Ricardo Reis

Is something really wrong with macroeconomics?

Discussion paper

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I. Introduction

I accepted the invitation to write this essay and take part in this debate with great reluctance. The company is distinguished and the purpose is important. I expect the effort and arguments to be intellectually serious. At the same time, I call myself an economist and I have achieved a modest standing in this profession on account of (I hope) my ability to make some progress thinking about and studying the economy. I have no expertise in studying *economists*. I go to work every day to understand why inflation goes up and down or why some fiscal systems deliver better outcomes than others. Making progress on these questions frequently requires taking detours into narrow technical points on definitions of equilibrium or the properties of statistical estimators. But the focus always remains on understanding the economy, not the profession of economics. I personally love reading biographies and delight in thinking about what a young Alfred Marshall would say to a young Kenneth Arrow. Yet, I do not confuse these pleasurable intellectual leisure times with my job as a researcher.

On top of this, asking an active researcher in macroeconomics to consider what is wrong with macroeconomics today is sure to produce a biased answer. The answer is simple: *everything* is wrong with macroeconomics. Every hour of my workday is spent identifying where our knowledge falls short and how can I improve it. Researchers are experts at identifying the flaws in our current knowledge and in proposing ways to fix these. That is what research *is.* So, whenever you ask me what is wrong with any part of economics, I am trained by years on the job to tell you many ways in which it is wrong. With some luck, I may even point you to a paper that I wrote proposing a way to fix one of the problems.

While preparing for this article, I read many of the recent essays on macroeconomics and its future. I agree with much of what is in them, and benefit from having other people reflect about economists and the progress in the field. But to join a debate on what is wrong with economics by adding what is wronger with economics is not terribly useful. In turn, it would have been easy to share my thoughts on how macroeconomic research should change, which is, unsurprisingly, in the direction of my own research. I could have insisted that macroeconomics has over-relied on rational expectations even though there are at least a couple of well-developed, tractable, and disciplined alternatives. I could have pleaded for research on fiscal policy to move away from the over-study of what was the spending of the past (purchases) and to focus instead on the spending that actually dominates the government budget today (transfers). Going more methodological, I could have elaborated on my decade-long frustration dealing with editors and journals that insist that one needs a model to look at data, which is only true in a redundant and meaningless way and leads to the dismissal of too many interesting statistics while wasting time on irrelevant theories.[2] However, while easy, this would not lead to a proper *debate.* A problem that too often plagues these discussions is that each panelist takes turns stating something else that is wrong with economics and pushing in a different direction. By the end, no opposing views are voiced, and the audience feels safe to agree with everything that was said while changing nothing in its day-to-day work, because there seem to be too many alternatives.

With all these caveats in mind, this essay will instead provide a critical evaluation of the state of macroeconomics. I will discuss four uses of macroeconomics, from those that are, in my view, less wrong, to those that perhaps need more change: research, policy, forecasting, and teaching. To contribute to the debate, I focus on responding to some of the negative verdicts on what is wrong with macroeconomics. The goal is to prevent these criticisms from being read as undisputed facts by the users of knowledge as opposed to the creators of knowledge. In substantive debates about actual economic policies, it is frustrating to have good economic thinking on macro topics being dismissed with a four-letter insult: it is a DSGE. It is worrying to see the practice of rigorously stating logic in precise mathematical terms described as a flaw instead of a virtue. It is perplexing to read arguments being boxed into macroeconomic theory (bad) as opposed to microeconomic empirical work (good), as if there was such a strong distinction. It is dangerous to see public grant awards become strictly tied to some methodological directions to deal with the crisis in macroeconomics. I am not, in any way, claiming that there are no problems in macroeconomics, or that there should be no changes. My goal is not to claim that there is no disease, but rather to evaluate existing diagnoses, so that changes and progress are made in a productive direction.

II. The present of macroeconomic research

Mortality imposes that the future of macroeconomics will be shaped by the youngest members of the profession. There is something wrong with a field when bright young minds no longer find its questions interesting, or just reproduce the thoughts of close-minded older members. There is something right with it when the graduate students don't miss the weekly seminar for work in progress, but are oblivious of the popular books in economics that newspapers and blogs debate furiously and tout as revolutionizing the field. To evaluate the state of macroeconomic *research*, as opposed to policy or the history of ideas, one should confront evaluations with evidence on what active researchers in the field are working on. Nobel prizes get most of the attention, and speeches of central bankers about their internal models are part of policy debates. But neither are the right place to look for the direction of the field. More accurate measures of the state of macroeconomics are what the journals have recently published, or what the recent hires of top departments are working on.

A good place to start is to read what some representative young macroeconomists actually work on. The *Review of Economic Studies* foreign editors select around 6 economists every year that were just on the academic job market to give a tour of a handful of European institutions and present their research. These are not necessarily the best economists, or the ones that had more job offers, but they are typically the candidates that the editors are more excited about and that got more attention in the job market. Because the composition of the jury that picks them is heterogeneous and changes regularly, the choices are arguably not biased in the direction of a particular field, although they are most likely all in the mainstream tradition.[3] Looking at their work gives a sample of what macroeconomic research is today. While they are at the top of the distribution when it comes to quality, these dissertation theses are fairly representative of what modern research in macroeconomics looks like. Here is my short description of what that is for the last 8 macroeconomists (with graduation date, PhD school, and first job in parentheses):

Martin Beraja (2016, Chicago, MIT): Beraja's job market paper developed a new method to identify the effectiveness of policies within models where the researcher is uncertain about some features of the economy that the data has a hard time distinguishing. His focus is on identification in DSGE models that assume incomplete financial markets and sticky wages and this comes with clear applications to questions of redistribution via fiscal policy across states.

Arlene Wong (2016, Northwestern, Princeton): Wong used micro data to show that it is mostly young people who adjust their consumption when monetary policy changes interest rates. Younger people are more likely to obtain a new mortgage once interest rate changes, either to buy a new home or to refinance an old one, and to spend new available funds. Her research has painstaking empirical work that focuses on the role of mortgages and their refinancing features, and a model with much heterogeneity across households.

