

Freedom of Research in Economics

A Goal to Strive for?

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1. Introduction

In the following essay, I will investigate whether freedom of scientific research (FSR) can be justifiably constrained. I will focus on the issue of FSR in economics. The common narrative is that the field is dominated by the scientific paradigm endorsed by orthodox economists, who marginalize academically those who rely on different approaches, namely heterodox economists (Romer, 2016; Roos, 2016).

In this essay, I will refer to orthodox economists as those representing the dominant school of thought in economics, which is constituted mainly by neoclassical economists (Colander et al., 2004; Dequech, 2007). Neoclassical economists are, in turn, characterised by shared theoretical and methodological features. The shared theoretical characteristics that feature in the work of most orthodox economists are at least the following three. Firstly, orthodox economists ground their economic models on the assumption that individuals are rational. I herein identify individual rationality with expected utility maximisation¹.

Secondly, they usually emphasise that the economy will tend to the equilibrium (at least in the long run). Thirdly, they tend to neglect the fact that people, in real life, might be affected by severe uncertainty, i.e. uncertainty that cannot be treated probabilistically (Dequech, 2007; Walker et al., 2013). Moreover, those economists usually share an emphasis on both mathematical formalism (i.e. they base their economic analysis on mathematical models) and axiomatism (i.e. they deductively derive their models from a set of axioms) as rigorous tools of investigation. I propose, however, to characterise orthodox economists through their shared theoretical and ideological assumptions (i.e. assumption of individual rationality, tendency of the economy to reach an equilibrium in the long run, lack of severe uncertainty), rather than focusing on their common methodological framework. The reason for this is that the three ideological assumptions mentioned above characterise orthodox economists specifically. In contrast, methodological assumptions such as mathematical formalism, while shared by most orthodox economists, are not exclusive to them. This is in line with the labelling proposed by some prominent

¹ An individual is an expected utility maximiser when she has preferences that are complete and transitive (Sen, 1973).

economists themselves such as Colander et al. (2004), Dequech (2007) and Hodgson (1999))².

Defining heterodox economists is complex, and I do not here pursue an unequivocal characterisation. Indeed, the label of heterodox economists applies to a variety of economic schools of thoughts, such as the Austrian school of economics, ecological economics, and institutional economics. In this case as well, I will draw on the definition proposed by Colander et al. (2004) and Dequech (2007). Under their reading, heterodox economists are those economists who do not share the ideological assumptions behind the orthodox narrative. This means that they usually reject the characterization of individuals as rational agents, do not believe that the economy inherently tends to the equilibrium, and admit that individuals can suffer from deep uncertainty. Some of them, such as Austrian economists, also reject the use of heavy mathematical formalism as the correct tool to investigate economic issues. This latter methodological characteristic is nevertheless not a common feature and, as mentioned above, will not be taken here as a defining characteristic. Note, once again, that what distinguishes orthodox from heterodox economists under the definition I have put forth is the theoretical and ideological assumptions that the two groups share, rather than methodological ones.

Many economists and philosophers have argued that heterodox economists do not have the possibility to express their voice in well-respected academic environments (Roos, 2016; Romer, 2016). Such an argument is usually advocated to justify two types of interventions, namely the adoption of a code of ethics and the imposition of external control over the current practice in economics, to render the discipline more pluralistic and foster knowledge in the field (DeMartino, 2005; Dow, 2016). In what follows, I will concentrate exclusively on these two interventions. I will argue that they do not foster knowledge in economics. Indeed, while academic economic practice (and its epistemological status) has many flaws, actively intervening on economists' FSR in the two specific ways mentioned above is the wrong solution to the problem.

The essay proceeds as follows. In Section 2, I will present the epistemological argument in favour of FSR. I will use this argument to show that, if certain background assumptions are in place, FSR is a proper condition to foster knowledge. In Section 3, I will consider the critiques of those who believe that conditions for the epistemological argument are not present, justifying the need for interventions. In Section 4, I will refute the objections outlined in the previous section. I will argue that, while it may be true that the conditions needed for the epistemological argument to work

² The reason for using this definition, rather than others, is that it expresses the perspective of heterodox economists on what criteria distinguish them from orthodox economists. Indeed, economists such as Colander, Dequech and Hodgson are part of the heterodoxy. Moreover, it should be noticed that, even if this definition is not uncontroversially accepted, it has the virtue of enjoying a certain agreement in the literature, which renders it a valuable starting point for the discussion in this paper.

are not fully present, there is no reason to believe that intervening on economists' FSR in the way proposed in Section 3 will enhance or restore them and, consequently, better promote the attainment of knowledge. In Section 5, I will outline my conclusion.

