

Pluralism and social epistemology in economics

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Abstract

Economics plays a significant role in decision-making in contemporary western societies, but its role is increasingly questioned. A recurring topic among the challenges raised by critics is that economics as a discipline lacks sufficient pluralism. That is, it fails to enable, encourage, and respect the use of different ontologies, methodologies, theories, and/or schools of thought to study economic reality. Has this been a productive critique? Does talk about pluralism help identify genuine problems in the discipline? Pluralism in economics could draw support from the current consensus in philosophy that pluralism in science is a good thing. I argue, however, that the claim that economic research is insufficiently pluralist is unlikely to convince economists who believe economics is already pluralist enough and that it does not offer unambiguous recommendations for change. This is because there are too many legitimate ways to interpret how pluralism maps to practice. There are numerous *variables* that pluralist ideals might focus on—the things that they seek multiple rather than one of—and different interpretations of how many of those variables economics has in practice. Yet, as I go on to argue, this does not mean that talk of pluralism is entirely beside the point, since the *reasons* pluralists offer for their ideals do help to identify genuine problems in economics. The social epistemic strategies that arguments for pluralism recommend point us to three concrete issues in the way economic research is organised: gender imbalances, a steep internal hierarchy, and a dismissive attitude to outsiders. I show that economic research could be more progressive, representative of the interests of those in society, accepted, and legitimate and less likely to fall into bias if the discipline alleviated its gender imbalances, if it were less hierarchical, and if it had a healthier relationship with outsiders.

In chapter 1, I outline the debate about pluralism in economics and explain how my thesis utilises a novel approach to social epistemology to offer a way out of the impasse in which that the debate presently resides. In chapter 2, I explain the different philosophical arguments for pluralism in science and categorise them using the variables they focus on and the reasons they give for pluralism. In chapter 3, I argue that interpreting pluralism as a particular arrangement of variables for economics to attain does not lead to unambiguous recommendations for change because it leaves too much open. Yet, I go on to argue, in chapter 4, that drawing on the reasons for pluralism can provide a set of heuristics for piecemeal evaluations of the social epistemic practices in economics. In chapters 5, 6, and 7, I apply these heuristics to economics. I provide evidence that [a] women are outnumbered in economics and face an adverse environment in the discipline, that [b] economics is steeply hierarchical, and that [c] economists form an in-group that assumes superiority and frequently dismisses outside voices. I argue that these three features of economic research block avenues for productive forms of feedback (mechanisms that help to challenge, justify, and refine scientific knowledge), block the interests of certain perspectives being heard, and block public scrutiny of the decisions made by economists.

Preface

This dissertation is the result of my own work and includes nothing which is the outcome of work done in collaboration except as declared in the Preface and specified in the text.

It is not substantially the same as any that I have submitted, or, is being concurrently submitted for a degree or diploma or other qualification at the University of Cambridge or any other University or similar institution except as declared in the Preface and specified in the text. I further state that no substantial part of my dissertation has already been submitted, or, is being concurrently submitted for any such degree, diploma or other qualification at the University of Cambridge or any other University or similar institution except as declared in the Preface and specified in the text

It does not exceed the prescribed word limit for the relevant Degree Committee.

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List of acronyms

AEA - American Economic Association

AER - *American Economic Review* (one of the 'top 5' journals in economics)

ASSA - Allied Social Science Association

CSWEP - Committee on the Status of Women in the Economics Profession

DSGE - Dynamic Stochastic General Equilibrium

ECMA - *Econometrica* (top 5)

EJMR - Economics Job Market Rumors

IGM - Booth Initiative on Global Markets

JEL - *Journal of Economic Literature*

JEP - *Journal of Economic Perspectives*

JPE - *Journal of Political Economy* (top 5)

LSE - the London School of Economics

MIT - Massachusetts Institute of Technology

ReStud - *Review of Economic Studies* (top 5)

QJE - *Quarterly Journal of Economics* (top 5)

1 Why a social epistemology of economics?

Is there anything wrong with the way that economic research is organised? If so, what are the issues?

These are the questions that this thesis seeks to answer.

Since the 2008 Global Financial Crisis economics has repeatedly come under attack. One common criticism is that it is not *pluralist* enough. A popular critique of economics argued, for example, that:

Pluralism is necessary because, by providing different ways of thinking about economics, it forces people to question how they think and brings issues that are often taken for granted back into the sphere of political discussion. (Earle, Moran, & Ward-Perkins, 2016, p. 80)

Although pluralists touch on a range of issues—from economic expertise to specific aspects of economic knowledge—their central target is the organisation of economic research and teaching. Pluralists typically argue that research and teaching in university economics departments fail to utilise an appropriately broad range of ontologies, methodologies, theories, and/or schools of thought. Such criticisms have not gone unnoticed. They have stimulated a notable public debate and a range of rebuttals from economists who disagree that their discipline is overly narrow (see 1.2).

Given that *scientific pluralism* has become popular among philosophers and historians of science as a description of and prescription for all scientific practice, it is reasonable to ask whether scientific pluralism can help determine who is correct in the debate about economics. Focussing on research, rather than teaching,¹ I will use scientific pluralism to ask: are there any issues in the way that economic research is organised? And, if so, have pluralists identified them appropriately?

I will argue that there are indeed issues with how economic research is organised, but that arguments that it is insufficiently pluralist focus on too high a level of abstraction to identify them. Arguments for pluralism typically target an ideal state, in which economics has multiple rather than one of certain *variables* (theories, methods, values, schools of thought, et cetera). But such arguments are unlikely to convince those that think there is sufficient variety in economics or to offer clear prescriptions for change because they leave too much open for interpretation. Where some see one of a given variable (schools of thought, for example), others see many, and pluralism does not offer criteria to rule either way.

Pluralism can, however, still help identify issues in economics. Instead of comparing economics to a particular pluralist state, I will identify three concrete issues with economics and use the underlying *reasons* for pluralism to explain why these issues are problematic: [a] significant gender imbalances

¹ Because it is not completely separate from research, I will also discuss some issues related to teaching, but my main concern will be the organisation of research in economics for two reasons. First, teaching has received much of the attention in the debate about pluralism to date. This is in part because students have played a central role in driving the argument for pluralism (see 1.2). Research in economics deserves equal scrutiny. Second, Davis has argued "the direction of causation for change in economics has been from research to instruction, with content of instruction generally emerging with a lag as a summary or set of summaries of earlier research that has gained general acceptance" (Davis, 2006, pp. 4-5). This makes the debate about pluralism in economic research potentially more consequential than the debate about pluralism in teaching.

within economics limit the range and strength of critical feedback and block the uptake of female interests in the discipline. [b] Economics is overly hierarchical, this creates perverse incentives and a concentration of power that undermines critical feedback and the interests of socially marginalised perspectives being heard in the discipline, as well as limiting avenues for public scrutiny. [c] Economists form an in-group that assumes superiority and frequently dismisses outside voices, which prevents the discipline from taking advantage of productive feedback, limits the range of perspectives it considers, and undermines important conditions for public scrutiny of the discipline.

In this introductory chapter, I will describe three background factors that shape the above argument and highlight why it is important. Firstly, economists and economic research play a significant role in decision-making in contemporary western societies. In 1.1, I will document the numerous important positions that economists have come to occupy and the outsize influence the tools and ideas developed by economists have had on the development of institutions all over the world. These factors make the way that economic research is organised highly consequential and amplifies the issues I will describe in this thesis. Secondly, the debate about pluralism in economics is hotly contested but shows no signs of closure. I describe this debate and the stalemate in which it currently sits in 1.2. By moving from a focus on ideal states of pluralism to concrete social epistemic issues within economics, my argument offers a way out of this stalemate. Thirdly, although social epistemologists have developed a number of tools for evaluating the social features of scientific practice, their focus has been on ideal models of research, translating questions in epistemology from individuals to social bodies, or looking at small cases. In 1.3, I argue that my thesis offers an alternative approach. By starting from existing rather than ideal practices in economics, and by focussing on the discipline as a whole rather than examples within it, I offer an example of how to extend the ideas and tools of social epistemology to new kinds applications (non-ideal, large scale). In 1.4 I outline the structure of the dissertation.

1.1 Economics is important

In this thesis I will investigate a range of questions about the way that research in economics is organised. Given the significant power that economists, economic thought, and economic institutions wield, these questions are not purely academic. They also lie at the heart of a number of debates about the role of economics in society. Some see economics as a unified project that narrows political debates, legitimises neoliberal policies, and undermines social democratic institutions.² Others contend that economics is a patchwork of quantitative social science techniques and ideas that is underappreciated by the public and too frequently ignored by politicians:

Council of Economic Advisers Chairman Murray Weidenbaum, when asked directly what weight of influence, on a scale of one to ten, economists had enjoyed in drafting the original

² See Harvey (2007), Mirowski and Plehwe (2009), Offer and Söderberg (2016), and Offer (2017).

tax program of the [Reagan] administration, replied, 'zero.' (Galbraith, 1988, p. 224; cited Reay, 2012, p. 45)

Because the power economics has in society also plays an important role in what I will say in the rest of this thesis, I will use this section to highlight some aspects of it. I describe two reasons to believe that economics has outsize societal influence, compared to other social sciences, and note how this can be squared with cases in which economists are ignored.

First, economists are demanded and influential in a number of key political roles all over the world. The demand for economists in the United States increased dramatically as a result of the role they played in coordinating the war effort during World War II (Bernstein, 1995). The United States' Council of Economic Advisors was created in 1946, and the number of economists in directly political or important advisory roles has increased ever since (Hirschman & Popp Berman, 2014). Economists are now represented in every part of the executive and legislative branches of the US government and they also control the Federal Reserve (as well as having their own office in the White House).

Bob Coats (2005) summarises a similar growth in employment opportunities for economists in Europe since 1945. Capable economic administration was a condition for many states to receive aid as part of the Marshall plan. The same was true for nations hoping to join the European Community or Union (as it became), and later the monetary union. Even when states did not join these institutions, economists were typically involved extensively in deciding whether to do so or not.³ Moreover, the institutions of the European Community/Union have seen large growth and employed many economists. All of this has increased the demand for economists in policy and in public and private bureaucracy across Europe.

John Markoff and Veronica Montecinos (1993) documented a similar trend in Chile. They argued that economists' methods and universalistic rhetoric (see below) made them technical experts that could lend legitimacy to policy in a wide variety of areas. This legitimising role led to the appointment of economists to powerful roles, particularly in the ministries of finance, in successive Chilean governments (both before and after the 1973 coup). The legitimacy economists brought helped these governments face down internal challenges and satisfy the requirements of international organisations.

The debtor nation may not be able to meet the IMF's goals, but it can at least show its serious participation in the correct ritual by appointing accomplished economists to draw up new plans and excuse the failure of the old ones. (Markoff & Montecinos, 1993, p. 47)

According to Markoff and Montecinos this led to economists replacing lawyers as *the* generalist policy experts.

Similar trends exist elsewhere too. Trond Bergh (1981) points to the displacement of lawyers by economists in Norway, Justin Fox (2014) has noted a similar trend on the US Federal Reserve board, and with Tiago Mata I argue that we are now witnessing a similar rise of behavioural economics trained policy makers (Wright & Mata, Under review). Moreover, economists have come to take up a

³ In Sweden, for example, a commission of economists was set up to evaluate the case for joining the Euro.

significant portion of the influential roles in the International Monetary Fund (Clift, 2018), and Cathrine Holst and Johan Christensen (2018) show that economists have massively increased their representation on Norwegian ad hoc commissions (committees set up to investigate the effects of possible policy and other changes).

Despite all of the social sciences expanding since 1945, the extent to which economists have been employed in politically consequential roles is unique. The White House does not have a Council of Sociological Advisors and Whitehall does not have a Government Sociological Service. Economics is far and away the social science (and possibly academic discipline in general) whose graduates and PhDs are best represented in powerful social positions (Christensen, 2017; Earle et al., 2016; Hirschman & Popp Berman, 2014).

Marion Fourcade (2006) argues that the universalistic rhetoric of economics has played an important role in creating this state of affairs. Many of the tools and techniques of economics abstract away from the messy aspects of social interaction and utilise simplified models of human decision-making. This has made economic concepts, theories, measurements, and knowledge seem ahistorical and completely transferable.⁴ This is highlighted by the fact that economists often point to technical areas as their locus of expertise (econometrics, game theory), rather than specific places or periods. Because of this the policy prescriptions and explanations common in economics often seem universally applicable. The increasing use of economics' universal concepts and measurements in policy and public life snowballed and increased demand for translatable or similar measurements and concepts in spaces in which economic thinking was not yet dominant (Coats, 2005). It also increased the necessity for policy makers and political elites to be able to talk in terms of the universal measurements and concepts of economics. Both of these factors aided the extraordinary growth in influence of economists in the second half of the twentieth and early twenty-first centuries.

In addition to helping further increase the demand for economists, the increasing role of economic concepts, measures, interventions, ideas, and policy tools has reconfigured the world in the image of economics. This is the second aspect of economics' societal significance. Three components of this can be picked out: the effects of economic policy devices and models, the influence of economic ideas, and the expansion of economic styles of reasoning (Hirschman & Popp Berman, 2014).

Firstly, economists have developed a variety of sociotechnical tools that influence the decisions of policymakers and large organisations. These range from the often criticised but nonetheless ubiquitous and important metrics of productivity (Gross National Product and Gross Domestic Product) to the more contingent bill scoring practices in the United States' Congressional Budget Office. Sociotechnical tools from economics proliferate in a variety of specialist domains too. Through the adoption of Quality-Adjusted Life Years economic thinking has become central to many health policy decisions, most notably by the United Kingdom's National Institute for Health and Care Excellence. Through cost-benefit analysis and payment for ecosystems services economic thinking has come to

⁴ For an example of this universalistic rhetoric in public see 'Are the poor different?' in the *Economist* (No author, 1996).

dominate environmental policy decisions and disputes. And, more recently the United States' Office for Information and Regulatory Affairs have mandated that other government offices follow specific information assessment and disclosure mechanisms that are directly inspired by behavioural economics (Wright & Mata, Under review).

The full list of influential tools developed by economists is extensive and includes the idea of 'the economy' itself, the inflation rate, the unemployment rate, and growth (Miller & Rose, 1990; Mitchell, 2002b). As Daniel Hirschman and Elizabeth Popp Berman (2014) note, even though some of these tools are often criticised, once they take hold and are followed by many—more than a hundred countries in the example of Gross Domestic Product—their influence is very difficult to dislodge. Some of these sociotechnical tools are more secure than others, but the power of economics comes from the fact that it is in a position to develop such policy devices, including some that last and shape how policies are viewed for decades. Moreover, once policy devices take hold the moral debates that occur in their development tend to recede into the background (Hirschman & Popp Berman, 2014).

Some sociologists have argued that particular economic tools have even helped create the behaviour they are predicated on (Callon, 1998; MacKenzie, 2006). MacKenzie (2006), for example, notes that as the Black-Scholes-Merton asset pricing model came to be adopted by financial market actors the behaviour predicted in the model became more common. And, Edward Nik-Khah (2008) has documented how the US Federal Communications Commission's (FCC) decision to hire game theorists to design auctions was met with telecommunications companies hiring their own game theorists, resulting in the companies behaving in exactly the ways the FCC's theorists predicted.

Secondly, a number of authors have pointed to the influential role of economic ideas. These include large macro-scale paradigms or ideologies—like Keynesianism or neoliberalism (Amadae, 2015; Blyth, 2002; Mirowski, 2013; Mirowski & Plehwe, 2009)—as well as more specialist policy ideas—like expansionary fiscal contraction (Blyth, 2013; Hirschman & Popp Berman, 2014). Avner Offer and Gabriel Söderberg (2016) argue, for example, that the foundation of the Nobel prize in economics was used to lend authority to anti social democratic economic ideas in Sweden. Elsewhere, Offer (2017) also argues that similar economic ideas were key to the shift from social democratic to individual market-based approaches to lifetime saving in the UK. One needs to be careful not to overplay the deterministic nature of these ideas or the conspiratorial intent behind them. The ideas generated by economists can have varied results and the national contexts in which they become realised can be crucial in determining their actual impact (Fourcade, 2009; Fourcade-Gourinchas & Babb, 2002). Nonetheless, it is fair to conclude that the ideas generated by economists do matter and have mattered.

The third aspect of economics' influence has been its role in shaping the ways topics are viewed and the ways that issues are approached. In the wake of increasing demand for economists post-World War II, a number of significant foundations, organisations, and government bodies adopted the assumption that economics offers a rigorous analysis of the social world and proceeded to introduce economic methods into policy analysis and recommendations. Methods and modes of analysis

originating in economics expanded across policymaking, from military strategy to public administration (Amadae, 2003; Amadae, 2015). Hirschman and Popp Berman use Ian Hacking (1992) to argue that economic methods and modes of analysis define a particular style of reasoning that:

[I]ncludes basic concepts such as incentives, growth, efficiency and externalities. It includes economic ways of approaching problems: by using models, systematically weighing costs and benefits, analysing quantitative empirical data, considering incentives, and thinking marginally. It suggests causal policy stories... linked to economic theories... [and] makes certain methodological assumptions: about the importance of quantification and the possibility of using monetary value as a means of commensuration, for example. (Hirschman & Popp Berman, 2014, p. 794)

Even if the idea that economics has a consistent or separate style of reasoning is rejected⁵, it is clear that aspects of economic thinking—from cost-benefit analysis, to econometrics, to the centrality of efficiency—are significantly prevalent in law, business, and policy schools (Lazear, 2000). This has resulted in a whole generation of policymakers, business managers, and lawyers leaning on the methods, theories, and tools developed in economics.

In summary, economists have been and continue to be employed in numerous significant policy positions, with the institutional roles for them expanding since 1945; and, through the tools they create, the proliferation of their ideas, and an expansion of economic ways of approaching problems, economists recreate the world in their own image. These points can be summarised by saying that economics is powerful, and given that these points cannot be attributed to sociology, anthropology, or political science, more so than other social sciences.

In contrast to this, many economists feel that their policy influence is limited. They point to events like Brexit or the pursuit of austerity throughout the European Union in the wake of the Global Financial Crisis, both things that economists almost unanimously advised against, as evidence that politicians and populations frequently reject their advice. Hirschman and Popp Berman (2014) argue that this contrast can be squared by close attention to the different mechanisms through which economics and economists can be influential. Economists that decry their lack of influence focus on different kinds of questions and situations to those that see unassailable dominance. It is undoubtedly true that economists are not always listened to and they might be particularly frustrated when giving specific policy recommendations. But they are often successful in gaining influence in situations that are not directly in the public eye, or in areas thought to be technical or ill-defined (for example, in situations of crisis and in framing issues early on in the policy process⁶). Moreover, their influence has been significant in defining the boundaries of topics, in creating tools for policy analysis, and in shaping the space of policy ideas. If their access to positions of power and standing within academic hierarchies are also considered (see chapter 7), it is hard to argue that economists are undervalued even though some significant pieces of their advice are rejected.

⁵ As those that argue that economics is more varied than critics give it credit for might do (Coyle, 2018; Oliver, 2018; Smith, 2018). I will return to this in chapters 2 and 3.

⁶ Examples of framing might include economists in government presenting an, as yet, undefined issue to politicians in terms of a cost-benefit calculation.

The powerful role economics plays in society makes the organisation of research in the discipline a critical case for philosophers of science interested in the social dimensions of science to study. Given economics' societal significance, whether or not it is beholden to particular interests or adequately scrutinised are societally significant issues. The power economics wields amplifies and reinforces the three issues with the way that economic research is organised that I will point to later in this thesis—gender imbalances, hierarchy, dismissive of outsiders. The significance of economics in policy making, for example, makes the poor representation of women within the discipline more problematic than it would be in other disciplines (see chapter 5).

1.2 Pleas for pluralism in economics

In part because of the role of economics in shaping many facets of political life, the 2008 Global Financial Crisis was interpreted by many as a failure the discipline. The crisis resulted in a raft of public criticism of the research, teaching, and advice of economists. A common criticism from commentators in the United States and Europe has been that economics is insufficiently *pluralist*. In 2014, the International Student Initiative for Pluralism in Economics, for example, argued that:

[The] lack of intellectual diversity [in economics] does not only restrain education and research. It limits our ability to contend with the multidimensional challenges of the 21st century. ... Pluralism will not *only* help to enrich teaching and research and reinvigorate the discipline. More than this, pluralism carries the promise of bringing economics back into the service of society. (International Student Initiative for Pluralism in Economics, 2014)

Despite the recent re-rise to prominence, calls for pluralism in economics are not actually new. The Foundation for European Economic Development, for example, funded the placement of “A Plea for a Pluralistic and Rigorous Economics” as a full-page advert in the *American Economic Review* back in 1992.⁷ Calls for pluralism are also not uniform. They have come from a variety of sources—students (International Student Initiative for Pluralism in Economics, 2014; Post-Crash Economics Society, 2014), policy makers (Haldane, 2015a; Haldane, 2015b), journalists (Elliott, 2017; Halligan, 2013; Halligan, 2018), historians (Davis, 2014b; Morgan & Rutherford, 1998), and economists themselves (Chang, 2014; Foundation for European Economic Development, 1992; Netzwerk Plurale Ökonomik, 2016)—and they understand pluralism in a variety of ways.

The post-2008 calls for pluralism have resulted in some changes.⁸ But many of the pluralists' demands have been met with annoyance and rejected as misunderstood. Many economists have argued that their discipline already contains many different approaches, theories, and models (Coyle, 2018; Deaton, 2011; Rodrik, 2015). A cycle of claim and counter claim has played out in full public view

⁷ Also see Wade Hands (1997), who argues that calls for various forms of pluralism were frequent throughout the twentieth century.

⁸ Take, for example, the development of a new ‘pluralistic’ ‘CORE’ undergraduate curriculum (Bowles, 2018).

through the pages of the *Guardian*, *Financial Times*, *Economist*, *Telegraph*, and *New York Times*.⁹ This debate shows no signs of closure, frustrating both critics and defenders of present practices.

In this thesis I offer a way out of this debate by changing the focus of pluralism. Instead of interpreting pluralism as a particular state (a given arrangement of certain variables, which can be methods, theories, schools of thought, et cetera) for economics to attain I will use the arguments for pluralism to point to three concrete social issues in how economic research is organised. To understand the context of this argument I will use this section to outline the debate in economics. I will describe four arguments for pluralism in economics—those coming from Tony Lawson, Post Keynesian economists, Ha-Joon Chang, and student critics of economics. I will note the different aspects of economics they focus on, outline the fraught back and forth between critics and defenders of economics, and point to an impasse: how can what counts as sufficient pluralism in practice be judged?

A number of critics have argued that the nature of economics' subject matter makes pluralism necessary. Tony Lawson (1997; 2003), for example, has argued that mathematical modelling is inherently unsuitable for the social world because modelling requires a base of consistent event regularities (x reliably following y); regularities that he thinks are rare in the social world. According to Lawson, the social world contains multiple levels of analysis which each require different ontologies (understood as classifications of what exists) and different methodologies (none of which involve mathematical modelling). To study the social world, economics, thus, needs a pluralism of ontologies and methodologies. Lawson argues that the rejection of mathematical modelling is likely to lead to just such a state. This is because he thinks that it is adherence to mathematical modelling that restricts the levels of analysis that economics investigates. Once modelling is out the way economists will be freer to analyse features of the world that do not require the assumption of consistent event regularities (Lawson, 2009).

A number of self-defined *Post Keynesian* economists have similarly argued that the nature of economics' subject matter should imply pluralism. Philip Arestis (1996) and Sheila Dow (1990) have argued that the "economic system" is an intersection of multiple social systems, each with their own dynamics, and that our knowledge of it can only ever be incomplete and uncertain. The study of such a system, therefore, requires analysis using a variety of approaches.¹⁰ Rather than focussing on ontologies and methodologies, Post Keynesians typically couch this variety in terms of 'schools of thought'—understood as a mixture of community association and shared theoretical, methodological, and ontological commitments. Historian and Post Keynesian Fred Lee (2009) has argued that Post Keynesianism's own community makeup exhibits a good archetype for the required variety. Lee argues that, although it started as a set of theoretical commitments, Post Keynesian economics developed into a community of economists from a variety of organisations and social networks united by critical stances to what they saw as the dominant school of thought in economics: 'neoclassical'

⁹ See, for examples, Oliver (2018), Bowles (2018; 2016), Halligan (2013), and Elliott (2017).

¹⁰ Norgaard (1989) also makes a similar argument.

economics.¹¹ This led the Post Keynesian community to be populated by economists with different ideas—drawing from Keynesianism, but also from other areas like institutionalist economics—but overlapping topics of inquiry.¹² Both Lee and Arestis (1996) argue that this mixed community composition has led to fruitful developments and lines of inquiry. Thus, because of the way that the economic system is and because variety can help develop research, economics should aim for a pluralism consisting of the interaction of multiple schools of thought.

Ha-Joon Chang (2014) also argues for a pluralism of schools of thought. He starts by arguing that economics should be defined around the study of ‘the economy’. Unlike Lawson and the Post Keynesians, H.-J. Chang does not think that the dominant neoclassical school is fundamentally flawed, just that it is limited. Neoclassical tools cannot address all the important issues that arise in analysing the economy, and the same goes for other schools of thought. H.-J. Chang outlines nine important schools of economic thought and discusses the benefits they each bring to the analysis of the economy in different contexts.¹³

[No school of thought] can claim superiority over others and still less a monopoly over truth.... All theories, including natural sciences like physics, necessarily involve abstraction and thus cannot capture every aspect of the complexity of the real world. This means that no theory is good at explaining everything. (Chang, 2014, p. 111)

H.-J. Chang argues that epistemic limitations involved in studying humans and of studying a varied entity like ‘the economy’ mean that it makes sense to utilise multiple approaches. Economists and policy makers can never know which approach will produce the most effective interventions and different approaches are likely to suit different contexts. Thus, rather than running with any one school of thought, economists and policy makers should draw on a variety of schools, choosing between them in different contexts. For this to work the discipline of economics needs to keep a variety of schools actively alive and ready for use (rather than simply treating them as objects of historical study). Although this conclusion is similar to the Post Keynesians’, H.-J. Chang’s argument is subtly different to the two above. Rather than arguing that certain schools of thought are required for different levels of description, H.-J. Chang is simply arguing that it should not be assumed that any one school will be useful for all economic problems. His argument comes from the perspective of use rather than

¹¹ Colander (2000) traces the term ‘neoclassical economics’ back to Thorstein Veblen (1900), who used it as a way of attacking Alfred Marshall’s synthesis of the marginalism of Jevons and Menger with broader themes in classical economics (from Smith, Ricardo, and Mill). Hicks and Stigler extended the term neoclassical to include other marginalists (not just Marshall), resulting in it referring to a combination of marginal thinking, relative prices, and the use of calculus. But others have also used the term differently. John Maynard Keynes (1936) lumped the classicals into the neoclassical framework to contrast them with his own thinking. When parts of Keynes’s own work were synthesised with marginalism, those parts became known as the neoclassical synthesis of Keynes, in contrast to the Keynesians who developed his work in a different way (eventually becoming the Post Keynesians). This brief history shows that the term neoclassical is difficult to pin down. For this reason, I will avoid the term except when referring to others that use it. In the case of the neoclassicism Post Keynesians marshal themselves against, there is not one clear definition. Hicks and Stigler’s understanding comes closest.

¹² Lee uses publication and professional membership patterns to support this claim by showing that Post Keynesian events and publications involve a number of economists that also self-identify with other non-neoclassical schools of thought. See Lee (2009, pp. 91-95) for the details of this argument.

¹³ H.-J. Chang’s nine schools are: Austrian, behavioural, classical, developmentalist, institutionalist, Keynesian, Marxist, neoclassical, and Schumpeterian.

metaphysics.

A similar pragmatic motivation is found in the arguments of university students, who have also taken a central role in debates about current practices in economics.¹⁴ On numerous occasions it has been student action and protest that has stimulated debate in newspaper comment pages. Despite differences between activist groups, the views of the student campaigns can be characterised. Student groups share the belief that economics degrees (both undergraduate and graduate, but with emphasis on the former) do not provide students with the tools they need to understand economic events (with the 2008 Global Financial Crisis looming large). Student critics typically point to the large number of repetitive models they are taught during their degrees, the lack of problem directed education they receive, and the micro, macro, metrics-based focus of their education as the source of the problem. Student critics typically conclude that teaching in economics should be changed in four ways: to be more application focussed, to include more history of economic thought, to be more interdisciplinary, and to include a wider variety of schools of thought. They argue that these four changes would amount to a pluralism of pedagogy.

Pluralism in economics has, thus, been argued for on the grounds that it offers the most suitable way to study economics given the nature of its subject matter (Lawson; Post Keynesians), and given the uncertainty and context dependence involved in the problems economics should be directed to solve (H.-J. Chang). It has been argued for as the most effective way to organise a community of inquirers (Post Keynesians). It has been argued for as a way of ensuring that economics provides the tools policy makers and students need to understand and intervene on economic processes (students; also see Turner, 2012). Finally, it has been argued that it is required to provide space for new forms of thinking (Lawson). These arguments have been used to prescribe pluralisms of ontologies and methodologies (Lawson); of schools of thought (interacting with each other—Post Keynesians; to draw on for problem solving—H.-J. Chang); and for pedagogy (students).

Many of those that defend against the need for pluralism in economics have countered these arguments by listing the range of things that economists do. Tim Harford, for example, lists auction theory, externalities, national accounts, measurements of growth and inequality, field experiments for policy experimentation, and a number of other tools developed by economics that have been useful for policy (Oliver, 2018). Noah Smith (2017; 2018) makes a similar argument about the range within economics and also attacks the idea that economics fails to deal with the ‘real world’. He argues that empirical work in economics, like quasi-experimental work, is becoming more policy relevant and less reliant on unrealistic assumptions, and that work driven by real world data is becoming more

¹⁴ See the student-written reports from Manchester (Post-Crash Economics Society, 2014), Cambridge (full disclosure: I was one of the authors of this report, Cambridge Society for Economic Pluralism, 2014), and more recently Durham (Svenlén, Sargent, Tyler, & Pedersen, 2018) for examples of student positions. I focus on students in the United Kingdom, but student movements exist in other countries too (for examples, see the list of signatories on the open letter from the International Student Initiative for Pluralism in Economics, 2014).

common.¹⁵ Moreover, he argues that contemporary economists study a much wider variety of topics than their predecessors—“gender relations in the workplace, racial gaps, changes in labor contracts, early childhood education, minimum-wage policy, regional opportunity gaps, automation and the future of jobs, and a vast array of other highly important, immediately relevant topics” (Smith, 2018).

Three points are implied by Smith’s and Harford’s arguments. The subject matter of economics is more varied than critics give it credit for. The domain economics seeks to understand may be complicated and difficult to get a handle on, but economists do, nonetheless, engage with it; they are not just the abstract model builders they are made out to be. And, by engaging with real world data economics has managed to devise useful and important interventions. If correct, these points undermine the pluralists’ arguments that the present practices in economics cannot deal with the complex nature of economics’ subject matter, and that economics does not provide the necessary tools to understand and intervene in the economic domain.

Critics are, however, likely to dispute the significance of Harford’s and Smith’s lists. Such an argument might follow four lines. First, critics might argue that, although useful, the tools and techniques listed by Harford and Smith don’t provide what is required. None of the techniques, for example, explain the causes of financial crises or how their ills can be mitigated. Second, critics might argue that Harford’s and Smith’s lists only provide anecdotal evidence of what is going on in economics. The useful applied tools listed are not representative of what most economists do. Third, and relatedly, critics could argue that Harford’s and Smith’s lists neglect the counterfactual (to use a trope often heard in economics). Maybe economics has started to develop some useful tools, but it could be doing so much more if only it were more pluralist. Lastly, critics might accept Harford’s and Smith’s arguments but argue that they all come from a single approach to economics and that the other reasons pluralism is needed—as the right way to organise a community of inquirers, and to provide space for new thinking—are still valid.¹⁶

Other defenders of economics have argued head on against the claim in this last response, assumed by most of the arguments for pluralism, that economics is dominated by a single approach. Diane Coyle (2018) makes just such an argument. Responding to a recent attack by Howard Reed (2018), Coyle rejects the idea that everything economists do is influenced by one theoretical framework—defined by Reed as the combination of individualism, profit maximisation, and rationality. She argues that economists employ all sorts of approaches to tackle all sorts of problems, many of which explicitly reject parts of the framework that Reed claims is dominant. As examples, Coyle lists work from complexity on environmentally beneficial innovation, and empirical work on how railroads affected the development of Indian districts.¹⁷ Coyle argues that things like complexity

¹⁵ The idea that economics was all theory and is now more empirical is common and backed up by some evidence (Hamermesh, 2013). Roger Backhouse and Beatrice Cherrier (2017) have, however, argued that this is more a question of significance than quantity. They argue that empirical economics gets more attention in the big journals now, but that there is no more of it overall.

¹⁶ In addition to these four responses, critics might argue that the tools Harford and Smith list are not useful; maybe because they are mathematical models unsuited to examining the social world. Because such an argument leads directly to an almost impassable disagreement, I leave it to one side.

¹⁷ Both of which reject the combination of individualism, profit maximisation, and rationality.

and empirical economics (as well as behavioural economics and game theory) have changed the discipline so that it already offers a variety of approaches that can be applied to a variety of problems and towards a variety of contextual and political goals. Thus, something like a pluralism of schools of thought, goals, ontologies, and methods already exists in economics. Coyle does accept that student pluralists may have a point that economics curriculums require change. But, she argues, there are changes afoot (see the ‘CORE’ curriculum cited above, Bowles, 2018), and it would be unfair to judge the research in economics based on the flaws in its textbooks.

Critics might respond in three further ways. They might argue, first, that as with Harford’s and Smith’s lists, Coyle’s argument only provides anecdotal evidence. Second, they might add that even if economics is genuinely populated by a number of new approaches, this need not imply that the models common in economic textbooks do not continue to play a dominant role in the way economists think. Many empirical papers that defenders of economics point to, for example, contain important sections—normally the sections suggesting explanations for the results found—that draw on theoretical constructs from textbook models (utility maximisation, human capital, et cetera). Moreover, in 2005 David Colander found that 94% of (American) economics graduate students agreed that “the neoclassical assumption of rational behaviour” is important (2005, p. 188). Third, critics might argue that, although the new approaches listed by Coyle differ from the caricature of economics painted by Reed, they are just variations of what is still fundamentally a unitary approach. Esther-Mirjam Sent (2004) argues, for example, that the behavioural economics that has been incorporated into economic practice is intentionally less threatening to textbook forms of economics than earlier iterations. She argues that the behavioural economics of recent Nobel prize winners Richard Thaler, Robert Shiller, and Daniel Kahneman always had ambitions to become part of the ‘mainstream’¹⁸, rather than offer a separate framework in the way that the behavioural economics of another Nobel prize winner, Herbert Simon, did.

Coyle is likely to deny all three of these points, but it is not clear how to measure who is right. It is plausible that the education of economists does indeed impact how they interpret their empirical results, but Coyle might be correct that there is also a lot more to economics than what is in the textbooks. Very few people deny that the methods within economics have changed in the last thirty years, but it is not clear if this has led to a proliferation of genuinely new approaches, or the evolution of one dominant approach. We reach an impasse. How do we judge between a discipline containing some different methods and approaches within a unitary environment, and a discipline that exhibits a genuine plurality of different approaches?

John B. Davis (2006) attempts to answer to this question. He argues that in the 1980s economics began importing a number of new ideas and techniques from neighbouring fields. Game theoretical, behavioural, and experimental approaches began to be used in economics (coming from mathematics, psychology, and the medical and biological sciences respectively). As the significance of these new

¹⁸ I borrow Sent’s terminology here. Throughout I interpret ‘mainstream’ to mean whatever is published in the top 20, and particularly the top 5, economics journals. See chapter 6 for a discussion of the importance of the top 5 journals in economics.

approaches within economics gradually grew, they were also joined by ideas from Darwinian biology, neuroscience, and physics and computer science (leading to evolutionary economics, neuroeconomics, and complexity theory respectively). Davis argues that because these new approaches came from outside economics, they should be considered genuinely new and different to what was on offer in the 1970s. This is because when things are imported from other fields, they are likely to come from conceptual and theoretical foundations different to those in the importing field. For the same reason, the imported ideas should also be considered genuinely different from each other. Davis adds that because these new approaches make up a significant portion of what recent cohorts to economics PhDs work on, economics looks to be developing in a pluralistic direction.

However, the fact that ideas come from different fields is not sufficient to guarantee different theoretical and conceptual foundations for two reasons. First, different fields might use approaches that develop in parallel from similar foundations. It might be argued, for example, that game theoretic approaches have the same roots as the mathematics used in economics prior to the 1980s. Even though game theoretic scenarios differ from the scenarios of consumer choice, they both draw from mathematical theories of optimisation. Fixed point theorems (both Kakutani's, 1941, and Brouwer's¹⁹) are, for example, an important component in the proofs of both game theoretical Nash equilibria (Nash, 1951; Nash, 1950) and the more traditional Arrow-Debreu models of general equilibria (Arrow & Debreu, 1954). A critic might thus argue that the uptake of game theory is an update to the basic mathematical optimisation tools used by economists, which allows them to analyse new scenarios, rather than a new approach.

Second, even where ideas come from fields that have genuinely different foundations, the actual ideas imported might be selected to fit with the foundations of the importing field. Critics might note, for example, that the aspects of psychology used in behavioural economics are still fundamentally about the optimisation of individual choice (Colander, 2000, p. 356; Rabin, 2002). The possibility that selection bias plays a role in what gets imported is evidenced by what has not been imported. Other ways of doing economics that might offer more radical departures from economics as it was practiced in the 1970s—the schools of thought critics would like to see more of: Marxian economics, ecological economics, Post Keynesian economics, et cetera—are notably absent in the new developments Davis points to. Moreover, citation analysis shows that the highest ranked journals in economics largely ignore contributions from some of these other fields (Cronin, 2010; Kapeller, 2010; Dobusch & Kapeller, 2012).

This debate could run and run. As Davis (2014a, p. 477) later points out, those that defend the ideal of pluralism and argue that economics does not live up to it typically want to see more 'heterodox' schools of thought within economics, and those that either ignore pluralism or defend

¹⁹ Brouwer was not the only person to prove this theorem, but his name stuck. He first proved the theorem in German in Brouwer (1911). I first encountered it in Franklin (1980).

economics against it typically do so to secure the authority of mainstream theory.²⁰ Those who are sympathetic to the current dominant methods of economics see it as pluralist enough, and those that are more critical of current practices typically see a lack of pluralism (Dutt, 2014). Thus, claims that economics is or is not pluralist seem to be driven as much by agenda as by clear principled differences. How should we judge who is correct; does economics need more pluralism or not?

Independently of the debate in economics, pluralism has developed into a popular interpretation of and recommendation for all scientific practice. Historians and philosophers of science have responded to early and mid-twentieth century calls for unity in science by pointing to multiple ways in which science is inherently pluralist, and scientific pluralism has become a popular prescription for scientific practice. In chapters 2-4 I will attempt to make headway into the debate about pluralism in economics by using philosophical accounts of pluralism to tackle four key questions: [1] What does/should pluralism entail? [2] Why is pluralism desirable? [3] Is economics pluralist? And, [4] what should be inferred from pluralism for practice in economics?

In chapter 2, I will answer [1] and [2] by explaining the different versions of scientific pluralism that philosophers have offered. I will categorise the plurality of pluralisms available using the *variables* they argue should be multiplied and the *reasons* they give for pluralism. In chapter 3, I will tackle [3]. I will argue that the range of variables that pluralism can focus on, and the fact that it is difficult to determine how much of each variable is required for pluralism and how much of each variable a discipline has in practice, makes it hard to answer [3] either way. This means that arguments that economics should be more pluralist, which typically claim that economic research does not match a particular arrangement of certain variables, are unlikely to convince those that think economics is pluralist enough and do not offer unambiguous recommendations for change. The indeterminacy of plural variables makes it hard to conclude the debate either way.

1.3 Social epistemology

Rather than comparing economics to states of pluralism I will evaluate some important existing social epistemic practices within economics. I understand *social epistemic practices* as the social arrangements and practices within a given scientific community that bear on knowledge acquisition, retention, assessment, and transmission.²¹ The social arrangements of a scientific community include the community makeup (who is involved) and institutional features of that community (how does one become a member, how is the community governed, et cetera). Examples of social arrangements are

²⁰ Like ‘neoclassical’, ‘heterodox’ can be defined in many different ways. For this reason, I will avoid the term except where others use it. In this case, I take Davis to imply the schools of thought that have typically not been prominent in economics text books and the big economics journals. That is, Marxian, Post Keynesian, ecological, evolutionary, Austrian, Institutional, and Schumpeterian schools of thought.

²¹ This is based on Goldberg’s definition of social epistemology as “the study of the social dimensions of knowledge acquisition, retention, transmission, and assessment.” (Goldberg, 2018, p. 1)

the kinds of people that are members of an experimental laboratory and how decision-making capacities are distributed in such a lab. Social practices are the ways that the inputs from more than one individual are related to one another and combined. These include norms of interaction, forms of aggregation, and mechanisms for assessment. Put more generally, social epistemic practices can be any way of arranging interaction between one or more people that affects the knowledge they produce.

Although I will argue that ideal accounts of pluralism cannot resolve the debate about pluralism in economics, pluralism can still be useful. There is a more concrete way to articulate many of the concerns that economic pluralists have. Instead of comparing economics to an ideal arrangement of certain *variables*, I will use social epistemic practices recommended by the *reasons* for pluralism to point to concrete issues in the way that economic research is currently organised. I make this argument in two steps.

First, in chapter 4, I explain how the reasons for pluralism can be used to outline three *heuristics* for judging social epistemic practices. I use arguments that pluralism assuages some *epistemic limitations* involved in science to argue that social epistemic practices can be evaluated by looking out for ways that such practices block productive forms of *feedback* (**H1**). Feedback that can come in the form of empirical testing, practical applications, modelling, or social criticism, and which helps challenge, justify, and refine scientific knowledge and practices. I use arguments that pluralism is important to make scientific practice compatible with *multiple goals* to argue that social epistemic practices can be evaluated by looking out for ways that such practices block particular *interests* from being considered in decisions about which goals scientific communities should pursue (**H2**), and ways that such practices block public *scrutiny* of those decisions (**H3**). These heuristics are my answer to questions [4] from the last section (1.2). Rather than defining ideals, **H1-H3** offer important considerations for evaluating existing social epistemic practices in economics.

In chapters 5-7, I apply **H1-H3** to a raft of empirical data social scientists have collected on how economic research is presently organised. This is my second step. In chapter 5, I will present evidence that economics has a very low ratio of women to men (particularly higher up the discipline) and that female economists face an adverse environment in the discipline. I argue that this blocks the feedback knowledge and practices undergo within economics, and that it also means that the interests of a particular segment of society (women) often fail to be considered in determining the direction of economics.

In chapter 6, I use a range of evidence to argue that economics is overly hierarchical, and more so than other social sciences. I argue that the extent of the hierarchy in economics reduces avenues for critical feedback in the discipline by disincentivising innovation and communication with outsiders and causing a concentration of power that constrains the development of new ideas and criticism. It also disincentivises mechanisms that help make economic research consider a broad range of interests, and makes public scrutiny more difficult by constraining criticism, encouraging consensus, and disincentivising outside communication.

Lastly, in chapter 7, I present evidence that economists form an in-group that assumes superiority,

forms barriers to the outside, and dismisses research and criticism from non-economists. This marginalises potential avenues for feedback, reduces the opportunities for those not well represented in economics to have their interests heard in the discipline, and makes public scrutiny of the discipline more difficult by lowering the prevalence of public debate.

By shifting from pluralism as an ideal to the evaluation of existing social epistemic practices in economics my thesis highlights some of the issues that pluralists complain about (a lack of diversity, hierarchy, a lack of interdisciplinarity) without identifying an ideal way of organising economic research. By focussing on the reasons for pluralism and the social epistemic practices they recommend, rather than ideal pluralist states, my argument, thus, offers a new way of making progress in the debate about pluralism in economics. Moreover, by doing *non-ideal normative* analysis of *existing social* features of economic research *as a whole*, I offer a novel way to apply social epistemology to scientific practice. In doing so I speak to gaps in social epistemology, economics, and sociology, history, and philosophy of economics. I describe how below.

Many social scientists have studied the social features of economic research. Those social scientists have, however, not discussed why the features they comment on are problematic. *Normative* commentary is either completely absent or the assumption is that what is described is obviously problematic.²²

Social epistemology does often do normative analysis. Social epistemologists have built sophisticated tools for investigating and evaluating the social features of knowledge and how social relations impact science. They, thus, seem well placed to comment on whether any social aspects of economic research ought to be changed. However, social epistemology has typically focussed on idealised models (Goldman, 2001a; Kitcher, 1993; Strevens, 2003; Zollman, 2007; Zollman, 2010) or ideal accounts (Kitcher, 2001; Kitcher, 2011; Wilholt, 2009) of how scientists interact, rather than starting from specific cases in which they do.

