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The Complexity of Economies and Pluralism in Economics*

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Abstract

From the two premises that (1) economies are complex systems and (2) the accumulation of knowledge about reality is desirable, I derive the conclusion that pluralism with regard to economic research programs is a more viable position to hold than monism. To substantiate this claim an epistemological framework of how scholars study their objects of inquiry and relate their models to reality is discussed. Furthermore, it is argued that given the current institutions of our scientific system, economics self-organizes towards a state of scientific unity. Since such a state is epistemologically inferior to a state of plurality, critical intervention is desirable.

JEL Codes: A1, A2, B4

1. Introduction

This paper contributes to the ongoing discussion about pluralism in economics from a complexity perspective. It starts from two premises: (1) economies are complex systems and (2) the accumulation of knowledge about reality is desirable. These two premises imply, I will argue, that in the current state of affairs pluralism is a reasonable attitude to hold. Pluralism demands a plurality of economic research programs. Yet, the current set of academic institutions in

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economics, such as the rules determining access to top-ranked outlays and the established way of teaching economics, make the academic system self-organize towards a state with one dominant research program. Pluralism, thus, entails the need for critical intervention in the academic system of economics.

As a first step, I will briefly clarify the terminology for the discussion ahead and make some clarifications. I begin with the concept of pluralism as such. Pluralism must be distinguished from *plurality* (Mäki 1997). In this paper the definition of Mäki will be used: Plurality refers to a state of affairs in which different instances of a certain item co-exist. *Pluralism* is seen as a theory or a principle that "justifies or legitimizes or prescribes the plurality of items of some sort" (*ibid.*, 38). This paper seeks to contribute to the question of whether pluralism *with respect to scientific research programs* in economics is a more reasonable principle than monism.

The term *research program* refers to a set of core assumptions on how theories should be built and related to reality, and the group of scientists that endorse this set of core assumptions. The latter group is also referred to as the *members of a research program*. This is similar to the original concept of research programs by Lakatos (1970), who postulates that research programs consist of a "hard core" of basic assumptions that remain untouched, whereas research tries to falsify / address the "protective belt" around the core. But here the hard core not only includes concrete hypotheses and axioms, but also conventions about what perspective to take on economic problems, about how models should be built, and how they should be related to reality. Thus, the hard core includes also an agreed upon summary of what Schumpeter (2006 [1954]) has called the *pre-analytic Vision* of researchers.

For example, the hard core of the neoclassical research program includes the prescription to explain empirical phenomena by models that contain optimizing agents and feature an equilibrium resembling the empirical observation to be explained. Thus, the hard core of neoclassical economics does not consist exclusively of a set of technical assumptions that are typically made. The hard core also refers to the convictions and visions of how models should be built and related to reality.[1]

Since pluralism is a normative concept it requires reference to underlying values. In other words, to be convincing, advocates of pluralism must be able to provide compelling reasons for a plurality of research programs in economics research. For the sake of brevity and clarity, this paper will be concerned mainly with ontological and epistemological justifications in favor of a plural-

[1] For the case of economics, the implications have been nicely summarized by Sugden: "There is a standing presumption in economics that, if an empirical statement is deduced from standard assumptions such as expected utility maximization and market-clearing, then that statement is reliable" (2000, 122). I will expound on research programs in section 3.

ity of research programs in economics. Thereby, it necessarily excludes a number of interesting and important justifications of pluralism from its analysis,[2] yet this focus allows for a more concrete elaboration on the implications of pluralism and the mechanisms through which a productive plurality of research program can (and cannot) be obtained.

Before moving to the main argument, two further clarifications regarding the scope of the paper are necessary: the first concerns the *kind* of plurality to which pluralism refers, and the second concerns the *extent* of plurality that pluralism implies.

With regard to the *kind of plurality*, if pluralism advocates the plurality of research programs, it must go beyond simple demands for the existence of more than one research program. Otherwise, pluralism would be a rather empty concept: the sole existence of several different research programs in economics is a fact (cf. Elsner 1986). Rather, pluralism – as understood here – refers to what Mäki (1997) calls *academic power*. Academic power describes the amount of resources that are given to researchers. Resources do not only include money and employment in good positions, but also status, prestige, the ability to grant or deny others academic power, and the possibility to distribute one's thoughts, e.g. by teaching students. While there are a number of different research programs in economics, academic power is unevenly distributed among them. Thus, pluralists criticize that members of a particular research program get more academic power simply *for the very fact that they are members of a certain research program*: in a state of "perfect plurality," academic power would be evenly distributed among research programs.

Such a state is, however, neither feasible nor desirable. This begs the question on the desired *extent of plurality*. The present paper does not seek to provide a general and decisive rule of how exactly academic power should be distributed among scholars. In fact, such a rule can probably not be derived by an individual scholar but only through open and emancipated debate within the scientific community. Yet this does not invalidate the basic argument: not knowing what the optimal state of affairs looks like does not prevent one from identifying certain states of affairs as unsatisfying, and from highlighting ways to improve them. This argument is analogous to that posited by Sen (2006) in his theory of justice: it is not necessary to have an ideal theory of justice to show that certain deprivations are bad and should be remedied. Likewise, we do not need an ideal theory of pluralism to argue that the current state of affairs in economics can be improved by a change towards more plurality. As in Sen's (2005) theory of capabilities, this path to improvement is necessarily character-

[2] For example, the paper does not consider the plurality of worldviews among scientists, or the justification of plurality via ethical or political reasons. These are important aspects of pluralism in economics that deserve a thorough discussion, which would, however, necessarily go beyond the scope of a single article.

ized by emancipated debate within the economics community, which is hopefully stimulated by this paper.

Now that the basic terminology has been clarified, the main argument of the paper can be summarized as follows:

1. Economies are complex systems.
2. From this it follows that there are multiple ways to represent them, i.e. to reduce their complexity such that they are amenable for investigation. Within different research programs in economics, there is consensus on how complexity reduction and explanation should take place. This consensus is part of the hard core of each research program.
3. Given the academic institutions in economics, the academic power of a research program grows roughly in the number of its members, and the more members it has, the more members it will attract in the future (*positive feedback with regard to membership/power*).[3] This implies that the academic system self-organizes itself towards a state of scientific unity.
4. The state of scientific unity is harmful to scientific progress if the subjects of investigation are complex economic systems. The reason lies in the impossibility to identify the one correct way of reducing their complexity and, thus, to identify the one correct research program.
5. Consequently, pluralism can be justified on the grounds of scientific progress. And since the plurality of research programs is no natural outcome of our current scientific institutions, pluralism entails the need for critical intervention.
6. A state of plurality of research programs is challenging because it is not by itself a productive state. Methods from the complexity sciences can help to address this challenge.

To give more substance to this line of argument the paper will proceed as follows: in section 2, it is clarified why economies should be considered complex systems and what is meant by the "complexity challenge." Section 3 suggests a general framework of how scholars reason about complex economic systems. This framework helps us in specifying the concept of a research program more precisely. Section 4 elaborates on the relationship between different research programs and section 5 derives some implications of the current academic institutions with regard to the framework developed before. In particular, it is argued that given these institutions the academic system self-organizes itself towards a state of unity. Section 6 discusses the epistemological implications of plurality and unity, substantiating the claim that a plurality of research programs is desirable. Finally, section 7 discusses how such a plurality can be achieved and how it can be made productive. Section 8 summarizes the paper.

