real-world economics review, issue no. 76
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Paul Romer's assault on "post-real" macroeconomics

Lars Pålsson Syll [Malmö University, Sweden]

Introduction

In a new, extremely well-written, brave, and interesting article, Paul Romer (2106a:4-5) goes to frontal attack on the theories that has put macroeconomics on a path of "intellectual regress" for three decades now:

> "Macroeconomists got comfortable with the idea that fluctuations in macroeconomic aggregates are caused by imaginary shocks, instead of actions that people take, after Kydland and Prescott (1982) launched the real business cycle (RBC) model …"
>
> "In response to the observation that the shocks are imaginary, a standard defence invokes Milton Friedman's (1953) methodological assertion from unnamed authority that 'the more significant the theory, the more unrealistic the assumptions.' More recently, 'all models are false' seems to have become the universal hand-wave for dismissing any fact that does not conform to the model that is the current favourite."
>
> "The noncommittal relationship with the truth revealed by these methodological evasions and the 'less than totally convinced …' dismissal of fact goes so far beyond post-modern irony that it deserves its own label. I suggest 'post-real.'"

There are many kinds of useless 'post-real' economics held in high regard within mainstream economics establishment today. Few – if any – are less deserved than the macroeconomic theory/method – mostly connected with Nobel laureates Finn Kydland, Robert Lucas, Edward Prescott and Thomas Sargent – called calibration.

Paul Romer and yours truly are certainly not the only ones having doubts about the scientific value of calibration. Nobel laureates Lars Peter Hansen and James J. Heckman (1996:88) writes:

> "It is only under very special circumstances that a micro parameter such as the inter-temporal elasticity of substitution or even a marginal propensity to consume out of income can be 'plugged into' a representative consumer model to produce an empirically concordant aggregate model … What credibility should we attach to numbers produced from their 'computational experiments', and why should we use their 'calibrated models' as a basis for serious quantitative policy evaluation? … There is no filing cabinet full of robust micro estimates ready to use in calibrating dynamic stochastic

> equilibrium models … The justification for what is called 'calibration' is vague and confusing."

Mathematical statistician Aris Spanos (Mayo & Spanos, 2010:240) – is no less critical:

> "Given that 'calibration' purposefully forsakes error probabilities and provides no way to assess the reliability of inference, how does one assess the adequacy of the calibrated model? …"
>
> "The idea that it should suffice that a theory 'is not obscenely at variance with the data' (Sargent, 1976, p. 233) is to disregard the work that statistical inference can perform in favor of some discretional subjective appraisal … it hardly recommends itself as an empirical methodology that lives up to the standards of scientific objectivity."

In physics it may possibly not be straining credulity too much to model processes as ergodic – where time and history do not really matter – but in social and historical sciences it is obviously ridiculous. If societies and economies were ergodic worlds, why do econometricians fervently discuss things such as structural breaks and regime shifts? That they do is an indication of the unrealisticness of treating open systems as analyzable with ergodic concepts.

The future is not reducible to a known set of prospects. It is not like sitting at the roulette table and calculating what the future outcomes of spinning the wheel will be. Reading Lucas, Sargent, Prescott, Kydland and other calibrationists one comes to think of Robert Clower's (1989:16) apt remark that

> "much economics is so far removed from anything that remotely resembles the real world that it's often difficult for economists to take their own subject seriously."

As Romer (2016a:12) says:

> "Math cannot establish the truth value of a fact. Never has. Never will."

So instead of assuming calibration and rational expectations to be right, one ought to confront the hypothesis with the available evidence. It is not enough to construct models. Anyone can construct models. To be seriously interesting, models have to come with an aim. They have to have an intended use. If the intention of calibration and rational expectations is to help us explain real economies, it has to be evaluated from that perspective. A model or hypothesis without a specific applicability is not really deserving our interest.

