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# The unfortunate uselessness of most 'state of the art' academic monetary economics

March 3, 2009 1:37pm |

The Monetary Policy Committee of the Bank of England I was privileged to be a 'founder' external member of during the years 1997-2000 contained, like its successor vintages of external and executive members, quite a strong representation of academic economists and other professional economists with serious technical training and backgrounds. This turned out to be a severe handicap when the central bank had to switch gears and change from being an inflation-targeting central bank under conditions of orderly financial markets to a financial stability-oriented central bank under conditions of widespread market illiquidity and funding illiquidity. Indeed, the typical graduate macroeconomics and monetary economics training received at Anglo-American universities during the past 30 years or so, may have set back by decades serious investigations of aggregate economic behaviour and economic policy-relevant understanding. It was a privately and socially costly waste of time and other resources.

Most mainstream macroeconomic theoretical innovations since the 1970s (the New Classical rational expectations revolution associated with such names as Robert E. Lucas Jr., Edward Prescott, Thomas Sargent, Robert Barro etc, and the New Keynesian theorizing of Michael Woodford and many others) have turned out to be self-referential, inward-looking distractions at best. Research tended to be motivated by the internal logic, intellectual sunk capital and esthetic puzzles of established research programmes rather than by a powerful desire to understand how the economy works - let alone how the economy works during times of stress and financial instability. So the economics profession was caught unprepared when the crisis struck.

**Complete markets**

The most influential New Classical and New Keynesian theorists all worked in what economists call a 'complete markets paradigm'. In a world where there are markets for contingent claims trading that span all possible states of nature (all possible contingencies and outcomes), and in which intertemporal budget constraints are always satisfied by assumption, default, bankruptcy and insolvency are impossible. As a result, illiquidity - both funding illiquidity and market illiquidity - are also impossible, unless the guilt-ridden economic theorist imposes some unnatural (given the structure of the models he is working with), arbitrary friction(s), that made something called 'money' more liquid than everything else, but for no good reason. The irony of modeling liquidity by imposing money as a constraint on trade was lost on the profession.

Both the New Classical and New Keynesian complete markets macroeconomic theories not only did not allow questions about insolvency and illiquidity to be *answered*. They did not allow such questions to be *asked*.

It is clear that, when searching for an appropriate simplification to address the intractable mess of modern market economies, the starting point of 'no markets', that is, autarky or no trade, is a much better one than that of 'complete markets'. Goods and services that are potentially tradable are indexed by time, place and state of nature or state of the world. Time is a continuous variable, meaning that for complete markets along the time dimension alone, there would have to be rather more markets for future delivery (infinitely many in any time interval, no matter how small) than you can shake a stick at. Location likewise is a continuous variable in a 3-dimensional space. Again rather too many markets. Add uncertainty (states of nature or states of the world), never mind private or asymmetric information, and 'too many potential markets', if I may ruin the wonderful quote from Amadeus attributed to Emperor Joseph II, comes to mind. If any market takes a finite amount of resources (however small) to function, complete markets would exhaust the resources of the universe.

Beyond this simple 'impossibility of complete markets' proposition, there is the deeper point, that the assumption of complete markets in most of the New Classical and New Keynesian macroeconomics assumes away the problem of contract enforcement. This problem is especially acute in trade over time or intertemporal trade, where the net value to each party to a contract of fulfilling the terms of the contract varies over time and can change sign. In a world with selfish, rational, opportunistic agents, able and willing to lie and deceive, only a small set of voluntary transactions will ever be observed, relative to the universe of all potentially feasible transactions.

The first set of voluntary exchange-based transactions we are likely to see are self-enforcing contracts - those based on long-term relationships, repeated interactions and trust. There are some of those, but not too many. The second are those voluntarily-entered-into contracts that are not self-enforcing (say because interactions between the same sets of agents are infrequent and market participants have a degree of anonymity that prevents the use of reputation as a self-enforcement mechanism) but are instead enforced by some external agent or third party, often the state, sometimes the Mafia (sometimes it's hard to tell who is who). Third party enforcement of contracts is again often complex and costly, which is why it covers relatively few contracts. It requires that the terms of the contract and the contingencies it contains be third-party observable and verifiable. Again, only a limited set of exchanges can be supported this way.

The conclusion, boys and girls, should be that trade - voluntary exchange - is the exception rather than the rule and that markets are inherently and hopelessly incomplete. Live with it and start from that fact. The benchmark is no trade - pre-Friday Robinson Crusoe autarky. For every good, service or financial instrument that plays a role in your 'model of the world', you should explain why a market for it exists - why it is traded at all. Perhaps we shall get somewhere this time.

**The Auctioneer at the end of time**

In both the New Classical and New Keynesian approaches to monetary theory (and to aggregative macroeconomics in general), the strongest version of the efficient markets hypothesis (EMH) was maintained. This is the hypothesis that asset prices aggregate and fully reflect all relevant fundamental information, and thus provide the proper signals for resource allocation. Even during the seventies, eighties, nineties and noughties before 2007, the manifest failure of the EMH in many key asset markets was obvious to virtually all those whose cognitive abilities had not been warped by a modern Anglo-American Ph.D. eduction. But most of the profession continued to swallow the EMH hook, line and sinker, although there were influential advocates of reason throughout, including James Tobin, Robert Shiller, George Akerlof, Hyman Minsky, Joseph Stiglitz and behaviourist approaches to finance. The influence of the heterodox approaches from within macroeconomics and from other fields of economics on mainstream macroeconomics - the New Classical and New Keynesian approaches - was, however, strictly limited.

In financial markets, and in asset markets, real and financial, in general, today's asset price depends on the view market participants take of the likely future behaviour of asset prices. If today's asset price depends on today's anticipation of tomorrow's price, and tomorrow's price likewise depends on tomorrow's expectation of the price the day after tomorrow, etc. *ad nauseam*, it is clear that today's asset price depends in part on today's anticipation of asset prices arbitralily far into the future. Since there is no obvious finite terminal date for the universe (few macroeconomists study cosmology in their spare time), most economic models with rational asset pricing imply that today's price depend in part on today's anticipation of the asset price in the infinitely remote future.

What can we say about the terminal behaviour of asset price expectations? The tools and techniques of dynamic mathematical optimisation imply that, when a mathematical programmer computes an optimal programme for some constrained dynamic optimisation problem he is trying to solve, it is a requirement of optimality that the influence of the infinitely distant future on the programmer's criterion function today be zero.

And then a small miracle happens. An optimality criterion from a mathematical dynamic optimisation approach is transplanted, lock, stock and barrel to the behaviour of long-term price expectations in a decentralised market economy. In the mathematical programming exercise it is clear where the terminal boundary condition in question comes from. The terminal boundary condition that the influence of the infinitely distant future on asset prices today vanishes, is a 'transversality condition' that is part of the necessary and sufficient conditions for an optimum. But in a decentralised market economy there is no mathematical programmer imposing the terminal boundary conditions to make sure everything will be all right.

The common practice of solving a dynamic general equilibrium model of a(n) (often competitive) market economy by solving an associated programming problem, that is, an optimisation problem, is evidence of the fatal confusion in the minds of much of the economics profession between shadow prices and market prices and between transversality conditions that are an integral part of the solution to an optimisation problem and the long-term expectations that characterise the behaviour of decentralised asset markets. The efficient markets hypothesis assumes that there is a friendly auctioneer at the end of time - a God-like father figure - who makes sure that nothing untoward happens with long-term price expectations or (in a complete markets model) with the present discounted value of terminal asset stocks or financial wealth.

What this shows, not for the first time, is that models of the economy that incorporate the EMH - and this includes the complete markets core of the New Classical and New Keynesian macroeconomics - are not models of decentralised market economies, but models of a centrally planned economy.

The friendly auctioneer at the end of time, who ensures that the right terminal boundary conditions are imposed to preclude, for instance, rational speculative bubbles, is none other than the omniscient, omnipotent and benevolent central planner. No wonder modern macroeconomics is in such bad shape. The EMH is surely the most notable empirical fatality of the financial crisis. By implication, the complete markets macroeconomics of Lucas, Woodford et. al. is the most prominent theoretical fatality. The future surely belongs to behavioural approaches relying on empirical studies on how market participants learn, form views about the future and change these views in response to changes in their environment, peer group effects etc. Confusing the equilibrium of a decentralised market economy, competitive or otherwise, with the outcome of a mathematical programming exercise should no longer be acceptable.

So, no Oikomenia, there is no pot of gold at the end of the rainbow, and no Auctioneer at the end of time.

**Linearize and trivialize**

If one were to hold one's nose and agree to play with the New Classical or New Keynesian complete markets toolkit, it would soon become clear that any potentially policy-relevant model would be highly non-linear, and that the interaction of these non-linearities and uncertainty makes for deep conceptual and technical problems. Macroeconomists are brave, but not that brave. So they took these non-linear stochastic dynamic general equilibrium models into the basement and beat them with a rubber hose until they behaved. This was achieved by completely stripping the model of its non-linearities and by achieving the transsubstantiation of complex convolutions of random variables and non-linear mappings into well-behaved additive stochastic disturbances.