Social epistemologists who use idealised models of interaction investigate different ways of organising scientific communities formally, usually using agent-based models drawing from game theory and network theory.²³ There is a lot of interesting work in this area, and I draw on some of it in chapter 6. But, because formal models of scientific communities are difficult to apply to practice, I take a different approach.²⁴ Rather than comparing different idealised community structures, I seek to do normative analysis of the *existing* features of economic research.

²² By invoking the word ‘normative’ I do not mean to contrast normative analysis with empirically founded (‘positive’) work. By ‘normative’ analysis I mean analysis that recommends particular norms or outcomes. Such analysis can be empirically grounded while also making claims about how research should be socially structured and why.

²³ Kevin Zollman’s (2007; 2010) work has been particularly significant in developing formal approaches to social epistemology. For other important formal studies, see Mayo-Wilson, Zollman, and Danks (2013), Holman and Bruner (2015), Rosenstock, Bruner, and O’Connor (2016), Rubin and O’Connor (2018), Thoma (2015), Bright, Malinsky, and Thompson (2015), and Bright (2017).

²⁴ The epistemic value of these formal models is also unclear (Frey & Šešelja, 2018). See also Alexandrova and Northcott (2013), Reiss (2012), and other critics of formal modelling in economics.

Social epistemologists who offer ideal accounts of scientific communities dictate ideal institutions or norms for interaction. Such accounts have similarities with what has become known as ideal political theory. By contrast, my aim is to suggest improvements to the present institutions and interactions in economics. My approach to social epistemology, therefore, mirrors that of *non-ideal* political theorists who focus on transitional improvements to present political circumstances, rather than theorising about perfect societal end-states.²⁵ John Rawls (1999) argued that both transitional and end-states have a role to play in normative analysis, but that end-states must be first determined in order to identify transitional-states. For Rawls, non-ideal theory involves reasoning about how society should get to the end-states determined by ideal theory. Amartya Sen (2006), in contrast, rejects the priority of end-states. He argues that justice improvements from a given position can be determined independently of determining what counts as a perfect world—to know whether mountain X or Y is higher we do not need to know the height of the tallest mountain in the world. Furthermore, an end-state could not guide evaluation of transitional-states without also defining a way of measuring how far potential transitional-states are from the ideal end-state.

Sen thinks that political theorists should focus on determining transitional improvements to existing circumstances without worrying about optimum end points. Given that he is surely correct that ideal theory is neither necessary nor sufficient for non-ideal theory, and that many ideal theorists accept as much (Valentini, 2012), I side with Sen over Rawls, but with one caveat. The lack of necessity or sufficiency of ideal for non-ideal theory does not imply that thinking abstractly about ideal social arrangements is useless. The reasons for particular ideals highlight features of political systems that are desirable, which can then be used as heuristics for evaluating transitional-states. G. A. Cohen's (2008; 2011) ideal discussions of justice might be used, for example, to say that other things being equal more equality (of opportunity, rights, income, or welfare) is a good thing.

Thus, although my approach to social epistemology will mirror Sen's non-ideal political theory, I will also utilise some of the arguments of those who develop social epistemic ideals (in particular those from Longino, 2002, and Kitcher, 2001) and arguments for pluralism (which might also be interpreted as a social epistemic ideal). I will draw on existing features of economic research and point out issues with and potential improvements to them, rather than ideals to aim at, but utilise the reasons that certain ideals seem desirable in doing so.

Where social epistemologists do start from existing practices and relations, they typically draw on small examples of social interaction and case studies of particular disputes or developments. I strike out a different path. I will use ideas from social epistemology to comment on three features of economic research *as a whole*. That is, I will comment on three features of economic research that are visible in data on the discipline as a whole, rather than in small examples or studies of parts of the discipline. Economics has significant gender imbalances and offers an unfriendly environment for

²⁵ In an overview, Laura Valentini (2012) notes two other interpretations of the ideal/non-ideal distinction: theorising about situations in which everybody fully complies with the demands of justice vs. theorising about situations in which they partially comply; and, theorising about 'utopian' accounts of justice vs. theorising about 'realistic' accounts of justice. See Robeyns (2008) for another overview of the ideal vs. non-ideal debate. See Rawls (1971) and Cohen (2008) for two classic understandings of the value of ideal political theory.

women, is overly hierarchical, and is dismissive of outsiders. In chapters 5-7, I discuss each of these features in turn; I use data collected by sociologists, historians, and economists to describe them, and use my three heuristics (**H1-H3**) to explain why they are problematic. By describing why the specific gender imbalances, hierarchy, and dismissive attitude to outsiders exhibited in economics are problematic I link the empirical work of social scientists who study economics to theories in the philosophy of science and political philosophy and make it easier to see how these problems might be remedied.

Despite intense debate among social scientists and those outside of academia, philosophers have been surprisingly mute on the societal and *social* features of economic research. Even those that work on the intersection of science and political considerations have largely ignored the issues thrown up by economics' role in society, and have focussed on issues like climate change and genetics instead (Kitcher, 2001; Kitcher, 2011; Moore, 2017; Brown, 2009). Philosophers of science and economists who do provide normative analysis of economic research typically do not focus on the social features of economic research. Instead they focus on particular methods, assumptions, and measures used by economists. Luis Mireles-Flores's (2018) recent overview of the literature in the philosophy of economics, for example, points to three main areas of research: modelling and explanation, causal inference and evidence, behavioural economics. Descriptions of and arguments about the discipline-wide social determinants of economic knowledge—be they the social make up of economics' professional groups or the role economists play in politics—do not feature. This is a mistake. Sociologists, historians, and economists have mapped out interesting social features of economic research. These are features that philosophers should pay attention to because they affect issues philosophers typically care about: political representation, injustice, scientific progress, bias, trust, corroboration and empirical testing, and objectivity, to name just a few. Moreover, the power economics wields makes the discipline's effect on these issues highly consequential.

Thus, by focussing on *non-ideal normative* analysis of *existing social* features of economic research *as a whole*, this dissertation speaks to gaps in social epistemology, economics, and the sociology, history, and philosophy of economics. It supplements the important work of social scientists working on the role of economics in society by providing normative analysis of their findings. By developing tools to discuss discipline-wide social features of actual research communities rather than ideal, idealised, or small-scale cases, this dissertation offers a new way for doing social epistemology and provides an example of how that might be applied to scientific practice. Moreover, it adds an important case (economics) that is understudied by social epistemologists. Lastly, by focussing on the social determinants of economic work it also adds a new dimension to work in the philosophy of economics.

Although this argument breaks new ground in social epistemology, economics, and the sociology, history, and philosophy of economics, there are two important things I will not do. First, limitations of space mean that I cannot comment on all important social aspects of economic research in detail. In highlighting gender imbalances, hierarchy, and a dismissive attitude to outsiders I have inevitably left other issues out. I will not discuss, for example, the facts that economics has been largely anglophone,

that it has often focussed on the west, or that its practitioners are predominantly white. I will also not discuss the structure of graduate education (except a few aspects that are relevant for my discussion of hierarchy) or research funding. I focus on the three issues that I do at the expense of other possibilities for four reasons. Firstly, there is more useful data available regarding these issues. There is much more data on male-female disparities than on the lack of racial diversity in economics, for example. Secondly, the three issues I focus on have begun to attract attention, both publicly and within economics. Although there is a growing awareness of their significance, thus far nobody has sought to bring the data available together, synthesise it and draw out concrete normative implications. Thirdly, the three issues I have chosen to focus on nicely balance three different kinds of social epistemic questions: *Who* is in economics? How are the *social relations within* economics structured? And, how do economists *relate to outsiders*? This allows me to talk about a range of topics that might be relevant to other features of the way that economic research is organised, while focussing on three concrete issues. This means that, fourthly, despite only analysing three issues, I provide a model for how we might also study other social features of economic research. Although what I will say is specific to the issues I focus on, some of the arguments I pursue can apply to others as well. For instance, the arguments I make about the importance of gender diversity can in part be applied to issues surrounding race and diversity in general.

Second, although my reorientation from pluralist ideals to concrete social epistemic issues is designed to make the debate about how economic research is organised more practically relevant, my main focus will be on identifying and explaining the problems. This does not, however, detract from my goal of being practically relevant for two reasons. Firstly, by identifying concrete issues, rather than an ideal for economics to match, my argument gets much closer to solutions than those that argue that economics is insufficiently pluralist. It does not map out a complete path of change but offers an important reorientation that opens up avenues for other work to do so. Secondly, for many of the specific features of economics that contribute to the three issues I comment on the solution is fairly easy: stop it. In highlighting the hierarchy in economics, for example, I will point out that 71 percent of those in governing positions of the American Economic Association got their PhDs from just four universities. One way to alleviate the economics' hierarchy would simply be to ensure its governing body comes from a more diverse background.²⁶

1.4 Overview of the thesis

I will start from pluralism. As noted in 1.2, four questions remain open in the debate about pluralism in economics: [1] what does/should pluralism entail? [2] Why is pluralism desirable? [3] Is economics pluralist? And, [4] what should be inferred from pluralism for practice in economics? I will use the philosophical accounts of pluralism to answer these questions. In chapter 2, I will outline the range of

²⁶ For instance, by creating posts that are only open to those not currently well represented.

answers to questions [1] and [2] by noting the different variables that can be targeted by pluralism and the different reasons for pluralism.

I will tackle [3] in chapter 3. I will argue that the range of potential plural variables and the range of ways that those variables can be interpreted in practice makes it difficult to determine whether economics should count as pluralist or not. The philosophical accounts of scientific pluralism do not offer easily applicable states with which to compare economics. This means that arguments for pluralism are unlikely to convince those that think economics is currently pluralist enough and that they do not offer unambiguous recommendations for change.

This does not mean, however, that philosophical accounts of pluralism are useless. In chapter 4, I use the arguments for scientific pluralism to suggest three heuristics for assessing the social epistemic practices in economics. Social epistemic practices can be evaluated by **H1**: looking for blocks to productive forms of *feedback*; **H2**: looking for blocks to certain kinds of *interests* being considered in decisions about which goals scientific communities should pursue; and, **H3**: looking for blocks to avenues for public *scrutiny*.

In chapters 5-7 I will turn these heuristics towards present practices in economics. I use data to describe three issues and use my heuristics to explain why they are problematic: [a] gender imbalances and an adverse environment for women in economics limit critical feedback and block the uptake of female interests in the discipline. [b] A steep hierarchy within economics creates perverse incentives and a concentration of power that limits critical feedback and the uptake of marginalised interests in economics, as well as constraining the potential for public scrutiny of the discipline. [c] Economists form an in-group that is dismissive of outsiders, which also reduces the amount of feedback economic ideas are exposed to and diminishes space for public scrutiny of and for a range of interests to be heard in the discipline.

I conclude that economics would be more progressive, representative of the interests of those in society, accepted, and legitimate, and less likely to fall into bias if it alleviated its gender imbalances, flattened its hierarchy, and developed a healthier relationship with outsiders.

2 A plurality of pluralisms

Pluralism is very much in vogue in the philosophy of science.²⁷ This is a remarkable turnaround considering that until the middle of the twentieth century unity was the dominant recommendation for science. The idea that science should be unified has a long history that runs at least from pre-Socratic Greece through Galileo, Descartes, Newton, Kant, and the early twentieth century logical positivists—who called for the “unity of science without metaphysics” (Cat, 2017). In the middle of the twentieth century philosophical attempts to unify the logic, methods, and concepts of the sciences were common. Mid-century advocates of the unity of science sought to determine *the* methods of causal inference and mode of explanation for science, and one set of concepts, kinds, and laws for which all others in science could be derived.²⁸ But in the middle of the twentieth century a number of currents also began to push against the idea that the sciences should aim for unity.²⁹ In part inspired by Pierre Duhem, Gaston Bachelard, and later Thomas Kuhn, a movement away from universalism and uniformity and towards pluralism and disunity began. A collection of scholars affiliated with Stanford University between (roughly) the 1970s and 90s played a particularly significant role in this movement—what has become known as the *Stanford School* included Patrick Suppes, Ian Hacking, Peter Galison, Nancy Cartwright, John Dupré, and Hasok Chang. Under the auspices of *scientific pluralism*, philosophers have argued that the nature of the world is such that it cannot be characterised by any singular set of kinds³⁰ (Dupré, 1993); that scientific laws are not universal (Cartwright, 1983; Cartwright, 1999); that the language of physics cannot be used to explain all features of the world (Fodor, 1974); that pragmatic interests play a role in determining scientific representations (Longino, 2002; Kellert, Longino, & Waters, 2006; Kitcher, 2001); and, that maintaining different incompatible approaches to a science can be fruitful regardless of the aims of that science (Chang, 2012).

In chapter 1, I noted four key questions that need to be answered to progress the debate about pluralism in economics:

1. (1) What does/should pluralism entail?
2. (2) Why is pluralism desirable?

²⁷ A number of philosophers have suggested that pluralism is now the consensus view (Mitchell, 2003, p. 180; Brigandt, 2010, p. 296). While this may be an overstatement, the number of philosophers that accept some form of scientific pluralism is much higher than it was and continues to rise fast (particularly amongst more junior philosophers).

²⁸ For a famous example, see Oppenheim and Putnam (1958).

²⁹ In reality the unity-of-science movement contained many different ideas, some of which are compatible with contemporary accounts of scientific pluralism (Cartwright, Cat, Fleck, & Uebel, 2008; Cat, Cartwright, & Chang, 1996; Ruphy, 2017). Nonetheless, there has at least been a shift of sentiment from the idea that science can and should be unified in some way to exploring the ways that it is and should be characterised by pluralism.

³⁰ This is against the thesis that there is one single correct way of categorising the world, determined by the way the world is (rather than, for example, the pragmatic goals or epistemic limitations of investigators). The idea that there is one set of ‘natural’ kinds is commonly thought to originate in Plato who has Socrates, in discussion with Phaedrus, suggest a process of “dividing things again by classes, where the natural joints are” (Phaedrus 265e in Plato, 1925).

3. (3) Is economics pluralist?
4. (4) What should be inferred from pluralism for practice in economics?

In this chapter, I will use philosophical accounts of scientific pluralism to begin to answer these questions. In 2.1, I will detail some of the most popular forms of scientific pluralism to show that [1] pluralism can entail a variety of things, and [2] is desirable for a variety of reasons.³¹ To explain [1] what pluralism entails, and to later answer [3] whether an economics is pluralist or not (tackled in chapter 3), it will be helpful to distinguish accounts of pluralism based on the *variables* they focus on. The variables of pluralism can include: the presuppositions scientists make; the abstract and concrete goals of science; the products of science (explanations, models, representations); the methods, epistemic standards, and forms of explanation scientists use; and, compounds of all these things. To understand [2] why pluralism is desirable and to later explain [4] what can be recommended for economics from pluralism (tackled in chapter 4), it will be helpful to categorise the *reasons* for pluralism into three types: because of the way the world is, because of the epistemic limitations involved in doing science, and because science is and/or should be used towards variety of ends. In 2.2, I will summarise the different plural variables and kinds of reasons for pluralism available.

In chapters 3 and 4 I will use the distinctions drawn in this chapter to answer questions [3] and [4]. Although I will argue that scientific pluralism does not offer an easy answer to [3], my description of the plurality of pluralisms available is helpful for two reasons. Firstly, it highlights that disagreement about what should count as pluralism is not confined to economics and clarifies that the stalemate outlined in chapter 1 is in part caused by disagreement over the variables for pluralism and their boundaries. Secondly, highlighting the reasons why pluralism is desirable informs my discussion of transitional improvements in the social epistemic practices in economics, in the same way that ideal might aid non-ideal political theory. Even in the face of disagreement about what should count as pluralism in economics, the reasons given for scientific pluralism can help articulate concrete lessons for change.

2.1 What does pluralism entail? Why pluralism?

In this section I outline versions of pluralism that are: anti-reductive, metaphysical, based on the representations of science, and aimed at more general ways of grouping scientific practices (systems of practice). In describing each account, I will draw attention to their reasons for pluralism and their plural variables—the things that they argue should be plural rather than singular.

³¹ It has been suggested that there are as many pluralisms as there are pluralists (Chang, 2012, p. 268). My sample was selected with three things in mind: trying to represent the range of different accounts available (in particular, the range of plural variables and reasons for pluralism), trying to select the most popular accounts, and the accounts that seem the most applicable to economics. This means I will not provide a complete review of all the accounts of pluralism and I will leave out some popular ones. See Galison and Stump (1996), Feyerabend (1975), Giere (2006), and Ruphy (2017) for some accounts of pluralism missing here.

2.1.1 Resisting reductionism

The idea that scientists should attempt to unite their languages, explanations, theories, and/or units of analysis has long been a goal of unity of science movements (Cat, 2017). Where this has meant professing the superiority of smaller units of analysis (the language of physics, for example), or converting propositions in terms of larger units into propositions in terms of smaller units, this has been labelled ‘reductionism’. In the second half of the twentieth century reductionism sustained a number of prominent attacks.

Two forms of anti-reductionist arguments have been offered: local and global. Global anti-reductionists make general arguments that certain kinds of reduction (translations from larger to smaller units) are inherently problematic and will always be so (Fodor, 1974; Jackson & Pettit, 1992). By contrast, local anti-reductionist arguments focus on particular reductions and articulate context specific reasons to believe that they are problematic (Sober, 1999). I focus on global anti-reductionist arguments, because it is these that give rise to accounts of pluralism (Ruphy, 2017).

Jerry Fodor (1974) and Frank Jackson and Philip Pettit (1992) have both offered what have become canonical global anti-reductive arguments.³² Both arguments utilise a similar logic. First, they offer examples in which translating scientific propositions into smaller from larger units of analysis (moving from chemical to physical generalisations, for example) results in some epistemically significant loss. Second, they offer reasons for believing that such losses are not contingent on their examples, but are always likely to occur.

Jackson and Pettit (1992) argue against preferences for explanations of phenomena in terms of physically smaller variables over larger ones (micro over macro variables). Such preferences seem to be justified by ‘causal fundamentalism’—the idea that causal processes act on different levels of description, but that the causal processes of some levels (dubbed the higher levels) are dependent on the causal processes of others (dubbed the lower levels). In this vein, Jon Elster (1985) argues that because the world is governed by local causality, our explanations of particular phenomena should strive to identify the mechanisms that account for that local causality. Jackson and Pettit endorse causal fundamentalism. But, although they think that a preference for explanations in terms of small variables implies causal fundamentalism—if there were higher level causal relations that are not also present in lower levels moving from larger to smaller variables would leave out something about the structure of causality—they deny that the implication goes both ways. Jackson and Pettit agree that there are circumstances in which macro-level explanations should be abandoned for micro-level ones. If a macro level explanation fails to “offer any distinctive information on the causal history of the thing to be explained” (1992, p. 10) over and above that offered by a micro level explanation, then the

³² Although most accounts of pluralism assume or argue for some form of anti-reductionism, I isolate Fodor’s and Jackson and Pettit’s arguments in this subsection because reductionism is all they focus on. Their accounts only imply a plurality of levels of description and not of representations on different levels or of other potential plural variables. Although Fodor and Jackson and Pettit are not the only philosophers who focus on anti-reductionism, I focus on them because they are fairly representative of a wider literature, canonical, and utilise examples from the social sciences.

micro level explanation should be preferred. But they deny that this must always be the case.

Jackson and Pettit argue that explanations are valuable where they provide information about relevant factors in the causal history of the explanandum, defined as factors that would have led to alternate outcomes if changed. Crucially, Jackson and Pettit then argue that there is a relevant kind of causal history information that macro level explanations can provide that is often missing from micro level explanations. They claim that micro explanations do better at providing contrastive information—information about how the actual world differs from other possible worlds—but that macro explanations do better at providing comparative information—information about how the actual world relates to other possible worlds. An explanation of a flask containing boiling water cracking in terms of individual molecules, for example, gives useful contrastive information: the flask breaks under the impact of a particular molecule rather than another. An explanation of the same event in terms of the fact that the water was boiling, provides useful comparative information, it teaches us that “in more or less all possible worlds where the relevant causal process is characterised by involving boiling water, the process will lead to the flask cracking” (Jackson & Pettit, 1992, p. 15). Because macro level comparative information can pick out relevant factors in the causal history of an explanandum that is lost when opting for more micro explanations, the assumption that lower level explanations always give more or better information than higher level explanations is false.

Fodor (1974) argues, similarly, that something important is lost in reductions from higher to lower levels. Instead of explanations, Fodor focusses on theories and laws. He defines reductionism as the thesis that the antecedents and consequents of the laws of the ‘special sciences’ can be reduced to physical predicates that admit the same lawfulness. That means that if $[A1 \Rightarrow A2]$ is a special science law, then there must exist some physical predicates B1 and B2 such that $[B1 \Rightarrow B2]$ is a law in physics and some bridge principles that connect A1 to B1 and A2 to B2. Fodor suggests that many philosophers are drawn to reductionism because they assume it necessary to support the ‘generality of physics’—“the view that all events which fall under the laws of any science are physical events and hence fall under the laws of physics” (1974, p. 97). He thinks that this assumption is a mistake.

Fodor argues that ‘token physicalism’—a position that asserts that every event that the sciences talk about is identical to a physical event, but that the properties in scientific laws and theories need not be identical to physical properties—is enough to support the generality of physics, without implying that all theories reduce to physical theories. Fodor argues that to go further than token physicalism and insist on the reduction of the theories and laws of the special sciences to physical theories and laws would imply that every special science natural kind must be identical to, or co-extensive with, a physical natural kind. This is because Fodor thinks that natural kinds must be defined as predicates “whose terms are the bound variables in its [science’s] proper laws” (1974, p. 102), and if reductionism were true then every law would be identical to, or co-extensive with, a physical law. He goes on to argue that such a conclusion is undesirable because it creates too strong a constraint on special science kinds:

(a) interesting generalizations (e.g., counter-factual supporting generalizations) can often be

made about events whose physical descriptions have nothing in common, (b) it is often the case that whether the physical descriptions of the events subsumed by these generalizations have anything in common is, in an obvious sense, entirely irrelevant to the truth of the generalizations, or to their interestingness, or to their degree of confirmation or, indeed, to any of their epistemologically important properties, and (c) the special sciences are very much in the business of making generalizations of this kind. (1974, p. 103)

He suggests Gresham's law—if there are two types of money in an economy that count as legal tender with one intrinsically more valuable than the other, then the more intrinsically valuable money will fall out of circulation as people save it and use the 'bad' money instead—as an example:

The point is that monetary exchanges have interesting things in common; Gresham's law, if true, says what one of these interesting things is. But what is interesting about monetary exchanges is surely not their commonalities under physical description. (1974, pp. 103-104)

He admits that some generalisations of the special sciences might eventually be matched to co-extensive predicates in physics but doubts whether those physical predicates will amount to anything more than brute enumeration. Even if the set of physical events identical to the events in a special science generalisation can be listed (given an infinite amount of time) that list is unlikely to amount to anything more than a list. It is unlikely to be a natural kind itself, and unlikely to admit a physical law.

Both Fodor and Jackson and Pettit, thus, conclude that reductionism is unnecessary and results in the loss of something important. For Jackson and Pettit, explanations that focus on different levels of detail may be interesting in different ways. Social explanations in terms of individuals should, for example, be seen as complements to, rather than replacements for, explanations in terms of social structures. For Fodor, interesting generalisations can emerge in the languages of different sciences and should not be constrained by generalisations in the language of physics. Both of these arguments amount to kinds of pluralism. Although their precise focus differs, Jackson and Pettit's and Fodor's anti-reductive arguments both argue that the sciences should retain multiple *levels of analysis* (different scales on which events can be explained for Jackson and Pettit, and different scientific languages that can be used to describe laws for Fodor). Within this are both pragmatic and metaphysical reasons for pluralism. Jackson and Pettit emphasise the contextual component in explanation: multiple levels are important because the sciences explain different kinds of events and different kinds of explanations can be *used towards different kinds of ends*. Both arguments also lean on metaphysical arguments for pluralism. The losses that occur in reductions are not contingent. They do not depend on the current organisation of science or on the epistemic limitations of investigators but are the result of how the world is organised.

[W]hereas the higher level explanations do better in providing comparative information, the lower level ones do better in providing information of a contrastive kind. (Jackson & Pettit, 1992, p. 16)

[T]here are special sciences not because of the nature of our epistemic relation to the world, but because of the way the world is put together: not all kinds . . . are, or correspond to, physical kinds. (Fodor, 1974, p. 113)

The way *the world is organised* is such that the sciences should use multiple levels of analysis. These two arguments, thus, give us our first possible plural variable—*levels of analysis*—and first two kinds of reasons for pluralism—to *satisfy different goals* and *because of the way the world is*.

2.1.2 Stanford disunity

Despite implying a plurality of levels of analysis, the anti-reductionisms of Fodor and Jackson and Pettit are compatible with many aspects of the unity of science movement. By endorsing causal fundamentalism and token physicalism, Jackson and Pettit and Fodor assert the priority of physical properties, objects, and processes. Moreover, both of their anti-reductionisms are compatible with there being a correct way to divide up each level of analysis into kinds and with the existence of a single ordered set of laws governing the kinds of each level. Two members of the Stanford School, Cartwright and Dupré, deny both of these possibilities and argue against reductionism as part of wider ranging attacks on the unity of science.³³

Dupré argues that ‘compositional materialism’—the thesis that all things are made of physical entities—need not imply that physical entities (objects and processes) are in some way existentially prior to entities on other levels of analysis (cells, people, et cetera). He argues that if ontology were just a list of the contents of space and time, then compositional materialism would imply that ontology is just a list of physical entities. The problem is this would leave out: “what can be said *about* what exists..., facts such as a certain set of physical entities constitutes a leaf. ... [I]f that is all there is to ontology, then there is a great deal more than ontology to metaphysics” (Dupré, 1993, p. 94). If, alternatively, we follow W. V. Quine (1960) and think of ontology as the entities needed to explain events, then compositional materialism would have to be combined with a form of reductionism for the propositions of science to assert that only physical things exist. Dupré rejects this possibility for similar reasons to Fodor. In addition to noting the interdependence of entities on different levels of analysis, he argues that the bridge laws that (he thinks) propositional reductionism requires would define necessary and sufficient conditions for kind membership and so imply a form of ‘essentialism’—the idea that over and above facts that refer to individual objects in the world there exists facts about the kind of thing each object is. He argues against essentialism by drawing on cases from biology.

Dupré argues, for example, that species concepts fail to result in adequate essentialist kinds. He endorses Philip Kitcher’s (1984; 1989) argument that the ‘biological species concept’—a principle that classifies species as groups of freely interbreeding organisms that are reproductively isolated from other groups (Mayr, 1942)—is useful for some biological research, but because it is difficult to characterise a group without already assuming some kind of species concept and to draw clearly defined reproductively isolated species, is not useful for all. Botanists who deal with asexually

³³ See Cat (2017) for some discussion of the ‘Stanford School’, and see the program of a 2013 conference celebrating the school for some of the people and work associated with it (Winther, 2013). I focus on Dupré and Cartwright in this subsection as they offer distinctly metaphysical accounts of pluralism. Others associated with the Stanford School will be discussed in the context of other forms of pluralism.

reproducing plants, for example, don't find the biological species concept particularly useful and normally refer to morphological characteristics to determine species. Dupré adds that hybridisation makes it impossible to ground essentialist categories in terms of the biological species concept because it results in some individuals not being assigned to any species and in reproductive links across different species. After adding similar arguments against deriving essentialist categories from morphological and phylogenetic species concepts, Dupré asserts that:

[T]here is no metaphysical guarantee that whatever accounts for the coherence of species and their distinction... from other species will be the same for all living things. (Dupré, 1993, p. 51)

Generalising from this and other examples, Dupré concludes that even where natural processes give rise to kinds, they provide no basis for non-arbitrarily picking out particular properties as essential to the resulting kinds.

Dupré argues that if essentialism is wrong then there can be no bridge laws for propositional reductionism, and consequently that compositional materialism need not imply the ontological priority of physical entities. Cartwright reaches a similar conclusion via a different route. She argues that the common intuition that the category of properties studied by physics is in some sense complete leads many to infer that all other (macro) properties in the world either reduce to physical properties or come out of nowhere. She argues, however, that the completeness of physics only makes sense as a set of properties that are predictively closed, and that predictive closure among a set of properties need not imply that those properties describe everything there is. There are plenty of macro properties that “have been here in the world all along, standing right beside the properties of microphysics” (Cartwright, 1999, p. 33). We interact with such properties all the time. Just because they do not seem to be a result of a set of properties that together are predictively closed, does not mean that we are mistaken in thinking that they exist.

Cartwright argues, similarly, that there are no laws with universal applicability. The laws that seem good candidates for universal coverage—the laws of classical mechanics for example—are in fact true of only a very limited set of circumstances. Circumstances in which the causal processes mentioned in the law are the only ones active. Such circumstances can arise from highly controlled environments, like experiments, but are not generally the case. Most real-life situations involve combinations of causes that laws treat separately (if at all). Where multiple causes covered by different laws interact together none of the laws involved typically describe what happens. Cartwright gives the example of a one-thousand-dollar bill swept away by the wind to illustrate her point.³⁴ Classical mechanics can only offer a partial model of this situation. A defender of the universal applicability of laws may argue that the flight the bill takes can in principle be modelled using the laws of classical mechanics when an adequate (and complicated) model of motion due to wind is added. But Cartwright points out that the success of classical mechanics in more circumscribed situations does not support such an argument. There are likely to be many causal factors involved and interacting in ways not predicted by classical

³⁴ This example is borrowed from Otto Neurath (1933).

mechanics and the assumption that the laws of classical mechanics can be incorporated into a bigger covering law including them all is just that, an assumption. If a particular kind of situation is important it may be studied in detail (as fluid dynamics is), but this will likely develop its own more detailed and local understandings of the causes involved that depart in some way from the laws of classical mechanics.

By rejecting the universality of fundamental laws, Cartwright rejects what she calls ‘cross-wise’ reductionism as well as downward reductions (that she rejects by rejecting the idea that physical properties can describe all there is). What she means by this is that even on a given level of analysis, causal processes are not arranged in a nice ordered way or covered by universal laws. She thinks that the sciences provide lots of important and highly specific knowledge of the workings of particular systems (one example she gives is the behaviour of Superconducting Quantum Interference Devices), but that such knowledge is limited to specific domains. Although such knowledge might occasionally be said to define local laws, there are lots of phenomena that can be explained and understood without being covered by laws. This includes knowledge of highly specific circumstances that combine causal processes on many levels of analysis. The picture of the world that emerges from Cartwright’s argument is of a contingent patchwork of local laws and causal processes. This amounts to a rejection of the unity of science idea that the world consists in a pyramid of clearly defined levels with regulating laws.

In addition to rejecting reductionism and essentialism, Dupré also adopts a disunified image of causal processes and our knowledge of them by rejecting what he calls ‘determinism’—the idea that all states of the world are necessitated by previous states. He argues that the determination of a state of the world from a previous state would require at least one level of analysis to be causally complete, otherwise all of the parts of the two states could not be linked. Moreover, he thinks that the causal closure of some level would necessitate reductionism. This is because if a particular level was causally complete nothing at higher levels would be required to bring about changes of things at that level. The movements of electrons in an individual’s thumb, for example, would be completely determined by causes at the level of atoms. This would mean that everything could be explained by micro level events. But since reductionism is false, according to Dupré, there cannot be any causally complete level of analysis. (The proposition [causal closure \Rightarrow reductionism] is equivalent to [not reductionism \Rightarrow not causal closure], so [not reductionism] implies [not causal closure].) Dupré concludes that determinism must also be false.

Both Cartwright and Dupré take their disunified metaphysical pictures to imply forms of pluralism. Dupré argues that his rejection of essentialism implies a pluralism of *kinds*; that his rejection of reductionism implies a pluralism of *levels of analysis*, both for propositions and in terms of existential priority; and, that the combination of anti-determinism, anti-essentialism, and anti-reductionism necessitates a pluralism of *ways of practicing science*. Cartwright argues that the contingent nature of laws and the patchwork coverage of causal processes means that the world is “dappled”. It is made up of different things on different levels, with different natures, behaving in different ways. This suggests a metaphysical pluralism of *kinds* and *levels of analysis* similar to Dupré,

and also a plurality of *causal processes*. In part because of this picture of multiple overlapping causal processes, Cartwright points out that where science offers abstract theoretical models it offers variety. This amounts to a pluralism of theoretical *models* that focus on different causal factors and that can be applied to different situations and combined in different ways, with no single model or way of combining models serving all purposes and circumstances. Moreover, she argues that because most actual circumstances do not fall under the precise concepts of scientific theories, scientists do not normally deduce manipulations of the world from theory. More often they combine theoretical knowledge with technical knowhow of the local situation they focus on (which is not normally completely coded into theoretical models). She argues that the mix of causal processes, local technical knowledge, and theoretical models involved in science means that what counts as a causal relation also likely varies, with causal relations in different domains requiring careful and local skill to tease out and test. This implies a pluralism of *causal inference* for the practice of science.

Together Cartwright and Dupré's arguments, thus, give us five new potential plural variables: *kinds, ways of practicing science, causal processes, models, and causal inferences*. Like the anti-reductionist arguments outlined in 2.1.1, pragmatic considerations play a role in both Cartwright's and Dupré's arguments. But their main focus is metaphysics. The practices, categorisations, theories, and applications of physics, economics, and biology provide evidence of metaphysical disunity. The resulting set of pluralisms is not a consequence of cognitive limitations on the part of scientists, it is *the result of how the world is*; it is a result of "the disorder of things" (1993) and the fact that the world is "deeply dappled" (Cartwright, 1999, p. 59).

2.1.3 Maps and models

In a similar fashion to Cartwright, Sandra Mitchell (2003; 2002a) argues that the complexity of the world necessitates that scientific theories and models of it must be partial. She argues that models capture particular causal processes and abstract from others. Different models capture different causal processes, and none are strictly true because no (or very few) circumstances exist in which the casual processes models identify are the only ones present. Explanations of particular scenarios require the integration of these different partial models. How they are integrated depends on the ends of the particular explanation (what is already known, what the explanation is to be used for). The facts that integrations occur to explain particular phenomena and that those integrations depend on pragmatic interests imply that there is no one way to integrate models for all uses. The plurality of models cannot be synthesised into one.

Mitchell, thus, argues for a pluralism of *models* as the result of a combination of *the way the world is* (complex) and *the idealised character of modelling*. Mitchell thinks that such a pluralism already exists in the practices of contemporary science and should not be sacrificed in order to try to unify science. For similar reasons to those outlined above, Mitchell also thinks that reductionism is false and that the sciences should make use of *different levels of analysis* (this is part of what creates complexity). But the main factor in determining different models is different causal factors rather than

different levels. This means there can be plurality on any given level, and models on different levels can teach each other things and/or compete to replace each other.

Although Mitchell's pluralism has much in common with Cartwright's, she puts more emphasis on epistemic reasons for pluralism, specifically limitations involved in representing the world. She is not the only one to do this. Kitcher and Helen Longino also develop arguments for pluralism focussed more on epistemic and pragmatic reasons than how the world is.

Like Dupré, Kitcher (1993; 2001) thinks that there is no privileged way to sort the objects of the world (however labelled) into kinds. He argues that language can be applied to nature to divide it up into objects in uncountably many legitimate ways (2001). Moreover, because kinds are not universally applicable—his argument that the biological species concept is not useful for all situations (discussed in 2.1.2) is intended to show this—there is no privileged way to sort the objects of the world, however labelled, into kinds.

Kitcher uses this to develop a pluralism of *representations*. He argues that the successful use of a representation should be taken to imply that it represents some aspect of the world accurately. Given a particular use, some representations can be better than others. But no representations are completely accurate because their accuracy is judged against how they are to be used, and they can be used in different ways. The kinds representations use and the things they focus on should be determined relative to human processes, capacities, and interests, and these can vary. Kitcher uses the example of maps to explain what he means. Map makers select certain features of a target to represent, normally with an intended use in mind. If a perfectly faithful representation was all that was desired, map makers would end up like those in Borges's imaginary empire:

In that Empire, the Art of Cartography attained such Perfection that the map of a single Province occupied the entirety of a City, and the map of the Empire, the entirety of a Province. In time, those Unconscionable Maps no longer satisfied, and the Cartographers Guilds struck a Map of the Empire whose size was that of the Empire, and which coincided point for point with it. The following Generations, who were not so fond of the Study of Cartography as their Forebears had been, saw that that vast map was Useless, and not without some Pitylessness was it, that they delivered it up to the Inclemencies of Sun and Winters. (Borges, 1975)

Where a map of something is the same size and has the same amount of detail as the thing it is supposed to represent it ceases to be useful for many purposes for which a map might be used (as a portable guide to the streets of a city, for example). In choosing what to include on a map, and what to leave out, trade-offs are made to facilitate particular uses. Kitcher notes that the London Underground map is an inaccurate depiction of many aspects of the geography of London, but that it does accurately depict the lines and connections of the Underground network in a way that is simple to understand. Even after throwing out completely inaccurate representations, there are many other ways of mapping London, each inaccurate on some features of the city but accurate on others. Context specific pragmatic reasons determine which maps will be useful in a given situation.

Kitcher argues that something similar is true for the representations of science—these include the models that Cartwright and Mitchell talk about and also hypotheses, explanations, and more loosely

defined theories. Accuracy underdetermines how science represents the world, what is significant to those that develop and utilise scientific representations also plays a role. This means that the representations of science are shaped by the past and evolve in relation to pragmatic as well as empirical concerns. Because Kitcher thinks that the representations of science should track truth, they must be compatible with each other. But, because truth underdetermines scientific representations, the world (and even specific features of it) will be represented in a plurality of different ways. This pluralism is intended to be normative as well as descriptive. Because pragmatic concerns are context dependent, the sciences do and *should* use a plurality of representations.

To this Kitcher adds the assertion that sciences should also pursue a plurality of different *strategies* towards specific problems (1993; 2001). Using a decision theoretic model, Kitcher argues that if there are diminishing returns to more scientists pursuing whatever strategy seems most likely to yield new successful scientific theories and results (as he suggests is often the case), then from the perspective of the whole scientific community it would be best to hedge, with different scientists pursuing different strategies. He argues that, taken together, these normative pluralisms of representations and research strategies have two further benefits. Firstly, because different communities will form different representations and research strategies (relating to their interests), pluralism allows science to better politically represent those differences and consequently the values and needs of diverse sets of communities (I will say more about why this is important in chapter 4). Secondly, a plurality of representations and strategies can help solve particular problems by giving us multiple ways of approaching them and can help us gain a wider understanding of specific issues by tackling different problems and sub problems that relate to them.

Longino arrives at a similar recommendation via a different route. In *The Fate of Knowledge* (2002), she argues that knowledge is social and that this necessitates a form of scientific pluralism. Longino draws on Duhem's (1914) argument that theory is underdetermined by evidence: because scientists postulate different entities and processes in explanations and theories of phenomena than those that occur in descriptions of phenomena, there exists a justificatory gap between evidence and hypotheses. This means that there is no way to formally specify precise evidential relations for a hypothesis, and data derived from observations cannot be determined to support one hypothesis over all others. In practice this justificatory gap is filled by background assumptions that determine evidential relations for specific hypotheses. These assumptions might themselves be justified using evidence and other background assumptions. But as the chain of justification goes backwards some assumptions will eventually be reached for which there can be no non-circular justification or evidence.

Longino argues that underdetermination does not mean that observations, and reasoning from them, are irrelevant to science. Rather, factors other than observation and logic must also play a role in whether or not theories and hypotheses are accepted. These factors need not be reasonless, political, aesthetic, or arbitrary, simply because they are not purely logical. A myriad of contextually relevant factors might make background assumptions reasonable (maybe certain problems are seen as more significant because they help connect important theories, maybe certain assumptions fit better with the

scientific instruments at hand, et cetera). But, crucially, criticism and interaction between scientists is required in order to ensure that the assumptions relied upon in particular contexts are reasonable. Via criticism, social interaction has the resources to identify and correct falsehoods, inappropriate assumptions, biases, and other failings. Since, according to Longino, it is these kinds of corrections that warrant the attribution of knowledge to cognitive processes, knowledge must also be attributed to the results of social critical interaction. Knowledge is, thus, social.

Longino argues that because knowledge is social, it must also be partial and plural. Because critical interaction in a particular community bounds the background assumptions used to develop it, knowledge is always conditioned by the preferred standards and goals of the scientific community it arises in. Moreover, Longino argues that it is rational for different communities to develop different standards by which to evaluate potential knowledge, or even for one community to develop different standards to evaluate beliefs in different contexts. Some of these standards might draw from the category of standards that philosophers often call ‘epistemic virtues’ (this includes things like simplicity, parsimony, et cetera). But, Longino argues that epistemic standards are neither universally applicable nor solve the underdetermination problem alone. This is because they either involve further substantive assumptions—a standard that prioritises simplicity, for example, assumes that the world is simple or amenable to simplicity—or are insufficient to bridge the gap. Furthermore, the epistemic standards often suggested for filling the underdetermination gap—simplicity, unification, parsimony—sometimes pull in different directions and are not optimally satisfied by any single theory or method (Van Fraassen, 1989). That standards can vary across communities does not mean that anything goes. Knowledge must still be empirically adequate (as defined by the standards of the community) and the standards of the community must also be open to criticism and tested by use. If the community’s standards repeatedly fail to give rise to successful interventions, then interactive criticism should force change in those standards. That standards can vary does, however, mean that knowledge is partial. Knowledge must be developed within standards but because those standards are not universal knowledge is also not universal.

The variety of standards of justification communities might have, plus likely variation in their goals, means that communities are likely to parse the world differently, to note different observations, and to produce different representations. Within the latter, Longino does not only mean that different communities will form different hypotheses and propositions, but that they will also produce different kinds of representations. Longino uses Cartwright’s (1983) argument that the laws of physics are false of most circumstances to argue that the sciences sometimes achieve something other than just true propositions (hypotheses, theories, et cetera) and that that philosophers should expand beyond propositions to include a whole range of representations (models, diagrams, maps, et cetera) as potential knowledge content. But since non-propositional representations do not have as easy a relationship to truth as propositions,³⁵ Longino thinks that philosophers should consider other representation-world relationships as markers of success. She suggests relations of homomorphism,

³⁵ See for example the debate about realism with respect to economic models (Alexandrova & Northcott, 2013; Mäki, 2005; Mäki, 2011; Reiss, 2012).

isomorphism, fit, approximation, and similarity should be added to truth, and proposes that this package of possible relations be labelled ‘conformation’. Like Kitcher, Longino then uses the metaphor of maps to highlight that different representations can emerge of the same phenomena.³⁶ Both the relevant features of a phenomena to emphasise in a representation and the appropriate conformation relation by which to judge that representation by will depend on the context in which it will be used and the standards of the scientific community it is developed in. This leads Longino to conclude that knowledge is plural: a plurality of acceptable *representations* can be generated by different considerations and standards in any inquiry (Longino, 2002, p. 184; Kellert et al., 2006).

Thus, despite some disagreements, all three of Mitchell, Longino, and Kitcher describe and endorse the plurality of *models* and *representations* (and for Longino *forms of representation*) that already exists in science.³⁷ The fact that the representations produced by science are partial plays a role in all three arguments. But while Mitchell sees this as a result of a combination of *the way the world is* and of the *limitations involved in representation* itself (in the form of the idealised nature of modelling), Longino sees this more as a result of the *limitations involved in practicing science* (underdetermination), and *variety in the goals and interests of different communities*. Kitcher’s position draws on a mix of all four of these factors. Longino and Kitcher also endorse pluralisms of *epistemic goals, standards, and methods/strategies*. These are based on a combination of the *insolubility of epistemic virtues*, the *variety of goals for science*, and the idea that some form of pluralism helps to *mitigate against some of the limitations involved in scientific practice*—different perspectives and different forms of knowledge critically interacting to overcome underdetermination (Longino), and different investigative strategies as the best way to approach the uncertainty involved in scientific inquiry (Kitcher).

2.1.4 Systems and styles

Hasok Chang offers a version of pluralism that includes many of the variables discussed above along with scientific abilities and applications. He argues for a pluralism of what he calls *systems of practice*—defined as “coherent and interacting set[s] of epistemic activities performed with a view to achieve certain aims” (2012, p. 260). Like Longino, H. Chang (2004) uses an insight from Duhem to question standard pictures of scientific justification. Duhem noted that empirical testing requires

³⁶ As an example of this, Longino analyses the different approaches to neurophysiology that come from neuroendocrinology and brain physiology (1990; 2002). She argues that the competing approaches “are characterised by different questions, vastly different experimental and investigative strategies, and different models of brain structure and function”, and are based on different epistemological values. The different questions they address arise in different contexts and their results “provide (partial) knowledge of brain and neural processes... that conform to their different domains sufficiently to support further research” (2002, p. 183). This results in two different inconsistent representations of neurophysiology.

