Is economics normal science: Do economists share a paradigm?

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Abstract. Thomas Kuhn provided a scheme for the development of the natural sciences. According to Kuhn, this development is marked by periods of cumulative normal science on the one hand and non-cumulative turning points, so-called scientific revolutions, on the other. This essay is meant to answer the question as to whether economics matches the picture Kuhn draws of normal science. I argue that economics should in fact be considered normal science since it has acquired efficient ways of puzzle-solving and a high degree of professionalization. However, economics' paradigm, neoclassic theory, is not as widely shared as paradigms in the natural sciences. It seems that this divergence is due to more general differences between the social and the natural sciences.

I. Introduction

In *The Structure of Scientific Revolutions* Kuhn provided a scheme for the development of the natural sciences. According to Kuhn, this development is marked by periods of cumulative 'normal science' on the one hand and non-cumulative turning points, so-called scientific revolutions, on the other.

This essay is meant to answer the question as to whether economics[1] matches the picture Kuhn draws of normal science. Normal science is research firmly based on one or more paradigms. Thus, if economics is normal science depends on whether economists share a paradigm. I argue that economics should in fact be considered normal science. Neoclassic theory should be regarded economists' paradigm, even though not all researchers are equally committed to it. It seems that this lack of consensus is due to the special properties of the social sciences as opposed to the natural sciences.

The argument proceeds in two steps. Section 2 tackles the question of whether Kuhn's scheme, initially meant to describe the development of the natural sciences, can be applied to economics. I argue that such an application is possible. Section 3 investigates economic science itself in order to determine if it meets Kuhn's notion of normal science. Considering the dominant status of neoclassic theory within economics and the resulting efficiency of puzzle-solving and high degree of professionalisation, I argue that economic science can indeed be regarded as normal science and that neoclassic theory is its paradigm. Section 4 concludes.

1 It should be noted, however, that this essay focuses on microeconomics. Even though I believe that extending the question to macroeconomics would not change the answer fundamentally, it would clearly exceed the boundaries of this paper.

II. The case for the applicability of Kuhnian terms

Before I turn to the question as to whether economics matches Kuhn's notion of normal science, another related, but more fundamental question has to be considered: to what extent is such an application of Kuhn's conception to economics, being a social science, even possible?

It might be doubted that the two questions can and should be dealt with separately. However, what we have to keep in mind is that a discipline can in principle match the picture Kuhn draws of the historical development of a science, but is currently not firmly governed by a paradigm and thus is not in the phase of normal science. Such a discipline could currently be in the pre-paradigmatic phase, i.e. approaching but not yet having reached the state of a mature science, or in the revolutionary phase, i.e. a time in which the recent paradigm has been abandoned by the scientific community but not yet replaced with a new one. Also, it might be the case that an application of Kuhnian terminology to economics is impossible in principle (e.g. economics might intrinsically be too different from the natural sciences), in which case the search for a paradigm guiding economic science would be futile.

Redman (1991: 151) contends that "none of the Kuhnian terminology is applicable" to economics. I will evaluate her arguments and argue that she overemphasizes the differences between economics and the natural sciences and thus fails to show that the application of Kuhnian terminology to economics is impossible.

First, Redman (1991: 151) notes that Kuhn in fact developed his conception of the structure of scientific revolutions in order to point out the major difference between the natural and the social sciences. She is certainly right in noting that Kuhn observed controversies about the very foundations of research in the social sciences which hardly occur in the natural sciences. However, it does not follow that Kuhnian terms can indeed never be applied to any of the social sciences.

Second, she argues that not even the Kuhnian term *preparadigmatic* should be applied to economics, since a social science cannot be expected to achieve something similar to the paradigms of physics that guide research and allow scientists to utter precise predictions (Redman 1991: 151). Again, she is perfectly right in noting that economics cannot be expected to be as precise and successful at prediction as the physical sciences for several reasons (cf. Rosenberg 1994, Rosenberg 2009). However, it does not follow that the attempt to find something similar to paradigms and revolutions in economics must be a fruitless enterprise. Kuhn himself did never claim that in every field of science the paradigm should lead to predictions as precise and successful as the ones of physics. In the *Postscript* to *The Structure* he contends that values concerning the accuracy, precision and quantifiability of predictions are usually part of a paradigm. He points out that

these "should be consistently satisfied in a given field" (Kuhn 1996: 185), but does not demand that they be satisfied consistently across all fields.