Those of us who have marvelled at the non-linear feedback loops between asset prices in illiquid markets and the funding illiquidity of financial institutions exposed to these asset prices through mark-to-market accounting, margin requirements, calls for additional collateral etc. will appreciate what is lost by this castration of the macroeconomic models. Threshold effects, critical mass, tipping points, non-linear accelerators - they are all out of the window. Those of us who worry about endogenous uncertainty arising from the interactions of boundedly rational market participants cannot but scratch our heads at the insistence of the mainline models that all uncertainty is exogenous and additive.

Technically, the non-linear stochastic dynamic models were linearised (often log-linearised) at a deterministic (non-stochastic) steady state. The analysis was further restricted by only considering forms of randomness that would become trivially small in the neigbourhood of the deterministic steady state. Linear models with additive random shocks we can handle - almost !

Even this was not quite enough to get going, however. As pointed out earlier, models with forward-looking (rational) expectations of asset prices will be driven not just by conventional, engineering-type dynamic processes where the past drives the present and the future, but also in part by past and present anticipations of the future. When you linearize a model, and shock it with additive random disturbances, an unfortunate by-product is that the resulting linearised model behaves either in a very strongly stabilising fashion or in a relentlessly explosive manner. There is no 'bounded instability' in such models. The dynamic stochastic general equilibrium (DSGE) crowd saw that the economy had not exploded without bound in the past, and concluded from this that it made sense to rule out, in the linearized model, the explosive solution trajectories. What they were left with was something that, following an exogenous random disturbance, would return to the deterministic steady state pretty smartly. No L-shaped recessions. No processes of cumulative causation and bounded but persistent decline or expansion. Just nice V-shaped recessions.

There actually are approaches to economics that treat non-linearities seriously. Much of this work is numerical - analytical results of a policy-relevant nature are few and far between - but at least it attempts to address the problems as they are, rather than as we would like them lest we be asked to venture outside the range of issued we can address with the existing toolkit.

The practice of removing all non-linearities and most of the interesting aspects of uncertainty from the models that were then let loose on actual numerical policy analysis, was a major step backwards. I trust it has been relegated to the dustbin of history by now in those central banks that matter.

**Conclusion**

Charles Goodhart, who was fortunate enough not to encounter complete markets macroeconomics and monetary economics during his impressionable, formative years, but only after he had acquired some intellectual immunity, once said of the Dynamic Stochastic General Equilibrium approach which for a while was the staple of central banks' internal modelling: *"It excludes everything I am interested in"*. He was right. It excludes everything relevant to the pursuit of financial stability.

The Bank of England in 2007 faced the onset of the credit crunch with too much Robert Lucas, Michael Woodford and Robert Merton in its intellectual cupboard. A drastic but chaotic re-education took place and is continuing.

I believe that the Bank has by now shed the conventional wisdom of the typical macroeconomics training of the past few decades. In its place is an intellectual potpourri of factoids, partial theories, empirical regulaties without firm theoretical foundations, hunches, intuitions and half-developed insights. It is not much, but knowing that you know nothing is the beginning of wisdom.

March 3, 2009 1:37pm in Culture, Economics, Financial Markets, Monetary Policy, Politics, Religion | 57 comments

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## Comments

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**Jon Stern** | March 3 2:39pm | Permalink Report

This is an important blog. I strongly agree that the crisis over the last 18 months shows the need to rethink macro-economics, including revisiting the thinking of the past. I'm delighted to hear that the Bank of England is now being far more eclectic in its thinking about relevant processes and modelling approaches

Economics regularly needs to redicover past insights. Every long period booom seems to produce a major revival of macro-models with super strong equilibrating tendencies which effectively remove uncertainty.

I was lucky enough to learn my economics in Cambridge the late 1960s when Keynes' pupils who had lived through and come of age in the 1930s were still active and teaching. So the notion that uncertainty can be captured by risk distributions never took hold in mind. We learnt that co-ordination failures and jumps in expectations could lead to widespread market failure - not often but maybe once every 25 or 50 years. Of course, with such lines of reasoning, there are risks of predicting far too many recessions and being too demand oriented but to deny the risks of major lurches in finacial markets and liquidity preference also runs major risks as we have seen.

The problem is that, as your blog clearly reveals, it is hugely difficult to develop coherent, mathematically coherent models that capture these 'Keynesian' truths. But, forecasters and polcy makers need models and professional status, at least in academia, is heavily bound up with building coherent, usually very complex models.

I haven't been actively involved in macro-economics for some years but remain an interested and semi-engaced observer. My hunch is that we should learn (and teach) more economic history as well as the history of economic thought. It is no coincidence that many of the most successful economic policy makers (like Alec Cairncross or now Ben Bernanke) have a background in economic history. There, you can't assume away the uncertainties.

PS I am currently reading Liaquat Ahamed's wonderful book 'Lords of Finance' on central banking in the 1920s and after the collapse of the Gold Standard? Are there things that you are recomending to read that would help us think about these issues more clearly?

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**NC** | March 3 2:39pm | Permalink Report

Perhaps Mr. Buiter shouldn't have stopped reading papers on monetary economics a decade ago.

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**User3087714** | March 3 4:09pm | Permalink Report

It is a wonderful piece of hindsight. It is all true, but writing this things now (as opposed to 10 years ago) is rather easy.

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**Donald Pretari** | March 3 5:00pm | Permalink Report

"The future surely belongs to behavioural approaches relying on empirical studies on how market participants learn, form views about the future and change these views in response to changes in their environment, peer group effects etc. Confusing the equilibrium of a decentralised market economy, competitive or otherwise, with the outcome of a mathematical programming exercise should no longer be acceptable."

I agree, and I'm reminded of a quote from my favorite philosopher, J.L. Austin:

"...our common stock of words embodies all the distinctions men have found worth drawing, and the connections they have found worth marking, in the lifetime of many generations: these surely are likely to be more numerous, more sound, since they have stood up to the long test of survival of the fittest, and more subtle, at least in all ordinary and reasonable practical matters, than any that you or I are likely to think up in our armchair of an afternoon – the most favorite alternative method."

I think that a Wittgensteinian/Austinian anlysis of the presuppositions and assumptions of Economic Models would be a worthwhile endeavor. Many of the models seem to presuppose a crude Behaviorism, although it's hard to tell, since it's not clear that they're claiming to be anything more than modelers.

I can't help but wonder if the desire to be scientific and professional has led to a sterile pursuit of clearly limited problems, much as it has in philosophy.

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**Derek Tunnicliffe** | March 3 5:42pm | Permalink Report

As others have stated, this is an important blog, whether posted with hindsight or based on new thinking. Who was it who said "When the facts change, I change my view. How about you, sir?".

Political analysts have argued for years over how to interpret events. On the one side are those who need "traditions" in order to pin down 'objective facts'. Others have argued that "objectivity" (in an anthropological sense) is more subjective.

Our basic beliefs give us away. If our experience (academic training?) has led us to believe in the EMH, or in equilibrium, then it is through that lens that we will view economic events. If we encounter events that do not accord with our "objectivity" we have a choice: change our view, or not.

What Prof Buiter has shown is the need for less "interpretivism" and more uncertainty in macro-economics.

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**John** | March 3 6:17pm | Permalink Report

Congratulations! The admission to oneself of an error is the first step to a better understanding: a public admission also helps other people to that end.

It would take too long to answer this fully (even though I shall omit my own errors which are irrelevant) but I should like to make a few comments.
Firstly EMH is not empirical: it is an academic hypothesis invented by some American academics to reduce the complexity of markets by a few orders of magnitude so that they could develop some theories (a few of which are actually useful) called Modern Portfolio Theory. I was pointing out, while you were still at Yale, that EMH does not correspond to reality because it ignores DEATH and taxes (it also ignores frictional costs (not all of which are taxes), investment fashion, herd instinct and, now relevant but then insignificant, short selling). I have demonstrated from empirical data on more than one occasion that EMH is false (as have others - I make no claim to be Newton or Kepler). I have never seen any empirical data to support it (I have seen reports that in most years the average - NOT the good - actively-managed fund underperforms the index but since ALL index funds underperform the index EVERY year, this merely tells us that custodian's and auditors' charges are non-zero and nothing about the validity or otherwise of EMH).
EMH does not rely on "an Auctioneer at the End of Time" because it assumes a positive real return on investment so the value at the end of time becomes vanishingly small: please remember that EMH was invented by a bunch of American academics, not the MPC of the BoE under a Labour government. EMH does not handle the concept of a continuing negative real long-term return on investment because in that context there are no rational retail investors and (in the USA) no stock market.
I believe that an "Inefficient Markets" hypothesis comes far closer to reality that either EMH or a "No Markets" hypothesis: of course this will be hated by academics because it makes it horrendously difficult to produce any numbers to illustrate or support their theories (hence the invention of EMH).
Thirdly most contracts are small and belong in the category that you call "self-enforcing" but as a natural English speaker (so less grammatical with poorer syntax) I regard as those supported by rational/ enlightened self-interest . Most people belong to a network of small communities and so choose to deal fairly, or to appear to do so, with members of intersecting communities as well as their core community.
Fourthly, computers do not and cannot think: they merely calculate. I think that reintroducing the stocks might do more to improve the quality of macroeconomic economic advice that reliance on the most sophisticated computer programmes. It is possible to solve by algebra and analysis some problems that cannot be handled by computers

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**RCS** | March 3 7:34pm | Permalink Report

A depressing post, even though lacking an appropriate training, I barely understood a word. But is it really true that assumptions and simplifications in economic theory have gotten us nowhere? Is it really believable that decades of theorising by the best and brightest have produced nothing of lasting value, that all Nobel prizes in macro-economics in recent years were mistakenly awarded? Is that not akin to what has been argued against other sciences, for example by leading past masters of physics such as Feynman or Penrose, who have disparaged string theory? Is it not possible that some of the motivation for such criticism lies in frustration for having missed out on contributing
to leading-edge research? Who knows? I have not the tools to judge, possibly Prof Buiter is the little boy crying that the king is without clothes.