³⁷ One of the main sources of disagreement is over the compatibility of different representations and to what extent they should be integrated. As noted, Kitcher thinks that the plurality of representations offered by the sciences should be compatible with each other. Longino disagrees with this and thinks that incompatible representations need not be problematic. Mitchell takes what might be seen as a middle position, arguing that incompatible models can be useful, but that models integrated to explain a particular event must be compatible and must result in a single explanation. This disagreement does not have an effect on what I will say in chapters 3 and 4, so I will ignore it.

observations that are in part based on scientific theories. Scientific instruments, for example, are developed and calibrated using scientific theories (think of the models used to produce brain images). Those theories themselves need to be justified. Without solid foundation this chain of justification never ends. But, the best candidates for such a foundation have been found wanting.³⁸ The upshot of this is that no observations (and consequently theories) in science can be completely justified. Yet, scientific observations are frequently taken to be sufficiently justified. H. Chang argues that this only works if we accept coherentism: if observations cohere with existing systems of practice that are as a whole successful,³⁹ then scientists accept them as justified and they are entitled to do so because that is the best they can do.

Despite arguing that scientists can only justify observations (and theories) within systems as a whole, H. Chang (2004) thinks that science can progress through iteration. To conduct experiments, scientists have to start somewhere, it makes sense for them to choose to work in what they judge to be the best available framework (system) even if they know it is imperfect. Scientists then work within that framework to produce results and theories that enrich or correct it.⁴⁰ But if truth is internal to systems of practice then how are systems as a whole to be judged? H. Chang's answer to this question is to look at what systems can do in the face of "resistance from reality" (2012, p. 215). For a scientific system to progress, scientists should aim to develop observations and theories that lead to enrichment of the system or corrections to maintain its coherence. To do so, they should arrange their investigations to maximise feedback from 'external reality'—whatever is not subject to one's own will—in a way that is likely to contradict their expectations. The iterative steps in the progress of a scientific system are driven by the incorporation of new observations from and interactions with external reality. Systems can then be criticised if they fail to expose themselves to feedback from reality.

But what makes the enrichments and self-corrections progressive rather than ad hoc? To answer this, H. Chang draws on Carl Hempel, Kuhn, and Bas Van Fraassen to point to a range of epistemic virtues—accuracy, consistency, scope, and fruitfulness for Kuhn (1977); elegance, completeness, unifying power, explanatory power, and most importantly empirical adequacy for Van Fraassen (1980); and, simplicity, support by more general theories, ability to predict previously unknown phenomena, and quantity, variety, and precision of evidence for Hempel (1965). H. Chang argues that a combination of successfully using systems of practice in new situations and improving adherence to some of these epistemic virtues can determine whether a system improves or not. How scientific systems satisfy epistemic virtues and the ways that they can be used without resistance from reality are also the grounds on which to choose between them. But this is not likely to recommend one system as superior to others for all purposes. Because there are a variety of possible uses for scientific results

³⁸ Supposedly self-evident theories like those of mathematics and logic don't inform us much about nature. And the theory-ladenness of language means that only unarticulated experience can give us foundational observations, but as soon as we start building on such experience we rely on theory laden language.

³⁹ When discussing coherence, H. Chang also talks about "systems of knowledge" (2004, p. 224). To avoid too many different concepts, I will stick to the variable he focuses on for pluralism: systems of practice.

⁴⁰ H. Chang (2004) cites the development of temperature as a successful example of this process in action.

and because, as noted in 2.1.3, epistemic virtues cannot be combined into one scale, H. Chang's understanding of progress and success of systems is pluri-axial. He, thus, advocates "the cultivation of multiple systems of practice in any given field of science" (2012, p. 260), i.e., a pluralism of systems of practice.

In addition to supporting pluri-axial notions of progress and success, H. Chang argues that pluralism delivers four other practical benefits to science. Firstly, no matter what the ultimate aims of science are, pluralism will best serve them. If non-coherentist truth (as correspondence, for example) were the sole aim of science, pluralism would still be the preferred method to attain it because of the amount of unpredictability in science. The history of science is replete with reversals and revisions. The path that looks most likely to yield truth now may not in the end do so. Lines of inquiry that look unlikely to deliver success at any one moment can surprise us and should be retained. Secondly, different systems often partly satisfy aims in different ways (think, for example, of using both national statistics and interviews with citizens to understand the health of an economy).⁴¹ Given this, it makes sense to divide labour between different approaches to see which satisfy whatever goals one might have best. Thirdly, when integrated together in a specific context, different systems of practice might be more successful than one system can be alone:

A nice cutting-edge high-tech example of such integration is the global positioning system (GPS): by means of satellites kept in place by Newtonian physics, and atomic clocks ruled by quantum mechanics and corrected by special and general relativity, this system maps the spherical surface of the round earth on a geocentric grid (or rather, a geostatic grid), and gives advice to people on the ground from a flat-earth point of view. (Chang, 2012, p. 266)

Lastly, progress within systems can be spurred on by co-opting observations, methods, theories, representations, et cetera, from each other, and by competition between systems.

H. Chang, thus, offers us a new variable for pluralism, *systems of practice*, that combines some of the others discussed above (including modes of causal inference, standards, representations, and kinds). He argues that both because science can be used toward *different contextually determined goals*, and because of a number of *limitations involved in scientific practice* (uncertainty, no one system covers all uses) we should encourage sciences to pursue a plurality of different systems of practice.

Ian Hacking (1992) might be interpreted as offering a similarly mixed variable for pluralism, based on *styles of reasoning*. Drawing on an idea of A. C. Crombie's (1988), Hacking notes six styles of reasoning: mathematical postulation, experimentation, modelling, comparison and taxonomy, statistical analysis, and historical derivation of genetic development. The details of each do not matter for present purposes. What matters is that styles of reasoning define conditions of possibility and admissible techniques for determining what is the case. Each style defines its own conceivable sentences, classes and categories, law-like sentences, objects, explanations, criteria of success, and methods of intersubjective comparison. Hacking argues, for example, that the sentence "the gross

⁴¹ This is, in fact, what the Bank of England will be doing to understand the impacts of Brexit.

national product of Württemberg in 1817 was 76.3 million adjusted 1820 crowns” uttered in 1817, before the invention of the statistical style of reasoning, would have had no truth value “because there was no procedure of reasoning about the relevant ideas” (1992, p. 143). As another example, Hacking argues that before the statistical style of reasoning, properties like standard errors and averages were not seen as features of objects in the world.

Hacking thinks that traditional correspondence theories of truth can be applied reasonably well to certain simple sentences (he gives the example of “my shoes are black”, 1992, p.134), but that they cannot apply to all. Many complex sentences involve complex questions that can only be answered by a form of reasoning that does not marry easily to the correspondence theory of truth. For different kinds of questions and problems different methods of verification and reasoning have emerged and define their own conditions of truth (or success) and objectivity. These methods form styles of reasoning. Styles that emerge from historically specific combinations of people, needs, interests, ideology, and abilities, but that, once established, transcend these things and define a new way of determining truth and objectivity. These styles of reasoning are themselves justified using their own definitions of success or truth. This may be circular, but Hacking does not think that is an issue:

I embrace it [circularity], I welcome it. For there *is* an odd way in which a style of reasoning and truth-conditions of some sentences are mutually self-authenticating (1992, p. 135).

This circularity need not imply a kind of radical subjectivism or relativism. Styles define the conditions of truth and only in styles can such conditions exist. But whether or not a given sentence meets those conditions is determined by the world not the style. Hacking does not think that styles of reasoning need to be exclusive. Styles may need to fight to be established, but once mature they do not tend to exclude one another. Styles offer different ways to reason and can be called upon together in a given research project.

Because truth as correspondence does not work for all kinds of statements and questions, different forms of truth have emerged as part of different styles of reasoning; styles that address different kinds of questions, that develop from different kinds of interests, and that can be used towards different ends or combined towards the same end. Thus, *styles of reasoning* add a final potential variable for pluralism, forming a kind of pluralism that is desirable because *of how the world is* and because of the *variety of kinds of questions and issues scientists might want to tackle*.

2.2 The variables of and reasons for pluralism

Recall the four key questions for the debate about pluralism in economics: [1] What does pluralism entail? [2] Why is pluralism desirable? [3] Is economics pluralist? And, [4] what does pluralism recommend for economics?

The above gives partial answers to [1] and [2]. Pluralism can mean the preservation of different

levels of analysis for scientific propositions (Fodor, 1974; Jackson & Pettit, 1992). It can mean a radically disordered metaphysics without a simple set of kinds or causal processes (Cartwright, 1983; Cartwright, 1999; Dupré, 1993). It can affirm a variety of representations of science (Kitcher, 2001; Longino, 2008; Longino, 2013; Mitchell, 2003). And, it can be a description of science as composed of multiple methodologies, explanations, theories, systems of practice, and styles of reasoning (Cartwright, 1999; Hacking, 1992; Longino, 2002), or a recommendation that it be so composed (Chang, 2012; Kitcher, 1993).

Below I categorise the accounts of and arguments for pluralism using their plural *variables* and *reasons* for pluralism with the aims of offering clearer answers to [1] and [2] and grounding my answers to [3] and [4]. To argue that economics is or is not pluralist one would need to pick a variable (or set of variables) for pluralism and show that economics does or does not attain a sufficient level of multiplicity for that variable (or set). In chapter 3, I will use the potential variables outlined in this chapter (summarised in 2.2.1) to argue that neither answer to [3] are obviously correct. In light of this, I will answer question [4] in chapters 4-7 by focusing on the reasons for pluralism (summarised in 2.2.2).

2.2.1 Plural variables

Table 1 summarises the potential variables for pluralism.⁴² Many of the accounts of pluralism outlined above focus on the presuppositions of scientific investigation. Jackson and Pettit's and Fodor's anti-reductionist arguments reject the notion that there is one privileged language or scale of analysis that should be preferred over others. They instead argue that the sciences should utilise a pluralism of *levels of analysis*. Explanations in terms of small objects (like particles) are not necessarily superior to those in terms of large objects (like social groups). Scientific laws can and should be made using generalisations in the languages of biology, chemistry, and economics, as well as the language of physics. Cartwright and Dupré agree but add that there can be disorder within and across levels. Cartwright argues that the *causal processes* of the world form a varied patchwork rather than an ordered system. Dupré argues against the idea there is one privileged way to parse the objects of the world (even given one level of analysis) and asserts a plurality of *kinds*. Different conceptualisations of things like species and sexuality can and should be used to develop different useful theories, hypotheses, and models.

A number of the arguments described in 2.1 take the existence of different goals and uses for science as a given and use that to argue for pluralism. But both abstract and concrete goals might also be taken to be plural variables themselves. Longino, Kitcher, and H. Chang all suggest that science should be open to a variety of abstract *epistemic goals*. Some scientific communities might focus on detailed descriptions of events and testimonies (maybe those concerned with understanding the practices of a specific tribe of people) whereas others might focus on maximising generality and

⁴² Other ways of categorising pluralism have been offered by Dutt (2014), Koskinen and Mäki (2016), and Mäki (1997).

Kinds of variables	Plural variables
Presuppositions	Levels of analysis Kinds Causal processes
Abstract goals	Values Epistemic goals
Concrete goals	Pragmatic constraints Questions Problems
Epistemic base units	Concrete standards Forms of representation Methods Causal inferences
Products	Explanations Representations (including models)
Compound variables	Styles of reasoning Systems of practice Ways of practicing science Strategies

Table 1. *The possible variables of pluralism.*

simplicity (maybe those trying to understand species population fluctuations). Although not noted above, implicit in Longino’s and Kitcher’s points that different scientists and communities might have different ends and make different judgements of significance is the idea that they are also in-part motivated by different *values* in general (not just epistemic ones). Values that can lead to different judgements about the amount of evidence required to accept or reject hypotheses (Rudner, 1953; Douglas, 2009; Wilholt, 2009), or to different things being emphasised when categorising the world. All of H. Chang, Longino, Kitcher, Mitchell, Hacking, Dupré, and Cartwright (and to a degree Jackson and Pettit) also suggest that it is legitimate for a plurality of concrete goals to also shape inquiry. Different abilities and contexts can give rise to different *questions, problems, and pragmatic constraints*, which can lead to different scientific representations.

The epistemic base units of inquiry (the things used to organise inquiry) also form a category of potential plural variables. Longino notes that science produces a variety of useful *forms of representation* (models, diagrams, pictures, as well as hypotheses, and theories). Within representations there might be further distinctions. Michael Weisberg (2012) has, for example, noted the variety of different kinds of models, and Kitcher (2001) argues that part of the reason that epistemic goals cannot be synthesised towards the single goal of explaining the world is because there are multiple legitimate forms of explanation. Longino, Hacking, and H. Chang also note the variety of *concrete standards* that scientists and communities of scientists use. These are based on values and epistemic goals but are their concrete instantiations; examples include different relations of conformation and different standards of measurement. Most of the accounts of pluralism discussed

above also affirm a variety of *methods* (or *causal inferences*). Although what counts as a method varies, it normally includes some combination of techniques, sources of evidence (interviews, models, different forms of measurement, et cetera), and standards of success.

The products of inquiry form another set of potential plural variables. The anti-reductionist arguments of Jackson and Pettit, Fodor, Dupré, Cartwright, and Kitcher all stress that a plurality of different *explanations* of a single event can be useful. Mitchell and Cartwright talk about different partial *models* covering different causal processes and parts of the world. And, Longino and Kitcher use the metaphor of maps to suggest that a plurality of *representations* (including models, theories, et cetera) can also be important.

Lastly, a number of the accounts above suggest what we might call compound variables for pluralism. H. Chang advocates a plurality of *systems of practice* that incorporate many of the other variables noted in table 1—including methods, kinds, standards, forms of representation and explanation, and collections of theories and assumptions. *Ways of practicing science* (Dupré), *strategies* (Kitcher), and *styles of reason* (Hacking) also fit into this category, as would things like Thomas Kuhn's *paradigms* (1962) and Imre Lakatos's *research programs* (1970).

(1) What does pluralism entail?

Pluralism can, thus, entail assuming plurality in various presuppositions used in scientific investigations; approaching investigations with different goals in mind; utilising a variety of standards, causal inferences, methods, and forms of representation (i.e., having different epistemic base units); embracing multiple explanations and representations; and/or some mix of these things.⁴³

2.2.2 Reasons for pluralism

The arguments for pluralism fit into three categories: (i) because pluralism is the correct description of *the way the world is* or the most appropriate way of arranging science given the way the world is; (ii) because pluralism provides an antidote to some of the *epistemic limitations* involved in science; and, (iii) because pluralism is the best way to make scientific practice compatible with the *variety of goals* involved in science. Some of the arguments in these categories relate to specific plural variables, but many apply to a number of variables. I will first describe how the arguments fit into the categories (i-iii) without reference to the variables in table 1, and then note the plural variables associated with each type of reason in table 2.

(i) The way the world is

Although small metaphysical claims are present in most arguments for pluralism, some are more explicitly metaphysical. For Dupré, the practices, categorisations, and theories of science provide

⁴³ Concepts like truth, objectivity, belief, and knowledge can also be the targets of pluralism. This kind of conceptual plurality will not be my focus, as it diverts attention away from what it means to be a plural science.

evidence that the world is such that it admits multiple levels of analysis and kinds and is not deterministic. Cartwright's argument that the sciences are characterised by a variety of models and causal inferences rests on a metaphysical argument that the world is dappled (or disunified). Mitchell, similarly, has the metaphysical claim that the world is complex at the core of her argument. The complexity of the world makes it necessary (in most circumstances) to integrate a variety of models to explain particular events. Hacking's idea of styles of reasoning is also self-consciously metaphysical. Styles make certain things possible and humans have developed and utilise a range of styles in part because truth as correspondence cannot cover all potentially truth-bearing sentences. Lastly, significant metaphysical claims are also part of Jackson and Pettit's and Fodor's anti-reductionist arguments outlined in section 2.1.1. The crucial step in their arguments is that moving from a higher to a lower level gives up something important, because of how the world is organised rather than because of the epistemic limitations of investigators.

(ii) Epistemic limitations

Many of the arguments discussed in 2.1 develop out of epistemic limitations involved in scientific practice. Underdetermination prevents the acceptance of hypotheses, experimental results, and measurements from ever being fully justified and necessitates a social dimension to knowledge production and, thus, pluralism. The nature of representation prevents the existence of one perfect representation for all uses; and even if a single fully justified perfect representation of a phenomena were possible, it is not clear how science would reach it. Scientists can't tell in advance which strategies will maximise their goals or the goals of society (even if they were static and clearly defined). Moreover, no single fully justified method for science or way of combining epistemic virtues has been developed, despite many attempts.

These limitations are epistemic in the sense that they are the result of limitations in what scientists do or can know and give rise to knowledge that is imperfect in some way (partial, not fully justified, et cetera). All of these epistemic limitations suggest that society at large (or science as a whole) should hedge by allowing for some form of variety in science. Kitcher's argument for the efficacy of a variety of research strategies falls into this category. As does Longino's argument that social critical interaction and the use of different representations helps overcome the justificatory gap created by underdetermination. Mitchell's argument that models can only include some factors and that integrating many models together can give a better picture of a phenomenon also fits here. Lastly, H. Chang's arguments that each science should hedge by containing a variety of systems of practice, that different systems can usefully satisfy aims in different partial ways, that integrating different approaches can be useful, and that different systems can benefit from interacting with each other all follow a similar logic. They rely on the insight that there is no perfect system of practice or that even if there could be, scientists are unlikely to be able to determine what that is.

Type of reason	Reason for pluralism	Plural variables implied ⁴⁴
(i) The way the world is	Propositions at higher levels of analysis cannot be reduced to lower levels without losing something	Levels of analysis
	The world is not made up of a privileged set of kinds or a privileged level of analysis	Kinds, Levels of analysis, Causal processes
	The world is not made up of an ordered set of laws or causal processes and/or is complex	Levels of analysis, Casual processes, Representations
	The failure of essentialism, reductionism, and determinism suggests a radically disordered world	Ways of practicing science, Causal inferences
	The correspondence theory of truth does not cover all sentences and representations	Styles of reasoning, Forms of representation
(ii) Epistemic limitations	Underdetermination implies a social dimension to science and knowledge production	Standards, Representations, Forms of representation, Goals
	Utilising a variety of strategies, methods, and systems hedges bets in the face of uncertainty	Systems of practice, Strategies
	Integrating different approaches can provide more complete pictures of particular phenomena	Systems of practice, Strategies
	Systems can be improved by co-opting ideas from others and through competition with others	Systems of practice
	A single method for science is not forthcoming, there can be many epistemic virtues	Abstract goals, Concrete goals
	There is no such thing as a perfect representation, different representations focus on different features of a target ⁴⁵	Representations, Abstract goals, Concrete goals
(iii) A variety of goals	The sciences explain different events and use different explanations for different ends	Explanations, Forms of representation, Levels of analysis
	Different communities have different capacities, processes, goals, and interests	Kinds, Standards, Representations, Forms of representation
	Representations should be judged with particular uses and contexts in mind	Representations, Kinds
	Basing science on a variety of goals allows for better political representation	Strategies, Representations
	Different systems and styles can be used towards different goals and questions	Systems of practice, Styles of reasoning

Table 2. The three types of reason for pluralism.

⁴⁴ If a compound variable is listed, then I have not listed variables that form part of that compound.

⁴⁵ This reason could also be placed in (iii).

(iii) *A variety of goals*

A third set of arguments for pluralism are based on the idea that science can and should serve a variety of goals and uses (both epistemic and pragmatic). Because the specific goals of science shift with context, some form of pluralism that allows for a variety of one of the variables discussed above is desired. Such arguments often take some plurality as given—for example that the sciences use different levels of analysis and different representations—and justify it by noting the different legitimate epistemic and pragmatic goals that scientific knowledge can serve. Within this category we might place arguments that kinds or levels of analysis should be defined relative to human processes, capacities, and interests (Jackson and Pettit, Kitcher, Dupré), and that different kinds of explanations might be used towards different ends. Longino’s argument that the social dimension of knowledge implies that a variety of contextual goals and interests legitimately influence science also fits in here. As do Kitcher’s arguments that representations should be judged with particular uses and contexts in mind, and that basing science on a variety of goals allows for better political representation. Lastly, H. Chang’s and Hacking’s arguments that different systems of practice and styles of reasoning can be used to pursue different goals and questions also fits here.

(2) *Why is pluralism desirable?*

Pluralists have, thus, argued that pluralism exists or should be encouraged because of (i) the way the world is, (ii) the epistemic limitations involved in inquiry, and (iii) the fact that the sciences can be used towards a variety of goals (or some combination of (i-iii)). Table 2 offers a stylised summary of the individual arguments for pluralism and how they fit into the three categories (i-iii).

2.2.3 Normative vs. descriptive pluralism

Before explaining what these distinctions mean for economics, it is instructive to note an important source of disagreement between some accounts of pluralism. H. Chang explicitly labels his version of pluralism ‘active *normative* epistemic pluralism’ (my emphasis) to distinguish it from *descriptive* accounts of pluralism. His normative position argues that sciences should actively cultivate a variety of systems of practice. Descriptive accounts of pluralism, by contrast, take pluralism to be the correct description of science or the world: the variety exhibited in the sciences suggests that characterising science as a unified project or assuming it will tend towards a unified set of theories does not make sense. Longino’s pluralism is presented as descriptive. Her main argument is not that the sciences should be pluralist, but that pluralism is the best way of describing the practices of science.

[P]hilosophical appraisal of research should encompass the full range of research approaches pursued in order to evaluate any one of them... the question becomes not which is the best or better, but what each contributes—both in terms of positive results and in terms of critical perspectives on the others—to our overall understanding of a given phenomenon. (Longino, 2013, p. 2)

Whereas H. Chang argues that pluralism is necessitated by coherentism and that it is the best way to arrange scientific investigations for multiple reasons, Longino focuses on showing that knowledge is developed by different communities with different results and that there is no reason to assume the different approaches should collapse into one. Whereas H. Chang aims to cultivate pluralist sciences, Longino wants philosophers of science to accept that the sciences already are pluralist.

Despite clarifying the evidential bases and implications of competing accounts of pluralism, the normative/descriptive division will not be central in how I use pluralism for two reasons. Firstly, the precise line between normative and descriptive accounts of pluralism is not entirely clear. Although H. Chang takes the strongest normative position, most accounts of pluralism have some normative elements. Longino's pluralism is based on descriptions of science but argues that philosophers should interpret science as a pluralist enterprise and this leads to other normative suggestions for science (democratised and representative institutions, for example). Although Dupré's focus is on metaphysics he has normative goals in mind too. He thinks that it is only with the metaphysical assumptions that he affirms that an open and democratic science that he supports can develop. Secondly, the debate regarding pluralism in economics is largely normative. 'Is economics pluralist?' is a descriptive question but behind it is a debate about what economics should be. What is really intended is a discussion about whether economics should be pluralist or whether it is sufficiently pluralist. The bulk of this thesis will, therefore, focus on the normative implications of pluralism even where the arguments for pluralism are in part descriptive.

Conclusion

In response to early twentieth century calls for unity in science, philosophers and historians have argued that the sciences are and should be pluralist. Arguments to this effect have taken numerous forms and resulted in numerous understandings of pluralism. Pluralists have taken issue with the metaphysical positions associated with the unity of science (Cartwright, 1983; Cartwright, 1999; Dupré, 1993; Fodor, 1974; Jackson & Pettit, 1992), pointed out that pragmatic considerations can lead to a variety of overlapping representations in science (Kitcher, 2001; Longino, 2008; Longino, 2013), and advocated the cultivation of different ways of approaching science (Chang, 2012; Kitcher, 2001; Dupré, 1993).

Pluralism can entail variety in the presuppositions used by science, the abstract and concrete goals of science, the epistemic aspects of investigation (standards, methods, forms of representation, et cetera), the products of science (the theories, representations, and explanations of science) and/or some compound of these things. Pluralists have argued for scientific pluralism for three collections of reasons: (i) because it is the right description of the world or the best way to organise science given how the world is organised, (ii) because pluralist scientific practices help mitigate some of the epistemic limitations involved in scientific practice, and (iii) because pluralism is the best way to

make the sciences compatible with the variety of different goals they do and should be used towards.

Now, what does this all mean for economics?

3 Is economics pluralist?

Much of the debate about pluralism in economics centres on disagreement about the answer to the question: ‘[3] is economics pluralist?’. Defenders of economics argue that the discipline admits more variety than critics notice, that it does better than critics think at investigating the real world, and that it does successfully develop many useful real-world interventions. Critics reject these points (see 1.2). Chapter 2 shows that if economists were to go to philosophers for clarity over what counts as pluralism, they would get a bewildering variety of options. What does this mean for the debate in economics? I will give two answers. First, the debate in economics runs into difficulties by focussing on comparing economics to ideal states of pluralism based on certain *variables*. But, second, the *reasons* for pluralism can offer concrete lessons for economics without going into the question of whether economics is (sufficiently) pluralist. In this chapter I make the first half of this argument—that focussing on variables leads to problems. I will make the second half of the argument in the chapters that follow.

Given the plurality of pluralisms available, disagreement about the status of economics is not surprising. Critics and defenders of the discipline might just have different accounts of pluralism in mind. The question ‘[3] is economics pluralist?’ can be answered in different ways by focussing on different variables. Defenders of economics can, for example, argue that it admits variety in the range of answers it gives to problems,⁴⁶ while critics could argue that it is overly reliant on methodological individualism (see 3.2). I will argue that the source of this disagreement is deeper still. Even if a specific variable (or set of variables) for pluralism were fixed, it would still not be clear whether economics should count as pluralist or not, because there are multiple plausible ways of mapping the plural variables onto the present state of research in the discipline. In addition to disagreement about which variables pluralism should focus on, there may be legitimate disagreement about the boundaries of specific variables and how much of each is required. Where some see multiple systems of practice in present economic research others see just one and the philosophical accounts of pluralism do not give us clear criteria to rule either way.

To make this argument complete I would have to go through all plausible accounts of pluralism and show that neither ‘economics is pluralist’ nor ‘economics is not pluralist’ can be inferred from them. This is a large task that would distract from the main points of this thesis, which are the concrete diagnoses in chapters 5-7. Instead, I will try to convince the reader that even when a plural variable is fixed, answering ‘[3] is economics pluralist?’ in a way that convinces anyone not already sympathetic to your answer is going to be difficult. I will do this by presenting a way of arguing that economics is not pluralist for two variables that *prima facie* offer the most promising routes to determinate answers

⁴⁶ In an interview with the *World Economic Association* (WEA), the prominent Harvard economist, Dani Rodrik, argues that: “There are healthy debates in the [economics] profession today on the minimum wage, fiscal policy, financial regulation, and many other areas too. I think many critics of the economics profession overlook these differences or view them as the exception rather than the rule. ...[E]conomics today is not a discipline that is characterized by a whole lot of unanimity.” (WEA, 2013, p. 10)

to [3]—compound variables (in 3.1) and presuppositions (in 3.2)—and showing that there is significant space for interpretation. I will then, in 3.3, briefly give reasons to believe that something similar is true for the other variables of pluralism. I will conclude that arguments that economics lacks sufficient pluralism are unlikely to convince those that think that economics is already pluralist enough and that they cannot offer concrete recommendations for change.

3.1 Compound variables

Many of the arguments about pluralism in economics, on both sides, focus on versions of pluralism defined around ‘schools of thought’—a compound variable involving a mixture of core theories and presuppositions, community association, and methods. A number of critics argue that economics should admit a broader range of schools of thought in order to better investigate the complex world (Arestis, 1996; Dow, 1990; Lawson, 1997; Lawson, 2003). On the other hand, Davis (2006) argues that the mainstream of economics should count as pluralist because it includes some important schools of thought that it did not 40 years ago (Colander, 2000, makes a similar point). As the debate in 1.2 showed, just identifying schools of thought (or some similar compound variable) as the key variable for pluralism does not necessarily lead to a clear answer to ‘[3] is economic pluralist?’ There is still significant space for interpretation about when schools of thought count as separate from one another and about which schools need to be included for economics to count as pluralist. Perhaps scientific pluralism can help draw clearer boundaries between schools of thought and determine how many schools of thought are required of economics?

H. Chang, Dupré, Hacking, and Kitcher all offer accounts of pluralism that have some role for compound variables. Kitcher (1993) argues that, given diminishing returns to more scientists pursuing a single strategy, scientific communities should aim to contain scientists pursuing a range of *strategies*. This is the case even if a community has a clear understanding of which strategy is most likely to be successful. If uncertainty about which strategies are likely to be successful is factored in, the rationale for pursuing multiple strategies seems even stronger. Something like this likely motivates some of the arguments for pluralism in economics. This does not, however, articulate what should count as different strategies or what should count as diversity and how much is necessary. Where Rodrik (2015) sees a diverse collection of different strategies in different models, Lawson (2003) sees one strategy (modelling). Dupré’s argument that disorder in the world should lead to a variety of *ways of practicing science* equally does not offer much guidance as to what might constitute different ‘ways’.

H. Chang’s (2012) *systems of practice* seem the best match for schools of thought accounts of pluralism. Like his brother H.-J. Chang’s (2014) argument for pluralism in economics, H. Chang argues that it is prudent for sciences to contain multiple systems of practice in the face of uncertainty and some of the epistemic limitations involved in science. Having different systems of practice is

important as they provide unexpected forms of resistance from reality. Resistance that helps sciences progress and helps justify coherentist systems.

Although H. Chang discusses systems of practice in greater detail than Kitcher or Dupré discuss strategies and ways of practicing science, his description of them—“coherent and interacting set[s] of epistemic activities performed with a view to achieve certain aims” (2012, p. 260)—still leaves a large amount of room for interpretation. If the goal is to describe and argue for pluralism in broad strokes this suffices. But it does not offer us much guidance in how to diagnose economics. H. Chang does, however, emphasise another factor that might. He thinks that the sciences ought to cultivate as many ‘incommensurable’ systems of practice as possible. Systems of practice are incommensurable when different standards of judgement—including the problems, epistemic values, and practice based metaphysical assumptions emphasised—prevent the evaluation of competing propositions across different systems.⁴⁷ This means that incommensurable systems of practice are likely to note different observations, arrive at different explanations, and generally provide different angles on reality.

Each system is conducive to revealing particular aspects of reality, and by cultivating multiple incommensurable systems we stand to gain most knowledge. (2012, p. 218)

Mapping this over to economics, maybe the criterion of difference between schools of thought could be incommensurability?

Unfortunately, this doesn’t solve the issue of identifying difference between schools, it just pushes it back one step. As noted in chapter 1, some see behavioural economics as a genuinely new way of practicing economics with new standards of judgement that have resulted in sharp disagreements with other parts of economics.⁴⁸ As an example they might point to the contrasting approaches to finance espoused by Robert Shiller (using behavioural economics) and Eugene Fama (using rational expectations) in their lectures for their joint (also with Lars Hansen) Nobel prize in 2014 (Fama, 2014; Shiller, 2014). Shiller emphasises the importance of experimental results in constructing his model of decision-making, whereas Fama stresses the importance of more parsimonious models of decision-making. Because of this Shiller incorporates behavioural findings that undermine the assumption of perfect rationality into his models of the housing and stock markets. This leads him to emphasise the ‘irrational exuberance’ in such markets and directly contradict Fama’s work, which that purports to show that stock and housing markets incorporate all information into prices efficiently.⁴⁹

⁴⁷ H. Chang refers to this as ‘methodological’ incommensurability. This is in contrast to ‘semantic’ incommensurability, which refers to cases in which some of the terms used in different systems of practice cannot be translated into one another. The distinction between these two types of incommensurability is due to Paul Hoyningen-Huene and Howard Sankey (2001). Because he spends most of his book talking about methodological incommensurability (with the chemical revolution being a key case), I interpret H. Chang (2012) as arguing that the sciences ought to cultivate as many methodologically incommensurable systems as possible.

⁴⁸ New epistemic values, for example, in the greater importance behavioural economics gives to experimental results over parsimony in formal models. New problems that behavioural economics sees as relevant by focussing on, for example, what drives choices, rather than the effects of an assumed model of choice.

⁴⁹ Shiller even pointed to the difference between the two approaches in his lecture (following Fama’s) for their joint award: “I guess my attitude is, ‘Why are we so interested in the efficient markets model?’ We haven’t seen any compelling evidence for it...Gene doesn’t agree with this at all...I’m going to present evidence, though” (Wile, 2013).

However, others interpret the relation between behavioural and other forms of economics differently. Sent (2004) contrasts the ‘new’ behavioural economics that has become common in economics departments and journals with ‘old’ behavioural economics. Old behavioural economics was practiced between the 1960s and 1990s by groups at Carnegie,⁵⁰ Michigan, Oxford, and Stirling.⁵¹ Sent argues that old behavioural economists were very critical of the assumptions and methodologies in economics at the time, in particular the use of equilibrium-based analysis and the focus on utility and profit maximisation. They sought to build an alternative approach to understanding decision-making by starting from empirical evidence and looking for laws that described behaviour as faithfully as possible.

New behavioural economics grew out of the work of Daniel Kahneman and Amos Tversky (Tversky & Kahneman, 1974; Kahneman & Tversky, 1979). Rather than starting from new theories of decision-making, work in new behavioural economics started from the same expected utility calculations common in economic theory and focussed on departures from that model. New behavioural economics sought to extend the explanatory power of economics by using heuristics, biases, and framing effects to explain why traditional rational choice models fail in certain circumstances, rather than replace it. Shiller is considered a new behavioural economist.

Sent argues that new behavioural economics’ ability to fit side by side with standard economic models—including the utility and profit maximisation and equilibrium analysis rejected by old behavioural economists—rather than replace them was a crucial factor in its rise at the expense of old behavioural economics during the 1980s and 90s. She quotes Colin Cramerer, a prominent new behavioural economist, as arguing that:

This sort of psychology provided a way to model bounded rationality which is more like standard economics than the more radical departure that Simon had in mind. Much of behavioral economics consists of trying to incorporate this kind of psychology into economics. (Cramerer, 1999)

This suggests that the incommensurability between the behavioural economics now common within economics departments and journals and other aspects of economics is not as clear as it might seem. New behavioural economics has offered some different standards of judgement that have resulted in the elevation of results and studies that disagree with the results endorsed by other parts of economics—like the contrast between the behavioural finance of Shiller and the traditional finance of Fama mentioned above. But do *some* different standards of judgement and *some* disagreements count as incommensurability? Disagreements that arise from slightly different standards of judgement can arise within systems of practice, and there are many ways that new behavioural economics is continuous with existing economic theory. Moreover, had behavioural economics gone down the ‘old’ route, it might have marked a significantly greater contrast with other parts of economics. How much

⁵⁰ Known at the time as Carnegie Institute of Technology, now part of Carnegie Mellon University.

⁵¹ Carnegie included Herbert Simon and focussed on bounded rationality. Michigan was led by George Kantona and focussed on attitude research. Oxford included Jacob Marschak and G. L. S. Shackle and focussed on uncertainty and coordination. Stirling included Peter Earl and Brian Loasby and focussed on eclecticism and integration.

disagreement is required for incommensurability? Those that see behavioural economics as a radical departure from what went before are likely to emphasise the ways that it is different and see incommensurability. Those that see it as a mild modification on the rational expectations models are likely to see continuity.⁵²

This resembles the impasse discussed in the introduction. Moving the criterion of difference to incommensurability doesn't resolve the issue of determining when schools of thought are genuinely different from each other. H. Chang makes a salient point that it is important for the sciences to be exposed to different angles on reality, but it is not clear how best to translate this across to a recommendation about what counts as a different angle.⁵³

Hacking's (1992) *styles of reasoning* are similarly too abstract to demarcate clear differences between schools of thought. Styles of reasoning are intended to be broader and more cross-cutting than schools of thought. They define conditions of possibility for claims to truth and objectivity. They come with their own accepted kinds of statement, objects, explanations, criteria of success, and methods of comparison, but are less attached to particular thinkers, traditions, and theoretical commitments than schools of thought. Yet, styles of reasoning may still be applied to the debate in economics. Rather than arguing that economics consists of a narrow school (or set of schools) of thought, critics may instead argue that all of economics utilises a particular style of reasoning. As noted in chapter 1, Hirschman and Popp Berman (2014) argue that economics might be thought of as defining its own style of reasoning:

The economic style of reasoning includes basic concepts such as incentives, growth, efficiency and externalities. It includes economic ways of approaching problems: by using models, systematically weighing costs and benefits, analysing quantitative empirical data, considering incentives, and thinking marginally. (Hirschman & Popp Berman, 2014, p. 794)

A critic of economics might buy this and argue that economics is not pluralist because it is dominated by a particular economic style of reasoning to the detriment of all others. But those that see pluralism in economics could have two responses. First, they could argue that the characterisation of economics as exhibiting its own style of reasoning rather than a mixture of the other styles is incorrect. They might argue that economics utilises modelling, statistical analysis, experimentation, and mathematical postulation, all separate styles of reasoning according to Hacking (and Crombie, 1988). Second, they may argue that even if economics can be said to define its own style of reasoning this need not exclude others. They may again point to the use of the modelling, statistical, experimental, and mathematical styles of reasoning in the discipline. Critics may respond to these points, but it's hard to imagine either side establishing a knock-down argument. Styles of reasoning are vague, intentionally so. Like Kuhn's paradigms and Lakatos's research programs, Hacking does not define styles of reasoning but illuminates them with examples. This gives the idea broad applicability and likely helps broad brush

⁵² For examples of these different interpretations, see the contrasting opinions on Richard Thaler's (a prominent new behavioural economist) 2017 Nobel prize win (Mata & Wright, 2017; Ritholtz, 2017; Shiller, 2017).

⁵³ In chapter 4, I will use this insight in a different way. Rather than trying to determine when angles count as different, I will suggest we look out for institutional blocks to particular kinds of angles coming into view.

historical descriptions of the development of science, but it makes it difficult to apply the idea of styles to cases of disagreement like the debate in economics.

Thus, none of the arguments for pluralism that focus on compound variables provide a clear way of determining an answer to ‘[3] is economics pluralist?’ They do not provide criteria for when schools of thought count as different amid dispute. Moreover, even if clear boundaries between schools of thought could be drawn, the philosophical accounts of pluralism do not give a clear way of determining how many schools are needed for pluralism or how the schools chosen should be determined. Without any of this it is going to be difficult to make an argument that economics is or is not pluralist to someone who is not already sympathetic to the idea.

This does not mean that compound variables are useless in discussing the different kinds of work going on in economics (or any science). Schools of thought can be useful for framing the material that users of economics (like the policy makers and the public that H.-J. Chang targets) and designers of economic curriculums draw on, and for discussing historical trends. The problems arise when using schools of thought to mediate in disputes about how contemporary research practices relate to one another. The fact that schools of thought form loose categories that are open to interpretation is part of their strength when offering an overview of different fields to students, but it makes them ill-suited to convincing an opposing interlocutor in an argument about pluralism.

3.2 Presuppositions

Instead of mixed concepts like schools of thought and styles of reasoning, might focussing on presuppositions help determine whether economics should count as pluralist? I will argue that the answer is no. Beyond arguing that reducing all *levels of analysis* to one is problematic, the arguments against reductionism do not tell us what is required of pluralism. Similar things can also be said of *causal processes* and *kinds*.

Economics has had a long history of debate about its basic units of analysis (Menger, 1871; Menger, 1883; Schmoller, 1873; Veblen, 1908). Some have defended approaches to economics that are based around one main unit (or type of unit)—normally individuals and their preferences (Elster, 1982; Menger, 1871; Menger, 1883). If we assumed that such approaches were characteristic of the whole of economics, and that the discipline only utilised one unit of analysis, then it would seem that the pluralist argument would be clear. The anti-reductionist positions in 2.1.1 and 2.1.2 argue that building from a single level of analysis gives up important generalisations, information, and kinds.

But the case is less open and shut than it seems. The simple fact that economics exists as a discipline separate from physics (and chemistry, biology, et cetera) could satisfy the anti-reductionist arguments of Fodor (1974) and Jackson and Pettit (1992). The idea that economics should investigate economic phenomena and create propositions or explanations in terms of economic phenomena

without trying to reduce them to physical phenomena assumes the existence of an economic level of description separate from physics, even if all those explanations are in terms of individuals and their preferences. Indeed, it was the separate existence of special sciences like economics that Fodor sought to defend. Once economics exists as its own discipline in any form, some form of anti-reductionism seems assured.

A pluralist may reject this line of reasoning by arguing that the things Jackson and Pettit and Fodor argue are sacrificed by reductionism need not just occur when reducing economics to physics and also occur when reducing different kinds of propositions in economics to a single economic level of description.⁵⁴ They might argue, for example, that focussing on individuals and their preferences obscures the visibility of other important causal factors—aggregate dynamics from the decisions of heterogeneous agents (Kirman, 1992), social norms (Lawson, 1997), links between finance and the real economy (Wren-Lewis, 2018), and so on. Such arguments rest on specific examples. To highlight how they play out I will briefly explain one set of arguments: those against microfoundations in macroeconomics.

In 1976, Robert Lucas famously argued that econometric models devised using only historical data cannot in principle be used to predict the effects of changes in economic policy.⁵⁵

[G]iven that the structure of an econometric model consists of optimal decision rules of economic agents, and that optimal decision rules vary systematically with changes in the structure of series relevant to the decision maker, it follows that any change in policy will systematically alter the structure of econometric models. ... [I]t appears that policy makers, if they wish to forecast the response of citizens, must take the latter into their confidence. (Lucas, 1976, pp. 41-42)

To be accurate, predictions of the effects of macroeconomic policy need to be grounded in analysis of the decision processes of individual agents. Macroeconomic models, thus, need to be *microfounded*; that is, they need to incorporate microeconomically determined models of individual agency. Microfounded models can be calibrated using aggregated historical data, but their functional form should be determined by the aggregation of individual behaviour consistent with microeconomic theory. Given the trends in microeconomics at the time, this meant that macroeconomics had to utilise well-specified optimisation problems.⁵⁶

Since Lucas's critique, microfounded macroeconomic models have proliferated (Kosnik, 2015). These include new Keynesian models, real business cycle models, and the models of dynamic

⁵⁴ Pluralists could also use this argument in circumstances in which economics assumes more than one base level of analysis (relaxing our assumption that assumes just one).

⁵⁵ As he acknowledges, the arguments in Lucas's critique were not new. Lucas and others (notably Edmund Phelps, who Backhouse and Boianovsky (2012) credit with laying the foundations for Lucas's approach) were already developing microfounded macro models in 1976, when Lucas's critique was published. But the timing and force of the 1976 paper is taken to be the determining factor in shifting macroeconomic practices.

⁵⁶ This was not always inevitable. As Backhouse and Boianovsky (2012) document, there was once a movement to develop disequilibrium microfoundations from Keynesian macroeconomics instead of using equilibrium microeconomics to change the macro theory. This movement, however, slowly collapsed after Barro, Grossman, and Howitt announced the end of the disequilibrium approach at the 1979 meetings of the American Economic Association (Grossman, 1979; Barro, 1979; Howitt, 1979).

stochastic general equilibrium (DSGE) theory. Microfounded models now dominate macroeconomic theorising and forecasting (Blanchard, 2016; Hoover, 2001; Romer, 2016). So much so that those that challenge the most popular microfounded models (those based on DSGE) were recently labelled “dilettantes”.⁵⁷

The dominance of microfounded models in macroeconomics is seen by critics as stifling and insufficiently pluralist. A number of critics have argued, in a fashion similar to Pettit and Jackson and Fodor, that even if macroeconomic processes are causally determined by or token identical to microeconomic properties, reducing the theories, generalisations, and explanations of the former to the later forces some important relations and properties to be ignored (Hartley, 1997; Hoover, 2001; Kirman, 1992; Wren-Lewis, 2018). Most arguments of this form attack the use of ‘representative agents’. In order to fruitfully aggregate individual decisions, microfounded macroeconomic models typically assume that all agents in a macroeconomy are the same. Microfounded models assume that aggregating the decisions of one ‘representative agent’ will suffice to model the macroeconomy. This representative agent approach has been criticised for confining macroeconomics to a limited set of circumstances, in which the representative agent approach might be appropriate (Stiglitz, 2018; Wren-Lewis, 2018). Further, Alan Kirman (Kirman, 1992) argues that the use of the preferences of a representative agent explicitly prevents the existence of other causal factors—like the dynamics that occur when the decisions of heterogenous agents are aggregated—showing up in macroeconomic models. Beyond the use of representative agents, Kevin Hoover (2001) argues that microfounded macroeconomic models also needlessly ignore the role simple non-microfounded macroeconomic models (or heuristics) play in individual decision-making itself. The details of all these arguments differ, but critics largely agree that insisting that macroeconomics be microfounded blocks research and information that might otherwise be important. Pluralists could conclude from this that economics is insufficiently pluralist in the presuppositions it utilises.

