

The problems of macroeconomics as institutional problems: complementing the ‘what went wrong’ story with a social epistemology perspective

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After the financial crisis of 2008, many economists expressed dissatisfaction with the state of macroeconomics. They criticised deficiencies in the dominant dynamic stochastic general equilibrium modelling approach and conceptions of good macroeconomic research behind that dominance. This paper argues that there is a deeper problem in macroeconomics, which remains unaddressed. I connect existing literature critical of the institutions of macroeconomics and of economics in general to the institutional preconditions of effective criticism outlined by the philosopher Helen Longino. I find that as an epistemic community, macroeconomics does not function in a way that adequately supports critical evaluation of established beliefs, norms and practices. This failure may partly explain why many views on macroeconomic modelling, the tenability of which economists questioned after the crisis, were able to persist for so long. My analysis gives additional support to several recent proposals for institutional reforms in economics.

Key words: Institutions, Criticism, Macroeconomics, Social epistemology, Helen Longino

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1. Introduction

The financial crisis of 2008 provoked accusations of a failure of economics and economists. Impossible wishes for accurate predictions of crises aside, many have legitimately asked why economists did not seem to have thought that such a crisis was possible. For most of them, the diagnosis was that macroeconomists had used unsatisfactory models

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that neglected some of the factors that produced the crisis. Some went further and sought to explain this state of affairs by claiming that the preference for those models was due to mistaken views about the scientific standards suitable for the study of the economy, aiming for precision, simplicity and/or uniformity of models when this was not appropriate.¹

In this paper, I complement these diagnoses and provide another account of what was wrong, and to some extent, still is wrong with macroeconomics. Briefly, my answer is that the institutional preconditions of critical interaction among researchers have not worked properly in macroeconomics. This, in turn, may partly explain the persistence of the problems that earlier authors have identified in macroeconomics. To provide this explanation, I have drawn on literature from the philosophy of science and connected it to empirical literature on the institutions of economics. Some philosophers of economics and economic methodologists have indeed called for more attention to the institutions and social organisation of economics (Dequech, 2017; Alexandrova *et al.*, 2021).

Contemporary philosophy of science (e.g. Rolin, 2019; Ludwig and Ruphy, 2021), and philosophy of economics specifically (Gräbner and Strunk, 2020; Lari, 2021; Nelson, 2021), emphasise that diversity and dissent have significant epistemic benefits (as well as drawbacks that must be weighed against the benefits). Among other things, diversity and dissent guard against misplaced consensus by facilitating criticism that can question beliefs that would otherwise remain unchallenged. But the institutions of macroeconomics are not set up in a way that would sufficiently support diversity and thereby enable economists to reap the epistemic benefits of diversity. In contrast with philosophers of science, most mainstream economists celebrate the degree of standardisation and uniformity economics has achieved. It is therefore not surprising that most economists have not connected the failures in the epistemic performance of macroeconomics to its lack of diversity.

This paper is organised as follows. I first outline the main criticisms of macroeconomics that were voiced after the financial crisis (Section 2). Section 3 argues that it would be insufficient only to correct these theoretical deficiencies, because this would ignore the deeper causes of these problems. A candidate for such a cause can be found in the social organisation of the field—specifically, in the poor institutional preconditions of effective critical interaction. The central analysis of the paper is presented in Section 4. There I contrast the existing literature on the institutions of macroeconomics and of economics more generally with Helen Longino's account of ideal preconditions of effective critical interaction and I find significant divergences between the two. Section 4 also comments on various reform proposals for economics, which gain additional support from my analysis. Section 5 concludes the paper.

2. Complaints after the financial crisis

Since the financial crisis, economists have debated what had gone wrong in the field of macroeconomics, if anything. Many prominent economists did not find much of a problem. Ben Bernanke, the chair of the US Federal Reserve at the time of the crisis, argued that academic economics was still in good shape and the major reasons for the crisis were instead failures in 'the risk-management systems of financial institutions and

¹ In this paper, I use the word 'science' in a way that includes the social sciences. Similarly, 'scientific' indicates an association with both the natural and the social sciences.

the financial regulatory systems of the United States and other countries' (Bernanke, 2010). Others were more critical of the state of macroeconomic theory and modelling. Among the disillusioned was the President of the European Central Bank:

When the crisis came, the serious limitations of existing economic and financial models immediately became apparent. [...] Macro models failed to predict the crisis and seemed incapable of explaining what was happening to the economy in a convincing manner. As a policy-maker during the crisis, I found the available models of limited help. In fact, I would go further: in the face of the crisis, we felt abandoned by conventional tools. (Trichet, 2010)

Economists' criticisms of pre-2008 macroeconomics can be found at two levels. Some can be called *model-centred criticisms*. These criticisms seek to convince the audience that the most popular models were unsatisfactory in some way or another. Other criticisms can be called *standards-centred criticisms*. These criticisms go a step further by questioning the standards in light of which economists evaluated different modelling approaches and chose the allegedly unsatisfactory modelling approach. These two characterisations might not capture all of the post-crisis criticism, but they are the clear main lines.²

Let me start by mentioning some of the model-centred criticisms. Many economists expressed dissatisfaction with the fact that one modelling approach, dynamic stochastic general equilibrium (DSGE) modelling, had enjoyed a clearly dominant position in macroeconomic research and policy advice. Among them was David Colander, according to whom only DSGE models had been seen as scientific enough to be used in applied research and policy advice (Colander, 2013).

