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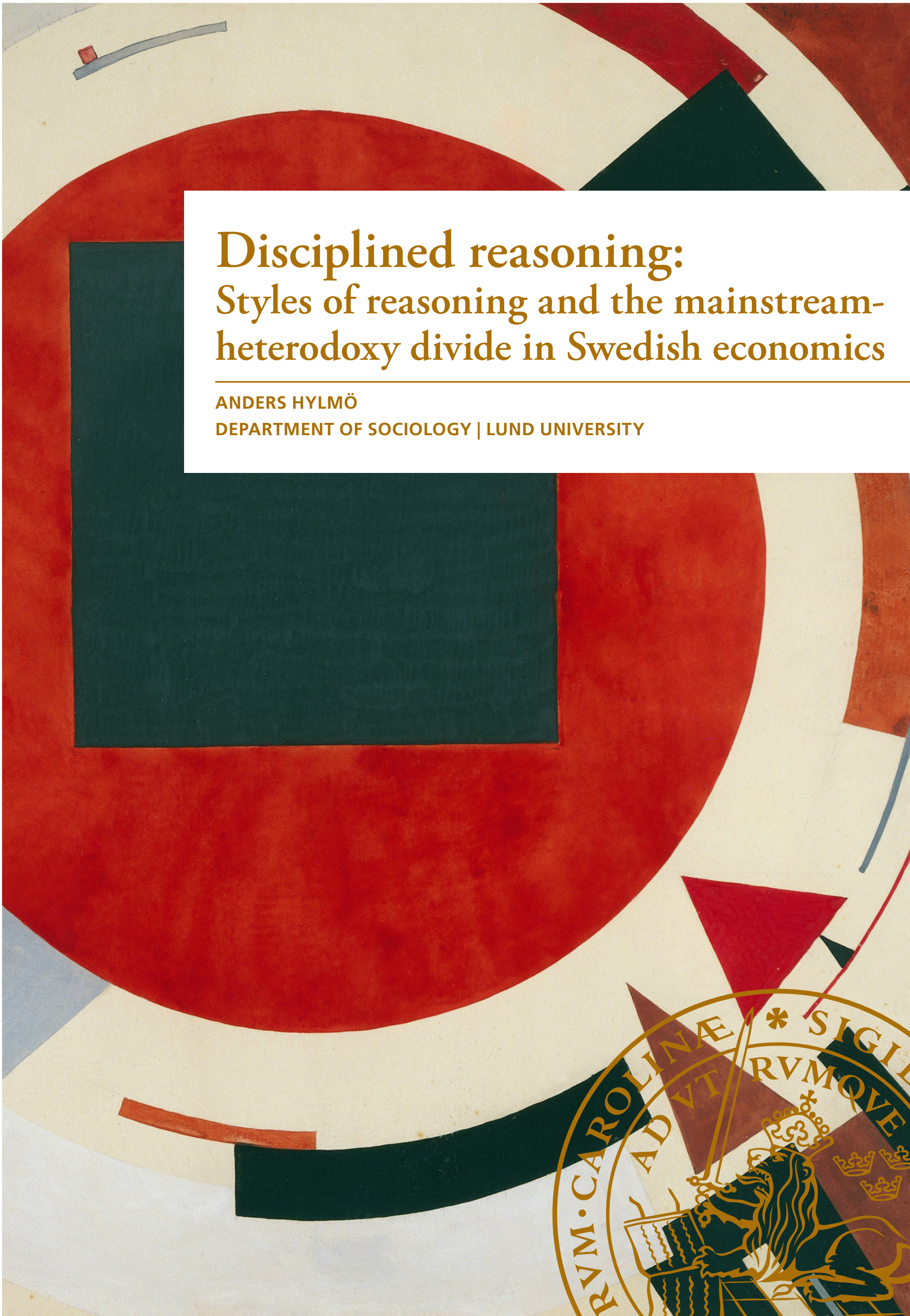

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Abstract

Economics is one of the most influential social science disciplines, with a high level of internal consent around a common theoretical and methodological approach to economic analysis. However, marginalised schools of thought have increasingly unified under the term "heterodox" economics, with their critical stance towards the "neoclassical mainstream" as common denominator. This has spawned debates among scholars about how to understand the nature of the mainstream-heterodoxy divide in economics.

This thesis sets out to explain how such a common approach to science is generalised and stabilised in modern economics, and how this process is related to heterodoxy. Grounded in the sociology of science, it aims first to provide an empirical account of the mainstream-heterodoxy dynamics in Swedish economics, and second, to contribute to theory development. Drawing on the literature on distinct styles of reasoning in the history of science, I develop a theoretical framework of relational disciplinary styles of reasoning, which is used to analyse two bodies of empirical material from Swedish economics. The first is an in-depth interview study with researchers in economics, and the second is a document study of expert evaluation reports from the hiring of professors of economics at four of the top Swedish universities during 25 years. Through the two empirical studies, the fine-grained qualitative material provides an insight into the ways economists understand their discipline and the character of proper knowledge production.

I argue that the mainstream-heterodoxy divide is fruitfully understood in terms of the institutionalised stabilisation of a disciplinary style of reasoning, and show how economists understand their scientific approach and its merits. The maintenance of the style of reasoning is the achievement of the thought collective of economists, where boundaries are constructed in relation to contesting heterodox economics and to other scientific disciplines. I show how the disciplinary style with its conception of good science and the notion of a core of the discipline is linked to the reproduction of disciplinary boundaries. I trace how this plays out through shifting quality evaluation practices, and show how top journal rankings have become a powerful judgement device which links the hierarchical ranking of top journals to the notion of a disciplinary core, and effectively functions as a mechanism of disciplinary stabilisation.

In conclusion, I argue that these processes form a self-stabilising system in which the disciplinary style of reasoning and its boundaries is reproduced, with potential implications for how we understand intellectual dynamics and pluralism.

Key words sociology of economics, heterodox economics, styles of reasoning, disciplinarity, quality evaluation

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Men make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly encountered from the past. The tradition of all the dead generations weighs like a nightmare on the brain of the living. (Marx 1970a:15)

We have to acknowledge that reason did not fall from heaven as a mysterious and forever inexplicable gift, and that it is therefore historical through and through; but we are not forced to conclude, as is often supposed, that it is reducible to history. (Bourdieu 2000:109)

A physicist, a chemist and an economist are stranded on a desert island, with nothing to eat. A can of soup washes ashore. The physicist says, "Let's climb that palm tree and drop it on the rocks." The chemist says, "No, let's build a fire and heat the can until it bursts." The economist says, "No, no. Let's assume a can-opener…" (Old joke)

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Acknowledgements

In this dissertation, I have written quite a lot about how academics learn to think and reason in particular ways from others who learned these ways before them. Knowledge, and knowledge about how to make it, is deeply social, always bigger than the individual who depends on this collective context. Of course, this applies to me too. As such, I am deeply indebted to a number of persons around me.

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Chapter 1. Introduction

Economics has a special status among the social sciences. At times dubbed its queen, it is certainly seen by many as the most rigorously scientific. With authority and confidence, economists comment on all matters economic, and most of us put our trust in their technically-advanced analyses of economic policies, growth scenarios, trade agreements, labour market issues and a whole range of other phenomena. Compared to other social sciences, economics is characterised by its high level of internal consensus around what is often loosely called neoclassical or "mainstream" economics, linked to a set of advanced technical methods, and a high level of social demand for the knowledge produced, resulting in both an objective and a subjective sense of "the superiority of economists" (Fourcade, Ollion, and Algan 2015).

The strong consensus on a common scientific approach that includes assumptions about the analysis of human behaviour in terms of instrumentally rational atomistic individuals, and the deductive modelling approach to problems, means that economics may be thought of as more "scientific" than neighbouring disciplines. Technically-skilled economists have found an efficient way of formulating and solving problems that is understood as productive, efficient and unambiguous by economists themselves, and by many who draw on their expertise in public administration, banking, or consultancy.

At the same time, economics as a discipline is the target of more distrust, suspicion and public critique than most other sciences (Ross 2012), perhaps precisely because of its great influence (Fourcade 2018). There is also a more serious form of sustained critique from within the profession itself which has found a wider audience since the 2008 financial crisis. *Heterodox economics*, which has become the general umbrella term for dissenting schools of thought, also sets economics apart through the marked conflict, or intellectual divide, between the dominant mainstream and a heterodox community that struggles to promote pluralism and a broader theoretical and methodological space.

On the one hand, economics is a scientific discipline with a successful and very dominant mainstream that has managed to establish internationally an approach to knowledge production that appears historically stable and uncontested in its core features. On the other hand, there is a small and marginalised minority which is strongly critical of the majority view and claims to be excluded by it. How should we understand this contradictory dual phenomenon? How is the disciplinary way of doing science reproduced and stabilised, and in what way is it related to the existence of heterodoxy?

This is the problem in the sociology of science that this study sets out to understand. It is a phenomenon that is of interest from at least three different perspectives. First, as a problem lying in the intersection of science and politics, it should be of interest to every citizen who reflects upon the role played by economic knowledge in modern society, increasingly reliant on scientific knowledge and expertise. How economists analyse our world has more or less real, direct effects on that very world.[1] These effects may range from limitations on what is considered possible in public political discourse at a certain point in time, via the influence of a certain style of reasoning, to the stronger so-called "performativity of economics" thesis: that economic theories do not so much reflect the way the world is, as they are materialised or performed when markets and actors are constructed after the image of economic theories (Callon 1998).

If politics and policymaking rely on economic expertise, disagreement among experts is surely disturbing. Of course, many would agree that macroeconomic issues, like the causes of unemployment or economic downturns and crises, are hard to disentangle from political stances. However, I will not try to show that there is a simple relationship between political ideology and economic theorising. Instead, my focus is on how different ways of thinking, analysing, and doing economic research, in short different styles of scientific reasoning, fundamentally shape research approaches in economics, and how we may understand the stabilisation of modern economics and the mainstream-heterodoxy divide in these terms.

This leads to a second way in which this dual phenomenon is relevant, namely as a theoretical problem in the sociology of science. If one applies some of the standard conceptual tools of science studies, the situation is in some sense an anomaly, for it does not neatly fit into common schemes. On the one hand, the situation looks like a *paradigm* in Thomas Kuhn's (1996) sense. Recall that normal science, for Kuhn, takes place within a paradigm that guides how scientists work, what sorts of questions they pose, what kinds of theories they use and which methods they consider legitimate in solving their everyday research puzzles. There is no science outside the paradigm, and a paradigm is only ever questioned in the

[1] For a thorough review of the literature on the conditions for the potential policy effects of economic knowledge, see Hirschman and Berman (2014).

event of a scientific crisis, which eventually leads to the abrupt replacement of one paradigm with another in a scientific revolution.

On the other hand, the element of heterodoxy in economics is not explicable by a Kuhnian view of the consensus in normal science. Perhaps this phenomenon looks instead like a case of *scientific controversy*, so often studied by sociologists of science? However, the ideal type of scientific controversy is a stand-off between competing parties (researchers, schools of thought, theories, etc.) about some contested issue, which ideally ends with one side establishing and stabilising its vision as the truth of the matter. A central innovation within the sociology of scientific knowledge is the insight that such processes may be the outcome of social causes as much as of objective natural causes. When a controversy has been settled facts become established, solidified, black-boxed, inscribed into textbooks and taken for granted. But with heterodoxy in economics, there is no such complete resolution, no final closure (even if mainstream economics is very near). Instead a longstanding controversy appears to have crystallised into a contested and asymmetric relation between a hegemonic orthodoxy and a very marginal heterodoxy.

Its sociopolitical and theoretical aspects are closely related to a third way in which the problem could be relevant, namely as a more general example of how scientific disciplines shape the way we work and reason. This should be of interest both to anyone engaged in some form of science, and more specifically as a problem for science policy. In a sense, it is a question of how knowledge is socially structured in a specific context. The starting point for the sociology of knowledge is the insight that all thinking and cognition is a social activity, "the most socially-conditioned activity of man" (Fleck 1979). The production of knowledge, and especially in the highly institutionalised setting of contemporary science, is a fundamentally social activity. It is dependent not only on the material and institutional framework of the university system, but more importantly, always builds on previous knowledge and practices transferred by a social community. It means that if we are interested in understanding how economic thought develops, what theories, methods and results are produced, and the limits of what it is possible and impossible to think in the domain, we need to look closer at the social conditions of possibility of economic knowledge production. This is the fundamental problem that drives this study.

1. The research problem

The overarching research problem of this study can be formulated simply as: *why do economists think the way they do?* Or, to be more precise: *how is a common approach to science generalised and stabilised in the modern economics discipline, and in what way is this process related to the existence of heterodoxy?*

These broad questions can be broken down into more specific ones. First there is a set of interrelated *explorative* questions: In what way is it reasonable to talk about "mainstream economics" and, by implication, how should we understand the relation between the mainstream and heterodoxy? What is it that unites the mainstream and makes it stick together so well? Conversely, what is the common ground, if any, of heterodox economists? And to what extent can the split between the mainstream and heterodoxy be thought of as a durable divide? A second set comprises interrelated *explanatory* questions: How can the stabilisation of the disciplinary mainstream and its relation to heterodoxy be explained in sociological terms? What social processes and mechanisms can account for it? How are the cognitive structures of ways of thinking, methodological orientations, conceptions of scientific quality, and assumptions about the world related to social structures and processes? The latter may include, for example, the institutionalised system of scientific disciplines, formation of social thought collectives, and boundary work.

The overarching aims of the study are twofold. The first is empirical: to provide an account of the mainstream approach in modern Swedish economics, its social process of stabilisation, and its relation to heterodoxy which is simultaneously descriptive, interpretative and explanatory. The second aim is to contribute to sociological theory development, more specifically to survey and synthesise relevant literatures in order to construct a viable theoretical framework in relation to previous research and the empirical material. The analytical goal of such a theoretical framework should be a combination of explanatory power in this particular case, while still being as general as possible, and cumulatively drawing on previous knowledge regarding social phenomena.

The overarching research strategy is a combination of a thorough engagement with previous literatures and a novel empirical case study of contemporary Swedish economics. I will first develop and specify the research problem through the first few chapters, where previous research is surveyed and a theoretical framework developed. Using Swedish economics as an empirical case study, I will then draw on two bodies of empirical data. The first is an in-depth interview study with academic economists to elicit their conception of the world of economics from within. This is then combined with a document analysis of expert evaluation reports from the hiring of full professors at leading Swedish universities, in order to investigate disciplinary standards of scientific quality.

2. Theoretical approach

The theoretical framework introduces the notion of *styles of reasoning*. This concept, which originates with Karl Mannheim's pioneering sociology of knowledge in the 1920s, has since acquired a more precise interpretation by Alistair Crombie in the history of science and Ian Hacking in the philosophy of science. The concept captures how cognition and reasoning is socially conditioned as part of scientific training, and a central claim is that during the long history of science, several distinct styles of reasoning have emerged (rather than one grand, unitary *scientific method*).

According to this approach, since the first philosophers and mathematicians in ancient Greece, Western science has been characterised by a number of broad but distinct styles of reasoning underlying the various sciences, paradigms and theories. One of the first examples is the mathematical style of axiomatic reasoning based on postulation and proof, with Euclidian geometry as an extremely influential exemplar. Other styles include the experimental style or statistical reasoning. One of the latest styles to develop is the historical-genetic style of the nineteenth century, expressed through the turn to historicism in philosophy and nascent social sciences, and in Darwin's historicisation of nature. Such styles include the notions of ontology (what sort of objects do we posit to exist?), epistemology (how can one validly proceed to find out?) and, importantly, as Hacking argues, that the ultimate grounds of scientific judgements are always based on a specific style. The result is a concept that reminds us of Kuhn's incommensurable paradigms, only the incommensurability is now no longer a matter of historical breaks, but rather of parallel, distinct (and potentially incommensurable) approaches to doing science: styles of reasoning. But a central notion remains, namely that there is no grounds for judging theories outside of the paradigm/style.

I will argue that the styles approach allows us to deepen our sociological understanding of the ways that scientific knowledge is produced and reproduced within a thought collective entangled in social institutions, leading to relatively enduring cognitive structures. In short, groups of scientists learn to think, argue and investigate in a specific way that simultaneously excludes other ways of reasoning, and transmits this approach to knowledge-making to its students, thereby reproducing itself. More specifically, I will claim that importing this approach back into sociology is a fruitful way to understand the stabilisation of a common mainstream approach and its enduring relation to heterodoxy in economics.

Importing the historical styles approach into a sociological setting means that we need to pay more attention to the role of actors. I argue that the styles approach lends itself well to incorporating Bourdieu's concept of a specific *scientific habitus*, an embodied sense of judgement acquired through training in a specific field. Our understanding of styles of reasoning can be enhanced by adding habitus as the mechanism by which they are actually operating and transmitted. Styles of reasoning are, furthermore, carried by social thought collectives. A more or less bounded social group makes a certain way of thinking and doing its own. An important aspect of the formation of social thought collectives is their relational character, the fact that they are formed in relation or opposition to other social groups: other sciences, non-scientists, inferior or non-scientific approaches to the topic, and so on. Drawing both on the literature on boundary work in science (Gieryn 1983), and on the more recent general notion of social boundary phenomena where classification, sorting and judgement tie symbolic and social boundaries together (Lamont 2012b; Lamont and Molnár 2002), I will argue that such processes are a central mechanism behind the creation of disciplinary thought collectives and their relation to other scientific disciplines, approaches, and to heterodoxy. In short, with this synthesised approach, I suggest the concept of *relational disciplinary styles*.

Furthermore, to understand how a style of doing science is reproduced, we need to include the institutional framework through which publications, promotion and hiring, and research funding are mediated. The central regulating institution here is the scientific peer review process, through which experts act as gatekeepers based on their professional scientific judgement. Given the role played by styles of reasoning as inscribed in the habitus of expert reviewers, the peer review system is also a cog in a big machine that reproduces itself through a certain level of cognitive co-optation (Travis and Collins 1991). Drawing also on recent work on scientific evaluation practices, I argue that conceptions of scientific quality are variable, and that institutionalised evaluation practices themselves evolve and differ over time and between disciplines (Gemzöe 2010; Lamont 2009), and may employ specific *judgement devices*, with potential effects on the outcomes of evaluations (Hammarfelt and Rushforth 2017; Rijcke et al. 2016). Finally, there is the institutional framework of the ever more internationally integrated scientific discipline. It functions as the macrostructure labour market, with the department as the local microstructure, creating a self-stabilising social structure.

A word of caution is in place here. I will talk about *science* a lot. I use the term throughout in its broader German/ Scandinavian sense, not as a shorthand for *natural* science. This use of the term is not intended as normative in the sense of having a scientistic ideal of knowledge production. Instead I make it a point to include *all* modern academic knowledge production in this concept, not because I believe in the unity of science, but because it allows us to be more attentive to the variations in the understanding of what rational, systematic knowledge production is and ought to be, beyond any preconceptions about the nature of different types of epistemic ideals.

Historical epistemology and scientific realism

At the heart of the styles of reasoning approach is the insight that there are multiple ways of understanding and practicing science, bundled with ontological and epistemological presuppositions about the nature of social reality, knowledge production, evaluation of valid arguments and proof, and so on. A fundamental insight from twentieth century post-positivist studies of science is that scientific knowledge production is not only a relation between a knowing theorising subject and an external empirical object; a third entity mediates the process. Scientific knowledge is always the knowledge about a discursively constituted object of knowledge, as distinct from the real object that it refers to. I think that it is useful for practical purposes to use *historical epistemology* as a general name for this insight.[2] It tells us not only, contra naïve versions of scientific realism, that we can never study reality in itself, unmediated, it reminds us that scientific knowledge is fundamentally social and historical, because all scientific cognition and perception rest on a fund of previous knowledge and its way of categorising the world. A related insight from science studies is the notion, contra the old positivist dream of the unity of science and the jargon of the one and only "scientific method", of *the disunity of science*. There is no one superior scientific method. There are different sciences, and within a single discipline there may be very different approaches to knowledge production, both regarding constituted objects and acceptable methods. Still, these various practices and procedures are all forms of an institutionalised, systematic and rational pursuit of knowledge.

Does this lead us down the infamous slippery slope of relativism? And what about the relation between the scientific object of knowledge and its referent? Is

[2] This insight has been made by many authors in slightly different forms under different names: *paradigm*, *discourse*, *style of thought*, *category*, *ontological model*, etc. (Brante 2009, 2014). The French tradition of historical epistemology has provided probably the broadest and most influential formulation of the general idea besides Kuhn (Broady 1997).

there any room here for scientific realism? I suggest that this slope is not as slippery as it may first seem. My position balances between acknowledging on the one hand the socially constructed nature of all human knowledge, and on the other hand, the reference of scientific knowledge to a real object. This dilemma is captured in what Roy Bhaskar called *the central paradox of science*:

> Any adequate philosophy of science must find a way of grappling with this central paradox of science: that men in their social activity produce knowledge which is a social product much like any other, which is no more independent of its production and the men who produce it than motor cars, armchairs or books, which has its own craftsmen, technicians, publicists, standards and skills and which is no less subject to change than any other commodity. This is one side of "knowledge". The other is that knowledge is *"of"* things which are not produced by men at all: the specific gravity of mercury, the process of electrolysis, the mechanism of light propagation. None of these "objects of knowledge" depend upon human activity. If men ceased to exist sound would continue to travel and heavy bodies fall to the earth in exactly the same way, though ex hypothesi there would be no-one to know it. (Bhaskar 1975a:11)

This dilemma implies a delicate balancing act. While realist theories of science, like Bhaskar's critical realism, often tend to overemphasise the *realist* aspect of science and the possibility of objective knowledge about its real object, the mainstream of post-positivist studies of science instead overemphasises the *socially constructed* nature of science, and is often quite suspicious of any notion of scientific realism.[3] My own interest as a sociologist, not a philosopher, lies rather in how the fundamentally social nature of science and the variability of basic epistemic conceptions may be understood, but without completely losing sight of realism. Therefore, Bhaskar's paradox is a very good navigation tool when thinking about science.[4]

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[3] Although this quote is taken from the opening chapter of Bhaskar's *A Realist Theory of Science*, the first major and foundational work of what later become known as *critical realism* in the philosophy of science, the critical realist movement has tended to emphasise the realist aspect and not really kept up with Bhaskar's task.

[4] Others have formulated it in similar terms. For example, Hacking (2002:2) remarks that "I think of myself as a 'dynamic nominalist,' interested in how our practices of naming interact with the things that we name—but I could equally be called a dialectical realist, preoccupied by the interactions between what there is (and what comes into being) and our conceptions of it". Thomas Brante (2014:163) similarly describes his realist version of historical epistemology with a phrase borrowed from Kuhn, who called himself "a Kantian with movable categories", the point being that Kant's *a priori* categories of understanding have become historicised and social.

Theorising as retroduction and critique

Theorising is a vague concept that may mean many different things, even within the sociological discipline (Abend 2008). The general structure of explanation that informs this study, and the relation between empirical material and theorising used here, is neither purely inductive nor deductive. I find it useful to think of the approach in terms of inference to the best possible explanation, what Charles Sanders Pierce called *retroduction* or abduction. As a general principle, this means that, given an empirical material and the phenomena and patterns found in it, theorising is employed to construct an explanation for the occurrence of these phenomena. Note that this differs from a conception of causality in terms of empirical event regularity of the type that underlies statistical causal inference, where constant relations between occurrences of variables are taken as indicators that could be explained in terms of a causal relation. Bhaskar shows that this understanding of causality as a "constant conjunction of events" can be traced directly back to David Hume and argues that such an empiricism is problematic in that it precludes more interesting theorising in terms of retroduction (a term also used by Bhaskar) of explanation to theoretical unobservable causal structures and mechanisms (Bhaskar 1998a; Collier 1994:163; Lawson 1997). This is how the role of theory is understood in this study. The careful constructing of an object based on previous theorising and conceptualisations (see chapters 2, 3 and 4) will inform the empirical analysis, which, in a spiralling movement in turn adjusts the theoretical framework. The role of the theoretical framework is to make sense of and explain the existence of the phenomena and patterns found in the empirical material.

This means that empirical phenomena or regularities are understood and explained by investigating their preconditions, i.e. the types of relations, limits, structures, discourses, preconceptions, arrangements, etc. that must be in place for the phenomenon to occur. We can think of this in terms of the investigation of the conditions of possibility of a given phenomenon. Understanding a phenomenon in terms of its conditions of possibility is also one of the original meanings of the term *critique*, and remains a central ingredient in the conception of social critique or critical theory (Callinicos 2006; Hörnqvist 2011). If critique in this sense can be traced back to Kant, critical theory is also an inheritance from Marx, presented in its most condensed form in the eleventh thesis on Feuerbach with the call to philosophers/scientists to not only interpret, but to change the world. It is also *interested* theory, in that it strives to not only passively observe and neutrally explain a social phenomenon (thereby potentially participating in its reproduction), but also in the last instance to change, transform, or transcend it (Callinicos 2006). However, critique in this sense should not be confused with simple advocacy or pure politics but understood as science in the broadest sense, that is, the systematic and rational attempt at understanding some aspect of the world. To clarify what this means in the context of this study, let me expand on my interpretation and application of the symmetry thesis.

The symmetry principle and heterodox economics

Fundamental to contemporary science studies is the symmetry principle, one of the four tenets of the so-called strong programme in the sociology of scientific knowledge developed in the early 1970s by David Bloor (1991), among others. Bloor argues that the sociologist of scientific knowledge should treat all knowledge as something to be causally explained. That is, all knowledge, irrespective of whether we view it as true or false today, should be treated as comprising causally explicable beliefs. There is always a social element (be it elements of general culture, the social interest and power resources of groups involved in controversies, scientific socialisation that affects how evidence is produced and evaluated, etc.) that influences the establishment of truth as the outcome of a social process of knowledge production. Therefore, we cannot explain its truth value by its being *actually true*. Under the symmetry principle *as a methodological principle*, we should study the production of all knowledge, both that which appears to be true, successful or accepted, and that which appears to comprise irrational mistakes, or is false or suspect, as symmetrical.[5] That is, we should apply the same sociological causal explanation for why people hold the beliefs they do, irrespective of the current status of their beliefs.

The research problem of this study is directly inspired by calls for pluralism in economics, the struggle of heterodox economists for scientific recognition, and their observation of the existence of a mainstream-heterodoxy divide. It is not uncommon for heterodox economists to claim that mainstream neoclassical economics is a sort of ideological pseudoscience, or that heterodox economics can provide, if not a better, then at least a similarly productive research programme. However, in my conception of a critical sociology of science, taking the symmetry principle seriously means that I suspend judgement on the truth claims of either

[5] Bloor's formulations are somewhat ambivalent, and the symmetry principle has certainly been interpreted as a radical epistemological statement (Bloor 1991; Zammito 2004). However, I think it is important to note that Bloor himself has emphasised that this is just a methodological principle, and that of course nature or any social object of science ("the way the world is") is a most important input in the scientific production of knowledge. In my reading, there is no contradiction between adopting the symmetry principle as a methodological suspension of judgement, while being committed to a weak form of realism, as explicated above.

side of the mainstream-heterodoxy divide, and instead treat the simple existence of a strong disciplinary mainstream, a marginal heterodoxy, and the relation between them, as a problem to be explained.

3. Methods and material

This study relies on extensive reviews of a few different bodies of secondary literature, and on two sources of primary empirical material on Swedish economics. The first of these is a set of twenty in-depth interviews with economists. The informants were researchers in economics in Sweden, although some were retired or, in a few cases, held positions outside economics departments. They were selected to represent a variety of career stages (from doctoral students to mid-career researchers to senior professors), institutional belonging (representatives from economics departments at the five leading universities departments, plus heterodox economists; in some cases, respondents from other departments), and research fields (from macroeconomics to econometrics to behavioural economics). A full account of the process of selection, interviewing, analysis and other considerations may be found in the methods chapter (chapter 6).

The second set of material used comprises expert evaluation reports (*sakkunnigutlåtanden*) from the recruitment of full professors in economics over 25 years (1989–2014) at four of the leading Swedish universities.[6] In the Swedish university system, these are public documents, which means that they are available in the university archives. These documents are therefore a unique source for explicit accounts of how scientific quality is interpreted and negotiated by expert reviewers. It is in the scientific peer review system that the boundaries between good science, worthy of publication, promotion, and funding, and the not-so-good science are drawn. The peer review system is a fundamental mechanism not only for the credibility of science, but also for the reproduction of disciplinary borders. If journal or grant peer review may be interdisciplinary, the expert reviews involved in hiring and promotion constitute the site where the definition of the boundaries of economics are drawn in practice. The expert reports, of varying

[6] The universities of Gothenburg, Lund, Stockholm, and Uppsala, and Stockholm School of Economics (SSE) are the leading five according to various measures discussed in chapter 5. However, since SSE is a private university, its evaluation reports could not be acquired under the Swedish Freedom of Information Act which governs public institutions. Thus these documents were only collected from the other four universities.

lengths, were collected from university archives, and include almost all evaluations of candidates for full professor (both chaired and promoted) during this 25-year period.[7] The twenty most promising documents were singled out for qualitative analysis of how quality criteria and disciplinary identity is expressed, as well as the observation of shifting evaluative practices during the period.

4. The contributions of this study

In this study I have tried to follow the call of Camic, Gross and Lamont (2011), to turn the science study gaze away from the natural and towards the understudied social sciences, and examine how *social knowledge* is actually made in practice. While many aspects of the general account of economics presented here will be familiar to anyone with an interest in these issues, this study provides at least four novel contributions to our knowledge about knowledge production in modern economics.

First of all, it is a theoretical contribution to the sociology of science through the theoretical framework of relational disciplinary styles of reasoning. While the literature on styles in the history and philosophy of science has been a fruitful way to emphasise the relatively stable cognitive macrostructure of science, it has lacked both a connection to the social structure of science, and a sociological account of actors in science and their role in the reproduction of styles. On the other hand, despite the flourishing of contemporary science and technology studies, and the large amount of work being produced, these studies often lack a structural macroperspective. In this study I bridge this theoretical gap and show how the relational disciplinary styles framework can help us think about the way that enduring intellectual dispositions and conceptions of science connect to and are stabilised by the social organisation of the discipline, how actors-economists are highly socialised through generalised formal training, and how their engagement in boundary work and quality judgement reinforce the disciplinary style in relation to its outside. This novel approach attempts to adapt the styles approach for empirical sociological purposes by synthesising it with the concepts of scientific habitus, boundary work, and scientific discipline. Thus, this overarching theoretical framework allows us to explain the self-stabilising dynamics of the economics discipline, and how it links the cognitive and social, enduring structures and actors to form a social system.

[7] Excluding a small number of documents that could not be found in the archives.

Second, on a more concrete level, while the concept of styles of reasoning has been used to analyse the way economists work and reason by philosopher Mary S. Morgan (2012), hers is an historical account of modern mainstream economics, and heterodox traditions and critique doesn't feature in it. The novel approach I use here is to understand not only mainstream economics in terms of such styles, but also the struggle and intellectual divide between the mainstream and heterodoxy as fundamentally a case of opposing and incommensurable styles of reasoning. I don't claim that everything can be reduced to different styles of reasoning, but I do think it makes up a substantial aspect of the disagreement, and that this theoretical framework may provide a fruitful and novel way to understand the essence of this contradiction and its persistence.

Third, a more specific contribution is the study of how journal rankings are used in quality evaluation. While a recent study has shown how evaluators increasingly use increasingly prevalent bibliometric indicators as judgement devices in peer review (Hammarfelt and Rushforth 2017), it only concluded that the use of journal rankings is widespread in economics. With the analysis of expert evaluation reports over 25 years, I can show both *how* the transformation of evaluation practices in Swedish economics gradually played out, and furthermore how these new evaluation practices, relying on top economics journal rankings, function in the reproduction of disciplinary boundaries. Understanding the role of quality evaluation is central to explaining the stability of a disciplinary style of reasoning, and I discuss how boundaries are reproduced through both traditional peer review and new forms of quantified evaluation practices relying on judgement devices. I point to the relation between evaluation practices and the bigger picture of disciplinary style. This is evident both in how evaluators draw on their disciplinary habitus in making judgements, but also through the way in which the idea of a set of "top" journals merges with the notion of a disciplinary core that influences normative and strategic scientific ideals.

Fourth, while there are many historical studies and some sociological studies of economists and the way they work, to my knowledge there is no prior study like this of economics in Sweden. I show that the discipline is very internationalised in outlook, and that Swedish economists understand themselves primarily as part of an international field of economics. This is however a recent and still ongoing transformation. I show how this plays out, for example as the internationalisation of expert reviewers in evaluation documents over 25 years. But there is also a marked strategic element evident in the interview material, when economists account for the internationalisation and standardisation of doctoral programmes, and more recently the gradual integration of Swedish departments into US and international job markets for junior academics. It is clear that this internationalisation is part of an ongoing process, and that it is highly intentional and desired. It also means that although this is a case study with empirical data from Sweden, we may expect many of the findings to be valid in other similar settings.

Limitations

Although the problem is formulated in general terms about modern economics, the scope of this study is limited to contemporary Swedish academic economics. A historical background sketch is provided using secondary literature in chapter 5, and the evaluation report material allows me to cover a 25-year period and analyse change over that time.I draw (like most economists) a sharp line between economics (*nationalekonomi*) and other disciplines or interdisciplinary fields that deal with economic phenomena, like business administration (*företagsekononomi*) or economic history. The reason is that the problem this study investigates is about how knowledge is shaped specifically within a particular scientific discipline. In the same manner, when studying *heterodox* economics, I focus on self-identified heterodox economists and their critical relation to the mainstream, rather than those forms of heterodoxies that are often found in interdisciplinary settings.

Furthermore, this study tries to explain how the macropattern of a style of reasoning is reproduced at the level of the discipline, while anchoring the phenomenon empirically at the microlevel of actors in the form of very general intellectual dispositions. However, the specific theoretical issues and debates among schools of thought, especially in macroeconomics, will have to remain outside my present focus. I draw extensively on secondary literature that analyse economic theory and schools of thought, but such analysis is itself outside the scope of this study.

There are many social factors that could have been brought into a sociological study of the economics discipline, which I had to leave out due to the scarcity of resources. These include the relation between economic theorising and political ideologies and movements, in both mainstream and heterodox approaches. Another such question is the harmony or resonance between ideas held in science and those that are part of the wider culture. For example, how does the experience of living in a capitalist society and cultural imagery of rationally calculating individualist actors resonate with and validate theoretical notions of markets and rational model actors? Similarly, but in a more materialist vein, this study does not focus on the class background of researchers, the economics of research funding, or the societal demand for particular forms of knowledge.

These are all valid objects of study, and many probably quite important for a full picture of the contemporary role and development of economics. Moreover, I do not attempt to show that economics is "bad science", or to define what good science in economics ought to be like. My object of study is how good science is understood and practiced within the economics discipline, and how such a collective understanding is socially reproduced. In this study, I am contributing a novel argument about the stabilising forces of a disciplinary style of reasoning. While I do not explicitly discuss a concept of power, the reader will perhaps notice that power is everywhere. The styles approach is all about structured and productive constraints on our minds. Power, if you will, or, disciplined reasoning.

5. Outline of the thesis

The thesis starts with an exploration and development of the research problem in chapter 2. It situates the recent rise of heterodox economics through a discussion of the various forms of critique that has been levelled at mainstream economics since the 2008 crisis. The consolidation of various schools of thought under the umbrella label "heterodoxy" since the 1990s has given rise to debates about what neoclassical and heterodox economics really mean. Through a strategic literature survey of the main positions in these debates among historians of economic thought and heterodox economists, different conceptions of the nature and historical durability of the mainstream and its relation to heterodoxy are explored. Finally, building on this review, I present three analytically distinct aspects of this divide that emerge from this literature. These are the *ontological* (axioms about the nature of social actors and relations), *epistemological* (methodological imperative of formal modelling practices), and *social* (a divide between distinct relationally constituted social thought collectives) aspects.

Chapter 3 presents an extensive review of previous theory and research in the relevant areas indicated in chapter 2. The chapter opens with a brief general account of the historical development of the modern field of science and technology studies as part of a broader cross-disciplinary development during the last century of post-positivist approaches to science. The next section devotes considerable attention to the styles of scientific reasoning literature, discussing its development and different formulations. The third section is a literature review focusing on previous research that takes economics as its object. I then zoom in on the work of Mary S. Morgan, who represents a combination of the two previous sections in her historical study of modern economics in terms of Hacking's styles of reasoning. However, her study takes a disciplinary style of reasoning for granted, and doesn't consider the existence of internal heterogeneity in the form of the mainstream-heterodoxy split. Finally, I review previous research on evaluation of scientific quality and, overlapping with that, studies of Swedish expert evaluation reports.

In chapter 4 the theoretical framework is developed. It proceeds with a synthesis of my interpretation of styles of reasoning, with relevant sociological concepts, and moves from the overarching cognitive structure of historical styles, towards the social structure of thought collectives and disciplines, and onto the socialisation and agency of actors engaged in boundary work and evaluation practices. The notion of relational disciplinary styles is thus developed as a sociological version of the styles approach.

Chapter 5 provides a brief background to the empirical case with a very brief account of the general development of economic thought, with a special focus on Sweden, and an overview and descriptive data on education and research in contemporary Swedish economics.

Chapter 6 present the empirical material and methodological considerations at some depth. The chapter is divided into two sections. The first covers the interview study and provides first a general background on interview methodology, and then the principles behind the selection of informants, the interviews, and their analysis. The second section provides an introduction to the institution of expert evaluation reports in Swedish academic hiring (*sakkunnigutlåtanden*), and reports the collection and selection and analysis of the material, together with some summarising quantitative data on the material.

The results of the interview study is the topic of chapter 7. The chapter is thematically organised, and illustrates the interpretation of the interviews with extensive excerpts from the transcripts. Among the themes covered are the disciplinary identity and the view of the doctoral programmes as an important institution for maintaining "a strong central paradigm". Discussions about the core of economics also circle around notions of a small set of common theoretical tools or points of departure, and the importance of strong methodological skills in modelling and econometrics. However, there are also accounts that downplay disciplinary identity and argue in terms of the strong similarity between economics and other sciences. Boundary work seems to sometimes be a matter of disciplinary boundaries, as in jokes about neighbouring disciplines, but it doesn't necessarily adhere to discipline. Instead, I show how the styles approach allows us to make sense of these situations as cases where similarities in style of reasoning cut across disciplinary boundaries and thus act as bridges, while in other cases it functions as effective barriers against heterodox approaches within the discipline. The heterodox interview narratives point to important sources of intellectual socialisation outside the disciplinary core, and they include accounts that diverge strongly from the majority view.

Chapter 8 is the second empirical chapter, devoted to the analysis of expert evaluation reports. The analysis uses the lengthy reports where expert evaluate scientific oeuvres of the candidates for professorships to elicit an image of the normal science of modern economics. Since the reviewers are by definition senior trusted professors who are chosen for being good representatives of the discipline, the collective effect of their judgement is not only the outcome of evaluations, but can also be taken to represent the most authoritative view of the core of the discipline. In some especially interesting cases, we can follow their argumentation about the boundaries of the discipline. I find that a few features are expected of an excellent economist. Among these are command of modelling as a central epistemic practice, and technical econometric skills. However, it is not enough to be technically or mathematically skilled; something more is required, namely a knowledge of the core of economics, which is the ability to reason with economic theory, and to be able to ask really interesting questions, real "economic" questions. This material also grants us insights into institutionalised evaluation practice and how experts reason to justify and legitimate their judgement. During the studied 25-year period, there has been a marked shift in these evaluation practices, as evaluators rely increasingly on the technical judgement device of journal rankings instead of extensive reading of submitted materials.

In chapter 9 I finally draw together and extend the empirical findings of the previous chapters into an overarching analytical discussion. Among other things, I argue that the increasing use of journal ranking metrics in expert evaluation reports has parallels to the plethora of similar social situations where metrics and numbers govern and regulate the social world, and suggest that we could think of it as yet another *classification situation*. The closing discussion broadens the perspective to a comparison with other disciplines, and opens a set of new questions generated by the relational disciplinary styles approach. Finally, I attempt to give some indications of the implications of this study, both for the advocates of pluralism in economics, and for other social scientists, including sociologists.

Chapter 2. Mainstream and heterodoxy in modern economics

The overarching problem of this thesis is to make sense of and explain the stability of the common scientific approach in modern economics, and how it relates to heterodoxy. This problem raises a number of further questions. For example, what exactly do "mainstream" and "heterodoxy" mean, and to what extent can we say this is an enduring intellectual divide? The purpose of this chapter is to provide background and context in order to specify the problem. The first section will give a brief background, starting with the popular critique of the economics discipline in the wake of the 2008 financial crisis as an entry point to the problem. From there, we will turn towards the emergence of a recognisable intellectual community of heterodox economics a decade or so earlier. The rise of heterodox economics meant that the nature of orthodoxy and heterodoxy became a core topic among heterodox economists and historians of economic thought. The section that follows is a strategic literature review of the major positions in these ensuing debates with the purpose of shedding light on seemingly basic concepts like mainstream, heterodox and neoclassical. The third and final section synthesises this survey and proposes an analytical distinction between three aspects (ontological, epistemological and social) of the intellectual divide that are present and variously emphasised in the literature.

1. The critique of mainstream economics

The critique of mainstream economics can be said to come in three different forms that might be termed the *popular critique*, the *student critique* and the *heterodox critique*. Against the common (at least among economists) perception that those who are critical of the way mainstream economics is done are simply ignorant, it should be emphasised that the heterodox critique of mainstream economics cannot be reduced to ignorance. While this may sometimes be partly true of popular or student critiques, heterodox economists often hold economics doctorates and are active as researchers within (albeit on the margins of) the discipline. Their points of view cannot be dismissed as plain ignorance. In any case, the existence of a vocal heterodox economics points to the problem of the stability and boundaries of mainstream economics, both through its mere existence, and through the arguments and research spurred by this group. Understanding the role of heterodox economics is easier when seen in the context of well known popular and student critiques.

Economics critique in the public debate after the 2008 crisis

An international wave of critique was directed against academic economics in the wake of the great 2008 financial crisis. Up until then, the economics profession had experienced great confidence; perhaps most of its members still do. The apex of trust in the economics profession is probably to be found just before the crisis hit. In 2003, Chicago economist Robert Lucas (2003:1), one of the leading figures in modern macroeconomics, claimed in his presidential address to the American Economics Association that macroeconomics "has succeeded: Its central problem of depression prevention has been solved, for all practical purposes, and has in fact been solved for many decades". In 2004 Princeton professor Ben Bernanke (2004), later head of the Federal Reserve, said in a similar vein that we had entered the age of "the Great Moderation", where economic ups and downs have been more or less smoothed out with the aid of better economic institutions and improved macroeconomic policies. Even when the now-infamous US subprime markets started to look shaky in 2007, leading economists were still reassuring: "If we have learned anything from the past 20 years it is that there is a lot of stability built into the real economy" (Lucas 2007). In short, the message was that the era of economic crises is over. If we just continue to leave the markets undisturbed by regulation, let independent central banks focus on targeting inflation, and don't let politicians interfere with populist economic stimuli, we do not need to worry about crises anymore. And we certainly don't need to worry about big downturns like the Wall Street crash of 1929 and the Great Depression that followed.

But in 2008, the crash happened nevertheless. The crisis led to a brief period of public questioning of the economics profession. "How did economists get it so wrong?" asked Paul Krugman (2009) from the heights of his influential *New York Times* column. His answer was: "the economics profession went astray because economists, as a group, mistook beauty, clad in impressive-looking mathematics, for truth". Economists before the Great Depression had tended to view capitalism as an intrinsically well-functioning and near perfect system; this seemed to have been the case once again. What is more, Krugman and other critics pointed to a set of general perceived problems with mainstream economics. First, there seemed to be a preoccupation with impressive-looking mathematics; second, this formed the backbone of abstract models far removed from the real world. Most critiques have revolved around these two factors in some way, sometimes connecting it to the seeming lack predictive or explanatory capacity of the discipline.

There are numerous well-known examples of this type of questioning of what academic economists were actually doing. For instance, the story has been often retold of how Queen Elizabeth addressed the economics profession at an official visit to the London School of Economics, asking how it could be that virtually no one in the profession foresaw the threat of a major financial crisis (Stewart 2009). Two years into the crisis, Robert Solow (2010), the Nobel laureate and well-known economist, gave a prepared statement before the United States Congress, in which he claimed that the modelling assumptions of modern macroeconomics do not "pass a smell test: Does it really make sense?". This kind of popular critique was widely heard in the public debate. For example, Paul Krugman has a huge following outside the economics profession, and many journalists were not slow to pick up his views. Unconstrained by professional allegiances, we heard opinions like that of Larry Elliot (2009) at *The Guardian*, who claimed that "As a profession, economics not only has nothing to say about what caused the world to come to the brink of financial collapse last autumn, but also a supreme lack of interest in it".

This type of critique was also voiced in the Swedish context. To cite just a few examples, macroeconomist Lars Jonung wrote in the newspaper *Dagens Nyheter*, that "Today's global financial crisis has triggered a crisis also for the economics discipline", commenting on the initiative by the investor George Soros to donate US$ 50 million to the creation of the Institute for New Economics Thinking (INET) to promote reorientation in economics (INET 2015; Jonung 2010). The supposed crisis within economics, and its failure to warn of the 2008 crisis, was the theme of a series of radio programmes aired by the public service science magazine *Vetandets värld* (Zachrisson 2012). That radio programme was just one of many occasions when the well-known aphorism of Swedish novelist August Strindberg, from the 1880s, was retold: economics is "a science invented by the upper class to get hold of the fruits of the labour of the underclass". Likewise, human ecology professor Alf Hornborg likened the role of modern economics to that of the church in mediaeval society. The profession also had to defend itself against what Lars Calmfors, another well-known macroeconomist, called "economist haters" (Zachrisson 2012). No doubt, economics and economists have increasingly been the targets of both interest and popular critique in the public debate since 2008. But they have also faced a second form of critique, with protests coming from closer to the profession.

Economics students become activists

Economics students, some of whom would form the next generation of the economics profession, soon joined its critics. In November 2011, a spark flew from the street protests of the Occupy Wall Street movement to Harvard University, where a group of students staged a walkout from top economist Greg Mankiew's introductory economics class (Beggs 2011; George 2011). In their open letter to Mankiew, the author of one of the standard textbooks internationally in economics, the students argued that the course did not give room to any critical discussion of the simplifying models taught, nor mention alternative approaches. According to the letter, "If Harvard fails to equip its students with a broad and critical understanding of economics, their actions are likely to harm the global financial system. The last five years of economic turmoil have been proof enough of this" (Concerned students of Economics 10 2011). The following year, on the other side of the Atlantic, the Bank of England arranged a conference on the topic of undergraduate economics curricula, with the telling title "Are Economics Graduates Fit for Purpose?" Inspired by the event, economics students in Manchester founded the Post-Crash Economics Society later the same year, with their main goal to "broaden the range of perspectives and the teaching methods used by the Manchester Economics Department" (The Post-Crash Economics Society 2013).

This student activism spread to universities around the world, and led to the formation in 2014 of an international network claiming a membership of over sixty student organisations from thirty countries under the name The International Student Initiative for Pluralism in Economics (ISIPE) (ISIPE 2014). The unique nature of this network was captured in a *Financial Times* column commenting on the student protests and the creation of ISIPE: "In no other subject do students express such organised dissatisfaction with their teaching" (Kay 2014). Among the local student organisations giving their support to the initiative were two Swedish groups, *Lunds kritiska ekonomer* (Lund's Critical Economists) founded in 2013 at Lund University, and Handels Students for Sustainability founded in 2012 at Gothenburg University (Karlsson 2012; Skoog 2013). In their widely-published open call for pluralism, ISIPE set the stage by arguing that "It is not only the world economy that is in crisis. The teaching of economics is in crisis too, and this crisis has consequences far beyond the university walls. What is taught shapes the minds of the next generation of policymakers, and therefore shapes the societies we live in" (ISIPE 2014).

According to the student groups behind the call, economics has experienced a "dramatic narrowing of the curriculum" over the last decades, leading to a discipline lacking in intellectual diversity and becoming increasingly irrelevant for tackling real world problems (ISIPE 2014). The call identifies the need for three forms of intellectual pluralism: theoretical, methodological and interdisciplinary. Here, theoretical pluralism means acknowledging that there are different schools of thought implying different conceptual tools and modes of analysis. It means resisting the textbook presentation of the theory in singular in favour of a presentation of various alternative theories. Methodological pluralism means acknowledging not only the advanced mathematical training required by much modern economic analysis, but also other methodological approaches. And interdisciplinary pluralism means striving for fruitful interdisciplinary interaction, lending useful insights from other disciplines were it is appropriate.

The distinction between the three forms of critique is used here in the hope of clarifying the overarching argument, that there are different voices in the debate, some of which belong to lay commenters and others to experts. With that said, it is also the case that the popular and student critiques draw heavily on the work of heterodox economists. More or less dissenting established economists are used to make a point about the shortcomings of the mainstream. Examples might include sharing Paul Krugman's *New York Times* blog posts via social media, journalists interviewing economists with a different viewpoint, as the Swedish current affairs magazine *Fokus* did in the 2011 piece "The fallen prophets", in which one of the most outspoken domestic critics of mainstream economics, the economic historian and economist Lars Pålsson Syll, was interviewed (Lönegård 2011). Similarly, an important part of what student organisations do is to familiarise their members with those *other* economists that are not taught as part of the curriculum. They may arrange their own evening lectures with invited economists talking about alternative forms of economics, as when the Harvard students who walked out on Greg Mankiw invited heterodox Harvard economist Stephen Marglin to give a lecture on "Heterodox Economics: Alternatives to Mankiw's Ideology" (Marglin 2011). Study groups focusing on neglected classics are also important activities, as when *Lunds kritiska ekonomer* arranged a reading group on Keynes' *General Theory* in 2013.

Furthermore, some economists (like Krugman or Jonung mentioned above), may not view themselves as heterodox economists, but rather as critical voices on the inside, and are sometimes publicly supportive of the student initiatives. For example, in 2013 a number of prominent academics wrote a letter to *The*

Guardian in support of the newly-founded Manchester Post-Crash Economics Society, claiming, in a tone similar to the students themselves, that the "dogmatic intellectual commitment" of contemporary mainstream economics "contrasts sharply with the openness of teaching in other social sciences, which routinely present competing paradigms" (Inman 2013).

While these student organisations are many and well-organised, and part of a growing international movement, it is easy for senior economists to dismiss their critique as uninformed. Perhaps the protesting students have only studied a few semesters in their economics departments, and perhaps there is vastly more to know about modern economic analysis. Maybe you only get to learn all the exceptions to the simplifying assumptions in graduate school. These are common ways of neutralising critique, and were voiced by many of the professional economists I interviewed in this study. If both popular critique and student critique can be dismissed thus, however, the same is not true for those critics who are actually trained economists themselves, and part of the profession as researchers, although often in marginal departments or areas. This third and most important form of critique is the internal scientific critique from heterodox economists.

The rise of heterodox economics

Zooming out from the post-2008 crisis, it becomes evident that none of the three forms of critique is new. There has certainly been a renewed interest in alternative approaches in economics as an effect of the crisis and the strongly felt need to do something about it, and the international scope and organisation of the critics seems to have reached new levels. But in essence, the type of critique that is voiced today has a longer history.

The immediate roots of the contemporary student movement are to be found in Paris. In 2000, a group of students at the École normale supérieure published a petition protesting the way economics was taught. [8] They claimed that economics had become autistic: living in its own imaginary model worlds, it had faded out of touch with the real world and its problems. A public debate followed, starting in *Le Monde*, and the French minister of education became involved. The debate spread to the United Kingdom, where a group of doctoral students set up a network of economists, harnessing the relatively new powers of the internet

[8] One could also point to an important "Call for a Pluralistic and Rigorous Economics" in *American Economic Review*, signed by a range of well-known economists including several Nobel Laurates in 1992 as an important starting signal for these types of discussions (Hodgson, Mäki, and McCloskey 1992; Lancastle 2014).

(Backhouse 2010:5–6). This was the start of the post-autistic movement in economics, and the network soon got many followers among both students and senior economists. An electronic newsletter was set up, *The Post-Autistic Economics Review*, which later changed its name to the more appropriate *The Real World Economics Review*. In 2011, the heterodox community that emerged around the network formed the World Economics Association (WEA), with the aim to fill "a gap in the international community of economists—the absence of a truly international, inclusive, pluralist, professional association." As of 2015, WEA claimed over 13,000 members worldwide, ranking as the world's second-largest professional economics association (World Economics Association (WEA) 2015).

Yet, while the post-autistic movement and the new mode of networking enabled by the internet grew in the first decade of the new millennium, heterodoxy in economics dates still further back. Frederic S. Lee, historian of heterodox economics, claims that the term "heterodoxy" was used in the old American institutionalist school of thought, deriving from Torstein Veblen among others. In that context, the term "heterodox" was used "as an identifier of an economic theory and/or economist that stands in some form of dissent relative to mainstream economics" from roughly the 1930s to the 1980s (Lee 2008). In the late 1980s and 1990s, the term became used to denote not only the old institutionalist school of thought, but to include Marxian and post-Keynesian theories under the same umbrella of dissent. According to common heterodox narratives, mainstream economics has been strictly dominated by a single paradigm at least since the end of the Second World War, if not longer. And there have always been other schools of thought. The most obvious example is that of Marxian economics, which predates the birth of modern marginalist economist in the 1870s. Other examples include institutionalism, mentioned above, which was alive and well in the United States amidst the relative pluralism of the interwar years (Morgan and Rutherford 1998), and of course John Maynard Keynes and his so-called post-Keynesian followers and colleagues like Joan Robinson.[9]

If one turns the perspective around and look at the critics of the mainstream from the point of view of the latter, it has been claimed that "Economics is the only established discipline that is regularly charged not just with including ideologically motivated research programmes and hypotheses, but with actually *being* (at least in its institutionalised mainstream form) an ideology", a charge

[9] Note the important distinction between, on the one hand, the formalised version of Keynes's theory and its integration with neoclassical economics—"neo-Keynesianism", one of the major schools of thought in modern macroeconomics—and on the other hand the heterodox so-called post-Keynesians, followers of Joan Robinson and others who criticize the formalization of Keynes's work, and tend to emphasize its qualitative aspects like risk and irrationality to a greater extent.

levelled by what has been called a tradition of "anti-economics" as old as economics (or rather, political economy) itself (Ross 2012:241). This sense among economists of being the constant target of outsiders' critique, and the observation that "economics is actively *hated* by a substantial number of people" (Ross 2012:241) sets it apart from most academic disciplines.[10]

If dissenting schools of thought within economics have a long history, it was only in the 1990s that heterodox economists started to think of their heterogeneous work as part of a wider network of approaches unified under the label of heterodox economics. According to Frederic Lee:

> By the 1990s, it became obvious that there were a number of theoretical approaches that stood, to some degree, in opposition to mainstream theory. These heterodox approaches included Austrian economics, feminist economics, Institutional-evolutionary economics, Marxian-radical economics, Post Keynesian and Sraffian economics, and social economics. (Lee 2008:2)

Lee gives a fairly standard list of the various approaches counted as heterodox, though one could also add, for example, ecological economics. In his account, it is important to note that heterodox economics not only denotes marginal schools of thought, but also a community of marginal economists. These are organised in a number of professional societies, many of them founded in the 1960s and 1970s, and they typically publish in specialised heterodox economics scholarly journals. For example, Lee notes that among the eight leading heterodox journals there are very different fields of focus, with the *Cambridge Journal of Economics* as the most influential, and other more specialised ones like *Feminist Economics* or *Journal of Post Keynesian Economics*. However, all cite each other to such a great extent that they belong to the same strong citation network. In 1998, the Association for Heterodox Economics (AHE) was established as an umbrella organisation and, along with its electronic newsletter, became an important part of the institutional infrastructure of the international community of heterodox economists.

Lee (2009) has been the primary promoter of the idea that heterodox economics has increasingly taken the shape of a broad community in opposition to mainstream economics in the last two decades, and argues that this process is coupled with increasing theoretical interaction and integration, and the creation of new institutions like the Association of Heterodox Economics. I call this notion of increasing heterodox integration and identification the *Lee thesis*. This

[10] Examples of disciplines experiencing similarly strong reactions by outsiders would include gender studies, although the reactions and social basis of critique is probably diametrically different.

unification does not rest on a shared perspective so much as a shared opposition to what is often called the neoclassical mainstream, and the promotion of scientific pluralism in economics. Heterodox economists often hold pluralism as a central goal, and argue in terms of opening research in economics to more than the one dominant paradigm.[11] But when it comes to shared ideas of more substance, most observers seem to agree that heterodox economics is really only, or at least primarily, unified by its opposition the neoclassical mainstream. But exactly what *neoclassical* means (or if it is even a useful term), or what unifies heterodoxy beyond that, is the object of heated debates today. Since heterodoxy seems to be a fundamentally relational concept, understood in relation to a neoclassical mainstream, understanding the intellectual divide between mainstream and heterodox economics requires a more refined understanding of that "neoclassical" mainstream.

2. Making sense of mainstream and heterodox economics

Heterodox economics is primarily understood in relation to a neoclassical mainstream, and heterodox economists typically reject what are taken to be the latter's core assumptions, for example scarcity, methodological individualism, rationality, equilibrium, mathematical modelling, closed systems, stable preferences, etc. But exactly what *neoclassical* means or what unifies heterodoxy is debated, both by historians of economic thought and by heterodox economists. Why this debate?

Clearly, the characterisation and definition of the mainstream, and thereby also of heterodoxy, seem to be at stake. At one level, defining an intellectual divide in economics (or claiming its non-existence), is a matter of producing symbolic boundaries, beyond a pure interest in better classification. But debating neoclassicism is also a matter of historiography. Has mainstream economics changed or remained more or less constant in some essential way during the greater part of the last century? Modern mainstream economics is of course not a homogenous body of knowledge. But many would agree that the discipline is comparatively homogenous, that it is unified by a rather tight set of shared beliefs and practices, and that this common understanding is both very much

[11] For some examples of recent discussions of pluralism, see the work by Dobusch and Kapeller, Garnett, and Mearman (Dobusch and Kapeller 2012; Garnett 2011; Mearman 2011).

international and US-oriented, and possible to study as such (Yonay and Breslau 2006:349).

In this section, I will draw on a survey of recent debates among historians of economic thought and heterodox economists on the nature of neoclassical, mainstream and heterodox economics. This preliminary literature review will serve to answer two main questions. First, in what sense can we talk about an enduring mainstream in modern economics? Second, what is the nature of its relation to heterodoxy? Starting with the relational classification concepts "mainstream", "orthodox" and "heterodox", we will then move on to discussions about the substance of neoclassical economics and the contested question of how to characterise continuity and change in modern mainstream economics.

Classification trouble in the history of economic thought

To make sense of any research field, we need some sort of terminology to reduce complexity and cut through the vast mass of actual research being done. We need a classification of different forms of intellectual production, and a rationale for classifying as we do. For economics, the job of creating classifications has largely been the domain of historians of economic thought, though explicit discussion about classification as such is rare even among them (Colander 2000:128). Nevertheless, every textbook on the history of economic thought has to rely on some sort of classification of at least the major periods or schools of thought. They commonly rely on labels like "classics", "marginalists", "neoclassicals" and so on. Of course, beyond textbooks, historians of thought dispel with such labels in close empirical studies of authors or connections between a smaller number of researchers or local environments. But once big, overarching issues are at stake, like questions about essential attributes of the research of a historical period, or core features of more or less historically persistent schools of thought, such concepts are needed. At first sight, these concepts may seem straightforward. But I will try to show that there is often no clear consensus, and that these concepts are in fact often contested. When it comes to describing the characteristics of contemporary economics and the type of research done in its mainstream, there is no consensus on terminology beyond some of the most basic and broad terms. Thus, we need to investigate the concepts involved and the way they have been used more closely.

The terms *mainstream*, *orthodox* and *neoclassical* economics are sometimes used as synonyms, which easily leads to conceptual confusion. Mainstream is a common but often rather loosely used term that could be thought of as a sociologically defined category. According to the renowned historian of economic thought David Colander and his co-authors,

> mainstream consists of the ideas that are held by those individuals who are dominant in the leading academic institutions, organizations, and journals at any given time, especially the leading graduate research institutions. Mainstream economics consists of the ideas that the elite in the profession finds acceptable (Colander et al. 2004:490)

To classify something as orthodox should also be rather straightforward. Colander and his co-authors point out that as opposed to mainstream, orthodoxy has a temporal dimension to it as a static representation of a backward-looking approach. To call a person or a school of thought "orthodox" clearly implies a conservative, frozen, unchanging quality, according to Colander.[12] This is also clear when one looks at the purpose of classifying something as orthodox: "in economics at least, the name for the orthodox school usually comes from a dissenter, who is opposed orthodox ideas, not from a supporter of the orthodox ideas" (Colander et al. 2004:491). Clearly then, applying the term orthodox to the currently dominant type of economics implies something more than the relatively neutral term "mainstream". It is, indeed, a pejorative term used mainly by critics of orthodoxy to point to what is conceived as its unchanging nature.

Sometimes the term "orthodoxy" is used in an even more specific sense, to contrast it with the dissenting "heterodoxy". Heterodox economist Frederic Lee makes explicit the connection to the etymological origin of the terms in theology. Just as in the matters of the church, orthodoxy means conforming to the established *doxa*. Lee (2009:4) claims that the theological distinction between heresy and blasphemy can be transposed to economics, arguing that while the heretic is a true believer who holds some dissenting views, the blasphemer "is a non-believer who explicitly, through reasoned arguments, wit, and ridicule, rejects the state religion and its sacred doctrines and institutions". Thus, while some degree of dissent is tolerated, "mainstream economists have attempted to suppress the economic ideas and arguments of blasphemous economists, whom they do not generally consider their brethren at all" (Lee 2009:6). This narrative of comparison between modern mainstream economics and the power dynamics of the pre-modern church shows just how strongly some heterodox economists identify as dissenters and oppositional, and it is perhaps also a sign of the extent to which this identification and the relation to the mainstream is bound up with

[12] However, against this one could also claim that it is a rational strategy and a sign of a mature science to stick to a well-proven paradigm, and that this is the case in many sciences.

an emotionally charged lived experience for many heterodox economists (Morgan 2015:13).

There seems to be general agreement that the term *heterodoxy* is clearly defined only in its opposition to the perceived orthodoxy. For example, in a study based on a series of interviews with historians of economic thought, Mary Wrenn (2007) shows that there is no clear consensus about the meaning of heterodoxy and the precise boundaries between the mainstream and heterodoxy. However, most scholars seemed to agree that there is something like a community of heterodox economists and that it is characterised by being "pushed out" from the mainstream, which the heterodox economists in turn more or less reject. In his history of the movement, Lee (2009) similarly describes the heterodox project as united by a central rejection of what he interchangeably calls mainstream or neoclassical economics. Standing closer to the mainstream, Colander et.al. (2004:492) also observe that among the multiple schools of thought that make up the heterodox movement, beyond the "rejection of the orthodoxy there is no single unifying element that we can discern that characterizes heterodox economics". Commenting on this and a range of other similar assessments in the search for the essence of heterodox economics, heterodox economist and philosopher Tony Lawson (2006:485) concludes that "we appear to reach an apparently widely shared assessment of heterodox economics only in terms of what it is not, or rather in terms of that to which it stands opposed; the one widely recognized and accepted feature of all the heterodox traditions is a rejection of the modern mainstream project."

We can now produce some first definitions. *Mainstream economics,* we might argue, following Colander and co-authors, encompasses *the ideas and practices that dominant economists in leading academic institutions, organisations and journals find acceptable at any given time.* This definition is agnostic with regard to any historical continuity or change. The term *orthodox economics* on the other hand implies that the mainstream is also *a historically enduring project with some sort of common, stable core that resists change*. Defining the self-conscious opposition to orthodoxy, *heterodox economics is an umbrella term for the various schools of thought whose minimal common ground is their rejection of orthodoxy, as they understand it.* If these three concepts should be somewhat clear by now, it is at least partly due to their relational character.[13] Let us now turn to the substantial but elusive concept of *neoclassical economics*.

[13] For example, what a mainstream theory is, is an empirical sociological question related to a specific scientific establishment at a particular time and place. Consider for example the role of Marxian economics during the Cold War at, say, universities in the Soviet Union and the United

First attempts to define "neoclassical" economics

The term neoclassical economics has been used for over a century to describe specific schools of thought or ways of doing economics. At least since the 1950s, it has been a common term known to anyone within the economics profession. Still, there exists no consensus on a stable definition of the term. Indeed, the term can be taken to mean quite different things. For example, some use it to denote a quite well-delineated school of thought, while others use it in a much wider sense to describe a style of thinking that includes many different schools of thought in the more narrow sense. And even in that wider sense, exactly what the term means is highly contested. In general, it is probably uncontroversial to say that the term is most often used by critics, that is, by heterodox economists, even to the extent that Colander (2000:132) irritably exclaims: "I can always tell when I am around heterodox economists by the number of times I hear the term". Still, the term is hardly foreign to most mainstream economists.

A measure of the extent to which the term is contested is a recent volume with contributions from leading heterodox economists, *What is neoclassical economics?* (Morgan 2015). This collection revolves around a recent paper by Cambridge philosopher of economics Tony Lawson (2013) bearing the same title. Lawson traces the origins of the term back to its coinage by Thorstein Veblen in a 1900 paper, in order to reinterpret what Veblen actually had in mind when he introduced the concept. It turns out that Veblen used the concept to shine a light on something very different from the various contemporary uses of the term. Lawson (2013:34) argues that the term "should be dropped from the literature". The ensuing debate has now filled the abovementioned volume (Morgan 2015) of reactions to the paper, which points to the fact that the debates on the definition of "mainstream" or neoclassical" economics is not an issue of orthodox versus heterodox interpretations. There is no consensus on what these terms imply even among heterodox economists, but the level of engagement shown in these debates is an indication of the sense of importance of this classificatory issue.

Let us begin with a simple dictionary definition. Under the entry for "neoclassical" in the *New Palgrave Dictionary of Economics*, the historian of economic thought Tony Aspromourgos (2008), a leading scholar on the historical origins of this term, gives us a first start with a historical definition of the term and its adoption.[14] As noted earlier, the term was coined by Thorstein Veblen in

States. See for example the discussion between Barnett (2006) and Backhouse (Backhouse 2006) on the topic.

[14] Aspromorgous's 1986 essay is cited together with Fayazmaneh's (1998) work by both Colander (2000:134) and Lawson (2013:2 n.3) as the two major in-depth studies on the history of the term.

a 1900 essay to characterise Alfred Marshall and Marshallian economics, that is, the main proponent in Britain of the new marginalist approach to economics that replaced the classical political economy from the 1870s onwards.[15] The point of characterising Marshall's school of thought as *neoclassical* was of course to establish a connection between Marshall and the *classical* economists of the earlier nineteenth century. But for Veblen, this connection was not based on *marginalism*, which was decidedly not present in classical economics, but in the "alleged basis of a common utilitarian approach and the common assumptions of a hedonistic psychology" (Aspromourgos 2008).

The term achieved a more general meaning after the Second World War through separate articles by John Hicks and George Stigler in which "neoclassical" came to denote not only Marshall's thinking, but marginalist theory in general. They both saw the unifying core of these theories in two assumptions: first their *methodological individualism*, and second their *marginal productivity theory* of distribution, resting upon a subjective theory of value. The term was widely adopted in the 1950s and 1960s, when it became well known in large part through the so called Cambridge capital controversy[16]. Aspromourgos (2008) writes that MIT economist Paul Samuelson's influential textbook *Economics* (first edition published in 1948) was also influential in popularising the term with its aim to set forth a "grand neoclassical synthesis" from the 1955 edition on.

A second dictionary entry written by the historian of economic thought Roy Weintraub gives a similar view but with a slightly different emphasis. In his entry on "neoclassical economics" in the *Concise Encyclopedia of Economics* Weintraub (2002b), like Aspromourgos, points to the fundamental difference between classical and neoclassical economics. Whereas everyone agreed in the mid-nineteenth century about an objective "substance" theory of value where the value of a commodity is determined by its costs of production, and the share of value could be divided among the factors of production (land, labour, capital) corresponding to the main social classes, this is exactly what the marginalist revolution of the 1870s changed. The perceived problem with the classical Ricardian value theory was that the inherent value so calculated often differed from the actual market price when people was willing to pay more than the "worth" of the commodity. The marginalists (Stanley Jevons, Leon Walras, Alfred Marshall and Carl Menger, though the latter was not a proper marginalist) all

[15] In the overview of the history of economic thought at the beginning of this chapter, I used the term "marginal revolution" and called the associated authors "marginalists", though many textbooks call this the "neoclassical" revolution and school. I avoided using the latter term altogether to prevent confusion.

[16] For an overview of this episode in the history of economics, see Cohen and Harcourt (2003).

shifted their focus to the relationship between the commodity object and the subjective valuation of the buyer. These economists in different ways all started to think about the relationship between the costs of production on the "supply" side, and the subjective valuation on the "demand" side. Weintraub (2002b) straightforwardly claims that "the overarching theory that developed from these ideas came to be called neoclassical economics." He argues that "The framework of neoclassical economics is easily summarized" as a set of axioms:

> Neoclassical economics is what is called a metatheory. That is, it is a set of implicit rules or understandings for constructing satisfactory economic theories. It is a scientific research programme that generates economic theories. Its fundamental assumptions are not open to discussion in that they define the shared understandings of those who call themselves neoclassical economists, or economists without any adjective. Those fundamental assumptions include the following: 1. People have rational preferences among outcomes. 2. Individuals maximize utility and firms maximize profits. 3. People act independently on the basis of full and relevant information. Theories based on, or guided by, these assumptions are neoclassical theories. (Weintraub 2002b)

Weintraub claims that neoclassical economics became the mainstream of economics after the mid-century, existing in parallel with alternative but marginal schools of thought with their own metatheoretical frameworks for constructing economic theories. Here Weintraub lists Marxian, Austrian, post-Keynesian and Institutional economics as the main alternative schools.[17] These schools, he emphasises,

> Are regarded by mainstream neoclassical economists as defenders of lost causes or as kooks, misguided critics, and antiscientific oddballs. The status of non-neoclassical economists in the economics departments in English-speaking universities is similar to that of flat-earthers in geography departments: it is safer to voice such opinions after one has tenure, if at all. (Weintraub 2002b)

The brief explanation for this orthodoxy is "connected to the 'scientifization' and 'mathematization' of economics in the twentieth century" according to Weintraub. When the neoclassical research programme is increasingly associated

[17] As a reference point, Colander et.al. (2014) list the same schools, with the addition of feminist economics. Lawson (2006:484) adds social economics in an article that seeks to elicit the common core of the different heterodox projects.

with "science", any contestation of that paradigm will also be seen as a contestation of science as such.

Problematising and deepening the concept of neoclassical economics

Lawson (2013) recently suggested that there are three main ways in which the term is employed in economics discourse. The two dictionary entries cited above are, in my view, good examples of two of these, and I think his distinction is helpful to better understand the debates about the term. First, Lawson mentions those who use it loosely and without elaboration, mostly as a *pejorative* term. The mere act of labelling something "neoclassical" is for many a sort of shorthand criticism. Lawson gives the example of Paul Krugman labelling Chicago "freshwater" economics as neoclassical, while the post-Keynesian Steve Keen in turn criticises Krugman for being neoclassical. Even more common and also worse, Colander argues (2000:130), is its usage "in the discussions by lay people who object to some portion of modern economic thought. To them bad economics and neoclassical economics are synonymous terms."

After dismissing this first loose use of "neoclassical", Lawson finds two more approaches that make serious and systematic use of the term, chiefly among economic methodologists and historians of thought. The *historical-comparative* approach, is found among historians interested in the ways in which the term denotes simultaneous continuity and difference with an idea of "classical" economics. However, all authors generally seem to conclude that no notion of continuity with something called "classical" economics holds any water. Instead, they claim that late nineteenth century marginalism should rather be labelled *counter-classical* (Maurice Dobb), *non-*, *counter-* or even *anti-classical* (Milan Zafirovski), and Joseph Schumpeter succinctly held that "there is no more sense in calling the Jevons-Menger-Walras theory neoclassic than there would be calling the Einsteinian theory neo-Newtonian" (Lawson 2013:2 n.3). Lawson also concludes that Tony Aspromorgous (cited above) and Sasan Fayamanesh, who have both studied the spread of the term since its origin in Veblen's 1900 essay, fail to find any proper grounds for continuity in Veblen himself.

Lawson himself provides an in-depth reading of Veblen's essays to provide his own, quite surprising, interpretation of what the term should mean, only to argue that even with that interpretation, the term is hardly useful. An important point, according to Lawson, is that when Veblen compared Marshall with the "classics", his conception differs from the term "classical political economy" originally coined by Marx (1970b).[18] Whereas Marx used the term in contrast with the superficial "vulgar economists" following Ricardo, in Veblen's usage the "classics" are Marx's "vulgar economists" (Lawson 2013:15). This usage has been followed by others, notably Keynes, and Colander (2000:131) even argues that Keynes lumped his predecessors together in the category "classicals", that is, pre-Keynesian economics as contrasted with Keynesian economics. It should be evident from this example that even the seemingly basic categories of intellectual history are shifting, and to add to the confusion, these shifts in terminology have not always been properly acknowledged even by historians of thought.

Weintraub's entry in the *Concise Encyclopedia of Economics* is an example of Lawson's third approach, which could be thought of as substantive or ontological. This approach is taken by a number of authors who try to systematise a coherent account of the core analytical features that characterise this school of thought. These accounts all seem to share a certain abstract nature, focusing on an abstract *metatheory*, set of *axioms*, or even *meta-axioms* underlying substantial theories. Lawson argues that most of these accounts seem to agree on some fundamentals: among the common identified neoclassical axioms is *methodological individualism* (individuals as units of analysis). Some form of assumption about *typical behavio*ur (often, but not necessarily, a classical conception of self-interested rationality) is normally also included, and often, but not always, *equilibrium analysis* (Lawson 2013:3). Weintraub's short definition of the metatheory mentioned above, for example, does not say anything at all about assumptions about equilibrium, or even the use of equilibrium analysis. In contrast, Frank Hahn, a self-identified "neoclassical" economist, lists the following essential features of neoclassicism: "(1) an individualistic perspective, a requirement that explanations be couched solely in terms of individuals, (2) an acceptance of some rationality axiom; and (3) a commitment to the study of equilibrium states" (Lawson 2013:3 n.4).[19]

[18] Lawson (2013:13) analyses Veblen's interest and purpose for coining the term in-depth. In his view, Veblen is interested in the metaphysical presumptions of economics and especially two competing ultimate "grounds for finality in science". He thereby wants to contrast the older "taxonomic" approach which basically compares matters of fact with a normal or ideal state, in late nineteenth century economics expressed as equilibrium analysis. In contrast to this, Veblen sees an emerging Darwinian evolutionary and causal approach to economics. Veblen finds that Marshall (as opposed to the earlier marginalists) realises the need for the latter approach, but cannot let go of the "classical" preoccupation with "taxonomy", i.e. equilibrium analysis. Veblen identifies an essential tension in Marshall's work, which justifies the label "neoclassical", and which he uses interchangeably with terms like "quasi-classical" or "modernised" economics (2013:19).

[19] Lawson notes that the "commitment to the study of equilibrium states" does not imply an assumption about equilibrium actually existing in any sense.

Another example of this approach is Christian Arnsperger and Yanis Varoufakis' (2006) account of neoclassical economics as based on three meta-axioms, as an argument against the thesis of a new mainstream pluralism.[20] Davis (2006) as well as Colander et al. (2004) claim that any list of necessary core features of neoclassical economics will soon encounter a modern approach that breaches one or more of the supposedly necessary core criteria, thus rendering any attempt to define the modern mainstream as "neoclassical" futile. In their view, this shows that there is no such thing as a coherent neoclassical research programme, or at least that the contemporary mainstream cannot reasonably be characterised in this way. Against this thesis of a new mainstream pluralism, Arnsperger and Varoufakis argue that the coherence of the research programme should be sought at a higher level of abstraction. Instead of listing necessary conditions, core ideas that must be held true, they claim that behind seemingly different ideas about, for example, rationality (selfishness, altruism, bounded rationality, and so on), we can nevertheless find a minimum common ground of modern mainstream economics. It consists of what they call three meta-axioms.