Adrien Auclert (2015, MIT, Stanford): Auclert also focused on how changes in monetary policy affect spending and the macroeconomy, and also emphasized the heterogeneous responses by different households. He argued that when central banks lower interest rates, households whose assets have shorter duration than their liabilities lose out to households whose assets are of longer maturity than their liabilities. He then found that in the data the winners from these cuts in interest rates have higher propensity to spend than the losers, so that cuts in interest rates will boost aggregate spending.

Gregor Jarosch (2015, Chicago, Stanford): Jarosch writes a model to explain why losing your job leads to a very long-lasting decline in your lifetime wages. His hypothesis is that this is due to people climbing a ladder of jobs that are increasingly secure, so that when one has the misfortune of losing a job, this leads to a fall down the ladder and a higher likelihood of having further spells of unemployment in the future. He uses administrative social security data to find some evidence for this hypothesis.

Luigi Bocola (2014, Penn, Northwestern): Bocola tries to explain the depth of the crisis in Italy after 2011. He writes a DSGE model where banks hold sovereign debt, so that bad news about a possible future sovereign default both puts a strain on the funding of banks but also induces them to cut their leverage as a precautionary reaction. This channel for the diabolic loop linking banks and sovereign debt fits reasonably well the behavior of credit spreads across Italian banks and firms, and predicts that the ECB’s interventions had a small effect.

Saki Bigio (2012, NYU, Columbia): Bigio wanted to understand why banks don't recapitalize fast enough after suffering large losses during a financial crisis, and this seems to be related with the slump in lending and real activity that follows these crises. His explanation is that after large losses, banks are less able to tolerate further losses, which lowers their ability to intermediate, and so their future profits. Equity holders can then be stuck in a coordination failure, where no one wants to inject new equity unless others do so as well, banks are stuck in a low profit equilibrium, and the recovery must come through the slow process of retaining earnings by banks.

Matteo Maggiori (2012, Berkeley, NYU): Maggiori postulates that countries with more developed financial markets are able to better deal with lack of funding in a financial crisis. They use this ability to sell insurance to less developed countries, so that in normal times they receive an insurance premium in the form of capital gains on foreign investments that sustain persistent trade deficits. During a crisis though, the advanced countries should suffer the heaviest of capital losses and a larger fall in consumption, a prediction consistent with what happened in the United States, but less so with what happened in Germany during the Euro crisis.

Joe Vavra (2012, Yale, Chicago): Vavra used data on individual prices to find that changes in prices tend to be more dispersed and more frequent in recessions. He explains this by firms adjusting more often their prices in recessions, in spite of the costs of doing so, because the volatility of their firm-specific productivity is higher. But, with this more frequent price adjustment, monetary policy shocks will be less effective at boosting real activity in recessions.

In my reading, this is all exciting work, connected to relevant applied questions, and that takes data and models seriously. In contrast, in the caricatures of the state of macroeconomics, there are only models with representative agents, perfect foresight, no role or care for inequality, and a cavalier disregard for financial markets, mortgage contracts, housing, or banks. Supposedly, macroeconomic research ignores identification and does not take advantage of plentiful microeconomic data to test its models, which anyway are too divorced from reality to be useful for any real world question. Compare this caricature with the research that I just described: the contrast is striking. Not a single one of these bright young minds that are the future of macroeconomics writes the papers that the critics claim are what all of macroeconomic research is like today. Instead, what they actually do is to mix theory and evidence, time-series aggregate data and micro data, methodological innovations and applied policy questions, with no clear patterns of ideology driven by geography.

Blanchard (2016), Korinek (2015) and Wren-Lewis (2017) worry that the current standards and editorial criteria in macroeconomics undermine promising ideas, deter needed diversity in the topics covered, and impose mindless work on DSGEs that brings little useful knowledge to policy discussions. Smith (2016) emphasizes that we have far less data than what we would need to adequately test our models, and Romer (2016) that identification is the perennial challenge for social sciences. Smith (2014) and Coyle and Haldane (2014) characterize the state of economics, not as the perennial glass half full and half empty, but rather as two glasses, one full and the other empty. In their view, applied empirical economists have been celebrating their successes, while macroeconomists lament their losses.

All of these criticisms contain some truth, but only up to a point. The research that I have just described is diverse, creative, and uses different data to identify causes. Young researchers in macroeconomics today do not seem bound by current standards or afraid to get their hands dirty. They are attacking these big challenges and trying to overcome the criticisms. The data and tools used by applied empirical economists are also used by macroeconomists. This is a sign of a field full of vitality, not of a field in trouble.

One might make the (elitist) criticism that, by focusing on these papers, I have looked only at the disruptive work that may cause scientific revolutions, while the problem is on what goes on in normal macroeconomic science. Table 3 reports the articles published in the latest issue of the top journal in macroeconomics, the *Journal of Monetary Economics*, including their authors, the title of the paper, and the highlights that the authors submitted. These include: theoretical papers on sovereign debt crises and capital controls, applied papers on the interrelation between financial indicators and macroeconomic aggregates, papers looking at extreme events like catastrophes and liquidity traps, and even purely empirical papers on measuring uncertainty in micro data and on forecasting time series in the macro data. There is originality and plurality, and a significant distance from the critics' portrayal of research.

Table 1. Articles in the *Journal of Monetary Economics,* volume 84, December 2016

