CHAPTER EIGHTEEN

Back to the Drawing Board for Macroeconomics

By Paul Ormerod and Dirk Helbing

Introduction and Background

In November 2010, Jean-Claude Trichet, then governor of the European Central Bank, opened the Bank's flagship annual Central Banking Conference. He challenged the scientific community to develop radically new approaches to understanding the economy:

> When the crisis came, the serious limitations of existing economic and financial models immediately became apparent. Macro models failed to predict the crisis and seemed incapable of explaining what was happening to the economy in a convincing manner. As a policymaker during the crisis, I found the available models of limited help. In fact, I would go further: in the face of the crisis, we felt abandoned by conventional tools.

Trichet went on to say:

> We need to develop complementary tools to improve the robustness of our overall framework. In this context, I would very much welcome inspiration from other disciplines: physics, engineering, psychology, biology. Bringing experts from these fields together with economists and central bankers is potentially very creative and valuable. Scientists have developed sophisticated tools for analysing complex dynamic systems in a rigorous way. These models have proved helpful in understanding many important but complex phenomena: epidemics, weather patterns, crowd psychology, magnetic fields.

It is instructive, in the light of this, to consider how the US authorities reacted to the financial crisis after the collapse of Lehman Brothers in September 2008. During the Great Depression of the 1930s, the cumulative fall in real gross domestic product was some 27%. The previous peak level of output, in 1929, was not regained until 1939, a whole decade later. The recent recession has been the most serious in peacetime for all Western economies since the second half of the nineteenth century (Ormerod 2010a). So far, the falls in output since 2008 that have come about as a result of the financial crisis have been less devastating. In the United States, output fell some 3% and has already risen above its previous peak level. In the major European economies, gross domestic product fell by around 6%. The strength of the recovery in these countries has not generally been as strong as the recovery in the United States, but even so the experience has been markedly better than it was in the 1930s.

However, during the current financial crisis, the authorities did not make use of the dominant intellectual concept in academic macroeconomics of the last thirty years. This concept started off as real business cycle theory (Kydland and Prescott 1982), and was then developed into dynamic stochastic general equilibrium (DSGE) theory. (For a description of the scientific state of the art when the financial crisis began, see, for example, Tovar (2009).)

Fortunately—and this illustrates the role of chance and contingency in human affairs—Ben Bernanke, governor of the Federal Reserve, was an expert not in DSGE models but in the economic history of the Great Depression itself. He and his colleagues operated under conditions of great uncertainty, and essentially they tried to avoid the key mistakes made during the early 1930s in the United States. They maintained an expansionary monetary policy, in contrast to the contractions of the Great Depression, and defended the banks. As previously described (Ormerod 2010b), in the crucial days following the collapse of Lehman Brothers, they took a number of purely administrative measures.

- They nationalized the main mortgage companies, Fannie Mae and Freddie Mac.
- They effectively nationalized the giant insurance company AIG.
- They eliminated investment banks.
- They forced mergers of giant retail banks.
- They guaranteed money-market funds.

This last action, although it has received little publicity, was possibly the single most important act carried out by the authorities, averting an immediate and catastrophic liquidity crisis. Its impact was probably even greater than that of the $700 billion Troubled Asset Relief Programme, which was played out in the full light of the democratic spotlight.

One immediate implication of the above is the pressing need to reintroduce economic history into the teaching of economics at all levels.

Although the world has seen many financial crises in its history (Reinhart and Rogoff 2009), there have been only two such global crises over the past century: in the early 1930s and the late 2000s. The Maddison database (Maddison 1995) of seventeen leading Western economies contains annual data on real gross domestic product going back to 1870. The only years when almost all these countries were simultaneously in recession (that is, when the growth in real gross domestic product was less than zero) were 1930–32 and 2009.

As noted above, an awareness of economic history rather than of DSGE models enabled the authorities to muddle through the latest crisis. Even after all this time economists have not arrived at a consensus on the precise cause of the Great Depression, but our knowledge about it is not zero.

