

THE JOURNAL OF
PHILOSOPHICAL ECONOMICS:
REFLECTIONS ON ECONOMIC
AND SOCIAL ISSUES

Volume XIII Issue 2
Spring- Autumn 2020

ISSN 1843-2298

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need for a comparative approach to
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Abstract: Pluralism in economics appears to be a double-edged sword: we need more than one theory to grasp and explain the entire economic world, yet a plurality of possible explanations undermines the aspiration of the economic discipline to provide ‘objective knowledge’ in the singular of the ‘one world one truth’ conception. Therefore, pluralism is often equated with relativism and obscurantism. In this article, I will explore both the demand for pluralism and the fear of relativism and obscurantism, scrutinising each position in order to evaluate their respective justification and devising a methodological proposal that may appease both the defender and the sceptic of economic pluralism

Keywords: pluralism, methodology, paradigm

Introduction

The economic discipline is concerned with decision-making in circumstances in which alternative uses of goods and factors of production need to be considered along with the general outcome of such individual decisions in terms of income generation and distribution and its degree of stability over time contributing to an increasingly complex world. Economic interaction is – like social interaction in general and, thus, unlike natural laws – not God-given, but man-made and dependent on cultural, institutional and random factors which may be different at different points in time and even in different regions of the world at the same time. Can we, then, expect to explain economic activity with just one set of ideas using only one general approach based on a single world view?

Received: 9 July 2020

That is certainly what economists tend to believe, as they aspire to produce objective knowledge and hold on to the 'one world one truth' conception of reality. It is, of course, the blueprint of the natural sciences that economics is following [1] in its belief that the economic system is governed by a unifying principle: rationality in the use of its resources and rationality in its actions. Since the dawning of the Age of Enlightenment, man came to understand that he has been endowed with reason in order to shape his fate beyond age-old, (mainly) religiously motivated traditions and conventions and, moreover, that reason will enable him to discover 'what makes the world go round': 'from the outset, what is unique to enlightenment is its immanent relation to a criterion of rational validity which acts as a standard against which opinions and convictions can be upheld by rational examination' (Honneth 1987, p. 693).

Accepting the restrictive motivational basis of economic action [2] – encapsulated in the figure of *homo oeconomicus* – will result in a unique approach to economics only if the ensuing ontological presumption (or, as Joseph Schumpeter puts it, the pre-analytical vision) was itself pre-determined by the nature of the system under investigation. Or to put it more bluntly: if the economic system was to be assumed a 'closed system' in which all elements are interconnected in a unique and known way (and which can be seen as an *apriori* analytical proposition) and behave entirely rationally, the paradigm of intertemporal exchange based on specialisation, market coordination and optimisation would be the one and only representation. This is not to say that all different aspects of economic inquiry – from labour markets to international trade, from economic growth to financial markets – would be treated by a single theoretical approach or, even less so, that different perspectives – static versus dynamic, equilibrium versus disequilibrium, equilibrating versus adaptive, to name just a few – or different methods of inquiry – formal or narrative, econometric or qualitative – would have to be compressed into a single methodical framework. But its significance would be that all different schools and approaches – which may superficially look quite different and even opposed to each other [3] – were commensurate and share some common postulates: Say's law of markets or its modern version, Walras's law, and the classical dichotomy including the (long-term) neutrality of money. Within this paradigmatic shell, a variety of different universes of discourse (international economics, financial economics, labour economics, etc.), different theoretical approaches which may eventually stabilise as 'schools of thought' (neoclassical economics, behavioural economics, Keynesian economics, etc.) and different perspectives (allocational, evolutionary, institutional, etc.) will evolve and happily co-exist. [4]

Taking this position – and I would go so far as to claim this is the position of most economists – it becomes clear why the call of ever more students to pluralise the economic discipline has not been received with much enthusiasm and support by the economic epistemic community in general. On the one hand, it has been claimed for the reasons described above that economics is already pluralistic with respect to theories, methods and perspectives (see e.g. Becker 2017; Cedrini and Fontana 2017) [5]. On the other hand, methodological and ontological pluralism has been rejected as a backward step in the development and professionalisation of the discipline only giving rise to relativism and obscurantism (see e.g. Backhouse 1998; Tirole 2014).

In the following, I will take up both the demand for pluralism and the fear of relativism and obscurantism and scrutinise each position in order to evaluate their respective justification and devise a methodological proposal that may appease both the defender and the sceptic of economic pluralism.

Pluralism, what pluralism?