To say, as Edward Prescott (1977:30) that

> "one can only test if some theory, whether it incorporates rational expectations or, for that matter, irrational expectations, is or is not consistent with observations"

is not enough. Without strong evidence, all kinds of absurd claims and nonsense may pretend to be science. We have to demand more of a justification than this rather watered-down version of “anything goes” when it comes to rationality postulates. If one proposes rational expectations one also has to support its underlying assumptions. None is given, which makes it rather puzzling how rational expectations has become the standard modeling assumption made in much of modern macroeconomics. Perhaps the reason is that economists often mistake mathematical beauty for truth.

Prescott’s view is also the reason why calibration economists are not particularly interested in empirical examinations of how real choices and decisions are made in real economies. In the hands of Lucas, Prescott and Sargent, rational expectations has been transformed from an – in principle – testable hypothesis to an irrefutable proposition. Believing in a set of irrefutable propositions may be comfortable – like religious convictions or ideological dogmas – but it is not science.

So where does this all lead us? What is the trouble ahead for economics? Putting a sticky-price DSGE lipstick on the RBC pig sure won’t do. Neither will – as Paul Romer (2016a:22) notices – just looking the other way and pretend it’s raining:

> “The trouble is not so much that macroeconomists say things that are inconsistent with the facts. The real trouble is that other economists do not care that the macroeconomists do not care about the facts. An indifferent tolerance of obvious error is even more corrosive to science than committed advocacy of error.”

Why critique in economics is so important

A part of why yours truly appreciate Romer’s article, and even find it “brave,” is that Romer (2016a: 21) dares to be explicit in his critique and name names:

> “Some of the economists who agree about the state of macro in private conversations will not say so in public. This is consistent with the explanation based on different prices. Yet some of them also discourage me from disagreeing openly, which calls for some other explanation.”
>
> “They may feel that they will pay a price too if they have to witness the unpleasant reaction that criticism of a revered leader provokes. There is no question that the emotions are intense. After I criticized a paper by Lucas, I had a chance encounter with someone who was so angry that at first he could not speak. Eventually, he told me, ‘You are killing Bob.’”
>
> “But my sense is that the problem goes even deeper that avoidance. Several economists I know seem to have assimilated a norm that the post-real macroeconomists actively promote – that it is an extremely serious violation of some honor code for anyone to criticize openly a revered authority figure – and that neither facts that are false, nor predictions that are wrong, nor models that make no sense matter enough to worry about …”
>
> “Science, and all the other research fields spawned by the enlightenment, survive by ‘turning the dial to zero’ on these innate moral senses. Members cultivate the conviction that nothing is sacred and that authority should

> always be challenged … By rejecting any reliance on central authority, the members of a research field can coordinate their independent efforts only by maintaining an unwavering commitment to the pursuit of truth, established imperfectly, via the rough consensus that emerges from many independent assessments of publicly disclosed facts and logic; assessments that are made by people who honor clearly stated disagreement, who accept their own fallibility, and relish the chance to subvert any claim of authority, not to mention any claim of infallibility."

Everyone knows what he says is true, but few have the courage to openly speak and write about it. The "honour code" in academia certainly needs revision.

The excessive formalization and mathematization of economics since WW II has made mainstream – neoclassical – economists more or less obsessed with formal, deductive-axiomatic models. Confronted with the critique that they do not solve real problems, they often react as Saint-Exupéry's Great Geographer, who, in response to the questions posed by The Little Prince, says that he is too occupied with his scientific work to be able to say anything about reality. Confronting economic theory's lack of relevance and ability to tackle real problems, one retreats into the wonderful world of economic models. While the economic problems in the world around us steadily increase, one is rather happily playing along with the latest toys in the mathematical toolbox.

Modern mainstream economics is sure very rigorous – but if it's rigorously wrong, who cares? Instead of making formal logical argumentation based on deductive-axiomatic models the message, we are better served by economists who more than anything else try to contribute to solving real problems. And then the motto of John Maynard Keynes is more valid than ever:

> "It is better to be vaguely right than precisely wrong."