Defenders of the idea that there is already pluralism in economics could give three responses to this conclusion. They could, first, defend the importance of microfoundations in the same terms as Lucas, but argue that they are not as restrictive as critics think. They could argue that microfoundations are important to ensure that macroeconomics models are able to make accurate policy decisions through political and societal shifts. In the wake of the 2008 Global Financial Crisis this argument has lost popularity. Modified versions of it, however, are still offered by economists. Fabio Ghironi (2018), and Lawrence Christiano, Martin Eichenbaum, and Mathias Trabandt (Forthcoming) have argued that microfoundations are important for Lucas’s reasons, but that the microfoundations need to be updated to include things like heterogenous agents and firms. They argue that this is being done and that if it continues then there is no reason why microfounded

⁵⁷ An early version of a paper by Christiano, Eichenbaum, and Trabandt (Forthcoming) caused a storm by claiming that “People who don’t like dynamic stochastic general equilibrium (DSGE) models are dilettantes. By this we mean they aren’t serious about policy analysis.” The word “dilettantes” was cut from the final version of the paper, but the message remained: microfounded DSGE models are the only serious way of doing policy analysis. See p. 2 of the November 9, 2017, version of the paper for the offending comment at <http://faculty.wcas.northwestern.edu/~yona/research/DSGE.pdf> [accessed: 3/4/18].

macroeconomic models can't incorporate the causal factors some of the critics say they leave out (aggregate dynamics, the role of finance, et cetera). They, thus, deny the idea that reducing macroeconomics to microfoundations need imply any sort of loss. This means that either macro and micro levels are not distinct in the way that Jackson and Pettit and Fodor argue that physics and economics are, that microfounded models actually do speak to the macro level causal factors, or that all relevant macroeconomic causal factors are fully reducible to the micro level.

A second response might also point to changes in macroeconomics but drop the centrality of microfoundations. Olivier Blanchard (2018), for example, has argued that although macroeconomists should retain microfoundations in their theoretical and conceptual discussions, they might employ non-microfounded models in policy related circumstances. This would enable a closer relation between policy models and actual data, which would enable them to pick up on things like the role of finance but ensure that macroeconomic theorising can still develop new tools (to then supplement policy models) from a well-grounded (i.e., microfounded) base. Blanchard also argues that something like this is starting to happen and that it is the future of macroeconomics. He, thus, implicitly agrees that past techniques in macro have been insufficiently pluralist but argues that they are changing and being supplemented with other ways of doing things.

Lastly, those seeking to defend the idea that there is pluralism within economics might argue that the existence of various criticisms of microfoundations prove that there is a healthy debate in economics. Some economists take the microfoundations approach because they believe it to be the most fruitful way to get at what they want to investigate—in the same way that some scientists seek to understand biological processes through physical properties—but dissent exists. There is, thus, sufficient space within economics for disagreement, and a variety of presuppositions.

These three arguments—microfoundations are important and can incorporate new things, microfoundations are important for some uses but are rightly being supplemented for others, the fact there is debate shows there is pluralism—are all likely to be rejected by pluralist critics of economics. They are likely to flat out reject the idea that everything can be incorporated into microfounded models. They are likely to reject the idea that macroeconomics is changing or that there is sufficient debate in the discipline and to point to the fact that very few macroeconomic models used by economists are non-microfounded. They are likely to also claim that Blanchard's argument that microfoundations should remain the base of theorising is still too restrictive.

Without going into further details, two features of this debate are salient for present purposes. First, the accounts of scientific pluralism I presented in chapter 2 give reasons to believe that the sciences should utilise multiple *levels of analysis* but do not provide a metric for judging what counts as different levels or how many are required. Whether macroeconomics should count as utilising multiple levels depends on how the boundaries between levels are drawn. Those that argue that models based on individual behaviour can incorporate everything on the macro level deny that there is a distinction between micro and macro levels or accept that there are two levels but argue that models based on individual behaviour speak to both. Pluralists would deny both these points. Whether

macroeconomics is too reductionist depends on whether or not its present practices are interpreted as obscuring useful information. Those that argue that macroeconomics has changed and now incorporates enough or that this whole debate signals pluralism claim that present practices incorporate all the causal factors that are necessary. Pluralists would also deny this.

All of this might be re-written in terms of *causal processes* and *kinds* by arguing that microfoundations mark the dominance of a particular way of parsing the world at the expense of others. This is what Kirman (1992) argues is the issue with microfoundations—he argues that causal processes like the dynamics that occur on the aggregate level of decisions of heterogeneous agents are ignored. But defenders of economics could respond with the same three points above—microfoundations are important and not that restrictive, microfoundations are being supplemented, the fact there is debate shows there is pluralism.⁵⁸ Thus, as with schools of thought, the answer to ‘[3] is [macro]economics pluralist?’ with regard to presuppositions (including levels of analysis, causal processes, and kinds) is a matter of degree with multiple possible interpretations.

Secondly, even if we could conclude that one area of economics (macro, for example) does utilise an insufficient number of presuppositions, would that be enough to say that economics is not pluralist in general? Can a lack of pluralism in a part of economics condemn the whole discipline? Rather than concluding that economics is not pluralist it would be more accurate to conclude that macroeconomics does not make sufficient use of multiple levels of analysis given its purposes. But suddenly pluralism has disappeared. In its place are local reasons about the fruitfulness of particular reductions. Such local debates are important. It seems appropriate for practicing economists and philosophers to engage in debates about how many levels of analysis particular economic models or theories should employ (and which ones). But I don’t see a clear way to parlay such debates into a judgement about the state of pluralism in economics, in general. Moreover, it is hard to see how talking about such a state would help those debates.

3.3 Is economics pluralist?

Even if we were to fix a compound variable or some kind of presupposition as the target for pluralism, the question ‘[3] is economics pluralist?’ does not appear to have a clear answer. Certainly not one that would convince those who initially disagree. In this section, I will briefly argue that it is likely that the same can be said if pluralism is defined for economics around the abstract or concrete goals, epistemic base units, or products of economic research.

⁵⁸ More generally, it would be fairly easy to argue that economists examine multiple causal processes and utilise multiple kinds. The variety of models approach to economics recently popularised by Dani Rodrik (2015) and the rise in significance of empirical work (Hamermesh, 2013) both indicate that economists assume some plurality of potential causal processes and kinds. Whether this plurality is sufficient to match Cartwright and Dupré’s pictures is questionable, but, as with levels of analysis, the degree of plurality required is open to interpretation.

As with compound variables and presuppositions, without a clear criterion of difference, it is hard to determine whether the range of *questions, problems, pragmatic constraints, values, and epistemic goals* (i.e., abstract and concrete goals) economics focuses on are sufficient. Pluralists could argue that Pareto optimality, efficiency, and utility maximisation are too often the targets of economic theorising and interventions. But economists could respond that they actually study a wide variety of topics with different goals in mind. As an example, they might point to the Oregon Health Insurance Experiment (Baicker et al., 2013) that sought to measure the effect of health insurance on health outcomes and a number of other key indicators.

The same thing is true for *explanations, representations, and models* (the products of inquiry). Economic research contains many different models, explanations, and representations (Rodrik, 2015). For pluralists, this variety belies the narrow approach from which it comes from: economics may have many models, but they are all of the same sort. Economists may disagree and argue that they use lots of different kinds of models—some based on behavioural economics, some on complexity economics, some on more traditional forms of welfare economics, et cetera. This back and forth very quickly puts us back into the impasse discussed earlier. Without a clear criterion of difference or of how much variety is necessary, there is no clear answer to ‘[3] is economics pluralist?’ based on the products of inquiry either.

But, maybe the focus on modelling itself might be where economics fails to be sufficiently pluralist? Even those that see pluralism in economics acknowledge that modelling is central to the discipline. David Colander, for example, acknowledges that “the modeling approach to problems is the central element of modern economics” (2000, p. 137), and Rodrik (2015) defines economics around the use of multiple models. Moreover, text analysis of economics journals points to a long term rise of mathematics in economics papers, signalling an increased role for modelling (Kosnik, 2015). Maybe, if we focus on *methods, forms of causal inference, concrete standards, and forms of representation* (epistemic base units) as the locus of pluralism, then the adherence to modelling within economics can finally give a clear ‘no’ to ‘[3] is economics pluralist?’

Defenders of economics, however, are likely to point to the range of different modelling techniques used in economics (from statistical models, to large computational agent-based models, to two-dimensional optimisation models). They might also add that a lot of economics now focuses on more concrete empirical work (Colander, 2005; Hamermesh, 2013; Rodrik, 2015; Smith, 2018), with models used as mere communicative devices for suggested causal relations in large datasets. This discussion raises all the same kinds of questions as with the other potential variables: When does a method count as a single approach? When does a discipline count as having a single set of methods? And, how much plurality is required? All sciences draw some epistemic boundaries. What kind of boundaries are necessary for a science to be considered pluralist rather than monist? A number of other sciences (physics, chemistry, biology, psychology) blend modelling with experimentation and data collection. What is it about economics’ blend that is more problematic than others?

All this gives reasons to believe that even if a specific variable (or compound of variables) for

pluralism was fixed, answering the question ‘[3] is economics pluralist?’ in a way that convinces those not already sympathetic to your position is going to be difficult. Determining what counts as difference between variables and how many are required for pluralism is open to interpretation.

The arguments for pluralism described in chapter 2 each recommend the multiplicity of particular *variables*. Answering the question ‘[3] is economics pluralist?’ seems to require laying out the desired arrangement of plural variables and deciding whether some interpretation of ‘economics’ (say economic research in Britain and American universities) matches that arrangement. Arguments for pluralism in economics typically follow this line of reasoning (Davis, 2014a; Turner, 2012). They typically claim that economic research does not match a given arrangement of certain variables. Some commentators on scientific pluralism also see pluralism as a particular arrangement of certain variables. Inkeri Koskinen and Uskali Mäki argue, for example, that: “Pluralism either justifies the actually obtaining (kind and degree of) plurality or recommends that there be (higher than-actual degrees or other-than-actual kinds of) plurality in science” (2016, pp. 423-424). Moreover, given that the word ‘pluralism’ calls to mind the counting of some thing, it seems natural to focus on plural variables in determining what pluralism should mean for economics.

But, as this chapter highlights, focussing on how economics compares to a particular arrangement of certain variables is difficult in practice. Although comparing economics to different potential pluralist configurations helps explain why there is a debate about whether economics should count as pluralist or not—too many variables and no clear way to determine how much of each is required—doing so does not offer an obvious way out of that debate. There is a lot of room for interpreting what should count as sufficient plurality in practice. Answers to the question ‘[3] is economics pluralist?’ can choose from the range of possible plural variables listed in table 1 and interpret the boundaries of variables and how much of each variable is required in multiple ways.

In making this argument, I do not claim that there can never be fruitful discussions about pluralism in economics, or elsewhere. There may be cases in which economists can quite easily agree on whether schools of thought or levels of description should count as distinct, and there may even be cases in which they disagree but where there is enough agreement that they can convince one another. I am not implying that there can never be a clear agreed distinction between schools of thought, presuppositions, incommensurable systems, et cetera. Outside of economics, there are many circumstances in which philosophers identify and agree on incommensurable differences between theoretical frameworks or systems of practice. Given some agreement, it may be helpful to ask: does our economics department require slightly more of X in order to more likely produce valuable models?

The issue I point to concerns cases of disagreement and specifically the present state of disagreement about economics. Arguing for change in how economic research is organised by arguing that economics should match a given arrangement of certain plural variables is unlikely to convince those that think economics is already pluralist enough. Due to the range of interpretations available,

such arguments are also unlikely to identify unambiguous avenues for change. Some see monism where others see variety. Some think economics needs more of a given variable, others think it is fine as it is. I neither see a clear principled way to determine who is correct, nor think that looking for one is the most effective way to note problems in present economic practice. Continuing such an abstract debate in the face of so much disagreement is only of so much use. Given the societal significance of economic research, it is important to focus on offering concrete avenues for improvement and not get bogged down in a debate that is many steps removed from potential avenues for change. In the chapters ahead, I aim to do just that. Instead of seeking to match economics to an abstract ideal, I will evaluate social epistemic practices in economics by starting from concrete and measurable features of the discipline.

The abstract accounts of pluralism do, however, have something to offer in this task. First, as the above highlights, the philosophical accounts of pluralism help to clarify key aspects of the disagreement about economics—different interpretations about the variables of pluralism and their boundaries. Second, and more importantly, the *reasons* for pluralism provide important insights into how the social epistemic practices in economics might be improved. I will show how in chapters 4-7. Focussing on improvements suggested by the reasons for pluralism rather than comparing economics to a particular arrangement of plural variables mirrors one aspect of the debate between ideal and non-ideal theory in political theory: whether normative political theory should focus on ‘end-states’ or ‘transitional-states’ (see 1.3). I will use the reasons for pluralism to make concrete recommendations for improvement in the social epistemic practices of economic research, rather than recommend an ideal end-state that economics should emulate.

Conclusion

Arguments that economics should be more pluralist are typically based on the claim that economic research does not match a particular arrangement of certain variables (methods, levels of analysis, epistemic values, schools of thought, et cetera). Interpreting pluralism this way does not lead to concrete recommendations for change because it leaves too much open. The accounts of pluralism explained in chapter 2 do not offer a clear way to answer whether economics is sufficiently pluralist or not, let alone what would be required for it to become more pluralist. The issue is that there are too many ways of interpreting what pluralism should mean. There are too many variables available, and even if a specific variable (or set of variables) for pluralism were fixed, there are too many ways to interpret how many of that variable economics has and should have. In chapters 4-7, I will offer a way out of this problem by focussing on the *reasons* for pluralism and the partial improvements they suggest to social epistemic practices, rather than comparing economics to an ideal state defined around specific plural *variables*.

4 An evaluative framework

The debate about pluralism in economics is stuck. This is in part a reflection of the range of variables pluralism could focus on and the range of ways in which they might be interpreted in practice. Like those who argue for pluralism in economics, I seek to evaluate how research in the discipline is organised. I will do so, however, by pointing to measurable social epistemic practices in the discipline rather than comparing economics to an abstract arrangement of plural variables. In doing so, I hope to point to more concrete issues that offer clearer pathways for change. This does not, however, mean that abstract arguments for pluralism offer nothing. In the same way that thinking about why political ideals might be desirable can offer some guidance for more immediate political improvements, even if they do not completely determine those improvements, the reasons why pluralism is desirable offer some insights into how social epistemic practices in economics can be improved. In this chapter I show how.

I will once again draw on the arguments for pluralism outlined in chapter 2. But this time I will focus on the *reasons* for pluralism and the social epistemic practices they suggest, rather than ideals of pluralism defined around particular *variables*. I will use the reasons for pluralism to develop three heuristics that I will later use to evaluate the social epistemic practices in economics. In 4.1, I will argue that the arguments that pluralism can mitigate some of the *epistemic limitations* involved in science (reasons for pluralism of type (ii) from chapter 2) can be used to highlight the importance of progressive *feedback* in science. Social epistemic practices can be evaluated by determining whether or not they block important avenues for such feedback. I will then, in 4.2, use the arguments that pluralism is necessary because the sciences can serve a *variety of goals* (iii) to argue that social epistemic practices can be evaluated by asking whether they block the influence of certain kinds of *interests* (in 4.2.1) or avenues for *public scrutiny* (in 4.2.2). In developing these three heuristics, I leave the arguments that pluralism is the best way to describe the way the world is (i) to one side, for reasons explained in 4.3.

I will explain how these three heuristics can be applied to economics to evaluate concrete issues in the way that research is organised in chapters 5-7. Together these three heuristics and the issues I use them to evaluate provide an answer to question [4]: what should be inferred from pluralism for practice in economics?

4.1 Epistemic limitations and feedback

H. Chang, Longino, Mitchell, and Kitcher all argue that pluralism helps to mitigate certain *epistemic limitations* involved in scientific practice (ii). Longino argues that pluralism is a necessary result of the social interaction required to justify knowledge in the face of underdetermination. Kitcher argues that

it is prudent for scientists to pursue different strategies in the face of uncertainty. H. Chang argues that tolerating different systems of practice can help overcome the facts that scientists can never be sure which practices will be most suitable to attain a particular goal; that different ways of practicing science only seem able to give partial pictures of given phenomena; and that encouraging interaction between different systems of practice helps to progress each individual system (via competition and co-opting ideas). Lastly, Mitchell argues that pluralism is the necessary result of the idealised character of modelling. I will show that all of these arguments, bar Mitchell's, can be interpreted as suggesting mechanisms for increasing *feedback* to scientific representations and practices, where feedback refers to any process by which scientific knowledge and practices are exposed to resistance. It can come in the form of empirical testing, competing models, but also social critical interaction. I will then suggest that social epistemic practices—that is social arrangements and practices that bare on knowledge acquisition, retention, assessment, and transmission— can be evaluated by checking to see if they block mechanisms for feedback.

Longino (2002) argues that social critical interaction between scientists is necessary to ensure that the background assumptions they rely upon to bridge the justificatory gap caused by the underdetermination of theory by evidence are reasonable. Social critical interaction provides a mechanism for correcting falsehoods, inappropriate assumptions, biases, and other failings. Pluralism comes as a consequence of this. Different scientific communities can have different goals and develop different legitimate standards. The knowledge accepted by the social critical interaction of different communities will, therefore, likely differ. As noted in 2.2.3, this is intended as a descriptive argument, but it also has a normative dimension. Longino's argument is both that knowledge is plural, and that scientists and philosophers of science should accept a plurality of representations within science because of the social way that knowledge is determined.⁵⁹ Openness to a variety of representations is one component of a well-functioning set of norms of critical interaction. Pluralism is part of the package needed to help scientific knowledge get over the justificatory problem arising from underdetermination. Science should, thus, exhibit pluralism, with social critical interaction within and between scientific communities regulating the boundaries of that pluralism.

But the argument that productive social interactions between scientists help the sciences develop stands alone. The step to pluralism is extra. Through challenges, corrections, and enrichments, social critical interaction provides a *feedback* mechanism for scientific knowledge. It is this feedback that bridges the justificatory gap caused by underdetermination. The feedback from social critical interaction provides checks that aim to ensure that the background assumptions different scientific communities use, as well as the knowledge they hold, are reasonable and directed towards suitable ends. Scientific knowledge may never be infallible, but at least its robustness can be increased by

⁵⁹ Longino states, for example, that “on a theoretical level pluralism might give rise to contradictions, but this should not lead to an idea of competition where one theory is chosen. Instead multiple theories must persist but be made into single explanations (i.e., undergo competition) only when explaining particular events.” (2013, p. 146)

ensuring it is regularly challenged and critiqued.⁶⁰

This picture draws heavily from John Stuart Mill's classic argument for freedom of thought and expression in *On Liberty* (1859). Like Longino, Mill sees criticism as a crucial source of justification for knowledge. Human rational beliefs (and actions) depend on the corrigibility of errors. Mistakes are possible but rectifiable as long as open discussion and criticism is available to guide people away from error.

The whole strength and value, then, of human judgement, depending on the one property, that it can be set right when it is wrong, reliance can be placed on it only when the means of setting it right are kept constantly at hand. (Mill, 1859, p. 146)

The truth of particular propositions can only be presumed if every opportunity has been afforded to efforts to disprove them. Mill adds that criticism is also important for our ideas to remain alive. To hold our beliefs rationally, we must understand the reasons for them, and so must also understand reasons against them. Without this, our understanding of our beliefs wanes and we are less likely to act on them. This turns them into dead dogmas, no longer being acted on effectively, but also blocking the ground for new ideas. Moreover, in addition to legitimising what we think we already know, Mill notes the obvious potential for critical positions to teach us new things. Criticism can lead us to replace whole propositions, and also to mix and synthesise what is correct in competing positions. He sees the social process of dialogue between competing truths as one of the most important mechanisms humans have for developing new, and better, ideas (see chapter 7, for a further discussion of this).

Mill, thus, foreshadows Longino's argument that the feedback provided by social critical interaction has a crucial role to play in grounding and improving knowledge. Critical feedback tests and corrects knowledge and justifies the retention of knowledge that is not rejected in cycles of challenge and response. It keeps our knowledge fresh and ensures we continue to act on it, rather than letting it become dead dogma. And, the interplay of competing truths can be an important mechanism by which our knowledge develops.

Social criticism is not the only mechanism for feedback available. There are many ways that knowledge can be challenged, probed, and tested. Cycles of challenge and response are famously central to Karl Popper's (1972) account of how science progresses. It is by comparing the predictions of theories to empirical observations that they are falsified (or revised). Otto Neurath's famous boat metaphor, similarly, emphasises the importance of never-ending cycles of iteration between scientific representations of the world and new observations (Cartwright et al., 2008).

H. Chang (2004) also emphasises the importance of iterative cycles of improvement. He argues that scientific systems are enriched and/or corrected via mechanisms that provide 'resistance from

⁶⁰ My understanding of robustness is based on Wimsatt (1961): a finding is *robust* when it is found to be invariant under independent measurement, derivation, or identification processes. Scientific knowledge can increase its robustness when it is shown to fit with a larger array of independent observations, models, uses, forms of reasoning, and critical perspectives.

reality'. By pointing to the progressive power of iteration, H. Chang locates the justification of scientific knowledge in the same place that Longino and Mill find it: in its ability to self-correct and to react. But H. Chang's focus is wider than Mill's. He focuses on systems of practice, meaning that he thinks iteration also helps to progress scientific methods, measurements, techniques, instruments, and abilities, rather than just representations or scientific knowledge.⁶¹ Moreover, instead of the social practices that Longino and Mill focus on, H. Chang emphasises repeated interaction with external reality as the mechanism of correction. This recommends a similar process to Popper's falsificationism, but with a looser understanding of what it means to interact with external reality and without Popper's insistence on a single system.⁶² As we saw, H. Chang then argues that having multiple systems of practice bouncing back and forth with reality can help scientific communities move towards whatever their goals are faster by offering them more paths to success. Like Mill, H. Chang also suggests the competitive interplay of different ideas as a mechanism to progress scientific knowledge and notes that systems might borrow ideas from each other.

Like Longino, then, H. Chang (2012) emphasises iteration in the development of science and pluralism as a mechanism of multiplying the avenues for iterative improvement. As with Longino's argument, the insight that iterative resistance is an important mechanism by which science progresses stands with or without the conclusion that some ideal of pluralism best maximises it.

Kitcher's (1993) point is similar. He argues that, under reasonable assumptions about the probability of success for different research strategies, communities with scientists that pursue different strategies are more likely to reach successful outcomes. This is because it is not clear what strategies will be successful, because different strategies can bring different useful information, and because the opposition between different strategies can stimulate each other. Increasing the ways that scientific communities interact with the world (and so draw feedback from it) can be fruitful under uncertainty.

Longino's, H. Chang's, and Kitcher's arguments for pluralism, thus, all suggest that different mechanisms for feedback should be utilised to help develop scientific knowledge and overcome some of the limitations inherent in scientific practice—underdetermination, no clear method to study what is unknown, no way to ensure the production of infallible knowledge, no clear foundational knowledge. Rather than taking the extra step to pluralism, I will focus directly on feedback. As my first heuristic, I suggest that the social epistemic practices in a given scientific community can be evaluated by:

H1: Looking for blocks to productive forms of feedback.

Feedback can come in many guises. It refers to any process by which scientific knowledge and practices are exposed to resistance, be that through ethical challenges, reasoned elaboration, new

⁶¹ Longino also talks about how community practices and forms of reasoning can be developed through social critical interaction. But her discussion mainly focusses on improvements in the knowledge that comes from these practices. H. Chang, on the other hand, stresses the equal billing of progress in scientific practices and abilities with knowledge.

⁶² This is implicit in Popper's assertion that: "a new theory... must always be able to explain fully the success of its predecessor." (Popper, 1981, p. 94)

observations, criticism, or empirical testing. Popper's already noted processes of conjecture and falsification of theories is one of the forms of feedback most well-known among scientists. But theories need not be the only things exposed to resistance. Neurath argued that observations and methods must also be revisable, and H. Chang stresses the importance of iterative improvement of abilities as well as scientific representations.

Popper, Neurath, and H. Chang's understanding of feedback come from interactions with external reality that provide new observations, refine abilities, and stimulate the creation of new representations. There are also other ways that scientific representations and abilities can be improved. Take the case of theoretical economic models, for example. Observations sometimes help to refine such models. But reasoned criticism or the development of simpler or more general models without new observations can also result in refinements. As noted by Longino and Mill, social critical interaction can also provide mechanisms for testing and replacing ideas by highlighting new observations and experimental results and/or suggesting alternative forms of reasoning, background assumptions, standards, values, or presuppositions. This is particularly helpful for ideas that are hard to operationalise empirically.

Feedback can come, thus, in the form of experiments designed to falsify a theory, social critical interaction that questions and clarifies a methodology, and everything in between.⁶³ Feedback can come from competition, the ways that research is financed, the application of knowledge in technologies or policy, and even simple things like presenting ideas to those without the same background assumptions or knowledge. It would be beyond the scope of this thesis to discuss all the possible mechanisms for testing and challenging scientific knowledge and practices. My point is, rather, that we can evaluate social epistemic practices in science by asking whether they block avenues for testing, resistance, and challenge. The loose category 'feedback' will suffice as a label to cover the various ways that philosophers have suggested that scientific knowledge and practices be exposed to resistance. In looking at feedback mechanisms in general, I extend H. Chang's insight that resistance from reality is an important way to develop science by adding Longino's point that social interactive processes can also provide crucial feedback. I also extend Longino's argument by adding H. Chang's insight that it is not just the outputs and background (assumptions) of science that should be tested in processes of feedback, but also the abilities.

Productive feedback is feedback that brings information, arguments, or observations to a scientific community that enable the enrichment, correction, or justification of the knowledge or practices in that community. H. Chang (2004) argues that theories and observations can only be justified by coherence within an existing system. But those systems can be said to improve iteratively through resistance from reality. Adding social forms of feedback to this, new observations, information, and arguments can help enrich or correct scientific systems. When feedback provides this, it is productive. Feedback that leads to greater justification of knowledge or practices in a community can also be productive by giving greater warrant for action. Bringing more information, arguments, or observations to bear on

⁶³ The idea that drawing on different forms of feedback to refine our practices and understanding of the world is important has also recently been picked up by those working on the philosophy of mind (Clark, 2015).

the knowledge and methods of a scientific community amounts to an improvement in the productive feedback in that community. As does bringing information, arguments, or observations that are more challenging or novel from the perspective of the community's assumed knowledge.

The idea that productive feedback improves knowledge is compatible with each of the four most popular understandings of scientific progress (Dellsén, 2018): as the solving of contextually determined problems (Laudan, 1978), as increases in truthlikeness (Niiniluoto, 2014), as increases in justified true belief (Bird, 2007), or as increases in understanding (Dellsén, 2016). The problem-solving account fits best. Feedback brings new information and ideas to bear on problems and challenges proposed solutions until they work (to whatever degree required). But even if scientific progress were defined in any of the three other ways, productive feedback also comes up as progressive. Testing hypotheses against reality, critiquing assumptions, and challenging the rationality of inferences all aim to increase the truthlikeness of knowledge. Bringing new information, perspectives, and observations to bear on scientific knowledge can also increase the justification (as well as truthlikeness) of beliefs. And, understanding can be improved by gaining new information, being forced to explain one's knowledge in new ways, and refining one's knowledge in the face of challenges.

H1 suggests looking for social epistemic practices that block productive forms of feedback. This takes a negative rather than a positive approach. Instead of evaluating all the mechanisms that scientific communities utilise for feedback, **H1** suggests looking for aspects of social epistemic practices that block feedback that if altered would lead to more productive feedback within the community, other things being equal. In many contexts there will be many different ways that social epistemic practices might be altered to allow for more feedback, some of which may pull in different directions. Determining the right balance of feedback enhancing techniques for different disciplines and kinds of knowledge is not going to be easy. I doubt an ideal balance can be determined for any discipline. The best avenues to pursue will have to be determined in context specific evaluations of the trade-offs involved. But looking out for the ways that social epistemic practices block feedback is the first step.

I will use chapters 5-7 of this thesis to explain how the feedback in a specific context can be improved. I give some examples of ways that productive feedback is blocked by the social epistemic practices in economics. A poor gender balance, a steep hierarchy, and a dismissive attitude to outsiders mean that economics forgoes productive forms of feedback that might otherwise progress the discipline.

4.2 Choosing goals: representation and scrutiny

Another set of arguments for pluralism outlined in chapter 2 cite (iii) the *variety of goals* that the sciences can and should be directed towards as a reason for pluralism. Because the specific goals of

science shift with context, some form of pluralism is desirable. There are many legitimate epistemic ends economic knowledge can serve—including parsimony, generality, empirical adequacy, consistency, scope, et cetera—and multiple potential uses for economic knowledge. Some aspects of economics may be used to make predictions (however loose) about future states of the economy. Other parts of economics may aim to aid the design of particular policy interventions (perhaps helping determine the rate of a new tax). Still other aspects of economics may aim to explain or aid the understanding of certain economic phenomena (strategic decision-making via game theory, for example). But it is not only variety in *epistemic* goals that (iii) target. The arguments for pluralism in category (iii) assert that because the sciences can serve different *epistemic and pragmatic* goals, it should be expected and accepted that they utilise a variety of methods, assume different presuppositions, create different representations, et cetera.

For Longino, underdetermination implies that knowledge can only be justified within the critical practices and standards of particular scientific communities, and those communities can reasonably use different background assumptions, apply different standards, and target different pragmatic goals. Similarly, H. Chang argues that scientific knowledge and practices can only be justified by coherence within a progressive system, and that different systems can be developed towards different pragmatic aims and utilise different epistemic virtues. Hacking also makes a similar point about styles of reasoning. Kitcher argues that because the ways that representations cohere with or depart from reality depend on the ways that they will be used, representations should be judged with particular contexts and uses in mind. Kitcher and Jackson and Pettit argue that something similar is true of the kinds and levels of analysis used in science.

The point from all these arguments is that, given that there is not a single goal or use for scientific knowledge, epistemic or otherwise, sciences that are suitably open should develop a variety of representations and ways of interacting with phenomena. As we saw in chapter 3, the potential version of pluralism these arguments recommend underdetermine practice. But they can have implications for scientific practice without necessitating some pluralist end-state. As a first step, it seems helpful to ensure that the social epistemic practices of any given science are open to a variety of different goals.

This approximation is just one step removed from pluralism. As stated, it might be just as difficult to apply to practice. Should maximum variety be aimed for? If not, how much variety? My suggestion is to ignore these questions and look at the problem in a different way. Rather than asking how much variety is desired (and aiming for a particular state), I suggest it is helpful to look for instances in which certain kinds of goals and uses are *illegitimately* constrained. But, what does it mean for the social epistemic practices of a community to ‘illegitimately’ constrain certain kinds of goals and uses? This question has two dimensions. What kinds of goals and uses should be considered? And, by what mechanism can the decisions of scientists to focus on certain goals and uses over others be legitimate? To answer the first question, I will, in 4.2.1, expand on Kitcher’s argument that the sciences serve different ends. I will argue that the social epistemic practices of sciences should ensure that the interests of all constituencies (in the societies they serve) play a role in determining the goals and uses of science. I will answer the second question, in 4.2.2, by arguing that scientists can only legitimately

ignore certain kinds of goals and uses if their decisions are open to public scrutiny.

4.2.1 Representation

How should the goals and uses of the sciences be decided? Kitcher (2001; 2011) attempts to answer a similar question. He argues from the absence of a simple epistemic goal for science to the democratic oversight of science. The basis of his argument is the claim that science does not just search for any old truth, but for significant truths. Some truths are more significant than others. Although some comparisons of significance seem obvious—the structure of the genome seems more valuable than the number of bottle caps that can be placed between Cambridge and London—⁶⁴many are not. Scientists must judge the significance of the truths they seek in order to determine what to investigate. These judgements do not just come about in deciding the goals of inquiry (as suggested by Max Weber, 1949), but are replete throughout science. They occur in determining resource allocation, how projects should be pursued, the weight of different pieces of evidence, and how truths should be used.

Kitcher (2001) argues that significance cannot be purely pragmatic. If significance were determined purely pragmatically, then areas of inquiry without immediate practical relevance would be neglected. He also argues that attempts to formulate context-independent epistemic grounds for significance have failed. Kitcher argues that directing science towards a unified picture of the world cannot provide an epistemic criterion of significance because it is not clear why unity should be so desirable. He argues that the same is true for making causal processes or laws the centre of epistemic significance. Learning about causal processes helps us intervene in the world. But, if we assume that significance is more than just pragmatic, then a question remains why causal processes are valuable. Moreover, some causal processes might seem more significant than others. Likewise, learning about natural laws (should they exist) might be useful, but separate from practical concerns it is not clear that laws demarcate significance and some laws might seem more significant than others. Kitcher thinks that the most promising epistemic criteria for significance comes from explanations. But he argues that if explanations were to ground significance, then the explanations provided by science must be systematic in some way. If science just provided a long list of explanations, one for each truth in each possible context, then every possible truth would figure somewhere in the list and ‘providing explanations’ would not be able to determine between more or less significant truths. He dismisses the idea that scientific explanations could be systematic in some way for the same reason that he argues for a plurality of kinds (explained in chapter 2): there does not seem to be a way to reduce the concepts of higher level sciences to lower level concepts and human interests play a role in determining what counts as phenomena.

Kitcher concludes that significance is determined by what seems surprising to us from the perspective of our capacities and cultures. Questions we take to be significant and how we try to answer them co-evolve with our practical projects. This means that, instead of being purely epistemic or pragmatic, Kitcher thinks that judgements of significance mix epistemic and pragmatic

⁶⁴ This is a modified version of the famous example attributed to Richard Popkin.

considerations through combining practical interests and curiosity from the perspective of our capacities. Thus, even if the main goal of the sciences were truth (as Kitcher thinks it should be), context dependent epistemic, pragmatic, and value-based considerations enter the process by helping to determine the truths that are significant.⁶⁵

If there is no context independent way to determine scientific significance, then issues important to certain kinds of people can easily be ignored if their perspectives do not form part of the context of science. This has long been something that feminists have pointed out. Julie Nelson, for example, has noted the failure of economic metrics like GDP to pick up female forms of labour (Nelson, 1995). There are at least two ways that certain perspectives can be left out of judgements of significance. Firstly, voices representing particular perspectives and interests in society may not be involved in the situations in which significance is judged (including in funding decisions prior to inquiry, and throughout choices made during inquiry). One way this might happen is if the only way to be involved in judgements of significance is to have a certain type of education not accessible to all. Secondly, certain kinds of voices may not be given equal weighting in discussions about significance. This might happen for legitimate reasons—those that consistently get particular agreed facts wrong might, for example, be considered too ill-informed to have equal voice to others. But it can also happen for more troubling reasons. Miranda Fricker (2007) has pointed to cases in which prejudices towards particular social types systematically negatively skew their credibility. She labels instances of this ‘testimonial injustice’. She gives the example of a black witness at a trial not being believed by prejudiced white jurors as an example of such an injustice.

There are at least two issues that might arise if conversations about significance lack certain voices or afford lower credibility to certain kinds of voices. Firstly, particular social groups may offer unique perspectives and abilities. If those groups are either excluded or marginalised, then judgements of significance may not draw on important information that their perspectives and abilities provide. Secondly, particular social groups may have unique interests that are not well represented in the rest of the population. If those groups are excluded, marginalised, or otherwise not heard in some way, then their interests are unlikely to figure in judgements of significance. This can make it seem as if the things they are concerned with do not exist. It can also contribute to what Fricker calls ‘hermeneutical injustice’, where society lacks the resources to understand the experiences of particular kinds of people (the example Fricker gives is the experience of sexual harassment before the concept entered public discourse). If the sciences have ambitions to be a positive force for society, as Kitcher and many scientists think they should, then systematically ignoring particular interests in judgements of

⁶⁵ Kitcher’s argument is just one way of articulating how science involves more than purely epistemic judgements. Longino (2002) points to several social aspects of scientific knowledge. A number of scholars have argued against the idea that the sciences can make value-free inductive inferences (Rudner, 1953; Douglas, 2009; Wilholt, 2009; Wilholt, 2013), and have attacked the idea that the sciences can view the world from a perspective (Putnam, 1981; Rorty, 1979; Rorty, 1991). Lastly, feminist philosophers and historians have discussed the role socially determined perspectives play in scientific judgements, inferences, framings, and methods (Haraway, 1988; Harding, 2015a; Harding, 2015b). I endorse a number of these arguments but will not explain them in detail. I use Kitcher because his position is likely the least controversial for economists, and because the ideal he recommends is useful foil for the heuristic I develop.

significance seems like a problem.

To forestall this possibility, Kitcher (2001) suggests an ideal for science based on deliberative accounts of democracy. Deliberative democrats argue that judgements about values are only legitimate when they emerge from discussions in which all participants come as equals (Dahl, 1956; Gutmann & Thompson, 1996; Gutmann & Thompson, 2004; Rawls, 1993; Rawls, 1971).⁶⁶ Since judgements of significance involve values and, like values, do not have an obvious context independent foundation, Kitcher thinks that legitimate decisions about significance must similarly emerge from equal discussions. Kitcher, thus, argues that, for a science to be *well ordered*, the significance judgements it makes should approximate an ideal deliberation between all members of society (or a representative sample determined by splitting people into groups with common perspectives), in which the participants come as equals. Kitcher thinks that such an ideal deliberation would have three stages: determining how resources should be allocated, determining how projects should be pursued, and determining how findings should be used. The first and third stages should involve discussions that respect the preference of all in the deliberation, as well as some form of aggregation that considers the needs of all, aided by a body of disinterested experts (selected by the deliberators). In the second stage, deliberators should decide between possible operational paths—determined by a disinterested arbiter using conclusions from the first stage of the deliberation—using an agreed upon decision mechanism (utility maximisation, for example).

Ideal deliberations are supposed to ensure that the interests of disparate groups of people are balanced in a fair and transparent way. By insisting that the sciences use judgements of significance and certification that emulate those that would be arrived at in an ideal deliberation, Kitcher aims to make the practices, processes, and uses of the sciences represent all kinds of people in a given society.

When he first presented his ideal of well-ordered science, Kitcher (2001) emphasised its counterfactual nature. If the judgements of significance matched those that would come out of an ideal deliberation, then science was well ordered. This was at odds with many arguments for deliberative democracy that consider actual conversations are necessary (Gutmann & Thompson, 2004). In later re-presenting his ideal, Kitcher (2011) acknowledged this and added that, to be well-ordered, a science must utilise some actual conversations, as well as reach conclusions that would be the same as those from an ideal deliberation. He is a little vague on what these conversations should entail. He discusses the creation of citizen panels that would have some say in the direction of science, but also other institutional forms that encourage conversations, like research review boards. He does not argue for any one institutional form but for the existence of conversations in general, arguing that nothing “can transcend the authority of the conversation” (Kitcher, 2011, p. 134).

Kitcher’s ideal, thus, has two aspects. Representation (balancing the interests of disparate groups) and legitimacy (decisions coming from actual conversations) are both needed to ensure that the sciences make acceptable judgements of significance. Both of these considerations are important, and

⁶⁶ Rawls’s work does not necessarily fit under the title deliberative democracy (Saward, 2018), but his use of ideal deliberations, nonetheless, inspires Kitcher’s account.

I will use them to develop two heuristics below. But, I am not convinced by the usefulness of Kitcher's ideal framework for three reasons.

Firstly, it is difficult to see how actually existing scientific institutions should be structured to be compatible with well-ordered science. Actually existing scientific institutions are quite far from ideal deliberations. Moreover, the counterfactual character of well-ordered science, which remains even after Kitcher's later addition that the sciences should include some actual deliberations, means that it does not have much to say on the actual institutional arrangements of an ideal or better science. Kitcher does offer some concrete issues in present scientific institutions—present incentives do not reflect human needs, public input is currently done badly, privatisation will make matters worse, and current science neglects the interests of too many—but it is not clear that an ideal is needed to point out that these things are problematic.

Secondly, given that Kitcher's ideal deliberations are based on a number of implausible assumptions (the disinterested arbiter in step two, for instance), it is not clear what the outcome of such a deliberation would look like for a number of issues. Without being able to infer an outcome, and without being able to emulate one in practice, we can't see how well reality matches the ideal decision. This makes it difficult to determine when sciences are in practice well ordered.

Lastly, Kitcher's focus and his examples all come from classical concerns of practical ethicists—should we, for example, allow for genetic testing of unborn babies? These sorts of questions do not have simple solutions, but they are at least well defined and fit easily into a deliberative frame. There are many other issues around science, however, that are much less clearly defined and fit much less easily into a deliberative framework. The power balance between technocrats, governments, and politics at large is, for example, an important issue (particularly for economic expertise). Should central bank policy be decided by appointed experts, as it currently is in most developed countries, rather than elected officials? Should the experts employed by the European Commission be able to determine as many facets of the EU regulations as they do? When experts do make what seem to be politically salient decisions, should there be any form of oversight? Institutional issues about who gets to decide and on what grounds (particularly when decisions are required quickly) are hidden by Kitcher's examples of well-defined ethical deliberations that lead to outcomes determined by disinterested experts.

For all of these reasons, Kitcher's well-ordered science does not offer us much to assess the social epistemic practices of economics. Nonetheless, his points that the sciences should aim to make judgements of significance that represent the viewpoints of their citizens and that conversations have an important role in legitimising judgements in science are important. Instead of formulating an alternative ideal for how sciences should deal with these facts, I will focus directly on how social epistemic practices score on them. I take up the issue of citizen conversations in legitimising decisions of significance in 4.2.2. I close this subsection by suggesting a second heuristic for evaluating social epistemic practices based on representation. Social epistemic practices in a given scientific community can be evaluated by:

H2: Looking for blocks to certain kinds of interests being considered in judgements of significance.

The point of this heuristic is to look for ways to improve the social epistemic practices within a given science by making the goals and interests they draw on more representative of the wider community they serve. The recognition that the sciences can serve many ends and that it is important for the sciences to make representative judgements of significance is enough to motivate piecemeal improvements in the social epistemic practices of a given science, without postulating an ideal state of well-ordered science.⁶⁷ There are all sorts of qualifications that can be made—that those judgements are informed, reasonable, et cetera—but simply checking whether particular social epistemic practices in a specific science block the possibility of making representative judgements offers a first step for identifying improvements.

As we saw in Kitcher's account above, judgements of significance entail decisions about what kinds of processes and information a science considers important and worthy of study. They play a central role in determining what scientists in the given field work on, how the phenomena in fields are framed, and when findings are deemed acceptable. The facts that judgements of significance are unavoidable in science, and that there are no context-independent or purely epistemic ways to judge significance, open up the possibility that certain perspectives of significance can be left out. To avoid that possibility, the sciences should avoid social epistemic practices that make it difficult for certain kinds of interests to be considered in judgements of significance. By 'certain kinds of interests' I mean the goals and uses for science considered important by particular constituencies in the societies that the science in question serves. These interests can be epistemic and/or pragmatic. Working class communities might, for example, want secure employment opportunities to be explicitly targeted by macroeconomics over more generic targets like economic growth. Intellectuals may, for instance, place higher value on parsimonious, systematic, or abstract knowledge than more detailed but messy knowledge. The heuristic above does not argue that the interests of all communities should always be realised, but that we should be wary of social epistemic practices that make particular interests invisible.

The practices that might make certain kinds of interests invisible could be quite simple. They might come in the form of institutional obstacles to particular kinds of interests or perspectives being considered by a given science, or they might simply come from the lack of particular kinds of perspectives in conversations about significance. I will highlight some specific ways that social epistemic practices in economics block certain kinds of interests being considered in judgements of significance in chapters 5-7. In chapter 5, for example, I will show, that economics has a high ratio of

⁶⁷ Interestingly, Kitcher (2011) nods to a similarly piecemeal approach on many occasions. On p. 78 he notes that democracy is best thought of as a continuing process of "democratization". On p. 47 ethical progress is presented as a process of refining practices to overcome practical problems. On p. 36 he says that "there is a strong temptation to believe that to show a decision is aligned with canons of good reason and good judgement requires producing explicit canons and demonstrating how the decision exemplifies them", implying that such a temptation should be avoided. He even suggests we should think about how judges are able to produce well-reasoned judgements in complex situations that do not easily admit ideals.

men to women, making it less likely that the concerns of women are heard in economic research.

4.2.2 Scrutiny

Economists might hope that their training, experience, and institutional structures (tenure, peer review, et cetera) give them the authority to make judgements about the directions of their work, without needing to constantly check that those judgements are representative. This is compatible with the idea that social epistemic practices should not systematically block certain kinds of interests from being considered in judgements of significance. As long as the interests of all play a role in suggesting goals and uses for economics, it seems plausible to accept that economists should have some freedom to judge which goals and uses make the most sense to pursue, given their knowledge of potential outcomes. However, given that it is quite possible for any expert group, economists included, to make biased decisions or close avenues for inquiry too soon,⁶⁸ when are such judgements legitimate and not?

Below I will discuss how judgements, including judgements of significance, by scientists that take place during the process of their work, can be legitimate. As we saw, Kitcher has something to say about this. In addition to emphasising the importance of representation, he stresses the role of citizen conversations in legitimising judgements about the goals and uses of science. I will, however, not use Kitcher's suggestion for two reasons. Firstly, it is vague; it is not clear what kind and how many conversations are required. Secondly, it is not clear how such conversations should be balanced with leaving space for the judgement of the trained and experienced specialists. I develop a different approach drawing on the potential for citizen criticism.