Authors	Title	Highlights
Gilles Chemla, Christopher A. Hennessy	Government as borrower of first resort	• A privately informed firm issues debt to a speculator and investors in safe assets. • With high uninformed safe asset demand, the private sector may pool at risky debt. • The government can increase welfare by issuing safe bonds, crowding out risky debt. • Government may eliminate risky debt and portfolio distortions, reducing investment. • Government debt can accommodate risky debt and distortions, encouraging investment.
David S. Miller	Commitment versus discretion in a political economy model of fiscal and monetary policy interaction	• Microfounding fiscal policy affects monetary policy decisions. • Time inconsistency is alleviated by the politically distorted fiscal authority. • Monetary responses mitigate the political distortion′s effect. • Price commitment results in lower welfare as it eliminates monetary responses.
Vasco Cúrdia, Michael Woodford	Credit Frictions and Optimal Monetary Policy	• A positive average spread has little quantitative effect in the transmission of shocks. • Time variation in credit spread affects the relation between spending and policy rate. • Time variation in credit spread affects the relation between inflation and real activity. • Basic NK optimal target criterion is approximately optimal with credit spread. • The target criterion can be implemented by an augmented forward-looking Taylor rule.
Christian Gollier	Evaluation of long-dated assets: The role of parameter uncertainty	• The parametric uncertainty affecting the annual growth rate magnifies long run risks. • It makes the term structure of interest rates decreasing, because of prudence. • It makes the term structure of risk premia increasing, because of risk aversion. • The uncertain trend or volatility of growth has a strong impact on asset prices. • The uncertain frequency of catastrophes plays a similar role.
Daniel Shoag, Stan Veuger	Uncertainty and the geography of the great recession	• Local policy uncertainty during the Great Recession matches unemployment outcomes. • This relationship is robust to numerous controls. •Increased uncertainty contributed to the severity of the Great Recession.
Zhu Wang, Alexander L. Wolman	Payment choice and currency use: Insights from two billion retail transactions	• Rich transactions data covering payment patterns for 3 years, thousands of stores. • Consistent with theory of consumers′ threshold transaction size for cash use. • Across transaction size, cash share falls and dispersion across locations rises. • Cash share displays weekly and monthly cycles, correlated with transaction volume. • Over the longer term, cash share has declined, largely replaced by debit.
Andrea L. Eisfeldt, Tyler Muir	Aggregate external financing and savings waves	• Provide external finance cost time series using firm financing and savings decisions. • Estimated average cost of external finance is 2.3%. • Provide evidence of external finance cost shocks. • Formally reject nested model without external finance cost shocks. • Document external finance and savings waves.
Adrien Auclert, Matthew Rognlie	Unique equilibrium in the Eaton–Gersovitz model of sovereign debt	• The Eaton–Gersovitz model is widely used for empirical analyses of sovereign debt markets. • We show that the model with exogenous default value and short-term debt admits a unique equilibrium. • This counters the common view that sovereign debt markets are prone to multiple equilibria. • Multiplicity requires altering the timing of the model, or considering long-term debt.
Gianluca Benigno, Huigang Chen, Christopher Otrok, Alessandro Rebucci, Eric R. Young	Optimal capital controls and real exchange rate policies: A pecuniary externality perspective	• A new literature studies the use of capital controls to prevent financial crises. • We show that if exchange rate policy has no cost, there is no need for capital controls. • If the exchange rate policy is costly, capital controls become part of the optimal policy mix. • This mix combines capital controls in tranquil times with exchange rate policy in crisis times. • It yields more borrowing, fewer and less sever crises, and higher welfare than capital controls alone.

Marco Cozzi, Giulio Fella	Job displacement risk and severance pay	• We study the insurance role of severance pay in the presence of displacement risk. • Post-displacement earnings losses are sizeable and persistent due to loss of tenure. • Asset markets are incomplete. • We find that severance pay entails substantial welfare gains. • These welfare gains are negligible if earnings losses are not persistent.
Michael Abrahams, Tobias Adrian, Richard K. Crump, Emanuel Moench, Rui Yu	Decomposing real and nominal yield curves	• A term structure model for nominal and inflation-indexed government bonds. • Model is used to decompose yields into expectations and risk premia. • Variations in nominal term premia are primarily due to movements in real term premia. • LSAP announcements lowered yields mainly through a reduction of real term premia. • Monetary policy surprises primarily affect real forwards through real term premia.
Domenico Giannone, Francesca Monti, Lucrezia Reichlin	Exploiting the monthly data flow in structural forecasting	• A framework for combining structural models and now-casting is proposed. • Conditions for deriving the monthly dynamics of the model are discussed. • Linking the model with auxiliary variables improves now-casting performance. • The proposed model traces in real time the shocks driving the business cycle.
Lena Mareen Boneva, R. Anton Braun, Yuichiro Waki	Some unpleasant properties of loglinearized solutions when the nominal rate is zero	• We show that it matters how one solves the New Keynesian model at the zero lower bound (ZLB). • The nonlinear solution exhibits new types of ZLB equilibria that cannot occur using a loglinearized solution. • Fiscal multipliers are small and orthodox at the ZLB for a large and plausible set of parameterizations of the model. • The New Keynesian model can be used to make a case for supply-side fiscal stimulus at the ZLB. • In situations where a labor tax rate cut increases employment, the government purchase multiplier is about one or less.
Yang K. Lu, Robert G. King, Ernesto Pasten	Optimal reputation building in the New Keynesian model	• We study how reputation building affects the optimal committed policy. • The reputation building effect can overturn the conventional policy prescriptions. • The reputation building effect is quantitatively important. • The reputation building effect is relevant over a large parameter space.

Yet, according to De Grauwe (2009) “The science of macroeconomics is in deep trouble.” while Skidelsky (2009) thinks that there has already been a “…discrediting of mainstream macroeconomics”. These opinions express feelings more than facts, so it is hard to debate them. But if the collapse in the reputation of macroeconomists was as large as they claim, there should be hints of it at least in some rough measures of academic output and prestige. Space in the top journals in the economics profession is scarce. If macroeconomics was in a crisis, journals would, at least slowly, publish fewer and fewer articles on macroeconomics. From the demand side, general interest journals would not be interested in publishing articles that non-macroeconomists have no interest in reading. From the supply side, enough articles in a field must be written for a select few to be of enough quality to pass the difficult standards of these top journals.

Card and Della Vigna (2013) split the papers published in the top general-interest journals in the profession according to their field. They find no discernible change in the share of articles on macroeconomics over the last four decades. Figure 1 uses their approach, with some slight changes, by plotting the share of articles on macroeconomics, identified by a JEL code of E, that were published in the official journals of the two largest regional associations in economics, the American Economic Association and the European Economic Association. The sample goes from the start of 2000 to the end of 2016, so there are roughly as many years after the start of the Great Recession, as there are before. Publication in the two journals follow the same trend: if anything, the share of papers in macroeconomics has been increasing over time. Figure 1 plots also the share of working papers published by the National Bureau of Economic Research on macroeconomic topics to account for possible lags in the decline in macroeconomics due to publication delays. While there was a temporary decline in the share of macroeconomic papers right after 2008, for the past 5 years it has been steadily rising, and is now at the highest level of the past 12 years.

