Back to the drawing board for macroeconomics

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1. Introduction and Background

In November 2010, European Central Bank (ECB) the then Governor, Jean-Claude Trichet, opened the ECBs 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 policy-maker 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 American authorities reacted to the financial crisis after the collapse of Lehman Brothers in September 2008. The cumulative fall in real GDP in the United States in 2008 and 2009 is around 4 per cent. The economy has recovered, and by the second half of 2011, levels of output have risen above their previous peak levels. In contrast, during the Great Depression of the 1930s, the cumulative fall in real GDP was some 27 per cent. The previous peak level of output, in 1929, was not regained until 1939, a whole decade. This latter recession was the most serious of any peacetime recession in any Western economy over the entire period since the second half of the 19th century (Ormerod, 2010a). So far, the falls in output during the financial crisis have been less devastating. In the United States, output fell some 3 per cent and has already passed its previous peak level. In the major European economies, GDP fell by around 6 per cent. The strength of the recovery in these has not in general been as strong as in America, but even so the experience has been markedly better than it was in the 1930s.

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However, at the time of the current financial crisis, the authorities did not make use of the dominant intellectual concept in academic macroeconomics over the previous 30 years. This started off as real business cycle theory (Kydland and Prescott 1982), and was then developed into dynamic stochastic general equilibrium theory (DSGE, see, for example, Tovar 2009 for a description of the scientific state of the art when the financial crisis began).

Fortunately – and this illustrates the role of chance and contingency in human affairs – Governor Bernanke at the Federal Reserve was not an expert 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 tried to avoid the key mistakes made during the early 1930s in America. They maintained an expansionary monetary policy, in contrast to the contractions of the Great Depression, and defended the banks. As described in Ormerod (2010b), in the crucial days following the collapse of Lehmans, they took a number of purely administrative measures. They

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

This latter, 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 the $700 billion Troubled Asset Relief Programme, which was played out in the full light of the democratic spotlight.

One immediate implication from the above is the pressing need to re-introduce economic history into the teaching of economics at all levels. Although there have been many financial crises in the history of the world (Reinhart and Rogoff, 2009), there have only been two such crises on a global scale over the past century, those of the early 1930s and the late 2000s. As an indicator of this, the Maddison database (Maddison, 1995) of 17 leading Western economies contains annual data on real GDP going back to 1870. The only years when almost all these countries were simultaneously in recession (real GDP growth less than zero) are 1930-1932 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 at this distance in 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, two which spring readily to mind are the contrast between the transitions to peace-time economies in the aftermaths of the world wars, and the oil shock crisis of the mid-1970s. In the former, the period after the First World War was characterised by deep recessions in some countries. Serious problems with both international trade and the world monetary system persisted throughout the inter-war 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 an immediate and dramatic widening of the difference between them. In 1975, for example, Inflation in Germany in 1975 was only 4 per cent, but was above 20 per cent in both Italy and the UK. The analysis of such formative periods raises many important questions in economic theory.

With this general background, we now move on to extend the themes of the paper. Section 2 briefly considers existing theory in macroeconomics, and specifically theories of the business cycle. Section 3 sets out what we regard as key intellectual concepts which are needed to rebuild macroeconomics, and section 4 offers a conclusion.

2. Business cycle theory

There are two features which distinguish capitalism from all previously existing forms of social and economic organisation. First, 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 of 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. Certainly, it would 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, even if an equilibrium exists in principle, in practice the economy spends most of its time out of equilibrium.

However, in our opinion too much time can be spent on the study of economic thought. Students of physics need to learn at some point Newtonian physics, because of the very powerful empirical evidence which 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 GDP growth which a scientific model should be able to replicate. For example, the autocorrelation function and its Fourier transform pair, the power spectrum. Both the duration and cumulative sizes of recessions follow highly non-Gaussian distributions, as does the time period between recessions. These are obvious examples of so-called 'stylised facts' which a genuinely scientific model ought to be able to explain.

Macroeconomics needs to become an empirically based science which is able to explain fundamental features of the economy. A glaring example of the lack of progress made in this area is the lack of consensus which still exists on the size of the fiscal multiplier, a fundamental concept in macroeconomic theory. In his *General Theory* in 1936, Keynes hazarded the opinion that for the UK economy, the eventual increase in real GDP 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 (and other European) economies and the associated marginal propensity to import mean that the multiplier is most unlikely to be so high. Indeed, the first systematic comparison of macro-econometric models of the UK carried out over 30 years ago (Laurie 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 (2011) argues that the multiplier in the US, which is of course a much more closed economy in trade terms than any individual European economy, is between 0.8 and 1.5. 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 that: '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.'