3 See Merton (1968) on the scientific Matthew effect.

2. The Complexity Challenge in Economics

Most of the modern sciences, such as physics, biology, or the cognitive sciences have accepted what could be termed the *complexity challenge*. They have recognized that their subjects of inquiry are parts of complex systems, and even constitute complex systems themselves. They have reacted to this recognition with new theories and methods. Economics is a notable exception. To judge the adequacy of the exceptional status of economics, the terms "system" and "complex system" require clarification.[4]

I follow the systemist approach of Mario Bunge, which starts from the idea that any object or entity is either "a system or a part of one" (1996, 20).[5] A system thus consists of parts and relations among these parts. What makes a system complex is the kind of its components and relations.

Based on Bunge's systemism and Weavers' (1948) definition of *organized complexity* the following properties may be considered as the essence of a complex system: (i) it consists of potentially heterogeneous and potentially adaptive parts, (ii) the relations among the parts are (at least partly) self-organized and represent their direct interdependence, and (iii) the system features a layered ontology in the sense that because of the nonlinear interactions among the parts, the system possesses some emergent features that its components lack.[6]

Given this list of properties, to say that economies are complex systems seems to be a rather uncontroversial (ontological) claim which is increasingly accepted in economics (see e.g. Beinhocker (2006) for an overview and Ramalingam (2013) for an application to development policy design). The epistemological and methodological implications, however, have not been accepted in large parts of economics, with the research program of complexity economics (Arthur 2010) being an obvious (yet not the only) exception. Particularly the methodological implications of complexity are far-reaching if one considers the reactions in physics or biology. Moreover, this definition of complex systems does not provide a full-fledged ontology, leaving considerable degrees of freedom for how one defines one's ontological approach. Yet, when it comes to the question of plurality of research programs, the definition already carries some significant implications to which we now turn.

4 There are many quantitative definitions of complexity, and the precise interpretation of the concept may vary among disciplines. Here a simple and verbal definition of complexity that captures the essence of the concept is used. For a review of existing definitions, see e.g. Mitchell (2009) or Elsner et al. (2015, ch. 11).

5 Gräbner and Kapeller (2017) argue that Bunge's systemism provides an excellent umbrella framework to align various schools of thought under one philosophical framework and is thus a useful underpinning for a pluralistic economics.

6 This definition is consistent with the elaborations of Arthur et al. (1997) and Rosser (2004). For an explanation of the concept of a "layered ontology" and its evolutionary interpretation, see e.g. Hodgson (2004, particularly 450 – 452).

3. How the Scientific Community Studies Complex Systems

From the complexity of economic systems it immediately follows that to perceive, discuss, and understand them we need to reduce their complexity via representations. There are numerous different ways of doing so (Frigg and Nguyen 2017). These different ways are not easily comparable, and relating them is difficult. In order to increase their productivity in "everyday work," scientists form research programs as introduced above: communities in which members share a particular set of pre-analytic visions (Schumpeter 2006 [1954]), i.e. communities in which members reduce the complexity of their objects of study via conventionally agreed upon representations.

3.1 Epistemological Implications of the Complexity Challenge

Every scientific activity is somehow concerned with the world around us.[7] Scholars are usually interested in understanding this world (although two scientists may differ on what they mean by 'understanding'), and sometimes interested in changing it. Whenever we are concerned with the world around us – be it in science or everyday life – we must work with representations of reality, often called "models:"

> Every person in his private life and in his business life instinctively uses models for decision making. The mental image of the world around you which you carry in your head is a model. One does not have a city or a government or a country in his head. He has only selected concepts, and relationships between them, and uses those to represent the real system. A mental image is a model. All of our decisions are made on the basis of models. […] The question is not to use or ignore models. The question is only a choice among alternatives (Forrester 1971, 112).

Similar claims are made more recently by cognition scientists and psychologists (see e.g. Johnson-Laird (2005) for an overview). Therefore, whenever we direct our scientific reasoning at the world, we produce *representations* of reality.[8] At this level, it does not matter whether we alter reality by our description of it, as social constructivists claim: whenever we communicate information about reality, be it in verbal or written form, we do this in the form

7 I assume there is only one world, the one we live in. This is consistent with the fact that people may have different perceptions of this world, and the fact that part of this world is socially constructed. Yet it is not consistent with the view that there are parallel worlds around us and researchers refer to different realities as such. This will become clearer within the epistemological framework to be developed below.

8 The philosophical literature on the topic is extensive and a survey is beyond the scope of this article. See Frigg and Nguyen (2017) for a survey on the topic of models as representations of reality. It is important to stress that (1) models are not limited to mathematical systems of equations, but also include verbal narratives, fictions, or physical objects; and (2) models are not the only way of representing reality.

of representations. It is a constituent fact of such representations that they are less complex than the real world. They *always* involve abstraction, which is to be considered "a necessary evil – a concession to the finite computing capacity of the scientists" (Archibald in Archibald et al. 1963, 231). If these representations are used within the scientific discourse and if the abstraction is undertaken explicitly, these representations are often called models (the everyday representations we use according to Forrester are often referred to as *mental models*, cf. Johnson-Laird 2005).[9] When most people hear the term "model" they often think of stylized mathematical models. However, this is not necessarily the case: a model can be verbal, algorithmic, equation-based, symbolic, or a mixture of these.

As with all representations, models – whether mental or scientific ones – are less complex than the system they represent. This is also what makes models useful, since this way we are able to reason and communicate about the world, which in its wholeness is too complex for us to perceive (see Robinson 1979). If models share some essential aspects with the real world, then we may learn something about reality through the study of our model. This is nothing else than to say that if the assumptions of our model are adequate, then we can learn through it about the world around us. This process is illustrated in Figure 1. We first reduce the complexity of the real world by building a representation of reality. This process can be thought of as a mapping from the world to the model, and thus be summarized via a complexity reduction function *r*.[10] Then we can study the behavior of our model and identify its driving mechanisms, i.e. pin down function *f*. This is called *model exploration*. Thereafter, we relate our model to reality in the hope that, for example, this helps us to conjecture mechanisms operating in reality, or on how the system under investigation will develop in the future.

Given the complexity of the real world, there are some important aspects of the reasoning process as outlined in Figure 1 that deserve mentioning: (1) there are, particularly for complex systems, many different ways to represent the world around us in a model; thus there are many different "complexity reduction functions" that we can reasonably think of; (2) we do not know of any

9 Again, the philosophical literature on the definition and ontology of models is extensive. See Gelfert (2017) for a survey. This literature, however, discusses the topic on a level of detail far beyond what is needed for our present purpose.

10 For a survey of philosophical treatments of the model-world relationship, see e.g. Frigg and Nguyen (2017). The stylized illustration in Figure 1, inspired by the structuralist conception of representation, is compatible with most modern conceptions of representation, including the popular DEKI account of Frigg and Nguyen (2016). Sometimes it is necessary to revert the direction of the mapping and to use the model as domain and to map the model on a part of the world represented by the model. Such augmentations do not affect the upcoming arguments substantially.