One perceived deficiency of DSGE models discussed in the post-crisis literature is the lack of feedback loops and other non-linear dynamics. Buiter (2009) lamented that for this reason, '[t]hreshold effects, critical mass, tipping points, [and] non-linear accelerators [...] are all out of the window'. Another source of complaint is the 'representative agent' assumption and the consequent lack of heterogeneity among the agents in the DSGE models, which implies the absence of macro-level phenomena emerging from interaction between the agents (Colander *et al.*, 2009). A third worry is the lack of concern with certain kinds of empirical data. Wren-Lewis (2016, p. 29) explains that unlike some other modelling approaches, the DSGE approach involves detrending techniques that pre-filter the data in a way that the models can set aside the interdependency of credit availability and consumption, better understanding of which would have been crucial for policymakers during and after the crisis. A fourth concern is that the models (or most of them) omitted the financial sector.

Many of the alleged deficiencies of models have since been refined by building more advanced models, while DSGE modelling as a general approach still holds a central place in macroeconomics. Kevin Hoover describes this incremental refinement:

Even the purveyors of the DSGE model found it to be inadequate to the financial crisis. They immediately recognized that it could not address the *financial* crisis when it did not have a financial sector. But their strategy for dealing with the crisis was not to embrace the notion of root-and-branch reform of economics, but to make incremental improvements to their model. If the model lacked a financial sector, then add one. [...] But all the time, the core structure and logic of the model remained untouched. (Hoover, 2023, p. 82, original emphasis)

² For example, Lawson (2009) finds fault not in certain kinds of mathematical models nor in the criteria used to evaluate models, but in what he claims to be the very limited usefulness of mathematical modelling *per se* for understanding the mechanisms behind the crisis.

In addition to the drawbacks of the DSGE approach, some economists also drew attention to the reasons behind the dominance of the DSGE approach—the explicit or implicit standards of evaluation according to which the DSGE approach is the preferable one. The DSGE models exhibit virtues—or epistemic values—that led many economists to value them at the cost of other model types. [Akerlof \(2020\)](#) argued that economics in general suffers from a ‘hardness bias’, which roughly means an exaggerated preference for methods that can yield precise quantitative results. According to him, economists in various subfields had studied the mechanisms and phenomena that together produced the crisis, but they had studied each of them individually, not in interaction with each other, which would have been necessary to conceive that a crisis was possible. He argued that economists’ insistence for ‘hardness’ in research is to blame. Research on the complex interactions of several mechanisms could not have been ‘hard’ enough to be published and thus it was not conducted:

[A] model with all the pieces could not have been published; it would have been considered too far from precise, simple ideas (such as those that motivate simple new Keynesian or dynamic stochastic general equilibrium (DSGE) models); and, in this way, too soft to merit publication. ([Akerlof, 2020](#), p. 412)

A related complaint was voiced by [Wren-Lewis \(2016, 2018\)](#). He argued that macroeconomics would have come out of the financial crisis with a better track record, had the standards of what counts as good macroeconomics been more relaxed. This way, some important research avenues would not have been neglected. He noted a trade-off in macroeconomic modelling strategies. On the one hand, one would like that the aggregate equations contained in macroeconomic models are ‘derived from microeconomic theory, and furthermore the theory behind each equation in the model has to be mutually consistent’ ([Wren-Lewis, 2016](#), p. 26). This desideratum is variously called ‘internal consistency’ or ‘theoretical coherence’. On the other hand, one would like the models to fit empirical data. This is called ‘external consistency’ or ‘empirical coherence’. So far, economists have not found ways to build models that fully satisfy both desiderata. In the decades after the so-called ‘New Classical Counter Revolution’, which was started by [Lucas and Sargent’s \(1979\)](#) criticism of Keynesian economics, it has been customary to require that macroeconomic models must be theoretically coherent. According to Wren-Lewis, this meant that an older, not theoretically coherent approach called ‘structural econometric modelling’ (SEM) came to be seen as inferior to DSGE modelling. However, SEM had the virtue of being more ‘empirically orientated’ than the DSGE approach and thus, according to [Wren-Lewis \(2016, p. 29\)](#) would have forced economists to pay attention to the effects of credit conditions in order to fit the models to the time series data on consumption. In his view, such research on the interaction between the financial and the real economy would have made macroeconomics a more useful field for policy advice, but the research was not conducted because it did not satisfy the prevailing, misplaced perceptions of good macroeconomic modelling.

The criticisms by Akerlof and Wren-Lewis are among the more elaborated ones—they attempted to indicate how alternative values would have led to better results. Passing references to values behind the mainstream approach were made by many others, such as [Eichengreen \(2015\)](#), according to whom economics training had prioritised ‘theoretical elegance over real-world relevance’, [Krugman \(2009\)](#), with the accusation of economists ‘mistaking beauty for truth’, and [Buiter \(2009\)](#), who likewise

claimed that research had been motivated more by its ‘internal logic’ and ‘aesthetic puzzles’ rather than by a desire ‘to understand how the economy works’.

3. Critical interaction in science

The model- and standards-centred criticisms are important and should be taken seriously. To some extent, the criticisms have indeed resulted in increased reflection of the advantages and disadvantages of DSGE models. A popular view has emerged that while DSGE models (especially the pre-2008 ones) have weaknesses, better variations of them have been developed, along with some complementary models for purposes not served by the DSGE approach (Blanchard, 2018; Vines and Wills, 2020).