Third, Redman (1991: 151) argues that, while Kuhnian revolutions are usually put forward by young scientists or people who are new to the field, "an argument that the younger economists are better than the older ones necessarily stretches the imaginations" considering the amount of study that is necessary in order to become a good economist. However, even if Redman was right in stating that there is a difference between the natural sciences and economics in this respect, this would not be of sufficient significance to show that Kuhnian terms are not applicable to economics. Kuhn merely mentions that revolutionary scientists usually happen to be young because younger scientists are less committed to the paradigm, since they have spent less time researching within it (Kuhn 1996: 144). Hence, this property of revolutions and revolutionary scientists is an accidental rather than a necessary one.

Finally, Redman (1991: 151) argues that the Kuhnian terms *normal* and *revolutionary* should not be used to describe economics since change in economic thought is different to change in the natural sciences in two respects. First, theory change in economics is linked to political considerations. Second, a model in economics is usually not replaced but slightly modified. Both claims might be correct, but none of them is an argument sufficiently persuasive to show that Kuhn's conception is not applicable to economics.

First, with regard to the presumed difference concerning political considerations, it should be noted that Kuhn (1996: 69) himself mentions that in mature sciences "external factors […] are principally significant in determining the timing of breakdown, the ease with which it can be recognized, and the area in which, because it is given particular attention, the breakdown first occurs". There is no obvious reason why external factors having impact on scientific revolutions should not include political considerations.

Second, her claim that economic models are slightly modified rather than replaced also fails to establish that economic science is too different from the natural sciences as to render an application of Kuhnian terminology to economics impossible. One of Kuhn's central theses is that there are not only large-scale revolutions, but smaller ones as well (Kuhn 1996: 6 and 49). So a change in theory does not have to be a dramatic event similar in scale to the Copernican revolution in order to be regarded as a Kuhnian scientific revolution. Furthermore, an anomaly (e.g. the discovery of a new phenomenon) does not necessarily have to cause a revolution, but may be solved within the paradigm or by adjusting the paradigm's categories without replacing it completely (Kuhn 1996: 52-66). Therefore, even if such small changes of theory in economics are not to be regarded as scientific revolutions, it still does not follow that Kuhn's concepts do not apply.

In this section, I hope to have shown that Redman's argument fail to establish the inapplicability of Kuhnian terms to economics. Hence, I suggest having a closer look at the field in order to determine whether there is any such thing as a paradigm that would indicate that economics actually is to be regarded as normal science.

However, in search of a paradigm in economics, one should bear in mind some particular properties of economics as a social science. First, as Redman correctly notes, a paradigm in economics cannot be expected to be as successful and precise at prediction as the ones of the natural sciences. Second, it is to be expected that consensus in economics will not be as strong and unequivocal as in the natural sciences. Proper experiments for the test of economic theory are seldom available and, thus, economic theory is even less determined by observation than physical theory (cf. Friedman 1966). Finally, as both Redman (1991: 151) and De Vroey (1975) observe, factors external to pure scientific enterprise, such as political and social factors, may play a more important role in economics than in the natural sciences. Although these special properties, as I argued above, do not necessarily render the application of Kuhnian concepts impossible, they indicate that one should be careful to view economics as the perfect match for Kuhn's conception.

III. Why economics is normal science

In the previous section I have argued that an application of Kuhn's terms to economic science is possible. In this section I turn to the question entitling this essay: Is economics normal science? I argue that economics should in fact be considered normal science. Neoclassic theory should be regarded as economists' paradigm, even though not all researchers are equally committed to it. It seems that this lack of consensus is due to the special properties of the social sciences as opposed to the natural sciences.

Normal science, according to Kuhn (1996: 10), is firmly based on one or more paradigms, "achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice". A paradigm supplies problems it renders worthy of research, promises that these problems are soluble and at the same time provides strict rules regulating what their solution should look like. Most of normal scientific research consists in solving such puzzles and is not at all aimed at the discovery of major novelties, let alone the introduction of new theories. Paradigms enable scientific communities to engage in highly precise, progressive and efficient research that would not be possible if it was not for its strict guidance. Hence, a scientific community is in a phase of normal science if and only if its members share a paradigm.

But what does Kuhn's notion of the paradigm entail? What is it that scientists commit to when they commit to a paradigm? Unfortunately, Kuhn's concept of the paradigm is somewhat vague.

In the 1969 *Postscript* he admits to have used the term in different ways and consequently tries to clarify the concept. In *The Structure* as well as in the *Postscript* he mentions certain features that paradigms have in common.

First, it seems safe to say that a paradigm equips its scientific community with an own language. This language is not known to the layman, but only to the particular scientific community (Kuhn 1996: 20). It often includes symbolisations and definitions which are taken to be tautologies (Kuhn 1996: 183-84). This particular feature of paradigms enables a very efficient way of puzzle solving by allowing scientists to state problems in a familiar language such that they know how to approach them.

