

Is the Economic Crisis a Crisis for Economics?

By Unlearning Economics

For critics of mainstream economics, the 2008 financial crisis represents the final nail in the coffin for a paradigm that should have died decades ago. Not only did economists fail to see it coming, they can't agree on how to get past it and they have yet to produce a model that can understand it fully. However, economists tend to see things quite differently – in my experience, your average economist will concede that although the crisis is a challenge, it's a challenge that has limited implications for the field as a whole. Some go even further and argue that it is all but irrelevant, whether due to progress being made in the field or because the crisis represents a fundamentally unforeseeable event in a complex world.

I have been compiling the 7 most common lines used to defend economic theory after the crisis, and will consider each of them in turn. I've started with what I consider the weakest argument, with the quality increasing further down the list. Hopefully this will be a useful resource to further debate and prevent heterodox and mainstream economists (and the public) talking past each other. Let me note that I do not intend these arguments as simple 'rebuttals' of every point (though it is of some, especially the weaker ones), but as a cumulative critique. Neither am I accusing *all* economists of endorsing *all* of the arguments presented here (especially the weaker ones).

Argument #1: "We did a great job in the boom!"

I've seen this argument floating around, and it actually takes two forms. The first, most infamously used by Alan Greenspan – and subsequently mocked by bloggers – is a political defence of boom-bust, or even capitalism itself: the crisis, and others like it, are just noise around a general trend of progression, and we should be thankful for this progression instead of focusing on such minor hiccups. The second form is more of a defence of economic theory: since the theory does a good job of explaining/predicting the boom periods, which apply most of the time, it's at least partially absolved of failing to 'predict' the behaviour of the economy. Both forms of the argument suffer from the same problems.

First, something which is expected to do a certain job – whether it's an economic system or the economists who study it – is expected to do this job *all the time*. If an engineer designs a bridge, you don't expect it to stand up *most* of the time. If your partner promises to be faithful, you don't expect them to do so *most* of the time. If your stock broker promises to make money but loses it after an asset bubble bursts, you won't be comforted by the fact that they were making money before the bubble burst. And if an economic system, or set of policies, promise to deliver stability, employment

and growth, then the fact that it fails to do so every 7 years means that it is not achieving its stated objectives. In other words, the “invisible hand” cannot be acquitted of the charge of failing to do its job by arguing it only fails to do its job every so often.

Second, the argument implies there was no causal link between the boom and the bust, so the stable period can be understood as separate from the unstable period. Yet if the boom and the bust are caused by the same process, then understanding one entails understanding the other. In this case, the same webs of credit which fuelled the boom created enormous problems once the bubble burst and people found their incomes scarce relative to their accumulated debts. Models which failed to spot this process in its first phase inevitably missed (and misdiagnosed) the second. As above, the job of macroeconomic models is to understand the economy, which entails understanding it at all times, not just when nothing is going wrong – which is when we need them least.

I can't help but wonder if this argument, even in its general political form, has roots in economic theory. Economic models (such as the Solow Growth Model) often treat the boom as the ‘underlying’ trend, buffeted only by exogenous shocks or slowed/stopped by frictions. A lot of the major macroeconomic frameworks (such as Infinite Horizons or Overlapping Generations models) have two main possibilities: a steady-state equilibrium path, or complete breakdown. In other words, either things are going well or they aren't – and if they aren't, it's usually because of an easily identifiable mechanism, one which constitutes a “notably rare exception” to the underlying mechanics of the model. Such a mentality implies problems, including recessions, are not of major analytical interest, or are at least easily diagnosed and remedied by a well-targeted policy. Subsequently, those versed in economic theory may have trouble envisaging a more complex process, whereby a seemingly tranquil period can contain the seeds of its own demise. This causes a mental separation of the boom and the bust periods, resulting in a failure to deal with either.

Argument #2: ““The EMH claims that crises are unpredictable, so the fact that economists didn't predict the crisis is not a problem for economics at all.”