But I doubt it. Maybe much of the criticism is justified in that the wrong simplifications were made, but I cannot accept the hyperbole that NO insights were achieved. Beautiful, simple models, usually point to some underlying truth.

On the other hand it is also true that macro-economic models are rarely beautiful, usually more of an ad-hoc patchwork, set-up at the level of dynamic equations and not based on more conceptual set-theoretic, game-theoretic and topological levels.

So here is a two-pronged criticism of modern macro-economics: on the one hand it simplifies reality in the wrong ways, on the other hand is very loosely based. The first is the problem of emprircal relevance, the second the problem of theoretical foundations -- could it be they are connected?

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**prestopundit** | March 3 8:35pm | Permalink Report

You've managed to discover what Hayekian economists have been explaining for 3 decades.

Robert Lucas explicitly says time and time again that he was attempting to recapture the pre-Keynesian "equilibrium" macroeconomics pioneered by FRIEDRICH HAYEK. But as any Hayekian will tell you (see the work of Roger Garrison) Lucas blew it. Lucas "didn't get it" and sent the macroeconomists off on a wild goose chase for 30 years.

I can't recommend more highly the work of Roger Garrison to those who want to understand where economics went wrong 30 years ago -- and 70 years ago. See in particular Garrison's _Time and Money_ or the articles collected at his Auburn University web site.

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**jamaku** | March 3 11:02pm | Permalink Report

reminds me of an nice afternoon at the beach, where there was this tiny stream meandering its way towards the sea. I cought the plan to help this stream of water to reach the sea faster, by bildings smal sanddikes and making its path a strait line to the shore. To by big surprice then, i soon found out that the highly efficient way the water was streaming now was not naturally supported, and soon, after letting the system evolve at its own, the dykes were flodded and washed away and a new meandering path was formed...The economics of nature are complex and predictable only in gross simplifications i so learned. Water flows towards the sea-but don't ask how.

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**In Light of Nihilism** | March 4 7:22am | Permalink Report

"I can't help but wonder if the desire to be scientific and professional has led to a sterile pursuit of clearly limited problems, much as it has in philosophy."

Academia is a sterile beast. Most academics begin to smell like old farts by their late 20's. Several fields of thought are finished. Such as logistics, psychology, sociology etc.

As for modern philosophy, I wouldn't worry. I know of no great philosopher thats actually received formal philisophical training. If there is one, I'm sure he thought it was the garbage.

I'm happy if mankind produces one great philosopher every 100 years.

Be Happy,

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**Gary Marshall** | March 4 7:32am | Permalink Report

Hello Mr. Buiter,

When you write these long tracts, I find it is because you want to send as much of a confused message as possible. Congratulations, you have succeeded again.

The reason that monetary policies or academic monetary economics do not work is because they are founded on an absurdity: That a nation's central bank controls or greatly influences interest rates.

Not one monetarist has ever shown me how these secretive institutions perform such miracles even after being hounded. You have yet to comply with my requests made numerous times. I guess the above is finally a hint in the right direction; that monetary economics is an impotent and bizarre little fellow.

Gary Marshall

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**In Light of Nihilism** | March 4 8:50am | Permalink Report

Hello Mr. Marshall,

Firstly, lower interest rates present an opportunity for a bank to increase it's profits. Since British and U.S. banks are taking losses, lowering interest rates seem to be a fine idea

You disparrage the assertion: 'That a nation's central bank controls or greatly influences interest rates.'
One should remember that to be truely free, one must understand and recognise the historical forces that shape the present. In the past, political pressure forced a bank to 'pass on cuts' in the interest rate. Perhaps, this historical practice is finished, but has yet to be recognised by your 'monetarist' companions.

Yours,

N

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**Enrique F.** | March 4 10:23am | Permalink Report

As a graduate in Monetary Economics I fully agree with Prof. Buitler observations. For those who don´t get it yet, please open any professional Journal to see what modern economics is all about: A lot of “technical” details with pseudo mathematical models and very little insight that really makes you think. Only after reading big thinkers such as Ricardo, Marx, Keynes and even Friedman (just to mention a few) I began to grasp the real beauty of economics (and think.. not just applying fancy econometric tools).

A very short and simple insight regarding the EMH debate: BY DEFINITION it´s impossible to tie “Rationality” to “Financial Markets “. Markets do not have opinions or feelings, down in the arena there are buyers, sellers, holders etc. Transactions (the essence of markets) are the meeting points between a seller and a buyer and one of them will eventually make money while the other will lose (real or opportunity cost). Since nobody wants to lose money ex ante, at least one half of the market players is always wrong ex post (which is an ex ante error for the new transaction at a new price…) . Therefore the “rational” outcome (price) in liquid markets is the result of a miscalculation. Put it in another way: If the current price reflects all the available information in the market, how it comes that the same database serves as justification for two opposed actions? Illiquid markets are awful but ironically they reflect a wide consensus about pricing …

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**wrp1** | March 4 11:01am | Permalink Report

Is macroeconomics just another of those failed intellectual adventures with which the Universities are littered? If you add to their number the previously fertile fields that are now exhausted, it's easy to see where Universities can make economies in the next few years. But it will remain their duty to employ sceptical scholars such as yourself, Mr Boots. All strength to your arm.

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**Peter** | March 4 11:17am | Permalink Report

Professor,

I find that this post is much more than a mere "I found a flaw" statement with hindsight. It is not a manifesto either, but the most important message that I will keep is that what modern economics misses the most is humility. Humility with respect to what we know in particular, and what we can actually hope to know one day in general.

A small anecdote related to rationality in markets : I once participated in an economic experiment with some 50 students from different fields of study. The experiment setting was an auction and in each round two identical goods were sold. The goods were either Good or Bad (the latter having zero value) and at each round a signal (with some known precision) indicated if the goods were both Good or both Bad. Being an economics student, I calculated my expected values of the goods conditionally on the signal value, and during each round my bidding for both goods was a risk adjusted conditional expected value. Besides the fact that my method almost made me bankrupt in the first 3 rounds (indeed, calculated expected values cannot protect you from bad luck) I made an important observation. My opponents were constantly bidding different prices for the two IDENTICAL goods at each round. Something that I would never do (fearing the wrath of my professors). What is more, I noted that the opponents almost systematically bidded less for the second good at each round. Rational as I wanted to be, I ended up bidding a lesser price for the second IDENTICAL good and making more profit on average. My final gain was more than three times my initial endowment (it was a zero sum setting: sorry, colleagues!). My lesson: irrationality is contagious. A limited number of irrational agents can make the whole market act irrationally. I know this is nothing new, but it is quite different to have this from a concrete life experience, than from a formal theoretical model.

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**M.G. in Progress** | March 4 11:44am | Permalink Report

At http://mgiannini.b...ness-of-being.html I agree that there is an unbearable lightness of being economists. Milan Kundera made an interesting case that kitsch is the absolute denial of <span class='profanity' title='A suspected profanity was blocked'>****</span>. I contend that during these days many economists are in this respect simply kitsch. Some aren't like Prof. Buiter here as he makes an interesting case for the unfortunate uselessness of most ’state of the art’ academic monetary economics. I am not sure that is unfortunate but I would extend this uselessness analysis also to most of state of the art academic fiscal economics and engineering finance.

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**JohnRDC** | March 4 12:08pm | Permalink Report

Healthy skepticism, which this article very well represents, is in short supply. It is needed elsewhere, not just in the economics profession. On p. 2 of the Wednesday edition of the Financial Times there is a complete run-down on how the oceans will rise, the ice caps will melt, the forests die, etc., without any reference I could find to how these conclusions were obtained.

Obviously, someone somewhere has developed a mathematical/computer model of the world's climate (an impossibility of course) and has convinced the FT's editors en masse that it can be used for making accurate 100-year projections. I cannot find the words to deal with such vapid intellectual activity, except to say that page 2 illustrates that it is not only in economics where dozens of fools can make a living.

On the whole, economics malfeasance can cause a lot more real-world damage, I should say. The environmental Cassandras will be seen to have no clothes soon enough. So Mr. B is making a signal contribution in making his case for more modesty on the part of his economics peers.

