

A new macroeconomics?

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July 2, 2021

[On Friday, July 2, I am taking part in a panel organized by Economics for Inclusive Prosperity on “A new macroeconomics?” This is my contribution.]

Jón Steinsson wrote up some thoughts about the current state of macroeconomics. He begins:

There is a narrative within our field that macroeconomics has lost its way. While I have some sympathy with this narrative, I think it is a better description of the field 10 years ago than of the field today. Today, macroeconomics is in the process of regaining its footing. Because of this, in my view, the state of macroeconomics is actually better than it has been for quite some time.

I can't help but be reminded of Olivier Blanchard's 2008 article on the state of macroeconomics, which opened with a flat assertion that “the state of macro is good.” I'm sorry to say that I'm not convinced today's positive assessment is going to hold up better than that one.

Where I do agree with Jón is that empirical work in macro is in better shape than theory. But I think theory is in much worse shape than he thinks. The problem is not some particular assumptions. It is the fundamental approach.

We need to be brutally honest: What is taught in today's graduate programs as macroeconomics is entirely useless for the kinds of questions we are interested in.

I have in front of me the macro comp from a well-regarded mainstream economics PhD program. The comp starts with the familiar Euler equation with a representative agent maximizing their utility from consumption over an infinite future. Then we introduce various complications — instead of a single good we have a final and intermediate good, we allow firms to have some market power, we introduce random variation in the production technology or markup. The problem at every step to find what is the optimal path chosen by the representative household under the new set of constraints.

This is what macroeconomics education looks like in 2021. I submit that it provides no preparation whatsoever for thinking about the substantive questions we are interested in. It's not that this or that assumption is unrealistic. It is that there is no point of contact between the world of these models and the real economies that we live in.

I don't think that anyone in this conversation reasons this way when they are thinking about real economic questions. If you are asked how serious inflation is likely to be over the next year, or how much of a constraint public debt is on public spending, or how income distribution is likely to change based on labor market conditions, you will not base your answer on some kind of vaguely analogous questions about a world of rational

households optimizing the tradeoff between labor and consumption over an infinite future. You will answer it based on your concrete institutional and historical knowledge of the world we live in today.

To be sure, once you have come up with a plausible answer to a real world question, you can go back and construct a microfounded model that supports it. But so what? Yes, with some ingenuity you can get a plausible Keynesian multiplier out of a microfounded model. But in terms of what we actually know about real economies, we don't learn anything from the exercise that the simple Keynesian multiplier didn't already tell us.

The heterogenous agent models that Jón talks about are to me symptoms of the problem, not signs of progress. You start with a fact about the world that we already knew, that consumption spending is sensitive to current income. Then you backfill a set of microfoundations that lead to that conclusion. The model doesn't add anything, it just gets you back to your starting point, with a lot of time and effort that you could have been using elsewhere. Why not just start from the existence of a marginal propensity to consume well above zero, and go forward from there?

Then on the other hand, think about what is not included in macroeconomics education at the graduate level. Nothing about national accounting. Nothing about about policy. Nothing about history. Nothing about the concrete institutions that structure real labor and product markets.

My personal view is that we need to roll back the clock at least 40 years, and throw out the whole existing macroeconomics curriculum. It's not going to happen tomorrow, of course. But if we want a macroeconomics that can contribute to public debates, that should be what we're aiming for.

What should we be doing instead? There's no fully-fledged alternative to the mainstream — let's be clear about that. There is no heterodox theory that is ready to step in to replace the existing macro curriculum. Still, we don't have to start from scratch. There are fragments, or building blocks, of a more scientific macroeconomics scattered around. We can find promising approaches in work from earlier generations, work in the margins of the profession, and work being done by people outside of economics, in the policy world, in finance, in other social sciences.

This work, it seems to me, shares a number of characteristics.

First, it is in close contact with broader public debates. Macroeconomics exists not to study "the economy" in the abstract — there isn't any such thing — but to help us address concrete problems with the economies that we live in. The questions of what topics are important, what assumptions are reasonable, what considerations are relevant, can only be answered from a perspective outside of theory itself. A useful macroeconomic theory cannot be an axiomatic system developed from first principles. It needs to start with the conversations among policymakers, business people, journalists, and so on, and then generalize and systematize them.

A corollary of this is that we are looking not for a general model of the economy, but a lot of specialized models for particular questions about the economy.

Second, it has national accounting at its center. Physical scientists spend an enormous amount of time refining and mastering their data collection tools. For macroeconomics, that means the national accounts, along with other sources of macro data. A major part of graduate education in economics should be gaining a deep understanding of existing accounting and data collection practices. If models are going to be relevant for policy or empirical work, they need to be built around the categories of macro data. One of the great vices of today's macroeconomics is to treat a variable in a model as equivalent to a similarly-named item in the national accounts, even when they are defined quite differently.

Third, this work is fundamentally aggregative. The questions that macroeconomics asks involve aggregate variables like output, inflation, the wage share, the trade balance, etc. No matter how it is derived, the operational content of the theory is a set of causal relationships between these aggregate variables. You can certainly shed light on relationships between aggregates using micro data. But the questions we are asking always need to be posed in terms of observable aggregates. The disdain for "reduced form" models is something we have to rid ourselves of.