As far as I'm aware, this argument was first used by John Cochrane, and it has reappeared multiple times since then: it was more recently referenced by Andrew Lilco, who was sadly echoed by the generally infallible Chris Dillow. The idea is that financial markets process new information faster than any one individual, government or institution could, and so for most people they may seem to behave unpredictably. However, economists cannot be expected to understand these sudden movements better than anyone else, so expecting them to foresee market crashes is absurd. As Cochrane puts it, “it makes no sense whatsoever to try to discredit efficient market theory in finance because its followers didn't see the crash coming”.

However, this logic is completely circular. The mere fact that a theory exists which *claims* crises are unpredictable does not mean that, if a crisis is not predicted – particularly by the proponents of said theory – this shows the theory is correct. If the EMH had, to the best of our knowledge, been shown to be correct, then the EMH-twist might hold some water, but we must establish this truth separately from the fact its proponents didn't predict the crisis (David Glasner recently made a similar point about the ubiquitous use of rational expectations in macroeconomics). While Cochrane does claim that the central tenet of the EMH “is probably the best-tested proposition in all the social sciences”, he fails to reference supporting evidence, and in fact goes on to add substantial qualifications to the empirical record of the EMH, admitting that market volatility might happen “because people are prey to bursts of irrational optimism and pessimism”.

It is not necessarily my aim to establish the truth or falsity of the EMH here: it has been discussed extensively elsewhere. However, there are a couple of key tests for whether or not it applies to 2008. The first is whether or not *anybody* - both adherents and detractors of the theory – foresaw the crisis. While the EMH claims nobody could, this is clearly wrong: some people in finance made a lot of money; some economists not only called it but had frameworks that explained it well once it happened; quite a few people (even mainstream economists) at least noted the existence of a housing bubble. The EMH can attribute these predictions to simple luck, but now we're back to circularity: assume the EMH is true, then appeal to it to rationalise any possible market movement. The second test of the EMH, since it depends on new information to trigger volatility, is to ask exactly what new information became available just before the crash. However, the financial instruments key to 2008 were used by investment banks for a good few years prior to the crash, so it's quite difficult to claim that new information about these suddenly became available in 2007-8. Instead, what happened was a collective realisation that everyone *knew very little* about the products they'd been trading, resulting in a classic panic.

In fairness, there is an element of truth to the EMH-twist. Financial markets *are* incredibly difficult to understand, and the argument that economists don't *yet* understand them, along with a *mea culpa*, might be acceptable – there are many things natural scientists still don't understand, such as dark matter, or what happened 'before' the big bang. However, the EMH-twist as used by Cochrane et al is phrased more strongly: it is the assertion that economists *can't and shouldn't* understand the movements of financial markets, simply because the EMH allows them to wash their hands of the task. We wouldn't accept this kind of attitude from any other field, so I can't help but feel Cochrane's claim that “the economist's job is not to 'explain' market fluctuations after the fact” can only be met with: “then what is the economists' job, exactly?”

Argument #3: “Economists aren't oracles. Just as seismologists don't predict earthquakes and meteorologists don't predict the weather, we can't be expected to predict recessions.”

This argument initially sounds quite persuasive: the economy is complex, and the future inherently unknowable, so we shouldn't expect economists to predict the future any better than we'd expect from other analysts of complex systems. However, the argument is actually a straw man of what critics mean when they say economists didn't foresee the recent crisis. It confuses conditional predictions of the form "if you don't do something about x, y might happen" with oracle-esque predictions of the form "y is going to happen on December 2003". Nobody should have expected the details of crisis – many of which were hidden – to be foreseen, and much less a prediction about exactly which banks would fail and when. Instead, what is expected is for economists to have the key indicators right and know how to deal with them, to be alert to the possibility of crisis at all times – even in seemingly tranquil periods – and to have measures in place to cushion the blow should a crisis occur.