However, the existing criticisms of macroeconomics leave important questions unaddressed: why did macroeconomists rely on those allegedly mistaken standards in the first place? Why did the complacent atmosphere persist for so long? Why had there to be a disastrous crisis before the critical discussion on the deficiencies of the DSGE models gained traction? This is not the place to decide the issue for or against DSGE modelling or particular standards of evaluating models, but it would be helpful to understand why there could be a consensus for so long over issues that later turned out to be controversial if not entirely mistaken. By understanding this, we might improve the chance that untenable views will be discovered before they result in poor policy advice and another crisis of economics. To answer the question, we can benefit from the tools that philosophy of science provides. In particular, I will draw on insights from social epistemology—the philosophical study of how groups of agents, including academic disciplines, should pursue knowledge.

The explanation offered in this paper is that the scientific self-correction process which proceeds by mutual criticism among researchers—‘organised scepticism’ as Robert Merton (e.g. Merton, 1942) put it—did not function quite as it should in macroeconomics. This allowed the consensus to persist overviews that were later questioned and even deemed to be erroneous. Thus, to complement the well-rehearsed model- and standards-centred accounts of what went wrong before the financial crisis, I will present a *criticism-centred criticism*—an account of what went wrong in the process of challenging received beliefs. Of course, it is difficult to advance with certainty counterfactual claims about the broad trends in a field of research. Accordingly, my intention is principally to indicate deficiencies in the workings of criticism in macroeconomics, and only secondarily to suggest that these deficiencies could explain why it was only after the financial crisis that the DSGE models and the associated scientific standards were strongly questioned.

In this paper, I examine macroeconomics as an *epistemic community*—a group of researchers who are united by various formal and informal institutions and who pursue partly shared epistemic goals in direct and indirect interaction.³ Any epistemic community has a multitude of institutions, that is, ‘socially shared systems of rules of behavior or thought’ (Dequech, 2014, p. 523), that affect and direct the knowledge production by the community. These include norms of various kinds, publication and hiring practices, conceptions of the nature and purpose of the community and other ‘behavioral rules’ and ‘rules of thought’ (Dequech, 2014, p. 524). Institutions also include ‘formal organizations’ (Dequech, 2014) such as universities and scientific journals. An

³ In this paper epistemic communities are understood primarily as knowledge-producing communities rather than as policy advice-providing expert communities (cf. Haas, 1992).

important *desideratum* of these institutions is that they support the critical examination of accumulated knowledge. Whether the institutions of macroeconomics achieve this is evaluated in the next section, with somewhat pessimistic conclusions.⁴

Before going further, I want to clarify two issues. First, below I do not mean to claim that economics needs endless criticism and scepticism without any constructive contributions. All research must build on the results and developments that seem well-established at the time. There is thus a tension between the need to provisionally accept the scientific consensus, and to question it and remain sceptical (Kuhn, 1977; Polanyi, 1962). This point has been recently emphasised by Hodgson (2019) who reminds us that like ‘all functioning sciences’, economics needs a dose of elitism. There should be incentives and sufficiently strong authority structures ‘to encourage respect for existing scientific claims, and to discourage endless or excessive criticism of the vital consensus’ (Hodgson, 2019, p. 140). It is thus not a problem if the institutions of macroeconomics are not *perfect* in the way they support and encourage criticism. Instead, what I intend to demonstrate is that the institutions of macroeconomics *excessively* restrain the functioning of critical interaction. Such a judgment of degree obviously leaves some room to disagree, but I will support my argument by referring to interdisciplinary comparisons where the evidence allows it, to show that the institutional preconditions of criticism are *particularly* weak in macroeconomics.

Second, I want to highlight a further aspect of how my analysis advances the critical examination of the state of economics. In Section 4, I will draw on existing literature on the academic institutions of economics. These analyses stem from sociology and history of economics as well as from perceptive casual observations by economists themselves. While providing insightful descriptive perspectives to the state of economics, they are rarely connected to any normative theory. That is, the descriptions of what economics is like are not connected to a carefully considered theory, account or such, about what economics should be like. Without an adequate normative theory, for example, claims that DSGE models enjoy a hegemonic position can be replied, ‘So what? What’s wrong with this?’ Of course, many authors writing on the state of economics offer some remarks on why certain states of affairs seem undesirable, but my analysis does this in a more systematic and careful way by using a theory developed in philosophy of science. Moreover, by providing a normative foundation for the descriptive literature, which consists of several smaller, somewhat disconnected bodies of literature, my analysis simultaneously provides it with structure. I will show how various previous critical observations can be connected. It turns out that many previous observations about the institutions of economics point to different aspects in one major failure: the collective failure to ensure that appropriate institutional conditions for well-functioning critical reflection are in place.

4. Preconditions of effective critical interaction

In this section, I introduce a social-epistemological account of how epistemic communities should function to support the critical evaluation of accumulated knowledge. I draw on Longino’s (1990, 2002) account of the institutional preconditions of effective critical interaction. Among theories in social epistemology, Longino’s ‘critical

⁴ Lawson (2017) suggests that the lack of methodological change in economics might need an institutional explanation, but in that paper, he does not develop such an explanation in detail.

contextual empiricism' gives a particularly central place for dissent and criticism, and thus seems particularly suitable for illuminating the present issue. I start by explaining the starting points behind the ideal that she outlines for science, and then move on to compare how macroeconomics fares with respect to the ideal.