Attempting to trivialize Romer's critique

Much discussion has been going on in the economics academia on Romer's critique. Some mainstream macroeconomists have tried to "save" what they consider advances in the macroeconomics of the last three decades from the critique. One prominent example is Simon Wren-Lewis (2016b), who argues that the critique is

> "unfair and wide of the mark in places … Paul's discussion of real effects from monetary policy, and the insistence on productivity shocks as business cycle drivers, is pretty dated … Yet it took a long time for RBC models to be replaced by New Keynesian models, and you will still see RBC models around. Elements of the New Classical counter revolution of the 1980s still persist in some places … The impression Paul Romer's article gives, might just have been true in a few years in the 1980s before New Keynesian theory arrived. Since the 1990s New Keynesian theory is now the orthodoxy, and is used by central banks around the world."

Now this rather unsuccessful attempt to disarm the real force of Romer's critique should come as no surprise for anyone who has been following Wren-Lewis' writings over the years.

In a recent paper Wren-Lewis (2016a:33-34) writes approvingly about all the "impressive" theoretical insights New Classical economics has brought to macroeconomics:

> "The theoretical insights that New Classical economists brought to the table were impressive: besides rational expectations, there was a rationalisation of permanent income and the life-cycle models using intertemporal optimisation, time inconsistency and more …"
>
> "A new revolution, that replaces current methods with older ways of doing macroeconomics, seems unlikely and I would argue is also undesirable. The discipline does not need to advance one revolution at a time …"
>
> "To understand modern academic macroeconomics, it is no longer essential that you start with The General Theory. It is far more important that you read Lucas and Sargent (1979), which is a central text in what is generally known as the New Classical Counter Revolution (NCCR). That gave birth to DSGE models and the microfoundations programme, which are central to mainstream macroeconomics today …"

There's something that just does not sit very well with this picture of modern macroeconomics.

"Read Lucas and Sargent (1979)". Yes, why not. That is exactly what Romer did!

One who has also read it is Wren-Lewis' "New Keynesian" colleague Paul Krugman (2015). And this is what he has to say on that reading experience:

> "Lucas and his school … went even further down the equilibrium rabbit hole, notably with real business cycle theory. And here is where the kind of willful obscurantism Romer is after became the norm. I wrote last year about the remarkable failure of RBC theorists ever to offer an intuitive explanation of how their models work, which I at least hinted was willful:
>
>> 'But the RBC theorists never seem to go there; it's right into calibration and statistical moments, with never a break for intuition. And because they never do the simple version, they don't realize (or at any rate don't admit to themselves) how fundamentally silly the whole thing sounds, how much it's at odds with lived experience.'"

And so has Truman Bewley (1999):

> "Lucas and Rapping (1969) claim that cyclical increases in unemployment occur when workers quit their jobs because wages or salaries fall below expectations …"
>
> "According to this explanation, when wages are unusually low, people become unemployed in order to enjoy free time, substituting leisure for income at a time when they lose the least income …"

> "According to the theory, quits into unemployment increase during recessions, whereas historically quits decrease sharply and roughly half of unremployed workers become jobless because they are laid off … During the recession I studied, people were even afraid to change jobs because new ones might prove unstable and lead to unemployment …"

> "If wages and salaries hardly ever fall, the intertemporal substitution theory is widely applicable only if the unemployed prefer jobless leisure to continued employment at their old pay. However, the attitude and circumstances of the unemployed are not consistent with their having made this choice …"

> "In real business cycle theory, unemployment is interpreted as leisure optimally selected by workers, as in the Lucas-Rapping model. It has proved difficult to construct business cycle models consistent with this assumption and with real wage fluctuations as small as they are in reality, relative to fluctuations in employment."

This is, of course, only what you would expect of New Classical Chicago economists.

So, what's the problem?

The problem is that sadly enough this rather extraterrestial view of unemployment is actually shared by Wren-Lewis and other so called 'New Keynesians' — a school whose microfounded dynamic stochastic general equilibrium models cannot even incorporate such a basic fact of reality as involuntary unemployment!