In fact, those who study earthquakes or hurricanes *do* 'predict' them in the above sense: they understand where they're most likely to occur (for example near fault lines), and at roughly which frequency, time and magnitude. They also have an idea of how best to combat them: areas which are prone to earthquakes and hurricanes – funding permitting – have dwellings built in such a way that they can withstand such occurrences. They understand *why* disasters happen, and their models tell us why they cannot be predicted. For example, it is common knowledge that weather forecasts get less accurate the further away they are due to the sensitivity of the model to initial conditions, a point based on complex mathematics but communicated well by meteorologists (not to mention that weather forecasts are improving all the time).

While there's been a lot of kerfuffle over exactly who 'predicted' the crisis and what that means, the most important point is that those who did warn of a crisis like the one we're going through identified key mechanisms (debt build up, asset price bubbles, global imbalances) and argued that, unless these processes were combated, we'd be in danger. I appreciate that the 'stopped clock' problem really is a problem: there are so many people predicting crises that eventually, one of them will seem to be right. However, this is easily countered by using the same framework to make predictions outside the crisis (predictions in the general sense of the word, not just about the future). For example, Peter Schiff predicted a financial crisis quite a lot like the one we've been through, but he also predicted hyperinflation, suggesting that his model is wrong in some way. Conversely, endogenous money models are consistent with both the financial crisis and the subsequent weak effects of monetary stimulus: since money is created as debt, private debt can have major effects on the economy, and since banks do not lend based on reserves, there's no reason for an increased monetary base do produce inflation.

Finally, while natural disasters are almost entirely exogenous phenomena, the economy is a social system, so we have a degree of control over it, both individually and collectively. It's perhaps a testament to how the neoclassical approach naturalises the economic system that some economists

feel recessions can be compared to natural disasters (not that this would mean they had no responsibility for alleviating their effects). Since economic models are frequently used to inform government policy, it's quite clear that economists appreciate this point; however, since they often admit they don't really understand what causes recessions, they are doing the equivalent of sending us up in toy planes. It's fair to say that you don't fully understand the economy; it's quite another thing to say this, and then recommend ways to manage the economy. But the relationship between economists and policy brings me to the next argument:

Argument #4: "Mainstream economics cannot be blamed for politicians inflating housing bubbles/pursuing austerity/deregulating the financial sector; our models generally go against this. Clearly, we do not have that much influence over policy."

This really raises two questions. The first is whether or not economic theory has had a major influence on policy. The second is whether or not this influence, if it exists, is culpable in creating the financial crisis.

The first is, in my opinion, easily answered in the affirmative. While it's entirely understandable that the majority of academic economists would scoff at the idea that they effect policy, this doesn't have to be the case for economic theory itself to hold sway among governments. After all, economics graduates are highly sought after and employed in policymaking positions. Famous economists lunch with the president; textbooks and macroeconomic papers are full of policy discussions; prize-winning economists such as Bob Shiller acknowledge that a "problem with economics is that it is necessarily focused on policy, rather than discovery of fundamentals." It's hard to imagine powerful institutions such as Central Banks, the World Bank or the IMF functioning with advice from any *but* economists, and government organisations are even set up based on new ideas coming out of economics. Economics is the language in which the media discuss policy: demand, stimulus, markets, and etcetera. I could go on.

However, as economists like to remind us, there's no reason to believe that advice based on mainstream economic theory should have led to the types of 'free market' policies typically implicated in the financial crisis and its aftermath. Even a basic economics education will leave you with an awareness of things like information asymmetry, moral hazard and externalities, and few economists support wanton deregulation of the financial sector. Modern macroeconomics is loosely pro-stimulus, not pro-austerity. So what's going on?