2. Freedom of scientific research

There are different arguments in favour of FSR. In this essay, I will consider one of them, which I will label as the "epistemological argument". The idea behind this is that FSR should be defended because it fosters the attainment of knowledge in a field of research. The argument has been devised in diverse forms by various philosophers. In the following, I will concentrate on the version proposed by Mill, which resembles most closely the rationale offered for FSR today (Wilholt, 2010). The upshot behind it is that limiting the topics and types of research that can be pursued is detrimental for the attainment of knowledge. The reason for this is that nobody can confidently discard as unfruitful a certain type of research by relying on the scientific standards adopted at the point in time in which the evaluation is made. It cannot be excluded with certainty that seemingly infertile approaches could prove ground-breaking in the long run. As a result, scientists should have complete freedom to choose their preferred approach. Such variety of research approaches will promote the attainment of knowledge. Indeed, at least some of those approaches, unpredictably, will be successful. This argument relies on two assumptions, which, in turn, hold if some prior premises are in place (Wilholt, 2010).

Assumption 1: FSR fosters a variety of research approaches.

Prior premise 1.1: The incentive structure of the research environment is adequate.

Prior premise 1.2: Researchers have accurate and updated information on the work of the relevant research community.

Assumption 2: A variety of approaches leads to genuine knowledge more often than to error.

Prior premise 2.1: Researchers are sufficiently independent from each other.

In what follows, I will dig deeper into these two assumptions and their prior premises. Subsequently, I will turn to how the epistemological argument itself, and its underlying assumptions, apply to economics.

I will start with **Assumption 1**, namely that FSR promotes a variety of approaches. Why is FSR better than other means at promoting a diversity of approaches? A compelling answer may be the following. Scientists normally want to be credited for a scientific discovery. Indeed, making a scientific discovery implies gaining a good reputation in one's own research environment. In order to be credited for a discovery, however, a scientist must be among the first ones to make it. Thus, the argument goes, if a certain approach has been already adopted by a large number of scientists, then an individual would have a strong incentive to pursue a different and novel scientific approach. By doing so, she would become more likely to make a new contribution to her

research field, which will give her recognition. But the option of pursuing such an approach is available only if scientists are left free to choose the research agenda they prefer (i.e. if there is FSR) (Wilholt, 2010). Note that **Assumption 1** holds only if two prior premises are in place. Firstly (**Prior Premise 1.1**), the research environment should be designed in a way that motivates researchers to come up with novel solutions by crediting them adequately. This is because researchers are really free to pursue their own research approaches only in an environment that presents an appropriate system of incentives. To see why this is the case, think, for instance, of a research environment in which pursuing one specific line of research is incredibly more profitable than all the others (i.e. there are more funding bodies that support that particular line of research, it is easier to get tenure, or it is easier to publish in well-respected journals). In such a case, those initially interested in a different strand of research might have a strong incentive to switch to the “most popular” one. Such an incentive structure might discourage the researcher from pursuing her/his willingness to pursue his own research, if this is different far away from the most “respected one”. This, in turn, might result in an implicit restriction on researchers’ freedom to pursue the research they prefer. Secondly (**Prior Premise 1.2**), researchers should have accurate and constantly updated information on the work carried out by the rest of the community. This happens only if there is free interaction and easily available information within and between fields (Wilholt, 2010).

Let me now turn to **Assumption 2** of the epistemological argument, namely that such diversity of approaches fosters the attainment of knowledge. Why does heterogeneity of approaches promote knowledge better than homogeneity? The answer is the following. Having various approaches means that there may be cases in which the success of one approach threatens the flourishing of another. In those cases, scientists who adopt a certain approach may have strong incentives to check the work of scientists using a different one. If they find flaws in it, their contrasting approach can gain credibility. But here also, **Assumption 2** holds only if a further prior premise is in place: that researchers are sufficiently independent from each other. Indeed, the peer-review system can be efficient exclusively when the relevant community of researchers does not share a systematically flawed background presumption. If this is the case, mutual criticisms cannot help in detecting this kind of mistaken assumptions (Wilholt, 2010).

The epistemological argument for FSR, specified in the form I have presented above, has been applied to economics. Many academic economists such as McCloskey (1990) and Boettke & Donnell (2013) conceive of the academic environment in economics exactly as a marketplace for ideas where perfect competition predominates. This means that there is free entry and perfect knowledge among those who are in the marketplace (Strassmann, 1993). Ideas and theories are exchanged, and the best economic theories thrive spontaneously. This is because the ideas produced by one economist are constantly subject to review by her peers.