The first is *methodological individualism*, which the authors claim underlies the neoclassical school since the marginal revolution, but not the classics, nor Keynes or Hayek. The second meta-axiom is *methodological instrumentalism*. This means that "all behaviour is preference-driven or, more precisely, it is to be understood as a means for maximizing preference-satisfaction" (Arnsperger and Varoufakis 2006). The standard view holds that all behaviour is fully determined by a given set of preferences. But even in later developments of evolutionary game theory, where preferences are modelled as developing as dependent on past outcomes, or expectations about others' expectations, the meta-axiom still holds true: "*homo economicus* is still exclusively motivated by a fierce means-ends instrumentalism". In more familiar sociological terms, this amounts to saying that all human action should be understood in terms of Weberian instrumental rationality. The third meta-axiom is *methodological equilibration*, in which the question is posed of what behaviour should be expected in a state of equilibrium. The point is that the questions about whether equilibrium is probable or even possible, and if so, how it can come about, are left behind in favour of the theoretical study of presumed behaviour, even when no demonstration of the actual emergence of equilibrium is provided.

The common feature of all these approaches is that they attempt to define their object in terms of substantial assumptions. Since these assumptions are rather

[20] The approach of Arnsperger and Varoufakis has been adopted by others, for example in the Swedish context by Lars Pålsson Syll (2013).

"meta-theoretical" and axiomatic, or even meta-axiomatic, it is useful to think of them as assumptions and presuppositions of social ontology. As in any science, the assumptions of a scientific ontology consists of regulating ideas about what sort of stuff and relations the object of scientific investigation is assumed to be made up, before any particular theories or methods are applied. These approaches all emphasise the *ontological aspect* of the understanding of the continuity of mainstream economics.

From neoclassical orthodoxy to mainstream pluralism?

An important and rather influential version of the substantive or ontological approach has already been mentioned above, what could be called the thesis of a new mainstream pluralism. It is the claim, put forward mainly by David Colander (2000; Colander et al. 2004) and John B. Davis (2006, 2008), that economics *was* formerly dominated by a neoclassical mainstream, defined as the adherence to a set of axioms, but that the current state of affairs is relatively pluralist. They characterise the situation today as one where a number of different approaches, that are decidedly not neoclassical according to the authors' definition, coexist within the very top ranks of economics departments, journals, and conferences. Using a term that is intentionally paradoxical, John Davis (2008) calls these new research programmes *mainstream heterodox*. This thesis of a new mainstream pluralism in contemporary economics is of course formulated in direct opposition to the idea of a single research programme that has dominated economics for the most part of the twentieth century, whether we use the term "neoclassical" or not to describe it. Therefore, let us examine this thesis a little more closely.

In his 2000 article "The Death of Neoclassical Economics", David Colander, then chairman of the History of Economics Society, set out to perform an "economist-assisted terminasia" on the term "neoclassical economics" on the occasion of its centenary. Colander lists six attributes of what he views as the historical school of neoclassical economics, which he claims bloomed roughly between the 1870s and the 1930s. The culminating works that captured the essence of this school were published in the 1930s and 1940s.[21] However, modern economics has since long moved away from these six fundamental attributes, and does not require any adherence to them (Colander 2000:135). Nevertheless, Colander (2000:136) maintains these ideas are still around and in use, but wants to make the central point that they are not *constraining attributes*. The issue at

[21] Colander argues they are John Hick's *Value and Capital* (1939) and Paul Samuelson's *Foundations of Economic Analysis* (1947).

stake is to prove that the "when it comes to content, modern economics is open to new ideas" (2000:137). Without making too much fuss about it, Colander instead emphasises that it is not axioms but *method* that characterises modern mainstream economics. This claim is more important than the authors perhaps realise, and we will return to it.

In their 2004 article, Colander and co-authors (2004) develop the argument further. They now claim that "economics is moving away from a strict adherence to the holy trinity—rationality, selfishness, and equilibrium—to a more eclectic position of purposeful behaviour, enlightened self-interest and sustainability" (2004:485). To emphasise the "increasing variance of acceptable views" (Colander et al. 2004:487), they introduce the concept of the *edge of economics*, where the elite of the profession gradually come to accept and adopt new ideas, so that the process of change is viewed as cumulative evolutionary changes, rather than in Kuhn's "funeral by funeral" view of radical paradigm shifts.[22] "The very concept of an edge of the profession is designed to suggest a profession in which there are multiple views held within the profession, and goes against the standard classifications of economics. Those standard classifications convey a sense of the profession as a single set of ideas" (Colander et al. 2004:486).

John B. Davis (2006, 2008) takes a similar but not identical approach. His point of departure is what he sees as the generally agreed historical fact of at least five previous periods in the history of economics that could unambiguously be called pluralist. These are the periods of the shift from classical to neoclassical economics in late nineteenth century Britain, the German *Methodenstreit* between the historical school and early Austrians, the situation in post-Marshall Cambridge labour and monetary economics, the period of US interwar pluralism with its strong American institutionalism, and finally the monetary/ fiscal debates of the 1970s. Davis argues that there has been an oscillation between times of pluralism and those dominated by different orthodoxies. Today, he claims, a lot of new research programmes are bringing insights from other sciences outside economics (like psychology and evolutionary biology) with the aim of changing some of what used to be core assumptions of the old orthodoxy. In contrast to the self-identified research programmes of "traditional heterodoxy", he calls the new ones "mainstream heterodox", because they have achieved a certain following by groups of economists who, by a sociological definition, clearly belong to the mainstream.

It should be noted that, in Davis' view, the difference between the two forms of "heterodoxy" is still very marked. For example, he also acknowledges the strong

[22] This is Paul Samuelson's re-interpretation of Max Planck's saying, made famous by Kuhn.

reliance on formal modelling and positivism in the mainstream, and its fundamental rejection by the traditional heterodoxy, while this seems not to be a fundamental issue for the new mainstream heterodoxies (Davis 2008:359). In sum, Davis explicitly acknowledges the mainstream heterodoxy divide as it is normally understood, but tries to refine our understanding of developments within the contemporary mainstream.

It seems, then, that although the Colander-Davis thesis of a new mainstream pluralism can tell us something interesting about developments in mainstream economics of the last few decades, their problematic differs from those authors who have a wider and more fundamental concept of the mainstream, like Lawson, and Arnsberger and Varoufakis. In fact, the difference in classification can be seen as a difference in the level of abstraction. Viewed thus, the latter group of authors are looking for a more fundamental type of glue (searching at "meta" levels), and they do find an enduring mainstream-heterodoxy division when the conceptual apparatus is tuned in this way. There is furthermore good reason to view the two approaches as compatible, as a matter of perspective, or level of abstraction. Looking more closely at the recent history of economic thought, things are happening that may warrant the description of a new mainstream pluralism. At the same time, if one looks for fundamental ontological assumptions (rather than more specific theoretical formulations), there seems to be striking historical continuity. But this discussion has so far covered accounts that centre on what I call the ontological aspect of the intellectual divide. Let us now turn to other aspects of this picture.

3. The ontological, epistemological and social aspects of the intellectual divide

The previous section surveyed some of the major positions that discuss intellectual divides in economics in substantive or ontological terms. However, other authors place emphasis on other aspects of the divide. I suggest that we can think of three relevant aspects of the divided structure of an intellectual field like economics. These are the ontological, epistemological, and social aspects. I will first briefly summarise the first, ontological aspect already touched on in the previous section. I will then turn to the role of methodological conceptions and practices emphasised by Lawson, or what I suggest we think of as an epistemological aspect of the intellectual divide. This is the notion that mainstream economics is fundamentally about a particular orientation towards certain methods and views on how proper knowledge should be produced. Third, I turn to more sociologically familiar ground with the social aspect, emphasising the importance of understanding the relational nature of intellectual conflict and the practice of boundary work involved in establishing professional and scientific legitimacy and authority.

The first aspect: Ontological assumptions and presuppositions

The first aspect relates to differences in core beliefs or axioms of social *ontology*, that is, fundamental ideas about how the aspect of the world one studies is made up. The core axioms of any science constitute its objects of study and direct attention to certain aspects of the world while excluding other aspects. It is useful to think of different authors drawing lines at slightly different levels of abstraction. Where to draw the line depends on what we wish to highlight. Are we interested in theoretical differences between schools of thought, for example in how they conceive of rationality? Or are we interested in similarities at the more fundamental level of social ontology, that is, about the basic presumptions about the nature of actors, markets, and so on? Here, I think it is useful to take the Arnsperger-Varoufakis approach, of thinking in terms of meta-axioms. This illuminates what many heterodox critics find to be a common ground, what they take to be their common problem with the neoclassical mainstream.

We may think about these distinctions as layered in a hierarchical way, in the sense that a few basic meta-axioms may be compatible with broader substantial theories. With such a model, we may then think about the formation of consensus and disagreement at different levels of abstraction. That is, there may be disagreement on the level of substantial theories, although there is still consensus about the underlying fundamentals. It is then a matter of shared beliefs about ontology rather than theory. These axiomatic ontological beliefs, that is, beliefs about fundamental aspects of reality that are taken for granted and never questioned as such, may then also be conceived at different levels of abstraction. We can think about them in terms of preceptions (as Veblen did), that is, the lower level meta-axioms that are not necessarily even explicit or explicable. This just means that holding some belief *presupposes* another at a higher level of abstraction. This is exactly the point that Arnsperger and Varoufakis and Lawson make, although from slightly different angles. Whereas the former authors are interested in meta-axioms as theoretical presuppositions, Lawson shifts his focus onto the ontological presuppositions of certain methods.

The second aspect: Epistemology and methodological ideals

The second aspect of the intellectual divide is *epistemological.* This is exemplified by the approach taken by Lawson, and as we have seen, also mentioned by other authors: the understanding of mainstream economics as a research programme unified on methodological rather than axiomatic ontological grounds. Lawson (2013) turns against not only the use of neoclassical to describe the core of mainstream economics, but also any attempt at defining mainstream economics in terms of some set of substantial theoretical assumptions or axioms (what I call ontology here).

He argues that this common approach is not useful from the point of view of a critical heterodoxy, and that it only leads to a superficial critique that does not get to the heart of the problem. Instead, he points to the centrality of the shared methodology of mainstream economics, and as a consequence of that, what he argues are the implicit ontological assumptions that comes as part and parcel of certain methodologies. In order to understand the problems of the mainstream, it is necessary to inquire into the more basic problem of the relation between methodology and ontological assumptions.

Weintraub, while discussing core theoretical assumptions, also emphasises the role of mathematics in modern economics. Weintraub (2002a) has studied the mathematisation of economics at depth, and points to the increasing role of formal mathematical modelling in modern economics. Colander and his co-authors (2000; Colander et al. 2004), while discussing the changing substantial features of the mainstream, also note the centrality of methodology, if only in passing. Colander finds that the modelling approach, rather than any substantial content, is the central attribute of modern economics. Following Robert Solow and Jürg Niehans, he argues that "*the modelling approach to problems is the central element of modern economics*" (Colander 2000:137; emphasis in original), and concludes with Niehans that our time should be characterised as "the era of modelling" (2000:141). The mention, in passing, of a core of methodological principles remains the same in the 2004 paper: "Our view is that the current elite are relatively open minded when it comes to new ideas, but quite close minded when it comes to alternative methodologies"(Colander et al. 2004:493). Even if the authors are keen to emphasise the openmindedness of those elite actors in the profession in their argument for a mainstream pluralism, they admit that there is "unconscious suppression" of heterodox views that does not fit nicely with the elite's way of thinking.[23] In such suppression, methodology is an important tool

[23] The authors centre their account on the elusive concept of "elite of the profession" as the central actor in the process of intellectual change, a notion they claim is "understood by those in the

(Colander et al. 2004:493). However complexity-oriented and openminded the elite may be when it comes to theory, there seems to still be one right approach when it comes to methodology: "If it isn't modelled, it isn't economics, no matter how insightful" (Colander et al. 2004:492).

Lawson comments on this observation, but is not wholly satisfied by the limited role Colander and his co-authors assign to it: "I am not sure they fully appreciate the significance of their observation (they give it little emphasis)". Instead, he wants to put full pressure on exactly this point. It is this methodological orientation which is the defining feature of contemporary mainstream economics:

> The mainstream project of modern economics just is an insistence, as a discipline-wide principle, that economic phenomena be investigated using only certain mathematical-deductive form of reasoning. This is the mainstream conception of proper economics. It is the one feature or presupposition that remains common to (if not always explicitly formulated in) all contributions regarded as mainstream, remaining in place throughout all the project's theoretical fads and fashions. (Lawson 2006:492)

This distancing from the Colander-Davis view of mainstream pluralism is repeated with full force in Lawson's 2013 piece, where he holds that:

> whilst the concrete substantive content, focus and policy orientations of [the mainstream tradition] are highly heterogeneous and continually changing, the project itself is adequately characterised in terms of its enduring reliance, indeed, unceasing insistence, upon methods of *mathematical modelling*. (Lawson 2013:4, emphasis in original)

Lawson's point is clear. To understand modern mainstream economics, we need to focus on its insistence on mathematical modelling. The problem of mainstream economics, according to Lawson (2013:7), may have many manifestations at the level of substantial theoretical claims and policy advice, but its real source lies at "the level of methodology and social ontology" Lawson's central argument is that the mathematical methods used bring with them certain ontological presuppositions that do not match the nature of social reality. This is where Lawson's critical realist philosophy of science comes into play. He argues that the use of mathematical techniques like functions and calculus presuppose event regularities in closed systems. Lawson calls this methodological approach

profession". There should be no doubt that the idea and actual importance of a disciplinary elite is very powerful in this profession (cf Fourcade, Ollion, and Algan 2015).

mathematical deductivism, the doctrine that explanations should be framed in terms of such closed systems and event regularities. A social ontology or model world that guarantees such event regularities must, according to Lawson, be a world of isolated "atoms" that, when triggered, have the same effect or action regardless of context. Note that I have talked about an ontological aspect here in much the same way as Lawson talks of "substantial axioms". However, I think it is fruitful to think of the Arnsperger-Varoufakis formulation of three meta-axioms as a social ontology, that is, a fundamental assumption about the basic stuff of which the social world is made.[24]

There are also, Lawson (2013:8) notes, exceptions to such unrealistic assumptions, mainly in the form of modern, overly atheoretical econometrics. But this, he claims, just shows that the common ground remains the use of mathematical models. Lawson's critique hinges on a critical realist argument about the discrepancy between the modellers' presupposed atomistic deductivist ontology, and the nature of social reality. The problem is not the mathematical modelling per se, but rather that this methodology is not fit for purpose, since it cannot model the fundamental ontological openness of the social world:

> The heavy use of these tools in conditions for which they are found to be inappropriate both explains the repeated explanatory failings of the discipline as well as why formulations are of a nature that are typically recognised by almost everyone as rather unrealistic. That, in summary, is the real cause of the discipline's problems. (Lawson 2013:7)

These, then, are the outlines of the *epistemological approach* to the mainstream-heterodoxy divide in economics, which are an important concern for Colander and Weintraub, and central for Lawson. While Colander focuses loosely on "the modelling approach", Weintraub has studied the increasing use of mathematics in economics. The mathematisation and modelling approach are also interrelated in important ways, but should not be seen as strictly identical. For example, Lawson (2013:27) points to the development of mathematics in the wake of the emergence of quantum physics in the early twentieth century as an important factor in the development of modern mathematical modelling. Whereas economists had always used some form of mathematics, previously this was

[24] Lawson explicitly argues against the "substantial" approach of not only Weintraub's idea of a "meta-theory", but also Arnsperger and Varoufakis' idea of three meta-axioms, in favour of his own methodological/ ontological approach. However, the resulting view of ontological presuppositions is perhaps not so distant from the meta-axiom view, that is, Lawson arrives at a very similar conclusion if we look at the actual ontological presuppositions or meta-axioms.

strongly influenced by a mechanistic reductive worldview borrowed from physics, where essential properties of nature were expressed in mathematical form. With quantum physics, however, mathematics was increasingly seen as a tool for examining *possible realities* represented as axiomatic systems in their own right. The need to interpret and apply the models in terms of real world events was thus effectively minimised: "In particular it was no longer regarded as necessary, or even relevant, to economic model construction to consider the nature of social reality, at least for the time being" (Lawson 2013:28; see also Backhouse 2002:259). If the ontological aspects focus on underlying assumptions about the nature of social reality, the epistemological approach focuses on assumptions, prescriptions and practices of social knowledge production. With that, let us turn to the social and symbolic organisation of actors in economic knowledge production.

The third aspect: The social nature of thought collectives in science

The social nature of all knowledge, science included, is one of the oldest insights not only of science studies, but of modern sociology in general. The *social aspect* of understanding the intellectual field of economics addresses the social nature of knowledge production. A good entry point to this aspect is historian of economic thought Roger Backhouse's (2004) "A Suggestion for Clarifying the Study of Dissent in Economics".

Backhouse wants to provide a better understanding of the role of heterodoxy in economics. To do this, he draws upon the sociology of science to emphasise the role that dissent and controversies play in studies of the natural sciences, "because they reveal things about science that would otherwise remain either concealed or obscured" (Backhouse 2004:262). Furthermore, he reminds us of the fundamental insight from science studies that "the resolution of controversy is a social process that determines the range of acceptable positions within the profession" (Backhouse 2004:264). We cannot just assume that scientific controversies are terminated through the establishment of truth by the objective arbiter of empirical data, necessarily moving us towards the singular truth of the matter. Instead, "interests, preconceptions, and ideology interact with the various theoretical and empirical techniques available to economists to produce an outcome that, in the circumstances, seems correct to those involved" (Backhouse 2004:264). Controversies are social affairs, the resolution of which establish what comes to be taken as truth; in this sociology of scientific knowledge view, truth is the outcome of social controversies, not an external input that determines their outcome.

Backhouse makes the important clarification, also drawing on science studies, that classifications should be naturalistic and workable. That is, they do not tell us which one of warring parties is wrong or right, but should be arrived at sociologically. Furthermore, definitions are not claimed to be absolute or exact, their "purpose is not to serve as the basis for formal logical deductions" (Backhouse 2004:263); an important point to remember in the heated debate about the exact definition of neoclassical economics, and whether that entity is still hegemonic. Central to Backhouse's argument about types of dissent is a distinction between, first, everyday *disagreement* on some topic which, second, may crystallise into more or less prolonged scientific *controversy*. A controversy may be resolved with one of the warring sides leaving as the victor, establishing the truth of the matter, or, third, a controversy may remain unresolved and turn into *dissent*. Dissent differs from the two first situations in that it implies asymmetric relations between insiders and outsiders/ dissenters.

Backhouse now helps us to make a distinction between dissent in general, and groups of dissenters that are in some way *organised* around dissenting views: *heterodoxy*. He then suggests that "heterodoxy be defined as involving self-identification, sociology, and core beliefs. [. . .] It is heterodoxy as understood by, for example, the Association for Heterodox Economics" (Backhouse 2000, see also 2004). With such a strict definition, we can see how dissenting core beliefs, the focus of what we have called the ontological approach, are not sufficient conditions for classifying a position as heterodox, according to Backhouse. We must also take social relations (that is, exclusion from mainstream sites of power, but also organisation in alternative networks, journals and conferences), and *self-identification* into account. The self-identification aspect is central to authors like Lee (2009), and also acknowledged by Lawson (2006) as a fundamental aspect of modern heterodoxy. Lee especially emphasises that understanding heterodoxy means understanding heterodox economics in terms of a specific social community. Combining these with Davis's (2008) position results in a synthesis that allows us to think more clearly about different forms of heterodoxy.

Backhouse also mentions a further aspect of dissent, where the dissenting party leaves the field altogether for another neighbouring discipline. Instead of staying within the discipline and struggling to change it, this means pursuing one's goals elsewhere, for example in business schools or economic history departments. To this, one could add the creation of new academic disciplines, such as the creation of economic history as a discipline in its own right, strongly influenced by what had earlier been the German historical school in economics. In Backhouse's terminology, Davis' idea of the new mainstream heterodoxy (clearly distinct from traditional heterodoxy) could perhaps be understood as a dissenting mainstream view, but not heterodox in the proper sense. It should also be noted that both authors point to the differences in how one relates to orthodox core beliefs. Whereas critics of the traditional heterodox variety tend to be ruthless in their attacks on these beliefs, with the ultimate aim of having them replaced (at least partially, under the banner of pluralism), the mainstream heterodox seeks rather to reform *some* of orthodoxy's core beliefs. Davis's (2008) explanation of the existence of a marked orthodoxy-heterodoxy split, and the active use of this terminology in economics, deserves attention. He claims that periods of orthodoxy produce heterodoxy through a splitting process. Drawing on the sociological literature on symbolic boundaries between scientific fields and boundary work (Abbot, Bourdieu, Collins, Gieryn), Davis claims that we must understand the process of how sciences try to achieve greater autonomy as well as scientific and professional legitimacy. In any such process, the production of legitimacy is connected to the conventional approaches in each science. That is, the use of a common, generally acknowledged fund of knowledge works as a professional resource to increase the legitimacy of that common project. A science structured around such dominant conventional approaches enables the formation of the profession as a coherent social group (Davis 2008:351).

But why is this tendency so marked in economics? Davis claims that the explanation should be sought in the high level of policy exposure, where the high stakes in the form of the "wide scope and profound impact of the market in modern society" in combination with the high uncertainty of prognosis in economics (Davis was writing before 2008) leads to a situation where the orthodoxy/ heterodoxy split functions as a sort of defence mechanism that "allows economics to claim economics is scientific by dismissing heterodoxy as unscientific" (Davis 2008:352). He also points to the fact that the value-laden nature of economics (like any science) is obvious in a pluralist situation where different schools may debate substantial issues, while a situation with a dominant orthodoxy may suppress such impressions.

The focus of studies on boundary work in science (Gieryn 1983; Lamont and Molnár 2002) is on how social groups are created in interaction with the creation and reproduction of symbolic boundaries. The central argument in the literature is that the boundaries of science have to be maintained in practice through the continuous symbolic work of demarcating science from non-science. Symbolic boundaries are related to social boundaries, when the boundaries of a social group like a profession are maintained and determined through symbolic boundaries that defines belonging and status. As Davis (2008) argues, boundary work in economics functions through the equation of the mainstream approach with science, and heterodoxy with non-science. Another way of saying this is that mainstream economists "command of the language of science and objectivity" (Morgan 2015), and the claims of heterodox economists can be refuted as unscientific and thereby not worth taking seriously. In this way, symbolic boundaries are turned into social boundaries. These symbolic boundaries are also an integral part of the construction of collective identities, both the identity as just "economist" without any adjective, and the identity as "heterodox economist", constructed in explicit opposition to mainstream identity.

The social aspect of understanding the mainstream-heterodoxy divide thus involves the notion of distinct thought collectives or social groups that hold different beliefs that stand in specific relations to each other, are involved in controversies where they employ various resources to promote their views, and whose members identify as members of the group. However, there is also a deeper sense in which these social relations and their dynamics are understood as social, namely as actively-maintained boundaries that link social groups and symbolic categories.

4. Conclusions

This chapter has provided background and substance to the overarching research problem of this thesis. The first section looked at different types of critique of mainstream economics, and argued that although there has been a surge in critical voices after the 2008 financial crisis, the phenomenon is not new or related to the economic crisis. The popular critique and the establishment of student organisations working for pluralism both draw on the critical voices of heterodox economists. Furthermore, I argued that heterodox traditions has existed for a very long time in parallel with a dominant mainstream approach. I then illustrated the lack of consensus on how to define terms like neoclassical or heterodox economics. The question of the historical stability and endurance of the mainstream-heterodoxy divide was found to be a matter of level of abstraction. If one looks not at the level of substantial theories, but rather below, at the level of ontological assumptions, there seems to be evidence of such a divide at least since the Second World War. This led to the final synthesising section and the analytical distinction between three aspects of the enduring mainstream-heterodoxy divide. There, I argued that literature converges on an ontological, epistemological, and a social aspect as central features of a proper understanding of the persistent intellectual divide. Drawing on this preliminary and selective review, the next chapter will follow with a more thorough and broader discussion of previous research in areas that were indicated as relevant here.

Chapter 3. Social studies of science and economics: Previous research

Our understanding of science, what it is and how it actually works, has expanded enormously during the last century, and especially the last half-century following the publication of Thomas Kuhn's *The Structure of Scientific Revolutions* in 1962 (1996 [1962]). Violently reducing a complex and multifaceted intellectual century into one sentence, we have witnessed an overarching shift away from the prescriptive philosophising characteristic of the received view of positivism, towards a variety of historicising, sociologising and more naturalistic approaches, generally known as science studies, or science and technology studies (STS) today. Besides the emerging specialised field of the history of science in the early twentieth century, and the internal philosophical critique of positivism, sociology was probably the single most important field where this development took place. Robert Merton's early pioneering work established the sociology of science firmly as a research field, and with the critique of the Mertonian school and the turn to the strong programme's *sociology of scientific knowledge* by the mid 1970s, the discipline kept up with the times and was at the forefront of research in the social study of science. Since then, science studies has grown manifold, broken out of sociology, transcended disciplinary boundaries, and established itself as a research field with its own professional associations, journals and doctoral programmes.[25] The social study of science is then no longer (if it ever was) a branch of sociology. On the contrary: some branches of contemporary sociology are now part of science studies.

[25] For a couple of introductory textbooks (among many) to the STS field, see Bucci (2004) and Sismondo (2010). The Society for Social Studies of Science (4S) publishes the journal *Science, Technology and Human Values*, while the leading journal in the field is considered to be *Social Studies of Science*. 4S furthermore hosts annual international conferences and otherwise functions as the unifying professional association. It publishes, among other things, *The Handbook of Science and Technology Studies* which represents a sample of the state of the art within the field, and is now in its fourth edition (Felt 2017).

This chapter serves two main purposes. The first is as an orientation in social studies of science in order to position this study roughly in that field, and the second is a review of previous research in relevant areas. The first section of the chapter will briefly introduce general theoretical developments in social studies of science, to position the approach used here from a bird's eye view. The second section will devote considerable attention to the development of theoretical ideas about styles of reasoning in science. The third section will then turn from a theoretical review to a selective review of previous research on the empirical topic of *economics* as a social phenomenon. The following section is devoted to Mary S. Morgan's work, which applies the styles approach to the study of economics. The final section reviews previous studies of the evaluation of scientific quality that are simultaneously theoretically and empirically relevant to the present study.

1. The development and variety of social studies of science

The development of science studies is a complex history that contains many quite different strands of research, debates, and theoretical approaches. This section will attempt to sketch out a rudimentary outline of the development and varieties of science studies, so as to better position the theoretical approach employed in this study.

From Mannheim to Merton: The birth of the sociologies of knowledge and science

There are many roots and founding figures to consider in the development of modern science studies. Robert Merton is often called the founder of the sociology of *science*. In 1945 Merton (1973a) himself claimed that such a field already existed, although it had fallen into an unproductive period of neglect since the 1930s, partly due to the Second World War. The sociology of science has twin roots, Merton argued. One is to be found in the French sociology of Durkheim, and especially his argument in *Elementary Forms of Religious Life* that the structures of thinking should be sought in social structures. The other root is the German *Wissenssoziologie* (sociology of knowledge) of Karl Mannheim and his contemporaries. As Mary Douglas (1987:11) has argued, there are key differences between these two strands and their origins. Whereas the French tradition originated in an anthropological problematic dealing with how variations in culture and cognitive orientation relate to variations in social order, the German tradition emerged from a Hegelian-Marxist political problematic acutely present in the political confrontations of the Weimar republic. This German approach took as its point of departure the relation of the individual to the social order, that is, the problem of social interest and the relation of knowledge to varying social standpoints, and the problem of relativism related to it.

If Merton founded the first research programme specifically in the sociology of *science*, Mannheim could rightly be considered the founder of the more general field of the sociology of *knowledge*. In *Ideology and Utopia* he traces the historical roots of the concept of ideology (Mannheim 1936). Mannheim argues that there is a long history behind the various forms of the concept of ideology, meaning essentially that one can impute errors in an opponent's thinking connected to his or her social or "existential" context. That is, instead of listening to what is actually said, the analysis looks at the social conditions of the subject and his statement. However, according to Mannheim a simple *particular* understanding of the concept of ideology, working with distortions on the level of the individual, has historically been supplemented by a more complex *total* conception with an interest in "the total structure of the mind of this epoch or of this group" (Mannheim 1936:56). With Hegel and the German historicism, the abstract subject of knowledge posited by Kant and the enlightenment became a collective and historically evolving subject, for instance in the form of the nation (Mannheim 1936:68). With Marx, the knowing subject becomes the social class, rather than the nation, and here we find a combination with the particular idea that the whole structure of thinking of a group (the total conception) can be distorted by its social determination.

Mannheim relies heavily on the Marxism of his youth friend Georg Lukács's *History and Class Consciousness*. But through a generalising argument, he transforms the Marxist concept of ideology into the sociology of knowledge. The Marxists, Mannheim claims, use the theory to invalidate their opponents' views as mere false consciousness stemming from a specific social position ("bourgeois thought" for example). However, Mannheim's contemporary political climate, the Weimar Republic, made it all too obvious that there is a range of different social and political groups, each with their own opposing truth claims, and this weapon has been found useful also by other groups, since "Nothing was to prevent the opponents of Marxism from availing themselves of the weapon and applying it to Marxism itself" (Mannheim 1936:75). Mannheim takes this argument to its logical conclusion and generalises the total conception of ideology, with the implication that the outlook of any group at any point in history is to be understood in terms of a collective system of thought with links to their social existence. And thus, "with the emergence of the general formulation of the total conception of ideology, the simple theory of ideology develops into the sociology of knowledge. What was once the armament of a party is transformed into a method of research in social and intellectual history generally" (Mannheim 1936:77–78).[26]

However, Mannheim is a good adherent of the German neo-Kantian doctrine of the strict duality between *Geistes- und Naturwissenschaften* that has been so influential, not least through Weber's mediation. While talking about the general existential determination of world views or total systems of thought, he makes an abrupt halt in front of what he calls the "exact sciences", which are understood as producing knowledge of a sort where sociology have nothing to say and existential determination doesn't reach. Mannheim's contemporary, the Polish medical doctor and lay historian of medicine Ludwik Fleck, builds further on Mannheim and is even stronger in his explicit anti-positivist stance. For, like the German hermeneutic movement, the sociology of knowledge developed to a considerable extent as a reaction to the strong positivist winds blowing at the time, and its conception of science. Although sociologists of science, like their colleagues in history and philosophy, have always had the so-called "hard" sciences as their preferred object of study, an important implication of science studies since Fleck has also been to tear down the false dichotomy between the social sciences and humanities on the one hand, and the natural sciences on the other hand.[27] The work of Mannheim thus lays the foundation for studying knowledge, and with Fleck's addition, any field of scientific knowledge, as a system of belief that is held by some social group and stands in some form of relation to the position of this group and its sociocultural context.

[26] Mannheim struggles to sort out the epistemological consequences of this move. He rejects what he sees as an old epistemology of absolute truths, but also the apparent resulting alternative, relativism. In order to step around the problem of relativism, he develops his position of *relationism*. In a generous interpretation, what he tries to achieve is the position that social determination of thinking doesn't undermine the truth value of claims. According to Merton, Mannheim, at least later in his life, resorted to basically the same position as Weber and others, that "values" only affect the choice and formulation of problems, not the valid solution of problems (Mannheim 1936; Merton 1968). In any case, the problem resurfaces in various forms in later science studies, and Weber's position might not hold water if we accept a Kuhnian or a styles of reasoning approach, where not only the formulations of problems, but the reasoning itself that is employed in their solution, is understood as socially variable.

[27] If Dilthey's hermeneutics and the dualism of natural and human sciences was indeed a reaction to positivism, as Zammito (2004:8) claims, it was also a great loss, since it accepted a false conception of the natural sciences, with detrimental consequences not only for the sociology of science, but also for the understanding of the task of the social sciences as something radically *other* than natural science, as argued by Roy Bhaskar (1998) among others.

If Mannheim established a research programme in the sociology of knowledge, the more specialised field of the *sociology of science* was established during the post-war years, largely synonymous with Robert Merton's functionalist research programme, especially in the ascending US sociology (Merton 1973e). In contrast to the anti-positivist stance of Mannheim and Fleck, Merton stood much closer to the received view of science. While Mannheim was interested in how a style of thinking, its form and content, stood in relation to its existential determination, Merton shifted the focus towards the institutional framework that allowed science to progress. Merton's central problem was the observation that only in some types of societies does science flourish. This was acutely apparent in relation to the totalitarian development of the 1930s, but was also grounded in his attempts to explain the scientific revolution in terms of a Protestant ethos, a transposing of Weber's Protestant ethic thesis known as "the Merton thesis" (Merton 1973d; Storer 1973).

For Merton, it was the social organisation of science as a functional system that, once in place and functioning, would allow for the autonomous growth of certified knowledge in the form of empirically verified propositions.[28] The *content* of the scientific knowledge produced remains outside of Merton's problematic. The problem for Merton's sociology of the normative institutional framework of science is then the social and institutional conditions for the production of scientific knowledge, while scientific knowledge itself remains beyond the reach of sociological explanation, a view fully compatible with the received view of science. However, Merton (1973a) also points to a certain vagueness in Mannheim's *Wissenssoziologie* and argues that this sociological approach fails to specify precisely how social or existential factors actually determine or influence thought, thus requesting that the sociology of science should be more precise in its causal claims.

The Vienna Circle, Quine and Kuhn: The turn to post-positivism

The so-called received view of science had its most radical formulation in the logical positivism of the Vienna Circle in the 1930s. The term "positivism" can be used to loosely describe both this position and a family of similar approaches, but it should always be kept in mind that there are few if any self-identified adherents of such a view after the Vienna Circle, even during the various so-called

[28] Merton's functionalism also included the notion of unexpected outcomes and dysfunctions, as when the contest for scientific priority may lead to fraud (1973b), or when the increasing specialization and complexity that comes with the progress of science may lead to decreased public understanding and trust in science (1973c).

"positivism disputes" around the 1960s (Adorno 1976; Heidegren 2016), and that "positivism" is almost always used as a pejorative and unspecific term, thereby easily losing analytical edge. A useful way to approach the concept is in terms of an ideal type against which actual instances may be compared.

Central features of ideal positivism include, first, its anti-metaphysical empiricism, the principle that science should only ever rely on sense data and at all cost avoid metaphysical speculation.[29] Second, its scientism: a belief in the inevitable progression and triumph of reason, and the related idea that the most advanced natural sciences are to be seen as role models. This is closely related to the idea of the unity of science, the notion that science can be characterised as *one* fundamentally homogenous approach across various fields. Third, the view, also connected to its empiricism, that science strives to explain nature in terms of general covering-laws, or, in Roy Bhaskar's (1975a) terminology, as "constant conjunctions of events". Fourth, its reliance on formal logic and the view of theories as parts of logical-deductive systems of sentences from which observation hypotheses can be derived, what Zammito (2004, 10) calls a "sentential view of theories". This furthermore presupposes, fifth, the strict separation of theory and observation, the notion that a logical system of theoretical sentences can be authoritatively controlled against systematic observations of nature, which functions as the great arbiter between rival theories.

Positivism in its classical formulation didn't last long before it was subjected to devastating internal philosophical critique and development. One of the most important influences in the development of post-positivism both within and outside the philosophy of science was Willard van Orman Quine's *Two Dogmas of Empiricism* (Quine 1951). Quine picks up an objection from the French physicist and positivist Pierre Duhem, who claimed that experiments could not arbitrate between competing theories as was often thought at the time, with the ideal of a "crucial experiment" as arbiter between two competing hypotheses. If the prediction derived from a theoretical sentence does not match the empirical phenomenon, "the only thing the experiment teaches us is that among the propositions used to predict the phenomenon and to establish whether it would be produced, there is at least one error; but where the error lies is just what it does not tell us" (Duhem quoted in Zammito 2004:18–19).

This idea is generalised and radicalised in Quine's so-called semantic ascent to the philosophy of language. He argues, as a general proposition, that "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body", which leads to the result that "any

[29] This list of features is loosely based primarily on Zammito (2004) and Heidegren (2016).

statements can be held true, *come what may*, if we make drastic enough adjustments elsewhere in the system" (Quine cited in Zammito 2004:19; 21–22; my emphasis). This idea became generalised into the universal philosophical principle that theories are in principle always *underdetermined* by evidence: there could, hypothetically, always be another theoretical system logically compatible with the same observation data. This has become known as the Duhem-Quine or the underdetermination thesis, and is one of the most influential underlying ideas, or dogmas, of later post-positivist science studies.[30] It can be generally interpreted to say that the choice of theory is arbitrary and in no way strictly determined by empirical evidence, a sort of "cognitive egalitarianism". However, others have also claimed, quite reasonably, that theory choice in science is not determined by philosophical possibility but by practically applied rationality, and may as such even be overdetermined (Zammito 2004:30).

Apart from Quine, Kuhn's *Structure of Scientific Revolutions* (1996), originally published in 1962, is probably the most influential work in the field, and laid the ground for the new sociology of scientific knowledge in the 1970s. It has influenced how we think about science more than any other single work. Kuhn is also a major influence on what I will call the styles approach, although with important differences. Among Kuhn's central ideas in his turn away from positivism was the notion, borrowed from the philosopher Norwood Russel Hanson, of *the theory-ladenness of observation*, which tore apart the neat positivist separation of theory and observation. Using examples from experiments in gestalt psychology, Kuhn shows that our conceptual framework is not only activated in interpretation of observations, but that we quite literally *see* objects already structured by our conceptual presuppositions. Kuhn was also influenced by Ludwik Fleck's notion of the fundamentally social nature of all knowledge, expressed in the theory of styles of thinking that Fleck took from Mannheim, and his original concept of thought collectives. Another important influence was the later philosophy of Ludwig Wittgenstein, not only for the idea of gestalt perception, but also for his analysis of rule-following. Wittgenstein's idea here is that rule-following is basically a learned and open-ended social practice, rather than the strict logical application of a set of formal rules (Sismondo 2010:30). Importantly, future action is not predetermined by the rules. The meaning of the rule is socially learned and its constant application is similarly an outcome of

[30] One of Zammito's (2004) central arguments in his close reading of the development of post-positivist science studies is exactly that the underdetermination thesis has been part of a new dogma of anti-empiricist post-positivism. He goes far in disentangling Duhem's thesis from Quine's, and shows how they have often become confused with the underdetermination thesis by later interpreters (or as in my presentation above, for the sake of simplicity).

continuous social pressure. It can thus be read as a theory of social learning by example, social practice, and it is also one of the meanings of Kuhn's paradigm concept, the notion of the paradigm as *exemplar* and the notion of science as a practice learnt through practice.

Among his best-known, but also most-contested claims are the ideas that scientific paradigm change is abrupt and revolutionary, and that paradigms are incommensurable. These concepts rely on the role of the paradigm in normal science. The core idea is that through their socialisation, scientists learn to see, discern, and manipulate a theoretically constituted object in a specific way. This learning of a practice through practice necessarily takes place in a controlled and bounded social setting of the particular scientific community in periods of normal science, guided by a common paradigm. Through effective socialisation by means of common textbooks, ideal examples, shared assumptions about ontological (what types of objects exists as valid and meaningful objects of knowledge) and epistemological aspects (what sort of epistemic values are held when it comes to evaluating evidence or theories), normal science becomes an effective apparatus for scientific puzzle solving, solving small and piecemeal problems one at a time, adding them to the body of solved problems.

The paradigm concept has not only been diluted by popularisation. In his substantial 1969 postscript to the second edition of *Structure*, Kuhn (1996) comments that one reader had identified 22 different usages of the concept, and sets out to clarify what he really means. The concept has two basic meanings. The first meaning of "paradigm" is a constellation commitments of a specific social group, that Kuhn (1996:181–82) calls a "disciplinary matrix", "because it refers to the common possession of the practitioners of a particular discipline". This disciplinary matrix consists of four parts. First are established and taken for granted *symbolic generalisations*. These are normally formalised in the form of equations, which represent laws of nature (like Ohm's law on the relationship between electrical current (I), voltage (V) and resistance (R): $I = V/R$), but often they simultaneously also function as definitions of the included symbols. The second component of the disciplinary matrix is shared commitments to beliefs about ontological assumptions, but also the weaker form of commitments to merely heuristic models: "the electric circuit *may be regarded* as a steady-state hydrodynamic system; the molecules of a gas *behave like* tiny elastic billiard balls in random motion" (Kuhn 1996:184; my emphasis). These disciplinary commitments play a central role in establishing preferred analogies and determine the range of acceptable solutions to problems.

The third component of the matrix is the shared values of the scientific community. These are epistemic values like simplicity, accuracy, and consistency that are at all times involved in the judgement of scientific work. On the one hand, they are more general than symbolic generalisations, and to a larger extent shared by different (natural) scientific communities. However, on the other hand, the *application* of values in any particular judgement is not homogenous and varies greatly among fields or individuals, especially when it comes to combining judgement of the relative weight of a set of relevant values (Kuhn 1996:185). The fourth component of the disciplinary matrix is also the second main meaning of "paradigm". This is the original meaning of the word "paradigm", as ideal example or exemplar. Kuhn (1996:23) takes his departure from the grammatical concept of paradigm. For example, comparing the adjective "fine" (fine–finer–finest) may be used as an exemplar in learning English grammar. Once the student understands the structure of transformation, she may use the principle of comparison when encountering any new adjective. The principle, when applied to the student of science equipped with basic symbolic generalisations, meeting new situations in the laboratory, is that "the student discovers [. . .] a way to see his problem as *like* a problem he has already encountered. Having seen the resemblance, grasped the analogy between two or more distinct problems, he can interrelate symbols and attach them to nature in the ways that have proved effective before" (Kuhn 1996:189; emphasis in original). Drawing on Michael Polanyi, Kuhn argues that this implies that we should understand science as *tacit knowledge*, learnt through practical experience, rather than as formalised knowledge of rules or laws. Science thus builds on the socially shared and trained intuitions of the specific scientific community (Kuhn 1996:191).

The role of exemplars and learning how to perceive natural phenomena as analogous to something previously known connects to the central role of gestalt perception in Kuhn's theory. Kuhn exemplifies this using the famous duck-rabbit drawing, pointing out that the observer doesn't perceive these lines of ink on paper *as a rabbit*, but rather the rabbit, or duck, is immediately perceived. However, for the trained scientist, there is only observation of nature *as something already known:* there is no primary perception of stimuli (ink on paper) that is *interpreted as* a rabbit in a second stage. The rabbit (or duck) is the primary perception. In the same way, where the untrained eye may see chaotic information in the images from the laboratory's bubble chamber, the trained physicist immediately sees a familiar event at the atomic scale (Kuhn 1996:111). This is the foundation of a detailed and more sociological account of the *theory-dependence of observation*. New members of the scientific community come to "learn to see the same things when confronted with the same stimuli [. . .] by being shown examples of situations that their predecessors in the group have already learned to see as like each other and as different from other sorts of situations", which again points to the central role of the paradigm as exemplar (Kuhn 1996:193–94).

This means that, according to a Kuhnian view, scientific disciplinary communities are fundamentally based on common ways of seeing the world, and scientific change is primarily driven by changes of vision, rather than by new data. Against Popperian falsification, Kuhn argues that a lot of anomalies are normally accommodated within a paradigm in normal science. While Kuhn has often been criticised for a radical interpretation of his concept of incommensurability between paradigms, he was himself very clear about his own modest interpretations (Kuhn 1996:198-; Sismondo 2010). Incommensurability does not mean that members of different paradigms debating their merits can have no communication at all, or no good reasons at all, or that it is purely subjective matter. Instead, it means that theory choice is never a matter of purely logical or mathematical proof from given premises. Instead, what are often at stake are the rules, values and premises of opposing social groups that may promote different meanings and applications to the same rules of argument or invoke other criteria of judgement, or weigh them differently. Such debates about premises do not have the coercive power of logic, but are instead a matter of persuasion on the collective level of the particular scientific community.

The theme of incommensurability and the boundaries of the world views of particular scientific groups have been much debated and discussed since Kuhn. It has been developed in terms of a looser concept of epistemic cultures (Cetina 1991), and the question of exchange, "trading zones" and translation over epistemic boundaries has opened another set of research questions (Lamont and Molnár 2002). I will come back to the question of boundaries in science below. However, one of the main influences from Kuhn is also to shift the interest of the sociology of science from Merton's focus on the *social* structure of the scientific community, to its *cognitive* structure, and the role of tacit knowledge and practice in science.

From the strong programme to actor-network theory

The influence of Kuhn and Quine came to fruition in the 1970s with the establishment of the strong programme in the sociology of knowledge, where David Bloor and others sought to radicalise and transcend Merton's rationalist and internalist programme with a new sociology of scientific knowledge. They saw that the old prescriptive philosophy of the received view, that tried to establish on philosophical grounds what science *ought* to be, had been superseded by Kuhn's descriptive historical account of how science *actually* worked. There was a sense that the time had come for the replacement of the philosophy by an empirical sociology of science. Bloor argues polemically that Merton, but also philosophers like Popper and Imre Lakatos, stood for a "sociology of error", which was only sociological in the explanation of error and failure in progress of reason, and of all the once promising theories and facts in the history of science that later became considered false. Against such a position, Bloor (1991, 7) offers the famous four tenets of his strong programme in the sociology of scientific knowledge.

First, such sociology should be based on a principle of *causality*, studying the causes of beliefs.[31] The sociologist should study beliefs or states of knowledge *as facts* in a naturalist, or if you will, scientific, way. Second, it should be *impartial* with respect to claims of truth and falsity. Every state of belief should require a causal explanation. Thus, it should be a sociology of both error and truth, in short, of scientific knowledge. The third tenet is perhaps the most well-known one, the so-called symmetry principle. Following logically from the first two, it states that the type of explanation for true and false beliefs should be *symmetrical*. The new sociology of scientific knowledge drew on a range of historical case studies, and the symmetry principle is an effective weapon against all forms of Whig history.[32] The fourth tenet is the principle of *reflexivity*, that the approach should be able to direct the same gaze towards itself.

If facts and theories are to an extent underdetermined by data, and if we should strive to causally explain how scientific facts are established and stabilised, how consensus is achieved and scientific controversies are terminated, social explanations enter into the equation. The new sociology of scientific knowledge developed along a few different lines. One strand tended to emphasise ethnographic work in laboratories and similar sites, following scientists around like anthropologists, meticulously examining the everyday micropolitics and

[31] "Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief", Bloor (1991:7) adds. In the afterword to the second edition, Bloor replies to critics to clarify this point again: "But doesn't the strong programme say that knowledge is purely social? Isn't that what the epithet 'strong' means? No. The strong programme says that the social component is always present and always constitutive of knowledge. It does not say that it is the *only* component, or that it is the component that must necessarily be located as the trigger of any and every change: it can be a background condition." (Bloor 1991:166; emphasis in original).

[32] *Whig history* is the type of history written from the point of view of the present as the inevitable and natural outcome of historical processes, constructing history as a natural path towards the present, higher stage of things, concealing historical contingency and the possibility that "it could have been otherwise". In the history of science and ideas, Whig history means taking what we today believe to be true as unproblematic and natural and in less need of explanation than those facts or theories which were once held to be true but today, against the fund of current knowledge and systems of thought, strike us as absurd and utterly wrong.

practices involved in interactions with instruments, gadgets, peers, publishers etc. in the complex establishment of scientific facts and the recruitment of proof and allies to support a certain view or fact.

A second strand of work tended to rely more on historical case studies and looked at social influence as a macrolevel phenomenon connected to social groups and social interest. This line of work shows influences from both Durkheimian (not least through the influence of Mary Douglas) and Marxist thinking. Against the "internalist" and rationalist accounts that saw scientific progress as immune to external (sociological) explanations, since its history could be rationally reconstructed as a purely internal history, Bloor and the others posed a sociological externalism. The explanations constructed are often considered to belong to a general class of interest explanations, where epistemic controversies are related to warring interests among social groups, often using historical case studies (Sismondo 2010:50). To cite just one example, Steven Shapin (1975) related controversies about the scientific status of phrenology in early nineteenth century Edinburgh between different learned societies to their different social bases, and connects it to the increasing intensity of class struggle.

An inspiration for a slightly different line of work within the strong programme was the anthropologist Mary Douglas, who developed the Durkheimian theme of the social structuring of knowledge. Her grid/group theory is a useful model for relating social structure to the cognitive domain in science (Douglas 1982). Here, the degree of organisation and hierarchy (grid) and boundedness of the social group (group) is used to explain different ways that scientific communities with different social structures may relate to, for example, anomalies, depending on their social characteristics. For example, Bloor (1978) used the idea to explain developments in the nineteenth century history of mathematics by relating theoretical development to the changing structure of academic research—with mathematical knowledge being territory where neither Durkheim nor Mannheim had dared to tread. The general lesson from this type of approach to social epistemology is that the structure and development of knowledge may be studied by relating it to the social organisation of knowledge production. However, Douglas and the strong programme have been of less direct influence on the recent styles literature.

The conceptualisation of the relation between social structure and specific ways of thinking characteristic of the strong programme have also been criticised for shortcutting the connection between social structure and thought. One such criticism came from Pierre Bourdieu, who developed his own programme for a sociology of science, although it was never actually put to use in any empirical studies, and never really interacted with the sociology of science (Bourdieu 1975, 1991, 2004:20; Kim 2009). Bourdieu argues against the overly rationalistic view of universal scientific reason held by the Mertonian approach, but also against the strong programme with its, to his mind, simplified connection between social interest and action, and what he termed the "interactionist" ethnographic approach that emerged concurrently. Bourdieu upholds a view of science as simultaneously rational and social, and argues that both approaches tend to short-circuit the route from social interest or strategic calculation to the settlement of facts, without accounting for the specific sense of reasoning and judgement of evidence that is required in each specific setting (Kim 2009:65). This is solved by the habitus concept, however, it is a specifically *scientific* habitus. Through training and practice in the field, scientists acquire a specific scheme of perception, an embodied sense of judgement in scientific matters: this is the scientific habitus. Bourdieu reminds us of the way that habitus works like Wittgenstein's rule-following: it may be possible to follow the rules in practice without being able to explicate them.

This means that the notion of a scientific habitus is something different from the Mertonian moral norms that some critics have confused it with, according to Kim:

> The concept of habitus tells us why scientists are able to determine whether a specific interpretation and criticism of other scientists' actions and beliefs can be justified or not without reference to the concept of "universal" norms. The "sense of justification" invoked in this interpretation and criticism [. . .] indicates the inextricability of what is political and what is epistemic. (Kim 2009:62)

In Bourdieu's view then, this implies that in order to by taken seriously by peers, the scientist must "be able to express his ideas and arguments according to the form imposed by the 'structural censorship of the field'" (Kim 2009:65). This notion of a scientific habitus is a very useful sociological tool for thinking about how the historically and socially variable organisation and production of knowledge is engrained in the professional disposition and judgement of the individual scientific actor, who may, according to Bourdieu, be struggling over positions and prestige in a scientific field, while simultaneously pursuing truth. Amidst more general developments in social theory, Bourdieu was one among the many who critiqued the use of simplified notions of interest and the possibility of identifying one dominant interest as the primary explanation for social action among the members of a particular social group, be it class interest or any other type of interest.

One line of research in science studies takes a slightly different approach to how social groups are constructed, which is highly relevant to this study: the literature on boundary work that has developed in the border zone between science studies and sociology. There has been a recent surge in interest across social science disciplines in *boundaries* as general relational social phenomena (Lamont and Molnár 2002; Wisselgren 2008). It is useful to follow Lamont and Molnár (2002, 168) and make a distinction between *symbolic* and *social* boundaries. The former are understood in general terms as "conceptual distinctions made by social actors to categorize objects, people, practices, and even time and space. They are tools by which individuals and groups struggle over and come to agree upon definitions of reality". In a general formulation, symbolic boundaries are central resources in classifying people into group membership, and through which they compete for status and resources (in relation to, among other things, gender, class, ethnicity, nationality, profession, scientific disciplines, etc.). Symbolic boundaries may, when generally agreed upon, turn into *social boundaries*, and function as a necessary but not sufficient condition for the latter (Lamont and Molnár 2002:169). Social boundaries are then understood as "objectified forms of social differences manifested in unequal access to and unequal distribution of resources (material and nonmaterial) and social opportunities" (Lamont and Molnár 2002:168). One of the main sources of this strand is Bourdieu's work on the role of classification of taste as constitutive of social class (Bourdieu 1984; Lamont 2012a:201; Lamont and Molnár 2002). However, another equally influential source of inspiration comes from science studies, and especially Thomas Gieryn's pioneering writings on boundary work.

According to Gieryn (1983), *boundary work* is an attempt to show how something like the demarcation of science from non-science is a practical problem and accomplishment, rather than a philosophical problem to be normatively established by armchair thinkers. Boundary work constitutes the practices employed by professional collectives in their struggle for authority and resources. There are three main types of boundary work: the *expansion* of the authority of science (as in the case of the fierce debates against religion in nineteenth century Britain), the *protection of its autonomy* against outside attempts to regulate or circumvent it, and finally the *monopolisation of scientific authority* against rivals with claims to the scientific game (Gieryn 1983:791–92). It is this lattermost case that is of the greatest interest to this study. The concept of boundary work allows us to see that the demarcation of science from non-science, as well as the boundaries between disciplines (which may often fill the same function) are ambiguous, variable and contested. They are the outcomes of, and repeatedly contested, object of disputes over what science or a scientific discipline really is or ought to be. The rhetorical practices of boundary work function in a relational way through the establishment of contrasts with an outside; just like the genius of

Sherlock Holmes is highlighted through the contrast with Watson, the scientificity of science is emphasised through the comparison with "pseudo science" (Gieryn 1983)—and, one could safely add, other attributes like "journalism", "theology", "opinion" or "common sense". In sum, the boundary work literature points to the construction of social and symbolic boundaries as a practical and relational achievement, and thus scientific authority and boundaries as outcomes of social power struggles.

The critique of the strong programme also led to the development of the next major approach in science studies, actor-network theory (ANT). Its authors, of whom Bruno Latour, Michel Callon and John Law are considered the most influential, claimed that the strong programme was not symmetrical enough. Just as Bloor had claimed that the Mertonians failed because of a lack of nerve, they now argued that the strong programme did not take their own insights sufficiently seriously. For while the sociologists claim that the scientists' facts are just contingent and underdetermined outcomes, they use theoretical concepts like "class" as if they were stable and unproblematic "facts" in order to account for the stabilisation of *natural* facts. Against this view, ANT poses *generalised symmetry*, the notion that both nature and society are constituted through the successful mobilisation of a network of "actants". The nature-society symmetry is thus also extended to the concept of action, and agency flattened in order to treat both humans and non-humans as causal agents enmeshed in heterogenous networks that translate the interest of its actants. For example, in Callon's (1986) pioneering study of the interaction between fishermen, scientists and scallops in St Brieux Bay, the scallops are understood as agents in the same way as the scientists who attempt successfully to enrol them as allies in an attempt to provide a new aquaculture livelihood for the fishermen through scallop farming. ANT in the widest sense has been fundamental to the contemporary STS field, and has provided new conceptual tools to understand the sociotechnical networks that are constitutive of modern life. The material turn that ANT represents, and its attraction, could probably also be understood in some respects as a reaction to the general discursive turn in the social sciences and humanities. More specifically for the study of science, the notion of hybridity and cognition as distributed and inscribed into arrays of machines, instruments, software, etc. is useful in thinking about the ways in which cognition may be stabilised, although one may likewise think of "distributed cognition" outside of ANT (Giere 2007).

From the point of view of a sociology of knowledge interested in structures in science, there are some potential problems with ANT. The thesis of "super-symmetry" precludes any macro or structural explanations, and reduces all macrophenomena to microinteractions. The argument for this symmetry seems unwarranted. Just because the establishment of sociological macroconcepts isn't "true" in an absolute sense, and is the outcome of social processes of negotiation and stabilisation—which, in the STS view, holds true for all scientific knowledge—this doesn't mean they can't be employed in explanations as the best available provisory knowledge in most contexts. Second, although it is certainly very important to integrate non-human causality into social analysis, the omission of intentionality and notions like culture and practice from *human* agency seems to be an unnecessary limitation of powerful social scientific conceptual tools (Sismondo 2010:87).

This overview of the development of the rich and multifaceted field of modern science studies is by its nature only a very preliminary sketch marked by a few "big names". Any such overview could also have been categorised in other ways, for example in terms of four main models of science as suggested by Callon (2001), or could have emphasised other aspects. One theme that is worth mentioning, for example, is the way in which science studies can be thought of as a form of critical theory. For example, the role of social movements and the radicalisation of the 1960s is an important piece of the puzzle to understand the critique of positivism and the rise of science studies. In this vein, David Edge points to the role of the democratic impulse in relation to science and the urge of the anti-war movement to democratise society and science, and that the birth of STS shares a social movement impulse with gender studies (Edge 2001; see also Sismondo 2010). In a similar vein, Carl-Göran Heidegren shows how the various positivism disputes around the 1960s often came to be interpreted by the New Left as a politically progressive critique of establishment "positivism". There is then clearly a link between the study of science and political movements and values. Social studies of science belong, to varying extents, to a tradition of critical theory, with the aim to use rational reasoning and scientific methods in service of an aim to show that the present "could have been otherwise", and point to the transcendence of the present through investigating its conditions of possibility (Callinicos 2006). This critical impulse and turn towards the conditions of possibility of dominant modes of knowledge production and their history is also paralleled in the interest that heterodox economists take in both philosophy of science and history of thought.

Primed with this historical background sketch, we now turn to literatures that are of more specific relevance to this study. The first is the literature on the macrolevel styles of thinking or reasoning in the history of science. It is a theoretical and empirical body of work with influences from Fleck and Kuhn, but reinterpreted by new generations in new contexts, and fuelled by empirical and theoretical work in the history and philosophy of science.

2. The literature on styles of scientific reasoning

This section will provide a deeper account of the recent literature on styles of scientific reasoning, primarily found in the history and philosophy of science. The point of departure will be the historical origin of the idea of styles of thinking in Karl Mannheim and Ludwik Fleck, before turning to the concept of styles as re-thought by its modern day originator, Alistair Crombie. I then turn to a more thorough investigation of how Ian Hacking, the central proponent of the concept, has developed it and departed from Crombie. After that, I present an analytical breakdown of nine central features of what I call the styles of reasoning approach. Following that, I contrast this interpretation of the styles project to Thomas Brante's recent formulation of *ontological models* as a similar sociological approach, which adds a sociological sensitivity to the triangular relation between a scientific field and its established ontological model, a profession socialised into thinking with that model, and the constitution of a common object for both professional practical intervention and scientific theoretical representation.

From art history to syphilis: Mannheim and Fleck on intellectual styles

The origin of the modern notion of aesthetic "style" is found in art history, where it slowly grew from its etymological origins in the Latin *stilus* (pen), used in antiquity to denote a personal style, and the use of *decorum* in rhetoric, according to the art historian Ernst Gombrich (1968). In the eighteenth century *style* came to be used to denote distinct historical periods (classical, gothic, baroque, etc.), and later became an established concept in art history. Following German romanticism and the influence of Hegel, the styles of art came to be understood and presupposed as one of many expressions of an assumed spirit or totality, whether in the idealist form directly following Hegel, or in its Marxist inversion. The idea that artistic styles stand in some relationship to a certain world view was then picked up by Karl Mannheim and transferred from the domain of art to the domain of thought through his early work, especially on the conservative style of thought, where Mannheim makes the concept of styles of thought central to his sociology of knowledge as a means of "grouping together the form and content of political-philosophical ideas as cultural products" (Mannheim 1953; Nelson 1992:26).

Mannheim uses the concept to fill what he sees as a void in intellectual history and find a middle ground between two false extreme positions: on the one hand the monolithic thesis that all thought is unitary, that for example in a given culture there is one way of thinking, and deviations from it. Such a view obviously overemphasises the collective and unchanging nature of thought. On the other hand, the opposite is held by an atomistic thesis that there are only individuals and individual thinking. Instead, he argues that "the most important unit must [. . .] be the style of an epoch, against the background of which the special contribution of each individual stands out and acquires its significance" (Mannheim 1953:76).

The concept of *Denkstil*, or style of thinking, became established in the German-speaking world through Mannheim in 1925 and the adjacent influence of Mannheim's and Max Scheeler's new sociology of knowledge. The concept was then employed by Ludwik Fleck in the first analysis of styles of thinking *in science*, where the concept was also paired with Fleck's innovation, the sociological concept of thought collective (*Denkkollektiv*) as the social foundation of a specific style (Fleck 1979; Trenn 1979:xv). Apart from the inspiration from Mannheim, Douglas reminds us that the Durkheimian school was also one of Fleck's main inspirations, and claims that his *thought collective* and *thought style* may be interpreted as developments of Durkheim's notions of the *social group* and *collective representation* (Douglas 1987:12; Fleck 1979:46). According to Douglas, this is essentially a Durkheimian theme: "On a Durkheimian approach a distinctive thought style develops as the communicative genre for a social unit speaking to itself about itself, and so constituting itself" (Douglas 1996:xii).

Fleck's (1979) demonstration of the idea that thinking comes in different discernible styles in the history of science was performed in his classic study of the history of the modern disease entity syphilis, *Genesis and Development of a Scientific Fact*. Fleck's book, published in German in 1935, would probably have ended on the trash heap of history had it not been discovered by Thomas Kuhn by happenstance, from a footnote in a work by Hans Reichenbach (Kuhn 1979). After Kuhn's citation of the book, it was translated into English in 1979 and soon became a classic of the emerging new sociology of scientific knowledge. However, by then the historian of science Alistair Crombie had already presented his work on styles of thinking in the history of science and influenced Ian Hacking (Hacking 2012).[33]

In Fleck's account, there is also a sensitivity to science as practice and a detailed analysis of the minutiae of laboratory observation. In a theme later developed by Kuhn, Fleck explains how perception must be understood in terms of what he calls "the readiness for directed perception". This is the idea that in science, relevant perception is never unmediated. Instead, it is the trained capacity for

[33] Crombie is apparently silent on the source of his use of the concept, but is most probably influenced directly by Mannheim.

perception of very specific forms and patterns. Fleck saw the thought-style as closely linked to a more or less bounded thought collective and its specific way of holding the same assumptions, ways of asking questions, use of methods, and so on. In Hacking's summary:

> [A] Thought-Collective is a small network of investigators who address a family of problems that they understand in much the same way, and which they attack using a group of mutually intelligible methods. Fleck's Thought-Styles were constituted by the types of questions asked, the range of possible answers that was envisaged, the methods which were useful, and the background information taken for granted. A Thought-Collective was a social unit identified by education, training, interests, and mutual communication. Thought-Collectives are local, cohesive, but relatively short-lived, for they tend to dissipate as questions become answered or problems prove to be intractable. People move on, and out of the collective. (Hacking 2012:604)

Here, we find a fundamentally sociological account of thought styles. In Fleck, the style is carried by a social group of knowers, the thought collective, which transmits its specific style to new members through education, practice and internal communication. Furthermore, an important aspect of Fleck's dual notion of thought collectives and styles is their open-ended and fluid character. Compared to Kuhn's vision of the paradigm in normal science, actors may belong to more than one thought collective and even move between them, which is coupled with an insight about the potentially creative effects of clashing styles of thought. Although Crombie and Hacking have largely followed their own direction with a different emphasis, Hacking (2012) has lately called for a return to the more sociological understanding found in Fleck.

Crombie and Hacking: Six styles of reasoning in the history of science

Crombie's notion of styles of thinking in the history of science is fully developed in his three-volume magnum opus *Styles of Thinking in the European Tradition*, that employs the concept to delineate the successive introduction of specific styles of thinking in Western science since the pre-Socratic Greek philosophers (Crombie 1994, 1995). However, Crombie had been working on the project for decades before publishing *Styles of Thinking* in 1994, and the ideas had been picked up by the philosopher of science Ian Hacking already in the late 1970s and gained momentum with his influential 1992 article (Hacking 1992, 2012; Ritchie 2012b). Today, Hacking's more philosophical elaboration of the idea is the most important spark for recent work in this vein, as evidenced by the recent special issue of *Studies in History and Philosophy of Science*, devoted to Hacking's work on styles (Ritchie 2012b).

The "styles project", as Hacking (2012) calls his philosophical adaptation, is also strongly influenced by the French intellectual tradition of historical epistemology, originating in Gaston Bachelard, not least through Foucault. This distinct tradition is strongly present in aspects of the works of such otherwise diverse figures as Louis Althusser, Michel Foucault, and Pierre Bourdieu (Broady 1997), and played a not insignificant role in influencing Kuhn. Historical epistemology fills the gap between history and philosophy (epistemology). Its core problematic is, in Hacking's words:

> a concern with very general or organizing concepts that have to do with knowledge, belief, opinion, objectivity, detachment, argument, reason, rationality, evidence, even facts and truth. [. . .] Proof, rationality, and the like sound so grand that we think of them as free-standing objects without history, Plato's friends. [. . .] The important point [. . .] is that the epistemological concepts are not constants, free-floating ideas that are just there, timelessly. (Hacking 2002:8)

One of the hallmarks of historical epistemology is the idea of an epistemological break, a radical rupture where the obstacles of scientific thought—common sense ideas and concepts—are replaced by a new scientific conceptual object, thus constituting a scientific realm of thought and practice radically distinct from surrounding lay knowledge (Tiles 2004). However, this does not mean that science can be treated in isolation. There is also an imperative in this tradition to understand the development of the sciences in their ecological context of wider knowing. Bachelard's successor, the historian of science Georges Canguilhem (2004:204), notes in this fashion that "the history of sciences [. . .] is related not only to a group of sciences without intrinsic cohesion but also to nonscience, to ideology, to political and social practice". Foucault, another of Hacking's inspirations, took his notion of *episteme* to account for the underlying organising structure of the knowledge of a whole epoch, constituting the conditions of possibility of discourse stretching through not only the sciences, but through the whole discursive landscape of the time. Foucault's (2002:228) archaeological approach left the emphasis on epistemology and the specificity of the sciences held by Bachelard and Canguilhem, focusing instead on the common regularities underlying science, knowledge, and discursive practices of an epoch, its *episteme*.

This notion of some sort of historical variation and development of the limits of what it is possible to think in the sciences is key to the Crombie-Hacking concept of styles of reasoning. A central idea is the insistence on putting the pursuit of knowledge and its conceptual apparatuses in its proper historical place, or rather places. For such an approach leads to a very sceptical view of the grand narrative of the unity of science, the belief that there is something that is "*the* scientific method" through history or across the sciences. Instead, as Hacking and others have long held, we face a fundamental *disunity of the sciences*, what one could perhaps call scientific pluralism (Bueno 2012). I will return to this historicising of reason and its implications according to Hacking, but first say a few words about Crombie's original conception.

Crombie's work is a grand narrative of the roots of Western science, which he locates in the commitments and dispositions that bred a specific form of rationality introduced by the Greeks. It is a work that draws "upon a lavish array of citations spanning three millennia, plus dense references to secondary studies—the lifetime collection of an erudite" (Hacking 1992:2). Such a wide grasp of course invites to search for the big picture and durable structures in history. The focus of this work is historical epistemology, an attempt to understand from within how epistemological assumptions and conceptions have been shaped throughout the history of science:

> The whole subject offers an invitation to look beneath the surface of immediate scientific results for deeper, continuing structures. In our comparative historical anthropology of thinking *we must look not only with, but also into, the eye of the beholder*. (Crombie 1995:232; my emphasis)

Crombie builds this grand history on two concepts, *styles of thinking* and *commitments or dispositions*. The latter are "intellectual and moral" commitments to conceptions of nature, conceptions of science, and conceptions of the desirable and possible. Taken together, the first two "establish, in advance of any particular research, the kinds of argument, evidence and explanation that will give satisfaction, because the supposedly discoverable has been discovered in conformity with the acceptable criteria" (Crombie 1995:232). "Within these general commitments", Crombie continues,

> scientific thinking became diversified into a number of different styles of inquiry, demonstration and explanation. [. . .] A scientific style, with its commitments, identified certain regularities in nature, which became the object of its inquiry, and defined its questions, methods and kinds of evidence appropriate to acceptable answers within that style. (Crombie 1995:234)

Crombie identifies six styles that emerged at different points in time throughout history, and have since become part of the scientific tradition. These are, in chronological order of appearance: i) the mathematical style, ii) the experimental style, iii) the hypothetical modelling style, iv) the classificatory style, v) the statistical style, and vi) the historico-genetic style.[34] Although Crombie's account is one of strong historical continuity of forms of rationality stretching over millennia, a fundamental argument is that the development of science was not a "monolithic system", but was instead marked by an essential disunity. Science has never been uniform or had a single scientific method; instead, it was always fractured. Crombie's six styles of thinking are very different ways of approaching nature (and society), however two fundamental overarching principles of the grand tradition of Western philosophy and science are established in antiquity

The first of these is the idea that nature is governed by general self-consistent causal principles that may be hidden but nevertheless possible to find out. Second, it is the idea of formal proof, of a rational system constraining the valid ways of finding out (Crombie 1995:225). One may find Crombie's emphasis on the unique Western cultural tradition Eurocentric. Hacking (2012:602) for one notes for example that "Crombie was a Roman Catholic by conversion, and attached far greater weight to mediaeval Christian (but not Islamic) contributions than they deserve.". However, this historiographical question of relative contributions does not affect the validity of the general analysis of the six styles. The strength of the identification of these styles lies in their being generally acknowledged and uncontroversial; in Hacking's (1992:8) judgement "It is a good workhorse of a list that holds no surprises".

Echoing the Kuhnian theme of incommensurability, Crombie explains that:

> these six styles and their objects are all different, sometimes incommensurable, assuming fundamentally different physical worlds, but frequently they are combined in any particular research. By identifying the regularities that become its object of inquiry, and by defining its questions and acceptable evidence and answers, a style both creates its own subject-matter and is created by it. A change of style introduces not only new subject-matter, but also new questions about the same subject-matter. (Crombie 1995:237)

This result is a view of scientific progress that, echoing Kuhn's (1996:205) metaphor of progress, is "not linear, but takes the form of branches growing at different levels in a variety of directions". Moreover, this growth of science involves an essential difference between, on the one hand, propositions about testable factual propositions that tend to be relatively historically stable and

[34] Note that Hacking, Crombie and other authors within this literature use subtly different labels for the six Crombian styles.

cumulative. On the other hand, abstract explanations involving theoretical entities that cannot be directly tested, and all presuppositions about the world, are changed instead by rethinking. This amounts to scientific revolutions that opens new areas of research and new types of questions. Crombie's position seems to be in line with Kuhn's later position where claims to radical incommensurability are countered and the progression of science painted in a similar manner (Kuhn 1996, see the 1969 afterword).

Central tenets of the styles approach

Drawing on Hacking's development of the styles concept, I will now analytically break out and summarise nine tenets of the styles approach that are of relevance in the present context.

i) Reasoning, not thinking

Hacking (1992:3) shifts from talking about styles of thinking or thought, to *reasoning*. It is more suitable to talk about styles of *reasoning* than styles of *thought*, because "thinking is too much in the head". The important difference is that *reasoning* takes place also in public, by argument, demonstration, persuasion, and furthermore through *doing*, as in the use of various working objects in reasoning, as Mary Morgan (2012:380) has pointed out. This also connects to the notion of distributed cognition, the understanding of reasoning and cognitive processes as taking place in a network of people, computers, and apparatus. In such a view, concepts may be just as much embedded in computer software as in the heads in thinking individual scientists or their discourse (Callon and Law 1995; MacKenzie 2008:16).

ii) Reason comes in different styles that all have a history

The approach takes the social and historical nature of reason seriously. I think Hacking is absolutely right to approve of Bourdieu's claim that "We have to acknowledge that reason did not fall from heaven as a mysterious and forever inexplicable gift, and that it is therefore historical through and through; but we are not forced to conclude, as is often supposed, that it is reducible to history" (Bourdieu 2000:109; Hacking 2012:600). Or, in Hackings (2012:603) typically succinct formulation, "we had to find out how to find out". The styles concept adds to the historicisation of reason the simultaneous coexistence of multiple forms of potentially incommensurable reason in the sciences. Reason has a history, and it may take different forms, but it is still reason.

This allows us to understand scientific practice as distinct from other forms of discourse contra the view expressed by Rorty and others about scientific inquiry as just another part of a grand "largely undifferentiated conversation of mankind" (Hacking 1992:17). We may then acknowledge that scientific reason and truth claims can be understood as distinct from other forms of knowledge, while simultaneously not thinking of science as monolithic. Instead, scientific reason comes in different styles, which may be complementary or contradictory depending on the specific case. But there is the possibility that two styles of scientific reasoning may be at odds, involved in heated disputes with no external arbiter. Perhaps the prime case in the history of economics to illustrate this would be the late nineteenth century German *Methodenstreit*, where, in a styles interpretation, the historical-genetic style of reasoning of the historical school was pitted against the axiomatic-mathematical style of the budding neoclassicists.

iii) The six styles in the list are not exhaustive or mutually exclusive

Crombie's list of six styles is not a complete list of possible styles, nor are the styles mutually exclusive (Hacking 1992:5). This means, first, that two or more styles may coexist in a particular scientific enterprise, as when Mary Morgan (2012) shows that in economics, the modelling style is intertwined with the axiomatic style of mathematical postulation and proof. Hacking exemplifies this with the tension between the early empiricist experimental style working at the level of observables, and the hypothetical modelling style, introducing theoretical unobservable entities. These two styles merged, according to Hacking (1992:6), into a hybrid that he calls the laboratory style, characterised by the construction of apparatus that produce phenomena to be compared to modelling results. Second, we could also envisage other styles not on Crombie's list. In his reflection on the styles project thirty years after its initial publication, Hacking notes that other authors have taken the concept to places he had not envisioned, and that he is happy with that. For example, Arnold Davidson has introduced the idea of a psychiatric style of reasoning (Hacking 2012:601). Thus, the possibility exists of identifying other styles of reasoning, or instances when established sets of styles merge into new constellations (as the combination of experiment and modelling in a new laboratory style), or when such constellations split up (Hacking 2012).

iv) Style as a longue durée concept

The concept of styles of reasoning has a wider scope, sweeping over more time and intellectual space, than most concepts of the limits of what it is possible to think. Concepts like Fleck's *thought style*, Foucault's *discursive formation* and even wider *episteme*, or Kuhn's *paradigm*, may seem grand, but are all more local and restricted in scope (Hacking 1992:3; Morgan 2012:15). The latter should perhaps be qualified at least in relation to Foucault. An important distinction between the styles concept and Foucault's *epistemes* is the distinction between scientific practice and other forms of knowledge. Whereas Foucault is interested in the common episteme as a foundation of all the knowing of an epoch, his focus is the historical period as a unit. The styles concept, on the other hand, focuses long-term historical continuity in scientific reasoning, even if the introduction of new styles is characterised as rather sharp breaks by Hacking (2012:604; Ritchie 2012a:650). The unit of analysis is then scientific reasoning throughout history as a species distinct from other forms of human knowledge.

Hacking (1992:9) notes, aware of how he is going against the tide of most science studies, that "regardless of interest, philosophical or historical, many of us may be glad that at a time of so many wonderfully dense and detailed but nevertheless fragmented studies of the sciences, we are offered such a long-term project". He places his own approach somewhere between the very local and "increasingly fine-grained analyses of incidents, sometimes made tape-recorder in hand" that verge towards "the fleeting" on the one hand, and the philosophers approach towards the "quasi-timeless end" (Hacking 1992:9). The styles of reasoning concept allows us to see the really big picture of how different styles, once they become autonomous after a historical moment of crystallisation, come to live their own autonomous lives as a timeless canon.[35] This is the *longue durée* of science studies.

However, with elaborations of other styles than Crombie's original six, and pulling the concept to be more fine-grained, it perhaps possible also to see the styles concept at a smaller scale. The philosopher and historical epistemologist Martin Kusch (2010) has produced what is probably the most thorough critique of Hacking's styles project, from a largely sympathetic position. One of his main points is a critique of Crombie's historiographic continuism which, in Kusch's view, Hacking uncritically accepts. Crombie's view in this account is both internalist (largely disregarding the context of science) and continuist, dismissing the importance of breaks and events like the seventeenth century scientific revolution and instead emphasising the mediaeval theological revolution leading to the "Jewish-Christian conception of God as an inscrutable creator" as the central impetus for the growth of scientific inquiry into nature (Kusch 2010:165). For the present purposes, the *longue durée* and early science aspects are not of

[35] Hacking's notion of a historical moment of crystallization of each style, and therefore the abrupt breaks in the history of styles as opposed to Crombie's emphasis on continuity, is one of the main point of difference between the two according to Ritchie (2012a). However, Martin Kusch (2010) disagrees, and criticizes Hacking for accepting Crombie's continuism.

interest. But Kusch has an interesting suggestion. He criticises Hacking's identification of styles as necessarily belonging to the long run, instead suggesting that just as Fernand Braudel's concept of *longue durée* was conceived as part of a three-layered history, we could perhaps also think about styles in the same way. Thus, *courte-durée* styles may be local and only span a few decades, while a middle layer of *moyenne-durée* styles lie between the two extremes (Kusch 2010:170).