Political philosophy contains a long history of discussions about *authority* and *legitimacy*. Rather than exploring all potential definitions of and relations between authority and legitimacy, I will adopt what has come to be known as the *democratic* account of legitimacy (Chambers, 2017; Moore, 2017). The democratic account of legitimacy takes legitimacy to be a virtue of authority. Authorities issue reasons for action and that those reasons create political obligations when authority is legitimate. By forcing others to act, authority can still be effective without being legitimate, but it is only when it is legitimate that it creates obligations (Raz, 1986; Peter, 2010).⁶⁹ Contrary to conceptualisations of legitimacy that mandate unquestioned acceptance of the demands of legitimate authorities, the democratic account of legitimacy then asserts that although subjects are obliged to act on the demands of legitimate authorities, they are free to judge those demands. Such judgements form the basis of democratic legitimacy; legitimate authority is authority that is judged over time to be acceptable by its subjects.

⁶⁸ An example of claims being accepted with too little evidence comes from Haraway's (1989) famous study of primatology. An example of the reverse comes from Epstein's (1996) account of HIV patients wanting access to anti-retroviral drugs at a faster rate than the *U.S. Food and Drug Administration* were willing to certify them.

⁶⁹ Not everybody agrees on this understanding of the relationship between authority and legitimacy. But because my focus will be on epistemic, rather than political, authority and legitimacy, these disagreements are not particularly crucial. For overviews of the different accounts of legitimacy and authority available see Peter (2010) and Christiano (2004).

This parallels Mill's account of the constitutive power of criticism for knowledge. For Mill, it is only through intense criticism that we can assume anything as knowledge. Similarly, it is through judgements by critical outsiders that authority is legitimised. The reasons that authority is judged legitimate can vary—maybe due to mutual benefit or some other convincing reason—but whatever those reasons are, they must be contested and challenged to ensure they still stand. Thus, although they are not required to persuade subjects to act in any one instance, explanations and justifications of demands are crucial for maintaining the legitimacy of authority relations over time. Judgements of the demands of authority are necessary for legitimacy in the same way that criticism is necessary for the existence of knowledge for Mill. Like knowledge, constant affirmation of authority need not be explicit. Affirmation can also come from the absence of disapproval, but only if avenues for disapproval exist. It is the potential and occasional exercise of disapproval that warrants trust in authority and, thus, its legitimacy. Democratic accounts of legitimacy, thus, utilise the importance of criticism in constituting effective authority highlighted by Mill; but, because subjects must still act on the specific demands of authorities, does not undermine the function of authority to enable binding collective decisions.

Democratic accounts of authority were developed with entities like states and state actors in mind, entities that can have what we might call *political authority*. The *epistemic authority* economists possess seems to be of a different sort to political authority for two reasons. First, epistemic authority is based on inequality of judgement (arising from training, experience, and knowledge), whereas political relations of authority typically assume equality of political judgement. Secondly, the actions that epistemic authority demands are the belief of certain propositions. This makes acting on the demands of epistemic authority, and thus believing what they say, while also judging it, seem impossible.

Despite these differences, Alfred Moore (2017) thinks that the democratic account of legitimacy and authority can be used as a good basis for understanding epistemic authority and the role of expertise in society. He suggests Joshua Cohen's (1989) distinction between acceptance and belief as a way of making it possible to act on epistemic authority while still judging it. Moore argues that, to accept the statement of an epistemic authority, we take it as a premise in our own deliberations. Even if we do not individually believe it, we accept it as a surrogate for a fact (Turner, 2003). Accepting a claim, but not believing it, then provides strong reasons to closely scrutinise how it came to be and can lead to the retrospective testing and criticism that constitutes the legitimate authority of experts. Like (democratic) legitimate political authority, legitimate epistemic authority warrants acceptance in each instance, but judgement over time. That judgement over time makes it important for epistemic authorities to engage with their subjects and give reasons for their claims.

The fact that epistemic authority assumes inequality rather than equality of judgement seems more difficult to square with the democratic account of authority. How can the public judge the claims of epistemic authorities given that they do not have the same knowledge or skills as those authorities? The regularity with which their ideas are discussed (and often dismissed) in popular and political discussions makes economists especially liable to flinch at the mention of public judgement. Many

studies have shown the public to be irrational (Sunstein & Thaler, 2008) and ill-informed (Pareek & Pattison, 2000). Numerous economic theories reach what seem from many people's perspectives to be counter-intuitive conclusions—the paradox of thrift, which states that, if everybody tries to save more, then aggregate savings decrease, or Ricardo's model of comparative advantage, are two classic examples. Moreover, it doesn't seem realistic that all citizens will be able to understand even basic economic concepts like elasticity of demand, let alone more complicated concepts, nor is it clear that citizens attaining such knowledge is desirable. The division of epistemic labour exists for a reason, economists might argue. It would be a waste of time for everyone to understand all the details of economic theory.

All of this can be accepted without dropping the idea that economists might admit some form of public oversight. It may be difficult for the public to understand the full detail of all expert conclusions, but that does not mean that there are no forms of judgement available to them (Moore, 2017). Two simple examples give an idea why this might be the case. In his *Politics*, Aristotle (1984) notes that guests are better able to judge the quality of a meal than the cook as they are the intended beneficiaries of the meal (III, XI). Similarly, I may not be a skilled computer technician, but I know when my computer has been fixed and I can tell when something has started to slow it down. There are two things going on in these examples. Firstly, aspects of the products of epistemic authority are often accessible to those without the full technical details, even when the complete picture is more detailed. Secondly, if the products of epistemic authority are supposed to benefit others, then whether or not they experience that benefit matters even if they do now know all the technical details in between. These points indicate that there can be and are ways to judge the claims made by those with epistemic authority even without possessing the full knowledge those with epistemic authority possess.

Moore (2017) draws on extensive literature in social epistemology to suggest a number of ways that 'experts'—those with epistemic authority—might be judged by non-experts. Assessing the *credentials* of experts and how those credentials are attained is one way of judging their conclusions (Collins & Evans, 2007; Goldman, 2001b). Although judging whether credentials are working well might sometimes involve substantive knowledge of the conclusions that those with them produce, some proxies for assessing credentials are available without such knowledge. If credentials can be bought or are distributed in some other troublesome fashion (without sufficient access to certain groups, for example), then they might not be worth significant consideration. The advice of particular experts or experts relying on particular tools and techniques can also be judged by looking at their *past track record* (Goldman, 2001b). Looking for *consensus* among experts is another way to judge their conclusions. Although, consensus alone tells us nothing about its conditions of production and only seems valuable if it arises from cycles of challenge and response between independent experts and different kinds of evidence, that is if it emerges from a science without significant obstacles to productive feedback. Witnessing *debate* or glimpsing parts of the cycles of challenge and response might then also be a way to judge expert conclusions. The existence of productive debate can signal that the conclusions drawn have been suitably challenged and refined. Moreover, it is in debates

(public facing debates in particular) that reasons for reliability and trustworthiness, and challenges to them, are often raised. In such debates, questions are often asked about funding patterns, biases, interests, and the values behind inferences, which are themselves useful proxies for judging expert conclusions.⁷⁰ Similarly, the processes of arriving at conclusions might be judged against common views about the norms required in deliberation. Were all sides listened to fairly? Were some groups unfairly marginalised? Were people judged on the knowledge and information they brought to the table?

Perhaps the most complicated of Moore's suggestions is for non-experts to fit and combine their own *lay knowledge* with expert conclusions.⁷¹ If parts of expert knowledge can fit in with and corroborate non-expert knowledge, while also extending it, then that might be a reason to trust expert knowledge. Moore gives an example of a patient listing symptoms to a doctor. If a doctor is able to respond with further symptoms that the patient finds they also have (but did not volunteer), then the patient might take that as a reason to trust the doctor's eventual diagnosis. Expert-lay interactions and the blending of knowledge in this way increases trust in expertise, actively helps people to augment how they understand the world, and can offer a resource for feedback to expert knowledge.

A mixture of all these factors makes it possible for reflective acceptance of epistemic authority. The above methods require some skill, but do not require the same skills as the experts they are used to judge.⁷² Moreover, although some of these methods require effort, the constant active judgement of every piece of expert testimony by all people is not required for legitimate democratic epistemic authority. All that is required is that those that act using expert advice reflect on the authority of those experts over time, and that the *potential* for critical reflection on expert advice is available to all. It is the existence of real potential for critical reflection that constitutes the democratic legitimacy of epistemic authority.

Certain ways of organising expert advice and certain social mechanisms for oversight of expert advice can aid the existence and potential for critical reflection. Institutional arrangements that ensure that systems of accreditation are reliable might be useful in this regard. Independent measurements of key aspects of expert advice might also be helpful—the way that Greenpeace collects information and monitors governments, or the way that think tanks provide alternative analysis or data. Institutional arrangements that support active public contestation that experts actually engage with are particularly important.⁷³ Many of the methods of judgement suggested above draw on information that often emerges under contestation. Through contestation, information about interests and biases is made more visible, the memory of the interests that lose out in collective conclusions are preserved, and

⁷⁰ It may not be the case that any expert advice is perfectly objective, but it might be useful to know whether those offering advice on particular topics have links (financial or otherwise) to parties that are affected by those topics. See Wright (2018) for a contextual account of objectivity that is motivated by this idea.

⁷¹ The idea of 'lay knowledge' comes from Brian Wynne (1992; 1996), who argues that knowledge relevant to scientific endeavours is often distributed through different actors in society.

⁷² They require what Urbanati terms "deliberative competence" (2002, p. 44). That is, more general deliberative skills, the kind of which are assumed by many political and social institutions in modern societies.

⁷³ By 'public' here, I do not necessarily mean in front of everybody. I mean publicly accessible. It is the potential for judgement, and so the availability of public deliberation, that is important.

reasons are tested, connected to active concerns, and made more visible to citizens (Moore, 2017). In addition to the existence of organisations like Greenpeace and think tanks, institutional arrangements that give rise to a variety of independent sources of expertise for any given issue can help encourage active public contestation. Examples of such arrangements include having experts study similar topics from different disciplines or discouraging single peaked hierarchies within disciplines (see chapter 6). The vigilant presence of concerned groups can have a similar effect. As can regular deliberations between different forms of expert (economists engaging with other social scientists, for example), public debate, and more common forms of public outreach.

The question is not whether citizens can know enough to develop their own economic theories or policies, but whether they can know enough to scrutinise and judge the claims of economists, the ways they arrive at their conclusions, and the role they play in decision-making processes. The legitimacy of epistemic authority is part constituted by critical reflection, which plays an active role in ensuring that such authority is secured or replaced where appropriate. Complete criticism all the time is not required, a few institutional norms and the active engagement by some can enable more passive but informed judgements by others. This should ensure that experts have some space to develop their own judgements. The exact balance between criticism and expert space should be assessed in specific institutional and scientific contexts. The point here is simply that it is in avenues for outside critical engagement and judgement that the legitimacy of expertise can be found. If economists want to retain legitimate authority, they need to engage with the questions, reasons, and concerns of citizens and not just assert their authority.⁷⁴ For epistemic authority to be legitimate and sustained—that is for it to be accepted, live, and resilient (rather than brittle)—it must be open to the potential for critical judgement. The social epistemic practices of economics should facilitate debates and discussions in the public sphere and should make the conclusions of economists potentially judgeable in some of the ways outlined above.

These considerations can ground legitimate judgements by economists of what goals and uses to act on. To be legitimate, the decisions of economists, which include judgements of significance, must be open to criticism. This can be used to make a third evaluative heuristic. Social epistemic practices in a given scientific community can be evaluated by:

H3: Looking for blocks to avenues for public scrutiny.

The choices economists make about the goals and uses they aim towards have authority. But that authority can only be legitimate and sustained by engaging with suitable practices for public scrutiny (in addition to ensuring no particular kinds of interests are blocked from being considered).

Economists can make their own decisions about which avenues to open and close, but for those decisions to be legitimate, they must be open to challenge. The potential for citizens to form informed

⁷⁴ This was part highlighted in the United Kingdom's recent vote to leave the European Union. Angus Armstrong, former head of macroeconomic analysis at the Treasury, noted that, in tours around the country to argue against Brexit before the referendum, his models and arguments weren't well adjusted to speak to concerns of ordinary people and he was largely dismissed (personal communication). He has since set up a project aiming to make macroeconomics more relevant.

judgements about their expert advice must be there.

The potential for public scrutiny can be blocked in a variety of ways. I mentioned some ways to aid public scrutiny above—reliable accreditation, independent measures, ensuring public contestation. **H3** suggests that circumstances in which any of these things are blocked are potentially problematic and deserve attention. Public scrutiny might also be blocked by overly technical discourse, or minimal attempts to communicate with the public (or worse, obscuring decisions from public understanding internationally), artificially constructed consensus (that obscures the debate within a discipline from public view), or a range of other conditions. I will highlight some ways that the potential for public scrutiny is blocked by social epistemic practices in economics in chapters 6 and 7. I will note in chapter 6, for example, that hierarchy within economics, and in particular the overemphasis on publications in a narrow set of journals, disincentivises economists from communicating with those outside the discipline.

4.3 The way the world is

There is a third set of reasons for pluralism in table 2 that I have not yet mentioned: those based on (i) the way the world is. Starting from the way the world is might also seem like a promising way to determine how economic research should be organised. Both Dupré (1993) and Cartwright (1999) have offered suggestions for how the sciences should be organised based on metaphysical diagnoses (don't give too much funding to projects that promise unification, for example). I will not, however, take this approach for two reasons: because metaphysical assertions are difficult to substantiate, and because there are better avenues to effect practical change.

Firstly, the metaphysical arguments for scientific pluralism rest on contentious evidential grounds. Take Jackson and Pettit's (1992) and Fodor's (1974) anti-reductionist arguments from chapter 2. Both argue that moving from a higher to a lower level results in important losses (of information and generalisations); losses that are permanent rather than contingent. The way the world is organised implies that reductions will always result in such losses. These permanent anti-reductionist arguments rely on two forms of evidence: examples of reductions in which information/generalisations are lost, and a justification as to why the higher level information/generalisations lost are unlikely to ever show up at the reduced level. The problem with the first part of these arguments is that examples cannot show permanent problems with reductions. The failure of a particular micro-level explanation to provide all the information of a macro-level explanation of the same phenomena might be the result of the present state of science, rather than the general impossibility of such micro-level explanations. The same goes for the failure of a proposition or generalisation in one language to reduce to another. General conclusions about the impossibility of explanations or propositions on one level, or in one language, to be reduced require more than the failure of some (possibly contingent) reductions.

This is what makes the second parts of Jackson and Pettit's and Fodor's arguments important: they

provide a rationale for general anti-reductionism beyond contingent examples. It turns out, however, that these rationales also rest on contingent evidence. Fodor's reason for thinking that failures of reduction are permanent arises from doubts over whether the physical predicates that special science generalisations are reduced to can ever be natural kinds or admit physical laws. Stephanie Ruphy (2017) notes, however, that even though it might be difficult to imagine such physical predicates being lawlike or natural kind-like, this still only amounts to contingent evidence. The history of science offers multiple examples of cases in which reductions previously thought impossible become possible.⁷⁵ Given that our knowledge of how the world is rests on the findings of our most developed sciences at any one point in time, there doesn't seem to be any way to verify the claim that the generalisations of the special sciences can *never* be described in terms of useful generalisations in the physical sciences.

Jackson and Pettit's argument rests on the assertion that there are different kinds of causal information, which can only be known at different levels. Comparative information comes from macro explanations, and contrastive information comes from micro ones. The assumption in asserting this trade-off is that the information given by micro level explanations cannot be aggregated together to provide something like comparative information. This is a claim that might be well supported when directed at particular examples of micro/macro phenomena (the divide between cellular and molecular levels, for example) and given the present state of science concerning those levels. But, as with Ruphy's objection to Fodor, the examples Jackson and Pettit give cannot show that developments in a given micro level science can never lead to a form of aggregation that allows for micro level explanations to contain all the comparative information of macro level ones.

Although I buy Jackson and Pettit's and Fodor's arguments that there exist (many) cases in which reducing present laws, explanations, or theories results in the loss of something (given the present state of science), such cases depend on their epistemic contexts. Although there may be good reasons why certain reductions give up something important given a particular state of development of science, it is a jump to claim that said reductions will be problematic in all possible future states of science. As the sciences change, reductions that were once unadvisable might become useful. It is a further jump to claim that there are permanently distinct levels or languages that provide different kinds of information and generalisations.

This highlights the difficulties involved in trying to draw normative conclusions for the practice of science from metaphysical analysis. To make general normative claims, Jackson and Pettit and Fodor draw general conclusions from contingent examples. Anti-reductionist doubts about the efficacy of particular, contextually situated, reductions might be well grounded, but the general anti-reductionist conclusions drawn from these doubts go too far.⁷⁶ Dupré has argued, however, that even if this is the case, "science itself cannot progress without powerful [metaphysical] assumptions about the world"

⁷⁵ Ruphy (2017) uses the once popular contention that chemical bonding could not be explained in terms of subatomic particles as an example. This contention is now defunct. Quantum mechanical developments have led to reductive explanations of the bonding of chemical elements (McLaughlin, 1992).

⁷⁶ Ruphy (2017) argues that something like the above is also true for Dupré's arguments for metaphysical disorder.

(Dupré, 1993, p. 1). Science has to make some metaphysical assumptions even if they are imperfect. Moreover, the existence of many cases in which reductionism fails, or of circumstances in which supposed universal laws, causal processes, or kinds do not apply at least provides some evidence against the assumptions that complete reductionism will be possible in some future state of science and that the world is nicely ordered.

I do not deny this. But, like the arguments that economics is insufficiently pluralist, it leaves a lot open. Some evidence for disunity seems unlikely to convince economists that their social epistemic practices need changing. This is my second reason for not drawing on the metaphysical reasons for pluralism. Even if it is true that metaphysical assumptions about the world are unavoidable, I don't see metaphysical analysis as the most productive avenue to impact social epistemic practices in economics.⁷⁷ As Rupy notes, "an appeal to metaphysical considerations comes at the price of a much weaker credibility and relevance to scientific practice" (2017, p. 77). This does not mean that philosophers of science should never do metaphysical analysis, just that it is a risky path to go down if one wants to impact actual scientific practice. It may still be an important path to go down in some circumstances. But, for economics at least, there are more fruitful avenues for philosophical analysis to contribute to practice. In chapters 5-7, I will use **H1-H3** to offer more concrete and less contentious reasons to change economics than challenging its metaphysical presuppositions. The social epistemic issues I will point to are problematic under a range of commonly accepted metaphysical assumptions.

4.4 Why heuristics rather than pluralism?

H1-H3 offer three starting points for evaluating and improving social epistemic practices. In chapters 5-7, I will give examples of how these heuristics can be used to identify concrete issues within economics. But before I do so, I allay one worry. I argued that pluralism does not offer unambiguous recommendations for change in economics because there are too many ways to interpret it in practice. Pluralism offers an abstract ideal that can fit both critics' and defenders' pictures of what economics should be. As such, it is not clear what, if anything, should change on account of pluralism. But do my heuristics not suffer a similar fate? Are they not also abstract and open to interpretation? Take my first heuristic for example. Is it not possible for defenders of economics to say that there is enough feedback in the discipline while critics argue that there is not? My response consists in four related points.

First, the framework I offer above suggests *heuristics*, rather than ideals in an attempt to support contextually grounded normative analysis. They are intended to be open to some interpretation in order to fit with the requirements of different contexts. They suggest looking out for particular issues

⁷⁷ See the reception and (lack of) impact of Tony Lawson's arguments that economics has something metaphysically wrong as an example of why I think that metaphysical arguments are not likely to be well received in economics (Fullbrook, 2008).

(practices that block feedback, the reception of certain interests, and public scrutiny) that, if resolved, can offer avenues for change, but must be weighed up against other factors in the context concerned. They are designed to aid the evaluation of specific practices by pointing to important considerations. They are not designed to determine how research communities should be organised *a priori*. In chapters 5-7, I will start from present practices in economics and use the heuristics to look for improvements, rather than suggest an ideal state and look for ways to reach it.

Second, although the three heuristics I suggest do leave some things open to interpretation, *in the context of economics* they offer a clearer diagnosis of the issues than comparing economics to particular arrangements of plural variables. I did not argue that pluralism as an ideal is not helpful purely because it is abstract and open to interpretation. My argument was that the specific ways that pluralism as an ideal has been articulated are difficult to map over to practice in economics. I will argue that this is not the case for **H1-H3**. In chapters 5-7, I will show how they can be brought to bear on the current social arrangement of economics to point to three concrete issues.

Third, my goal in this thesis is to argue that those concerned with how economic research is organised should turn towards *concrete issues* in economics rather than argue for ideal end-states. Ideals can be helpful, but in the context of economics, the ideal of pluralism has led to an impasse that blocks more obvious issues from view. My heuristics should be directly applied to practice and not held as alternate ideals. I will not use **H1** to argue that there is not enough feedback in economics *in general*. Rather, I will take specific features of the way that economic research is organised and ask: do these restrict feedback in some way? I will argue that there are three features of the way that economics is organised for which the answer to this question is yes. Severe gender imbalances in economics limit the quality and scope of feedback in the discipline. A steep hierarchy in economics creates perverse incentives and a concentration of power that also limit feedback in the discipline. And, the way that economists interact with outsiders leads them to ignore or dismiss ideas and criticism from without, again limiting the feedback in the discipline.

Fourth, my approach points to a number of the same concerns that pluralists have, but without postulating an ideal state. I highlight some aspects of economics that pluralists complain about as barriers to pluralism (a hierarchy that impedes non-mainstream approaches, for example). But I do so by pointing out the issues with those aspects of economics directly (the hierarchy in economics blocks avenues for feedback, the uptake of certain interests, and conditions for public scrutiny), rather than by arguing that they block the path to an ideal pluralist state. This gives concrete evidence to support some of the changes pluralists would like to see and relies less on debatable interpretations of how economics compares to an ideal state. Moreover, my recommendations are potentially more palatable to economists because I suggest concrete ways of making economics more responsive to important epistemic and political considerations (feedback, the interests of a wide variety of citizens, public scrutiny), without arguing that economics must change the theoretical and methodological frameworks they utilise.

Conclusion

Whether economics should count as pluralist or not is difficult to answer. But the *arguments* for pluralism offer useful insights about the kinds of social epistemic practices that are likely to be progressive and that are likely to best serve society. Pluralists highlight mechanisms for *feedback* as an important way to iteratively progress sciences towards whatever goals they are taken to have. Feedback can involve interaction with external reality, social criticism, and any way of challenging the knowledge and practices of science. Some suggest pluralism as a way to increase the forms of feedback scientific knowledge and practices are exposed to. I suggest we look for feedback directly and evaluate social epistemic practices based on whether or not they block productive forms of feedback. I will show a number of ways that feedback is blocked in economic research in chapters 5-7.

Pluralists also argue that the sciences do and should be used towards a variety of different goals and uses. But how should the goals and uses the sciences draw on be decided? I have argued that they should be decided in a way that does not systematically leave out particular kinds of interests in society, and that the decisions scientists make should be potentially open to criticism. Social epistemic practices can be evaluated by looking to see if they block certain kinds of interests being considered when deciding the goals and uses for science or block the public scrutiny of those decisions.

I summarise how the evaluative heuristics I suggest in this chapter relate to the arguments for pluralism in table 3. It is these heuristics that I will take forward to argue from pluralism to practical changes in economics.

Type of reason	Reason for pluralism	Social epistemic practices can be evaluated by
(i) The way the world is	<p>Propositions at higher levels of analysis cannot be reduced to lower levels without losing something</p> <p>The world is not made up of a privileged set of kinds or a privileged level of analysis</p> <p>The world is not made up of an ordered set of laws or causal processes and/or is complex</p> <p>The failure of essentialism, reductionism, and determinism suggests a radically disordered world</p> <p>The correspondence theory of truth does not cover all sentences and representations</p>	
(ii) Epistemic limitations	<p>Underdetermination implies a social dimension to science and knowledge production</p> <p>Utilising a variety of strategies, methods, and systems hedges bets in the face of uncertainty</p> <p>Integrating different approaches can provide more complete pictures of particular phenomena</p> <p>Systems can be improved by co-opting ideas from others and through competition with others</p> <p>A single method for science is not forthcoming, there can be many epistemic virtues</p> <p>There is no such thing as a perfect representation, different representations focus on different features of a target</p>	<p>H1: Looking for blocks to productive forms of feedback</p>
(iii) A variety of goals	<p>The sciences explain and different events and use different explanations for different ends</p> <p>Different communities have different capacities, processes, goals, and interests</p> <p>Representations should be judged with particular uses and contexts in mind</p> <p>Basing science on a variety of goals allows for better political representation</p> <p>Different systems and styles can be used towards different goals and questions</p>	<p>H2: Looking for blocks to certain kinds of interests being considered in judgements of significance</p> <p>H3: Looking for blocks to avenues for public scrutiny</p>

Table 3. Three heuristics for evaluating social epistemic practices and how they relate to arguments for pluralism.

5 Gender imbalances

A lack of gender diversity has become an issue of contention in many research communities. Leading mathematicians, physicists, philosophers, and economists have all bemoaned their disciplines' skewed male-female ratios. On some metrics, economics does no worse than others (Larivière, Ni, Gingras, Cronin, & Sugimoto, 2013). The percentage of PhDs awarded to women is no lower in economics than it is, for instance, in the physical sciences (Fourcade, Ollion, & Algan, 2015). Yet, there are two reasons that make the lack of gender diversity in economics more concerning than elsewhere. Firstly, because economic research and economists are closer to power and have a more direct line to policy (see 1.1 for evidence of this), issues of diversity are more acute. Secondly, empirical evidence suggests not only that there are not many women in economics, but that women face an adverse environment in the discipline. This is sometimes subtle—female economists typically form less productive networks—and other times less subtle—women typically receive more hostile teaching evaluations and face abusive and misogynistic behaviour in economics' online forums.

In this chapter, I highlight the gender imbalances in economics and describe why they are problematic. In 5.1, I present statistics on the number of women in economics, and show evidence that women face an adverse environment in the discipline. In section 5.2, I use the heuristics developed in 4 to argue that this state of affairs is a problem.

5.1 Women in economics

5.1.1 The numbers

At every level of the academic hierarchy, female economists are a minority, and one that shrinks as you move up the levels of seniority. At departments with doctoral programs in the United States in 2016, women made up 31 percent of the PhDs granted, 28.3 percent of the untenured assistant professors, 25.6 percent of the tenured associate professors, and 13.1 percent of the full tenured professors (Lundberg, 2017, p. 767).⁷⁸ This gives the economics profession the character of a 'leaky

⁷⁸ All data in this section is for universities in the United States unless otherwise stated. There are two reasons why this should not undermine its significance. Firstly, American universities tend to dominate economic research and thinking internationally (Backhouse & Fontaine, 2010). Their faculty win more prizes (see any list of Nobel or John Bates Clark winners), they train more policy makers (Fourcade, 2006), they dominate the big journals (see chapter 6), and the ideas produced within them carry international significance (Backhouse & Fontaine, 2010; Coats, 2005; Fourcade-Gourinchas & Babb, 2002; Harvey, 2007; Markoff & Montecinos, 1993). Secondly, evidence suggests that similar gender imbalances exist in other countries (Tenreiro, 2017).

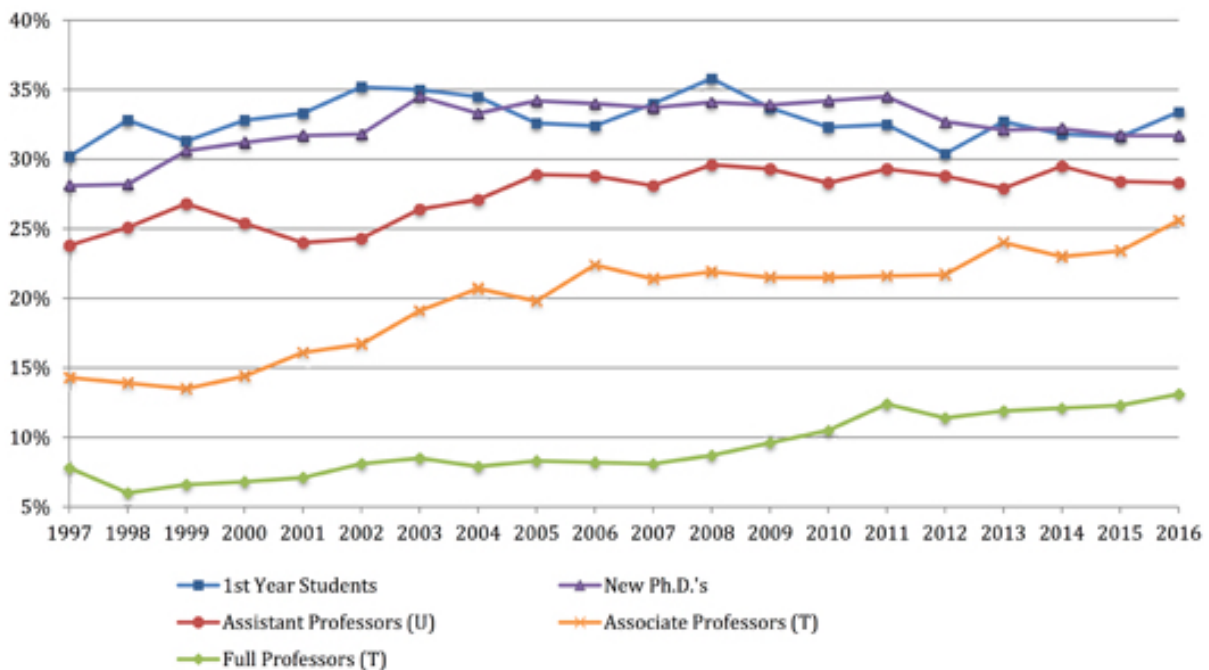


Figure 1. The “leaky pipeline” (Lundberg, 2017, p. 769).

pipeline’, with fewer women entering than men and women leaving at a higher rate at every rung of the ladder (see figure 1).⁷⁹

Donna Ginther and Shulamit Kahn (2004) show that, although similar leaky pipelines exist in other social sciences, the rate of attrition is higher in economics. Female economists are less likely to get tenure and take longer to achieve it than women in other disciplines and men in economics.⁸⁰

Given this leaky pipeline, progress towards equal representation of women and men in economics has been reliant on growth in the share of women entering the discipline. The last quarter of the twentieth century did see such growth. The share of PhDs granted to women increased from 7.6 percent in 1972 to between 30-34 percent in the late nineties and early two-thousands. However, recent reports from the American Economic Association’s (henceforth AEA) Committee on the Status of Women in the Economics Profession (CSWEP) suggest that this increase has since stagnated (McElroy, 2016; Lundberg, 2017), and that similar stagnation can be seen in the share of female assistant professors.⁸¹ Without a return to growth in the share of women entering economics, or fixes to the leaky pipeline, there is unlikely to be any movement towards equal representation of women and men in economics.

Further disparity is evident in the actual jobs women get. In 2016, women made up 20.1 percent of all tenured faculty at schools with doctoral programs, but 35.3 percent of teaching posts without tenure

⁷⁹ This leaky pipeline also shows up in simple lock-step models based on PhD graduation year. There is a decrease in the share of women at each stage of the academic hierarchy even if you account for a time lag in the increases in women moving into economics (Lundberg, 2017). This is supported by recent work showing that women who graduated with economics PhDs in 2008 were 9.6 percent less likely to have tenure 8 years later than men graduating the same year (Chen, Kim, & Liu, 2017).

⁸⁰ Ginther and Kahn (2014) find the same result with updated data.

⁸¹ Although the share of female associate professors has continued to rise slowly, this is due to the longer lag in time it takes to move from recent PhD graduate to higher levels.

eligibility at the same schools. At the top 20 ranked departments, this difference was even more pronounced, with women occupying only 14.9 percent of all tenured posts, but 39.8 percent of teaching posts without tenure eligibility. In addition to being better represented in non-tenure track posts at top departments, women are also better represented at departments without doctoral programs (i.e., departments that are in general lower ranked and give faculty members less time for research). Women occupy 32.7 percent of all tenured posts at departments without PhD programs, compared to the 20.1 percent of tenured posts at departments with PhD programs just mentioned, and this trend runs across all academic ranks. This pattern suggests that, where women do continue with academic careers in economics, they end up in less research focussed and less secure roles:

[T]he representation of women declines as the emphasis on research increases, averaging 40.4 percent for (full-time) non-tenure track teaching positions in non-doctoral departments, 35.3 percent of non-tenure track teaching positions in doctoral departments, 32.7 percent of all tenure track positions in non-doctoral departments, 20.1 percent in all doctoral departments, 14.9 percent in the top-20 departments, and 13.3 percent in the top-10 departments. (McElroy, 2016; Lundberg, 2017, p. 769)

5.1.2 An adverse environment

The above statistics show that women are underrepresented within economics, particularly in the upper echelons of the discipline (see chapter 6 for a discussion of how significant the hierarchy in economics is). This might be partially explained by six adversarial environmental aspects for women in economics, which I outline below.

Tougher reviewing

Using readability scores of over nine-thousand abstracts, Erin Hengel (2017) finds that female-authored papers in the top four economics journals—the *American Economic Review* (*AER*), *Quarterly Journal of Economics* (*QJE*), *Econometrica* (*ECMA*), and *Journal of Political Economy* (*JPE*)—are 1-6 percent ‘better’ written than male-authored papers, with this gap in quality widening in review.⁸² Hengel also uses review times at *ECMA* to estimate that female-authored papers spend six months longer in review (this estimate is produced after controlling for motherhood, childbirth, citations, and field).⁸³ Using a decision theoretic model from the perspective of female authors, Hengel argues that, because the gap in quality between male and female authored papers widens in review, the only explanation for the difference in readability is that women face tougher editorial standards. If

⁸² Readability scores are algorithm generated scores that are supposed to pick out more clearly written prose. Hengel (2017) uses Harley et al.’s (2003) finding that readability scores are correlated across an article’s abstract, introduction, and discussion to justify only analysing abstracts. 40 percent of the male-female gap in readability scores originates in peer review. Hengel determines this by comparing pre-reviewed working papers and drafts to the finished output.

⁸³ Richard Tol (2018) recently argued that this result is not replicable in the journal *Energy Economics*. After a conversation on Twitter and an exchange of data, however, Hengel argued, and Tol conceded, that Tol’s data was biased by the fact that it counts desk rejections. Because desk rejections are faster and because women receive more desk rejections, including these data points made it seem like review time for women was shorter than it is. When only accepted publications are counted, women do encounter statistically significantly longer review times at *Energy Economics*.

correct, this highlights a serious disparity between the treatment of male and female economists.⁸⁴ But even if Hengel's decision theoretic explanation were rejected, her empirical findings are enough to suggest a less-than-level playing field for women. They face longer review times and for some reason seem to be compelled to hone their articles to a greater degree than men. The implications of both these facts is that an average female economist spends more time polishing her articles and less time writing new ones compared to her male colleagues.

Less credit for co-publishing

Heather Sarsons (2017) finds that, even once women publish, they receive less credit for their work if it is co-authored. After controlling for paper quality (using journal ranking and citations as a proxy), men who co-author most of their papers are no less likely to receive tenure than those who single-author most of their work. In contrast, women who co-author, rather than single-author, papers receive tenure at a lower rate. Sarsons puts this down to the credit men get for co-authored work. Her measures suggest that an extra co-authored paper increases a man's chances of tenure by 8 percent, but only increases a woman's chance of tenure by 2 percent. This is the case for women who co-author in general (with women and with men) but the disparity is greater for women who co-author with men. Sarsons argues that this is explained by the fact that the authorship order of economics articles is alphabetised, making it less clear how much work each author puts in. This allows space for assumptions and judgements to enter into the allocation of credit.⁸⁵ Sarsons' data indicates that tenure committees generally assume women put in less work to co-authored papers than men. This both highlights the problems women have in getting credit for their work, and also suggests the presence of biases that might also influence other judgements.

Stereotyping and unequal treatment

Alice Wu (2017), Justin Wolfers (2015; 2016; 2017), and Friederike Mengel, Jan Sauermann, and Ulf Zölitz (2018) all show more evidence of bias in the way that women are treated and judged within economics. Wu shows that women are stereotyped on the anonymous online forum *Economics Job Market Rumors* (EJMR). By scraping text from threads between 2013 and 2017, Wu finds that posts and threads about women are significantly more likely to involve words that relate to personal and physical characteristics, whereas threads and posts about men are more likely to involve words that relate to academic and professional characteristics. Wolfers argues that the stereotyping of women is not confined to EJMR and that Wu's work merely shows one aspect of a climate in which women are systematically stereotyped (2017). He documents multiple cases in which significant female

⁸⁴ Most economics journals are single rather than double blinded. Even the few that are double blinded may not always be successful in keeping authors hidden. Academic networks and events make it common for academics to know what others are working on. Moreover, the rise in significance of working papers (which is particularly strong in economics) makes it easy to search for an early version of a paper that identifies the author (I will discuss some other implications of this in chapter 6).

⁸⁵ To support this, Sarsons shows that the disparity does not exist in sociology (where authorship is ordered by input into the work).

economists are demoted in favour of their male co-authors (and in some cases spouses) (2015). In a particularly striking example, Wolfers notes that:

The great economic historian Deirdre McCloskey, a distinguished professor at the University of Illinois at Chicago, has a unique perspective on all of this, having spent the first half of her career as the male economist Donald McCloskey. Today, she reports that it is quite common for her colleagues not to acknowledge a point she has made until it is reinforced by another male economist. That rarely happened when she was Donald. (Wolfers, 2016)

That women are not taken as seriously and held to different standards is also highlighted by Mengel, Sauermann, and Zölitz (2018), who report that female instructors at an economics and business school in the Netherlands systematically receive lower teaching evaluations than their male colleagues, after controlling for performance.

Lower remuneration

Female economists are also paid less than their male counterparts. Stephen Ceci, Donna Ginther, Shulamit Kahn, and Wendy Williams (2014) report that female full professors in the United States earned on average 75 percent of their male counterparts in 2010. Large differences also exist in the UK, where a more rigid pay structure is supposed to partly protect against such disparities. At the London School of Economics (LSE), an average female academic in the economics, finance, or business departments “receive 18.8% less than her male counterpart... despite controlling for the fact that women might have a different age, tenure and research productivity than their male colleagues” (Bandiera, 2016, p. 11). A gender pay gap exists in other departments at the LSE too, but, at 6 percent, it is significantly lower.

Lower output

It is well known that female economists score worse on a number of metrics of academic output than their male colleagues. Lorenzo Ductor, Sanjeev Goyal, and Anja Prummer (2018), for example, show that, since the 1980s, men have on average had a productivity rating about 50 percent higher than women (54 percent in 2006-2010).⁸⁶ Daniel Hamermesh (2013) notes that women account for only 12.6 percent of authors in the *AER*, *JPE*, and *QJE* in 2011.⁸⁷ This is particularly low when we note that women accounted for 29 percent of all authors in economics journals between 2010 and 2016 (Ductor et al., 2018).⁸⁸ 12.6 percent does mark a rise from 4.7 percent in 1963, but the share of female authors rose almost four times in a similar time frame (from 8 percent in the early 1970s to 29 percent in

⁸⁶ This is measured as the sum of articles produced multiplied by a measure of quality of the journal in which the article appeared (based on citations weighted by journal, with self-citations excluded (Kodrzycki & Yu, 2006)), divided by the number of authors of each article over a five year period.

⁸⁷ These are the only three of the “top 5” (see chapter 6) that Hamermesh focuses on. This is also the highest figure of all the years he considers. In other years, the percentage of women publishing in these journals is even lower. Tommaso Colussi (2018) finds similar figures. He notes that 90 percent of the share of authors in the top four economics journals—*AER*, *JPE*, *ECMA*, and *QJE*—were men between 2000 and 2006.

⁸⁸ Based on journals on *EconLit*, a bibliography of economics journals compiled by the *Journal of Economic Literature*.

2010-16 (Ductor et al., 2018)), making the rise from 4.7 percent to 12.6 percent in the *AER*, *JPE*, and *QJE* look comparatively low. Moreover, the 12.6 percent share in authorship of these journals is way below the share of women that are assistant professors. Even when limiting these percentages to those aged below 35, women only make up 16 percent of the authors in *AER*, *JPE*, and *QJE* in 2011, but 29.3 percent of the assistant professors in the same year.

Less productive networks

Ductor et al. (2018) argue that the lower output among female economists can be explained by the networks they form. They focus on networks of co-authorship, where nodes are defined as authors and edges between nodes represent relations of co-authorship. Ductor, Fafchamps, Goyal, and van der Leij (2013) show that higher levels of ‘inbetweenness’ (how likely it is that a given economist acts as a connection for others) and ‘degree’ (the number of others an economist is connected to) are positively correlated with research output.⁸⁹ In contrast, higher degrees of ‘clustering’ (a measure of how much an author’s co-authors collaborate with each other) and ‘stronger’ ties (how regularly one co-authors with a collaborator) are negatively correlated with output.⁹⁰ Ductor et al. (2018) find that women typically have fewer co-authors and a lower level of inbetweenness, with higher clustering and strength of connection, all characteristics associated with lower output.

Ductor et al. (2018) go on to speculate as to why this might be the case. Their data suggests that it is not due to any desire from women to collaborate with each other, due to family commitments, or due to general discrimination against choosing women as co-authors. Instead, they suggest differences in risk taking as a causal factor. They justify this with the fact that research output variance is higher for men than women and that the network behaviours exhibited by women—clustering, strong connections—are less risky behaviours than those exhibited by men—more, looser connections. Ductor et al. also add that women typically produce less single-authored papers and co-author with more senior academics, both behaviours that indicate risk aversion. Ductor et al. suggest that higher degrees of risk aversion among female economists might be explained by differences in underlying risk preferences or some of the features of the adverse environment for women sketched above, making female economists more cautious and less comfortable in their environment (not being comfortable in one’s environment is also associated with lower risk taking, Slovic, Fischhoff, & Lichtenstein, 1979).

Although the arguments that network features have an impact on output and that women form less productive networks are convincing, the conjecture that the latter is a result of risk aversion and that risk aversion is the main cause of women having lower outputs is less convincing. No reason is given to rule out the possibility that other features of the adverse environment for women—the longer

⁸⁹ This is the same coefficient of output used in Ductor et al. (2018).

⁹⁰ Ilse Lindenlaub and Prummer (2014) also argue this theoretically. They argue that having a high number of connections and being centrally located in a network gives access to new ideas, whereas having a high degree of clustering and forming stronger ties brings greater peer pressure and trust. They argue that the former is more beneficial in high uncertainty environments—which characterises academia—with the latter being more beneficial in more stable environments.

review times they face, the more time they have to spend on teaching to get decent evaluations, stereotyping, et cetera—lower their opportunities to form productive networks or their output directly. Nonetheless, whatever the causal implications, the fact that women end up in less productive networks adds evidence to the point that the climate for women in economics is, at best, worse than for men.

5.1.3 Responses

Women are poorly represented at every level in economics with numbers decreasing as you move up levels of both seniority and prestige. This is exacerbated by the fact that at least six features of the structure of research in economics, described above, are unfavourable to women. Women are subject to longer review times before publication, and possibly tougher standards. They get less credit for co-authoring papers. They face a heavily stereotyped environment in which their voices are taken less seriously, and they are judged to be worse teachers. They are paid less. They publish less in the top journals and have an overall lower output. And, they form less productive networks, possibly due to risk aversion stimulated by the other adversarial environmental factors they face.

Defenders of economics might have a number of responses to this. First, they might argue that the leaky pipeline, and the resulting lack of women within the profession, is due to environmental conditions that the discipline cannot control, like the fact that women often have babies in the crucial early career period. Childrearing might be a factor, but the fact that some academic disciplines have more balanced distributions of women and men suggests it is not the primary cause of the disparities (Ceci et al., 2014). Moreover, Ginther and Kahn (2014) find that, whereas in academia overall childrearing can explain the lower rates at which women receive tenure, economics is the exception.

Defenders of current practices in the discipline might respond by arguing that, even if certain environmental factors can be ruled out, the leaky pipeline need not be the result of any individual or collective biases. Women are, after all, less productive and also poorly represented in other STEM disciplines. Maybe women just choose to work less, and in less math-intensive fields. It is only those who want to be there that can be promoted and it seems fair to make promotion decisions using productivity metrics. However, although it may well be the case that promotion decisions are not biased and some self-selection does occur, Ginther and Kahn's (2014) measurements show that the productivity gap cannot fully explain the promotion gap in economics, which has barriers that don't seem to exist in other math heavy disciplines (where productivity does account for the lower rate of promotion of women). As Ceci et al. (2014) summarise:

Economics is an outlier, with a persistent sex gap in promotion that cannot be readily explained by productivity differences. (2014, p. 116)

Moreover, the causation might also go the other way. The fact that female economists score badly on metrics of output might be partly caused by the dominance of men in the discipline or the adverse environmental factors they face. In addition to the numbers above, 92 percent of the editors of the top 4 journals between 2000 and 2006 were men. In an environment in which women are a minority and are often treated differently, they may be risk-averse, which may lower their output and chances of

success in the discipline (as Ductor et al. (2018) suggest). A more radical explanation might be that women are cited less⁹¹ in part because they are women.⁹² This would lower their output scores directly and also lower their position in the profession, in terms of both the jobs they end up in and how people think of them, in turn lowering their chances of developing productive networks or further damaging their output in other ways. The fact that women typically occupy less research focussed and less secure jobs at less research focussed universities might also explain a chunk of the productivity gap in economics rather than the other way around.⁹³

The causes of the poor gender balance and adverse environment in economics do not seem easily reducible to a single cause (Ceci et al., 2014). But identifying precise causal factors is not my focus. The descriptive statistics are enough to show that women are underrepresented (particularly higher up) and that at least some features of the discipline are unfavourable to them. This need not imply that economists are biased individually, on average, or in aggregate. Some of those I have referred to do argue this, some of the explanations of lower female output might cite this, and some of the above might count as circumstantial evidence of bias. But, it can both be the case that women are underrepresented and face an adverse environment in economics and that most economists are not sexist (even the stereotyping and unequal treatment could be the work of a minority within the discipline).