Before we consider the state of economics as a scientific discipline, let us first do some philosophical groundwork: the set of theories and models which form the backbone of scientific inquiry are often called 'paradigms' (Thomas Kuhn), 'scientific research programmes' (SRPs; Imre Lakatos) or 'thought styles' (Ludwik Fleck). What is more important than the label is the content: paradigms, scientific research programmes or thought styles comprise three dimensions:

1) The *ontological dimension* is concerned with the essence of the object of inquiry, its basic constituents. Once the object of inquiry – in our case: the economic system – is taken as a closed system, in principle it is fully analysable (despite its potential complexity [6]) and its essence is given. In contrast, open systems in which some elements or their relations are unknown become complex to a higher degree (depending on the number of unknowns), indeterminate and in essence open to specification. In order to close the system, such unknowns must be axiomatically assumed (dubbed 'core axioms' in Lakatosian SRPs). These assumptions, again, are *a priori* purely synthetic, can be contested, and shape the essence of our object of inquiry. As mentioned above, open systems allow for different ontological specifications or, as they are sometimes called, different pre-analytic visions.

2) The *epistemological dimension* breaks down the pre-analytical vision situated in the ontological dimension into core and auxiliary assumptions or, in Lakatosian

terms, determines the 'negative heuristic' which 'specifies certain claims of the research programme as not revisable' (Brahmachari 2016, p. 5) and the 'positive heuristic' forming a protective belt around the core axioms. This can be tinkered with if, for instance, empirical evidence or the pursuit of a particular perspective indicate it would be politic to do so.

3) The *methodological dimension* can be seen as 'meta-methodical', as it specifies the procedures accepted by the epistemic community to discriminate between 'truth' and 'non-truth' or 'science' and 'non-science'. It is part of the professionalisation of a scientific discipline to agree on a common methodological foundation.

The dispute about pluralism in economics can only be understood against the backcloth of conceptual confusion. This confusion stems from a lack of precision in notions and a proper distinction between pluralism and mere variety. As shown in Heise (2017), pluralism not only entails a multitude of particular occurrences, but includes an oppositional stance including incommensurability of elements. Therefore, simply pointing to a multitude of theories and models does not necessarily prove the existence of pluralism in economics if these various theories either only complement each other with respect to their topical focus or are commensurable with respect to their ontological foundation but take different epistemological or methodical perspectives. In that case, instead of acknowledging pluralism we would rather have to speak of a variety of theories or models in the epistemological dimension – whose existence and basic necessity can hardly be denied.

On the other hand, pluralism thus not only entails some incommensurability but also points to the ontological dimension of a paradigm, since in the interests of quality control and standardisation the methodological dimension necessarily assumes monism. [7] And here, denying pluralism implies the acceptance of the economic system as being closed or, to put it differently, as having a unique ontological specification or pre-analytic vision. Conversely, pluralism in a meaningful sense (i.e. beyond mere intra-paradigmatic variation) is based on the confirmation of the economic system as an open system and therefore the acknowledgement of ontological pluralism as the *conditio sine qua non* of paradigmatic pluralism.

Is there any plausible reason to believe that the economic system is closed? It is surely plausible to close the system by some ontological assumptions, otherwise it would be impossible to analyse it and make any meaningful predictions. However, the necessity of some closures does not imply a peculiar closure from the outset.

Therefore, paradigmatic pluralism based on alternative (mutually exclusive) ontological foundations [8] is much more likely to be the state of a healthy, progressive economic discipline than paradigmatic monism merely allowing for intra-paradigmatic variation often misleadingly dubbed pluralism. Moreover, due to the famous Duhem-Quine thesis (see e.g. Cross 1982; Sawyer et al. 1997) stating 'it is not possible to falsify single hypotheses because it is invariably conjunctions of hypotheses which are being tested' (Cross 1982, p. 320), and disclosing the manifest methodological restrictions of non-experimental science, paradigms can never be invariably refuted. Therefore, paradigmatic pluralism is not merely an ethical norm ('tolerance' or 'fairness') but a scientific imperative: only if paradigmatic pluralism is approved and practised within the epistemic community can the accusation of ideological distortion often lodged against the economic discipline be rejected and academic freedom acknowledged.[9]

Pluralism versus relativism

In the past, many approaches have been considered to be economics: do physiocracy, the Scholastic School of Salamanca or the racial economics of the Nazis need academic representation? Most of the heterodoxies rather appear to have been unfruitful scientific cul-de-sacs. At any rate, the burden of proof of being would otherwise lie with the proponents of the heterodox approaches (Bachmann 2016, p. 598; my translation).

Although this remark may not be taken as an appropriate contention with the issue of pluralism, it still contains the reservations most economists have about a pluralisation of the economic discipline: [10]

- 1) pluralisation implies an unconditional 'anything goes' opening the doors to obscurantism or even solipsism;
- 2) contending approaches ('heterodoxies') have not been successful in the past;
- 3) mainstream economics is the buck against which heterodox contenders have to actively prove their right to exist.