First, it should be noted that not only 'free market' thinking was implicated in the crisis. Central Banks around the world used inflation targeting, based on the New Keynesian idea that this would be sufficient to achieve macroeconomic stability, which blinded them to problems brewing in the financial

sector. What's more, the approach to regulation favoured by economics was, not atypically, quite narrow and didn't favour systemic thinking. For example, I have previously spoken about Value at Risk (VaR) regulation, which forces firms to sell off assets when markets are volatile and hence increase their insurance against risk. However, while this looks good from the perspective of individual firms, it worsens systemic risk because the asset sell-offs result in increased volatility. Overall, the reductionist nature of economic theory tended to blind policymakers to systemic problems and made them focus on the wrong variables, things they might not have done if they'd been familiar with more holistic viewpoints.

Having said this, it's clear that at the heart of the financial crisis were lax regulatory policies, justified by a belief in the self-stabilising power of financial markets. And while a majority of individual economists may not endorse such a view, theoretical frameworks or 'ways of thinking' came out of economics which were used to justify this deregulation. Whether or not efficient markets, perfect competition, rational expectations and other theories which imply financial markets will run smoothly are endorsed by most economists, the fact that they are common knowledge in economics (and usually the benchmark for more complex analysis) is significant. As I've argued before, familiarity with economic theory *lends itself* to a pro-market view, even if a lot of modern work is done pushing the core framework away from this. And as I've argued before, the nuances of this work are often lost in popular translation, as the elegance of the most Panglossian theories proves too tempting when economists speak to the public. Alternative theories which use different starting points for analysis, such as input-output matrices, sectoral balances, or class struggle, would help to combat the deeply ingrained nature of the neoclassical theories.

This issue does not necessarily fit into a narrow 'government versus market' policy perspective. Instead, the point is that acknowledging different approaches in economic theory can give us a different way of thinking about policy, illuminating rather than obfuscating debates. A key complaint about economics graduates is that they have overly narrow, abstract tools, so the enemy is not so much any particular approach as it is one sided thinking. Providing both economics students and professional economists with an awareness of different theories, as well as making economics more politically, historically and ethically engaged, would hopefully at least temper the zeal and enthusiasm with which pet policies are recommended, and partially dislodge whatever pedestal economics currently sits on as a rationale for policy.

Argument #5: "We got this one wrong, sure, but we've made (or are making) progress in macroeconomics, so there's no need for a fundamental rethink."

Many macroeconomists deserve credit for their mea culpa and subsequent refocus following the financial crisis. Nevertheless, the nature of the rethink, particularly the unwillingness to abandon

certain modelling techniques and ideas, leads me to question whether progress can be made without a more fundamental upheaval. To see why, it will help to have a brief overview of how macro models work.

In macroeconomic models, the optimisation of agents means that economic outcomes such as prices, quantities, wages and rents adjust to the conditions imposed by input parameters such as preferences, technology and demographics. A consequence of this is that sustained inefficiency, unemployment and other chaotic behaviour usually occur when something 'gets in the way' of this adjustment. Hence economists introduce ad hoc modifications such as sticky prices, shocks and transaction costs to generate sub-optimal behaviour: for example, if a firm's cost of changing prices exceeds the benefit, prices will not be changed and the outcome will not be Pareto efficient. Since there are countless ways in which the world 'deviates' from the perfectly competitive baseline, it's mathematically troublesome (or impossible) to include *every* possible friction. The result is that macroeconomists tend to decide which frictions are important based on real world experience: since the crisis, the focus has been on finance. On the surface this sounds fine – who isn't for informing our models with experience? However, it is my contention that this approach does not offer us any more understanding than would experience alone.

Perhaps an analogy will illustrate this better. I was once walking past a field of cows as it began to rain, and I noticed some of them start to sit down. It occurred to me that there was no use them doing this *after the storm started*; they are supposed to give us adequate warning by sitting down before it happens. Sitting down during a storm is just telling us what we already know. Similarly, although the models used by economists and policy makers did not predict and could not account for the crisis before it happened, they have since built models that try to do so. They generally do this by attributing the crisis to frictions that revealed themselves to be important during the crisis. *Ex post*, a friction can always be found to make models behave a certain way, but the models do not make identifying the source of problems before they happen any easier, and they don't add much afterwards, either – we certainly didn't need economists to tell us finance was important following 2008. In other words, when a storm comes, macroeconomists promptly sit down and declare that they've solved the problem of understanding storms. It becomes difficult to escape the circularity of defining the relevant friction by its outcome, hence stripping the idea of 'frictions' of predictive power or falsifiability.