Fourth, it is historical. There are few if any general laws for how "an economy" operates; what there are, are patterns that are more or less consistent over a certain span of time and space. Macroeconomics is also historical in a second sense: It deals with developments that unfold in historical time. (This, among other reasons, is why the intertemporal approach is fundamentally unsuitable.) We need fewer models of "the" business cycle, and more narrative descriptions of individual cycles. This requires a sort of figure-ground reversal in our thinking — instead of seeing concrete developments as case studies or tests of models, we need to see models as embedded in concrete stories.

Fifth, it is monetary. The economies we live in are organized around money commitments and money flows, and most of the variables we are interested in are defined and measured in terms of money. These facts are not incidental. A model of a hypothetical non-monetary economy is not going to generate reliable intuitions about real economies. Of course it is sometimes useful to adjust money values for inflation, but the habit of referring to the result quantities as "real" is misleading — it suggests that there is some objective quantity lying behind the monetary one, which is in no way the case.

In my ideal world, a macroeconomics education would proceed like this. First, here are the problems the external world is posing to us — the economic questions being asked by historians, policy makers, the business press. Second, here is the observable data relevant to those questions, here's how the variables are defined and measured. Third, here are how those observables have evolved in some important historical cases. Fourth, here are some general patterns that seem to hold over a certain range — and just as

important, here is the range where they don't. Finally, here are some stories that might explain those patterns, that are plausible given what we know about how economic activity is organized.

Well, that's my vision. Does it have anything to do with a plausible future of macroeconomics?

I certainly don't expect established macroeconomists to throw out the work they've been doing their whole careers. Among younger economists, at least those whose interest in the economy is not strictly professional, I do think there is a fairly widespread recognition that macroeconomic theory is at an intellectual dead end. But the response is usually to do basically atheoretical empirical work, or go into a different field, like labor, where the constraints on theory are not so rigid. Then there is the heterodox community, which I come out of. I think there has been a great deal of interesting and valuable work within heterodox economics, and I'm glad to be associated with it. But as a project to change the views of the rest of the economics profession, it is clearly a failure.

As far as I can see, orthodox macroeconomic theory is basically unchallenged on its home ground. Nonetheless, I am moderately hopeful for the future, for two reasons.

First, academic macroeconomics has lost much of its hold on public debate. I have a fair amount of contact with policymakers, and in my experience, there is much less deference to mainstream economic theory than there used to be, and much more interest in alternative approaches. Strong deductive claims about the relationships between employment, inflation, wage growth, etc. are no longer taken seriously.

To be sure, there was always a gulf between macroeconomic theory and practical policymaking. But at one time, this could be papered over by a kind of folk wisdom — low unemployment leads to inflation, public deficits lead to higher interest rates, etc. — that both sides could accept. Under the pressure of the extraordinary developments of the past dozen years, the policy conversation has largely abandoned this folk wisdom — which, from my point of view, is real progress. At some point, I think, academic economics will recognize that it has lost contact with the policy conversation, and make a jump to catch up.

Keynes got a lot of things right, but one thing I think he got wrong was that "practical men are slaves to some defunct economist." The relationship is more often the other way round. When practical people come to think about economy in new ways, economic theory eventually follows.

I think this is true to some extent even of people who in their day job do theory in the approved style. They don't think in terms of their models when they are answering real world questions. And this in turn makes our problem easier. We don't need to create a new body of macroeconomic theory out of whole cloth. We just need to take the implicit models that we already use in conversations like this one, and bring them into scholarship.

That brings me to my second reason for optimism. Once people realize you don't have to have microfoundations, that you don't need to base your models on optimization by anyone, I think they will find that profoundly liberating. If you are wondering about, say, the effect of corporate taxation on productivity growth, there is absolutely no reason you need to model the labor supply decision of the representative household as some kind of intertemporal optimization. You can just, not do that. Whatever the story you're telling, a simple aggregate relationship will capture it.

The microfounded approach is not helping people answer the questions they're interested in. It's just a hoop they have to jump through if they want other people in the profession to take their work seriously. As Jón suggests, a lot of what people see as essential in theory, is really just sociological conventions within the discipline. These sorts of professional norms can be powerful, but they are also brittle. The strongest prop the current orthodoxy has is that it *is* the orthodoxy. Once people realize they don't have to do theory this way, it's going to open up enormous space for asking substantive questions about the real world.

I think that once that dam breaks, it is going to sweep away most of what is now taught as macroeconomics. I hope that we'll see something quite different in its place.

Once we stop chasing the will-o-wisp of general equilibrium, we can focus on developing a toolkit of models addressed to particular questions. I hope in the years ahead we'll see a more modest but useful body of theory, one that is oriented to the concrete questions that motivate public debates; that embeds its formal models in a historical narrative; that starts from the economy as we observe it, rather than a set of abstract first principles; that dispenses with utility and other unobservables; and that is ready to learn from historians and other social scientists.