Scientific practices and scientific reasoning rest on a variety of background assumptions, many of which are taken for granted in the everyday conduct of research. The term 'assumption' is understood broadly, as commitments that are provisionally accepted to guide inquiry but modifiable in face of good reasons to do so. The assumptions may concern, for example:

- The ontology of the subject matter—what (relevant) entities and processes are there? (e.g. individuals with stable preferences and imperfect information, firms with price-taking behaviour, adjustments towards equilibrium ...)
- Methods of data collection—what are admissible data and how should they be gathered? (e.g. statistical information on market transactions and prices, laboratory or field experiments, surveys, interviews ...)
- Methods of analysis—how should empirical and theoretical analysis proceed? (e.g. by the use of regression analysis, by differential calculus, by simulations ...)
- Values that guide research—what virtues should a good theory, model or explanation have? (e.g. simplicity, predictive success, fruitfulness, usefulness for policy advice ...)

Different background assumptions and values may be prevalent in different epistemic communities. Epistemic communities also differ in the kinds of knowledge they try to produce. For these reasons, the acceptability of reasoning is contextual—there are few if any universal criteria for correct reasoning. For this reason, the reliability of science cannot be guaranteed by adherence to a single standard of correct reasoning. Instead, reliable knowledge can be achieved by an ongoing evaluation of reasoning and its background assumptions.

The reliability of science is based on the fact that researchers discover each other's mistakes, suggest alternative interpretations of evidence, point out implicit assumptions, check if achieved results are replicable and in other ways critically evaluate the tenability of each other's research. In other words, the reliability of science is a result of social interaction. Moreover, diversity can be harnessed to make criticism more effective. Often implicit background assumptions can be more clearly seen by those who not endorse the same assumptions, and alternative explanations can be suggested by those who have different convictions on the subject matter. In addition to cognitive diversity (people having different perspectives and views to the subject matter of inquiry), also social diversity (people having different social backgrounds in terms of gender, race, class, education, etc.) is potentially important. A particular social background may involve experience about social phenomena that is not readily available to others, and it may allow one to question assumptions that others are inclined to make (Rolín, 2019).

Building on this social view of science, Longino outlines institutional conditions that epistemic communities should satisfy in order to ensure that criticism can effectively correct errors, biases and unwarranted background assumptions—including untenable value commitments—in the knowledge produced by that community (Longino, 1990, pp. 76–81, 2002, pp. 128–35). She calls these conditions the 'conditions of effective criticism' (Longino, 2002, p. 134). Philosophers of science have previously used Longino's account to evaluate the functioning of various research fields such as

biomedical research (Jukola, 2015) and indigenous studies (Koskinen, 2015). This paper turns the spotlight on macroeconomics.

In the following, I introduce four conditions of effective criticism in turn and discuss them in light of previous research on the institutions of economics.⁵ I will argue that the field of macroeconomics has problems with respect to the two first conditions, while the discipline of economics as a whole fails to live up to the two latter conditions, which arguably also has consequences for macroeconomics.

4.1 Forums and incentives for criticism

The first condition of effective criticism is that epistemic communities need ‘publicly recognised forums for the criticism of evidence, of methods and of assumptions and reasoning’ (Longino, 2002, p. 129). In science, the primary forums for criticism are academic journals, alongside conferences and other events that offer opportunities to express disagreement in public. Importantly for our purposes, Longino adds that this norm includes a requirement for *incentives* for community members to develop and publish criticism in these forums. Challenging conventional assumptions, even established and foundational ones, must be among the kinds of activities that are rewarded by peers. The community must value and reward critical research in addition to original research, so that critical discussion is not marginalised. In addition to the negative part of expressing discontent with a view, effective criticism also involves the positive part of presenting and developing alternatives to that view. In research practice, it has little effect to point out that an approach or framework has drawbacks if one cannot offer an alternative. And the more developed alternative there is, the more effective the criticism is likely to be. Thus, that there should be recognised forums for *criticism* is to be understood broadly, meaning forums for *pointing out drawbacks of established views and developing alternatives to them*. The situation described by many economists, in which there are poor incentives to conduct macroeconomic research using alternative approaches to DSGE modelling, violates this condition of effective criticism.

The DSGE modelling approach has enjoyed a clearly dominant position in macroeconomic research and publication, and it has been difficult to publish studies using other macroeconomic modelling approaches in the most prestigious journals, and such publication possibilities strongly affect whether research using those approaches takes place. This is the picture presented by several well-known economists. Wren-Lewis (2018, p. 65) mentions that the UK’s Social Science Research Council ceased to finance research of SEM models because it did not result in publications in the top journals. Similarly, Akerlof (2020) notes that modelling approaches that were not perceived to be ‘hard’ enough would not be published, so the approaches were not developed with the result that some crucial interactions in the economic system remained unexamined. Likewise, in a Congress hearing, Colander provides his assessment of the funding environment of US macroeconomics:

[W]hen, over drinks, I have pushed macroeconomic researchers on why they focused on the DSGE model, and why they implied, or at least allowed others to believe, that it had policy