When thinking about the scope and level of abstraction of scientific styles, there may also be openings for combinations of styles with other similar concepts. Commenting on Hacking's styles project, Rasmus Grønfeldt Winther (Winther 2012) argues that we do not necessarily have to choose between concepts like styles of reasoning, epistemes, research programmes or paradigms in the search for a single superior concept. Instead, he presents a heuristic image of how we may think of styles, paradigms and models in relation to each other. The view he suggests is that:

> models are nested within (and guided by/realize) paradigms which, in turn, are nested within (and guided by/realize) styles. This picture is a useful start, even if it is also a false and overly simplified idealization. This hierarchical image entails that properties and parts of the upper category (e.g., styles) are inherited by the lower category (e.g., paradigms), and that categories above guide and are realized in the categories below. The structure of actual scientific practice is of course more complex. Multiple realization among category levels, and hybridization within a level, are commonplace, and new parts and aspects sometimes emerge at lower-level categories. Case studies matter. (Winther 2012:629)

This is an interesting suggestion. However, already from the above quote, the difficulties of accounting for hybrids and multiple realisations within such a general abstract model structure become obvious. Perhaps it can be seen as just a heuristic device to help us think about these concepts as concepts at different levels of abstraction with each level granted relative autonomy, as it were. For our purposes, this loosening of the styles concept and introduction of the possibility of *courte durée* styles by Kusch allows us to employ the concept more flexibly. For example, is the modelling style in economics studied by Morgan really meaningfully studied as part of a *longue durée* style?

v) Styles introduce novel objects and methods for finding out

The crystallisation and introduction of a new style introduces a range of novelties, including new types of objects, evidence, and "new ways of being a candidate for truth or falsehood"(Hacking 1992:11). These novelties are the instances that make up the style, just like a style of art exists through its instantiation in a set of artworks. But what does it mean to say that a style of reasoning introduces a new class of objects into science? While the idea of styles as distinct ways of finding out, or methods of reasoning, should be clear by now, the idea that this also entails new types of objects can perhaps use some examples. Hacking lists a few of them:

> Crombie said his styles have distinct objects and methods of reasoning. These words seem anodyne. The objects with which mathematics concerns itself are often called, by analytic philosophers, abstract objects, such as numbers, shapes, and groups. The objects with which taxonomy concerns itself are, for example, the species and genera of systematic biology, not mere classifications of living things, which are found in all languages, but objects bearing a definite role of sub- and super-ordination to other objects of the same sort. Hypothetical modelling introduces non-observable theoretical entities. (Hacking 2012:600–601; emphasis in original)

Not only is the idea that a style of reasoning is connected to different types of scientific object interesting. But for our purposes, what Hacking claims to be the effect of this is even more thrilling. The introduction of new objects leads to ontological debates about the existence of the novel class objects:

> Every style of reasoning is associated with an ontological debate about a new type of object. Do the abstract objects of mathematics exist? That is the problem of Platonism in mathematics. Do the unobservable theoretical entities of the laboratory style really exist? That is the problem of scientific realism in the philosophy of the natural sciences. Do the taxa exist in nature, or are they, as Buffon urged, mere artifacts of the human mind? [. . .] Each style of reasoning has its own existence debate, as illustrated, because the style introduces a new type of object, individuated using the style, and not previously noticeable among the things that exist. Indeed the realism-antirealism debates so familiar in recent philosophy will now be understood in a new and encyclopaedic fashion, as a by-product of styles of reasoning. (Hacking 1992:11)

While Hacking (2002:606) himself thinks of such debates as “deserts of ontology where nothing flourishes but controversy”, in the present context, the prospect of relating the ontological debates in economics to different styles of reasoning seems fertile.[36] It allows us to think about how ontological debates are connected to the relation between mainstream and heterodox economics, and the close relation

[36] For a slightly different view, still sympathetic to the styles project and that takes realism/ anti-realism debates a little more seriously, see Ritchie (2012a).

between heterodox economics and work in the philosophy of economics. In particular, the ontological arguments pioneered by Tony Lawson in the 1990s have been quite important, and are now seen as important tools for heterodox critique of the mainstream in terms of lacking scientific realism. While his realist argument about the need to talk about ontology seemed like "someone standing alone at a party" when first introduced in 1994, "thirteen years later . . . anyone in economics who knows anything about methodology knows what 'ontology' means. They also have come to realise that if Lawson's basic conclusion were applied it would entail a programme of reform that would fundamentally change economics" (Fullbrook 2009:1–2).

vi) A style of reasoning carries its own possibilities of truth

Among the novelties that belong to a style are the "new ways of being a candidate for truth or falsehood". In the language of Auguste Comte or Foucault, Hacking's main source of inspiration for the idea of styles, "they introduce new kinds of 'positivity', ways to have a positive truth value, to be up for grabs as true or false" (Hacking 1992:12). In his more recent writings, Hacking has started to talk about this in terms of the distinction between *truth* and *truthfulness*, a notion borrowed from the philosopher Bernard Williams. According to this conception, the basic concept of truth is universal across cultures and history. However, truthfulness, the criteria, practices and possibilities of telling the truth about something, does have a history. A style of reasoning introduces and carries its own distinctive "conceptions of what it is to tell the truth about X" (Hacking 2012:605).

This means that there is not a single scientific way in which to search the truth about something, but that there are different, complementary or competing, ways to pursue truth and objectivity in science. Hacking argues that his project is a "continuation of Kant's project of explaining how objectivity is possible", but that since Kant, we have come to understand the historical and communal nature of scientific knowledge. Hacking argues that his ". . . styles of reasoning, eminently public, are part of what we need to understand what we call objectivity. This is not because styles are objective (i.e. we have found the best impartial ways to get at the truth), but because they have settled what it is to be objective (truths of certain sorts are just what we obtain by conducting certain sorts of investigations, answering to certain standards)" (Hacking 1992:4).

Again, the idea of a range of different styles in science focuses our attention to the fact that there is not one form of successful scientific reasoning, but several. We are not tearing down the walls of science, but just pointing to the multiplicity of ways that science can and has in fact been pursued.

> Each style has become what we think of as a rather timeless canon of objectivity, a standard or model of what it is to be reasonable about this or that type of subject matter. We do not check to see whether mathematical proof or laboratory investigation or statistical "studies" are the right way to reason: they have become (after fierce struggles) what it is to reason rightly, to be reasonable in this or that domain. (Hacking 1992:10)

This is also how Morgan claims we must understand the establishment of modelling in twentieth century economics. But it is critical to remember two parenthetical words in Hacking's quote above: "fierce struggles". For I think that this is just what is needed to account for the enduring theoretical struggles in economics. The styles concept allows us to think about conflicting ways of doing science without needing to relegate either of them to the status of non-scientific.

vii) Styles as conditions of possibility

It should be clear by now that there is a sense in which conditions of possibility are central to Hacking's notion of styles. Hacking (1992:3) notes this as a central feature of similar concepts, like Fleck's: "Fleck intended to limn what it was possible to think; a *Denkstil* makes possible certain ideas and renders others unthinkable". James Elwick (2012) brings this feature to the forefront in his suggestion to understand styles of reasoning as "stratified conditions of possibility". Elwick highlights that Hacking, like Foucault, tends to think in terms of "possibility" rather than "cause". In Hacking's interpretation, Foucault's archaeological project is really about "systems of possibility" shaping discourse (Elwick 2012:621). Elwick emphasises that conditions of possibilities are not causes. This is relevant for facing potential critics of determinism that claim the idea of styles to be deterministic.

That is the case with Martin Kusch's (2010) critical paper mentioned above. Kusch's argument employs the doctrine of meaning finitism, the central idea in social studies of science since the strong programme. Wittgensteinian *meaning finitism* is the claim that the application of a rule is never predetermined by the rule or the facts of the matter in advance. Rule application is always an act by an actor whereby the actor draws on a limited pool of experience of previous instances of applications to judge the way the rule should be interpreted in each new instance.[37] Meaning finitism is a fundamental argument for the role of agency and contingency of scientific practice that turns against perceived determinism, be it

[37] For an introduction to the concept of *finitism*, see Sismondo (2010:49).

mathematical reasoning (Bloor 1978), scientific experiment (Collins 1981), or technological development (Pinch and Bijker 1984).

According to Kusch (2010, 166), Hacking's view of the stability of styles rests on a fundamentally flawed doctrine of meaning determinism, where in Kusch's view questions determine unique answers without needing to account for the agency of meaning or rule use. While this is not the place to enter into an examination of the vitality of the doctrine of meaning finitism, what it boils down to is clearly how one views necessity or possibility versus contingency. Of course, a finitist account is bound to (and this is my quite determinist interpretation of finitism) emphasise agency and constant renegotiation. But this emphasis on contingency is simultaneously shading, if not hiding, a view that focuses on continuity and reproduction, on the enduring nature of (some) social structure. Elwick (2012:621) now suggests that by thinking about styles as conditions of possibility, we can also harbour the central finitist insight about rule (style) following as the accomplishment of an agent in practice drawing on a finite number of previous applications. The style is not *determining* or *causing* a researcher working within, say, the statistical style to use it in a predetermined way. But it provides the conditions of possibility of doing statistical analysis: reapplying the rules. While Elwick's suggestion doesn't really solve the fundamental issue of how to think about necessity and contingency in social reproduction, we should keep this emphasis on the role of actors in mind. In fact, both Elwick and Kusch point out that Hacking himself, during the last decade, has turned increasingly to a position of acknowledging the importance of accounting for actors and the social basis of styles (Elwick 2012:621; Kusch 2010:169). I will return to the issue of the social basis of styles below.

viii) Styles are self-authenticating

One of the more controversial features of Hacking's (2012:605) account is his view that styles of reasoning "do not answer to some other, higher, or deeper, standard of truth and reason than their own", in other words that they are "self-authenticating". He claims that "The styles in our list do not answer to any criteria of truthfulness other than their own. They are not 'chosen' because they 'work'. They help determine what counts as working" (Hacking 2012:607–8). Note that this is not a matter of the truth of scientific propositions or even theoretical systems, but styles themselves as vehicles of reason. Styles determine what it is to reason rightly. By introducing new possibilities for truth, and new ways of reasoning rightly, styles nevertheless lead us towards objectivity according to Hacking. Importantly, he also emphasises that:

> the doctrine of self-authenticating styles is distinct from "constructionist" accounts of scientific discovery. For in those accounts individual facts of a typically familiar kind become constructed-as-facts in the course of research and negotiation. There was no fact "there" to discover until constructed. According to my doctrine, if a sentence is a candidate for truth or falsehood, then by using the appropriate style of reasoning we may find out whether it is true or false. (Hacking 1992:13)

This distinction between Hacking's view and that of constructivist account should not go unnoticed. For what may at first sight look like a very relativist account is in fact only moderately relativist. Martin Kusch (2010) in fact makes the lack of proper relativism a central feature of his critique of Hacking. Hacking himself is very clear:

> My doctrine of self-authentication, which sounds like part of the current mood for sceptically undermining the sciences, turns out to be a conservative strategy explaining what is peculiar about science, distinguishing it, to some extent, from humanistic and ethical inquiry (Hacking 1992:17).

Now comes the trickier part. Even if a style provides means to get at the truth, to reach objectivity, this only means that there are several ways of doing that. There are ways of reasoning scientifically which lead us to determine if a proposition is true or not. But since they are contained within styles, and the styles are self-authenticating, there is no arbiter between propositions formulated within different styles. This is Kuhn's incommensurability of paradigms transposed to new terrain. A shift between styles may be regarded as refutation of error or solution to problems, but not necessarily so. Remember that shifting styles also means that the "positivities", the kind of sentences that are up for grabs as true or false, also shift. The ontological objects that are part and parcel of the styles also shift. For example, a statement about the mean age of a population only makes sense within the statistical style of reasoning. The object "mean" was only introduced with the development of statistical and probabilistic ways of reasoning, and made absolutely no sense to anyone before that. This means that different styles mean different ways to approach reality. It may not even be correct to say that they approach their object differently, for there will be a set of style-dependent theoretical objects (like the concept of a statistical mean) that cannot be compared across styles. This leads Hacking (1992:12) to a sceptical view of correspondence theories of truth for the kind of propositions that are related to style, while also maintaining that correspondence is an accurate description of a whole range of simpler "pre-style" propositions.

What does this mean in understanding conflicting styles within a science, which is one interpretation of the heterodoxy-orthodoxy struggles in economics? First, this means that different styles bring with them new objects, truth-candidates, methods and so on. Different styles will mean different ontological objects, different conceptions of the right way to reason and prove, different questions asked and so on. A switch between styles could be thought of in one sense as parallel to a paradigm shift, leading to what Roy Bhaskar (1975) once termed "Kuhn loss", where the idea of a steadily increasing explanatory capacity of science is combined with the insight that the transition from one paradigm to another also means that some unique aspects of the explanatory capacity of the abandoned paradigm are lost. Second, the self-authenticating nature of styles of reasoning, the absence of any higher instance of arbitration between styles, must also mean that there cannot be a definitive answer to the question "which style lends the greatest explanatory power to science X?" Of course there will be answers from everyone involved, but following Hacking's idea of self-authentication, there is no right way of reasoning at this level. Answers given will probably involve support for the involved styles in their own terminology.

Furthermore, a pragmatic explanation in terms of what works, that is, that a theory of style is chosen because it is proven successful, also doesn't solve the problem of self-authentication:

> The maxim, that nothing succeeds like success, is a deeper saying than is usually understood. For success helps determine what will count as success. Success has a lot to do with future success because it helps characterize what in the future will count as success. We continue to change the world mostly to our present liking partly because what we like has been so profoundly affected by things we have grown to like and which are products of the sciences. The styles flourish in a complex web of interactions whose evolution they help determine. (Hacking 2012:605)

If we combine this with the insights of the strong programme, and follow this account strictly, there must also be some other mechanism of dispute settlement (aside from the purely rational) that plays a part in determining which styles of reasoning are actually used. This is, to a great degree, determined by the social forces involved. In this interpretation then, situations of disputes between styles calls for the sociology of science and the analysis of controversies and heterodoxies as social phenomena.

ix) Styles and the quasi-stability of science and sociology

We have already touched upon the issue of stability and contingency in science. Hacking claims that the concept of styles helps us to understand what he calls the "quasi-stability" in the history of science. All six styles on Crombie's list have crystallised and become more or less "timeless" and autonomous. Now Hacking also adds the possibility of the extinction of styles. His frequently-cited example is that of the once-widespread Paracelsian school of Renaissance medicine, the "reasoning by similitude" which included infamous ideas (now only surviving in homeopathic medicine) like the "belief that syphilis must be treated with the metal mercury because it stands for the planet Mercury, which is the sign of the marketplace where syphilis is acquired" (Elwick 2012:622; Hacking 1992:16).[38] However, this leads to the question of how styles are reproduced (and why at least one of them died). Hacking claims, in line with the strong programme, that we cannot regard the truth of a theory (or style) to be explanatory of why someone holds it. Instead, he claims that a style comes equipped with a set of self-stabilisation techniques.

His account is rather thin, but it does lead to us to sociology. It has already been noted how, in recent years, Hacking has leaned more towards the social aspects. In fact, in his 2012 paper, he invokes Fleck as a missing piece in his project: "Fleck has been picked up as a pioneer, perhaps a founding father, of social studies of science. Some say he was a deeper thinker than T. S. Kuhn, who himself acknowledged his debt to the man. The styles project would have been richer had it invoked Fleck thirty years ago" (Hacking 2012:604). The key missing notion in the styles project is of course the thought collective. But this also reminds us of an aspect of Crombie's work that Hacking has neglected. In Crombie's "historical anthropology of thinking", a lot of emphasis is placed on general commitments and dispositions, which are not only cognitive, but also of a moral nature. This is not to resurrect Crombie or Fleck, but only to point to a certain limitation in Hacking's account from a sociological perspective. Hacking's interests are of course not sociological, and he may have good reasons for abandoning these notions. But for the aims of the present study, understanding epistemic stability and its relation to heterodoxy in the economics discipline must involve a conception both of social groups and their relations, as well as a fine-grained understanding of the cognitive and moral commitments and dispositions of the

[38] For a historical account drawing on the Crombie-Hacking approach to detail the multifaceted development of the various styles of reasoning, and the influence of mysticism, alchemy and the thought of Paracelsus on the development of modern experimental science and Newton, among other things, see Kwa (2011).

scientific actors involved, which could be conceived in terms of their scientific habitus, to borrow Bourdieu's concept (Bourdieu 1975, 1991; Kim 2009).

Adding a professional triangle: Brante's ontological models

If the idea of styles of reasoning draws our attention to distinct and sometimes incommensurable *ways of finding out*, I will now turn to a recent attempt to summarise thinking about how the substantial intellectual content of the sciences is structured. This is the concept of ontological models as organising matrices in the sciences, developed by Thomas Brante, which draws on the same strand of thinking about science as Kuhn, Foucault and Hacking. In this section, I will pinpoint the central tenets of Brante's concept of ontological models, and then move on to synthesising discussion of the relationship between the concepts of styles of reasoning and ontological models.

Brante's concept of ontological models is introduced as an intervention in the sociology of professions, while drawing heavily on science studies.[39] In the long running debate about the proper definition of a profession, all parties connect the concept to lengthy academic training and contact with scientific research. Opposing both the classical functionalist or "naive" view of professions as legitimately high-status social groups with important functional roles in modern society, and the later neo-Weberian "cynical" view of professions as engaged in power struggles for extending professional jurisdictions and achieving professional closure, Brante (2014 ch 3,4,6) argues that to understand their unique characteristics, we need to look at the specificity of the knowledge that professions form around. Based on a realist conception of science drawing both on Bhaskar's critical realism, but also importantly on the French epistemological tradition, Brante (2014, 163) argues that the diverse set of thinkers, including Durkheim, Bachelard, Althusser, Foucault, but also Bhaskar, Kuhn, and later interpreters as Ronald Giere, in a sense all are working as "Kantians with movable categories".[40]

[39] Brante's model of the "truth regimes" of science-based professions is well described in a 2010 article (Brante 2010b) and fully expanded in the recent monograph *Den professionella logiken. Hur vetenskap och praktik förenas i det moderna kunskapssamhället* (Brante 2014). In a review article on Ronald N. Giere's *Scientific Perspectivism*, Brante also reflects on this version of scientific realism which informs his work on ontological models (Brante 2010a).

[40] This phrase that Kuhn used to describe his own position captures a central shared idea of his paradigm theory, present in some form also in the French epistemological tradition including Bachelard, Althusser, Foucault, and Bourdieu, and the Stanford School in the philosophy of science including Ian Hacking, Nancy Cartwright, Helen Longino and Peter Gallison, the closely related perspectival realism of Ronald N. Giere, and the original critical realism of Roy Bhaskar.

There is a central argument here, directed against classical positivism (or more generally empiricism), namely that the categories of thought and reasoning are neither universal nor transparent. The question of the relationship between thought and reality must take the categories of reason into account, and (contra Kant) they have a social origin and thus subject to historical transformation, or in Kuhn's terms, "movable". In Brante's synthesising account, the movable categories of thought are described as filters between substantial scientific theories and unmediated reality, and he claims that this "third entity" should be understood as a set of ordered elements (hence *model)* that accounts for the ultimate reality of a specific science (hence *ontological*) (Brante 2014:164–65). To understand the production of scientific knowledge in any specialisation, we must therefore take their specific *ontological models* into account.

Ontological models are characterised by a number of properties or functions.[41] First, *ontological models are not theories but the foundation of theorising*. They delimit the fundamental building blocks of reality, which guide the construction of substantial theories. This means that conflicting theories built upon different ontological models may be incommensurable, since the different ontological models each provide different sets of building blocks. Second, *ontological models guide perception of reality*. Observation of reality is never unmediated, but guided by our categories. This is the central insight that Kuhn (1996) classically formulated, that it is not only a question of interpreting the observed facts differently, but that perception itself is bound to our preconceived categories of understanding. In a more mundane sense, the categories of the model determines which facts are significant and which are not. In this way, the ontological model bridges theory and observation, but in this way also blurs the neat distinction between theory and empirical data.

Third, *ontological models have relative autonomy*. Drawing on the recent surge in research on model use in science, Brante (2014:167) claims that models have a relatively autonomous existence in relation to both theories and reality. One ontological model may harbour several different substantial theories. Fourth, *ontological models bring with them specific ways of thinking in a science.* Synthesising Fleck's *Denkstil,* Douglas's *thought style,* Hacking's *style of reasoning* and Kuhn's *paradigm,* Brante claims that ontological models bring with them a superstructure of criteria of what scientific means: conceptions of good science, important problems and acceptable methods (Brante 2014:168).

[41] I deviate slightly from Brante's list of eight, highlighting those that are of relevance here, and for the sake of simplicity making the list a bit shorter.

Fifth, *the ontological model of a scientific community structures the thinking of individual scientists*. Drawing on modern cognitive scientists like Nancy Nercessian, Brante (2014:168) claims that cognition must be understood not mainly as based on pure logic and abstract principles, but to a large extent also on what is known as a *mental model.* This research draws our attention to reasoning and concept formation based on analogy, metaphor, structural similarity, for short *model-based reasoning* as a modern version of Kuhn's notion of paradigm as *exemplar*. But where cognitive science relates such model-based reasoning to the psyche of the individual, a sociology of science must understand that *models are institutionalised*, socially learned and shared (Brante 2014:169).

Sixth, *ontological models guide intervention in reality*. Ontological models do not only function in sciences with the aim of representing reality. They may also guide practice. Here, Ian Hacking's (1983) classic distinction between science as representation and intervention comes to mind. But whereas Hacking focuses on scientific intervention in nature through experimentation, Brante has a more far-reaching aim. He uses the notion of ontological models to explain how scientific knowledge functions as a guide to practice outside scientific research when employed by the professions.

This last function of ontological models as guides to practice plays a central role in Brante's conception of professions as centred around a specific ontological model acquired by academic training. In this account, the idea of an epistemological break as conceived by Bachelard and the French historical epistemology, is important. Because science does not operate with only simple empirical observations on the one hand, and theories on the other, but instead is dependent on an object of knowledge, the "third" entity Brante (2014:144) calls ontological models, leaving common sense and entering the cognitive world of science is necessary. According to this view, science is constituted by breaking with the spontaneous categories of everyday thought and replacing them with an ontological object, opening a new world of possibilities, or at least new "continents" of knowledge, in Althusser's words. Put differently, the different sciences develop their own set of concepts and theoretical terms, that include assumptions about the specific epistemic domain under study, as well as assumptions about what sort of entities exist, how they function and how they can be studied. This is the ontological model. For the individual student, it means that through formal schooling in a discipline, the categories of the ontological model become internalised and structure thinking about the epistemic domain. To the student, later scientist or professional in the field, the ontological model *becomes* the fundamental reality to be investigated. Common sense knowledge is a problem to be overcome. In the words of Bourdieu, speaking about the necessity of avoiding "spontaneous sociology": "For the sociologist, familiarity with his social universe is the epistemological obstacle *par excellence*" (Bourdieu quoted in Brante 2010b:856). Brante sums up this view concisely: "*Science investigates a preconstituted ontological model that breaks with common sense knowledge"* (Brante 2010:856; emphasis in original).

In Brante's (2014:268) account, the representing practice of a science and the intervening practice of a profession are linked with their common object of knowledge to form what he calls a "professional triangle". The shared object is constituted by the ontological model, in what Brante calls a "truth regime" in Foucault's sense. In fact, he claims that the prime model of the historical development of such a professional triangle, or truth regime, is to be found in Foucault's early works. Foucault's detailed description of how scientific discourse (the origins of modern psychiatry), professional practice (the establishment of clinics) and an object constituted by a shared ontological model is the paradigmatic illustration of this idea (Brante 2010b:848, 2014:267). Brante makes the important observation that Foucault, contrary to most current readings, may well be read as a scientific realist. I think that this reading, at least of Foucault's early works, is well in line with a scientific realism inspired by the French epistemological tradition of the sort Brante proposes.[42]

However, while being realist in assuming a mind-independent real or intransitive object of science, this conception simultaneously allows for a largely self-validating (in Hacking's terms) object of knowledge (or transitive object in Bhaskar's terms (1975a)). The model of the professional triangle as truth regime has the advantage that it offers a nice account of how this circularity works. In Brante's words, each ontological model is "supported by its own paradigm and its own theories, and is matched with its own facts" (Brante 2010b:858). The science-profession-object triangle lets us understand the division of labour of such circularity:

> The scientific corner in the triangle produces "truths" (there is no higher authority in society) about the constituted object/ontological model. In turn, the object confirms the truths when being observed and measured. Truths are also confirmed by the interventions of professional practice: if interventions do not function effectively, this can be accounted for by considering them as exceptions or can be

[42] Again, it all comes down to definitions. Hacking (2002), for example, who has taken a good deal from Foucault, and is very close to the position I am suggesting, describes himself in the following manner: ". . . I think of myself as a 'dynamic nominalist', interested in how our practices of naming interact with the things that we name–but I could equally be called a dialectical realist, preoccupied by the interaction between what there is (and what comes into being) and our conceptions of it".

> dealt with by a protective belt of *ad hoc*-hypotheses, as Imre Lakatos would have called them. The truth regime constitutes a self-supporting spiral. (Brante 2010b:865)

The proximity of Brante's and Hacking's views is clear, but there are differences in emphasis and additions. First, while Brante's focus is on purely *ontological* aspects, Hacking puts weight also on *epistemological* and methodological beliefs as part of a style. The difference is really one of emphasis rather than kind. For both authors, ways of finding out are connected to conceptions of good science, preferred ways of knowing, and to ontological assumptions. Where Hacking's emphasis is on the ways of finding out, Brante's is on the structure of ontological assumptions. Second, Brante adds the emphasis of an epistemological break as a prerequisite for entering a scientific community, while the notion of a mode of reasoning breaking with common sense is implicit in the styles approach. Third, both approaches give the structure of scientific thought relative autonomy. Styles or ontological models are both robust and may harbour different specific theories, and are relatively immune to change inflicted by real world data. Fourth, while both accounts, like Fleck and Kuhn before, emphasise the communal nature of scientific thought, Brante adds insights from modern cognitive science to anchor this social cognition in individuals with the concept of model-based reasoning. Fifth, Brante's greatest contribution is the notion of the "professional triangle" that links the structure of scientific thought, or ontological model, to the social formation of a profession and to its intervention in their joint object constituted by the model. This creates a forceful feedback triangle as a model for the mutual stabilisation of science, profession, and object.

In summary, there is a wide cross-disciplinary literature that includes variations on the styles of reasoning approach, that all hold a rather similar view of science, although there are variations in emphasis. This broad approach will inform the formulation of a general theoretical framework in the next chapter. It will also inform the search for relevant studies of economics as a science. Let us then move from general theorising in science studies to economics as a specific empirical object of investigation.

3. Previous studies of the economics discipline

It is time now to turn away from theorising about science, and instead turn towards surveying previous social studies of economics. Economics has been studied from different perspectives and disciplinary vantage points, and this selective review attempts to provide an overview over these approaches and their main findings. I take as my point of entry a piece of work by an economist: a humorous mock-ethnography of the primitive *Econ* tribe.

Econography

> The Econ tribe occupies a vast territory in the far North. Their land appears bleak and dismal to the outsider, and travelling through it makes for rough sledding; but the Econ, through a long period of adaptation, have learned to wrest a living of sorts from it. They are not without some genuine and sometimes even fierce attachment to their ancestral grounds, and their young are brought up to feel contempt for the softer living in the warmer lands of their neighbours, such as the Polscis and the Sociogs. Despite a common genetical heritage, relations with these tribes are strained—the distrust and contempt that the average Econ feels for these neighbours being heartily reciprocated by the latter—and social intercourse with them is inhibited by numerous taboos. The extreme clannishness, not to say xenophobia, of the Econ makes life among them difficult and perhaps even somewhat dangerous for the outsider. This probably accounts for the fact that the Econ have so far not been systematically studied. Information about their social structure and ways of life is fragmentary and not well validated. More research on this interesting tribe is badly needed. (Leijonhufvud 1973)

This is the opening passage of Swedish-American economist Axel Leijonhufvud's amusing and enlightening 1973 essay "Life among the Econ". Paraphrasing nineteenth century anthropological accounts of "primitive" tribes, it provides an entertaining account of the characteristics of the epistemic "Econ tribe".[43] Leijonhufvud coins the term "econography" for the study of this social group in his imaginary world. To my knowledge, no such label exists in the real world for research on the economics discipline. The social study of economics is spread among a few neighbouring disciplines, although these disciplinary appellations are themselves often blurred. There are specimens of such work in economics, economic history, science studies, history of science, and sociology. In the following, I will try to provide a brief overview of some of the most important previous work that has been done in these fields.

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[43] More than one of my informants at the top departments recommended this article to me, and I think this conveys a sense in which it is not only an excellent piece of academic humour, but humour that is "funny because it's true". Like much good humour it comments on some aspect of our world with sharp insight.

The primary field for studies of the development of the economics discipline is without doubt the history of economic thought. The institutional location of this specialisation in intellectual history has varied and been threatened in many cases. Increasingly during the last quarter century, if not longer, the history of thought has been removed from doctoral curricula in economics and from the range of covered subfields in journals (Colander 2005; Klamer and Colander 1989). The mainstream neglect, if not active dislike, of history was aptly described by the historian Mark Blaug (2001): “No history of ideas, please, we’re economists”, noting that this neglect was strongly related to a technocratic and positivist self-understanding among the mainstream, but that, on the other hand, the history of thought had seemed to become a haven for heterodox economists of other leanings and styles of mind.

For professional historians of thought, the work has often been understood as a struggle to counter the Whiggish historiography that often results with the lack of specific training. For example, John Davis claims in a review of the oeuvre of Mark Blaug that he had to make a distinct argument for a history of thought that countered the common view at the end of the twentieth century that

> entailed a particular view of the historiography of economics, specifically, a combination of two related propositions: (1) the Whig idea that science always makes progress which renders past knowledge irrelevant to current knowledge, and (2) the view that progress in economics as a science consists in analytical achievements which are by nature strongly separable from their origins and manner of development. (Davis 2013:45)

Against the first notion of rational reconstruction of steady progress, Blaug countered with the historical reconstruction and historical loss (so-called Kuhn-loss) of knowledge. Against the second notion of atomistic analytical achievements, he held up the evolutionary intellectual path dependency of economic thought: the insight “that economic knowledge at any one point in time depended crucially upon what had previously occurred along the path economists had until then pursued” (Davis 2013:46).[44]

Much history of thought, Whiggish or not, could however be thought of as more or less internalist in the simple sense that its object is often primarily to chart the intellectual developments of great economists, schools of thought and research fields, or specific concepts, unrelated to the social context of economics. This may

[44] Davis (2013) argues that Blaug made a turn in his position when faced with increasing Whiggishness. For Blaug (1975) himself had earlier adhered to the rational reconstruction view of Lakatos, against a Kuhnian view.

range from standard textbooks from a Swedish point of view (Kragh 2012; Pålsson Syll 2011) or more specialised volumes about a specifically Swedish history of economic thought (Sandelin 1991b), to internationally well-known works that are also sensitive to the emergence of a distinct heterodox economics (Landreth and Colander 2002).

Some history of economic thought is more sociological in orientation, often influenced by science studies. For example, Coats and others have studied the professionalisation of economics and the increasing influence of economists in government in international perspective (Coats 1981). Coats early on emphasised the need for historians of economic thought to understand the sociology of economics, an insight driven both by his experience of the socially grounded difference between the history of American and British economic thought, and his encounter with Kuhn (Coats 1993:4). However, he turns against the perceived relativism of the strong programme's symmetry thesis and finds that it underemphasises the central role of rationally grounded epistemic judgement in science. Drawing instead on the work of Richard Whitley, he argues that the history of thought should take sociology seriously, and understand economic science as a collective and ongoing process, focusing on the rise, development, and interaction between schools of thought and their survival capacity. Coats points to an important task in moving outside the conventional history of thought to study also the important role of "the significance and influence of the perennial dissenting tradition within and on the fringes of the professional establishment" (Coats 1992:26). In this respect he argues that Whitley goes too far in emphasising the control of the disciplinary elite and the coherence of the theoretical disciplinary core.

Other historians of economic thought have also attempted to write more social and contextual intellectual histories. Roger Backhouse has for example emphasised that writing the history of economics should also be done against the backdrop of economic and institutional history (Backhouse 2002, 1994a; Backhouse and Fontaine 2010). He has been central in the field of economic methodology, the subfield that applies philosophy and, more recently also social study of, science, to economics and epistemic and ontological questions arising in economics. In his landmark 1994 volume, Backhouse (1994b:2) claims that the subfield has recently grown, and part of the impetus is both the general and overarching influence of Kuhn's *Structure*, but also the issues increasingly raised by heterodox economists like the Austrians, post-Keynesians and Institutionalists: "For many economists associated with these schools' methodology provided a way to criticize orthodox economics. Some of this heterodox work addressed issues related to those discussed above, raising questions concerning, for example, the nature of explanations in economics".

Backhouse claims that until the 1990s, the methodology of economics was dominated by the early work of Mark Blaug. This popularised a Popperian outlook among economists, although Blaug himself turned increasingly away from his early rationalist Popperian-Lakatos framework, as noted above (Davis 2013). Backhouse, by contrast, introduced a range of other approaches, like McCloskey's (1983) rhetoric of economics approach, that employed the analysis of rhetoric from literature to study economic discourse. The book also includes a more up-to-date account of science studies, and played an important role in sparking interest in scientific realism among economic methodologists, primarily through the work of Uskali Mäki and Tony Lawson (Fullbrook 2009; Hodge 2008).

Work on heterodox economics and pluralism

With the increasing use of the label "heterodox economics" since the late 1990s, Backhouse and others with a science studies sensitivity working in the history of economic thought have emphasised the need to acknowledge, conceptualise and study heterodox economics. For example, Backhouse, David Colander, John B. Davis, Tony Lawson, Frederic Lee and others were all involved in the debates around the question of defining and understanding heterodox economics and the mainstream-heterodoxy divide, which was covered in chapter 2 (Backhouse 2000, 2004; Colander 2000; Colander et al. 2004; Davis 2006, 2008; Lawson 2006, 2012, 2013; Lee 2009).

A closely related strand of work deals with the question of pluralism in economics and, later, its relation to heterodox economics. From a historical perspective, Morgan and Rutherford (1998) and Yonay (1994) have studied the actual pluralism of US economics in the interwar period, which will be discussed in chapter 5. Calls for pluralism in economics first emerged among different heterodox economists, although without much interaction, in the 1970s, but the current discussion on pluralism took a new and more integrative turn starting with the well-circulated "Plea for a Pluralistic and Rigorous Economics" in the *American Economic Review* in 1992, by a range of well-known economists, followed by a call for pluralism by doctoral students at Cambridge in 2001 (Garnett 2011; Hodgson, Mäki, and McCloskey 1992; Sent 2003).

More recent work in this vein discusses pluralism as a concept and strategy in relation to heterodox economics, where Dobusch and Kapeller (2012) for example make a distinction between three varieties of pluralism, drawing on what they call a post-Kuhnian view of paradigms. *Selfish pluralism* among some heterodox economists amounts to a rhetorical vehicle for promoting one's preferred heterodox approach in the face of perceived monolithic orthodoxy. Second, *disinterested pluralism* is a live-and-let-live approach that allows for multiple paradigms or schools of thought to coexist without significant interaction. This, they note, is a common situation in many social sciences today. Their preferred variety is *interested pluralism*, which strives to found a new meta-paradigm of pluralism, which strives for interaction and refinement across schools of thought and particular traditions, and views pluralism and interaction across difference as a potentially scientifically productive stance. In the case of heterodox economics, this would mean constructive dialogue and search for complementarities as well as relative merits of different traditions of thought.

In a similar vein, Garnett argues, as one of many participants in the ongoing debate on pluralism in economics, against the conception proposed by Frederic Lee. Garnett (2011) claims that Lee views pluralism as just tolerance, and that his view of the mainstream-heterodoxy divide is too black and white, inspired by a Cold War logic of dividing all territory between friend and foe. This, in Garnett's view, entails that heterodox economists should only request their freedom to exist, but continue to do so as an autonomous scholarly project, cut off from mainstream economics. Like Lee, Garnett interprets pluralism in terms of academic freedom, but unlike him, he proposes a view of pluralism as an open project of freedom, the freedom of autonomous subject to learn and to critically engage with other perspectives. It includes not only the negative freedom from persecution, but also positive freedom, in the sense of resources and literacy. This is a view close to that of Dobusch and Kapeller.

Neoclassical economics and the neoliberal thought collective

A quite different line of work warns from another angle against conflating the notion of neoclassical or mainstream economics with neoliberalism. In an attempt to seriously understand neoliberalism, not as a general concept of a mode of production or phase of capitalism, but as a specific body of ideas, prominent historian of economic thought Philip Mirowski and others have recently studied the origins of what they call the neoliberal thought collective (Mirowski 2009, 2016; Mirowski and Plehwe 2009; Pilkington 2013). Mirowski and Plehwe (2009) argue against what they see as a common misconception that neoliberalism, in line with Ludwik Fleck, should be understood as a concrete thought collective with the explicit project to change political-economic-philosophical discourse in the long run, starting as a reaction in the 1940s against encroaching totalitarianism and the perceived failure of liberalism.

One of Mirowski's key points is the warning against the common conflation of neoliberalism with neoclassical economics, which he claims are two distinct bodies of thought, although with occasional historical points of merger, especially in Milton Friedman's thought.[45] A second central point is to move theoretically beyond vague concepts of neoliberalism as impersonal discourses and Foucauldian power, and instead focus on instances where a political movement on the level of social thought mobilised largescale resources (both in terms of membership by economists and other intellectuals, and in terms of capital invested in the project) and established vast think-tank networks in order to actively disseminate its worldview (Mirowski 2009, 2014). One important implication is to illustrate how the boundaries between science and politics can at times be porous, and that intellectual change and movements are sometimes the result of conscious and strategic intervention by organised interests with good financial backing. Mirowski and others have also pointed to the need for an economics of scientific knowledge, that takes insights from science studies seriously (Sent 2013). These bodies of work points to ways that both organised political and intellectual movements, and structures of funding, may have epistemic effects on scientific knowledge.

Organisational and sociological approaches

Among more sociologically-oriented work, Richard Whitley's (1986, 2000) organisational perspective has been very influential, both as a general framework for studying the organisational basis of scientific knowledge production, and economics more specifically. Whitley's framework focuses on how the organisation of science influences the production and validation of scientific knowledge. Scientists study the world not as atomistic subjects, but are mutually dependent, pursuing collective goals of new knowledge production within different systems of work organisation, which may take very different forms. According to Whitley (1986:184), the organisation of the modern academic labour market is one of the most important innovations of modern science, that by the turn of the last century led to the rise of systematic education and

[45] Key here is that while neoclassical economics imagines free markets without states, which translates to an ideal of natural state of more market and less state, Friedrich Hayek and later neoliberalism instead realises that this was precisely the failure of classical liberalism. One of Hayek's key insights, according to Mirowski (2009, 2014), is that markets must be *actively constructed* by states.

reproduction of epistemic procedures and standards between generations. It also led to the departmentalisation of science, where the department became the unit of day-to-day scientific practice, and where scientific establishments or elites came to increasingly control research and the allocation of immaterial and material recognition and resources:

> Particular scientific ideals, goals, and standards thus became entrenched in departments as separate "disciplines", and problems or issues which did not fit into such units received little attention because they were not central to these disciplines and hence would not lead to high reputations in it. (Whitley 1986:185)

In his view, economics was established at the beginning of the century as a modern work organisation, and came to develop a stronger consensus and academic standardisation, when economists become increasingly oriented towards each other in the professional community, and less towards the general educated public. Whitley writes that by the 1930s, an international reputational system, more and more dominated by North Americans, had been established, as part of the development of the autonomy of the scientific field of economics. The theoretical core of Whitley's argument consists of a typology of scientific fields along two dimensions: the degree of mutual dependence, and the degree of task uncertainty. The work organisation of different scientific fields can be categorised along these dimensions and the specific working of knowledge production better understood. Economics is characterised by a high degree of mutual dependence, that is, research typically builds in incremental steps on previous results, in a cumulative fashion. Uncertainty is very low for theoretical studies, while it is high for applied, empirical economics. This creates a potential tension in the discipline, which is solved by what Whitley calls compartmentalisation, where work in the theoretical core is given superior status, while subfields may harbour partial divergence from the analytical core, which however doesn't feed back into the disciplinary core in any significant way. The social reproduction of this disciplinary core, he argues, is highly reminiscent of Kuhn's notion of normal science:

> Indeed, economics training manifests many characteristics of Kuhn's (1977) account of training in "normal" paradigm-bound science: as "a dogmatic initiation in a pre-established tradition that the student is not equipped to evaluate", it develops a capacity to solve analytical problems in the prescribed manner with standardized techniques and formalisms. As a result, economists share common analytical skills, a standardized symbol system for communicating the results of analytical research, a strong consciousness of the boundaries of economics and of

> appropriate ways of formulating intellectual problems in the field, and an overwhelming commitment to theoretical goals and priorities since none of the skills they have acquired deal with empirical research or the problems of turning data into information. (Whitley 1986:193)

More recently, Marion Fourcade's (2009) *Economists and Societies*, presents a detailed comparative study of how the international academic discipline of economics have taken different forms in the United States, Britain and France, in relation to the institutional structure of these three different societies. What "economics" has come to mean varies between these national contexts, where economists and economic knowledge is entangled in institutions and culture in different constellations. A more recent study by Fourcade and co-authors (Fourcade et al. 2015) investigates the "Superiority of Economists" as both an objective and a subjective fact. It relates this superiority to, among other things, the pronounced internal control and hierarchy within the discipline, connected to a centralised and hierarchical conception of top journal rankings and interest in the elite of the profession. Using a range of data, from bibliometric data on cross-disciplinary citation data to membership structures of professional organisations, the authors show that "economics more than the other fields looks both inward and toward the top of its internal hierarchy" (Fourcade et al. 2015:96). This is the result of both greater intellectual consensus and homogenised training, and of a higher degree of internal control. It is also connected to a disciplinary understanding of hierarchy. Economists, they argue, "tend to see institutionalized hierarchies as emergent, truthful indicators of some underlying worth, and consequently are obsessed with them. For instance, in no other social science can one find the extraordinary volume of data and research about rankings (of journals, departments, and individuals) that economists produce", whereas others might react to the existence of hierarchies by pointing to alternative metrics and multiple criteria of worth (Fourcade et al. 2015:98).

In a similar manner, this inclination has been studied as an *elitism dispositif* by Jens Maesse (2017) in relation to transformation of economics institutions to modern US-style elite departments in the United Kingdom and the German-speaking world. The *elitism dispositif* is founded upon the interaction of symbolic order of rankings in economics, and the transformation of research organisation guided by these ideals, for example the recruitment of staff selected based on top journal publications, and the establishment of modern US-style doctoral programmes, teaching new generations how to reason like the professional elite, and where the current research front lies. Thus, the symbolic order of ranking systems is not only transformed into differential funding and allocation of resources and symbolic prestige, but also reproduces a specific research orientation. However, Maesse argues that economics in the studied areas is not homogenous. Rather, there is an internationally and top journal-oriented economics, pursued by elite economists at elite departments, and another economics at other, lower-tier, departments, pursuing somewhat different goals.

There is a final strand of sociological literature that is worth mentioning. This is work that engages not with the production, but with the circulation, use and impact of economics knowledge. Steiner (2001) shows how economic knowledge and economic literacy is an important part of modern societies and economic behaviour. He creates a typology between different forms of economic knowledge, and argues that the formal economic theory of academic economics, or rational economic knowledge is but one of several types of economic knowledge, and that this domain of values that guide economic action has been and continues to be a domain where economic sociology has an important role to play. Focusing on economic expertise, Reay (2012) draws on an extensive interview study with economists working in different contexts, and presents a solution to the apparent paradox that economists are often claimed to simultaneously have a powerful and unified pro-market way of thinking, and on the other hand to be fragmented and come to varying conclusions in practical applications and weak influence. On the one hand, Reay argues that:

> economics is unified by a cognitive/cultural frame that both academics and nonacademics try to transmit to lay people and get established as part of institutional routines. This frame is not based on promoting markets per se but on a more general “core” of intuitions and techniques concerning quantitative empiricism, macrolevel connections, and microlevel responses to incentives. (Reay 2012:47)

On the other hand, the actual application of this core framing is very flexible and context-dependent, and thus the apparent paradox is dissolved.

In a similar vein, Hirschman and Berman (2014) discuss the conditions under which economic knowledge influences economic policy. Through a thorough review of literatures in sociology, political science and STS, they argue that we should not ask how economic theory influences economic policy, but rather, under what specific conditions and what form such influence takes. Synthesising the findings from these literatures, they identify three modes through which economics may have policy influence. First, the historically variable authority of the economics discipline grants economists *professional authority*, which has tended to increase with the post-war formalisation of economics. This authority is a condition for a wide range of forms of influence and interventions in policy processes by economists. Second, economists influence policy through their *institutional position* inside organisations that affect or even produce policy, ranging from elite networks via national to international institutions peopled by economists, like the IMF and the World Bank. The influence of institutional position is strongest when economists are directly involved as policy makers. For example, they argue that:

> monetary policy in much of the world works this way: economists are appointed to head central banks which are in turn staffed by economists, and these central banks have wide latitude to set the course of monetary policy based on accepted monetary theories. Economists trained at prestigious institutions have increasingly taken lead positions in ministries of finance as well. (Hirschman and Berman 2014:781)

The third identified mode of influence is the most interesting for the purpose of this study. It is what they call *cognitive infrastructure*. Here, focus is shifted from individual economists and their institutional roles to the way that economics structures thought and practice. This may take two forms. Hirschman and Berman argue, first, drawing on Hacking's work, that the influence of economics could be thought of in terms of a *style of reasoning*, much like Reay's "core", but that this influence is broader, and does not solely work through economist experts.

> As people trained in economics, whether at the undergraduate or graduate level, take jobs in think tanks, policy-focused research institutes, and government itself, their way of thinking will subtly shape policy. The professional authority of the discipline may also lead policymakers to perceive the economic style of reasoning as superior to other forms of knowledge. (Hirschman and Berman 2014:795)

Second, cognitive infrastructure can also crystallise into what they call *policy devices,* sociotechnical devices that draw people, knowledge and material things and practices together in durable and formal ways that allows for the ordered production of knowledge and policy. A prime example here is the production of gross domestic product (GDP) measures, which, while often criticised, relies on an internationally standardised procedure of data collection, engaging institutions and university economics, feeding into research and policy alike (Hirschman and Berman 2014:798). Although the construction of such data is of course contingent and could have been constructed otherwise, such policy devices and the numbers they produce are very hard to change. For the purposes of the present study, their work points to the fruitfulness of the styles approach, and the way it may be used to connect the production of knowledge with studies of its circulation and use in society.

The most influential body of work in this vein, however, is the so-called performativity of economics literature, initiated by Michel Callon, Donald MacKenzie and others (Callon 1998, 2007; MacKenzie, Muniesa, and Siu 2007). This work argues, drawing on a notion of social performativity, that economic theory is performative in the sense that it is itself an active part in the creation or alteration of its object. This idea draws on Austin's ordinary language philosophy and its distinction between *constative* utterances that represent a certain state of affairs ("the cat is on the mat", or "in the prisoner's dilemma rational agents choose suboptimal configurations") and the *performative* use of language, where actors pragmatically attempt to achieve something and engage in the world ("I promise you", or "I sentence you to ten years' imprisonment") (Callon 2007:317).

Drawing heavily on the actor-network tradition in science studies, this literature investigates how economics is performed by entangled networks of actors and devices that allow economic agencies and object to be performed in a stable way. For example, MacKenzie and Millo (2003) have shown how options pricing theory, far from being a description of some pre-existing function of financial markets, became performed and, so to speak, became real as the Black-Scholes-Merton theory of options pricing was incorporated in pricing and calculations in the financial market at the Chicago Board Options Exchange. However, this literature has also been criticised for the way it seems to align science studies with neoclassical economics and the commercialisations of research (Mirowski and Nik-Khah 2007).

I have attempted to highlight some of the bodies of research that study economics from different angles. This has been a selective review, with the aim of showing that the social study of economics may take very different forms, and to create a map for orientation in this field. The theoretical framework developed in the next chapter will draw on the insights in this review. I now turn to a more in-depth discussion of the work of Mary S. Morgan, who has used the styles of reasoning approach in her study of how modelling is used in modern economics.

4. Mary S. Morgan and modelling as a style of reasoning in economics

When I synthesised the debates on the nature of modern economics in chapter 2, I found that one of the characteristics of modern mainstream economics is its mathematical or formal modelling approach to problems. The use of models in science has attracted increasing attention among historians and philosophers of science during the last decades. Mary S. Morgan has thoroughly investigated the use of models throughout the history of economics, most notably in her recent monograph *The World in the Model: How Economists Work and Think* (2012). She has also summarised her studies of modelling in shorter pieces (Morgan 2008; Morgan and Knuuttila 2012). Her studies shows not only how the theoretical objects of economists have been transformed, in the vein of intellectual history, but focuses on how modelling has become a generally acknowledged and shared tool to reason with, that has helped economists to ask questions and find answers. Using historical case studies in which she reconstructs the knowledge-production practices of economists, Morgan traces the development of the full-blown modelling approach of modern economics.

What is a *model*? According to Morgan, a model may be a physical object, like a miniature toy aircraft. Physical models may be used in the sciences to represent some aspect of the world; think for example of anatomical models of the human body used for teaching. More often, models are what Morgan calls pen-and-paper objects: think of the basic supply and demand curves taught to every economics undergraduate. As models, they share certain features:

> Despite their variation in form, these objects share recognisable characteristics: each depicts, renders, denotes, or in some way provides, some kind of representation of ideas about some aspect of the economy. Yet, and this is very important point to stress, these representations are not just pictures. [. . .] For economists it is the possibility to reason with the different kinds of representations [. . .] that makes them all into economic models. (Morgan 2012:13)

For models to be tools of reasoning, they must first be *manipulable*. As opposed to an image, which is also a representation of some aspect of the world, a model can be altered in some of its parameters, so that the modeller can investigate what happens if this or that variable is manipulated. Second, models must be small enough so that their manipulation is manageable. But what does “small” mean here? Since most economic models are pen-and-paper or computerised models, physical scale isn’t the issue. “Small enough” rather refers to limited complexity and number of variables included, so that the modeller may work with a model in a feasible way.

To make the concept of a model more concrete, let me exemplify. An important milestone in the prehistory of modern modelling is the French eighteenth century economist François Quesnay’s famous *Tableau Economique*. This early model in the form of a printed table is a simple form of model, Morgan argues, since it allowed Quesnay to reason about flows of agricultural surplus between the three main economic classes. The construction and interpretation of the *Tableau,* which is not at all obvious to either our nor Quesnay's contemporary economists, points to the importance of understanding models as objects created with some degree of imagination and creativity, that also require training to understand and use (Morgan 2012:5). A similar sort of primitive model is found in Ricardo's tables of farm accounting, created to reason about income distribution in the agricultural economy. Similar in style also are Marx's schemes of reproduction in volume II of *Capital* (Marx 1978; see also Reuten 1999), inspired by both Ricardo and Quesnay.

These early models belong to the prehistory of modern economic modelling. Before the turn of the century, economists did not talk about "models", even if they become increasingly used at the time of the marginal revolution of the 1870s. They were often described as "tools", or "representative particular", and could range from simple pen-and-paper diagrams exploring particular problems, for example Alfred Marshall's diagrams of trade relations between two countries at shifting terms of trade. A completely different example from the early twentieth century is the model that US economist Irving Fisher designed and built in the form of a physical hydraulic model "to represent, explore, and so to understand the workings of a mini-economy, one with only three goods and three consumers" (Morgan 2012:8, and the hydraulic macroeconomic model known as the Philips-Newlyn Machine, with coloured water used to represent monetary flows. If the classical economists prior to the marginal revolution had mainly reasoned with words, with some exceptional cases of models being used to supplement the verbal arguments, they became increasingly common, and by 1945, "modelling" became commonplace.

The word "model" became stabilised as the name for such objects in the 1930s, when an understanding of the possibilities of modelling as a mode of enquiry really took off, according to Morgan (2012:10). Two persons were central in this development, the Norwegian economist Ragnar Frisch, and the Dutch economist Jan Tinbergen. In 1933, during the Great Depression, Frisch constructed a mathematical model built on a number of equations that could generate a cyclical pattern, thus founding the modelling of business cycles. In 1936, Tinbergen expanded Frisch's insights to a model of an entire economy. With this model object built from a system of equations incorporating a theory of the business cycle, as well as statistical data as input to the parameters of the equations, Tinbergen could study ways to get the Netherlands out of the crisis. By the end of the Second World War, everyone talked about "models" when they referred to the different diagrammatic, mathematical or material objects used to reason with in economics (Morgan 2012:12). When models were everywhere, they also brought with them their own particular way of reasoning. Morgan (2012:14) emphasises that this meant that a new epistemic genre became the preferred way of doing science, so that it was no longer *a* way of reasoning, but had become *the* way of reasoning in economics: "In other words, disciplinary arguments at all levels of economics came to hinge not just on the objects—models, but on economists' abilities to reason with them—modelling". This prevalence of the model and modelling terminology also led to the current situation where "theory" is hardly used as a term among economists, and when it is, it is often used interchangeably with the term "model" (Morgan 2012:14 n. 16).

This new way of reasoning that became dominant in economics during the second half of the nineteenth century can be understood in terms of a distinct scientific *style of reasoning*. In her historical analysis, Morgan adopts the Crombie-Hacking notion of six distinct styles of reasoning, discussed earlier (Hacking 1992; Morgan 2012:16). Modelling as a distinct style in modern economics developed in tandem with two other Crombian styles of reasoning. Modelling arose together with mathematical postulation and proof in the late nineteenth century, and in the interwar years the *statistical* style of reasoning begun to enter economics in the form of econometrics. The increasing use of mathematics happened within both the *postulation and proof* and the *modelling* style at the same time (Morgan 2012:18). These Crombian styles have been deeply entangled in modern economics, although modelling plays a primary role. But in actual practice, Morgan acknowledges, it may be hard to disentangle the different styles.

Models as working objects

Models are not just the products of reasoning, according to Morgan, but play an essential role as *tools* of reasoning, as working objects in the production of knowledge. Economists have thought about the use of models in different ways, describing it as recipe-making, visualisation, representation or analogy. Visualisation focuses on the creative and intuitive aspects involved, in other words, *imagination* is needed to *make an image* of economic ideas. Idealisation on the other hand points to the abstraction of specific relations of interest from the real world to study them in isolation in the representative model world. This understanding of model constructions seems to have been employed chiefly by philosophers of science, for example, Morgan notices Uskali Mäki's use of the term "isolation" and Nancy Cartwright's "causal idealisation" in relation to modelling.

However, there is also the view of modelling as analogy in economics, where a model is not thought of as a *representation* of the world, but rather as a little miniature world to play with and explore in itself.[46] For example, the macroeconomist Robert Lucas has called his business cycle model "a mechanical, imitation economy", and Robert Sugden has argued that his models should be seen as a "credible world", where credibility is the similarity in outcome to the real world phenomena (both quoted in Morgan 2012:24). Commenting this view of modelling, Morgan writes:

> In seeking to capture not the workings of real economies but to mimic some aspect of it via an imagined analogous world, these practices of design take us back to one of the historical roots of modelling in the arts where craftsmen built mechanical birds that would "sing" but did not suppose that birds were mechanical automata. (Morgan 2012:24)

Nevertheless, Morgan concludes that all different views of model construction entail that "scientists form some kind of a representation of something in the economy", even if this is variously understood, and that the "important point here is that whatever term is used should not unduly limit our understanding of what models are and how models work as a means of enquiry" (2012:24–25).

If the construction of a model means creating an object to manipulate and inquire with, then this object must also obey some form of rules. Models are formal in the sense that they are rule-bound, and since "in each particular case, these rules form the rules of reasoning with that model, they effectively determine the economist's valid manipulation or use of that model" (Morgan 2012:26). Morgan makes a distinction between two types of rules, the *formal* and *economic*. The former are given by the stuff that the model is made of, be it hydraulics, mechanics or algebra. For example, a mathematically-formulated model must naturally obey the manipulation rules of algebra. Economic rules are the rules of economic behaviour imposed by the model constructor, like when "the reasoning in the Prisoner's Dilemma model is determined by the economists' view of how the economic model man will act in the world of the model" (Morgan 2012:26). Importantly, such economic rules are formed by the economist's concepts of the economic relations under study, which are not the same thing as unmediated representation of the real world economy. Taken together, "these two different sources of rules—from a model's format and from its subject content—determine and limit how each particular model can be used, and so, *constitute the kinds of*

[46] Morgan (2012:25 n.33) notes that the issue of *representation* is a topic of a hot debate among philosophers for the sort of issues it raises. She is herself agnostic in relation to this issue, instead taking a naturalist approach, studying "how scientists use models" rather than a philosophical analysis of them.

right reasoning that are possible with that particular model" (Morgan 2012:27; emphasis in original). In practice however, it is often no longer possible to disentangle these two separate sources in contemporary economics, because it has "reached the point where [. . .] the concepts and arguments of economics are so thoroughly intertwined with, and even drenched in, the terms of their habitual mathematical expression that they can no longer be pulled apart" (Morgan 2012:27).

The use of models as means of reasoning in economics comes in two forms, according to Morgan. There seems to be a central tension between using models as "*objects to enquire into" and "objects to enquire with*" (Morgan 2012:31; emphasis in original). A model as an object to enquire *into* is the sense in which economists creates a small miniature world to "explore their theories and intuitions" in a way that would not be possible without the model as an autonomous object (Morgan 2012:32). Even though a model is an artefact, it is an artefact that helps its creator to reason and to understand the implications of the assumptions built into the model. For example, merging a few known relationships in a model means that the outcome of different combinations can be studied through tinkering with the model in a way that would not be possible as a pure operation carried out in thought. Therefore, there is a significant sense in which a model must be understood as an autonomous object which is put to use as a means of reasoning. The autonomy of the model also means that models can "kick back", although in a limited sense. Even if the modeller constructed the model and knows its assumptions, the model can nevertheless return *surprising* results that were neither known nor anticipated by the modeller. But if this can happen with the model as an object of investigation in itself, what about the model as an object to enquire *with*?

As an object to enquire *with*, models "also serves as an object to investigate the aspect of real people or real world that it is taken to represent."(Morgan 2012:32). This second aspect of model use is about inference from model to world, where the former is used to perform experiments to gain knowledge about the real world which it represents. This, of course, brings with it big philosophical issues about the nature of representations, and of treating elements abstracted from the real world in isolation in the model (Morgan 2012:32 see also p.25, n.33). When models are used to enquire with, that is, when inference from model to the real world is at stake, there is a similarity to experimentation. Morgan points to the fact that economists' inference arguments are of an informal kind:

> When economists talk of "testing their models" (having assured themselves of their internal mathematical qualities and coherence) they are interested in judging the usefulness of their model experiments by comparing the behaviour of the model

> world to that of the real world in a kind of matching or bench-marking process. They may compare the model experimental behaviour of their thin model of economic man with the behaviour of real people, or surmise how a particular policy change instituted in a model compares with the equivalent actual policy in the world. (Morgan 2012:34)

However, she also reminds us that the situation is essentially the same in the experimental laboratory sciences, where scientists' trained judgement rather than formal decision rules must be employed in interpretation of results and their possible external validity. But there are also limits to the similarity with experimentation. Models and the world they represent are, after all, made of different stuff, and a representation is not identical to that which it represents. This means that the "kicking back" mentioned above is of a different kind: "While model experiments may *surprise* the economist with unexpected results, laboratory experiments may *confound* the economist-scientist by producing results that are not only unexpected but potentially unexplainable given existing knowledge" (Morgan 2012:34; emphasis in original).

In Morgan's account then, models are primarily objects that guide the economist's reasoning. They are what she calls *working objects*.[47] To be useful, they must be manageable, as we have already noted, and communal, that is, standardised in a way that they may be shared, understood and validated by the specific scientific community. Certain models in economics have become standard stock and their symbolic expression have become stabilised so that everyone trained in the use of the model can readily adopt it for their own specific purposes. Making models manageable means making them sufficiently "small", which requires omissions of detail. This is of course a delicate question intertwined with the philosophical problem of representation. The essential point she makes is that "like map-makers, those creating economic models must pick out what they take to be salient points of the economy so that their representations not only remain manageable but also focus on the elements and their relationships that are of particular interest to them" (Morgan 2012:383). But how do we know

[47] The term "working objects" was introduced by Lorraine Daston and Peter Galison (1992) in their historical study of how objectivity was achieved with the aid of different visual devices during the development of modern natural sciences. The idea of working objects captures the way that each science relies on a specific form of standardised object, whether these are found natural objects (like type specimens of animals) or artefacts (like atlas images), or halfway between (like modern genetically standardised model organisms like mice or *Drosophila* flies used in laboratories), because unmediated and unrefined nature is "too quirkily particular" (Daston and Galison 1992:380; Morgan 2012:380).

that there are not too many omissions, or that the small model worlds are not too simplified as representations? Morgan acknowledges this common form of unease:

> To an outsider coming to the field of economics, one of the most striking things is the way that economists feel that they can express so much of what happens in the economy within their small worlds, within these little chunks of mathematics or puzzling diagrams. Don't they seem much too small? [. . .] Even some inside the field question whether models are a valid way of doing economic science because of this combination of scale reduction, simplification (to omit things), and transposition into mathematical and diagrammatical forms. (Morgan 2012:384)

Morgan does not engage with critics of the modelling approach, like Lawson and others that were discussed in chapter 2. Instead, she points to the usefulness, to economists, of the modelling style of reasoning. Understanding the function of modelling means that we must be aware of any simplistic critique that just focuses on omission and simplification as problems in themselves. Models are means of reasoning that *may* help us to gain knowledge of a target world even if the model is very simple. "So, yes of course, economic models don't capture all the detail of the world in their mathematical languages. They are simplified, but that does not mean that what is expressed is necessarily simplistic or silly, or even simple to understand—though individual models might well be all those" (Morgan 2012:386). This leaves us instead with the question of in what ways modelling and the simplification entailed in modelling affect the knowledge produced.

The flexible glue of the modelling community

The modelling approach has created what Morgan calls a patchwork of apparently separate models in modern economics. But this patchwork is stitched together, and unites economists. Morgan argues that the force binding it all together is made up of "those two general assumptions that modern economists came to share and use: the *individual utility maximization* of economic man [. . .] and the *equilibrium tendency*" (Morgan 2012:394; emphasis in original). These assumptions function as formal rules, giving stability to the models. She talks of them as the "two assumptions of neoclassical economics", and argues that they function as very powerful modelling rules. The strength of these twin assumptions lies in their general and omni-present character. An important aspect of understanding the role of the assumptions in modelling is their dual role as simultaneously substantial economic assumptions that provide motivations and constraints, and as mathematical formal rules in the models.

However, Morgan also notices that while at least one of these rule-assumptions is present in all models, both are not necessarily present in every model. If the thread that stitches economic models together is made of these two assumptions, it still forms a quite loose network of models with a lot of glitches and far from universal coverage. For example, Morgan (2012:395 n.19) points to the yearning for so-called micro-foundations of macroeconomic models as an example of an attempt to stitch models in different domains closer together. Here, Morgan's two assumptions take us back to what was found in chapter 2 to be a general consensus on ontological assumptions as one of the core features of mainstream neoclassical economics. Thus, while Morgan provides an alternative account that points to the role of *ontological* assumptions (here, utility maximisation and equilibrium) as one of the defining features of mainstream orthodoxy, she also helps us understand how these axioms relate to what I called the *epistemological* reliance on modelling and the patchwork of models as the working objects of modern economics. Thus, the ontological and the epistemological aspects are both part and parcel of the dominant style of reasoning.

Ontological assumptions and epistemological orientation were two of the three central features identified in the various accounts of mainstream economics in chapter 2. The third was what I termed the social aspect. The social aspect of modelling practices for the economics community is also highlighted by Morgan:

> We know that during the twentieth century, modelling became *the way* to do economics. The term "model" changed from being a noun to being a verb as economists adopted a new way of reasoning and of finding out about the world. "To model" and "modelling" became understood, used, and accepted as the way to reason properly in the field. (Morgan 2012:399; emphasis in original)

Here we approach the social aspect of what it means for a style of reasoning to become *the way* to reason in a field. First, modelling is of course a craft that requires professional training and experience, and "this shared practice of craft work—as for any other mode of doing science and in any other scientific community—operates as a flexible methodological glue for doing that science in a particular way. If it comes to be thought to be 'the right way' to do that science, it becomes a community commitment" (Morgan 2012:399). More importantly, the reproduction of the profession is not only a matter of facilitation, but also of control:

> Since the acceptance of a new way of doing science is a community matter, it depended on disciplinary training, norms, and purposes that reinforced, but also constrained or even policed, professional practices [. . .]. So once modelling became

> the way to do economics, the way to reason rightly, the approach itself *created a professional commitment that became very hard to break out of, or indeed for a new way of doing economics to break into.* (Morgan 2012:399–400; my emphasis)

Here Morgan's view of modelling practices as a "flexible glue" harmonises clearly with the accounts of the methodological imperative of modern mainstream economics surveyed in chapter 2.

One of the fundamental effects of the modelling revolution in Morgan's account is how the new way to reason rightly led to a new understanding of the economic world:

> Economists came to understand—in the sense of both *perceive* and *recognise*—their economic world in terms of their models, and by working with such objects, they came to see the world differently than before. This cognitive and perceptual shift is a necessary precursor to acting with such models in the economy, and to the extent that these actions change the world for us all, their new ways of world making make new worlds for us all to live in. (Morgan 2012:405; emphasis in original)

The crucial transformation that Morgan describes is from an early situation where models are constructed as accounts of some aspect of the world. These models then function as working objects that allowed economists to discover new things that were previously hidden from view. As time goes by, "these newfound things become so familiar that the model moves from being the lens that enables economics to interpret the world in this new way, to being the things they find and see in the world" (Morgan 2012:406). Such a shift has taken place in the minds of economists through the modelling revolution, and it is not innocent:

> Moving to a mathematical or diagrammatic way of describing the world and of reasoning with it is not just a change in the mode of representation for them, nor even just an historical change in the way of world-making and shaping, but it *naturalizes* what they see: what they recognise and understand in the world. Economists came to see the economy differently after they had learnt to represent it in models, to express claims about it, and reason about it in terms of those models. [. . .] It is these changes in the representations of many particular bits of the world taken together that—for economists—*led to a broader creation of a whole new way of looking and seeing that involved depicting, understanding, and theorizing everything in the economy in terms of their models.* This is why *models and modelling involve changes in imagination, perception, and cognition* for economists of a kind

> that parallels the effects of radical changes in other fields of representation. (Morgan 2012:406–7; my emphasis)

Here, we find a fine instance of what could be called a Kuhnian theme of theory-laden perception that is also fundamental to the styles approach. But I think that when Morgan talks about perceiving *and recognising*, she also points to something beyond Kuhn's scope. When she not only speaks about economists *perceiving* the world in a way that is structured by their models (a Kuhnian notion), but also that it is an act of *recognition*—that it "naturalizes what they see" —she points to the reification or naturalisation of a world view, the process whereby a specific outlook comes to be taken for granted, recognised as familiar and seen as the only possible way, indeed the *natural* way, to view the world. This insight is close to much of Hacking's work and to Brante's idea of the professional triangle discussed above.

In summary, Morgan's sociohistorical epistemology of modelling in modern economics is a fruitful approach that addresses the nature of modern mainstream economics, conceived in terms of the framework of the modelling style of reasoning, entangled with the mathematical and statistical style. Her account adds an understanding of how the epistemological practice of producing knowledge with models, semi-autonomous working objects that guide and enhance reasoning is used by economists, and how the patchwork of models is stabilised with the flexible glue of two simple but powerful ontological assumptions of utility maximisation and equilibrium. She furthermore connects this style of reasoning to its institutionalised social foundation, when the style of reasoning is understood as a *collective* cognitive outlook of a bounded professional thought collective.

However, while Morgan fruitfully illustrates how the styles framework can illuminate knowledge production in modern economics, the drawback of her account is that she treats the discipline in a homogenising manner. Morgan's interest is not in charting conflicts within the field of economics, nor studying dissenting views or defining the mainstream. Nevertheless, she sometimes talks about "neoclassical economics", as in the quote above, as an unproblematic description of modern economics. While her focus is not on economic heterodoxy, her treatment of the discipline as homogenous, in the sense that she equates modern economics with neoclassical economics, tends to neglect and remove the internal disciplinary mainstream-heterodoxy tension from theoretical sight. Furthermore, her immediate interest is not in studying the mechanisms by which the thought collective and its style of reasoning is reproduced. Her account is an excellent case study in historical epistemology, rather than sociology, and while it is insightful about the social foundation of the economist's style of reasoning, it points to the need to study the social mechanisms behind the reproduction of styles. One such central mechanism, scientific quality judgement, is the topic of the next section.

5. The sociology of valuation and quality judgement in science

I have now discussed the literature on styles of reasoning and its application to economics extensively. I have also discussed Bourdieu's notion of a scientific habitus as a locus for the reproduction of styles by individual actors in scientific fields. However, if styles are reproduced by social actors, they are not just passive dopes, repeating a learned pattern of behaviour. The commitments and dispositions of scientists are reproduced through social practices, and a central such practice is the activation of the embodied dispositions of the habitus in quality judgement, above all in formal peer review processes. This section provides a review of a broad literature on the evaluation of quality in various scientific peer review processes. It starts with a review of selected studies on peer review processes and quality judgements, and then turns towards a more general literature that considers valuation, evaluation, as general social phenomena. Finally, it briefly turns to recent work on the epistemic impact of bibliometric indicators on science, and to applied work using expert evaluation reports from academic hiring and promotion as empirical material.

The judgement and evaluation of scientific quality in peer review processes forms the backbone of modern science. Quality evaluation takes place in settings at different levels of the academic system. At the macrolevel, we find largescale evaluations of national research systems, research policy and scientific areas. Second, the distribution of research grants from public or private research funding agencies evaluates large long-term interdisciplinary collaborative projects as well as smaller grants to individual scholars within single disciplines or research fields. Third, in processes of academic hiring and promotion, expert evaluations of the merits of applicants rank individuals and provide the foundation for fair and meritocratic processes. Fourth, and increasingly important as bibliometric measures come to play a greater role, every day microlevel quality judgements are made about scientific articles by journal reviewers and editors. To this we could also add the everyday personal evaluation of papers by researchers—evaluations that generate citations when found relevant and interesting.

Although the setting and the object of evaluation varies, the principles of quality evaluation are similar. Peer experts in the scientific field (or, in interdisciplinary settings, from another scientific field) use their trained judgement, acquired through training and practice, to evaluate the quality of the object in question using a set of implicit or explicit criteria. The outcome of the process affects distribution of academic recognition and resources. Studies of these processes use a range of approaches and focus on different aspects. In the following, I review the relevant literature, divided into five parts.

First is a range of Swedish studies on scientific quality judgement mainly from the mid 1980s to the 1990s, some of which use expert evaluation reports. This literature focuses mainly on explicating the quality criteria used by experts. Second, a number of writers in the Swedish context since the early 2000s adopt a primarily Bourdieusian perspective, with a focus on the conservative effect of a system that perpetuates its *doxa* through quality judgements that rely on experts' professional habitus. Third, in the literature that one could perhaps call post-Bourdieusian, inspired both by authors like Michele Lamont, but also to a significant degree by the sociology of science, the focus shifts from quality criteria and the system-preserving effects of peer evaluation to the complex *practices* of quality judgement as a process where notions of quality are applied and negotiated. In the fourth part, I briefly widen the perspective with some recent works that discuss the sociology of valuation and evaluation as more general social phenomena, to show that the principles behind evaluation of scientific quality is an instance of a wider class of phenomena. The fifth and final part discusses recent literature in the intersection between bibliometrics and science studies focused on the epistemic impact of quantitative bibliometric indicators, and their use as judgement devices in research evaluation practices. The review will not cover the large literatures on, for example, quantitative studies of research quality evaluation or gender bias, but will be limited to aspects that are relevant for the present chapter, including the use of expert evaluation reports (*sakkunnigutlåtanden*), the conservative effect of habitus in quality judgement, the practice of applying quality criteria in evaluation, and the use of bibliometrics as judgement devices.

Pioneering studies of quality criteria

Pioneering the Swedish studies on scientific quality judgements were a group of psychologists in Gothenburg (Hemlin, Montgomery and Johansson), that conducted a first interview study (published in Swedish in 1985) on conceptions of scientific quality, and a later similar study using peer expert evaluations (Hemlin and Montgomery 1990, 1993; Nilsson 2009:141). The interview study (Hemlin and Montgomery 1990) first extracted a set of quality criteria from the scientific literature, and then investigated how these were actually understood through interviews with professors from different faculties. The quality criteria were divided into six *aspects* of the research to be evaluated (problem, method, theory, results, reasoning, and writing style) in terms of eight different *attributes* (correctness, novelty, stringency, intra-scientific effects, extra-scientific effects, utility in general, breadth, and competence). While the interview study mainly emphasises the generality of quality conceptions, in terms of "consistency in views from different disciplines", the authors also note a marked difference in how "theory" is differently emphasised at different faculties (Hemlin and Montgomery 1990:80).

In their subsequent study of peer evaluation reports, 31 evaluation reports of professorship candidates at five faculties were coded according to four categories (including the already mentioned aspects and attributes) in a semi-quantitative analysis. The authors maintain in this study that scientists across faculties use "approximately the same conceptual system" in evaluating scientific quality, but that the "stress laid on particular components . . . may vary across disciplines" (Hemlin and Montgomery 1993:20). According to the authors there seems to be an evident distinction between what they call "soft" and "hard" sciences, where evaluators in the former write longer evaluations, concentrate more on individual publications, and emphasise the theory aspect to a greater extent. This could be interpreted, they argue, as the difference between Kuhnian normal science ("hard") versus pre-paradigmatic ("soft") sciences, or between Whitley's "restricted" versus "configurational" sciences. "Restricted" sciences in this sense share theoretical ideals and conceptual assumptions, and use mathematical formalism to a greater extent. The same general approach to studying scientific quality is also presented in a more recent article by Hemlin (2009).

The public availability in Sweden of expert evaluation reports from academic hiring and promotion *(sakkunnigutlåtanden)* has also been exploited by historians of ideas as a rich source of primary material. A good recent example is Rangnar Nilsson's recent doctoral dissertation (2009). Her study uses *sakkunnigutlåtanden* to study conceptions of good science and how it has changed during the post-war years in three disciplines: literature, political science and physics. Drawing largely on the methodological framework of Montgomery and Hemlin, Nilsson (2009:50) shifts the focus from the universality of quality criteria to their differentiation over time and between disciplines. She notes that the conceptions of good science and quality criteria are normally not fully explicit, but the implicit must be explicated in writing evaluation reports as reviewers need to form arguments and use them as rhetorical devices. Since the expert reviewers are senior scholars chosen as legitimate authorities within their fields, they are representatives of their disciplines, and their reasoning must take the disciplinary audience into account in the choice of relevant and reasonable arguments. Thus, evaluation reports provide an excellent window into the conception of science at work in a particular time and discipline. Nilsson's study is methodologically thorough and provides qualitative depth, and her main contribution is to show

how the supposedly universal criteria found by Hemlin and Montgomery turn out to be applied with variations with respect to time and discipline. However, the common focus of all authors here is the explicated quality criteria found in evaluation reports or in interviews, and the problems of the process of evaluation itself falls outside the scope of this literature.

Old boy-ism, habitus and potential conservatism in peer review

A second set of studies is driven by a strong Bourdieusian theoretical influence, and a focus on the potential system-conserving properties of evaluation systems relying on peer judgement. Since the first studies of scientific evaluation in the 1970s, a central question has been whether the peer review system actually functions according to the universalist norm codified by Merton (1973), or if there is anything to the frequent suspicions of "old boy-ism", the nepotist bias towards members of one's own social network (Gemzöe 2010). A central aspect here has long been gender, and the question of the existence and extent of discrimination against women in academia, which has been shown to exist in a range of studies (for reviews, see Gemzöe 2010; Mark 2003). One effect is an interest in the study of scientific quality motivated from a gender perspective. For example, a series of reports commissioned by Gothenburg University in 2003 investigated gender equality in academic recruitment practices using different approaches.

