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25-8 Convergence? Thoughts about the Evolution of Mainstream Macroeconomics over the Last 40 Years

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ABSTRACT

This year marks the 40th anniversary of the NBER Macro Annual Conference, founded in 1986. This paper reviews the evolution of mainstream macroeconomics since then. It presents my views, informed by a survey of a number of researchers who have made important contributions to the field. I develop two main arguments. The first is that, starting from strikingly different positions, there has been substantial convergence, in terms of methodology, architecture, and main mechanisms. Methodology: Explicit micro foundations, explicit treatment of distortions, with, at the same time, an increased willingness to deviate from rational expectations, neoclassical utility and profit maximization. Architecture: The wide acceptance of nominal rigidities as an essential distortion, although with mixed feelings. Mechanisms: The wide nature of the shocks to both the demand and the supply side. The second is that this convergence has been, for the most part, good convergence, i.e., the creation of a generally accepted conceptual and analytical structure, a core to which additional distortions can be added, allowing for discussions and integration of new ideas and evidence, rather than fights about basic methodology. Not everything is right however, with too much emphasis on general equilibrium implications from the start, rather than, first, on partial equilibrium analysis of the phenomenon at hand. The appendix to the paper gives a sample of the views of the members of the survey on each of these arguments.

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Author's Note: I thank the economists (too many of them to be able to thank individually) who answered my survey and made comments. In many cases, it led to further interactions and fascinating discussions, which, hopefully, are reflected in this paper.

This year marks the 40th anniversary of the NBER Macro Annual Conference, founded by Stanley Fischer in 1986. At this occasion, I was asked by the organizers to look back and give my assessment of the evolution of mainstream macroeconomics over those 40 years. I unwisely accepted.

The remarks below reflect my own subjective and rather optimistic views. To get a sense of how the profession feels, however, and how off I might be in my perceptions, I decided to ask a few questions to some of the researchers who have played a central role in this evolution. I tried to choose researchers from different horizons (although, admittedly, all within the (wide) mainstream, with most of them being members of one of the NBER macro programs). I chose 50 researchers, of whom 27 responded. Looking at the set of researchers who responded, I feel it is a fair representation of the views of researchers in the field. The full set of comments is fascinating, and I hope that I can make it available later. For the time being, I report some of them in appendix boxes below.

Let me state my two main conclusions. First, starting from sharply different views, there has been substantial convergence, both in terms of methodology and in terms of architecture. Second, this convergence has been mostly in the right direction, allowing future research to build on the existing conceptual structure. Put strongly, macroeconomics may have a claim to calling itself a mature science.[1]

[1]Warning: In the summer of 2008, when asked to review the state of macroeconomics, I stated that "the state of macroeconomics is good." Given what happened that very fall, I have been roundly criticized for that statement. Indeed, we (including me), as a profession, had missed the complexity and importance of the financial system. But my belief was that we were building a structure on which we could agree and build further, and 17 years later, I would double down on that statement.

Let me start with some accolades.

When Martin Feldstein and Stanley Fischer decided to create the NBER Macro Annual in 1986, they did so because they thought there was a need to strengthen the two-way links between empirics and theory. Thus, Stan commissioned articles on theoretical developments and articles using theory to analyze facts and policy. He also made sure that there would be serious exchanges between researchers with widely different views.[2] The editors and co-editors who followed adhered to the same principles, and, in these dimensions, the NBER Macro Annual is a glowing success.[3] The quality of the papers has been uniformly high, but much of the value has also come, and maybe even more so, from the quality of the discussions, giving a sense of the heterogeneity of views. The range of topics—from partial equilibrium explorations to general equilibrium closures, from the analysis of major evolutions and crises to the increasing focus on distribution implications—is simply impressive. To prepare for this paper, I actually looked at all 40 years of articles and comments.[4] This took a few weeks, but it was the best refresher course I could have taken. (If I had a minor complaint about the journal, it is the austere graphs in black and white that often felt more like the 19th than the 21st century.) I shall cite along the way a number of NBER Macro Annual papers (and a few other papers) that I found particularly interesting or representative.

[2] It is worth pointing out the important role of the NBER (thanks in particular to Robert Hall as the head of the Economic Fluctuations program for many years) in avoiding a schism that could well have emerged, given the intellectual atmosphere and the tensions at the end of the 1970s.