⁵ I diverge slightly from Longino’s exposition of these norms. I will not discuss the requirement presented by Longino that scientific discussants need to share some ‘public standards of argumentation’, because the problem of not having any common standards does not arise in intra-disciplinary discussion (Longino, 1990). Moreover, I discuss separately the issues of concentration of power and cultivation of diversity, which Longino discusses jointly under the heading of ‘tempered equality of intellectual authority’.

relevance beyond what could reasonably be given to it, they responded that that was what they believed the National Science Foundation, and other research support providers, wanted. That view of what funding agencies wanted fits my sense of the macroeconomic research funding environment of the last thirty years. During that time the NSF and other research funding institutions strongly supported DSGE research, and were far less likely to fund alternative macroeconomic research. The process became self-fulfilling, and ultimately, all macro researchers knew that to get funding you needed to accept the DSGE modelling approach, and draw policy conclusions from that DSGE model in your research. Ultimately, successful researchers follow the money and provide what funders want, even if those funders want the impossible. If you told funders it is impossible, you did not stay in the research game. (Statement by Colander in [Solow et al., 2010](#), p. 42)

Scientific research also often aims to settle at the right answers instead of endlessly considering all possible theories, models, approaches and so on. But settling on one theory, model or approach is reasonable only when there are (i) sufficient reasons to believe that a single correct alternative exists in the first place, and (ii) sufficient reasons to believe that the currently preferred alternative is indeed the correct one. Neither condition holds in the case of DSGE modelling. For the sake of argument, even if we granted that macroeconomics should ultimately aim to settle on a single theory or modelling framework, the DSGE approach cannot credibly claim to be *the* single correct framework with the same certainty as, say, evolutionary theory can claim to be the correct theory of the origin of biological species. The superiority or appropriateness of the DSGE approach is far from a settled matter, as it constantly faces criticism, even if this opposition does not reach a wide audience ([Storm, 2021](#)). For these reasons, the lack of proper forums and incentives to develop alternatives to the DSGE approach, and thus to effectively challenge that approach, conflicts with the conditions of effective criticism.

4.2 Responsiveness to criticism

One of the fundamental characteristics of science is that it corrects itself when mistakes are discovered. Accordingly, a 2nd condition of effective criticism is that members of an epistemic community pay attention to critical discussions that are taking place and are responsive to the criticism either by defending the targeted views or revising them ([Longino, 2002](#), p. 129). This does not mean that the community must keep on responding to criticism that has already been responded to—rather, the critics also need to be responsive to the answers they receive, or they may lose their status as qualified participants in a discussion.⁶ In any case, a failure by an epistemic community to address the criticism it faces contradicts the conditions of effective criticism. Crucially, it is not only mistakes in observation and analysis that need to be subjected to critical scrutiny, but epistemic communities also need to be responsive to criticism of their deeper commitments, such as views about the value of a particular assumption, method or framework.

Obviously, macroeconomists engage in critical discussion. But this is true only as long as the discussion stays within the established conceptual and methodological frameworks, rather than questioning the appropriateness of those frameworks themselves. This is another violation of the conditions of effective criticism. As in all fields of economics, macroeconomics also has periods of more active methodological discussion,

⁶ A weak point of Longino's theory is that it gives only very abstract and general guidelines as to which criticisms are worthy of serious attention and what should count as an adequate response in the sense that it fulfils the duty to respond to criticism ([Intemann and de Melo-Martín, 2014](#)).

but these are limited to occasional periods of crisis or controversy, and even then it is mostly the elite of the discipline who participates in the discussion (Mäki, 2021).

Several methodologists have noted the culture of avoiding methodological discussion in macroeconomics. For example, Aki Lehtinen notes the tension in macroeconomics between the prevalent view that macroeconomics needs a single, DSGE-type ‘core model’, and the scarcity of serious defences of this methodological position:

Macroeconomics is governed by a stronger methodological discipline than just about any other academic field. At the same time, the culture of shunning and disparaging methodological discussions is prevalent and perhaps even stronger than in other parts of economics [...] it is difficult to find explicit defences of the mainstream approach. (Lehtinen, 2021, p. 254)

Similarly, in response to Storm’s (2021) criticism of the DSGE approach, Colander expresses a very pessimistic view about the chances of gaining attention to the criticisms of methodological foundations.

I suspect that recognition of DSGE fallacies will change little in macroeconomics. The fact that the same criticisms are being made of DSGE macroeconomics today as were made 20–30 years ago demonstrates the failure our post-Walrasian project to change the trajectory of macroeconomic thinking, and the difficulty of gaining audience. The reality is that macroeconomic theoretical research has a life and internal logic of its own, and the arguments and thinking of the small core of inner circle macroeconomic theorists are not going to change without some inner circle ‘in-vogue’ macroeconomist leading the way. That will at some point happen, but the timing is likely to be governed by serendipity, not recognition of fallacies in the model. (Colander, 2021, p. 99)

Hoover (2012) provides an example of a largely neglected criticism targeted at the very fundamentals of the mainstream approach. He observes that Kirman’s (1992) criticism of the representative agent assumption, although published in the *Journal of Economic Perspectives*, received hardly any citations from the advocates of that assumption: ‘there is little evidence that advocates have even noticed the argument against their approach’ (Hoover, 2012, p. 51).