One of these reports used discourse analysis to illuminate the gendered language used in *sakkunnigutlåtanden* (Gunnarsdotter Grönberg 2003), and the philosopher Eva Mark (2003) has provided a conceptual analysis of the evaluation and recruitment process relying strongly on the conceptual framework of Bourdieu. Mark argues that Bourdieu's theory of practice and the notion of the habitus as internalised and embodied dispositions works to reproduce an extra-individual system through the practice (in this case, the quality judgements) of individuals. Scientific quality judgement is a form of practical or tacit knowledge, which means that it is a practice learnt through practice, and competence does not necessarily mean the ability to explicate how the practice is performed or the principles behind it. The evaluator may not even be fully aware of the criteria he or she puts to use, or they may not be the criteria he or she explicitly says are used (Mark 2003:59).

A recent doctoral thesis by Ingegerd Gunvik-Grönbladh (Gunvik-Grönbladh 2014) studies the evaluation of pedagogical skills empirically in *sakkunnigutlåtanden* using a similar Bourdieusian approach, constructing habitus based on the actor's position in the academic field and in terms of cultural (academic) capital, measured by proxy indicators for academic authority. For both authors, Bourdieu's theoretical framework provides a general model of how conservative and field-perpetuating forces potentially arise through the central role of the habitus in peer review quality judgements. While both authors mainly follow Bourdieu and his notions of *habitus* and *doxa*, Mark also voices serious concerns about the lack of reflexivity and potential for actors initiating social change in his conception of the habitus. Furthermore, the notion of *doxa* as the taken-for-granted common ground of a field is also adopted without modification from Bourdieu. These are two areas where scholars in the next set of works develop both empirically and theoretically beyond the Bourdieusian understanding.

Quality judgements as practice and cognitive particularism

The third theme can be found in a number of different studies that draw together insights from both the sociology of scientific knowledge and from a post-Bourdieusian conception of quality judgement as practice. Lena Gemzöe (2010) has provided a valuable and up-to-date literature review of studies on peer review for the expert group on gender at the Swedish Research Council (*Vetenskapsrådet*). The study draws heavily on a seminal study by Travis and Collins (1991), the work of Norwegian scholar Liv Langfeldt, and the work of Michele Lamont and colleagues. These all share a basic approach to studying science with my present study, and furthermore provide important insights that are useful to understanding the role of the peer review process in modern economics which is characterised by, on the one hand, a broad cognitive consensus, and on the other, the mainstream-heterodoxy divide and the presence of a minority of heterodox scholars.

Travis and Collins (1991) utilise insights from the sociology of scientific knowledge to further the understanding of peer review processes and the types of potential bias these may generate. They show that earlier studies of potential bias in peer review have confused two types of potential bias: cognitive and social. The latter is close to the common sense notion of bias, whereby reviewers are biased for example regarding to social position (e.g. gender) or institutional affiliation or social network (e.g. old boy-ism). However, in line with the sociology of scientific knowledge, the authors argue that we must understand science not primarily as a social, but rather as a cognitive structure.[48] They coin the term "cognitive

[48] This shift echoes the move from a Mertonian sociology of science that emphasizes the social organisation of science, to a Kuhnian sociology of scientific *knowledge*, where the cognitive structure of a paradigm, discourse, style of thought, etc. becomes the main object.

particularism" to pinpoint a more relevant aspect of potential peer review bias, whereby reviewers "sometimes make decisions based upon their membership in scientific schools of thought", so that biasing occurs not primarily on the basis of institutional or social, but on the basis of *cognitive similarity* (Travis and Collins 1991:323).

They furthermore argue that this may or may not be of great importance, depending on the level of consensus and cognitive boundaries in the specific area of science. If there is widespread consensus and no clear boundaries, "all the scientist working in the area share a similar conception of the current paradigm", whereas "where there are well-defined cognitive communities, on the other hand, the more pronounced the divisions, the greater will be the effect on the development of science of drawing reviewers from one side of a cognitive boundary rather than the other" (Travis and Collins 1991:327, 328). While there may be strong links between social and cognitive organisation, that is, connections between a social group and a way of thinking, this primarily holds true at the macrolevel. On closer inspection, the authors argue, we may well find that cognitive boundaries don't map completely onto social or institutional boundaries, as when scientists from a dissenting school of thought find themselves scattered in small numbers across university departments. The implications for analysing the role of the peer review system in economics should be apparent. In such a situation, cognitive particularism will potentially work against heterodox economics.

The cognitive aspects of peer review has also been emphasised in works on scientific quality judgement by Michele Lamont, culminating in her monograph on quality judgement in interdisciplinary grant panels, *How Professors Think* (Lamont 2009). She emphasises the fundamentally diversified nature of quality concepts and practices of quality judgement and evaluation, and points to the existence of different epistemic styles in different disciplines. However, these epistemic style do not follow disciplinary boundaries neatly, and sometimes cross disciplinary boundaries. Evaluation is here seen fundamentally as a social practice, and quality judgement as outcomes that are not predetermined. For example, a central finding is the way that experts across disciplines manage to reach agreement and deliberate, despite their varying conceptions of quality and evaluation. Similar work has been done by Liv Langfelt (2004, 2006), showing how quality evaluation is made up of somewhat messy practices, relying on tacit knowledge and silent agreements as consensus is reached in cooperative panels.

The important insight from this strand of work is that quality evaluation cannot be reduced to a set of criteria like "originality", like in the early studies by Hemlin and others, because these criteria are empty in themselves. The interesting question is how a criteria is applied in practice by experts, and the variations in the ways that these practices take place. Criteria are resources that are used by experts to achieve an outcome—a valuation in accordance with their judgement in a specific epistemic style—a social practice that is highly institutionalised, although it may be institutionalised in a variety of ways. This insight then lines up well with the idea of the disunity of science and of distinct styles of reasoning: the fundamental notion that although there is not one monolithic science, there are several scientific approaches, which may or may not align well in any particular case. However, an important message of both Lamont's and Langfeldt's work is also the possibility, after all, of communication and establishment of consensus *across* epistemic styles.

The recent literature on scientific quality evaluation also connects to a growing more general literature on valuation and evaluation as a broader class of basic social phenomena, focusing on the process whereby actors produce classifications and establish value of some object, ranging from art and commodities to scientific oeuvres. A thorough review of this literature is provided by Lamont (Beljean, Chong, and Lamont 2016; Lamont 2012b), bringing together insights from the social nature of evaluation in economic sociology (for example Fourcade 2011) and classification practices in cultural sociology (Beljean et al. 2016). The general social phenomenon of (e)valuation then includes the various processes by which social actors construct, use, maintain, or justify symbolic/ cognitive classifications or schemes, and the ways these social phenomena are sorted or classified.

The epistemic impact of bibliometric indicators and judgement devices

A final strand of literature that also draws to some extent on the literature on evaluation practices comprises recent work, primarily by professional bibliometricians, working at the intersection between bibliometrics and science studies. I refer particularly to work on the role of bibliometrics and bibliometric indicators in scientific evaluation practices. For example, the epistemic impact of bibliometric indicators has attracted much recent attention in this field (Castellani, Pontecorvo, and Valente 2016; Rijcke et al. 2016). This attention has followed partly from the increasing actual prevalence and use of bibliometrics in research evaluation at all different levels, a development which has even been called a "metric tide" (Wilsdon et al. 2016), and countered by calls from bibliometricians for sensible use of quantitative data in tandem with peer review in evaluation practices (Hicks et al. 2015).

The integration of bibliometric indicators in peer review has been studied using Swedish expert evaluation report data in recent work by Swedish bibliometrician

Björn Hammarfelt and colleagues (Hammarfelt 2017; Hammarfelt and Rushforth 2017). Hammarfelt and Rushforth (2017) suggest that the use of bibliometric indicators, like citation counts, h-index, and journal rankings, can fruitfully be understood in terms of *judgement devices*. They borrow this concept from Lucien Karpik who used it to denote the methods and devices used in the valuation of intangible goods, like unique pieces of art. A judgement device is then some form of device that can be used as a tool by an evaluator to support and simplify qualitative judgement and classification, like the appellations of wine producers or various forms of ranked lists. They argue that the use of such indicators should be understood not as an opposite of traditional peer review that has come to replace it, but as an integrated aspect and tool used by evaluators to form judgements on scientific oeuvres in peer review.

An important finding of their work, comparing the fields of biomedicine, history and economics, is the variable way in which evaluation and the use of indicators is put into practice across disciplines. These are interesting and novel studies, pointing to the understudied nature of this type of evaluation of scientific oeuvres, which plays a crucial role in academic careers, and arguably the reproduction of thought collectives. The role of journal rankings is mentioned, in passing, as having a special status in economics by tradition, although the authors do not focus on this particular aspect in their studies. On the other hand, journal impact factors (JIF) as a quantitative measure of the status of publication outlets are used both in economics and biomedicine (Hammarfelt and Rushforth 2017).

The role of top journal rankings in maintaining the disciplinary mainstream has also increasingly been discussed by heterodox economists, for example in relation to the United Kingdom's Research Assessment Exercise, and has resulted in attempts to produce alternative rankings of heterodox economics journals (Lee 2009; Lee et al. 2010; Lee, Pham, and Gu 2013). There has also been considerable interest within the economics discipline in constructing rankings of the discipline's top journals (Kalaitzidakis, Mamuneas, and Stengos 2003, 1999, 2011), often focusing on the technical aspects of producing better rankings. However, these studies and ranking exercises by economists, whether mainstream or heterodox, are further evidence of the importance of *rankings as such* in the economics discipline.

Chapter 4. Theoretical framework: Relational disciplinary styles of reasoning

It is now time to construct the theoretical framework that will inform this study and function as its engine of explanation. Drawing on the development of the problem in chapter 2 and the thorough survey of previous research and theorising in chapter 3, and in dialogue with the empirical material, I propose a sociological theoretical framework of *relational disciplinary styles of reasoning*. This is a development of the styles of reasoning framework that adds a sociological emphasis to the institutionalised form of reproduction of specific combinations of Crombian styles within academic *disciplines*. I argue, as an important addition to the styles approach, that in order to understand the constitution and reproduction of styles of reasoning within disciplinary thought collectives, we need to understand how they are often simultaneously engaged in reproducing symbolic and social boundaries. The maintenance of a style of reasoning is thus also *relational*, because it presupposes ideas about and practical interventions that reproduce boundaries towards that which is too different, inferior, or even unacceptable as scientific practice.

I present the theoretical framework here in the form of a theory of relational disciplinary styles *in general*, and will demonstrate its analytical usefulness for the present study in subsequent chapters. The argument for the framework is developed in a series of steps below. There is a logic behind the order of presentation. It begins with styles as the macrostructures and institutions that always pre-exist for new actors entering the scientific field, and proceeds via the institutional context of disciplines and socialisation of a scientific habitus. It ends with actors and their engagement in boundary work and scientific quality judgement, that is, agency which potentially reproduces the structures of the field.

1. Styles of reasoning

The styles of reasoning approach forms the point of departure for the framework of *relational* and *disciplinary* styles that I will develop here. It lets us conceptually grasp the enduring macrostructure of science, and I will argue that the central problem of this study, that of stability of a disciplinary approach in modern economics and its enduring and antagonistic relationship with heterodox approaches, is fruitfully understood using this framework.

In chapter 2, I synthesised the findings from a survey of debates on the nature of modern economics and the mainstream-heterodoxy divide. It is clear that the economics discipline is somewhat paradoxically characterised on the one hand by a strong scientific consensus, and on the other hand by a small minority of vocal heterodox opponents. While *heterodox economics* as a concept and identification only took off around the turn of the millennium, there are good reasons to claim that the phenomenon of a mainstream-heterodoxy divide has existed in some form at least since the post-war years. Summarising the survey, I identified three central features of this divide.

First, the economics mainstream is built around a consensus on a small set of axioms of social *ontology*, that is, about the behaviour of the actors and interactions figuring in economic analysis. These are *methodological individualism* (atomistic individual actors as unit of analysis), *methodological instrumentalism* (individuals' behaviour fully determined by set preferences), and *methodological equilibration* (analysis in terms of equilibrium tendencies or states). These are ontological assumptions, for they define the fundamentals of the scientific social ontology, which then allows for a wide range of more specific theories building on these assumptions. Second, there is what I called the *epistemological* aspect, the disciplinary consensus on how scientific knowledge should be made, with formal modelling as the methodological approach par excellence. In this idealised conception, these two features are embraced by the mainstream, but rejected by heterodoxy in favour of a heterogeneous set of alternative theoretical and methodological approaches, so that the central divide seems to hinge on these fundamental ontological and epistemic issues. The third feature is the *social* nature of this split, as a divide between two oppositional groups of intellectual actors with boundaries maintained and contested in a relational process.

Drawing on the literature review in chapter 3, I argue that it is fruitful to think about this situation in economics using the concept of *styles of reasoning*. This allows us to understand it as a particular instance of a more general class of sociocognitive phenomena. I will first explicate my understanding of the

Crombian styles, and in the subsequent sections present the sociological extension of the *disciplinary* and *relational* aspects of styles.

Styles of reasoning as enduring collective ways of knowing

A style of reasoning is an enduring, relatively stable, and collective enterprise. It is an epistemological approach to scientific problems, a certain way of finding out, that also implicates ontological presumptions and values orientations. From a sociological perspective, styles point to the enduring or structured nature of knowledge production as a social phenomenon. Styles let us keep what Hacking calls the *quasi-stability* of the sciences in view. But if there is long-term stability in science, it is not primarily the content of science (facts and theories) that is stable. For while this may undergo evolutionary or revolutionary historical transformation, it is *how* we find out, rather than *what* we find out, that tends to be historically stable, and this is what the styles of reasoning concept captures (Hacking 1992).

A style of reasoning is thus a historically stable and collective *way of finding out*. It includes both objects and methods of discovery which may be unique to the specific style. A style introduces its specific types of *objects*, *evidence*, *ways of being a candidate for truth*, *laws* and *possibilities* (Hacking 1992:11). In other words, a style is not only a set of epistemic attitudes or standards (for example, what counts as valid evidence?), it is also an ontological orientation: what sort of objects and laws are included or even possible in the scientific ontology?

To understand styles sociologically, we need to see these fundamental epistemological and ontological conceptions as part of scientific socialisation. This was one of the great achievements of Fleck (1979) and Kuhn (1996), but tends to slide out of sight in Crombie's historical and Hacking's philosophical accounts. Fleck and Kuhn showed how the most simple scientific practice, and cognition itself, relies on meticulous training and the development of the readiness for directed perception, "the readiness for one particular way of seeing and no other" (Fleck 1979, 64). In similar terms, Kuhn (1996) pointed to the paradigm as an *exemplar* used in training, a generic way of solving problems and establishing scientific standards. Thus, the style provides the basic cognitive framework, within which productive scientific work of normal science may be performed. This structuring of thinking can be understood, using insights from modern cognitive science, in terms of *model-based reasoning* (Brante 2014:169).

It is useful to also think about styles not only in terms of strictly rational reasoning, but also in terms of normative dispositions. For a style of reasoning is also in an important sense a moral commitment, a set of basic scientific values and virtues. Crombie (1995:234) emphasised this aspect more than Hacking, and talked of styles as being made up by a set of underlying "intellectual and moral commitments or dispositions" towards nature, science and the nature and purpose of human life. In other words, conceptions of good science and what it means to reason rightly are central to a style of reasoning, together with wider dispositions regarding the role of science in society, and fundamental values regarding our relation to nature (or society). This dual set of intellectual and moral commitments or dispositions plays a central role in the social organisation of scientific reproduction.

In the conception of styles proposed here, I follow Hacking's notion of the "self-authentication" of scientific styles of reasoning. As a framework for reasoning and for finding out, an epistemic genre, such styles are themselves criteria for truth: "The styles in our list do not answer to any criteria of truthfulness other than their own. They are not 'chosen' because they 'work'. They help determine what counts as working" (Hacking 2012, 607–8). This conception is not a case for constructivism that treats scientific facts as outcomes of contingent processes of negotiation without anything there preceding the construction of the fact. It is rather a form of anti-foundationalist historical and social epistemology that takes the disunity of the sciences and the historicity of reason as a fact, but nevertheless allows for the production of rational knowledge *within* styles:

> To say that these styles of thinking & doing are self-authenticating is to say that they are autonomous: they do not answer to some other, higher, or deeper, standard of truth and reason than their own. To repeat: No foundation. The style does not answer to some external canon of truth independent of itself. (Hacking 2012:605)

Self-authentication shifts Kuhn's idea of incommensurability, the absence of external foundations for rational choice between competing paradigms, from the diachronic paradigm shift to the synchronic co-existence of potentially (but not necessarily) incommensurable styles of reasoning. I emphasise this notion because I believe it has interesting sociological consequences. Just as Hacking argues that the introduction of ontological novelties introduced by new styles tend to lead to philosophical realist debates (does this or that theoretical entity really exist?), the same could be said about the methodological aspect of styles. If styles harbour internal truth criteria, contexts with competing styles will lead to prolonged debates about the nature of the scientific enterprise, because there are no proper scientific ways to settle the issue. That this is in fact the case with the mainstream-heterodoxy divide in economics will be one of my main arguments.

Crombian styles and disciplinary styles

I make a distinction between, on the one hand, styles of reasoning as historically stable and discipline-transcending styles in the original sense used by Crombie, Hacking and others and, on the other hand the specific constellation of styles found in a particular discipline at a specific point in time. I have called the former *Crombian styles*. Crombie's list of six styles of reasoning includes, using Morgan's (2012, 15) labels, mathematical postulation and proof, experiment, hypothetical modelling, taxonomy, statistics, and the historical-genetic style. This, as Hacking (1992:8) writes, is a "good workhorse of a list" that should be readily acceptable to historians of science. The list is not exhaustive, nor is it mutually exclusive. There are good reasons to believe that there are also other distinct styles, like modern psychiatry (Hacking 2012), or perhaps engineering (Kwa 2011). The point here is simply that the list of styles should by no means be treated as exhaustive.

An important difference between styles and Kuhn's paradigm, and similar concepts, is that a style is not only historically enduring, but also that Crombian styles are more fundamental than scientific fields or disciplines or paradigms. Take statistics for example. When the statistical style of reasoning became established, it not only introduced ontological novelties (like the statistical mean) and new methodological approaches, it became integral to many sciences with otherwise very different objects. Styles in this Crombian meaning thus have relative autonomy from individual sciences. In this sense, while styles of reasoning may be incommensurable and act as *barriers* to mutual understanding, they are also supra-disciplinary, existing beyond particular scientific disciplines, thus potentially acting as *bridges* between scientific enterprises across disciplinary boundaries.

To sum up, the styles approach as I use it here involves bringing to our attention the various epistemological and ontological presumptions that guide scientific work, and the ways in which this has varied both diachronically through the history of the sciences, and synchronously at a single point in time. Using this focus it is possible to understand scientific reasoning neither as something singular, timeless and universal, nor as infinitely varied, a matter of personal taste. Instead, the styles approach invites us to direct our gaze towards the social and collective nature of scientific reasoning. At this level of analysis, a certain "quasi-stability" may be observed in the basic modes of scientific reasoning, that endures despite transformations in theories, facts and research programmes. This perspective does not deny that science is always novelty-producing and evolving, but also treats science as multi-layered, and shifts the perspective to the more stable and slow-changing underlying styles of reasoning.

In real academic life, the various Crombian styles are often entangled in any particular disciplinary setting. This is the case with the modelling style in economics, which mixes with both the statistical style and with deductive postulation and proof (Morgan 2012). Morgan claims that while modelling has become the trademark style of modern economics, it has always been entangled with these styles. But the modelling style is the most fundamental, according to her account.

Morgan shows how the styles approach may be fruitfully applied to modern economics. She argues that while models are partial and may appear as independent and separate reasoning objects, together the abundant models in all fields of modern economics create something like a flexible patchwork of models, stitching together subfields of the discipline. To that, she adds two general assumptions that “hold economic ideas together” (Morgan 2012:394). These are, first, *the individual utility maximisation of economic man*, and second, the *equilibrium tendency*. This is just another way of framing the three ontological assumptions delineated above, since “individual utility maximisation” contains the first two (individualism and instrumentalism). Although these are general assumptions of the discipline, they are not necessarily simultaneously present in every model. However, Morgan illustrates how the ontological and epistemological aspect of modern economics can be understood within the styles framework, and as noted in chapter 3, emphasises the social aspect of the style when it becomes a community matter to be transmitted by training, reinforced, and policed.

I will use the term *disciplinary style* to describe the actual entanglement of a set of Crombian styles that Morgan illustrates. My distinction between Crombian styles in the original sense, and the disciplinary style as the specific constellation of styles found in a particular discipline at a particular point in time, is introduced to bring analytical clarity to the framework. It enables a distinction to be made between, on the one hand, the quasi-timeless Crombian styles, and on the other, the disciplinary style that is open to change in terms of re-combinations of styles or changes in emphasis. Thus conceived, the concept of a disciplinary style has more resemblance to a Kuhnian paradigm. However, the focus is shifted from revolutionary paradigm change, towards historical continuity and evolutionary combinatory transformation. I understand the specific style of reasoning that became established in economics after 1945 as a quasi-stable disciplinary style which relies strongly on the Crombian modelling style, enmeshed with the deductive and statistical styles. It is also entangled with the set of three ontological assumptions about social actors (methodological individualism, instrumentalism and equilibration). Finally, the disciplinary style of modern mainstream economics is the collective property of an institutionalised disciplinary thought collective.

But—and this is my important extension of Morgan's understanding of styles in economics—the profession and its dominant style do not exist in a vacuum. Instead, there is an important sense in which styles and thought collectives are bounded in relational processes. Specifically, the disciplinary style must be understood in relation to heterodox economics.

2. Thought collectives as the social foundation of styles

Reflecting on thirty years of work on the styles project, Hacking (2012) recently remarked that the sociological gaze of Ludwik Fleck and his connection of styles of thought to thought collectives should be a fruitful addition to the styles approach. In this spirit, the link between a style of reasoning and a social thought collective is also central to my attempt to sociologise the styles project. As Fleck, Kuhn and many others after them realised, scientific thinking and reasoning are deeply social processes. The social transmission of a particular style of thought is connected to a thought collective in two basic ways.

First, scientific practice requires years of formal training. New generations of scientist need to learn the theoretical language of the discipline, the canon of great authors, standard problems and paradigmatic exemplars. They also need to learn how to master technical aspects of the science, scientific methods and techniques, arguing in the seminar room as well as in the established written format. Through the scientific training, today formally institutionalised in the form of doctoral programmes, new generations are gradually taught not only the explicit knowledge requirements of formal education, but also, importantly, the tacit knowledge of the professional thought collective. This tacit knowledge ranges from the way one behaves in an academic seminar room, or how one performs common procedures using established computer software, to how one formulates interesting research problems or evaluates and criticises scientific claims and arguments. The often quite practical aspects of this gradual integration into the established thought collective points to the usefulness of talking about *reasoning*, rather than *thinking*, as Hacking has argued, because scientific reasoning processes happens not only in the heads of individuals, but are to a large extent public and communal. They are intersubjective processes that draw on a collective pool of knowledge and practice.

Controversies and heterodoxy

The fundamental social organisational unit of modern sciences is the modern scientific discipline. The disciplinary style of reasoning is the collective property of a disciplinary thought collective. In economics, the professional thought collective is firmly intellectually institutionalised with its own educational programmes, literature, textbooks and scientific journals, and organisationally with an international labour market and control over jobs and resources by economics departments, and generally acknowledged through the concept of the "economist". When thinking about knowledge production at the macrolevel, the discipline comes in as the most natural and handy unit of analysis.

However, while treating scientific disciplines as generally homogenous may be warranted in many cases, this homogenising perspective means that we lose sight of internal diversity. Given my research problem we need to be able to focus also on structured internal difference *within* disciplines. This could be thought of as the next step following the movement from a conception of unitary *science* to the *disunity of science*, with a plurality of "*sciences*". This next step would then be an invitation to think about the *internal disunity of the sciences*. Here we should not lose sight of the importance of hierarchy in science, and the central role of scientific elites in controlling departments and disciplines in general (Elias and Whitley 1982), and specifically in economics (Fourcade et al. 2015). In other words, we need to be able to make a conceptual distinction between a scientific establishment or orthodoxy on the one hand, and scientific heterodoxies on the other. Note that I am not interested in the more fine-grained variation that may also be found among research fields, schools of thought, or even more local research groups. Those are all valid and interesting foci, but not the subject of the present study.

Studying scientific heterodoxies could perhaps be seen as a macro version of well-established study of controversies in science studies. Scientific controversies function as excellent sites to study cases of where the tacit assumptions of normal science is upset and forced to be explicated, thus providing a good sociological entry point into the assumptions and background epistemic ideas and strategies behind the parties of a controversy. In the study of scientific controversies, one may distinguish between different focus points of controversies, from controversies over facts (was it really mountains Galileo observed on the moon?), through theories (is combustion to be explained in terms of "phlogiston" or "oxygen"?) to basic principles or entire worldviews (the Copernican revolution) (Brante and Elzinga 1990). Controversies that revolve around *methodology* may also be "extremely interesting" for sociologists of science (Yonay 1994). With the styles approach, we can think of such instances as controversies involving incommensurable styles of reasoning.

Scientific controversies are distinct from everyday disagreements, in that they normally have some extension in time, and tend to divide proponents and opponents into opposing groups. However, we could also think of controversies that remain unresolved for longer periods of time and turn into *dissent*, which often implies asymmetric relations between dominant insiders and dissenting outsiders (Backhouse 2004). Furthermore, groups of dissenters may in various ways *organise* around dissenting views. We could then, again following Backhouse, think of scientific *heterodoxy* in terms of *permanent controversies* involving asymmetric organised dissent.

The theoretical conception I am proposing here is that the mainstream-heterodoxy divide in modern economics may be thought of in terms of an asymmetric permanent controversy between an established disciplinary thought collective, and a dissenting heterodox thought collective, that revolves around the fundamental style of reasoning that economic research should have. The sociologisation of the styles approach involves the acknowledgement of the relational dynamics of controversies and heterodoxy. However, in understanding the dynamics of the asymmetrical relation and the stability of a disciplinary style of reasoning, the institutional stabilising force of the scientific discipline is a key factor.

3. The institutionalisation of disciplinary styles

I introduced the concept of a *disciplinary style* of reasoning as a specific constellation of Crombian styles that has become stabilised in a particular scientific discipline at some point in time. The institutional context that is the discipline provides an important potential stabilising mechanism to the style. I say "potential" since the stability of disciplinary styles is an open question. As I have argued, a primary difference between the Crombian and disciplinary styles is that while the former are seemingly timeless *longue durée* phenomena, disciplinary styles are open to recombination and evolutionary transformation. An example is the gradual introduction and establishment of the statistical style in economics during the mid-twentieth century. The specific combination of Crombian styles in a discipline and its stability is thus an empirical question. But scientific disciplines are not all the same. I will argue that is useful to think of disciplines as self-reinforcing systems, while the degree of *disciplinarity* is a variable property. Abbott (2001) argues that a modern US-based discipline functions as a self-reproducing *dual institution*. Using this conception I will suggest a simple model of the relationship between the social and intellectual organisation of disciplines, where the four basic elements consist of the social macrostructure of (inter)national *scientific disciplines*, mirrored in the micro-organisation of *departments* at the university level, the system of *scientific journals* as institutionalised intellectual infrastructure with strong links to disciplines, and the *peer review* process as a central regulating mechanism.

The modern scientific discipline is the fundamental organisational structure of modern science that ties together a social thought collective and a collective archive of knowledge in a disciplinary style of reasoning. The organisation of scientific fields into distinct disciplines is the primary framework for, and result of, the collective self-discipline of a scientific thought collective. Disciplines may be regarded as "conservative novelty producing systems" (Whitley 1986:187), with remarkable stability provided by self-stabilising mechanisms. A discipline should not be understood in a too general or ahistorical way. As Abbott (2001) has argued, there is something very peculiar about the specific US type of scientific discipline that emerged in the early twentieth century, and has exhibited a striking, almost static, stability since then. Compared to the organisational forms of German, French or British universities, the form in the United States has its own self-stabilising dynamics. Since the Swedish university system, like many others, has increasingly been inspired by the US model, at least since 1945, and still today looks west for inspiration, this model is of great relevance to understanding the development of the economics discipline in the Swedish context.

According to Abbott (2001 ch 5), a number of features define the US model, which emerged as a hybrid of the European systems at the end of the nineteenth century and the first decades of the twentieth. First, US universities were, like in Germany, many and decentralised. Second, like the British college model, they were educational institutions for undergraduate education. Third, they were simultaneously also graduate research institutions, like their German counterparts. However, while the doctorate was a core token of legitimacy in the German system, its role was more of a general qualification, as professorships and doctorates were tied to individual chairs and professorships, rather than bounded disciplines. The US system turned the PhD into a specific disciplinary authorisation certificate (i.e. in sociology, economics, psychology), and a doctorate soon became an entry requirement for an emerging national disciplinary labour market. Fifth, unlike the hierarchical German and French systems, US departments became departments of equals. Sixth, the creation of the modern department and discipline coincided with the institution of national societies that professionalised the disciplines through the exclusion of lay intellectuals and the establishment of disciplinary journals.

The consolidation of the modern (US-type) discipline may be thought of in terms of what Abbott (2001:128) calls a *dual institutionalisation* of a social structure. It consists, on the one hand, of the macrostructure of the national discipline, which forms the interactional field and a functional labour market in which careers may be pursued. On the other hand, at the microlevel, every university is, with few exceptions, organised into a familiar set of departments (economics, sociology, psychology, etc.). The isomorphic structure of departments and organisation of teaching and research makes for smooth exchange of faculty within the discipline, where departments hire staff only with a doctoral degree from their own discipline. Through control over standard undergraduate curricula and doctoral programmes, departments "discipline" a large number of students and the future members of the discipline. The role of the disciplinary doctorate is crucial here, and Abbott (2001, 139) argues that the "one central social structure signifying full disciplinarity" is the "reciprocity in acceptance of PhD faculty", so that disciplines "become true disciplines in the social structural sense once they hire mainly PhDs in their own field".

It is within this institutional structure that the reproduction of a disciplinary style of reasoning may be understood. Its self-stabilising mechanisms are central in this respect. Abbott (2001:127) goes so far as to claim that "absent any radical change in the process of academic hiring the current social structure of disciplines will endlessly re-create itself". This exemplary rigidity of the dually institutional system helps us to account for the enduring existence of styles. While Abbott (2001:130) doesn't use the concept of styles, he talks of much the same thing in terms of the cultural structure of disciplines, regulating "dreams and models both of reality and learning", of professional identity and as a mode and legitimation of necessarily partial knowledge. Abbott illustrates this nicely with a view from undergraduate students in interdisciplinary settings. In these settings, he argues, a feature which goes

> unnoticed by faculty but all too plain to students, is that teachers disagree profoundly about relatively commonplace matters. Undergraduates subject to distributional requirements learn to live with flagrant differences in the scholarly interpretation of social events. Economists tell them poverty reflects incentives, anthropologists that it arises in the culture of globalization, sociologists that it shows the potency of job migration in urban settings, and so on. The very phenomenon itself appears different in the different classes. Like their elders, most

undergraduates eventually learn to tune out all but one version of the problem.[49] (Abbott 2001:143–44)

The relationship between disciplines is then not a question of a division of their objects, but about adopting a specific way of formulating and solving scientific problems, exactly what I call a disciplinary style of reasoning.

Disciplinary elites and scientific journals

On top of Abbott's model of the dual institutionalisation of departments and disciplines, the modern ecosystem of scientific journals adds a third institutional layer. Journals may be more or less connected to one scientific discipline, and they may be of a more or less specialised nature. There are also great variations over time and between disciplines in how different journals are valued and positioned in the conceptual universe of a discipline. Economics has been shown to be a scientific discipline that is more hierarchical than comparable disciplines, with a sense of "superiority" (Fourcade et al. 2015) or an "elitism dispositif" (Maesse 2017) fuelling disciplinary hierarchisation. Disciplinary elites may exert power in the form of strategic decisions regarding the disciplines, for example when it comes to influencing the general direction of research, strategic hiring decisions and forms of education. However, one of the arguably most important forms of influence is through the indication of the style of the elite: if the style is transmitted through exemplars, in a strong hierarchy, the role of elite research becomes even more central in its role as exemplar.

In a situation with a small, clearly-defined set of generally acknowledged international (or rather US-based) journals, these fill an increasingly important role as institutions that filter and categorise the general knowledge and approaches that could be understood as the disciplinary core and define valid theoretical approaches, choice of problems and acceptable methodology; in short, the general disciplinary style of reasoning. This is not an inherent function of the journal system. Instead, it remains an open question how and to what extent some journals come to play this role. The existence of a set of high-status journals first requires their consecration, the general acknowledgement and stabilisation of

[49] Abbott's (1999, 2001) reasoning about disciplines and disciplinary knowledge is not only descriptive and analytical, it is also openly supportive of the intellectual merit of intellectual discipline(s), against what he claims to be a repetitive critique of "narrow disciplines" which has resurfaced as variations on a theme since at least the 1920s (Abbott 2001:122 n. 1). Against the threat of the balkanisation of organisation around problem-based research, Abbott (2001, 135) promotes the "problem-portable" knowledge generated by disciplines.

them as the “top” journals, while in turn filling the role as consecrators of top research, marked with a symbolic stamp of approval as “top research”.

The fourth institutional arrangement is that of peer review processes. The filtering and sorting function of peer review is at work in the everyday working of the journals, by editors and reviewers judging the quality of submitted papers. Somewhat larger and more important decisions are taken by various funding agencies, and the arguably most decisive form of peer review is at work in hiring and promotion, where entire scientific oeuvres are evaluated and ranked by reviewers. Here, the selection of reviewers from within the discipline means that a trusted quality judgement with a strong disciplinary footing is obtained. In the evaluation practice itself, the evaluator’s scientific habitus is activated, and while it should not be understood in a deterministic way, the law of large numbers means that the averaging of a large number of evaluation practices will tend to stabilise and reproduce a disciplinary style of reasoning.

A final feature should be mentioned. Abbot talks about the dual institutionalisation of disciplines in an American national setting. However, while the sciences have always been part of international communities, the extent of integration into an international research community is an open question. Given the above institutional mechanisms, a crucial question for the present study, that extends beyond Abbott’s scope, is the extent to which a national discipline is integrated into and part of the international discipline. Among the factors we should expect to play an important role are the international standardisation of the doctoral degree, the extent of internationalisation of the labour market, and the reliance on internationally acknowledged journals for scientific communication, and the international recruitment of peer reviewers.

4. The scientific habitus

To understand the reproduction of styles of reasoning in sociological terms, we need to understand the microlevel role of the individual actors in the reproduction of their commitments and dispositions. The link between the macrostructure of styles and the role of individual scientists is fruitfully conceived in terms of Bourdieu’s concept of a specific *scientific habitus*. This concept of *habitus* is well known and has been discussed in chapter 3. Habitus is the product of socialisation and adjustment through continual adaptation through practical interaction with the objective social world. While the concept is normally thought of as a set of deep dispositions that are active across very different social fields, Bourdieu also talked about a specific scientific habitus in his programme for a sociology of science (Bourdieu 1975, 1991, 2004).

Through training and practice in the field, scientists acquires a specific scheme of perception, an embodied sense of judgement in scientific matters. It forms the basis of the never-fully-explicable but nevertheless central capacity of scientific judgement and sense of justification in various settings. This set of dispositions that are neither unconscious nor fully conscious, consisting both of cognitive schema and practical embodied dispositions, the sort of predisposition that generates automated social action that takes place without conscious reasoning.[50] Habitus works like Wittgenstein's rule-following: we may be able to follow rules in practice without being able to really explicate them. The concept of a scientific habitus allows us to account for, at the level of the actors, how the "commitments and dispositions" connected to styles of scientific reasoning are transferred and reproduced, embodied in the individual scientist: "A scientist is a scientific field made flesh" (Bourdieu 2004:41). In sum, the *scientific habitus* comprises the structured dispositions toward scientific matters that are acquired through years of immersing oneself in formal education and practice in a scientific field. It is the readiness to act, perceive and reason in a certain way rather than another. It is the source of the professional sense of judgement.

The notion of a scientific habitus used here is then only the aspects that Bourdieu sees as disciplinary habitus, rather than the habitus that is linked to the individual's social trajectory or position in a scientific field (Bourdieu 2004:42). I borrow Bourdieu's concept in its simpler form as a useful tool without relying on the rest of his analytical apparatus of *field* and *capital*. I consider the concept as a good "detachable capsule for a dispositional theory of action" (Wacquant 2014:5). The habitus concept does zoom in on the *structured* aspect of social life, telling us that we should expect that similar training and practice tends to a similar habitus, while greater differences in background should lead to actors with more heterogeneous scientific habitus.

While the concept of habitus invites a focus on how social structure is imprinted in actors, it does not entail determinism about actors or lack of agency. A scientific habitus is simultaneously *constraining* and *enabling*. Scientific training and experience not only constrains actors from a potentially unlimited set of possible ways of thinking, working and reasoning. These constraints include both

[50] If one has a taste for "harder" or more biological accounts, it should be noted that Bourdieu (2000:136) himself talked of *habitus* as "the selective and durable transformation of the body that operates via the strengthening or weakening of synaptic connections". This interpretation has also been promoted by the French neurobiologist Jean-Pierre Changeux in terms of "The Neuronal Bases of Habitus" (Wacquant 2014:8).

explicit and formal rules and theories, unquestioned assumptions, and that typically implicit and tacit knowledge, the scientific habitus, the practical sense of what is reasonable and right and interesting. A field-specific scientific habitus is then a prerequisite for productive and creative normal science within a thought collective. But, in the final analysis, we still need to account for social stability in terms of the stability of scientific practices: why are the established ways of reasoning still considered adequate, reasonable, unquestionable, or without alternative? I will assume, like Kuhn, that a major part of the explanation when it comes to science is that the disciplinary style of reasoning is scientifically productive within a state of normal science. It is perceived as a framework that provides ways of formulating soluble problems and their solutions within a highly institutionalised setting. Science is conservative not for its own sake, but because a scientific community has found a mode of working that is perceived as productive by its own standards, and integrated into an institutionalised system of normal science.

If the discipline is the primary institutional anchor of a disciplinary style, then the habitus is the point where individual actors' commitments and dispositions are structured by the structure of the style. Through formal training and repeated practice, actors become shaped and predisposed towards a certain way of thinking, doing and reasoning within a thought collective, which gives stability to (while not determining in an absolute sense) future practice. The role of heterodoxy means that thought collectives and disciplinary styles are not always homogeneous, and that the boundaries of a thought collective, its style of reasoning cannot be taken as unproblematic givens. We also need to understand how such social and scientific boundaries are sustained and contested in practice in their institutionalised setting.

5. Boundary work and the relational nature of scientific styles

I have argued that the styles approach needs to pay attention to the potential internal differentiation of scientific disciplines and the existence of heterodoxies. To do so, we must understand the *relational nature* of styles as a general phenomenon, and connect it to a sociological conception of how this relational nature is reproduced by actors. As I have argued above, the reproduction of a style of reasoning is achieved by a thought collective that erects external boundaries towards other thought collectives and other styles of reasoning, and towards non-scientific common sense understandings of the world. Furthermore, if the reproduction and stability of a disciplinary style of thought in modern economics must also be understood in the context of the highly institutionalised setting that is the scientific discipline, another way of talking about the degree of disciplinarity is to think of it in terms of groupness, the extent to which the disciplinary thought collective forms a well-defined, self-identified and bounded group. In this section, I argue that the sociological concept of *boundary work* can be integrated into the styles approach and provide the conceptual grounds for developing a *relational theory of styles*.

Social and symbolic boundaries as outcomes of practice

In accordance with the overarching sociological understanding of styles of reasoning in terms of institutionally stabilised practices, the relation between mainstream economics and other scientific disciplines, non-science, and heterodox economics should be understood in the same terms, as structured but not unchanging outcomes of boundary practices, or *boundary work*. Following Lamont and Molnár's (2002) synthesising review of boundary and boundary work as a general social phenomenon, it is useful to make an analytical distinction between symbolic and social boundaries. Symbolic boundaries are understood as the conceptual distinctions and schemata used to categorise and structure some aspect of the world. Social boundaries are the resulting objectified social structural difference that result from symbolic sorting and boundary processes, which becomes manifest in differential group membership and access to relevant resources.

Boundary work, following Gieryn's (1983) classic formulation, is understood as the work of actors that accomplish the creation and maintenance of boundaries. Gieryn's important contribution shows how demarcation of proper science from non-science, but also how disciplines are demarcated from others, is not predetermined, but an achievement by professional groups in their struggle for authority and valuable resources. Gieryn makes a distinction between three forms of boundary work, as discussed in chapter 3. These are the *expansion* of scientific authority, *protection* of its authority, and the *monopolisation* of authority against other groups. In all forms of boundary work, the relational nature is fundamental: boundary work is a struggle to establish who has the right to legitimately speak as a scientist, and what types of practices could legitimately be seen as scientific, or as belonging to the particular discipline. The boundary work performed by a thought collective is an excellent site to study how styles of reasoning are legitimated and contrasted as outsiders and their style of reasoning are rhetorically excluded.

When considering styles of reasoning, both Crombian, but especially disciplinary, in the light of boundary work, their relational nature becomes clearer. For, to hold, transmit, and guard a collective set of commitments and dispositions is to establish that there is not only a right way to do things, but also that the space of possible sanctioned approaches has an outside. There is inevitably a larger space of that which is non-scientific, inferior, and unacceptable, and the maintenance of the style, at least implicitly if not explicitly, presupposes a relation to that other side of the boundary. Once a style of reasoning becomes institutionally stabilised within a discipline, the defence of proper science becomes synonymous with maintaining disciplinary boundaries. A different form of boundary work is performed by heterodox thought collectives which attempt to redefine and redraw the boundaries of science and the discipline by any legitimate means. If the boundaries are contested by a heterodox thought collective, it means that the dominant disciplinary style cannot just silently presume the integrity of its boundaries. The style has to be actively and forcefully defended against heterodoxy or other attempts at loosening or reconfiguring the boundaries of proper science. Establishing a disciplinary style of reasoning in such a context is then hardly a innocent and inward-looking matter. This is, in essence, my conception of *relational* disciplinary styles.

However, if Gieryn's approach emphasises struggles and conflicts over authority and resources and the exclusionary use of boundaries, other studies have instead emphasised how the role of porous boundaries, of creolisation and "trading zones" leads to mutual understanding and exchange across boundaries (Lamont and Molnár 2002; Wisselgren 2008). Lamont and Molnár argue that one fruitful direction for further research is the empirical study of the ways that boundaries *actually* function, rather than taking their demarcating and exclusive function for granted. The question then becomes *under what conditions* boundaries become porous and permeable rather than rigid and exclusionary. In the present context, this leads to the question, first, of how disciplinary boundaries in economics relate to conceptions of "science" in general, and what role heterodox economics and other disciplines play in the construction work that maintains the boundaries of proper economics. Second, the relation between disciplinary and Crombian styles is an open question. Can we talk about boundary work not only in relation to the discipline, but also to Crombian styles? And in what sense do Crombian styles act as barriers or bridges in the interaction with other scientific disciplines. To conclude, we should think of boundary work not only in relation to the discipline, and we should be sensitive to the roles that styles of reasoning plays in relation to the boundaries of mainstream and heterodox economics and their variable permeability.

Boundary work draws our attention to the constructed and contested nature of symbolic and social boundaries. It lets us highlight the agency of actors involved in the rhetorical reproduction or transformation of bounded structures. But is not only a rhetorical practice. For when the symbolic boundaries of what is considered to be proper economics, real science, are generally agreed and sufficiently established, symbolic boundaries may become social boundaries. That is, conceptions of what constitutes good science, of the boundaries of the accepted mode of reasoning, affects the distribution of resources and opportunities. In other words, resources in terms of both material reward (research grants and academic positions) as well as ever-important scientific recognition (publications in variably valued outlets and subsequent citations) are dependent on ongoing arguments about ambiguous and contested boundaries of proper economic science. The central mechanism through which this transformation of symbolic into social boundaries takes place are the various locations of the institutionalised practice of peer review.

6. Peer review and scientific quality judgement

If boundary work allows a conceptualisation of the maintenance work that reproduces boundaries, and the role of actors in this work, an even more specific site for the reproduction of scientific styles is the practice of quality evaluation involved in various peer review processes. The judgement of scientific quality is a prime example of the activation of the professional scientific habitus, and points to a central mechanism for the social reproduction of disciplinary styles.

Evaluation practices and cognitive particularism

The institution of peer review is the central regulating mechanism in modern science, through which standards of scientific quality are controlled and checked. It is the gatekeeping instance that sorts winners from losers in the competition for research grants, awards, attractive positions and the publication of results. It is a social institution that depends on the categorisation and legitimation practices of expert peer reviewers, relying on their capacity for professional judgement and various technical judgement aids. Peer review could fruitfully be understood as an instance of a wider class of social phenomena which all involve some sort of valuation or evaluation, ranging from the economic valuation of commodities to the cultural sociology of tastes to judgement of scientific quality (Beljean et al. 2016:201; Lamont 2012b). Here, I draw on insights from specific studies of peer review processes and the recent general sociology of valuation and evaluation that, in Lamont's (2012a, 202) words, "can be useful for understanding the cultural or organizational dimensions of all forms of sorting processes and for connecting microdynamics of exclusion to macrodefinitions of symbolic community and patterns of boundary work".

Evaluation practices are closely related to the notion of boundaries and boundary work. In both cases, we are dealing with social practices that establish some sort of boundary or sorting. However, I suggest that boundary work and evaluation practices can be considered analytically distinct phenomena which are often entangled in practice. Boundary work ideally deals with the establishment of a singular dividing boundary between an inside and an outside of a symbolic category, like (proper) professional "economics", and often includes a connection to a (more or less) macrolevel bounded social group. Evaluation practices, on the other hand, are first of all a wider class of phenomena, often involving a more formal and fine-grained classification and sorting. While boundary work may be an intended or unintended rhetorical device, it is seldom an explicit purpose in itself to establish symbolic boundaries, but rather its by-effect. Research evaluation on the other hand is a highly formalised practice with the explicit and purposive task of establishing a classification according to some quality criteria.

Evaluation practices can be said to be properly *social* processes, as distinct from psychological, on at least three grounds (Lamont 2012b:205). First, evaluation requires *intersubjective agreement* on a set of evaluation criteria or referents. In our specific case, this consists of disciplinary conceptions of good science, proper methodology, valid research questions and fields of study, etc. Second, evaluation involves *negotiations* about both choice and interpretation of quality criteria, as well as who is a legitimate evaluator. Third, it relies on a *relational* or indexical process of comparison. In our setting, being competent to evaluate research requires, among other things, good knowledge of the discipline, its research fields and their state of art, to have a yardstick for comparison. In all these processes, power struggles, positioning and boundary work potentially play important roles.

To say that evaluation is a social *practice* highlights that the outcomes of evaluation processes is not predetermined by the object of evaluation, but that there is always a measure of contingency involved in the social process of evaluation. In research evaluation, it means that the quality of the object to be evaluated doesn't just exist as an objective property to be read like one would read a table of numbers. Instead, the outcome of the evaluation process is an achievement that is underdetermined by its object. Clearly, social aspects may enter into the evaluation process in different ways. There is thus a clear parallel between the way that the outcome of empirical investigations are understood as underdetermined by data in science studies, and the way that quality evaluation is underdetermined by the object of evaluation. In neither case does this mean that results are constructed out of thin air, but that if there is not a single necessary outcome of these epistemic processes, social factors may tip the balance in one direction or the other. In research evaluation, it may mean that certain interpretations or quality criteria or, say, relying on a specific technique of categorisation or emphasising some criteria above others as important to the field, may turn the final decisions in different directions.

It is useful to distinguish between two aspects of evaluation practices (Lamont 2012b:206). First, it entails some method or process for *categorising*. That is, evaluators may employ different strategies for determining how to categorise their object of evaluation. For example, they may rely on only their professional judgement and knowledge and their deep feel for quality ("I know it when I see it"). Or, they may rely on more formalised or mechanised techniques such as external ranking systems or metrics. Second, categorisations need *legitimation* or consecration. That is, the value of an entity or validity of an evaluation needs to be justified so as to be recognised by other relevant actors as legitimate. The power of consecration has been a central topic in much cultural sociology following Bourdieu, and deals with the struggle for power over the ability to impose one's taste or judgement on a field. In academic peer review, the selection of reviewers is highly important to the reproduction of the field and its style of reasoning. But legitimation is also a central aspect of every single evaluation practice, where the evaluator not only needs to arrive at a categorisation, but also to justify beyond doubt why a particular result should be accepted as the outcome of the evaluation process.

Scientific peer review, as with any evaluation practice, relies to a great extent on the field-specific habitus of the reviewer, activated in the form of his or her trained judgement. Although peer review involves formal evaluation criteria about which reviewers are often explicit and reflexive, informal criteria also play a very important part in review practices, something that reviewers themselves may acknowledge (Gemzöe 2010; Lamont 2009). These informal criteria may range from the moral character of an applicant to the intangible qualities of a research proposal or paper as exciting, interesting or elegant. This is a site that well illustrates the activation of a scientific habitus, the semi-conscious dispositions and practical sense of what is reasonable, good, exciting and so on, that is seldom explicated but rather functions as the mastery of a practice and its tacit dimensions learnt through practice. But this also explains another prevalent feature of peer review processes, namely the tendency towards *homophily* and system-preserving conservatism.

However, while peer review processes may be imperfect and biased, for example in terms of old boy networks or gender, it is crucial to distinguish *social bias* from *cognitive bias* (Gemzöe 2010; Travis and Collins 1991). Cognitive bias of peer review processes means that it is not a bias against certain individuals or even social groups or categories. Rather, it is a bias in ways of thinking and understanding science. In disciplines with a very high level of consensus, this is unlikely to be a problem. However, if there exist marked intellectual boundaries and minorities, that is, scientific heterodoxies, the effects of what Travis and Collins call *cognitive particularism* will be significant (Travis and Collins 1991). What matters then is from which side of an intellectual boundary evaluators are recruited. However, if such boundaries, as in the case of minority heterodoxies, is not well known or acknowledged, we can expect that peer review processes are marked by such cognitive particularism, and that they will tend to reinforce the dominant style of reasoning.

This insight has been developed in more recent research on peer review, that emphasises not only that scientific quality is a contingent outcome of evaluation processes (rather than a pre-existing objective property), but also that the conceptions of scientific quality are *diversified*, rather than universal (Gemzöe 2010). Furthermore, this diversification forms distinct disciplinary cultures and "epistemic styles" in evaluation, but as Lamont (Lamont 2009) shows in her study on interdisciplinary panels, it is not as simple as a one-to-one mapping of epistemic styles onto disciplines. This translates well to the theoretical framework presented here, and the imperative to look simultaneously at the level of disciplinary styles of reasoning, and at the various ways in which Crombian styles of reasoning may act as barriers or bridges in the maintenance of boundaries with heterodoxy and other disciplines.

Objectivising metrics as judgement devices

The role of professional scientific judgement is central to peer review. But there are also techniques that can be used in evaluation that rely on external quantitative metrics as objectivising tools. The last decades have seen a rapid increase in the availability and use of bibliometric indicators in research assessment and evaluation, a veritable "metric tide" (Wilsdon et al. 2016). As argued in the literature review in chapter 3, recent research at the crossroads between bibliometrics and science studies has started to investigate the *epistemic impact* of the use of bibliometric indicators (Rijcke et al. 2016). In this vein, Hammarfelt and Rushforth have shown how indicators, like authors' h-index, Google scholar citation counts, JIF or journal rankings are used as aids in the evaluation of scientific oeuvres, drawing on Swedish expert evaluation reports from three different disciplines (biomedicine, history, and economics) (Hammarfelt 2017; Hammarfelt and Rushforth 2017). I will draw on their notion that this use of bibliometrics in evaluation reports can be understood as a judgement device, and that we should study how these are employed by evaluators and integrated into their judgement practices. The use of such judgement devices adds a very different social aspect to the evaluation practice, which potentially contributes to the determination of evaluation outcomes.

If the role of the habitus in peer review functions means that the individual evaluator's judgement functions as a subjective mediator in the reproduction of disciplinary standards, the use of bibliometric indicators instead reallocates more of the judgement to the system of academic journals and their editors and reviewers. For example, imagine a reviewer of candidates for a professorship. This expert may rely solely on reading the applicants' submitted texts, using his or her deep knowledge of the field and scholarly judgement to rank the candidates and provide arguments legitimising the ranking in terms of quality criteria. The reviewer may also invoke an external quantitative indicator, like the JIF of the applicants' publications and use it both to *categorise* (that is, base the ranking fully or partly on it), and furthermore to *legitimise* the ranking, where the measure becomes an indicator of quality, impact or similar evaluation criteria. In effect, the evaluation outcome relies to a greater extent on previous evaluations distributed among reviewers and editors at various scientific journals. This way, the evaluation becomes in a sense more objective, but it is nevertheless a social and susceptible form of cognitive particularism, a form of social objectivity where the outcome of social processes, once categorised and quantified into numbers, achieve an air of inevitability and objectivity. This complex and distributed quantification of evaluation is an instance of what Fourcade and Healy (2017) have called a *classification situation*, where new powerful technologies of quantification and categorisation come to take on a life of their own in the ordering of social life.

The important shift emphasised in this literature is the transformation of mechanisms of reproduction of scholarly standards or styles of reasoning, where relative weight has shifted from the judgement of the individual evaluator to the distributed evaluative capacity of an institutionalised system of scientific journals more or less connected to a scientific discipline. In my integration of this insight into the styles framework, I will particularly attend to how evaluators simultaneously reproduce detailed accounts of the disciplinary style and its boundaries, while accomplishing categorisation and legitimation in their evaluations. Furthermore, the extent to which evaluation relies on judgement grounded in the scientific habitus on the one hand, and on various judgement devices on the other, and how these are integrated, should be a central question for investigation. On an overarching level, the question is, to what extent and in what way are peer review processes involved in the reproduction of the disciplinary (or Crombian) styles through cognitive particularism?

7. Concluding remarks: A sociological theory of styles

The theoretical framework I have presented here is an attempt to provide explanatory mechanisms that will make it possible to analyse the empirical material of this study without losing sight of either the macrostructures of styles or the micropractices of economist actors. My framework of *relational disciplinary styles* develops and adapts the styles approach for sociological research. It connects Crombian styles of reasoning, the cognitive macrostructures of science, to their specific manifestations in scientific disciplines, with innovation of the concept of *disciplinary styles* which relies on the notion of disciplinary thought collectives and the dual institutionalisation of scientific disciplines as its institutional setting. The dual institutional framework of disciplines and departments as institutionalised labour markets and training organisations account for substantial institutional stability.

The central link that connect actors to styles within disciplines, is the notion of the scientific habitus as the embodied "commitments and dispositions" of the members of the disciplinary thought collective. Relatively similar dispositions and epistemic tastes are reproduced through thorough and standardised training and practice. The relational nature of styles points to the fundamental role that boundary work plays in science, and that the maintenance of stable boundaries of scientific thought collectives, disciplines and styles of reasoning require the active work of actors. However, I have also pointed to several variables in this theoretical framework that are left open. For example, there may be processes where styles of reasoning bridge boundaries based on similarities in style, rather than reinforcing barriers. The actual role of styles of reasoning in the variable porousness of intellectual boundaries is then up for empirical investigation. Finally, quality evaluation involved in peer review processes plays a central role in the sorting and stabilisation processes in science. Reviewers activate their professional habitus when making quality judgements and categorisations, and may also rely on objectivising judgement devices. In both cases, cognitive particularism may result, which means that through cognitive homophily, peer review processes come to function as mechanisms for the reproduction of the macrostructure of styles. This theoretical framework models, how, *ceteris paribus*, there is a strong social impetus in the form of styles of reasoning, that determine why researchers in an academic discipline think the way they do. Building on historical and philosophical work on styles, I take a small step further by trying to anchor these macrostructures in a theoretical framework that attends to the role of institutions and actors in a relational way.

Chapter 5. Swedish economics: From unique contributions to international integration

Sweden is a relatively small country, but with a research field in economics that is strong for its numerical size. In the early twentieth century, Swedish economists like Knut Wicksell, Gustav Cassel, Eli Heckscher, Bertil Ohlin and Gunnar Myrdal had a great international reputation and influence on the growing international field of economics. The early pioneers of economics in the country around the turn of the twentieth century, and the Stockholm school of economic thought, formed by their pupils in the 1920s and 1930s has been held to be an unparalleled achievement for such a small country (Dixit, Honkapohja, and Solow 1992). The profession is still very strong today. Using per capita productivity measures of recent international top publications, Swedish economics performs at a level far above its neighbouring countries, and the Stockholm-Uppsala cluster of economics departments and research institutes is amongst the most prominent in Europe (Björklund 2014). However, if the founding generation and the Stockholm school represent a homegrown Swedish style of economics, today there are hardly any traces left of a national character of Swedish economics today. Commentators seem to agree that the discipline has become almost completely integrated into an international community and body of work of mainstream economics (Jonung and Gunarsson 1992). Today, Swedish economists publish their work in international journals (preferably highly ranked), doctoral programmes are modelled on those in the United States, and academic careers are increasingly international.

This international integration makes the Swedish case similar to economics in many other Western countries. Thus, there are grounds to believe that findings from Swedish economics also may have bearing in other national contexts. The Swedish economics profession may be of particular interest since it hosts the Nobel committee, influencing the discipline by awarding the Bank of Sweden economics prize (Lebaron 2006). There are other features that set the Swedish case apart. One is the strong presence of economic history as an independent discipline, with an active base of researchers almost the size of the profession in the United States or Britain (Waldenström 2005).

In order to place the empirical analysis in the subsequent chapters more firmly in its historical and social context, this chapter will provide a short historical context for Swedish economics. One purpose is also to introduce the general sociological reader to some developments in the general history of economic thought. Another is to point to some ways in which the history of economic thought cannot be understood without considering its intellectual academic context. That is, I will argue that an important aspect of the history of economics is the history of discipline formation and the splitting off of economics from broader social science. This is the focus of the next section, which sketches the birth of the modern conception of economics in the late nineteenth century. That section is devoted to the general history of economic thought rather than its specific Swedish history. The third section continues with the formation of the discipline up to the Second World War, told as a general and international history of economics. It continues with sketching the post-war stabilisation of the economics discipline, and the parallel origins of heterodox economics in the Anglophone world. Section four turns to the history of Swedish economics, and provides a brief overview of significant intellectual developments during the last century. The final section gives descriptive data about contemporary Swedish economics and institutions, and provides an institutional history of Swedish economics as contextualisation of the history of ideas. It identifies a group of six leading contemporary departments at five universities that will guide the selection of informants and evaluation reports for the empirical chapters that follow.

1. The genesis of modern economics as a historical splitting process

Although economic thinking existed already in antiquity, and there were schools like the physiocrats and mercantilism whose economic doctrines had been influential in explaining the wealth of European nations, modern economics traces its origins to the classical political economy of the late eighteenth and early nineteenth centuries. Adam Smith towers above all as the founding father of modern economic scholarship, with David Ricardo, John Stuart Mill and Malthus also belonging to this crucial period that laid the foundation of modern economics. Marx could also be seen to be firmly rooted in Ricardian political economy, although he saw himself as expanding and overcoming the limitations of that paradigm. Despite internal differences, the classics all shared a general harmony-orientation, with the view that market forces would provide relatively harmonious solutions to social problems rooted in scarcity, and a focus on long-term economic growth and macro-distribution (Landreth and Colander 2002:72).

The core problem at hand in the classical political economy period was to understand the production and distribution of material wealth. In doing so, the concept of a national economy with broad classes (landowners, capitalists and workers) defined by their roles in the production process and respective sources of income (land rents, profits and wages) was a given starting point, and market harmony was coupled with insights in the often conflictual and dismal long-term developments. Still, they were fundamentally supportive of political and economic freedom. Ricardo, and later Marx, worked with different forms of labour theories of value, that is, value in the market of a commodity is determined objectively by the cost of producing it. However, the relative prices of commodities or optimal resource allocation was not their primary interest. Rather, the labour theory of value was a way to understand long-term structural changes in resource distribution, and for Marx, how exploitation of the surplus product of labour was embedded into a system of exchange based on exchange value (Landreth and Colander 2002:75; Marx 1976).

The classical political economy predates the modern academic disciplines. Political economy was a broad area of inquiry that included a multiplicity of aspects that later split into separate social sciences. Consider for example the elements of moral philosophy in Adam Smith, or the analysis of power relations in Marx, whose work only became canonised as a sociological classic much later (Levine 1995). Similarly, Landreth and Colander (2002:73) emphasise that the classical political economy contained the seeds of both modern orthodox and heterodox economics, with its simultaneous belief in the harmony of the market and insights into its dismal tendencies. The scientific revolution and beginning of the long splitting process that led to modern mainstream economics occurred in the so-called marginal revolution of the 1870s.

At this time, four authors independently brought about a new understanding of value, initiating a long process that would eventually bring about the emergence of neoclassical economics that more or less still forms the core of the modern discipline. The three central figures in this marginal revolution were Stanley Jevons, Leon Walras and Carl Menger. The fourth contributor and great synthesiser of the new marginal utility theory of value was Alfred Marshall in Cambridge, England. From then on, the discipline divorced the moral-political, social and historical aspects of the economy, and strove to become a pure science modelled after the natural sciences, building upon abstract deductive methodology and searching for general and universal principles of economic phenomena. The neoclassical economics of 1870–1930 neglected macroeconomic questions about growth and distribution, and focused instead on the microeconomic issue of how markets under competition function to allocate scarce resources with alternative uses (Landreth and Colander 2002:219). Marginalist economics adopted a fundamentally deductive approach, most pronounced in Walras's general-equilibrium approach where the formal logic of a system analysed simultaneously in its interrelations was the focus. An offshoot of the marginal revolution that illustrates the tensions it generated was the great German *Methodenstreit* of the 1880s, where the marginalist conception came into conflict with the historicist, empirical and inductive approach of the so-called historical school in Germany. The German tradition of multifaceted and descriptive particularistic economic history had already clashed earlier in the century when prior generations of the historical school dismissed the universalist and deductive pretentions of Ricardian economics and British utilitarianism.

In their extensive historical account of how the economics discipline became divorced from the social and historical aspects of economic processes, Dimitris Milonakis and Ben Fine (2009) devote considerable attention to the effects of the marginal revolution and the *Methodenstreit*. I will devote some attention to their account, since it sheds light on the contemporary mainstream-heterodoxy divide, and puts it in historical relief.

The main combatants in the methodological battle were Carl Menger, who had published his major contribution to marginalism in 1871, and Gustav Schmoller, the leading proponent of the historical school at the time. Their main exchanges took place in 1883–1884 when Menger launched a frontal attack on the historical school and what he felt was its lack of a proper conception of economic analysis. According to Milonakis and Fine (2009), marginalists and the historical school were in agreement that classical political economy was in need of reinvigoration or replacement. However, opinions differed greatly on what the problem was. The marginalists shared a basic conception of economic analysis as an abstract and deductive undertaking with Ricardo and the classics, to which the historicists had been principally opposed since the early years of the century when Ricardo was active. However, the marginalists were highly critical of the cost of production theory of value, and other aspects of the Ricardo-Mill economics they considered defunct. Furthermore, the marginalists emphasised a positivist conception of economics that had also been inherent in some of the classics (like Mill) that economics should strive to become a pure theoretical and positive science, strictly separated from a notion of economics as an art, or the sort of moral-political questions inherent in earlier political economy conceptions, like Adam Smith. This attempt at a very rigid separation between what Walras termed "pure economics" on the one hand, and values and politics on the other hand, was a part of the project to put economics on par with the natural sciences. The marginalists furthermore shifted the focus of analysis from a societal macrolevel to the microlevel, towards the (hypothetical) economising behaviour of individuals. This was something that lay at the very heart of Menger's approach, and he promoted what he called "atomism", the methodological imperative to always reduce economic processes to the interactions of individuals, a principle that Schumpeter would later rename "methodological individualism" (Milonakis and Fine 2009:106). Menger criticised the historical school for falsely analysing collectives or institutions other than as the sum of their parts, the results of individual action.

The historicist Schmoller criticised Menger for falsely attributing universal laws to particular social phenomena, and asked in his 1873 critical review of Menger's attempts at abstraction: "Is the author not herewith reviving the old, slanted English fiction, namely that economic life could be properly derived from the constant basic driving force of the abstract average man?" (Quoted in Milonakis and Fine 2009:104). However, despite the recurrent historical interest attributed to the battle, it did not last for very long, and both sides seemingly entered into a spirit of reconciliation a decade later. The often-narrated story of how one purely deductive and universal theoretical side (Menger) stood against another purely inductive, historical and antitheoretical side (Schmoller) seems unwarranted, given the explicit testimonies on either side afterwards that both are needed in economic analysis. However, Milonakis and Fine also point to the fact that perhaps more than purely methodological issues, this battle was about both substantial and epistemological problems. First, it seems that Schmoller stood for a descriptive and Humean view of causality (with an interest in whether events actually seem to follow in regular sequences as grounds for attributing causality), whereas Menger stood for an Aristotelian essentialism, striving to abstract the essences of phenomena in isolation.

Furthermore, a second and important aspect is the relation between the emerging Bismarckian social policy, with its interventionist and regulatory approach, against the laissez-faire critique of the same from the followers of pure economic theory. In a telling account, Menger's student Ludwig von Mises, the founder of modern Austrian economics, gave his view of the controversy:

> The government of Bismarck began to inaugurate its Sozialpolitic, the system of interventionist measures such as labor legislation, social security, pro-union

> attitudes, progressive taxation, protective tariffs, cartels, and dumping. If one tries to refute the devastating criticism leveled by economics against the suitability of all these interventionist schemes, one is forced to deny the very existence—not to mention the epistemological claims—of a science of economics [. . .]. This is what all the champions of authoritarianism, government omnipotence, and "welfare" policies have always done. They blame economics for being "abstract" and advocate a "visualising" mode of dealing with the problems involved. They emphasize that matters in this field are too complicated to be described in formulas and theorems. (von Mises quoted in Milonakis and Fine 2009:114)

The reverse view of the situation is similarly telling—that theoretical economics represents a Panglossian liberalism and political free market advocacy that masquerades as positive economics. From a sociological and naturalistic vantage point, we can conclude that when contemporary struggles rage over the soul of economics and its relation to social and political issues, the basic themes and the positions seem to have been rehearsed before.

If the *Methodenstreit* functioned to clarify two opposing positions on what economic analysis ought to be, it is clear that in hindsight, the marginalists won, even if this debate in itself played a minor role. If there was a single conception that proved important for the new pure economics, it was the notion of marginal utility. In the words of Joseph Schumpeter, "the concept of marginal utility was the new ferment which has changed the inner structure of modern theory into something quite different from that of the classical economists" (quoted in Milonakis and Fine 2009:98). Milonakis and Fine (2009:110) identify the central effects of the marginalist victory as three forms of reductionism that narrows the scope of economic research. First, an *anti-historicist* reduction, squarely placing economic analysis outside all attempts at historicising economic analysis, that is, considering the historical evolution and transformation of economic phenomena. Second, an *individualist* reduction, the framing of analysis in methodological individualist terms, connected to a third *asocial* reductionism, whereby economic processes become abstracted from all other social relations, including the concept of class. These reductions imply two further things: first, that focus is not only shifted toward the individual, but to a very specific type of economically optimising behaviour. Second, "the economy" and "the economic" become identical to the market, while broader social relations in which real economic processes are embedded fade into the background of exogenously given variables.

The void that this implies was soon to be filled with the new academic disciplines of sociology and economic history, consolidating the disciplinary rift between the economic and the historical and social, according to Milonakis and Fine. The consolidation of neoclassical economics by the 1930s is heuristically illustrated by the work of Lionel Robbins in giving economics its best-known modern definition, in an act of strategic boundary work. This is not to say that a phrasing of the task of economics influenced the development of the discipline, more than it is an emblem of a process that had already taken place, and as an example it offers great insight into the process. Simultaneously, this story also offers a window into the consolidation of modern sociology as the other side of this splitting process where new disciplinary boundaries are drawn.

Defining a discipline: Narrowing the boundaries of "the economic"

How should economics be defined? One of the best-known definitions today is Lionel Robbins's (1932:15) formulation: "Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses". But is this not a rather limited view of the nature of the economic? In his history of economic thought, Roger Backhouse (2002:4) argues as much, stating: "It is perhaps ironic that Robbins's definition dates from 1932, during the depths of the Great Depression, when the world's major economic problem was that vast resources of capital and labour were lying idle". Backhouse (2002:4) instead uses a competing conception, borrowed from Alfred Marshall, that economics is "the study of mankind in the ordinary business of life", or to be more precise, that "economics deals with the production, distribution and consumption of wealth or, even more precisely, is about how production is organised in order to satisfy human wants". Explicit or implicit definitions of economics have been a contested topic, as they are related to how the subject matter and proper methodology of research is conceived.

Robbins's definition has played a key role in the consolidation of the modern discipline (Milonakis and Fine 2009). This influential definition was not just introduced out of the blue, as something to be quoted in introductory textbooks almost a century later. Robbins (1932) explicitly crafted his "scarcity" definition against the competing "materialist" definition that he connected to Marshall and, indeed, to Marx. For Robbins, a materialist definition of economics is also connected to the Marxists and their historical materialism, and his redefinition allows him to decisively pull economic science away from any such project. He is quite explicit about this:

> The Materialist Interpretation of History has come to be called the Economic Interpretation of History, because it was thought that the subject-matter of Economics was "the causes of material welfare". Once it is realised that this is not

> the case, the Materialist Interpretation must stand or fall by itself. Economic Science lends no support to its doctrines. (Robbins 1932:43–44)

This is not to say that Robbins's primary goal in redefining economics was its anti-Marxist uses, but it was surely a welcome by-product. These definitional attempts are clearly also a fundamental form of boundary work, where the symbolic boundaries of the discipline are reworked and provided with rational justification.

Robbins's redefinition of economics in the 1930s is part of the great transformation of economics from a nineteenth century political economy centred on the production and distribution of use-values on a societal scale, to the modern marginalist conception of the economics of economising actors that Robbins represents. In an essay on the fate of the *material* in economics, Richard Swedberg captures this transition. He notes what seems like the disappearance of the material from economics around the turn of the century, and claims this slow shift "may be related to the disappearance of the term use-value from the vocabulary of modern economics, and the related attempt to replace it with a more subjective terminology, such as 'utility' and 'preferences'" (Swedberg 2008:78). According to him, the disappearance of anything material in modern economics can be clearly seen in the pioneering formulation of an ideal *homo economicus* by Frank Knight in his *Risk, Uncertainty and Profit* in 1921. It is generally agreed that this idea was first formulated by John Stuart Mill in the mid-nineteenth century and then expanded and explicated by Knight. However, while this "heroic abstraction" makes a range of claims about "complete rationality", action without restraint, perfect competition and information, the theory is absolutely free from material bodies, technologies, material goods or objects, or for that matter, a physical world with geographical properties (Swedberg 2008:78). Knight's formulation serves as an illustration of the shift, which can be described in Marx's classic wording, with a twist: "When Marx famously said that 'all that is solid melts into air,' he was thinking of the corrosive impact of bourgeois conditions on feudal values, but his statement also fits the transition from political economy to modern economic theory when it comes to materiality" (Swedberg 2008:79).

A later example of just how entrenched the non-materiality of Robbins and Knight has become is the famous definition of economics by Gary Becker as "the combined assumptions of maximising behaviour, market equilibrium, and stable preferences, used relentlessly and unflinchingly"; simultaneously, in relation to the older definition that Robbins called "materialist", Becker claims that "the definition of economics in terms of material goods is the narrowest and the least satisfactory''(Becker quoted in Swedberg 2008:57–58).

This leaves us with two seemingly opposing definitions of the economic. One is the "materialist" conception of the economists of the period of classical political economy like Adam Smith and David Ricardo, but also of Marx and later Alfred Marshall. The materialist conception studies the production, distribution and consumption of use-values. Against this stands what Robbins called the "scarcity" conception, which sees economics as the science of the logical relation between scarce means and ends. This is the science of rationally choosing and calculating actors, portrayed in the "heroic abstraction" of the *homo economicus*. But the two definitions do not only map onto the historical shift from political economy to marginal economics, they also seem to map onto the divide between economics and economic sociology represented by Swedberg. But there are very strong affinities with elements in economic sociology and heterodox economics, where both define themselves oppositionally and draw strong boundaries with mainstream neoclassical economics. There are also links between economic sociology and heterodox economics in an emphasis on culture, institutions and historicism.

2. Neoclassical economics and interwar pluralism

The period between the two world wars is often acknowledged to be one of intellectual pluralism in economics, as opposed to the strong reliance on a single approach in the post-war period. This situation was perhaps most marked in US economics, which would play a central role after the Second World War in shaping the new international discipline. But there were also at least three significant theoretical developments in Europe that diverted from neoclassicism, more or less explicitly in response to the Great Depression and mass unemployment (Sandelin 1991a:8). Most importantly, John Maynard Keynes developed the theory of effective demand as an explanation of how markets may fail. In Austria, Friedrich Hayek laid the foundation of the Austrian school, still today considered a heterodox school of thought by some. And in Sweden, the Stockholm school stood for a unique but short-lived approach that developed around the notion of dynamic method, that independently paralleled Keynes to some extent (Hansson 1991). However, since US economics came to dominate international economics almost completely after 1945, it is relevant to look at the country's economics landscape before the end of the Second World War.

During the interwar years, two viable and influential positions emerged in US economics, the neoclassicals and the (American/ Old) institutionalists. In their introduction to a special issue on interwar pluralism in US economics, Mary S.

Morgan and Malcolm Rutherford (1998:23) claim that the long controversy between these two parties was in no way predetermined (as in Whiggish accounts) to be a victory for the neoclassicals: "the decline of pluralism in US economics was neither a simple nor an obvious result of the development of neoclassical economics and vice versa. No logical relation says that this must have been so, nor does the evidence support such a direct causal story."

Early twentieth century US economics was characterised by a genuine pluralism of positions on economic analysis, as well as on policy advice. Morgan and Rutherford further point out that this was fundamentally married to a view of science that did not exclude advocacy from the role of the scientist, and it was fundamentally field- or problem-oriented. What was changing was the general conception of science and objectivity towards a view where advocacy became increasingly suspect and objectivity less connected to personal trust and character, and instead became a question of proper tools and methodology. This observation, in line with historian of science Lorraine Daston's work on the history of conceptions of objectivity, was connected to the rise of a new type of economist:

> Economists who could rely on such technical methods no longer had to be so scrupulously evenhanded or to depend so entirely on their virtues. These technical approaches created a new kind of professional expertise that enabled economists to offer "objective" policy advice, for they could argue that the objectivity of their methods warranted the objectivity of the results of the analysis and of the associated policy advice. (Morgan and Rutherford 1998:9)

Furthermore, the set of "objective" methodological equipment rather than the earlier personal virtues would come to function as a defence system for the economist against attacks by political opponents: "the turn to technical expertise (rules of calculation, mathematical formulas, and statistical data) provided economists with a defense of their analysis against attacks by those promoting political agendas or those with strong opposing values" (Morgan and Rutherford 1998:9). This is also evident as an important mechanism in later periods, as in the case of Paul Samuelson's consolidation of a very technical type of economics.