Gender imbalances are also not exclusively an issue within economics. Ginther and Kahn (2014) give us reason to believe that the leaky pipeline is worse in economics than elsewhere, and issues like the lack of credit for co-authored articles, lower pay, and unequal treatment are either not present or less pronounced in other disciplines. But even if economics were no worse than other disciplines, the gender balances described above seem problematic. Defenders of economics may, however, offer one final response. They might argue that: ‘this may be a regrettable situation for ethical or fairness-based reasons, but it has no impact on the quality of economic research. Economics is a rigorous social science; its findings are valid and should be trusted no matter how many economists are women.’ I will argue against this response in the next section.

⁹¹ A fact noted, but not discussed in detail, by Ductor et al. (2018). Female economists receive on average 5.69 citations whereas male economists receive 12.86.

⁹² A recent paper by Dion, Sumner, and Mitchell (2018) supports this possibility. They find that, across a number of social science fields (including economics), men cite women less than they cite other men and are less likely to attribute original ideas to women.

⁹³ This is part supported by Xie and Shauman (2003) who show that in STEM as a whole higher demands on female faculty’s time for teaching and/or service can explain a significant portion of their lower research productivity. This point is exacerbated by common efforts within universities to make committees as balanced as possible while drawing from a pool that contains more men (Porter, 2007), and by the commonly held, and well founded, belief that men are more likely to avoid doing admin tasks (Babcock, Recalde, Vesterlund, & Weingart, 2017). These effects are also part evidenced by Misra, Lundquist, Holmes, and Agiomavritis (2011), who show that, even within a single university, male STEM faculty devote more time to research than their female counterparts.

5.2 Why worry about gender imbalances?

Women are poorly represented and face a number of adverse environmental factors in economics. I will now argue that these facts are problematic regardless of their causes.

Recall my argument, in chapter 4, that, regardless of the goal, mechanisms for *feedback* are an important way of developing science. By providing different forms of criticism, new observations, and testing and challenging the knowledge and practices of science in different ways (from experimentation to providing alternative explanations) feedback helps to weed out biases, bad forms of reasoning, falsities, and out of date observations and theories, and generally helps to progress science.

I also argued that, given that the sciences can be used towards many different kinds of goals (both pragmatic and epistemic), and that these goals cannot be determined by epistemic criteria alone, social epistemic practices should also be evaluated based on the *goals they enable*. In particular, scientists necessarily make judgements about what they take to be significant throughout their work. I argued that this makes it important that the choices scientific communities make about what goals to pursue, and the judgements of significance they make, are legitimate from a societal perspective. I argued that, for this to be the case, certain kinds of *interests* should not be systematically blocked from consideration, and that the decisions scientists make should be open to public *scrutiny*.⁹⁴

I combined these points to argue that social epistemic practices can be evaluated by:

H1: Looking for blocks to productive forms of feedback.

H2: Looking for blocks to certain kinds of interests being considered in judgements of significance.

H3: Looking for blocks to avenues for public scrutiny.

In this section, I will use **H1** and **H2** to argue that the gender imbalances outlined in 5.1 are problematic. Although many of the issues involved speak to both heuristics simultaneously, in order to make the implications of each heuristic as clear as possible, I will discuss how the issues described above affect each heuristic in separate subsections. In 5.2.1, I will argue that the gender imbalances within economics lead to lower quality and less varied feedback (**H1**). In 5.2.2, I will argue that the gender imbalances within economics block the interests of a large section of society—women—from being considered in judgements of significance in economics (**H2**). **H3** is less relevant to the issues surrounding gender in economics. An argument might be made that incorporating more women in the discipline would make it more varied, and so contain more of the critical debates that help citizens make judgements. But given the more obvious relevance of **H1** and **H2**, I will not pursue that possibility here.

⁹⁴ Most of the points in this paragraph were based on bringing together arguments from others. I have omitted references here in order to not repeat myself. See chapter 4 for details.

5.2.1 Feedback

Stanford economist and the first female John Bates Clark Medal winner (in 2007) Susan Athey has argued that the gender imbalances in economics amount to a waste of human capital (Romero, 2013). **H1** and the idea of feedback can highlight two ways in which Athey's claim might be correct.

Firstly, a lack of women within economics plausibly leads to lower quality critical interaction, observations, and research within the discipline. If social critical interaction is one important feedback mechanism for testing knowledge, as I argued in chapter 4, then a range of critical interaction, rather than repeatedly responding to similar points, seems valuable. With respect to economic knowledge, this might simply be taken to imply that as many economists as possible should be involved in any one conversation. If we assume that there exists some variety in the quality of critical feedback people can provide, we might also hope to have more of those that provide higher quality feedback. Critical feedback will be more effective if those that are more skilled in reasoning within the community's standards partake in it. But, if we also assume that the quality of the kind of critical feedback relevant to economics is evenly distributed between men and women, then the gender disparity within economics is likely to result in some men holding positions at the expense of more skilled women, lowering the overall quality of the critical feedback in comparison to a scenario with a more even gender balance.⁹⁵ Similar comments might be made for other forms of feedback (in addition to critical interaction). More important observations and more useful models are likely to be developed if economics contains more skilled people. Women likely make up more than just 13.3 percent of the most able economists (Ceci et al., 2014), so the fact that they make up just 13.3 percent of the tenured professors in economics means the discipline is missing out on talent—talent that would likely increase the effectiveness of feedback in the discipline.

Secondly, female economists might offer a unique set of perspectives simply because they are women. In arguing for the importance of social critical feedback, Longino adds that:

A diversity of perspectives is necessary for vigorous and epistemically effective critical discourse. The social position or economic power of an individual or group in a community ought not determine who or what perspectives are taken seriously in that community. ... [This] is to ensure the exposure of hypotheses to the broadest range of criticism. (2002, pp. 131-132)

This suggests that increasing the number of perspectives that critical interactive feedback draws upon increases the number of ways that knowledge can be challenged and tested. Something similar might be said for other important forms of feedback. If we want to maximise resistance from reality, then practicing forms of economics, testing hypotheses, and incorporating observations that come from a range of different perspectives can help. Moreover, utilising different perspectives might lead to the

⁹⁵ Empirical evidence suggests this assumption is correct, even at the far right hand side of the distribution (see Ceci et al. (2014) for a detailed review of the evidence), despite Larry Summers' infamous comments to the contrary (Summers, 2005). Moreover, even if skills are not evenly distributed, they are unlikely to be as unevenly distributed as women are in economics (recall women account for only 13.3 percent of tenured faculty in the top 10 departments).

partial satisfaction of different aims, and when integrated in either an ad hoc or systematic fashion they might offer more complete or useful pictures of their targets.

Situated knowledge is the name given to the unique knowledge any social group might offer by dint of their social position (Haraway, 1988; Harding, 1991). The idea of situated knowledge is sometimes interpreted as radical, but it need not be. It need not imply that what counts as truth and knowledge varies across individuals or social groups (although it can). At its most basic level, it simply assumes that circumstances in which people live have an impact on what they come to know. Elizabeth Anderson points out many ways that this can happen:

People experience the world by using their bodies, which have different constitutions and are differently located in space and time. ... People have first-personal access to some of their own bodily and mental states, yielding direct knowledge of phenomenological facts about what it is like for them to be in these states. ... People often represent objects in relation to their emotions, attitudes and interests. ... People have different skills, which may also be a source of different propositional knowledge. ... People have different styles of investigation and representation. What looks like one phenomenon to a lumper may look like three to a splitter. ... [People have different] background beliefs and worldviews. ... People may stand in different epistemic relations to other inquirers—for example, as informants, interlocutors, students—which affects their access to relevant information and their ability to convey their beliefs to others. (Anderson, 2000)

The social positions people occupy effect all of these things. Whether somebody is a factory worker or accountant is, for example, likely to bear heavily on the skills they have, the people they rely on for information, their understanding of tax law or of how it feels to do manual labour for long stretches of time, et cetera. Social positions affect the access individuals have to information, the form of their knowledge, their standards of justification, and their judgement about which claims are significant (Anderson, 2000). This seems intuitively correct. Most would agree that it makes more sense to ask a factory labourer about the tools of their work than a tax accountant. And, without the assumption of situated knowledge, there would be no reason for designers to ask end users for feedback on products.

Situated knowledge simply assumes that the regular experience one has in life and the position from which one perceives the world has some bearing on what one knows. This idea is perfectly compatible with many assumptions economists make about the world and has even been used to justify a number of popular economic theses. Friedrich von Hayek's (Hayek, 1948) famous argument against planning and for the price mechanism, for example, is based on the idea that the knowledge relevant for planning draws on personal circumstances, is dispersed throughout society, and cannot be fully transmitted. This developed into the view, now commonly held among economists, that market-like structures and prices should be developed for all social interactions that require coordination

(Wright & Mata, Under review; Mirowski & Plehwe, 2009; Mirowski & Nik-Khah, 2017).⁹⁶

The idea that female economists provide a unique and valuable set of perspectives assumes that some of the situated knowledge they possess by being women forms an epistemically relevant perspective on economic issues. Historians of science have pointed to a number of examples in the history of science in which the absence of female scientists has caused the absence of particular forms of critical feedback.⁹⁷ Feminist philosophers of science have argued that the different roles and norms that men and women play and comply with in society regularly lead to the development of different traits, skills, background beliefs, and world-views, which produce differences in knowledge significant enough to affect the sciences. The higher propensity of women to be involved in care, for example, often means that they have greater knowledge of what such care requires, greater caring skills, and are likely to place greater significance of the role of care within a society (Nelson, 1995).

This does not imply that men and women are inherently different. Rather, that they have differential access to the circumstances that lead to the development of skills and knowledge. Simple forms of propositional knowledge are unlikely to be influenced by these differences. Men and women have the same propensity to grasp the basic rules of arithmetic (Ceci et al., 2014). This sometimes leads commentators to argue that gender differences do not influence science. But, as feminists point out, many forms of knowledge differ significantly from the simple propositional statements of arithmetic. Where knowledge is more reliant on judgement, background assumptions, detailed understandings of particular processes, or the weighing up of the significance of different factors, the different aspects of knowledge that accrue to men and women are more likely to make a difference. This means that the significance of situated knowledge in determining the kind of critical feedback a person provides likely varies with field.

Economists might hope their rigorous methods make situated knowledge insignificant to their field, but there are four reasons to think to the contrary. Firstly, the variability of conclusions economists draw from their models suggests that the field is not characterised by simple propositions similar to arithmetic (Rodrik, 2015). Secondly, the large degrees of uncertainty within economics mean that judgements and background assumptions play a big role in determining what is accepted. This is highlighted by the recent attention the role of good judgement in model selection (i.e., in determining which model fits which problem) has received (Aydinonat, 2018). Thirdly, the history of economics suggests that women do bring different perspectives to the discipline. Julie Nelson (1995) has shown, for example, that the ways that economic metrics developed when the discipline contained

⁹⁶ Even the President of the *Federal Reserve Bank of Minneapolis* (home of rational expectations macro policy and not known for their radicalism), Narayana Kocherlakota (2014), and the head of the *Institute for Fiscal Studies*, Paul Johnson (2018), have endorsed the point that peoples' background history and social position gives them different perspectives that are useful in economic policy analysis. Kocherlakota, for example, states that "Ultimately, a person's ideas are a culmination of his or her journey through life. We will have access to more and better ideas if our employees have a large number of distinct life journeys. And we need those more and better ideas if we are to be effective in solving the various public policy challenges that we confront." (Kocherlakota, 2014)

⁹⁷ The prevalence of androcentric views, for example, in historical periods of science where female scientists were not present; views that were only critiqued and replaced as women entered the sciences (Haraway, 1989; Keller, 1985).

very few women left out typically female forms of labour.

Lastly, Ann Mari May, Mary McGarvey, and Robert Whaples (2014) have shown that on average male and female economists in the United States reach different conclusions on a number of central economic issues (differences that are statistically significant even after controlling for place of employment and decade of PhD). In general, male economists are more likely to support pro-market propositions than their female counterparts. Men are more likely to think government regulation is excessive, and more likely to support market solutions like voucher systems for school funding. Women are more likely to support policies for equalising the distribution of income, to support progressive taxation, to support linking trade agreements to labour standards, and to require employers to provide health insurance. Moreover, women are much more likely to think that job opportunities for women are not equal to those of men and are more likely to disagree with the view that the gender wage gap is explained by differences in human capital and choice (a proposition men generally agree with). Women are also much more likely to think that there are better opportunities for men within economics than women. Interestingly, this is all while men and women largely agree on a number of core assumptions in economics—that human wants are unlimited, that people are utility maximisers, and that mathematical modelling is important—indicating that lots of gendered situated knowledge and judgements enter between the starting and end points of economic analysis.⁹⁸

This all suggests that female economists offer a unique perspective that is underutilised within economics. If Longino is correct, then including more of this unique perspective should increase the range and effectiveness of critical interactive feedback in the discipline. Empirical evidence suggests something like this does indeed happen; that group decisions and deliberations are positively impacted by incorporating a diverse range of perspectives. Bayer and Rouse (2016), Leung, Maddux, Galinsky, and Chiu (2008), Rubin and O'Connor (2018), and Page (2007) all offer extensive reviews of the literature on this topic. Among the studies they point to, Sommers (2006) finds that racially diverse mock juries exchange more information, discuss for longer, engage with more perspectives, and make fewer errors. Levine et al. (2014) find that bubbles are less common in markets with an ethnically diverse mix of traders. Hoogendoorn, Oosterbeek, and Van Praag (2013) find that mixed-gender groups of business students produce better course projects. Phillips, Northcraft, and Neale (2006) find that racially diverse groups do better at solving complicated problems than other groups. Freeman and Huang (2015) find that academic papers produced by ethnically diverse groups have higher impact and more citations (this is from a sample of 2.5 million US based authored papers between 1985 and 2008). Richard, Murthi, and Ismail (2007) and Richard et al. (2007; 2012) both find that companies whose leadership positions are filled by people from diverse backgrounds are financially more successful.

Hélène Landemore (2012) and Cass Sunstein (2006) list similar findings and note that the epistemic benefits of drawing on the dispersed knowledge of crowds (Surowiecki, 2005) and the

⁹⁸ In a later paper, May, McGarvey and David Kucera (2018) show similar results for European economists, with the exception that female European economists are more likely to disagree with some of the core economic assumptions.

positive effects of deliberation depend on diversity. Moreover, in their models of group problem solving, Lu Hong and Scott Page (Hong & Page, 2004) find that diverse groups outperform groups with higher ability individuals in many scenarios (note that this is based on modelling rather than empirical data). The important point all these studies make is that an individual's value to a group depends on their ability to improve collective decisions, which is determined more by the makeup of the rest of the group than their individual ability.

Care is needed in drawing general conclusions from these studies. Many of them target quite simple cases of group action and decision-making. But they do provide support for the idea that groups that contain diverse perspectives are more likely to have productive conversations, more likely to come up with novel approaches, and less likely to rely on biased forms of reasoning. This supports what I have argued above: a diversity of perspectives in a community is an effective mechanism for producing productive feedback. Diversity leads to more deliberation and better critical interaction, lowering the propensity for biases and groupthink and improving the reliability of outcomes. Through a lack of female economists, research in economics forgoes some of the feedback improvements that diversity can provide (different perspectives, different forms of reasons, different observations, as well as different skills).

Thus, under plausible (and empirically supported) assumptions about the distribution of critical faculties between the genders, the lack of female economists leads to lower overall quality feedback in the discipline. Moreover, given that female economists likely offer an epistemically relevant perspective for economics, their absence also likely leads to less varied feedback within the discipline.⁹⁹ It is easy to see how these constraints are further exasperated by the adverse environment that women in economics face.

If it is true that female economists face tougher review standards, then they are more likely to publish in lower-ranked journals, have a lower publication rate, and potentially even be more conservative in what they write (i.e., less likely to challenge entrenched positions or powerful individuals). Even if female economists do not face tougher standards, they do at least face longer review times, which delays how fast they can produce work. This is exacerbated by the fact that the less favourable teaching evaluations they receive likely induces a reallocation of the time and resources away from research and towards teaching activities. All of this reduces the ability of female economists to offer effective critical feedback, develop new observations, et cetera.

These constraints on the ability of female economists to do research also likely hurts their job prospects and reduce their chances of getting into powerful positions or even stay in the discipline. This is again compounded by the fact that they get less credit for co-authored work, are stereotyped, and are treated differently (as well as lower teaching evaluations also lowering their job projects

⁹⁹ It is possible for women to be as skilled in the practices of economics in general—seeing reasoning errors, crating mathematical models, noticing important observations, et cetera—while also offering an epistemically relevant different perspective. There is no reason to think that men are better than women in getting from A to B (i.e. from start to end of analysis), but they will, on average, make qualitatively different judgements along the way, and these judgements offer crucially different perspectives that expose economics to more forms of feedback.

directly). This feeds back into the issues outlined above by likely lowering the number of women within economics (particularly high up) offering feedback from their unique perspectives.

The stereotyping and differential treatment women face also combine with constraints on their output to undermine what they have to say if they do stay in economics. This likely also combines with the other factors discussed to affect female economists' self-confidence and beliefs about their teaching abilities and abilities as economists.¹⁰⁰ All this is likely to dull the effectiveness of the feedback female economists provide.

Taken together, the adverse environmental factors women face in economics, thus, exacerbate the constraints on feedback in economics caused by the low ratio of women to men. It lowers the ability of female economists to provide productive feedback, lowers the prevalence of feedback from them in general, and lowers the effectiveness of whatever feedback they provide.

All of this means that the skewed gender balance within economics and the adverse environment for women within the discipline reduce the prevalence and effectiveness of the kind of feedback women typically provide (for example, about issues for which their social situatedness gives them a better understanding), and likely lowers the quality of feedback within economics overall. This means that the hypotheses produced by economists are exposed to less varied and less stringent tests, and that the idiosyncratic biases of individuals or subgroups within the community are more likely to go unchecked. This point can be accepted from a purely epistemic point of view and assuming that the goals of economics can be determined completely by truth. Even if we assume that in each stage purely epistemic grounds are used to make inferences, record data, et cetera, the people doing this play a crucial role. It is people that ensure lines of argument are reasonable, that evidence is recorded and treated fairly, and that biases are avoided. The argument here is that the balance of women and men within economics impacts this in an epistemically relevant way. Fewer female economists means less variety and lower overall quality feedback. If economists want to improve productive iteration within their discipline and improve the knowledge economics produces, they should aim to improve the forms of feedback within the discipline. Other things being equal, making the gender distribution within economics more balanced and making the environment within the discipline less hostile for women would do that.

5.2.2 Interests

Progressive iterative feedback is a process that takes many forms and can be directed towards a variety of goals. It is desirable even if economics is taken to have just one goal. But, as we saw in chapters 2 and 4, there are a number of reasons to believe that sciences should be open to a variety of different

¹⁰⁰ Sarsons and Xu (2015) report that top female economists are 7 percent less likely to give 'extreme' answers when asked about the economy, less confident in the accuracy of their answers, and less confident about answers to questions outside of their main field than top male economists. These results persist when controlled for PhD institution and year, current institution, and number of publications. Lower confidence and cautious behaviour are also likely to lead to lower comparative significance of female critical feedback within economics.

goals. Given that it can serve multiple ends and that there is no purely epistemic and context independent way of determining significance, it is also important that economics is able to incorporate the interests of those in the societies it serves if it is going to investigate issues that are relevant to them (**H2**). This is particularly important given the significant role economics plays in shaping policy thinking and designing interventions that impact many facets of life in contemporary western societies.

Given that women form a significant constituency, with different interests and perspectives to men due to their social positions in most societies, any blocks to women's interests being heard in judgements of significance would be an issue worth paying attention to according to **H2**. Can we say that the data presented in 5.1 shows evidence of blocks to the interests of women being heard? I think we can. As pointed out already, there are plenty of reasons to suggest that average female and male economists have slightly different goals,¹⁰¹ make different judgements, observe different things (or at least emphasise different things in their observations), and parse the world in different ways. On average, female economists are likely to offer different perspectives to their male colleagues; perspectives that plausibly have some things in common with female perspectives in the wider population (being better acquainted with the importance of care, for example). It seems intuitive, then, that having fewer women in economics, and marginalising female voices within the discipline, limits how much influence female perspectives and interests have in economic research.

Three objections might be offered to this. Firstly, a balance of male and female economists need not be necessary to incorporate a fair balance of female interests into economics. Representation can occur without inclusion. The problem is that the extent to which people from significantly different social positions are able to represent the perspectives of each other is questionable. This is particularly true for perspectives that are less well represented in popular culture more generally. As described above, the history of science is replete with episodes in which the absence of women has led to the absence of the consideration of their interests (Haraway, 1989; Harding, 1991).

Secondly, parity between the number of economists coming from various social groups need not be required for the interests or goals from different perspectives to be considered. If female voices within economics are effective, then their perspectives could be heard without matching male economists for numbers. This objection might be partly correct. Complete parity may not be necessary to ensure the adequate representation of the interests of women. However, it is likely that economics could do better. In a discipline where only 13.3 percent of the full professors, 8 percent of the editors of the top four journals, and 12.6 percent of the authors in the top three journals are women, the female economists are going to need very effective voices to be able to make the interests of women known on an equal basis as those of men. Moreover, for all the reasons discussed in 5.2.1, the adverse environmental conditions women face within economics are likely to make that job even harder. It is difficult to quantify exactly what balance would be necessary to ensure that female perspectives are

¹⁰¹ The fact that many more women sign up to economics courses when they are explicitly socially meaningful adds further evidence to this (Varathan, 2017).

heard within economics. Complete parity may not be necessary. But improvement surely is.

Thirdly, it might be objected that my reasoning assumes that all women are the same. It would be possible for women of particular social categories (rich, white Ivy leaguers, for example) to dominate within economics even if numbers of men and women were equal. This is a legitimate worry and calls for more forms of diversity within economics. Nonetheless, for all the reasons presented above, women do on average offer slightly different perspectives to men. Women may not all be the same, but they differ around an average that is currently underrepresented. The adequate inclusion of the interests and goals of women is surely preferable to their exclusion, other things being equal.

Thus, the lack of women in economics combined with the adverse environment they face within the discipline part block female interests being heard in judgements of significance within economics. Incorporating more female economists into economics and/or improving the environment for women within the discipline would increase the presence of the interests and goals of women in judgements of significance in economics.

Conclusion

The current social epistemic practices within economics block important avenues for productive feedback by leaving out female abilities and perspectives (**H1**). This is because there are not many women in economics, and because those that are in the discipline face an adverse environment. For all the reasons discussed in chapter 4, this makes it less likely that errors and biases within economics will be exposed, and less likely that economics will develop new interesting forms of research utilising the situated knowledge female economists possess. The gender imbalances in economics also block the influence of female interests in the discipline (**H2**). This makes economics less likely to focus on issues and notice things that matter to women. This makes the discipline less able to respond to and further the needs of a significant constituency in the societies it serves. Both of the issues I point to—feedback and representation of interests—are made more acute by the significant societal role economics plays in most countries. I summarise these points in table 4. Other things being equal, the mechanisms of feedback in economics and how well the discipline incorporates the range of interests in the societies it serves can be improved by increasing the share of women within the discipline and by making the environment for women within economics less adverse.

Two objections could be offered to this conclusion. First, assuming more gender diversity is a good thing, how much diversity is enough? Does what I have said not just fall into the same problem as the debate about pluralism? Is the question still not just: how much X (diversity, pluralism) is needed? This objection misunderstands my claim. My point in moving away from comparing economics to pluralist ideals was to move to a discussion about *improvements* rather than *end-states*. I have no issue with arguing that certain aspects of economics need more of this or that plural variable (although, as I argued in chapter 3, even that is a difficult in practice). Local debates about methods

Why pluralism?	Evaluative heuristic	Issues caused by gender imbalances
(ii) Epistemic limitations	H1: Looking for blocks to productive forms of feedback	Lower quality feedback and a loss of important situated knowledge. Together these factors make it less likely that errors and biases will be spotted and less likely that new interesting avenues will be explored
(iii) A variety of goals	H2: Looking for blocks to certain kinds of interests being considered in judgements of significance H3: Looking for blocks to avenues for public scrutiny	More difficult for the interests of women to be heard in economics. Complete parity may not be necessary, but economics could do better

Table 4. The issues caused by gender imbalances in economics and how they relate to **H1-H3** and the reasons for pluralism.

and ontologies are appropriate. The problem was that comparing economics to an overall ideal state (pluralism) does not offer sufficiently concrete advice regarding next steps; pointing out that economics has a serious gender imbalance does. The latter offers a clearer claim (than pluralism) regarding what is currently problematic, why, and where improvement lies. The gender imbalances are a clear and *measurable* feature of economics that is harder to wave away under the guise of a different interpretation (unlike the claim that economics does not have enough schools of thought). By starting from a concrete and verifiable point about how economics is currently organised, rather than an ideal of how it might be, unambiguous avenues for change and more easily identifiable. Once some improvement has been made, the situation can be assessed again.

Second, how can we be sure that alleviating gender imbalances is a good thing to do? Doing so may have negative impacts elsewhere (affirmative action may be unfair on some young male economists, for example). Or even if it does not, spending resources changing the gender balance in economics could reduce resources available to tackle other issues. Are there no trade-offs? This is an open question. The existence of trade-offs, however, do not detract from my point. My goal is not to provide a final answer as to what should be done when all things are considered. That would be a conversation that I encourage, but my point has simply been to point out that gender imbalances are an issue that both philosophers and economists should care about because they affect the output of economics and how economics relates to society. They are an important and concrete social epistemic issue worth alleviating, *other things being equal*. Doing so may have knock-on effects and trade-offs elsewhere. But that does not change my point that improving the gender dynamics in economics is a change worth thinking about in detail, given the present situation.

Some of the issues described in this chapter have been noticed by powerful governing bodies within economics (the AEA in the United States, the Royal Economic Society in Britain). The numbers presented in 5.1.1 are mainly collected by the AEA's CSWEP, who have expressed concern at the flatlining of women entering economics. 2017 saw the popular press on both sides of the Atlantic become increasingly interested in issues of gender discrimination and sexual harassment. In that

climate, Alice Wu's work on the unfavourable climate women face on EJMR received significant popular attention. In response, the AEA commissioned an Ad Hoc Committee on the Professional Climate in Economics. The committee's main recommendation was a new Code of Professional Conduct, which was adopted in April 2018. The code is short, but states explicitly that:

Economists have a professional obligation to conduct civil and respectful discourse in all forums, including those that allow confidential or anonymous participation.

The AEA seeks to create a professional environment with equal opportunity and fair treatment for all economists, regardless of age, sex, gender identity and expression, race, ethnicity, national origin, religion, sexual orientation, disability, health condition, marital status, parental status, genetic information, political affiliation, professional status, or personal connections. (AEA, 2018)

In addition to this code, the ad hoc committee also recommended regular surveys to check the professional climate in economics, disseminating guidelines for how to reduce biases, and efforts to increase the range of students studying and continuing in economics. All of these measures should be welcomed. But it is not clear that they will make a significant dent in the issues described in 5.1. It is not clear that the code of conduct will change the culture exhibited on EJMR.¹⁰² Moreover, given that many of the issues described above either come from subtle biases or hierarchies repeating themselves (sub-fields, for example, are typically gender skewed and people typically look more favourably on people who work in the same sub-field as them), it is not clear that one aspirational sentence in a code of conduct will alleviate them. Perhaps more stringent measures like mandating more women on tenure committees,¹⁰³ hiring committees, and journal editorial boards are needed.¹⁰⁴ In the next chapter, I will argue that the steep hierarchy within the discipline creates similar issues to the lack of women for feedback in the discipline: important perspectives are likely to be marginalised. This suggests that both issues should be tackled together.

¹⁰² A brief look at comments on the pages where these changes have been announced attests to that.

¹⁰³ Evidence suggests this makes a difference (Bagues, Sylos-Labini, & Zinovyeva, 2017).

¹⁰⁴ This could have the negative effect of increasing the amount of non-research tasks female economists take on. But maybe this can be mitigated by taking these tasks into account in promotion decisions (this would help women in itself as they typically take on more tasks like this) and exempting women from other non-research tasks.

6 Internal hierarchy

The economics profession today is hierarchical. Although most academic disciplines have some internal hierarchical structures, the hierarchy in economics is steeper and more consequential. The hierarchy within economics creates perverse incentives and centralises power, both of which cause blocks that fall foul of **H1-H3**.

A *hierarchy* is typically defined as some kind of partially ordered set. In organisational terms, a hierarchy is an ordering of individuals along one or more socially important dimensions (Anderson & Brown, 2010; Magee & Galinsky, 2008). Six features of the way that research is governed, evaluated, and rewarded in economics illustrate the orderings that make up economics' hierarchy. Economics places particular significance on journal rankings (and the top 5 journals in particular); networks in the discipline are dominated by 'stars'; those that govern the discipline are drawn from a narrow subsection of economists; those at more highly ranked institutions are more likely to be editors of, and authors in, high-prestige journals; and, the rankings of institutions in which economists reside, get their PhDs, and get their first jobs play significant roles in career success.

In this chapter I show evidence of all these factors (in 6.1), and use the framework developed in chapter 4 to argue that the hierarchy they combine to form is problematic (in 6.2). The perverse incentives and centralisation of power caused by economics' hierarchy block avenues for productive feedback, make economics less responsive to certain interests, and make public scrutiny more difficult.

6.1 Economics is hierarchica

The top 5

Journal rankings play a big role in economics. Publications (or lack thereof) in the journals with the best reputations are significant in determining academic success or failure in most fields. In economics this is more acute, with the 'top 5' journals particularly significant—the *American Economic Review* (*AER*), *Quarterly Journal of Economics* (*QJE*), *Econometrica* (*ECMA*), *Journal of Political Economy* (*JPE*), and *Review of Economic Studies* (*ReStud*).¹⁰⁵ Many economics departments have informal or formal targets for junior academics to publish in the top 5, with tenure resting on little else (teaching ability, other publications, impact, et cetera, do not feature significantly). James Heckman and Sidharth Moktan (2017) show that at the top fifteen economics departments in the United States,¹⁰⁶

¹⁰⁵ There is some dispute as to whether *ReStud* should count as a member of the top 5, or whether the 5 should become 4. For now, at least, it is normally 5.

¹⁰⁶ Heckman and Moktan do not say which ranking they use to pick out the top departments, but their data would be striking for all commonly used rankings.

those that receive tenure only achieve marginally more non-top 5 publications than those that do not (for the very top departments, those ranked 1-5, there is almost no difference). For top 5 publications, however, the difference is significant. Those that secure tenure at departments ranked 1-5 publish an average of 2.7 more top 5 articles over an eight-year period than those that do not (the difference is 1.6 at departments ranked 6-15). This suggests that publications in non-top 5 publications are not considered (or at least not significantly) in tenure decisions, with the tenure resting almost exclusively on ability to publish in the top 5.

The importance of the top 5 does not end at tenure. Top 5 publications are also instrumental in securing pay rises, research grants (sometimes via internal faculty rankings), requests for professional advice, and invitations to conferences and similar events. Given this, it is not surprising that submissions to the top 5 journals more than doubled, from 2 800 to 5 800, between 1990 and 2011 (Card & DellaVigna, 2013).¹⁰⁷ The top 5 has become so dominant that Heckman (2017) reports that young economists now frequently measure themselves and their progress by their number of published or forthcoming top 5 articles. Nattavudh Powdthavee, Yohanes Riyanto, and Jack Knetsch (2018) even find that, other things being equal, economists will judge a colleague's CV as worse if it includes a reference to an article in a lower ranked journal.

Stars

Economists are prone to elevating particular individuals. The prizes in economics (the *Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel* and the *John Bates Clark Medal*, in particular) serve to separate out particularly venerated stars. Prize awardees are typically afforded greater voice and authority in the discipline and their citations are significantly boosted by their awards (Offer & Söderberg, 2016). Ben Clift (2018) also notes that famous economists play an important role in many policy institutions. In the *International Monetary Fund* (IMF), for example, new ideas, memos, and proposals do not catch on unless they cite at least one household name economist.

Sanjeev Goyal, Marco J. van der Leij, and José Luis Moraga-González (2006) highlight the significance of certain individuals in economics by analysing networks of co-authorship.¹⁰⁸ They argue that economics looks like a network of 'interlinked stars'. By this they mean that the distribution of co-authorship is very unequal, with the 100 most linked economists (those with most co-authors) averaging 25 connections compared to the population average of 1.67 (a difference of 10 times the standard deviation). The well-connected stars also have a very low clustering coefficient compared to the network average, meaning that most of their co-authors do not collaborate with each other. As an example of what a network of such an interlinked star looks like, figure 2 shows the network of the most connected economist in the 1990s, Joseph Stiglitz. Between 1990 and 1999, the largest linked component of the co-authorship network accounted for 40.7 percent of all the authors of economics

¹⁰⁷ The number continues to grow. The *AER*, for example, went from 1 644 submissions in 2011 to 1 929 in 2016, an increase of 17% (Goldberg, 2017).

¹⁰⁸ Using a similar methodology to Ductor et al. (2013) and Ductor et al. (2018) (described in chapter 5).

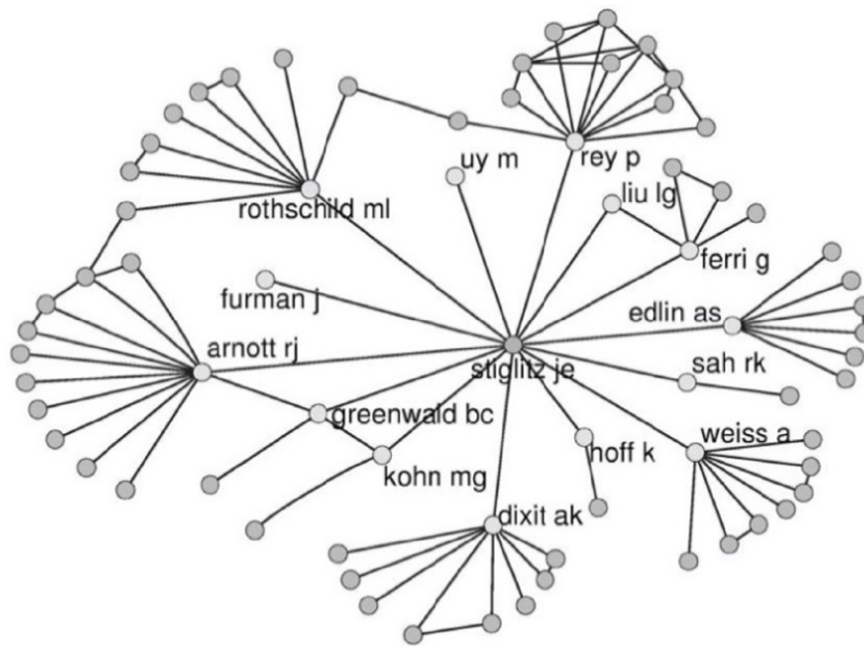


Figure 2. “Local network of collaboration of Joseph E. Stiglitz in the 1990s. The figure shows all nodes within distance 2 of J. E. Stiglitz as well as the links between them” Goyal et al. (2006, p. 410).

journals. That means that 40.7 percent of the authors of economics journals, including authors of just one or two papers (who might normally publish outside of economics journals) and authors of only single-authored papers, could be connected to each other by co-authorship connections. The existence of well-connected stars is crucial for holding this group together, if the most connected 5 percent of nodes are removed from the network it completely breaks down. This suggests that the stars in the network are important go-betweens for others in the network. This is borne out by the fact that the stars have much higher rates of ‘inbetweenness’ (a measure of the frequency that the shortest links between others will pass through them).¹⁰⁹

The significance of interlinked stars within economics has two effects. Firstly, the role certain economists play in connecting others likely gives them outsize influence on the research questions, frameworks, and methodologies in the discipline. Secondly, the output of interlinked stars likely benefits from their networks and reinforces their centrality and influence within economics. Given increases in the length of papers and in the time peer review takes (Ellison, 2002), it would be fair to assume that collaborations entail a significant amount of communication. Lorenzo Ductor, Marcel Fafchamps, Sanjeev Goyal, and Marco J. van der Leij (2013) argue that this increases the research output of interlinked stars by keeping them close to the spread of important (or at least fashionable) ideas. This concurs with the fact, discussed in chapter 5, that co-authorship connections and inbetweenness are positively correlated with research output.

¹⁰⁹ It might be argued that, given these networks are only based on co-authorships, the stars will not really be the only thing connecting different economists. While this may be true, the assumption is that large co-authorship networks signal large networks of communication and connection in general. So, even though the stars may not be the only connection between the economists they link in the co-authorship network, the fact that they have high inbetweenness of co-authorship suggests they also have high inbetweenness of other networks.

Centralisation of governance

Hierarchy is also evident in the governance of economics' professional bodies. Kevin Hoover and Andrej Svorenčik's¹¹⁰ data on the American Economic Association (AEA) shows that from 1982 to 2014, 42 percent of the governing positions of the AEA went to academics from four universities: Harvard, MIT, Chicago, and Stanford. In the same period, 71 percent of those in governing positions held doctorates from the same four universities (Harvard, MIT, Chicago, and Stanford). To see the scale of this centralisation of power it is worth noting that the AEA has a membership of 21,000 and Harvard, MIT, Chicago, and Stanford make up less than 3 percent of the PhD granting institutions in the United States. Fourcade et al. (2015) compare similar findings to the professional associations of other social sciences. For the period 2010-14 they find that 72 percent of the AEA's executive committee came from the top five ranked departments (with no committee members outside the top twenty departments). This compares to 12 percent for the American Political Science Association (APSA) and 20 percent for the American Sociological Association (ASA).¹¹¹

This centralisation is not without consequence. The AEA controls a number of the most important economics journals, including the *AER* (one of the top 5), *Journal of Economic Literature* (*JEL*) and *Journal of Economic Perspectives* (*JEP*)—the latter two being invitation based.¹¹² Thomas Schelling notes that the AEA's three main journals (*AER*, *JEL*, and *JEP*) “represent the scholarly profession; their policies and procedures determine what gets published in them; and what gets published strongly influences the image of economics in America” (Schelling, 2000, p. 528). The *AER* is particularly notable because it accounts for a large chunk of the top 5 papers published (40 percent in 2011-12).¹¹³ Given the significance of top 5 publications on the careers of economists and under the assumption that top 5 publications are weighted roughly equally: “the *AER* now carries substantially more weight in determining the job opportunities and salaries of economists than other top-five journals” (Card & DellaVigna, 2013, p. 150).

The AEA's importance also spreads beyond journals. It plays a significant role in the job market for economists in the United States.¹¹⁴ The president-elect and program committee determine the annual meeting program of the Allied Social Science Associations (ASSA). The latter includes controlling the meetings of 59 other organisations that also come under the ASSA's umbrella (including a number of less ‘mainstream’ ones) and selecting which papers are included in the “Papers and Proceedings” issue of the *AER*. As Fourcade et al. point out, “[t]his procedure ensures a flagging

¹¹⁰ This paper is unpublished. A summary of some of its findings is available online, see (Hoover, 2017).

¹¹¹ This uses *US News* rankings for economics departments from 2013. For some reason, the US news rankings included six departments in the top five that year: Harvard, MIT, Chicago, Princeton and Berkeley tied for first, with Stanford fifth. However, because the numbers would not alter very much if we also used six departments for political science and sociology (i.e. the APSA would still have roughly 15 percent and ASA 20 percent from the top six), and because other data in this section points in the same direction, we can disregard this anomaly.

¹¹² Other journals the AEA controls are: *American Economic Journal: Macroeconomics*; *American Economic Journal: Applied Economics*; *American Economic Journal: Economic Policy*; and *American Economic Journal: Microeconomics*.

¹¹³ An increase from 25 percent in the 1970s, this excludes “papers that can be identified as comments, replies, corrections, or announcements” (Card & DellaVigna, 2013, p. 146). 2011-12 is the last period in the sample.

¹¹⁴ It is not uncommon for universities in other countries to also run searches through the AEA. Both the University of Copenhagen and the University of Oslo have done this recently.

of topics and authors deemed most important by the organization's leadership" (2015, p. 100).

Centralised publishing

In addition to dominating the governing bodies, elite departments make up a significant share of the authors and editors in the top journals (Colussi, 2018; Fourcade et al., 2015; Heckman & Moktan, 2017; Wu, 2007). Tommaso Colussi (2018) finds that, between 2000 and 2006, 25 percent of all the authors in the top four economics journals—the top 5 minus *ReStud*—were employed by just six US universities (Harvard, Chicago, MIT, Stanford, Berkeley, and Princeton), and 47 percent of the authors got their PhDs from the same six universities.

The cases of individual journals are particularly striking. Stephen Wu (2007) shows that, between 2000 and 2003, 14 percent of the pages of the *JPE* were authored by economists from the University of Chicago (where the journal is based), with 15 and 13 percent of the pages of *QJE* coming from MIT and Harvard respectively (both based in Cambridge, Ma., like the journal). The eight departments that provide the most authors to these journals account for 40 percent of the pages of *JPE* and 58 percent of the pages of *QJE*. Even more striking, Colussi (2018) finds that those with PhDs from Harvard and MIT accounted for just under 50 percent of the authors in *QJE* in a similar period (2000-06). Updating these findings, Heckman and Moktan (2017) find that, between 2000 and 2016, 11.9 percent of the papers in the *AER* and 24.7 percent of the papers in *QJE* had at least one author affiliated with Harvard (15.4 percent of *QJE* papers had authors from Chicago, 13.9 percent from MIT, 9.7 percent from Berkeley, and 9.3 percent from Stanford). They also found 14.3 percent of *JPE* papers had authors affiliated with Chicago with similarly high numbers (although not as high as *QJE*) for schools that provided second to fifth most authors.

Colussi suggests that part of the cause of this concentration of authorship is connections to editors. The same six universities mentioned above (Harvard, Chicago, MIT, Stanford, Berkeley, and Princeton) provided a massive 56 percent of the editors of the top four journals between 2000 and 2006, with most of those coming from Chicago (26 percent) and Harvard (15 percent) (Colussi, 2018). Moreover, PhDs from those same six universities accounted for 64 percent of the editors of the top four journals in the same period. The roles of MIT and Harvard PhDs are particularly striking, providing 31 and 13 percent of the editors of these journals respectively. That means that 44 percent of the editors of the top four journals got their PhDs from just two institutions based in the same town and known to have similar philosophies. Colussi uses these figures and adds to them to note that 43 percent of the articles published between 2000 and 2006 were authored by at least one scholar 'connected' with at least one editor at the time of publication—'connected' meaning that an author was employed or studied at the same place at the same time as an editor, had their PhD advised by an editor (this measure uses a proxy),¹¹⁵ or has coauthored with an editor. In particular, 15 percent were former graduate students of editors and 29 percent one-time faculty colleagues. As a percentage of

¹¹⁵ "A Ph.D. Advisor connection is established when an editor had an academic position at the same university and in the same year in which the author obtained his or her Ph.D.; moreover, the two scholars also share at least one research field." (Colussi, 2018, p. 46)

total authors (rather than articles), these numbers are even higher; 46 percent of the authors in the top four journals had at one time been colleagues of editors, and 32 percent had editors as former advisors. Colussi uses this data to estimate that colleagues of a journal editor publish 8 percent more articles, and advisees of an editor publish 14 percent more articles, in the connecting editor's journal during their tenure.

Given the fact that editors and authors are concentrated at so few universities, Colussi's data on connections isn't that surprising, being as it is based on shared institutional history. But even if Colussi's causal claim that proximity to editors causes the concentration of authorship were incorrect, his data sharpens the issues raised by the numbers in this sub-section. Publishing within the top economics journals seems to be easier if one is connected to the top departments, and some institutions seem to gain additional home advantage (*JPE* and Chicago, *QJE* and Harvard and MIT).¹¹⁶ Even if there were no unfair advantages gained (that is even if all articles submitted were judged fairly according to the same standards), this paints a picture of economists connected to just a few US departments being afforded larger voices in some of the premier journals.

