Popper as Austrian economist: Equilibrium process vs. equilibrium attainment

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When I was a graduate student studying for a PhD in mathematical economics, everyone seemed to be deeply concerned that economic models must be testable. At about the same time my philosophy of science teacher, Joseph Agassi, introduced me to Karl Popper's theory of science where testability seemed to play a central role. In Popper's famous *Logic of Scientific Discovery*, he even went as far as talking about degrees of testability. To me this seemed to suggest immediately that economic modeling could be critically examined by applying Popper's degrees of testability to commonplace models used in the economics of the day. I was able to show that even the simplest models would require an unrealistic number of observations to generate *even one* potential counter-example. While we all can understand why a PhD thesis from an obscure economics program might be ignored, this result was also ignored when it was published in my 1989 book. While that was one of my best selling books and it elicited many comments on other aspects of model building, there has been no published mention of my critique of the practical significance of tests of ordinary economic models.

Eventually I came to understand the silence and why the results of my PhD thesis do not matter. The commonplace discussion of testability had nothing to do with Popper or even with Terrence Hutchison who is claimed to have introduced economists to Popper's theory of science in 1938. But actually, in economics testability was instead only promoted as a defense against the critics of the mathematization of economics in the 1930s and 40s. The main promoter of mathematics and thus testability in economics was the 1970 Nobel Prize laureate Paul Samuelson. He was eager to show that his mathematical models were testable – or as he said, they were "operationally meaningful". I suspect he merely thought he was thereby proving that mathematical economics did not produce only tautologies as was claimed by early critics of mathematical model building.

By the mid-1970s I had dropped my interest in applying Popper as an exercise in philosophical methodology and instead began looking at the assumptions made by economic theorists and model builders when they characterize the individual decision maker. I was inspired by Friedrich Hayek's famous 1937 article, "Economics and knowledge", which raised a question about the completeness of such a model whenever it did not explain how the individual decision maker "acquired" the knowledge needed to make the successful maximizing decision presumed in economic models. But I faced a huge obstacle when at this point I tried to apply Popper's theory of knowledge to ordinary economics: namely, almost everything discussed in mainstream economics is about the logically necessary conditions for the existence of a state of equilibrium. In part, but primarily, this is due to the fact that mathematical models of a *dis*equilibrium are very complicated and not fruitful when it comes to useful necessary conditions. At minimum, a disequilibrium model must recognize dynamics or at least the possibility of change.

Mainstream economists see this differently. I, however, had taken a graduate course about economic dynamics taught by Hans Brems and so I was aware of a central theoretical problem facing mainstream economists. That problem, as they would see it, is concerned with how change and dynamics could be dealt with while at the same time relying on equilibrium-based explanations. Of course, this would be a problem primarily for those economists inclined to see theory development as a domain where only mathematical model building is appropriate.

In the 1970s I became aware that the Austrian school of economics considered change a central feature of market-oriented economics but I was also aware that many of the Austrians claimed to have little use for equilibrium-based explanations. Having read Hayek's famous 1933 Copenhagen lecture, it also seemed obvious to me that – with his 1937 article also in mind – the key shortcoming of equilibrium-based models is that they did not explain how decision makers came to know what they needed to know in order to guarantee the attainment of any presumed equilibrium.

Hayek and some of his Austrian colleagues, particularly Ludwig Lachmann, were also concerned with the limitations imposed by assuming the existence of a state of equilibrium – although recognizing, of course, that such an assumption is still necessary for most mainstream economics arguments. The Austrians' concern often focused on capital as a factor of production – where, for Austrians, capital is

always about machines or factories, that is, real things. Specifically, such capital always takes time to produce. In Hayek's 1933 lecture, he had also noted how particularly important knowledge was whenever one is trying to explain a capital investment market. His focus on knowledge meant that while the market for capital investment might be in equilibrium today, today's market equilibrium depends on what we know about the future. But, knowledge of the future could easily be faulty – particularly in the market where orders are placed for new capital to be produced and its production takes time. Given the possibility of such faulty knowledge, it is clear that even when there is an equilibrium in today's market for new capital, if at the later time when that capital is delivered it turns out to be the wrong amount for the state of that later economy or that later market, the usefulness of any equilibrium-based explanation of today's economy would *be* at the least questionable. So, it seemed obvious to me that time and knowledge are intertwined and both must be dealt with if a realistic model of the economy were to be constructed.

Given the importance of recognizing knowledge and its role in economics – as the Austrians rightfully advocate – I thought this was fertile ground for Popper's view of knowledge as well as his theory of science – particularly, his implicit focus on science as a process rather than as a state of attainment. So, based on this perspective, in 1978 I published an article that critically examined how time is dealt with in mainstream economics. My article was along the lines suggested by the related works of Sir John Hicks. Hicks saw the issue not as a problem of putting time in economics but instead putting economics *in* time by which he meant in real, irreversible time – the same notion of time that the physicist Arthur Eddington had promoted at the beginning of the 20th Century. In particular, Hicks thought when time is properly recognized in economic models, it should be fundamentally irreversible rather than be just a parameter or coefficient. I called this the *Hayek Problem* since satisfying Hicks seemed to necessitate following Hayek by recognizing knowledge – and particularly recognizing that learning is a dynamic process as well as an irreversible one. To me that was the key issue.