It is important to highlight that the lack of methodological discussion is not due to a coincidental lack of interest in such discussion. Rather, it is a *norm* (and thus an informal institution) not to question some fundamental assumptions, because there are sanctions that discourage economists from voicing criticism in public (Dequech, 2017). In his criticism of the state of macroeconomics, Romer (2016, pp. 20–21) notes the ‘price associated with open disagreement’ with the most admired macroeconomists, adding that it seems to be a norm ‘that it is an extremely serious violation of some honour code for anyone to criticise openly a revered authority figure’. Moreover, the historian of economics Duarte (2012, p. 218) notes that the ‘new neoclassical synthesis’, which emerged during the late 1990s and led to the current DSGE-based macroeconomics, threatened dissenters with exclusion, as in effect it defined macroeconomics in terms by certain assumptions such as intertemporal optimisation of rational agents.

Since the turn of the millennium, there has been a habit of proclaiming consensus on the correctness of the DSGE approach, which certainly serves to support the authority of macroeconomists over policy questions (Duarte, 2012), but this practice arguably further entrenches the view that dissent on some fundamental questions does not need to be taken seriously. The methodological unanimity is emphasised in statements like ‘[Macroeconomists] agree that a disciplined debate rests on communication

in the language of dynamic general equilibrium theory' (Kehoe *et al.*, 2018, p. 164), and, 'If you have an interesting and coherent story to tell, you can tell it in a DSGE model. If you cannot, your story is incoherent'. (Statement by Chari in Solow *et al.*, 2010, p. 35). As Hoover (2012, p. 19) underlines, the dominant view of recent history of macroeconomics 'omits or minimizes alternative paths, including heterodox programs, such as post-Keynesian macroeconomics, and heterodox criticisms, such as those lodged by the Austrian school'.

A dismissive attitude towards methodological questions is not new and not constrained to macroeconomics. Lawson (1994, 2017) notes that the indifference towards methodology seems to have become part of the 'common sense' among economists more generally. Frank Hahn famously advised young economists 'to avoid discussion of "mathematics in economics" like the plague, and to give no thought at all to "methodology"' (Hahn, 1992, quoted in Hutchinson, 1994, p. 287). According to Hoover (1995, p. 716), this tendency to 'dismiss [methodology] as practically irrelevant' goes back at least to Fisher (1933). Also Caldwell (1990, p. 64) assures the prevalence of 'indifference' and even 'hostility' towards methodology among US economists (see also Drakopoulos, 2016).

It seems that the post-2008 years have brought some change for the better, as there has been critical discussion on the state of macroeconomics, such as the Rebuilding Macroeconomic Theory project (Vines and Wills, 2018, 2020). As Hoover (2010, p. 397) notes, 'Economists who had previously thought that methodology should be avoided as a diversion from practical knowledge found themselves more or less openly examining their own methodology'. Assessing the magnitude of the recent change is beyond the scope of this paper, but in any case, it seems clear that before the financial crisis, engagement with criticism of the methodological foundations of the DSGE approach was not very active.

It is unclear what could be done to make mainstream economists defend their preferred approaches more actively against challengers. Clearly, among students and researchers alike, it would be important to encourage and cultivate 'big-think critical thought' (Siegfried and Colander, 2022) that reflects on the limitations of particular models and theories. Some potential remedies have been suggested, although these do not target problems that might be specific to the culture of macroeconomics. Including history of economic thought (Shiller, 2010; Dow, 2019), reading groups of classic non-technical texts (Mankiw, 2022), or philosophy of science in economics curricula might help the students to appreciate the diversity of perspectives one may take to economic phenomena and foster a wider conception of good research. My response to the obvious counterargument—how to find the time to teach all this?—is to point to the value of diversity again. It is the prevalent assumption among economists that the economics curriculum needs to be standardised, so that all students need to acquire largely the same skills. However, this is not obvious. While standardisation has its advantages, so does the opposite. If economics curricula were more heterogeneous, the student population as a whole could learn a wider variety of content, thereby making the pool of skills and knowledge the next generation of economists learns more diverse.⁷

⁷ Economics students have formed activist networks such as Rethinking Economics and the German Netzwerk Plurale Ökonomik, to push for changes in the teaching of economics. Assessing the achievements of this campaigning would go outside the scope of this paper, but it can be noted that the CORE teaching materials (<https://www.core-econ.org/>) which address real-world problems more than was previously customary, have gained popularity. However, I am not aware of significant shifts towards teaching more history of economic thought and philosophy of science to economics students.

4.3 Cultivation of diversity

Scientific research involves judgments on matters in which either there is no unambiguously right choice, or it is not known. These judgments concern the credibility of assumptions, the appropriateness of methods, the sufficiency of evidence, the acceptability of value commitments, the promise of a novel theoretical approach and so on. Conditions of effective criticism require that the power to make these judgments should be distributed. This requirement reflects the Habermasian view that deliberation is more likely to reach a satisfactory conclusion when all views are heard and considered, compared to a situation in which one person or group has the power to exclude or ignore some voices. This distribution of judgment is beneficial only in combination with sufficient diversity of views. It is easy to achieve consensus in a dialogue among like-minded people, but any consensus achieved by a group of heterogeneous thinkers is more indicative that the resulting views have been thoroughly tested for counterarguments. Accordingly, epistemic communities must also support the diversity of views among the members of the community. They should aim ‘to ensure the exposure of hypotheses to the broadest range of criticism’ (Longino, 2002, p. 132) by actively supporting the existence of dissent:

Thus a community must not only treat its acknowledged members as equally capable of providing persuasive and decisive reasons and must do more than be open to the expression of multiple points of view; it must also take active steps to ensure that alternative points of view are developed enough to be a source of criticism and new perspectives. Not only must potentially dissenting voices not be discounted; they must be cultivated. (Longino, 2002, p. 132)

It is worth emphasising that Longino calls for diversity and distribution of judgment on epistemological rather than ethical grounds. Respecting a diversity of perspectives in economics has been called for on ethical grounds (Dow, 2007) and injustice in the context of knowledge and testimony is an established topic of philosophical discussion (Fricker, 2007; Kidd *et al.*, 2017), but the epistemological reasons to value diversity hold independently of the ethical issues.