All students of economics would benefit from studying certain key episodes in economic history. In addition to the Great Depression, the oil shock crisis of the 1970s and the contrasting transitions to peacetime after the two world wars are examples that spring readily to mind. The period after the First World War was characterized by deep recessions in some countries. Serious problems with both international trade and the world monetary system persisted throughout the interwar period. In contrast, output after the Second World War recovered very rapidly, even in the defeated countries, and a long period of unparalleled growth followed. During this period, inflation rates in the West were close to each other. However, in the mid 1970s there was a rapid and dramatic widening of the differences between them. In 1975, for example, the inflation rate in Germany was only 4%, but it was above 20% in both Italy and the United Kingdom. The analysis of such formative periods raises many important questions in economic theory.

With this general background comment in mind, the second section of this chapter briefly considers existing theory in macroeconomics, and specifically theories of the business cycle. The third section sets out what we regard as the key intellectual concepts that are needed to rebuild macroeconomics. The final section offers a conclusion.

Business Cycle Theory

There are two features that distinguish capitalism from all previously existing forms of social and economic organization. First, there is slow but steady real economic growth over time. Second, there are persistent fluctuations around the underlying rate of expansion. It is not our concern in this paper to discuss theories of economic growth except to note that economics does not really have a satisfactory theory to explain this phenomenon. Our focus is instead on the fluctuations in growth: so-called business cycles.

A recent theme in discussions about redesigning the teaching of macroeconomics is that of the need for students to know, in addition to economic history, the history of economic thought. Difficult problems have been addressed by great scholars in the past, and it is argued that students should be familiar with these efforts. It is hard to disagree completely with such an argument, if only because the current generation in any science, or would-be science, stands on the shoulders of the giants of the past. It would certainly be helpful for students to be aware of the work of business cycle theorists such as Marx, Keynes, Hayek and Schumpeter, all of whom can be regarded as working in theoretical frameworks in which in practice the economy spends most of its time out of equilibrium, even if an equilibrium exists in principle.

However, in our opinion too much time could be spent on the study of economic thought. Students of physics need to learn Newtonian physics at some point because of the very powerful empirical evidence that supports such theories (within the appropriate settings). However, such matching of theory with empirical evidence is very much lacking in macroeconomic theories of the business cycle.

There are several key features of real gross domestic product growth that a scientific model should be able to replicate: for example, the autocorrelation function of the time series and its Fourier transform pair, the power spectrum. Both the duration of recessions and their cumulative sizes follow highly non-Gaussian distributions, as does the time period between recessions. These are obvious examples of so-called stylized facts that a genuinely scientific model ought to be able to explain.

Macroeconomics needs to become an empirically based science that is able to explain fundamental features of the economy. A glaring example of the lack of progress made in this area is the continuing lack of consensus on the size of the fiscal multiplier: a fundamental concept in macroeconomic theory. In his *General Theory* of 1936, Keynes hazarded the opinion that for the UK economy, the eventual increase in real gross domestic product following a debt-financed expansion of public expenditure would be between two and three times the initial stimulus. However, the openness of the current UK economy (as well as of other European economies) and the associated marginal propensity to import mean that the multiplier is very unlikely to be so high. Indeed, the first systematic comparison of macroeconometric models of the United Kingdom, which was carried out over thirty years ago (Laury *et al.* 1978), obtained estimates from three different models of between 0.5 and 1.2.

Even now, there are huge differences in the literature on the estimated size of this basic macroeconomic concept. Ramey argues that the multiplier in the United States, which is of course a much more closed economy in trade terms than any individual European economy, is between 0.8 and 1.5 (Ramey 2011). However, Barro and Redlick (2011) argue that multipliers for non-defence purchases cannot even be reliably estimated at all because of a lack of suitable instruments. They conclude:

> The estimated multiplier for defense spending is 0.6–0.7 at the median unemployment rate. There is some evidence that this multiplier rises with the extent of economic slack and reaches 1.0 when the unemployment rate is around 12%. Since the defense-spending multiplier is typically less than one, greater spending tends to crowd out other components of GDP [gross domestic product].

It is not our purpose to adjudicate in any way on this debate. We simply note that, some eighty years after the concept was first developed, empirical estimates of the fiscal multiplier, a fundamental concept taught to all levels of students, vary enormously.