A related criticism of macroeconomics is that it ignores financial factors. Macroeconomists supposedly failed to anticipate the crisis because they were enamored by models where financial markets and institutions were absent, as all financing was assumed to be efficient (De Grawe, 2009, Skidelsky, 2009). The field would be in denial if it continued to ignore these macro-financial links. Figure 2 checks this hypothesis in the article database measuring the share of papers in the journals that have both the E and the F JEL fields, so they contain research at the intersection of both macroeconomics and finance. The figure shows that research in macro-finance has increased continuously over the sample. The share of macro-finance papers more than doubled for both the AER and the NBER from pre to post crisis, but was already on the rise since 2000. Of the increase in the macro share on average between 2000-07 and 2009-16, which was 3.7%, 2.0%, and 5.1% for the AER, JEEA and NBER respectively, a very large part of it is accounted by macro-finance papers, which increased by 4.3%, 1.3%, and 3.9%, respectively. Almost half of all macroeconomic papers in the AER in 2012 were also listed as finance papers. A more anecdotal piece of evidence comes from the 2012 survey by Brunnermeier, Eisenbach and Sannikov (2013) on macroeconomics with financial frictions. It runs for 93 pages, it cites 177 references, most written before the crisis, and it references 6 other books and surveys that the authors state that one must read to get a full picture of the research on the intersection between macroeconomics and financial factors. One can safely argue that there is a hole in our knowledge of macro financial interactions; one might also argue more controversially that economists have filled this hole with rocks as opposed to diamonds; but it is harder to argue that the hole is empty.

Figure 1. Share of macro papers published in the AER, JEEA and NBER

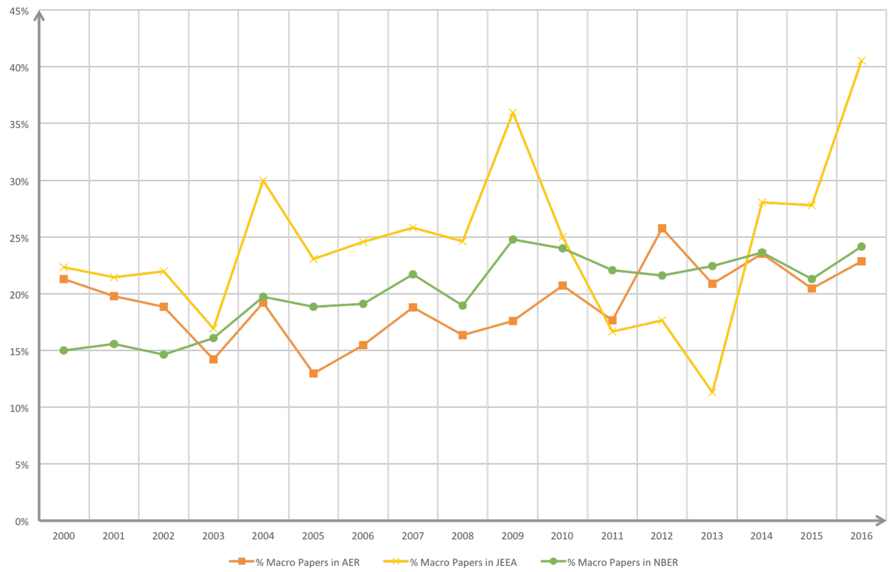
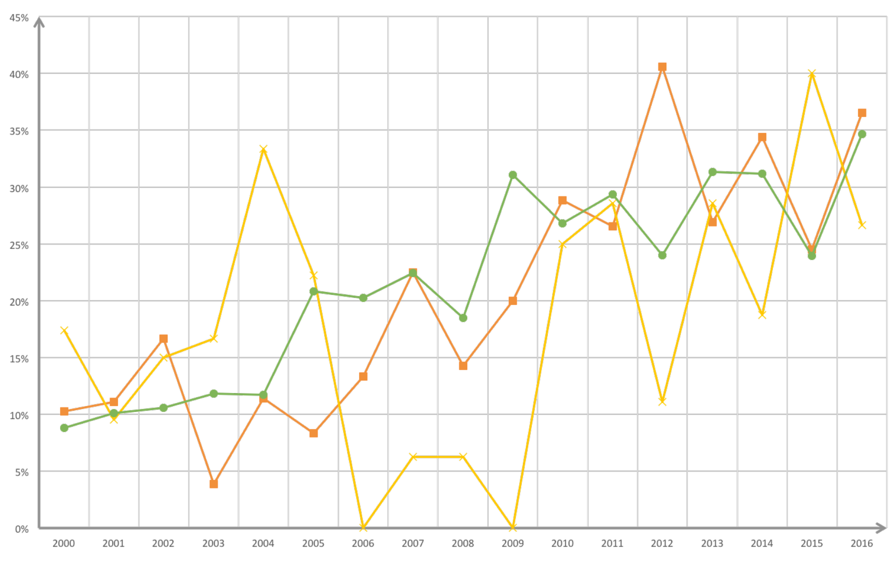
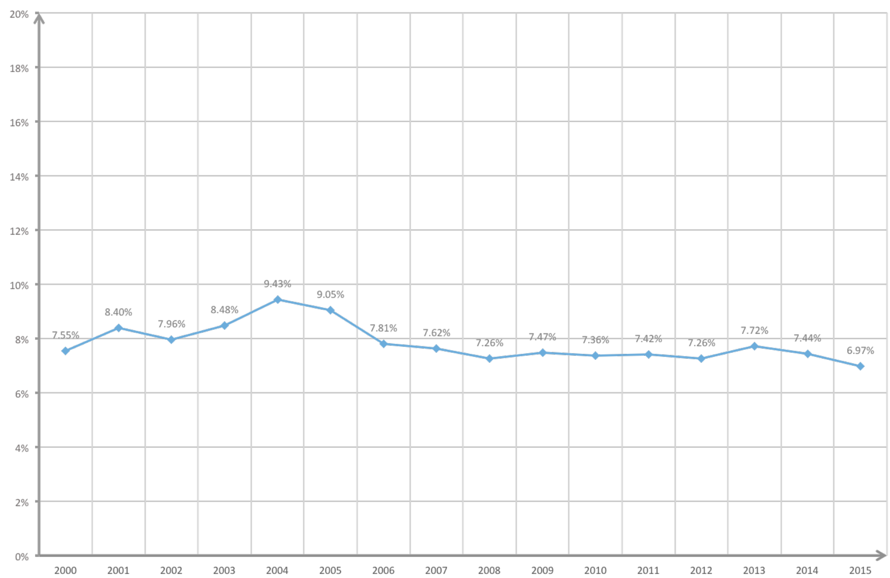


Figure 2. Share of macro-finance papers published in the AER, JEEA and NBER



Finally, on the demand side, macroeconomics can only have a future if there are still academic jobs for the young macroeconomists. Figure 3 shows the share of job posting in Jobs Openings for Economists, the main board for job advertisements for freshly minted PhDs, that again list macroeconomics as identified by its JEL code as the desired hire. The share is remarkably constant over the past 15 years. At least for now, the marketplace seems to continue to appreciate what macroeconomists do.

