

# Behavioural Economics as a Style of Reasoning

On the Rise of Psychological and Social Perspectives  
in Academic Economics

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SOCK04, Autumn Term 2017

## Abstract

Though there are numerous studies attempting to describe and analyse the internal dynamics of academic economics, the results of these studies are often contradictory. In order to assess the potential usefulness of certain theoretical tools to the sociology of economics, this study presents and analyses an in-depth interview study of behavioural economists at Swedish universities. The theoretical framework under assessment is derived from the social theory of Pierre Bourdieu and the styles of reasoning approach. Utilizing the concept of styles of scientific reasoning, field theory, capital theory, and the notion of scientific habitus to analyse the rise of behavioural economics into the mainstream of the economics discipline, this study concludes that the theoretical framework provides tools that are very useful in the analysis of social phenomena in academic economics.

*Keywords:* sociology of economics, behavioral economics, Pierre Bourdieu, styles of reasoning, interdisciplinary research

## Acknowledgements

In addition to thanking all of my interview subjects for participating in my study, I would like to particularly acknowledge the help I have received from Bo Isenberg and Anders Hylmö – thank you so much your invaluable guidance, your interesting conversations, and your patience!

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# 1 Introduction

In recent years, there have been numerous studies attempting to describe and analyse the internal dynamics of the economics discipline. Fascinated or frustrated with the rise of an almost exclusively empirical discipline centred around quantitative reasoning, statistics, and mathematical modelling, sociologists of science and economists alike have published numerous articles and books on the subject. One particular branch of these studies commonly discusses or criticises a perceived lack of theoretical pluralism within the discipline, often understood to be a division between what is broadly referred to as heterodox economics and neoclassical or mainstream economics. Though this branch of studies is united by its subject matter, it is by no means unanimous – the studies alternatively conclude that a scientific revolution within economics is desirable, undesirable, unlikely, or ongoing. Regardless of the disagreement between the arguments coming from this branch of study, there seems to exist, above all, a frustration towards economics within the other social science disciplines. This frustration points to the need for more studies that evaluate theoretical and methodological approaches that can help researchers to describe and analyse the nature of academic economics.

One rather recent – and arguably successful – example of theoretical diversification within economics is the integration of psychological and social perspectives made possible through the advancement of behavioural economics into the mainstream of the discipline. Behavioural economics seems to have been able to advance past the accusation of heterodoxy, perhaps best exemplified by the awarding of the Nobel Memorial Prize in Economic Science to prominent behavioural economists such as Daniel Kahneman, Vernon L. Smith, and Richard H. Thaler. Arguing that the process by which behavioural economics has become an accepted part of mainstream economics offers relevant material for a study of the internal dynamics of the discipline, this thesis specifically attempts to evaluate the usefulness of Pierre Bourdieu's social theory and the concept of styles of reasoning to the sociology of economics.

Beginning with an account of my research questions as well as some literature concerning the economics discipline, this thesis presents and analyses a qualitative study of behavioural economists at Swedish universities. As the field of research concerning the economics discipline is so vast, the previous studies discussed here are those that connect most fittingly to my subject area. The thorough presentation of my theoretical framework and methodology connect to my analysis of behavioural economics as well as my evaluation of the usefulness of these approaches.

## 1.1 Purpose and Research Questions

This thesis attempts to assign itself a particular place within the large field of the sociology of economics. The purpose of my study is, as mentioned, to evaluate how useful the social theory of Pierre Bourdieu and the styles of reasoning approach can be to the study of academic economics. As I evaluate this by way of an in-depth interview study of behavioural economists at Swedish universities, this thesis also seeks to complement the knowledge concerning how behavioural economics has become a part of the mainstream of the economics discipline. Though the scope of this thesis is quite limited, I make reference to several previous studies in order to connect my arguments to a larger context. The relevance of my findings to other settings is also discussed more thoroughly below.

In order for me to realize the ambition of my study, this thesis relates to two broadly delineated research questions:

1. How do behavioural economists at Swedish universities view the advancement of their subfield in the economics discipline?
2. How can a theoretical approach informed by the social theory of Pierre Bourdieu and the concept of styles of reasoning add to the understanding of disciplinary dynamics in academic economics?

Though the second research question perhaps relates the most to the aspirations of this thesis, the first question is the one that my interview study can most readily provide answers to. Findings pertaining to both of the questions are discussed under the different headings of my results and analysis section. In my concluding remarks, I offer specific answers to my research questions.

## 1.2 The Economics Discipline

Broadly speaking, economics is the science that studies how economies function, often with a focus on how economic agents behave and interact with each other (Blaug, 2017). The discipline is, however, more readily characterized by its analytical methods rather than by the subjects upon which these methods are applied (Siven, 2017). Although initially quite literary, the economics discipline has become increasingly reliant on mathematical and statistical methods in the post-World War II era (Fourcade, Ollion & Algan, 2015; Siven, 2017). The mathematical methods used by contemporary economists are often aimed at producing *models* – simplified theoretical representations of real-world phenomena – that can be used to create verifiable hypotheses about

economic behaviour (Blaug, 2017; Colander, 2000). Economic models frequently build upon the assumption that economic agents are rational:

The implication of the rationality is that the behaviour is consciously or unconsciously goal-oriented. When economists describe the behaviour of a company, they assume that companies strive to maximize an objective function (e.g. profit). The exact assumptions that are made about the objective function depend on the rest of the assumptions (e.g. if the owners have a decisive influence or if the company's management team is relatively independent of the owners). The model is in the next step confronted with empirical facts. Theory and empirics interact since the theoretical analysis often does not supply definite conclusions. When this is the case, statistical inquiries can clarify which of the possible theoretical effects that are prevalent. (Siven, 2017, my translation.)

The segment of economics that builds its models around an assumption of rationality is often referred to as *neoclassical* or simply *the mainstream* of the discipline (Bergh, 2017; Heise, 2014). There is a strong consensus within the economics discipline about the theories and methods of its mainstream, which has led to increasingly standardized international textbooks and PhD programmes, as well as a strong emphasis on the importance of being published in the top economics journals (Hylmö, 2017). The consensus within economics has led the discipline to have a far more unitary disciplinary core than the other social sciences (Fourcade, Ollion & Algan, 2015).

Other than the development towards mathematical and statistical methods, the focus on rationality-assumption models, and the rise of a strong disciplinary core, the economics discipline has been going through what can be referred to as an "empirical revolution" since the 1990's (Fourcade, Ollion & Algan, 2015, p. 92). In an article attempting to analyse the publishing trends in some of the top economics journals during the 1963–2011 timespan, Daniel S. Hamermesh (2013) finds that the publication of purely theoretical papers has steadily declined, making way for empirical studies analysing original data as well as studies examining data from laboratory or field experiments (see Table 1). While the upsurge of experimental papers in economics journals largely correlates with the rise of behavioural economics described further below, the other developments within economics provide contextualization for the increasing divide between the mainstream and the non-mainstream of the discipline.

PERCENT DISTRIBUTIONS OF METHODOLOGY OF PUBLISHED ARTICLES, 1963–2011\*

Year	Type of study				
	Theory	Theory with simulation	Empirical: borrowed data	Empirical: own data	Experiment
1963	50.7	1.5	39.1	8.7	0
1973	54.6	4.2	37.0	4.2	0
1983	57.6	4.0	35.2	2.4	0.8
1993	32.4	7.3	47.8	8.8	3.7
2003	28.9	11.1	38.5	17.8	3.7
2011	19.1	8.8	29.9	34.0	8.2

\* A type could not be assigned to seventeen of the articles published in 1963.