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**David Vonka** | March 4 12:53pm | Permalink Report

In which sense does not this apply to any field of academic economics ? I'm doing a PhD in economics and I happen to be surrounded by (micro)econometrics people. At the first sight, econometrics looked much more scientific, with all the data and observations and stuff. But when I look closer at the results, I very rarely find something really convincing. Convincing in the sense that I would not hesitate to take the published results and build policies on them in a country, which I do not intend to flee.

One part of the field is completely lost in mathematical complexity. The value added of the complexity is unclear to me, although my master training is in mathematics. Other parts use strange assumptions about human behavior, especially the intertemporal stuff is ridiculous. The less technical papers often concentrate on questions, which are hardly relevant, but for which panel data happen to be available. They generate these 'very interesting' results on the impact of alphabetical ordering of names on school results ... Not to speak about the publication bias, where people only publish 'significant' results, or, better, just results which are similar to previously published results.

The whole community seems to be really enthusiastic and confident about their work, so I'm surely getting something wrong. But to conclude, when I'm the prime minister, I will not hire an econometrician to explain me how policies should be set up. And now Willem Buiter tells me that I should not hire a macro theorist either ... Too bad ...

PS: I like the 'economics as a historical science' idea of Jon Stern

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**Johnstone** | March 4 12:58pm | Permalink Report

So, what was 'science' turned out to be ideology. Wouldn't be the first time, would it? Don't worry about the humility: Will's a Christian, he's got that one down. The question is: are we where he says we are yet? It seems to me, if we were, Will would be worried about his job.

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**shooter242** | March 4 1:44pm | Permalink Report

I barely escaped undergraduate with an economics degree, so I may be a case of ignorant bliss. But it seems to me that economics is nothing more than studying what any given person (or institution) will do with a dollar in a nearly infinite set of circumstances. Isn't this more sociology than physics? I have to think reducing human nature to a mathematical model is academic hubris.

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**Tim Young** | March 4 2:24pm | Permalink Report

While I am not sure how viable the alternatives are to the working assumptions you criticise, Willem, I do agree with your second paragraph – academic economics, and therefore the training of most central bankers, seems to have become detached from the quest to understand the economy and economic events. While the reasons for this may be complex, having had some experience of trying to make the transition from practitioner to academic myself, I would emphasise the influence of the pressure on academics to publish in "reputable" journals. First, these journals tend to demand an unnecessarily mathematical style of presentation that takes time to learn and produce and therefore drives budding academics to become highly specialised, usually requires simplification and abstraction of the problem to fit the method, and makes such journals so tedious to read that they do not engage the practitioners who might otherwise help to keep economic research informed and relevant. Second, since quality teaching and writing synthesis publications like books and press articles counts for relatively little in an academic career, many of the most ambitious academics minimise their teaching effort or draw much of it from their own research, and therefore lose familiarity with the roots of economics and its basic questions. I would be interested to hear your thoughts about what you, as a prominent and influential academic yourself, can do to change the culture of academic economics.

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**docG** | March 4 8:08pm | Permalink Report

I enjoyed this piece very much, Professor Buiter -- as much as I could understand, since I am not an economist. To a long-time fan of Derrida your excellent article suggests that the whole field of economics is long overdue for some thorough deconstruction. Actually the events of the last several months could in themselves be regarded as what might be called "hard deconstruction." With little hope of reconstruction in the offing, I'm afraid. And as far as I'm concerned, that may be just as well. I've come up with some thoughts and even some possible solutions to the current mess and would very much welcome some feedback from you -- or anyone else reading here -- on my blog, http://amoleintheground.blogspot.com/. A post that might especially interest you can be found here: http://amoleintheg...-come-part-12.html
Thank you.

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**Benign Brodwicz** | March 4 8:12pm | Permalink Report

For a highly nonlinear view of the American economy based on the yield curve and "animal spirits" cited in nature in 2001 see http://animalspiritspage.com

the model called the current slump, but not its severity, and is now calling for a bounce in "animal spirits" and a stabilization of the American economy.... unless parameters have changed. the model is updated after the unemployment release monthly.

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**atomic** | March 4 11:31pm | Permalink Report

While we needed more people writing and listening to blog posts like this years ago, there is nothing wrong with putting a few more nails in the coffin.

Economists have tried to elevate their discipline into a hard science that can be studied indepedently of politics, sociology and psychology and are finally being brought back down to earth. The future must lie in an integrative approach including insights from fields as disparate as sociology, psychology, environmental science and philosophy, just to name a few. My greatest hope is that we will finally move beyond blunt measures of the economic health of a country such as GDP and begin considering motivators of human behaviour other than money

For an example close to my heart, how do we explain the open-source software movement that is changing the face of technology as we know it? With the rapidly diminishing cost of hardware and network connectivity, individuals, firms and other entities over the last 30 years have spontaneously recognized software as a non-rivalrous, non-excludable good and contribute vast amounts of unpaid time contributing to the pool of software that is available. There is a good chance that some portions, if not most, of the software that run this blog were developed in such a manner

The people that contribute to open source software often make very conscious choices to forgo money and are instead motivated by prestige, respect, altruism and to some extent, ideology (in that they expect they will receive much greater benefits from the community in exchange for their work). There is a great deal of evidence that open-source software is of higher quality and allocates resources to the development of software much more efficiently than top-down approaches of proprietary software development.

Such behaviour can't be modelled by monetary incentives alone, so it is effectively treated as anomalous, yet there is nothing anomalous about it, given the impact it is having. Google as a company would not have been possible if not for the open-source movement (they make extensive use of decentralised, low-cost hardware all running free and open source software and thus pay no fees to companies such as Microsoft)

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**groucho** | March 5 12:32am | Permalink Report

Willem, Do you and Vincent WILLEM Van Gogh have anything else in common besides HYPERGRAPHIA?

On a more serious note, I commend you for your stick in the eye to mainstream EQUILIBRIUM economics ideology(as sorry an arse religion as there's ever been)

Have you followed Steve Keen's work on disequilibrium economics?

From a piece on his debtdeflation blog..................

(Bernanke)

“Fisher envisioned a dynamic process in which falling asset and commodity prices created pressure on nominal debtors, forcing them into distress sales of assets, which in turn led to further price declines and financial difficulties. His diagnosis led him to urge President Roosevelt to subordinate exchange-rate considerations to the need for reflation, advice that (ultimately) FDR followed.

He then explains that neoclassical economists in general readily dismissed Fisher’s theory, for reasons that are very instructive:

Fisher’s idea was less influential in academic circles, though, because of the counterargument that debt-deflation represented no more than a redistribution from one group (debtors) to another (creditors). Absent implausibly large differences in marginal spending propensities among the groups, it was suggested, pure redistributions should have no significant macroeconomic effects. ” (Bernanke 1995, p. 17)

Bernanke himself does try to make sense of Fisher within a neoclassical framework, which I’ll get to below; but the general neoclassical reaction to Fisher that he describes is a perfect example of the old (and very apt!) joke that an economist is someone who, having heard that something works in practice, then ripostes “Ah! But does it work in theory?”.

It is also–I’m sorry, there’s just no other word for it–mind-numbingly stupid. A debt-deflation transfers income from debtors to creditors? From, um, people who default on their mortgages to the people who own the mortgage-backed securities, or the banks?

Well then, put your hands up, all those creditors who now feel substantially better off courtesy of our contemporary debt-deflation…

What??? No-one? But surely you can see that in theory…

The only way that I can make sense of this nonsense is that neoclassical economists assume that an increase in debt means a transfer of income from debtors to creditors (equal to the servicing cost of the debt), and that this has no effect on the economy apart from redistributing income from debtors to creditors. So rising debt is not a problem.

Similarly, a debt-deflation then means that current nominal incomes fall, relative to accumulated debt that remains constant. This increases the real value of interest payments on the debt, so that a debt-deflation also causes a transfer from debtors to creditors–though this time in real (inflation-adjusted) terms.

Do I have to spell out the problem here? Only to neoclassical economists, I expect: during a debt-deflation, debtors don’t pay the interest on the debt–they go bankrupt. So debtors lose their assets to the creditors, and the creditors get less–losing both their interest payments and large slabs of their principal, and getting no or drastically devalued assets in return. Nobody feels better off during a debt-deflation (apart from those who have accumulated lots of cash beforehand). Both debtors and creditors feel and are poorer, and the problem of non-payment of interest and non-repayment of principal often makes creditors comparatively worse off than debtors (just ask any of Bernie Madoff’s ex-clients).

Back to Bernanke’s take on Fisher, rather than the generic neoclassical idiocy on debt-deflation. Firstly, Bernanke’s “summary” of Fisher’s argument starts with asset price deflation: ”Fisher envisioned a dynamic process in which falling asset and commodity prices created pressure on nominal debtors…”.