Figure 3. Share of macro listings in Job Market Openings



Surely, when looking back in the future, some current directions of research will have turned out to have been unproductive or even misguided. Journals have many flaws, and editors and referees are naturally biased towards propagating old paradigms, and to stick out for their turfs. But my reading of the evidence is that macroeconomic research is not on the path to self-destruction implied by its critics. Looking at the current research frontier led to a different description from the one that one gets from the critics, and one that is at odds with the pessimistic tone of their criticisms.

III. The performance of macroeconomic policy

Among all fields of economics, macroeconomics seems to be the one that attracts the most attention by the popular media. At the same time, macroeconomists are very far from running the world. In deciding the size of the budget deficit, or whether a fiscal stimulus or austerity package is adopted, macroeconomists will often be heard by the press or policymakers, but almost never play a decisive role in any of the decisions that are made. Most macroeconomists support countercyclical fiscal policy, where public deficits rise in recessions, both in order to smooth tax rates over time and to provide some stimulus to aggregate demand. Looking at fiscal policy across the OECD countries over the last 30 years, it is hard to see too much of this advice being taken. Rather, policy is best described as deficits almost all the time, which does not match normative macroeconomics. Moreover, in popular decisions, like the vote in the United Kingdom to leave the European Union, macroeconomic considerations seemed to play a very small role in the choices of voters.[4] Critics that blame the underperformance of the economy on economists vastly overstate the influence that economists actually have on economic policy.

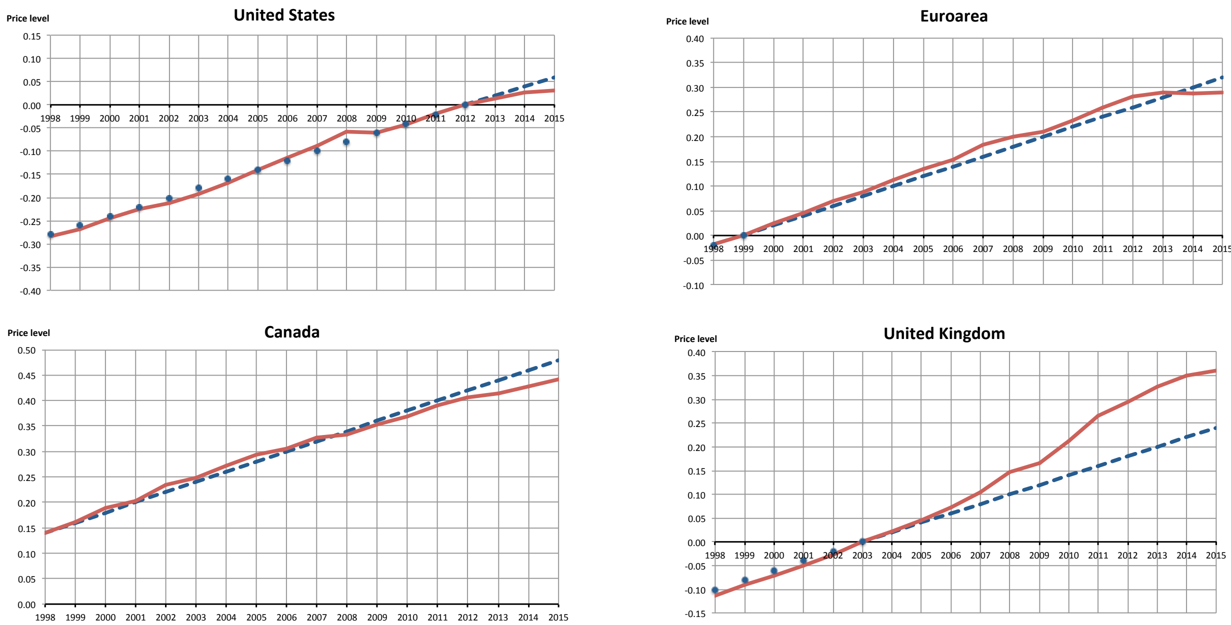
One area where macroeconomists have perhaps more of an influence is in monetary policy. Central banks hire more PhD economists than any other policy institution, and in the United States, the current and past chair of the Federal Reserve are distinguished academic macroeconomists, as have been several members of the FOMC over the years. In any given week, there are at least one conference and dozens of seminars hosted at central banks all over the world where the latest academic research is discussed. The speeches of central bank governors refer to academic papers in macroeconomics more than those by any other policymaker.

Looking at the major changes in the monetary policy landscape of the last few decades —central bank independence, inflation targeting, financial stability—they all followed long academic literatures. Even individual policies, like increasing transparency, the saturation of the market for reserves, forward guidance, and balance-sheet policy were adopted following academic arguments and debates. In the small sub-field of monetary economics, one can at least partially assess its successes and failures in the real world by judging how central banks have done over the past few decades.

Every central bank that I know of in the developed world is in charge of keeping inflation low and stable. Some central banks have this as their only goal, others as one of several, but there is strong agreement across societies as reflected in central bank mandates that central banks can control inflation in the long run and keeping it stable is their main task. Figure 4, reproduced and updated from Reis (2016), compares the performance of four major central banks with regards to the measure of the price level that is stated in their legal mandates. In red is the actual outcome, in dashed blue is the target moving forward since a 2% target was officially adopted, and in dotted blue is a hypothetical target from extrapolating the 2% backwards in time. The hypothetical is important for the United States, since it had long been noted that the Federal Reserve behaved as if it had a target of 2% even before this was decided. Comparing actual and expected, the conclusion for the United States, the Eurozone, and Canada is clear: monetary policy has been remarkably successful. For the United Kingdom, the price level drifted upwards after the crisis, although in its defense, the Bank of England interpreted its mandate as stating that bygones are bygones when it comes to past deviations, so that since 2011, the slope of the price level has been approximately on target.