Thus, the system of "awards and punishments" of economists is based exclusively on their merit. In this way, every possible agreement (i.e. the "attained knowledge" in the field) between economists is only the result of rigorous investigation of their subject matter. In a perfect market, the most efficient outcome is obtained if the market is left free to operate. Similarly, the optimal way to attain knowledge in economics is to let economists free to pursue their own research (Boettke & Donnell, 2013). Hence, the idea is that the conditions needed for the epistemological argument to work are present in economics' academic environment. Therefore, FSR in economics, in its current form, is the best way to produce knowledge.

3. Scepticisms on freedom of scientific research in economics

In the previous section, I have presented the epistemological argument in favour of FSR. Subsequently, I have shown how the argument applies to the realm of economics, according to some economists such as McCloskey, Boettke, and Donnell. However, this position is not uncontroversial. Many philosophers and economists (Garnett, 2011; Romer, 2016; Roos, 2016) believe that there is nothing more inaccurate than depicting the academic environment in economics as a free market where ideas and ideologies are exchanged and the "best one" wins. Or, others argue³, supporters of the

epistemological argument and supporters of free market make the same mistake. They both show an ungrounded optimism towards the optimality of free exchange. In this section, I will present the rationale behind the idea of those who believe that the conditions needed for the epistemological argument to hold do not apply in economics. Subsequently, I will consider their two main proposals for actively intervening in the field to restore them.

According to the "detractors" of the current research practice in economics, none of the background conditions needed for the two assumptions in support of the epistemological argument are in place in economics. As a result, they should be restored forcefully by actively intervening on economists' FSR⁴. Let me analyse the two conditions in turn.

I will start with the premise that FSR leads to a variety of approaches (**Assumption 1**) and its two underlying background conditions (**Prior Premise 1.1** and **Prior Premise 1.2**). According to many (Romer, 2016; Roos, 2016; Strassmann, 1993), there is neither an adequate incentive structure that fosters novel ideas in the field (**Prior Premise 1.1**) nor sufficient communication between economists (**Prior Premise 1.2**). Rather than promoting the flourishing of different research approaches, the academic environment of economics suffers from being stationary and characterised by

³ See, for instance, Wilholt (2010)

⁴ It is important to note that, whenever I refer to intervention on FSR, I mean practices that actively impose a form of control/check on the economists' freedom to pursue the research she prefers and to discard from her research practice those brands of research that she deems unfertile.

evident homogeneity. As an example, reviewers in top economic journals are usually orthodox economists, who represent the biggest and most prestigious group of economists (Roos, 2016). This is not unproblematic. Indeed, as Strassmann (1993) notes, the appeal and persuasiveness of an argument hinges partly on its affinity (in terms of methods and style used) to the judging audience. This means that journal reviewers will be more likely to appreciate articles written by colleagues who are in the same methodological ballpark. Moreover, as Roos (2016) notes, reviewers may have strong incentives to use rules of thumbs (such as discriminating authors that do not teach in top-universities) for peer-reviewing rather than doing a high-quality work. The reason for it is that editors in journals are economists themselves. Thus, peer-reviewing subtracts time and effort from their own research. This is coupled with low rewards for good peer-reviewing. Indeed, reviewers do not receive a monetary reward based on the quality of their review nor on the accuracy of the reviewing process⁵. The two considerations made above highlight that articles written by orthodox economists who teach in good universities are more likely to be published. As a matter of fact, they are both aligned to the methodological standards used by reviewers and they require less work from the reviewers. As

an example, Roos (2016, p. 10) points out that the top five economics departments account for 28.7% of the papers published in the Journal of Political Economy and 37.5% in the Quarterly Journal of Economics. Note that the number and quality of publications influences, in turn, the likelihood of teaching at elite universities. The institution to which a certain researcher belongs influences, in turn, the likelihood of publishing in respected journals. Therefore, the system of incentives open to researchers seems to have the structure of a vicious cycle rather than the one of a market with free entry. To have a voice in the economics' field, a researcher should aim to become a well-respected economist, who teaches in an elite university. Publishing in well-respected journals is a powerful way to ease such endeavour. However, those prestigious journals are likely to accept primarily papers by top economists, who teach in elite universities.