[3] Full disclosure: I was an editor from 1989 to 1993.

[4] All papers are accessible in free access, starting with the 1986 volume, at https://www.journals.uchicago.edu/toc/ma/1986/1.

I have talked about the quality of the NBER Macroeconomics Annual. But it clear that this journal, along with a number of other journals, has been both motor and mirror of the wider evolution of the field. As macroeconomists, we should stop self-flagellating and not accept flagellation from others.

1 Convergence?

The history of macroeconomics since 1986 is one of convergence. Let me go through a quick and well-known history. The field had exploded roughly ten years earlier, with Robert Lucas, Thomas Sargent, and Edward Prescott leading the charge against the then-dominant approach, led by James Tobin, Franco Modigliani, and, at the younger end, Stanley Fischer and John Taylor, among others. Tensions were sharp, with strong disagreements both about methodology and about the source of economic fluctuations.

By 1986, the profession was clearly divided. The first two articles published in the first Macroeconomics Annual were representative. An article by Martin Eichenbaum and Ken Singleton [13] looked at the potential role of money in a real business cycle (RBC) model, using a fully micro-founded model. The other, by Lawrence Summers and myself [7], explored the role of hysteresis, in the form of persistent effects of demand shocks, using a simple, ad hoc (and admittedly rather clunky) model.

The discussion continued over the next four decades, largely focused on the empirical evidence. How much evidence was there for technological shocks? Could labor supply be sufficiently elastic to explain movements in output? Did money explain a large proportion of fluctuations? What were the effects of fiscal policy? There were some tense moments. A paper by Leeper and Sims in 1994 [23], proposing a theory-based, medium-sized empirical model, triggered a strong response from Larry Meyer, defending the traditional approach. A paper by Jordi Gali and Pau Rabanal in 2004 [17], arguing that the RBC model did not fit the facts, was strongly attacked by Ellen McGrattan, one of the discussants. But over time, the force of facts and open discussions led to cross-pollination. By the mid-2000s, full-fledged dynamic stochastic general equilibrium models (DSGEs) appeared, perhaps most notably Frank Smets and Rafael Wouters (2007) [27], and since then, all general equilibrium models published in the NBER Macro Annual have had a DSGE-like structure.

Where are we today?

The first question I asked in the survey was: "Focusing on economic fluctuations rather than growth, do you think that we (the profession) have largely converged (in terms of methodology and main mechanisms)?" There was broad agreement that there had been convergence: 17 researchers answered yes, 6 answered no, and 4 did not take a stand. The answers, however, came with many caveats, which I shall relate below. A sample of comments is given in the first appendix box.

Let me distill what I see as the main conclusions, separating methodology, architecture, and specific mechanisms.[5]

[5] I wonder whether this is not, in fact, historically the second attempt at convergence. One can see the work of Friedman and Modigliani on consumption, Jorgenson and Hall on investment, Tobin on financial markets, and many others as developing the required elements of a general equilibrium macroeconomic model, based, although more loosely than today, on micro foundations. These contributions were then integrated in large

There has broad convergence on methodology, namely the requirement to start from micro foundations, defined as some form of optimizing behavior by people and firms, and a list of specific distortions. Note the "some form of." Ironically, while two of the initial pillars of the new approach were the assumptions of neoclassical optimizing behavior and rational expectations, some of the progress has come from relaxing these two assumptions—for example, allowing for behavioral traits such as myopia or the formation of non-rational expectations. But micro foundations is now a sine qua non of any theoretical paper, making macro more similar to other fields, such as public finance or industrial organization.

The landscape looks very different when considering the variety of empirical methods. The initial fights between calibration and econometrics are gone. From the outside (as I am not an econometrician), it looks like (Bayesian) estimation is seen as the default option for DSGEs, although with the empirical evidence coming from many more convincing sources (more on this below). Increased attention is paid to identification. Structural VARs, or their cousins, local projections, or approaches based on identifying the response to particular measures or events—such as the paper by David and Christina Romer on monetary policy (1994) [24]—and the use of more disaggregated evidence, such as the paper by Adam Guren et al. (2020) [20]