Second, a paradigm usually supplies certain metaphysic views of what entities the world consists in and how they are interrelated (Kuhn 1996: 184). Such a metaphysic guides research by providing a pattern for puzzles that can be solved within the paradigm. Kuhn's claim is that a scientist does not know what to look for, unless she is guided by an overarching theoretical framework that dictates what entities there are. Thus, this feature of the paradigm is crucial in enabling efficient puzzle-solving. Commitment to such a metaphysic can also be described as commitment to certain models (Kuhn 1996: 184).

Third, a paradigm usually gives rise to certain values and norms of accuracy (Kuhn 1996: 185). Specifically, a Kuhnian paradigm does not only supply certain puzzles to be solved, but also dictates what the solution should look like (Kuhn 1996: 38). Thus, a scientist's puzzle-solving attempts will only be accepted by the community if they accord with the paradigm's values and standards of method.

As a result of these features a paradigm enables a high degree of professionalisation. If it was not for the paradigm, scientists would constantly be arguing about the foundations of their field and therefore address competing scientists in order to convince them of their approaches (Kuhn 1996: 12-13). In contrast, once a paradigm is taken for granted highly specific literature addressing only the initiated scientific community arises. Researchers that do not accept the paradigm are ignored and no longer regarded as participants in the field (Kuhn 1996: 20). While the more advanced literature in the field rests on the paradigm, the paradigm itself is usually taught to students of the field through textbooks (Kuhn 1996: 10).

If economic science has a paradigm, what is it? Some authors argue that the dominant paradigm did not change throughout the history of the discipline and consists in the "theory of economic equilibrium via the market mechanism" (Coats 1969: 292). Others identify the marginalist revolution as a paradigm change and therefore distinguish between a classic and a neoclassic paradigm (cf. De Vroey 1975, Jalladeau 1978).

The bottom line is that the basic theory, assumptions and methods of the so-called neoclassic school, may they be the same as the classic's or not, are often identified as a Kuhnian paradigm (cf. Coats 1969, De Vroey 1975, Jalladeau 1978, Rosenberg 2009, Solo 1991). But what does neoclassic theory entail? Joël Jalladeau (1978: 601) outlines it as follows:

"The neoclassical model is an analytical system based upon subjective choices of individual economic agents. It is a network of exchange relationships linking separate primary units. The theoretical effort involves the concept of economic equilibrium, which means the economy is an interdependent system leading to a stable equilibrium, and it is the conditions by which this stability is realized that are studied."

Neoclassic economics, I take it, establishes what might be referred to as "mainstream economics"[2] (Solo 1991: 39), which is represented at most of the world's universities. But does neoclassic theory really supply the epistemological and sociological features of a Kuhnian paradigm?

First, with regard to the proprietary language Kuhn takes a paradigm to provide its scientific community with, Rosenberg (2009: 55) argues that neoclassic economic theory has a language of its own, one that is highly mathematical. Before one can really understand the models of mainstream economics, one has to acquire an education in advanced mathematics, thus learning economics' language (Rosenberg 2009: 55, Solo 1991: 40). The mathematical apparatus of neoclassic theory enables mainstream economists to formulate problems in the discipline's language such that they come in a familiar structure. Once a problem is sufficiently formalized, they know how and where to look for a solution to the puzzle and what that solution should look like. The mathematic language of neoclassic theory thus clearly counts as a reason to consider it a paradigm.

Second, Rosenberg (2009: 56) contends that "the discipline has identifiable proprietary laws, albeit inexact ones, and a set of proprietary kinds". What he refers to is the kind of metaphysic views that has been outlined above, although in the case of economics the formulation "beliefs in particular models" (Kuhn 1996: 184) seems more appropriate. Concepts such as supply, demand and equilibrium and their interrelations enable scientists to apply neoclassic theory to more and more puzzles, which leads to the phenomenon often referred to as "economic imperialism" (Rosenberg 2009: 56). Similarly, Friedman (1966: 7) refers to an "analytical filing system" that allows economists to subsume phenomena under fixed categories.

Third, with regards to paradigms' values of accuracy, a solution to an economic problem will

2 In this essay the terms "neoclassic economics", "the neoclassic school" and "mainstream economics" will be used interchangeably. Instead of "the basic assumptions, method and theory of neoclassic economics" I will often simply write "neoclassic theory".

hardly be accepted unless it is sufficiently formalized and comes in the form of a mathematical proof (cf. Jalladeau 1978, Blaug 1984). Moreover, as Jalladeau (1978: 602) notes, economists are not only able to apply neoclassic concepts to problems, but they have to do so. Likewise, Rosenberg (2009: 56) speaks of "the discipline's 'discipline'" requiring that the proprietary kinds and laws of neoclassic theory be applied to the solution of puzzles.