To Wren-Lewis is seems as though the "New Keynesian" acceptance of rational expectations, representative agents and microfounded DSGE models is something more or less self-evidently good. Not all economists (yours truly included) share that view. One of those economists, Sebastian Dullien (2011:181) writes:

> "While one can understand that some of the elements in DSGE models seem to appeal to Keynesians at first sight, after closer examination, these models are in fundamental contradiction to Post-Keynesian and even traditional Keynesian thinking. The DSGE model is a model in which output is determined in the labour market as in New Classical models and in which aggregate demand plays only a very secondary role, even in the short run."

> "In addition, given the fundamental philosophical problems presented for the use of DSGE models for policy simulation, namely the fact that a number of parameters used have completely implausible magnitudes and that the degree of freedom for different parameters is so large that DSGE models with fundamentally different parametrization (and therefore different policy conclusions) equally well produce time series which fit the real-world data, it is also very hard to understand why DSGE models have reached such a prominence in economic science in general."

Neither New Classical nor "New Keynesian" microfounded DSGE macro models have helped us foresee, understand or craft solutions to the problems of today's economies.

Wren-Lewis ultimately falls back on the same kind of models that he criticize, and it would sure be interesting to once hear him explain *how* silly assumptions like "hyperrationality" and "representative agents" help him work out the fundamentals of a truly relevant macroeconomic analysis.

In a recent paper on modern macroeconomics, another "New Keynesian" macroeconomist, Greg Mankiw (2006:42-43), writes:

> "The real world of macroeconomic policymaking can be disheartening for those of us who have spent most of our careers in academia. The sad truth is that the macroeconomic research of the past three decades has had only minor impact on the practical analysis of monetary or fiscal policy. The explanation is not that economists in the policy arena are ignorant of recent developments. Quite the contrary: The staff of the Federal Reserve includes some of the best young Ph.D.s, and the Council of Economic Advisers under both Democratic and Republican administrations draws talent from the nation's top research universities. The fact that modern macroeconomic research is not widely used in practical policymaking is prima facie evidence that it is of little use for this purpose. The research may have been successful as a matter of science, but it has not contributed significantly to macroeconomic engineering."

So, then what is the *raison d'être* of macroeconomics, if it has nothing to say about the real world and the economic problems out there?

If macoeconomic models – no matter of what ilk – assume representative actors, rational expectations, market clearing and equilibrium, and we know that real people and markets cannot be expected to obey these assumptions, the warrants for supposing that conclusions or hypothesis of causally relevant mechanisms or regularities can be bridged, are obviously non-justifiable. Macroeconomic theorists – regardless of being "'New Monetarist", "New Classical" or "New Keynesian" – ought to do some ontological reflection and heed Keynes' (2012 (1936):297) warnings on using thought-models in economics:

> "The object of our analysis is, not to provide a machine, or method of blind manipulation, which will furnish an infallible answer, but to provide ourselves with an organized and orderly method of thinking out particular problems; and, after we have reached a provisional conclusion by isolating the complicating factors one by one, we then have to go back on ourselves and allow, as well as we can, for the probable interactions of the factors amongst themselves. This is the nature of economic thinking. Any other way of applying our formal principles of thought (without which, however, we shall be lost in the wood) will lead us into error."

Wren-Lewis ought to be more critical of the present state of macroeconomics – including "New Keynesian" macroeconomics – than he is. Trying to represent real-world target systems with models flagrantly at odds with reality is futile. And if those models are New Classical or "New Keynesian" makes very little difference.

Fortunately – when you've got tired of the kind of macroeconomic apologetics produced by "New Keynesian" macroeconomists like Wren-Lewis, Mankiw, and Krugman, there still are some real Keynesian macroeconomists to read. One of them – Axel Leijonhufvud (2008:5) -- writes:

> "For many years now, the main alternative to Real Business Cycle Theory has been a somewhat loose cluster of models given the label of *New Keynesian theory*. New Keynesians adhere on the whole to the same DSGE modeling technology as RBC macroeconomists but differ in the extent to which they emphasise inflexibilities of prices or other contract terms as sources of short term adjustment problems in the economy. The 'New Keynesian' label refers back to the 'rigid wages' brand of Keynesian theory of 40 or 50 years ago. Except for this stress on inflexibilities this brand of contemporary macroeconomic theory has basically nothing Keynesian about it …"

> "I conclude that dynamic stochastic general equilibrium theory has shown itself an intellectually bankrupt enterprise. But this does not mean that we should revert to the old Keynesian theory that preceded it (or adopt the New Keynesian theory that has tried to compete with it). What we need to learn from Keynes … are about how to view our responsibilities and how to approach our subject."