**Table 1.** From “Six Decades of Top Economics Publishing: Who and How?” by D. S. Hamermesh, 2013, *Journal of Economic Literature*, p. 168. Copyright 2013 American Economic Association.

### 1.2.1 The Mainstream–Heterodox Divide

The strong turn towards a unified economics discipline has been met with criticism, primarily from economists who connect to economic traditions that are not theoretically or methodologically compatible with neoclassical or mainstream economics (Dobusch and Kapeller, 2012; Heise, 2014; Hylmö, 2017). These non-mainstream economists are often considered to belong to a group within the economics discipline referred to as *heterodox*, generally defined as the economic traditions that reject some or all of the principles of mainstream economics (Heise, 2014). The disagreement between the strong mainstream of the economics discipline and its heterodoxy can then be generally referred to as *the mainstream–heterodox divide* (Hylmö, 2017). Though the abundance of articles and books discussing different aspects of the mainstream–heterodoxy divide makes a comprehensive overview of the debate impossible within the scope of this thesis, a few prior studies that connect to the subject under study bare mentioning here.

Arne Heise (2014) argues that the criticism towards the economics discipline has become more prominent since the 2008 financial crisis, further contending that the dualistic division between mainstream and heterodox economics risks obscuring the competing perspectives within the discipline and instead categorizes several of the disciplinary subfields within economics by their respective epistemology, methodology and heuristics (see Table 2). Heise bases his categorization of schools of economic thought on their adherence to, or rejection of, what he

### Classification of Economic Paradigms

Epistemology (Core assumptions)	Methodology	Heuristics	Paradigm	Theoretical school
<ul style="list-style-type: none"> <li>–Rationality assumption</li> <li>–Ergodicity assumption</li> <li>–Substitutionality assumption</li> </ul>	Deductive, positivist reductionism based on formal mathematics + highly developed empiricism/experimentalism	Acceptance of the stability of market clearing as an “ideal solution”	DSGE	<ul style="list-style-type: none"> <li>–New classical macroeconomics</li> <li>–Neo-Keynesianism</li> <li>–Standard Keynesianism</li> </ul>
Questioning some core assumptions	Deductive, positivist reductionism based on formal mathematics + highly developed empiricism/experimentalism	Acceptance of the stability of market clearing as an “ideal solution”	DSGE dissenters	<ul style="list-style-type: none"> <li>–Behavioral economics</li> <li>–Neuroeconomics</li> <li>–Complexity economics</li> <li>–Evolutionary economics</li> </ul>
<ul style="list-style-type: none"> <li>–Rationality assumption</li> <li>–Ergodicity assumption</li> <li>–Substitutionality assumption</li> </ul>	Rejection of deductive, positivist reductionism based on formal mathematics	Acceptance of the stability of market clearing as an “ideal solution”	DSGE dissenters	<ul style="list-style-type: none"> <li>–<i>Ordnungs-ökonomie</i></li> <li>–The Austrian School</li> <li>–Critical Neoclassicism</li> </ul>
<ul style="list-style-type: none"> <li>–Rationality assumption</li> <li>–Ergodicity assumption</li> <li>–Substitutionality assumption</li> <li>–Asymmetric information assumption</li> </ul>	Deductive reductionism based on formal mathematics + highly developed empiricism/experimentalism	Rejection of the stability of market clearing as an “ideal solution”	Dissenters/ Heterodoxy	–Information economics
Questioning some core assumptions	Acceptance of deduction based on formal mathematics + narrative analysis	Rejection of the stability of market clearing as an “ideal solution”	Heterodoxy	<ul style="list-style-type: none"> <li>–Post-Keynesianism</li> <li>–Social economics/Social economic institutionalism</li> <li>–Regulation theory/ SSA/ Radicals</li> <li>–The Historical School</li> </ul>
<ul style="list-style-type: none"> <li>–Rationality assumption</li> <li>–Ergodicity assumption</li> <li>–Substitutionality assumption</li> </ul>	Deductive reductionism based on formal mathematics + highly developed empiricism/experimentalism	Rejection of the stability of market clearing as an “ideal solution”	Heterodoxy	–Neo-Ricardianism

**Table 2.** From “The Future of Economics in a Lakatos–Bourdieu Framework” by A. Heise, 2014, *International Journal of Political Economy*, p. 82. Copyright 2015 Taylor & Francis Group, LLC.

considers to be the main aspect of mainstream economics: the creation of rationality-assumption models known as *dynamic-stochastic general equilibrium models* (DSGEs). His categorization of economic subfields is of particular relevance to my study since behavioural economics is classified as a *dissenting*, rather than outright heterodox, theoretical school.

Marion Fourcade, Etienne Ollion and Yann Algan (2015) contend that the economics discipline possesses a dominant position within the social sciences in the USA and attempt to analyse the nature of this dominance. One of their main arguments is that economics, to a larger degree than the other social sciences, is characterized by what they label *insularity*, a phenomenon that they demonstrate with bibliometric data. Using this data to show the tendency of US economists to chiefly cite texts from within their own discipline, the authors argue that:

[t]here are several reasons for the insularity of economics, most importantly the different epistemological cultures of the various social science disciplines and the power inequalities between them. First, the theory of action that comes with economists’ analytical style is hardly compatible with the basic premise of much of the human sciences, namely that social



processes shape individual preferences (rather than the other way around). In economics, by contrast, “de gustibus non est disputandum” (Stigler and Becker 1977): preferences are “usually assumed to be fixed” (Baron and Hannan 1994, p. 1116). Second, the qualitative methods that underpin the work of many interpretive social scientists often do not square well with economists’ formal aspirations, with their views on causality, or with their predilection for methodological and theoretical precision over real-world accuracy. Third, even when the substantive terrains overlap, the explicit or implicit pecking order between the disciplines often stands in the way of a desirable form of intellectual engagement. (Fourcade, Ollion & Algan, 2015, p. 93.)

The authors also contend that though economists often venture into potentially interdisciplinary fields of study “they are unlikely to learn much from [other scientists], as they often prefer to deploy their own techniques” (Fourcade, Ollion & Algan, 2015, p. 94). Citing the responses of US professors from different disciplines in an opinion survey (see Table 3), they suggest that the insularity of economics is caused by an intellectual self-confidence that economists possess due to the dominant position of their science.