Finally, economics is a highly professionalized discipline. To begin with, Rosenberg (2009: 55) emphasises the uniformity of the field's largest selling textbooks. What is also striking about economics textbooks is that, apart from the fact that they are indeed very similar to each other, they supply the basic theory and assumptions of neoclassic economics. Mankiw's best-selling textbook *Principles of Economics*, for instance, in its very introduction provides a catalogue of the so-called "Ten Principles of Economics" (Mankiw 2008: 3). They include statements, such as "Rational people think at the margin", "Trade can make everyone better off" and "Markets are usually a good way to organize economic activity", which clearly reflect the neoclassic research tradition. Moreover, it seems that neoclassic economists often equate neoclassic thought with economists' way of thinking in general and, thus, do not even regard writers that are not committed to neoclassic theory as economists. Again, consider Mankiw's *Principles* implicitly regarding something as the principles of all economics which, say, a Marxist would object to. Finally, neoclassic economists also do not write books addressing competing schools trying to convince them of their research foundations. By contrast, there is a huge apparatus of highly specific journals for mainstream economics (Redman 1991: 163).

Apparently, neoclassic theory has striking features of a Kuhnian paradigm. However, one could object that not all economists are neoclassic economists. Redman (1991: 151) argues that "there is no paradigm [...] that is unquestioned by all economists." As Coats (1969: 292) notes, there has always been and still is a variety of heterodox schools of economic thought including, for instance, socialist or institutionalist approaches. Considering that Kuhn (1996: 170) denied the possibility of several coexisting paradigms in a mature science that has crossed the border from the pre-paradigm stage to normal science, from the variety of schools in economics one may be tempted to conclude that neoclassic economics is not in a phase of normal science.

However, Solo (1991: 39) argues that the neoclassic school of economics has reached a far wider scope than any other social science or alternative approach to economics. Considering the differences between the social and the natural sciences that have been outlined in the previous section it was to be expected that consensus would not be as strong in economics as in the sciences Kuhn initially described. It might be that the consensus and scope that neoclassic theory has reached in economics is the status closest to that of natural scientific paradigms a theory in the social sciences can possibly reach. Hence, neoclassic theory might have reached some sort of a paradigm-like status tailored to the social sciences.

Furthermore, since Kuhnian terms may be applied to economics, we may raise the question of which term suits best. Now, if economics was not normal science, according to Kuhn's scheme, we would have to regard it either as an intellectual discipline in the pre-paradigmatic stage or as a science in the middle of a revolution. The latter cannot be the case since the heterodox schools competing with neoclassic economics did not just pop up, but partly are even older than the latter and never replaced it (Coats 1969: 292). Considering neoclassic economics' efficient ways of puzzle solving and high degree of professionalisation it does not seem plausible that economics should be in the pre-paradigmatic stage either. So, if economics is neither a field of discovery in the pre-paradigm stage, nor a once paradigm-based science in a revolutionary phase, it should be regarded as being in the stage of paradigm-based normal science, although there are heterodox schools.

In this section I have argued that economics should in fact be regarded as normal science. Neoclassic theory provides economists with their own mathematic language, proprietary concepts and laws and values of accuracy. Thereby it enables highly efficient puzzle-solving and a high degree of professionalisation. Thus, although Redman is partly right in stating that there is no paradigm that all economists take for granted, there are good reasons to consider neoclassic theory a paradigm. The fact that consensus in economics is weaker than in the natural sciences seems to be grounded in the very nature of social sciences.

IV. Conclusion

In section 2, I have argued that the application of Kuhnian terms to economics is possible, even though these terms were not designed to describe the social sciences. Certainly there are differences between economics and the natural sciences that should be taken into account, but this is not a sufficient reason to give up on the enterprise completely.

In section 3, I turned to the question if economics is in fact normal science. It turned out that if economics is normal science there should be a paradigm that economists take for granted. I argued that neoclassic theory should be regarded as economics' paradigm, since it provides economists with an own, mathematical language, proprietary concepts and laws, and values of accuracy. Since these features enable efficient ways of puzzle-solving and a high degree of professionalisation, economics should be regarded as normal science. It seems that the fact that not all researchers in the field are committed to the neoclassic paradigm is due to the very nature of the social sciences.

Therefore, economics should be regarded as normal science, even though its paradigm is not as widely shared as the paradigms in the natural sciences.

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