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What Do We Actually Know About the Economy? (Wonkish)

Macroeconomics is better than you think, microeconomics worse, and data are limited



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In a couple of days I'm giving a luncheon talk to the New York chapter of the National Association of Business Economists, and the title of this essay was the title I provided for the talk. To be honest, it was a bit of a dummy title, and I wasn't at all sure what I would actually say; but I've been spending some time trying to pin things down, and found myself wanting to put it together in a little essay. So here are some meta reflections on economic knowledge, inspired in part but not entirely by the financial crisis and its aftermath.

Now, obviously the crisis has inspired both soul-searching among economists and a lot of outside criticism. But I'd argue that most of both the internal soul-searching and the outsider criticism is off-base.

Among macroeconomists, the self-criticism seems to me to be mainly too narrow: people berate themselves for, say, not giving financial markets a bigger role in their models, but few have done what they should, which is to question the whole direction macroeconomics has gone these past four decades or so.

Among economists more generally, a lot of the criticism seems to amount to the view that macroeconomics is bunk, and that we should stick to microeconomics, which is the real, solid stuff. As I'll explain in a moment, that's all wrong. In fact, in an important sense the past decade has been a huge validation for textbook macroeconomics; meanwhile, the exaltation of micro as the only "real" economics both gives microeconomics too much credit and is largely responsible for the ways macroeconomic theory has gone wrong.

Finally, many outsiders and some insiders have concluded from the crisis that economic theory in general is bunk, that we should take guidance from people immersed in the real world – say, business leaders — and/or concentrate on empirical results and skip the models. In reality, however, advice from business leaders has generally been worse than useless this past decade, while the voices in the air heard by madmen in authority have, as usual, given very bad advice. And while empirical evidence is important and we need more of it, the data almost never speak for themselves – a point amply illustrated by recent monetary events.

So let me talk about three things:

- The unsung success of macroeconomics
- The excessive prestige of microeconomics
- The limits of empiricism, vital though it is

The clean little secret of macroeconomics

There's a story about quantum physics – not sure where I read it – about the rivalry between the physicists Julian Schwinger and Richard Feynman. Schwinger was first to work out how to do quantum electrodynamics, but his methods were incredibly difficult and cumbersome. Feynman hit upon a much simpler approach – his famous diagrams – which turned out to be equivalent, but vastly easier to use.

Schwinger, as I remember the story, was never seen to use a Feynman diagram. But he had a locked room in his house, and the rumor was that that room was where he kept the Feynman diagrams he used in secret.

Modern macroeconomics is a bit like that, if you can imagine Schwinger in control of all the journals and in a position to prevent anyone from publishing the simpler version. What's the equivalent of Feynman diagrams? Something like IS-LM, which is the simplest model you can write down of how interest rates and output are jointly determined, and is how most practicing

macroeconomists actually think about short-run economic fluctuations. It's also how they talk about macroeconomics to each other. But it's not what they put in their papers, because the journals demand that your model have "microfoundations."

Now, the thing about IS-LM-type analysis is that using it isn't that big a deal in normal times, but it makes some very strong predictions – predictions very much at odds with many peoples' priors — about abnormal times. Specifically, this kind of analysis says that when there is a really big adverse shock to demand – say, from the collapse of a major housing bubble – there's a regime change, and neither monetary nor fiscal policy have the same effects they do in normal times.

On the monetary side, old-fashioned macro says that once interest rates have been driven down to the zero lower bound, monetary policy loses traction. Even huge increases in the monetary base (bank reserves plus currency in circulation) won't be inflationary. In fact, if you add in another old-fashioned approach some of us keep in our locked offices – Tobin-style analysis of the banking system – you conclude that big increases in the monetary base won't even do much to expand broader measures of the money supply.

We all know what happened. The Bernanke Fed massively expanded the monetary base, by a factor of almost five. There were dire warnings that this would cause inflation and "debase the dollar." But prices went nowhere, and not much happened to broader monetary aggregates (a result that, weirdly, some economists seemed to find deeply puzzling even though it was exactly what should have been expected.)