macroeconomic models, such as the MIT-Penn-SSRC model, developed by Ando and Modigliani, or the Wharton model, developed by Larry Klein. These models got attacked for various reasons (on the surface, for their treatment of expectations and the Lucas critique), but the main problem was that the integration was not fully successful: While the pieces made sense, the overall models had strange internal properties, and, as Chris Sims pointed out, they often properties often at odds with the reduced form empirical evidence. Something had failed along the way. One can see this second convergence as a more successful attempt at integration of the parts using cross-regional evidence to look at the effects of housing on consumption, all coexist. The use of big data for macroeconomics is also starting, as in the paper by Raj Chetty et al. (2012) [11] on the relation between micro and macro estimates of labor supply elasticities. There is convergence, however, in the sense that these methods are generally seen as complements rather than substitutes. What they have in common is increased attention to identification and creativity in getting information from new data sets.

Turning to architecture, there has also been convergence to one broad class of models, namely the New Keynesian (NK) model, both in its minimalist three-equation incarnation and its many dynamic stochastic general equilibrium (DSGE) extensions, allowing for distortions, heterogeneity, and deviations from neo-classical optimization. What is striking (at least from the perspective of the 1980s) is the role of nominal rigidities. With this in mind, I asked the following question: "Do you see nominal rigidities as an essential ingredient in explaining economic fluctuations?" 24 respondents said yes, 3 said no. But many of the comments convey a sense of unease—that we assume nominal rigidities partly by default, for lack of a better assumption. A sample of the comments is given in the second appendix box.

From the comments, it appears that the unease has two sources. The first is nearly philosophical. One would hardly have thought that, starting from the Arrow-Debreu model, perhaps the most important deviation (from the point of view of explaining aggregate fluctuations) would be nominal rigidities (although earlier theoretical papers had explored convergence to equilibrium in the absence of an auctioneer and shown that it was far from obvious.). At the same time, there is a wide belief that movements in aggregate demand can have a large effect on output and that the interest rate does not move enough in response. Alternative explanations have not proven convincing, whereas nominal rigidities deliver.

The second source is practical: The widely used Calvo formalization is seen as elegant but clearly at odds with reality, serving as a placeholder until we better understand the nature of both wage and price rigidity. Indeed, in empirical models, to fit the data, researchers have often felt the need to add an ugly add-on—indexation of price inflation to lagged inflation—an indexation that barely exists in reality.

Turning finally to mechanisms, a number of comments reflect the belief that convergence has not yet been achieved. Much of the discussion over the years focused on the role and nature of technological shocks and the role and nature of demand shocks. It is fair to say that today, most researchers believe that both types of shocks are relevant. But it is clear that "technological" and "demand" shocks are semantic placeholders. Much of the movement in the measured Solow residual is endogenous, and the true underlying shocks are probably many in nature, with different short- and long-run implications. Similarly, what is behind demand shocks—namely, whether it is changes in preferences (typically the demand shocks assumed in model simulations), shifts in expectations, or changes in perceived risk—remains unclear. Recessions differ significantly in the composition of the output decline, pointing to many different sources of shocks. The same type of discussion applies to mechanisms: Even with the large amount of research done at central banks, the precise channels through which monetary policy works—such as the respective roles of credit versus financial markets—remain uncertain.

I am actually more optimistic than the median comment. I fully agree that we do not have a unified and convincing explanation of shocks and mechanisms. I am, however, impressed by how quickly researchers were able to analyze the effects of the COVID-19 shock, or by the nature of the discussions about the recent inflation burst. Even there, while there are still disagreements about the relative importance of shocks and specific transmission mechanisms, the discussion is mostly about relative magnitudes, with much in common—both methodologically and empirically—between the different approaches.

2 Good convergence?

Convergence does not necessarily mean good convergence. It is indeed a risky bet to start from an Arrow-Debreu economy and get much of the way—through the closing of many markets, the introduction of distortions, deviations from standard maximizing behavior, and the introduction of macroeconomic policy and political economy considerations—to something resembling the actual economy. Think, for example, of the distance between an Arrow-Debreu economy and the Greek economy during the Greek crisis (Gourinchas et al. 2016) [19]. Yet, this is the strategy behind the convergence I have described above. To use the old cliché: if we wanted to get there, should we have started from here?