Sorry Ben, but (to use a bit of crude Australian vernacular), this is an “arse about <span class='profanity' title='A suspected profanity was blocked'>***</span>” reading of Fisher. Fisher’s dynamic process began with excessive debt, not with falling asset prices. You have confused cause and effect in Fisher’s theory: excessive debt and the deleveraging process that engendered lead to falling asset and commodity prices as symptoms (which then amplify the initial problem of excessive debt in a positive feedback process). To make this concrete, Fisher referred to:

“two dominant factors, namely over-indebtedness to start with and deflation following soon after” (Fisher 1933, p. 341)

I hope that’s clear enough that, in Fisher’s argument, overindebtedness is the first factor

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**xyzleont** | March 5 12:35am | Permalink Report

Being more general, some might find What Is Neoclassical Economics?
The three axioms responsible for its theoretical oeuvre, practical irrelevance and, thus, discursive power interesting:
http://www.paecon....erVaroufakis38.htm

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**Nicole Tedesco** | March 5 6:07am | Permalink Report

While sitting in a Portfolio Theory class, my inner physicist self laughed upon reading the tome that Black & Scholes had written to win them their Nobel Price. So much, "I think this..." and "I think that..." and I couldn't help but giggle. This class was filled with many other scientists as well (it was a night class) who also laughed it up. Though laughing we didn't just walk out of the class assuming it was all complete hogwash. Most people in that class were current practitioners of something or other (I, software architecture, others were quants-to-be, etc.) and understood that economists had no choice but to develop these silly, simplified models because they simply had no choice. Criticize all you want these silly, simplified models but realize that the only other rational alternative (and it's not a bad alternative, actually) is to hedge your bets by simply rolling die or reading tea leaves.

Give quants a better model and they will use it--there is no shortage of quants and managers looking for a mathematical advantage. However you can give them a "better" model but if they can't work out the math "by hand" or if the model is noncomputable for theoretical or practical reasons, the model is useless. Pointing out flaws in current economic theories is as easy as shooting fish in a barrel. Providing quants and managers something they can use, and better yet actionable knowledge to develop policy by, and you have struck gold. I doubt Buiter is rich yet.

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**enplaned** | March 5 6:51am | Permalink Report

20 years ago, I took a class by Tom Sargent at Stanford University. It was an exercise in mathematical self-gratification by Sargent (I was a math guy, so it was hard for him to snow me). I thought at the time he was basically a fraud. A pity the world economy had to go into the toilet to confirm that.

A year earlier, I'd taken a grad class at Harvard from Barro. Another snide, self-important w*nker who appeared to have little to say about the real world.

If there was any justice in the world, these guys would be in stocks in the public square being pelted with rotten fruit. I'll settle for this blog, and other similar public condemnations recently.

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**Peter Dorman** | March 5 7:24am | Permalink Report

This is an extremely important post, one that deserves widespread attention and influence. It certainly resonates with my own experience as an academic economist who early on sniffed the effusion of hyper-elaborate equilibrium models and smelled a fish.

One story: I attended a seminar given by one of the intellectual giants in international finance. He had presented a paper that developed a model of credit markets and calibrated it with data from post-bubble Japan. I stared at his equation for interest rate determination and noticed that assets were not capable of being differentiated by default risk. When he was done, I waited to see if someone else would raise the obvious point: how could you talk about the financial condition of Japan during the past two decades and not incorporate the risk of default? I was surrounded by the best and the brightest from a reputable university econ department; it was a Big Deal that they had arranged a seminar by such an illustrious guest. After waiting a bit and hearing nothing, I asked the question myself as innocently as I could: it appears that equation 9 (or whatever it was) does not account for default risk—is this absent from the model? The illustrious one, who seemed to me to be a perfectly decent person, by the way, and not in the least bit defensive, thought for a moment and said, "That's an interesting point. It might be useful sometime to try to build that in." I think I had caused some embarrassment for the department, however, since the question session was abruptly ended then and there.

Surely I had missed the point of the whole business. To be scientific in economics, for the stars and drones alike, has meant being able to develop and implement a model that derives from optimizing behavior by all parties and can be tweaked to be consistent with the data from the sample at hand. As I argued in a recent piece in Post-Autistic Economics, however, this falls far, far short of the true aim of an empirical science, which is to minimize the risk of Type I error (false positives). To do that you would have to go out of your way to pose the most stringent, and not only the most convenient, tests of your hypothesis. I would think, for instance, that a theory that claims that default risk played no role in Japanese stagnation would have to be scrutinized with great thoroughness for any indication that the risk was consequential.

As for the technical side, the one point I would add to your screed is that equilibrium solutions often possess properties that hold only on the knife edge. (Demonstrating this is one of George Akerlof's great accomplishments, incidentally.) What really should matter are the stability properties: how do people adjust in disequilibrium contexts? Adjustment has been abandoned wholesale in much of modern economics, alas. To self-refer again, this is what I found out when I investigated why so many economists were treating identity relations as if they were behavioral/functional, giving us models in which nothing, other than the hyper-rationality of agents, prevents the system from veering out of identity. They never noticed because they never looked to the left or right of their equilibrium solution.

What we are now experiencing is a terrible revenge of the actual world of economic life against policies and strategies predicated on universal optimizing behavior and equilibrium outcomes. Even the policy games in which default (private, sovereign) is an option, and which should therefore have something to say to us at this hour, turn out to be subgame perfect, so that default never actually occurs. If I could leave this world right now and waltz into the one painted by mainstream economics, I would do it in a flash. It is so much more orderly there, and so much easier to take decisions we will not bitterly regret with the passing of time.

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**sheetal chand** | March 5 9:25am | Permalink Report

This is a brilliant piece of deconstructionism. I was also impressed with several of the blogs and regard the quote from JL Austin as most apposite. There is a school of thought regarding how macroeconomics should be practiced, which has unfortunately now been greatly neglected including in its place of origin, Oslo, followng the rise of modern mainstream macroeconomics. However, I beleive this school associated with Ranar Frisch, Trygve Haavlemo and Leif Johansen has much to offer, especially methodologically, towards policy thinking in the present predicament. . Reading the originals is much to be preferred but, and excuse the self-referral, I have garnered a few of their key insights in http://www.nopecjo...NOPEC_2004_a02.pdf

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**charles monneron** | March 5 9:36am | Permalink Report

AMEN !
This is a masterpiece.

The only thing is doesn't address is, why ? Why such an "useless" academia has been allowed to prosper on fake theories for more than half a century ?

The answer is that they are not "useless". On the contrary, they are useful, but in the sense that Lenin coined (or was alleged to coin) : "useful idiots". In the same way the bolcheviks nurtured their communist friends in the west to hide their failures, the FIRE (Finance,Insurance, Real Estate) industry showered money on economics academia so that the bubble that enriched it could strive as long as possible.

It is a very long and subtle process, where dissenting voices are either silenced (by hindering their academic carreer through relative scarcity of research grants) or bought (the few that are able to see through the smokescreen realize how much they can gain from their knowledge, Cf. Edward Thorpe or James Simons).
It is not a phenomenon that is easy to fight : Thanks to the money shower, economics departments leads to prestigious and lucrative careers(do you prefer to be an economist that sits on a Central Bank Policy Board or, say, a marine zoologist that sit in a committee that allocates fish catches quotas ?).
Economics,and especially finance, is also the main material for MBA curriculum, it is more lucrative for an university to offer MBA's than Master of Science (in Harvard, 46,000 USD p.a. for the former, 35,000 for the latter), and it is easier to recruit rich young kids for the former as well (Maybe I am mistaken, but I think that "Dubya" would have had much more trouble completing a MSc. ...)

If one wants to improve from this situation, let me suggest this : economist should first spend a significant time in business or markets before going to academia. It should not be necessarily successful like Keynes, Fisher or Ricardo (Shumpeter was involved in a nasty bankruptcy for instance), but feeling the emotional commitment of real money is a prerequisite before one can talks about it. As Taleb says : what was the performance of your own portfolio in the last 3 years ? Not a sufficient condition for competence I admit, but a necessary one for sure.

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**erikawonka** | March 5 10:35am | Permalink Report

"This problem (of contract enforcement) is especially acute in trade over time or intertemporal trade, where the net value to each party to a contract of fulfilling the terms of the contract varies over time and can change sign. In a world with selfish, rational, opportunistic agents, able and willing to lie and deceive, only a small set of voluntary transactions will ever be observed, relative to the universe of all potentially feasible transactions."

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But if voluntary exchange is the exception rather than the rule, it is difficult to explain why voluntary exchange is the widespread phenomenon it is.

It seems likely that market participants are well aware the world actually IS populated by selfish, rational, opportunistic agents, able and willing to lie and deceive. But they proceed with transactions anyway, in the optimistic belief that the party with whom they are contracting is more likely to be honest than not. This optimism stems largely from the existence of a framework by which an injured party can obtain redress should a counterparty fail to perform as agreed.

The only thing guaranteed to upset this happy state and bring about a vast reduction in the number of transactions is to change the ground rules of the game halfway through. Unfortunately, this is exactly where government policy is taking us….

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**duvinrouge** | March 5 1:27pm | Permalink — Report

History shows that religion exists to attempt to justify state rule.
Economics has played a similar role - it is an ideology.

Where's the economic model that shows how the capital within a capitalist system circulates and importantly how it self-valorises?

Marx's reproduction schemas in volume II of Capital are worth recalling.
He struggled to get to grips with a model of expanded reproduction.