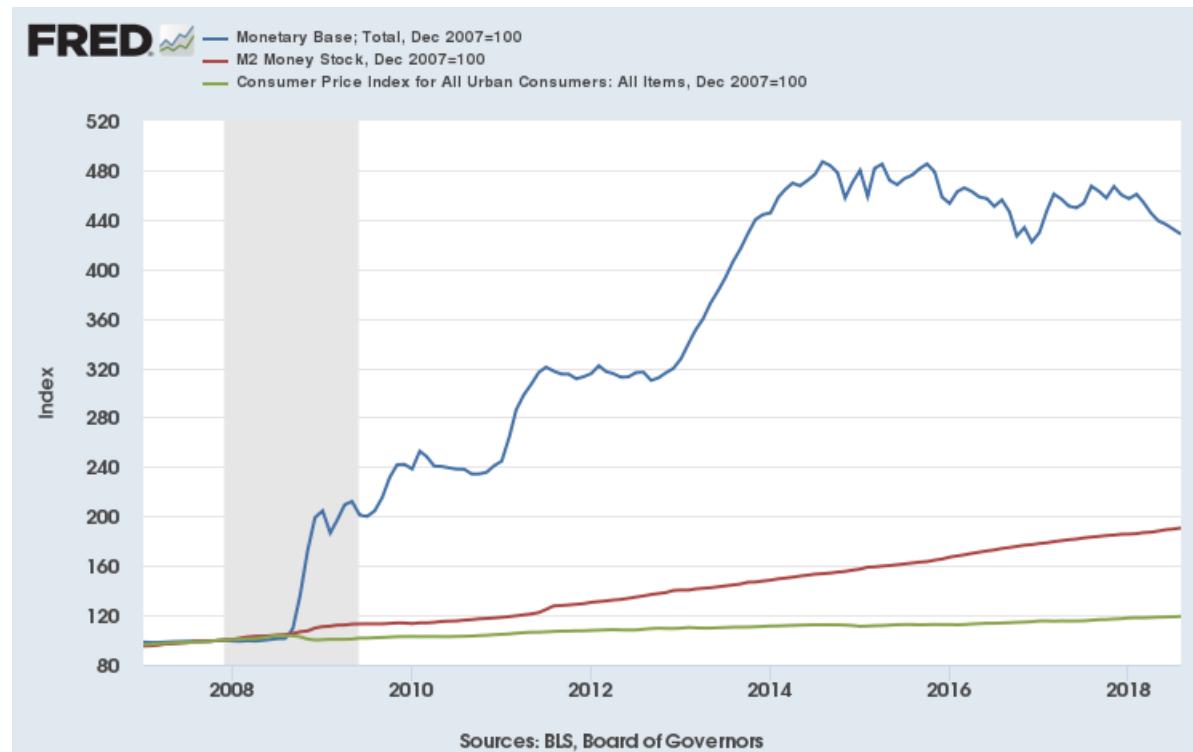


Figure 1 Federal Reserve, BLS

What about fiscal policy? Traditional macro said that at the zero lower bound there would be no crowding out – that deficits wouldn't drive up interest rates, and that fiscal multipliers would be larger than under normal conditions. The first of these predictions was obviously borne out, as rates stayed low even when deficits were very large. The second prediction is a bit harder to test, for reasons I'll get into when I talk about the limits of empiricism. But the evidence does indeed suggest large positive multipliers.

The overall story, then, is one of overwhelming predictive success. Basic, old-fashioned macroeconomics didn't fail in the crisis – it worked extremely well. In fact, it's hard to think of any other example of economic models working this well – making predictions that most non-economists (and some economists) refused to believe, indeed found implausible, but which came true. Where, for example, can you find any comparable successes in microeconomics?

But, you say, we didn't see the Great Recession coming. Well, what do you mean "we," white man? OK, what's true is that few economists realized that there was a huge housing bubble. But that's not a failure of fundamental models: the models certainly would have predicted that a bursting bubble that slashed residential investment by 4 percent of GDP and destroyed \$7 trillion in homeowners' equity would cause a severe recession. What happened was that economists refused to believe that home prices could be that out of touch with reality.

That's not exactly a problem with macroeconomics; to some extent it's a problem with financial economics, but mainly I think it reflected the general unwillingness of human beings (a category that includes many though not necessarily all economists) to believe that so many people can be so wrong about something so big.

The bottom line: the past decade has been a vindication, not a refutation, of good old-fashioned macro. Which brings me to the flip side: microeconomics is not as great as advertised.

The dirty little secret of microeconomics

I spent much of my academic, pre-public intellectual career straddling two surprisingly distinct economics sub-fields. To normal human beings the study of international trade and that of international macroeconomics might sound like pretty much the same thing. In reality, however, the two fields used very different models, had very different intellectual cultures, and tended to look down on each other. Trade people tended to consider international macro people semi-charlatans, doing ad hoc stuff devoid of rigor. International macro people considered trade people boring, obsessed with proving theorems and offering little of real-world use.

Both sides were, of course, right.

Anyway, I think it's fair to say that over the past few decades the economics profession has tended to take the micro side of this debate. Microeconomic theory, grounded in rigorous derivation of individual behavior from utility maximization, was taken as the gold standard. Old-fashioned macroeconomics, based on loose psychological propositions like the marginal propensity to consume, and often describing aggregate relationships without explicitly describing what individuals were doing, was considered dubious and uncouth.