In this section, I focus on whether the institutions of economics indeed cultivate the epistemically beneficial dissent. While the encouragement of dissent is closely intertwined with the incentives to publish research that criticises established views (Section 4.1), here I intend to focus on how academic institutions support or suppress the presence of those critical views in the epistemic community in the first place. Do employment and funding decisions—and the (explicit or implicit) criteria behind those decisions—as well as contents and practices of education, reflect a view that challenges to consensus views are worth cultivating? I postpone the issue of distribution of judgment to the next section.

Studies from several countries have shown that research evaluation practices in economics have strong homogenising tendencies. In France, Chavance and Labrousse (2018) report that the proportion of appointed economics professors that classify as ‘heterodox’ has decreased starkly since the beginning of the 2000s. They attribute this development to research assessment that relies on journal rankings that do not include heterodox-oriented journals or rank them at the bottom. In the UK, Frederic Lee attributes the increased dominance of mainstream over heterodox economics in research and teaching partly to the Research Assessment Exercise, in which publication in ‘Diamond List’ journals appears to be ‘the dominant factor’ in department rankings and hence in hiring and promotion decisions (Lee, 2007; Lee *et al.*, 2013). Similarly,

Corsi *et al.* (2010) report that research assessment in Italy has favoured ‘research of distinctly mainstream character’. In the Italian context, peer-review-based qualifications of eligibility for professorship have also shown that ‘visibly heterodox’ publications in one’s CV (as measured by two different constructs of ‘heterodox publication’) negatively impact a candidate’s qualification even when controlling for a number of bibliometric indicators such as publications in ‘A-list’ journals (Corsi *et al.*, 2019). This is consistent with an international survey that found that adding publications in lower-ranked journals to a CV with publications in high-ranked journals negatively impacted peer evaluation by economists (Powdthavee *et al.*, 2018). The common finding of these studies is that research evaluation practices in economics employ criteria that reduce the diversity of ideas in economics, preferring mainstream ideas to innovative and intellectually risky approaches. However, by reducing diversity and potential competition, these practices diminish the need of the mainstream approaches to demonstrate their superiority to justify their dominance.

The social–epistemological perspective advanced in this paper gives additional support for proposals that have been made to counter the tendency of research evaluation to homogenise research. It would be a necessary first step to reduce the reliance on publications in the Top 5 journals in the evaluation of economists’ relative merit (Heckman and Muktan, 2020; Frey, 2021), but more should be done. If citation metrics continue to be used, as it is realistic to expect, they should at least be constructed in a way that also rewards contributing to the development of non-mainstream ideas. Such a reform was indeed attempted (unsuccessfully, regrettably) by the French Association for Political Economy (Chavance and Labrousse, 2018, p. 200). Citation counts should be complemented with other ways to evaluate researchers’ contributions. One proposal worth considering is to develop ways to measure the novelty of research, even though such a project is obviously difficult (Bhattacharya and Packalen, 2020).

That epistemic communities include people with diverse theoretical views can also be supported by having diversity in terms of social background. In recent years, there has indeed been increasing concern for the long-standing lack of gender and racial diversity in the economics profession. However, it has not been sufficiently recognised that this is not only an equality problem, that is, an ethical problem, but also an epistemic problem, if people with different social backgrounds differ in their research interests, methodological views and the conclusions they are inclined to draw. This indeed seems to be the case. Lundberg and Stearns (2019) find that women and men, on average, differ in the fields they choose to write their PhD thesis in. An economist’s gender has also been found to predict their views on government regulation, redistribution, environmental protection and even methodological views (May *et al.*, 2014, 2018). Additionally, a person’s socioeconomic background arguably affects his or her views about issues like inequality and redistribution (Brown-Iannuzzi *et al.*, 2017).

Is economics doing particularly badly in terms of social diversity, then? Several studies indicate that this is the case, regardless of whether we look at gender, race or socioeconomic background. These studies mostly focus on the USA, although this arguably does not give an overly biased view of the whole discipline, given the dominance of the USA in economics (see Section 4.4). Starting with the socioeconomic background, in the USA, ‘economics PhD recipients are substantially more likely to have highly educated parents, and less likely to have parents without a college degree, than PhD recipients in other disciplines’ (Schultz and Stansbury, 2022, p. 1). Among the USA-born PhD recipients, economists are more than twice as

likely to have a BA from an elite ‘Ivy Plus’ university, compared to USA-born PhD recipients across all disciplines (Schultz and Stansbury, 2022, p. 4). In economics, the proportion of PhDs awarded to racial and ethnic minorities is lower than in STEM fields, let alone in other social sciences (Bayer and Rouse, 2016). The proportion of women among PhD recipients has stagnated in economics in recent decades while it has grown more equal in many other disciplines, and it remains among the lowest ones across disciplines (Bayer and Rouse, 2016; Schultz and Stansbury, 2022). Moreover, Lundberg and Stearns (2019) found that among the various fields of economics, macroeconomics and finance are the ones in which women are least well represented.