**Agreement or Disagreement with the Proposition: “In general, interdisciplinary knowledge is better than knowledge obtained by a single discipline.”**

<i>American university professors in</i>	<i>% Agree/ Strongly agree</i>	<i>% Disagree/ Strongly disagree</i>	<i>% No answer/ Don't know</i>
Economics	42.1	57.3	0.6
Sociology	72.9	25.3	1.8
Political science	59.8	28.0	12.2
Psychology	78.7	9.4	11.9
Finance	86.6	9.6	3.8
History	68.2	31.7	0.1

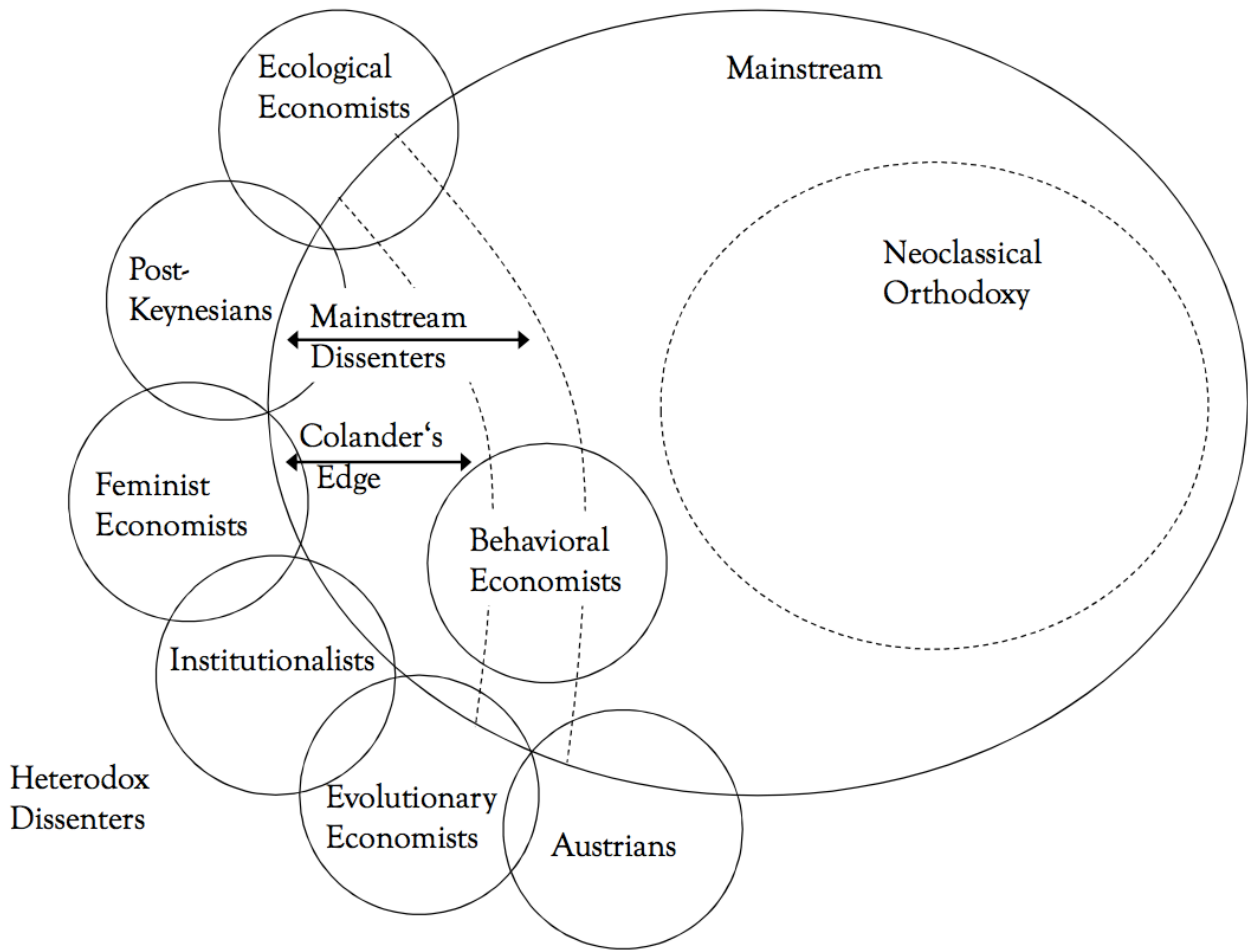
*Source:* From Gross and Simmons’ survey about the politics of the American professoriate. The survey was conducted in 2006. The authors sampled 100 individuals in each field. Return rates are low (though not unusually low for this kind of survey) and varied importantly across disciplines (economists: 44%; sociologists: 55%; political scientists: 54%; psychologists 49%; finance professors: 37%; historians: 54%). We are grateful to Neil Gross for running the cross-tabulations on this survey for us here and elsewhere in the paper. See Gross and Simmons (2007) for details about the survey and Gross (2013) for a broader analysis.

**Table 3.** From “The Superiority of Economists” by M. Fourcade, E. Ollion, and Y. Algan, 2015, *Journal of Economic Perspectives*, p. 95. Copyright 2015 American Economic Association.

Anders Hylmö (2017) has conducted an interview study that is in some ways similar to my own. Between 2015 and 2016, Hylmö conducted in-depth interviews with 20 representatives of both mainstream and heterodox economics in Sweden. The aim of the study was largely to provide further insights into how the mainstream–heterodox divide is understood by economists themselves, and one of Hylmö’s (2017, p. 4) central claims is that a styles of reasoning approach can provide analysts with a theoretical framework that casts additional light on the subject of how disciplinary dynamics function and “help us understand important aspects of the intellectual divides in economics”. For instance, Hylmö (2017, p. 20) argues that “heterodox impulses” originate in exposure to different styles of reasoning. He furthermore argues that findings from within Swedish economics are “probably relevant” for other settings due to the US-oriented internationalization of the discipline (Hylmö, 2017, p. 3).

## 1.2.2 The Rise of Behavioural Economics

In the midst of the criticism towards the excluding nature of mainstream economics, one subfield has managed to gain ground within the discipline: *behavioural economics*. Where neoclassical modelling is often built upon an assumption of rationality in agents, researchers within behavioural economics attempt to account for the impact of psychological and social effects on economic behaviour when they create models (Bergh, 2017). Though research in behavioural economics has been going on since at least the 1950’s, it is largely since the 1980’s and during the 1990’s that the field has gained recognition as a prominent subfield within the economics discipline (Geiger, 2017; Heukelom, 2014; Weber & Dawes, 2005). Though there is disagreement as to whether behavioural economics is to be considered a part of the mainstream or not, Leonhard Dobusch and Jakob Kapeller (2012, p. 1036) provide what they call “a stylized overview of the current economic discourse in the form of a simplified paradigmatic map” (see Figure 1). This paradigmatic map categorizes behavioural economics in much the same way as Heise (2014, see Table 2) and offers a helpful visualization of the subfield’s position within economics. The position on the map called “Colander’s Edge” is a reference to a categorization made by David Colander (2005, p. 192), who contends that “while [behavioural economics] is not center mainstream, it is clearly at the edge of mainstream”.



**Figure 1.** Map of paradigms in economic discourse from “Heterodox United vs. Mainstream City? Sketching a Framework for Interested Pluralism in Economics” by L. Dobusch and J. Kapeller, 2012, *Journal of Economic Issues*, p. 1037. Copyright 2012 *Journal of Economic Issues* and the Association for Evolutionary Economics.

With the historical development and the current position of behavioural economics in mind, what is of interest in this thesis is *how* the subfield has managed to gain such recognition within the economics discipline while other perspectives are still labelled heterodox (see Table 2 and Figure 1). Roberto Weber and Robyn Dawes (2005) provide a general outline of the process that research in behavioural economics goes through in order to become accepted by neoclassical economists, defining three general stages:

- (1) the demonstration of a behavioral result anomalous with traditional economic theory, (2) the replication, collection, and synthesis of a behavioral regularity, and (3) the development of behavioral theory incorporating the regularity. (Weber & Dawes, 2005, p. 91.)