Of course the current dominant concept in macroeconomic theory—that of DSGE models—has an altogether different intellectual pedigree. The numerical solution of such models is itself a non-trivial task.[1] This in itself is not necessarily an objection to such models, but we note that the maximum number of agents considered in the referenced special issue was as low as ten. As the number of agents in the model grows, the dimension of the state space expands, and forming expectations entails integrating over a higher-dimensional set of shocks. This does seem to limit the degree of heterogeneity that can currently be captured by these models.

This is not the place to carry out a detailed critique of such models. It is, however, important to note one point. The intellectual pedigree of DSGE models goes back to the famous Lucas critique of macroeconometric models (Lucas 1976). Lucas argued that policy evaluation (for example, using the fiscal multiplier estimate as discussed earlier) in such models could be very misleading, because the parameters of such models might themselves not be invariant to the policies carried out. One of the key features of the models of the 'new macroeconomics' was held to be that the

[1] See, for example, the 2011 special issue of the *Journal of Economic Dynamics and Control* that was edited by R. Wouter, J. Den Haan, K. L. Judd and M. Juillard.

parameters that characterize the preferences of a representative agent and the production technologies of a representative firm as well as the exogenous structural shocks are policy invariant. However, Chang, Kim and Schorfheide have recently shown that this is not necessarily true (Chang *et al.* 2011).

Olivier Blanchard, chief economist at the International Monetary Fund, expressed a change of mind about such models as a result of the financial crisis. In an MIT working paper released in August 2008 (Blanchard 2008), merely weeks before the collapse of Lehman Brothers, Blanchard argued that:

> For a long while after the explosion of macroeconomics in the 1970s, the field looked like a battlefield. Over time, however, largely because facts do not go away, a largely shared vision both of fluctuations and of methodology has emerged.... The state of macro is good.

This echoed Lucas's 2003 presidential address to the American Economic Association, when he claimed that 'the central problem of depression-prevention has been solved'. Blanchard went on to say:

> DSGE models have become ubiquitous. Dozens of teams of researchers are involved in their construction. Nearly every central bank has one, or wants to have one. They are used to evaluate policy rules, to do conditional forecasting, or even sometimes to do actual forecasting.... Macroeconomics is going through a period of great progress.

However, within a matter of months, Blanchard changed his opinions dramatically. In another MIT working paper, written in January 2009 (Blanchard 2009), he identified four main reasons for the crisis.

- Assets were created, sold and bought that appeared much less risky than they truly were.
- Securitization led to complex and hard-to-value assets being on the balance sheets of financial institutions.
- Securitization and globalization led to increasing connectedness between financial institutions, both within and across countries.
- Leverage increased.

We now move on to consider ways in which macroeconomics might address such issues.

Rebuilding Economics

We are not suggesting that standard economics be replaced completely. For example, the concept that agents alter their behaviour when incentives change appears to be a universal law of behaviour in the social sciences.

However, a fundamental shift in the underlying view of economics is needed. We live now in a densely networked, strongly coupled and largely interdependent world, which behaves completely differently from a system of independently optimizing decision makers. These systems often behave in a counter-intuitive way and, because of the speed of change, the ability of agents to learn is severely constrained. Networking promotes cascading effects and extreme events (see, for example, Helbing *et al.* 2005; Ormerod and Colbaugh 2006; Helbing 2010) and crises. Furthermore, strong couplings can cause systemic instabilities (See, for example, Helbing 2011; Gao *et al.* 2012).

Traditional ways of thinking about both the business cycle in general and crises in particular are essentially based on an equilibrium view of the world. In this view, the economy can certainly be out of equilibrium, and sometimes far from it. However, such behaviour essentially arises from exogenous shocks. Instead, we need to appreciate the endogenous nature of the business cycle that is inherent to the system and not generally caused by external shocks. The idea that the cycle is endogenous has a long tradition in economics, starting with Marx and including Keynes, Hayek, Schumpeter and Goodwin, for example. Modern network theory can be regarded as emerging from this tradition, rather than from what has become the dominant, equilibrium paradigm of modern economics. A detailed discussion of the vision of the economy as an evolving complex system is given in, for example, the volumes edited by Anderson *et al.* (1988), Arthur *et al.* (1997), Blume and Durlauf (2006) and Farmer *et al.* (2012).