The second background condition (**Prior Premise 1.2**) needed for the assumption of FSR to lead to a variety of approaches (**Assumption 1**), namely that there is sufficient interaction both within the field and with other fields, does not hold either. Interaction within the field and between sciences seems severely limited when it comes to economics. The reason is that the most copious group of economists, orthodox economists, is usually either uninterested or highly

⁵ There might be two possible counterarguments to this statement. The first one is that reviewers might be intrinsically motivated to produce a high-quality review. While this might well be the case, it should be noticed that, according to a model developed by Thurner & Hanel (2010), it is sufficient that one third of the reviewers produces poor reviews that the overall review process is compromised. A second counterargument might be that even reviewers who are not intrinsically motivated might have a strong incentive to produce high-quality reviews. Indeed, if they produced systematically poor reviews, they would be expelled from the referees board. This might be the case. However, the problem can be avoided by implementing the lowest possible effort (i.e. using the simple rule of thumb mentioned above).

sceptical of what different groups of economic researchers (namely, heterodox economists) work on. Furthermore, orthodox economists seem equally uninterested in what other social scientists, and other scientists in general, are working on. The field seems to be suffering from a lack of interdisciplinarity.

Let me turn to the main objection to the **Assumption 2**, namely that a variety of approaches fosters knowledge. Here also, many believe that the relevant **Prior premise 2.1** (sufficient independence among economists) is not in place. The majority of economists consists of orthodox economists. While there is some heterogeneity of approaches among them⁶, they share the same background assumptions, and they use the same methodology (i.e. formal approaches). Economists do not seem to be independent, but biased towards only one approach.

According to such line of argument, the situation in the academic environment of Economics is so bleak that there should be active interventions. If the assumptions for the epistemological argument are not in place, then the epistemological argument does not hold. This means, in turn, that the present situation does not foster the attainment of knowledge. In order to advance the epistemological status of economics, such conditions should be restored forcefully. I will consider two main proposals for doing so. The first one is the adoption of a code of ethics. This proposal was

endorsed on April 2018⁷, through the adoption of a code of conduct by the American Economic Association. The idea is to provide economists with a unified code of scientific practice, which encourages intellectual and professional integrity. In the document, the definition of professional and intellectual integrity encompasses ideas such as honesty, transparency in conducting research and a disinterested assessment of other economists' ideas. The hunch is that, by making economists abide by the professional practices recommended by the code, it is possible to steer their professional behaviour towards one that is more respectful of others' ideas. This would result in an attitude that is more open to engage in a serious debate with economists who belong to different schools of thought. The augmented openness to divergent ideas in the economic field would imply, in turn, an increase in the knowledge attained. The second proposal for intervention consists of regulating the field through external checks. These may be controls done by politicians or economic experts. Such checks would advance economic knowledge by actively allowing free exchange of ideas.

4. A reply to the sceptics

In the previous section, I have presented the position of those who believe that the epistemological argument for FSR does not apply to the current practice in economics. In this section, I will attempt to refute this argument. Firstly, I will

⁶ See Roos (2016, p. 8).

⁷ For more information, see <https://www.aeaweb.org/about-aea/code-of-conduct>.

argue that while the problems mentioned in Section 3 are real, they have been exaggerated. Secondly, I will point out that, while it is theoretically desirable to solve such (deflated) problems, the proposals made in Section 3 are ineffective for promoting knowledge.

There seem to be serious reasons to worry about the epistemological status of economics. It seems plagued by the overriding dominance of orthodox economists, who turn a blind eye to the promising fields of research that surround them. This argument, however, relies on a caricature of the practice of orthodox economists. The latter are charged on two points. First, they share the same methodology and marginalize with ungrounded scepticism those who pursue different methods and lines of research. Second, they are not open to confrontation with other social scientists. While these charges might apply to some orthodox economists, I will attempt to show that they do not characterise the whole orthodoxy. Then, I will turn to the analysis of how (if at all) active interventions are effective to enable the epistemological argument to work.

There are valuable counterexamples to the argument that dissenting positions are marginalized in economics. I will mention two. The first is constituted by the rise of behavioural economics. Indeed, the subject started as a heterodox approach to economics and is now part of the mainstream (Garnett, 2011). The second is represented by two meetings that were held at the Santa Fe Institute in 1988 and 1997. In those two meetings, orthodox economists such as the Nobel laureate Kenneth Arrow discussed with

prominent natural scientists a set of ideas and approaches that would later be known as complexity economics, a current approach in the heterodox school. Especially during the second meeting, major orthodox economists engaged seriously with the proposals underlying complexity economics and discussed openly the problems associated with the orthodox account (Colander et al., 2004).