With this in mind, I asked a third question in my survey: "If you think we have converged or are converging, are we converging in the right direction (methodology and main mechanisms)?" 17 respondents answered yes, 6 no. Again, most of the comments reflected mixed assessments. Few questioned the general direction. Some argued that DSGE models had become too large, too unwieldy, too much of a black box to be truly useful. Others argued that we were still missing essential elements—for example, that the division of labor between work on the short run and work on the long run had left a gap in research on the important medium run. Yet others argued for deeper interaction with other fields, with one citing "behavioral economics, socioeconomics, psychological economics, political economy, experimental economics, anthropological economics, neuroeconomics, and now cognitive economics." One worried that convergence might be mainly the result of an echo chamber. A sample of the comments is given in the third appendix box.

Nobody, however, offered or even argued for a fundamentally different approach. (One may fairly argue that the conclusion is biased by only asking mainstream researchers what they thought, even if the mainstream is a very large one. It is, however, also fair to say that heterodox views have not coalesced into a widely agreed alternative approach. What has happened, however, is that some heterodox ideas have made it to the mainstream. Perhaps the most obvious one is the work by Hyman Minsky on financial markets.)

Here is my own take. Put bluntly: Is the minimalist model—say, the NK model developed by Jordi Gali in his book on monetary policy (2015) [16]—better than the IS-LM model or its open-economy cousin, the Mundell-

Fleming model? After all, the NK model is composed of three equations, two of them obviously false as descriptions of reality—namely, the Euler equation as a description of consumer behavior and the Calvo equation as a description of the behavior of price setters. (The third equation is a policy rule, giving the interest rate as a function of output and inflation.) The answer, thus, has to be a clear no... But things are more complicated:

To me, the right analogy is Daniel Kahneman's two systems of thinking, fast and slow.[6] I think of the IS-LM model as the fast-thinking system, allowing one to quickly organize thoughts when faced with a complex economic situation (this is indeed how I think of teaching at the undergraduate level and how I organize my undergraduate textbook. I wish it was a skill taught to graduate students as well.) I think instead of the minimalist model and its DSGE extensions, as the slow-thinking system.

To mix metaphors, I see the minimalist model as the basic unit in an erector set.[7] By itself, the basic unit is not extremely useful, but you can plug into it a whole set of extensions. You can extend it to introduce myopia, as Gabaix (AER 2020) [15] or Woodford (2018) [29] have done, and reduce the role of expectations. You can replace rational expectations with other expectation formation mechanisms. You can extend it to include borrowing constraints, which lead to a more important role for current variables and more realistic consumption dynamics. You can extend it to more than one country. You can extend it to introduce various forms of heterogeneity

[6] Daniel Kahneman. Thinking, Fast and Slow. New York, NY: Farrar, Straus and Giroux, 2011 [22].

[7] In discussions with students, I have used the analogy of the Meccano erector set. As a child, I dreamed of getting more and more units so as to build increasingly complex contraptions. I have found, however, that most students have no idea what Meccano is.

and derive aggregate implications. In short, it provides a common and generally understood structure from which to start and organize research and discussion.

Two specific issues I want however to raise in this context.

The first is about can be called "DSGE imperialism". I want to endorse a criticism made by Ricardo Caballero in a 2010 paper[10], against the implicit requirement, sometimes imposed by journal editors, that every paper exploring a particular mechanism or event provide a general equilibrium closure. In most cases, the right approach is indeed to study, theoretically or empirically, the mechanism in a partial equilibrium context, even if, eventually, the contribution has to be integrated in a general equilibrium model. Going too fast to the second step comes too often at the cost of counterproductive contorsions.

The second is about the minimalist model itself. It remains true that two of the three equations of the minimalist model are strongly at odds with reality. I would be happier if we agreed to move to a slightly different one. While heterogeneous agent (HANK) models are complex to handle, two-agent models (TANK), with both hand-to-mouth and unconstrained consumers, go a long way toward capturing the effect of current as opposed to future expected income. And myopia, for example à la Gabaix, reduces the role of far-ahead expectations, eliminating some of the puzzling implications of their overly strong role in the minimalist model. This would still leave only three equations and lead to the addition of two parameters—the degree of myopia and the proportion of constrained consumers—and the model would remain simple and a useful starting point.[8]

Let me end this section with some remarks on macroeconomics and aggregation.