In all these reservations, one critical consideration shines through: how can a science aspiring to produce objective knowledge accept a state of plurality as desirable if plurality implies incommensurability and even an oppositional stance? Would that not necessarily imply a relativism that runs counter the discipline's own self-acclaim and self-conception? Sheila Dow (2004, p. 282) is surely right when she

states, 'Were it defensible, a monist approach would be more satisfactory'. Yet such an approach is simply indefensible given the methodological restrictions outlined above. All that economics can ever claim to produce is 'conjectural knowledge' that needs to be tested empirically and must be accepted as long as it has not been clearly falsified. And, of course, the number of proponents and users of a paradigm is only a measure of its 'fruitfulness' once the 'battleground of economic ideas' is even (or, to use a more appropriate metaphor, if the 'market for economic ideas' is perfect) – and there is much evidence that this is not the case (Heise and Thieme 2016; Fourcade et al. 2015). No paradigm can claim priority over others *per se*.

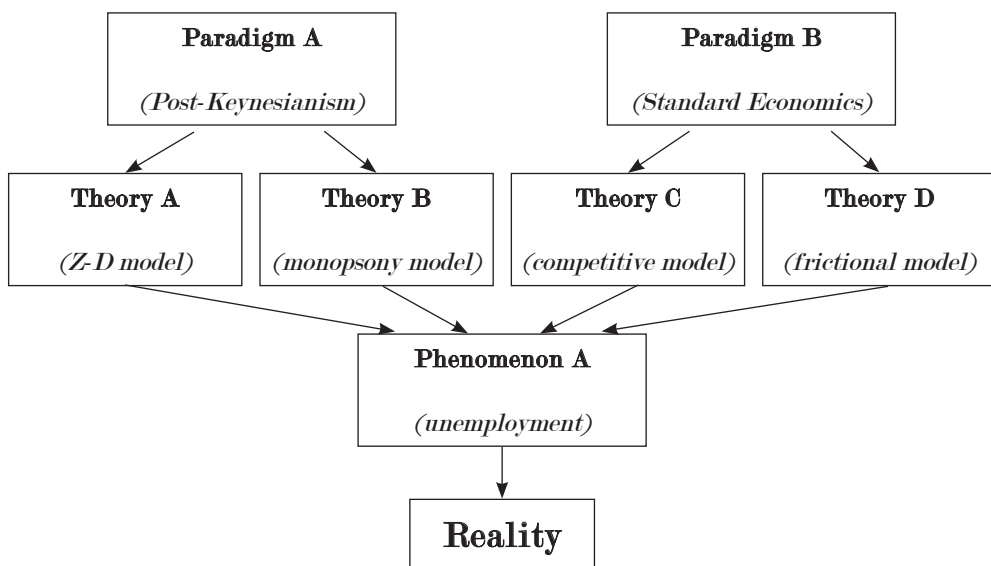
However, the potential co-existence of competing conjectural knowledge based on incommensurable paradigms is not to be misunderstood as relativism but simply reminds us of the possibility of deception. And to equate pluralism with obscurantism is, however, another *non-sequitur* fallacy: acknowledging the imperative of paradigmatic pluralism does not deny the acceptance of quality standards and, indeed, methodological monism has been approved precisely in order to distinguish accepted from non-accepted (conjectural) knowledge [11]. If we grant the Salamanca School of economics, physiocracy or the racial economics of the Nazis (probably chosen for their apparent obscurity) the status of economic paradigms [12], it is self-evident that they do not fulfil the methodological requirements of logical rigor and empirical non-falsification necessary for substantiated academic representation. However, paradigms such as post-Keynesianism, Marxism, neo-Ricardianism or complexity economics, for instance, which all share the fate of near-extinction in academic economics, do have legitimate claims to representation (see e.g. Lee and Lavoie 2014).

Comparing scientific research programmes and the choice of paradigms

Economics is caught somewhere between highest scientific aspirations and methodological restrictions. It wants to be a science as 'hard' as a social science can possibly be [13], yet not dogmatic or ideological. This balancing act is practised rather unsuccessfully: instead of advocating and defending its methodological standards while fostering paradigmatic pluralism to bolster against any ideological bias and restriction of academic freedom, methodological diversity and epistemological variation is mistakenly adduced to counter the lack of paradigmatic plurality while still defending an orthodox 'citadel' (see e.g.

MacKenzie 2017). But even this practice, although inappropriately restricting the number of available paradigms to conduct research, does not prevent the users of economics – researchers as 'producers', politicians and laymen as 'consumers' and students as prospective producers alike – from having to choose which model or theory to select. If for example we want to explain the impact of minimum wages on employment, we always have the choice between different models or theories (see fig. 1): within standard economics, we can choose between competitive labour market models and labour market models based on imperfect competition, between labour market models with or without frictions (all models showing different auxiliary assumptions) – however, they all model the labour market on the idea of intertemporal exchange (core assumptions). But there could also be the choice between models based on intertemporal exchange (standard economics) and those based on nominal obligations (post-Keynesian economics) or power relations (Marxian economics) – in all cases, the impact of minimum wages on employment may be very different (see e.g. Heise 2018).

