

**Keynote address at the International Association for Research in Economic Psychology/Society for the Advancement of Behavioural Economics, University of Bolton, 26 March 2010**

**1. Introduction**

The financial crisis raised fundamental problems for mainstream economic theory, and in particular how it deals with risk and uncertainty. It is the ideas at the heart of modern macroeconomics which provided the intellectual justification of the economic policies which help create the crash. The ideas are at the root of the financial crisis.

Economic psychology offers much more realistic models of how decision makers – individuals, firms, governments – actually behave.

I am going to talk first of all about the role of macroeconomic theory in the financial crisis, and then offer some more positive thoughts on how to take forward economic psychology.

Economics can be rebuilt on the basis of more scientifically credible rules of individual behaviour. One of the great successes of economic psychology has been to show that the behaviour of agents when confronted with either risk or uncertainty is often quite different from that postulated by mainstream economics.

**2. Risk and uncertainty**

Some of the best known names in economic thought made an important distinction between risk and uncertainty. The person who formalised the distinction was Frank Knight in Chicago, his key book on the topic being published in 1921<sup>1</sup>. Knight is much less well known than two other names I am about to mention, but of such stature that Milton Friedman described him as 'one of the most original and influential social scientists of the twentieth century'.

Knight essentially argued that the concept of risk applied in circumstances where we know, or at least have a very good idea of, the probability distribution of possible outcomes. But where our knowledge of this is imprecise or even absent altogether, uncertainty is the relevant concept. So, for example, placing money on a fair roulette table is risk. We know the exact probability of each of the various outcomes on

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<sup>1</sup> <http://www.econlib.org/library/Knight/knRUP.html>

which we can bet. Uncertainty would arise, for example, if we bet but not only did not know how many numbers there were on the wheel, but even whether the ball would be spun on it at all. The essential feature of true uncertainty is that it is incalculable, no matter how smart we may be. Practical situations may be closer to the uncertainty paradigm than to that of risk because, for example, we may have too small a sample of events to estimate a probability or, more generally, we have inadequate knowledge about the causal mechanisms involved in any given situation.

Keynes believed that uncertainty was the more important of the two, and that it was the key reason for the business cycle, the booms and busts of capitalism. In the *General Theory* he wrote: ‘the outstanding fact is the extreme precariousness of the basis of knowledge on which our estimates of prospective yield [of a new investment] have to be made ... If we speak frankly, we have to admit that our basis of knowledge for estimating the yield ten years hence of a railway, a copper mine, a textile factory, the goodwill of a patent medicine, an Atlantic liner, a building in the City of London amounts to little and sometimes to nothing; or even five years hence’. On a purely rational calculation, very few investments would ever be carried out, very few new firms started, since most such ventures fail. It is the irrational optimism of the investor, the entrepreneur, which enables them to happen, the belief that he or she has a better idea, a better concept, which is bound to succeed.

Hayek went even further believing that there are inherent limits to knowledge which no amount of intellect can overcome. His 1974 Nobel lecture<sup>2</sup>, for example, is entitled ‘The Pretence of Knowledge’. On this view, inherent and inescapable uncertainty pervades the economy. In many ways, Hayek is an intellectual precursor of modern complex systems theory.

Although Hayek has suffered a form of guilt by association in that his work has been cited approvingly by rational expectations theorists, his analysis of the business cycle is in many ways similar to that of Keynes – though they did differ over policy matters. Hayek certainly thought that general equilibrium should be the foundation of business cycle theory. However, the theory had to be extended considerably in order to be able to explain the persistent fluctuations in aggregate output. Firms and governments operate in such a complex environment that not only are their expectations often proved wrong, but they are unable to learn sufficient from the past in order to avoid the same mistake in future. The level of uncertainty is so high that even the central bank cannot learn to offset expectations by changes in monetary policy in order to smooth out the cycle and restore equilibrium.

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<sup>2</sup> [http://nobelprize.org/nobel\\_prizes/economics/laureates/1974/hayek-lecture.html](http://nobelprize.org/nobel_prizes/economics/laureates/1974/hayek-lecture.html)

Modern macroeconomic theory failed in its appreciation of *both* risk and uncertainty.