Macroeconomics is about the behavior of aggregates. One criticism of so-called representative agent models was their ignorance of aggregation problems. Indeed, the Sonnenschein-Mantel-Debreu theorem reminded us that, absent other restrictions, aggregating the decisions derived from rational individual preferences imposes very few restrictions on the shape of the aggregate excess demand function. Progress has come from research on aggregation. For example, the characterization of aggregate consumption when people are exposed to idiosyncratic shocks and face borrowing constraints has helped us understand consumption dynamics (and replace, for example, some of the kludges such as external habit formation, which were needed to explain consumption dynamics in the initial DSGE models). Work on the aggregation of (S,s) rules—whether in pricing, investment, or purchases of durables (for example, Giuseppe Bertola and Ricardo Caballero, 1990 [4])—has shown how aggregation leads to smoother but complex dynamics at the aggregate level.

I have, however, a remaining worry, coming from two episodes I have experienced and done research on. It is whether we always have the degree of granular information needed to predict aggregate dynamics. The first episode is the transition in Eastern Europe in the 1990s. Contrary to

[8] What "simple" means has changed with technology. In the 1970s, much time was spent reducing models to two dimensions to create phase diagrams and work out analytic solutions. This often required unpleasant contortions. Today, a model with three or four dimensions can be fully understood with a few simulations and would qualify as simple.

most predictions, the move to market prices was associated with a large initial decline in output. I have become convinced that much of this had to do with the breakdown of bilateral relations between suppliers and buyers—relationships that had been enforced under central planning but were now left to the discretion of buyers and sellers, leading to network failures (Olivier Blanchard and Michael Kremer, 1997) [6]. Even if we had understood in real time the implications of these breakdowns, I am not sure we could have had sufficiently granular information to predict the size of the output fall. The second episode is the Global Financial Crisis, which made clear how relationships between specific financial institutions could lead to a collapse of the financial system (Blanchard, 2014) [5]. We have made progress in understanding the implications of networks (for example, Acemoglu et al. 2015 [1]), and governments have put in place rules to limit some adverse network effects in financial markets, but I suspect there will be times when we lack the granular knowledge needed to understand major economic developments.[9] The current and impending tariff wars might provide such examples.

3 A closer look. Five issues.

Moving away from convergence, let me make remarks on five issues.

Is the economy stable?

An age-old question is whether the economy is intrinsically stable or not. I do not think the issue is settled. Within the class of models we have been

[9] See also Caballero 2010[10] on this issue.

using, there are at least two relevant dimensions. The first is the issue of stability in linear models. The second is the importance of non-linearities. On the first, in the (log-linearized version of the) minimalist model, one needs surprisingly strong conditions to achieve stability. The interest rate set by the central bank has to be sufficiently reactive to output and inflation to stabilize the economy. The condition fails, for example, if the economy is expected to be at the zero lower bound permanently (see the discussion in Troy Davig and Eric Leeper, 2006 [12]). An important issue in this context is the role of expectations: myopia, for example, leads to the need for a weaker stability condition. An intriguing paper by Robert Hall and Marianna Kudlyak (2022) [21], however, shows a strong tendency of the U.S. economy to return to equilibrium after sharp increases in unemployment, suggesting the possibility of a natural adjustment mechanism at work that we may be missing. On non-linearities, there is plenty of evidence that they are sometimes important. Multiple equilibria appear to be behind many currency or sovereign debt crises. The open issue is whether non-linearities are indeed important in quieter environments, in explaining regular fluctuations (see the discussion in Beaudry et al. 2016 [3]), or only in "dark corners."

What is the role of expectations?

The assumption of rational expectations was at the center of the attack against "old macroeconomics." In effect, expectations disappeared as an exogenous variable. Much of the research since then, however, has been about going beyond that assumption and understanding how people and firms actually form expectations—the relevance of salience, people's interpretation of history, and what implications this has for fluctuations. Papers by Andreas Fuster et al. (2011) [14], Pedro Bordalo et al. (2024) [8], and Marios Angeletos et al. (2020) [2] are in that mode. Given the difficulty of explaining investment empirically, the paper by Nicola Gennaioli et al. (2015) [18] on long-term earnings forecasts and investment is particularly interesting, showing both a deviation of these forecasts from rational expectations, as well as a good fit of investment with those forecasts. Work on the actual formation of expectations, together with the exploration of myopia, is representative of the increasing attention paid to actual behavior and the influence of behavioral economics, but more has to come. For example, the degree of liquidity constraints needed to fit actual consumption is, I believe, higher than the true hard borrowing constraints faced by consumers. I suspect that, to a large extent, they reflect behavioral traits—putting different sources of income and wealth in different boxes, with different marginal propensities to consume.