The representative agent approach must be abandoned. For example, the representative agent model cannot describe cascading effects well. These are not determined by the *average* stability but by the *weakest* link (see, for example, Ormerod and Evans 2011). The representative agent approach also neglects effects of spatial interactions and heterogeneities in the preferences of market participants. When these are considered, the conclusions can be completely different, sometimes even opposite. For example, there may be an 'outbreak' rather than a breakdown of cooperative behaviour (see, for example, Helbing and Yu 2009).

Furthermore, the representative agent approach does not allow one to understand particular effects of the structure of interactions within the network, which may promote or obstruct cooperativeness, trust, public safety, etc. Neglecting such network effects can lead to a serious underestimation of the importance that 'social capital' has for the creation of economic value and social well-being.

Awareness among economists of the limitations of the rational agent paradigm has steadily increased. In fact, these limitations have been stressed already in the past by well-known economists. For example, Pareto said that people spent some of their time making non-rational decisions and the rest of their time rationalizing them. Keynes emphasized that 'expectations matter' and placed great importance on market psychology ('animal spirits').

In a seminal 1955 paper, Simon forcefully argued that 'economic man' is in general unable to compute optimal decisions, but instead uses 'satisficing' rules of thumb (Simon 1955). Mainstream economics has subverted Simon's revolutionary message, so that satisificing is now seen as an example of rationality. Agents search amongst alternatives until they find one that is satisfactory, judging that the time and effort involved in further search for a better one is not likely to be matched by the potential increase in benefits. However, Simon argued that in many situations, even *ex post*, the optimal decision can never be known. Tversky and Kahnemann (1974) showed in laboratory experiments that individual decisions under uncertainty are much better described by heuristics, and that these may lead to systematic biases. Schumpeter and Hayek already had a 'complexity' view of economics with competing strategies and evolutionary selection. But these ideas were largely forgotten when the rational expectations paradigm in macroeconomics spread in the 1970s and 1980s.

Therefore, it is still a key challenge in economics to develop a fundamental model of agent behaviour that is empirically grounded in twenty-first-century reality. Incentives still matter, but in addition agent behaviour in networked systems is influenced directly by the behaviour of others. Tastes and preferences are not fixed, but evolve in ways that depend upon the decisions of others. Schelling introduced one such model, the so-called binary choice with externalities (Schelling 1973). Agents face a binary choice—for example, buy or sell—and their decision can influence the decisions of others directly. More recent formulations draw on models of social learning behaviour from other social sciences such as anthropology and cultural evolution to build an empirically grounded theory of learning and adaptive behaviour for complex socioeconomic systems (see, for example, Brock and Durlauf 2001; Rendell *et al.* 2010; Bentley *et al.* 2011a,b; Hahn and Bentley 2003; Bentley and Ormerod 2011).

Macroeconomics has, in recent decades, been much less receptive than microeconomics to new ideas from other disciplines. For example, it is by now well known that one of the causes of the financial crisis was the non-Gaussian nature of the distribution of asset price changes. The fat tails of these distributions mean that large changes, although still infrequent, are many orders of magnitude more likely than is implied by the Gaussian distribution. Yet the Value at Risk models implemented in many financial institutions assumed that price changes were normally distributed. Mandelbrot had shown as early as 1963 that this was not the case. In the late 1990s, econophysicists demonstrated beyond doubt, using evidence from a very wide range of markets, that price changes were non-Gaussian (see, for example, Mantegna and Stanley 2000; Bouchaud and Potters 2003). Literally hundreds of scientific papers are published and/or presented at scientific conferences on this topic.

Furthermore, covariance matrices of financial returns are the key input parameters to Markowitz’s classical portfolio selection problem (Markowitz 1952), which forms the basis of modern investment theory. For any practical use of the theory, it is therefore necessary to obtain reliable estimates for the covariance matrices of real-life financial returns. The effect of estimation noise (in the covariance matrix of financial returns) on the solution of the classical portfolio selection problem was studied extensively as long ago as the 1980s.

The problem of noise in financial covariance matrices was put in a new light by the findings of econophysicists obtained by the application of random matrix theory (see, for example, Laloux *et al.* 1999, 2000; Plerou *et al.* 1999). These studies showed that correlation matrices determined from financial return series contain such a high amount of noise that, apart from a few large eigenvalues and the corresponding eigenvectors, their structure can be regarded as random (in the example analysed by Laloux *et al.* (1999), 94% of the spectrum could be fitted by that of a purely random matrix). This not only showed that the amount of estimation noise in financial correlation matrices is large, but also provided the basis for a technique that can be used for an improved estimation of the correlations. Again, however, the findings of econophysics were ignored by mainstream economics.