### 3. 'Rational' behaviour

The appropriation by economics of the word 'rational' to describe the behaviour of agents – individuals, firms, governments – in its core view of how the world operates is a great propaganda coup. Who, after all, would want to be thought irrational, or even have the temerity to suggest models in which agents behaved irrationally?

Rational expectations do not require that an agent's predictions about the future are always correct. Indeed, such predictions may turn out to be incorrect in every single period, but still be rational. The requirement is that on average over a long period of time, expectations are correct. Agents are assumed to take into account all relevant information, and to make predictions which are on average unbiased. Deviations from perfect foresight in any given period are an inherent feature of this behavioural postulate, but such deviations can only be random. If there were any systematic pattern to the deviations, the agent would be assumed to incorporate the pattern into his or her expectations. Again, on average over a long period, such expectations are correct.

It will be apparent that the theory is difficult to falsify to someone who really believes in its validity. Even the most dramatic failure to predict the future, such as the 2008 financial crisis, can be explained away as a random error. A rational expectations enthusiast can still continue to maintain the correctness of the theory by simply assuming that over some (theoretically indeterminate) period of time, on average agents' expectations prove accurate.

An assumption of the theory is that, as part of the set of information being processed, the agent is in possession of *the* correct model of the economy. Indeed, on the logic of the theory itself, if the model being used to make predictions were not correct, the forecasts would exhibit some sort of bias, some systematic error, and agents would realise that it was wrong.

It might reasonably be argued that it is difficult to subscribe to the view that agents understand the correct model of the economy given that economists themselves differ in their views as to how the economy operates. For example, in the autumn of 2008, many prominent American economists, including a number of Nobel Prize winners, vigorously opposed any form of bail-out of the financial

system, arguing that it was better to let banks fail. Others, including decision makers at the Federal Reserve and Treasury, took a different view entirely.

The response of the academic mainstream has been to insist that there have been strong moves towards convergence within the profession on opinions about macroeconomic theory. By implication, anyone who takes a different view and is not part of this intellectual convergence is not really a proper economist. Olivier Blanchard, Chief Economist at the International Monetary Fund, published an MIT discussion paper in August 2008<sup>3</sup> on the state of modern macroeconomics. He concluded 'the state of macro is good'. The state of macro is good! Just three weeks before the financial crisis nearly brought capitalism to a halt!

Policy makers were strongly influenced by the intellectual underpinnings of the RARE view of macroeconomics. It is flattering to believe that you have succeeded through your cleverness and that of your advisers in eliminating an apparently inherent feature of the Western capitalist economies, first analysed systematically by Marx in the middle of the 19<sup>th</sup> century, that of boom and bust.

This intellectual view of how the world worked misunderstood in a fundamental way both risk and uncertainty:

- It assumed risk – where we have a good approximation of the probability distribution of outcomes – followed a normal, or Gaussian, distribution. Very large falls are effectively ruled out. For the most part, asset price changes do follow this distribution, but the tails are 'fat', the chances of observing a massive fall, whilst small, are very much more than if the Gaussian really does describe price changes over the whole range
- It ruled out the possibility of cascades across the system, that interconnections of the system meant that failure in one part of it would be transmitted to another part. Agents faced risk and not uncertainty, and risk by definition had been optimally priced
- It failed to take account of the fact that in many parts of the financial markets, uncertainty rather than risk formed the environment. Many of the derivatives products which were written were so complex that even the physicists and mathematicians who invented them didn't really have much of a clue of the likely gains and losses involved

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<sup>3</sup> Blanchard OJ (2008), 'The state of macro', MIT Department of Economics Working Papers, 08-17

Fortunately, policy makers, led by the Americans, took a completely different view of the world. They simply junked the whole of modern macroeconomic theory of the past 30 years and tried to apply the lessons of the Great Depression in the 1930s.

It was fortuitous that the Chairman of the Federal Reserve at the time, Ben Bernanke, was a leading academic authority on the Great Depression. He knew that, above all, the banks had to be protected. It may seem monstrously unfair that the bankers themselves escaped penalties – indeed it *is* unfair – but the abiding lesson of the 1930s is that in a financial crisis the banks have to be defended.