What about the medium run?

Research on the short run (fluctuations) and the long run (growth) have largely gone their separate ways.[10] I have focused above on research on fluctuations, but many papers have explored models of endogenous growth—looking into the nature of the Solow residual (for example, Paul Romer, 1987 [25]), the innovation process (Ricardo Caballero and Adam Jaffe, 1993) [9], the

[10]For a long time, ironically, there was a difference in strategy between the two, with real business cycle research exploring the implications of an economy without distortions, while research on growth focused from the start on the role of distortions and externalities in generating the Solow residual. Distortions now play a major role in both.

role of different growth strategies (for example, Alwyn Young, 1992, on Hong Kong versus Singapore [30]), and the relation between climate, climate policy, and growth (James Stock, 2019) [28]. However, the medium run per se, and issues such as the sources and implications of changes in the labor/profit share—a central issue, for example, in Marxist economics—has largely fallen into the void.[11] The relevance of hysteresis, or more generally, whether we can largely separate the short and the long run, is also not settled. The changes in the labor market following COVID-19 may be the strongest example we have of long-lasting impacts of temporary shocks.

Do we really agree about mechanisms and shocks?

This is where the answers from the survey were the most mixed. Obviously, we must keep digging to better understand the nature of shocks and the relevant mechanisms (leaving aside the nearly philosophical issue of what an "exogenous shock" is). But while journalists (and some of us) point to major disagreements among economists about the sources of shocks and mechanisms in, say, the recent inflation episode, these disagreements are taking place within a largely agreed-upon methodology. The same is true of the heated discussions about the need for and effects of fiscal austerity during the euro crisis. Reality is complex, and even with the same methodology and tools, answers can differ. There are some issues where we are clearly not where we would like to be. Each of us has a list. Here are the three at the top of mine:

We are a long way from understanding the specifics of nominal rigidities.

[11] I tried to convince the profession that it was essential (Blanchard, BPEA, 1997), but with little impact.

Much of the initial emphasis was on price setting, because, in large part, of the convenience of Dixit-Stiglitz monopolistic competition and Calvo pricing assumptions (for an important early exception, and an exploration of oligopolistic pricing, see Julio Rotemberg and Michael Woodford (1991)[26]). It is clear, however, that most nominal rigidities come from wage setting. Despite much work on the labor market, the nature of flows, and the scope for bargaining, we are some way from truly understanding the relation between inflation and activity.

We are also far from understanding the perceptions of risk by people, firms, and investors, and their implications for the economy. While risk has obviously always been central to finance, it is clear that risk perceptions can have a first-order effect on activity. As we know from the forward premium puzzle, movements in exchange rates reflect as much the risk perceptions of investors as they do interest rate differentials or current account balances. The effects of current U.S. policy uncertainty on investment, and the associated option value of waiting, are another example.

Finally, and related to the role of nominal rigidities, the relevance of the choice of the unit of account—for example, the implications of producer currency versus local currency pricing in the international context and its policy implications—must be explored further.

How much has macroeconomic research affected macroeconomic policy?

Presumably, one of the end products of research must be better policy. How influential has macroeconomic research been on policymakers? The answer is: Very influential in central banks. Most central banks have developed their own DSGE models and use them for simulations and even as an input for forecasts. More importantly, the discussions have led to major changes in monetary policy—the shift from monetary aggregates to interest rates, the adoption of inflation targeting, the discussion of forward guidance, and the use of quantitative easing when nominal interest rates became constrained by the effective lower bound. Beyond monetary policy, research has been less influential—but part of the blame lays with policy makers rather than macroeconomists—, but extremely reactive to specific events. The Global Financial Crisis and the COVID-19 crisis all led very quickly to abundant new research; the research may not have contributed much to policy in real time, but it has led to a better understanding of financial markets and of supply disruptions, which hopefully will be useful for the future. The new protectionist policies of the Trump administration—which surely did not derive from macroeconomic research—are likely to have the same academic effect, leading to a better understanding of tariffs and other protectionist measures on activity.