Figure 1 Navigating among models (examples in brackets)



Source: author's representation

Having to choose between different approaches implies knowing and comparing them in order to select the appropriate one. The comparative methodology has

been used in economics quite comprehensively to better understand the impact of institutions or policy regimes on economic outcomes such as productivity, growth, employment or inflation (see e.g. the literature on 'varieties of capitalism' and 'comparative economics'; Hall and Soskice 2001; Djankov et al. 2003); it is almost unknown as a tool for discriminating between theories, models, or paradigms. It is one thing to speculate on the reasons for this omission, but another to notice its consequences: the omission may be due to a sheer lack of demand. If researchers do not feel they have to make an informed choice – which would be the case if they were unaware of alternatives or simply selected theories because of their alleged novelty – there would be no need for comparison to provide information about differences or communalities. The consequence of the omission, however, is clear: the choice every user (consumer or producer) of the economic discipline has to face will be undertaken uninformed, intuitively or based on particular incentives (e.g. external monetary incentives or the quest to produce novelties).

Comparativism has hardly penetrated the philosophy of science (see e.g. Pearce 1991; Burger and Heidema 1994), and I am not aware of any comparative approach in economics. Occasionally at best, different theories are juxtaposed (as in Walsh and Gram 1980; Crotty 2011; Munoz 2011; Wolff and Resnick 2012, which has been termed 'descriptive comparison'; see Giesen and Schmid 1978, p. 180 f.) [14] or viewed from a history of thought perspective but not compared analytically (termed 'methodological comparison'; see Giesen and Schmid 1978, p. 180 f.) so as to provide a basis for choice. The situation is quite different in sociology, which has always been characterised (some would say: plagued) by a great variety of approaches (see e.g. Ritzer 1975). As early as the late 1960s and early 1970s, the discipline in Germany and elsewhere discussed the possibilities and limitations of a systematic comparison of theories as a 'particularly effective critique of the involved approaches which could contribute to the discovery of superior and, consequently, the segregation of suboptimal theories or subtheories' (Klinkmann 1981, p. 249; my translation). It seems fair to state that the endeavour to establish 'comparative sociology' as an institutionalised branch of research was unsuccessful (see e.g. Greshoff et al. 2007, p. 6 f.). Although the situation in economics is different from that in sociology [15], economics may be learning from the failures in sociology – we will come back to this later.

Before focussing on the concept of comparing paradigms, we should differentiate more clearly between notions not always accurately defined: models, theories and paradigms. A paradigm is the overarching collection of theories and 'model

solutions' sharing a common negative heuristic (ontology or pre-analytic vision) and methodology culminating in very few generally accepted postulates, yet differing in methodical approach, subject-matter and positive heuristic. Theories are epistemological systems of deductive logic focussing on a particular section of reality and thus providing the analytical structure of the paradigm. And models are sets of reductionist, quantifiable relations by which quantitative predictions of economic outcomes can be produced. In terms of empirical testing, models can clearly be falsified while theories and in particular paradigms entail enough leeway (auxiliary assumptions) to provide, at least *ex post*, predictions which cannot easily be rejected by empirical evidence.

Against this backdrop, comparativism in economics must be discussed in relation to its potential achievements. We need to compare in order to make an informed choice. If this choice is made on purely scientific grounds [16], verisimilitude would be the best candidate as its objective: we should take the approach which explains reality best – i.e. according to its explanatory power. Yet this comes with serious problems. And this does not simply mean that the empirical test of theoretical concepts is always disputable [17]; it rather reintroduces the Duhem–Quine critique: models may be compared in terms of their verisimilitude and, consequently, discriminated by this measure: if, for example, a model based on traditional labour market theory including frictions predicts the experienced (i.e. empirically measured) employment effects of introducing a minimum wage more appropriately than a model based on traditional labour market theory without frictions, the former should displace the latter. And if both models were to produce quantitative and qualitative (positive versus negative employment effects) predictions which conflict with empirical evidence, both models should be rejected. Yet the falsification of a model neither necessarily falsifies the underlying theory (here the standard neoclassical labour market theory) nor the foundational paradigm (here the intertemporal exchange paradigm of the neoclassical school): quantitative (e.g. quantitative changes in some of the parameters involved) and qualitative (e.g. introducing imperfect competition) adjustments to auxiliary assumptions can (and will) be used to reconcile *ex post* theoretical prediction with empirical evidence. Hence verisimilitude is a necessary but not sufficient measure in comparison.