It can be argued that the first two of these three stages are largely dependent on the way behavioural economists have made use of the experimental method, since it is generally through laboratory experiments that both theoretical anomalies and behavioural regularities have been demonstrated (Syll, 2011; Weber & Dawes, 2005). Lars Pålsson Syll (2011, p. 148, my translation) has previously reasoned that since early behavioural economics was “more about incorporating psychology in economic theory rather than developing an entirely new theory” it was not perceived as threatening by mainstream economists, meaning that the subfield was “able to more easily gain acceptance”.

Economic experiments are largely similar to the laboratory experiments used by psychologists and other social scientists: subjects are brought into a lab or an artificial environment where their actions can be thoroughly recorded, and they are presented with, for example, tasks to be completed, problems to be solved, or decisions to be made (Heukelom, 2014; Weber & Dawes, 2005). The experimental method is commonly used by researchers to test behavioural assumptions and explore the potential sources or causes of specific behaviours, yet behavioural economists have distinguished their use of the method through their intentional focus on “experiments that are likely to be convincing to traditional economists skeptical of experimental research” (Weber & Dawes, 2005, p. 92).

## 2 Theoretical framework

In this section, I present the theoretical approaches that I will attempt to utilize and evaluate in the course of my analysis of the advancement of behavioural economics in the economics discipline. The approaches are the social theory of Pierre Bourdieu and the concept of styles of reasoning. Bourdieu referred to his theory as a theory of *practice*, where practice denotes the dynamic and dialectical process by which social agents interact with the social structures that shape their experiences. The styles of reasoning approach allows for the conceptualization of different scientific epistemologies and methodologies as differences in scientific styles. These two frameworks are used more or less interchangeably in my analysis.

## 2.1 The Social Theory of Pierre Bourdieu

One of the aspects that sets Bourdieu's sociology apart from much other sociological production is the way in which it strives to challenge certain categories and boundaries in social science. For example, Bourdieu (1990, p. 25) expressed a need to transcend the divide between subjectivism and objectivism "while preserving the gains from each of them". This need to consolidate insights gained from separate and sometimes opposing perspectives also permeates much of Bourdieu's theory, which is largely indebted to the classical social theories of Karl Marx, Émile Durkheim and Max Weber – meaning that Bourdieu essentially builds his theory from what is commonly viewed as separate sociological paradigms (Brubaker, 1985). Other than working to transcend established thoughts about the nature of truth and combining material from different schools of thought, Bourdieu sought to give his theories a thoroughly empirical ground to stand on by collecting data with the help of a wide variety of methodologies (Brubaker, 1985; Townley, 2014). The result of his theorizing and his empirical research are what he preferred to refer to as "a set of *thinking tools*" (Wacquant, 1989, p. 50). Several of these thinking tools are utilized in my analysis and therefore require some introduction.

### 2.1.1 Field Theory

Bourdieu conceptualizes the spaces in which social practice take place as *fields*, which, in the context of the large social cosmos of a differentiated society, are defined as "relatively autonomous social microcosms" that are governed by their own internal rules and hierarchies (Wacquant, 1989, p. 39). A field can be further defined as a relational network that consists of the social positions of individual agents – a network which is internally structured by the relationships of power between agents that are continually struggling with each other in pursuit of the social gains that can be attained within that specific field (Bourdieu, 1984; Griller, 1996; Wacquant, 1989). When discussing different fields, it is possible to be either very general or very precise in scope – since all fields have subfields, a specific field may be conceptualized on several levels: "a broad field (e.g. education); a specific field (e.g. a discipline); or the social agents within a field (e.g. a department of a school)" (Townley, 2014, p. 42).

In addition to the general concept of fields, Bourdieu's theory offers some terminology that can be used to discuss the inner processes of these social spaces. For example, Bourdieu (1990, p.

66) calls the “presuppositions” of a field – its internal rules and logic – the *doxa* of that particular field. Agents wishing to be seen as legitimate members of a field must accept the *doxa*, as they otherwise run the risk of being called inexperienced or unqualified by other members of the field (Heidegren & Lundberg, 2010). The tendency by social agents to fully accept the *doxa* and to further internalize a “fundamental belief in the value of the stakes” of a field is referred to as *illusio* (Bourdieu, 2005, p. 9). Together, the *doxa* of the field and the *illusio* of the agents create boundaries of what is considered to be appropriate, possible, and true within a field – boundaries which, in extension, create the limits that distinguishes one field from another (Heidegren & Lundberg, 2010; Wacquant, 1989).

Where *doxa* denotes the fundamental beliefs within a field, the term *nomos* refers to the specific value-systems or perspectives that are inherent to different fields (Bourdieu, 1990; Wacquant, 1989). For instance, Bourdieu (2005) argues that the *nomos* specific to the field of business can be understood by recognizing the way in which it principally values the pursuit of economic gain over social and familial obligations. It is consequently possible to talk about the *nomos* of different scientific disciplines:

Every scientific discipline is also defined by a particular *nomos*. *Nomos* refers to the specific way a subject or discipline constructs its object, that is, how it views and delimits the subject from other disciplines. If *doxa* consists of a series of beliefs, *nomos* is rather a particular perspective. *Nomos* functions as a criterion of relevance that excludes certain approaches as irrelevant or illegitimate. (Heidegren & Lundberg, 2010, p. 13.)

In short, then, one of the aims of this thesis is to analyse and describe the *doxa*, *illusio*, and *nomos* of the field of academic economics and the subfield of behavioural economics.

### 2.1.2 Capital Theory

Bourdieu refers to the social gains pursued by agents in fields as different forms of *capital*, of which economic, cultural, social, and symbolic are the main varieties (Bourdieu 2011; Townley, 2014). *Economic capital* consists of money or assets that are easily transformed into money, such as property rights and other tangible business assets (Pret, Shaw & Dodd, 2016; Townley, 2014). This largely material form of capital is perhaps the most easily understood form of resource, as it is the form of capital that is generally “recognized by economic theory” (Bourdieu, 2011, p. 78). *Cultural capital* and *social capital*, however, should not be confused with the concept from economic theory referred to as *human capital* – a concept that Bourdieu (2005, p. 2) considered

“vague and flabby” as well as “heavily laden with sociologically unacceptable assumptions”. Rather, cultural capital consists of different constellations of “personal dispositions, cultural goods, skills and education”, while social capital has to do with “membership in societies, relations, networks and alliances” (Pret, Shaw & Dodd, 2016, p. 1007). *Symbolic capital* is a slightly different form of capital, as the other capitals are all capable of functioning as symbolic resources within a field (Townley, 2014). In short, economic, cultural, and social capital become symbolic capital when they are recognized by other agents within a field to be “legitimate and credible” (Lee & Shaw, 2016, p. 1737), such as when they take on the form of “awards, trophies, diplomas, publicity, reputation and prestige” (Pret, Shaw & Dodd, 2016, p. 1007). Though economic, cultural, social, and symbolic capital together make up the fundamentals of Bourdieu’s capital theory, there are occasionally references to other, more specific, forms of capital, such as when Bourdieu (2005, p. 194) states that “technological capital, juridical capital and organizational capital” can constitute assets in the field of business.

