated from factors in the intellectual and institutional environment of collaborations. TD fields that incorporate the publishing of a journal can have issues pertaining to methods closed by the institution of peer-review, and TD grant application can have the perimeters of the notion of societal relevance closed by the funding organization through the institutionalized norms surrounding the awarding of research grants.

It is by being in the middle between levels of analysis that I can address these issues. It would thus be valuable for future studies of TD to utilize this kind of middle-level approach where some effects of macro-level factors on micro-level cognitive content and some effects of micro-level dynamics on meso-level structure can be highlighted. One area of interest would be the staff at research councils and the process of organizing TD calls for funding, from formulating the call text, to inviting reviewers, to processing outcomes of panel meetings.

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Appendix