The implication of the non-Gaussian statistics and the portfolio estimation noise was that assets were created, bought and sold that were actually much more risky than they appeared. The fat-tail problem meant that the probability of large price changes was drastically underestimated in the case of individual assets. And the poor empirical determination of the covariance matrix meant that the level of diversification in portfolios was thought to be considerably greater than it really was.

More generally, the authorities deluded themselves that the massive volume of debt contracts had been priced rationally and hence optimally. In the true spirit of the rational agent, rational expectations view of the world, they believed that agents had used the correct model in setting the prices, whereas of course they had not. If loans and debts had been priced rationally and optimally, the logical implication was that the interest payments receivable would exactly cover the risks involved on the loans. So, if an individual or institution defaulted on a loan, sufficient provision had been made via the optimal pricing of the loan to cover the loss arising from any such default. There was no need to tie up capital unnecessarily in liquid assets when it could be lent out at a profit. Across a portfolio of many such loans, the default of a single loan simply could not cause any problem.

In the brave new world of DSGE theory, the possibility of a systemic collapse—of a cascade of defaults across the system—was therefore never envisaged at all. Modern complexity theory, and specifically network theory, tells us that in an interconnected system the *same* initial shock would lead to dramatically different outcomes if we could replay history many times. Most of the time, shocks are contained and do not spread very far through the system. But, in principle, a shock of identical size could instead trigger a cascade of global proportions. We are in the realm of uncertainty here—finding it hard even to determine the probability distribution of expected outcomes—rather than a world of precise calculation of risk. Indeed, it may even be impossible to calculate the risk. The mean value and the standard deviation of a sample can always be calculated, but with fat tails the population mean and standard deviation may not exist at all.

We have to be very careful in drawing conclusions about the degree of connectivity and the vulnerability of a system to a global cascade following a shock. In principle, greater connectedness can strengthen a system, but given the ludicrously low liquid asset ratios with which banks were operating, it appears plausible that the opposite actually happened (Battiston *et al.* 2012).

As a final specific comment we note Blanchard's final *mea culpa* from his 2009 working paper (cited earlier): that leverage increased. Blanchard helpfully translates this phrase into English:

> Financial institutions financed their portfolios with less and less capital, thus increasing the rate of return on that capital. What were the reasons behind it? Surely, optimism, and the underestimation of risk, was again part of it.

Further comment is superfluous.

Conclusions

It is hard to escape the conclusion that mainstream macroeconomics needs to go back to the drawing board and be largely redesigned, more or less from scratch. Key aspects of the research programme to rebuild macroeconomics include the following.

- Abandon the representative agent.
- Introduce agent heterogeneity.
- Develop more realistic models of agent behavior that incorporate, for example,
  - cognitive complexity, subjectivity, emotions and learning,
  - the interaction of emotional states with belief-based processes,
  - social learning and
  - self-image.
- Recognize that the macroeconomy is a system of coupled dynamic networks.
- Build in the potential for cascades across various networks:
  - the direct financial linkages between institutions,
  - the networks across which opinions are formed in financial markets,
  - the networks across which the 'animal spirits' of commercial companies are determined, and
  - the networks across which optimism or pessimism spread amongst consumers.

Important questions to address include the following.

- What phenomena may occur in strongly coupled and interdependent systems and under what conditions? How should we characterize these phenomena? What universality classes exist?

- What are the interdependencies between structure, dynamics and function? How do networks affect the behaviour of their components? Are there properties that individual components 'inherit' from the networks they belong to?
- What are the limits of predictability and controllability in complex, networked systems? Is the economic system designed in a controllable way from a cybernetic point of view? If not, what would have to be changed to make the system more manageable?

In short, we have an agenda that is essentially focused on networks. The properties of networks in terms of their propensity to either transmit or absorb shocks, the interactions between the agents in any given network, the interactions between the emergent macro features of networks and individual agents, and the interactions between the various coupled networks that make up the economy of the twenty-first century.

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