### 2.1.3 Habitus

Another central thinking tool in Bourdieusian social theory is the notion of *habitus*, an internalized and embodied system of social structures that guides the perceptions, dispositions, and behaviour of individual agents (Bourdieu, 1984). Habitus is the product of both collective and individual history and provides agents with schemes of everything from appreciation to action, leading Bourdieu (2005, p. 211) to describe it as a form of “socialized subjectivity”. The notion that what guides individual agency is not purely the result of personal experiences, but rather the product of an individual’s historical positions in specific social spaces, separates the notion of habitus from more specifically psychological postulations (Griller, 1996). Though habitus acts as “the organizing principle” of individual agency and informs “all thought and action” (Bourdieu, 1977, p. 17), the concept is in no way meant to be deterministic, nor is it meant to imply that agents consciously follow a specific set of rules – rather, habitus should be understood as an acquired and largely unconscious system of dispositions that has its basis in social structures and therefore guides practical activity in social fields (Brubaker, 1985). Since the activity in fields is largely governed by the habitus of individual agents, habitus and field can be said to interact in producing the strategies that agents unconsciously follow in pursuit of field-specific capital (Griller, 1996). The separation between *the primary habitus* and *a specific habitus* should also be made clear:

while the primary habitus is largely formed during early socialization, a specific habitus can be formed much later as the result of activity within a particular field (Townley, 2014).

#### 2.1.4 Sociology of Science

Though Bourdieu is perhaps generally not thought of as a sociologist of science, he did utilize and adapt his thinking tools to research and discuss the nature of scientific production (e.g. Bourdieu, 1975, 1991, 2004). In *Homo Academicus* (1988), Bourdieu even examined, in detail, the social backgrounds and current behaviour of many of his contemporary scientists in an attempt to map the struggles for power and capital within the French intellectual field, demonstrating that science is not merely characterized by the production of knowledge. Though such an extensive study of the economics discipline would no doubt be interesting, it is beyond both the focus and scope of this thesis. What is of interest here is rather the way in which Bourdieu operationalized his concepts in his sociology of science.

One specific habitus that Bourdieu (1975, p. 30) mentions is “the scientific habitus”, meaning the habitus a social agent acquires during the course of an advanced academic education. This habitus provides schemes of perception and appreciation specific to the scientific field, meaning, in short, that this specific habitus becomes their “embodied sense of judgement in scientific matters” (Hylmö, p. 11).

In addition to the scientific habitus, Bourdieu (1975) sees the concept of *scientific authority* as a particular form of social capital. In discussing the ability of actors in the field of scientific production to acquire this social capital, he contends that:

a particular producer cannot expect recognition of the value of his products (“reputation”, “prestige”, “authority”, “competence”, etc.) from anyone except other producers, who, being his competitors too, are those least inclined to grant recognition without discussion and scrutiny. This is true *de facto*: only scientists involved in the area have the means of symbolically appropriating his work and assessing its merits. And it is also true *de jure*; the scientist who appeals to an authority outside the field cannot fail to incur discredit. (Bourdieu, 1975, p. 23)

Bourdieu therefore argues that only scientists who share a discipline can bestow scientific authority upon each other’s work – in other words: only other actors in a certain scientific field that are endowed with the specific habitus of that field can grant the capital specific to that field. Bourdieu (1975, p. 30) further argues that scientific journals serve the function of “holding out the



example of what deserves the name of science”, meaning that they can be said to “exercise a *de facto* censorship of heretical productions”.

## 2.2 The Styles of Reasoning Approach

In addition to utilizing Bourdieu’s social theory and its terminology, I will, in discussing the results of my interview study, make use of what can be referred to as *the styles of reasoning approach*. Though initially labelled differing *Denkstile* or *styles of thinking* by theorists such as Karl Mannheim, Ludwik Fleck and Alistair Crombie (cf. Crombie, 1995; Fleck, 1979; Nelson, 1992), Ian Hacking (1992, 2012) has suggested a shift of focus to styles of *reasoning* when discussing scientific disciplines in order to incorporate the more social and practical aspects of scientific production. Mary S. Morgan (2012, p. 15) has previously utilized this concept of styles of reasoning in her study of the rise of mathematical modelling in economics, where she contends that different styles are simply different “ways of finding out about the world”.

In short, the styles of reasoning approach conceptualizes differences in the epistemological assumptions, or preferred methodologies, of scientific disciplines as variances of *style*. Hylmö (2017), as previously mentioned, has argued that the concept of scientific styles makes it possible to understand the mainstream–heterodox divide in economics as a dispute over the legitimacy of differing styles. I would therefore argue that the styles framework should act to simplify the analysis of scientific reasoning, as it consolidates rather complex notions such as theory, epistemology, and methodology into the more easily accessible notion of styles.

Though the framework may simplify the discussion and analysis of the reasoning specific to different scientific disciplines, the concept of scientific styles can also be argued to have comprehensive implications for the understanding of scientific production:

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, p. 10.)

The notion that the accepted styles of reasoning in different scientific disciplines are the result of struggles connects to Bourdieu’s (1988) study of power struggles in the French intellectual field. As a style of reasoning defines what is considered reasonable or true within a scientific discipline,

the approach also shares some characteristics with certain Bourdieusian notions – notably doxa, illusio, nomos, and habitus.

### 3 Methodology

In addition to introducing relevant previous studies and presenting the theoretical perspectives employed in my analysis, I will review the methods of data collection and processing that this thesis has utilized. As previously mentioned, the study presented here gained its empirical substance through a series of in-depth interviews at the economics departments at Swedish universities – specifically Lund University and the Stockholm School of Economics. Though the questions in my interview guide (see Appendix) are structured according to particular themes, the questions were specifically written to allow unstructured and open-ended answers; meaning that my guide was designed to facilitate what is commonly referred to as “semi-structured interviews” (Trost, 2010, p. 42, my translation). I conducted three interviews at each institution, totalling six interviews with three professors and three PhD students. Though all of the interview subjects were at the time of the interview study focused on behavioural economics research, all of them did not identify *solely* as behavioural economists – this term is, however, utilized when referring to them for the sake of simplicity. My choice to conduct interviews at Lund University and the Stockholm School of Economics was founded on the fact that, according to the *Web of Science*, these two institutions together with Stockholm University are the top three Swedish contributors to economics journals. Since the economics departments at Lund University and the Stockholm School of Economics also tend to describe themselves as “distinguished” and “leading”, making reference to international rankings of university faculties and departments (see, e.g., Lund University, 2015, 2016; Stockholm School of Economics, 2017), I would argue that these departments are well-suited for a study of mainstream economics. Though I had initially intended to interview behavioural economists at all three of the prolific institutions, I was unable to find willing participants at Stockholm University. Had my study included informants at Stockholm University, or had my study been conducted at completely different economics departments, the findings may have been different. As the economics discipline is quite internationally unitary, however, I believe that the results would have been analogous. I also believe, in line with Hylmö (2017), that results from studies in Sweden are likely to be relevant in other similar settings.

As several of my informants were international researchers not fluent in Swedish, all of my informants were asked if they were comfortable with conducting their interviews in English. None of them expressed any anxiety towards this, so all of my interviews were conducted in the language of this thesis. All of my informants agreed to have their interviews recorded, and a gap of three weeks between the interviews in Lund and those in Stockholm allowed me to transcribe the first three interviews and update my interview guide into its final form (see Appendix). My interviews were between 40 minutes to an hour long. When I had carried out and transcribed all of my interviews, they were thoroughly re-read in order for me to be able to structure my findings and present informative excerpts in the results and analysis section of this thesis. Since the society of researchers at Swedish universities interested in behavioural economics is quite small, some of my informants expressed a desire to be as anonymous as possible. Because of this, I have chosen to not gender the quoted responses from my interviewees, as well as to leave out passages where they describe themselves or their research very explicitly.