This is what Rosa Luxemburg analysed extensively in her book The Accumulation of Capital.
Of course, her conclusion that capitalism needs a non-capitalist sphere to loot is uncomfortable, because this is the ultimate capitalist contradiction: by accumulating and converting the non-capitalist sphere to capitalism it can no longer accumulated and must be superceeded.

I don't expect the economics profession to become scientific, but those individuals who do objectively seek knowledge would do well to read Luxemburg.

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**chacona** | March 5 2:41pm | Permalink — Report

Many thanks for a timely intervention!

If mathematical (mainstream) economics had really made great overall progress since 30s, we would presumably have now a more rigorous and deeper versions of Irving Fisher´s "The debt-deflation theory of great depressions" (Econometrica, 1933). Alas, what we do have now is, thanks to
DSGE and similar theories, mathematically very sophisticated versions of Fisher 1929!

Which leads me to ask a question: what is your take on on the "microfoundations for macroeconomic models" debates of 70s? In particular, (a) why did the debates take the turn from "disequilibrium" Keynesianism to Real Business Cycles views and (b) why did the results of Sonnenschein, Debreu and Mantel fail to make on impact on the "micro for macro" debate? How much did political and economic developments affect the academic debates?

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**User4425598** | March 5 5:08pm | Permalink — Report

I confess that I am one of those souls trained in an "Anglo-American" university. And I thank my lucky stars that I was. My training consisted of taking money and banking seriously. I learned to view the New Keynesian paradigm (the conventional wisdom) with suspicion. It was a program designed to legitimize what central bankers viewed as their day-to-day job of "stabilizing" the economy by adjusting the short-term interest rate to smooth out hypothetical "output gaps." It was a program that did not take financial markets or the role of a central bank as a clearing mechanism seriously. I followed (and made modest contributions to) a literature that focussed on issues like financial market stability. This literature was largely ignored by central bankers and by economists advising them (and I lump our Prof. Buiter in this category). I viewed that state of affairs with some bemusement; this is normal in science and it is not something to get overly excited about.

But now I see Herr Buiter taking broad swipes at us silly "Anglo-American" economists. He questions the motives of a vast and diverse literature (some good, some bad). He calls for a new research program that abandons silly notions like internal consistency (a logical argument). He has gone too far; and I am now led to question his own motives in this matter (to promote himself as the new guru, no doubt).

What has Herr Buiter ever contributed, academic-wise, to a literature that can help us understand the current predicament? How have Professor Buiter's theories helped us forecast the recent disaster? His hypocrisy is revolting (check out his lecture notes for students and you'll see the sort of crap he was teaching).

It is indeed a good thing for us all to be reminded of the limits to what we know. I have been humbled myself and I am sure that most of us have been as well. We are all entitled to our own opinions and interpretations, of course. But I think that Buiter goes too far in questioning the motives of honest researchers. His comments cater to the lunatic fringe and they serve no useful purpose. Criticism is always welcome; but it should be more constructive in nature.

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**Gary Marshall** | March 6 3:44am | Permalink — Report

Hello Nightlight,

If the central bank does indeed control interest rates, then do please enlighten us all as to how it is done!

Just because the central bank lowers its rates does not mean every other lender will follow, unless of course a constable is involved.

And exactly how does lower interest rates or less revenue on loans increase a bank's profits?

Gary Marshall

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**patientr** | March 6 3:51am | Permalink — Report

Much good sense is dispensed here, as is often true of Willem Buiter. But I think the tendency to blame failure of the models on their lack of non linear complexity and psychology is a bit too technical, or at least points us in an unnecessarily complicated direction. I think that simple reversion-to-mean tendencies in the models used for policy would make a lot of sense, and could have avoided much of the destruction we are seeing now.

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**dangay** | March 6 11:16am | Permalink — Report

"What has Herr Buiter ever contributed, academic-wise, to a literature that can help us understand the current predicament? How have Professor Buiter's theories helped us forecast the recent disaster? His hypocrisy is revolting (check out his lecture notes for students and you'll see the sort of crap he was teaching)."

Exactly. Many people in heterodox and post-autistic circles have been saying that mainstream economics is useless for years. eg.
http://www.paecon..../issue19/Gay19.htm , written in 2003. Why should we listen to the recantations of past withdoctors? Read the greats instead - Keynes, Kalecki, Minsky, Davidson, Sen.

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**Bill Avery** | March 6 1:11pm | Permalink Report

This article could have been much shorter:

"Mainstream business cycle models are not designed to describe financial crises or asset bubbles, ergo they are useless for describing financial crises or asset bubbles.
"

Doesn't look quite so damning, written like that. Especially once you include the fact that if you want models that describe financial crises and asset bubbles, there are plenty of them that do just that (have a nose around google scholar).

The limitations of mainstream business cycle models have been widely acknowledged for decades. The idea that most economists simply swallow the EMH is a joke. Lots of useful things are doubtless going to come out of models with tipping points etc., but they will have their own shortcomings and, just like the models Mr Buiter criticizes, will be useful for somethings and not for others . Any fool knows there's ample room for improvement in economics, but this article takes a blunderbuss to the problem.

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**wpaul** | March 6 3:21pm | Permalink Report

Amen.
The sad truth is that the many of the actors of this crisis are already looking for a way to eke out a second mandate.
Yet others see in this bubble the chance of a lifetime to be able to grab the goodies and consolidate their power even more. I tend to agree with the poster who stated that letting these economists have their way was the best bet to strike it big. Had logic, science and common-sense prevailed, many of the so called "power brokers" wouldn't be around today. But alas it ain't so.
As usual, thank you to Willem Buiter for taking the system to task instead of brown-nosing his life and academic credentials to the altar of mass stupidity and deception.

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**Steve Koch** | March 6 5:07pm | Permalink Report

In 2006, Greg Mankiw wrote(http://www.economi...t_as_Scientist.pdf):

"New classical and new Keynesian research has had little
impact on practical macroeconomists who are charged with the messy task of conducting
actual monetary and fiscal policy. It has also had little impact on what teachers tell future
voters about macroeconomic policy when they enter the undergraduate classroom. From the
standpoint of macroeconomic engineering, the work of the past several decades looks like
an unfortunate wrong turn."

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**Attitude_Check** | March 6 9:39pm | Permalink Report

As a trained physicist and practicing engineer, this critique could be applied to engineering as well. The real world really is non-linear (Relitivity theory, E&M - with the presence of charge, etc.). From an engineering perspective the math is "too hard" to solve so approximations are made - typically linearization around a specific point. The problem is when engineers and scientists "forget" that the linear model is only acurate in a small neighborhood near the lineraization point, and begin to use the lineraized equation(s) in invalid domains. I fight this constantly amoung my engineering collegues who while very bright generally "don't get it". The fact that something similar is true in macro-economics is not surprising.

A viable half-step that is tractable is to develop a theory that develops a continuously changing linerization based on present "observables". An example of an approach like this is the Extended Kalman Filter which constantly updates a linearization of a non-linear system based on the previous "state-estimate". This results in a system that cannot make long-term predictions however. But it's prediction error can be bounded, subject to the assumption that the external boundary conditions do not change significantly!!

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**chacona** | March 7 12:06am | Permalink Report

Steve Koch,

Thanks for referring to Mankiw´s article.

Mankiw´s account is disputed by no other than Michael Woodford who takes Mankiw for task in his "Convergence in Macroeconomics: Elements of the New Synthesis", remarks for a panel discussion at the AEA meeting, January 2008. (see: http://www.columbia.edu/~mw2230/)

I am sorry that my citation goes ad nauseam, as far as the length is concerned, but because this is essential for assessing Willem Buiter´s case, I shall dismiss the etiquette and pursue relentlessly the cause of truth and nothing but the truth. Here is Woodford:

"In a recent essay, Mankiw (2006) criticizes the current state of macroeconomics from the opposite perspective to the one just discussed. In Mankiw's view, since the 1970s too much stress has been placed on the development of macroeconomics as a science, with clear conceptual foundations, and too little on macroeconomics as a branch of engineering, a body of lore about how to solve problems. As a result, he argues, the conceptual developments of the past several decades have “had little impact on practical macroeconomists who are charged with the messy task of conducting actual monetary and fiscal policy” (p. 44). In this respect, he asserts, all of the different recent currents of thought among academic macroeconomists have equally failed.
For example, Mankiw states that the models used for quantitative policy analysis in policy institutions like the Federal Reserve today “are the direct descendents of the early modeling efforts of Klein, Modigliani and Eckstein. Research by new classicals and new Keynesians has had minimal influence on the construction of these models” (p. 42). But this a misleading picture of the current state of affairs. It is true that the modeling efforts of many policy institutions today can reasonably be seen as an evolutionary development within the macroeconometric modeling program of the postwar Keynesians; thus if one expected, with the early New Classicals, that adoption of the new tools would require building anew from the ground up, one might conclude that the new tools have not been put to use. But in fact they have been put to use, only not with such radical consequences as had once been expected.