Now, the challenge for economic theorists is, and has always been, to provide explanations that deal with equilibrium as a process rather than equilibrium as an attained static state. The theorist's general problem of explaining change[1] in the context of rational decision-making is that the decision-maker's knowledge[2] is hopelessly static. The real source of the difficulty is not that our knowledge itself is static, but rather that in economics the traditional *views of knowledge* assert that knowledge is static. I argued that there is not necessarily a problem with rational decision-making, except when either the individual's knowledge or its acquisition is presumed to be exogenously given.

Parenthetically, it is important to recognize here that according to Popper's theory, science is analogously seen as a process rather than an attained state – specifically, science is a process where truth status matters rather than one where science is seen as the successful embodiment of proven-true theories.

Now, the methodological problem with equilibrium models is that they, too, presume all decision makers are successful in achieving their desired state – namely, that they have obtained the right amount of capital for the current situation or the right number of tomatoes for tonight's dinner. What Hayek leads us to question is what we think the individual decision makers do whenever they fail to achieve their desired state of affairs. For example, if you were a supplier of tomatoes, first you would have to plant and then pick the tomatoes before going to the market. So, how many would you plant or pick? Your answer would depend on what you expect the price will be at the market where you plan to sell them. Second, if the price subsequently turned out to be different than what you expected and hence your supply quantity would not be optimum, what would you do? I have argued that in such a case the decision maker's answer depends on his or her theory of knowledge and learning. Moreover, it is also the case that the economic theorists' explanations must depend on their own views of knowledge and its role in explaining the market process. And this would seem to me to open the door for applying Popper's theory of science and learning at exactly the point where the economic theorists have to make their assumption about the dynamic behaviour of the individual decision makers. This is even more important when we recognize Hayek's 1933 view that for an equilibrium economy as a whole, there would be evidence of a day-to-day change *only if* there were systematic errors or disappointed expectations[3].

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[1] or dynamics.

[2] of his or her situation.

[3] otherwise, errors might cancel out.

To be specific, for years I have been arguing that we should recognize that any decision maker could be a conventionalist, or an inductivist, or a skeptic, or even an instrumentalist. Each implies a different response to discovering one's error in expectations. A different response leads to a different type of dynamics. Some types of dynamics are compatible with equilibrium attainment, others are not.

Now, I can understand why few, if any, economists might be eager to jump onto my Popperian bandwagon; the reason is that few economic model builders know much about theories of knowledge. Almost every economic theorist today thinks all learning is inductive. But of course, were economic model builders to read a little of Popper or even David Hume, they might be cured of this shortcoming.

Again, the main issue I have been concerned with is how it is possible *in an equilibrium process* to maintain what economists call rational decision making – by which they only mean that the decision maker is a maximizer. That is, can we conceive of a decision maker responding to a discovered error in a rational or maximizing manner? I called this the *Problem of Rational Dynamics*. My proposed solution to this problem involves an application of Popper's theory of science and learning to Hayek's situational dynamics. It consists of two key parts of Popper's epistemology: his anti-justificationism (namely, that all knowledge is theoretical and hence conjectural and thus fallible) *and* his anti-psychologism (namely, that all knowledge is potentially objective – that is, one can write it on the blackboard). I applied both parts to Hayek's and Hicks' theories of rational decision making[4] and I applied them as well to Hayek's situational dynamics[5].

Perhaps I should finish by briefly demonstrating how my Popper-Hayek formulation solves the problem of explaining an equilibrium process – and how it does so while still allowing for a completion of the neoclassical program for explaining any market economy. The key issue is somewhat technical and concerns what economic theorists of the 1950s and '60s called the *Stability Problem*. Briefly stated, it says that characterizing a market equilibrium as simply the equality of demand and supply is misleading. What is needed for an equilibrium to be obtained, or as they say, for an equilibrium to exist, is some dynamics that will necessarily lead to that equilibrium.

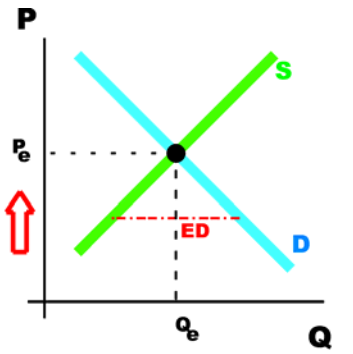

Fig 1: Stable market

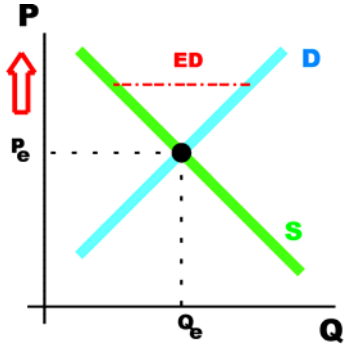

Fig 2: Unstable market

To illustrate, consider the economist's commonly presumed notion that whenever a buyer has an excess of demand over what is supplied, he or she can simply offer a higher price. Now, for this behaviour to work it must be the case that as the price rises the excess demand must decrease (see Figure 1). This is the case, of course, whenever all markets' demand curves are negatively sloped and all supply curves are positively sloped. When this is the case, the usual market equilibrium will be obtained and thus is evidently stable. But unfortunately, economists have never been able to prove that all demand and supply curves are sloped as required for this stability. Thus the critical question is: what would happen (to take an illustrative extreme case) were the demand curves to be positively sloped and supply's negatively sloped?[6] In that case (see Figure 2), a disappointed demander's bidding the price up would make things worse since the resulting excess demand would grow, not decrease – that is, the market price would in