Admittedly, the authorities did try the experiment of allowing Lehman's to fail. But it very rapidly became very evident that any repeat of this risked the total collapse of Western capitalism. No monetary authority since has seen fit to repeat this experiment.

Much of the publicity and controversy surrounded the Troubled Asset Relief Program (TARP), which required political approval and so was played out in full light of the democratic process in America. But in many ways this was of second-order importance to the purely administrative actions of the American authorities. They:

- Nationalised the main mortgage companies
- Effectively nationalised AIG
- Eliminated investment banks
- Forced mergers of giant retail banks
- Guaranteed money market funds

This latter in particular has attracted very little attention, but was crucial, without the guarantee there could have been a complete seizure of the world credit system within a matter of days.

The key point about all these actions is that the American authorities paid no attention to academic macro-economic theory of the past 30 years. Real business cycle theory, dynamic stochastic general equilibrium models, rational expectations – all the myriad of erudite papers on these topics might just as well have never been written. Instead, the authorities acted. They acted imperfectly, in conditions of huge uncertainty, drawing on the lessons of the 1930s and hoping that the mistakes of that period could be avoided. It was not a grand plan, nor did one ever exist. This was a process of people responding to events on the basis of imperfect knowledge and trying and seeing what did and did not seem to work.

So far, this seems to have worked. In the 1930s, US GDP fell by nearly 30 per cent and almost 1 in 4 Americans were unemployed. This time round, GDP fell by 4 per cent is recovering.

Shortly after his August 2008 MIT economics working paper, Olivier Blanchard wrote a further paper in the series, which appeared in January 2009<sup>4</sup>. It is a remarkable about-turn, which recalls another of Keynes' famous phrases: 'When someone persuades me that I am wrong I change my mind. What do you do?'

Blanchard identifies four main reasons for the crisis.

*First: 'Assets were created, sold, and bought, which appeared much less risky than they truly were'.*

Exactly! Blanchard gives the example of sub-prime mortgages. But his strictures could be applied just as well to the more complicated world of derivatives, where the probability of very large changes in prices was grossly underestimated by the conventional pricing models.

This, the so-called 'fat tails' problem, had been established beyond doubt by econophysics research, yet was effectively ignored. Large changes in asset prices are not frequent, but they happen many times more frequently than is implied by the conventional assumption that they follow the normal (Gaussian) probability distribution.

Blanchard wonders why this happened: 'History teaches us that benign economic environments often lead to credit booms, and to the creation of marginal assets and the issuance of marginal loans. Borrowers and lenders look at recent historical distributions of returns, and become more optimistic, indeed too optimistic about future returns'.

We are a world away from rationality and DSGE models. Indeed, we are back in the world of both Keynes and Hayek, where agents can make persistent mistakes, and even the central bank does not learn from the past.

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<sup>4</sup> Blanchard OJ (2009), 'The crisis: basic mechanism and appropriate policies', MIT Department of Economics Working Paper Series 09-01

*Second:* ‘Securitization led to complex and hard to value assets on the balance sheets of financial institutions’.

The maths of pricing even the simplest derivative are hard enough<sup>5</sup>, but the truly exotic nature of many of the products which were created meant that neither the buyer nor the seller had a good approximation to the probability distribution of the likely outcomes. In other words, they faced a world in which uncertainty was even more important than risk.

*Third:* ‘Securitization and globalization led to increasing connectedness between financial institutions, both within and across countries’.

We have to be very careful in drawing conclusions about the degree of connectedness and the vulnerability of a system to a global cascade following a shock. Greater connectedness can in principle strengthen rather than weaken a system. But given the ludicrously low liquid asset ratios with which banks were operating, it appears plausible that this was a factor in the crisis.

*Fourth:* ‘Leverage increased’

Blanchard 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’. Further comment is superfluous.

In this latest paper, Blanchard is effectively discarding the entire corpus of mainstream macroeconomic theory of the past 30 years.