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 dynamic stochastic general equilibrium (DSGE) models, has an altogether different intellectual pedigree. The numerical solution of such models is itself a non-trivial task (see, for example, the special issue of the *Journal of Economic Dynamics and Control* in 2011, edited by Wouter, Den Haan, Judd and Juillard). 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 which 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 macro-econometric models (1976). Lucas argued that policy evaluation (such as the estimate of the fiscal multiplier) 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 parameters which 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 (2011) have recently shown that this is not necessarily true.

It is useful to consider the change in the view on such models that Olivier Blanchard, Chief Economist at the IMF, expressed as a result of the financial crisis. In an MIT Working Paper published in August 2008, merely a few 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' Presidential address to the American Economic Association in 2003, 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' and he concluded '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, he identified four main reasons for the crisis:

- Assets were created, sold, and bought, which appeared much less risky than they truly were
- Securitization led to complex and hard to value assets 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 macroeconomic might address such issues.

3 Re-building 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 due to their fast change, the ability of agents to learn is severely constrained. Networking promotes cascading effects and extreme events (for example, Helbing et.al. 2005, Ormerod and Colbaugh 2006, Helbing 2010) and crises. Further, strong couplings can cause systemic instabilities (for example, Helbing 2011, Gao et.al. 2011).

Traditional ways of thinking about both the business cycle in general and crises in particular are essentially based upon 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 which is inherent in 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 by Farmer et.al. (2012, forthcoming).

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 (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 (for example Helbing and Yu, 2009).

Further, the representative agent approach does not allow one to understand particular effects of the interaction network structure, 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 of the limitations of the rational agent paradigm among economists 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 rationalising them. Keynes emphasized that "expectations matter" and placed great importance on market psychology ("animal spirits").

Simon (1955), in a seminal paper, forcefully argued that Economic Man is in general unable to compute optimal decisions, but instead uses satisficing rules of thumb. 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 which 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 is to develop a fundamental model of agent behaviour which is empirically grounded in 21st 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 which depend upon the decisions of others. Schelling (1973) introduced one such model, the so-called 'binary choice with externalities'. Agents face a binary choice – for example, buy or sell – and their decision can influence directly the decisions of others. 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 socio-economic systems (for example Brock and Durlauf (2001), Rendell et.al. (2010), Bentley et.al. (2011a, 2011b,), Hahn and Bentley (2003), Bentley and Ormerod (2011)).

Macroeconomics in recent decades has been much less receptive to new ideas from other disciplines than microeconomics. 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 (VaR) models implemented in many financial institutions assumed that price changes were normally distributed. Mandlelbrot 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 were 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 (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 (for example, Laloux et.al. 1999, Plerou et.al 1999, Laloux et.al. 2000). 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 analyzed by Laloux et. al. 1999, 94 per cent of the spectrum could be fitted by that of a purely random matrix). These 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 such. 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 which 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 considerable greater than it really was.

More generally, the authorities deluded themselves that the massive amounts of loans and debts 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 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 individual or institution A defaulted on a loan, sufficient provision via the optimal pricing of the loan had been made 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, the possibility of a systemic collapse, of a cascade of defaults across the system, was 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 to these can trigger a cascade of global proportions. Here, we are much more in the realm of uncertainty, finding it hard even to determine the probability distribution of expected outcomes, than we are in the world of the 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 in principle strengthen a system. But given the ludicrously low liquid asset ratios with which banks were operating, it appears plausible that rather the opposite happened (Battiston et.al. 2012).

As a final specific comment, we note Blanchard's final *mea culpa*. 'Leverage increased', a phrase which he helpfully translates this 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 '(Blanchard 2009). Further comment is superfluous.

4 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:

- Abandoning the representative agent
- Introducing agent heterogeneity
- More realistic models of agent behavior, incorporating, for example:
    - cognitive complexity, subjectivity, emotions, and learning
    - the interaction of emotional states with belief-based processes
    - social learning
    - self-image

- Recognising that the macro economy is a system of coupled dynamic networks
- Building 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
    - the networks across which optimism/pessimism spread amongst consumers

Important questions to address include:

- What phenomena may occur in strongly coupled and interdependent systems and under what conditions? How to 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 better manageable?

In short, we have an agenda which 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 network and individual agents, and the interactions between the various coupled networks which make up the economy of the 21st century.

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