Qualitative in-depth interviews are a relevant method of gathering data for this study for several reasons. The main reason lies in the formulation of my principal research question, where I make it clear that it is the *views* of behavioural economists that I am interested in. Where quantitative studies are fit for mapping different volumes and frequencies, data gathered through qualitative studies can be used to analyse and understand the ways humans interpret and organize their social surroundings (Troost, 2010). In line with Pierre Bourdieu (1975, 1991, 2004), this thesis views the production of scientific knowledge as a largely social phenomenon and consequently focuses on social explanations of this phenomenon. Since my intention is to mainly study the social aspects of scientific production, I would argue that qualitative interviews offer me more relevant empirical data to build my analysis from – a quantitative approach would presumably not have given me as clear insights into the perceptions and attitudes of behavioural economists.

In re-reading and structuring my qualitative data, I have attempted to follow the methodological recommendations presented by Jens Rennstam and David Wästerfors (2015). When discussing the processing of qualitative data, Rennstam and Wästerfors (2015) argue that the presentation of qualitative data can be done through categorical and illustrative reduction. *Categorical reduction* refers to the active choice of presenting and discussing specific parts of the sorted qualitative data, meaning “a reduction of the material that (more or less) excludes some parts of it” (Rennstam & Wästerfors, 2015, p. 105, my translation). Having done this initial

reduction, the analyst can do an illustrative reduction of the data, which means looking for excerpts that emphasize certain aspects of the data in order to “illustrate a phenomenon as clearly as possible” (Rennstam & Wästerfors, 2015, p. 112, my translation). The categorical and illustrative reductions I have made in the processing of my data have largely been made in relation to the findings of the previous studies and the theoretical framework I have presented above. Presenting findings that can be compared with those of previous studies enables me to put my relatively small study in relation to the larger context of research concerning the economics discipline, while connecting my theoretical framework to specific parts of my data allows me to answer my research question regarding the usefulness of these perspectives.

As an assessment of my theoretical framework is central to the purpose of this thesis, it is important to further discuss the relationship between my theory and my methodology. Though my methods are not inspired by Bourdieu’s approach to qualitative interviews (see Hamel, 1998), much of the work done in preparation of my interview study was informed by his social theory as well as the styles of reasoning approach. Bourdieu himself utilized his thinking tools to theorize as well as to conduct empirical research, meaning that his social theory can be considered a *meta-theory* intended to assist with research design as well as to prompt the creation of relevant research questions (Hurtado, 2010). Similarly to this understanding of Bourdieu, I would argue that the styles of reasoning approach primarily offers ways of describing the subject matter of this thesis, rather than it being a theory to be validated. As such, my theoretical framework has informed the creation of my research questions and my interview guide, meaning that this study does not conform to, for instance, Howard S. Becker’s (1998, p. 123) principle that the analyst should “let the case define the concept”. Since Becker (1998) argues that deciding upon a theory before conducting research increases the risk of leaving out the data that cannot be understood through that particular perspective, I have actively looked for potential clashes between my data and my theoretical framework in order to decrease the risk of my study excluding theory-incompatible data. This search for clashes is furthermore in line with my purpose to evaluate the usefulness of the theoretical framework utilized in this thesis.

## 4 Results and Analysis

In this section, I present and analyse some findings from my interviews by discussing quotations and excerpts with the help of my theoretical framework. The section is broadly structured into

three subsections that all have a connection to my purpose and my research questions. Outlining first the relationships between behavioural economics and the accepted styles of reasoning in mainstream academic economics, I move on to discuss the scientific habitus of behavioural economists, as well as deliberate on some insights gained from their attitudes towards interdisciplinary research.

## 4.1 The Styles of Reasoning in Academic Economics

When asked about the specificity of the economics discipline, my informants were unanimous: academic economics is defined by its focus on quantitative methods and the creation of models. Speaking with reverence about mathematical modelling, my informants have variously spoken about how “math simplifies your thought”, how “models allow you to see unexpected connections”, and how qualitative data is “not taken into account” in economics. The behavioural economists I have spoken to have all also made reference to the unitary nature of their disciplinary field, generally discussed by way of its standardized textbooks and its mainstream’s belief in the importance of “the top journals”. The self-understanding of behavioural economists regarding their position within the economics discipline seems to be in line with the paradigmatic classifications I have previously presented (see Table 2 and Figure 1), as they generally describe themselves in relation to – rather than in contrast with – mainstream and neoclassical economists. My informants did not express any particular interest in the mainstream–heterodox divide, nor did they articulate any agreement with, for example, Heise’s (2014) notion that the criticism towards academic economics has increased since the 2008 financial crisis. However, by and large, the responses from the behavioural economists I have spoken to can be said to confirm the picture of mainstream economics outlined in the definitions and previous studies presented above.

Drawing from Bourdieu, it is possible to view the economics discipline as a specific field, where mainstream economics can be seen as the field’s dominant core and behavioural economics can be understood as a subfield mostly positioned within this core – Dobusch and Kapeller’s (2012) stylized map (Figure 1) lends itself rather well to a visualization of this understanding of the field of academic economics. Based on a comparison between the qualitative data I have gathered and the existing literature I have presented, it is further possible to assert that the agents in the behavioural economics subfield appear to share the *illusio* of the mainstream economics core, as all agents seem to adhere to a similar *doxa* and *nomos*. In the case of academic economics,

doxa and nomos even appear to be highly interrelated, since, for instance, economists' belief in the superiority of quantitative studies exemplify both the internal presuppositions *and* the criteria of relevance within their field.

An example of when the behavioural economists in my study have given a rather direct insight into the illusion of agents in the field of economics can be found in their tendency to bring up the centrality of *causality* to their discipline. All of my interview subjects have indicated that the search for significant correlations is central to any economics study wishing to be published in a good journal. Several of my informants have – entirely independently of each other – referred to this search for causal links as an “obsession”. One professor furthermore suggested that the importance of causality in economics is one of the contributing reasons for the increase in laboratory experiments (see Table 1) the discipline has seen in recent years:

I [informant]: I think the underlying aspect is that we are hunting for causalities. Any kind of method that tries to be better – or pretends to be better – at doing that is enjoying, at first, a relatively broad acceptance. Behavioural economics has a little bit the appeal, I believe, that it makes social scientists feel like they are doing as serious work as people in natural science.

When asked about the difference between experiments and other forms of quantitative data, another professor stated that the answer was “of course causality”, going on to praise economic experiments for the way in which they allow researchers to “change just one variable” and consequently “make much more firm inferences about whether this variable mattered for the behaviour”.