However, there are also other, more external, factors that played important roles in the transformation. Among these are, first, the gradual but steady shift in general societal values towards positive views of free market solutions: "The moment at which society's values line up with those of economists is a point to watch" (Morgan and Rutherford 1998:14). Furthermore, an important historical event in the transformation of economics is the role played by economists in the US war effort, where many were drafted to work alongside engineers and mathematicians to solve the practical problems of warfare, and did so well when they

> turned their techniques to any number of wartime questions, using simple mathematical optimising models, linear programming techniques, and statistical measurement devices. Economists were brought in to fight the war directly, planning the optimum bombing-raid design and statistically analysing firing patterns. Economists found that by using tool-kit economics and the developing neoclassical technical expertise they could answer questions in very different fields. Economics emerged from the war covered in glory, perhaps launching the "economic imperialism" in social sciences over the last half century. (Morgan and Rutherford 1998:13)

The post-war outcome was not simply on a direct continuum with the neoclassicals, but a new breed of formalist economics. From a slightly different approach, Yuval Yonay (1994:42) also directs a searchlight onto the methodological character of the interwar disputes. He has studied the controversy between neoclassicals and institutionalists as a rare case of scientific controversy that revolves not around the interpretation of a particular phenomenon or a specific theory, but rather about the soul of economics itself— "What should economics be like?". What this controversy in a rather unique period of intellectual pluralism in the history of economics shows, is *that the principles of scientific judgement are themselves at stake,* a notion that aligns nicely with the notion of self-authenticating styles. Yonay explains that:

> previous writers have often shown how rival approaches claimed allegiance to the same scientific ideals of objectivity, rigour, empirical grounding and so forth. But too often the analyses assumed that there was a way to determine whose pledge was a "genuine" one, and whose was "false". [. . .] "Scientific method" cannot be viewed as the arbitrator between the conflicting paradigms in economics, because both paradigms claimed to have spoken in its name. Like Nature, "the Scientific Method" cannot speak for itself. (Yonay 1994:67)

The mathematisation of economics that took definitive shape has several aspects. The historian of science Roy Porter (2001) warns not to think that the history of measurement has been connected to developments in pure theory, whether in the natural sciences or in economics. Rather, quantitative measurement has always been a practical matter, connected to the world of practice, commerce, and government. The integration of empirical quantitative measurement (as opposed to deductive mathematical modelling) was integrated simultaneously into economics and government during the 1930s and 1940s, not least with the development of econometrics on the one hand and, on the other, systems of national accounting that produced empirical data. But in sum, measurement in economics was a tie between the universities and the practical world of government:

> Notwithstanding the prominence of university social science and basic research, the modern history of economic measurement remains in important ways a practical one, a history of bureaucratic devices as well as scientific ambitions. Increasing technicality is not necessarily a mark of disciplinary autonomy; often it is an adaptation to this world of applications. [. . .] [T]he process might best be characterized as one of mutual adjustment, of the reshaping of economics and government through reciprocal interactions. (Porter 2001:19)

3. The stabilisation of the discipline after 1945 and the origins of heterodox economics

The outcome after the experiences of the Second World War was a new formalist "toolkit" economics with a new professional style that offered consensus policy advice founded on the new conception of objectivity connected to a set of impersonal techniques rather than the old set of personal scientific virtues. Economists now had a new way to approach and cut up problems into small and tidy pieces to be solved and, as a bonus, were given a protective shield against accusations of political advocacy. This position was further strengthened by the cold war nationalism and the McCarthyism of the immediate post-war period (Morgan and Rutherford 1998).

One of the most important developments in the formation of the post-war paradigm in economics was the inclusion of the newly-established field of macroeconomics, pioneered by John Maynard Keynes before the war. He belonged to an older Marshallian variety of marginalism that saw as its main task as tackling practical problems, and it is no surprise that Keynes and the neoclassicist Lionel Robbins were long-time opponents. Keynes to a great extent explicitly pointed to the underlying philosophical and methodological problems of neoclassicism, and argued for the impossibility of economics becoming an exact science. Instead, he meant, it should rather be thought of as a moral science, where radical uncertainty (as opposed to calculable risk) played a fundamental role (Milonakis and Fine 2009:ch 14). However, Keynes's macroeconomic theory was soon popularised in the form of a highly simplified formalised model representation of the *General Theory*, namely John Hick's famous IS/LM model. Later, Paul Samuelsson would incorporate this model of Keynesianism and marry it to neoclassical microeconomics in what he called a "Great Neoclassical Synthesis" in his widely-used textbook *Economics*, thus popularising the concept of "neoclassical economics". Keynes' intellectual heirs would contest the label "Keynesian" as it has normally been used, and instead point to the heterodox implications of the General Theory beyond its formalised model, preferring the term "post-Keynesian" for their own intellectual project.

The roots of modern heterodox economics

The neoclassical conception of economics was consolidated in the immediate post-war years, laying the foundation of the modern discipline as we know it today. According to the historian of heterodox economics Frederic Lee (2009:14), neoclassical economics is characterised by a "theoretical core that comprises primary theoretical concepts and propositions that are accepted unconditionally", that are not (in principle possible to be) empirically tested or evaluated, and from which secondary assumptions are deduced. Among such unquestionable core assumptions are agents' rationality, for example. Lee furthermore shows through a survey of over a hundred textbooks how a set of 29 identified core neoclassical tools and models have become more and more prevalent in a range of well-used economics textbooks during the last century. For example, between 1911 and 1940, economics was only defined in terms of allocation of scarce resources in 19 per cent of sampled textbooks, whereas 86 per cent of textbooks used this definition in the 1971–2002 period. Similarly, price was defined as marginal cost in 38 per cent of textbooks in the interwar period, but 100 per cent of 74 sampled textbooks between 1941 and 2002 (Lee 2009:3 Table 1.1).

Another aspect that plays an important role in Lee's account is his view of the closed and controlling nature of the mainstream that stands in stark contrast to the conscious intellectual pluralist attitude of heterodox economists. The various schools of thought that are identified under Lee's (2009:190) heterodox umbrella are Austrian, institutional-evolutionary, Marxian-radical, post-Keynesian, Sraffian, social and ecological economics. However, it is obvious that some schools have been more prominent than others and have played a more important role in the history of heterodox economics. In Lee's account, it is fair to say that the two heterodox schools that fill most of the narrative are the post-Keynesian school, and what he calls the Marxian-radical school.

In this account, the last few decades of the nineteenth century saw rising social unease and unemployment, strikes and growing social unrest, combined with the growing labour movement, which together led to an increasing interest in the study of political economy, soon to be relabelled "economics" in the 1890s. Among late nineteenth century economists there was growing support for socialism and homegrown American populism among the followers of Henry George, but US economists became divided between the proponents of government regulations and reforms, and a majority of free market supporters (Lee 2009:24). With the introduction of the new neoclassical economics, most notably through Marshall's *Principles of Economics*, and the spectre of class conflicts, a new consensus on neoclassical price theory and the search for politically "safe topics" ensued, according to Lee. Only the Georgists, Marxists and Veblenian institutionalists remained unaffected by the marginalist theoretical turn, but were increasingly targeted by attempts to save the moral order. This search for "an economic theory that would promote the status quo and hence be appropriate to teach to students" by academics, administrators and business interests with funding relations to academia was combined with the professionalisation of academic economics. This latter process entailed, most importantly, attempts to cleanse academic economics of normative or ethical content in the search for objective science: "positive economics" (Lee 2009:26). In this process, political advocacy that questioned the functioning of the capitalist system became anathema.

By 1940, neoclassical economics had been almost universally established in economics departments in the United States and Britain (the two countries covered in Lee's study), and virtually all American universities were teaching the same thing; indeed, "they were drilling it into their students" (Lee 2009:27). In previous years, growing interest in socialist politics and teaching of Marxian economics had been curbed by the "red scare" whereby academics sympathetic to the workers' movement or Marxist ideas were spied upon, institutions and departments branded Bolshevik hotbeds, and some economists discharged from their institutions (Lee 2009:31). In a similar fashion, growing interest in New Deal style government intervention and the new Keynesian economics led to political radicalisation as well as countermeasures involving the FBI keeping dossiers on students and professors who attended rallies and meetings or joined socialist organisations (Lee 2009:32). These anti-radicalisation practices were intensified during the post-war McCarthyist anti-communist fever, when, for example, over thirty states required academics to take oaths of loyalty towards the United States, leading to lost positions or job opportunities for those who refused (Lee 2009:37).

McCarthyism was not only a direct threat to members of the Communist party or even Marxist economists. Even Keynesians promoting the American synthesis of Keynesian interventionist macroeconomics and neoclassical microeconomics, popularised by Paul Samuelson's immensely influential textbook *Economics* "were targeted by letter writing campaigns, in the popular media and by legislators" (Aspromourgos 2008; Giraud 2014; Lee 2009:38). This is the general story throughout Lee's book: the theoretical status quo in economics has been protected not only within the profession and through the regular academic channels where it has influenced control over publication outlets, recruitment and promotion, and research funding, but also by organised conservative business interests and, at some points, in cooperation with the state, as in the FBI involvement during the McCarthy period.

Other forces have however operated to establish heterodox alternatives within economics, including establishing a critical mass network of economists interested in similar theoretical perspectives or issues, conferences and workshops for interaction, publishing outlets for scholarly communication and publication opportunities, newsletters keeping the community together, and in some instances even institutional bases, for example at the University of Massachusetts in Amherst and Rutgers University, Brooklyn (Lee 2009:72). The impetus for the rapid expansion of what Lee terms radical-Marxian economics was the New Left and student activism of the 1960s, spurred not least by the activism against the Vietnam War. The *Students for a Democratic Society* played a pivotal role, as it was here that the ideas of establishing a radical professional organisation for economist grew, leading to the formation of the *Union for Radical Political Economy* (Lee 2009:59). Other important institutions included the journal *Monthly Review* and the associated new Marxist economics of Paul Sweezy, Paul Baran and others around the journal, but also European Marxist authors like Ernest Mandel. However, the "old left" and the orthodox Marxist literature of the interwar years played a very minor role in this movement, according to Lee.

The rise of post-Keynesian economics in the United States occurred in the same environment but was, in Lee's account, largely the work of a few individuals in the early 1970s, among them Paul Davidsson, Alfred Eichner and Sidney Weintraub, who actively built up a network of likeminded economists not least with the help of the Left Keynesian (and former colleague of Keynes) Joan Robinson who was invited over from Cambridge on several occasions. The group began to consistently use the term "post-Keynesian" to refer to themselves in the early 1970s, and by 1978 the *Journal of Post-Keynesian Economics* was established as an important publishing outlet for this group (Lee 2009:86).

The situation in Britain was in many respects similar by 1945. Although there had been an interest in and teaching of Marxism in the schools of the Independent Working Class Education movement, Marxism was more or less excluded from the universities by the end of the war, when neoclassical economics was established everywhere (Lee 2009:108). Existing alternative academic approaches included a handful of Marxists, among them Maurice Dobb at Cambridge and his student Ronald Meek. The other vital heterodox school in Britain was Left Keynesianism associated with Joan Robinson, whose engagement with Marxist thought can be seen as its starting point. It is also associated with Michal Kalecki, Nicholas Kaldor and Luigi Pasinetti and their development of Keynes's work, and in general the radical notion (compared to Keynes) that effective demand generated by private investment would never be enough to reach full employment in a capitalist system (Lee 2009:120).

As in the United States, the New Left was an important impetus for heterodox economics in Britain. Important publishing channels were founded, like the *New Left Review* in 1960, which originally carried a lot of Marxist economic analyses. In 1970 the *Conference for Socialist Economists* was established, and its journal (later renamed *Capital and Class*) became an important forum for heterodox economists. Although the 1960s and 1970s saw heated debates between post-Keynesians and Marxists in the wake of Piero Sraffa's influential *The Production of Commodities by Means of Commodities* in 1960, regarding the status of the Marxian labour theory of value, Lee argues that heterodox economics was in general pluralist in orientation, engaging in debates with other schools of thought, for example on the issue of the theory of value.

A key development that Lee (2009:189) puts centre stage, and that that I termed the "Lee thesis" in chapter 2, is the historical formation of heterodox economics in the 1990s and early 2000s. Prior to this period, a few economists, notably institutionalists, had used "heterodox" to refer to themselves as early as the 1930s. Apart from that, there were different schools of thought or followers of individual theorists (Marxists, Sraffians, etc.) and a general acknowledgement of the shared critique of mainstream neoclassical economics. However, in the early 1990s, economists from various schools increasingly started to talk of themselves as "heterodox", which started to become a collective identity. This was followed by the establishment of heterodox professional organisations; Frederic Lee (2009:192) himself was the originator of the *Association of Heterodox Economics* and the *Heterodox Economics Newsletter* in 1999. Another important electronic publication was the *Post-Autistic Economics Review*, later renamed and developed into today's open-access journal *Real World Economics Review*. Importantly, and mirroring these developments, there was also increasing theoretical integration and pluralist inter-journal exchange, so that "by 1990 many heterodox economists could no longer see distinct theoretical boundaries between the various approaches, an outcome that mirrors the professional integration already taking place" (Lee 2009:201).

The bottom line of the Lee thesis is that only *after* the theoretical integration of the community of heterodox economists and the historical crystallisation of their binary opposition with mainstream neoclassical economists did it become possible to conceptualise the history of economics in these dualistic terms. The owl of Minerva watches, and nods approvingly.

The macroeconomics counter-revolution and the broadening of economics after the 1970s

In the post-war decades, the new field of macroeconomics built upon the neoclassical synthesis became highly technical and mathematised. This development is sometimes known as the "formalist revolution". The breakdown of this neoclassical consensus came in the 1970s, when Keynesian macroeconomics and policy broke down in the face of the problem of "stagflation", introducing Milton Friedman's monetarism and the "new classical economics", based on the theory of rational expectations, as a contester to the Keynesian focus on effective demand (Pålsson Syll 2007:399). Since then, macroeconomics has seen a struggle between these two broad positions. With the consolidation of the new formal neoclassical approach around the Second World War, economics was divorced from its final remnants of social and historical analysis, and became an abstract science of *choice*, in the view of Milonakis and Fine (2009). This created space for the establishment of the durable disciplines of economic history and sociology to study the areas ceded by economics.

The discipline had sufficiently narrowed and sharpened its core analytical apparatus so that it was on the one hand divorced from the analysis of real market economies, and on the other hand ready for ventures into territories outside its borders. If the analysis was reduced in Robbins's classic formulation from a conception of the social and material processes of production and distribution of wealth to any abstract situations of choice, there was no reason why this analysis should not be extended across history to other forms of economies than market economies, and also to non-economic forms of choice as the principal force that drives human behaviour.

The prime example of this economics imperialism is the "economic approach" of Gary Becker, which extended the marginalist model of the atomistic rationally calculating self-interested actor into fields of study that lay well beyond the confines of economics. This extension of the economic approach is not only important for understanding the development and confidence within the economics discipline, but as a driving force of the counter-reaction from sociology through the development of the new economic sociology in the 1980s.

During the last few decades, a widespread narrative has emerged that economics is indeed changing, and turning away from a narrow neoclassical mainstream because of recent theoretical developments and refinements (Colander 2000).[51] One of the new subfields of economics that is often mentioned in this context is behavioural economics and, in connection with it, the even more recent turn towards empirical behavioural experimentation. Behavioural economics traces its roots to the work of Herbert Simon on bounded rationality in the 1950s, but its great development happened during the 1990s, and was marked by the Nobel Prize awarded in 2002 to Daniel Kahneman and Vernon Smith (Weber and Dawes 2005). In their overview of the field, Roberto Weber and Robyn Dawes give the following definition:

> Behavioral economics is the combination of economics and other, more behaviourally descriptive, social sciences. More precisely, behavioural economics results when economists combine research and methods from economics and other social sciences with the goal of improving the descriptive value of economic theory. (Weber and Dawes 2005:91)

However, two points should be noted. First, "other social sciences" primarily refers to psychology, and this is manifest in both the focus of interest (cognitive bias, conceptions of fairness etc.) and methods (experiment) used (Weber and Dawes 2005:102). Second, behavioural economists seek to reform standard economic theory rather than to revolutionise or replace it with something else. In this sense, behavioural economics has strong similarities to the new institutional economics which likewise tries to reform economics by building further on existing fundamental assumptions although modifying some of them.

According to Weber and Dawes, behavioural economics has done important work in three main areas. Its main areas of focus are non-egoistic preferences, intertemporal choice, and reference-dependence in preferences. To an outsider, these developments may seem common-sensical or mere curiosities. For example, "non-egoistic preferences" means that behavioural economists have shown through repeated experiments (for example so-called dictator or ultimatum games, often performed on college students) that people are not as egotistical as predicted

[51] See also the review of these debates in chapter 2.

by the classical *homo economicus* assumption. Instead, behavioural economists have shown that students (the experimental subjects) given a sum of money to share as they see fit with an unknown co-player (a dictator game) tend to be somewhat fair without technically having to be; they show a preference for fairness aside from self-interest. Similarly, researchers have shown that individuals are not so rationally calculating when it comes to comparing the utility of using some resource today with possibly using it in the future. Instead, there are behavioural explanations in terms of various cognitive biases or impulsivity.

Behavioural economics is an important development in economics. However, its seemingly interdisciplinary character is very much a question of borrowing some small parts about behavioural assumptions mainly from psychology to try to alter specific assumptions in some circumstances, rather than replacing the core assumptions of the discipline. And this process seems to take a very long time, despite its seemingly modest claims. For example, when it comes to self-interest: "While formal economic theory relaxing self-interest now exists, the models are often specific to a particular type of problem. More importantly, and partly because of the lack of a general model, the traditional approach is still the basis for an overwhelming majority of research within economics" (Weber and Dawes 2005:96). It is clear that we are still looking at a science of choice in Robbins's sense. Behavioural economics may look new and promising, but it is not very different from the rest of the twentieth century's marginalist tradition that stripped away the social and historical aspects that once were a part of political economy.

4. Swedish economics: From unique contributions to Anglo-American absorption

I have laid out the history of modern economics, considered as an international phenomenon, in rough outline, and turn now to the specifics of Swedish economics, which to a large extent parallels to overarching picture. However, when it comes to the specific question of the relation between a unique Swedish economics and the international research field, there are a few noteworthy features. First, although there was a history of chairs in political economy in Sweden stretching back to the eighteenth century, modern marginalist economics was firmly established by a group of "founding fathers" around the turn of the twentieth century. The founders, including Knut Wicksell and Gustav Cassel, represented a first burst of creativity of international level, and their work both took inspiration from and was read by an international community of economists. Second, following the founders, the so-called Stockholm school developed a uniquely Swedish school of thought in the 1920s and 1930s, which had some lingering influence until the 1950s. The achievements of these early economists has been hailed as an unparalleled contribution to economics by a small country. Whereas prominent individual economists have emerged from, for example, Norway and the Netherlands, the rise of a *group* of economists with such influence is seen as remarkable by a 1992 evaluation of the field, *Economics in Sweden*. The international evaluators, Dixit, Honkapohja and Solow (1992:129), emphasise that "those episodes are without parallel in the modern history of economics. No other small country has produced anything genuinely comparable".

However, the evaluators see no reason to expect something similar happening again, since Swedish economics has become integrated into the international field:

> over the past three or four decades the literature of analytical economics has become almost completely homogeneous worldwide. Mainstream economists in all countries now contribute to a single international literature as part of a single intellectual community. (Dixit et al. 1992:129)

The historian of economic thought Bo Sandelin gave the same verdict, arguing that there is no reason to write the history of Swedish economic thought after the Second World War, since

> After the Stockholm school there is hardly any such thing as a unique Swedish economics. It has, with a few important exceptions [. . .] been absorbed by, or, rather, has been eager to join, the Anglo-American mainstream tradition, which is, of course, not literally Anglo-American. (Sandelin 1991a:9)

Following this line of argument, the sketch of the history of Swedish economic thought presented below will emphasise the unique contributions and the increasingly smaller differences between it and the international intellectual community of modern economics.

The prehistory of modern economics in Sweden dates back to 1741, when Anders Berch took the first chair in political economy at Uppsala University. This was the fourth chair in political economy in the whole of Europe, following those established in the preceding decades in Germany (Lönnroth 1991:18). During the nineteenth century, influences came from Germany with the historical school, and from England with the works of Smith, Ricardo, Malthus, Mill and others, together with political and economic liberalism, which lagged in Sweden, just like industrialisation and the capitalist transformation (Lönnroth 1991). Although the historical school dominated during the first half of the century, it is often downplayed in historical accounts, since "[h]istory is written by victors, i.e. by economists brought up in the classical/neoclassical tradition, who seldom regard the writings of the historical school, with its inductive method and organic conception of the State, interesting" (Sandelin 1991a:4).

In the very first generation of modern Swedish economists, Knut Wicksell had already published three major books in German before being appointed to the professorship at Lund University, giving him international fame, especially for his work in the theory of capital. Unlike his peer David Davidson at Uppsala University, he fully embraced and worked within the new marginalist theory of distribution in the 1890s, and was thus the first modern economist in Sweden (Sandelin 1991a:6). Wicksell was a radical social liberal, known in his time for creating outrage, and even served a two-month prison term for publicly "ridiculing the holy word of God" in a 1908 speech. An important aspect of Wicksell's work was his interest in growth and business cycles, phenomena he understood in relation to uneven bursts of technological development that causes fluctuations in an otherwise static equilibrium state (Pålsson Syll 2007:251). However, he explained the distinction between the business cycle and crises by the former being caused primarily by investments in fixed capital, while the latter were caused by false expectations. Like a rocking horse, sudden exogenous shocks could set it in motion, but the properties of the horse itself—society and its psyche—would determine how and to what extent the impulse would resonate and spread (Pålsson Syll 2007:252).

Gustav Cassel of Stockholm University was among the internationally best-known economists of his day, "probably the best-known economist internationally prior to the rise of Keynes", according to Jonung and Gunnarsson (1992:23). Eli Heckscher started out as Cassel's assistant, and became known primarily for his work in economic history and his study of mercantilism. He also became the first professor in the new subject of economic history in 1929 at the Stockholm School of Economics, pioneering an approach combining historical methods for collecting data with neoclassical theory to analyse the data (Sandelin 1991a:7).

If these founders of Swedish economics were strongly internationally oriented, a group of talented students of Cassel and Heckscher in Stockholm came to develop economic theory in a unique direction in a series of works that broke with the liberalism of their predecessors. They took an interventionist position that they developed independently, but which was strikingly close to the one developed simultaneously by Keynes in England. The idea of a distinct Stockholm school was coined in an *Economic Journal* article by Bertil Ohlin in 1937 and included a body of work since 1927 primarily by Dag Hammarsköld, Erik Lundberg, Erik Lindahl, Gunnar Myrdal, and Bertil Ohlin (Hansson 1991). Sometimes, Ingvar Svennilsson is also included among its members (Pålsson Syll 2007:386). It should be noted that Ohlin's construction of a coherent "Stockholm school" has been questioned and that it was in many ways a loose group. Like Keynes, the Stockholm school emphasised the role of expectations in economics analysis. However, a fundamental difference compared to Keynes, and its *differentia specifica* was the development of the *dynamic method* as a way out of static equilibrium analysis. Myrdal introduced the idea of anticipation and planning with the notion of ex ante expectations in planning, and ex post outcomes at the end of a period, which becomes the start of new adjusted expectations (Hansson 1991). These themes were later developed in the focus on the dynamics of economic planning by companies and governments.

Like Keynes, the Stockholm school opposed lowering wages as a means to solve the unemployment problem. However, while Keynes was pessimistic about the prospect of remedying instabilities caused by lack of investments, the Stockholm school members were more optimistic about the possibility of using public investment to create effective demand to reduce unemployment. Whilst cautionary about price stability, expansionary politics would be able to also drive private investment (Pålsson Syll 2007:392). While the Stockholm school in a stricter sense only existed roughly during 1927–1937, it did not suddenly die with the rise of Keynes, but had a lingering influence at least until the 1950s. One reason for its decline was the practical problem of using its advanced dynamic methods, as even its members admitted (Hansson 1991:213).

After the Stockholm school, "only stray signs of anything that could be called a Swedish way of economic thinking" remained, according to Sandelin (1991a). The verdicts of the international evaluation of Swedish economics as well as Sandelin's history of economic thought, both published in the early 1990s, are in agreement that the mainstream of Swedish economics has eagerly become part and parcel of an international research field in the post-war years. In the words of Jonung and Gunnarson, who compare the institutions of Swedish economics to US economics in the 1992 international evaluation:

> Academic research in Sweden is now part of the international economics marketplace, in roughly the same way that research by economists in the state of Michigan does not differ much from the research of their colleagues in other parts of the United States. Swedish economists active in research are often linked into various international networks. They generally accept American models and techniques and try to improve upon these. There is no common principles

> textbook in Swedish. This important section of the literature is completely dominated by the standard American products. (Jonung and Gunarsson 1992:47)

Among the few stray signs Sandelin mentions are the institutionalism of the later Myrdal, the trade union economics of the Rehn-Meidner model, and the legacy of Johan Åkerman. I address these three phenomena briefly, since they are all relevant to understanding potential deviations from the international mainstream, and add a brief note on the virtual absence of Marxism in Swedish economics.

Gunnar Myrdal became internationally well-known and established as a leading institutional economist after *An American Dilemma* (Myrdal 1944; Pålsson Syll 2007:278). While his dissertation and starting signal for the Stockholm school was a theoretical work, Myrdal had encountered American institutionalism on a visit to the United States in the late 1920s. *An American Dilemma*, today probably better remembered by sociologists than by economists, was a very broad social scientific enterprise. Myrdal was critical of economic orthodoxy, and had already criticised its inherent market liberal bias and falsely positive character in *Vetenskap och politik i nationalekonomin* (Myrdal 1930). The central target of his critique was the idea of equilibrium. He emphasised that all real economic processes always move away from equilibria, and that there are no warrants for the harmony assumptions of free market proponents. Instead of equilibrium, he emphasised the notion of cumulative causation, borrowed from Veblen and Wicksell, exemplified by processes like the Matthew effect or vicious circles (Pålsson Syll 2007:281). Despite his international fame, and position as the first director of the Institute for International Economic Studies (IIES) in Stockholm, Myrdal exerted little influence on a Swedish economics profession that was moving along other tracks. Sandelin's verdict is that, unlike Bertil Ohlin whose

> contribution to the theory of trade can be considered an achievement within the neoclassical paradigm—a result of "normal science", to use Kuhn's term—Myrdal was fundamentally an economic heretic. As such he seems to have exerted more influence on researchers in other disciplines than on mainstream economists. (Sandelin 1991a:218)

An important example of economists working mainly outside academia, but influencing economic theory to some extent, are the trade union economists Gösta Rehn and Rudolf Meidner, whose Rehn-Meidner model became official economic policy, often known as the "Swedish model". They worked on a critical development of some aspects of Keynesian expansionary politics already in the 1940s, and developed their full model in a 1951 report for the Swedish Trade

Union Confederation, LO (Erixon 2003; Pålsson Syll 2007:393). The report was an output of LO's research department, where both economists worked at the time (Erixon 2011a). Their aim was a politics for full employment, while guarding strongly against inflation, which would be the outcome of too expansionary policy. Instead, they proposed a generally restrained model with active labour market policy and selective expansionary measures, combined with the important element of solidarity wage policy where strengthened trade unions would demand wages rising with productivity, while forcing less productive sectors into automation and structural adjustment. The model became a backbone of Social Democratic government policy through the 1960s (Pålsson Syll 2007:394). There are clear connections back to the Stockholm school, and especially some of the fundamental ideas from Myrdal, according to Erixon:

> Both Meidner and Rehn considered themselves as Myrdal's disciples. Rehn often expressed a spiritual affinity to Myrdal. These fathers of the "Swedish model" inherited, for instance, Myrdal's doubts about axiomatic-deductive theorising in economics. They also shared Myrdal's scepticism, typical for the Stockholm school, towards wage cuts as a remedy against recession. (Erixon 2011a:98)

However, the actual influence of the Stockholm school, or for that matter, the legacy of Johan Åkerman and the Swedish growth school of structural analysis, was limited. Instead, Erixon (2011a:117) points to the original character of the Rehn-Meidner model, both as a theory and as a social innovation in the form of a complete policy package. It was also unique in the way that it built on practical experience and intuitions from central wage negotiations: "The RM model was primarily based on intuitive, experience-based theorising by Rehn and Meidner in their role as trade-union economists, not on deductive economic modelling, or even on economic research in a conventional sense". Although the model had an international breakthrough in the late 1960s, changing macroeconomic orientations in the face of declining profitability in OECD countries meant that the model became out of tune with the new macroeconomics of the 1970s (Erixon 2011a:113).

If Myrdal's later institutionalism has become internationally renowned, another strand of economic analysis has played less of an international role, but influenced later Swedish theory development somewhat more. This is the tradition of structural analysis, initiated by Johan Åkerman at Lund University in the 1940s. Åkerman was dissatisfied with the lack of causal realism and dynamics in neoclassicism. He was inspired by both Schumpeter and Veblen and, importantly, found established economic analysis and its equilibrium assumptions and lack of structural and historical analysis at fault. Instead, he argued, economics needs true causal analysis which reconstructs actual sequences of events (Pålsson Syll 2007:276). Åkerman argued that all the economic principles analysed by economists are really structure-dependent and only valid for a specific period when a certain structure exists. Therefore, the analysis of structural transformations lies at the core of causal analysis. According to Pålsson Syll (2007:507), Åkerman was the most important European institutionalist before the war, besides John Hobson (Pålsson Syll 2007:277). In Pålsson Syll'ss account, Åkerman's legacy also forms the main strand of Swedish heterodoxy, followed by the work of Erik Dahmén and Ingvar Svennilsson in the same vein, an alternative institutional theory of business cycles with slightly different focus from that of Schumpeter (Erixon 2011b). However, this tradition of structural analysis has primarily been influential among economic historians, where Lennart Schön and others have connected it to a Swedish version of a neo-Schumpeterian programme of empirical analysis of long waves and structural transformations since the 1970s (Pålsson Syll 2007:451).

Swedish economists have participated in political life and public debate to an unusually high degree. This is especially true for the founding generation, who produced vast numbers of articles for the daily press. For example, Wicksell wrote some 450 newspaper articles, Cassel 1,500 in *Svenska Dagbladet* alone, and Bertil Ohlin over 2,000, while Myrdal produced a mere 50 pieces for the daily press (Carlson and Jonung 2006:513). In the views of Lars Jonung and Benny Carlsson (2006:512), "In Sweden, economists probably have more influence than any other category of social scientists. In other countries there is usually a wider gulf between academically active economists and the world of politics and the media, more so in the United States than in Europe". Several leading economists have also had close relations to, or been central players in, political parties. For example, of those already mentioned, Ohlin was the leader of the Liberal Party, while Myrdal was a Social Democratic member of parliament in the 1930s, and later served as trade minister in a Social Democratic government. Swedish economists have often been heavily involved in producing various government reports besides their purely academic work, and this has meant that the work was both more domestic and problem-oriented, and published in Swedish (Jonung and Gunarsson 1992). The 1992 international evaluation looks rather critically at this tendency of what the international evaluators call "routine studies" (Dixit et al. 1992; see also Sandelin 2000:67).

Many prominent economists, from Gunnar Myrdal to Assar Lindbeck, have had close ties to the Social Democratic party. This was a central social and political force in twentieth century Sweden, and the strong role of the labour movement meant that economists like Rehn and Meidner, who had one foot in academia and the other in the movement, for a while had considerable status even among academic economists (Erixon 2011a). However, economists with Social Democratic ties also played an important role in keeping the discipline all but completely free from Marxist influences. For example, Marxist economist Johan Lönnroth recalls how the University of Gothenburg's economics department had a lively group of Marxist economists heavily influenced by the New Left in the early 1970s, to the detriment of the new professor and social democrat Bo Södersten (Lönnroth 2011).

Similarly, Stockholm University saw the short-lived group *Kritiska ekonomer* (critical economists) in the late 1960s, influenced by the wider group *Unga filosofer* (young philosophers), who introduced and took a strong interest in continental philosophy and theory of science (Nycander and Agell 2005:170). The group was influenced both by American neo-Marxism and French structuralist social theory, and was founded with a critical stance towards the economics mainstream in mind. The group believed that economics had difficulties grappling with the real problems of society, and neither economists nor the general public were aware of the value premises and outlook that grounded economic theory. The virtual absence of any Marxian economics in Sweden is in fact emphasised by historians of thought. For example, Jonung and Gunnarsson claim that

> a peculiar feature of Swedish economics is the almost complete absence of any influence or impact from Marxism. To my knowledge no Swedish economist, after becoming a professor of economics, has ever openly declared himself to be a Marxist. This absence of Marxism may be partly due to Knut Wicksell's harsh criticism of Marx. Wicksell was highly regarded in the social democratic movement as well as in the economics profession. Unlike the older generation, the members of the Stockholm school who were drawn to the left did not accept any communist or Marxist influence. The 1968 New Left had no impact on Swedish economics. In this context Assar Lindbeck adopted a stance that was representative of the attitude of most economists. (Jonung and Gunarsson 1992:45)

As indicated, Assar Lindbeck's (1971) critique of the New Left (Lindbeck 1971) was probably quite influential both in an international and the domestic context. The influence of Marxism was thus already kept at bay by the founding fathers while establishing modern economics around the turn of the twentieth century, and this heritage of boundary keeping was maintained throughout the century by the elite of the profession.

On the other hand, Jonung and Gunnarsson note that in the neighbouring discipline of economic history, Marxists gained a "strong foothold" in the 1960s and 1970s, and still exerted some influence in the early 1990s. In an international context, the role of the discipline of economic history in Sweden is fundamental for understanding the history of heterodox economics in Sweden. In a 2003 paper on the international exposure of economic history in Sweden, Daniel Waldenström (Waldenström 2005:11–12) estimates the size of economic history communities, and concludes that the Swedish community is probably almost as large as the British or US community *in absolute numbers*, and that the three largest economic history departments in the world are probably located in Sweden. Thus, it is a fair guess that the discipline of economic history have served as a safety valve for economics, and, from the point of view of aspiring researchers discontented with the economics mainstream, as a good alternative career option. This brings us to the institutional infrastructure of Swedish economics.

5. An institutional history of Swedish economics

The institutional conditions for modern academic economics were established around the turn of the twentieth century with the founding generation. Over a relatively short period of time, chairs in economics (as they are understood today) were established at Swedish universities and colleges. David Davidsson was appointed to the first chair at Uppsala in 1889, followed by Knut Wicksell at Lund in 1901 (temporary; made permanent in 1904), Gustaf Steffen to a chair in economics and sociology at the University College (later University) of Gothenburg in 1903, and Gustav Cassel to a chair in economics and public finance in 1904 at the University College (later University) of Stockholm, while Eli Heckscher was appointed to the newly-established private Stockholm School of Economics (SSE; not to be confused with the Stockholm school of thought) in 1909 (Jonung and Gunarsson 1992). Among these, Davidsson, Wicksell, Cassel and Heckscher can be called the four "founding fathers" of Swedish economics (Jonung and Gunarsson 1992:20), and established the discipline's high level of international ambition in Sweden, as well as its journal, *Ekonomisk tidskrift* (in 1889 by Davidsson, today *Scandinavian Journal of Economics*).

In their contribution to the 1992 international evaluation, Jonung and Gunnarsson point to the role of the professors and chairs in Swedish economics for most of the twentieth century. Lacking large department organisations compared to those in the United States, the history of Swedish economics has largely been the "history of its professors", commonly with only one or two academics per department at least during the first half of century. After the first wave of institutional positions listed above, new departments with professors in economics were added at the Gothenburg School of Economics (1923), IIES at Stockholm University (1962), Umeå (1965), and the Swedish Institute for Social Research (SOFI) at Stockholm University (1972) (Jonung and Gunarsson 1992:26). Smaller departments with professorships have been established since the 1990s at the new universities in Karlstad, Linköping, Luleå, Linnéuniversitetet, and Örebro. The total number of professors rose above 25 only in the late 1980s, most of them in the Stockholm area (Jonung and Gunarsson 1992:26). During the 1980s, there was a new wave of chairs established as external funding opportunities increased. From 1993 the hiring procedure was deregulated and devolved to the university whereas it had previously rested on government decisions. This also contributed to an increasing number of professors being hired, reaching a total of 57 in 1996 (Sandelin 2000:60).

The institutional development of economics in Sweden to today's internationally-oriented discipline has been a long process, involving a shifting language of sources and publications, research publication formats and outlets, and not least a slow and lagging transformation of doctoral programmes towards the US model. Despite a 1968 reform of doctoral programmes, the 1992 evaluation of Swedish economics found that Sweden lacked proper US-style doctoral programmes, and identified addressing this as an urgent recommendation (Wadensjö 1992). Compared to the United States, several features were found to be lacking in Swedish economics, namely, "a common Ph.D. programme, a common professional organisation like the American Economic Association with its prestigious journals, and a common 'paradigm'" (Jonung and Gunarsson 1992:38). This last feature is somewhat surprising, given that most commenters talk explicitly of a common framework or paradigm that became established during this time. This statement should be read in the context of a comparison with an ideal of US economics, and we can then understand the authors' sense of a lack of common paradigm *in comparison* with the United States. The authors also note a clear convergence, where US academic values and standards are taking over, and an older tradition of writing monograph dissertations and other publications in Swedish is fading away:

> The emphasis is now on the rigorous application of mathematical and statistical techniques. Doctoral candidates aiming at an academic career write their theses in English, attempting to build upon the latest international results. In this way the corps of Swedish economists is becoming professional to an extent unmatched before. There are signs that the skills and knowledge of a good Swedish economics Ph.D. are slowly converging with those of a Ph.D. from a good American department. (Jonung and Gunarsson 1992:47)

Today, doctoral programmes have come a long way since the 1992 evaluation. Theses are written as compilations of papers in English, and there are well-structured doctoral programmes; in the Stockholm area these are in the form of a collaboration between different departments, the Stockholm Doctoral Programme in Economics (Nycander and Agell 2005:186). Internationalisation has brought about a strong shift in cited literature, from over 60 per cent of cited works in Swedish dissertations being written in Swedish in the 1940s, to less than 10 per cent in 1990–1995 (Sandelin 2000). However, Sandelin argues that this internationalisation is not linear, but in fact somewhat cyclical, with a very low share (around 20 per cent) of Swedish references in the interwar decades. Internationalisation has also brought about the mathematisation of economics, as evidenced by the average share of pages of dissertations that contain mathematics and econometrics rising between the 1940s and the 1990s from almost none (1 per cent maths and 0 per cent econometrics) to a considerable proportion (around 30 per cent and 10 per cent) (Sandelin 2000).

Research in economics is conducted at university departments of economics, and at university and non-university research institutes. For a long time, up until the 1990s, there were six university departments: Gothenburg, SSE, Lund, Stockholm University, Uppsala University, and Umeå. There is one academic research centre which stands out. At Stockholm University, the IIES has long been the foremost centre of international-standard economics research. Founded by Gunnar Myrdal in 1962 as a broad research institute, it became an important centre for the reinvigoration and internationalisation of Swedish economics under the leadership of Assar Lindbeck from 1971 (Nycander and Agell 2005). However, rather than a broad interdisciplinary institute, under Lindbeck the IIES came to be a bridge to Anglo-American economics research, and arguably an central institutional driver for the internationalisation of Swedish economics and the definition of top-quality economics research. In the 1980s, IIES received the largest share among all departments, almost 25 per cent of total Swedish faculty grants or 21 per cent of total funding for economics research (Stenkula and Engwall 1992). Looking at its publication activities, IIES contributed a third of all Swedish articles in international economics journals (indexed by SSCI) in the 1970s and 1980s (Persson, Stern, and Gunnarsson 1992). In a 2003 ranking exercise, top authors and departments in Swedish economics were given a score based on the then-novel and influential ranking system developed by Kalaitzidakis et.al. (Kalaitzidakis et al. 2003). This concluded that the three top authors (Lars E O Svensson, Assar Lindbeck and Torsten Persson) were quite far ahead of others; all were active at the IIES, and the institute stood out clearly as the leading department in terms of this particular scoring system (Lindqvist 2003).

A recent study by economist Anders Björklund examined publications in six top international economics journals by authors with a Swedish address during the twelve-year period 2002–2013 (Björklund 2014). Like the 1992 evaluation, he finds that in international comparisons, Sweden has high productivity in top economic journal articles per capita. Of the 65 identified publications, IIES has produced thirty, or almost 50 per cent of all top publications, followed by SSE (fifteen), and Stockholm University, Uppsala University, the private research institute IFN, and the interdisciplinary institute at Stockholm University, SOFI within the range of five to seven articles. Only one of 65 top publications (from Lund) originate outside what Björklund calls the Stockholm-Uppsala geographical cluster. Björklund furthermore looks at the general orientation of research, and notes that the international trend towards empirical data shown by Hamermesh (2013), is also evident in Swedish research, with a large share of studies using unique empirical data, but where the connection to economic theory is sometimes lacking, while there is also a large share of purely theoretical articles. However, he concludes that while the general public might think of economics as dealing primarily with macroeconomic cyclical phenomena, this is not really the case: among the 65 top articles, "I actually find it hard to see any product that deals with the problems that were actualised by the economic crisis that started in 2008" (Björklund 2014:17).

In the Swedish system, six universities with economics institutions are amongst the largest and best-established research universities (Henrekson and Waldenström 2011:1151). These are Lund, Gothenburg, Stockholm, Uppsala and Umeå, and the SSE. All have had economics doctoral programmes for over forty years. Beside these, the large university reform of the 1990s granted university status to a number of colleges, some of which have now started doctoral programmes in economics. If these six stand out regarding heritage, the picture shifts somewhat when one looks at their role in producing new generations of economists. Using metrics for contemporary undergraduate and graduate education gives us another measure of the relative size of economics departments.

During the three academic years 2011/2012 through 2013/2014, a total of 2,077 first-cycle bachelor exams in economics were awarded in Sweden. Among these, 73 per cent were awarded by five departments: the SSE and the universities of Stockholm, Uppsala, Gothenburg and Lund, with each producing over 200 bachelors during the period. Sixth according to this measure is the younger University of Linköping with 116 bachelor degrees. Umeå ranks ninth in this measure with only fifty students awarded exams. The master's degree was recently introduced in the Swedish university system, gradually replacing the older *magister* degree as part of the Bologna process. During the same three year period both degrees were rewarded as second-cycle or advanced degrees, totalling 1,583 awarded degrees. Of these, more than 50 per cent were awarded by two departments, at Lund University and the SSE. Together, the five largest departments accounted for 81 per cent of all advanced degrees.

The most important phase in the education system for understanding disciplinary reproduction is arguably the third-cycle doctoral programme. Doctoral exams are represented in the statistics for calendar years instead of academic years. During the four-year period 2011–2014, a total of 181 doctorates were awarded in economics, of which the five departments produced 80 per cent. Among these, the SSE awarded 39 PhDs, while Lund only awarded nineteen, ranking fifth on this measure. The sixth, Umeå, awarded nine doctorates during the same period.

These metrics are reflected in the increasingly common international university rankings. For example, in the 2015 QS World University Rankings by Subject, which draw on a wide range of indicators, the big five are the only Swedish economics departments present in the listing, with the SSE ranked 31st, and the remainder in the 50–200 range (QS Stars 2015). This is also in line with the somewhat older but widely recognised rankings of journals and departments by Kalaitzidakis, Mamuneas and Stengos (2003). In their ranking of European economics departments, the picture is only slightly different. Four Swedish departments are found in the top 120, with SSE seventeenth, Stockholm University 24th, Uppsala 43rd, Lund 78th and Umeå 91st, and Gothenburg is left out (Kalaitzidakis et al. 2003).

Even though rankings differ slightly in the different measures, it should be uncontroversial to view the five economics departments (SSE, Stockholm, Uppsala, Gothenburg and Lund) together with IIES as the core six institutions of contemporary Swedish economics. Based this overview they will be considered the key Swedish economics departments for the empirical parts of this study.

6. Conclusions

This chapter has provided a background context through drawing a few big lines in the history of economics in general, in order to contextualize the previous chapters, and to serve as a very general introduction to the history of economic thought. It then turned to the Swedish context to concretise and localise that history. In the first section, I described the history of economics as a splitting process where elements of historical, social and institutional analysis from the earlier political economy was left outside the boundaries of the new and more narrow conception of scientific economics. This process originated in the late nineteenth century marginal revolution, and the new neoclassical economics was increasingly solidified during the interwar year. This process was illustrated with the famous attempt to define the scope of economics by Lionel Robbins in terms of the science of choice. In this definition, Marxian and other materialist conceptions of economics as the science of the production, distribution and consumption of material goods, were explicitly excluded.

While the new neoclassical approach emerged before the Second World War, the American interwar period has been characterised by historians of thought as a period of pluralism with a number of competing schools of thought. However, following the war, the modern neoclassical and highly technical conception of economics became firmly established in the United States, and slowly spread further in the Western world. This consolidation of the modern economics discipline was paralleled by the marginal existence of heterodox schools of thought, not least institutional, post-Keynesian, Marxist and radical economics that saw a great upswing as part of the rise of the 1960's New Left.

Turning to the Swedish context, the history of economics is marked by a generation of founders in the beginning of the nineteenth century that had a remarkable international influence, considering the small size of the country and its few economics chairs. Following this generation, the Stockholm School represented another innovative period, although it was not strictly held together and failed to have more than marginal influence in the post-war period. The institutional history of Swedish economics is one of very small numbers, aptly described as a history of its few chaired professors. Only after the 1970's did modern larger economics departments with American style doctoral programs slowly emerge, a development that the 1992 international evaluation of Swedish economics saw as very promising, although still ongoing. The evaluators agreed that Swedish economics had now left all marks of national characteristics, and was becoming increasingly integrated into the international US-led discipline. Although a larger number of universities today offer both undergraduate education and doctoral programs in economics, when the metrics on top ranked researchers and the production of doctorates is weighed together, six research departments clearly stand out as dominant by those standards. These are the universities of Lund, Gothenburg, Stockholm, and Uppsala, the SSE and IIES.

Chapter 6. Methods and material

Apart from the reviews of various literatures and earlier research presented in previous chapters, this thesis draws on two different bodies of empirical material. The first comprises twenty in-depth interviews conducted with twenty Swedish economists in 2015–2016, and the second is an analysis of expert evaluation reports of professorship candidates, collected from four top universities, covering a 25-year period (1989–2014). In this chapter, both general methodological considerations and some more specific questions are presented, together with an introduction to the empirical material, selection processes and a brief description of the analytical process. The chapter is divided into two, corresponding to the two parts of the empirical material.

1. Interviewing economists

The decision to use an interview study as one of the two empirical studies was driven by several factors. The overarching problem this study attempts to understand, the dynamics of the styles of reasoning of the economics discipline, could have been studied using a variety of approaches. As covered in the literature reviews in chapters 2 and 3, as well as the historical overview in chapter 5, there has been quite a lot written on similar topics. A lot of this literature exists within the history of economic thought, especially by those authors inspired by various STS approaches. However, most of those studies examine scientific writings, that is, the finished products of scientific knowledge production. Following the general thrust both of the STS field, and of the more recent turn towards the sociology of social knowledge (Camic et al. 2011), my aim is to reach closer to actual knowledge-producing practices and the knowledge producers themselves. The method of choice in science studies has long been ethnography. While this could have been an interesting and viable option, there is an obvious problem with ethnographic fieldwork on something that, after all, takes place to such a large extent “in the head”. Of course, scientific seminars, conferences, and in this case perhaps also doctoral coursework, should be interesting sites to study ethnographically. On a yet smaller scale, collaborative research work, and even shadowing single researchers, could have been good methodological options.

However, since my core interest is also in "sociologizing" the concept of styles of reasoning, and exploring how it can be understood on the level of individual actors and a scientific habitus, I also needed to get closer to the actors and be able to elicit ideas, conceptions and dispositions that may not naturally emerge. For, as I will argue, interviewing can fruitfully be understood in a realist sense as similar to experimentation, in that it may bring out and produce phenomena that do not (often) occur spontaneously in nature or society. In the following sections, I will start with a very general discussion about the epistemology of interviewing, and move towards the more concrete questions of interview techniques, selection of informants, and the handling and analysis of the material.

The epistemology of interviewing—three views and their problems

The approach to interviewing employed here is close to that in the well-known handbook *Doing Interviews* by Norwegian psychologist Steinar Kvale (Kvale 2007), and, like Kvale, I am indebted to Bourdieu's fine methodological piece "Understanding" (Bourdieu 1996). The latter combines lessons for the practical craft of interviewing with a sound epistemological framework for thinking about sociological interview research. Kvale is probably one of the foremost authorities on qualitative research interviews in the social sciences, with widely-read handbooks on the topic (see also Kvale and Brinkmann 2009, 2014). To briefly explain this approach to interviewing, it can usefully be contrasted to, first, an older view of interview methodology inspired by positivism, second, a hermeneutical view, and third, the currently more widespread constructivist approach.

The positivist approach treats the interviewee as a "vessel-of-answers" (Gubrium and Holstein 1999, 2001b; Marvasti, Holstein, and Gubrium 2012) and the interviewer as a neutral "miner" of information (Kvale 2007:19). If the informant is a vessel containing information to be mined by the miner-interviewer, one of the central methodological problems becomes how to retrieve the valuable information-ore without the miner contaminating it. The interviewer must be neutral in order not to affect the interviewee. Such neutrality is often framed in terms of practical advice like dressing properly and avoiding leading questions at all costs. Such a "vessel view" is based upon the ontological assumption that there already exists something (beliefs, ideas, opinions, etc.) out there in the informants for the researcher to collect as-is. Furthermore, the empiricist epistemology emphasises the neutral observer who neutrally perceives empirical reality as the prime road to knowledge. The goal is exact and generalisable knowledge, framed in terms of validity and reliability. If you ask a range of informants the right questions, they will tell it like it is. However, you must not influence the informant by formulating questions that are suggestive or introduce bias of any sort, which will ruin the validity of the study. There is a whole literature in witness psychology that experimentally shows the large extent to which the formulation of questions may shape answers.

A major problem with the positivist conception is that it grossly underestimates the role of the interviewer. For example, Kvale emphasises that interviewing is a craft rather than the neutral application of methodological rules. Instead, the interviewer's person is the research instrument. Against what he calls the "bureaucratic conception of method", where "the ideal interview would be an interviewer-free method", he posits the highly trained and skilled interviewer exercising judgement rather than context-free rules of method as a prerequisite for high quality interview data (Kvale 2007:48). The insight that the understanding of human existence requires another human has always existed as a parallel to positivism in hermeneutics and in anthropology fieldwork, which has been a main source of influence for contemporary qualitative methods. According to this conception, qualitative interview research is all about *understanding* the rich particularities of actors' life-worlds. Questions of validity and objectivity are replaced by the search for rich authentic descriptions of particular local settings. If the interviewer can get access to and listen carefully and attentively to the informant, the researcher may enter into the meaningful life-world of the informant. Ontologically, according to the hermeneutic conception, there are universes of meaning out there to discover (just like in the positivist conception), but epistemologically, there can be no separation of knower and known, no neutral outside observer. There is no escape from the researcher using his or her self as the only viable "instrument" of knowing.

The caricatured conception of interviewing I label "constructivism" is also formed as a critique of positivism, but in a more radical sense than hermeneutics. Paraphrasing Marx on commodity fetishism in *Capital* (1976), Jaber Gubrium and James Holstein claim that if "[a]t first glance, the interview seems simple and self-evident" (Gubrium and Holstein 2001a:1), a closer examination shows that it is more intriguing than that. Gubrium and Holstein (2001a:13) turn against the vessel-of-answers view of interviews, where "the subjects behind respondents are basically conceived as passive *vessels of answers* for experiential questions put to them by interviewers. Subjects are repositories of facts, feelings, and the related particulars of experience" (emphasis in original). Instead, they claim that the discourse produced in an interview is *constructed in the act of interviewing*. It is not a representation of something that already existed out there (i.e. in the vessels).

Taken in its pure form, such constructivism rests on an ontology of radical *becoming*, where persons do not hold any opinions or have any personalities, cultures, habitus or what have you, prior to the act of performing/ constructing belief etc. in the act of speech. Here, the interviewer is active in a much stronger sense (compared to the hermeneutic position), in that the interview discourse is a co-construction by interviewer and interviewee. The critique of the vessels-of-answers view has its merits. It leads us to consider the extent to which narratives are actively constructed by the agencies of both interviewer and interviewee as participants in the interview situation, and to the way that the subjectivity expressed by interviewees may in fact belong to different subject positions and voices (Gubrium and Holstein 2001a:22). For example, the respondent may speak from the position of individual experience, or as a representative of the profession, or perhaps as a citizen, shifting subjectivities during a single interview. It also leads us to think beyond individual subjects, instead focusing on the institutional discursive environment of subjects (Gubrium and Holstein 2001a:26). This means that the origin of beliefs and opinions must be sought beyond the individual, in the institutions that provide distinctive ways of speaking and interpreting everyday life. A similar point is made by Bourdieu, who turns against what he calls the naïve personalism in some interview research that doesn't understand how persons are not as unique as they may appear, but instead always products of social structures (Bourdieu 1996:27).

Interviewing as Socratic sociological realism

Let me now try to elaborate the conception of interviewing that informs this study, based primarily on Kvale (Kvale 2007) and Bourdieu (Bourdieu 1996), and filtered and refined through a largely critical realist understanding of social scientific research (see Bhaskar 1998; Sayer 2000). This conception draws heavily on insights from the other three positions, but also tries to remedy their respective weak spots. From positivism, we learn to avoid leading questions and adverse effects of the interviewer. From hermeneutics, we learn the necessity of understanding through the active and personal engagement of the interviewer. From constructivism, we learn that the interview is a setup, a constructed situation with a constructed outcome. Added to that, the conception I am proposing introduces a number of ideas.

First, the "sociological" aspect of interviewing is one of Bourdieu's central claims. Bourdieu, like Kvale, argues that interviewing must be understood as a craft that requires experienced and active interviewers. But Bourdieu is not a constructivist emphasising the local construction act of the interview, like Gubrium and Holstein. He positions himself against two common conceptions: the scientistic, rigorously methodological, stance of the positivist tradition, which seeks to be free of any influence of the interviewer. On the other hand, he is equally critical of what he terms the "antiscientific" advocates of a hermeneutical approach that seeks the "mystic union" of interviewer and interviewee in a supposedly distortion-free melting together of understandings (Bourdieu 1996:18). The main obstacle to understanding subjects in Bourdieu's view is the *objective social distance* between the interviewer and the interviewed, which inevitably influences and distorts the social exchange.

However, the only way to counter the distortion caused by social distance is through the interviewer's sociological grasp of the social conditions that structure the subject's life through giving oneself

> a *general and genetic comprehension* of who the person is, based on the (theoretical or practical) command of the social conditions of which she is the product: a command of the conditions of existence and the social mechanisms which exert their effects on the whole ensemble of the category to which such a person belongs [. . .] and a command of the conditions, psychological and social, both associated with a particular position and a particular trajectory in social space. Against the old Diltheyan distinction, it must he accepted that *understanding and explaining are one.* (Bourdieu 1996:22–23, emphasis in original)

Contra the positivist non-interference view, Bourdieu emphasises the craftsmanship of interviewing centred on calibrating the effects of social distance and countering them continuously during the interview situation. This goes beyond the mechanic implementation of a methodology, it requires what he calls a "*reflex reflexivity*" that "enables one to perceive and monitor *on the spot*, as the interview is actually being carried out, the effects of the social structure within which it is taking place" (Bourdieu 1996:18). Such a conception is founded upon a realist social ontology of pre-existing social structures (contra radical constructivism).

Second, interviewing involves *active and methodological listening*, which goes beyond hermeneutic understanding or constructivist co-construction. In this conception, the *active* interviewer acts as a Socratic midwife who uses encouragement, attentiveness and follow-up propositions, helping to deliver the subject's "truth" which was *already out there* (contra Gubrium and Holstein), but which required an ideal and constructed situation (contra the positivist emphasis on interviewer neutrality) for the informant to deliver ideas and conceptions that are deeply held, but that would not be shared under most social circumstances.

This is in line with Kvale(2006), who claims that there is a common but problematic conception of interviews as dialogues. The interview method has sometimes been depicted as more egalitarian than "the objectifying positivist quantification of questionnaires", giving voice to common people in a gentle, mutual and caring way (Kvale 2006:481). However, such a conception of interview research is as false as notions of dialogue in contemporary management, politics or education, because it is blind to inbuilt power asymmetries. This he asserts, is a false view:

> In contrast to the mutuality of the dialogue, in an interview, one part seeks understanding and the other part serves as a means for the interviewer's knowledge interest. The term *interview dialogue* is therefore a misnomer. It gives an illusion of mutual interests in a conversation, which in actuality takes place for the purpose of just the one part—the interviewer. (Kvale 2006:483)

As an alternative to the conception of interview-as-dialogue, Kvale proposes a range of interview practices that acknowledge this fundamental power asymmetry. These alternative conceptions all share an *agonistic* component, which means that the interviewer should actively follow up on answers and provide some form of resistance to the interviewee. The level of conflict ranges from what Kvale (2006:486) calls the "*Platonic dialogue*", mentioned above. This may be suitable for expert interviewing, "where the interviewer confronts and contributes with his or her conceptions of the interview theme". The interview then becomes a conversation that stimulates both parties to formulate and sharpen ideas that were perhaps not formulated previously. A more agonistic interview style would be what Kvale (2006:487) calls the "*actively confronting interview*", where the goal is not consensus, but where the interviewer confronts the informant with critical questioning if, for example, the informant contradicts herself. The goal is not to impose the interviewer's ideas on the informant, but to uncover and make explicit the informants' hidden assumptions. This is the Socratic interview style employed by Bourdieu in *The Weight of the World*, held forth as a prime example of interview craft by Kvale (Bourdieu 1996; Kvale 2007). The practical implication of this is the imperative to actively and critically follow up on the interviewee's answers, to probe for assumptions and to take a maieutic approach to conversationally formulating conceptions that were perhaps not already consciously formulated by the informants.

Third, high quality "spontaneous" accounts of informants' life-worlds are produced not through the *passivity* of the interviewer, but in a carefully *constructed* interview situation with an active "Socratic" interviewer. This conception of interview epistemology is in turn based on a realist ontology:

> Thus, against the illusion which consists in searching for neutrality through the elimination of the observer, it must he admitted that, paradoxically, the only "spontaneous" process is one that is constructed, but it is *a realist construction.* [. . .] It is only when it rests on prior knowledge of realities that research can bring the realities it wishes to record to the surface. (Bourdieu 1996:28)

The realities Bourdieu speaks of here comprise the inherently structured social world that invisibly shapes us as social beings. In the present study this is, for example, the contours of a scientific habitus shaped through socialisation into the economics discipline, pointing to structures beyond individuals as their carriers.

Against Gubrium and Holstein's constructivist view, we see here a robust formulation of a non-naïve realist conception of the ontology and epistemology of interview research. One can compare this to a caricatured empiricist/ positivist versus a scientific realist understanding of experimentation in the natural sciences. Whereas the empiricist would claim that experiments work through observing event regularities in a controlled setting (without researcher bias/ interference) to generalise about causality, the realist would point to the necessarily highly constructed nature of experiment. To perform even the simplest experiments in classical physics, one needs to carefully construct the experimental situation. We cannot usefully study gravity in spontaneous events in nature (since very few things are preoccupied with just constantly falling before us), so we need to carefully construct inclined planes and perfectly round balls to roll down them to observe how the real but unobservable law of gravity produces effects that can be empirically observed. Furthermore, what we want to understand are the properties of the general underlying law of gravity, not what actually happened to a particular pile of balls.[52]

To sum up this "Socratic sociological realism", the purpose of sociological interview research is to uncover real pre-existing systems of belief and social structures beyond individuals' particular conceptions. However, interviews require interviewers who actively listen and ask critically "Socratic" questions, and who can help informants "deliver their truth" through sensitive follow-up questions. However, one must simultaneously be aware that this is not the same things as posing "leading questions", and recognise the obstacles that social distances may create. Interviewers should strive to reduce social distance through

[52] This account of experimentation and the contrast of empiricism to realism draws on Bhaskar's work (Bhaskar 1975a; Collier 1994).

being consciously reflexive about the social interaction, through all aspects of self-presentation in the interview situation.

On the ground: Guidelines for interviewing

Kvale (2007:80) lists six qualities of a good interview, that I have taken as useful ideals. A high-quality interview should: i) provide spontaneous answers that are qualitatively rich and relevant; ii) have short questions that result in long answers; iii) have a high level of follow-up questions and clarification of informants' answers where needed; iv) be characterised by "on-the-fly interpretation", where the researcher immediately interprets what is said; which ideally leads to v) a high level of verification of interpretations, as informants agree (or by disagreement point to other interpretations) on and so verify the interviewer's interpretations; and, finally, vi) to a large extent be "self-reported", that is, form a story that could be quoted and presented as-is without the need for further interpretation.

Good interviews require skilled craftsperson-interviewers. According to Kvale (2007:81) the qualities of such an interviewer include being: i) *knowledgeable* about the topic; ii) *structuring* in relation to the interview situation; iii) produce questions with *clarity*; *and* iv) be *gentle*, letting informants go on, pause, etc. Furthermore, the interviewer should be v) *sensitive* and attentively listening to what is being said; vi) *open* to unexpected turns with a "hovering attention"; but vii) *steering* when interviewees slide off topic. Finally, a good interviewer is also viii) *critical* and does not automatically accept information at face value; ix) *remembering*, to avoid repetition and connect to what has been covered earlier; and x) *interpreting*, actively trying to clarify the meaning of what is being said for informants to confirm or disconfirm on the spot. These are all notions that should be treated as ideals to strive for and develop as part of the craftsmanship of interviewing. I do not claim to have been an outstanding follower of these steps, but have consciously strived to continuously improve my interviewing practice following these guidelines.

Selection of informants

The informants selected for interviewing were all active Swedish researchers (including doctoral students) in economics or in two cases, had been researching, written and engaged in some form of heterodox economics, or had an economics educational background. "Swedish" means active at a Swedish institution and has nothing to do with ethnicity, although all interviews were conducted in Swedish. The selection of informants did not follow the principles of random sampling since the qualitative interview data are not going to be used for a statistically representative analysis. However, I do not think it is warranted to drop the issue of representativeness altogether just because one works with qualitative methods. Therefore, care has been taken to achieve a sample that is not obviously biased in important dimensions.

A wish list of informants was constructed based on a number of criteria. Since the aim of this study is to investigate mainstream economists, as well as the nature of Swedish heterodoxy and the relationship between these two supposed groupings, this led to a list of criteria for selecting respondents. First, the majority of informants were selected as representatives of a broad mainstream. For this group of informants, I used seven criteria for selection. A wish list was constructed so that all criteria were satisfied by at minimum of one informant. Only the first criterion, i.e. belonging to one of the top five universities, was used as a necessary condition for inclusion in the mainstream group. The selection criteria are presented in table 1.

The interviewed informants in this group: i) all belonged to one of the top six economics departments.[53] In a hierarchical and strongly top-down discipline, this ensures that informants are recruited from economics departments held in high esteem by economists themselves (see Fourcade et al. 2015). Some were ii) selected from among the top-ranked researchers according to bibliometric measures,[54] also a measure to include some of the most influential persons as measured by scientific output. Some were, furthermore, iii) authors of widely-used textbooks, which is another very important channel for exercising influence on the reproduction of the discipline. Others held iv) influential positions in doctoral programmes (director of studies or similar), and were thus authorities on the structure and content of these programmes, arguably a very important determinant of disciplinary reproduction (see Colander 2005). Even if doctoral programmes are never the domain of one or even a few persons, but rather the collective responsibility of senior faculty, such persons are well-informed about the doctoral programmes and any discussions about them.

[53] These are the economics departments at the universities of Lund, Uppsala, Gothenburg and Stockholm, and Stockholm School of Economics. However, Stockholm University hosts both a department of economics and a research institution, the Institute of International Economic Studies (IIES). See chapter 5 for metrics. While administratively separate units, they are located just one floor apart and there is naturally a not insignificant degree of interaction between the two departments. However, I made sure to include informants from both departments.

[54] Rankings of authors from recent literature were used. However, to protect the anonymity of informants, these rankings are not referenced here.

Another selection criteria was v) a position as public intellectuals, being known in public debate. This is arguably also a way of exercising influence on the academic profession, as demonstrated by the example of Paul Krugman, who is widely read not only by the general public, but importantly also by his fellow economists. The next basis of selection was vi) membership in the Nobel Committee. The Bank of Sweden Prize in Economic Sciences is an internationally unique mechanism of creating disciplinary hierarchy in the social sciences. The winners are selected by a small committee of ten members picked from Swedish academia; most, but not all, are academic economists. Membership in the committee is both an acknowledgement of excellence and a uniquely powerful position when it comes to influencing the future of the economics discipline internationally. Therefore, I have also included informants that are or have been members of the Nobel committee. Finally, I included vii) representatives from both ends of the academic career trajectory, which means that informants were selected not only from among well-established professors, but the also doctoral students who will form the next generation of the profession. Some informants also represented career steps in between these two extremes, i.e. lecturer or similar.

Table 1. ***Selection criteria for informants***

REPRESENTATIVES OF THE MAINSTREAM		REPRESENTATIVES OF HETERODOXY
i)	All from top-5 university departments	*Selection based on expressed critique or heterodox self-identification, or identification by others as to some extent heterodox.* *Selection not limited to top departments.*
At least one informant matches each of the following criteria:		
ii)	Top-ranked researcher	
iii)	Textbook author	
iv)	Doctoral programme director	
v)	Public intellectual	
vi)	Current or former Nobel committee member	
vii)	Different career stages (from doctoral students to professors)	

A second group of informants comprised those who could, in any sense of the term, be thought of as “heterodox”. Almost all informants in this group held an economics doctorate; two exceptions were included because of their role in heterodox circles and close relation to the discipline. However, only a few in this group belonged to any of the top five university departments. Some held, or were on their way towards, positions at smaller universities or even in other disciplines. These informants were selected based on my previous knowledge of them and their work, often explicitly presented as critical of mainstream economics in some sense. In a few cases, informants whom I thought of as borderline cases or perhaps "mainstream heterodox" were also selected. Some were added as a result of snowballing in the earlier interviews. Of all informants, about two thirds, or 14 persons, were selected in the first group, and a third, or six informants were selected in the second, heterodox group.

The interviews

Informants on the wish list were contacted with an invitation to participate in the interview study, with brief information about the study and confidentiality. The positive response rate to my invitation was high, with only five out of 25 (20 per cent) of requests turned down. Of the twenty informants in my sample, seven were doctoral students or similar, nine were full professors (including senior and emeritus professors), and four were researchers at intermediate positions as lecturer or similar.

Apart from the twenty researchers in economics, I also did a few brief background interviews with economics students active in two student organisations that had in some way promoted pluralism. However, these interviews have only been used for background understanding of these movements, and are not used in the main analysis. The semi-structured interviews took place between May 2015 and February 2016, in the majority of cases at the respondent's home department, in a few cases at a café or similar, the interviewee's home, and in one case over a Skype videolink. The twenty researcher interviews lasted between 45 minutes and two hours, with most ninety minutes or longer. The interviews were recorded for later transcription and analysis, with a total of more than thirty hours of recorded material.

The format of the interviews can be seen as a hybrid between life-world interviews, where the purpose is to get a grasp of the informant's point of view, and expert interviews, where the interviewees report facts that the interviewer inquires about. I used a flexible interview guide with a range of themes and questions to potentially use, depending on the turns of the interview conversation. The interview guide functioned as a resource to be used flexibly, following up as topics evolved, breaking as necessary the order of questions in the interview guide. The guide was continually updated with refined questions as I learned during the process which questions were productive and which were not. Before each interview I also did some background research and read or skimmed some of each author's work or other relevant sources as preparation. The interview guide was also adjusted to include questions specific to the particular interviewee, sometimes relating to their position, career stage, or specific issues in their work.

The question of anonymity presented an unexpected dilemma. When contacting potential informants, brief information about the project and assurance of confidentiality was provided. In many if not most interview studies, the anonymity of informants is an absolute requirement. However, in this case, the interviewees mostly hold government-funded university positions. They are intellectual actors who are well-versed in arguing and expressing their views publicly. Given this, I could also have contacted the economists in my study for a non-anonymous study, where the informants could speak out under their own names. Similar and very interesting work has been done by high-profile economists and sociologists in the "conversations with economists" genre (Colander et al. 2004; Swedberg 1990). However, while I am sure that many of the full professors I have interviewed would probably have agreed to be interviewed non-anonymously, this is less certain when it comes to the younger researchers and doctoral students who have only just begun their careers. Granting anonymity has thus been an essential way of making sure that economists in very different positions feel comfortable talking to me.

However, a further dilemma occurred when some of the heterodox economists explicitly requested to *not* be anonymised. Their reasons for this were very good: they argued that as a publicly-funded and publicly active intellectual, one should be clear and upfront about one's views. These shouldn't be hidden behind a veil of anonymity. They argued that this is a democratic principle, to take responsibility for one's position, and to defend it publicly. Should I represent the opinions of a few persons with their full names, but let everyone else remain anonymous? Would that not lead to them being in a sense overrepresented, their voices taken as more real, or more important, than other voices? On the other hand, there were some non-heterodox economists (all of them full professors) who said that they did not care about anonymity. The decision to let all accounts remain anonymous—against their request in some cases—is founded upon the methodological principle of symmetry. As argued in chapter 4, it is my intent to apply the symmetry principle regarding the views of mainstream and heterodox economists as far as possible. On these grounds, all interview excerpts will be presented anonymously.

Transcription, analysis and presentation of the interview data

All interview recordings were transcribed using a principle of almost full transcriptions.[55] Transcription was done verbatim, only altering very slightly to correct grammar, or excluding insignificant minor hesitations or repetitions while leaving most. In the presentation of interview excerpts, slight editing has sometimes been done to give a more proper representation of the informants' voices. I have altered informants' sentences slightly in a few cases in order to represent them as I think they would have preferred, in order not to make them sound unnecessarily hesitant, confused, or crude, but still an accurate representation of their natural speech.

The transcripts were transcribed and coded using the NVivo software package. Software coding has not been used according to any strict coding scheme or as an engine of analysis. Instead, it serves as a digital tool for simplifying the mark-up of themes, subjects and types of examples in the material. The analysis consists of a qualitative interpretation of the material, moving back and forth through interview and analytical notes, reading and rereading transcriptions and listening to recordings, with theoretical ideas and themes. Thus, the analysis, as well as the formulation of interview guides and interview questions, has been guided by theoretical preconceptions and ideas, while still being attentive to the material, rather than a purely inductive atheoretical process of the grounded theory type. The aim has been to listen carefully to the themes and ideas that emerge from the material, driven by a theoretical interest and set of questions.

In chapter 7, quotations from the interview transcripts are used extensively to exemplify ideas and themes. Both shorter and longer excerpts are used where warranted. The purpose is to give the reader an opportunity to read and interpret the transcripts for him or herself, and to let the informants' accounts stand for themselves as much as possible. All quotations are presented anonymously, and are not connected to any one informant. Rather, I use them as different voices that together create a fuller account of economists' points of view, where the individual accounts are but examples of the larger thought collective to which they belong. In some cases personal idiosyncrasies will be obvious in the accounts. The point is however to go beyond the individual, and elicit the generally-held views and dispositions of the profession. The informants will be presented with contextualising descriptions that are non-identifying, yet provides some minimal information about the person and his/ her role. This could be "a doctoral

[55] A minority of passages of obvious irrelevance to the analysis were not fully transcribed, but instead summarized and marked in the transcription. Roughly 90 per cent of the recordings were fully transcribed at the end.

student”, or “a researcher in charge of the graduate programme”, or “a senior professor”, or “a young economist”. “Senior professor” as I use it here only means an older full professor who may or may not be retired.

2. Analysing expert evaluation reports

The second empirical part of this study is an analysis of expert evaluation reports from the recruitment of full professors at top economics departments in Sweden over 25 years. The use of this material allows us to complement the views from earlier studies on economics and the interview study with a unique insight into the reasoning at work in the evaluation of scientific quality in economics. As argued in chapter 4, the institution of peer review is a central mechanism for the reproduction of thought collectives and cognitive styles. Drawing on that theoretical insight, the purpose of this study is to investigate what the discipline, scientific quality, and the practices of quality judgement have actually looked like during a particular period in the recent past. Using material that is slightly spread out slightly also makes it potentially possible to discern trends and developments.

Since the public availability of expert evaluation reports is almost unique to the Swedish university system, this material presents a useful, rich but internationally less known source. Therefore, the following sections will start with a brief contextualisation of the institution of expert evaluations in Sweden, followed by a presentation of the selection of material, and an overview. Finally, the literary genre of the evaluation reports and the process of analysis is briefly discussed.

The institution of peer evaluation in the Swedish university system

Hiring and promotion in the Swedish university system relies on a transparent process based on peer evaluation of candidates for academic positions, conducted by a panel of supreme experts evaluating in the form of publicly available reports. This system was originally instituted in 1876 as a means of defending the autonomy of science at a time when there was a felt need for legitimate authoritative decisions in the promotion of university professors (Nilsson 2009:60). This was a question with high stakes for everyone involved, since Sweden at this point in time had only two universities (Uppsala and Lund) with two faculties each. Applying for any of the few professor chairs, which were often held for very long periods, was often a once-in-a-lifetime chance. Naturally, the often-questioned collegial hiring decision needed a firm and legitimate judgement to rely on. Since then the institution has been one of the pillars of the highly centralised Swedish university system, functioning in the same way across universities and disciplines.

The expert evaluators are appointed from among peers of equal or higher standing according to the traditional academic principle of elitism (Nilsson 2009:26). Because the task of quality judgement is awarded to senior scholars of high standing representing the established research community, who tend to be scientifically conservative, it also creates a significant amount of inertia in scientific disciplines. The purpose of the expert evaluations is to guide academics, often within slightly different fields, whose task it is to make final decisions on the evaluated candidates. Therefore, the evaluation reports are generally written in a way that is relatively accessible to non-experts (Nilsson 2009:25).

Even if it is not the purpose of the experts' evaluation reports, they do nevertheless express the sort of shared judgements, values and presumptions that make up a not-insignificant part of any discipline's shared conception of science (Nilsson 2009:27). The judgement of candidates' scientific quality based on such conceptions of good science may of course be thought to be inaccurate or unfair, or the judgement may be contested in individual cases. But what is of interest for our present purpose is not the individual accuracy of judgement, but rather the range of arguments and values that experts collectively draw upon, which must belong to a generally held and recognised value system within the discipline. Thus, when senior experts are evaluating candidates for professor positions in economics, they need to explicitly frame their claims in terms of the current shared notion of "scientific excellence" within the discipline.

Since the experts whose conceptions of scientific quality are expressed in the reports are appointed as good representatives of the discipline, they should be understood not as representatives of specific departments, but rather of the national (and to some extent the international) discipline as a whole. Delegating the responsibility to function as an expert regarding other professor positions is indeed to delegate responsibility for the discipline (Nilsson 2009:34). This is also the case with experts from abroad: being appointed to the panel means being acknowledged as a legitimate scientific authority by the Swedish scientific community (Nilsson 2009:42).

The Swedish university system has been under state administration since its origin. University employees are embraced by the same regulations as the rest of the public administration within the Swedish tradition of transparent governance. The constitutional Freedom of Press Act (*Tryckfrihetsförordningen)* entitles every citizen to free access to public official documents, which includes all documents related to decisions within public authorities, university administrations included

(Tryckfrihetsförordning 1949). This means that the expert evaluations produced in relation to academic hiring and promotion are also public documents, available upon request to any citizen (Nilsson 2009:25). These evaluation reports have been an important source for historical research about science in the past, not least in intellectual history (Nilsson 2009:39). The next section will discuss some of the earlier research that has utilised this rich source.

In general, three experts are appointed to the evaluative panel, but this rule is not without exceptions (Nilsson 2009:35). The length of evaluation reports is also markedly different between disciplines and has shifted over time. According to Rangnar Nilsson's study, in literature it is common for reports to be in the range of 50–100 pages; in political science the range is 10–70 pages, whereas in physics there are no reports over twenty pages in length. During the period 1985–1995 covered by Nilsson's study, experts in political science were drawn from Sweden and the other Nordic countries equally, and all reports are written in Swedish or another Scandinavian language. However in physics during the same period, only a third of the experts were based in Sweden, while the majority were based in Europe outside the Nordic countries or the United States. Most of these reports were written in English (Nilsson 2009:38–39). As I will show, there is also a marked trend towards shorter and more international evaluation reports in economics during the 25-year period between 1989 and 2014.

Selection and analysis of evaluation reports

In the present study I have collected evaluation reports from a 25-year period (1989–2014) from four of the top five Swedish universities (considering economics). These are, in alphabetic order, the University of Gothenburg, Lund University, SSE, Stockholm University, and Uppsala University. Note that Stockholm University includes two research departments: the department of economics and the IIES. The SSE is one of the very few private universities in Sweden. A request to take part of the evaluation reports from all appointments of professors in economics since 1989 was sent to the archives of all top five universities. However, since SSE is a private university it is not bound by the Freedom of Press Act. While public universities are bound to follow the principles of transparency, it employs a much more secretive process and not even evaluated candidates know who the experts in the panel are, and are not permitted to read the evaluation report. In fact, the reports are kept confidential to anyone outside the narrow group involved in the appointment process[56]. For this reason, no reports could be collected from SSE.