Indeed, macroeconomists were sufficiently hurt by the sneers of microeconomists that they spent several decades trying to make their field as much like micro as they could.

But does microeconomics really deserve its reputation of moral and intellectual superiority? No.

Even before the rise of behavioral economics, any halfway self-aware economist realized that utility maximization – indeed, the very concept of utility — wasn’t a fact about the world; it was more of a thought experiment, whose conclusions should always have been stated in the subjunctive.

Yes, we believe that people tend to act in their self-interest and don’t usually pass up obvious opportunities to make themselves better off. So it made sense to follow that line of thought to its end point. What if we imagined individuals who knew what they wanted and pursued the optimal strategy, given the constraints they faced, to achieve as much of those goals as possible? If that were the case, what would that predict about behavior?

It’s an interesting and sometimes illuminating exercise. But it’s not proof that the world actually works that way. And the truth is that it often doesn’t. Kahneman and Tversky and Thaler and so on deserved all the honors they received for helping to document the specific ways in which utility maximization falls short, but even before their work we should never have expected perfect maximization to be a good description of reality.

True, a model doesn’t have to be perfect to provide hugely important insights. But here’s my question: where are the examples of microeconomic theory providing strong, counterintuitive, successful predictions on the same order as the success of IS-LM macroeconomics after 2008? Maybe there are some, but I can’t come up with any.

Just to be clear: there’s plenty of excellent micro work, both theory and empirical. What I’m talking about, however, is the kind of mind-altering, the-world-doesn’t-work-the-way-you-think-it-does stuff macro has achieved. When I look at the American Economic Review’s list of its top 20 papers, I think I may see one micro paper like that – Kenneth Arrow on health care. Other candidates?

In fact, when I try to come up with mind-altering empirical work on microeconomics, the example that comes most strongly to mind is the literature on the effect of minimum wage hikes – which happens to be an example of the facts *refuting* what the standard model told us to expect.

The point is not that micro theory is useless and we should stop doing it. But it doesn’t deserve to be seen as superior to macro modeling.

And the effort to make macro more and more like micro – to ground everything in rational behavior – has to be seen now as destructive. True, that effort did lead to some strong predictions: e.g., only unanticipated money should affect real output, transitory income changes shouldn’t affect consumer spending, government spending should crowd out private demand, etc. But

all of those predictions have turned out to be wrong.

Meanwhile, the demand that macro become ever more rigorous in the narrow, misguided sense that it look like micro led to useful approaches being locked up in Schwinger's back room, and in all too many cases forgotten. When the crisis struck, it was amazing how many successful academics turned out not to know things every economist would have known in 1970, and indeed resurrected 1930-vintage fallacies in the belief that they were profound insights.

What data can and can't tell us

Data are good (they are also, as far as I'm concerned, plural, although this is looking like a losing battle.) Some of my best friends are data. The growing focus of economists on empirical evidence is very much a good thing.

But data never speak for themselves, for a couple of reasons. One, which is familiar, is that economists don't get to do many experiments, and natural experiments are rare: the vast bulk of the data we see reflect the confounding effects of variables we aren't interested in, and reverse causation on the variables we are trying to assess.

The other problem is that even when we do get something like natural experiments, they often took place under economic regimes that aren't relevant to current problems.

Both of these problems were extremely relevant in the years following the 2008 crisis.

Start with the effects of monetary expansion. History actually provides us with many examples of countries that rapidly expanded their money supplies, and the great majority of these examples look like, say, Brazil in the 80s and 90s:

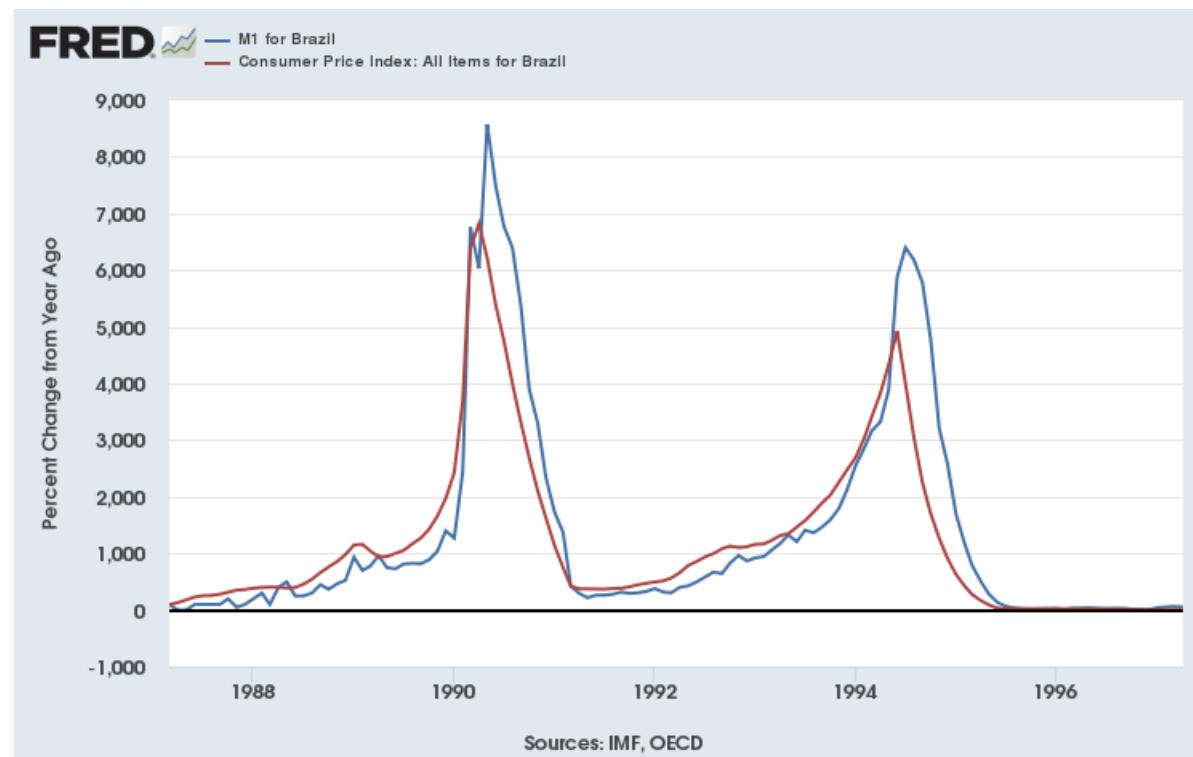


Figure 2 IMF

That is, you might be tempted to conclude that the empirical evidence is that monetary expansion is inflationary, indeed roughly one-for-one.

But the question, as the Fed embarked on quantitative easing, was what effect this would have *on an economy at the zero lower bound*. And while there were many historical examples of big monetary expansion, examples at the ZLB were much rarer – in fact, basically two: the U.S. in the 1930s and Japan in the early 2000s. These examples told a very different story: that expansion would not, in fact, be inflationary, that it would work out the way it did.

But you needed a model, something like IS-LM-with-Tobin, to tell you which examples were relevant. The data didn't speak for themselves.

What about fiscal policy? The raw correlation between budget deficits and real output is negative, not positive, but everyone knows that this is because most of the causation runs from GDP to the budget, not the other way around.

So you wanted to look for examples of major shifts in fiscal policy that didn't reflect automatic stabilizers. For the U.S. that mainly meant wars; across a broader set of countries, you could look at the effects of austerity programs. Both these sources of sort-of natural experiments suggested a positive multiplier, but less than one, i.e., private-sector crowding out or in.

But again, what we wanted was the effect of fiscal policy *at the zero lower bound*. Simple macro models suggested that the multiplier would be much larger in that case, and a variety of evidence – e.g., Blanchard and Leigh on austerity, or Nakamura and Steinsson on regional shocks now supports that conclusion.

I would also note that because assessing fiscal policy changes can be subject to a serious error-in-variables problem, you really want to look at extreme changes. This means, in particular, that when we're talking about austerity policies you want to look at the period 2009-2012, the post-Greece panic; everything after that is relatively small changes at the edges, subject to so much measurement error that you wouldn't expect to find clear results.

The point is that empirical evidence can only do certain things. It can certainly prove that your theory is wrong! And it can also make a theory much more persuasive in those cases where the theory makes surprising predictions, which the data bear out. But the data can never absolve you from the necessity of having theories.

So what do we know about the economy?

Over this past decade, I've watched a number of economists try to argue from authority: I am a famous professor, therefore you should believe what I say. This never ends well. I've also seen a lot of nihilism: economists don't know anything, and we should tear the field down and start over.

Obviously I differ with both views. Economists haven't earned the right to be snooty and superior, especially if their reputation comes from the ability to do hard math: hard math has been remarkably little help lately, if ever.

On the other hand, economists do turn out to know quite a lot: they do have some extremely useful models, usually pretty simple ones, that have stood up well in the face of evidence and events. And they definitely shouldn't defer to important and/or rich people on policy: compare Janet Yellen's macroeconomic track record with that of the multiple billionaires who warned that Bernanke would debase the dollar. Or take my favorite Business Week headline from 2010: "Krugman or [John] Paulson: Who You Gonna Bet On?" Um.

The important thing is to be aware of what we do know, and why.

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