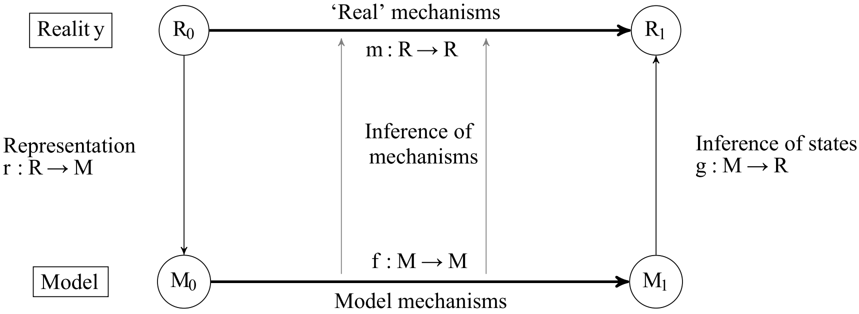

Figure 1: An illustration of how scientists might use models to reason about their object of inquiry.

general ex-ante criterion according to which we could say that the assumptions of our model are "adequate" (although it is easy to dismiss some assumptions as nonsense, e.g. if they are inconsistent or do not carry any information, see e.g. Popper 2002); (3) it is not clear under which conditions we can rightfully say that our model has helped us to "understand" the world around us (although it is sometimes easy to argue that a model did *not* help us in this respect). The reason for this is that there are many different ways according to which one can relate a model to the real world and judge its quality (see e.g. Gräbner 2017 for practical and Bokulich 2017 for philosophical reasons), and which one to choose depends on the purpose of the model or its audience (Mäki 2010); (4) we also lack any *general* criterion of when this reasoning process can be called 'scientific' and when it does not deserve this status. The problem of distinguishing between science and non-science is called the "demarcation problem" and remains an unresolved puzzle of modern theory of science (cf. Pigliucci and Boudry 2013).

In effect, it is difficult for scholars to form a consensus on how the world around us should be represented in scientific discourse, even if we accept the claim that there is only one real world. Two related arguments are particularly important in this context: first, the statement that the pre-analytic visions and *Weltanschauungen* of researchers necessary affect their work, and second, that this fact cannot be overcome by a focus on empirical observations, which are always theory-laden.

The first argument has been nicely summarized by Schumpeter (2006 [1954]) who declares that in economics the "sphere of the strictly provable is limited in that there are always fringe ends of things that are matters of personal experience and impression from which it is practically impossible to drive ideology or for that matter conscious dishonesty, completely" (40). This claim is shared even among scholars who thrive to practice economics as an empirically-oriented discipline and mostly value-free science, such as Max Weber, who wrote that it "is correct that in our discipline the personal worldviews [*Weltanschauungen*] always bias the scientific argument, and affect the judgment of the importance of scientific arguments, even if one tries to identify the simplest causal relationships of facts" (1922, 151, author's translation). These personal worldviews are to some extent also dependent on the social environment of the researcher, which is constituted by other members of her research program (see below).

The second argument relates to this and denies that the role of the necessarily diverse and subjective perspectives could be remedied by a stronger focus on empirical work: that is because even the process of *perceiving* the world (which has to precede its representation) involves implicit assumptions about *how* this world can be perceived, and these assumptions (summarized in Schumpeter's vision) frequently differ among researchers. This claim is not as esoteric as it may sound in the first place. In fact, Albert Einstein already highlighted it long ago: "It is absolutely wrong to build a theory only on observations. Because it is only the prior theory that decides what we can actually observe" (cited in Heisenberg 1986, author's translation).[11] This claim is also at the heart of many modern movements in science, such as the Quantum Bayesianism in physics or Bayesian statistics.

In the end, there is no way to identify the one correct way to represent and study reality, an insight that is – in its formulation as the principle of fallibilism – a central part of Karl Popper's (2002) critical rationalism. This does not mean that we as academics cannot be successful in accumulating new, important, and policy-relevant knowledge about the functioning of economic systems. As for physicists working on quantum mechanics, there can be scientific progress for economists. Yet, this process is not a successive approximation of a "correct theory" or "correct approach," but rather an open-ended cumulative process of building (and discarding) ever-new theories and models of (parts of) the economy, which is itself constantly evolving in an open-ended process (see also section 6.3 below). Keeping in mind the fundamental uncertainty about the "correctness" of our theories is essential whenever one wishes to understand the coexistence of and the relationship between different research programs in economics.

3.2 Research Programs in Economics

The co-existence of several admissible ways to represent reality in a scientific model seems to be an unsatisfying state of affairs for many scholars. As

[11] The German original reads as follows: "Vom prinzipiellen Standpunkt aus ist es ganz falsch, eine Theorie nur auf beobachtbare Größen gründen zu wollen. Erst die Theorie entscheidet darüber, was man beobachten kann!"

scientists who are concerned with solving challenging problems, they do not want to concern themselves all the time with the meta-theoretical foundations of their modeling exercises, with questions of whether they can perceive reality directly or to what extent their observation alters reality, and how exactly their models become meaningful. Therefore, we all as economists make some very fundamental assumptions on our subject of investigation that we simply accept, and whose discussion we leave to other fields, preferably philosophy. Since scientific work nevertheless relies on the cooperation among scholars, scholars form *research programs.*

Part of their hard core are axioms about how science should work, including the process of how one should represent reality in a model, and how the model behavior should be explored.[12] Because discussing fundamental assumptions about the nature of reality, and reintroducing the functioning of one's model over and over again hinders scientists to advance with their everyday-problems, the membership in a research program is attractive and makes scientists more productive in their scientific 'puzzle-solving' (see Kuhn 1962). Within their research program, they can take many assumptions and mechanisms for granted and proceed with (at least to them) more interesting puzzles. This is not a trivial advantage, but a solution to one of the most fundamental challenges of scientific work: to collaborate with each other, scientists must construct shared meanings for certain terms, and develop a common language that helps them to communicate their research. Such shared meanings are easier to construct in smaller groups than in the whole economics community, and there is more effective standardization of thinking and working within the sub-communities.[13]

Let us consider the research program of neoclassical economics as an example: it starts with the shared perception that economics is "the science which studies human behavior as a relationship between ends and scarce means" (Robbins 1932, 15; see also Backhouse and Medema 2009). From this fundamental starting point,[14] neoclassical economists have agreed upon a particular epistemological and methodological toolkit: they represent reality via models, consisting of optimizing agents and are explored by imposing an equilibrium condition on the relationships in the model. Neoclassical economics has not only agreed upon the idea that an empirical fact counts as 'explained' once it has been derived from a model consistent with the so called *optimization-cum-*

12 Thus, the hard core includes aspects of the ontology, epistemology, and methodology of the research program (Dow 1997), but it is not limited to these contents.

13 One may also suppose that the formation of research programs form and reinforce the personal worldviews of researchers, which has been argued to be of importance in section 3.1 above. Although surveys about the attitudes of economists towards certain questions of public policy are consistent with this statement, more research is required to make a definite statement about this relationship.

14 This starting point is not shared among economists in general, see e.g. Jo (2011) who considers economics to be the science of the social provisioning process.

equilibrium approach (Sugden 2000), it has also agreed upon certain quality indicators for its models, e.g. deductive exactness at the expense of descriptive accuracy (Lehtinen and Kuorikoski 2007; Reiss 2011). From this it follows that some methods are more frequently used within the neoclassical research program than others, and it is only natural that these methods are more frequently taught to the students of neoclassical economists.[15]

Thus, it seems useful to say with Figure 1 that members of a research program have agreed upon a particular set of admissible complexity reduction functions and the ways of model exploration (i.e. to pin down function *f*).

4. The Relationship Between Research Programs

This section elaborates on the relationships between different research programs, and on how their relations are shaped by the institutions of our scientific system.