It seems that in order to become a mainstream style of reasoning in the field of academic economics, the style in question must be quantitative and allow for the generation of causal inferences. The rather reflexive analogy between behavioural economics and the natural sciences in the excerpt above arguably offers some nuance to Fourcade, Ollion, and Algan's (2015, p. 93) assertion that qualitative methodologies “do not square well with economists' formal aspirations”, since the analogy additionally indicates that the approaches acceptable to mainstream economics have as much to do with economists' self-image as they have to do with scientifically motivated rigour. Speaking specifically about the process by which the behavioural and experimental style gained a central position in the economics discipline, one professor focused on the strategies employed in the 1980's and 1990's by early behavioural and experimental economists, and later Nobel laureate, Richard H. Thaler. Focusing specifically on the ways in which Thaler attempted to integrate psychological insights into economics, this professor reasoned like this:

I: So I'm reading Thaler's book *Misbehaving* now, and he was getting so much pushback for his ideas – it's ridiculous, how he had to defend his ideas! [- - -] When you're reading Thaler's book, he's trying to explain some phenomena and people are really trying to come up with, like, traditional explanations to his observations – and they make no sense! That's not very scientific. I found that hilarious. But I guess, I mean someone like Thaler, he managed to show so systematically how there were biases, and how people are boundedly rational, have social preferences, self-control problems... He managed to show it so systematically that people had to update, basically. So, he could show it for individual decision-making, more aggregate outcomes like market outcomes, with different types of models... The fact that he used such a big toolbox, I think helped behavioural economics quite a lot.

Other than the fact that this understanding of rise of behavioural economics is similar to the history and framework put forward by Weber and Dawes (2005), it also exemplifies a specific case of a style-related struggle in the field of academic economics. Though behavioural economics differs from neoclassical economics in its theoretical assumptions, the centrality of causal inference to mainstream economics allowed for the integration of the experimental style, which in turn allowed behavioural economists to demonstrate the validity of their theories.

The previous literature and my theoretical framework have both helped me contextualize my analysis of the mainstream styles of reasoning in academic economics. An interesting aspect of my data is the fact that behavioural economists exhibit a rather full understanding of their position within the field of academic economics, as well as the way in which their subfield has gained this position. Since quantitative processing and causal inference are so central to the pursuit of scientific authority – i.e. social capital – in academic economics, it is perhaps possible to speak of *causality capital* as a form of social gain specific to the field. Viewed like this, I would say that it is further possible to speak of the rise of behavioural economics as an example of the strategic leveraging of field-specific capital in pursuit of a more central and dominant position within a field. The fact that economics journals are perceived to primarily publish articles concerning studies that present statistically significant correlations is a clear example of what is held up as good science in economics, demonstrating what Bourdieu (1975, p. 30) called the “censorship of heretical productions” that journals can exercise in a disciplinary field.

## 4.2 The Scientific Habitus of Behavioural Economists

In the course of my interviews, I found that most of my informants had either received formal training in disciplines other than economics during their undergraduate studies, or they had initially considered applying to educational programmes in other disciplines. Interestingly, very few of them had taken undergraduate courses in psychology – rather, most of them had taken elective courses in mathematics and philosophy as a part of their bachelor and master degrees. Discussing the impact of undergraduate studies on later choices regarding what doctoral programme to apply for and which courses to do within it, one PhD student reasoned like this:

I: I think my reasoning was to find a subject that combined philosophy and mathematics. Economics is a bit mathematical, but also more philosophical than mathematics.

WH: And what brought you to behavioural economics, specifically?

I: Actually my first thought was to do something more mathematical within economics, so maybe some kind of macrofinance or econometrics stuff. But when I began doing PhD-courses I found the behavioural approach appealing. [- - -] It was more philosophical, you know, how it goes deeper into the human part of economics than maybe other parts of economics does. So, apparently I'm more of a philosopher than a mathematician – I thought it was vice versa!

All of my informants have tended to express variations of this conviction that behavioural economics offers more important insights into human behaviour than economics in general, as well as stress the belief that economics research generates more practical knowledge than other forms of social science research. The fact that most of my informants have received training in scientific disciplines other than economics is in line with Hylmö's (2017, p. 20) assertion that the contrarian impulses of heterodox economists often have their "roots in formal or informal training or studies in areas closer to other social sciences". It would appear that early exposure to a variety of different styles of reasoning during the course of academic training impacts the specific scientific habitus of agents, guiding them towards interdisciplinary subfields that may be considered unorthodox or dissenting by the rest of the members of their field. The fact that my informants have, however, expressed confidence in both the importance of behavioural economics and the practicality of general economic knowledge arguably shows that they possess a fundamental belief in the stakes – i.e. *illutio* – of both the field of academic economics and the behavioural economics subfield. This *illutio* correlates with their position within the mainstream core of academic economics.



Though I have stated that the responses from the behavioural economists seem to confirm the image of the economics discipline sketched in my introduction, and although they exhibit a belief in the doxa and nomos of academic economics, my informants have at times demonstrated a rather critical attitude towards the scientific production of their discipline. The professors I have interviewed have, in particular, displayed a high level of self-reflexivity regarding the nature of behavioural economics research. When discussing the methodology and findings of behavioural economists, one professor exemplified a piece of self-critique like this:

I: A huge problem with laboratory experiments in behavioural economics is the fact that so many studies cannot be replicated, that there are different results as soon as the next person comes along and does the same thing. This confirms that we are not really having very robust findings – and the findings that are robust tend to be so general and broad that they are almost part of what your grandmother could tell you as a proverb about how people behave.

This professor went on to describe many findings from behavioural economics research as “unsurprising” and “pretty baseline” insights into human behaviour, while a different professor went even further and suggested that behavioural economists “reinvented the wheel, in a sense”. Though generally displaying a very favourable attitude towards mathematical methods, my informants also expressed some disapproval towards economists who take these tools too far; one of the PhD students noted that “you can prove something very obvious in a very, very, very tedious way with math”.

Utilizing Bourdieu’s thinking tools and the concept of styles of thinking arguably offers multifaceted ways of understanding the internal dynamics of the field of academic economics. Their training in different styles of thinking seems to be a factor that influences behavioural economists towards their subfield, though they also seem to maintain an *illusio* that heterodox economists would presumably not display. The dual positions my informants have taken when discussing their disciplinary subfield also seems to originate in their pluralistic training. As I have, for instance, not interviewed any neoclassical economists, this study is, however, not in a position to say that behavioural economists are trained in other disciplines more frequently than other economists, nor that they are more reflexive and critical about their scientific production than their mainstream colleagues.

### 4.3 Thoughts on Interdisciplinary Research

One unanticipated theme that arose from my informants' responses to the open-ended questions in my interview guide (see Appendix) was their articulation of opinions regarding the nature of interdisciplinary dynamics. When I asked them to agree or disagree with the statement "In general, interdisciplinary knowledge is better than knowledge obtained by a single discipline" (in line with Table 3), all of the behavioural economists I have spoken to agreed. Though the question "Have you ever conducted research together with academics from other disciplines?" was primarily formulated to further allow the behavioural economists to express their attitudes towards other scientific fields, the interview-responses often took the form of long musings regarding potential knowledge-exchange. While the responses to the questions regarding other disciplines were often where my informants talked about the methodological differences between economics and the other social sciences, they also deliberated on how different social science disciplines often have "different names for the same things" and "offer different perspectives" on human behaviour. The response of one professor is exemplary for this kind of dual position-taking:

WH: And then, what are your opinions of the other social sciences? We've touched upon the other social sciences; we've touched a bit on this...