There is also the open question of whether understanding the impact of a 'friction' relative to a perfectly competitive baseline entails understanding its impact in the real world. As theorists from Joe Stiglitz to Yanis Varoufakis have argued, neoclassical economics is trapped in a permanent fight against indeterminacy: the quest to understand things relative to a perfectly competitive, microfounded baseline leads to aggregation problems and intractable complexities that, if included, result in "anything goes" conclusions. To put in another way, the real world is so complex and full of

frictions that whichever mechanics would be driving the perfectly competitive model are swamped. The actions of individual agents are so intertwined that their aggregate behaviour cannot be predicted from each of their 'objective functions'. Subsequently, our knowledge of the real world must be informed by either models which use different methodologies or, more crucially, by historical experience.

Finally, the ad hoc approach also contradicts another key aspect of contemporary macroeconomics: microfoundations. The typical justification for these is that, to use the words of the ECB, they impose "theoretical discipline" and are "less subject to the Lucas critique" than a simple VAR, Old Keynesian model or another more aggregative framework. Yet even if we take those propositions to be true, the modifications and frictions that are so crucial to making the models more realistic are often *not* microfounded, sometimes taking the form of entirely arbitrary, exogenous constraints. Even worse is when the mechanism is profoundly unrealistic, such as prices being sticky because firms are randomly unable to change them for some reason. In other words, macroeconomics starts by sacrificing realism in the name of rigour, but reality forces it in the opposite direction, and the end result is that it has neither.

Macroeconomists may well defend their approach as just a 'story telling' approach, from which they can draw lessons but which isn't meant to hold in the same manner as engineering theory. Perhaps this is defensible in itself, but (a) personally, I'd hope for better and (b) in practice, this seems to mean each economist can pick and choose whichever story they want to tell based on their prior political beliefs. If macroeconomists are content conversing in mathematical fables, they should keep these conversations to themselves and refrain from forecasting or using them to inform policy. Until then, I'll rely on macroeconomic frameworks which are less mathematically 'sophisticated', but which generate *ex ante* predictions that cover a wide range of observations, and which do not rely on the invocation of special frictions to explain persistent deviations from these predictions.

Argument #6: "Sure, modern macroeconomics is pretty weak. But most economists don't even work on macro, so they are unaffected."

Quite a lot of economists consider the debate about the financial crisis irrelevant to what they do. After all, why should a crisis at the macro level invalidate econometrics, game theory or auction theory? Attacking these fields and others for the recession is like blaming mechanical engineers for a bridge collapse. In fact, many economists hold macro in the same (low) esteem as the public: Daniel Hamermesh goes so far as to claim that "most of what the macro guys do in academia is just worthless rubbish", but adds that the kind of field he works in "has contributed tremendously and continues to contribute". Even some of the discipline's most vehement defenders are willing to concede macroeconomics is bunk.

There is a considerable amount of truth to this view. While there may be *critiques* of all areas in economics, the claim that the financial crisis is what's thrown them into disrepute is a non sequitur. Critics should therefore be careful to distinguish macroeconomists from their colleagues when (rightly) dismissing the former's failure to deal with the crisis. Nevertheless, there are two major ways in which the failings of macroeconomics are symptomatic of more general problems with economic theory, so the discipline as a whole cannot be let off the hook.