For the level of their theoretical content, Kuhn has coined the pessimistic concept of 'incommensurability' to describe the inability of researchers to think through the lenses of another paradigm. This may describe well the case of scholars who are not explicitly reflecting upon the hard core of their own research program. But once scholars actively start thinking in terms of a pluralist meta-paradigm, as outlined by Kapeller and Dobusch (2012), they should be able to critically assess and appreciate other research programs as well. Kapeller and Dobusch use such an approach to compare different research programs at the level of their theories applied to concrete economic phenomena. Although explanations and policy recommendations are not uniform within research programs and are affected by more fundamental meta-theoretical considerations on the ontological and epistemological level, Kapeller and Dobusch argue that comparing research programs on the applied level is insightful since it focuses on the practical implications of such meta-theoretical considerations. There is, however, no reason of not using their framework in a slightly adjusted form for the comparison of research programs on the meta-theoretical level, even if such a comparison is more difficult (see also section 7.2 below). According to their framework, two research programs can relate to each other in one of the following ways: The theoretical implications could be *identical* (i.e. the conclusion could be virtually the same) or *convergent* (the conclusions go into the same direction); thus there is no conflict between them, but rather the potential for cooperation and immediate cross-fertilization. The theoretical implications of two research programs could also be *compatible* with each other,

15 Even if we consider only mathematical methods, we see that some parts of mathematics are prominent in neoclassical economics, e.g. Lagrangian optimization techniques, but others are less widely accepted, e.g. agent-based simulations.

meaning that they offer different explanations for different aspects of the subject under investigation, resulting in a potential division of labor among them. Sometimes, research programs simply deal with different subjects and are neither easy to compare, nor in serious competition to each other – they are *neutral* and thus co-exist. Yet, research programs could also offer *divergent* or even *contradictory* explanations, in which a scientific controversy with one winning and one defeated side might result.[16]

This classification is useful once one wishes to analyze the relation among research programs with regard to their theoretical content. However, science is always – at least to some extent – part of politics: within the scientific system there is a struggle for power, be it the power over ideas, academic power, and often also political power.[17]

As with any other group in human history, there exists some kind of *in-group solidarity* among members of the same research program, such that they support themselves in getting more academic power compared to members of other research programs. Thus, on a non-scientific level, we often observe a more or less open conflict between various research programs for academic power that takes place within the institutions of the scientific system.[18] Therefore, to understand the dynamics of the relationship between research programs, and to understand when conflict is more likely than cooperation, these institutions must be given consideration. This is the subject of the next section.

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16 The "Cambridge Capital Controversy" (Burmeister 2000) is a nice example for a fundamental controversy of this kind.

17 There are certainly many scientists who try to be as a-political as possible, and who are mainly interested in the accumulation of new knowledge. Nevertheless, even for them it is impossible to prevent their values and *Weltanschauung* of having an effect on their research. Moreover, there are others who explicitly try to practice politics. For Germany, for example, this is evidenced by the strong networks between certain clusters of scientists and political think tanks with very clear political orientations (Pühringer and Hirte 2014; Pühringer 2017).

18 There are, of course, notable exceptions, particularly on the level of individual researchers who are honestly interested in inter-paradigmatic discourse and reasonable debate. This is particularly challenging because the scholars involved have to identify and overcome the many (implicit or explicit) agreements within research programs, such that this process requires openness and self-reflection. It also requires researchers to have the ability of *paradigm switching,* i.e. the ability to understand the perspective and the arguments of the member of the other research program. This is a rare skill in economics that most graduate programs do not teach (see e.g. Beckenbach et al. 2016 for evidence on Germany).

5. The Academic Institutions and a Tendency Towards Unity

This section elaborates on the claim that given our current academic institutions, there is a tendency towards a situation in which one dominant research program accumulates the vast majority of academic power (see Dobusch and Kapeller 2009 for a related argument). To justify this claim, which is in line with what Merton (1968) termed the "Matthew effect of the scientific reward system," the different channels through which the reproduction of research programs takes place and through which they accumulate academic power are discussed. Using a simple model, the conditions implied by these channels are shown to be sufficient for a tendency towards one dominant research program, irrespective of its explanatory superiority.

5.1 Channels for Positive Feedback Mechanisms

The struggle for academic power between different research programs differs from the intellectual struggle of ideas. The latter is concerned with the question of which theory or research program offers the best explanation of a given phenomenon, or which research program provides more useful ideas to the general public. Such arguments sometimes enter the struggle on academic power when they are used to justify the superiority (or the neglect) of a particular research program, yet such arguments are not necessarily decisive in the conflict about academic power. Thus, we shall now study the main channels through which a research program accumulates academic power. It should become clear that these mechanisms lead to positive feedback loops.

1. **Educational programs and material**: the more academic power a research program has accumulated, the stronger is its influence on the teaching material such as textbooks, and on what is considered the core content of the discipline every student should know.[19] More academic power also allows research programs to affect accreditations of study programs.
2. **Education and guidance for students**: aside from the teaching material, members of a research program mostly teach students the material in accordance with the hard core of their research program – a mechanism that has been highlighted already by Schumpeter (2006 [1954], 40ff). Thus, the more members a research program has, the more its members affect young scholars. In graduate training, a PhD supervisor teaches her students not only about content, but also about social rules, such as accepting the hard

[19] For empirical evidence on the bias of most economics textbook in favor of neoclassical economics, see e.g. Lee and Keen (2004). For the bias of study programs, see e.g. Beckenbach et al. (2016).

core of the research program, respecting other members of the research programs, asking some questions but not others, going to the respective conferences, and the like.

3. **Higher productivity in the normal science**: the more members a research program has, the more efficiently can its members solve the daily puzzles of their research program. For example, neoclassical economists usually engage in the TAPAS approach of building new models: *Take A Previous model and Add Something*. This makes communication within the research program highly effective and the members of the research program will proceed faster in solving their scientific puzzles. This in turn increases the academic power of the research program by increasing its prestige (which depends – *inter alia* – on its ability to solve these scientific puzzles).
4. **Editorial boards and outlays**: much of the recognition and influence of scholars depends on their publication record. It is ultimately the editors of book series and journals who grant or deny access to prestigious outlays. Aside from quality, editors also consider their social relation to the authors of the submitted manuscripts. For example, Colussi (2017) has shown that about 43 % of the articles in the top-5 economics journals are published by authors that are connected to at least one editor via joint research projects, through a joint *alma mater*, via a PhD student-teacher relationship, or as a faculty colleague. The editors, in turn, are recruited to 56 % from only six different economics departments in the US (Colussi 2017), all with a similar teaching and research focus on the neoclassical (i.e. *optimization-cum-equilibrium)* research program.[20] In effect, the compatibility of a submitted article with the scope of the outlay, which in turn depends on the convictions of the founders and editors, is highly relevant for publication. In other words, journals are more likely to publish work of those scholars who are members of the same research program as that of the editors and the majority of the readership of the outlay. This is not to say that quality does not play a role for the decision of whether a paper should be published,[21] but to highlight the fact

[20] For further empirical evidence on the role of the social relations of editors for the restrictive access to high-ranked journals see, e.g. Laband and Piette (1994), Brogaard et al. (2014) or D'Ippoliti (2017) and the further references provided therein.

[21] One may also claim that the high concentration of publications and editorships for a few departments only reflect the quality of the latter: the departments are good in attracting good students and faculty, and editors are good in judging the quality of manuscripts (Kim, Morse, and Zingales 2009). This interpretation is, however, not plausible since first, the concentration in economics is by magnitudes larger than in other sciences (Fourcade, Ollion, and Algan 2015; Aistleitner, Kapeller, and Steinerberger 2017). Second, Kapeller and Steinerberger (2016) used an agent-based model to model the publication process of scientists submitting papers to journals, which then publish papers based on referee reports. The model shows that even if one makes overly optimistic assumptions about the exactness and objectivity of the assessment process, selecting publishable papers only on the basis of quality does not work, with many excellent papers remaining

that editors are more likely to appreciate (and publish) research of peers, which tends to belong to their own research program. This mechanism favors research programs that have already accumulated many members.