However, 'repair work' performed on theories and models to avoid empirical falsification must not produce what Ludwik Fleck called a 'harmony of deception' locking in their followers and proponents in a 'thought compulsion' which immunises a paradigm against any critical review. Comparing theories and paradigms may,

therefore, be aimed not so much at their respective verisimilitude as at the 'laborious efforts (...) made to explain an exception in terms that do not contradict the system' (Fleck 1979, p. 27). Comparing theories and paradigms may contribute to their evaluation and, finally, an informed choice with respect to three dimensions:

- 1) the amount and direction of adaptations of basic theories can inform us about the state of the paradigms or scientific research programmes in a Lakatosian sense: are they in a progressive or degenerating state? [18]
- 2) using the concept of Occam's razor, the degree of simplicity and parsimony of theories comprising a paradigm may provide information about the required complicatedness (or, as in common parlance: complexity) needed to explain a certain phenomenon.
- 3) if the choice between theories and paradigms cannot be based on their explanatory power (i.e. their output), the accurateness or approximation to reality of their (basic and auxiliary) assumptions (i.e. their inputs) can be taken as a selection criterion.

Let us again take the example of a minimum wage introduction and its impact on employment. Within the standard economic paradigm of intertemporal exchange and its allocational perspective, setting a minimum wage above the market clearing level – and this is the only sensible setting, as a minimum wage below the market clearing level would be useless – will have to have negative employment effects. These effects may be smaller if we assume a labour market with imperfections (transaction cost and imperfect competition) rather than a 'perfect' labour market, yet the allocational distortion is unavoidable (see Braun et al. 2020). Choosing, alternatively, a post-Keynesian paradigm based on nominal obligations and taking a macroeconomic perspective to determine the employment effect of minimum wages, the prediction based on a simple Z-D theory without any particular assumptions about 'employment market' [19] frictions would be quite different: there is a high likelihood of minimum wages having not employment but rather price effects (see Heise and Pusch 2019). When it comes to empirical testing – and the real-world experiment of introducing a binding statutory minimum wage in Germany in 2015 fully replicates these findings – a vast number of studies using a variety of methods suggest no discernible employment effect (see e.g. Schmitt 2013). According to the verisimilitude criterion, surely the post-Keynesian employment theory should be selected over any model based on standard labour market theory. Moreover, with respect to the approximation to reality of the parametric assumptions necessary to

produce results in line with the empirical evidence, again the post-Keynesian model fares better than the standard labour market models of any variety (see Heise 2018). Also, the post-Keynesian employment theory suggests, besides the (undiscernible) employment impact, further macroeconomic (increase in prices) and structural effects (employment shifts at branch level causing a structural change) which can be empirically tested and, thus, allow us to rate post-Keynesian theory as progressive in Lakatosian terms. The many extensions and supplementations of standard labour market theory appear, in contrast, rather to be 'laborious efforts' to somehow reconcile theoretical prediction with troublesome reality, putting it not only in a state of degeneration but also padding out the simple labour market theory in a way that any test based on Occam's razor criterion of simplicity and parsimony would surely give way to the post-Keynesian approach.

Discussion

Comparing models, theories and paradigms may provide information allowing the scholar to make a better (i.e. more informed) choice of model based on a theoretical foundation embedded in a paradigmatic framework in order to address a problem at hand. However, even if all criteria – verisimilitude, state of evolution, Occam's razor and the realisticness of assumptions – privilege one theory over the other(s) as in the case above, this does not inexorably imply the superiority of the respective paradigm, since, again, the Duhem–Quine critique applies: only certain sections of the real world can be placed under scrutiny in the above fashion but never entire paradigms (or world views or pre-analytic visions). Nevertheless, the state of models and theories with respect to their verisimilitude, progressiveness and realisticness may well rub off on the attractiveness of a whole paradigm: if the neoclassical labour market theory has proven inferior to post-Keynesian determination of employment in a core section of its theoretical deduction – the price-quantity link – why should we still favour standard economics over post-Keynesian theory in their application to other phenomena? Or, more bluntly: why should we not expect a broad shift in the economic discipline from the neoclassical intertemporal exchange paradigm to a post-Keynesian paradigm of nominal obligations to explain financial crisis, determine fiscal policy orientations or model the implications of zero-growth economies?

The answer is twofold: on the one hand, such a paradigm shift or scientific revolution in a Kuhnian sense may not be feasible because the 'market for economic ideas' is imperfect and there are formal and informal incentives (such as 'measuring' scientific quality by way of impact factors and rankings) which interfere with