The first is a lack of holism. A large amount of economic theories are built in an abstract theoretical vacuum, with little reference to what is happening around the individual agent. But the importance of the macroeconomy for behaviour in specific sectors or by specific actors cannot be ignored. For example, if you drop the macroeconomic assumption of full employment, this affects theories in areas from public goods provision to labour markets to Walrasian equilibrium. Consumers' and firms' expectations are strongly informed by the macroeconomic and political environment around them. Considering the effects of political institutions such as unions on the labour market, but ignoring their broader political role, can create narrow and misguided conclusions about their efficacy. New Institutional economics often takes 'institutions' as exogenous, failing to consider to two-way interaction between institutions and agents. The in-vogue 'Randomised Control Trial' restricts the economic environment to such a degree that it's questionable whether one can generalise the results at all. And so forth.

Don't get me wrong: there is an obvious case for different areas of economics being separate from one another: taking certain parameters as exogenous to look at a certain area, and using different tools for different areas. But even the most specialised fields should never forget the broader scope and context of their ideas, and this should be reflected in the theoretical approach. Thomas Piketty's *Capital* is a shining example of how to intertwine theory, history, statistics and politics to build a better understanding of capitalism. Another is the attempt by ecological economists to place the economy in its environmental context, rather than simply taking resource endowments as a given and assuming pollution just sort of...disappears, save for its monetary cost. Minsky's Financial Instability Hypothesis shows one way to make an effective link between the behaviour of investors and broader economic performance, integrating finance and macroeconomics. Overspecialisation may cause economists to miss these key insights.

The second issue is that many of the problems with macroeconomics can be applied to, or are relevant for, other areas of the discipline. One of the key complaints about macroeconomics – that it relies on microfoundations – is a problem *precisely because* it imports unrealistic assumptions about economic behaviour from microeconomics. The problem of having an abundance of abstract models, each seeking to explain one or two 'things', but with no real way to tell which model is applicable and

when, applies not just to macroeconomics but also to behavioural economics, microeconomics, oligopoly theory. Endogenous money, which is central to macroeconomists' lack of understanding of the crisis, also has major implications for finance. To reuse my above analogy, you might well be concerned about mechanical engineers after a bridge collapse if they largely relied on the same methods used by the civil engineers.

Your average economist is probably right to point out that the public's ire should be focused not on them, but on macroeconomics. However, this doesn't mean that they are immune from the serious questions the crisis raised about the methodology, assumptions and ethics of the field. It's a case-by-case matter which areas are impacted and by how much, but any attempt to box off macroeconomic theory entirely should be resisted. There's plenty of room for fruitful debate about all areas of economic theory, much of which will benefit from being informed by the shortcomings of economic theory as exposed by the financial crisis.

Argument #7: "Economists had the tools in place, but we overspecialised and systemic problems caught us off guard."

Raghuram Rajan was probably the first to take this sort of line, arguing that overspecialisation prevented economists from using the tools they had to foresee and deal with the crisis. But while Rajan's piece also made a number of other criticisms of economics, over time the discipline seems to have reasserted this argument more strongly: not too long ago, Paul Krugman argued that although "few economists saw the crisis coming...basic textbook macroeconomics has performed very well". Similarly, Tim Harford claimed at an INET conference last year that the tools necessary to understand the crisis already existed in mainstream economics, and the problem was simply one of knowing when and how to use them. He compared financial crises to engineering disasters, which were understandable using current knowledge but happened nonetheless, due to negligence or oversight on the part of the engineers.

So how true is this claim? Certainly, a number of economic models exist for understanding things like panics, liquidity problems and moral hazard. The most well-known of these are the Diamond-Dybvig (DD) model of bank runs – which shows what happens when banks have liquid liabilities (such as demand deposits) which must be available at any time, but have illiquid assets (such as loans) which are not fully convertible to cash on demand – and the Akerlof-Romer (AR) model of financial 'looting', which shows that deposit guarantees may create moral hazard as investors gamble other peoples' money. If you combine tools like these, which help us understand the financial sector, with tools like IS/LM, which tell us how to escape a downturn once it happens, in theory you have a pretty solid set of tools for dealing with the recent crisis.