The material collected from the five departments in the other four universities represent almost every case of hiring an economics professor in the past quarter-century at these departments. While a few reports could not be found in the archives, this sample gives a good insight into how expert evaluations in Swedish economics around the turn of the millennium expressed conceptions of scientific quality, normal science, and the nature of evaluation reports have changed during the period.

In total, evaluation reports from 58 cases of hiring or promotion of full professors were collected. Since the late 1990s reform, there are two paths available to a full professorship: through competition for a vacant position, or through the new path of promotion to full professor. In both cases competency is evaluated in the same way, but in the case of promotion, only one candidate is evaluated, and thus no ranking can be done. There is also a third form of professorship (*adjungerad professor*), where the competency for professorship of a single candidate is evaluated the same way. The material includes all three forms, but of the 58 cases, forty are cases of competitive hiring.

To create an overview of the material and describe some aspects of the increasing internationalisation of Swedish economics, the material was divided into two time periods, 1989–1999 and 2000–2014. It is apparent that there is a marked transition between the two time periods, from expert evaluations written in Scandinavian languages by economists at a university in one of the Nordic countries, to reports increasingly written in English and, also increasingly but not to the same degree, by professors outside the Nordic countries. While only 37 per cent of documents were written in English in the earlier period, 79 per cent are in English in the later period. However, the share of extra-Nordic experts has only risen from 23 per cent to 31 per cent. One important reason for the dramatic and more rapid increase in English is probably that an increasing number of applicants are not speakers of Nordic languages. But, as I will show, increasing internationalisation is also clearly reflected in how economists orient themselves in their publishing activities, away from monographs and reports written in local languages towards English language journal articles as the only proper mode of scientific communication. Moreover, "non-Nordic experts" only refers to those with a current institutional affiliation outside the Nordic countries. In some cases (as with one expert who writes several evaluation reports for different universities), this is a Swedish-speaking person active abroad. This also points to the fact that

[56] Personal communication with SSE.

it is not uncommon for experts to reappear in the collected material. So, in fact, the pool of experts drawn on by the profession is actually smaller than it may at first seem.

The total sample of 58 expert evaluation reports contained 40 cases of competitive hiring. Since arguments about quality become really interesting when one needs to compare and justify a judgement, only reports from this subset of reports where applicants compete for a professorship (as opposed to evaluation of competency for promotion to professor) was chosen. Of these, the 20 most analytically promising cases were selected. First, all nine cases of general professorships in economics were selected, since these are arguably the cases where the nature of the economics discipline is most open to discussion. Second, cases of different specialised professorships were selected to assure overall breadth of specialisation. For example, these positions include professorships in public economics, the economics of local government (*kommunal ekonomi*), theoretical economics, international economics, environmental economics and econometrics, among others. These twenty reports were sampled from across the 25-year period to ensure that evaluations from different points in time, as well as the four different universities, were represented.

The selected evaluation reports were initially read and qualitatively analysed with the intent of discerning how the expert evaluators argued about the various aspects of scientific quality. What fields of research, problems and methods are presented as normal science? What aspects of research are positively valued by reviewers? How are the experts arguing about quality: how do they reason about aspects of quality, and what are the central arguments or devices used to differentiate and categorise candidates? Furthermore, is there any development in the way reports are written and how experts argue over time? In the process of analysis, I chose an open, interpretative and reiterative mode of reading, rather than relying on more formalised models of coding of the text material. During the process, a picture of how the experts argue and how they view the discipline and its style of reasoning soon emerged and was refined with further reading and rereading. Furthermore, a central theme that I had not anticipated at the outset to be important emerged from the material when I started comparing more recent reports with the oldest ones. Only then did I realise the extent of, and start analysing, the transformation of quality evaluation from a close reading of a broad range of publications in Swedish and English into the current practice where evaluators rely heavily on journal rankings as a quantitative judgement device, and where the material evaluated has become much more narrowly defined in terms of English language articles in ranked international economics journals. This is the prime example of how the process of interpretation and analysis itself was transformed in rereading the material in close relation to theories of scientific quality judgement.

The genre of expert evaluation reports

The format of evaluation reports is not strictly formalised, but there is a certain institutionalised structure that most reports tend to follow, though this has changed since the late 1980s. In all but a very few cases, three experts are assigned the task of producing an evaluation. In cases of promotion, it is not uncommon to use only two experts. Of the twenty cases of competitive hiring selected here, all but one of the evaluations were performed by a panel of three experts. The experts are always external, i.e. recruited from another university. As already mentioned, in the early period (1989–1999) this often meant another Swedish or at least one expert from another Nordic university, while in the latter period (2000–2014) experts are increasingly drawn from the rest of Europe and the United States, although it should be noted that "international" here is almost always synonymous with the Anglophone world. Furthermore, one should note that some reviewers appear as experts on more than one report. Indeed, some have been involved in producing multiple reports, from two or three up to eight different cases for the most trusted professor.

Originally, the three experts were supposed to be strictly independent and produce separate evaluation reports. However, the practice of producing an evaluation report jointly emerged gradually during the middle of the twentieth century, earlier in some sciences than others, and had become established practice by the end of the 1980s (Nilsson 2009:95). This is the normal case in the material studied here. The three experts write a joint report where the work of each applicant is evaluated. Finally, each experts produces an individual ranking of applicants and includes an explanation for that ranking. However, there are always some exceptions to the rule. In the material we find a few cases that deviate from this pattern, where for example two experts write a report together, while the third produces his or her own. But the standard form is quite well established.

The joint evaluation reports often start with a very brief introduction on how the experts have interpreted the task at hand, sometimes explicating the quality criteria by they will use in the evaluation. The report then proceeds with a review of the work of each applicant in turn. The scope of these reviews varies to an extreme degree. For example, in one report from 1989, the experts devote twenty pages to reviewing just one applicant. At the other extreme, in the later part of the period, it becomes increasingly common for experts to use some method of coarse filtering, so that applicants considered less competitive are not evaluated and reviewed as thoroughly as the top candidates. In these reports, we learn very little about these deselected applicants. Sometimes as little as three lines of text are used to present an applicant and his or her work. We will come back to the question of how such a primary screening of applicants can be performed with the aid of judgement devices like journal rankings.

While the final rankings are submitted individually, they are often the product of deliberations among the three experts. This is sometimes discussed explicitly, and it is not uncommon for the three individual reports to share the same verbatim formulations, especially in the introductions discussing the principles of evaluation. However, this does not necessarily mean that the three experts produce the same final ranking of the top candidates, even though they normally agree on one top candidate. There are also exceptions to this rule where the experts disagree more strongly, which can be an interesting insight into the practice of evaluation and what it tells us about the self-conception of a discipline, as I will show.

The length of the reports varies from over eighty to a mere two pages; a typical report is 20–40 pages. They have generally tended to become shorter during the studied period, though there is also considerable variation between reports in the earlier part of the period. Most reports in the earlier period are typically around 30–50 pages, and most in the later period are in the range of 15–25 pages. The evaluation report, which is normally jointly written, takes up the bulk of pages, and the typical individual ranking is a very brief and effective piece of 1–2 pages. While the final rankings often repeat or rephrase the experts' arguments from the main reports in clear and condensed form, it is from the text of the latter that one can create an image of the normal science of the discipline. Here, we get a sense of the different research fields of most highly qualified applicants for professorships, and the different problems, theories, and methods they work with. Through the reasoning of the experts and their valuations, arguments and justifications, an image of a disciplinary core soon emerges through the type of research and qualifications that experts argue are most valuable for an economics professor.

Chapter 7. The discipline from the economists' points of view: Interviews with economists

How do economists reason about science, their discipline, and its relation to other fields? How do they understand their common ground as a profession, and what makes them different from others? This chapter presents the results of my in-depth interviews with Swedish academic economists. Through the interview study, I approach questions of how economists understand their discipline and scientific practice, from the perspectives of both general or mainstream economists, and of heterodox economists. The chapter is structured to convey a sense of the themes and views that emerge from the interview material. Some themes have grown out of my theoretical interest, follow questions used in the interview guide, and correspond to theoretical issues raised in the theory chapter; others have grown inductively out of the twenty interviews. The intent of this presentation and analysis is to portray common elements of the thought collective, for example how economists view their discipline in relation to other academic fields, or scientific quality.

The theoretical background assumption is that years of socialisation into a scientific discipline shape some of the ways in which one thinks, one's intellectual disposition; in short, the scientific habitus. One does not have to call it "indoctrination" as one doctoral student did when reflecting on the shaping of his own methodological preferences in the disciplinary training. But given that, everyone is of course influenced not only by their common training, but also by other intellectual and social circumstances, and by their intellectual choices. I hope that some of this dialectic between on the one hand a recognisable structure and similarities, and on the other hand individuality and plurality of viewpoints, will be visible throughout the presentation.

1. Becoming an economist: Self-selection, training and professional identity

A central theoretical assumption that guides this study is that economists, like any bounded scientific thought collective, to some extent share certain ways of thinking about science, society and their own discipline. There are two basic forces shaping such a thought collective: it is formed through self-selection, and through socialisation. If self-selection works as a primary coarse filter, socialisation through scientific training and practice potentially works as active moulding of a more or less unitary intellectual collective. I say potentially, because this unifying or homogenising outcome is by no means universal or inevitable. But, drawing on the stories that the economists in my interviews told, I will argue that their first-hand experiences of contemporary Swedish economics show that such a process is very much taking place, and that it is furthermore a conscious and desired outcome. But first, let us look at self-selection as part of the economists' background stories. Why and how did they chose to become economists?

Self-selection: The joy of economic analysis

The reasons for and causes behind one's career choices are of course variable and not always transparent. My interviewees provided varying narratives, ranging from more strategic and informed choices, to mere chance and luck. Many of them explained how they were driven by a general interest in society and politics from early on. For some, this was connected to an interest both in politics and the economy. Especially for some in an older generation that came of age in the 1960s and 1970s, the interest was driven by what could loosely be called a political economy approach. That is, an interest in largescale processes in society and politics coupled with an insight that to understand it all, you need to understand its foundation in economics. This general idea comes not only in Marxist, but also in liberal versions. Others had more specific interests. Some told stories about early experiences in smallscale trading in the stock market, driving curiosity and an urge to understand how it works. More common were general notions that economics is very important to understanding how society works.

Some informants were driven more by the form of thinking and analysis they encountered, and recalled that they were attracted to economics early on because they liked the way that their economics teachers reasoned. For example, one young economist told me about a high school teacher who had really influenced her through the use of real-world examples and tasks, showing how they could be expressed in simplified and often diagrammatic form, leaving the impression that this sort of analysis is fun. Another young economist explained that he was about to pursue a *PolKand* exam,[57] and liked political science very much due to his general interest in society and politics. Although the simplifications of economics teaching were sometimes frustrating, there was also something fascinating about it. Here is how he explained the special attraction of economics:

> I: [. . .] and I also think that, I think that I am in some way attracted to the . . . this concrete . . . It is often said . . . economists have a tendency to sort of deliver results using numbers, and I think I am attracted to that.
>
> AH: Concrete results?
>
> I: Exactly.
>
> AH: Mmm.
>
> I: I think I have some sort of fetish for that basically. . . I think it may have helped. So I sort of continued and took the next course.

What he, and others with him, tried to convey was a very basic sense in which a way of thinking resonated with a personal cognitive disposition, and how this has been something almost unreflected, a background condition for thriving in the profession.

Several informants thought that they ended up where they did more or less by chance. According to their narratives, they for various reasons started to study economics, and found that it was fun and that they were talented. Another young economist explained how he quit the undergraduate programme he intended to take and, in search of another line of study, by chance contacted the department of economics, which later turned out to suit him very well:

> I: It was not at all my plan to become an economist. I started out in the psychology programme, because I was interested in organisation theory, really. [. . .] So I started the psychology programme, but soon realised that it was not for me. Both because of the group and because of the curriculum. So I quit it, and I phoned around to hear where they had a spare place for me. The first department I called was economics, and it turned out that they had a spare place. So I joined it, and

[57] Bachelor of Science in Politics and Economics, a common bachelor's programme with a major in political science, economics, or human geography.

> realised it suited me very well, because they worked with models of human behaviour that I could relate to.
>
> AH: How come you could relate to them?
>
> I: Well, I guess it probably has to do with my personality. In the beginning, that I liked to categorise and simplify, to bring things down to the same level. Later I also thought that game theory was very fun. So I got into that, and it turned out to suit me very well. It went very well, and I enjoyed it very much.

We see here there was something in the mode of analysis and the modelling of human behaviour that resonated "with [his] personality".

Another recurrent and significant feature is an emphasis on the joy of a certain approach to analysis, and the resonance between one's own disposition and the mode of analysis one encounters in economics. This is something that many of my informants, both young and senior, touched upon in passing in different contexts. Here we can discern a first form of the work of habitus. It is not yet a proper scientific habitus, but I think that these accounts point to the way that cognitive aspects of habitus ("something with my personality"), as a certain predisposition for a mode of thinking and reasoning comes out to play in the experience of joy, and in finding oneself at home.[58]

When it comes to other background influences, several informants also had some sort of background in mathematics, making the step to economics easier. To conclude, there are a multiplicity of accounts of how one originally became an economist. However, common to many accounts is a sense of belonging, of finding an intellectual home and a way of thinking or analysing that harmonises with one's disposition. If this is the way a cognitive aspect of habitus comes to play in self-selection, a properly scientific habitus is slowly shaped and takes form through formal training and scientific practice.

Engines of disciplinary standardisation: Doctoral programmes

If my informants' accounts of how they ended up in economics varied, it points to a multiplicity of reasons and circumstances that influence an individual's choice, even if there are broad patterns in self-selection. Self-selection is probably

[58] Interestingly, Ian Hacking (2012) has recently suggested, drawing on the classic work *Homo Ludens* by cultural historian Johan Huizinga, which points to the foundational role of play in human culture, that we should direct our attention to the role of play not only in culture at large, but more specifically in the study of scientific styles of reasoning.

a very weak factor in shaping the outlook of the profession, compared to the education itself. In contrast to self-selection, formalised training is the primary site where a common and more or less unitary way of thinking and doing is really shaped. What every economist by definition has in common is many years of training in economics. And if the vast majority of undergraduate students continue to careers outside academia, doctoral education is where new generations of researchers are really shaped, and where we need to look if we want to understand the profession (Colander 2005). This is of course true of all sciences, but what sets economics apart, at least from other social sciences, is the very high level of standardisation of the graduate programmes. This is known from the literature, but it is also something that the interviewees were not only aware of, but proud of. The structure of doctoral programmes at the major Swedish departments today follows a US-inspired model with a first year of mandatory courses and a second year of coursework where courses can be chosen from those provided at the home department or elsewhere. The first year is to a very large extent standardised according to the American model.

The standardisation of education is not only an institutional fact, it is generally acknowledged as beneficial, and the result of intentional planning by the departments. Apart from being designed to be universal and universalising, increasingly following an international or American model, doctoral programmes were understood to be very demanding, functioning as a selection mechanism where not everyone passes. The internationally standardised contents of doctoral programmes are the foundations of similarity in outlook and practice. One director of a doctoral programme explained the course contents of the programme this way:

> I: It is micro, macro, and econometrics, you could say. Then it depends on your background, for example, you may take courses in mathematical methods too, if you don't have a maths background, above what's included in the economics programme. So these are the four basic courses, and in that respect we don't differ from any place in the world. It is very standardised in the sense that you have these four basic courses in micro, macro, econometrics, and possibly maths, so you get that everywhere all over the world. So the subject as such is quite standardised.

This standardisation was generally acknowledged as a very positive thing, allowing one to immediately relate to any economist anywhere in the world. According to the interviewees' accounts, it gives the young economist a universal training and a common language. It was understood as scientifically productive to be able to participate in and understand most research in economics, because you share the same basic approach. Furthermore, mastering mathematical and technical skills allows you to be constructively precise, to establish a firm foundation of knowledge together.

Another feature of graduate school in economics is that it functions like an input filtering mechanism, letting only those who can cope with what is often seen as a very tough experience into the profession. This makes it work in a sense like a professional rite of passage. This is perhaps especially pronounced in Stockholm, where Stockholm School of Economics, IIES and the department of economics at Stockholm University, KTH and Uppsala have a common graduate programme which is deemed to be of a high international standard. One doctoral student in Stockholm explained how it works, and I will quote at length to convey the many aspects of doctoral life in this narrative:

> AH: Would you like to tell me a bit about how the PhD education in economics works? I know that you have a cooperation with Handels and IIES.[59]
>
> I: Yes, right, Stockholm University, IIES, Handels. And KTH is in it too. They don't have such a high frequency of recruitment, so they are only one or two or so. We also have a cooperation with Uppsala the second year. It works like this, that the first year you only take courses, corresponding to eight 7.5 credit courses, even if it's not structured exactly that way. Two in math, two micro, two macro, two in econometrics. It is really an . . . acid test. You read an incredible amount of materials, and hand in problem sets every week in each course, so you have two problem sets each week, where the answers are normally 25 pages, so you write fifty pages per week of algebra, really, that you have to hand in. Then you take an exam on each course. There is quite a high failure rate. What they do is that they force everyone to learn, to hear all concepts, so that everyone has heard and seen everything, very much of the standard in the discipline. Then they check who can actually cope, so there is a falling-off.
>
> [. . .]
>
> AH: So there is some sifting out of some then?
>
> I: Yes, exactly. [. . .] It is mostly that they check our capacity, test us. They want to teach us, they want us to . . . as an economist, you are more of a generalist

[59] "Handels" is the commonly used shortened name for *Handelshögskolan*, i.e. the Stockholm School of Economics (SSE).

> compared to PhD students from other disciplines. I mean, I can have a say about . . . I can sit at a seminar on any topic in economics and participate in it.
>
> AH: So you have something in common there?
>
> I: Yes, you have that year, that year when the only thing you did for seventy hours a week was to study everything, you have those common frames of reference somewhere. Maybe you won't say something clever, but you can still follow. In my experience, when I talk with many others, is that there are great distinctions in their discipline. If you are a political scientist, for example, if you are qualitative or quantitative then maybe you have no idea of the other. So I think that it . . . To be sure, I hated it pretty much the first year when I did it, but now I am very glad that I did it, and I think that is the general experience.
>
> AH: So it is somewhat of a bootcamp?
>
> I: Yes, it's a bootcamp.

Here, intentional standardisation coupled with the intense workload is explicitly understood as beneficial for creating a common disciplinary language. This doctoral student reflexively understood the design of the doctoral programme to be part of a very good training in a paradigmatic scientific discipline, contrasting with political science as an example of what he understood to be the obviously detrimental effects of the lack of a common language or way of reasoning. At another department, a professor with responsibility for the graduate programme spoke about the doctoral programme and its design from the department's point of view:

> AH: Would you like to tell me a bit about the PhD programme in economics? Are there any thoughts behind its design?
>
> I: We have a PhD programme which is very similar to other PhD programmes in Sweden, which in turn are very similar to other PhD programmes, especially in the Anglo-Saxon world. So there is a very . . . we are very internationally attuned in that respect. We have not had any intention to have a unique one of our own, rather the opposite. We want to be attuned. So . . . what it looks like is basically a first year with mandatory core courses: micro, macro, econometrics, pure mathematics . . . Some years we have also had pure statistics. And we have several econometrics courses. There has been a certain shift towards more micro, slightly less macro, and slightly more econometrics.

> [. . .] You are very busy, it is a tough year. Traditionally, historically it has been used a little like a selection and sorting mechanism. You test them to see who hangs on and who are perhaps not so fit for it. And that culture lives on a little, even if it is not like that formally any more. Today we give everyone employment from day one, so it is of course our duty that we should . . . that those we have accepted to the programme, we want them to continue so to speak, to finish. But, there is, you have to say, that there is still such a culture the first year, and then it becomes easier the second year, when you exit the cave, so to speak.

These accounts illustrate what seems to be a generally shared meaning of doctoral education, from both the students' perspective below, and from the perspective above. First, the high level of standardisation is a positive thing in two ways. A PhD in economics means the same thing no matter where you got it. Of course, there are differences in status between departments and universities, but when it comes to the content, everyone in the profession has learnt the same micro theory and the same econometric methods and so on. This is also true internally, as the doctoral student explained: as opposed to political scientists who don't even always understand their own colleagues, economists are able to understand at least the basics of what everyone else is doing. It means that there is a clear and substantial set of shared theoretical knowledge and methodological skills ("common frames of reference"). Belonging to the profession means sharing this knowledge and skills.

Second, it is clear that doctoral education, and certainly the standardised first year, is experienced as very demanding and tough: "we had to hand in fifty pages of algebra every week" and work with this "seventy hours per week", leading to the experience: "I hated it". But once one gets through it together, it gets a bit brighter. It is a good thing to really have been forced through it. These accounts are also very clear that this is a selection mechanism which winnows out those who were not meant for it in the first place, or for some other reason couldn't cope with the high demands. It is the survival of the fittest, an intellectual elite. However, it is not necessarily a matter of doctoral students competing with each other. Rather, the student quoted above continued by stressing that cohorts that make it are those that cooperate. Maybe the parallel to military bootcamp that I suggested and the informant confirmed is not only a popular expression for going through tough training. Just like bootcamp, being forced by the professors to undergo extremely demanding training together strengthens group solidarity and collective identity.

Forming a disciplinary thought collective and habitus

Finally, it is evident that standardisation is highly intentional. Even though self-selection and undergraduate training play a role, doctoral programmes are the primary institutional mechanism shaping a coherent thought collective. In this sense, they function as engines of intellectual standardisation. Furthermore, this is related to the function of the doctorate as an essential entry ticket into the discipline and the disciplinary labour market, something that Abbott (2001) has emphasised in the account of the modern US academic disciplines as a dual institution consisting of a national labour market structure, and a local department organisation. The function of the doctorate as a token of legitimacy was evident, often in passing, in the interviews.

The common experience of doctoral programmes and exposure to the same type of theoretical and methodological training lays the foundation for a specific disciplinary habitus. The scientific habitus becomes specialised as one learns to write papers, attend seminars, read research papers, and develop a professional sense of viable analytical approaches, valid sources of data, fruitful study designs, acceptable arguments, and shared ontological presumptions. If the scientific habitus is a semiconscious disposition and readiness for perceiving, acting, thinking and reasoning together, doctoral training also introduces a professional identity as researcher and economist. The interviewed economists generally spoke in terms of *us* (economists), and were often ready to exemplify different points by comparing economics with other disciplines, emphasising disciplinary identity through this contrast. This furthermore points to the relational construction of a disciplinary thought collective and its identity.

The disciplinary identity is also highly internationalised. It is strongly connected to an international, if US-centred, community of academic economists, and to their unique approach to analysing society. This disciplinary identity is of course also strengthened by participation in international conferences, and not least through the orientation towards a set of highly esteemed international scholarly journals. However, these factors exist in all academic settings. What is peculiar and stands out here is the strong identification with a bounded discipline and its unified and highly standardised intellectual outlook. There are several factors that need to be in place for such a strong disciplinary identity to form and stabilise, like a steady flow of material resources, and a high external social status, etc. However, in the establishment and reproduction of a tight thought collective, the intentionally standardised design of doctoral programmes plays a most important role. It is here that a disciplinary thought collective is shaped.

2. A "strong, central body of theory": The disciplinary core and style of reasoning

If doctoral programmes in economics are highly standardised and economists take pride in their standardised courses and common frames of reference, what is the content of these frames of reference? In other words, if one has to condense the question, what is economics? How do economists think about the defining characteristics of their discipline?

Defining the discipline

The interviewed economists were asked how they would define economics in simple terms. Many of them returned some version of Lionel Robbins's classic definition. In the interview conversations around this definitional issue, there were different approaches presented to the question, sometimes as subsequent attempts at highlighting some sort of disciplinary core. Furthermore, the very notion that there is something like a disciplinary core did not meet with much resistance, but was mostly taken as given, leading the conversations to the problem of defining it properly.

I asked my informants to define what economics means to them. Here are two long excerpts from interviews with two senior professors at two departments. The first is a senior professor who specialised mainly in macroeconomics. This piece of conversation is worth quoting at length, since it displays a very condensed and rich account of one conception of the disciplinary core.

> AH: If we were to go into questions of basic definitions, like you would in a textbook, for example, what would be your definition of economics?
>
> I: Economics deals with how humans choose in a world of scarce resources. That's the basic model, so to speak. That's one way of defining economics, which is fruitful.
>
> AH: Lionel Robbins?
>
> I: That's Robbins, yes. I think it takes us far, because it shows us that we have scarce resources, and that we have to choose how to use them. But then it is not only about us choosing, for our choice naturally also has consequences for incomes and the distribution of wealth, which has consequences for stability in the social economy. That's one way of defining economics. Another way of defining

economics, that is more evasive, is to say that economics is what economists do. And then economists really do everything. It has become a science with a very strong paradigm, a very strong model. A strong micro model, that can be applied to law, business administration, sociology, in economic history, broadly in all areas where you are facing choice, which really is the case in all parts of society.

AH: Mmm.

I: So in some way economics has become sort of the king, the queen of the social sciences, but also with marked imperialistic tendencies, because we consider this paradigm applicable in many areas.

AH: Mmm.

I: If you ask an economist "what model do you use?", they will tell you that "I use the basic micro model". But if you ask a sociologist, economic historian or a political scientist "what model do you use?", they may reply that "well, I use this particular model, but my colleague has another model, and those in Umeå and Uppsala have something completely different". So you have in fact a much greater lack of discipline, theoretical discipline, in other subjects. That's what I find attractive with economics. It is the same textbook in Stockholm as in Sydney or Santiago de Chile or any other place. We have this . . . strong, central body of theory. The more you move out from that, the more of controversy you get. [. . .]

AH: Yes, right, you describe this in this [article], and say that the neoclassical body of theory works as a paradigm for modern economics. What do you mean by "the neoclassical body of theory" then? Is it the same thing as you describe as the standard micro model now?

I: Yes, I would say that it is the same thing . . . so to speak, the Slutsky equation. That is to say, that your points of departure are a negative demand curve, you have price elasticity, you have income elasticity, you have individuals who make choices, and if the price relations change, then the choice changes. That is to say: human beings respond to incentives. Incentives may come in many forms, it does not necessarily have to be money, it could be time, it could be feelings, it could be a range of different things. When price relations change, your choice will also change. That's one way of looking at economics, which I think is quite fruitful.

AH: So it is this you really refer to when you say "the neoclassical model"?

> I: Yes. But then it can also be extended in any direction. You can enter agreements and contracts, you can enter law, and contract law, you can enter various institutional circumstances and make the model more complicated, but the basic idea is this, that you should have a theory that is stable over time and space, so to speak. And it is in effect this simple, neoclassical approach. It is something you find already in Adam Smith, it is there in micro theory all the way during the last, what is it, 250 years soon. It is stable. But then you also have other areas in economics, that I work in, macroeconomics. There you have a completely different, very different traditions, you have several schools, and schools or paradigms that compete with each other, and there often seems to be some sort of long-term pendulum swing between the different schools. And depending on what phenomena we observe in society, you will want to choose a macro theory that is most suited to explain it, that appears to be most relevant.
>
> So when you write about views on economics, I think you should have a certain ambiguity. When it comes to the basic core, there is very great commonality. But when it comes to macro, macro issues, macroeconomics, then you have multiple schools, more perspectives that either complement each other, or they substitute each other, they compete with each other. In the general debate, the image that ordinary people, the lay person gets of economics, is that economists are not in agreement, they have different opinions. But that is because they stand for different interpretations, different theories. So it can be a paradox. On the one hand, economics appears exceptionally unanimous, on the other hand it appears divided and in disagreement.

In this excerpt, the role of the neoclassical microeconomic model of human behaviour is clearly viewed as the core of a paradigm that, according to this professor, stretches far beyond the marginal revolution of the 1870s, normally seen as the origin of neoclassical marginalism. In his view, this model is clearly what defines the paradigm of economics. Furthermore, we see here again that this unity, this community centred on a shared basic model, is understood as something very positive, which explains the role of economics as the queen (or king?) of the social sciences. It is also evident in the way that so-called economics imperialism is explained not as something problematic to be deprecated but as the effect of the productive and widely applicable nature of the general economics "paradigm". This professor explicitly contrasts this with his view of sociology or political science, fields that seem to completely lack any consensus around central theoretical models. Again, the way that contrast is immediately invoked when making sense of the disciplinary intellectual approach and its merits illustrates that the idea of a disciplinary core is not unrelated to the question of disciplinary boundaries. To the contrary, the maintenance of the disciplinary style draws directly on the boundary work that constructs a contrasting outside.

But the picture is more complicated than this. After concluding that economics everywhere is based on a shared microeconomic model, the informant claimed that in macroeconomics, there is no such consensus. Instead, there are different schools, or even paradigms, competing and replacing each other. This explains the seeming paradox that economists are both in fundamental agreement, and simultaneously in grave disagreement. This interesting paradox has direct bearing on the discussions about what neoclassical economics really is in chapter 2. There, I argued that classification of schools of thought is dependent on the level of abstraction used. This professor's account can thus be read in line with my argument that it is indeed possible to think about the historical continuity of an underlying framework of neoclassical economics, while at the same time acknowledging different theoretical schools of thought and research fields at a more concrete level. In chapter 2 I made a distinction between the ontological, epistemological and social aspects of the core of mainstream (or neoclassical if you will) economics. In this interview excerpt, we find a good example of a definition according to what I called the ontological aspects, in line with Varoufakis and Arnsperger's three meta-axioms. First, the unity of analysis is individuals that choose (methodological individualism). Second, individual behaviour is driven by instrumental rationality, with actors assumed to satisfy some given utility function: "actors respond to incentives" (methodological instrumentalism). Third, individuals' choices have interactive equilibrium effects, for example on the stability of the economy (methodological equilibration).[60]

[60] Another thing to note about this excerpt is the way that this professor uses terminology from the sociology of science ("paradigm") to describe the context of his own activities. This is perhaps not so unexpected when interviewing other social scientists, but it brings to mind an anecdote about the pitfalls of theoretical reflexivity in sociological research. In an interview with Robin Celikates, Luc Boltanski, once a Bourdieusian sociologist, explains this problem: "The belief in the clear-cut distinction between actors and scientists should have been lost no later than in the 1970s and 1980s. In this period, social reality was literally swamped with social-scientific schemes of thinking; in this sense, society became reflexive. When I embarked upon my fieldwork for *The Making of a Class: Cadres in French Society*, I went to talk to various associations and asked them about their respective definitions of manager [cadre]; they responded: 'What? Have you not read Bourdieu and Touraine? Go and ask them!' In other words, actors themselves had begun to use the works of sociologists as resources for the construction of their own group." (Boltanski, Honneth, and Celikates 2014). This is not only an issue in relation to concepts like "paradigm", but even more so when it comes to the terminology of historians of economic thought, where economists blend with themes borrowed from the philosophy and sociology of science. So how, for example, does sociologists' and historians' different conceptualisations of schools of thought, or of the relationship between heterodoxy and orthodoxy, affect economists' own constructions of groups and symbolic boundaries?

In the next interview excerpt, a version of the same question was posed to another senior professor.

> I: Well, how to understand economics? The simple definition that we can start with, as some sort of starting point, is that economics is about economising with scarce resources. [. . .]
>
> AH: That's right, Lionel Robbins's classic definition?
>
> I: Yes, exactly. And that's a good definition, I suppose you could say. Then there is much more. There is a famous essay by Buchanan that says that if it's economising on scarce resources, isn't it just maximisation theory then? That is, you are to maximise something under some secondary constraint, you are to extract as much utility as possibly from the scarce resources of the earth, or maximise profit . . . or customers have a certain budget restraint, or so. But then Buchanan says that this is not really a good definition of economics, this economising on scarce resources. Historically it is a given, and it certainly has had its role. But is this what is characteristic of economists? Buchanan doesn't think so, because economising on scarce resources, that's something any damn engineer who studied optimisation theory can do. No, what's special for us is that we study equilibria between different actors, each acting in their own self-interest, leading to a market equilibrium. And then it doesn't really matter if it is a perfectly functioning competitive market or a monopoly market or so, but the result of a number . . . of rational actors acting in their self-interest. And with rational I mean that they act in their self-interest, they don't have to be rational in any profound sense, they don't have to be well-informed, but can really be very uninformed, but still consider themselves to act in their self-interest. Here, Buchanan says that this is really the core of what precisely what we economists do. And there is something to that, too. [. . .]
>
> But then, a lot of economics is just descriptive, that's the old economics . . . German economics from the nineteenth century, it only describes: how large is the production of steel, how large is . . . employment in different branches of industry.

Here, the definition of economics is expanded to emphasise the importance of equilibrium analysis (methodological equilibration). It is a good example of the conception of methodological equilibration, that is, analysis in equilibrium terms, whether or not one observes or believes in actually existing equilibria. The formulation of three meta-axioms seems a very apt way of pinpointing the multifaceted formulations of the defining features of the economics discipline that emerged during the interviews. However, apart from these basic ontological assumptions, the interviewees also talked about the role of modelling, and other distinguishing methodological features at length. One prevalent theme here was the slow but marked shift away from purely theoretical modelling, towards more work involving empirical data analysed with econometric methods. Furthermore, the final paragraph of this excerpt illustrates once more the relational nature of the style of reasoning through contrasting examples of the nineteenth century descriptive historical school as something that modern economics is definitely not: proper economics is not *descriptive.*

Evolution and difference within the disciplinary style

Modelling comes in various flavours. Economists can work with purely theoretical modelling, trying to find out the implications altering any of the variables in a hypothetical model. They can also, and do so more frequently today, work with empirical data, trying to work out simple models that explain the empirical data. Economists have a rigorous training in mathematics and statistical methods, which have led to an increasing amount of empirical studies by economists that do not deal with "economic" issues in any simple sense. Following the above interview excerpt about Buchanan's definition of economics, the conversation turned from abstract definitions to what economists actually do:

> I: Then there are . . . many in the younger generation, that are very skilled economists that write a lot of interesting research papers, that I don't really think is economics, but it is still interesting as general social science. I will give you a few examples, listen.

He then gave me a detailed summary of a large study that some economists recently did. Using register data, the authors identified individuals whose mothers had been pregnant with them in those geographical areas of Sweden that were most heavily affected by the radioactive fallout in the period immediately following the Chernobyl meltdown. They correlated this in utero exposure to a range of different social indicators thirty years later, like school grades, employment and drug abuse. He concluded that they had found such a correlation:

> I: Very interesting! But it isn't economics.
>
> AH: You don't think so?

I: No. I don't think so. Because it is general social science. A sociologist could have done that. Or someone in social medicine. But economists do it. And that's what I find so exciting and interesting.

He then gave two other extended examples of empirical studies using unconventional data and field experiments in two different African settings, and concluded the same way:

I: But it's not economics! That's general social science. But it is incredibly interesting and incredibly relevant, important. But it doesn't suit any definition of economics. It is not optimisation theory, and it is not householding with scarce resources, other than in any very general sense [. . .]. And it isn't market equilibria, it's something else. But it is done by economists, so then you could call it economics.

This professor then emphasised how the subject has turned towards such empirical studies, made possible by the computing revolution that has made it possible to have a desktop machine running statistical analyses that would have been technically impossible when his generation were students. This technologically driven shift is something that was mentioned and discussed in several other interviews, as something that has opened new possibilities for analysis which were constrained by lack of computing power until a few decades ago. Continuing the interview above, I asked this professor if the examples he gave didn't directly correspond to this development:

AH: Isn't this directly linked to [the examples of empirical studies], where economists venture into completely different fields?

I: Yes, that's right. We have studied much more statistics than most other social scientists. We have. In principle, a sociologist or a political scientist or a social medicine researcher could have done [such studies], but in practice there is not so much statistics in their PhD programmes. But they could have done it! But yes, they don't do it today. But it will probably happen in a few years. Because this is sort of coming . . . in all social sciences. It then means that we can move into areas of other sciences, say the Chernobyl fallout, or so on. Not because it is economics, but because . . . we have studied so much statistics, so I find it quite natural.

This interview excerpt is an example of how the empirical turn in economics is understood. During the interviews, many informants emphasised this turn in different ways, but with the main message that any image of economics as a purely deductive theoretical science without contact with empirical data, is severely outdated. Pure theoretical economics reached its apex in some time in the 1970s, and is a mode of analysis that the above informant was himself heavily trained in. This trend towards empirical studies in economics is generally acknowledged and has been confirmed in bibliometric studies (Hamermesh 2013). An obvious explanation, is twofold. Today, as already mentioned, there are personal computers and software available that just did not exist before. For example, running a regression analysis on a dataset with 20 million cases, as one paper by an informant discussing the topic of the empirical turn did, was technologically impossible or even inconceivable in the 1970s. On the other hand, we also have the other side of the data revolution with increasing availability of these types of data. Ever more data are collected by ever more government agencies, companies, and researchers, and it is easier to get hold of and use them.

This empirical turn seems to have strengthened the econometric (statistical) and empiricist tradition in modern economics. In many of the interviews, it is evident that there is a strong epistemic inclination towards empiricism. Statistics is viewed as an essential aspect of what it means to be scientific. For example, in the last interview above we see an expression of a sort of fatalism, the idea that the technical possibilities present today will inevitably bring this form of statistical empiricism to all social sciences. We observe here the gradual rise of the statistical style of reasoning as an increasingly important component of the disciplinary style in modern economics.

We also get a glimpse of a new form of economics imperialism in this excerpt. Such imperialism is a seemingly well-known phenomenon among economists. In the interviews, there were some who talked with me about it in a slightly self-conscious and humble way, while others presented it (like in the interview with the macroeconomics professor above) as plain fact: economics is an imperialist science. However, what is noteworthy in this new form of what could be called econometric imperialism, is the notion that these types of studies are perhaps not really economics. But, they are interesting, important, and implicitly, good examples of "general social science". What we find here is a case where the disciplinary identity is downplayed, overshadowed by a more general "scientific" identity. The Crombian style of statistics, which transcends disciplinary boundaries, has become central to the disciplinary style of economics. The identification of statistics with science and the notion of the inevitable spread to other disciplines involves boundary work centred not around the discipline and its specific style, but instead around the Crombian style. It shows how Crombian styles may function as either barriers or bridges over disciplinary boundaries, and that we should not take the object of boundary work as given. Examples of such boundary-crossing identifications are also sometimes present in another recent empirical field, that of experimental economics.

Experimental economics and empiricism

If the general trend in economics is a shift from a theoretical to a more empirical focus, the rise of the statistical style in the form of econometrics is only one component at play. Another development is much more recent, and perhaps therefore also often mentioned by informants as an example of a new and creative field, or an example of how economics breaks out of old theoretical orientations. Just like the turn to econometrics, the gradual introduction of the Crombian experimental style, in the form of the rise of experimental economics seems to mean a simultaneous shift to an empiricist emphasis on data and on empirical falsification, that is, an overarching empiricist epistemological orientation.

In an interview with one researcher specialising in experimental economics, we came to talk about the role of formalised models and empirical data. This economist exhibited an attitude that judging from my interviews, appears to be increasingly common and strongly tied to the turn towards empirical economics, both experimental studies and econometric studies on pre-existing data.

> AH: I'm thinking about what role formalisation plays there, in your conception, of being scientific? Is formalisation always a necessary, or desirable, mode of expression?
>
> I: Well, I never formalise. [. . .] But then, I have data.
>
> AH: Yes?
>
> I: So I think that it's a bit like, and now I'm half joking, but "It's not science if you can't bet on it." So you sort of need hypotheses that you can test. And in a formal model you can easily see how the hypothesis can be tested. In a verbal model it is harder to see if it really holds. Do you see how I mean?
>
> AH: Yeah.
>
> I: But I mean, the theoretical model could also be too narrow, so that it all hinges on one assumption. But then it is really easy to see that it is that assumption, that if you change that, then the results change, or not. In a model with only words, it becomes very hard to see for outsiders if it really is correct. Then you really have to sit down and write a theoretical model. Which I wouldn't do . . . I mean, I

> handle data, you create an experiment with some form of randomisation to, in the research that I do, to test a hypothesis [. . .]
>
> AH: Yes, right, this is like Popper's notion of falsification, isn't it?
>
> I: Yes, yes.
>
> AH: Could you say, wouldn't it be possible to imagine that there are questions that are not immediately testable, but that you can nevertheless say something meaningful about? That is, where you can have, where it is hard to produce this type of rigorous empirical evidence?
>
> I: Yes, but then you could have a rigorous theoretical model instead?

Here we see an example of an attitude that I think has become more prevalent. It was mostly evident in the younger generation of economists who emphasised the role that experimental economics plays, and the importance of empirical evidence, data, and hypothesis testing. However, the experimental style is only now becoming a minor part of the disciplinary style, and it is evident that some established economists, not least in the older generation, are sceptical about the merits of experimentation in economics. This points to the slow-changing nature of the disciplinary style as well as the work that needs to be performed in terms of convincing other actors in the thought collective and the integration of the new style with the core assumptions of the discipline. But if experimental economics is a new and rather small field, although with larger symbolic significance, there are epistemic orientations at play here, which seem to be more generally established in the discipline. These are the epistemic preferences for clarity, rigorous evidence and certainty.

A rigorous and unambiguous science

As I argued in chapters 3 and 4, the interpretation of fundamental concepts of science and epistemology like "science", "objectivity", "truth", "evidence", "reason", etc. are not timeless ideas that just fell out of the sky. Scientific reason has a history, and it comes in different forms. There is not a single scientific method or conception of proper science, but several. In the interview study, there seemed to be a general gravitation towards rigour and clarity as epistemic values held in high esteem by the interviewed economists. The use of formal models, econometrics, and clarity of expression is strongly related to an understanding of what it means for something to be "scientific".

Let us start with the idea of rigour and clarity. Here is an extended excerpt from an interview with a doctoral student, who talked about a preference for "concrete" results in a previous excerpt. Later in the same interview, we spoke about conceptions of good science:

> AH: If we are talking about the topic of scientific quality, can you say, in general, that there is something that distinguishes good research in economics? Both if you have an idea of what you think is the generally established opinion among your Swedish colleagues, and if you have any ideas of your own?
>
> I: Yes . . . in general, maybe we have this [. . .] penchant for concrete conclusions, perhaps. Which is fatal, of course, it is a bit like a defence mechanism, that someone else contradicts your black and white comment with another black and white comment, sort of.
>
> AH: What does it mean that the conclusions are concrete?
>
> I: Well . . . it's a bit hazy, it's just my feeling since I try to speak about everything at once. But really . . . you want a number for example, that's very nice. And if you have a conclusion, it should be, well ok, that's not unique for us, but . . . it should be very . . . clear, which like I said can be dangerous sometimes.
>
> AH: The same thing as exact? That you want exact answers?
>
> I: Yes . . . exactly.
>
> AH: That are not ambiguous?
>
> I: Yes, absolutely . . . It's perhaps something general. But I don't know, perhaps I would have found out the same thing if I had been hanging out more at other [disciplinary] departments, sort of.
>
> AH: What is it that . . . why would you want an exact answer? Perhaps it is a stupid question?
>
> I: No, [laughter] it is really a very good question, because one should want . . . the only thing important should really be to have the right answer, regardless of whether it is exact or not. I think that it makes it easier to take a stand on it, I would personally say. It has actually been quite . . . subconscious, why you are attracted to that. But it feels, it feels good when a person can point to something, then I know exactly what that person wants, so to speak, and that makes it easier

to take a stand on it, to discuss based on it. [. . .] And this has also been shaped by the fact that it is easier to get published as empirical researcher if you have that perfect result, and it is easier to be picked up by the media if you have that perfect example.

AH: Ok, so you are interested in a clear and distinct answer, and to know where a researcher stands?

I: Mmm . . . precisely . . . yes. And also the models during all the courses from the first micro course are always very concrete. You can see exactly what person x, given postulates one to five, does, and even if they are completely unreasonable and . . . useless, they are very clear. And, the nice thing is that you know they are unreasonable because postulate four is completely sick. It is very easy to get an overview, and discussions often become very concrete: "this is a strange postulate, if we remove it, ok, then that changes, then we find something different". You get that in macro and trade models too.

AH: So you can sacrifice something to reach that clarity in models and answers?

I: Precisely, absolutely. All these mathematical models are really . . . which they could have been more careful with, but are often quite careful with, that we, as I said, we are trying to model behaviour here, and you can't do that, so hence we simplify. I think that's extremely reasonable. You do that also when you make empirical econometric models, you put your variables so that they have effects in a specific way, linearly, or that there are intentions, and so on. And that's a crude simplification, but perhaps it is close enough. But if you are to assess, well, 20,000 individuals, then you will sort of have to simplify. So I have nothing against that. Then you can do it in good ways, and bad ways.

This is a good and reflexive example of this epistemic preference for the exact and unambiguous. It is also evident how this epistemic disposition is related to the modelling approach, and the necessary acceptance of reductive simplifications of social behaviour. The unrealistic simplifications were fully clear to the informant, who even called them "completely sick", but they nevertheless play a necessary role in a strategy for producing knowledge. His example of the necessity to simplify in order to be able to analyse very broad patterns or big data (trying to grasp 20,000 individuals in an analysis) is also a good example of a common argument about the necessity of complexity reduction.

Another example of this orientation is found in another interview with a young economist, where we talked about what economics really is, and if it has some sort of defining core (the same theme as was covered above). This informant connected the approach of economics to a very pronounced conception of the task of economics. In this case, it was understood as answering questions that someone else (politicians for example) has asked, that is, as objective technical expertise producing knowledge and answering questions posed by some separate, external demand. I asked whether there is something like a disciplinary core in economics:

> I: . . . It's a good question . . . I think that the core itself has been transformed a little since you had the first political scientists that started to explore economic problems. Sure, the basic core is really all questions regarding economics situations, economic exchange, or, everything that . . . relates to people's standard of living. But one part that has become more and more prominent is really that . . . the core of economics is also the choice of methods and the perspective that is our vantage point . . . You can't deny that economics is very much about theorising various relationships in the world, that you see in reality, and make them comprehensible in a quantitative way. Which I think corresponds very much with the requirement on economists [. . .] that the government wants a number, they don't want a book of 300 pages where you qualitatively ponder different theories, how growth can look or develop based on different theories. Instead, what they are interested in is the number, and they are not even interested in the confidence interval, or the uncertainty, of this number, but it is really . . . well, the methodological development is really a reaction to the requirement put on economists, to really distil everything in a . . .
>
> AH: Requirement for exact knowledge for decision-making?
>
> I: No, not make . . . not exact knowledge . . . but the requirement . . . to distil really . . . the whole . . . the whole subject and extract some sort of core message, or some sort of core number, than can very easily be digested by politicians or the government.
>
> AH: You view that as some sort of core, the way economics delivers its results?
>
> I: Yes, in a way. The core is probably the methodology, which is about theorising a bit, and to look for quantitative answers to questions that have been asked.

This is another example of the epistemic inclination, which also brings out an understanding of the role of economists as scientific experts who answer someone else's questions. There are different formulations among the interviewees, which seem to circle around a very similar epistemic approach or attitude. As the previous interviewee explicitly reflected, this preference seems to be something almost "subconscious", a primary orientation that is in itself not subject to argumentation or proof, but forms the starting point of a scientific approach. This is then a good example of a component of a scientific habitus, which includes exactly these sorts of predispositions and inclinations that form the basis of scientific practice and judgement. It is furthermore, as we will return to, an example of how such dispositions are part of a scientific style of reasoning, and what Hacking means with the self-authentication of styles. For the basic approach and vantage point used is itself not open to dispute. The use of statistics, modelling, and lately also experiment, are more or less taken for granted as epistemic practices within the discipline.

This focus on Popperian falsification was prevalent in several other interviews as well. One professor described how it can often get quite rough at economics seminars, and explained that the point is to throw out your paper and see if it withstands colleagues' attempts at tearing it apart. Everyone, according to his account, is trying as hard as possible to find weaknesses, and this collective process of attempted falsification works as a filtering process in a very Popperian sense. What is noteworthy here is the focus on "hard" and "robust" knowledge. The aim is not primarily to produce knowledge that is thought-provoking, interesting, and relevant (although these criteria are far from irrelevant according to the interviews). But the prime focus is on certainty, on knowledge that is undisputable, that is clear and unambiguous.

Another thing to note here is the prevalence of adjectives like "hard", "robust", "powerful" and "strong" in the interview conversations. Such language seems to be ubiquitous in the professional thought collective. Parallel to this, the view of lesser sciences as "softer" has a barely-disguised gendered connotation. Marion Fourcade and her co-authors (2015) have pointed to this masculine side of economics due to the strong overrepresentation of men in the discipline, and the gendered language and metaphor has long been brought up by feminist economists (Nelson 1995). This quest for robustness and certainty also seems to be closely connected to the central role of modelling.

The modelling requirement of the disciplinary style

As the literature reviewed in previous chapters has shown, the role of the modelling approach in modern economics is central. This view was confirmed in the interviews, where informants in various contexts gave examples of the role that modelling plays. In the following excerpt, a senior professor who had written approvingly in another context about the merits of Hyman Minsky, an economist who is generally acknowledged as an important heterodox author, argues about the fundamental role of modelling approaches. I asked him about his somewhat unexpected approval of Minsky's ideas:

> AH: In another article [. . .] you talk about . . . you compare modern macro theory with an alternative tradition with Minsky and others that is more historically oriented and, as you say, emphasises endogenous crises, psychological explanations, non-rational expectations, genuine uncertainty, and so on. What role does this type of economics play today? Is there any space for it, and isn't a lot of that research, that is, isn't a lot of it based on a form of historical, more verbal arguments and perhaps not so much on mathematical models?
>
> I: Yes, it is, and that's the problem with this research, that it does not use formalised models. Because as soon as you formalise an approach in testable models, you get a whole different ground to stand on. It becomes more stable, and can more easily be incorporated into the discipline. But these, say the Minsky approach, these theories you mention, verbal, historical, they are verbal and historical precisely because we can't formalise them. And given that they are not formalised, they don't have the same chances of survival in the economics mainstream. The discipline is not verbal, it is mathematical. It has become that way. It used to be much more verbal earlier. The great economists, like Wicksell and Cassel, they could at many times be verbal, but behind their . . . words, there was also a more or less explicit model. That model could become formalised, and live on.
>
> But the problem with Minsky is that his . . . his description is not formalised, and then it has a harder time surviving in the scientific competition. One day, Minsky will maybe lend his name to a model that can be tested. But it will probably be so complicated and so tricky to handle that it can't give any clear conclusions, and will thereby be hard to test. It is the same thing with the Stockholm school, they had a lot of promising thoughts on dynamics and expectations, but it didn't turn into a model. Keynes was turned into a model, therefore, Keynes won.
>
> AH: Through Hicks's IS/LM model?
>
> I: Yes, through the fact that it could be formalised. And you find it in any macroeconomics textbook. You don't find the Stockholm school [in the regular textbooks], because it didn't provide a model. The lesson of this is thus that to get a tradition that will survive and develop, it has to be formalised into a model. It's not enough with a good story, it's not enough with a good anecdote.

This is a strong account of how important formal modelling is as an epistemic practice. Although the theories seem interesting, they have a fatal flaw in that they can't be formalised. The conclusion one may draw is that according to this view, if you can't formally model it, it isn't science. Formal models have become a *sine qua non* of modern economics. This professor explicitly connects the use of formalised modelling to the notion of falsifiability, arguing that this is the reason why economic theory must be expressed in formal models, and that verbal or historical accounts can only survive if someone formalises them.

In another interview, another professor talked about the significance of the econometric turn, and I asked him about the role of modelling in this form of economics:

> AH: Is there a tendency that [. . .] it becomes a requirement . . . that formal modelling is something which to a large extent is characteristic of research in economics?
>
> I: Mmm . . . I don't know . . . That's how it used to be. When you didn't have computers and could do these big empirical studies, then formal modelling was required. So my generation is raised with that. And now they are not used to formal modelling in the same way. But I don't think it is needed. But then it is the case that you do need to know what statistical model you are dealing with. That is, what is somehow generating these data? After all, you do need some sort of model when you do empirical studies. What has happened is that it has become less theory, and much more empirics. But even if you are doing empirics, you can't just go out there take a heap of data and shove them into the computer. You kind of have to tell the computer what it is that it's supposed to estimate, and so on. So you still need a formal model, you need to be aware of that. So, it is only that they have become used to slightly different formal models than what I have been. I mean, if you look at this model [in a recent empirical article], it is full of formal models, and then we test the data against these models. It's only that the models look slightly different. They are more adapted to being brought to empirical data today, compared to what they used to be. Back then, that data material didn't exist at all. There wasn't even any point in imagining how you would write these models to test them against empirical data. Now we can do that.

The point made explicit here is that although there may have been a marked shift from pure deductive theory towards econometrics using empirical data, the latter still rely to a great extent on formalised and necessarily simplified models of human behaviour. Despite the decline of "pure theory", according to the interviews, using behavioural models is still a central practice, even though these are now brought from deductive proof to the role of hypothesis generation, to be submitted to rigorous empirical testing.

The components of a disciplinary style of reasoning

Bringing these ideas of a disciplinary core and scientific identity together, we find first a set of what I have called ontological assumptions, which seems to fit well into the notion of the three meta-axioms of methodological individualism, instrumentalism and equilibration. These conversations, just like this conception of *meta*-axioms, have of course taken place on a very general and perhaps abstract level, without venturing into the details of different theories or theoretical or ontological assumptions. However, from the different conversations a recognisable general conception soon emerges that is perhaps not surprising as it confirms that the impression from the international literature is valid also in this national context.

I conceive of a style of reasoning as being composed not only of such ontological assumptions, but also of epistemological assumptions about methods and the nature of knowledge. In this case, the spontaneous responses by the interviewees were often striking confirmations of what was already expected from the literature about the disciplinary style, that is, the central role of simplified modelling of behaviour and modelling practices (the Crombian modelling style), but also of the increasing role played by empirical econometrics (the Crombian statistical style), and even experimental economics (the Crombian experimental style). As I have tried to show, modelling has a core role in the complex of Crombian styles that make up the disciplinary style.

Finally, I have tried to show the extent to which the reproduction of a common style of reasoning relies on the conscious and deliberate agency of the professional elite at local departments that design and maintain doctoral programmes with the intention of mimicking what are considered the best international/ US programmes. The maintenance and reproduction of the disciplinary thought collective is directly related to a more or less unitary disciplinary scientific identity, the boundaries of which constantly needs maintenance work.

3. Discipline, neighbours and history: Relational identity and boundary work

The basic idea of boundary work is that symbolic boundaries of some sort or another are not pre-established in themselves, but need constant maintenance, negotiation and justification. As discussed in chapter 4, the classic notion of boundary work in the maintenance of the boundaries of science is also applicable in our case. One of the three forms of boundary work identified by Gieryn (1983) is attempts at monopolisation of scientific authority. This is also the form of boundary work that features in the relationship between economics and its neighbouring sciences. Boundary work is often a more or less unintended by-product of other social practices, and not necessarily an end in itself. For example, one curious form of identity construction and boundary work is effectively performed in the form of jokes and humorous anecdotes about other sciences.

When it comes to conceptions of other sciences, economists often exhibit a form of physics envy, though of course this is not necessarily strongly held by every member of the profession. As in other matters, most economists are also very reflective about these things. In general, mathematics and physics are regarded with awe or respect, if not envy, while the other social sciences are often, although of course not always, looked down upon. Several of my informants talked about neighbouring disciplines in joking terms. One saying that was retold by several economists of different generations explains the pecking order of the sciences: "bad mathematicians become economists, bad economists become economic historians, bad economic historians become theologians, and bad theologians become priests". While this is obviously a funny and well-known saying, there is often a sense of self-recognition at the bottom of such jokes.

In a similar way a senior professor told me about the ambivalent and rather scornful relationship to the discipline of business administration. He recalled with amusement what Assar Lindbeck said about its lack of real theory: "he once told a story, that he thought about business administration when he put butter in his frying pan. When I gets hot, it melts . . . *thinly, thinly, thinly*." The point of this anecdote is the obvious (to the economist) lack of any proper theory in business administration. Instead of theory, according to such conceptions, the discipline is just a collection of "business tricks". To further explain this view of the neighbouring discipline, "it should be compared to a doughnut", the professor said, again with delight; "the core is empty, there's just a hole in the middle", implying that where the theoretical core should go there is nothing but a void.

The sense of superiority (Fourcade et al. 2015) of economics in relation to other disciplines is perhaps conveyed in what this professor has to say about the faculties of theology and the social sciences. The account starts with a discussion of the abovementioned data revolution and turn towards empirical studies. It is also a fine example of how falsification and cumulative science is viewed, before it turns to the differences between economics and other social sciences.

> I: It's data that are necessary to be able to test the theories. And to be able to develop science, we need to build models, test them, verify, or . . . disqualify, disqualify theories. If we can't have that purification process, then there is a grave risk that we stagnate, and that this whole thing becomes a religion, that we believe.
>
> AH: Mmm.
>
> I: So if I look at economics of the last fifty years, it is really the data revolution that I think it is worth emphasising, it has been very rewarding. To separate the worst madness. So this thing, to filter out some theories and refine the theories and improve them . . .
>
> AH: Yes, ok, has it had this effect, in your view?
>
> I: Yes, you have to . . . A good point of departure for a dissertation is to start with some empirical observations you want to explain. Which theories are these observations consistent with? Then science becomes testable, thereby it can be tested.
>
> AH: Yes, it is . . .
>
> I: It is the great difference in contrast to the theological faculty and the social science faculty. . . We can test our data, we can test our theories with data. And we are interested in it. If you can disprove a theory with the aid of data, then you have built a good foundation for your career.
>
> AH: Mmm, so you think that's a difference compared to the social science faculty, too?
>
> I: Yes, precisely this that we test and test, we can do that by means of our foundational central model. But if you don't have this foundational central model, say in political science, or sociology, what can you test then? It becomes so . . . so much softer, so much more sweeping, and so much more insecure.

This demonstrates once more how explaining what is peculiar about one discipline requires the contrasting examples of other disciplines. And while doing that, some of the other major social sciences (sociology and political science) are without hesitation caricatured as "theology", completely lacking scientific rigour. Such examples, of other social sciences used to exemplify something that is not fully scientific, are abundant. Apart from pointing to the relational nature of the disciplinary style of reasoning, it also clearly conveys this relation in terms of epistemic superiority, that economics is *more scientific*. There is also a clear sense in these forms of argumentation of the necessity of protecting and upholding this scientific practice against the encroachment of sloppy or unscientific knowledge. Thus, again, the orientation towards the disciplinary *core* is closely related to the maintenance of the disciplinary *boundaries*. In a sense, this is also at play in the relationship with fields that are even closer: the discipline of economic history, and the field of history of economic thought, and the philosophy of economics.

The relation to economic history, history of thought and philosophy

As discussed in chapter 5, one result of the late nineteenth century *Methodenstreit* was the separation of economics from economic history. In Germany, a nascent neoclassicism, with its abstract and universal theory, was pitted against the historical school and its empirically descriptive historical accounts, with figures like Max Weber and Werner Sombart on the latter side (Milonakis and Fine 2009; Pålsson Syll 2007). This played out differently in different national contexts. In Sweden, economic history now has a long history as a separate and vital discipline, with an active research community and very large departments (Waldenström 2005).

Economic history was often looked upon with some interest by the interviewed economists, but often also with friendly disregard. More than one of my informants said that it is of course interesting, and that the disciplinary boundaries are arbitrary, but after all, economic historians mostly differ in their lower methodological and theoretical skills. The separation of the disciplines means that not only actual economic history, but also the intellectual history of economics, has become the subject of a separate academic discipline. Full courses in economic history or in the history of economic thought are not normally included either in undergraduate or graduate economics programmes. Ambitious or curious doctoral students may sometimes take optional courses from their neighbours over at economics history, but these are exceptions. When it comes to courses in the philosophy of science, or economic methodology as it is often called, the situation is possibly even worse.

As David Colander (Colander 2005; Klamer and Colander 1989) notes in a follow-up to his well-known study (with Klamer) of elite graduate programmes in economics in the United States, a few decades ago doctoral students could sometimes complain about the lack of courses in the history of thought or philosophy of economics, whereas after the turn of the millennium, these subjects were not even missed by surveyed students. The lack of courses in these subjects is often pointed to as a central problem by heterodox economists, as well as by the pluralist students' movement (ISIPE 2014). However, attitudes towards economic history and history of thought differ widely.

In the interview material there are a few economists of an older generation, who thought that these courses should really be included in a broad and relevant curriculum, or else we risk creating a generation of very smart economists who don't know a thing about the real economy or "anything written before 2014" as one professor pointedly put it. Some younger doctoral students were also very much in favour of taking courses in these subjects, and one of my informants had even taken a course in the philosophy of science in the social science faculty out of pure curiosity. However, there is also a prevalent attitude that these may be interesting subjects, but they do not really matter for real research. Often, these subjects are viewed as curiosities that are of no relevance in a modern, cumulative science, as exemplified by this professor:

> I: [Some economists say that] we have too little knowledge of history and history of thought. And perhaps we have. I mean . . . it would be desirable if one had much better knowledge in the history of thought. But life is limited and one doesn't have time for everything. Someone who is interested in the history of thought [. . .] would say that everyone should be forced to study history of thought. Someone who is interested in mathematical models would probably say that everyone should be forced to solve mathematical models. But life is sort of too short for both. You don't have time for both. And I am a person who is actually somewhat interested in the history of thought.
>
> AH: So you don't see it as . . .
>
> I: I see it as a fun thing. The little I have done in the history of thought, I did as a pastime, because it's fun. But it has in no way affected my other research in economics.

A little later, he explains that he thinks that mandatory courses in the history of economics would not be scientifically relevant for doctoral students in economics:

I: I don't think it affects the way that research is done, really.

AH: Basically because the classics or history [of economics] has . . . fallen behind?

I: Yes, yes. There are those who say that to understand the financial crisis and all that, you have to read Keynes.

AH: Mmm, yes that's a common claim.

I: But I don't believe in that.

AH: No?

I: If you were to read Keynes, then you wouldn't have time to read so very much else, and then you would have missed a lot of importance in the financial crisis. On the other hand you could say like this: it would be preferable if we had such an incredible intellectual capacity, so that we can both read Keynes and read Wicksell, and read Latin poets from Augustus's time, and study philosophy, and also be skilled in econometrics and statistical theory, and mathematics, and be able to program in MathLab, and program in Stata, and to do all that. That would have been nice, wouldn't it?

AH: Yes, well, it would have been fantastic.

I: But unfortunately, we don't have such a large intellectual capacity. So we have to drop something. So first, we drop the Latin poets. And then we drop Wicksell, because Wicksell is rather unclear. Then we would prefer Keynes, who is somewhat clearer. But then, my intellectual capacity is still not enough. And then I drop Keynes, for he is still not clear enough, and I say that as someone who has really read Keynes, that is. Then unfortunately, I drop mathematics too, because I am not sufficiently [laughter] . . . don't have that capacity either, or the statistical theory. There is so much that you have to drop. So you have to cheat a bit. [. . .]

It's such a cheap critique of economics that some persons come with, that say you must, that economists are not sufficiently knowledgeable in the history of thought. And it is true! We are not sufficiently knowledgeable. But we are not sufficiently knowledgeable in anything.

After explaining that history of thought is not worth prioritising in economics education, the informant agreed it may still be important as part of one's broad, general *bildung*, and that historical knowledge in general may something good in that sense. He then revealed that in his view, the reason behind calls for more history of thought, is that they "only want to create an image that they themselves have such a large intellectual capacity so that they can handle what we do, plus that historical *bildung* on top of that. And I don't think they have that capacity. I only think they are being snobbish."

This professor clearly does not think that studying history of thought is important, although he takes pride in having done so himself. But his rhetorical strategy is interesting in that he dismisses other economists' claims that the subject is important by projecting a motive of self-interest and snobbery on them. His own argument rests on the notion of scarce resources: there is simply not enough time to read Keynes.

Other professors, more sympathetic towards the history of thought, took a slightly different tone, but in the end to the same effect. One opinion for example, was that it would be unwise to advise a doctoral student to study a topic that is far removed from the research frontier. However, it is something that one may well do after one has tenure, or when one is older and reaches retirement age. Still another professor thought that it is probably not very fruitful, but *if* we should study it, it must be the history of theories that are in use today. It seems that this approach to history, what historians refer to as "Whig history", is also widespread.

When it comes to the philosophy of science, opinions seem even more adverse. Clearly, the philosophy of economics is even more remote from the everyday world of the contemporary economist, and the problems of philosophy of science seem to be of no great interest in general. Bringing up the topic led to very little response in most interviews. Philosophy and philosophical issues, like the question of how the model–world relationship can be understood in terms of realism or instrumentalism, so central to much writing by heterodox economists, does not seem to be an issue that is even marginally present or interesting. My interpretation is that this is a question that is so little studied and discussed that its absence is not even noticed.

The hatred of economists and the dismissal of heterodoxy

If topics like the history of thought and philosophy of economics often seemed to be rather uninteresting to the mainstream economists interviewed, there was one topic that certainly engaged them, raising irritated or defensive reactions more than anything else. This was any mention of critiques of the economics discipline or of heterodox economics. Many of the informants talked about critique of the discipline in a tired way, well aware of what was often seen as an unfair and uninformed criticism by people who don't understand what economists actually do and what economics is about. As one doctoral student described it:

> I: There is a very strong hatred of economics out there . . . But then, we often also say, that the only thing that is worse than someone who hasn't studied economics and spew their hostility all over it, is someone who has taken thirty credits of economics and think they know the subject, and spew their hostility over it. It is the classic "I've studied economics, and you guys always assume perfect information", for example. But no, we don't. We do it at the A [introductory] level, we do, maybe also on the B level. But they take it that these [introductory courses] in microeconomics, that's how far economics has come, that's all there is to economics, and then they hate it because of that. And if it was true it would have been very reasonable to hate economics.

A similar sense of unfair hatred directed against economics was mentioned by several informants, and seems to be a prevalent part of the experience and self-understanding of the thought collective. The experience of unfair criticism, and the readiness to disregard it as uniformed, as "hatred", or as mere "talking points" by "journalists" or critics not interested in dialogue, is shared in the professional habitus of mainstream economists.

It is also obvious that a tendency to make far-reaching abstractions, which is so often held against economics, for example by other social scientists, is something that economists themselves are fully aware of and reflective about. This stance may be understood roughly along the following lines. Just because one makes very stylised theoretical modelling assumptions doesn't mean that one actually believes that this is an accurate or realistic description of the world in any ordinary sense. Here, it is probably useful to make a distinction between different ontologies, that is, the notion that we may hold multiple and possibly conflicting ontologies. The different I want to emphasise is the one between an everyday common-sense or spontaneous ontology on the one hand, and on the other, a scientific ontology, that is, the sorts of entities and relations that can be covered and understood by a theoretical, scientific account of a phenomenon.[61] What the above and similar accounts convey is in a sense the necessity of an epistemic break: we can't treat the

[61] It is perhaps useful to compare with the scientific ontology of sociologists to make this point clearer. As sociologists, there is rarely any consideration of the biological bodily functions of human beings. We are interested in social phenomena, and constitute our object of knowledge in terms of social relations, actors, meaning, etc., but only rarely in terms of neurons or hormones or genetics. However, this doesn't mean that we, in our everyday lives, outside the seminar room and after we have finished our fieldwork or writing, believe that social human beings are not also biological organisms. But it is not part of our sociological *scientific ontology*.

simplified models of actors as they are presented in the beginning as a simple realistic representation of reality. Instead, we need to follow all the way through the epistemic break, and on a personal level experience how a system of concepts or a scientific ontology makes sense and is scientifically productive.

However, during several of the interviews, a slight irritation seemed to emerge when heterodox economics was mentioned. Among many interviewees, there was a clear and curious reluctance to talk about heterodox economics. The immediate impression was that this is something that doesn't really exist. It is a phenomenon that doesn't really have a proper place in the worldview of the thought collective. For example, when I asked if informants could an example of someone who practiced heterodox economics, few could think of any names at all. Heterodox economics as a concept clearly doesn't exist on the cognitive map of mainstream economists, or it is consciously misrecognised. For example, one heterodox economist, who explicitly identifies as heterodox, was denied this status by a professor who denied that his colleague was anything but a regular mainstream economist who had just happened to have become a bit old and nurtured his own peculiar research interests. The general tendency seems to be to reduce heterodox criticism to the status of uninformed critics, or even to journalism, but in any case to deny the existence or scientific status of a body of work and a group of researchers pursuing some sort of alternative to mainstream economics. Alternatively, a common strategy is to counter by pointing to the openness and new roads being taken for example in behavioural economics. Others in the older generation mentioned, with relief, that there had once been what they considered an ideologically-driven programme of the Marxists in the 1970s, but that this thankfully does not exist anymore in contemporary economics.

This denial of the very existence or status of heterodox economics, and dismissal of critics as uneducated or non-scientific, is one of the basic forms of boundary work. Instead of acknowledging the existence of a dissenting view, the critic is defined as an outsider, whose views are not legitimate and not necessary to attend to. Similarly, failure to give recognition to someone's status as critic has the effect of not acknowledging dissent. My interviews thus seem to offer a very clear case of the form of boundary work that seeks to monopolise scientific authority by denying the existence of other approaches that could possible interfere with or blur a nice, clear-cut image of basic scientific agreement. As an example, when asked about his view on heterodox economics as a meaningful concept, one professor claimed that while there is a broad mainstream in economics, there are no heterodox economists, even internationally. Although it is clearly the case that in absolute numbers, heterodox economics is currently very weak, bordering on non-existent, especially if one only looks at researchers with positions in the economics discipline, the claim that it doesn't exist at all internationally is quite remarkable. After this claim that heterodoxy doesn't exist, I asked how he understood the concept of "heterodoxy". He responded with a snap:

> I: I don't know! [laughter] "Heterodoxy" was your term, and it made me think about some shady types who hate mainstream economics, while themselves being distinguished economists that write a lot . . . and that doesn't exist. On the other hand, there are perhaps a lot of journalists or such that write about economics, perhaps there is, I don't know.

Heterodox economics and its critique of the foundations of the mainstream project is then normally completely neglected, treated as irrelevant, or non-existent. On the level of the thought collectives, there is then clearly a manifest lack of recognition of another, heterodox, thought collective. This non-recognition is thus a product of the boundary work that draws the boundary of economics tightly around a disciplinary mainstream and its style of reasoning, leaving heterodox economics outside the notion of proper, scientific economics. This boundary work is also coupled with the collective experience of unjust criticism, which is directed at the heart of the disciplinary style, for example through over-simplification. There is then a collectively-felt sense that the common style of reasoning must be defended, again addressing the relational link between the valuation of the core and the protection of the boundaries of science and the discipline.

Disciplinary styles as barriers: Approaching other social sciences

The maintenance of a boundary with heterodox economics is closely related to the neglect of similar fields of study, for example in economic history or economic sociology. Although many economists working in newer fields like behavioural economics may have a more open attitude towards research in these fields than towards critical voices from heterodox economists, there still seems to be a symbolic boundary that is closely related to the economists' style of reasoning. For example, one doctoral student self-reflexively talked about experiences of mutual lack of understanding with friends working in other social sciences that, in his opinion, use vague and unclear concepts, unlike the clearly defined and additive nature of reasoning in economics:

> I: I notice that I don't follow, you know. It could be that they have a language that I don't understand. I mean, they use concepts that I don't understand, just like I

> use concepts they don't understand. In my experience I think that the concepts I use have a definition, while concepts I sometimes hear from them . . . I still can't get a grasp of what a "discourse" is. I have had this conversation with them, I don't know how many times, but I still don't get it. When they say "discourse", it isn't obvious what they mean.
>
> This informant then adds that it seemed that in sociology and other social sciences reasoning which is hard to follow and complicated has a higher status, while in economics it signals a failure to simplify sufficiently. This could be taken as a general example of difference in epistemic attitudes following different disciplinary styles. However, the difference becomes clearer when one approaches areas that are more obviously "economic", where there still seem to be some forms of epistemic import restrictions. This was evident when discussing research, drawing on concepts like norms and culture, with some economists working in such fields. While interested in, and in some cases very knowledgeable about research in the neighbouring social sciences, the problem with importing these insights seemed to be a lack of a way to model these relational phenomena, and a lack of an established common language or framework of analysis. Thus, for those economists, working really on the "edge" of economics, to some extent drawing on other social sciences, there is still something fundamentally lacking in the approaches of sociologists, anthropologists, etc.

Thus, a boundary is maintained within a broader class of research on economic issues, one that leaves heterodox economics outside real economics, together with areas like economic history or even economic sociology. Although these fields deal with issues that are certainly "economic", they don't follow the same ontological and epistemological assumptions. They are generally considered uninteresting, or at least scientifically and methodologically inferior or undeveloped, compared to economics. We may summarise this observation by saying that the way practitioners in these disciplines formulate and solve a scientific problem is too different, that they draw their power from other ontological and epistemological assumptions. That is to say, their style of reasoning is different. So it seems that although there may be interest and intellectual curiosity more than hostility or neglect of this very edge of the discipline, the disciplinary style of reasoning still limits the range of possible or reasonable approaches to producing economic knowledge.

From barriers to bridges: Crombian styles and the crossing of disciplinary boundaries

On the other hand, some of the economists in the interview study also had a background in mathematics or information technology, or had cooperated with natural scientists. In contrast to the examples of mutual misunderstanding or epistemic import restrictions in the previous section, there are examples of the ease with which economists may conduct studies in fields that are not in any obvious sense "economic", for example using statistical techniques and big datasets in epidemiology, or cooperating with computer scientists, mathematicians or biologists.

One researcher who had worked very much in interdisciplinary settings discussed how uninteresting it is to define what economics is, and that personally, drawing on the interdisciplinary experience, the delineation of disciplinary boundaries seemed irrelevant. This young economist then explained that there is a lot to learn from, and it is easy to cooperate with, theoretical biologists because, according to this account, they use the same basic behavioural models of choice constrained by limited resources when studying (for instance) fruit flies or the behaviour of yeast genes. Discussing the irrelevance of defining economics, this economist gave me a counterexample:

> I: For example, I think that someone like Robert Trivers, who is an . . . evolutionary biologist, should get the Nobel prize in economics, because he uses the sort of theoretical, quantitative models, not just verbal models, to study things like altruism, cooperation, sexual selection, and a lot of interesting phenomena that has spawned a range of new empirical fields in modern biology. And his models don't differ very much from what you usually see in economics, so why shouldn't he be able to be rewarded for this?

We see here that clearly, there are great similarities, according to this economist. Both economists and biologists use simple behavioural models and test hypotheses with rigorous statistical analysis. This example that seems to counter the idea developed above, that a disciplinary style of reasoning is involved in and maintained through boundary work and the maintenance of disciplinary boundaries. As mentioned, there are other examples in the interview material of economists with interdisciplinary experience, or even background in other disciplines, like mathematics, that seems to downplay the role of the discipline as a fundamental unit of boundary work.

I suggest is that in these cases, conceptions of proper science and scientific boundary work has less to do with economics and its disciplinary style, than with

Crombian styles of reasoning: proper modelling, statistical analysis and an experimental setup. The concept of styles of reasoning can help us see beyond disciplinary boundaries, using the notion I suggest of disciplinary styles as bundles of certain Crombian styles of reasoning: for example, modelling, statistics and more recently, experimentation. The central point in this conception is that Crombian styles transcend and are more enduring than specific disciplinary styles. Since the same small set of enduring Crombian styles of reasoning are employed, although in different constellations, in various disciplines, they may act as bridges spanning disciplinary boundaries.

These examples of mutual understanding or misunderstanding across disciplinary boundaries show how styles of reasoning may act as both bridges and barriers between disciplines, and that boundary work is not always a matter of disciplinary boundaries. Sometimes, it is the style of reasoning itself that must be demarcated as scientific. The construction of science and its boundaries does not necessarily hinge on a discipline, but may be more directly linked to a strong conception of what science is, which fundamentally relies on a specific Crombian style or constellation of styles. The consequence of such an understanding is also that the object of boundary work is not predetermined or given. That is, when boundary work does occur, it is not necessarily the discipline, or science, that is the object of boundary work. It may be a somewhat messy hybrid of the two, or it may vary between contexts, or between actors. Empirically, in our case, it allows us to see how the symbolic boundaries of the scientific project of modern economics is primarily linked to a style of reasoning. The demarcation between science and non-science (or irrelevant science) is drawn right through a broader area of studies of economic issues. At times, this line loops outwards from the discipline, and generously includes other fields of study that to the untrained outsider appear as very "un-economic".