No matter how brilliantly silly "New Keynesian" DSGE models central banks, Wren-Lewis, and his mainstream colleagues come up with, they do not help us working with the fundamental issues of modern economies. Using that kind of models only confirm Robert Gordon's (1976) dictum that today,

> "rigor competes with relevance in macroeconomic and monetary theory, and in some lines of development macro and monetary theorists, like many of their colleagues in micro theory, seem to consider relevance to be more or less irrelevant."

Romer follows up his critique

Romer (2016b) has himself commented on the critique he has got from other mainstreamers:

> "The one reaction that puzzles me goes something like this: 'Romer's critique of RBC models is dated; we've known all along that those models make no sense.'"

> "If we know that the RBC model makes no sense, why was it left as the core of the DSGE model? Those phlogiston shocks are still there. Now they are mixed together with a bunch of other made-up shocks."

> "Moreover, I see no reason to be confident about what we will learn if some econometrician adds sticky prices and then runs a horse to see if the shocks are more or less important than the sticky prices. The essence of the identification problem is that the data do not tell you who wins this kind of race. The econometrician picks the winner."

Those of us in the economics community who have been impolite enough to dare questioning the preferred methods and models applied in macroeconomics are as a rule met with disapproval. Although people seem to get very agitated and upset by the critique, defenders of "received theory" always say that the critique is "nothing new," that they have always been "well aware" of the problems, and so on, and so on.

But the rhetorical swindle that New Classical and "New Keynesian" macroeconomics have tried to impose upon us with their microfounded calibrations and DSGE models, has not gone unnoticed until Paul Romer came along. Thirty years before Paul Romer, James Tobin – in (Klamer (1984:110-111) – explained why real business cycle theory and microfounded DSGE models are such a total waste of time.

> "They try to explain business cycles solely as problems of information, such as asymmetries and imperfections in the information agents have. Those assumptions are just as arbitrary as the institutional rigidities and inertia they find objectionable in other theories of business fluctuations … I try to point out how incapable the new equilibrium business cycles models are of explaining the most obvious observed facts of cyclical fluctuations … I don't think that models so far from realistic description should be taken seriously as a guide to policy … I don't think that there is a way to write down any model which at one hand respects the possible diversity of agents in taste, circumstances, and so on, and at the other hand also grounds behavior rigorously in utility maximization and which has any substantive content to it."

And more recently, Rober Solow (2008:243-249) had this to say on "modern" macroeconomics:

> "I think that Professors Lucas and Sargent really seem to be serious in what they say, and in turn they have a proposal for constructive research that I find hard to talk about sympathetically. They call it equilibrium business cycle theory, and they say very firmly that it is based on two terribly important postulates – optimizing behavior and perpetual market clearing. When you read closely, they seem to regard the postulate of optimizing behavior as self-evident and the postulate of market-clearing behavior as essentially meaningless. I think they are too optimistic, since the one that they think is self-evident I regard as meaningless and the one that they think is meaningless, I regard as false. The assumption that everyone optimizes implies only weak and uninteresting consistency conditions on their behavior. Anything useful has to come from knowing what they optimize, and what constraints they perceive. Lucas and Sargent's casual assumptions have no special claim to attention …"
>
> "It is plain as the nose on my face that the labor market and many markets for produced goods do not clear in any meaningful sense. Professors Lucas and Sargent say after all there is no evidence that labor markets do not clear, just the unemployment survey. That seems to me to be evidence. Suppose an unemployed worker says to you 'Yes, I would be glad to take a job like the one I have already proved I can do because I had it six months ago or three or four months ago. And I will be glad to work at exactly the same wage that is being paid to those exactly like myself who used to be working at that job