#### **4. Where do we go from here?**

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<sup>5</sup> The seminal article is Black F and Scholes M (1973), ‘The pricing of options and corporate liabilities’, *Journal of Political Economy*, 81, 637-654

Economic psychology offers much more realistic models of agent behaviour in the face of risk and/or uncertainty. But sometimes its conclusions can be surprising. Let me give just one example, chosen because it is fresh in my mind. Neil Stewart, a psychologist at Warwick, gave it at an event last week organised by Greater London Authority Economics as part of the ESRC's Social Science week festival. The topic was 'What can economists learn from the other social sciences, it was a very good event.

There is legislation which requires that a minimum percentage of credit card balances has to be paid off every month. The idea is to prevent people getting too indebted. The current percentage is very low, just 2 per cent. An obvious – well, apparently obvious – thing to do if you are concerned about the build up of debt is to increase this percentage, to make people pay more of the debt off each month. Neil did some experiments on this, and his conclusion is surprising. Increasing the minimum percentage which *has* to be paid off actually *reduces* the overall percentage which is repaid. Raising the minimum seems to signal to people that the debt is under control, and they become less likely to make voluntary repayments.

I've just had to review an excellent book by Graham Loomes, Robert Sugden and colleagues called *Experimental Economics: Rethinking the Rules*. They provide a detailed reflection of their work in this area stretching back over 20 years. It is comprehensive, with over 500 journal references – 500! There is a big literature in this field.

As we all know, experimental economics raises profound difficulties for the conventional economic model of preference theory based upon rational choice. The extension of the latter by the concept of bounded rationality does not evade the problems.

Many of the key results were discovered in fairly simple experiments in the early years of the whole enterprise of experimental economics. For example, consumer preferences appear in general to be non-transitive. Agents' decisions are influenced by irrelevant alternatives. Preference reversal is widespread, in other words the revealed preference ordering of a pair of alternatives depends on the process used to elicit the preference. These are just some of the examples.

Loomes, Sugden et.al. discuss both the standard criticisms of experimental economics by the mainstream, and point out how experiments have become more sophisticated over the years, often in response to such criticisms.



Above all, of course, we have the Nobel Prize awarded in 2002 to Vernon Smith and Daniel Kahneman for their work in experimental economics and economic psychology. A key conclusion of Kahneman's work<sup>6</sup> is that 'humans reason poorly and act intuitively'. 'Humans reason poorly and act intuitively'!

Why doesn't mainstream economics pay any real attention to this massive and impressive literature? After all, the most prestigious prize in the discipline has been awarded to two practitioners, Kahneman and Smith.

We might usefully pause and reflect how mainstream economics appears breathtakingly able to either downplay or ignore completely the works of its Nobel Prize winners. Almost all the prizes in the 21st century have come from people working outside, often way outside the Rational Agent Rational Expectation paradigm.

Let me go back to 2000 to give an example, and the micro econometricians Heckman and McFadden. Quite rightly, their statistical techniques are taught. But what about the implications of their work? Here is Heckman in his Prize lecture: 'an important empirical regularity is the diversity and heterogeneity of behaviour'. Again, let me repeat this for emphasis: 'an important empirical regularity is the diversity and heterogeneity of behaviour'. So why are we still bothering to teach models in which the behaviour of the whole economy is reduced to that of a single 'representative agent'? We have decisive empirical evidence that agents are heterogeneous.

Elinor Ostrom's award in 2009 emphasised the importance of institutions and culture, and how behaviour can be very different in different institutional and cultural contexts. She has attracted vitriolic attacks from many economists, seemingly on the grounds that she is a political scientist and not a 'proper' economist. So someone who has been awarded the Nobel Prize in economics is not thought to be a good enough economist by economists who themselves have not been awarded the Prize and never will be! There is some sort of psychological disjuncture going on here, perhaps they need the help not of psychologists but psychiatrists.

But, point scoring aside, this is a really fundamental issue. Why haven't empirical findings on agent behaviour – where economic psychology has led the way – really found their way into mainstream economics, into economic theory. Side by side with the growth and development of economic psychology and experimental economics in the 1980s and 1990s went the growth and eventual

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<sup>6</sup> [http://nobelprize.org/nobel\\_prizes/economics/laureates/2002/kahneman-lecture.html](http://nobelprize.org/nobel_prizes/economics/laureates/2002/kahneman-lecture.html)

complete domination of mainstream macro by rational agent equilibrium models. First of all real business cycle models, which were then developed in turn into dynamic stochastic general equilibrium.