Another way to judge the performance of macroeconomics as applied to central banking is through the response to the crises of the last decade. Macroeconomists did not prevent the crises, but following the collapse of Lehman or the Greek default, news reports were dominated by non-economists claiming that capitalism was about to end and all that we knew was no longer valid, while economists used their analytical tools to make sense of events and suggest policies. In the United States in 2007-08, the Federal Reserve, led by the certified academic macroeconomist Ben Bernanke, acted swiftly and decisively. In terms of its conventional instruments, the Federal Reserve cut interest rates as far as it could and announced it would keep them low for a very long time. Moreover, it saturated the market for reserves by paying interest on reserves, and it expanded its balance sheet in order to affect interest rates at many horizons. Finally, it adopted a series of unconventional policies, intervening in financial markets to prevent shortages of liquidity. Some of these decisions are more controversial than others, and some were more grounded in macroeconomic research than others. But overall, facing an adverse shock that seems to have been as serious as the one behind the Great Depression, monetary policy responded, and the economy recovered. While the recession was deep, it was nowhere as devastating as a depression. The economic profession had spent decades studying the Great Depression, and documenting the policy mistakes that contributed to its severity; these mistakes were all avoided in 2008-10.[5]

Figure 4. Actual price level and targets in four major central banks



Notes: The target price level is in the dashed blue line from the date of the announcement of the target forward, the hypothetical target is the extension of the target backwards in time, and the actual price level is in the solid red line. All are normalized to equal zero at the date of adoption of the target, except for Canada that is normalized to zero in 1998. For the United States, the inflation target was adopted in January of 2012 using the personal consumption expenditures deflator as the reference measure. For the Euroarea, the target was adopted in January of 1999 for the harmonized consumer price index. For Canada, the target for the total consumer price index was adopted in 1991. For the Bank of England, the current target for the consumer price index target was adopted in December of 2003. The target for all four is a 2% annual growth in the price level. The vertical axis is in a log scale.

Turning to the Eurozone crisis, many agree that the intervention of the ECB in defending the euro "whatever it takes", in Mario Draghi's famous words, was decisive to prevent a collapse of European sovereign debt markets. In turn, while other European and national authorities had difficulty agreeing on a response to the crisis, the ECB intervened quickly and decisively, and the supply of credit stayed up, even in the periphery countries with banking problems. Again, most of the interventions, both in stopping the sovereign debt crisis, and in using longer-term liquidity interventions, were justified and based on academic papers in macroeconomics. Without taking credit away from the policymakers who had the courage to implement these policies, like the practical men in Keynes famous quote, they were following the principles of macroeconomists.[6]

A separate criticism of macroeconomic policy advice accuses it of being politically biased. Since the early days of the field, with Keynes and the Great Depression, macroeconomics was associated with aggressive and controversial policies and with researchers that wore other hats as public intellectuals. Even more recently, during the rational-expectations microfoundations revolution of the 1970s, early papers had radical policy recommendations, like the result that all systematic aggregate-demand policy is ineffective, and some leading researchers had strong political views. Romer (2016) criticizes modern macroeconomics for raising questions about what should be obvious truths, like the effect of monetary policy on output. He lays blame on the influence that Edward Prescott, Robert Lucas and Thomas Sargent had on field. Krugman (2009) in turn, claims the problem of macroeconomics is ideology, and in particular points to the fierce battles between different types of macroeconomists in the 1970s and 1980s, described by Hall (1976) in terms of saltwater versus freshwater camps.

These features of the history of macroeconomics should be pointed out and discussed. But if they are crucial for diagnosing the state of the field, then they should stand out as very different from what happens in other fields in economics. Yet, labor economics also has a history of heated debates and strong ideological priors, as well as continuous re-examination of truths previously held as obvious, such as the effects of the minimum wage on employment or of immigration on wages.[7] The father figures of modern public economics, like Anthony Atkinson, Joseph Stiglitz or Martin Feldstein, have also actively participated in popular debates with strong views in their role as public intellectuals. Researchers in both fields make frequently policy prescriptions, and their work is picked up by the media and publicly promoted by the profession more than that of macroeconomists: of the last ten John Bates Clark medallists, a prize given by the American Economic Association to honor economists under the age of 40, five of them have gone to researchers who list labor or public economics as one of their main fields of research.[8] Macroeconomics does not stand out from labor and public economics in the features that the critics point out as the source of its crisis.

The point is not claim there are weaknesses in different fields of economics. The point is rather to note that macroeconomics is not all that special relative to the other fields. Economists across all fields were in part surprised by the crisis, but also eager to study it and analyze it. Economic theorists understood that we needed to invest more time on characterizing the role of speculation and sudden shifts in equilibrium, industrial organization economists turned their attention to auctions ran by central banks and to the operation of payment systems, and financial economists realized how little we had paid attention to understand rare events or to the measurement of systemic risk. There have been important debates on methods in development economics and in labor economics.[9] Researchers in these fields, as in macroeconomics, perpetually feel unsatisfied with the state of their knowledge and work every day to improve it. Data has expanded and progress was made, but this is true both in microeconomics and macroeconomics.

To conclude, some of the diagnoses of the crisis in macroeconomics presuppose that macroeconomics is very different from the rest of economics, in having an outsized influence on policy, having more ideological researchers, or being especially hit in its credibility and methods by the crisis. This section noted that this specialness of macroeconomics is more apparent than real. As such, explanations for the problems of macroeconomics today that are too field specific may miss the target.

IV. Poor forecasting yes, but relative to what?

One way that macroeconomics stands out from other fields in economics is in how often it produces forecasts. The vast majority of empirical models in economics can be very successful at identifying causal relations or at fitting explaining behavior, but they are never used to provide unconditional forecasts, nor do people expect them to. Macroeconomists, instead, are asked to routinely produce forecasts to guide fiscal and monetary policy, and are perhaps too eager to comply. As I wrote in Reis (2010) "…by setting themselves the goal of unconditional forecasting of aggregate variables, macroeconomists are setting such a high bar that they are almost sure to fail."