5. **Rankings**: academic rankings work in a similar way. Journal rankings based on citation data, which are ubiquitously used for the assessment of scholars, are not suited to measure the quality of individual research papers or their authors (see e.g. Moed 2005; Kapeller 2010; Frey and Rost 2010; Kapeller and Steinerberger 2016; D'Ippoliti 2017). Scientometricians, thus, have stressed for long that rankings in general should be seen, if anything, as measures of *impact* and not of *quality* (Martin and Irvine 1983). This point has recently been highlighted in a joint report of the International Mathematical Union (IMU), the International Council of Industrial and Applied Mathematics (ICIAM) and the Institute of Mathematical Statistics (IMS), in which they advised against using rankings based on citation data for judging the quality of a paper and the ability of researchers (Adler, Ewing, and Taylor 2009). This does not mean that publications in high-ranked journals are necessarily of low quality. It rather means that many excellent papers are not necessarily published by highly ranked journals, particularly when they originate from a minor research program. Despite these shortcomings, rankings have important implications for the distribution of academic power in economics and on the reproduction of research programs (Kapeller 2010; Aistleitner *et al.* 2015; D'Ippoliti 2017). The very nature of the way journals accumulate citations – which is a path-dependent process with positive feedback mechanisms (cf. Merton 1968) – stabilizes these rankings (Sterman and Wittenberg 1999; Medoff 2006; Haucap and Muck 2015). Journals that have accumulated significant numbers of citations in the past are most likely to accumulate more citations in the future since the probability of an article to be cited increases with the number of citations it has already accumulated (Price 1976; Newman 2009). In economics, the relevance of this mechanism is evidenced by the fact that (1) the "Top Five" journals dominate the citation landscape in economics and are the by far most important outlets in terms of rankings,[22] and (2) that these top journals are remarkably stable: the set of "Top Five" journals, which is the outcome of citation data analysis, has not

unpublished (see Moed 2005 for a similar model; see Heckman, Akerlof, Deaton, Fudenberg, and Hansen 2017 for a similar argument).

22 For example, studying a sample of all articles published in 675 economics journals between 1956 and 2016, Glötzl and Aigner (2017) find that over the whole period the articles published in the Top Five journals account for 71 per cent of total citations. In 2016, still 66% of the 100 most-cited articles were published in the "Top Five" journals. Although the articles published in these journals make up 2% of all published articles, they account for 22 per cent of all citations (*ibid.*, 10–11). A similar concentration can be found with regard to institutions: five departments account for about 20% of all citations. This share has been relatively stable since the 1980s (*ibid.*, 15). For further empirical evidence see, for example, Hodgson and Rothman (1999), Kocher and Sutter (2001),

changed and includes the *American Economic Review*, *Econometrica*, the *Journal of Political Economy*, the *Quarterly Journal of Economics* and the *Review of Economic Studies* (Pieters and Baumgartner 2002; Card and DellaVigna 2013; Glötzl and Aigner 2017). In effect, the use of citation metrics tends to stabilize the *status quo*, i.e. the dominance of a single research program (Sterman and Wittenberg 1999; Lee and Elsner 2008; Dobusch and Kapeller 2009).[23]

6. **Access to commissions**: scholars who are members of commissions deciding on research grants, the allocation of academic positions (particularly professorships), or the accreditations of research and teaching institutes tend to favor members of their own research program. Note that this has only partly to do with nepotism: members of one research program frequently have difficulties in understanding and appreciating the output of other research programs, even if they try. As argued above, most economists are poorly educated in the foundations of the theory of science, which would facilitate such dialogue. In total, the greater the academic power accumulated by a research program, the more it can dominate commissions and consequently allocate even more resources to its members.

These channels (and there might be more) suggest that a research program has similarities with a technology and features positive feedback loops with regard to the *number* of its members: the more members it has, the more likely it is to further increase its number of members. Similarly, the more academic power it has already accumulated, the easier it is to accumulate even more. The process of power accumulation of economic research programs is, thus, characterized by what Myrdal (1944) termed "circular cumulative causation."

5.2 A Model of Scientific Monopolization

To illustrate how the mechanisms mentioned in the previous section can lead to intellectual unity, a simple model in the spirit of the generalized urn models as introduced by Arthur (1989) and Dosi et al. (1994) to study technological change is used. There are more complex and refined models of this type (e.g. Sterman and Wittenberg 1999), but a simple one suffices to illustrate how the positive feedback mechanisms mentioned in the previous section can lead to the dominance of a single research program.

Medoff (2003; 2006), Dobusch and Kapeller (2009), Card and DellaVigna (2013), or the AEA panel discussion of Heckman, Akerlof, Deaton, Fudenberg and Hansen (2017).

[23] If one considers a wider class of renowned journals one may also find some more recently founded journals. Examples include the AEJ journals of the AEA and some journals on experimental economics. Their success is exceptional and has to do with the huge institutional support given to the AEJ journals, as well as the novelty of the research area of experimental economics.

The model assumes an academic system with n research programs in which once a person has joined a particular research program, she will remain there for the rest of her career. The choice of a newcomer to the scientific system is stochastic, and the probability to choose a particular research program depends positively on the number of members (or of the accumulated academic power) of the research programs. Alternatively, the mechanisms could be interpreted in the sense that the person chooses a mentor, e.g. the PhD supervisor or some academic role model who influences her in her choice of her research program. She then chooses the research program of her mentor.

This process has the general structure of a Polya Urn process (Dosi and Kaniovski 1994). Denoting the share of members of research program i in t by $x_{i,t} \in [0,1]$ and the number of members at $t = 0$ with n_0, the dynamics of the process for $t > 1$ are given by:

$$x_{i,t+1} = x_{i,t} - \frac{x_{i,t} + \varphi(x_i)}{n_0 + t + 1}$$

where $\varphi(x_i)$ is the Bernoulli operator which takes 1 according to a probability function $p(\cdot)$. Here we use a functional form that has been shown to describe well the dynamics of technology adoption:

$$p(x_{i,t}) = \frac{12x_i^2 - 5x_i^3}{\sum_{j=1}^{d} 12x_j^2 - 5x_j^3}$$

where d indicates the number of different research programs. Figure 2 shows this function for the situation with 2, 3, 4 or 5 different research programs. Note that the coefficients in the model are used as shape parameters and affect only the location of the unstable equilibria, but do not affect the model conclusions more fundamentally.[24]

The model can be analyzed as a replicator dynamic, and we can identify the equilibria of the process analytically by solving the model for the equilibrium condition $x_{i,t+1} = x_{i,t} \forall i$. For the simplest case of two research programs this leaves us with three equilibria: $x_1^* = 0, x_2^* = 1$ and $x_3^* = 0.5$. This means the system may settle either to a state of scientific unity, where one research program dominates the system, or a state of plurality where both research programs co-exist. If we consider the stability conditions for the equilibria, however, we find that only the situations of scientific unity are stable, whereas the state of plurality is unstable.[25]

24 The code of the model and a manual to recreate the figures is available on GitHub such that the reader can explore its behavior in more detail: https://graebnerc.github.io/complexity-pluralism/.