The driving force here, it should be emphasised, is not so much the monopolisation of a scientific approach for its own sake, but a strongly-held belief in the value of science, and the notion that this scientific project, understood in terms of the disciplinary style of reasoning, must be nurtured, fostered and protected from dilution or corruption, and worst of all, outright attacks by outsiders. This scientific project and its reasoning style is worth reproducing and protecting because of its obvious usefulness. It is an approach to science that is used by a strong and influential international thought collective, and it obviously allows the formulation and solution of problems according to its own internal logic, and the skilled application of the approach is obviously successful in landing publications in the top journals in the field, as well as research grants and professorships. Why would one want to change something that is proven to work so very well? [62]

4. Heterodox economics: Alternative trajectories, communities and relation to the mainstream

So far, we have dealt only with regular economists or the mainstream thought collective and their understanding of the discipline and science. It is now time to turn to the heterodox point of view. Among the interviewees are both those economists who have publicly taken heterodox positions, and who view themselves as alternative, as heterodox. However, there were also a few liminal cases of economists who have written pieces that can be thought of as heterodox work, but who, for various reasons, resist labelling themselves as heterodox despite their somewhat outsider view of the discipline. These semi-heterodox economists also expressed some views that seemed congruent with those of their more outspoken heterodox colleagues. Common to these accounts are some sense of alternative trajectories and background, an orientation to alternative communities or networks, an interest in scientific approaches understood as falling outside of normal economics, and experiences or awareness of exclusion and power. As I will show, these accounts can all be understood as unified by a rejection of the mainstream style of reasoning, although there is no single alternative project, or even a unified thought collective.

Alternative trajectories and scientific habitus

If there is a strongly bounded and relatively homogenous economics thought collective, one of the central mechanisms in its reproduction and of the similarly structured scientific habitus of its members, is by way of education, and essentially through modern, standardised doctoral programmes. However, none of the heterodox or semi-heterodox economists in the sample had passed through a modern doctoral programme. This was mainly due to the fact that most of them

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[62] This point is succinctly expressed in general terms by Hacking in his discussion of the self-authentication of styles as discussed in chapter 3. Hacking emphasizes the notion again that styles carry their own conditions for truthfulness, but outside that, there is no foundation, not even a pragmatic one. For the pragmatic notion of being useful, of success, is itself circular: "success helps determine what will count as success. Success has a lot to do with future success because it helps characterize what in the future will count as success" (2012:605).

belonged to an older generation, but also, in two cases, due to careers that only partly intersected with economics, with doctorates pursued in neighbouring disciplines. These informants are included in the sample since they have written on and hold opinions about topics that clearly relate to heterodox economics and its relationship to the economics discipline. Nevertheless it is already clear that heterodox economics contests the boundaries of the discipline, and is itself a phenomenon with rather blurry boundaries.

What seems to be common to all the (semi-)heterodox economists interviewed is a scientific habitus that is strongly shaped by experiences outside the economics discipline. Among the formative influences that were mentioned and generally seemed to play an important role, is a background in social movements: in left-wing or green movements. It is noteworthy that all heterodox economists in my sample described their background as engaged in or influenced by social movements, from the 1960s and 1970s New Left, via the Left Party (formerly VPK) to the environmental movement. It is important to emphasise that this was primarily a formative background factor rather than an ongoing engagement in activism. That is, the experience and intellectual approach they gained from involvement seems to have fundamentally shaped their intellectual and scientific outlook and sensitivity. It is then a scientific habitus that has been partly formed in the intellectual milieu of social movements, rather than engagement in the present context, that is of central importance.

For example, one of the heterodox economists told me how his interest leading to pursuing economics started with the New Left and an early engagement in the activist FNL movement, leading to a lot of reading in leftist and Marxist literature even before becoming a student of economics. This Marxist priming led to the impression that economic issues are of prime importance for understanding sociopolitical issues. Apart from this, a few of my informants at the major departments had connections to the Social Democratic Party but did not consider themselves heterodox in any sense, although one reflected that it had influenced the choice of research topics. Among several informants, it seemed to be regarded as fact that a significant share of doctoral students at least in the three Stockholm departments (Stockholm School of Economics, IIES and the economics department at Stockholm University) are active Social Democrats today.[63] However, no one linked social democracy with economics heterodoxy, and it seems that a Weberian type of argument about the value neutrality of research, but not the selection of research questions, is well established and rehearsed here.

[63] An explanation given for this was that after the Social Democrats lost the 2006 election, ambitious young Social Democratic economics students had fewer career opportunities, and thus found pursuing a doctorate an attractive option.

Others emphasised the role of explicitly accounting for values and democracy, or politics, as fundamental to studying economic issues, or political economy. Heterodox scholars seemed to be united by their general and deep interest in issues of methodology and philosophy of science. These topics that are, as I argued above, very much neglected by mainstream economists, or at best considered scientifically irrelevant, are central issues for heterodox economists. For example, more than one professor recalled the formative influence of discussions on Althusser's philosophy of science, among others. Later, as I will develop below, the critical realism of Roy Bhaskar and Tony Lawson seems to have been an important common reference point for some of the heterodox economists, drawing a line from this strand of philosophy back to the Althusserian discussions of the 1970s.

Mainstream economists are of course not just passively enacting a scientific habitus, but in their work with solving new problems, exploring new data and novel areas, tend to simultaneously reproduce an overarching general approach or style of reasoning, because it is understood as scientifically productive and valuable. The same thing is true of heterodox economists, only their intellectual projects are not the same as those of their mainstream counterparts, and their intellectual dispositions and active choices lead them to emphasise other things. These orientations boil down to a common critical stance towards mainstream economics. This critique can rest primarily on a metatheoretical critique of its ontological and methodological assumptions, close to the position of Tony Lawson, or as the general impossibility of formally modelling social life. Or it can primarily be a critique framed in terms of the absence of actors' values and democratic agency in too-abstract modelling. Or it may be framed more simply as a critique of a narrow insistence on a small set of methods rather than employing a broader methodological toolbox, borrowing more from neighbouring sciences. The bottom line is that common to the heterodox economists is a rejection of part or all of the fundamental building blocks of the mainstream project: the meta-axioms that make up its ontological presumptions, and the epistemological insistence on the primacy of modelling behaviour. But these rejections come from different angles, and there is hardly a common alternative ground, or a coherent alternative style of reasoning. Of course, the heterodox economists in my interview sample are just a handful of voices, but that also reflects the status of heterodoxy in Swedish economics. This observation that heterodoxy seems to lack a well-defined common ground, is also to be expected from the literature reviewed in chapter 2.

Exclusion and alternative communities

In this material, there are several examples of experiences of exclusion, often deeply felt, which is in strong contrast to the sense of professional belonging to a worldwide community of researchers thanks to the strong common ground of the profession. There seems to be a general sense of not belonging, being an outsider to a powerful "them", or of oneself or others in a similar position being actively pursued and excluded. Among these stories are for example accounts of what is perceived as organised "extra opposition" by the disciplinary elite at a PhD defence. Another heterodox economist described how, in his telling, the department decided to get rid of its few non-neoclassical economists many years ago, and how this strategy included hiring a new head of department with the task of making life hard for him. As part of this perceived strategy, he recollected how at one point he was forced to meet with a psychologist: "they tried to make me a basket case" he averred. At another university, one economist with a background in late 1960s Marxian economics described how a new professor came to "clean up" in the 1970s, and how they all thought that it was part of a larger plot devised in Stockholm by Assar Lindbeck. Every one of the Marxists were expelled, apart from him, because he happened to be a very skilled mathematician. This is how he recalled the meeting when he was called to the office of the new head of department:

> I: He says [. . .] more or less explicitly, roughly, that "you have two options. Either, you become my mathematical slave and do my formulas for me, or you fuck off." And I chose to fuck off. And then a war started.

> Of course, we should take the truth values of narratives like these for what they are: accounts used to express and convey a relation to what is perceived as a thought collective that is a hostile other, to which one is an outsider. Accounts like these and the fact that they are recollected and retold as part of a narrative about what it means to be a heterodox economist, tell us something about this experience of belonging not only to a different thought collective, but also of being subject to intentional exclusion and the exercise of power. Such narratives also convey a sense of identity and belonging to some form of other, heterodox, collective, a thought collective shaped relationally in direct opposition to the mainstream, in and through these antagonistic experiences of exclusion. I argue that to understand the reproduction of the mainstream disciplinary style of reasoning, we must understand how the attention to its core is in fact relationally dependent on contrasting to an outside. When it comes to heterodox economists, the relational nature of the thought collective and its critique of the mainstream style of

> reasoning is an obvious and apparent fact that doesn't require analytical work to be uncovered.

Apart from these more spectacular narratives, there are also lower-key stories and reflections about the working of power in the discipline. One informant recalled stories from acquaintances in grants committees that, according to this him, will not grant funding if the applicant is not considered "a proper economist", adding that this is of course anecdotal and can never be proven. In conclusion, he claimed that:

> I: In my view, power plays a very central role when it comes to economics, and those who rule the funding agencies have a very large influence and responsibility for where research money within economics ends up, and they almost solely go to that which in some sense is mainstream.

There seems to be a marked difference between those heterodox economists who have voiced critique of the mainstream publicly, and some economists who share a lot of that critique, but at the same time don't want to make trouble. One such professor described how intellectually lonely it is in many ways, and that his network in international heterodox circles and among economic historians provide spaces where "the air is easier to breathe". This points to the role that alternative networks and communities plays in forming a loose heterodox thought collective.

The heterodox economists I met were largely not in close contact with each other, nor formed an integrated thought collective on a national level, but instead had other international heterodox networks, for example in connection to their specific field of study or school of thought, and their international conferences and networks. The importance of such alternative networks was also highlighted by one leading heterodox economist who increasingly turned to the explosion of economics blogs after the 2008 crisis, and then started a blog himself. He described his large audience of likeminded economists, and the sense of being recognised when internationally well-known economists interact with his writings in the blogosphere. The role of this blog and other blogs as a heterodox meeting point was also mentioned by other heterodox economists. This points to the general recognition of a belonging to a heterodox collective, beyond internal differences; that networks, conferences and journals or blogs provide an infrastructure for an otherwise heterogeneous thought collective. Among the different schools of thought within heterodox economics, it seems that the recent establishment of an informal Swedish network of post-Keynesian economists that gathers together many researchers working broadly within that approach, beyond disciplinary boundaries, is currently the most important attempt at building a more integrated network in heterodox economics.[64]

Heterodoxy, philosophy and the critique of formal modelling

In the critical stance toward the disciplinary mainstream, philosophical or meta-theoretical reflective arguments about the nature and role of science and scientific explanations play a central role. In terms of styles of reasoning, the heterodox critique of the mainstream disciplinary style is synonymous with a more or less critical stance towards a perceived over-emphasis on formal modelling. It is interesting to note that Tony Lawson's philosophy of economics was mentioned as a very important source of inspiration by several heterodox economists. Since Lawson extensively critiques the insistence on mathematical modelling in economics, and has developed it through several books and a range of papers since the mid 1990s, his brand of philosophical realism has received much attention in heterodox circles internationally (see Fullbrook 2009). In an interview with one professor, we came to talk about the role of different heterodox economists and works in creating a heterodox thought collective:

> AH: You were telling me previously about Fred Lee and his role as a facilitator and network builder within heterodox circles. Has this been important to you, to be a part of international networks and to have international contacts?
>
> I: Well, in practice it hasn't really been, to the extent that I haven't really used it for personal contacts with these people, but it's been through the web more, using these sites that he founded, his newsletter for example to get information and discover what others do and find interesting articles, books, conferences [. . .] But in that way it surely has been important, as information channels. I think that's an important thing, not least for those who don't stand for the mainstream, that you realise that you're not alone with these thoughts, that you feel that "wow, I have thought about this and believed I was a bit odd, and then you discover that there are a lot of reasonable people that has exactly the same ideas. Perhaps I am not as dumb as I and people around me at the department have thought." In cases like that it has a really strong effect on you, especially a young academic with deviating views, I think, to see that there are others that have had similar thoughts. You are

[64] The post-Keynesian network launched a website at www.post-keynesianer.org, under the banner "for a pluralist economics", in 2017.

> not as odd and weird as people would have it. Just to mention one example would be Tony Lawson's book *Economics and Reality*, when that was published . . .
>
> AH: Yes, right, in 1997?
>
> I: That's right, yes it came in 1997. That was really one of those books that was just "wow!". I often come to mention it when I talk to colleagues, that this was really a book that has meant a lot to me. Not because it brought in a lot of things that I didn't know, but on the contrary, there it was, in writing, very very much of what I had held and thought myself, but never really got the opportunity to write and express myself. It really gave a hell of a lot of strength. He is an esteemed, well-known economist, and he thinks exactly, that's what I thought then, exactly the way I have been thinking for a while. Damn, how can they go on that way, and does that mathematisation really hold tight, and the deductive-axiomatic method, and so on? These were things that you sort of never had gotten any response to when you voiced critique, but more of a "well, well", being silenced, and no one thought it was interesting. And then, suddenly, you realise that here is a person that writes about it, and it is not just any unknown publishing house that has published it. So that's really a book that I often come back to. It was an important book to me, really. [. . .]
>
> No one can take that book from me, it was incredibly important. I think it was, to many within the heterodox movement, that it was really significant. I think that history will show that. It is a milestone, somehow.

In this account, we find a story that connects the experience of being dismissed and misunderstood with the power of finding others who share similar ideas, the microworkings of a thought collective in the making. An interesting feature here is also the role of the internet and the electronic newsletters and blogs as information centres. The role of network builders like Frederic S. Lee and the philosophical work of Tony Lawson is also strongly emphasised in the above excerpt, and was mentioned also by some of the other heterodox economists. Lawson's is probably amongst the most important works uniting heterodox economists who point to the need for other modes of analysis in economics, relying on, for example, historical, evolutionary or verbal accounts. The above excerpt also exemplifies another scientific habitus, expressed here as the deeply-held feeling that that there is something wrong, something that one is not in agreement with, in the dominant intellectual environment, and the subsequent sense of relief and revelation when finding a work that resonates deeply with one's own disposition and ideas.

Of course, all arguments cannot be reduced to habitus. On top, so to speak, of one's intellectual dispositions and more or less unconscious preconceptions and assumptions there are always well-developed rational arguments and justifications for holding the position one does. This is also clear in the role that arguments from the philosophy of science plays in much of heterodox economics. For example, when I mentioned the common notion that the formal modelling is used in economics because it functions as a powerful or even necessary tool for formulating scientific problems and solutions, as understood by most economists, a heterodox economist protested:

> I: But it becomes upside-down if you reason in such a way! You really need to first consider the ontological question that the economists are unwilling to handle: what is the world like? It is another thing that many economists have a predilection, an unfortunate predilection for mathematics and natural science and are eager to use the arsenal of means those sciences have, formal logic and mathematics, to analyse something that perhaps does not suit handling with these arithmomorph concepts, which is implied in mathematics, that everything becomes numbers.

Here, this critic of the dominant style of reasoning uses the resources of the philosophy of science as a tool to dispute and undermine the scientific claims of the mainstream. The work of Bhaskar and not least Lawson were also mentioned as important sources of inspiration by other heterodox economists, or other work in the philosophy or methodology of economics. It is worth emphasising that while heterodox economists engage very much in philosophical critique, and take philosophical questions to be a serious and important matter, in the normal science of the mainstream, philosophy is disregarded as possibly fun but of no scientific use at all for the research front. This is not only a question of a scientific habitus or intellectual disposition towards this form of inquiry as relevant and interesting. It also means that philosophy or philosophical arguments are employed as means (tools, or perhaps weapons) by heterodox economists to contest the boundaries of economics constructed in terms of a distinct style of reasoning. If the heterodox critique of mainstream economics can be understood as a critique of very fundamental assumptions about the nature of social (economic) reality and relations, and the valid and reasonable methods that can be used to study them, that is, as a critique of the dominance of this style of reasoning, it seems natural that such rejection of fundamental assumptions leads back to philosophical problems.

Heterodox critics strive to construct the boundaries of a field of economics or economic analysis in a different way, to include a broader set of ontological and epistemological assumptions. If this is a contestation of the dominant and powerful construction and maintenance of the boundaries of the disciplinary style of economics, it is also a form of active work in the social construction of the symbolic boundaries of a scientific field. Perhaps we could think of it as anti-boundary work (in its capacity of poking holes in the boundary walls built from the other side), or alternatively and perhaps more fruitfully, we can think of the construction of boundaries as the negotiation or struggle in a field of forces, the outcome of which is the partial stabilisation of contingent boundaries.

Heterodoxy as heterogeneous rejections of the mainstream style

Heterodox economics, or even semi-heterodox views, may mean quite different things, and don't form a strong coherent intellectual movement in the Swedish context. Although the international consolidation of heterodoxy and increasing use of the term heterodoxy for self-identification seems to have been influential and some of the interviewees explicitly talked of themselves as "heterodox economists" and referred to a broader international community, there does not currently seem to be a critical mass with sufficiently tight networks to form a heterodox Swedish thought collective, although some attempts, noted earlier, have recently been made in this direction.

If heterodox economists come from different theoretical backgrounds, ranging from environmental economics to institutionalism, Marxist or post-Keynesian economics, there seems to be a common ground in their arguments about the deficits of the contemporary mainstream. This is coupled with a marked interest in methodological and philosophical problems, and well-developed arguments, for example about the lack of consideration about the ontological make-up of social reality, or a narrow focus on econometrics and formalised models of behaviour and the problem of unrealistic modelling assumptions. In this sense, heterodox economics is unified in its rejection of the established disciplinary style of reasoning. However, it does not present a coherent common alternative style of reasoning, but instead seems to be marked by a plurality of heterogeneous positions. The unity is found in the common critical relation towards the disciplinary mainstream, and the promotion of pluralist ideals. But there are evidently also signs of an increasing unification that partially confirms the Lee thesis mentioned in chapter 2, the notion that heterodox economics since the 1990s has increasingly been used as a common identification for particular schools of thought, and that there is a movement towards heterodox theoretical integration. While a few of the semi-heterodox economists in my sample rejected the heterodox label, it seemed to be well established and used for self-identification by others, sometimes interchanged for more specific identities like "post-Keynesian". Post-Keynesianism seems to be the theoretical orientation that Swedish heterodoxy mostly gravitates towards, evident not least through the recent establishment of the post-Keynesian network.

5. Journal rankings, the new job market, and the internationalisation of economics

I have discussed the formation of a scientific habitus, the relational character of the disciplinary style and the role of boundaries and boundary work, and investigated the community of heterodox critics. I now turn to some aspects of the institutional structure that stabilises the disciplinary style and integrates the national with the international discipline. Swedish economics is indeed today a branch of the international economics discipline. From the accounts of, especially, the generation that came of age academically in the 1960s and 1970s, this integration has been a very welcome development. A professor from this generation who pursued his doctorate in the United States at the time bore witness to the intimate atmosphere then common in small Swedish departments, with a small number of faculty, and lively discussions on the sociopolitical topics of the day. However, he also described the experience of the modern "economics department" in the United States as something of a revelation, a new form of very productive scientific machinery, which fortunately has since become established in Sweden today.

Connected to this is a strong favourable feeling of identification with the international discipline, and a related widespread notion of the *top journals* in economics as very important publication outlets. In several of the interviews, doctoral students could name the top five journals, at times with hesitation, but clearly aware of roughly which they were and able to discuss their merits. Many were also very aware of the need to land good publications to secure one's career. The increasing reliance on top journal publications was also mentioned as a fact, although there were also reflexive and somewhat critical voices about this development. For example, I was told by an economist who thought this development had gone too far, that there is a rumour that apparently you can get a professorship on the sole merit of one article published in *Econometrica*, one of the top economics journals. This informant explained how this may shape both publication and research strategies:

> I: If you get a publication in *Econometrica*, then it is enough to become full professor at many places in Sweden. That's how I understood it, at least, so if you can make it there, then you have it almost sorted already. So that's rather special. It means that if you want to become a full professor, if that's your goal, you will start to plan your research in such a way that it gets published in these journals. It is quite hard then, if you think that you are just going to pursue free research, or if you have ideas that you want to . . . that your research will build upon, completely or to a large extent, then you will probably not be published in *Econometrica*. So it becomes sort of a converse thing, that you design your research to get into those journals.

This reflexive account of the epistemic effects of an orientation towards the top journals speaks well for itself. It follows that if research is oriented more or less explicitly towards the demands of a small set of top journals, the conception of good science and publishable pieces shapes research in a very large circle, the radius of which is determined by how far the belief in the disciplinary core and the professional elite extends. And in a second step, if the reproduction of the thought collective is built on a filtering mechanism that requires future professors to compete through publications in top journals, we see the contours emerging of a very strong mechanism for the stabilisation of a disciplinary thought collective and style of reasoning.

This strategic orientation towards the receiving end of research output is also evident in the interviews with doctoral students. But what is peculiar here is the importance of the very recent and ongoing integration of the Swedish discipline into what is known as the "job market", a US-international formalised institution of disciplinary career allocation. The recruitment process for holders of recent economics doctorates in Sweden is rapidly being transformed by this process. All big Swedish departments are now more or less fully integrated in the so-called job market circuit, where doctoral students apply for positions at a big conference held as part of the ASSA conference held in the United States every January.[65] Thousands of fresh PhD holders use the online platform to apply for positions and are scheduled for presentations of their best paper, known as "the job market paper", and interviews with potential future employers at the conference venue, while their seniors enjoy a regular conference experience. Those who impress potential employers get a so-called "fly out" to that department anywhere in the world, and travel along other top candidates to present the paper at a seminar, and are thoroughly interviewed by faculty. Swedish departments now send teams

[65] The American Economic Association hosts an annual conference in the United States known as the Allied Social Science Associations.

to interview candidates in this very standardised procedure. Stockholm School of Economics led the way about a decade ago, and since then, one department after another has followed suit.

This centralised and formalised procedure means that the recruitment process used in US academia is now spreading around the world, and is being integrated into the Swedish discipline. This standardisation and increased internationalisation is a strong mechanism for disciplinary stability along the lines argued by Andrew Abbott (see chapter 4). Abbott argues that modern US academic disciplines get their stability from the dual institutionalisation, that each local department is in its structure a clone of every other at each university, and that the labour market, with the doctorate as its entry requirement, ensures the circulation of people within the national disciplinary labour market. However, this labour market is no longer national, but increasingly internationalised in a very formalised way through this institution of the job market.

Like the orientation towards top publications, this process also has its epistemic effects. This was indicated by informants who explained that many new doctoral students nowadays, really in the last few years, start thinking of the job market from their very first year. These students are adapting towards writing a very good job market paper which can be strategically planned in advance to increase their competitiveness in the job market. However, the emphasis on the job market paper means that the dissertation, which is always a compilation thesis nowadays, changes character. Some doctoral students as well as their seniors mentioned this increasing focus on the job market paper. For, if competition in the job market hinges on just one paper, the other two or three papers in the dissertation decrease in importance. So the tendency highlighted by these informants is that new doctoral students with a strategic inclination tend to put a lot of work and effort into one paper. This paper, students told me, should of course be very good, but it is also understood to be a show-off piece, where one wants to show that one is really skilled in a particular methodology or technique, and so stand out from the crowd. The tendencies that these remarks point to suggest another potentially strong mechanism for disciplinary stabilisation as doctoral students increasingly narrow their focus and orient themselves to the demand of the disciplinary elite, in an objective and institutionalised competition for career opportunities.

To sum up, economists are very aware of the existence of a set of top journals, although some are sceptical of the perceived over-emphasis. Career opportunities seem to increasingly depend on publication in these outlets, so that their power of disciplinary stabilisation increasingly becomes stronger. Through the recent integration into the job market circuit, the internationalisation of the discipline have become even more marked, and yet another mechanism of disciplinary stabilisation added.

6. Professional identity, styles of reasoning and intentionality: Concluding remarks

The interviews with economist provide broad insights into how the discipline appears from within, from the perspective of economists. While scientific habitus seems to be at least partially a question of self-selection, but it is clearly in the PhD programs were the important training and formation of a scientific habitus takes place. As evidenced by both doctoral students and directors of PhD programs, the international standardization of training is understood as beneficial, and is an intentional design. It is linked to a strong sense of disciplinary identification with the international scientific discipline, and the notion of a strong common core of the discipline. The economists' accounts of this disciplinary core aligns well with what we know from the literature survey in chapter 2, and is a central component in the disciplinary style of reasoning. However, the interview material also illustrate the slow shift in the disciplinary constellation of styles, with the increasing importance of empirical econometrics as part of the data revolution, but also the nascent emergence of the experimental style in economics, which is still not fully embraced within the mainstream. I also showed how the epistemic ideals of a rigorous and unambiguous science are essential to the style, linked to the appreciation of a shared common language and universal clarity of argument.

The relational nature of disciplinary identity and epistemic inclination was demonstrated in the various ways in which contrasting was used to define the core of economics. One aspect of this disciplinary identity, which also confirms the literature, is the general neglect of philosophy of science and the history of thought as unproductive (albeit sometimes fun) areas of study, in stark contrast to the importance of these topics to heterodox economists. The intellectual trajectories, scientific habitus and choices of heterodox economists are in general often markedly different from those of their mainstream colleagues. An important aspect of the relational construction of disciplinary identity is the widespread feeling of mistrust and hatred directed against economists and economics, which is mirrored by the way that heterodox economists instead experience exclusion from the mainstream. Thus, identification with either the mainstream or heterodoxy is hardly innocent, but involves a certain commitment to one's thought collective. However, while boundary work is oftentimes applied to disciplinary boundaries, I showed how the styles approach can allow us to understand variations in the object of boundary work. This is exemplified by instances when economists argue about proper science and aligns with practices (like modelling or statistics) in other sciences. I argued that this exemplifies the way in which a Crombian style may act as a bridge over disciplinary boundaries, while in other instances it may reinforce barriers within a discipline, as when heterodox economics is misrecognised and denied status as valid approach in economics.

Finally, the interviews pointed to the institutional role of the journal rankings and the emphasis on top journals in contemporary economics. A related institutional evolution, that likewise serve to integrate the national discipline more closely with the international-American discipline, is the very recent integration into the job market circuit. As some of the interviewees reasoned, this may have potential effects for the way new generations of economists align, strategically and epistemically, with the demands of these institutions.

Chapter 8. The transformation of quality judgement: Style and boundaries in expert evaluation reports

The peer review system is a central site where the distribution of recognition and reward in science is regulated. In the institutionalised practices of evaluation processes, the commitment and disposition of researchers—their scientific habitus—is activated when they use their professional judgement to determine and rank the quality of articles, project proposals or entire scholarly oeuvres. There are also tools, or judgement devices, that evaluators may use to supplement or reinforce their judgement. In this chapter, I present an analysis of the expert evaluation reports of applicants for professorships at four top Swedish universities. I will show how the institutional form of evaluation practices has been transformed during the studied 25-year period, and argue that this amounts to a transformation also in the mechanisms that reproduce the disciplinary style.

The first sections are devoted to presenting the normal science of the discipline as it emerges from the expert evaluation reports or *sakkunnigutlåtanden*. This analysis reveals how the evaluators understand the disciplinary core and what good science means to them. The first of these sections discusses the role of modelling practices as conveyed by the evaluators. The second section turns to the elevated role of econometrics and technical skill and the role of applied economics. The third section focuses on evaluators' discussions about the breadth and depth of the applicants, and argues that a notion of a disciplinary core, a general ability to formulate problems and reason in economic terms, is apparent in the material.

The second part of the chapter turns from the image of the discipline in these documents to the practice of evaluation itself. Experts need to employ effective ways of differentiating among candidates and to justify their judgements to an audience of other economists as well as non-economists. This produces succinct and concentrated statements of their conception of scientific excellence in economics. As a by-product, the differentiating evaluation practice also reproduces the notion of a disciplinary core, what it means to be a really good economist, and its negative, that which is not proper economics. This way, the evaluation practices also perform boundary work, and the identification of the most highly valued research within the collective understanding of the disciplinary style is also fundamentally relational. The reassertion of the symbolic categories and boundaries of top-ranked and acceptable economics, is forcefully transformed into the social categories and boundaries of the discipline, since the allocation of professorships hinges on the top ranking of an applicant by reviewers (although they do not formally make the decision), and academic career opportunities are in a wider sense dependent on peer review of scientific quality.

As I show in section four, the institutionalised practices of quality evaluation that leave their traces in the evaluation reports have been transformed drastically during the quarter century from 1989 to 2014. Whereas experts 25 years ago relied solely on their own expert judgement of their peers' work through thorough reading, by the end of the period they have come to rely to a very large extent on a new type of judgement device. This is the use, in evaluations, of the top economics journal rankings that the discipline is so fond of. The fifth section returns to the earlier part of the studied period and, using the new judgement device of journal rankings as a contrast, shows how evaluators used to rely more extensively on reading a broader range of submitted materials, including books, using their professional judgement to produce lengthier qualitative judgements.

Normal science, or, how to be an excellent economist

The economists who produce the evaluation reports are recruited by the hiring faculty in their capacity as senior experts and representatives of the discipline. These are senior economists who are entrusted with the central task of evaluating new professors by their colleagues. Their evaluations will have effects, not only directly through the evaluation and final recruitment of new professors, but also through their valuation and categorisation of different forms and aspects of scientific production, since evaluation reports also function as authoritative representations of the scientific field. First of all, the reports produce an overview of what sort of work aspiring professors have hitherto been doing. This is not just a neutral reporting of the heterogeneous material that applicants have submitted. Rather, the experts' presentation may contain more or less a primary screening of the material and individual studies or fields of study that will be represented, and how much attention will be devoted to different areas of each applicant's work.

The representation of normal science is thereby already more or less filtered through the experts' judgement.

The research fields represented in the reports are manifold. The applicants work in more theoretical research fields like microeconomic theory, game theory, theories of fairness and optimal taxation. Most applicants work in several fields, and may combine theoretical studies with applied studies using empirical data and real world cases. These may include, for instance, empirically applied theories of optimal taxation, public economics, macroeconomic issues like international trade, monetary economics, macroeconomics and politics, or public choice approaches studying political actors as rational decision-makers. Other studies may draw more or less on statistical techniques and apply econometrics to various areas ranging from demographics, historical time series and rational expectations, to more technical and methodologically-focused studies where properties of measurements or models themselves are the focus. We also find some studies, especially in the earlier part of the period, which fall within the history of economics thought, while we increasingly start to see studies in experimental and behavioural economics after the turn of the millennium. The experts, like economists in general, often make the abovementioned coarse distinction between *theoretical* and *applied* economics. This conceptual distinction presupposes the idea that economics has an abstract *theoretical core*, which may then be *applied* to a broad range of specific fields. "Applied", then, normally seems to mean that theoretical models or econometric techniques are used on some form of empirical data. Apart from the theoretical and applied sides of economics, experts also discuss methodological or technical mastery as a central area of economics, whether this is shown through empirical studies in applied fields, or in more purely methodological work. I will present these central areas in turn in the following two sections.

1. Modelling as a central practice

We know from previous research that the modelling approach plays a central role in economics, at least since the middle of the twentieth century. This is also evident in the reports which provide direct evidence of the ways that experts actually report and discuss modelling. In their reviews, we are repeatedly presented with brief accounts of studies that employ one or another form of modelling. In an ideal case, the reviewed researcher constructs a model based on some assumptions and shows, using the model, its consequences. Such modelling practices are ubiquitous, taken for granted, and expected. It is not a marginal practice, but at the core of this style of research. As in any science, creativity is one of, if not the, most highly valued characteristics of excellent research. But Kuhnian normal science in economics consists, to a large degree, of applying established standardised models. These standard models may be applied in a new setting, with some parameters altered in a novel way to investigate the effects on the outcome, or applied to new data. Reviewers often report on applicants using different forms of "standard models" as if it is both an expected and normal scientific practice. Although the reports tell us about a range of successful researchers, often among the most highly ranked, who use standard models of some sort, it is also clear that excessive repeated use of the same standard model framework by the same researcher is not a sign of scientific creativity.

Simple models and essential questions

Modelling presupposes simplicity and far-reaching abstraction. First, simplicity is a prevalent and valued feature of models. The simplicity and clarity of models is primary, and the question of realism of their assumptions is at best secondary. Second, simple models are valued because they help economists bring clarity and illuminate interesting relations. Simplicity is understood as essential to good modelling practice, and is normally not treated as a problem. Instead, the simplicity of such "toy models" allows the investigator to illuminate a relationship and shed light on its properties as they appear in the model. Let me provide a few illustrations to how this is expressed in the reports. In one report from 2009 (SKU UU 2009)[66] the evaluators praise an applicant's fine modelling of a problem that is not obviously an economic problem to an outsider. They explain that "the best of Edlund's early papers are characterised by a powerful idea investigated in a simple model that leads to important insights", which is exemplified by a paper that "develops a simple model in which parents have preferences over the sex and marital status of their children".[67] The experts conclude that "with this simple

[66] References to the cited expert evaluation reports use a system of human-readable abbreviation. All documents are prefaced SKU (*sakkunnigutlåtande*). The two following letters indicate the university (UU=Uppsala, SU=Stockholm, LU=Lund, GU=Gothenburg), followed by the year of the report. A full list of the analysed evaluation reports is found in Appendix 1.

[67] I have chosen to present excerpts from the material without anonymisation. Hammarfelt and Rushforth (2017) have argued, using the same type of material, that evaluators and evaluated candidates have not necessarily been aware that the documents may become publicly available and read, and their identities don't add to their analysis. However, in the present study, keeping all names adds a slight dimension of context to the qualitative analysis. More importantly, given the important role that expert evaluation reports have played, often read and discussed by colleagues, I

model (and various modifications of it)", the applicant "is able to derive very interesting predictions", namely that it leads to "an equilibrium in which the top half of society has boys", which they consider "a striking conclusion". This first example illustrates both the way that experts sometimes talk explicitly about what is taken to be a fruitful practice in terms of "simple models", and the way that the modelling framework may be brought to bear on a wide range of questions, sometimes outside the traditional core areas of economics.

Another example of even more remote theorising is found in the review of one applicant who has analysed slavery as an abstract phenomenon, tackling

> the ambitious question of endogenous property rights and why societies progressed from a relatively egalitarian state with no land property or slavery to one with property in land and people (slavery) and lastly to one with only property rights in land. He sets up a dynamic model in which the elite chooses at each moment the property rights regime that most benefits itself. The choice depends on the value of two state variables: land productivity and population size. (SKU SU 2009)

Historical realism or complexity are obviously not the epistemic goals of this model. But the reviewers are impressed: "This is a nice paper in which economic institutions change endogenously over time" (SKU SU 2009). The beauty of a simple model that is able to explain the historical transformation of modes of production using two basic variables is evident here. This example, like the previous one, also point to the way that economists may venture into areas outside the core areas without the reviewers even commenting on it in the reports. But while the topics may seem "un-economic" to the outsider, the modelling approach certainly is not.

A third example is an applicant working in a more classical topic in an early evaluation report (SKU SU 1989). This is an example of an equilibrium model used to study taxation. In the reviewed paper, the author

> constructs a two sector general equilibrium model with perfect competition. The first sector is taxed and the second is untaxed (home production, black sector, spare time). The model is used to investigate how different types of increase in tax and expenditure affects resource allocation, tax revenue and welfare. (SKU SU 1989; my translation)

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would suggest that the public nature of these documents is well known to everyone involved. When reports are written in Nordic languages, I have translated the excerpts.

The author then uses Swedish data from 1979 as parameters in the model and "draws the conclusion that Sweden probably has reached the limits for what the traditional taxation of labour can give when spending is used for goods that are perfect substitutes to private goods." Examples of this form of simple models are numerous. The preference for simplicity over complexity seems to be taken for granted, and is sometimes even explicitly discussed. In one review (SKU GU 2005), an applicant has been working with models of financial markets, and the reviewers first note that modelling frameworks with heterogeneous agents "are often very complex". In contrast, the applicant and a co-author "develop a simple model of heterogeneous agents", resulting in a simulation which results in the same key facts as the more complex models. Here, one important technical feature of model simplicity is made explicit by the evaluators who highlights this feature: "Due to the model's simplicity it can be solved analytically".

Striving for simplicity is not only an epistemic virtue in itself. It is also a technical requirement for mathematical modelling with the goal of finding unique solutions. Sometimes, novel work includes formalising ideas that have previously only been verbally expressed. This is what one line of work of the mathematician-turned-economist Jörgen Weibull does, according to an evaluation report where we learn that these papers "can all be seen as very meritorious mathematical formalisations and precisions and specifications of ideas earlier presented in Kornai's work on non-equilibrium economies."(SKU SU 1989). This example shows that not only is modelling itself valued, but also the groundwork in producing simplifications and formalisations that are the prerequisites for modelling.

The common use of simple models is well known from the literature and should not come as a surprise. The way that the expert reports discuss this aspect of modelling practice, take it for granted, and even praise it, confirms this image, as does the fact that it appears to be as widespread and accepted today as it was a quarter century ago. I would like to emphasise that most of the examples used here are taken from the very brief passages of text, where reviewers express their judgement about the essential qualities of an applicants' total scientific output in just a few sentences. When similar ideas appear across evaluators and evaluated applicants, we are seeing evidence of a structure of judgement, one aspect of which is the emphasis on simplicity and clarity of models.

That said, there are also the odd counterexamples in the material, where experts comment that a particular model used in a paper is perhaps too simplified. This points to the practical sense of judgement of the level of allowed simplicity, and what sort of abstractions one may undertake in different circumstances. Even if the level of simplification may be striking to an outsider like a sociologist, it is not without bounds. There seems to be a somewhat established understanding, of the range of possible simplifications. This practical sense of how to model, and how to judge a reasonable level of abstraction, is one important aspect of the scientific habitus, which here plays out in the evaluation practice. This is evidenced in the scattered examples of when experts comment on too simplified, or even unrealistic assumptions. This is the case in one report reviewing an applicant's theoretical paper on commitment problems of governments, where reviewers find that "Analytically the paper is impressive"—here understood in terms of elegant modelling—but continue with a caveat: "even though the chosen setting is one where the government has to balance the budget each period, consistent with the chosen equilibrium concept", it is "debatable as a description of what governments actually do" (SKU SU 2009). Comments like this one on the questionable realism of modelling assumptions imply—contra Friedman's instrumentalism—that realism of assumptions is not altogether irrelevant.

In another example, the reviewer comments that an applicant sometimes seems to use models that are a bit too simple, but on the other hand, acknowledges that this may be a deviation from the established disciplinary view, as exemplified by the top journals:

> Tore Ellingsen appears as a very prolific writer with a quite wide field of interests, who is very well versed in his subject. Occasionally, it is difficult to avoid the impression that the balance between the very promising titles and the many allusions to applications in the introductions to the papers on the one side and the rather simple models, often having the character of illustrative particular cases due to the severe specification of all relations, is somewhat uneasy, but it should be added that much of the literature, also in so-called "leading" journals, are presently of this type. (SKU SU 2000)

The limits of simplification seems to be not only a question of more or less simplification, but also a judgement of what the important central economic questions and relations are. Although it may sometimes seem that unrealistic simplifications are permitted, this points to a shared understanding of fundamental aspects of real economic relations. In other words, the generally acceptable modelling assumptions are part of the scientific ontology of the disciplinary style of reasoning. This consists of a shared understanding of what sort of assumptions about economic actors and relations can legitimately be made. If one understands such shared presuppositions not as a fixed dogma, but rather as a body of knowledge and intuitions that has somewhat fuzzy boundaries, we can see how there are liminal cases of simplifications that are "debatable"—

acceptable to some but not to others, while the standard core assumptions are not normally questioned.

Clarity and illumination

If economists prefer simple models, it is because of their ability to provide clarity and insight, as was evident from the example of parents' preferences regarding their offspring cited above. In fact, the preference for clear, unambiguous and illuminating results is one of the central shared values of the economics profession, as it appears in the reports. In their evaluations, experts repeatedly use the language of clarity approvingly. This is not just one among several important aspects, but seems to be a very fundamental one. Thus, for example, reviewers in a 2006 report for a general economics professorship (SKU LU 2006) summarise their review of the applicant they all rank as number one: "In summary, the work of Karl Wärneryd is characterised by an expertise in game theory. The papers are always very clear, and the models elegantly and adequately enlighten the problem under consideration". The same reviewers also summarise the work of one of the other competitors in these words: "In summary the work of Mich Tvede is characterized by a strong competence in general equilibrium theory and decision procedures in economic contexts. Tvede's work is in the tradition of mathematical economics: rigorous, elegant, and deep" (SKU LU 2006). Another summary from the late 1980s illustrates the same thing:

> Svensson is a relatively productive author with a certain breadth of production. He is a distinctively theoretical economist and very technically skilled. The analysis is always clear and stringent, writings have high quality, and Svensson has a significant international publication record with several heavy contributions in good journals. (SKU SU 1989)

This applicant is not top ranked, but in the final ranking of candidates, one of the experts uses the same concept of clarity in his final verdict of the top-ranked candidate for the professorship in general economics at IIES. In the very brief concluding paragraph, we learn that "Persson is extremely talented. He has a clear and fertile mind, and the nature of his contribution has altered the direction of the literature." Here again, in the concentrated form of the final characterisation of an excellent economist, the reviewer choses to describe him, besides his obvious creativity and ability to influence research fields, as having a "clear" mind.

A notable feature of modelling is the independence of the model as a reasoning device. This is well expressed by the notion that just as scientific realists often point out, “reality kicks back” in experimentation in the form of resistance, theoretically unexpected results or serendipity, we may also think about situations where the model kicks back (Morgan 2012). This means that the modeller uses a modelling framework to investigate some relations that are too complex to analyse just using our own mental capacities. A model is constructed and its relations and parameters defined, which means that it is given fixed properties, with which the modeller can investigate what will happen when a variable or parameter is altered. The model may be a very simple one, for example the basic supply curve from the introductory microeconomics textbook that helps the student think more clearly about the relationship between the price and demand for a commodity. However, while the economics student will soon internalise that model and be able to “see” the relationship without the actual graph, other models include calculations that are rather more complex, and where the effects of tinkering with the model is not obvious to the modeller. In such cases, the model may provide answers that are not intuitively expected (even if they may resonate with the economist’s trained intuition, once proven), but may instead be striking or even surprising at first.

In the reviews, experts sometimes point out and emphasise these unexpected results. This is, for example, how reviewers express it in the example of parental offspring preferences above: the results of the modelling effort is that given the premises, the outcome will be an equilibrium where the top half of society will have boys—a “striking” result. The experts may discuss this in more general terms, for example in one review (SKU SU 2009) one of the applicants is very positively described: “all of Rosén’s papers are carefully crafted theoretical models to analyze very specific questions”, and she has furthermore “challenged well-established results and has come up with surprising and convincing new insight.” This valuation of “surprising results” should, in my view, be understood in terms of the independence and power of the model as a reasoning device. The strict deductive power of the model proves that given a certain model and parameters as input, the output effect must necessarily follow. The results of the modelling practice thus appear inevitable and independent of the subjective input of the researcher. There is then a sense in which the model provides a certain amount of epistemic authority, making such “surprising” scientific findings evidence of the “hardness” and objectivity of facts derived through modelling.

The fact that modelling relies both on simplification of assumptions and on wide-ranging technical skills should not lead us to believe that economists don’t care about language and writing style. This is, to the contrary, something that is very often remarked on in the reports. We repeatedly hear the reviewers commenting that an applicant writes very well. One report notes in the summary of the top-ranked candidate that he not only has very fine pedagogical skills, but also that “it is often a pleasure to read Person’s writings—both the scientific and the popular science works” (SKU SU 1989, my translation). Reviewers may comment in passing that an applicant is “a very good writer” and again in the final rankings argue that she “writes extremely clearly”, while some of the other applicants’ papers are “convincing and a pleasure to read” (SKU SU 2009). Another reviewer for the same case even uses clarity of writing as a central argument for the ranking order: “My reason for ranking Flodén before Fredriksson rests primarily on the clarity with which the former writes [. . .] and his somewhat greater creativity” (SKU SU 2009). The preferred style of writing is obviously connected to the preference for clarity. We learn that to write well is to write clearly and elegantly, to be able to convey and illuminate in condensed form the central interesting issue under study.

Valuing modelling: Epistemic virtue and cognitive particularism

The prevalent overarching notion of good modelling practice values its power of simplification of essential relations in a way that is illuminating and clear. It draws, as should be obvious, on the metaphor of seeing and enlightenment, where the good economist shines a bright light on a problem that disperses darkness and ignorance, lets us reduce complexity and highlights a phenomenon in isolation. The epistemic preference is complexity reduction, with the goal to isolate and abstract an interesting relationship and consider it *ceteris paribus*. This could perhaps be thought of as a general scientific epistemic ethos. However, if one considers the strong emphasis on the virtue of simplification and clarity, there are other possible epistemic virtues that must necessarily be downplayed. For example, one may regard the pursuit of complex and full accounts of social phenomena to be an epistemic virtue. Or, one could think of descriptive realism and multifaceted empirical truthfulness as a similar epistemic goal, where the researcher should strive to do justice to a multifaceted and messy reality, not being too far removed by means of theoretical abstractions from actual events as they appear. On could also think of an epistemic stance with the goal of accounting for social phenomena in terms of evolving causal processes in historical time, as in the Crombian historical-genetic style.

These examples should all be familiar from other social sciences, and, I would argue, heterodox economics. The point here is that modelling as a central practice in the style of reasoning of modern mainstream economics draws heavily upon simplification and the epistemic goal of clarity. Holding this goal as a central epistemic value means that other possible epistemic values are downplayed or disregarded. This creates boundary effects, where the classification of work that strives for other epistemic goals will be less valued. Thus, scientific quality is connected to a certain epistemic goal. If styles of reasoning and epistemic goals are in fact plural—the disunity of science—it means that the peer review sorting processes that rely on the quality judgement of peer experts in a discipline with a heavily dominant disciplinary style of reasoning will show signs of cognitive particularism of the sort investigated by Travis and Collins (1991) and discussed in chapter 3. The peer review system will then tend to be biased against researchers who pursue other epistemic goals than the dominant one. Such cognitive particularism is evident in the studied reports, and we will come back to this topic in relation to heterodox tendencies.

2. Econometrics, technical skill and applied economics

If modelling is one of the two Crombian styles of reasoning that primarily shapes reasoning in economics, the other is the statistical style, known as econometrics to economists. If modelling builds upon a drive for simplification and clarity, econometrics highlights a rather different element of reasoning. This is the value of technical mastery, and the acknowledgement of the need for advanced technical methodological skills in economic analysis. The role of technical methodological mastery and skilfulness is also present in modelling—and as Mary S. Morgan has argued, it is often impossible to disentangle the different styles of reasoning as they are often nested in practice. However, in the evaluation reports, the valuation of technical skill stands out most clearly in relation to econometric aspects of applicants' work. It is clear that methodological or technical issues play an important role in the representation of applicants' work, and a significant share of normal science in modern economics consists of solving piecemeal methodological issues, tweaking established methods or investigating some aspect of methodological procedures.

Econometrics has been central to economics during the whole period of study. But the general trend in economics from "pure theory" towards econometric studies using empirical data is also evident in this material. Reflecting this, there is a greater number of studies relying on large datasets in recent years, but the number of narrowly technical econometric papers also seems to have increased. This development is nicely commented on in one review (SKU UU 1994), which notes the development of the work done by one applicant which was severely constrained by lack of computer power when he started out in the 1970s, but that has since (in the mid-1990s) become readily available. The technical aspects of economics research are highly valued in applicants, besides the ability to think theoretically like an economist, in terms of the ability to reason with simplified models. Technical or methodological skills seems to be almost as important as thinking with economic models. As I will argue below, the combination of these skills is fundamental to being evaluated as an excellent economist. Their valuation is evident in how they are often described in the summarising paragraphs on each applicant. For example, we may learn that one is a really skilled “technical economist”, or that another masters a “wide range of methods”, etc.

Policy orientation, values, and objectivity

If mastery of technical methods and problems is valued as a skill in itself, most often mastery of method is required in empirical studies, or applied economics as it is often called. Applied studies draw on theory, methods and empirical data, and make up a large share of studies across the sub-fields of economics. Here, evaluators may choose various aspects of the studies to emphasise as examples of good work. Apart from theoretical modelling and technical skills, experts often discuss applied work on its own, often in contrast to purely theoretical work. As with balance between theory and methods, pairing theoretical insight with the ability to also derive interesting results in applied studies stands out as an important balancing act necessary to an excellent economist. The good economist should work not only with purely theoretical models, but is also able to apply his or her reasoning to empirical material.

The distance from applied work to policy relevance is short. Obviously, if an economist can use a model of, say optimal taxation, and show with empirical data that some aspect of taxation at a given place and time is sub-optimal, this implies that a better, more optimal, taxation policy could be conceived. Here, the modelling style puts the economist in a very favourable position as an expert on policy issues. For if the economist can deductively show that, given the established premises and some parameter change (for example a change in tax rates), the outcome would in fact be different, this amounts to a seemingly quite powerful prediction of a sort that is unavailable to social scientists who are not using modelling. This powerful epistemic toolbox, coupled with a good dose of self-confidence, puts the economist in a natural role as expert policy adviser to politicians. The notion of economics as a highly policy-relevant science is also evident in the reports, from the way that experts often choose to highlight the policy relevance of a paper or area of work or show appreciation for an economist whose work is not only theoretically and empirically competent, but also relevant to policy.

Being policy-relevant means having a certain close relationship to politics. However, this does not mean becoming politicised or explicitly normative. Quite the opposite; the role of economics as a policy-oriented science, as a profession of expert advisors to politics, rather implies that the distance between science and politics must be strictly maintained and emphasised at all times. When economists study highly political issues, they do so in a cool and distanced, objective and scientific fashion. Some of the studied issues are obviously deeply political and value-laden, from theoretical conceptions of fairness to models relying on a concept of optimal taxation or questions of income distribution. There is a stark contrast between the deeply political and very real human life courses that must ultimately be the real referents of these models, and the distanced, formal and objective manner in which these issues are discussed. This ideal of objectivity which is evident in the way that experts report on applicants' work is not only part of an old and widespread scientific ideal of objectivity, it is probably also related to the discipline's close relation to policy, which makes objectivity a scientific prerequisite.

It is evident from the way that policy-related and politically- or normatively-relevant research is discussed in these reports, that there is a sense in which the role of the objective and distanced expert is not only a choice of individual researchers but a collective, professional undertaking. An objective and scientific approach is part of the disciplinary understanding of what it means to be a proper economist. And since it relates not only to questions of outward behaviour, but to the very epistemic question of how one formulates and solves problems, this objective scientific ideal is part and parcel of the disciplinary style of reasoning in modern economics. This becomes obvious when one considers the difference between, on the one hand, the cool and objective attitude of economics research, and on the other, the involved and perhaps emotional, common-sense approach, but also the way that normatively engaged research is variously accepted in other social sciences. Here, the epistemic break required to enter the economics profession should become obvious. It is not that politics or values are banned, but that the role of the expert should at all cost be kept separate from the politicians. An objective researcher cannot be partisan. The objectivity of analysis means that it can be a collective undertaking of the profession. Given a set of premises, *any* economist should be able to follow the authoritative logic of analysis and accept the results that necessarily follow. Perhaps, methodological procedure and data may be questioned, but such questioning should be based on purely technical grounds.

The ideal of objectivity and the separation of fact and value presupposes and reinforces a sense of a common disciplinary ground, a consensus on a set of theoretical modelling assumptions and acceptable methodological approaches that may be used irrespective of the value assumptions that influence the choice of research topics or personally held beliefs. The shared framework, the collectively accepted style of reasoning, brings a sense of transparency to the analysis, the idea that any professional economist should be able to follow and accept the argument and its validity. Disagreement is instead primarily expressed through methodological or technical theoretical critiques of the way studies are designed, the applicable assumptions, and the data used.

This ideal of objectivity and consensus is not limited to policy-relevant research. A more general feature that stands out in the evaluation reports is the way that research in economics seems to be a question of piecemeal puzzle solving, much like Kuhn's conception of normal science. Very different fields of research are discussed in the reports, with both theoretical and highly empirical work. But there is hardly ever any mention by experts of something like schools of thought, conflicting intellectual traditions or concepts. The impression we get is that there is one body of general economic theory, and various specialised adaptations of this theory for different specific fields or problems, but that there are no real opposing theories on a matter that researchers may choose from. If there are conflicting theories, they seem rather to be cases of small controversies that will supposedly be solved by one solution which turns out to be superior.

The counterexample that springs to mind is the well-known conflicting positions within macroeconomics. However, macroeconomics is, first of all, only one of many research fields within the disciple, and most economists are not macroeconomists. Second, if conflicts exist, the expert reviews do not take an interest in them or emphasise them. The image presented is rather that all work, even within possibly contested areas, amounts to the collective effort of puzzle solving. There is hardly, if any, mention of applicants belonging to contesting schools of thought. This is perhaps most evident with some applicants who appear to be heterodox. No mention is made of this, and their work is often presented as contributing to some part of the greater puzzle (however not to the same extent as their competitors), rather than being at work on *another puzzle* altogether.

It is impossible to tell whether this aspect is consciously downplayed by experts. But we can say for certain that in the genre of evaluation reports, there is not really such a thing as "schools of thought". The normal science of modern economics seems to be one big, collective project of puzzle solving, where there is disagreement and innovation, but that tends to lead to the establishment of stable and generally agreed *solutions* to problems, rather than open controversies. It is as if the underlying motto is: *all problems have one and only one unique solution, and this solution should be evident to anyone who is sufficiently economically literate* (*i.e.*

anyone in the profession). In such a world, heterodox economics as permanent controversy is a monster.

3. Breadth, depth and the core of economics

If there is consensus on the form and tasks of economic analysis, there is also an understanding that some areas are more central to the discipline than others, and that an excellent economist should be an all-round "general economist". If experts seem to emphasise and value the theoretical modelling skills and insights of applicants, they also value the ability to apply theoretical models to empirical data, and the technical mastery of methodological issues involved in applied economics. Furthermore, the policy relevance of research is also often valued by the experts. But to be a really excellent economist, it is not enough to be a widely publicised and innovative theoretician, nor to be a masterful empirical analyst, nor to produce a broad range of policy-relevant work. One must also strike a fine balance between the various aspects of research, and experts are often explicit about this.

Breadth, and the central areas of economics

Reviewers may comment approvingly on the breadth of an applicant, and emphasise that spanning theory and empirical study is a strength over focusing on either one. In one recent review, the expert describes an applicant in the following manner: "Fredriksson is an applied scholar in the best tradition of the discipline. He combines theory and empirical research in a fine blend" (SKU SU 2009). When his scientific work is summarised, the bottom line is: "He addresses important questions, applies relevant theories and empirical methods, and derives clear and intuitive answers". A much lengthier evaluation report from two decades earlier discusses a portion of one applicant's work and, in passing, notes that these papers "make excellent examples of Persson's ability to formulate advanced, relevant, policy-oriented models, perform econometric estimations and use the results for analyses rich in conclusions" (SKU SU1989; my translation). In the final ranking, another of the experts similarly applauds Persson:

> Summarizing Mats Persson's research it must be said that his work cover [sic!] a surprisingly wide range of topics and it is both original and of uniformly high quality. The analysis is typically well set out in a simplified but transparent form to bring out the essence of the issue at hand. Though Mats Persson has some

> contributions to pure theory and technique he has foremostly demonstrated his skills in applied theoretical and empirical research. The picture is nicely complemented by some useful policy-oriented work. (SKU SU 1989)

Here we find a depiction of an almost ideal researcher, producing innovative work spanning a broad range of topics, from pure theory to applied studies, combined with work of policy relevance. Again, the analysis is also characterised by transparent simplification that cuts through to the essence of the problem under consideration. In other cases, experts often discuss the relative breadth or narrowness of applicants in comparison, and it is in general clearly preferred to show broad competence over cutting-edge competence in only one area, even if that happens to be core theory.

Similar, but not identical, to the evaluation of the breadth of an applicant's research, is the notion of central areas of economics. This is sometimes explicitly discussed by experts, even if the exact content of these "central areas" is implicit and left to the reader to understand. In the report discussed above, one of the experts opens his report with a discussion of evaluation criteria for a professorship in general economics: "Looking at different areas of economics, contributions to central areas in economics, as well as the skilful use of econometric and mathematical methods in both theoretical and empirical research, carry naturally the most significant weight" (SKU SU 1989). This notion of central areas is coupled with the emphasis on publications in general economics journals, and to how papers in such journals are understood not only as competing where competition is toughest, but also in terms of speaking to a wide general audience of economists.

For example, in a 2007 report (SKU LU 2007), we learn that one applicant has not only published in the best journals in his subfields, but that he "also has publications in some good general interest journals such as the *Economic Journal* and *European Economic Review*, showing that his work has made an impact on the broader community of economists". This connection between publication outlets and communicating with a large audience at the centre of the discipline is sometimes made explicit by experts. As I will show, the notion of the good international general economics journals has increasingly come to be interpreted in a hierarchical fashion, expressed in terms of economics journal rankings. Publishing in the *central* journals then becomes synonymous with publishing in the *top* journals, and as I will argue, this notion has itself been turned into a device that increasingly allows experts to rely on quantitative bibliometric measures in evaluating scientific quality.

The core of economic reasoning

There is also another, deeper sense in which one can talk about the core of economics, or general economic reasoning. It is the idea, emerging from the reports, that there is a certain way of reasoning as an economist, in economic terms. This amounts to something more fundamental than merely mastering techniques or the formal modelling of problems. An image of a broad economist similar to the examples given above is found in the evaluation report for a professorship in public economics. Here, the experts approvingly describe one applicant in similar terms: "In addition to Agell's more technical contributions he has also done work where the emphasis is more on interpretation and synthesis". An example of this is a book, which "is firmly based on economic theory, although the amount of formal modelling is limited, and the combination of theory and empirical research is impressive. It adds to the picture of Agell as an all-round public economist" (SKU UU 2001). Here, experts point not only to the breadth, but also to the ability to reason about economic problems without the aid of formal modelling. While this applicant has obviously shown himself to be technically competent, the ability to *reason* with economic theory is apparently a central skill. Here we approach the core of the disciplinary style of reasoning, which involves being able to view the world through the basic ontological assumptions about actors and equilibria, and building small models as reasoning tools that aid in formulating soluble problems: *interesting economic problems*.

Let me illustrate this with a couple of contrasting examples. A reviewer may emphasise when a technically-oriented researcher also has that ability to think like an economist. For example: "In his research Johansson has focused on methodological issues. His work demonstrates a deep understanding of statistical analyses but also an ability to choose relevant economic issues" (SKU UU 2001). On the flip side, experts may also comment on the *lack* of this ability to formulate relevant economic problems in otherwise technically competent economists, as when reviewers conclude that "Weibull's mathematical talents cannot conceal his inexperience in economics which shows itself most obviously in a difficulty in choosing really important economic problems to analyze" (SKU SU 1989).

These conceptions of the importance of bridging theory and applied research, of the existence of central areas and theory in economics, and the implicit notion of what an interesting problem in economics is, all point towards an intuitive sense of what real economic issues and problem-solving is. One expert describes the different areas of work of one applicant:

> The first, and largest, area involves the application of the theory of contests, or rent seeking. The same basic model of agents competing for a share of a given pie is

> applied in different contexts, typically with a view to showing how institutions can reduce the wasteful use of resources. (SKU UU 2001)

This line of work is then summarised as follows: "These papers all exploit features of rent-seeking contests in innovative and intuitively appealing ways, and are pleasant and instructive to read". When experts claim that an analysis is "intuitively appealing" in this way, or when they approvingly discuss the work of someone as being reasonable by "common sense", or that it is just "makes sense" or is "reasonable", it becomes clear that these peer experts are talking about something that is collectively considered as the common sense of the discipline, and that "intuitively appealing" presupposes a trained scientific intuition. This way the expert reports let us glimpse something of the trained judgement, semiconscious intuitions and sense of the reasonable that are part of the scientific habitus of professional economists and their collective style of reasoning.

The explicit and implicit understanding of this core of the disciplinary style plays out in yet another way. This is when experts report on work that in some respect crosses disciplinary boundaries. There are many examples in the material of papers or areas of work of applicants that may be considered interdisciplinary, and are often praised as such by the reviewers. However, what emerges from these accounts is the feeling that most of this work is perhaps not so interdisciplinary in a deeper sense. Instead, these appear to be examples of economists extending their preferred disciplinary style of reasoning to topics that have hitherto or are normally mostly studied by other disciplines. The *object*, or *area* of study may belong to another discipline, but *the way of approaching the problem* is well within the disciplinary style. Without reading the work itself, we can only rely on the interpretation of it given by the experts. But in most cases there is not much in these accounts that leads us to believe that these works involve borrowing scientific approaches from other disciplines.

Let me provide a few examples. First, in a report from 2006, one applicant is praised for what experts explicitly label "interdisciplinary work" that "bring elements of economics and sociology together in interesting analyses of behavioural issues including habits, norms and reflection (rationality)" (SKU LU 2006). These include for example studies of classical antiquity, explaining the institutions of Athenian democracy from a "rational actor perspective". Greek antiquity is of course not a common topic in economics. But from the experts' evaluation, employing a "rational actor perspective" sounds more like a form of economics imperialism than a case of true interdisciplinarity. Other examples include ventures into economic history, sociobiology or the history of economic thought.

However, the history of thought is a slightly different case, since it amounts to writing the history of the discipline and its stars, rather than *employing* the tools of the discipline. Writing papers in the history of economic thought seems to have been more common in the early period, but even then, reviewers were not too positive about it. They may comment that such work is important for the discipline, but it is quite clear that it is not considered meritorious or as publishable as proper economic analysis is. Furthermore, the described cases of history of ideas seem, from the experts' descriptions, not to be influenced by any important discussions from historiography or professional historians of thought. This relative neglect and downplaying of the history of thought is then congruent with both what we know from the literature, and the understanding of it given in the interview study.

Encounters with heterodoxy and the boundaries of economics

How do experts judge applicants whose work lies at or outside the boundaries of mainstream economics? While there are a few examples both of applicants that work closer to other disciplines, and of applicants that could reasonably be classified as more or less heterodox in the studied reports, the experts do not at first sight treat the latter as special. Even in cases where it is evident that applicants have worked in alternative theoretical traditions, this is seldom highlighted as such by the reviewers. This is congruent with the conception of economics as one homogenous field with one central body of theory without significantly different schools of thought. Furthermore, in the established practice of reviewing, experts mostly emphasise merits and rarely give critical comments.

Reviewers often find it necessary to point to weaknesses in some of the best applicants, in order to establish a differentiation between top candidates. Thus, applicants who are obviously lower ranked may be more approvingly and constructively described by reviewers. However, there are sometimes small glitches in this, small winks from the reviewers that signal disapproval or distancing from approaches that lie too far from the acknowledged core of the discipline. In one recent review, a heterodox applicant is quite clearly presented as such in the experts' summary:

> Hall's submitted work places him as an institutional economist specializing on Central Europe, whose writings have often a polemic edge against mainstream economics, and are based on verbal arguments and descriptive evidence rather than formal modelling or econometric methods. (SKU LU 2010)

This reviewer could hardly have been more clear as to why this applicant does not fit within the established style of reasoning, when highlighting the "verbal" and "descriptive" character of his work, as opposed to a more proper modelling or statistical mode of reasoning. This is a very good example of adjectives like descriptive and verbal being used as invectives, as signs of something that is really the opposite of good science. Characterising someone's work as "descriptive" clearly demarcates it as belonging to something other than proper economics, and signifies a lack of theoretical skill, theoretical clarity, or worst of all, both. This is then another instance that exemplifies the way in which the maintenance of the boundaries and the orientation towards the core of the disciplinary style works in a relational way through contrasting.

In a second example, in the tough competition for a professorship in Stockholm, one economist with work in heterodox topics is disqualified as non-competitive by two of the reviewers, and not even described in their report. But the third reviewer reports on his work and finds him competent for the position, ranking him fourth out of the eight competitors qualified as competent. But see how the expert nevertheless summarises his work:

> Summing up, the published works of Hans-Michael Trautwein show that he is a well-established researcher with a very deep knowledge of the history of economic theory, in particular in relation to Wicksell and his followers. However, he is also able to contribute to empirical macroeconomics and descriptive economics at a high level, and some of his works show that he has outstanding abilities of communicating complicated ideas in a way that they become easier to grasp for a non-specialized public. Turning these qualities around, it can be argued that the fields in which Hans-Michael Trautwein shows undisputable competence *are to some degree out of mainstream economics.* (SKU SU 2000; my emphasis)

What does this mean? Most economists do of course work more or less in specialised sub-fields. But that is seldom considered a problem, if it is also combined with a proven knowledge in the general theory. Here, something else seems to be at stake. Is this not instead an example of how the expert reviewer—the only one of the three to even report on this applicant—feels the need to indicate that this body of work is "to some degree out of mainstream economics", thereby indicating the distance to this work, reaffirming the existence and boundaries of the intellectual entity of "mainstream economics"? This example serves again to illuminate how the establishment of a shared notion of a core or mainstream of economics implies its own outside, that which is not the core of economics, its other. Thus, the conceptual reproduction of the disciplinary style is intimately linked to symbolic boundary work and the demarcation of the core of the discipline from other scientific approach.

To conclude, the analysis of the expert evaluation reports provides a view into the normal science of the economics discipline, and what scientific quality means to evaluators in economics. A good economist must be fluent in central economic theory, and be able to reason by means of deductive modelling, not only in purely theoretical terms, but also applying theories to empirical material, combining theoretical expertise with mastery of econometric method. Studies that have policy relevance have extra merit. Furthermore, the profession clearly views itself as engaged in one big collective puzzle-solving, where the transparent and objective work of finding generally-agreed solutions to problems is imperative. There is a wide variety of specialisations, subfields and topics in economics, but from the point of view of the elite of the profession, here represented by the senior reviewers, it is not really relevant to think about fellow economists in terms of contending schools of thought. There is, after all, one generally acknowledged disciplinary style of reasoning. The excellent economist has an innovative ability to extend the central modelling framework and commonly understood abstractions to new problems or settings. The idea of an intuitively sound approach, and the notion of what constitutes a relevant economic problem, points to a shared scientific habitus, a sense of judgement in fundamental scientific matters that rely on presuppositions about social ontology—that certain types of economic objects and relations—rational actors, equilibrium phenomena, etc.—are central to what is understood as "science" in the disciplinary style of reasoning. Let us now turn from the conceptions of excellence in economics that emerged from the expert reports, to the question of the evaluation practices themselves.

4. Evaluation practices: producing quality difference and boundaries

At this point, there should be a clear image of the normal science of modern economics, and the way that reviewers portray excellent economics as a broad combination of mastery of a technical methodology and the use of the central theory apparatus in constructing models. This is the substantial scientific content that we can read from the evaluation reports. However, following the theoretical work on scientific quality evaluation discussed in previous chapters, we know that evaluation is a practice with an outcome that is not fully predetermined by the object of evaluation or the evaluation criteria. In this section, I will first show that this is the case also in the present evaluation reports. Evaluators are not mechanical machines that scan a material, apply a set of rules (the quality criteria) and derive an unambiguous result (for example, a ranking of applicants). Instead, quality evaluation rely on a practical sense of judgement, grounded in a scientific habitus that is activated in the evaluation process. Furthermore, experts need to reason about and justify their evaluations, or deliberate with their fellow experts involved in the evaluation process.

The social determination of valuation

The evaluative work is not completely determined by (but of course constrained by) evaluation criteria. There is also an important sense in which experts use criteria as resources to reach conclusions and justify them. The end result, the outcome of the evaluation process, is thus not fully determined by objects and rules of evaluation. There is always some degree of freedom, so that the outcome could have been different. This may be termed *the social determination of valuation*, in parallel to the (partly) social determination of scientific facts or closure of controversies. This social determination may take place either at the level of individual variation, or on the level of collective, institutionalised practice.

First, there is individual variation in how evaluative resources are used. Evaluators may have personal idiosyncrasies, belong to different generations or be trained in different settings or fields of study, and may reason differently within the frames of the discipline. They may employ different evaluative and argumentative strategies. They may weigh the evaluation criteria differently or stress some aspects more strongly than the next expert. This is an inevitable part of peer review processes, and the potential effects of individual idiosyncrasies or biases are counterbalanced by the use of multiple experts in review processes. We can find many examples of this in the evaluation reports, ranging from subtle differences in how applicants' work is described and discussed, to diverging opinions on the ranking of top candidates.

In the typical evaluation report, the three experts each produce a very brief individual ranking of applicants (1–2 pages) after a lengthier joint descriptive evaluation report. In this typical case, the experts also agree on one top candidate, while they may sometimes provide slightly different rankings of the other candidates. Here, it is often evident that although final rankings are delivered individually, there has been a considerable degree of deliberation among the experts. Sometimes, the experts use verbatim wording in parts of the individual documents.[68] It often appears that the evaluators have deliberated to find consensus on the top candidate, but let their opinions on, say, candidates 3 and 4 differ, since they will likely not be practically relevant in any case. However, even if experts do agree on a top candidate, their justifications for that ranking may differ depending on the criteria they emphasise.

The experts are also often explicitly reflexive about the difficulty and contingency involved in the task. For example, one expert reflects on his final ranking: "From a pure comparison of breadth, independence and statistical rigor I believe that Per Johansson ranks slightly above Carling. I have therefore decided to rank Per Johansson above Kenneth Carling. However, if policy motivation and age were to be given a strong weight this ranking would easily reverse" (SKU UU 2001). In another report, this time from 1994, another expert summarises a number of reasons for his choice of top candidate: "Gottfries' interests are wide-ranging, his innovative scientific contributions are both in theory and in empirical economics, he has strong interests in policy, and he has an impressive track record for publishing in first-rate international journals" (SKU UU 1994). Note that this expert lists "impressive track record for publishing in first-rate international journals" *last* in a list of four reasons, while the candidate's publication record is not even mentioned by the two other experts. The three experts do discuss their ranking in a way that reflects that they have been discussing it among themselves (which they also explicitly claim they have). They agree on the ranking of all shortlisted applicants, where the next three are placed equally in the second tier. One of the experts explains that "how one ranks these three candidates depends therefore on one's tastes for theoretical vs. empirical research, and past accomplishments vs. future potential". This is a good illustration of the reflexivity of evaluators and their awareness of a necessarily arbitrary element in the task.

While such small differences within the framework of consensus on a top candidate will normally not have direct effects on final hiring decisions, there are also cases where experts disagree more fundamentally and propose different top candidates. In cases like that, one can see even more clearly how experts use

[68] The evaluating experts have, since 1916, been required by subsequent laws and regulations to deliver their evaluation reports individually. Since 1934, they were allowed to deliberate about their evaluations. However, as Rangnar Nilsson (2009:95–97) describes in detail, by the late 1980s a practice had been established in some fields for experts to produce fully or partly joint reports, even if this was not formally sanctioned. This was found by the samples taken by the Government report *Professorstillsättningsutredningen* in 1989. This report recommended joint evaluation reports to be formally permitted, since they were already an established practice. However, this recommendation was not implemented by in the 1993 Higher Education Ordinance, *Högskoleförordningen*.

argumentative strategies and employ various criteria or emphasises different aspects of an applicant's work in order to produce maximal differentiation.

5. Enter journal rankings: The transformation of institutionalised evaluation practices

Apart from the idiosyncrasies and different strategies employed by individual experts, there is a second form of social determination of valuation which is arguably more interesting from a sociological point of view. This is when widespread established practices of evaluation change in such a way that the variation is no longer found only on the level of individual experts, but on the level of collective, institutionalised practice. So far, I have not discussed the aspect of historical change in the material apart from in some details. The main reason is that the work reported on, and the way experts discuss and value it, seems to be relatively stable. However, there is one aspect of the evaluation reports where there is a marked change over time. The studied 25-year period bears witness to a radical institutional transformation in the evaluation practices found in the evaluation reports. This change involves not only *what* sort of material is evaluated, but also *how* it is evaluated and presented by experts. In essence, this transformation entails a shift from the in-depth reading (how) of a wide range of published work in Swedish as well as English (what), to the extensive reliance on quantified measures of English language international journal publications (what) classified in terms of top journal rankings (how).

As discussed in chapter 3, Hammarfeldt and Rushfort (2017:173–74) have recently argued that the now-widespread use of various bibliometric indicators like the h-index, JIF and top journal rankings can fruitfully be understood as judgement devices, tools to aid evaluators in the valuation of unique products. However, their use is field-specific. Comparing biomedicine and economics, they "found that reviewers in biomedicine tended to use JIF scores and the h-index, while journal rankings—which were not used at all in biomedicine—form a tradition in economics" (Hammarfelt and Rushforth 2017:5–6). They claim that this prevalence of the use of top journal rankings is connected to the hierarchical idea of a disciplinary elite in economics (Fourcade et al. 2015; Maesse 2017) and the related notion of all-important top journals and a relatively narrow disciplinary core, and conclude that "The extensive use of journal rankings also suggests that the most relevant literature in economics is found in a distinctive and rather small set of key journals" (Hammarfelt and Rushforth 2017:174).

While they argue that the use of journal rankings is both particular to, and widespread in economics, their study only covers the period 2005–2014. However, with the material I have analysed here, it becomes evident that this was not always the case. In fact, it became an established practice in Swedish economics precisely around that time, whereas it has been widespread for longer in US and UK economics (see Lee 2009 ch 4, 8; Lee et al. 2013). I will now show how the institutionalised practices of relying on top journal rankings as a judgement device in Swedish evaluation reports gradually emerged around the turn of the millennium.

The concept of top journals as a judgement device

The expert evaluators have referred to publications in leading international journals as an important merit during the whole period studied here. But in the early part of the period, it is one among many factors used by evaluators to categorise and justify their resulting rankings of candidates for professorship positions. In the later part of the period studied here, top journal rankings has become an established tool used by evaluators—a full-blown judgement device. It is taken for granted and generally accepted as a tool for both sorting and justifying purposes. Consider the wording used by the experts in an evaluation report from 2012. When discussing one of the applicants, Karl Wärneryd, they explain:

> Overall, he has twenty nine articles published in refereed journals. While he does not have a publication in one of the top 5 economics journals, he has a publication in the *Journal of Economic Theory* (a top economic theory journal) and one in the *RAND journal of Economics*, one of the top 10 journals. These two articles are his most highly cited articles. The first, "Cheap Talk, Coordination and Evolutionary Stability" (120 citations according to Google Scholar) uses concepts from evolutionary game theory in an environment in which cheap talk is possible. (SKU UU 2012)

We see here how the classification of journals into tiers (top-5, top-10) is used in conjunction with another judgement device, Google Scholar citations. But it is the use of top journal rankings which is most prevalent, and of central interest to us here. In the same manner, we learn about another applicant in the same report, Per Holmberg, that:

> While he does not have a publication in one of the top 5 economics journals, he does have a forthcoming article in the *Journal of Economic Theory* which is viewed

> by many economists as close to the level of the top 5 journals, and he has a recent publication in one of the top 10 journals. (SKU UU 2012)

In this report, about one page of text is devoted to the evaluation each applicant. The bulk of the text presenting the authors consists of this type of statements. What we see here is the curious use of the notion of "top-5" and "top-10" journals, together with Google Scholar citation counts, while the descriptive discussion of applicants' work is extremely short and covers only a few top papers, apart from brief general description of the applicants' field of research. A quantitative measurement of the number of papers published in "top journals", and number of citations, has entered the evaluation. Note here, that the report doesn't refer to the specific JIF, but works with the symbolic categories of "top-5" and "top-10". This is emphasised by the way that the expert in the second excerpt seeks to persuade the reader that the journal is "viewed by many economists as close to the level of the top 5 journals".

These metrics are not only descriptive, but function as guides for judgement: judgement devices. The way they do this is twofold: first, they provide a principle of classification, that is, they help perform the evaluation practice; and second, they function as an argumentative device to justify the evaluation. Thus, one of the three experts in the abovementioned report argues in her final individual ranking as follows—and this quote comprises about half of the very short document:

> To my mind, Petterson-Lidbom and Fredriksson have stronger research records. They have more publications in the very top international journals. Petterson-Lidbom is a bit more impressive as he has a sole-authored publication in the *Journal of Political Economy* and a recently accepted publication in *Econometrica*. All of his papers are in highly regarded international journals. Fredriksson has a longer publication list, but not all of these are in quite as good journals, but several are very well cited. I have put him second because his most highly cited papers were joint with senior colleagues. I regard these two as the top candidates. (SKU UU 2012)

In this report, this particular US-based expert employs top journal rankings and citation counts as a judgement device used to classify and justify, whereas her two colleagues do not. It show that while this judgement device has gradually become more prevalent, there is still a considerable variation in how evaluators work.