The first objection I have to these models is that many of their insights could be considered trivial or at least common sense. The DD model came to the conclusion that deposit insurance might be helpful way to prevent bank runs, which is hardly a revelation considering it came 50 years after FDR and the general public figured out the same thing. The AR model came to the conclusion that deposit insurance and limited liability might create perverse incentives as banks gamble 'other people's money', which again must have been obvious to the policymakers who put Glass-Steagall and other financial regulations in place. Perhaps this point is a little harsh, and I don't want to overstate it: on the whole, these papers are asking important questions, and in the case of AR they answer them well. Nevertheless, there's no point in economic theory if it can't tell us things we didn't already know. Even the idea that central banks should provide emergency liquidity to banks in trouble is quite obvious, and it predates modern economic theory by a good while.

However, this is not the most important point. The issue I have with these models is that in many of them everything interesting happens *outside the model*. In Krugman's favoured IS/LM, a 'crisis' is represented by a simple shift in the IS curve, which in English means that a decline in production is caused by...a decline in production. Where this decline came from is presumably a matter for outside the model. Even the most sophisticated macroeconomic models often follow a similar tack, merely describing what happens when the economy suffers from a shock, without exploring possible causes for the shock. Likewise, the DD model suggests bank runs happen because everyone panics, but what causes these panics is not explored: it is assumed depositors' expectations are exogenous, whether fixed or following a stochastic (random) pattern. Yet studies such as Mishkin (1991) find that bank runs generally follow periods of stress elsewhere in the economy, a fact which DD simply cannot capture.

Economic models are narrowly focused like this because they are generally designed to answer straightforward questions about causality: does the minimum wage cause unemployment; does expansionary fiscal policy cause growth; does a mismatch between illiquid assets and liquid liabilities cause bank runs. But the crisis was an endogenously generated process in which different aspects of the economy – the housing market, the financial sector, government policy – combined to create something bigger than the sum of its parts, and in which it is not possible to isolate a single cause. Consider: the collapse of Lehman Brothers may have *triggered* the worst of the crisis, but was it really to *blame*? The economy was already in a fragile place due to systemic trends that can't necessarily be traced to a single law, institution or actor. Just like the murder of Franz Ferdinand in World War 1, we have to look beyond the immediate and focus on the general if we truly want to understand what happened.

To sum up, the economists above want to argue that they are only culpable insofar as they overspecialised and failed to focus on the right areas *in this particular instance*. However, the reason for this was not just because of personal myopia; it's because their chosen methodology means they lack the tools to do so. A model of one aspect of the economy which takes the effect

of other areas as exogenous will fail to detect potential positive feedback loops and emergent properties. A model which takes the crisis itself as an exogenous 'shock' is even worse, and in many ways is hardly a model of the crisis at all, since it offers no understanding of why crises might happen in the first place. Are there alternatives? I have previously written about how post-Keynesian and Marxist models offer more comprehensive understandings of the financial crisis and antecedent decades; I shan't repeat myself here. Other promising areas include network theory, evolutionary economics and Agent-Based Modelling. All of these models share that they take the system as a whole instead of focusing on isolated mechanics.

Conclusion

I see the crisis in economics as a shock (!!) which hits macroeconomics hard and reverberates throughout the discipline. Regardless of the pleas of some, such events *can* be seen coming, and they cannot be hand waved away as part of an overall upward trend. And even if individual economists are not in control of policy, key economists have substantial influence, not to mention the theories and ideas in economics as a whole. Recent developments in macroeconomics still leave a lot to be desired, while previously existing tools suffer from similar problems: a lack of holism; a wooden insistence on microfoundations; and attempt to understand everything in terms of simplistic causal links, often relative to a frictionless baseline. Finally, although many areas of economics are not *directly* indicted by the crisis, many of them share key problems with macroeconomics, and as such the crisis should prompt at least a degree of introspection throughout the discipline.