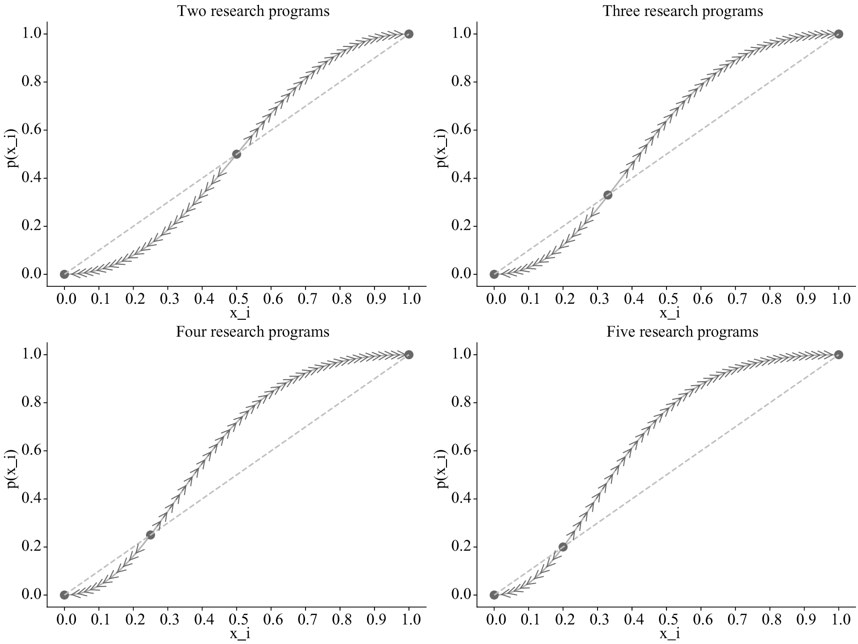

Figure 2: The dynamics of the model for different numbers of research programs. The x-axis refers to the share of members of x_i in t and the y-axis on the expected share of x_i in t+1.

Figure 3 illustrates the dynamics of the process for 2, 3, 4 and 5 different research programs, all starting with an equal share of members. The simulations illustrate that the process is heavily path dependent. This implies that if one research program starts with a majority, it is most likely to dominate the academic system in the future. If all research programs start with equal shares, it is not clear which will become dominant, but the state of plurality is in any case unstable.

This simple model shows that if the assumption of positive feedback effects is adequate – and section 5.1 summarized a number of arguments for why this is the case – the academic system tends towards a situation of scientific unity. This argument does not refer to the particular intentions of individual research-

[25] The stability of the equilibria can be assessed by studying their Eigenvalues. If the dominant Eigenvalue has a modulus smaller than one, the equilibrium is stable. The Eigenvalues in the present example are given by $\lambda = \frac{\partial x_{i,t}}{\partial x_t} = \frac{t+n_0}{t+n_0+1} + \frac{24x_t - 15x_t^2}{t+n_0+1}$ so that we end up with $\lambda_1 = \lambda(0) = \frac{t+n_0}{t+n_0+1} < 1$, $\lambda_2 = \lambda(0.5) = \frac{t+n_0+8.25}{t+n_0+1} > 1$, and $\lambda_3 = \lambda(1) = \frac{t+n_0}{t+n_0+1} < 1$.

ers or the "quality" of the research programs, which, as has been argued above, is often difficult to assess.

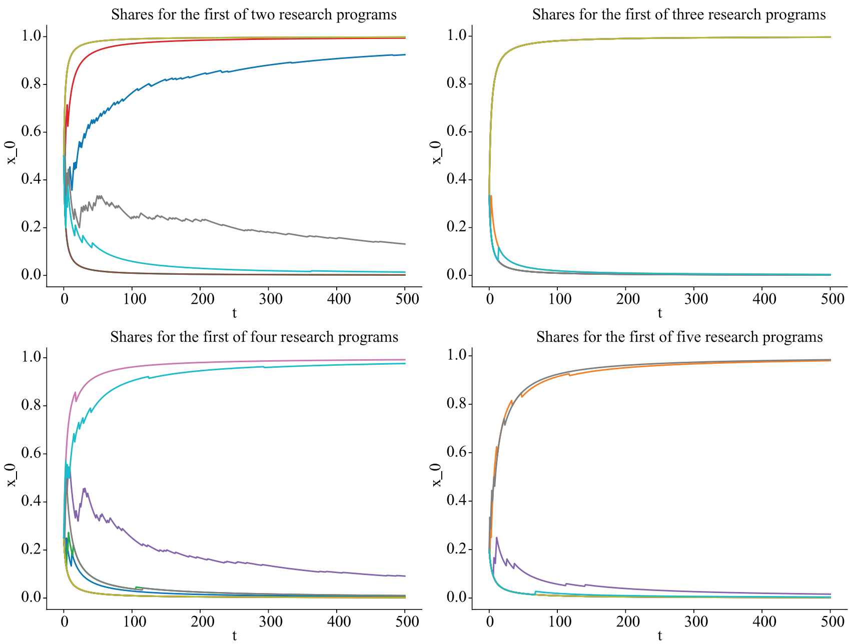

Figure 3: Selected dynamics for the model. In all cases we see that a research program either becomes dominant and takes over large parts of the population, or slowly goes extinct.

5.3 Plurality Among Research Programs vs. Plurality Within a Research Program

Some might argue that the arguments and the model do not describe well what is happening in economics since economics is in fact a pluralist science. This argument is frequently made by scholars who defend the *status quo*. This defense, however, rests on a fallacious confusion of plurality *among* research programs, and plurality *within* a single research program.

No doubt, the neoclassical research program is within itself very pluralist with respect to models, approaches, and topics. Yet, if one looks closely at the variety of different models that are accepted as part of the neoclassical research program, all of them are consistent with the 'optimization-cum-equilibrium' approach, i.e., to speak with Figure 1, they share the same fundamental complexity reduction function and the same means to explore the mechanisms of the model.

This is true even for critical voices, such as Dani Rodrik, whose recent book *Economics Rules* (2016) has been considered to be proof of the plurality of economics by many economists. Though I think the book is an excellent description (and critique) of how neoclassical economics is practiced, it does not address economics as an entire discipline, and thus does not allow for any conclusions regarding its plurality. The reason is that for Rodrik, "economics is a way of doing social science, using particular tools. In this interpretation the discipline is associated with an apparatus of formal modeling and statistical analysis rather than particular hypotheses or theories about the economy" (2015, 7).

Thus, the reason why Rodrik's arguments do not show that economics is actually pluralist is his underlying definition of economics: he defines the science in terms of its methods, i.e. in the way reality gets represented in the models and the way these models are built. Other approaches dedicated to questions of how the economy works, using, for example, agent-based simulations or evolutionary dynamics, are simply not considered to be proper economics. Unfortunately, we are left wondering what they are then.

To consider a second example that highlights the difference between plurality within and among research programs, one may consider the field of behavioral economics, a sub-discipline that serves as a popular example when it comes to the characterization of neoclassical economics as pluralist: there is one part of behavioral economics, represented by people such as Ernst Fehr or Richard Thaler, that challenges the descriptive rationality assumption of economic models. The researchers try to provide more realistic accounts of observed human behavior by integrating new behavioral assumptions into equilibrium models with optimizing agents. This research gets regularly published in top mainstream journals and researchers enjoy a high prestige within the community. The other part of behavioral economics, represented by researchers such as Gerd Gigerenzer or Kumaraswamy Velupillai, argues that – *inter alia* – the concept of optimization is wrong. Thus, these researchers disagree with the hard core of the neoclassical research program (Berg and Gigerenzer 2010). Their research is not published in top rank journals and they enjoy little prestige within the community, if they are known at all.