It could be argued that such cases constitute the exception to the rule. Indeed, behavioural economics has thrived thanks to the work of scholars such as Thaler and Kahneman, who adopted methodological tools (i.e. mathematical formalisation) similar to the ones used in the standard practice in economics. By their own admission, they did everything in their power to gain attention from orthodox economists (Garnett, 2011). Similarly, someone might argue that orthodox economists who took part to the Santa Fe meetings cannot be taken as a representative sample, since the very act of participating signals a certain degree of openness to different schools of thought. However, these critiques may be too harsh. Indeed, while it is true that behavioural economists share largely the same methodology used by orthodox economists, it is also true that one of their core messages is that expected utility maximisation, which is at the basis of orthodox economic theory, should be rejected as a valid descriptive theory. Therefore, the message they proposed was greatly in contrast with the accepted orthodoxy. In a similar vein, while it is not implausible that the orthodox economists contact with the Santa Fe Institute might not be so common, it is also true that such cases of openness are not unique. As an example,

in the 1970s, in the committee involved in the decision of awarding a tenure at Harvard to Samuel Bowles, a prominent heterodox economist, those who were favourable to his tenure were three economists who were formerly presidents of the American Economic Association and one Nobel Prize winner, stereotypically part of the orthodoxy (Colander et al., 2007)⁸. Moreover, it should be noticed that the conversation between orthodox and some fringes of heterodox economists is made particularly complex by scepticism coming from both sides. As Garnett (2011) points out, some heterodox economists conceive of their field as completely detached from orthodox economics. Hence, no attempt to initiate a constructive discussion with the "opposite camp" is made. Consequently, the gap between orthodox and heterodox economists may be enhanced also by choices of the latter group. Thus, while there is undeniable scepticism among mainstream economists towards methodologies and background assumptions that are different from the ones they usually adopt, conceiving of the field as inaccessible to those who do not follow the precise dictates of the orthodoxy does not seem to be an accurate picture.

A second charge that is usually made to the field is that economists look at other sciences with superiority. There are

at least two relevant counterexamples. The first draws on a well-known figure, George Akerlof. The economist, who was awarded the Nobel prize in 2001, showed a deep interest towards other social sciences such as psychology and anthropology throughout his career. He incorporated some findings of those sciences in his work by explicitly labelling them as "the most convincing explanations" for the phenomenon at hand (efficiency wages, in the case I am referring to) (Akerlof, 2002, p. 371). The second example regards the introduction of RCTs in Economics. RCTs is an experimental method used to test the efficacy of a certain treatment (a drug in medicine, a policy measure in development economics, which was introduced in 2003 by Banerjee and Duflo, two world-leading economists who later founded JPAL (the Abdul Latif Jameel Poverty Action Lab) at MIT (Banerjee & Duflo, 2011). The method was welcomed with overwhelming enthusiasm by many, but also had prominent critics. Among the most notable of them are Deaton, a Nobel Prize winner in Economics, and Cartwright, a renewed philosopher of science, who co-authored a significant amount of papers on the issue⁹. The intellectual bond between the two fosters the hypothesis that there are some seeds of cooperation between economists and other scientists. Note that I am not arguing that economics is

⁸ A further objection might be that the heterodox economists that I mention, contrary to other strands of heterodox economics, embraced methods used by the orthodoxy. If methods of the orthodoxy stand for "mathematical formalism", then this critique fails to hit the mark. I have indeed contrasted heterodox and orthodox economists on the basis of the ideology they share, rather than the methodology they use. If, on the other hand, methods of the orthodoxy mean "expected utility maximization", the criticism does not apply either. Expected utility maximization, in the way I am using it in this paper, simply implies that an individual has preferences that are transitive and complete, which is not entailed by behavioral economics.

⁹ See, for instance, Deaton & Cartwright (2018).

deeply interdisciplinary. Rather, I am making the minor point that the situation is less bleak than depicted in Section 3. While the problems that were highlighted in that Section are real, they may be less pervasive and serious than believed.

After having deflated the criticisms of current economic practice, let me turn to the analysis of the two proposals mentioned in Section 3, namely the adoption of a code of ethics or the imposition of external checks on economists. In the remaining, I will assume, in line with those who believe that the background conditions for the epistemological argument to work should be guaranteed, that such conditions are not fully in place. Moreover, I will also assume that restoring those conditions is theoretically desirable for prompting the epistemological advancement of the field. However, I will argue that the two proposals considered in Section 3 are ineffective for this.