The Fed's current main policy model, the FRB/US model, was developed in the mid-1990s, and so before the recent renaissance of research on empirical DSGE models, but it incorporated many insights from the research literature of the 1970s and 1980s. As Flint Brayton et al. (1997) explain, it departed sharply from the previous generation of Federal Reserve Board models in giving much more attention to modeling the endogenous evolution of expectations, and allowed model simulations to be conducted under an assumption of model-consistent (or rational) expectations, among other possibilities. The modelers also gave much more attention to ensuring that the model implied long-run dynamics consistent with an equilibrium model, for example that the dynamics of both government debt and the external debt satisfy transversality conditions. Finally, adjustment dynamics were modeled not by simply adding arbitrary lags to structural relations, or even by ad hoc “partial adjustment” dynamics, but on the basis of dynamic optimization problems for the various decisionmakers that incorporated explicit (though flexibly parameterized) adjustment costs.

Around the same time, new macroeconomic models were introduced at other central banks, such as the Bank of Canada's Quarterly Projection Model (Donald Coletti et al., 1996) and the Reserve Bank of New Zealand's Forecasting and Projection System (Richard Black et al., 1997), that were similarly modern in their emphasis on endogenous expectations and long-run dynamics consistent with an equilibrium model. And these were not mere research projects, but models routinely used for practical policy deliberations, under the "forecast targeting" approach to monetary policy employed by both of those central banks starting in the 1990s.

In the decade since then, as the scholarly literature has devoted more attention to the development of models that are both theoretically consistent and empirically tested, the rate at which ideas from the research literature are incorporated into modeling practice in policy institutions has accelerated, with forecast-targeting central banks often playing a leading role. Examples of the more theoretically ambitious recent projects include the International Monetary Fund's Global Economy Model (GEM: Tamim Bayoumi, 2004), the Swedish Riksbank's RAMSES (Malin Adolfson et al., 2007), the European Central Bank's New Area-Wide Model (Kai Christoffel, Guenter Coenen, and Chris Warne, 2007), and the Norwegian Economic Model (NEMO) under development by the Norges Bank (Leif Brubakk et al., 2006). All of these are fully coherent DSGE models reflecting the current methodological consensus discussed above, matched to data

Mankiw also states that central bankers find no use for modern developments in macroeconomics in their thinking about the economy and the decisions that they face. As evidence, he cites the memoir of former Federal Reserve Governor Larry Meyer, saying that "Meyer's analysis of economic fluctuations and monetary policy is intelligent and nuanced, but it shows no traces of modern macroeconomic theory. It would seem almost completely familiar to someone who was schooled in the neoclassical-Keynesian synthesis that prevailed around 1970 and has ignored the scholarly literature ever since" (p. 40). This does not sound like Larry Meyer the Fed Governor to me, though like Harvard professors, he probably adopts a simpler manner when addressing the general public than among his peers. Mankiw's interpretation also assumes, yet again, that acceptance of any part of the scholarly literature since 1970 would require thorough repudiation of pre-1970 ways of thinking, while this is not so.8
In any event, it is hard to see how anyone could say this of the current members of the Federal Open Market Committee. The speeches of Ben S. Bernanke and other current Governors and Federal Reserve Bank presidents are often laced with footnotes to the recent research literature. As one telling example, when the Fed recently introduced a new policy of discussing the quantitative forecasts of the FOMC members as part of the Committee's published minutes, Governor Frederic S. Mishkin explained the policy change to the public in a speech titled "The Federal Reserve's Enhanced Communication Strategy and the Science of Monetary Policy" (Mishkin, 2007). This suggests, to me at least, that the "disconnect" between the science of macroeconomics and the engineering side is not as great as Mankiw claims.9

8 On the extent to which aspects of the conventional wisdom circa 1970 have been repudiated by practicing central bankers, and the role of the academic literature in this development, see Goodfriend (2007). In Goodfriend's characterization, "the story is one of mutually reinforcing advances in theory and practice" (p. 57).

9 For further comment on Mankiw's argument, see David Warsh (2006)."

(Warsh, David. 2006. "It Isn't All in Adam Smith." www.economicprinci...sues/06.12.10.html )

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So, Woodford does think that DSGE models have played a major role; while Mankiw thinks that they have played a role only within the academia. Either way we must ask what positive contribution they might have made for analysing the workings of the financial system that led to a disaster. It seems that an honest answer will demand much soul-searching from many leading economists.

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**User4443476** | March 7 4:40pm | Permalink Report

I agree that mainstream macroeconomic theory needs to be redone. Macroeconomics is a human science. The science part needs to proceed from studying the economic activity itself not by studying economic theories and then adding abstraction to abstraction resulting in a heap of abstractions unrelated to reality. As human, it involves free will (as quantum mechanics involves uncertainty) and thus is not a science of prediction. People are not particles to be manipulated by technicians. They have intelligence and can at least follow economic precepts (like thrift and savings) if they believe those prescribing them know what they are talking about.
In his works Macroeconomic Dynamics: an Essay in Circulation Analysis and For a New Political Economy, Bernard Lonergan discovers the channels within which money flows and the markets operate. It puts each part of the economy (consumer production, producer production, finance) in their proper relations. He breaksdown the dynamic cycle of expansion of means of production and expansion of standard of living that Keynes completely misses with his static theory of equilibrium. He explains economic growth and why the busibess cycle is not necessary but caused by our maladaptation to the intrinsic norms of economic process.
I just hope it is not too late to learn and help ourselves.

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**Morph366** | March 7 6:14pm | Permalink Report

I would not wish to comment on some of the reasoning in this piece because some of it went over my head.
But to take just one point it does seem extraordinary that macro-economists, from what you have said, really have no adequate conceptual framework for explaining market liquidity and seem to just take it as a given in their modeling exercises. The paradox of liquidity is well expressed in the following quotation (which I believe comes from William Janeway)

Liquidity declines more than proportionally with the intensity of the demand for it. The more you need cash, the higher the price you have to pay to get it. And when average opinion comes to believe that average opinion will decide to turn assets into cash, then liquidity may be confidently expected to go to zero. By definition, no market can hedge this risk; no individual participant is rich enough not to need the hedge.

For me this has always suggested that no amount of sophisticated financial modeling will ever properly prepare and protect investors for the risks inherent in markets.

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**Steve Koch** | March 8 4:58am | Permalink Report

This is from the ECB link (http://www.ecb.int...archer_swm.en.html):

"Smets-Wouters (2003) Model

Recent developments in the construction, simulation and estimation of dynamic stochastic general equilibrium (DSGE) models have made it possible to combine a rigorous microeconomic derivation of the behavioural equations of macro models with an empirically plausible calibration or estimation which fits the main features of the macroeconomic time series.

The main difference between empirical DSGE models and the more traditional macroeconometric models (such as the AWM) is that both the parameters and the shocks to the structural equations are related to deeper structural parameters describing household preferences and technological and institutional constraints.

These micro foundations have three advantages:
They provide a theoretical discipline on the structure of the model that is being estimated, which may be particularly helpful in those cases where the data themselves are not very informative, for example regarding the long-run behaviour of the economy or because there has been a regime change.
Being able to relate the reduced-form parameters to deeper structural parameters makes the use of the model for policy analysis more appropriate, i.e. less subject to the Lucas critique, as those structural parameters are less likely to change in response to changes in policy regime.
Micro-founded models may provide a more suitable framework for analysing the optimality of various policy strategies as the utility of the agents in the economy can be taken as a measure of welfare.
For these reasons, staff at the ECB and the Eurosystem have started to develop empirical DSGE models for monetary policy analysis. The Smets-Wouters (2003) Model is an example of such a medium-sized DSGE model, which has been estimated on the basis of quarterly euro area macro data. The model features three types of

economic agents: households, firms and the central bank. Households decide how much to consume, how much to invest and how much to work and at what wage. Firms employ workers and capital and decide how much to produce and at what price to sell their products.

In addition to a number of real frictions such as habit formation in consumption and adjustment costs in investment, the model features nominal price and wage rigidities. The model is estimated using seven euro area macroeconomic series (real GDP, consumption, investment, employment, real wages, inflation and the nominal short-term interest rate). Using Bayesian estimation and validation techniques, it is shown that the estimated model is able to compete with more standard, unrestricted time series models, such as vector auto regressions (VARs), in out-of-sample forecasting."

Sounds like the ECB thinks that the DSGE is useful (i.e. that the last few decades of economics science research has not been a wrong turn). You would think that it would be simple enough to compare the predictive performance of the DSGE to that of the more traditional models that Greg Mankiw mentioned to see which performs better (in retrospect).

It seems to be true that it is easier to build the DSGE models so they will have an easier time evolving/improving and should be easier to customize for particular economies. I wonder if anyone is working on an open source version of DSGE. Anybody know if there is a standard XML representation of economic (test) data?