6. The Epistemological Implications of Unity and Plurality

The previous section provided evidence that economic research programs feature positive feedback with regard to members and power, and that this implies a tendency towards scientific unity in economics. However, is the emergence of scientific unity necessarily bad? To answer this question, one has to study the epistemological implications of scientific monism and compare them to that of plurality.

6.1 Unity of Research Programs Leads to Intellectual Lock-Ins

The previous section drew an analogy between research programs and technologies: both feature positive feedback in terms of users (technologies) or members and power (research programs). Here it is argued that the analogy can be pushed further: as positive feedback mechanisms in the context of technology adoption can lead to technological lock-in, positive feedback mechanisms in the context of the reproduction of research programs may yield an intellectual lock-in. A technological lock-in is a situation in which people use a technology not because of its inherent superiority, but because many others do so. Arthur (1989) illustrates this by splitting the utility of a technology into two parts, i.e.:

$$u_i = r_i + v(n_i)$$

where u_i is the utility of an agent for using technology i, r_i is the "intrinsic utility" derived from the intrinsic quality of the technology, and $v(n_i)$ is the "network utility," which stems from the fact that others use this technology. Thus, $v(\cdot)$ is monotonically increasing in the number (or the share) of users n_i. From this it follows that under certain conditions people choose a technology not because it has higher intrinsic quality, but because many others use it. If even people who intrinsically prefer technology i over technology j choose j because of its higher network utility, we speak of a technological *lock-in*. This logic, it is argued here, also holds for the case of research programs: they can expand their academic power despite not being necessarily superior in terms of content.

Aside from this, there are a number of reasons why the situation of one dominant research program can be harmful for scientific progress in the long run. First, since every research program is characterized by a particular way of reducing the complexity of reality, it inevitably has some "blind spots," which might be addressed by a different approach. In a situation of scientific unity, applied scholars cannot choose pragmatically between several approaches since there is only one research program.

Second, new knowledge is usually a combination of previous ideas. If all the existing ideas are similar, the development space for new theories is narrowed down. Just as biological evolution requires a certain variety on the level on genes, intellectual evolution requires variety on the level of ideas.

Third, if the scientific system is locked-in in a particular research program, then radically new ideas cannot enter it. Just as new technologies usually need some kind of initial subsidy to become ready for the market (even if they are intrinsically superior to competing technologies right from the start), new research programs cannot emancipate on their own.[26] With a focus on one re-

[26] The development of quantitative measures for weather forecasting provides a nice example: it took many years of research in nonlinear dynamics until weather forecasts

search program, the potential inherent in radical, new ideas would be much more difficult to materialize. The potential of radical jumps on the landscape of ideas, however, has been shown to be an important aspect of any learning strategy in uncertain environments (see e.g. Beinhocker 2006, 207–211), and it should therefore be explicitly encouraged in a scientific discipline dedicated to the understanding of a complex object of investigation.

6.2 A Plurality of Research Programs Prevents Intellectual Lock-Ins

Just as technological *lock-ins* are inefficient from the perspective of society (or the "social planner"), intellectual lock-ins should be avoided as well. The dominance of a single paradigm is nothing desirable, but a potential obstacle for further scientific progress. What is an adequate response to this? The following example from the 'real business world' illustrates why ensuring a plurality of research programs is the only viable reaction. The story is about Bill Gates and the development of the Windows Operating System (Beinhocker 2006; Elsner, Heinrich, and Schwardt 2015). In 1987, Bill Gates faced a difficult choice: the operating system he had developed together with Peter Allen two years earlier, MS-DOS, was in danger of being outcompeted. One strategic option was therefore to invest into the development of a new operating system. Given the competition he faced through companies such as Apple and IBM, this strategy was risky, but, if successful, also promised high returns.

On the other hand, Gates could also exit the market for operating systems, wait until a standard system emerged, and then focus on developing applied programs for the new standard. Finally, Gates could also have sold Microsoft to one of the big companies and become part of one of the major players in the field. Gates faced fundamental uncertainty with regard to the best strategy – as do we with regard to the question of which research program in economics is the "best" or will turn out to be the best. In the end, Gates pursued all of the available strategies simultaneously: he started a joint venture with IBM, bought himself into the UNIX market, and invested into the development of his own operating system. Thus, as one cannot say for sure that one research program is the objectively superior one, then we – as the scientific community as a whole – should allow for the co-existence of several research programs, just as Bill Gates ensured the co-existence of different strategies until he knew which one was best.

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based on quantitative models became better than the forecasts based on personal experience and the qualitative study of the past. Nevertheless, it was important to do research in quantitative climate research, because otherwise the now superior techniques for weather forecasting would never have come into existence.

6.3 The "Best" Research Program is Not Identifiable

While the analogy of the Gates example is illustrative, its final part is misleading: in real science, the point where we ultimately know which research program is "best" will never come to pass. This is unfortunate, since selecting an optimal research program would allow the scientific community to harvest the effects of positive feedbacks, thus making the academic more efficient. However, as with the "demarcation problem," this quest has not yet been solved, and most philosophers consider the search for a universally valid set of criteria, according to which we can judge the *general* adequacy of research programs, a chimera. In the following, four different justifications for this pessimistic stance are discussed.

The first justification refers directly to the nature of the object of investigation: Dow (2002), for example, argues that for a monist approach to science to be admissible, i.e. for the scientific community to be able to identify a unique best research program, the systems of interest must have, at least, the following properties: (i) identifiability of all relevant variables, (ii) separability of the components of the system, i.e. components must be independent and atomistic, and their nature must be constant, and (iii) the structure of the relationship among the components must be known (or predetermined). Getting back to our definition of complex economic systems given above, it is clear that (i) it is hard to know whether all relevant variables have been considered, (ii) components are adaptive and interact with each other such that emergent properties at the system level may emerge, and (iii) the relationship among the components is non-linear and one of the major subjects of investigation of complexity economics (Mitchell 2009; Arthur 2010). Thus, complex economic systems would fall into Dow's category of "open systems," which is why it seems doubtful that the "right" research program will ever be discovered.

Second, Rosser (2004) argues in a similar vein that if one wishes to obtain an ultimate understanding of a system, the dynamic properties of complex systems, which are implied by the characteristics outlined above, require one to measure its states with unlimited precision. This is clearly impossible. Additionally, the self-referential relationships between economic agents often make it impossible to discover analytical solutions to their decision problems. While this does not mean that we cannot expand our knowledge of complex systems, it implies the impossibility of creating definite knowledge about the nature of the system and its behavior. This also means that it is not possible to identify the one correct approach to study such systems.

Third, Cartwright (2007) stresses the uncertainty with respect to the adequacy of model assumptions: whether the conclusions of a model hold for a particular situation in reality always depends on the adequacy of assumptions. Yet, it is impossible to test the adequacy of all assumptions at once. This fact is known as the eminent *Duhem-Quine thesis* (Duhem 1991; Quine 1951) and is related to the more general problem of the *underdetermination* of scientific theory, a challenge that applies not only to the social sciences, but to the physical sciences as well.[27]

Finally, the identification of the one correct approach to study economics is incompatible with the principle of "fallibilism," a core ingredient to Popper's critical rationalism, which is often praised (but seldom practiced, cf. Rodrik 2015) by economists: it is logically impossible to prove a certain theory or a certain approach to be "correct" because one lacks the means for such a proof once one departs from a purely theoretical and formal system and turns one's attention to empirical work (Kapeller 2013). Here, there are often different explanations for the same observed phenomenon that can hardly be pinned down exactly.