Fourcade et al. (2015) note that, although sociology exhibits some centralisation of publishing, the extent of the centralisation in economics is unique. Between 2003 and 2012 employees at the top five economics departments accounted for 28.7 percent of all authors in *JPE* and 37.5 percent in *QJE*, compared to 22.3 percent in the *American Journal of Sociology*. Moreover, PhDs from the top five economics departments account for 45.4 and 57.6 percent of authors in *JPE* and *QJE* respectively, compared to 35.4 percent for the *American Journal of Sociology*. Fourcade et al. also note that the home advantage *JPE* and *QJE* seems to give economists in Chicago and Cambridge (Ma.) respectively is "virtually nonexistent in the main sociology journal edited out of a university department, the *American Journal of Sociology*" (2015, pp. 99-100).

Prestige

Unsurprisingly, hierarchy also plays a role in hiring patterns. Shin-Kap Han (2003) shows that a 'prestige principle', where departments hire from similarly ranked or above departments, exists in many academic departments, but notes two unique things about economics. Firstly, the main 'faction' in economics is more dominant than in other disciplines (Han, 2003, p. 264). This means that there seems to be one accepted hierarchy in economics, whereas other disciplines contain parallel hierarchies that interpret prestige differently. Secondly, the prestige principle is stronger in economics than in any of the other disciplines Han considers.¹¹⁷ Top economics departments are more likely to hire students from other top departments than are top departments in sociology, political science, history, psychology, English, and even mathematics.¹¹⁸ Hierarchy is also built into hiring practices in

¹¹⁶ This is likely helped by the fact that one-quarter and one-third of the submissions to *JPE* and *QJE* were desk rejected in 2000-03.

¹¹⁷ This is also shown by Stephen Wu (2005).

¹¹⁸ Using network analysis of different departments (with vertices defined by one department hiring from another), Han also shows that, in economics, centrality indices—a collection of measures of how central a node is in the network—are also much more strongly correlated with the US News rankings than in other disciplines.

economics in more systematic ways than in other departments. Economics departments frequently offer internal rankings of job market candidates for hiring departments to use (Fourcade et al., 2015).

The first job

For doctoral students that do not train at the best departments, these hiring practices are compounded by Paul Oyer's (2006) analysis that getting a good initial job has a causal effect on having a good job later. Oyer stresses this is a *causal* effect because he does not regress the quality of later jobs with candidates' first jobs but with job market 'health' when they finish their PhD—using *Job Openings for Economists* (JOE) listings as a measure of job market health. The logic is that because the strength of the job market when a candidate finishes their PhD affects the quality of their first job (Oyer also shows this) regardless of their underlying quality as a candidate, job market strength can be used as a proxy for initial job quality. Using this technique, Oyer claims to screen out differences between candidates and focus solely on the effects from their first jobs. He finds that candidates that initially place into tenure track jobs are 55 percent more likely to secure tenure in a later year. And, initially placing into a top 50 department makes a candidate 60 percent more likely to be in a top 50 department in a later year.

Oyer suggests that this effect is in part caused by better quality initial jobs having a causal effect on productivity (measured as publishing activity in terms of number of top 5 publications). He estimates that getting placed in a school ranked thirteenth rather than sixteenth equates to one extra paper in ten years for a person who would normally publish 10-15 papers in ten years. He also estimates that being placed at a top 50 school increases the probability of publishing in the top 5 journals by 50 percent. But he notes that it is not clear to what degree this productivity increase explains the positive effect of initial first jobs. Indeed, Kim, Morse, and Zingales (2009) have contested the idea that elite universities provide productivity gains. They argue that most of the productivity gains that elite universities historically provided have dissipated due to changes in communication technology. It is plausible that placing into an academically stronger department causes candidates to settle on different norms of success and gives them access to more fruitful ideas and collaborations. But it is also plausible that later search committees do not consider the possibility that a candidate's first placement may be in part due to luck (of how strong the job market was when they graduated), and instead see the prestige factor in their first job as a sign of underlying quality. An alternative explanation might be that some other factors that make one attractive to search committees, in addition to journal publication productivity, are increased by getting a better initial job. A candidate with a better initial job is likely to be more central in the hierarchical economic networks, they are more likely to know someone involved in the governance of the AEA or who edits one of the big journals, and they are more likely to receive national research grants.

Regardless of the explanation, everything points to a reinforcing loop. Economics is hierarchical, and the higher up the hierarchy you are able to place (due to luck or otherwise), the more likely you are to stay in that position relative to others.

A steep and consequential hierarchy

Career advancement within economics is heavily determined by publications in the top 5 journals. A few star economists benefit from being central in economic networks. The AEA is dominated by those from a few top universities. Being connected to top economics departments increases an individual's propensity to publish in or edit top economics journals. There is an extremely strong prestige factor in hiring in economics, based on a single hierarchy. And, the success of an economist's career is strongly affected by the rank of the institution that first hires them. Together these features paint a picture in which some economists, some institutions, and some journals are afforded more significance than others in a variety of ways.¹¹⁹ These things come together; the more significant economists are in the more significant departments and publish in the more significant journals.

I suggest that these features give a good picture of the hierarchy in economics and demonstrate that it is *consequential* and *steep*. By 'consequential' I mean that where an individual finds themselves in the hierarchy is likely to have a significant impact on what they can do and where in the hierarchy they will be later down the line. The 'steepness' of a hierarchy is the degree of asymmetry between the power, status, and influence of its members.¹²⁰ By saying that the hierarchy in economics is 'steep', I mean that the asymmetries between economists at different rungs of the hierarchy is large. Each aspect of the way economics is organised outlined above increases the gradient of economics' hierarchy by pointing to new ways in which the distribution of power, status, and influence in economics is asymmetrical. Those at the top might end up as editors of big journals or high up in the AEA, those lower down are much less likely to be in such positions.

6.2 Why worry about economics' hierarchy?

There are two ways to respond to the conclusion that economics is steeply and consequentially hierarchical. The hierarchy in economics, or certain aspects of it, might be justified, either as an unavoidable feature of something else desirable or by bringing positive benefits in its own right. Or, the hierarchy in economics might be taken to be a problem to remedy. In the below, I will argue against the first response and for the second.

¹¹⁹ Although I have not provided a systematic meta-analysis of the data I presented to highlight these features, I have attempted to present the most significant and up to date research on the institutional structures of economics. Even if I have missed some important literature on this topic, what I have presented is no less important. Moreover, even if some of the data I have presented is challenged, the fact that I presented a large amount of evidence, from various different sources, which all point in the same direction, suggests a pattern.

¹²⁰ I draw this from literatures on hierarchy in organisational studies and studies of animal behaviour. In steeper hierarchical organisations, the differences in power, status, or influence between those at the top and bottom is larger. In steeper dominance hierarchies (found in animals), those towards the top end are more likely to defeat those at the lower end in contests. See Cronin, Acheson, Hernández, and Sánchez (2015) and de Vries, Stevens, and Vervaecke (2006) for discussions of hierarchies in animals. See Anderson and Brown (2010) for a review of the literature on hierarchies in organisational studies.

Steeper organisational structures need not be intrinsically problematic. Organisations with steeper hierarchies seem to be able to act more swiftly and decisively. If those in positions of power in a hierarchy want to shift the way an organisation thinks they often have the power to do so. Clift (2018), for example, argues that the IMF's hierarchical structure helped it adapt in the wake of the 2008 Global Financial Crisis. He argues that Olivier Blanchard was instrumental in shifting ideas away from those characterised as the Washington Consensus in his job as Economic Counsellor (often called Chief Economist, the role makes him a member of the IMF's senior leadership team and head of research at the fund), between 2008 and 2015. Clift credits Blanchard, and the power he had as Economic Counsellor, with opening up the fund to new ideas against the preferences of many fund employees.

Steep hierarchies are often thought to benefit group action in two ways: by motivating members to work towards the interests of the group and by making collective decisions more efficient (Levine & Moreland, 1990; Cartwright & Zander, 1953).

Hierarchies are thought to motivate members of an organisation by conferring benefits on those that successfully climb them, under the assumption that when an individual successfully climbs a hierarchy they also contribute to the group's goals. There are two issues with applying this to economics. First, most empirical evidence (across small lab experiments and larger field studies) has found that the effect of hierarchical structures on motivation is negative, rather than positive (for a review, see Anderson & Brown, 2010). Second, the assumption that successful hierarchy climbing by individuals contributes to the wider community's goals is unwarranted in economics. I will argue below that the ways that economists climb up economics' hierarchy often hinders, rather than benefits, the development of research in the field, because the hierarchy creates a number of perverse incentives.

Hierarchies are also thought to make group decision-making more efficient by giving disproportionate control to members with superior abilities and leadership skills and by reining in conflict. Again, there are two issues with applying this to economics. Firstly, it is questionable whether the community goals of economics are anything like efficient and conflict-free decision-making. Under the interpretation of economic knowledge as dispassionate and technical policy advice, this may be the case. The problem is that efficient and conflict-free decision-making would also mean less feedback, less debate, and the consideration of fewer, not more, interests. As we saw in chapter 4, all of these things are important for the sciences (which are not organisations like firms or armies). Secondly, even if efficient and conflict-free decision-making were the goal of economics, empirical findings on the benefits of hierarchical structures on performance in such tasks is mixed.¹²¹ Even

¹²¹ See Anderson and Brown (2010) for a review. Some small scale laboratory studies find that hierarchical structures do better at simple problem solving tasks (i.e., they find the correct answer more quickly) (Maier & Solem, 1952; Shaw, 1964). Other lab experiments have found the reverse to be true (Berdahl & Anderson, 2005). A number of field experiments of more complex organisations have found that steeper hierarchies (in terms of pay or power) perform worse. See, for examples, Ouchi's (2006) study of school districts that found that less centralised districts had better test scores, or Pfeffer and Langton's (1993) study that found greater wage disparities in academia are associated with lower productivity (in terms of publications).

worse for any arguments that a steep hierarchy might benefit economics, Cameron Anderson and Courtney Brown (2010) point out that the studies that find steep hierarchies to be beneficial tend to involve relatively simple, stable, unambiguous tasks, whereas studies that look at more complicated group tasks, or tasks that are ambiguous or require creativity, tend to find that steep hierarchies hinder rather than help performance.

Thus, there is no evidence to suggest that steeper hierarchical forms either motivate economists in a productive way or help to make the decisions and findings in economics more accurate and efficient. In fact, the general literature on organisational forms suggests that, given the kinds of tasks involved in economic research, the opposite might be the case. In the rest of this section I will make this general worry more specific.

Three questions seem important when assessing a specific group decision-making hierarchy. Is the hierarchy facilitating or constraining productive (or correct, or good) choices? Is it able to react to the needs of those it should serve? Is the hierarchy accountable? These three considerations are directly analogous to the three heuristics for evaluating social epistemic practices outlined in chapter 4:

H1: Looking for blocks to productive forms of feedback.

H2: Looking for blocks to certain kinds of interests being considered in judgements of significance.

H3: Looking for blocks to avenues for public scrutiny.

My point in the below will not be that steep hierarchies in research environments are always problematic. The precise structures that lead to fruitful feedback, the consideration of a wide variety of interests, and openness to public scrutiny will depend a lot on the context of the community involved. My point will be, rather, that the specific practices discussed in 6.1 are problematic from the perspective of the framework developed in chapter 4. That is, the steepness of the hierarchy in economics is presently problematic and efforts should be made to alleviate it.

I will break the negative implications of the steep hierarchy in economics into the *perverse incentives* it causes and the *concentration of power* it amounts to. I will argue that both of these factors lead to problems from the perspective of **H1-H3**. I will make this argument in stages. I will start by examining, in 6.2.1, one aspect of economics' hierarchy: the significance of the top 5. I will highlight the perverse incentives and concentration of power the importance of the top 5 causes in turn, and as I do so I will highlight how the issues discussed might be evaluated from the perspective of **H1-H3**. I will then, in 6.2.2, show how similar issues arise for the other aspects of economics' hierarchy and with hierarchical social epistemic practices in general. Again, I focus on perverse incentives and the concentration of power in turn and highlighting the relation to **H1-H3** as I go.

6.2.1 The top 5

Hamermesh (2018) analyses the citations of papers in a range of journals and concludes that the top 5 vs non-top 5 dichotomy does not do a particularly good job of separating out what it intends to: the

most important, or at least most cited, articles in economics. This is corroborated by the fact that only two of the top 5 were among the five journals with the highest two, five, and ten-year impact factors in 2015 (Heckman & Moktan, 2017).¹²² Moreover, from the perspective of specialist subfields (development, labour, et cetera), the top 5 journals only publish a small fraction of the most influential (by citations) work. Yet, the same economists that publish work in non-top 5 specialist journals still use the top 5 as an important marker in hiring and tenure decisions (Heckman & Moktan, 2017).

Even if we leave the issue of whether or not the top 5 captures what it aims to (the five most important economics journals) to one side, a number of other issues remain. In particular, the top 5 creates perverse incentives and concentrates power in the hands of a small subset of economists.

Perverse incentives

The dominance of the top 5 combines with long review times, high rejection rates, and short tenure clocks,¹²³ to necessitate individual economists to think strategically about the work they pursue. This creates a number of perverse incentives, particularly for early career economists, who might otherwise be hoped to be the most productive and creative. Many young economists will tell you that whether or not a piece of work would get into the top 5 is at the forefront of their mind when starting a project. This consideration crowds out other concerns, including innovation. Publishing in the top 5 and doing innovative work need not be mutually exclusive, but there are reasons to believe they often conflict. The importance of the top 5 incentivises economists to follow known paths to top 5 success. According to a former editor of two top 5 journals (*JPE* and *ECMA*), Lars Hansen, this makes “high-quality follow up papers” more common, as they are the easiest way into a top 5 journal (circa 55-56 mins of Heckman, Akerlof, Deaton, Fudenberg, & Hansen, 2017). It also encourages the herd-following behaviour that creates fads.¹²⁴ And, it incentivises spending more time polishing papers to satisfy editors over taking those papers elsewhere and moving onto new projects. All of this reduces overall output by increasing the time economists spend refining single papers and means that economists spend less time developing innovative frameworks or ideas.

Although there may occasionally be good reasons to limit innovation in certain parts of science (perhaps because taking some things as fixed is important for making progress on others, see chapter 7 for how this can be squared with allowing for feedback), the crowding out of incentives to innovation

¹²² These were *QJE* (2nd) and *ReStud* (5th) for two-year, *QJE* (2nd) and *JPE* (5th) for five-year, and *QJE* (2nd) and *JPE* (4th) for ten-year impact factors. The journal that tops two, five, and ten-year impact factors is the invited *JEL*. Third in all cases is *Journal of Finance*, and fourth for two and five-year and fifth for ten-year impact factors is the invited *JEP*.

¹²³ Tenure track systems are fast becoming the model of employment (for the top end of economics departments at least) all over the world. Even where tenure track systems are not in place, something resembling the process (evaluation based on top publications between 5 and 8 years into an economist’s first job) is quite common.

¹²⁴ Take for example the quick rise in excitement towards the use of machine learning or randomised control trials. The rush to use both these methodologies obscured other factors from view (see ‘Economists are prone to fads, and the latest is machine learning’, No author, 2016). In the case of randomised control trails, Deaton and Cartwright (2018) have argued that they are less reliable than often touted, and that they make it seem like theoretical discussions about causes is not important. Moreover, the elevation of randomisation obscures questions for which randomised trails are hard to apply, on the effects of institutions or monetary policy for example.

in general seems problematic. **(H1)** Feedback helps explain one reason why. Innovative ideas can provide criticisms of existing theories that can then be replaced, enriched, or further justified. Innovative forms of measurement, observation, experimentation, and intervention offer new ways of providing feedback to existing theories and practices through providing new angles on and new forms of resistance from the world. Innovation is, thus, an important mechanism by which scientific knowledge and practices progress (however construed), by which sciences can develop new applications, and by which scientific knowledge and practices are justified. Incentives that undercut innovative work for an instrumental goal that only serves to develop individual careers lowers the prevalence of productive feedback. George Akerlof (in Heckman et al., 2017) argues along these lines by claiming that whole potential forms and types of economics are often ignored by economists as they are unlikely to produce top 5 journals articles.¹²⁵ Similarly, lower productivity means that economists put out less new ideas, likely lowering the amount of interesting observations and ideas in economics.

The emphasis on the top 5 also has an impact on the interests economics considers **(H2)**. Gaps in the big journals highlight that preferences for research that is significant from a societal perspective (or from the perspective of some sub-community) are crowded out by instrumental choices. George Akerlof, for example, laments the lack of detailed reportage that might help shed light on the workings of certain parts of the economy, like the financial system (Heckman et al., 2017). Such crowding out of considerations of significance from any perspective other than what will aid top 5 publications likely curtails the extent to which economists consider the interests of those that are not top 5 journal editors. Given that top 5 journal editors come from a small community (which is 92 percent male), this likely leads to the interests of large sections of society not being considered in determining what to research. Moreover, given that the top 5 are all generalist rather than specialist journals,¹²⁶ the importance of the top 5 also disincentivises economists from doing specialist work, which referees and editors are likely to refer to specific field journals. A lot of important work has historically been published in field journals but the increasing importance of the top 5 disincentivises similar future work. This undercuts niche work that might have particular significance to certain sections of society, work on economic development or the economics of climate change for example. (It also narrows the kinds of feedback that ideas in the discipline are exposed to by pushing more economists into similar generalist areas and discouraging the development of alternative niches **(H1)**.)

The dominance of the top 5 journals within economics also marginalises other mechanisms for communicating economics ideas. Take books, for example. Some of the most historically significant works in economics have been books. Not only where Adam Smith and John Maynard Keynes's most

¹²⁵ Akerlof doesn't offer examples of types of economics he sees as missing. But he does lament the absence of reportage (see next paragraph) and that might suggest he is particularly annoyed at the lack of detailed empirical work that examines particular facets of the economy.

¹²⁶ This means that they publish work that all economists should be able to understand (at least to an extent). This does not mean that they never publish specialist work, but they are much less likely to do so. *ECMA* is slightly between a specialist and a generalist journal. Published by the Econometric Society, *ECMA* aims to promote the connection between economic theory and mathematics and statistics. This means its papers tend to be more technical. But given the wide use of mathematics and statistics in economics, it is debatable whether this constitutes a particular specialist field.

significant works, *The Wealth of Nations* and *The General Theory*, books, but books have been pivotal in shifting more recent economic debates—take Thomas Piketty's *Capital* (2014), Gary Becker's *Human Capital* (1964), or even a more technical book like Lars Hansen and Thomas Sargent's *Robustness* (2008). That is not to mention the role of textbooks. It seems perverse to incentivise researchers (especially the young) to avoid a medium that has been key to developing the field.

Even more troubling, marginalising books marginalises a crucially important mode of communication to those outside of economics, thus undermining an important mechanism for encouraging public scrutiny (**H3**). Books tend to be more visible, more complete, easier to access without university affiliation or an understanding of journal platforms, and more accessible than journal articles. All of these factors make books more likely to be used by other academics, journalists, policy makers, and other consumers and users of knowledge to understand topics. If the value of books among economists is downgraded, then we should expect other disciplines and the public to be less informed about the most recent developments in the discipline. Moreover, it is not only books that are marginalised. Anything that is not a top 5 publication is seen as less important.

Anyone interacting with economic knowledge or practices from an outside perspective is going to find it more difficult to engage and be listened to if they cannot get some handle on what is going on close to the frontier of economic research. As I noted in chapter 4, outside scrutiny need not require the knowledge of an internal expert. But ways of grasping some understanding of what is going on at the frontier are important for outside actors if they are to judge what goes on in a discipline and add relevant criticisms and observations. Some public communication is, thus, needed to cultivate the potential for the public scrutiny of expertise, something that I argued is necessary for legitimate epistemic authority. This is important for all forms of expertise, but particularly important for economics, given the significant societal role it plays. Books and non-journal article forms of communication are crucial mediums by which outsiders can gain this understanding; undermining them undermines public scrutiny and the epistemic legitimacy of economics.

The fact that there is a lack of mediums for those outside economics to understand what goes on in the discipline is highlighted by the way that economists often respond to criticism. Those arguing for pluralism are often, for example, told that they have an outdated understanding of what is going on within economics and that economics should not be judged by its textbooks (Smith, 2017; Smith, 2018; Coyle, 2018). If this is correct, then the significance of the top 5 and the consequent lack of importance of books and other forms of communication with those outside the discipline likely plays a role in causing this situation. Without valuing some forms of communication targeted at a wider audience (even just a slightly wider audience, like other academics), it is difficult for a productive dialogue to form between economists and those outside the discipline. All of this contributes to a sense of detachment between debates on economic issues in the public sphere and those within economics, which makes public scrutiny of the decisions that economists make more difficult. (It also blunts avenues for feedback from outside perspectives (**H1**), and makes it more difficult for those outside to communicate their interests to economists (**H2**).)

Concentration of power

The acceptance (both implicit and explicit) of a clear top 5 by economists justifies one of its causes: a strategy taken by deans, administrators, and hiring and tenure committees of simply counting top publications rather than reading papers to determine an individual or department's worth. This amounts to a delegation of the selection of good scholars and work to the editors and reviewers of top 5 journals. In and of itself, this is an odd thing to do. It seems to be a dereliction of the duties of those tasked with selection and seems to sacrifice the interests of the institutions involved (who may not get the most talented individuals) to save time for the selectors. But the wider effect on the balance of power in the discipline is even more troubling. It increases the significance of the already important decisions that journal editors make.

This can hamper mechanisms for feedback in economics (**H1**). Journal editors choose what to send to reviewers, what to reject before review, who should review papers, and make final decisions on what to publish. They also influence the kinds of topics and work that journals publish. Given the importance of the top 5 journals, their editors have a disproportionate impact on the direction of the discipline. Such a concentration of power amplifies small biases on the part of editors. Michèle Lamont (2009) notes that at many levels—evaluation of student work, admission to post graduate studies, hiring, acceptance of papers, awards of grants, invitations—people favour work that they see as similar to their own.¹²⁷ Social psychologists and sociologists have also noted the prevalence of more general versions of this effect. In many contexts people favour and connect with those that are similar to themselves (McPherson, Smith-Lovin, & Cook, 2001). Moreover, it makes sense for academics to favour work that conforms to positions they have previously defended or that criticises positions of their rivals (Holst & Christensen, 2018). All this means that it is plausible to assume that economic journal editors (and reviewers) exhibit at least weak biases in favour of forms of research similar to their own.¹²⁸

It might be hoped that such biases are countered by one of two mechanisms: a clear way of determining good or 'correct' work or drawing editorships from a wide pool of perspectives. If there is a clear mechanism by which work can be singled out as of the kind that should be published (maybe by some agreed standards for accuracy and significance), then editor and reviewer biases are less likely to be decisive. George Akerlof and Pascal Michailat (2017) draw a comparison with statistics and call the extent to which such a mechanism exists the 'power of tests'. In a model of tenure decisions over time, they argue that the combination of a low power of tests (i.e., a large margin of error in the mechanism for deciding quality) with weak biases in favour of work from one's own

¹²⁷ Also see Tellmann (2016).

¹²⁸ This need not imply any great conspiracy. Top 5 journal editors can be diligent, hardworking, and principled economists and still admit weak biases towards particular forms of research. This is also not specific to economics. What is specific to economics is the concentration of power that amplifies these biases.

paradigm leads to dynamics in which disciplines can be stuck in inferior paradigms.¹²⁹ They argue that economics does not have particularly strong tests because there is no consensus on mechanisms for determining quality. This makes the drawing of editorships from a wide pool of perspectives very important. Diversity in the intellectual perspectives of editors can alleviate the effects of selection biases by spreading them out.

As we saw in chapter 3, determining what should count as a diversity of perspectives admits a wide variety of interpretations. Nonetheless, there are two things we can say about the diversity of intellectual perspectives amongst editors of the top 5. Firstly, the outside influence of a narrow set of journals puts limits on diversity by enshrining a small collection of editors with influence. Secondly, the editors in this already small pool are mostly trained at a small group of elite universities (as a reminder, six universities provided 64 percent of the editors of the top four journals between 2000 and 2006, with MIT alone providing 31 percent (Colussi, 2018)). While not showing categorically that the intellectual perspectives of top 5 editors are not diverse enough, these two facts at least indicate constraints on how diverse they might be. If the top 5 were extended to a top 10 or 20 (or a multi-dimensional ordering) the pool of editors holding power would widen (and likely diversify). If the top 5 journals were less dominated by economists educated at a small subsection of universities, they would also likely bring more diverse perspectives (for some of the reasons outlined in 5.2.1).

Thus, because journal editors in part determine what research is approved, because they plausibly exhibit some selection bias, and because a key mechanism for mitigating bias—diversity—is undermined by the concentration of power in economics, important new ideas and ways of practicing economics are less likely to develop. Moreover, this situation incentivises young economists to follow the preferences of the select powerful older economists in the discipline.¹³⁰ Both of these things severely undercut the feedback within the discipline (**H1**). A lack of new ways of practicing economics undercuts possibilities for new observations, applications, judgements of significance, arguments, et cetera. The incentives for young economists to follow their lauded seniors crowds out innovation and helps to further concentrate power at the top of the discipline by making the work of senior economists even more visible and successful, thus increasing their influence further.

The concentration of power in the hands of a few editors also raises questions about how well economics can incorporate a wide array of interests (**H2**). Can such a small group adequately represent the diversity of interests relevant to economic issues? As I argued in chapter 4, the variety of goals and uses the sciences can be directed towards makes it important that decisions about what topics to pursue, and how, are informed by the interests of significant constituencies in society. Decisions need not always be representative but given that there is no purely epistemic or context independent way of determining significance or the goals of economics, it is important that certain interests are not

¹²⁹ Their concept of paradigm is drawn from Kuhn (1962). Their thought experiment imagines a phase in which two paradigms are competing, with one superior to the other. The idea is that a test in favour of the superior paradigm on aggregate, which has a wide margin of error, can be overwhelmed by biases among those that are supposed to execute the test.

¹³⁰ The latter is highlighted by the fact that 47 percent of the articles in the top four journals in 2000-06 cite one of the journal's editors (Colussi, 2018).

systematically excluded from being considered. The significant societal role economics plays makes this even more the case. The two comments I made about the diversity among top 5 editors above—the importance of the top 5 makes for a small pool of editors and that pool is dominated by a few elite universities—both make it more likely that certain interests will be systematically excluded in the discipline.

Thus, by creating a collection of perverse incentives and concentrating power in economics into the hands of a few editors, the reification of the top 5 falls foul of **H1-H3** in a number of ways. These are summarised in table 5.

The *perverse incentives* the top 5 creates block important avenues for feedback in the discipline by crowding out innovation, lowering output, disincentivising specialist work, and lowering the importance of communicating with the outside (**H1**). They also make it more difficult for interests that are not already well represented within economics (women, minorities, workers, and people from socio-economic backgrounds or without elite educations) to be considered by disincentivising research that might be socially significant, the development of specialist work that might speak to particular concerns, and dialogue between economists and outsiders (**H2**). Lastly, the marginalisation of outside communication and books by the importance of the top 5 makes public scrutiny of decisions by economists more difficult (**H3**). Given their interest in incentives, none of this should surprise economists. In fixed reward systems people should be expected to work to the test. Where rewards depend on where you publish rather than the most important or interesting problems to solve, the former is likely to crowd out the later.

The dominance of the top 5 and their importance in determining career paths also serves to *concentrate influence* over economics into the hands of a small group of editors (who themselves come from a small subsection of universities). This constrains avenues for new ideas within the discipline and means that the preferences of those at the top dominate (**H1**). It also constrains the extent to which research in economics is informed by the interests of those in wider population, particularly those that are not well represented in the higher echelons of the discipline (**H2**).

6.2.2 Hierarchy in general

The reification of the top 5 is, however, not the only thing that makes economics steeply hierarchical. I have described how the top 5 (plus the fact that top 5 editors are concentrated in a few universities) blocks avenues for productive feedback, the consideration of certain interests, and some conditions that aid public scrutiny by creating perverse incentives and centralising power. The problems with other aspects of economics' steeply hierarchical structures can also be viewed through the ways that incentives and power affect **H1-H3**.

Top 5 creates	This leads to	Which blocks
Perverse incentives	Less innovation and lower output	H1 avenues for feedback
	Lower incentives for societally important work	H2 the consideration of a wide variety of interests
	Lower incentives for specialist work that might satisfy niche communities	H2 the consideration of certain interests (plus part affects H1)
	A marginalization of books and other forms of external communication	H3 public scrutiny (plus part affects H1 and H2)
Concentration of power	The amplification of biases	H1 avenues for feedback
	A higher likelihood that certain perspectives and interests will not be considered	H2 the consideration of certain interests

Table 5. The issues caused by the reification of the top 5 and how they relate to **H1-H3**.

Perverse incentives

If hierarchies are recognised as highly consequential, then climbing them can distract from other, possibly more important, goals. Above I noted that focussing on top 5 publications can distract from innovation, wider significance, special interest, and communicating with a wider audience. The pressure to place well in hierarchies that are steep and competitive crowd out other considerations too. As evidenced from the recent replication crisis in psychology and some other disciplines (Baker, 2016), the pressure to publish in academia can encourage p-hacking and other malign research practices that seem to run counter to truth (or the development of socially useful abilities). There are many dimensions to this. In this subsection I focus on one: ‘maverick’ behaviour. The hierarchical structures in economics disincentivise maverick behaviour, meaning that the discipline forgoes the important critical feedback and new ideas that mavericks bring (**H1**).

A number of social epistemologists have used formal models to investigate the kinds of behaviour desirable to form productive epistemic communities. Both Kitcher (1993) and Michael Weisberg and Ryan Muldoon (2009) develop models of scientific communities in which some scientists actively pursue research strategies that are different to all—‘mavericks’—whereas others are more conservative (in the case of Weisberg and Muldoon, these are either followers of others or those that work alone and ignore what others do). In Weisberg and Muldoon’s model the research communities with mavericks (either communities of just mavericks or mixed communities) outperform the research communities that do not contain mavericks. Similarly, Kitcher finds that communities that contain scientists that pursue maverick strategies are likely to be more successful than those that do not. While the abstraction in these models makes it difficult to apply them to practice it is instructive that they both find that maverick behaviour—actively pursuing research strategies that are different to others in the community and going against conventions—as important for a scientific community. The point I

want to make is that many of the hierarchical aspects of economics disincentivise maverick behaviour.

The probability of success of new strategies in economics (that is the probability of being published and influential) is lowered by a small number of generalist journals having such importance and the fact that those journals are dominated by people from a small selection of universities. This is compounded by the fact that the AEA is dominated by the economists from the same schools; meaning that the ASSA meetings and the directions of the AEA journals are dominated by the interests of a select set of economists. The awareness among economists (and particularly young economists) of a clear and consequential hierarchy in economics—where you work affects where you publish,¹³¹ your first job matters a lot, and top 5 publication is crucial for job placement and career progression—also serves to make the competition in economics intense and stressful (Fourcade, 2009). A number of studies have provided reasons to believe that high stakes competitive environments that are perceived to be risky heighten risk-averse behaviour (Loewenstein, Weber, Hsee, & Welch, 2001; Slovic et al., 1979; Weber, Blais, & Betz, 2002). When combined with the lower success rate of new strategies, this likely makes maverick-like behaviour less common in economics.

This is highlighted by, former Chief Economist of the World Bank, Paul Romer's explosive broadside against present practices in macroeconomics. Upon joining the Bank, Romer commented that he felt free to deliver his wide-ranging (and in places funny and rude) critique of macroeconomic methods because he was leaving academia:

This price [of dissenting] is lower for me because I am no longer an academic. I am a practitioner, by which I mean that I want to put useful knowledge to work. I care little about whether I ever publish again in leading economics journals or receive any professional honor because neither will be of much help to me in achieving my goals. (Romer, Forthcoming, p. 20)¹³²

Romer adds that, although some of his academic colleagues agree with his comments about macroeconomics in private, they would never do so in public for fear of reprisals. The intense competition in economics and the pressures and incentives internalised by economists from a young age, makes them unlikely to rock the boat. In addition to disincentivising disagreement with common practices, Romer notes that the culture within economics creates a climate in which economists are unlikely to challenge certain (generally famous) individuals:

[I]t is an extremely serious violation of some honor code for anyone to criticize openly a revered authority figure... After I criticized a paper by Lucas, I had a chance encounter with someone who was so angry that at first he could not speak. Eventually, he told me, 'You are

¹³¹ Many economists outside the top ranked institutions (who call themselves 'LRMs'—low ranked monkeys) often assume that the fact that peer review is only single blinded in most economics journals means their papers are rejected from top publications based on their institutional affiliations (this is regularly openly discussed online at *Economics Job Market Rumors* and at conference social events). Even if this is not true, the fact that there is a lack of trust is an issue.

¹³² This paper has not been published yet. All quotes refer to the September 14, 2016, version available here <https://paulromer.net/wp-content/uploads/2016/09/WP-Trouble.pdf> [accessed: 28/9/18].

‘killing Bob.’ (Romer, Forthcoming, p. 21)¹³³

All of this highlights the pressures that work against criticism, new ideas, and other forms of maverick-like behaviour in economics. New ideas are unlikely to succeed, and economists are less likely to take the risk given the high costs of failure and the stressful and competitive environment in the discipline. Disagreement with accepted practices is likely to come at a personal cost, and disagreement with renowned individuals is frowned upon. Empirical studies on hierarchical organisations show that such effects are common. Steeper hierarchies encourage members to adopt the perspectives of high-ranking individuals (Newcomb, 1943), and discourage those with dissenting opinions from articulating them (Kish-Gephart, Detert, Treviño, & Edmondson, 2009; Milanovich, Driskell, Stout, & Salas, 1998). This all serves to reinforce the issues described in 6.2.1. The steeply hierarchical social epistemic practices within economics disincentivise productive forms of feedback in the form of new ideas and critical interaction (**H1**). Furthermore, the lower instances of criticism within the discipline also makes public scrutiny more difficult (**H3**). This is because, as noted in chapter 4, the existence of debate helps bring out factors that help the public make judgements about expert and scientific practices (it highlights things like biases, untested assumptions, background assumptions, et cetera). When internal criticism is marginalised, the public receive less information by which they might scrutinise the decisions of economists.

Concentration of power

The elevation of certain individuals, the fact that the AEA’s governing body is dominated by those at the top universities, and the fact that where you work seems to affect where you publish all point to positive benefits accruing to those further up economics’ hierarchy. Those higher up are more likely to engage in productive networks, have greater platforms, find it easier to publish in and edit the big journals, and influence (via direct control or contacts) the running of the AEA, its journals, the ASSA meetings, and the economics job market. When combined with the prestige factor in hiring and the fact that an economist’s first job has a big impact on their later career progression, a pattern emerges in which those that get PhDs from the top universities (and likely degrees from the top universities, and come from privileged background before that (Chetty, Friedman, Saez, Turner, & Yagan, 2017)) find it easier to secure influential roles, have their voices heard, and reshape the discipline.

An economist may respond that this is justified. They may argue that all that I have described is a well-functioning meritocracy, in which the best economists rightly rise to the top. I have two answers to this. Firstly, the limited data available on whether benefits are earned or based on a combination of luck and initial position points to the latter playing a significant role. Oyer’s (2006) work, outlined above, indicates that even when luck is involved in where an economist first gets employed, the rank

¹³³ Incidentally, Romer’s attack did not seem to hurt him that much. He went on to win the 2018 Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel. But this serves to highlight another aspect of economics’ hierarchy: once an economist has become renowned enough, critical viewpoints are more accepted. Those lower down the hierarchy are not normally afforded such critical space. Only a select few (who invariably have spent the rest of their careers being less critical) are afforded critical platforms.

of that institution plays a causal role in their later career progression. This might either be due to the benefits that accrue to those higher up the hierarchy, or due to later (hiring, tenure, or grant) committees taking position in the hierarchy to be a signal of underlying quality. In both cases it is initial placement rather than some hidden underlying quality that is important. Secondly, even if the concentration of power in economics could be said to be a well-functioning meritocracy, the extent of that concentration (i.e., the steepness of the hierarchy) causes issues. I already described why concentration of power in the hands of a small group of top 5 editors is problematic in 6.2.1. Regardless of whether those at the top are the best or not, the concentration of power in the hands of a small sub-group of economists limits possibilities for new ideas, incentivises the young to follow the preferences of older luminaries, and limits the extent to which research within economics considers a wide array of interests.

The other aspects of economics' hierarchy have similar effects. The elevation of certain individuals, the domination of the AEA by economists from certain universities, the fact that those at the same universities find it easier to publish in the big journals, and the prestige factor in hiring all serve to further concentrate influence into the hands of the same subsection of economists. This results in the preferences of a small subsection of economists being better heard in debates and more influential in decisions on what gets published, what gets discussed at conferences, how the job market is structured, and how the AEA is run.¹³⁴ Even if that small subsection of economists were the best on some scale of merit, the facts that they have such a large amount of influence and that speaking out is discouraged incentivises following their lead and constrains avenues for new ways of thinking and for criticism in economics (Wu, 2007).¹³⁵ This conclusion is supported by multiple studies that show that hierarchies in general tend to produce homogeneity in thought through selection biases, especially when those higher up have discretion or who is able to rise up.¹³⁶ The concentration of power within economics thus hampers mechanisms for critical feedback in the discipline by lowering the direct criticism of the ideas of those higher up in the discipline and lowering the propensity of new ideas to develop **(H1)**.¹³⁷

This is compounded by the fact that it is difficult for economists who start at lower ranked institutions to rise up the ranks (even if they have important new ideas). Because of the prestige factor in hiring and the fact that those outside the top find it more difficult to publish in the top journals, the voices and preference of those at the top dominate and there are few avenues available for those outside the top to make it there. This might even discourage some from entering or continuing in

¹³⁴ A number of commentators have suggested that this gives economics the character of an oligopolistic market (Hodgson & Rothman, 2001; Hoover, 2017; Akerlof in Heckman et al., 2017). Colussi (2018) also points this out and adds that, when measured on a Herfindahl index of market power, *QJE* is over the threshold considered to signal oligopoly (0.15) for both scholars employed by Harvard and those that got their PhDs from Harvard or MIT.

¹³⁵ There may be exceptions. My point is not that being contrary or innovative can never succeed. Rather, that the steepness of economics' hierarchy lowers the likelihood of being successfully contrary, and likely lowers the extent to which economists feel able to speak out.

¹³⁶ See Gruenfeld and Tiedens (2010) for a review.

¹³⁷ The rankings and hierarchies in economics have contributed to real cases in which certain kinds of ideas have been marginalised. See for example Lee's (2009) history of heterodox economics or Bouchikhi and Kimberly's (2017) recent history of economics at Notre Dame.

economics if do not feel they will be able to adequately progress through the hierarchy (due to their initial position or the kind of work they want to pursue).¹³⁸ Moreover, given that those on PhD programs at higher ranked universities typically come from other well-ranked universities that are more accessible to those from privileged backgrounds (Chetty et al., 2017), the economists who dominate the discipline are also more likely to come from privileged backgrounds.¹³⁹ For the reasons discussed in chapter 5, this likely makes economic research as a whole less considerate of the interests and judgements of significance of less privileged sections of society (**H2**).

Given the power that economics has as a profession (see chapter 1), this has important effects. The domination of a small section of economists is not isolated to the academy but also plays a role in who gets to be involved in policy. Economists from the big universities dominate political and policy appointments. Ariel Rubinstein (2016), for example, draws attention to the recently created Booth Initiative on Global Markets (IGM). The IGM is a panel of economists that are occasionally asked for their opinion on specific policy matters. The IGM declares that it includes distinguished scholars familiar to economists and the media. As Rubinstein points out, these distinguished scholars come from the same small set of universities that dominate publishing and governance within economics:

[A]ll fifty-one experts (yes, all of them) come from six universities (and you guessed them correctly: Harvard, MIT, Stanford, Yale, Princeton, and Chicago). Rubinstein (2016, p. 166)

That a small group of economists at a small section of universities dominate governance, publication, and discussions within economics also makes it easier for premature consensus to form on certain topics. Consensus among experts is an important mechanism by which non-experts can judge the reliability of particular propositions, and thus scrutinise the decisions of experts (**H3**). But consensus only speaks to reliability if opinions are reached independently. Both the small size of the group of dominant economists and the fact that they were trained at a small selection of universities makes the independence of their opinions questionable. Moreover, consensus arrives faster in steeper hierarchical structures because people tend to overestimate the abilities of those at the top (Barnard, 1948), because hierarchies tend to make it harder for a wide selection of members to influence group discussions, and because of the aforementioned tendency towards homogeneity of thought in hierarchies. This means that the concentration of power in economics undermines an important mechanism by which the wider public can judge economic knowledge and thus undermines their capacities for providing scrutiny. In the long run, premature consensus is also likely to undermine the justification of expert authority. Take the public reactions to the 2008 Global Financial Crisis. The seeming consensus among economists that multiple economies (Ireland, Iceland, the United States, et cetera) were healthy prior to the crash, and the regularity with which many economists argued for deregulation, jarred after the crisis. This severely undermined the public faith in economists on a wide

¹³⁸ Again, this is supported by empirical studies showing that organisations with steeper hierarchies tend to have members that are less satisfied and more inclined to leave (Anderson & Brown, 2010).

¹³⁹ Even where their childhoods are less privileged, the fact that they would have had to go to elite universities, and mixed with more privileged students from the age of 18 onwards, is likely to make them less likely to understand the positions of those lower down the socioeconomic hierarchy.

Hierarchy creates	This leads to	Which blocks
Perverse incentives	Less maverick-like behaviour: less new ideas, disagreement, and criticism	H1 avenues for feedback
	Constraints on criticism, which lowers the prevalence of debate for the public to witness	H3 public scrutiny
Concentration of power	The interests of those at the top given greater weighting and younger economists following those higher up. This constrains new thinking and criticism	H1 avenues for feedback
	A lower likelihood that those in marginalised groups will be heard or make it to the top	H2 the consideration of certain interests
	A higher likelihood that consensus will be reached prematurely, undermining a useful mechanism for judging the decisions of economists	H3 public scrutiny

*Table 6. The issues caused by the economics' hierarchy in general and how they relate to **H1-H3**.*

range of issues (even those that have nothing to do with finance or the macroeconomy).

In sum, in addition to undermining feedback and public scrutiny via the creation of perverse incentives, the general features of economics' hierarchy cause issues by concentrating power in the hands of a small sub-section of economists. Economists at the top end of the disciplinary hierarchy accrue benefits that further elevate their positions and give them outsize roles in shaping the field. Even if this is meritocratic, it is a problem. It limits space for new ideas, constrains criticism, and incentivises economists to follow those high up the hierarchy, which hampers mechanisms for feedback (**H1**) and makes it less likely that the discipline will contain perspectives or come into contact with those from less privileged backgrounds (**H2**). Moreover, it makes it more likely that the discipline will come to and/or present consensus earlier than it should (**H3**). These issues are summarised together in table 6.

Conclusion

Economics puts a lot of store in its top 5 journals. Certain individuals are particularly central to the discipline. The governing body of the AEA is heavily dominated by a small selection of universities (with 71 of the recent members getting their PhDs at four universities), and those same universities dominate the big journals. Lastly, prestige has a strong influence on who gets hired in economics: where one gets their PhD or works has a very large impact—larger than any other field—on their future career progression, even when controlling for underlying quality.

These features of economics make it steeply and consequentially hierarchical. The gradient (steepness) of that hierarchy is greater than in other disciplines. The hierarchical structures in economics create a number of perverse incentives and lead to a problematic concentration of power. It lowers avenues for feedback in the discipline by disincentivising innovation, specialist work, and communicating with the outside, and constraining the development of new ideas and criticism in the field (**H1**). It makes it less likely that certain interests (those not well represented by economists) will be considered in economic research by disincentivising considerations of significance, specialist work, and communicating with the outside, and by concentrating power in the hands of a small group of (elite) economists (**H2**). Lastly, it makes public scrutiny of judgements in the discipline more difficult by disincentivising communication with the outside, constraining criticism in the discipline, and pushing it towards premature consensus (**H3**). These issues are summarised in tables 5 and 6. Table 7 restates these issues to highlight their relation to **H1-H3** and the reasons for pluralism.

Recently, a number of high-ranking economists have started to comment on the dominance of the top 5.¹⁴⁰ This is to be welcomed. But the solutions they suggest are insufficient because their focus is too narrow. Lars Hansen suggests that editors should be bossier, look for good ideas themselves, and rely less on referees' reports (which should mandate less changes). This may lower the amount of time it takes to publish in the top 5 and may lower the effect review has on homogenising papers towards commonly held ideas, mitigating some perverse incentives. But it misses the issues caused by the centralisation of power, and even intensifies the power of journals editors. Angus Deaton suggests that big data can offer a way of making the publication process less biased. But Deaton does not give details and given the issues with metrics of academic quality (Wilsdon et al., 2015), his suggestion seems ambitious and implausible. James Heckman has suggested shifting away from the top 5 to a pre-publication arXiv-like model for economics. This has some merit. It would likely speed up review time and enable innovative ideas that might not make it through top 5 review to impact the field, thus mitigating some of the perverse incentives. However, without equal attention to the other aspects of economics' hierarchy, and the concentration of power in particular, such a model may increase rather than decrease asymmetries. Much like journals articles, working papers are not all read. Economists use proxies to weed out what is worth attention. One of those is the journal that articles are published in. Without this proxy, they are likely to lean on other proxies of quality. Given the widespread belief that economics' hierarchy is meritocratic, this might lead to even more asymmetries of attention to the works of those at higher ranked universities (or those with big reputations). This would likely be compounded by the abilities of higher ranked universities to market the ideas of their academics.¹⁴¹ There are problems with peer review, but that all papers are in principle actually read and evaluated by someone (even in the case of desk rejections)¹⁴² means that everyone gets a shot. This is to be preferred to a situation in which the only people read at all are those that dominate by virtue of their reputation and institutional backing.