One of the defences – I'm not going to go through them all, people here will be even more familiar with them than I am – one of the defences of mainstream economics to the findings of economic psychology is that all scientific theories are wrong to some extent. Strictly speaking that is true. Quantum mechanics passes some incredibly stringent empirical tests, but its theoretical predictions are not *absolutely* identical to the real world. But it seems safe enough to teach the theory as if – the favourite phrase of all economists – it were true.

On the other hand, it seems equally certain that the Sun doesn't go round the Earth. There is not much point in teaching a theory if it deviates a long way from empirical reality. In some limited circumstances, the postulates of rational agent behaviour are a good approximation to how the world works, but in general it is not. This is particularly the case when decisions taken now may have important consequences in the future. Choosing between different brands of baked beans on the supermarket shelves is not of any great import, but choosing, say, a pension scheme is.

So why are the findings of economic psychology not absorbed into mainstream economic theory? Humans reason poorly and act intuitively. But we don't exactly see many theoretical models in the top journals based on this result.

Let me say that I am aware of the literature on the nature of scientific progress, Popper, Kuhn, Lakatos. But I never did philosophy as a student, I mainlined on straight economics. You will just have to bear with me setting much of this debate aside.

## **5. How we can make progress**

I want to focus on practical ways of overcoming the resistance to change which pervades mainstream economics. To warn you in advance, I am going to suggest building theoretical models, but with a very definite difference.

For all its faults, economics does have one deep insight which other social sciences do not have. Namely, that agents react to incentives. Many aspects of the social and economic worlds are difficult to understand unless you appreciate this fact. Just think of how universities and their staffs have reacted to the various rules, regulations and targets which have been set in recent years. The set of incentives they face has altered, and as a result their behaviour has changed.

Conventional theory then goes on to rather spoil things by insisting there is a universal mode of behaviour by which agents react to changes in incentives, that of optimisation subject to constraints. But the latter isn't really needed to get insights into lots of problems. Think, for example, of Steve Levitt's blockbuster *Freakonomics*. This is a whole book about how the fact that agents react to incentives is able to illuminate a whole range of issues, from the serious to the quirky. But Levitt does not need to invoke rationality in order to write the book. It is sufficient that agents react to incentives.

I think that this insight is so powerful that it is often able to give a half-decent account of what is going on, even when it is overlaid with the assumption of rationality, which as we know in many contexts is not a sensible postulate at all.

The net result is that the mainstream *is* able to give accounts of many social and economic phenomena, its theory contains an important truth, so it is not like the theory that the Sun goes round the Earth.

Now, this is a fully articulated theory developed over the course of well over 100 years. In contrast, economic psychology offers a whole series of empirical findings. On the one hand a formalised theory in elegant maths. On the other, a set of empirics. Further, a set of empirics which implicitly denies the possibility (certainly in the current state of scientific knowledge) of a general theory of agent behaviour. Each particular question being addressed requires its own particular mode of agent behaviour. There may be some generalisations available, but in economic psychology we are essentially saying that the behaviour of any given agent will be different in different circumstances. Not just different because the incentive set he or she faces is different, but fundamentally different in the sense that the agent uses different rules in different circumstances.

We need to use the findings of economic psychology to construct theoretical models which offer a better account of empirical phenomena than those provided by rational agent economics. Not only do such models have empirically grounded rules of behaviour, but they have micro-economic foundations, a concept to which many economists attach great importance. Indeed, this is the very essence of the models, their properties arise out of the behaviour of the agents within them.

In order to do this, we need to abandon the fixation of conventional economics with analytical results, mathematical models which have an analytical solution. Before the advent of the personal computer, this was a perfectly reasonable approach to take. The only way the properties of a theoretical model could be properly understood was by obtaining an explicit solution to the equations. Well, I say the

personal computer, the old mainframes could also have been used, but the PC makes life much easier and their power, as we all know, is enormous even compared to most mainframes of 30 years ago.