4.4 Concentration of power

A community can only reap the epistemic benefits of diversity if the power to make judgments that shape the direction of the field is not overly concentrated. Regrettably, research shows that economics has relatively steep hierarchies in terms of universities, journals and geographical location.

In any discipline, journal editors and reviewers have a significant role in making the judgments that direct how the discipline develops. Several researchers have expressed worries about the fact that in economics, a small number of journals has remarkable power in deciding the direction in which the discipline develops.

As Heckman and Moktan (2020) have documented, it is important for tenure-seeking economists to publish in the so-called ‘Top 5’ journals, as publication in these journals is the most important predictor of achieving tenure. At the same time, these journals are conservative regarding research approaches. Heckman and Moktan (2020, p. 456) claim that ‘Truly innovative papers often do not survive the gauntlet of mainstream refereeing and editing that feature “normal science” and not “novel science”’. Moreover, Heckman and Moktan (2020, p. 460) worry that the reliance on publication in Top 5 journals leads to a ‘concentration of power in the hands of a few editors and leaves the discipline vulnerable to potential bias and corruption’.

The steepness of the hierarchy of journals is exacerbated by the fact that the elite journals are controlled by a small number of economics departments. Furthermore, these departments are located in only a few places within one country. Angus *et al.* find that 63% of the editors of 49 highly ranked economics journals were located in the USA, and ‘any one of the three states of California, Massachusetts and Illinois has more power than the four continents of Asia, South America, Africa and Australasia combined’ (2021, p. 257). Similarly, Ductor and Visser (2023, p. 201) find that ‘Sixty-five percent of all editorial board members of the Top 5 are located in the United States’, and ‘Nearly 30% of all editorial board members of the Top 5 are employed at six universities in the United States’. In an earlier study using 1995 data, Hodgson and Rothman (1999) found that of the editors of Top 15 (by impact factor) economics journals, 81.9% were affiliated with a US university and only 9.6% with a university outside the USA and UK. Similarly to Heckman and Moktan, Hodgson and Rothman (1999, pp. F180–1) also worried ‘that the dominance of the profession by a few leading institutions is likely to reduce the diversity in approaches and beliefs’. Furthermore, against the backdrop of the disillusionment by economists after 2008 about the purportedly rigorous standards that had disqualified all but DSGE macroeconomics, the following insight by Hodgson and Rothman is noteworthy. Already in 1999, they were

concerned that the concentration of power might lead to a ‘lock-in’ in the criteria of evaluation of economic research (Hodgson and Rothman, 1999, p. F182).

While economics is not the only discipline having diversity problems in editorial boards (Espin *et al.*, 2017), comparisons across disciplines do suggest that there is something peculiar in the degree of concentration of power and influence in economics (Wright, 2023). For example, Fourcade *et al.* (2015, p. 100) compare the leadership of the American Economic Association to similar organisations in political science and sociology. They find that ‘72 percent of the AEA non-appointed council members are from the top five departments, in contrast with only 12 and 20 percent respectively for APSA and ASA’. Concerning the geography of publications in various disciplines’ top journals, Gibson (2021, p. 467) notes that ‘Three U.S. ZIP codes are associated with over 40 percent of articles in the top five economics journals and those articles garnered one-half of all citations in these journals from 2000 to 2015’. He compares this result to the disciplines of sociology, psychology, marketing, philosophy and chemistry, where the respective numbers ranged from 5% to 15% (for author affiliations) and 6% to 23% (for citations).

5. Conclusion

In this paper, I have argued that macroeconomics as an epistemic community fails to satisfy the conditions of effective criticism outlined by Longino. The institutions of macroeconomics reduce diversity and therefore weaken the possibilities of effective criticism. Furthermore, these diversity-reducing tendencies may partly explain why the dominance of the DSGE approach went unquestioned for so long. While the need for criticism must always be balanced by some measure of agreement and consensus, the community of macroeconomists seems to have overly emphasised the latter.

I divided the post-2008 criticisms of macroeconomics into two interrelated categories. Some are directed at the dominant DSGE models, while others target the standards in light of which macroeconomists came to appreciate those models more than others. While these criticisms have raised some discussion, the institutional issues highlighted in this paper have not received sufficient attention. Fixing macroeconomics requires not only theoretical work but also enhancing the capacity of the macroeconomics community for self-reflection and self-correction. The consensus–criticism balance should be re-adjusted by reforming academic institutions. In particular, more weight needs to be given for actively cultivating the diversity of views present in the macroeconomics community, for providing forums and incentives to question established methodological tenets, for reducing the concentration of power over research evaluation and for attending to the voices critical of established views.

While this paper has focussed on the state of macroeconomics, it is possible that other fields of economics suffer from similar problems. Indeed, the concentration of power and relatively low support for diversity highlighted in Section 4 are institutional features of the economics discipline in general. Further research should examine how well other fields of economics satisfy the remaining two conditions of effective criticism—the incentives and forums for criticism and responsiveness to it. Examining and improving the institutions of economics is made even more pressing by the unprecedented environmental threats and considerable societal problems that humanity is facing. The ability of economics to provide useful advice and understanding depends on this.

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