¹⁴⁰ All of the suggested solutions discussed here come from a recent ASSA panel (Heckman et al., 2017) or from Heckman and Moktan (2017).

¹⁴¹ Brand, reputation, and marketing are as big a part of what sells in a market as quality of product.

¹⁴² Although there are suspicions that this is not always the case.

Why pluralism?	Evaluative heuristic	Issues caused by economics' steep hierarchy
(ii) Epistemic limitations	H1: Looking for blocks to productive forms of feedback	<i>Perverse incentives</i> that lead to less innovation, lower output, less disagreement, and less criticism <i>A concentration of power</i> that amplifies biases and incentivise the young to follow those higher up. This constrains new ideas and criticism
(iii) A variety of goals	H2: Looking for blocks to certain kinds of interests being considered in judgements of significance	<i>Perverse incentives</i> that lower the significance of societally important and specialist work <i>A concentration of power</i> that makes it more likely that certain perspectives and interests will not be considered and less likely that those from marginalised communities will make it to the top of the discipline
	H3: Looking for blocks to avenues for public scrutiny	<i>Perverse incentives</i> that marginalise books and other forms of external communication and lower the prevalence of debate for the public to witness <i>A concentration of power</i> that makes premature consensus more likely, undermining a useful mechanism for judging the decisions of economists

Table 7. *The issues caused by the hierarchical structure within economics and how they relate to H1-H3 and the reasons for pluralism.*

What might be done instead? I will not offer a complete set of suggestions here. My point has been to show *how* and *why* the steep hierarchical practices in economics are problematic. But I offer two comments. First, in all of the issues discussed above it is the gradient in economics' hierarchy that is the issue. If the steepness of the hierarchy was lessened, the implications would also lessen. A weaker prestige factor in hiring would make it easier for ideas to move from lower ranked universities to those higher up, diversifying possible ideas in economics. A wider selection of important journals would lower the intensity of the perverse incentives caused by the top 5. More diverse governance would widen the interests considered in setting the ASSA program. And so on. Changes should be made with care, however, because: second, the issues discussed above are not separate. The suggestions of Hansen and Heckman for mitigating the perverse incentives caused by the top 5 are not useless. The issue is taking them in isolation. It is not only the top 5 that is an issue, but the wider hierarchical practices in economics that have manifested themselves in obsession over the top 5. The different aspects of economics' hierarchy serve to reinforce each other. Any solutions should look to act on many of them at once. A combination of Heckman's suggestion of prepublication with some method of reducing the dominance of certain universities could, for instance, be a first step.

7 Insiders and outsiders

In the previous two chapters I pointed to issues in the social makeup and internal social organisation of economic inquiry. Economics is very male dominated and steeply hierarchical. In this chapter I focus on the way economists relate to non-economists. I will argue that economists often assume superiority to other social scientists and that they are dismissive to outsiders.

I will describe how economics relates to those outside the discipline in 7.1. Even though it may be argued that the theories and methods of economics exhibit diversity, economists are socially unified through their training and internal hierarchy. Economists tend to assume that the technical skills they accrue through their common training are more valuable than the skills other social scientists offer. This assumption is in part supported by the fact that economists are better paid and employed in a number of politically significant roles. Economists' technical skills also promise universal application, have been instrumental in shaping many political decision-making procedures, and create barriers to entry. I will argue that the net effect of these factors is that economists form an in-group that regularly disregards criticism, ideas, and observations from without.

I will use **H1-H3** to argue that this way of relating to those outside of economics is problematic in 7.2. It constrains feedback within economics and in wider discussions on economic topics by skewing the competitive interplay of ideas in the social sciences, and by blocking the development of 'complementary science', whereby topics not normally tackled within economics can be developed (**H1**). It makes it difficult for those not already well represented in economics to have their voices heard in the discipline (**H2**). Lastly, it undermines the public scrutiny of economic research and of decisions made by economists by reducing the prevalence of debate for the public to witness and giving a false impression of consensus (**H3**).

7.1 External hierarchy

Despite the lack of clear answers to whether or not economics should count as theoretically or methodologically pluralist, two factors indicate that the discipline exhibits a kind of social unity. Firstly, economists tend to have similar educational backgrounds. Despite some recent debate about the content of undergraduate education in the UK, British and North American economics departments largely agree on a core set of principles and tools that structure graduate training.¹⁴³ They also rely on textbooks—which tend to be written by faculty from elite departments—to a greater degree than other social sciences, and on fairly homogenous standards of evaluations (Lamont, 2009). Since the middle

¹⁴³ See Post-Crash Economics Society (2014), Cambridge Society for Economic Pluralism (2014), and (Svenlén et al., 2018) for the debate on education in the UK. See Fourcade et al. (2015) and Earle et al. (2016) for a summary of the literatures on the similarities between economics programs.

of the twentieth century economists from mainland Europe and elsewhere have increasingly received their training in the United States and Britain.¹⁴⁴ In contrast, the share of foreign students going to France and Germany has been steadily in decline. This has given the educational core that is common in Britain and America global significance. Foreign economists trained in the US and UK have gone back to their home countries to reshape economic research and policy. US and UK trained economists also dominate a range of international organisations—the World Bank, the United Nations, regional development banks, the International Monetary Fund—including posts reserved for foreigners (Coats, 2005; Fourcade, 2006; Fourcade, 2009). Fourcade argues that this trend has helped develop economics into a profession with a unified and global identity (2006; 2009).¹⁴⁵

Secondly, metrics on co-authorship and hiring suggest that economics is dominated by a single network and hierarchy. The percentage of authors in economics journals connected by co-authorship to a single network grew from 15.6 percent between 1970-79 to 40.7 percent between 1990-99, while the second largest network fell from 122 authors to just 30 (Goyal et al., 2006). Given that this data includes authors of just one or two papers (who might not normally publish in economics journals) and those who only single-author papers, this indicates that most economists are part of a single network, without significant rival networks. Moreover, hiring prestige in economics follows a single hierarchy (see chapter 6). Academic disciplines tend to have multi peaked prestige hierarchies, meaning that different universities are seen as better for certain ways of approaching those disciplines. Economics is the one exception. This indicates that economists agree on the top universities regardless of their approach or specialisation.

This social unity is reinforced by the view, common among economists, that the perspective economics offers is more valuable than the perspectives offered by other social sciences (Fourcade, 2009; Fourcade et al., 2015; Lazear, 2000). Not only do economists recognise a hierarchy in their own discipline (see chapter 6), they often conceive of a hierarchy within the social sciences, with economics on top. David Colander, for example, found that 77 percent of economics graduate students agreed that “[e]conomics is the most scientific of the social sciences” (2005, p. 184) (50 percent strongly agree, 27 percent agree somewhat).

This assumption of superiority is supported by economic theory. Economists are paid more than other academics in the humanities and social sciences, and even narrowly outstrip engineers. Economists in the United States, for instance, are paid around 30 percent more than their colleagues in sociology (this applies to both median earners and to the top 10 percent, Fourcade et al., 2015). The difference is likely to be higher still as this figure does not account for outside sources of income (consultancy roles, sitting on boards, et cetera), on which economists outstrip other social scientists (Weyl, 2017). In the UK, economics departments now typically break the national academic pay scale

¹⁴⁴ Fourcade (2006) notes that, in 2000, 54 percent of economics graduate students in the United States had temporary student visas. Around the same time, only 10 percent of those in top-rated UK economics PhD programs were British.

¹⁴⁵ This is in contrast to most other professions that grow within nation states, which partly regulate their conditions of entry (Abbott, 1988).

to add an “Economics Market Supplement”.¹⁴⁶ By teaching that employees are (or should be) paid the marginal product of their labour and that value is marginal, economic textbooks, thus, seemingly imply that economists are right to assume that what they offer is more valuable than what other academics offer.

Economists typically put the higher value of their work down to their technical skills.¹⁴⁷ Skills that make them easily employable elsewhere. Even if the inference made from this, that their skills are in fact more valuable, could be questioned, the fact that economists are more able to find influential and well-paid employment outside of the academy than other social scientists is true. In addition to earning lots in finance and consulting, economists are frequently employed in powerful and politically consequential roles. As I described in chapter 1, the demand for economists increased dramatically after World War II. Economists were brought into every branch of government in the United States, and were instrumental in constructing the institutions of the European Union and in readying member states for entry (Bernstein, 1995; Hirschman & Popp Berman, 2014; Coats, 2005). Elsewhere, economists replaced lawyers as the go-to experts for policy and have frequently been used to provide cover for governments against internal critics and to provide legitimacy for states on the world stage (Bergh, 1981; Christensen, 2017; Clift, 2018; Markoff & Montecinos, 1993; Fox, 2014; Wright & Mata, Under review). This growth in demand for economists in positions of power went hand in hand with a rise in influence of policy devices and measures designed by economists (Fourcade, 2006; Hirschman & Popp Berman, 2014). Economics offered universally applicable constructs, and in the latter part of the twentieth century it fast became necessary for states, governments, corporations, and others in public life to understand and engage in debates via those constructs (targeting growth of Gross Domestic Product or utilising cost-benefits analysis, for example).¹⁴⁸

The technical and universal-seeming tools of economics have, thus, helped reinforce the power of the discipline and helped justify economists’ assumed superiority. But these techniques and tools are not always easily understood by those outside of the discipline. Much of economic reasoning is opaque to social scientists unfamiliar with mathematics, and the different notation and labels economists use often makes economic techniques difficult to understand even for those with the requisite mathematics skills. This protects economics from the ‘anybody could do that’ style of criticism that sometimes befalls other social sciences, and also helps to protect economists’ domain of

¹⁴⁶ A recently advertised University College London lectureship (an entry level post) in economics, for example, added a £15,300.00 per annum “market supplement” to the standard pay. This represented an addition of 29-36 percent to the salary of an equivalent post in another social science (depending on where in the salary spine the appointee would have started). Market supplements are in principle available to all departments. In practice, they are regularly advertised as part of the deal in economics, finance, and business departments, but not elsewhere. LSE’s market supplement policy even explicitly mentions economists as its target group (London School of Economics, 2017). There is evidence to suggest that economists are paid better elsewhere too. Even in social democratic and equality concerned Norway, for example, the highest paid academics are economists (Lindqvist, 2017).

¹⁴⁷ See Lazear’s (2000) claim that the power of economics over other disciplines lies in its rigour, and Freeman’s (1999) assessment that other social scientists have less powerful analytical tools and are less able to deal with mathematically demanding topics. Also see Fourcade (2009) and Fourcade et al. (2015).

¹⁴⁸ Economic ideas have also played important roles in a number of political shifts (Amadae, 2015; Blyth, 2002; Blyth, 2013; Christensen, 2017; Hirschman & Popp Berman, 2014; Mirowski, 2013; Mirowski & Plehwe, 2009; Offer & Söderberg, 2016).

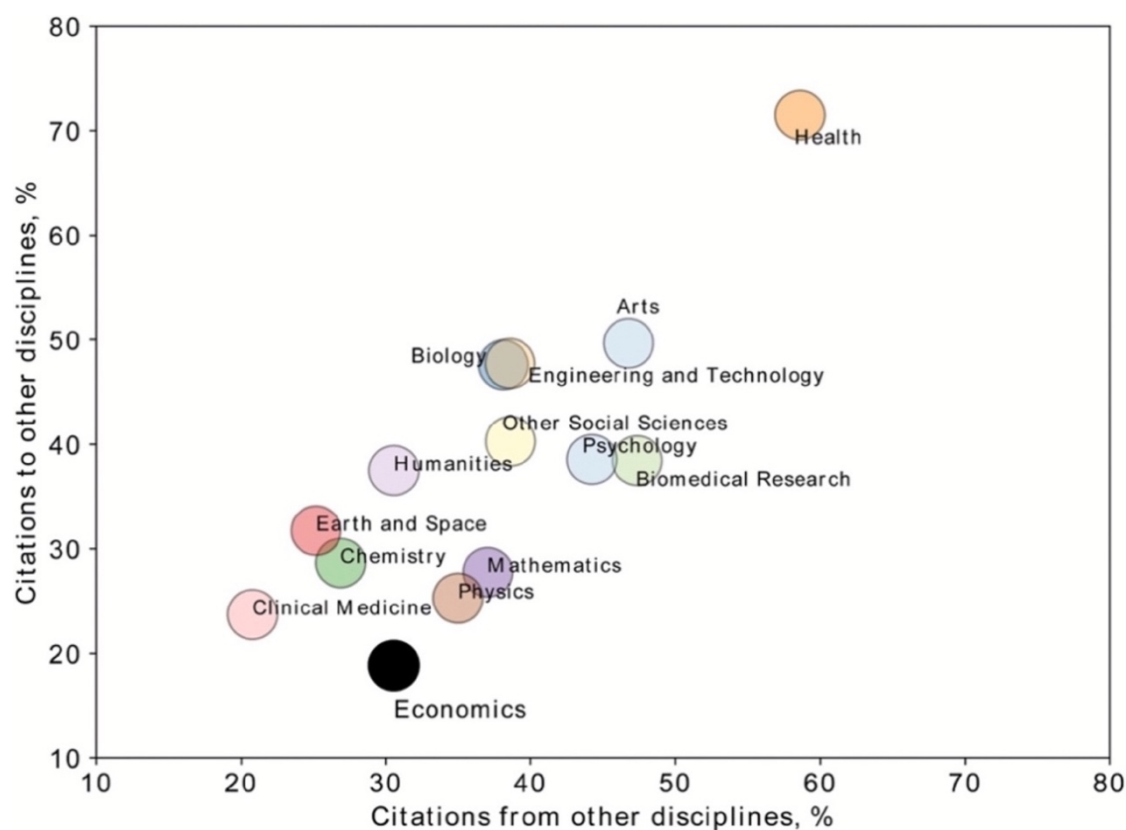


Figure 3. Citations to other fields as a percentage of total citations from a given field (y-axis) and citations from other fields as a percentage of total citations to a given field (x-axis). From Haldane and Turrell (2018, p. 221) with data from Van Noorden (2015).

expertise.¹⁴⁹ But it also puts barriers up to those that might want to talk across disciplines. Those that might have something to say on topics that concern economics (on labour practices or the reasons for migration, for example) are often not up to date enough with economic tools to have their voices heard. This is compounded by the disincentives for economists to write books and other forms of accessible output that would help communicate their reasoning and ideas to those outside the discipline (see chapter 6). The combination of opaque tools and a lack of effort to engage in external communication gives economic recommendations the appearance of emerging from a black box and undermines reasoned engagement between economists and those outside of the discipline.

This and the fact that economists assume themselves superior to other social scientists is reflected in how little they refer to research outside of economics. Between 2000 and 2009, 40.3 percent of the citations in the *AER*, for example, went to articles in the top 25 economics journals, with only 0.8 percent and 0.3 percent going to the top 25 political science and sociology journals respectively. In comparison, 22 percent of the citations in the *American Sociological Review* went to the top 25 sociology journals, with 2.3 and 2 percent going to the top 25 economics and political science journals respectively (Fourcade et al., 2015).¹⁵⁰ Jerry Jacobs (2014) estimates that in 1997 only 19.3 percent of economics citations went to work outside of economics, compared to interdisciplinary citation rates of

¹⁴⁹ This concurs with Abbott's understanding of professionalism as growing out of abstraction, by using it to demarcate domains of expert authority: "[P]ractical skill grows out of an abstract system of knowledge, and control of the occupation lies in control of the abstractions that generate the practical techniques." (1988, p. 44)

¹⁵⁰ For the *American Political Science Review*, it was 17.5, 4.1, and 1 percent to the top 25 political science, economics, and sociology journals respectively.

51.9 and 58.7 percent in sociology and political science respectively. Similarly, figure 3 highlights that the percentages of citations from economics journals to other disciplines between 2001 and 2010 were well below those of other social sciences (Haldane & Turrell, 2018; Van Noorden, 2015). This pattern is not accidental; 57.3 percent of economists disagree or strongly disagree with the statement “in general, interdisciplinary knowledge is better than knowledge obtained by a single discipline”. This is compared to 25.3 percent of sociologists and 28 percent of political scientists.¹⁵¹

The social unity, assumed superiority, societal influence, universal application, barriers to outsiders, and insularity described above combine to create an in-group mentality in economics.¹⁵² This mentality serves to further separate economists from other social scientists. They do not only isolate themselves; they also often positively dismiss the possibility of criticism from the outside. At conferences, in seminars, and in casual discussions economists will often make it known that criticisms of the theories or methods of economics from without aren't to be taken seriously. In a paper that is otherwise fairly critical of present practices in economic research, Ariel Rubinstein (2016) recently defended such a position while celebrating some mild criticisms of economics from Dani Rodrik:

Critiques by noneconomists often leave the impression that they have misunderstood what economists do... One needs to be an outsider to criticize economics, but one needs to be an economist to do it sufficiently well so as not to be drawn astray by stereotypes. What I call ‘in-outsider economists’ are rare and Dani is one of them. (Rubinstein, 2016, p. 163)

Although difficult to measure, this prickly attitude to external criticism can be seen in the reaction to almost any criticism that comes from outside of economics.¹⁵³ It is also highlighted by Mohsen Javdani and H.-J. Chang's (Under review) recent study on how economists react to statements made about their field. Javdani and H.-J. Chang find that the source attributed to quotes significantly affects how economists rate their content. Statements about aspects of and common assumptions in economic research¹⁵⁴ attributed to those seen as outside the ‘mainstream’ in-group or given without a source receive considerably less agreement than those from within the ‘mainstream’ in-group.¹⁵⁵

This reaction to external criticism is compounded by some of the internal pressures against criticism outlined in chapter 6. But even where internal criticism is tolerated, taking internal debates public is frowned upon. Dani Rodrik claims, for example, that behind closed doors economists discuss the many different ways that free trade can fail or be problematic, but that they conceal their doubts in

¹⁵¹ This data comes from Gross and Simmons' 2006 survey of American professors in a range of disciplines (2014). Similar results to those in this paragraph have been found by Pieters and Baumgartner (2002), who note asymmetric citation patterns between economics and the other social sciences.

¹⁵² Passing comments by economists regularly highlight this. Hamermesh (2008), for example, describes a PhD in economics as a “union card” and Rubinstein (2016) comments on the economics profession as “the guild”.

¹⁵³ For other examples, see Athreya (2010), Smith (2018), or Jason Collins's (2013) often shared blog post that claims that one of the signs that a criticism of economics is good is that it comes from an economist. Also, see Chris Auld's (2013) popular post outlining what makes a bad criticism of economics. Auld's 18 signs of a bad criticism imply that, unless a critic assumes much of the theory in economics textbooks, they are doomed to fail.

¹⁵⁴ An example of one of the statements they gave was: “Unlike most other science and social science disciplines, economics has made little progress in closing its gender gap over the last several decades.”

¹⁵⁵ The economists Javdani and H.-J. Chang classify as ‘mainstream’ fit with my definition of mainstream as ‘what gets published in the top 20 journals’.

their public pronouncements. He recounts exchanges with economists over this disparity in which they argue that publicly airing doubts about free trade gives ammunition to “the barbarians” (Rodrik, 2015; WEA, 2013). For fear of giving ammunition to those that would go too far (in their view) economists hide their doubts over the efficacy of free trade. On this and a number of issues economists present an outward facing consensus despite internal debate.

The internal hierarchical structures in economics encourage this outward facing consensus. As I described in chapter 6, the incentives in economics are directed away from any forms of public communication, and certainly away from dissenting opinions. Moreover, the organisational studies literature suggests that hierarchical structures encourage those lower down the hierarchy to either adopt the opinions of those higher up or stay silent (Kish-Gephart et al., 2009; Milanovich et al., 1998). This is highlighted by Romer’s (Forthcoming) statement that speaking out about methodological flaws or criticising renowned economists is frowned upon. Rubinstein (2016) notes a similar point. He claims that although “soft” criticism is welcomed in the discipline (from economists at least), any open-mindedness recedes if someone is seen as a threat to the profession. Economists are often aware of the outward image of their discipline. Their universal-seeming tools and the consensus they present makes it appear as though their research is the outcome of cumulative progress (Fourcade, 2006). Economists are careful not to undermine this and, in this way, diminish the discipline’s power. In doing so they often constrain debate.

In sum, economists have developed a kind of social unity. They typically see themselves as superior to other social scientists, in part due to their higher pay and technical prowess. They are employed in a range of politically influential roles (much more so than other social scientists). Through those roles and through theory that claims universal domain, they have shaped many public debates and decisions. Their techniques have become necessary in order to comment on many socially pertinent decisions. At the same time, their techniques form barriers to outsiders. This is reinforced by those within economics disregarding the research, views, and criticisms of those outside the discipline. This is compounded by the fact that criticism, communication with the outside, and the public airing of internal debate are all disincentivized within economics. Taken together these factors suggest that economists assume and reinforce a steep external hierarchy in addition to the internal hierarchy within the discipline. Economists and techniques practiced by economists sit on top of this hierarchy. Those outside of the discipline are marginalised and ignored, both by economists and often in public debate on economic issues.

7.2 Why worry that economics assumes superiority and ignores others?

Economists might respond that none of the above highlights any issues. What is the problem with economists forming a socially unified in-group? What is the problem with them assuming their techniques are better? If economists thought sociology had more to offer, they’d have done PhDs in

sociology. Moreover, given the regularity with which ‘unfair’ or ‘misguided’ criticisms are levelled at economics they might argue that dismissing external criticism is justifiable.¹⁵⁶ In what follows I will use the heuristics developed in chapter 4 to argue that these responses are misguided.

Recall that social epistemic practices in a given scientific community can be evaluated by:

H1: Looking for blocks to productive forms of feedback.

H2: Looking for blocks to certain kinds of interests being considered in judgements of significance.

H3: Looking for blocks to avenues for public scrutiny.

I will argue that the external hierarchy that economists assume, coupled with the fact that they often do not listen to those outside the discipline, cause three issues from the perspectives of **H1-H3**. It skews any ‘marketplace of ideas’, and assumes outside voices can only offer deficient, rather than enriching, perspectives (**H1**). It blocks the cultivation of mechanisms that help encourage public judgements and scrutiny (**H3**). And, it reduces opportunities for those not well represented in the discipline to have their interests heard (**H2**).

Economists frequently apply the metaphor of a market to the development of knowledge (Coase, 1974; Director, 1964). The idea of a ‘marketplace of ideas’ is normally traced back to John Milton (1644) and Mill (1859), although neither used the phrase. Mill’s position comes as part of his argument that criticism plays an important constitutive role for knowledge (see chapter 4). Surviving criticism legitimises propositions, and free and open criticism can open knowledge up to new and better ideas. This is by replacing whole propositions with new ones and also mixing and synthesising what is correct in competing positions. This is seen as an argument for a space for ideas to compete and combine with each other under the pressure of criticism—a free ‘marketplace of ideas’.

Alvin Goldman and James Cox (1996) point out that the relevance of the market metaphor is not as obvious as often assumed. They argue that the most favourable interpretation of the metaphor is: given that economic theory shows that competitive markets produce superior products, the same must go for ideas, with products being propositions and superiority implying more truth. They note, however, that the link between markets and superior products is not actually shown by economic theory. The welfare theorems of economics show that market-based allocations of goods are (Pareto) efficient, given preferences, but not that each individual good will be superior or even preferred to alternatives. The relation between a free marketplace of ideas and truth is, thus, not clear.

I side with Goldman and Cox and am doubtful of the market part of the marketplace of ideas metaphor. But there is a slightly weaker version of the metaphor that seems more defensible.¹⁵⁷ Something like: openness and competition can aid scientific progress and the development of true

¹⁵⁶ Examples that economists get particular annoyed at include the arguments that they do not engage with the real world and that they did not predict the 2008 Global Financial Crisis (Coyle, 2018; Rubinstein, 2016; Smith, 2017).

¹⁵⁷ I use the metaphor despite doubts because economists often utilise it and because the aspects of economics outlined in 7.1 undermine their assumption that economics plays a productive role in any free marketplace.

and/or useful propositions. This claim can be defended using two points. First, H. Chang's arguments (outlined in chapters 2 and 4) that competition between systems of practice can spur each other on to cover more circumstances and develop new abilities. Second, Mill argues that, even though true statements do not always win out in competition, they are at least likely to be revived because people will observe the same true features of the world. So, even though a competitive market may not be the best model for the development of knowledge, competition can help, and it is important that space exists for ideas that might potentially be true to be stated.

The problem with everything described in 7.1 is that it gives economists immense market power. Power that can skew the competitive interplay of ideas. The fact that economic tools and ideas dominate policy makes it much less likely that ideas from other disciplines will be heard in public debates. This is compounded by the number of roles economists take in public life, by the fact that economic tools, ideas, and prescriptions can be quite opaque, and by the fact that economists must be employed in some roles for states to have international legitimacy.¹⁵⁸ The fact that economists are comfortable ignoring other disciplines and criticisms from outsiders makes it even less likely that the voices of others will be heard. That economists have a clear idea of accreditation and of their own hierarchy, and that they see those with a training in economics as possessing a more valuable skill set helps support and reinforce the idea that their market power is as it should be and gives them less reason to look elsewhere for ideas. This curtails important avenues for feedback (**H1**), both within economics and in wider policy debates, by narrowing the range of acceptable ideas and criticism. It also makes it less likely that economics will be up to date with ideas and observations as they develop in other fields.

Economists might respond to this by reciting Rubinstein's (2016) argument above. They might argue that they ignore ideas and criticism from the outside out of legitimate frustration with the regular misguided attacks they face. The idea that it takes an economist to criticise economics effectively is common in the discipline. Given the high barriers to understanding what is discussed in economics this does seem like a plausible position to take. How should a sociologist or philosopher be able to comment on what goes on in economics given that they do not have a fraction of the experience or education most economists have? And, if outsiders can't understand big chunks of what is going on in even the generalist economics journals, how are they supposed to comment on economics as a whole? This line of reasoning is based on two assumptions. First, that there is a clear outsider perspective which is distinct from and deficient to an insider perspective. Second, that deficient perspectives can be ignored without problems.

The problem with the first assumption is that, even if external perspectives are assumed to lack some of the technical prowess of internal ones, there can be other ways in which they can offer much needed and enriching, rather than simply deficient, critical input. One example is provided by what H. Chang's (2004) calls *complementary science*: the cultivation of research into questions that scientific

¹⁵⁸ See Coats (1981) for the story in European countries and Markoff and Montecinos (1993) for the story in Latin America. Also, see my discussion of the power of economics in chapter 1. For arguments that this also applies to many other organisations, see Earle et al. (2016) and Kwak (2017).

communities assume or ignore (including foundational metaphysical, pragmatic, and value-based questions) outside of those communities.

Regular scientific practice, what H. Chang calls ‘specialist science’, has two features that make complementary science important. First, H. Chang uses Kuhn (1962) to point out that the existence of most scientific work (from modelling, to experimentation, to measurement) is predicated on taking some knowledge for granted. The knowledge taken for granted may not be settled but must be assumed in order to generate new observations and abilities. It is necessary to assume, for instance, the theories of quantum mechanics for the new standard kilogram measure to be coherent (Kaplan, 2018). Critical questions about the knowledge assumed must be suppressed within the specialist community, otherwise they would undermine the observations and developments built on it. Second, specialist sciences cannot investigate all worthwhile questions, even ignoring ones that need to be held fixed, because they have limited resources.

The history of science suggests that both of these features of specialist science are important. Sciences function and develop well when certain aspects of knowledge are held fixed and scientific communities choose to focus on some questions at the expense of others (Chang, 2004; Kuhn, 1962). This is in part because doing so enables specialist scientists to move on from the early stages of science in which basic assumptions are constantly challenged and focus their attention on detailed work. However, these two features of specialist science come at a cost. They prevent foundational questions and questions outside the main focus of specialist science from being iteratively developed (recall H. Chang’s coherentism in which every aspect of a system must be constantly challenged and refined or replaced).

Complementary science can mitigate this by probing foundational assumptions and following up on questions that specialist sciences ignore outside of the specialist community.¹⁵⁹ This ensures that foundational assumptions are challenged and changed in the light of new information, circumstance, and goals, but that they can also be assumed and utilised within the specialist community in the meantime. Cultivating space for questions necessarily ignored but relevant to specialist science can help bring once forgotten ideas back to life in new contexts, ensure specialist scientific knowledge is criticised and justified, and offer new paths to developing science (Chang, 2004). The assumption that outside perspectives can only be deficient is, thus, wrong. It fails to recognise that they can offer an enriching perspective by providing ideas and observations from outside of the norms and assumptions of specialist science. The development of behavioural economics first in psychology, then as a sub-field in economics, and now as a part of almost all sub-fields in economics, is an example of the positive influence complementary science can have. Ideas developed outside of economics are often relevant to the discipline and can enrich, or even radically alter, what goes on within it given the chance.

¹⁵⁹ H. Chang has History and Philosophy of Science in mind as the ideal place for the practice of complementary science, but different disciplines also seem well placed to play complementary roles to each other. By holding different aspects of knowledge fixed, political science and sociology seem well placed to play a complementary role to economics.

But, for specialist science to benefit from complementary science's enriching potential, complementary science must exist and a path between specialist and complementary science must be available. Specialist sciences need not constantly engage with complementary science (to do so would defeat the point of it being separate), but the recognition that those not enmeshed in the detail of specialist science (and what it takes as given) can offer useful insights is important. The existence of some dialogue with those outside of the day to day practice of specific parts of science is an important way to keep critical feedback about foundational assumptions and questions not seen as central alive without them blocking the development of more detailed economic work. It is this that economics closes off by ignoring outside work and assuming external criticism to be misguided.¹⁶⁰ This is compounded by high barriers to engaging with economic research. This reinforces what I argued under the frame of market power above. In its conceited and dismissive relationship to outsiders, economic research sacrifices potentially productive feedback (**H1**).

Even if we ignore this and assume that Rubinstein's claim that external perspectives can only be deficient, the assumption that there are no problems associated with ignoring deficient viewpoints is false. As discussed in chapter 4, external non-expert scrutiny is an important mechanism by which expert knowledge can be justified and held to account (**H3**). But external public scrutiny requires the cultivation of practices that aid non-expert judgement of expert knowledge: reliable credentials, transparent track records, public communication and engagement, independently formed consensus, public debate, and the availability of information on things like funding patterns, the processes by which judgements are made, biases, and conflicts of interest. None of these things make for reliable judgements in isolation. But together they can lead non-experts to make capable judgements about expert decisions and knowledge.

Thus, to take advantage of the benefits offered by public scrutiny—holding experts to account, justifying expert judgements, ensuring expert knowledge is accepted in an informed manner—and to ensure that its judgements remain legitimate, economics should encourage rather than block public debate and engagement. The features of economics discussed in 7.1 do the opposite. The high barriers to entry in economics, the presumed superiority of economists, and incentives against engaging in popular communication (outlined in chapter 6) combine to lower the development of a shared public-expert language for economic issues and lower the public understanding of economic topics. This reduces the knowledge and the interactions the public might draw on to scrutinise the decisions and judgements economists make. The facts that debate within economics is hidden and that economists rarely debate with others curtail the prevalence of debate that the public can witness on economic issues and reduces these to a set of often misunderstood soundbites. This absence of debate further diminishes the public understanding of economics while also curtailing one of the ways by which information about biases, funding, methods, and conflicts of interest come to light. Moreover, attempts to hide internal debate create a false image of consensus, thereby undermining consensus as a way of judging the reliability of the pronouncements of economists. If the consensus on free trade is in reality

¹⁶⁰ According to Sent (2004), this may have slowed down the development of behavioural economics.

false, then how is the public to know which other consensus statements from economists to take seriously? All of these aspects together—reduced public understanding, not much debate, false consensus—serve to undermine the potential for public scrutiny of decisions by economists (**H3**).

All of this is also an issue from the perspective of **H2**. Take the representation of female interests within economics, discussed in chapter 5, for example. I argued that the absence of women in economics makes it less likely that their interests will be heard in the discipline. One way of partially mitigating this is to ensure that the interests of women are able to enter consideration in another way. But if economists don't listen to outsiders, then that becomes more difficult. Without women in the discipline or female voices on the outside being listened to, how are economists expected to be aware of the interests of women? Something similar might also be said for other sections of society that are underrepresented in economics.¹⁶¹

Conclusion

Economists have a sometimes fraught and often dismissive relationship with those outside of the discipline. In developing a sense of superiority and internal unity, economists have formed barriers to those outside the discipline. They have also developed a habit of ignoring external research and dismissing external criticism, while hiding internal debate. This is reinforced by the fact that economists are paid better and that their ideas carry significant weight. All of this blocks important avenues for critical feedback (**H1**), both within economics and in wider public discussions, by skewing the competitive interplay of ideas coming from economics and elsewhere. It also prevents economic research from benefiting from the feedback that can come from complementary science—which can help investigate important questions around economic topics without the whole discipline having to revert back to foundational debates. The dismissal of internal criticism and the obscuring of internal debate also reduce the prevalence of debate for the public to witness and makes external public scrutiny more difficult (**H3**). This is compounded by a lack of engagement with the public and a false image of consensus, both of which also undermine the potential for public scrutiny. Lastly, all of these issues reduce the opportunities for those not well represented within economics to have their interests heard within the discipline (**H2**). These issues are summarised in table 8. Together they imply that both the development of research in economics and the legitimacy of economic expertise would benefit from a less conceited and dismissive relationship to outsiders.

¹⁶¹ The data is strongest on gender imbalances, but some data suggests that the situation is worse still for ethnic and class diversity (Bayer & Rouse, 2016).

Why pluralism?	Evaluative heuristic	Issues caused by the dismissal of outsiders
(ii) Epistemic limitations	H1: Looking for blocks to productive forms of feedback	Skews the competitive interplay of ideas, both within economics and in wider discussions. Also, constrains the potential for complementary science to enrich economic research
(iii) A variety of goals	H2: Looking for blocks to certain kinds of interests being considered in judgements of significance	Constrains the means by which those not in the discipline can have their voices heard in it
	H3: Looking for blocks to avenues for public scrutiny	Obscures internal debate from public view and reduces debate across disciplines, diminishes the public understanding of economics, and makes it seem like there is consensus within the discipline when there is not

*Table 8. The issues caused by economics' relationship to outsiders and how they relate to **H1-H3** and the reasons for pluralism.*

8 Conclusion

At the beginning of this thesis I asked: *Is there anything wrong with the way that economic research is organised? If so, what are the issues?* My answer is, yes, there are at least three problems with the way that economic research is currently organised: [a] there are not many female economists (particularly high up the discipline), and women face an adverse environment in economics. [b] Economics is steeply and consequentially hierarchical. [c] Economists form an in-group that considers itself superior to other social scientists, and that has a habit of not listening to outsiders. These features of the way that economic research is organised are problematic because they limit: the feedback economic research is exposed to (**H1**), the uptake of certain kinds of interests into economic research (**H2**), and avenues for public scrutiny of economic research (**H3**).

My questions were in part motivated by a fraught debate about whether or not economics is sufficiently pluralist. What do my answers mean for the debate regarding pluralism in economics? Given the significant societal role of economists and economic knowledge, pluralists are right to critically scrutinise the organisation of economic research. I am sympathetic to many of the arguments they make but have argued that [a-c] offer less ambiguous diagnoses of what is problematic in the way that economic research is organised than the claim that it is insufficiently pluralist.

Those that argue for pluralism in economics typically claim that the discipline needs more of certain variables (ontologies, methodologies, schools of thought, et cetera). But there are many variables available, many ways of interpreting the boundaries between variables in practice, and different ways of interpreting how much of a given variable is sufficient for pluralism. It is hard to argue that economics is either insufficiently or sufficiently pluralist because there is a lot of room for interpretation, both regarding the ideal of pluralism and how economics scores in comparison. Where some see one, others see many; where some see enough, others see a lack. By pointing to [a-c], my argument redirects the debate from a fraught and often abstract discussion about pluralism towards a set of more easily measurable issues.

In addition to moving forward the debate about pluralism in economics, my argument is novel in two other ways. Firstly, it brings together and supplements what is, at present, a disparate literature on the social organisation of economic research. By tying empirical data from various sources together, and to social epistemology, I am able to describe three important features of economic research *as a whole* ([a-c]) and explain *how* and *why* they are problematic. Secondly, in offering *non-ideal normative* analysis about the *existing social* epistemic practices of economic research as a whole, my argument offers a new way of approaching social epistemology. Although I focus on just three features of economics, this approach can also be applied to other aspects of the way that economic research is organised (racial diversity or funding patterns for instance) or to pressing issues in the way that other sciences relate to society.

I argued that, although pluralist ideals do not offer clear avenues for change in economics, the

reasons given in arguments for pluralism help to highlight how and why the issues I point to—[a] gender imbalances, [b] hierarchy, and [c] a dismissive attitude to outsiders—are problematic. I showed that the reasons given for pluralism can be categorised into three types based on *the way the world is*, the *epistemic limitations* involved in scientific practice, and the *variety of goals the sciences can have*, and focused on the last two types of reason to develop three heuristics, **H1-H3**, for evaluating social epistemic practices. I then used these heuristics to evaluate the existing practices associated with [a-c].

Arguments that pluralism can help alleviate some of the *epistemic limitations* involved in scientific practice highlight the importance of *feedback* mechanisms that expose scientific knowledge and practices to resistance. Feedback mechanisms can come in many forms, including criticism, reasoned elaboration, and empirical testing. Feedback is important in order to ensure that knowledge in economics undergoes stringent testing, avoids biases and errors, incorporates new observations about a changing world, and progresses. I therefore argued that social epistemic practices can be evaluated by:

H1: Looking for blocks to productive forms of feedback.

All of [a-c] constrain avenues for productive feedback within economics in some way. The gender imbalances in economics block avenues for productive feedback by leaving out female perspectives and lowering the overall quality of critical interaction. The steep hierarchy in economics disincentivises innovation, specialist work, and communication with the outside. It also creates a concentration of power that constrains new ideas and criticism in the field. All of these factors reduce feedback. The fact that economists dismiss outside criticism and ignore work in other fields also constrains the feedback to which economic research is exposed. Moreover, the dismissive attitude to outsiders also combines with an attitude of superiority and the power of economics to limit feedback in wider discussions (not just in economics) by skewing how ideas about economic issues interact. Taken together, [a-c] thus make it less likely that biases and errors in economic work will be spotted, make it less likely that the discipline will incorporate new observations or a range of perspectives, and hamper progress.

Arguments that pluralism is necessary to make economics open to a *variety of goals* draw attention to the choices scientists must make regarding the ends (both epistemic and pragmatic) and uses of their work. Instead of determining exactly how open economics should be to different goals, I suggested evaluating social epistemic practices based on whether they illegitimately constrain certain kinds of goals. This has two aspects: the goals that are considered for selection, and the decisions scientists make about which goals to pursue from those that are considered.

For the first aspect, I drew on Kitcher and deliberative accounts of democracy to argue that certain kinds of *interests* must not be systematically neglected from consideration when scientists and scientific communities decide what should be investigated and to what end (i.e., when they make

judgements of significance), in order for those decisions to be legitimate.¹⁶² As well as satisfying a democratic condition for legitimacy, this ensures that the sciences are able to respond to and further the needs of those they serve, and that they are more likely to pick up on important information. I therefore suggested that social epistemic practices can be evaluated by:

H2: Looking for blocks to certain kinds of interests being considered in judgements of significance.

All of [a-c] are problematic from the perspective of this heuristic. The gender imbalances in economics make it more difficult for the interests of women to be heard in the discipline, meaning that economic research is less likely to pick up on issues and observations that are important to them.¹⁶³ The disincentives to specialist work and communicating with the outside caused by economics' steep hierarchy, along with a lack of incentives to work on societally significant issues, makes it less likely that those not well represented within the discipline (women, minorities, people from poorer socio-economic backgrounds, workers) will have their interests considered. This is compounded by the concentration of power in the hands of a small group of economists. The fact that economics is dismissive of outside voices similarly makes it difficult for interests not already well represented within the discipline to be heard. Collectively, these factors make economics less able to respond to the interests and further the needs of a number of significant constituencies in the societies it serves.

For the second aspect of legitimacy—how scientists choose their goals—I argued that, in order to be legitimate, the decisions economists make must be open to the potential for public scrutiny. Without enabling public judgement of the decisions by economists, the discipline risks the public forgetting why it granted epistemic authority to economists and withdrawing the acceptance of that authority. Scientific communities can enable public scrutiny through a variety of practices: reliable credentials, transparent track records, public communication and engagement, independently formed consensus, public debate, and the availability of information on things like funding patterns, the processes by which judgements are made, biases, and conflicts of interest. I suggested that social epistemic practices can be evaluated by:

H3: Looking for blocks to avenues for public scrutiny.

Both [b] and [c] block important conditions for public scrutiny in a variety of ways. Both the steep hierarchy within economics and the way that economics relates to outsiders diminish communication between economists and non-economists, and between economists and the general public (through disincentivising non-journal articles and barriers to entry/assumed superiority). This makes it difficult for the public to make judgements about what goes on inside economics. This is compounded by the facts that economics' hierarchy puts internal constraints on criticism and that economics dismisses

¹⁶² These interests can include both pragmatic and epistemic preferences. They can include pragmatic preferences to investigate one thing over another, but they can also include epistemic preferences for one kind of knowledge (say abstract models) over another.

¹⁶³ This is separate to the argument above that, regardless of the goals of science, women offer unique epistemically relevant perspectives that if left out diminish the feedback within economics. Regardless of the goals of science female perspective are important, but given that the goals of science are open, female interests should be considered in determining them.

criticism from outsiders and hides what internal criticism exists from public view. These practices lower the prevalence of debate amongst economists and between economists and others for the public to witness, limiting another way that the public can garner information by which to judge the decisions of economists. The potential for public scrutiny of economics is further undermined by the fact that [b] and [c] also hamper the usefulness of consensus as an indicator for reliable knowledge. The hierarchical structures within economics make premature consensus more likely, and the hiding of internal debate makes it seem like there is consensus in economics when there is not. Collectively, all of these factors imply that the decisions economists make about what goals to pursue, and how to pursue them, are illegitimate. Even if economists don't care about their relation to the democratic account of legitimacy I draw on, they should not ignore this diagnosis. As I argued in chapter 4, a lack of potential for informed judgements by the public about the work of economists makes the authority and power that economics wields brittle. By not being open to public scrutiny and manufacturing consensus, economists risk undermining any trust the public have in them, making their expertise less resilient and less likely to be accepted in the future.

Thus, economic research would be more progressive, representative of the interests of those in society, accepted, and legitimate, and less likely to fall into bias if economics admitted more women and made the environment in the discipline less adverse for them, if it were less hierarchical, and if it had a healthier relationship with outsiders.

Calls for pluralism tend to be based on a number of underlying reasons relating to the nature of the world, epistemic limitations, and/or the goals of sciences. The above argument offers a way to circumvent the problems that arise when focussing on pluralism, by analysing instead the direct implications of these underlying reasons for economic research. By starting with evidence of existing practices—rather than pluralist ideals—and by utilising the *arguments* for pluralism, I am able to offer tangible recommendations for change. These recommendations are very much in line with the changes pluralists would like to see (more openness to different perspectives, less hierarchy), but they avoid getting stuck in a seemingly unresolvable debate about whether economics sufficiently matches the abstract ideal of pluralism. Furthermore, because these are *social epistemic* recommendations, rather than recommendations regarding the theoretical or methodological underpinnings of economics, which typically stem from a focus on pluralism, they may be more palatable to economists.

My diagnosis of some of the issues in economics—gender imbalances, a steep hierarchy, and a dismissive attitude to outsiders—may not be exclusive to economics. But I offered reasons to believe that these issues are more pronounced in economics (see chapters 5-7). Moreover, their negative effects are more acute in economics because of the significant power the discipline and its practitioners wield in many societies. The fact that female perspectives are often neglected within economics can and has led to serious gaps in measurements and policy prescriptions that have had significant impacts. The fact that economics is steeply hierarchical slows down the discipline's, and consequently many policy institutions', ability to develop the ideas and tools required as the world

changes (exemplified by the slow pace to recognise the importance of inequality). And the dismissive attitude economists have to outsiders limits the uptake of ideas from other areas of research into public and political life. Given the societal impact economic research has, economists and those around the discipline should endeavour to change the problematic social epistemic practices which I have identified. They should restructure the way that research in economics is organised in order to alleviate its gender imbalances, flatten its hierarchy, and develop a healthier relationship with outsiders.

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