Once we start with theoretical models which have more realistic behavioural foundations - the key evidence which economic psychology and behavioural economics provides - obtaining explicit analytical solutions becomes much harder, and often impossible. A crucial feature of much real world behaviour is that the tastes and preferences of individuals are not fixed, but can be altered directly by the behaviour of others. The maths of conventional economic theory can be hard enough, but it relies on the simplifying assumption that tastes and preferences are fixed.

We can explore the implications of realistic models of agent behaviour through simulation, by obtaining numerical solutions on the computer rather than being constrained by analytical ones. This is a perfectly standard practice. Systems of partial differential equations in the natural sciences, for example, are routinely explored and solved in this way. Standard algorithms exist to do this. Increasingly, hard science and even maths itself is being done on the computer. Simulation is the scientific technique of the 21<sup>st</sup> century. We don't rely on the abacus or even the slide rule any more, so our models should not be constrained by a fixation with analytical solutions.

Let me give a simple but brilliant example. Duncan Watts, a mathematical sociologist at Columbia and now head of consumer research at Yahoo, published a paper in the *Proceedings of the National Academy of Sciences* in 2002 under the austere topic of global cascades on random networks<sup>7</sup>. He was, however, addressing some very important practical questions with a minimalist but nevertheless realistic model of agent behaviour in many circumstances.

In many situations, individuals face making a decision when either the cost of gathering information is prohibitively high, or there is a strong element of genuine uncertainty about the potential outcome. In such circumstances, a sensible way to behave is to copy what other people whose judgement you respect are already doing. It may turn out to be the wrong decision for you, but it is a reasonable rule of thumb to use. A classic example is going out to eat in a strange city. You can of course spend a lot of

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<sup>7</sup> Watts D J (2002) 'A simple model of global cascades on random networks', *Proc. National Academy of Science* 99: 5776-5771

time consulting guide books, searching the web, but a reasonable strategy is to go to a restaurant which is pretty full rather than one which is empty.

This decision rule was described as long ago as 1973 by Thomas Schelling, Nobel Prize winner in 2005. He called it 'binary decisions with externalities'<sup>8</sup>. It is binary in the sense that you either buy a product or you don't, you adopt a particular mode of behaviour or you don't. It has externalities in the sense that the behaviour of any single agent in itself can alter the behaviour of others. It is a simple but very powerful concept which has been usefully applied in areas such as fashions, riots, crime, competing technologies, the spread of innovations, the emergence of social norms to name but a few.

In Watts' model, agents are initially in state zero of the world – they haven't adopted an idea or bought a product. A few are selected at random to choose state one of the world. Agents are allocated at random a level of 'persuadability': what proportion of people you are connected to in this context – people you take notice of – need to be in state one for you to switch to it from state zero, and vice versa.

It is simple, but a good approximation to many practical situations. A key question Watts examined was: how do cascades spread across this network, how far do people switch to state one of the world.

He programmed the model and obtained large numbers of solutions. In each one, the initial conditions were identical: a small number of agents is selected at random to be in state one of the world. Yet the outcomes are massively different. Most of the time, the cascade is small, few change their behaviour. But occasionally, a cascade on a global scale takes place.

This is a profound piece of work. In systems where agents base in whole or in part their decisions on the basis of what other people do, the common sense view of the connections between the scale of an event and its consequences no longer holds. Small events, the initial choice by a few agents of state one, can have dramatic consequences. Further, systems which are exposed to continuous shocks for long periods of time may suddenly and seemingly inexplicably exhibit a large cascade.

This is just one example. There are of course all sorts of issues around the validation of such models, but the field is evolving rapidly and a lot of progress has been made. These models are much more genuinely scientific than the sophisticated curve fitting of econometrics, because that is all the latter is.

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<sup>8</sup> Schelling T.C. (1973). Hockey Helmets, Concealed Weapons, and Daylights Savings, *Journal of Conflict Resolution*, 13, 381-428

They have postulates about individual behaviour, and the implications for the system as a whole which is being investigated emerge from the interactions of individuals.

These models have the potential to unify the social sciences, to provide non-economists with theoretical models which both have more realistic behavioural foundations and give better scientific explanations of events than conventional economics. Economic psychology has a vital role to play, indeed has already had a vital role, in providing the scientific evidence for more realistic behavioural rules.

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