> and happen to be lucky enough still to be working at it.' Then I'm inclined to label that a case of excess supply of labor and I'm not inclined to make up an elaborate story of search or misinformation or anything of the sort. By the way I find the misinformation story another gross implausibility. I would like to see direct evidence that the unemployed are more misinformed than the employed, as I presume would have to be the case if everybody is on his or her supply curve of employment … Now you could ask, why do not prices and wages erode and crumble under those circumstances? Why doesn't the unemployed worker who told me 'Yes, I would like to work, at the going wage, at the old job that my brother-in-law or my brother-in-law's brother-in-law is still holding', why doesn't that person offer to work at that job for less? Indeed why doesn't the employer try to encourage wage reduction? That doesn't happen either … Those are questions that I think an adult person might spend a lifetime studying. They are important and serious questions, but the notion that the excess supply is not there strikes me as utterly implausible."

The purported strength of New Classical and 'New Keynesian' macroeconomics is that they have firm anchorage in preference-based microeconomics, and especially the decisions taken by inter-temporal utility maximizing "forward-loooking" individuals.

To some of us, however, this has come at too high a price. The almost quasi-religious insistence that macroeconomics has to have microfoundations – without ever presenting neither ontological nor epistemological justifications for this claim – has put a blind eye to the weakness of the whole enterprise of trying to depict a complex economy based on an all-embracing representative actor equipped with superhuman knowledge, forecasting abilities and forward-looking rational expectations.

That anyone should take that kind of ludicrous stuff seriously is totally and unbelievably ridiculous. Or as Solow – in Klamer (1984:146) – has it:

> "Suppose someone sits down where you are sitting right now and announces to me that he is Napoleon Bonaparte. The last thing I want to do with him is to get involved in a technical discussion of cavalry tactics at the battle of Austerlitz. If I do that, I'm getting tacitly drawn into the game that he is Napoleon. Now, Bob Lucas and Tom Sargent like nothing better than to get drawn into technical discussions, because then you have tacitly gone along with their fundamental assumptions; your attention is attracted away from the basic weakness of the whole story. Since I find that fundamental framework ludicrous, I respond by treating it as ludicrous – that is, by laughing at it – so as not to fall into the trap of taking it seriously and passing on to matters of technique."

> "on ourselves the same high standards we had criticized the Keynesians for failing to live up to. But after about five years of doing likelihood ratio tests on rational expectations models, I recall Bob Lucas and Ed Prescott both telling me that those tests were rejecting too many good models. The idea of calibration is to ignore some of the probabilistic implications of your model but to retain others. Somehow, calibration was intended as a balanced response to professing that your model, although not correct, is still worthy as a vehicle for quantitative policy analysis…"

Conclusion

It is – sad to say – a fact that within mainstream economics internal validity is everything and external validity and truth nothing. Why anyone should be interested in that kind of theories and models – as long as mainstream economists do not come up with any export licenses for their theories and models to the real world in which we live – is beyond comprehension. Stupid models are of no or little help in understanding the real world.

In Chicago economics one is cultivating the view that scientific theories has nothing to do with truth. Constructing theories and building models is not even considered an activity with the intent of approximating truth. For New Classical Chicago economists like Lucas and Sargent it is only an endeavour to organize their thoughts in a "useful" manner.

What a handy view of science!

What Sargent and other defenders of scientific storytelling "forgets" is that potential explanatory power achieved in thought experimental models is not enough for attaining real explanations. Model explanations are at best conjectures, and whether they do or do not explain things in the real world is something we have to test. As Romer has argued forcefully in his latest articles – to just *believe* that you understand or explain things better with thought experiments is not enough! Without a warranted export certificate to the real world, model explanations are pretty worthless. Proving things in "post-real" macroeconomic models is not enough. Truth is an important concept in real science.

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Author contact: lars.palsson-syll@mah.se

SUGGESTED CITATION:

Lars Pålsson Syll, "Paul Romer's assault on 'post-real' macroeconomics", *real-world economics review*, issue no. 76, 30 September 2016, pp. 43-54, http://www.paecon.net/PAEReview/issue76/Syll76.pdf

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