4 Bottom line: Is Macroeconomics now a mature science?

To conclude, I decided to ask ClaudeAI what defines a mature science. Its answer, and my assessment next to each item as to where we are today:

A mature science can be characterized by several key elements that distinguish it from emerging fields:

1. Established theoretical frameworks - Possesses widely accepted core theories or paradigms that explain fundamental phenomena in the field. YES

2. Predictive power - Theories generate accurate, testable predictions that can be verified experimentally. LIMITED

3. Methodological consensus - General agreement about appropriate research methods, standards of evidence, and what constitutes valid knowledge. YES

4. Accumulation of knowledge - Builds systematically on previous findings rather than continually revisiting foundational questions. YES

5. Specialization and integration - Development of specialized subfields that nonetheless remain integrated within the broader discipline. YES

6. Professional institutions - Established journals, academic departments, professional societies, and educational pathways. YES

7. Practical applications - Produces reliable knowledge that can be applied to solve real-world problems. YES

8. Self-correction mechanisms - Errors are identified and corrected via evidence, peer scrutiny, and replication, refining knowledge over time (e.g., the shift from Newtonian to Einsteinian physics after anomalies in Mercury's orbit). YES

I would score us at 7.5 (out of 8).

Appendix

Do you think that, focusing on economic fluctuations rather than growth, we (the profession) have largely converged (in terms of methodology, and main mechanisms)?

In a very broad sense, there is widespread agreement that ”frictions” are important, and some of those frictions (labor, goods, financial, ...) make fluctuations, especially recessions, an unwelcome phenomenon. Long gone are the days when the first welfare theorem would apply to a model of fluctuations.

In terms of methodology, we are virtually at the steady state (modulo the fact that some recent frontiers, like heterogeneous-agent models, have required the development of new methods to obtain the equilibrium that everyone agrees we should be solving). In terms of mechanisms, I think there are still debates, but it’s mostly about magnitudes rather than conceptually new channels.

We have converged in methodology, less so in mechanisms, and much convergence is an unfortunate stale echo chamber. There is, in my mind, a healthy disagreement about methods and mechanisms in our field at the moment. The empirical evidence that exists in our field is limited and does not support strong convergence (of the kind that exists in physics, for example).

The New Keynesian model has very much become the benchmark paradigm when thinking about macroeconomic fluctuations and monetary policy. However, we seem more divided than ever as to the main causes of aggregate fluctuations. Is it productivity shocks? "Demand shocks" (and which ones—shocks to the discount rate? Government expenditures?) Is it a risk shock? Sudden capital depreciation? I am not even sure we will see much of the leading research within the "New Keynesian" paradigm in the future. All the low-hanging fruits have been harvested. What is next? I do not know. There is broad convergence in methodology. I do not think there is broad agreement about mechanisms, however, e.g., about which are the most important shocks.

I think there is much division on both causes and propagation mechanisms. I do not think there is much that is persuasively identified. In terms of very broad methodology, we have converged. It is all about general equilibrium and DSGE modeling now. In terms of the mechanisms, such as sources of frictions and whether heterogeneity plays a role, we are still debating.

Do you see nominal rigidities as an essential ingredient in explaining economic fluctuations?

I think nominal rigidities, combined with constraints on monetary policy, are the easiest way for us to discuss the role of aggregate demand in economic fluctuations. Since it may be the right way to think about it, I am comfortable using such a framework. However, I believe we need to remain open to other possibilities. In my mind, the key reason that aggregate demand is important in fluctuations is that real interest rates cannot adjust sufficiently rapidly to clear the goods market and support full employment. [...] Even if all prices were fully flexible, I doubt that real interest rates would adjust sufficiently to always clear the goods markets and maintain full employment.

Money is surely not superneutral, and nominal rigidities are key to understanding inflation dynamics. Just how much power they give central banks to boost output is less clear.

The essential ingredient, as I see it, is the existence of an aggregate demand channel, which requires what I would call "elastic markups," i.e., the willingness of firms and workers to accommodate changes in the demand for goods/labor with limited price/wage adjustment. Nominal rigidities are a natural way to achieve this: they are pervasive in the micro data and consistent with important features of the macro data (e.g., correlation between nominal and real interest rates, exchange rates, etc.).