I: Well, I think that one should be... I mean I must be somewhat careful, because I should read much more about... I read now and then some sociology, I read some literature in management, I read some psychology... What I think is – I mean, I think that economics is a little bit ahead when it comes to quantitative testing and quantitative reasoning, and treating data. That I think. But I also think that economics, compared to some other sciences, is a little bit... lags behind when it comes to systematic theoretically generated methods based on induction.

WH: Oh, okay.

I: We don't really have this. So, for instance, when you an interview, an in-depth interview and so on, these are techniques to quickly – as I see it – to quickly learn something about, perhaps, a new phenomenon. I mean we, with our statistical tools, we cannot measure new phenomena because the data is not there. And then I think these methods in other social sciences are more, more suitable – better. But I think that another feature would be that we, in economics, are about more about testing things, like classical positivists, while other social scientists are more descriptive in manner. For instance, psychologists, a lot of their papers are about describing things. And they are quite happy with just describing things, but we would like to test things.

It appears that, although immersed in the mathematical, statistical, and causal styles of reasoning characteristic of the economics field, behavioural economists perhaps have a larger appreciation for the styles of the other social sciences than mainstream economists generally have. The

likelihood of this appreciation could perhaps have been foreseen due to the behavioural economics subfield's historical connection with, in particular, the psychology discipline. One aspect of my data that is, however, particularly enlightening is the difference in the attitudes of the PhD students in comparison to the professors. Though they shared the same general appreciation of the other social sciences with the professors, the PhD students were somewhat anxious towards interdisciplinary research, as they for instance believed that “you can go further in your own discipline” due to not having to make compromises. The professors, who had all, unlike the PhD students, actually conducted research with academics from other fields, displayed a much more positive stance towards the compatibility of different disciplines. The professors specifically noted that they felt very favourably disposed towards knowledge exchange between behavioural economists and researchers in neuropsychology and economic sociology. Regarding the latter subject, one professor specified that they felt more compatible with researchers from the US, as “they are more quantitative than European researchers”.

My theoretical framework again offers interesting ways of describing the results of my study. The way the behavioural economists remain favourably inclined towards other sciences while maintaining a belief in the doxa and nomos of academic economics is in line with their scientific habitus as well as their position within their field. Just as behavioural economists tend to have received training in different styles of reasoning, economists who have conducted interdisciplinary research seem to be more positively inclined towards these types of endeavours. This suggests that the dispositions of the specific scientific habitus require practical experience in order to shift. Though the behavioural economists I have spoken to may be of the opinion that qualitative styles of reasoning are sometimes more suitable, they prefer to utilize the quantitative tools of their discipline. The fact that some of the professors expressed a compatibility with quantitative economic sociologists in the US suggests that styles of reasoning are not necessarily limited to a specific scientific discipline. Rather, styles of reasoning can act as bridges between separate scientific fields.

## 5 Concluding Remarks

Grown out of an aspiration to provide analytical tools for the study of the economics discipline, this thesis has at least succeeded in its attempt to fill a particular place in the sociology of economics. My in-depth interview study has, in collaboration with my presentation of previous

literature on the subject, provided my analysis with ample data on which to utilize the theoretical framework that I set out to evaluate. Below, I offer some concluding remarks concerning my study and make an attempt to provide answers to my research questions.

With regards to my first research question – “How do behavioural economists at Swedish universities view the advancement of their subfield in the economics discipline?” – I believe that I have provided ample contextualization and some new insights into the rise of behavioural economics. Though my interview study focused on professors and PhD students at the economics departments at Lund University and the Stockholm School of Economics, I believe that the data gathered from this small sample of behavioural economists is likely to be relevant to other settings, since my data is by and large in agreement with several other studies. Behavioural economists at Swedish universities, then, understand academic economics to be defined by its methods, which are quantitative, and its aspirations, which largely concern pinning down causalities. The behavioural economists recognizes their subfield to be positioned within the mainstream of economics, and they display an understanding of the way in which the subfield has gained ground that is in line with historical accounts of the process. It seems that behavioural economics has superseded the insularity of the economics discipline by utilizing the possibilities of causal inferences enabled by the experimental method in order to provide proof of the validity of its theories.

Concerning my second research question – “How can a theoretical approach informed by the social theory of Pierre Bourdieu and the concept of styles of reasoning add to the understanding of disciplinary dynamics in academic economics?” – my results are also quite promising. Bourdieu’s thinking tools have in my analysis allowed me to comprehensively describe many disciplinary phenomena that I have found evidence of in my study. Though I have utilized much of Bourdieu’s theory, I believe that his sociology of science could be used much more exhaustively to great effect in the study of disciplinary dynamics. An understanding of doxa, nomos, and the scientific habitus especially assist with the analysis of how the social process of scientific production functions. The styles of reasoning approach offers a simplified way of discussing many disciplinary phenomena, making it possible to outline several important aspects of the sociology of the economics discipline.

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## 7 Appendix: Interview Guide

THEME	QUESTIONS
<b>Background</b>	<ul style="list-style-type: none"> <li>• How did you become interested in economics?</li> <li>• What did your academic training look like?</li> <li>• How did you become interested in behavioural economics, specifically?</li> </ul>
<b>Scientific Production and the Economics Discipline</b>	<ul style="list-style-type: none"> <li>• What is important in science?</li> <li>• What separates good science from bad science?</li> <li>• What are your opinions of the other social sciences?</li> <li>• What particularly sets economics apart from the other social sciences?</li> <li>• Have you ever conducted research together with academics from other disciplines?</li> </ul>
<b>The Mainstream–Heterodox Divide and the 2008 Financial Crisis</b>	<ul style="list-style-type: none"> <li>• What is your perspective on the “mainstream–heterodox”-debate in economics?</li> <li>• Has the debate intensified since the 2008 financial crisis?</li> <li>• Has the debate influenced your view of economics?</li> <li>• Was the failure to predict the crisis indicative of a failure of academic economics?</li> <li>• Is behavioural economics better equipped to explain economic crises?</li> </ul>
<b>The Rise of Behavioural Economics</b>	<ul style="list-style-type: none"> <li>• Why do you think behavioural economics has gained such renown in recent years?</li> <li>• What is the most important aspect that behavioural economics adds to economics?</li> <li>• What major barriers do you think behavioural economics has had to face in gaining ground within economics?</li> <li>• Is the rise of behavioural economics indicative of a larger</li> </ul>

	<p>shift within economics?</p> <ul style="list-style-type: none"> <li>• What is your view on behavioural economists winning the Nobel Prize?</li> <li>• Would you consider behavioural economics “mainstream”?</li> <li>• Why has behavioural economics succeeded in gaining ground when other dissenting theories are still considered “heterodox”?</li> </ul>
<p><b>The Experimental Method</b></p>	<ul style="list-style-type: none"> <li>• What are your thoughts on the difference between experimental methods and other forms of empirical data?</li> <li>• Why has economics only fairly recently turned to experiments?</li> <li>• What is the difference between economic experiments and psychological experiments?</li> <li>• What sets the experimental method apart from other methodologies excluded from economics?</li> <li>• What are your opinions on the mathematical nature of economics?</li> <li>• Which is more important, theory or methodology? Why?</li> <li>• Statement: “In general, interdisciplinary knowledge is better than knowledge obtained by a single discipline”. Agree or disagree?</li> </ul>
<p><b>Conclusion</b></p>	<ul style="list-style-type: none"> <li>• Do you have any questions?</li> <li>• Do you want to make any concluding remarks?</li> </ul>