Summing up, since the characteristics of complex systems pose some hard epistemological constraints to their understanding, and since it is impossible to ultimately select the one "true ontology" of economics (Rosser 2004), it is also impossible to identify the one superior research program. The only viable reaction to this state of affairs is not only to tolerate a plurality of research programs, but to actively foster it: this is the only way to avoid intellectual lock-ins in the face of fundamental uncertainty on what ultimately is the right way of doing science. And to ensure something such as the "battle of ideas" (and to avoid scientific monopolization), we need to establish a plurality of research programs, just as Bill Gates followed a number of business strategies simultaneously.

7. Discussion

The arguments so far suggest that a state of plurality of research programs in economics is desirable. Two questions follow immediately from this conclusion: first, how can a state of plurality be achieved? Second, how can a state of plurality be made productive? These two questions will be the subject of this section.

7.1 Plurality Deserves Pluralist Intervention

A crucial challenge stems from the fact that although a plurality of research programs in economics is desirable from a system perspective, the scientific system in its current state tends to self-organize itself towards a state of unity.

To overcome this challenge, critical interventions in the scientific system are necessary. It is difficult to develop beneficial interventions and I do not claim

[27] See Stanford (2009) for an extensive survey of the relevant philosophical literature.

that this article contains the correct ones. Interventions in the scientific system share a serious challenge with intervention into markets: there are significant conflicts of interest and there are many groups that would like to exploit the pluralist movement to extend their influence and claim monetary and intellectual resources for their own research program. The following measures, however, seem to be examples for quite uncontroversial steps that can help to pave the way towards a more pluralist economics:

First, it seems desirable to teach more theory of science and history of economic thought in economics programs at universities.[28] This would help students to reflect on the advantages and disadvantages of different theoretical approaches and to accommodate them into their socio-cultural environment in which they have emerged. It also provides them with the philosophical tools to identify and compare various research programs and their meta-theoretical foundations.

Second, one should already allow undergraduate students to learn about different contemporary research programs. This could be done via seminars taught by representatives of different research programs or by the inclusion of lectures from different perspectives. Lecture series to which representatives from different research programs are invited are another possibility.

Third, since the source for the self-reinforcing mechanisms of research programs' reproduction are the institutions of the scientific system, one must change some scientific institutions to reduce the self-reinforcing effects. Reconsidering the use of rankings for the evaluation of scientific work and taking the diversity of research programs seriously when setting up new permanent jobs for scholars is one important measure, which, if supported by enough "mainstream" economists, is also straightforward to implement.

Finally, there should be more discussions and conferences that seek to bring together members from different research programs and engage in dialogue. Such a dialogue must have both applied and meta-theoretical aspects such that scholars build up both understanding and critical assessment of other research programs.

While these measures will not be sufficient, they may nevertheless be a good start.

[28] Grüne-Yanoff (2013) discusses the need to teach philosophy of science to science students more generally and makes constructive suggestions of how such courses could be implemented.

7.2 How to Make a Pluralist Economics Productive

Finally, some remarks on what can be done to ensure the productivity of a state of plurality are in order. Despite the fact that a plurality of research programs is necessary for a well-functioning academic system, mere plurality is not sufficient: productively relating different research programs to each other is a big challenge. To exemplify this, three potential difficulties will be described. At the same time, some preliminary suggestions for how to address these difficulties will be presented.

First, for a plurality of research programs to work effectively, regular discourse among them is necessary. This requires scholars who take the results of various research programs on particular questions and then study how these results relate to each other. These skills are hard to acquire since it requires scholars not only to learn about the basics of more than one approach of doing economics; it also requires a basic understanding in the theory of science. While this is not part of the core curricula in economics yet, the numerous student movements around the world indicate that many young scholars are able and willing to take up this challenge.[29]

Second, a productive pluralism requires scholars to engage in cross-paradigmatic discussions and to critically reflect the hard core and the common auxiliary assumptions of one's own research program. Thus, without scholars taking a fallibilistic attitude in the Popperian sense, a productive plurality of research programs is not conceivable. Unfortunately, there is no general measure to convince scholars to take such an attitude. Yet, it seems to be the case that including courses in the theory of science in undergraduate and graduate teaching could foster such a fallibilistic attitude.

Third, while it is difficult enough to allow for discussion among research programs on concrete research questions, it is even more of a challenge to settle meta-theoretical disagreements, e.g. about what counts as an explanation. I believe that general frameworks such as the one illustrated in Figure 1 (or, even better, more elaborated frameworks such as the one by Frigg and Nguyen 2016) can help scholars to make their disagreements more transparent and discuss them: only if a neoclassical and a Post-Keynesian economist make trans-

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[29] The following three examples illustrate the engagement of the students and are not meant as an exhaustive list of such initiatives: First, members of the German Network for Pluralism in Economics designed the online learning platform "Exploring Economics," which compares and relates various research programs on both the applied and meta-theoretical level. For the underlying theory of the project see Dimmelmeier et al. (2017). Second, in the UK students have edited a textbook that introduces and compares a number of different research programs (Fischer et al. 2017). Third, the open letter of the *International Student Initiative for Pluralism in Economics* (ISIPE) has been signed by more than 45 student associations from all over the world (see http://www.isipe.net/open-letter/).

parent to each other even the implicit assumptions of their inquiry and clearly state under which conditions they consider an empirical fact as "explained" can discourse among them actually take place.

Finally, different research programs use different vocabularies. In fact, one reason for scholars to gather in research programs is to work in a group of people who share the same language. If, however, one wishes to establish a dialogue between research programs one has to find a common language from scratch. Here, flexible modeling frameworks, i.e. frameworks that can accommodate many different assumptions and theoretical mechanisms, can be helpful. One such framework that becomes increasingly popular is that of agent-based simulations. They are useful since they are written in programming languages that might provide a common language which is so flexible that it can formalize a large variety of theories and is compatible with many different meta-theoretical foundations. This is not to say that agent based models are suitable to integrate all research programs, but in the past they have been shown to be successful in marrying, *inter alia,* neo-Schumpeterian and Post-Keynesian (Dosi, Fagiolo, and Roventini 2010), evolutionary-institutional and complexity (Gräbner 2016) or evolutionary and Marxian approaches (Wäckerle 2015). If two scholars belonging to different research programs interact with each other and are forced to put their theories into a common, algorithmic language, this can assist them in their mutual discussion to overcome the alleged incommensurability of their research programs, no matter whether the resulting agent-based model is useful on its own.

8. Summary

The main objective of this article was to argue that since economies are complex systems, pluralism with regard to economic research programs is the viable normative principle to adopt if one values scientific progress.

This conclusion was reached by arguing that whenever scholars study complex systems, they need to represent them in (verbal, symbolic or algorithmic) models, thus reducing their complexity. Since there are many different ways of reducing the complexity of reality, of exploring one's models, and of relating the models to reality, scholars gather together in research programs. These research programs accumulate academic power in a process characterized by positive feedback loops, which is why – given our current scientific institutions – the scientific system self-organizes towards a state of scientific unity. This is not desirable because it may lead to intellectual lock-ins. To prevent such lock-ins, a productive plurality of research programs is needed, and since such plurality does not emerge on its own, critical intervention is desirable.

Some potential measures to foster plurality and to boost the productivity of a pluralist economics were suggested. Most importantly, however, a pluralist economics requires economists that engage in discussion about the content and value of pluralism. As it is impossible to define a set of capabilities without public discourse (Sen 2005), it is impossible to define the right degree of pluralism without academic discourse. This requires openness and willingness to enter the debate, something this article has sought to stimulate.

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