One of the other experts instead ranks Fredriksson first in his final individual ranking, arguing in slightly broader terms, without explicitly relying on these metrics, that "the research output holds a high standard and addresses policy relevant issues. In terms of breadth he covers a relatively wide area of interest and his work includes theory, empirical work and policy analysis. Moreover he has extensive experience with research direction" (SKU UU 2012). This is a nice example of how two experts in the same review use very different evaluation strategies, and how these lead to two different outcomes. In this second case, the breadth of work, together with experience in research direction, is instead invoked as an argumentative resource.

Further examples can elaborate on this use of journal rankings as a judgement device. In a 2009 report is an example of how the experts, in the joint report, use the notion of journal rank as a coarse filtering mechanism for classifying some applicants as ineligible for the position. Some applicants receive extremely little attention. The conclusion on one applicant, after a mere six lines of text, is that "Although he has published extensively, almost all his publications are in less prestigious journals", or on another applicant: "He has published 25 articles in international journals during a 10-year period. Quantity-wise he is obviously very productive, but at the expense of quality, and so far he lacks publications in first- or second-tier journals" (SKU SU 2009). Twenty-five international articles does sound quite productive. But this shows us that publication is not just a question of quantity, but of quality, where quality becomes synonymous with the rank of the journal one publishes in.

This is elegantly illustrated in the same review, where the expert discuss one of the top applicants, Åsa Rosén:

> She has eight published articles. One of these is in the *Review of Economic Studies (2004),* generally regarded by economists to be one of the top 5 economics journals. [. . .] It is notable that that most of her publications are in well-regarded international journals indicating their high quality. (SKU SU 2009)

Notice how the experts explain to the reader that the journal is "generally regarded" to be a top-5 journal by fellow economists. Just like the earlier example, the experts rely on the existence of a generally agreed notion of journal hierarchy, which is not necessarily anchored in further metrics such as JIF. The hierarchy of economics journals is a symbolic hierarchy of prestige that can also be used as a judgement device. If this symbolic hierarchy can also be strengthened by objective bibliometrics, so much the better. But the origin, importance and meaning of this hierarchy should rather be understood in terms of the symbolic hierarchy of prestige of modern economics.

The above example further highlights the importance of quality versus quantity when it comes to publications. In economics, publish or perish is in no way a game of pure quantity. Rather, one seems to look bad if one publishes too large a share of papers in "lower tier" journals. Experts are often quick to comment on overlap between papers as an indicator of lack of originality and hunting for quantity. For example, in another report from the same year, we learn of one applicant that "He has published internationally in well-known journals, but there are no top publications, and only few in top field journals. It should also be noted that there is considerable overlap between the submitted papers" (SKU LU 2009). These examples illustrate that in the evaluation reports, experts are on their guard against quantity without quality. They are interested in international "well-known journals", but even this is not really enough: it is only papers in the top-ranked journals that finally count as the currency of prestige.

There is also a generally used and acknowledged distinction between two types of top journals: field and general. In a report from 2006, the experts evaluate the publication profile of one scholar, concluding: "The papers are published in international journals, but there are no publications in either top journals or top field journals". However, another applicant

> does not have a very long list of publications in international journals, [but] his publications are in reputable international journals within the field. He also has publications in some good general interest journals [. . .] showing that his work has made an impact on the broader community of economists. (SKU LU 2006)

This widely-used distinction between top field and general journals parallels the notion of general economics discussed above, the idea that although one has depth in specialisation, there is a central core of the discipline, a general economics discussion that is shared by the "broad community" of the profession.

We can now see more clearly how this idea of the disciplinary core is anchored in an international set of journals. This means that the reproduction of the disciplinary style of reasoning also gets its stability from the institutional links to a set of journals, and the symbolic ordering of the space of journals in a hierarchical fashion. For when experts talked vaguely about "good" or "important" international journals in the 1990s, this categorisation of some journals was not hierarchical in the same pronounced manner. We can think of that earlier situation in terms of a *heterarchy*, that is, as a symbolic space that has multiple potential dimensions of valuation (Lamont 2012b). In a heterarchy, it is potentially possible for an evaluator to point to at least somewhat different sets of reputable journals as examples of "good journals". By contrast, when the notion of top journals is established, the multidimensional heterarchy collapses into a unidimensional hierarchy of rank. All journals have a fixed position along a continuum, ranging from the narrow space of the most valued journals at the top, and down into the infinite space of zero-ranked journals at the bottom. When this symbolic ordering of scientific publication outlets resonates with the disciplinary style of reasoning and the idea of an elite and core of the profession, an important institutional stabilising mechanism is added that further solidifies the disciplinary style of reasoning.

A fully developed practice

The use of journal rankings as a judgement device has become more widespread and plays a very different role in the latter years of the studied period. Let me provide two examples of this practice in full bloom. First, here is one expert reflecting on the requirements of becoming a professor in the introduction to his individual ranking in a 2009 report:

> In evaluating whether the applicants are qualified, I interpret the requirement for professorship to be a good international research profile documented in the form of a strong publication record in good international journals - as it is normally applied at major economics departments in the Nordic countries. [. . .] As good international economics journals I consider, roughly speaking, the top 50 economics journals ranked by the *Article Influence Score* in *Journal Citations Reports*. This includes top general economics journals and top field journals. This does not mean that other publications should not count, but that to be qualified for a professorship in economics at a major Nordic university, one should normally have some publications in these 50 journals. (SKU LU 2009)

The argument moves quickly from "good international research profile" via "strong publication record" to "top 50 economics journals", so that the presence of papers in "top-50 journals" becomes the key qualification for becoming a full professor. The top journal ranking list has become an effective and comfortable judgement device. In this case, the expert is also disappointed with the applicants, and concludes:

> To sum up, while they differ in their research orientation, the candidates share a very modest publication record in upper tier international economics journals and thus none of them can be said to have a strong international research profile. On this basis, and based on my own reading of their work, I do not find any of the applicants to be qualified for the professorship in economics. (SKU LU 2009)

Note how the journal ranking judgement device provides the primary argument for the conclusion that none of the applicants is deemed competent. Only second, after the fact of lacking top publications, does he add "my own reading" as a reason for this judgement.

The second example illustrates in almost ideal typical form the full-blown use of judgement devices in a report from 2006 (SKU GU 2005). The joint report starts out with the presentation of the experts' evaluative strategy: "In the selection of a set of top candidates we will apply a two-step strategy. First we will check publication records. Given the large number of applicants, to be selected to the final core group we require publications in the most prestigious journals" (SKU GU 2005). The reviewers then create two tables. The first of these (reproduced as table 2 below) is based on the most influential economics journal ranking at the time (Kalaitzidakis et al. 2003). Each applicant is presented with the number of papers published in the different ranking tiers, from the very top journals to those ranked 100–150. Finally, a column is added for all "other journals". This includes economics journals ranked below 150. But what is of fundamental importance here is how strictly *disciplinary* this ranking is. Even very influential journals outside of economics, like top-ranked journals in political science, sociology or for that matter general science journals like *Nature* or *Science*, also end up in this column. This means then that what is of interest and valued here are only the *top economics journals*. All other published articles are next to worthless, even if they are published in prestigious non-economics journals.

The second table in the report (not reproduced here) presents alternative ranking intervals, which also include a point system, whereby a top-5 paper gets 1 point and a top-60 paper 1/2 point. Papers in *economics* journals ranked 60–266 are given either 1/3 or 1/6 point, and all papers in all other journals are given 1/12 point. Finally, a third table is presented, now with productivity metrics. These are calculated based publication points from all papers down to journals ranked 266, divided by years from PhD. This measure is then supplemented with a second measure of "quality-adjusted productivity", where only points from papers in top-60 journals are used to calculate the productivity score. The results from these metric exercises are then extensively used in the evaluation report, both to filter out exclude a weak group that only receive brief consideration, and in arguing about the scientific achievements of the other applicants, as will be discussed further in one of the case studies below.

Table 2. An example of he use of journal rankings in evaluation
Reprinted from Table 1, SKU GU 2005.

Table 1. Number and weight of publications in international journals according to the ranking in Kalaitzidakis et al. (2003)[3]. OJ indicates all other journal publications including publications in non-economic journals. Numbers in parentheses represents resubmitted papers or papers under revision for resubmission.

		Journal ranking according to Kalaitzidakis et al. (2003)												
Nr	Name	1-10	11-20	21-30	31-40	41-50	51-60	61-70	71-80	81-90	91-100	101-150	1-150	OJ
1	Khalil				2	1	1	3	2	3	1	11	24	53
2	Izhar Ahmad												0	3
3	Kanth[c]												0	0
4	Sharif						2		1	1			4	15
5	Hvide		1	2	2	1							6	3
6	Heshmati		1	2	1	1		1	2			7	15	32(+2)
7	Sutter		3(+1)	4	2	3		3				2	17	20(+3)
8	Lux		1	2	3							2	8	23
9	Wenzelburger		1	1									2	15
10	Muchlinski												0	2
11	Rosén	1	2(+1)	2		1							6	3
12	Wärneryd	1	6	4	3	1		2					17	5
13	Bergman			1		2	1			2			6	13
14	Fredriksson	1	1(+3)	2			2		(+1)				6	4(+1)
15	Lundborg		3	8	2	4	2				1		20	4
16	Lindh		1	1	2			3			1	3	11	12
17	Dahlberg			1(+1)		2		1	(+1)			3	7	2(+1)
18	Curti												0	0
19	Hansson		1	3		1			1				6	3

Throughout this report, the importance of publications in top journals is central, as the experts explain in the introduction: "In our selection of a group of top candidates for final ranking, we have put a considerably larger weight on research published in first- or second-tier journals than in other journals" (SKU GU 2005). When reviewing the candidates that were not filtered out and excluded in the weak group, they provide the following type of information for each one: "He has published 19 articles (14 articles in journals included in EconLit) [Indicating that they are *economics* journals]; three of them in top-50 journals, but none in top-20 journals" (SKU GU 2005). In the final verdict on this particular unfortunate applicant, they explain in a sentence why he is not included in the final group:

"The main reason for this is his relatively low quality adjusted output and productivity". This evaluation report provides a clear illustration of the use of top journal rankings as a judgement device. Here, the use of economics journal rankings functions as the primary tool of both classification and justification. Other reasons are also added, but they appear more as supplementary to this main mode of evaluation, when top-ranked journal publications have become the *sine qua non* of an economics professorship. The contrast becomes striking when compared to the evaluation practice in the earlier period.

6. Evaluations before rankings: Reading and professional judgement

I began this discussion with a description of the situation today, when the use of top journal rankings have become an established practice. I now turn back in time a decade or two, to look at the evaluation practice before the advent of the new judgement device. It should be noted again that I am constructing ideal types here. While the use of the new judgement device is widespread today, it is not universally used. Similarly, there are early examples of the use of top journal rankings. Reality is, as always, messy. But, during roughly the first five years (1989–1994) of the period under study, there is nothing like the later use of top journal rankings. Experts may mention "good journals" in passing, but this does not play a central role either as a way of producing classifications, or in justifying them. It is at most used as one of many secondary justifications provided for an evaluation. This is followed by a long transition period where one can trace the increasing use and eventual full development of the new judgement device. Let me first illustrate some aspects of the evaluation practice of the early period, and then describe the transition.

Reading books: The breadth of evaluated material

The transformation of evaluation practices includes both the *how* and the *what* of evaluation. It involves *how* evaluations are carried out, and *what* types of objects are considered by evaluators. In the later period, when it comes to an actual qualitative evaluation based on reading of supplied material, evaluators only focus on international English-language journal articles. Sometimes applicants supply other types of material (like textbooks or popular science, and many other pieces written in Swedish), but this is often disregarded. The joint reports normally devote one or two pages to describing an applicant, sometimes, especially with weaker candidates (predetermined with the aid of the judgement device), much less. In the earlier period by contrast, we find the experts devoting themselves to reading a wide range of the supplied materials. The evaluated materials range from journal articles to books, government reports, textbooks and popular science texts, written in English, but also often in Swedish. These "other" forms of text output are not as important, but may nevertheless be considered as merits. For example, we learn from a 1989 report that: "Hansson's highly important and genuine work as author of reports and as a pedagogical and popular science writer should also be given a considerable merit value" (SKU SU 1989; my translation).

The type of evaluated material is intimately connected to the question of language, which is in turn closely connected to the internationalisation of peer review. When reviewers were drawn from Nordic language speakers, language was not an issue. Evaluation reports were often written in Norwegian or Danish, if not Swedish. As experts increasingly come to be recruited from the outside world, these international experts are only able to evaluate material written in English. In a historical perspective, this shift from writing and evaluation in Swedish to English becoming the only valid scientific language is quite marked and is more or less fully accomplished during the 25-year period, as discussed in chapter 5. Note that the example from the 1989 report cited above is a quote from the Swedish reviewer, whereas the two other experts were English speakers who state explicitly that they will only evaluate "scientific" works. Thus even in this one review, we can see how the internationalisation of the review process limits language access, which directly affects the evaluation practice in terms of *what* type of material the evaluators consider. This process of internationalisation also means that Swedish economists have increasingly become oriented towards the international discipline. Even if this process was already well underway at the start of the period, if there was any traces left of a national tradition and discourse in economics, it was gone by the end of the period, when the scientific production of Swedish economists had become fully integrated in the international research field, with English as the standard working language, and the international journal article the unit of scientific communication.

The amount of text devoted to each applicant is also larger in the early period. For example, in the abovementioned 1989 report, more than twenty pages are devoted to a single applicant (SKU SU 1989), even if a review of around five pages is perhaps more typical in a report for a general economics professorship at the time. In general, reviews are lengthier early on, and become shorter and more condensed over time, although there is considerable individual variation depending on type of position, number of applicants, and reviewers. It is also evident in the early period that the experts have actually read a lot of the material in depth, and take care to report on it. For example, there may be a thorough chapter-by-chapter review of the doctoral dissertation, and we find reviews of textbooks with judgement on their contents.

As an example marked by its time, one reviewer from this early period explains the principles behind his ranking: “A ranking of the applicants on the basis of simplified criteria like number of monographs and published journal articles does not lead to a clear conclusion, and therefore a real evaluation of the applicants must be done” (SKU SU 1989). Three things are worth noting here: first, that this senior expert can imagine a quantified measure that even includes *monographs*, and second that when discussing articles, he only mentions “published journal articles”, without any mention of the ranking of the journals they are published in. Third, the mention of “real evaluation” is implicitly understood as the use of qualitative expert judgement, in contrast to the use of judgement devices (“simplified criteria”). This passage continues with his conclusion that “it is therefore natural to put weight on the heavier contributions”, rather than on total volume produced. “Heavy contributions” are here understood primarily as international publications in “good journals”, but we are not provided with further specification. It is implied that an expert is obviously competent to judge what heavy contributions are, without relying on objective external metrics.

In these early reports, the experts appear to have read applicants’ work thoroughly and discuss it qualitatively, and they are aided by their professional judgement, grounded in their scientific habitus. Both collective and personal, this judgement functions as a representation of the discipline, although with a subjective and individual element to it. This expert judgement is of course also active in the later period, although the increasing reliance on the judgement device of top journal rankings (and other similar metrics) relieves the expert from some of his or her burden. There is an important sense in which the potential problem of subjectivity or ambiguity at the level of the individual expert is solved by the reliance of a seemingly objective judgement device, relying on external quantitative data. In any case, the reports of the early period are to a larger extent based on the authoritative qualitative reasoning of the experts, compared to the increasing reliance on apparently objective metrics in the later period.

The role of publication outlets before top journal rankings

The claim that the early period did not rely on the judgement device does not mean that there was no awareness of the merit of publishing in the most respected international journals. However, when studying the older reports with the present-day use of the judgement device in mind, its *absence* becomes acutely present. We have already seen many recent examples of how top journal rankings are used, and how experts are eager to explain to the reader how good a top publication really is. In contrast, consider a few examples from the early period. In a 1989 report we find a very competitive set of applicants to a professor chair at Lund. While the experts are very aware of the high international quality of some of the submitted work, there is no discussion of top journal rankings, nor of "top journals". The experts may for example explain that "his works are of high quality, and they are published in good international journals", or that he "is a rather theoretical economist, that has published works of high quality in leading international journals" (SKU LU 1989; my translation). This is as close to any notion of "top-5" as we get here. But what this reviewer is talking about is actually an economist with *two* papers in *Econometrica*, a journal with definite top 5-status, and something that would be heavily emphasised by a contemporary reviewer. And if this reviewer seems relaxed about the *Econometrica* papers, the other two reviewers don't even mention the applicants' publication outlets in their final rankings.

This is how one of the other reviewers justifies the eventual top ranking of this applicant: "All in all, Svensson appears to be an applicant with great scientific depth, reasonable scientific breadth, and an ability to produce independent contributions of international class" (SKU LU 1989; my translation). Again, no mention is made of the status of publication outlets in order to justify the applicant's position as the most competent candidate. At the same time, this expert allocates attention to explaining that one of the candidates *writes better* than the others. Clearly, journal rankings have not yet become a judgement device at this point in time. Although good international journal publications are respected and read, evaluators rank and justify their rankings based on a broad argumentative conception of scientific quality, founded on the authority of the personal professional judgement of senior experts. We find the same pattern in most reports from the early period. Reviewers may mention publications in "good journals", but this doesn't function as a basis for judgement above others.

During the long transition period, we begin to see more mentions of the idea of top journals, but the extent to which this affects the actual judgement is seldom very large. We may see, for example, that "His publications are reasonably often to be found in international journals, although mostly in not particularly strong ones" (SKU GU 1995). Here, we find the embryonic stage of what will become a new judgement device. In this case, "particularly strong ones" is not specified or quantified, nor is this fact invoked in the actual argumentation about ranking of candidates. The first case where we find something that looks like the modern judgement device is in a review for a chair in macroeconomics written in 1995 (SKU GU 1993), where reviewers explicitly talk of a "minimum requirement" for a professorship as "a substantial number of publications in internationally recognized journals" (SKU GU 1993). While this criterion is formulated in terms of "recognised" journals, one of the reviewers also explicitly talks about "number of papers in top journals" as an argument in the final ranking. However, this expert nevertheless concludes that this number cannot be an arbiter in the ranking of candidates. Instead, he points in a very different direction and explains that what finally settles the ranking of applicants in this case is a single excellent paper: "This paper, although hardly relevant for a chair in macroeconomics, is a pearl, which is unparalleled in the material topical for this position. In other words, he has documented a better scientific potential than Ohlsson" (SKU GU 1993). Despite explicitly mentioning top journal publications, this expert instead relies on his judgement, almost in aesthetic terms, with a single paper dubbed "a pearl". Examples like this abound in the transition period, where something that looks like the later judgement device is used, but without really playing the same decisive role.

In the 2000s, the use of the judgement device becomes more established and common, often also coupled with Google Scholar citations in the lattermost years, especially after Google released an update of Scholar in 2012 which allowed users to view author-level metrics. There is also a clear indication in this development of an inflation in international scholarly publishing. To cite one example, in a rather special case of a professorship in economics focused on women studies (*kvinnoforskning*) in 1991, the experts struggled with the three applicants, all of whom were considered almost, but not clearly, competent. One of these applicants had only produced *two* international journal articles, and this was not in any way a question of "top journals", but international journals of any kind (SKU LU 1991). By contrast, in the later period, in the ideal typical case referred to with table 2 above, where some applicants with well over fifty international journal articles were not even shortlisted. Such increased pressure for international publication and competition for positions is most probably one of the important drivers of the top journal ranking judgement device.

7. Evaluation practices, disciplinary boundaries, and the case of Hibbs

The evaluation of scientific quality is a practice that is underdetermined by the object of evaluation and evaluation criteria, so that there is always some degree of social determination of valuation outcomes. I have illustrated how this may play out as individual differences (idiosyncrasies or bias) among evaluators. More importantly, I have also shown how this has taken the form of a transformation of institutionalised evaluation practices, and argued that the way professorship candidates are evaluated have shifted quite markedly during the studied 25-year period particularly with the use of the judgement device of top journal rankings to produce and justify evaluations. The gradual establishment of this new practice has meant not only that a new method of evaluation has been introduced, but also that the object of evaluation has narrowed to international English language journal articles. This development has been closely connected to the internationalisation of the evaluation process, but also to an increasing orientation among economics researchers towards competitive international journal publishing. While Hammarfelt and Rushforth (2017) introduced the notion of judgement devices in studies of peer review and argued that the use of journal rankings seems to be unique to economics, I have added an account of how this institutionalised practice gradually emerged.

In the analysed reports, the notion of top journals seems to merge with an understanding of something like a disciplinary core and an area of "general economics", a central and common scientific approach and area in which every economist may interact and compete for attention. An important aspect here is also the strong rejection of quantity without quality, the idea that it is somewhat suspect to publish a lot without a good share of papers in top journals. This indicates also how the notion of top journals points to the obligatory area of "general economics", and that *not* directing a good share of articles towards the top of the journal hierarchy can be interpreted as a failure to recognise the collective symbolic acknowledgement of the disciplinary core and its established disciplinary style of reasoning.

This points to one of the bigger consequences of the institutional transformation of the review process, namely that a fair share of judgement power is transferred from expert reviewers to journal editors and reviewers. For if the primary currency of scientific quality has become top journal publications, and high-stakes hiring decisions increasingly rely on evaluations based on this judgement device, the primary evaluation done at the top journals becomes increasingly important, both in terms of determining individual careers, and in terms of its boundary effects. The experts' role in continually upholding the disciplinary boundaries of proper economics (boundary work) is partially handed over to these. If one cannot publish in the top journals, one is not a competitive economist. The consequences for the disciplinary boundaries seems to be that an institutional stabilising force has been added.

Cognitive particularism and the case of Hibbs

In peer review processes in a discipline like economics, with a dominant disciplinary style of thought on the one hand, and marginal heterodox groups on the other, there is a significant risk of *cognitive particularism*: that reviewers tend to favour approaches that are similar to their own (dominant) style of reasoning (Travis and Collins 1991). As I have shown, the report material suggests a broad consensus around a view of what excellence means in economics, i.e. of the disciplinary style of reasoning. This includes command of what is considered core theory, combined with proof of technical skills and the application of models based on some aspect of the core theory to empirical data, and the ability to formulate interesting "economic" problems and solve them. Cognitive particularism means that reviewers tend to favour work that adheres to this shared disciplinary style. It functions as a powerful mechanism for the reproduction of disciplinary boundaries.

But if cognitive particularism is grounded in the scientific habitus of reviewers, which is entrenched in the disciplinary style, what does the transformation of evaluation practices and the introduction of the objectivising judgement device of top journal rankings mean? It means that, as noted above, more evaluative work and potential cognitive particularism is handed over to and distributed among editors and reviewers at high-ranking journals. This has the effect that the power of evaluation becomes not only increasingly social, when the relative importance of a few more or less local experts in hiring decisions is distributed over a potentially large number of peer evaluators within the discipline. It also, and importantly, means that the power of judgement is shifted towards the central elite of the international discipline, and thus reinforces its inward-looking orientation, and potentially contributes to the reproduction of the disciplinary style of reasoning. This distribution of judgement thereby functions to strengthen disciplinary boundaries. A side-effect of this shift is a decreasing burden of boundary work for evaluators in hiring decisions, since this service can now increasingly be performed at the level of journal review processes. In the following example, I will highlight a single case that illustrates well the contestation of disciplinary boundaries, and the argumentative labour that resulted.

If reviewers 25 years ago already had a clear idea of what good economics is, it was already very hard, if not impossible, for someone who did not adhere to the disciplinary ideals to compete for a professorship. But there was still a certain degree of flexibility of interpretation. In the evaluative reviews of the period we find that sometimes evaluators disagree on how to draw boundaries. The evaluation of applicants for a professorship in macroeconomics in Gothenburg from 1995 (SKU GU 1993) illustrates this very well.[69] Two Scandinavian reviewers who frequently acted as experts in hiring cases, Torben Andersen and Karl-Gustaf Löfgren, wrote a joint evaluation report with individual rankings.

In this report, they first establish that candidates must fulfil the minimum requirement of a "substantial number of publications in internationally recognised journals on macroeconomic topics". Note the wording used. First, this is not a ranking; second, and of utmost importance, "macroeconomic topics" is an area open to interpretation. Both rank Douglas Hibbs Jr. as the top candidate. Hibbs has a political science doctorate and has already held a chair as professor of government at Harvard University. According to the evaluation, he has been central to the establishment of the international research field of "macroeconomics and politics". He is obviously a very successful scholar. But the question is: is Hibbs an economist? That, is should his oeuvre be included within the boundaries of the economics discipline? Andersen and Löfgren think so, and argue extensively to prove this and to justify the top ranking they have awarded him. For example, we learn that

> His research in this area is collected in the monograph *The American Political Economy: Macroeconomics and Electoral Politics* [. . .] The chapter on the cost of inflation shows that Hibbs, indeed, is an excellent economist and not only an econometrician. All the chapters are not of interest to an economist, but the analytical rigour and the drive to produce relevant scientific results would impress any reader interested in social science. (SKU GU 1993)

Here, we find not only that they explicitly argue in favour of including him within the disciplinary boundaries, they realise that econometrics is in itself not enough, so they also emphasise his "analytical rigour" and scientific drive to show that his style of reasoning is actually of the kind required of an economist.

[69] The call for the professorship is dated 1993, however the evaluation report was not delivered until 1995.

The third reviewer, the US economist Thomas D. Willet, is however of a completely contrary opinion. Willet delivers his own evaluation report and ranking. He produces one of the first evaluations that draw on top economics journals as a judgement device. Andersen and Löfgren have actually also used the term "top journals", but in a rather loose sense. Willet, in his short evaluation statements, mentions good journals directly as an equivalent of scientific quality, without needing to say anything else about the quality of some candidates. But good, or even top, journals are not enough. The real issue at stake becomes clear in his evaluation of Hibbs. He argues at length about why Hibbs is not suitable for the professorship. One of the foundations of that argument is that

> while he has many publications in top political science and political economy journals, his only publication in a regular economics journal was in an invited paper and proceedings issue of the *American Economic Review* (May 1986). In other words, at the time his material was submitted for the chair, he had no regularly referred paper published in a regular economic journal. (SKU GU 1993)

This line of argument is an excellent example of how disciplinary boundaries are contested. While Löfgren and Andersen see Hibbs as an excellent social scientist and political economist of international class who could be harboured within their conception of economics, Willet is much stricter about disciplinary boundaries. For him, "top journals" in themselves are worth nothing. In this act of boundary work, Willet contests the other evaluators' conclusions, and redraws the disciplinary boundaries, denying Hibbs the status of a proper economist. This hinges on Hibbs's lack of publications in "regular economics journals". Here, the argument does not hinge on *top* journals as such, but on the disciplinary affiliation of the journals. Willet holds another applicant, Hutchinson, to be the unquestionable top candidate, and here, the notion of *top economics journals* is really the backbone of his argument:

> He is a superb candidate with an excellent mix of technical economics and applied economic policy pieces. His work is notable both for its prodigious quantity and high quality. He has published numerous papers in top economics journals including *Economic Inquiry*, *Journal of International Economics*, *Journal of International Money and Finance*, *Journal of Money, Credit, and Banking*, *Oxford Economic Papers*, and *Review of Economics and Statistics* as well as contributing to a number of major books. (SKU GU 1993)

In his final ranking, Hutchinson's technical skill and disciplinary belonging are both emphasised again. Willet presents his final verdict in a rhetorical style with an unambiguous and irrefutable conclusion, arguing that Hutchinson

> is the top candidate *by a considerable margin*. He has a distinguished set of publications in *top technical economics journals* as well as many useful policy oriented papers which collectively *cover a broad spectrum* of macroeconomic theory and policy issues. Indeed, he is the only candidate who I think is clearly worthy of a chair in macroeconomics at a major university. *Douglas Hibbs is quite distinguished, but is primarily a political scientist* and, as I indicate, I'm concerned whether he'd be able to offer the advanced graduate macroeconomic analysis that is needed. ((SKU GU 1993; my emphasis)

Here, Willet states that there is "a considerable margin", justifying his evaluation outcome as certain and unambiguous. What is more, he emphasises not only that he has published in top *economics* journals (as opposed to top journals in other fields), but choses to accentuate this by adding "technical": there is something special, something inaccessible to uninitiated outsiders, in these core disciplinary journals.

We find here a prime example of boundary work, where it also has very direct effects on hiring decisions and the reproduction of the thought collective. That is, the negotiation of the symbolic boundaries of an imaginary space of economics is translated almost directly into social boundaries: who gets to be an economics professor?[70] But there is more. The evaluator is concerned about choosing the right professor, on the grounds that he is concerned about proper advanced economics courses, that is, about the reproduction of the thought collective. The case of Hibbs illustrates very well the freedom available to evaluators, the underdetermination of evaluation, and how a final verdict that appears as solid and unambiguous requires a lot of argumentation and justification, which could potentially be performed otherwise, given different premises. It is a good example since Hibbs is such an obviously competent, indeed top-class researcher. That fact is never questioned. What is at stake here are instead the boundaries of the discipline, which the two reviewer sides work hard to pull in different directions.

Let us now use this example as a little deductive model for heuristic purposes. What happens if we could have altered one variable, namely the institutionalised reviewing practice? Assume that the use of top economics journal rankings had been firmly established and used by all evaluators. Would Hibbs's chance of

[70] It should be noted that hiring decisions are taken by the faculty. They are not obliged to follow the rankings recommended by expert evaluations, although they normally do.

receiving a professor chair in macroeconomics have changed? The answer must quite clearly be affirmative. If all evaluators had agreed on the central importance of top economics journal rankings, and used them to make and justify their judgement, Hibbs would not even have been shortlisted; he would have been disqualified in the first coarse filtering of applicants. This thought experiment also highlights what is so special about top economics journal rankings when used as a judgement device, namely its *disciplinary* character. This makes the use of economics journal rankings very different from other bibliometric judgement devices, like citations or JIF. From this example we can extrapolate and conclude that the increasing use and establishment of top journal rankings in economics functions as a powerful mechanism for strengthening and stabilising the disciplinary boundaries. It functions as an institutionalised mechanism of reproduction of a disciplinary thought collective and its specific style of reasoning.

8. Conclusions

The expert evaluation reports provide insight both into how the scientific field is understood by the profession, and into the evaluation practices involved, and their transformation. It confirms the image of the core areas and especially the centrality of modelling and econometrics as the core epistemic strategies in the disciplinary style of reasoning. Both modelling and technical skill in constructing models and econometric analysis are highly valued. Policy relevance is also important, and points to the sense of practical problem-solving and applied orientation that is a part of professional identity, although not necessarily of all economists or work in economics. In the evaluations, there is also a notion of a common core of economics, expressed both in terms of the ability to formulate proper, interesting economic problems, applying the core of general economic theory, and publishing in the generally-acknowledged top journals.

Over the studied 25-year period from 1989 to 2014, the practice of expert evaluation was transformed. If judgement of scientific quality is a practical accomplishment, never fully determined by its object of evaluation, there is always a degree of social determination in quality judgement. This is, in idealised form, either a case of individual variation and outcome, evident in the various conclusions reached by different experts presented with the same applicants to evaluate, or collective and institutionalised. The studied period bears witness to such an institutional change in the way that expert evaluation is performed. It entails primarily the introduction of the hierarchical concept of top economics journal rankings that function both as metrics that can aid in classifying quality differences, and as a justification for the resulting evaluation. The transformation of evaluation from relying only on the professional judgement of experts, to the use of seemingly objective quantitative metrics does not mean that evaluation is no longer socially determined. One institutionalised form of social determination is merely replaced by another. It is even reasonable to argue that economics journal rankings, as the objectification of a large number of distributed judgements, is more deeply social, and represents the objectification of the ideals of the disciplinary thought collective as naturalised fact.

It also results in a shift in the types of materials that are subjected to evaluation: from a wide range of textual production in Swedish or English to a narrowed focus on international journal articles. This transformation is furthermore paralleled by a transformation of the reviewers themselves, and the integration of the national discipline more fully into the international scientific field. And in this transformation process, the central locus of disciplinary boundary work seems to have move to the review processes of the top journals.

Chapter 9. Disciplined reasoning: Concluding discussion

It is now time to bring the findings of the two partial empirical studies into a broader analytical discussion, and to discuss the wider implications of this study. In this concluding chapter, I will first take stock of the overall argument, draw the different chapters together, and point to what I believe are the main contributions of this study. I will then broaden the perspective, discuss some possible parallels in other sciences, and highlight new questions for future research. Finally, I will discuss what I believe are some of the main implications of this study—for the pursuit of pluralism in economics, but also for other social sciences, and sociology.

1. A summary of the argument and contributions

I started this study with two overarching aims. The first aim was empirical, namely to explore, describe, and explain the mainstream approach to science in contemporary Swedish economics, its social stabilisation, and its relation to heterodox economics. The general formulation of the research problem focuses on the social causes of the stability of the scientific approach in mainstream economics, and its relation to other disciplines and heterodoxy. The notion of a clear mainstream-heterodoxy distinction was derived from the literature and already in itself presupposed a rather abstract understanding, first that there is such a thing as an intellectual divide in economics, understood as a broad international research field, and second, that this divide is in some sense historically stable. The subsequent chapters fleshed out more concretely how this problem has been approached by historians of economic thought (chapter 2), and how science in general and economics in particular has been theorised and studied as a social phenomenon (chapter 3).

Drawing on these earlier conceptions and empirical studies, I suggested a theoretical framework (chapter 4) that draws on the literature on styles of reasoning, but adapts it to sociological use. The framework of relational disciplinary styles of reasoning was constructed in order to make analytical sense of the stabilisation and reproduction of mainstream economics, while keeping its relation to heterodox economics and other disciplines firmly in mind. My version adds sociological attention to the actors that actually work within and reproduce the style, through the notion of a scientific habitus, and through the understanding of boundaries as contested outcomes of actors' practices. Furthermore, I pointed to the evaluation of scientific quality as a critical site for studying how conceptions of the disciplinary style and its symbolic boundaries are negotiated and transformed into the social boundaries of the disciplinary thought collective.

In the two empirical studies I then demonstrated how this conceptual framework allows us to make sense of economists' approach to science, and how the relational disciplinary style of reasoning actually appears in reality, in all its messy and ambiguous imperfection. Through both empirical studies, a representation emerges of the disciplinary mainstream thought collective and its style of reasoning. Furthermore, I emphasised the relational aspect of styles, evident in the boundary work involved in the maintenance of disciplinary boundaries, in the rejection or even misrecognition of heterodox economics by the mainstream, and critique of fundamental elements of the mainstream style of reasoning by heterodox economists.

The second overarching aim was theory development, more precisely to develop a theoretical framework for understanding the stabilisation of an intellectual approach in mainstream economics and its relation to heterodoxy in terms of the combination of a set of more general social processes. I did so by first synthesising the literature on this divide, second by re-describing it analytically as a particular case of stable styles of reasoning, drawing on literature from the history and philosophy of science, and third by going beyond this literature and adding to it a sociological and relational layer. The framework grew out of a spiralling engagement with knowledge from previous research, various theoretical approaches, and my empirical material. I will now try to summarise the most important points of my argument and findings.

Mainstream, heterodoxy, and styles of reasoning

In the survey in chapter 2 of debates among historians of economic thought on the nature of mainstream economics and its relation to heterodoxy, I found broad convergence towards three ideas. First, the modern economics mainstream can be described as formed around a small set of axioms or meta-axioms. This is what I call the ontological aspect. This social ontology is conveniently captured by the formulation of the three meta-axioms of methodological individualism, methodological instrumentalism, and methodological equilibration. Second, the authors variously emphasise what I term the epistemological aspect, which is the methodological emphasis or insistence on a certain way of producing knowledge, primarily the formal modelling approach to problems as the central and fundamental approach to knowledge production. The third is what I call the social aspect, the observation that the mainstream ontological and epistemological assumptions are criticised and rejected by a distinct thought collective of heterodox economics, so that we must understand these intellectual divisions in terms of more or less bounded social thought collectives engaged in boundary work. Understanding the cognitive structure of economics thus requires an understanding of its social organisation and relational nature.

Drawing on the styles literature of Crombie and Hacking, I argue that we can re-describe this situation in terms of contested styles of reasoning. This reformulation allows us to understand the situation in economics in terms of a more general phenomenon of how the sciences and the pursuit of knowledge have historically been intellectually structured. Mary S. Morgan has fruitfully shown how knowledge production in modern economics can be understood in terms of the styles approach, with the central modelling style nested with the deductive style and the statistical style, flexibly glued together by a small set of assumptions of social ontology, echoing what I have termed the ontological aspect. However, while the styles approach allows us to perceive these phenomena in more general terms, it lacks, first, a way of accounting for heterodoxies, and second, its largely historical literature lacks a sociological understanding of the role of actors and institutions in the negotiation and reproduction of the larger structures of styles. One of the central theoretical contributions of this thesis is therefore to sociologise the styles approach.

Relational disciplinary styles

I have emphasised the need to understand how semi-timeless Crombian styles of reasoning are embedded in actual institutional settings in modern sciences. I argue, with Abbott, that the modern discipline is normally the primary social structure in science, with a disciplinary thought collective that reproduces itself through formal training, with the disciplinary doctorate as an entry ticket to a disciplinary labour market that ensures circulation and intra-disciplinary exchange. I suggest that we make a conceptual distinction between the transhistorical and transdisciplinary Crombian styles on the one hand, and the specific combination of nested Crombian styles that Morgan describes as being a stabilised core within modern economics (modelling combined with statistics and deduction) in terms of its particular disciplinary style. Disciplines, and the degree and form of disciplinarity, are variable social outcomes, and I argue that we should treat these variables as questions for empirical research.

In the empirical chapters, I showed that the high degree of disciplinarity of modern economics is valid not only in the Anglo-Saxon world, but to the same extent in the Swedish context. Furthermore, both the interview study and the document analysis provide a great amount of detail to our understanding of how disciplinarity plays out, and how economists understand their discipline, actively engage in reproducing it and protecting the boundaries of proper science, and how a sense of a disciplinary core is maintained. This points to the fundamentally relational nature of styles of reasoning.

It is quite clear, as indicated by the literature review in chapter 2, that there is a distinct but asymmetrical divide between mainstream and heterodox economics. Heterodox economics is unified in its critical stance towards the fundamental assumptions of the mainstream, and there are many accounts of how heterodox approaches are neglected, defined as un-scientific, or irrelevant, while heterodox economists experience both neglect and active resistance. This was also evidenced in the interview material, not least in the interviews with heterodox economists. I argue that to understand the establishment of a disciplinary style, we need to bring the styles approach in contact with the sociological literature on boundaries and boundary work, and understand disciplinary styles as fundamentally relational phenomena. As is evident for example in the ways that interviewees talk about their discipline, this is often done in a relational way, contrasting with other disciplines that are explicitly or implicitly understood as scientifically inferior. But there is also a deeper sense in which the establishment of a disciplinary style is relational, because the idea of a core of the discipline means choosing *one way* of perceiving economic problems, and not another. When those *other ways* of perceiving problems and approaching knowledge production are pursued by another social group (discipline or heterodoxy), the maintenance of the disciplinary core is simultaneously maintenance of its boundaries.

However, I have also argued that when economists engage in boundary work, it is not always the *discipline* that takes the role of primary boundary. Using the interview material, I have shown how boundaries are sometimes constructed along Crombian styles, when economists engage or cooperate with non-economists working within the same style. In those situations, styles function as bridges rather than barriers. I think that this is a great benefit of the styles approach. It allows us to think about how incommensurable and self-authenticating styles can both create a gulf of mutual incomprehension and active distancing *within* disciplines, as is the case with heterodox economics, but simultaneously bridge disciplinary boundaries, when a stylistic common ground (for example the use of modelling or statistical analysis) allows for ease of boundary crossing. What may initially seem like a refutation of the boundary work thesis instead becomes an expansion of our understanding of boundary work when the notion styles as barriers or bridges is added. It shows us that boundary work may be complex, and that the unit of boundary work (i.e. disciplinary or Crombian style) should not be taken as a given, but must be subject to empirical investigation. It also directs our attention to a finer-grained understanding of *interdisciplinarity* that focuses on similarity and difference in terms of Crombian styles. It allows us to comprehend why some forms of interdisciplinarity (where Crombian styles are aligned) can be expected to be more frequent and consensual, whereas others (working across Crombian styles) should be expected to be less probable and consensual.

According to what I called the Lee thesis, heterodox economists of different schools of thought have increasingly started to unify since the 1990s and engage in synthesising work, while also increasingly using the term "heterodox" as self-identification. I find partial support for the Lee thesis. While the number of heterodox or semi-heterodox economists is too small to draw strong conclusions, it seems that they are somewhat divided on these matters. While a few clearly are part of this movement, and talk about and find themselves to be part of an international heterodox community, others resist the use of the label, while still having a clear idea of the economics mainstream and its faults. While reality is always messy, however, the mainstream-heterodoxy divide is still reasonable as an analytical concept.

Scientific habitus, agency and the reproduction of the disciplinary style

The concept I have proposed, of styles as relational, and the role of boundary work, points towards the role of actors in maintaining the cognitive structure of the disciplinary style and the social structure of the discipline. I argue that the "commitments and dispositions" that Crombie posits lie at the base of styles of reasoning are fruitfully theorised in terms of a specific scientific habitus, whereby scientific actors come to embody and learn to perceive, reason in the manner of their discipline. The habitus furthermore forms the ground for the semi-intuitive scientific judgements, involved, among other things, in assessing quality in gatekeeping peer review practices. Using the interview material I showed how economists reflect on these almost intuitive epistemic dispositions, and argue that although there is evidently an element of self-selection, rigorous and standardised doctoral programmes play an important role in aligning professional economists' understanding of what it means to reason rightly. This is achieved both in terms of technical and theoretical proficiency through a common set of theory (standard micro- and macroeconomics) and methods (econometrics and mathematics) courses, but it is also sedimented in a shared—but not fully explicated—sense of a disciplinary core and what good science means.

Throughout the material, a strong sense of disciplinary identity is evident. Economists have a clear sense of what it means to be an economist, and that they are technically and theoretically successful and scientifically productive, if not plainly superior, to other disciplines in the social sciences. Furthermore, this sense of disciplinary identity is strongly related to a notion of the international discipline. While Abbott (2001) in his model of the discipline as institutional structure thinks of the discipline as a national (in the United States) self-stabilising phenomenon, it is clear from the material I present here that we are witnessing a national discipline that increasingly is becoming part and parcel of the international discipline of economics. The historical background in chapter 5 gave a good picture of how late this development really occurred, and the expert evaluation reports show us one aspect of this transformation over a 25-year period, where both evaluations and applicants for professorships are increasingly international. A special role is played here by the institutional system of scientific economics journals. Also noteworthy is the way that economists conceive the international standardisation of doctoral programmes and the universal ability of economists to interact across specialisation boundaries. Coupled with the importance of the discipline's top journals, this is linked to the key symbolic notion of a common core of economics. This core is actively promoted and guarded, and consciously reproduced through the active design of doctoral programmes with the intent to provide exactly this universal disciplinary core.

Quality judgement, evaluation practices, and the core of economics

I have argued that the disciplinary style and the maintenance of its boundaries is intimately connected to how scientific quality is understood and evaluated in the discipline. Just as there is not one scientific method or approach, but a set of different and potentially incommensurable styles, according to the styles approach, the same is true for judgement of scientific quality, where Lamont (2009) and others have pointed to the variability in conceptions of quality between disciplines. Drawing on the expert evaluation reports, I showed that reviewers in economics expect modelling, simplification and clarity, but also technical competence, in the most highly-valued work. Furthermore, the reviewers have a conception of a common disciplinary core, a general economics ability to formulate and solve interesting economic problems. The relational nature of the conception of quality is evident from the way that reviewers use contrast as a rhetorical device and disapprove of certain approaches, for example the merely descriptive, or arguing that someone is being merely technical and lacking command of core theory. When reviewers discuss this taken-for-granted general area of economics, or when they claim an analysis to be intuitively compelling, we can observe the work of a disciplinary habitus which forms the semiconscious intuitive sense of what an interesting problem or a sound solution is, and the foundation of the professional judgement of reviewers.

I have furthermore argued that a strong link between, on the one hand a disciplinary style and conceptions of quality, and on the other the reproduction of boundaries, is provided by the mechanism of cognitive particularism (Travis and Collins 1991). Distinct from sources of *social bias* (like gender bias or old boy networks) this is an arguably more interesting form of bias where experts in peer review processes favour work that has cognitive and intellectual stylistic similarities to their own way of thinking. Oftentimes, in disciplines with a strong consensus, this does not present a problem, since there is general agreement on a disciplinary style of reasoning. In situations of acknowledged pluralism, it is more obvious to those involved, and can be handled by careful selection of reviewers. However, in situations with a dominant style of thought and marginal heterodoxies, this creates an exclusionary mechanism that tends to favour the dominant style. I have showed how this is expressed in the analysed evaluation reports through the consensus on a disciplinary conception of quality that aligns with the disciplinary style of reasoning. In the few examples where reviewers encounter more heterodox work, it is evident that the existence of different schools of thought is downplayed in favour of an implicit view of the consensual and cumulative nature of economics. Sometimes, reviewers are even explicit about work being outside of the mainstream as a sufficient argument against it, which amounts to a sort of pure boundary work that doesn't spend time on elaborate justification.

However, if conceptions of quality are variable, so are the socially institutionalised practices of quality evaluation. The evaluation reports showed that during the studied quarter century (1989–2014) a marked transformation has taken place in evaluation practices in Swedish economics. This includes both *what* sort of material and *how* it is evaluated. Constructing two ideal types, I showed how the early part of the period is characterised by the use of qualitative professional judgement of experts, the classical form of peer review. Evaluators read a broad range of materials, often written in Swedish or other Scandinavian languages. In the later period, the evaluated material became exclusively English language articles in international journals, and evaluators had started to use a new judgement device.

The concept of judgement devices in peer review was recently suggested by Hammarfelt and Rushfort (2017). It denotes the use of bibliometric indicators as tools used by reviewers. Their study compares recent expert reports in Swedish biomedicine and economics, and they conclude that in both disciplines indicators (like citations, h-index and rankings) are used as judgement devices, but that top journal rankings are completely absent in biomedicine, while being widely used in economics. With the material analysed here, I am able to show both how this practice has become established gradually and very recently in economics, and how it is connected to the idea of a disciplinary core and to the delineation of disciplinary boundaries. With economics journal rankings as judgement devices used by evaluators both to categorise and to justify, publication in top journals becomes not only a powerful device for sorting, it also becomes synonymous with belonging to the disciplinary core, and with quality, without the need for further legitimation.

If cognitive particularism links disciplinary style and boundaries through the habitus and professional judgement in peer review, the link between disciplinary core and boundaries becomes objectivised and increasingly stable. What is so special about this judgement device it that it draws specifically on rankings of *economics* journals. This means that it encompasses pre-established disciplinary boundaries, as opposed to other bibliometric indicators. This means, first, that the negotiation of disciplinary boundaries that was previously possible, as I showed in a case where an outstanding political scientist was held as the top candidate for a professorship by two reviewers but contested by the third who argued at length that the candidate was not an economist, and therefore should not be considered. With the rise of journal rankings, reviewers increasingly come to rely on publications in top economics journals. This has a dual effect: first, it automatically defines disciplinary boundaries in terms of these top journals, so that the requirement for a professorship becomes a publication profile with a clear distribution towards top disciplinary journals, and second, it redistributes evaluative power to the top journal editors and reviewers in the discipline.

Drawing it all together: Disciplined reasoning in modern economics

Drawing these various aspects together, we can see how economists and their way of thinking is reproduced and stabilised in a highly social context. These aspects form an interacting and self-reinforcing system that seems rather hard to break out of. The disciplinary style of reasoning is the basic approach and assumptions of scientific knowledge production, an entangled set of ontological and epistemological assumptions beyond particular theories. The style is the collective property of a thought collective that explicitly identifies as economists. Heterodoxy means placing oneself outside of this intellectual style and social community, in an oppositional identity, as part of the rejection of some or all of the mainstream's ontological or epistemological assumptions. As the institutional framework of the mainstream economics thought collective, the discipline functions in the dual role of first socializing new members, and second, in regulating the labour market, with the economics PhD as proof of socialization as entry requirement. The intentional international integration of the discipline means that socialization through doctoral programs, circulation of research output and normative alignment with the top international journals, and the labour market for post-doctoral economists, are intensified and more closely aligned.

The discipline is not only an institutional structure, it is also a symbolic space of which economists have a clear idea. This space could be imagined as a circle, with boundaries to be relationally defined and protected against its outside, and a centre where the most important common knowledge, the core of the disciplinary style, is found. However, I think that a three-dimensional figure would in a sense better illustrate this symbolic space: the surface of a cone. To move towards the centre of the cone is then simultaneously to move in the vertical dimension, upwards toward the top of the discipline. In this way we can make sense of how the idea of the core of the discipline is linked to the top journal rankings and the disciplinary elite. The rankings of economics journals with its built in disciplinary filtering is further contributing to the self-stabilising system. As I have argued, the reproduction of the disciplinary style must be understood relationally. Boundary work takes several different forms, for example in the ways in which economists talk and think about their discipline through contrasting with other disciplines and poorer epistemic approach, like being merely descriptive, verbal, or as exemplified by imaginaries of the disorder that results from lack of a clear common language. However, the most efficient form of boundary work is that involved in quality judgement, which has the potential effect of translating the symbolic boundaries of the discipline and its style into the social thought collective in the competition for publications, research grants and positions.

The quality judgement involved in peer review practices is also a site where the professional habitus is activated, when the achieved sense of professional judgement is put to use. The standardized formal training in PhD programs, the alignment towards a tight set of top journals and the general orientation towards the practices of the disciplinary elite, means that the scientific habitus of economists are reinforced and homogenized, so that we can expect similarity in fundamental epistemic commitments and dispositions. As I have argued, the role of the scientific habitus in quality evaluation means that there is an inherent tendency towards cognitive particularism, acting as a strong systemic stabilizing force, which is now also increasingly combined with the automated filtering achieved with top journal rankings as judgement devices. The role of the habitus and the agency involved in boundary work and evaluation practices lets us sociologically understand the place of actors in the reproduction of styles of reasoning. However, at the same time it should be clear that the style cannot be reduced to merely the sum of the dispositions of individuals. The disciplining of reasoning takes place in a context where the intellectual style and the social thought collective, institutionally embedded in the discipline, always precedes the socialization of any individual, and it is therefore a truly structural social phenomenon.

Thinking of the enduring style of the economics discipline and its internal relation to heterodox economics in terms of a self-stabilising system does not mean that it is unchanging or unchangeable. It helps us to conceptualise the quasi-stability or sluggishness of a fundamental way to approach science at a very general level of abstraction that is the disciplinary style of reasoning, while there may be innovation and change at the level of particular theories. That is, it helps us to conceive of change within stability as a matter of levels of abstraction. But as I have shown, there is also an ongoing slow transformation in the constellation of Crombian styles that constitute the disciplinary style of economics, in the form of the increasing role of empirical statistics, and more recently of experimentation. However, the core of the disciplinary style seem to adapts to these changes only very gradually, despite experimental economics being often used as an example of radical innovation. From the perspective of the disciplinary style, there is hardly any scientific revolution in sight.

However, it is not that economics and economists are scientifically successful and productive *despite* of this, but rather *because* of it. The disciplinary organisation and style of reasoning has become an efficient and appreciated way of working and producing knowledge by the relevant internal scientific standards. While my overall argument is one of the self-reproducing nature of the disciplinary style, there are also fundamental external conditions of this stability, like the high status and demand for knowledge, and the material or economic preconditions for research. Through the high demand for and status of economists, the style of reasoning is also apparently externally efficient and successful. However, with these external conditions in place, the systemic self-stabilising forces of the discipline should not be underestimated. Taken together, this vision of the discipline as a self-stabilising socio-cognitive system means that the conditions of possibility of economics analysis is strongly structured and regulated: it explains how economic reasoning is disciplined.

2. On styles, boundaries and classification situations

I have moved between the details of the empirical material, the larger social structure of the discipline, the intellectual structure of disciplinary style, and the role of actors involved in boundary work and quality evaluation. Let me now attempt to draw the larger picture and tie these parts together. I argue that the use of top economics journal rankings as a judgement device provides a window through which we can more clearly see how orientation toward the disciplinary core is related to boundary work. The appreciation of quality and interest in the elite may seemingly be very different from the maintenance of disciplinary boundaries and the establishment of what lies outside the boundary of proper science. But, as I have argued in the relational conception of styles, they are intimately linked. However, thinking of journal rankings in terms of what Fourcade and Healy (2013, 2017) have called a "classification situation" will add another dimension to this understanding.

Top journal rankings as classification situation

We can now start to appreciate how the disciplinary style of reasoning, the professional thought collective and its orientation towards the top and core of the discipline, and the maintenance of its symbolic and social boundaries, are linked and stabilised by the institution of top journal rankings. As I show in the analysis of the reports, these rankings are tightly linked to the notion of a unidimensional disciplinary hierarchy where the concepts of top authors, papers and journals, and idea of the disciplinary core of economics feed into each other. The system of journal rankings forms a system of metrics with outstanding objectivity. The ordinal rankings of journals orders the universe of economics research output in a hierarchy from the very top to the infinity of indifference. While these rankings are often understood and talked about primarily as an ordinal ranking by economists, publications of well-known journal rankings are reinforced by cardinal scores like the journal impact factor (JIF).

As Fourcade and Healy (2017:289) remind us, rankings and quantitative scores build upon categorical classification, "classifications on top of other classifications", and rankings and scores in turn tend to form the basis of new nominal categories involved in the judgements that shape social life. In the analysis of the evaluation reports, I have shown how rankings are turned into loose symbolic categories of "top journals" or the "core of economics". The disciplinary style of reasoning as a set of epistemic commitments and dispositions is now reinforced and stabilised when it becomes synonymous with the researchers' work being published in a recognisable set of top journals. The impact factors that construct the rankings show, objectively, that these are indeed the best-cited journals and most influential papers. When academic hiring and promotion—the reproduction of the thought collective—increasingly relies on publications in these top journals, the symbolic boundaries are effectively transformed into the social boundaries of the discipline.

It is fruitful to think of the place of journal rankings in terms of a *classification situation* (Fourcade and Healy 2013), where a system that produces objective rankings or scores becomes intrinsic to the ordering of social life, with very real effects for the individuals who are affected and have to learn to live in relation to the ranking. Fourcade and Healy draw on Weber's notion of the *class situation* as the common market situation and life chances that some group finds itself in, notably its position in the occupational structure, which is central to Weberian theories of social class. However, they argue that "[w]hat is missing from this view is the notion that allocation to particular market-situations might depend on some formal, institutionalized classification procedures", and that the algorithmic credit score calculations in the United States today not only have the effect of sorting and slotting people into varying economic opportunities, but also that the scores become reactive when self-monitoring subjects strategise about them (Fourcade and Healy 2013:561). While Fourcade and Healy coined the phrase "classification situation" specifically to study the role of algorithmic credit scoring on socioeconomic life chances, the concept links to a wider literature on the sociology of quantification that attempts to understand the role and effects of the contemporary rise of various forms of an institutionalised and more or less automated quantification (and its links to ranking and classification) of social life (Berman and Hirschman 2018; Espeland and Stevens 2008; Fourcade and Healy 2017)

The top journal rankings in economics, just like the notion of a disciplinary core, also have the normative function of indicating what scientific quality is and what the elite of the profession does. Any economist who thinks seriously about an academic career has to aim for publication in top journals, and thus the scope of acceptable research largely comes to be defined by what is acceptable to publish in those journals. When members of the discipline use these rankings to orient themselves towards scientific ideals or just as a strategic tool in a competitive labour market, journal rankings become reactive. Of course, all sciences require that work be published in scholarly journals, and that it be recognised by a group of peers. But as I showed with the evaluation reports, experts in the early period would talk of recognised or good international journals without paying attention to their ranking. This situation would be the case in a pluralistic discipline, where there might be several different sets of good journals, each with slightly different conceptions of quality. This would be a multidimensional heterarchy, where multiple dimensions of value coexist, rather than a hierarchy produced by an ordinal ranking, which is unidimensional. These idealisations should make the point clear, that a heterarchy allows for a plurality of valuations or orientations to coexist, without a well-defined centre. In a hierarchically-oriented discipline like economics, the classification situation becomes more pronounced, as there is a one-dimensional measure of the quality of publication outlets to relate to.

The normative and reactive nature of rankings mean that they potentially have some degree of epistemic impact. That is, the nature of top-ranked journals will have a normative effect when new generations of researchers try to learn from the paradigmatic examples. And if the discipline becomes explicitly oriented towards its top journals, the necessary boundary work is performed by the journal system and its reviewers. Micro-boundary work will still be performed by reviewers in individual cases, but the need for manual boundary work is potentially reduced when the discipline is increasingly defined by its top journals. A shift from traditional peer review towards a reliance on rankings means that a new and more powerful form of social classification mechanism has become active. Such new numerical systems of social classification and categorisation tend to become very powerful as, in the words of Fourcade and Healy (2017:294), they "fuse the rational legitimacy of technical analysis with the enigmatic but undeniable force of a Delphic oracle", and thereby "have the potential to produce the sort of naturalized facticity characteristic of truly social facts". We can thus see how the journal rankings become a new powerful stabilising mechanism that effectively hinders and punishes heterodox economics, while naturalising the disciplinary core.

3. Outlooks and new questions

In what respect is the situation in economics a unique case, and can we find elements of this disciplinary dynamic in other settings? Here, I can only speculate, but I do think that the styles approach allows some parallels to be drawn. One obvious partial parallel is the unease with which humanities-oriented qualitative approaches coexist with quantitatively-oriented and more scientistic approaches, and perhaps as a third position, with more theoretically-oriented approaches linked to continental philosophy, in sociology and other social sciences. The styles approach and the notion of self-authentication let us think about the gulf of mutual incomprehension that sometimes arises, and the fiery engagement with which approaches are defended as the most reasonable way to produce social knowledge. We may also think about the intradisciplinary conflicts as the renegotiation of the specific disciplinary mix of styles at a certain point in time and place. But the bridging function of styles is as evident here. A quantitatively-oriented sociologist may work more easily with a quantitative political scientist or economist, than with a *verstehen*-oriented fellow sociologist, who may in turn find collaboration much easier with an anthropologist or historian.

Although these are clear parallels, I think it is safe to say that all the abovementioned disciplines have a lower degree of disciplinarity than economics. This is evident from the weaker consensus on anything resembling a disciplinary core, and a lack of similarly standardised doctorate programmes, among other things. This points to my argument that disciplines are not only containers that are filled with different objects of study or styles of reasoning; the degree of disciplinarity should itself be thought of as a variable, which opens up interesting comparative questions for future studies.

A clearer example of a discipline with parallels to economics is philosophy. At least in the Scandinavian context, academic philosophy was almost completely dominated during the post-war years by analytic philosophy, as Heidegren (2016, 2015) has described. However, the dominance of analytic philosophy was contested by continental philosophy, and this analytic-continental divide became charged with meaning in the 1960s when the critique of positivism became an urgent matter for the students of the New Left. There are further parallels to be drawn between the epistemic orientation of the dominant philosophy and that of mainstream economics in their emphasis on clarity, rigour, and specialisation, but also in the way that heterodoxy (continental philosophy) has been disregarded as unscientific.

However, the rising interest in continental philosophy and the critique of positivism was clearly connected to the rise of social movements and political opinions in society. This, as I have argued drawing on my interview study, is also the case with heterodox economics. Therefore, a limitation of this study, and an opening for future research, is to take into serious consideration the variable academic impacts of social movements on intellectual movements and positions in science. Furthermore, the example of continental philosophy has parallels to other developments in the social sciences and humanities, in that it is linked to a critical attitude, or perhaps to critical theory in the widest possible sense. I think of this as the will to take the perspective of subordinate actors against systems of power, the refusal to participate in a naturalisation of the social order, and systematic attempts to lay bare the conditions of possibility of oppression and inequality, all with the goal to increase human freedom. We find this critical approach also in heterodox economics. But critical theory is not part of what we normally think of as (natural) science, and unsurprisingly, the Crombian typology of styles has no room for it. In my view, a proper understanding of the development of the social sciences, including economics, cannot do without a conception of critical theory as one of several very influential strains of thought that transcend schools of thought, national boundaries and academic disciplines.

A lacuna in the styles literature is work on the social sciences, since most if not all work hitherto has been done on the natural sciences, engineering and economics. Would it be possible to think of a tradition of critical theory as a distinct style of reasoning in the social sciences, perhaps with Marx' *Theses on Feuerbach* as foundational myth? And can we perhaps also think of a broad *hermeneutic* or interpretative style of reasoning in the humanities and social sciences, since at least the late nineteenth century, covering the wide array of attempts at understanding human meaning-making across disciplines? These must remain questions for future studies, but if the styles approach is to be fruitful for studies in the social sciences, they should be taken seriously.

There are more open ends that lead us towards new questions. I have emphasised the role of styles of reasoning in creating an intellectual and social divide, but have also pointed to the fact that styles act as bridges over disciplinary boundaries, and this bridging function of styles is a possible site for further study. In what settings, and under which circumstances can common styles bridge different disciplines and facilitate mutual understanding? And when it comes to disciplines, we may take the level of disciplinarity seriously as an open empirical question, and investigate to what extent various disciplines actually *discipline* reasoning. Following this is the even more interesting question of how the disciplining of reasoning is actually accomplished in particular cases. Through which historical processes and conditions of possibility (ideational, economic and institutional) is discipline produced, and which actors and institutions maintain and stabilise it? We may also investigate by what counteracting centrifugal forces the disciplining and stabilisation of disciplines is kept at bay or reversed.

On a more empirical level, I have described the economics job market as a novel institution to which Swedish economics departments have recently started to adapt. This institution potentially has strong integrating effects in drawing local departments closer into the international US-centred discipline. It is also an institution that affects young scholars particularly, and one effect, as mentioned by my interviewees, is the potential epistemic impact if doctoral dissertations become oriented towards preparing a job market paper. The job market and its epistemic effects thus merit thorough study as a novel institutional innovation in science.

4. Some implications for pluralist economists and other social scientists

I turn now to what I believe to be the implications of this study. First of all, the overarching problem that was my point of departure was formulated with inspiration from the movement for heterodox or pluralist economics. Few would disagree that intellectual and scientific pluralism is a universal goal in itself. Nor, probably, would most economists. This leads to a number of questions: What does intellectual pluralism mean? What sort of pluralism is preferable? How could greater pluralism be achieved? These questions have been discussed at length by heterodox economists, and I will not attempt to provide a definite answer here. I will however try to say something about what the insights of this study imply for projects of intellectual pluralism.

Styles and pluralism in economics

I have shown, as a central theme, how the disciplinary style of reasoning of mainstream economics is institutionalised, and how its stabilisation is linked to the simultaneous promotion of paradigmatic examples of good science and to the rejection of approaches to science understood as less scientific, including heterodox economics. I argued that styles of reasoning may be partially incommensurable, and that we should understand the mainstream-heterodoxy divide in such terms. This means that there are different and parallel conceptions of science (against a conception of the unity of science), including fundamental conceptions about how the world should be understood and studied. Then there are open questions about the degree of disciplinarity of scientific disciplines, and the specific constellation of styles of reasoning and its stability. In my conception of disciplinary styles, I argue that the combination of Crombian styles in a discipline may be relatively stable, but compared to the paradigm concept, it allows us to account for the evolutionary change of the disciplinary style.

Although I have focused primarily and extensively on the relative stability of disciplinary style, this does in no way mean that there is no change, and also contestation and competition between researchers, theories, fields and theoretical schools of thought. On the level of Crombian styles, I point to two such recent changes in economics. The first is the shift where the statistical style has gained ascendance over the modelling style since the 1970s, particularly more recently through econometric work on empirical data. The second is the more recent and still ongoing introduction of the experimental style. Here, a certain degree of contestation is evident, when for instance the senior generation of researchers may be sceptical about its epistemic merits, while it is held by many, not least in the younger generation, as a prime example of how the discipline is open and transforming. While this represents a shift in the disciplinary style mix, I argue that, overall, these styles seem to work in relative harmony in contemporary economics, so that the disciplinary style retains most of its identity and stability.

The styles approach allows us to think about continuity and change at different levels of abstraction. It directs our attention away from the skirmishes between theorists or even schools of thought, towards the more general level of the common fundamental assumptions about the world and how we can know about it. It also means that some of the recent developments, like behavioural economics, which are often mentioned as examples of change and broadening of modern economics, a new "mainstream pluralism", can be understood as a broadening of the approach only within the confines of the dominant and accepted disciplinary style. According to the conception of the mainstream-heterodoxy divide in terms of contested styles of reasoning that I have proposed, mainstream economics should be understood not in terms of any specific substantial theory, but rather in terms of a meta-theoretical approach to science, with a commitment to a set of very general assumptions of social ontology (methodological individualism, instrumental action, and the study of equilibrium phenomena), coupled with epistemological commitments to modelling, statistics, and more recently, experimentation. It is these questions that are at stake in calls for pluralism in economics: a relaxation of the disciplinarity of economics in its current form, and an opening of the disciplinary style towards heterodox theoretical and methodological assumptions and approaches. What is at stake in such a conception of pluralism is not this or that specific theory, but the fundamental conceptions of social ontology and epistemology that are contested by heterodox economics.

However, that deeply social nature of science emphasised by the styles approach also means that productive science presupposes a thought collective with a certain degree of common epistemic ground. Becoming part of a discipline then means learning to understand and reason about a specific aspect of the world in the same way. This is very clear in the self-understanding of most economists: the high degree of consensus on fundamentals in modern economics is understood as a great scientific asset. Becoming a member of the discipline also means accepting an epistemic break, letting go of prior concepts and preconceptions, accepting the disciplinary style and aligning oneself with it: this is the fundamental sense in which reasoning is disciplined by a scientific discipline. It is Kuhn's (1996) productive normal science, or Whitley's (1986:187) "conservative novelty producing systems". This break is directly related to the disciplinarity and autonomy of the discipline and the degree to which it becomes its own relevant audience, divorced from the general public (other than in popular form) or other scientific fields.

This means, first, a notion of pluralism that challenges this autonomy will naturally tend to be perceived as a threat by members of the discipline that understands its collective nature. In their view, as in Kuhn's, to leave the paradigm or disciplinary style is to leave science. Second, it means that if pluralists are to learn from this, the collective nature of knowledge production must be taken seriously if heterodoxy is to be viable. This is also largely done in discussions on the integration of heterodox economics to unify a single pluralistic heterodox approach as a strategic imperative for challenging mainstream economics. A reasonable proposal in this vein is Dobusch and Kapeller's (2012) notion of *interested pluralism,* by which they mean that pluralist economics should neither be just strategic pluralism (promoting one's own school with the ultimate aim to dominate) or disinterested (tolerating the existence of different approaches without much interaction, exemplified by the situation in sociology). Instead, their conception of interested pluralism has the goal of creating an increasingly integrated and synthesised paradigm through active integration and interaction between various heterodox schools of thought, to strategically create a single stronger challenger. Perhaps such a pluralist heterodox economics would not only be able to synthesise insights from various heterodox schools of thought. It could also potentially provide strong interdisciplinary bridges to other fields studying economic phenomena, like cultural geography, economic sociology and economic history.

Barriers and roads to pluralism

For anyone seriously interested in pluralism in economic science, understanding the conditions that enable disciplinary stabilisation should be a first step. This is also the overarching topic of this study. What does knowledge of these structural conditions imply for strategies to move towards pluralism?

First, I have argued that in a sociological understanding, an essential component of a disciplinary style of reasoning is the way that standardised education and practices tend to reinforce similarities in disciplinary habitus. To put it simply, it means that becoming an economist is not only a matter of the explicit use of theories and techniques learnt, but also about acquiring a deeper and more intuitive sense of how problems are approached and solved, the scope of valid scientific argument, and the overall professional capacity of scientific quality judgement. Here, the homogenisation of formal training means homogenisation of scientific habitus. Thus, broadening economics curricula to include a wider range of theoretical approaches and interdisciplinary outlooks is a natural strategy, and it is obvious to heterodox groups and, not least, student groups that struggle for broadened curricula. Exposure to a wider range of approaches to analysing and solving the pressing problems of our contemporary world is a reasonable demand. But as I have suggested, drawing on the interviews with heterodox economists, such exposure and learning can also be fuelled outside formal education, by social movements, or (for example) by student-organised reading groups. I have argued at length about the institutionalised power of a disciplinary style of reasoning. However, more powerful and older social institutions have been changed before by collective movements that successfully manage to convince supporters and work towards a common goal.

As I have argued, a key to understanding the appeal of mainstream economics as a common intellectual project is its perceived productivity and efficiency as a toolbox for formulating and solving problems in a way that not only provides clear and objective answers in a world of subjective disagreement, but does so in a seemingly universal and portable way across topics and problems. To present a viable alternative, heterodox economics must somehow face this challenge, and manage to convince supporters of the relevance and benefits of the alternative approach. One such opening might be a demand from policymakers for new forms of economic expertise. When it comes to intramural relevance, this will be harder. Judgement of scientific quality and the autonomy of science hinges on the peer review system, which in turn depends on a professional judgement grounded in a scientific habitus. The high degree of disciplinarity tends to lead to cognitive particularism—bias against deviating, heterodox approaches. In order to counteract this, an awareness of the phenomenon, and careful selection of reviewers is of utmost importance.

The shift towards the increasing use of economics journal rankings as judgement devices that I pointed to, however, transforms this problem. The relevant forms of quality judgement now become distributed and achieve a new and hardened level of social objectivity. Adding to suggestions by heterodox economists to create alternative rankings for heterodox journals (Lee et al. 2010), or strategic contributions to tighter heterodox citation networks (Dobusch and Kapeller 2012), I think that sociological studies of such systems are of great importance. These types of systems or classification situations have only recently begun to develop, and it is an essential task to study them, understand their social and epistemic effects, and critically and publicly discuss them. This is not to say that metrics should be banned from peer review, but only that the use and effects of various metrics by reviewers need to be considered very carefully, and also by academic employers, funding bodies and journal editors.

This leads us to the role of fundamental epistemic values in science and their role. I have shown how mainstream economists tend to have a disposition that favours the precise, certain and unambiguous, and cumulative knowledge. The flip side of this is a neglect of philosophical or metatheoretical reflection on the presumptions of the models and techniques that seemingly work so well, and the history of thought and its contingencies. I believe that there is a deep sense in which teaching the history of thought, competing heterodox theories, and the philosophy of science has an inherent critical potential. For this will not only equip students with a broader analytical understanding, or even require hiring qualified teachers in these areas, opening this box of reflexivity means that the currently-established best practice, the disciplinary style of reasoning, loses some of its natural taken-for-granted nature, and it becomes evident that *it could have been otherwise*.

Lessons for sociologists and other social scientists

Finally, I believe that there are lessons to be learnt by other social scientists, including sociologists. First, the dominance of the mainstream style in economics and the way that its institutional stability depends on relationally establishing boundaries against heterodoxy can serve as an example to learn from. Sociology, for example, is not free from struggles over styles of reasoning and conceptions of "proper" science. One need only to think of the old qual–quant divide. The difference of course is that sociology does not have a degree of disciplinarity or a disciplinary consensus that is anywhere near that of economics. While quantitative sociologists to some extent share the Crombian statistical style and scientific ideals with economists, qualitative sociologists are probably as entrenched in a particular shared style of reasoning, and the same goes for what could perhaps be seen as a third camp, of theoretically-inclined sociologists. In every case, being part of a thought collective, having an established common frame of reference and way of working, is fundamental to being scientifically productive.

The question is then how large that thought collective is and ought to be, and how it relates to others. It is analogous to the question of intellectual pluralism encountered in economics. Is the relative pluralism in sociology just a contingent outcome of the lack of sufficiently strong actors to dominate the discipline? Or is it related to a more pluralistic orientation? There are surely sociologists with different orientations, but I would like to claim that there is, if I may speak of a core of the sociological discipline, an insight that lends itself towards reflexivity and pluralism. It is the insight that that our understanding of the world is always contingently socially constructed, and following this, that the difference between lay and scientific knowledge is one of degree, rather than kind. Here, I believe that the sociology of knowledge, and the general problem of understanding how the categories people use to make sense of themselves and their world are socially rooted and subject to change and contestation, belongs, if anything does, to a sociological core. It is a unique and most valuable general idea that sensitises us to the merits of pluralism and the dangers of naturalising the given.

Sociology could also learn more directly from economics, and particularly heterodox economics. I have argued that the recent history of the social sciences and the rise of modern economics could be read as a splitting process where the economic has been divorced from the historical and the social, and shown how the maintenance of the boundaries of a style of reasoning is an relational affair including contrasting against other less favourable approaches. Sociology has also taken part in this process and boundary work, as evidenced by the widespread suspicion of economics and economic modes of explanation. However, heterodox economics, in its broader conception of economic phenomena, blurs the boundaries, and shows that economic analysis is not necessarily synonymous with modelling *homo economicus* style actors. Here, there are great opportunities for economic sociologists to continue bridging disciplinary boundaries and perhaps even for economic sociology to regain ground as a fundamental part of sociology.

In a sense, this study can be read as a critique of mainstream economics. It is a critique in the sense that it attempts to denaturalise it and understand the conditions of possibility of the dominance of the mainstream approach in modern economics, and its relationship to heterodoxy. Or to put it differently, it attempts to understand the social causes of why economists think the way they do. By implication, it also means that it could have been otherwise: it is possible to imagine other conditions and another economics. I do not claim that mainstream economists are wrong or unscientific. Such claims have been made extensively by heterodox economists, and their specific arguments should be read and evaluated in their own right as arguments within the discipline. Instead, I claim, with the sociological styles approach, that a specific disciplinary style has become institutionally stabilised and come to count as "science" in modern economics. It is not inevitable or necessary, nor is it the only style of reasoning that could legitimately claim to be scientific. At the same time, it is a good example of the fact that if something is socially caused and contingent doesn't in any sense imply that it is easily changed.

But, if sociologists (as is often the case) find a problem with economics and the extra-academic prevalence and superiority of economists and economics discourse, we need to be clear about why this is a problem. Is it the degree of disciplinarity and the disciplining of reasoning in general? Or is it the specific economic style of reasoning and its influence on policymakers (Hirschman and Berman 2014) or as general frame of thought or cultural repertoire? In relation to the latter, Lamont (2016) recently argued that compared to economists, "many sociologists behave like kindergarteners when it comes to increasing our impact", and that sociologists should be more strategic, as a profession, in order to increase the impact of alternative and less individualistic frameworks for making sense of social life. It is a great irony that the supposedly unsocial economists with their individualist scientific ontology are acting as a collective *in practice* (identifying with the disciplinary thought collective, safeguarding the collective disciplinary boundaries, reproducing institutions of socialisation, and so on), while we sociologists, supposedly experts on social institutions and collective phenomena, are not so impressive *in practice*.

Maybe we can learn something from mainstream economics here as well. Kieran Healy has recently suggested that sociologists should be theoretically bolder and let go of the widespread use of nuance as a theoretical virtue and habit of thought, in his words, to "Fuck nuance". For example, it is extremely simple to ridicule the simplifications of economists, but we should remember that simplification is very powerful. "There is no less nuanced a character than *homo economicus*. While it is easy to snipe at him on this basis, the strategy of assuming a can opener, as the old desert island joke goes, has been an unreasonably effective way of generating some powerful ideas" (Healy 2017:124). Healy reminds us that such simple ideas, exemplified by the enormous influence of Gary Becker, were also central to Foucault's notion of neoliberalism as a new conception of human nature and form of subjectivity, perhaps even the final and actual realisation of *homo economicus* (Read 2009). If sociology is to have a greater societal impact, perhaps there is something to be learnt from economics about being collectively strategic as a profession, about being less nuanced and more cumulative, and contributing to society by more successfully providing another and more social framework for reflecting on our world and its ills.

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Appendix 1. List of analysed expert evaluation reports.

University	Year	Archival No.	Title (specialisation)
Gothenburg	1993	E 311 2918/93	Professor of economics (macroeconomics)
Gothenburg	1994	E 311 1099/94	Professor of economics (environmental economics)
Gothenburg	1995	E 311 1260/95	Professor of econometrics
Gothenburg	1997	E 311 1927/97	Professor of economics (social policy)
Gothenburg	2005	E 311 2527/05	Professor of economics
Gothenburg	2008	E 311 2041/08	Professor of economics
Lund	1989	VI E311 11536-89	Professor of economics
Lund	1991	VI E311 7847-91	Professor of economics (women studies)
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