Because nominal wage and price rigidities are important, I think we should explain better why money is used as a unit of account. It is easy to construct models that generate fluctuations in output and employment without nominal rigidities (and without cyclical fluctuations in exogenous variables). However, plausible models of cyclical recessions and overheating require at least nominal wage rigidity and possibly nominal price rigidities as well.

We don't know how to get aggregate demand or monetary policy to matter any other way, and both clearly do.

Nominal rigidities are a crutch and not a great assumption, but almost everyone appears to be using them.

We have not converged at all on the cost of inflation as perceived by households. The relative price distortions of the NK model seem to miss the first-order issue.

No, this is my biggest quarrel with the mainstream view.

My understanding of nominal rigidities here is "some reason why money is not neutral." I'm still skeptical that sticky prices are the main reason why this is the case.

If you think we have converged, or are converging, are we converging in the right direction (methodology, and main mechanisms)?

I think we are at a late 19th-century physics moment, complacent in a consensus methodology, ignoring its internal inconsistencies and its more and more tenuous description of elephants in the room, content to add big-box epicycles (HANK, that means you) of stupendous computational complexity but little practical import or connection to policy.

The most important development in macro over the past 20 years has been an increased emphasis on empirical work. Prior to that, macro was unbalanced in favor of theory for several decades (ever since the rational expectations revolution). With little empirical evidence, the field was less well anchored in reality, and methodological squabbles were therefore more intense. A greater emphasis on empirical work has slowly anchored methodological disputes.

I think that [work on the medium run] is where we are converging in macroeconomics, but it is not sufficiently recognized. Fifteen years after the GFC, we still feel its effects, and the GFC is hard to understand without referring to the Dot-Com boom and bust. COVID-19 will also likely have a long tail of effects. These are issues that last longer than traditional business cycle analysis but do not quite fit into growth theory either.

The methodology in macroeconomics has made great progress. In particular, the use of micro-level data in macro has brought many good insights. While I am a fan of using micro data for studying many macro issues, I wonder if the pendulum may be swinging too far. Sometimes I feel that we may be missing the big picture—the macro part—when overly focusing on micro data.

I do not think that the New Keynesian consensus interpretation of technology shocks and nominal rigidities is empirically successful or theoretically compelling. The foundations of the consumption Euler equation have no empirical support. Micro evidence on both the timing and frequency of price changes and their implications for quantities is inconsistent with sticky price models. There is no plausible detailed mapping between observed events and the models' ubiquitous technology shocks.

I think there has been more convergence than warranted given what we (do not) know. To give just one—at this point, I think quite widely acknowledged—example: an excessive focus on linear methods in empirics, theory, and even "quantitative" structural work. A lot of pressing questions lie outside the purview of linear methods, and I think the profession is somewhat ill-equipped to tackle them.

Even though there is convergence, the cost is total ignorance of medium run. Anything outside business cycle frequency, like 2-5 years but not long run growth (decades) is very important, however, not studied enough.

We are converging in the right direction, but there needs to be more applied, identified data work that tests each of the mechanisms we are converging on. Converging only based on theory is not the correct path to designing the right policies.

I do not think macroeconomists have converged. What is macro anyway? There are many different fields studying macro data, fields with vague boundaries between them. Their research connects with different aspects of the phenomena. We have seen over the last 40 years the growth of behavioral economics, socioeconomics, psychological economics, political economy, experimental economics, anthropological economics, neuroeconomics, and now cognitive economics. [...] There are even more such fields than I just listed. And they have tended to be housed on separate floors or in separate buildings on campus and mostly haven't communicated much with each other. [...] The emerging AI revolution may provide a way of integrating all these different approaches to macro.

I am not sure we have been well served by the very heavy emphasis on DSGE models. We have, to be sure, learned things about the properties of our models from these exercises, but too often people confuse that with learning things about the real world. DSGE models have proved to be more of a hindrance than a help when it comes to practical policymaking, where I still believe older-style semi-structural and econometric models have a useful role to play. Mechanisms: I am a bit more comfortable thinking we have most of the key mechanisms present in some form or other.

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