[4] which thereby must include assumptions about the decision maker's own theory of knowledge and learning.

[5] which allows for changes in the decision maker's knowledge as well as changes in the objective situation facing the decision maker.

[6] Or when the demand's slope is more negatively sloped than a negatively sloped supply curve or less positively sloped than a positively sloped one (see Figure 3 on the last page).

effect blow up. If one is going to advocate that markets rather than governments should be relied on to guide society, stable markets must be assured, that is, the Stability Problem must be solved rather than hidden in the closet.

Now, I think the Stability Problem will never be solved until economic theorists give up the success-orientation of their equilibrium models. And this is where Popper with the help of Hayek comes to the rescue. Since knowledge is required *and is fallible*, the individual decision makers never know for sure whether they are actually maximizing and thus there will always be someone testing their market decision. If this is always so, then we know that any potentially unstable market would have long ago blown up and thus would not have to be considered possible. And thus, in all markets that continue to exist, bidding the price up would always cause the excess demand to decrease. In other words, by recognizing Popper's idea of learning by testing, the economic theorists' Stability Problem would be circumvented.

Obviously I do not have the time here to explain the details of how all this works but if you are interested, I invite you to download from my web page the PDF of my 1986 book in which I apply Popper's theory of science to the troublesome *Stability Problem*. And for a discussion of my Popperian solution to the *Problem of Rational Dynamics*, you can also download the PDF for my 1982 book (or if you are really wealthy, you could instead buy its 2003 second edition – titled *The Foundation of Economic Methodology: A Popperian Perspective* – it provides a more up-to-date view).

References

Boland, L. [1978] Time in economics vs. economics in time: the 'Hayek Problem', *Canadian Journal of Economics, 11*, 240–62

Boland, L. [1982] *The Foundations of Economic Method* (London: Geo. Allen & Unwin)

Boland, L. [1986] *Methodology for a New Microeconomics* (Boston: Allen & Unwin)

Boland, L. [1989] *The Methodology of Economic Model Building: Methodology after Samuelson* (London: Routledge)

Boland, L. [2003] *The Foundation of Economic Methodology: A Popperian Perspective* (London: Routledge)

Eddington, A. [1928/58] *The Nature of the Physical World* (Cambridge: Cambridge Univ. Press)

Hayek, F. [1933/39] Price expectations, monetary disturbances and malinvestments, in *Profits, Interest and Investments* (London: Routledge)

Hayek, F. [1937/48] Economics and knowledge, *Economica*, 4 (NS), 33–54, reprinted in Hayek [48], 33–56

Hicks, J. [1976] Some questions of time *in* economics, in A. Tang, F. Westfield and J. Worley (eds), *Evolution, Welfare and Time in Economics* (Toronto: Heath), 135–51

Hutchison, T. [1938] *The Significance and Basic Postulates of Economic Theory* (London: Macmillan)

Popper, K. [1934/59] *Logic of Scientific Discovery* (New York: Science Editions)

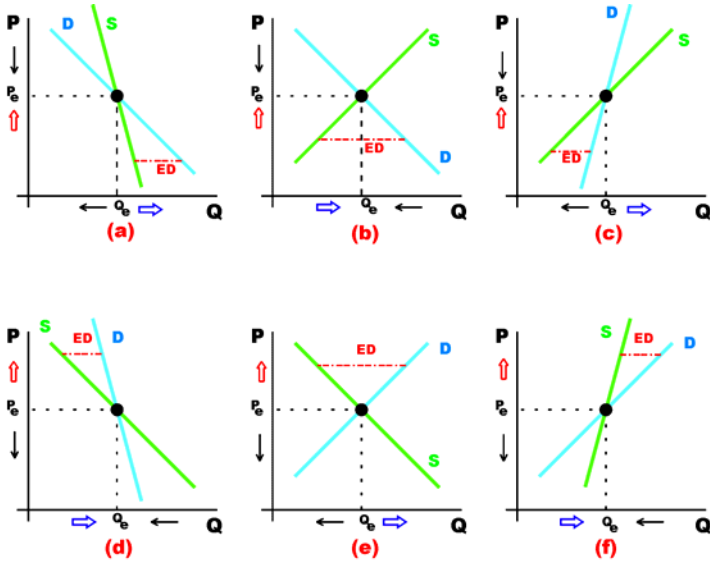

Fig 3: Six distinctive market configurations