the choice to be made. Part of the imperfection is simply the lack of information about alternative paradigms on the part of most standard economists. [20] On the other hand, the superiority of the post-Keynesian paradigm with respect to the criteria offered may not be as obvious as I have pretended in the example above: the limitations of the verisimilitude criterion have already been mentioned; moreover ever since Paul Feyerabend's *Against Method* (1975), the aptitude to discriminate theories along the fault line of progressiveness versus degeneration in an unambiguous way has been questioned. [21] Introducing frictions in the neoclassical labour market theory, for instance, may not only be considered 'repair work' in the face of new and troublesome evidence somewhat undermining the theory, but may also provide new insights and testable hypotheses (e.g. about search and hire activities in the labour market) which may be considered a progressive problem shift increasing the predictive power of the paradigm. Also, the criterion of Occam's razor may not be made accountable as easily as it seems at first glance: let us assume an economic outcome EMP is determined via two different channels (a and b) which impact on EMP in a conclusive way. Yet the same impact could be modelled via four different channels (c, d, e and f), two of which (e and f) have the same quantitative impact only with opposite signs (i.e. channel e increases EMP while channel f decreases EMP to exactly the same degree). Moreover, we have two models (A and B) with the same paradigmatic background and one model with a different paradigmatic basis (C). The two models of the same paradigmatic background determine EMP basically in the same way, yet one model (A) is simpler or more parsimonious in the sense that it only relies on two channels of impact (c and d) while the other model (B) describes all four channels (c, d, e and f). The third model with a different paradigmatic background (C) determines EMP correctly via the two different channels of impact (a and b). All three model predictions can provide the empirically measured value for EMP only if certain *ad hoc* parameter specifications are used. According to Occam's razor, within the one paradigm, the superior model would be the simplest, i.e. the one that estimates EMP on only two channels of impact (c and d), ignoring two other channels (e and f) and thus oversimplifying the real world, since important information about the determination of EMP could be lost. With respect to the inter-paradigmatic choice, Occam's razor would be indifferent and could not discriminate between models A and C [22].

This is where the models' or theories' approximation to reality assumptions enters the picture. For quite some time, validity was assumed for Milton Friedman's verdict that although model assumptions are judged to be unrealistic, the model may still be considered valid as long as its predictions are not empirically falsified

(Friedman 1953) – what is known as the 'F-twist'. To put it differently, Friedman favoured an assessment of theories against their predictions, not their assumptions. A long discussion about the 'F-twist' may be summarised by the statement that the realisticness of assumptions matters for the evaluation of theories and even that Friedman never argued otherwise (see e.g. Musgrave 1981; Mäki 2009). If anything, Friedman's verdict may be taken implying a hierarchical ordering of outputs and inputs: first and foremost, models, theories and paradigms should be valued by their explanatory power (output) and the realisticness of their assumptions (inputs) is of only secondary importance. However, if a final assessment about the relative or comparative virtue of a model, theory or paradigm cannot be based on verisimilitude alone (or, as outlined above, if the correspondence with reality can only be achieved by making particular assumptions (e.g. about parametric settings)), the approximation to reality of the assumptions of the model, theory or paradigm will become an important measure of discrimination. Alas, the realisticness of assumptions is often not easy to evaluate: are the 'closed system assumption' or the assumption of transitivity of preferences or the 'gross substitutability assumption' realistic or not? Quite often, such assumptions beyond empirical control are termed 'axioms' and compensate for the core assumptions (i.e. the deductive foundation which cannot be questioned) while testable assumptions form the protective belt. And it is this protective belt which needs to comprise unrealistic assumptions because the complexity and complicatedness of reality can never be grasped in totality (otherwise it would merely be an image of reality losing all its explanatory power) – the 'Abstractionist Defense' (see e.g. Rappaport 1996). But will it be possible to assess the degree of realisticness of assumptions of a model, theory or paradigm relative to alternative models, theories or paradigms? Probably, with respect to empirically quantifiable parameters in models, but much less so with respect to unquantifiable assumptions in theories or axioms in paradigms.

Conclusion

The economic discipline finds itself in a somewhat precarious state: its self-image is that of a 'hard science' producing objective knowledge based on a single paradigm including a variety of different theories and a common methodological standard which has finally evolved after a long history of competing ideas and some *Methodenstreite* in the wake of the discipline's professionalisation. A pluralisation of economics as demanded by a small but growing number of scientists, students and practitioners would be seen as an undesirable backwards step in the maturation of

the discipline, opening up economics to relativism and obscurantism. On the other hand, methodological restrictions in a non-experimental science simply make the unambiguous selection of a single, monistic – 'true' – theory or paradigm impossible and, thus, imperatively demand the 'grandfathering' of not only a variety of competing theories (within a given paradigm) but also a true pluralism of competing theories and paradigms as long as certain standards are met. How can both positions be reconciled? We can surely not have a monist economic science without the comprehensible accusation of ideological bias and restrictions on academic freedom but nor can we have, on the other hand, a pluralist economic science without the comprehensible accusation of relativism or non-science. Therefore, neither monism nor 'radical pluralism' appear to be acceptable and desirable, but a form of 'regulated or structured pluralism' based on a quality assurance procedure comprising the acceptance of a common methodological foundation and the development an institutionalised system comparing economic theories as an acknowledged branch of scientific inquiry within the field of economics.