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**User4443476** | March 8 3:29pm | Permalink Report

I would like to point out a problem with the assumptions of the ECB with regard to the economic agents in their DSGE model. First, households, firms and central bank are descriptive and need to be replaced by more general explanatory terms such as income, outlay, finance. These terms move one away from picturing factories and houses and allow one to get at what applies to economics in every case: a flow of goods and service and the monetary flows that move opposite these. Second, doing so allows one to understand there are two main markets that are distinct yet related. This is the key to understanding the conditions of the possibility of growth and sustainability that has been missing in macroeconomics. The other part is getting people to understand how to act economically and choosing to do so.
There is a difference between someone who is paid to produce food, houses, or automobiles, and someone who is paid to produce combines, bulldozers, and conveyors. The former it is easy to see will buy the fruits of their labor (in the aggregate). The latter will not since households do not purchase the means of production as part of their standard of living. These people will buy food, shelter, and transportation that they did not (in the aggregate) produce. This creates two markets: one for consumer goods and one for producer goods. In order to sustain this cycle, makers of consumer goods have to buy an amount of producer goods equal to the income paid for production of producer goods. If this balance is not achieved, one market will be drained of money while the other is inflated. You might argue that consumers pay for producer goods indirectly because the cost is in the prices they pay for consumer goods. This leaves out an important step in economic process; one that can imbalance the flow of payments. Firms (in the aggregate) can decide not to buy new equipment, or to buy too much
This distinction is especially relevant during an expansion when expansion of the means of production precedes production yet creates jobs and pays wages (by borrowing from the financial sector) that workers will try to use to buy consumer goods. Yet, the increase in production of consumer goods is waiting for new plant and equipment to be produced to increase it production. So, spending becomes inflationary (to the consumer market) during an expansion and takes away money that could be used to fuel the expansion. That's why the motto “thrift and savings” works during an expansion.

This brings up another problem with dismissing the distinction between the two markets. Economic expansion (a long-term major one) requires the producer goods production to increase first (this will be much smoother if we admit it exists). This is followed by an expansion of buying power to consume the greater quantity of goods produced using increased producer goods. This part of the dynamic cycle of economic expansion is the one most often missed and which causes the bust after the boom.
It is sustainable as long as the increased income from the producer market is balanced by purchase from consumer firms to producer firms. To make it more complicated, some firms produce both types of goods. That's why in economics (as in any science) we need to move from descriptive terms to explanatory ones.

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**Steve Koch** | March 8 4:23pm | Permalink Report

User4443476:

I am no expert in economics (far from it, the few weeks I have spent reading about it recently is the sum total of my knowledge). Having said that, my impression is that the DSGE is generally a bottom up approach (i.e. microeconomics based approach). There should be a steady improvement in the ability to model the actions of the various individual actors in an economy because of improvements in data representation/interchange technology, open source availability of modeling tools, and the fact that critical actors such as specific corporations can be modeled quite accurately by the actor itself (because of the actor's intimate domain specific knowledge). It is also easier to model the economic actions of a particular actor (such as a specific corporation, for example) than to model the economy as a whole.

The advantage of the microeconomic based approach is that it can incorporate more domain actor specific knowledge into the prediction and that the errors in the myriad small models of all these actors will tend to cancel when aggregated into a macro model. The distributed nature of the microeconomic based approach means that the instead of trying to optimize one huge model, many (1000's) of different modelers can be optimizing many different smaller models in parallel.

Another advantage of a microeconomic modeling based approach is that it is much more loosely coupled than the traditional centralized approach to macroeconomic modeling. This should facilitate experimentation, thus leading to quicker evolution/improvement of modeling capability.

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**Steve Koch** | March 8 8:13pm | Permalink Report

Chacona:

The Glass-Steagall Act was actually repealed during the Clinton presidency and Robert Rubin was an advisor of Clinton's (not Bush II). This is not to imply that you did not know these facts.

No need to wait for John B. Taylor's book, the following article ("How Government Created the Financial Crisis; Research shows the failure to rescue Lehman did not trigger the fall panic.") should be a good indicator of what he thinks:
http://online.wsj....4310280561945.html

Taylor thinks that monetary excesses by the Fed was the main cause of the false housing boom that inevitably would lead to a subsequent bust. He also indicts subprime and adjustable-rate mortgages and securitizing same in complex packages. He also indicts the rating agencies for letting the securitizers get away with this. He also criticized Fannie May and Freddie Mac.

Taylor also mentions that USA economic policy makers in August of 2007 (i.e. during the Bush II regime) initially misdiagnosed the crisis as a liquidity crisis.

The article is short and worth reading. My impression is that you presumed that Dr. Taylor would not attempt to be fair and professional in his analysis. Is that true and, if so, why would you presume that? Does Dr. Taylor have a reputation as a political hack?

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**Steve Koch** | March 8 8:36pm | Permalink Report

Chacona

You state:
"Keynes and Minsky that permitted Roubini (and others) to call the meltdown".

Keynes had a disastrous record in predicting the great depression and lost the bulk of his fortune during the depression. Hayek, a classical economist who opposed Keynes academically, did predict the great depression quite accurately.

The way Roubini predicted the current crisis was actually by analyzing historical data of countries whose financial systems imploded. After studying the data, he realized that the USA was in a similar situation to that of the countries that had previously had financial system disasters. An example of a similar historical economic analysis approach is Friedman's "A Monetary History of the USA, 1867 -- 1960". Did Keynes write any books that were primarily based on analysis of historical economic records?

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**Steve Koch** | March 9 4:06am | Permalink Report

Chacona

You state about Taylor:
"how to write about the crisis without ever coming into grips with the huge speculative asset bubble".

But Taylor did say (in the article I cited) that the primary problem was the result of a huge speculative bubble in real estate and identified the most important cause as easy money from the Fed (Greenspan). He went on to blame several other government entities (during the Bush II administration) for contributing to the crisis.

My guess is that you are correct that he will not diagnose any fundamental problems in capitalism in his book.

You did not actually respond directly to my point about Keynes: Keynes not only did not predict the great depression but was so disastrously wrong about the great depression that he lost the bulk of his fortune in the great depression. Can we agree that is a historical fact? My point was about Keynes's ability to predict the depression (i.e. something Roubini might have studied to predict the current financial fiasco), not how he responded to the depression after it arrived.

My point was not that Roubini doesn't admire Keynes or that Keynes was not a good investor (usually) but that Keynes's general academic approach wasn't a particular template for Roubini's historical analysis that enabled Roubini to predict the current financial fiasco.

"And yes, for me history and money do matter, enormously: their full recognition - in post-meltdown economics - leads to a demand of careful comparative research on the workings of real economies. Maybe we could agree on this?"

Yes, we can agree on that.

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**jamescash** | March 9 1:17pm | Permalink Report

Wow, a real page turner. Gripping. I conclude "the emperor has no clothes." Keep up your good work. Hope you will be successful in spreading your message to the public policy folks.

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**David Heigham** | March 10 12:37pm | Permalink Report

I shall cherish 'The Auctioneer at the End of Time'. I hope that he will haunt macro-economic research and teaching for the coming decades.

There are macro-economic models which work for the real economy. Wynne Godley's model, with latest results here is the one I most respect. Academic economics has a problem in that the models which work are not fully 'coherent'.

As an applied economist I have always found that fully coherent models in any type of economics always miss out something - often many things - which are important in practice. That working assumption also seems to fit in fields outside economics.

The result is that at any given moment , the best model to have in mind when designing or implementing economic policy is and will be a lash-up of the most important theoretical developments to date which appear to affect factors that are likely to matter. It will not be fully consistent let alone coherent; and it will (or should) change both as new theoretical work appears and as factors that seem likely to matter change. That perspective seems to leave many academic economists floundering, though others fall into it in their comments on public policies. If it has been forgotten in such places as the Bank of England, no wonder there have been gaping omissions in our economic policies.

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**David Heigham** | March 10 12:41pm | Permalink Report

The link to Wynne Godley's latest published model results has been lost from my earlier post. The paper is at http://www.levy.org/pubs/sa_dec_08.pdf .

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**AF** | March 18 9:51am | Permalink Report

Rogoff said it first, in a more coherent way and in a less 'throw the baby with the bathwater' way:

http://www.minneap...n/08-12/rogoff.pdf

One also wonders what was the active and remarkable contribution of Professor Buiter to change the state of affairs in academic Economics in all those years he had been in academia...

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**chris.tormey** | September 10 2:48am | Permalink Report

This article is nearly as clueless as the Krugman piece in the NY Times last week. Professor Buiter, have you ever heard of Austrian Economics? If not, please take a look around the LSE library, there should be some good material there. I recommend Mises' four great books, but also Hayek, Rothbard and anything from Henry Hazlitt. Under a gold standard and without fraudulent fractional reserve banking, none of this hanky panky ever would have come about. And don't believe that nonsense from Josef Schumpeter, who got so overwhelmed by his "creative destruction" truism that he felt the economy needed a bunch of overlending to find a few great ideas. The banks have had literally NOTHING to do with ANY innovation of import that I can remember. MSFT, GOOG, IBM, HPQ, etc etc all venture capital funded and nothing to do with the banks. Banks are no more than fraudulent ponzi schemes supported by central banks to extend the fraud when the bust inevitably occurs. Yes, you are correct that typical macroeconomics training is bunk. But you have missed the solution, Austrian Economics.

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**Grand Supercycle** | October 25 6:30am | Permalink Report

Using analysis derived from my proprietary indicators, in early 2007 I was able to warn of an extremely overbought market and an impending stock market crash.

Will the USD rally when the DOW bear market rally ends ?

www.zerohedge.com/forum/market-outlook-0

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