I have outlined that the situation in economic academia might be problematic, but not hopelessly so. Proposed solutions such as those in Section 3 should be carefully evaluated against the present situation. Such solutions would be justifiable only if they represent an epistemological advancement over the present situation. Let me analyse them in turn. I will start with the "external check" proposal. Would some kind of external control better promote the attainment of knowledge than the present situation? I think not. The main problem with this solution concerns the possible candidates for retaining external control on economists. They would probably be either economists themselves or politicians. Both solutions are problematic. The first would

perhaps not worsen the present situation but would represent no epistemological improvement. Indeed, economists are already evaluating the merit of the research made by their peers by holding the position of reviewers in economic journals or belonging to university committees that decide whom to hire. The second solution (politicians exerting the necessary control) might only worsen the present situation. First, politicians would not be competent enough. They may not have a formal training in economics that allows them to independently evaluate the different proposals put forth by economists. This would render them easy to manipulate. Second, even if they were competent, they would likely promote research lines that are aligned with their political positions. This would result in economic knowledge that is biased towards a certain political stance. One could argue that the knowledge produced by economists is already biased towards only a certain conception of "the good" or a certain methodology. Thus, there would be no difference. As I have attempted to show in the previous paragraphs, however, the economic field is not so obstinately closed to divergent approaches as one might think. Hence, the "external control" solution seems at best ineffective and at worst detrimental to foster knowledge.

A second solution is the adoption of a code of ethics. As Boettke & Donnell (2013) argue, such a solution would be ineffective for at least two reasons. Firstly, it would target the wrong area. It would target the ethical behaviour of individual economists. Instead, the efficient target would be the overall incentive structure, which is made mainly by the institutions and funding available. What should be reformed

in order to restore the conditions needed for the epistemological argument to apply is the incentives that economists have to behave in a certain way. As an example, the practice of devoting too little attention to the quality of peer-reviewing is fostered by the absence of rewards for doing the opposite. It is this incentive structure, rather than the "ethical" beliefs of the economist, that should be revised. Secondly, and related to this latter issue, there is the fact that a code of ethics is effective only when it is strongly endorsed by those who adopt it. This means that the code would be either irrelevant or ineffective. Either it is irrelevant since the one who adopts it would have done it also in the absence of it (i.e. she strongly believes in the ethical principles behind the code). Or it is ineffective, because the code is not binding and a non-ethically-motivated economist has no incentive to comply with it. The idea behind a code of ethics is the one of a change in the incentives that economists have. However, promoting plurality in the field requires a change in incentives that is different from the ethical beliefs of the individual economist. Hence, this solution would be ineffective to advance the attainment of knowledge.

The academic environment of economics seems to suffer from being an imperfect market of ideas. However, as with imperfectly-operating markets, interventions are needed only if they are effective at addressing these imperfections. In the same way, intervening on economists' FSR would be justifiable for fostering knowledge only if it improved the background conditions needed for the epistemological argument to work. The two proposals that I have considered do not seem to do so.

5. Conclusion

I have argued that the two proposals to intervene on economics' FSR to improve the attainment of knowledge, namely the adoption of a code of ethics and the imposition of external controls, are ineffective. To do so, I have first presented the epistemological argument for FSR and how it applies to the field of economics. Subsequently, I have turned to the common critique that the academic environment in economics does not present the conditions needed for the epistemological argument to work. According to that view, there should be active interventions to restore such conditions. I have argued that this objection fails to hit the mark. To do so, I have first outlined that the charge that orthodox economists, as a group, marginalise heterodox ones is often exaggerated. Thus, while such accusation may well apply to some cases, the seriousness of the problem is less prominent than usually thought. Then, I have argued that, while it would be theoretically desirable to restore the conditions needed for the epistemological argument to be fully in place, the two interventions that have been proposed are unsatisfactory.

Acknowledgements

I would like to thank Constanze Binder for precious feedback on an earlier draft of this essay and nomination thereof to the editors of ESJP. By doing so, she provided me with the opportunity to engage with an incredibly challenging and, at the same time, funny activity. Moreover, I wish to express my gratitude to Casper Keizer. The quality, level of detail and thoughtfulness of his comments on a previous version of this essay have not suffered from my last-minute

attitude to deadlines, which he handled with outstanding patience. Lastly, I am deeply indebted to some anonymous ESJP referees. Their feedback on a number of previous versions of this essay, which has always been extremely prompt, constructive and mindful, has contributed greatly to its improvement.

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