Although comparison of economic theories will also be unable – like empirical testing – to unambiguously discriminate between economic theories and paradigms with respect to their verisimilitude, it will shed some light on the state of a paradigm, its complicatedness and the realisticness of its assumptions and, indispensably, allows an informed choice of theories which cannot be avoided. Additionally and beyond the function of selection criteria, systematic comparison of economic theories may also provide information about *inter alia*: a) the axiomatic core and differences within it, thus, its paradigmatic affiliations; b) the relationship of different hypothesis (explanations) to different core axioms (pre-analytic visions) and their normativity; c) its paradigmatic foundations (heterodox or orthodox); d) their relatedness (commensurate or incommensurate; and e) their novelty. All this information is necessary to improve the quality and accurateness of scientific economic inquiry: the paradigmatic pluralists need to accept that plurality must concede quality control to avoid obscurantism, while the paradigmatic monists need to accept that an unavoidable variety and plurality of theories and paradigms requires procedures for a traceable choice. After its empirical turn, the economic discipline is in need of a 'pluralistic and comparativist turn'.

Although this is not the place to speculate about the viability of such a path of development, we need to return to the fact that a similar project of initiating an institutionalised comparison of theories failed in sociology. The main concern of those sociologists that promoted a comparison of sociological theories as an

acknowledged branch of scientific inquiry was to find a common basis on which something like 'normal science' in a Kuhnian sense could be established – something considered by many other sociologists to undermine the very nature of their discipline and rejected on the grounds of an 'impossibility of objective comparison' (see Klinkmann 1981, Stäheli 2000, p. 18). For Greshoff et al. (2007, p. 7), the failure to establish a consensus about the methodological requirements and possibilities of systematic comparisons was responsible for the collapse of the 'comparativist turn' resulting in the continuation of the *status quo* of a 'pseudo pluralism' in which "the particular positions and their relations were unresolved in such a way that they could not serve 'as a critical authority' for each other and could not question each other in order to advance knowledge" (Greshoff et al. 2007, p. 5; my translation).

There is reason to believe that *status quo* forces in economics (all those proponents of paradigms and theories that see their material basis and reputational position undermined) will also try to prevent a 'comparativist turn' – the likelihood of their success will depend on how much the economic discipline will be able to keep its methodological self-reflection at a low level [23]. Or, as Dany Rodrik (2018, p. 280) writes: 'One wishes that economics training would do a better job of instructing future professional economists about their responsibilities – the need for multiple models, sound political economy analysis, and the ethics of public participation (...)'. Uskali Mäki's (2018, p. 234) response to Rodrik can be taken as slightly sceptical in outlook: "I wonder if it would be too generous to require no more than envisioning a feasible strategy of re-designing the institutions of economics, perhaps re-educating the economic profession, imposing an ethical code of conduct, and the like – [...]". However, the fact that an acclaimed standard economist such as Dany Rodrik has deliberately engaged in methodological discussions can be taken as a sign of hope that comparing economic theories will become a recognised branch of inquiry and the occupation with different theories and paradigms will become the norm for a discipline that is aware of its many theoretical foundations.

Endnotes

[1] See e.g. Mirowski (1989), who shows that '(t)he development of physics [...] served as a template for the development of economics as a science' (Brahmachari 2016).

[2] Which, of course, dissenting schools of thought such as behavioural economics or complexity economics refuse to do.

- [3] For instance, the relationship between 'classical' political economics and 'neo-classical economics' is often seen as conflictual or non- or contra-classical rather than as continuous, as the term 'neo' seems to suggest; see e.g. Dobb 1973, p. 248; Schumpeter 1954, p. 919; Zafirovski 1999, p. 46).
- [4] Such a friendly coexistence does not rule out the possibility of one particular school of thought using particular methods and perspectives becoming dominant at a given point in time, nor of this changing over time.
- [5] Occasionally it is argued that 'the full panoply of perspectives' may be disguised in introductory courses 'where the professor is keen to demonstrate how markets work' (Rodrik 2015, p. 198). Thus, charging economics with a lack of pluralism is portrayed either as a matter of informational distortion (on the part of students) or bad communication (on the part of teachers).
- [6] Everything has a certain complexity. In this respect, referring to something as being 'complex' is virtually meaningless. In ordinary parlance, 'complexity' generally means 'complicatedness' in terms of numbers of elements and relations between them. Yet, if all the elements and their relations are known – i.e. if we consider a 'closed system' – then the complexity that is low for the system is (pre-)determined even if the number of elements and relations is high.
- [7] Methodological monism denies neither a variety of methods and perspectives nor the potential targeting of different methodologies – however, accepting methodological pluralism would force an interdisciplinary scope beyond the disciplinary borders to economics.
- [8] As far as I can see, there are three different pre-analytical visions in economic theorizing: mainstream or standard economics is based, as already mentioned above, on intertemporal exchange, post-Keynesianism on nominal obligations (creditor-debtor-relationships) and Marxism on power relations as basic constituents.
- [9] An investigatory committee of the Canadian Association of University Teachers (CAUT), for instance, ruled that the reduction or elimination of approaches and views outside mainstream economics at the Economics Department of the University of Manitoba must be seen as a serious violation of academic freedom.
- [10] Usually, they are introduced and supplemented with the pretention that 'it is never accurately substantiated why paradigmatic plurality in economics is necessary

or, at least, desirable' (Bachmann 2016, p. 598) – I hope to have shown above that this claim is clearly wrong.

