

Economic theory and the crisis^{*}

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“A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.”

– Max Planck, A Scientific Autobiography (1949)

The first thing that comes to mind as one follows the debate among economists about the crisis is that economic theory was locked into a bubble that has now burst. The reactions have either been to ignore this and just to wait for the crisis to pass or to herald the return of one of our old heroes, Keynes. Economists seem to be victims of extremely short memories and an inability to anticipate the next theoretical development. We periodically come to believe that we have hit upon the “right model” and that all previous efforts can be consigned to the wastebasket of history. When the current model turns out to be completely at odds with reality, the reflex reaction is to go back to the previous model and to chide the modernists for having lost sight of it. A number of economists have tried to add a little historical perspective (in particular [Reinhart](#) and [Rogoff](#) (2008) have argued that this crisis is [but one of many similar events](#)), but the debate overall remains very short-sighted and ideologically motivated.

All of this seems to be misplaced. Suppose we accept that economic theory, like the economy, is a complex adaptive system. We should then expect to see it continually evolving to take into account both new theoretical insights and the evolution of the economy itself. We will not see theory evolving into a given model that more closely represents the economy since the economy itself is changing. However, we might expect theory to evolve to at least be able to envisage the occurrence of the major crises that periodically shake the economy, and this is where the problem with the response that “we have seen all this before” arises. If we believe that such crises are an inherent feature of the evolution of economies, then surely we should develop models that incorporate them. We might then avoid the usual habit of falling back on the standard equilibrium notions and claim that some major exogenous shock has hit the system. The latter rarely identifies the shock, and it is now widely recognised that almost every significant turning point in all of the major stock price indices was accompanied by no notable news, and hence no shock at all.

A much more reasonable approach would be to accept that these large and abrupt movements are due to the endogenous dynamics of the system. What has become the standard macroeconomic model, DGSE, is justified by its proponents on the grounds that it has more “scientific” foundations than its predecessors. By this they mean that it is based on rational, maximising individuals. But there are two problems with this.

Equilibrium

Firstly, we have known since the mid-1970s that aggregating the behaviour of lots of rational individuals will not necessarily lead to behaviour consistent with that of some “representative agent”. The well-known Sonnenschein-Mantel-Debreu results show this and undermine the foundations of macroeconomics in general. Despite our heroic assumptions on the rationality of individuals, we can guarantee neither the uniqueness nor the stability of equilibria.

These are not just technical problems. If you cannot show that economies or markets will ever reach equilibrium, then the latter becomes a purely intellectual artefact. If you cannot guarantee the uniqueness of equilibria, then the usual exercise of studying the impact of some policy measure, i.e. comparative statics, makes no sense. The reason for resorting to the “representative agent” becomes obvious. In the one-person economy, there is a unique and stable equilibrium! However, this raises yet another problem. Even if we can construct a representative agent whose excess demand corresponds to the average excess demand of all the economic agents, it may be the case, as Mike Jerison (1997) pointed out, that those preferences contradict those of the population supposedly represented. Thus we can make no welfare judgements on the basis of that agent’s utility (for a discussion of the weaknesses of the representative agent approach see Kirman 1992).

Theory vs. evidence

All of this assumes that we accept the standard axioms of rationality that lead to the second problem. The axioms that are used to define “rationality” are based on the introspection of economists and not on the observed behaviour of individuals. Economists from Pareto through Hicks to Koopmans have long made this point. Thus we have wound up in the weird position of developing models that unjustifiably claim to be scientific because they are based on the idea that the economy behaves like a rational individual, when behavioural economics provides a wealth of evidence showing that the rationality in question has little or nothing to do with how people behave.

Why do I say we do not look back far enough? Consider the efficient markets hypothesis, which has ruled the roost for some years in finance. Its originator was, by common accord, Louis Bachelier, who developed the notion of Brownian motion at the turn of the twentieth century. His argument that stock prices should follow this sort of stochastic process, after years of being ignored, was acclaimed by economists both for analytic and ideological reasons. Yet, shortly after having written his report on Bachelier’s thesis, the great French mathematician Henri Poincaré (1905) observed that it would not be sensible to take this model as a basis for analysing financial markets. As he said, individuals who are close to each other, as they are in a market, do not take independent decisions – they watch each other and “herd”. Thus Poincaré clearly envisaged one of the most prevalent features of financial markets long before modern economists took this theme up to explain “excess volatility”.

And still, theory continued on its way, ignoring both the empirical evidence and theoretical criticisms, not just once but twice. The first of these was the development of optimal portfolio theory by Markowitz (1952), who developed his theory using the assumption that the changes in returns on assets had a Gaussian distribution. Despite the empirical evidence and the pleas of Mandelbrot and others, this assumption prevailed, since one could apply the central limit theorem to it, unlike the family of Levy stable distributions favoured by Mandelbrot. The same thing applies to the development of Black-Scholes (1973) option pricing. This again relies on the refutable and often-refuted assumption that the price of an asset follows a lognormal process. Theory ploughed ahead ignoring its own weaknesses, despite the criticisms of many mathematicians and economists.

With regard to general equilibrium models, which Walras pioneered, the same Poincaré wrote to Walras and chided him for his assumptions of infinite farsightedness and infinite selfishness. The latter he could believe at a pinch, but the former he found dubious to say the least. Yet, while in other areas of economics we have moved on from these assumptions, we are still faced today with macroeconomic models based on general equilibrium models, in which these two assumptions are central.

Thus both the development of the DSGE model and the evolution of the efficient market hypothesis share a common feature – despite the empirical evidence and despite their theoretical weaknesses, their development proceeded as if the criticism did not exist.

The future of economic theory

Why are we so reluctant to envisage different models and different tools? As somebody said, we economists went through the twentieth century developing and perfecting a model based on nineteenth-century physics, so perhaps in the twenty-first century we could move on to a model based on twentieth-century physics. But as [Paul Krugman](#) has pointed out, the vested interests are strong and to ask economists to take up a new set of tools is probably asking too much. To discard equilibrium in the standard sense and study out-of-equilibrium dynamics is perhaps too big a step.

No doubt it will eventually come, and undergraduates will study systems of interacting agents rather than *homo economicus*. Hans Foellmer (1974), in a pioneering paper, made the point that we cannot ignore the direct interaction between individuals and its influence on preferences. Yet he was, and still is, too far ahead of his time. For the moment, placing externalities, the influence of one person's actions on another, at the centre of the action rather than regarding them as “imperfections” in our equilibrium model is a necessary step. Then, if we argue that the interaction between individuals is important, we have to specify the structure of that interaction. This means that we have to study the structure and fragility of the networks that govern the interaction between individuals and make this central in our analysis and not just a peripheral, albeit fascinating, topic. I have suggested how we might turn our minds to the idea of the economy as an interactive system and look at the problems just mentioned in Kirman (2009).

Such changes are essential if we are to progress, but the inertia in the economics profession is strong and whilst the economy has shown that it is capable of sliding rapidly into a new phase, economists may well organise to prevent this from happening in the immediate future. But in the end, we will move forward, as Max Planck said.

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