Forecasting is hard. Forecasting what people will do when their behavior is affected by many interrelated personal, local, and national variables is even harder. Forecasting when the forecasts cause changes in policy, which make people change their choices, which in turn make it required to revise the forecasts, is iteratively hard. Forecasting when economic agents themselves are forecasting your forecast to anticipate the policies that will be adopted involves strategic thinking and game theory that goes well beyond the standard statistical toolbox. Very few economists that I know of would defend themselves too vigorously against the frequent criticisms of forecasting failures by economists. As is regularly shown, macroeconomic forecasts come with large and often serially correlated errors.[10]

At the same time, the way that forecasts are mis-read and mis-interpreted is part of the problem. As much as economists state that their forecasts are probabilities, and come with confidence bands, they are reported in the media always as point estimates. The Bank of England struggled to introduce fan charts as a way to display the uncertainty in its policy forecasts. Moreover, the supposedly most embarrassing forecast errors come with regards to large crises. Yet, these crises are rare events that happen once every many decades. Since typical economic time series only extend over a little more than one hundred years, statistically forecasting the eruption of a crisis will always come with large imprecision.[11]

Compare how economics does relative to the medical sciences. Analogies across sciences are always very tricky, and must be taken with a large grain of salt. Moreover, surely economists are still far from being as useful as dentists, like Keynes dreamed of, let alone to have made a contribution to human welfare that is close to the one by doctors or biologists. The comparison to make is much more narrow and limited, restricted only to how economic forecasts compare to medical forecasts.

Imagine going to your doctor and asking her to forecast whether you will be alive 2 years from now. That would sound like a preposterous request to the physician, but perhaps having some actuarial mortality tables in her head, she would tell you the probability of death for someone of your age. For all but the older readers of this article, this will be well below 50%. Yet, one year later, you have a heart attack and die. Should there be outrage at the state of medicine for missing the forecast, with such deadly consequences?

One defense by the medical profession would be to say that their job is not to predict time of death. They are driven to understand what causes diseases, how to prevent them, how to treat them, and altogether how to lower the chances of mortality while trading this off against life quality and satisfaction. Shocks are by definition unexpected, they cannot be predicted. In fact, in practice, most doctors would refuse to answer the question in the first place, or they would shield any forecast with a blank statement that anything can happen. This argument applies, word for word, to economics once the word disease is replaced by the words financial crisis.

A more sophisticated defense would note that medical sciences are about making conditional forecasts: if you make some lifestyle choices, then your odds of dying change by this or that much. These forecasts are at best probabilistic. Medical science can quantify in terms of conditional probabilities how certain behaviors affect mortality. Moreover, once the disease sets in, health researchers have given us the tools to understand what just happened to your body, rationalize it, and predict which treatments have some chances of helping, with what side effects. These lead to better choices and to better treatments, and they are a major contribution of the biomedical sciences to knowledge and human welfare.

Economics is not so different, even in 2007-08. Within days or weeks of the failure of Bear Sterns or Lehman Brothers, economists provided diagnoses of the crisis, and central banks and finance ministries implemented aggressive measures to minimize the damage, all of which were heavily influenced by economic theory. Economic concepts like asymmetric information, bank runs, the role of liquidity, saturating the market for reserves, and forward guidance at the zero lower bound, all provided concrete interpretations of the crisis, suggestions for policies, and discussion of trade-offs. The economy did not die, and a Great Depression was avoided, in no small part due to the advances on economics over many decades.

Too many people all over the world are today being unexpectedly diagnosed with cancer, undergo enormously painful treatment, and recover to live for many more years. This is rightly hailed as a triumph of modern oncology, even if so much more remains to be done. After suffering the worst shock in many decades, the global economy's problems were diagnosed by economists, who designed policies to respond to them, and in the end we had a painful recession but no melt down. Some, somehow, conclude that economics is at fault.

At the same time, a doctor examining you in an emergency room can predict quite accurately how quickly the virus in your body will spread, and what the state of your health will be in 24 hours. Biologists and chemists can make remarkable sharp predictions on what will happen to your body after you take a certain medicine. Economists surely do not come even close to this. Perhaps, but the equivalent to these successes would be for me to crunch through the data on sales, customer characteristics, and others at the coffee shop downstairs, run many experiments varying the prices in the menu, and then use the economic model of a demand curve to predict what happens to coffee sales over the next week if we double the price. I conjecture that the economic forecast would be quite good. Macroeconomists are instead asked to predict what will happen to the changes in the CPI or GDP over the next 1-5 years. The comparison of forecast quality must be made *for the same time horizon* and for a *similar level of aggregation*. The fairer comparison would be to ask doctors to predict what will be the percentage change in the annual number of patients that eventually die after being admitted to an emergency room due to a stroke. For these similar units, my guess is that medical forecasts will look almost as bad as macroeconomic forecasts.

Currently, the major and almost single public funder for economic research in the United States is the National Science Foundation. Its 2015 budget for the whole of social, behavioral and economic sciences was $276 million. The part attributed to its social and economic sciences group was $98 million. The main public funder of health studies in the United States is the National Institute of Health, but there are many more including several substantial private funders. The NIH's budget for 2015 was $29 billion dollars. Its National Institute of Allergy and Infectious Diseases alone received $4.2 billion in funding. A very conservative estimate is that society invests at least 40 times more trying to study infectious diseases, including forecasting the next flu season or the next viral outbreak, than it does in economics. More likely, the ratio of public investment to science devoted to predicting and preventing the next disease is two or even three orders of magnitude larger than the budget of science dedicated to predicting and preventing economics crises. There is no simple way to compare the output per unit of funding across different fields, but relative to its meager funding, the performance of economics forecasting is perhaps not so bad.

A detour for another comparison may drive the point of this section in. There has been much progress in weather forecasting, such that predicting the weather over the next few days is done with less uncertainty than it was a decade ago. Forecasting the weather is an activity that takes as many or more resources as forecasting the economy, and that also affects a series of policy choices and economic decisions. Comparing macroeconomic forecasts to forecasts of average temperature or precipitation over the next 1-5 years, as opposed to over the next few days, it is far from clear that economics forecasting is doing so poorly.