[11] Jon Elster (2007, p. 445) highlights the importance of methodological monism as a criterion for judging whether a discipline has become a science or not.

[12] Which can reasonably be disputed.

[13] Elster (2007, p. 445ff.) measures the 'hardness' of a science by a) its methodological monism, b) the irreversibility of its scientific progress, c) its ability to summarise commonly shared basics in textbooks and d) its old masters being only of historical interest. Seemingly, he believes economics not to be a truly hard science yet, because although he acknowledges economics fulfils c) and d), fulfilment of a) and b) is still lacking. Particularly the fact that economics is still debating many basic economic phenomena such as the explanation for unemployment – due to a variety (not necessarily plurality!) of theories – is taken as proof of the discipline's 'softness' as a science. Taking the ability to produce a single, commonly shared and accepted 'knowledge' ('one world one truth') as an indicator of a 'hard science', Elster's reservations against economics may be traceable – yet, this judgement does not conform to his own definition. As we have seen, methodological monism does not necessarily come with epistemological and ontological or, more generally, paradigmatic monism. Therefore, accepting Elster's imputations, equating paradigmatic variation or even pluralism with the softness of a science may be intuitive but is simply incorrect.

[14] Only recently, the German quarterly journal *List Forum für Wirtschafts- und Finanzpolitik* published a special issue entitled *Mainstream versus Heterodox Economics: Research Programmes in Comparison* (see Erlei and Haucap 2019), which assembled a great number of different economic approaches without the slightest attempt to do what the title promised: to compare the different schools.

[15] While in economics a single paradigm has evolved into an orthodoxy which marginalises all other paradigms as heterodox and, thus, comparativism would be an attempt to reconcile (more) pluralism with less fear of obscurantism, in sociology no approach has yet become dominant in that sense that a Kuhnian state of 'normal science' has been reached. Here, comparativism would rather be an attempt to search for common grounds and, in this respect, to restrict the full panoply of ideas 'in order to decisively improve the conditions for producing cumulative knowledge in sociology to a greater extent than has hitherto been the case' (Greshoff et al. 2007, p. 9; my translation).

[16] If this is an economic choice, the decision will depend on the preferences of the scientist. It may be very different depending on the preference ordering of objectives such as 'finding truth', 'maximising income' or 'maximising reputation'.

[17] Klinkmann (1981, p. 251 ff.) takes a more critical position not shared here: he argues that paradigms cannot be compared effectively because 'comparing' implies 'knowing' and the comparativist can know only the paradigm he is 'using' (which means: being part of the epistemic community, sharing the pre-analytic vision, being involved in common interactions), not the one he compares it with. This position is not shared because of the inherent subjectivist assumption of science.

[18] According to Imre Lakatos, a scientific research programme (SRP) can be considered 'progressive' when a new theory within that domain of inquiry predicts more than its predecessor theory ('theoretically progressive') and these predictions are confirmed by actual observations ('empirically progressive'). If an SRP fails to display these characteristics, it has become 'degenerating'.

[19] We use the unfamiliar term 'employment market' instead of 'labour market' in a post-Keynesian context in order to avoid the idea of real-wage-driven employment determination as in the standard economics paradigm.

[20] Of course, we have no information about standard economists' awareness of alternative paradigms. All we know – taking their citation behaviour as an indicator – is that they do not care about alternative paradigms, not even in a critical way; see e.g. Kapeller 2010a, Kapeller 2010b.

[21] See e.g. Harman (2019) and the dispute about the progressiveness of the Keynesian SRP between Hands (1985 and 1990) and Blaug (1987) and Ahonen (1989). In Brzezinski and Dzielinski (2009) it becomes clear that the concepts of progressiveness and degenerativeness of SRP are not only difficult to pin down analytically but also to evaluate empirically.

[22] If we take EMP to be an abbreviation of 'employment level' and if a stands for 'aggregate demand', b for 'aggregate supply', c for 'real wage', d for 'market structure', e for 'hiring incentives' and f for 'search incentives', model A would be the standard neoclassical labour market model with imperfections, model B the standard neoclassical labour market model with frictions and model C the simple post-Keynesian Z-D model.

[23] 'One day an exasperated theorist turned to me and explained: "Look, it is all about the division of labour. We do economics as it is currently practiced. If someday a philosopher of economics or a specialist in economic methodology comes up with a better idea, then somebody will tell us about it and we will know." Economists' lack of curiosity on the philosophy of science is reflected in the fact that informed discussion on these questions is relegated to specialized journals (...). More damagingly, it is reflected in the lack of training in graduate school in methodology' (Rodrik 2018, p. 276).

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