To conclude with the most important message, yes, economics models do a poor job forecasting macroeconomic variables. This deserves to be exposed, discussed, and even sometimes ridiculed. Critics like Haldane (2016) are surely right, and the alternatives that they propose for improvement are definitely worth exploring. If nothing else, this may help the media and the public to start reporting and reading forecasts as probabilistic statements where the confidence bands or fan charts are as or more important than the point forecasts. But, before jumping to the conclusion that this is a damning critique of the state of macroeconomics, this section asked for an evaluation of forecasting performance in relative terms. Relative to other conditional predictions on the effectiveness of policies, relative to other forecasts for large diverse populations also made many years out, and relative to their accuracy per dollar of funding. From these perspectives, I am less convinced that economics forecasting is all that far behind other scientific fields.

IV. Redirecting the criticisms to teaching macroeconomics

If I replace “macroeconomic research” with “macroeconomics as taught in entryway classes” in the critiques of macroeconomics, they seem much more on point. The doubts raised in this essay were on the descriptions of the state of knowledge, as opposed to the way that macroeconomics is taught or used by policymakers. Like Rodrik (2015) in his overall defense of economics, the validity of the criticisms and the scope for reform seem much clearer in regard to how macroeconomics is taught as opposed to how it is researched. The popularity of criticisms of macroeconomics likely has less to do with research, which most people know and care little about, but rather with their exposure to macroeconomics in the way it is taught and used in policy discussions.

At the undergraduate level, I see a productive debate taking place. The leading textbook in intermediate macroeconomics, *Macroeconomics* by N. G. Mankiw, is regularly revised, and many chapters changed significantly in the last decade to address the issues raised by the crisis. In the fringes, there are new entrants to this market and healthy competition of ideas and approaches, including exciting radical changes such as the one in the core-econ.org project. Macroeconomics is not alone here, as similar debates take place for instance in econometrics.[12]

At the graduate level, there is more room for improvement. Teaching is still tied to a benchmark neoclassical framework in masters class in macroeconomics, or in the core PhD sequence, and this deserves to be questioned. Researchers in modern macroeconomics have made much progress in the last three decades to provide alternatives to the assumptions of: (i) infinite lives, (ii) time-separable preferences over a single good, (iii) exponential discounting, (iv) rational expectations, (v) full risk-sharing, (vi) competitive firms, (vii) flexible prices, (viii) efficient financial markets, (ix) lump-sum taxes, or (x) no special role for money or the central bank, to name ten main ones. For each of these ten assumptions, there are separate tractable, simple, analytical models, that could be taught in an introductory class. The challenge is to bring these together in a bare-bones model that can provide a new benchmark.

This has not been done yet, but even if it takes some effort to do so, it does not seem infeasible. For instance, one could teach a macroeconomics class where the baseline model has (i) finite lives with overlapping generations, (ii) preferences over non-durables and housing, (iii) naive hyperbolic discounting, (iv) sticky information in forming expectations, (v) incomplete markets for individual income risk with maximally tight borrowing constraints, (vi) monopolistic competition and firm entry with fixed costs, (viii) nominal rigidities, (viii) simple banks with a net worth constraint (ix) distortionary taxes and government spending, and (x) a desire for liquidity for exchanges in decentralized markets. This alternative model makes stark assumptions that make the model incredible but also quite tractable and insightful on a series of important features of the world. I put forward that spending more effort debating what should be in such a model and trying to write it down would lead to the highest marginal return produced by debates on the state of macroeconomics.

Further, empirical work in macroeconomics today includes a rich set of tools and approaches. Macroeconomists need to be trained in time series, and also to understand the fundamental identification problems, and the rich datasets that can be used to test behavior. There are classic empirical questions that one could structure an entire class in core macroeconomics around, and taking the model to the data is today not an after-thought but an integral part of almost all research projects. Macroeconomics could be taught in a much more data-driven way than what is done today.

This is a debate worth having, especially as I am sure that many would disagree with the ten alternatives that I have proposed to the ten assumptions above, and with what weight should empirical work have in the core sequence. Criticisms and discussions of macroeconomics focussed on this discussion would be more constructive and get the wider community of macroeconomists involved. With more students pursuing graduate studies and higher demand for people trained in advanced economic tools, graduate-level macroeconomics especially at the Masters level cannot be taught as if its only role was to train future academic researchers. As Mankiw (2006) and Blanchard (2017) emphasize, there is an important role for macroeconomists as engineers, as opposed to scientists, and this requires small usable models.

IV. Conclusion

I have argued that while there is much that is wrong with macroeconomics today, most critiques of the state of macroeconomics are off target. Current macroeconomic research is not mindless DSGE modeling filled with ridiculous assumptions and oblivious of data. Rather, young macroeconomists are doing vibrant, varied, and exciting work, getting jobs, and being published. Macroeconomics informs economic policy only moderately and not more nor all that differently than other fields in economics. Monetary policy has benefitted significantly from this advice in keeping inflation under control and preventing a new Great Depression. Macroeconomic forecasts perform poorly in absolute terms and given the size of the challenge probably always will. But relative to the level of aggregation, the time horizon, and the amount of funding, they are not so obviously worst than those in other fields. What is most wrong with macroeconomics today is perhaps that there is too little discussion of which models to teach and too little investment in graduate-level textbooks.

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Endnotes:

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[2] For my view on these three points, see Mankiw and Reis (2010), Oh and Reis (2012), and Hilscher, Raviv and Reis (2014), respectively.

[3] The list of participants is available here: http://www.restud.com/wp-content/uploads/2010/12/May-Meeting-speakers.pdf

[4] Not even economists think they had much of an impact on the Brexit vote; see Den Haan et al (2016).

[5] See Reis (2009) and Blinder (2013).

[6] See Baldwin et al (2015) and Brunnermeier and Reis (2016).

[7] See Card (2016), Borjas (2014) and Card and Peri (2016).

[8] They are, in reverse chronological order, Roland Fryer, Raj Chetty, Amy Finkelstein, Emmanuel Saez and Daron Acemoglu. The full list is here: https://www.aeaweb.org/about-aea/honors-awards/bates-clark

[9] Deaton and Cartwright (2016), Angrist and Pischke (2009) and Wolpin (2013) among others.

[10] Se for instance Edge and Gurkaynak (2010) and Wieland and Wolters (2012).

[11] For assessments of the state of forecasting see Clemens and Hendry (2011) or Elliott and Timmermann (2013).

[12] Angrist and Pischke (2017).