# Truth or precision?
# Some reflections on the economists' failure to predict the financial crisis*

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*The failure of professional economic forecasters to predict the financial crises has led many to question the credibility of modern economics as a reliable foundation for economic policy. If economists were unable to foresee so big a crisis, how can they be trusted to cure or prevent it? Several accounts of this failure exist. The paper offers a tentative answer based on the lessons that may be drawn from the wisdom of a short list of past and present economists: Hayek, Neville Keynes, Mankiw, Tinbergen, Maynard Keynes and Lucas. The glue to keep such an odd bunch together is provided by science historian Ted Porter. A natural science field that recently managed to survive a similar credibility crisis offers a success story to imitate. Not casually, it is a story that confirms the validity of Hayek's pattern predictions argument.*

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## 1. Introduction: a credibility crisis

"The obvious conclusion is that forecasts should not be taken seriously. There is not a lot of point asking an economist to tell you what will happen to the economy next year – nobody knows". These words by Tim Harford closed his *Undercover Economist* column in the *Financial Times Magazine* of May 30, 2014.[1] The following picture, taken from a recent short paper by IMF economists Prakash Loungani and Hites Ahir, seems to validate them. Based on the data of Consensus Economics, the world's leading macroeconomic survey firm, which provides forecasts of real GDP for a large group of countries made by more than 700 professional forecasters around the world,[2] the picture shows the number of countries experiencing recession in the period 2008-2012 and the number of countries that, according to economists' consensus predictions made by September of a given year, would experience recession the following year (Ahir & Loungani 2014).

[INSERT FIGURE 1 HERE]

The negative record is impressive. Of the 77 countries under consideration, 49 of them were in recession in 2009. Yet, economists – as reflected in the averages published in a report available for sale, named *Consensus Forecasts* – had not called a single one of these recessions by September 2008! In his column Hartford underscores that this is *not* the famous complaint by the English Queen that no economist saw the financial crisis coming.[3] The crisis was already on its way (Northern Rock and Bear Stearns had already collapsed) when these forecasts were made. Moving forward in time, there were 15 recessions in 2012 – that is, after the best economic minds of a generation had studied the crisis and its explanations for the previous four years. Still, only two of these recessions were predicted by September 2011. Not that the financial crisis was a special event either. Indeed, Loungani had made his name back in 2001, with a paper on the same topic and based on the same data source for the period 1989-1998, whose main result could be grasped already in the abstract: "the record of failure to predict recessions is virtually unblemished" (Loungani 2001, 419).[4]

[1] See http://www.ft.com/intl/cms/s/2/14e323ee-e602-11e3-aeef-00144feabdc0.html (retrieved Dec 1, 2014).
[2] So its website boasts: see www.consensuseconmoics.com (retrieved Dec 1, 2014).
[3] See http://www.telegraph.co.uk/news/uknews/theroyalfamily/3386353/The-Queen-asks-why-no-one-saw-the-credit-crunch-coming.html
[4] More precisely: "Only two of the 60 recessions that occurred over the sample were predicted a year in advance, two-thirds remained undetected by the April of the year in which the recession occurred, and in about a quarter of the cases the forecast in October was still for positive growth (albeit small)" (Loungani 2001, 430).

This is the portrait of a disaster. To say the least, economists active in the forecasting business suffer from a serious credibility issue. One may wonder what anybody should be willing to pay for the privilege of receiving twelve months of Consensus Economics's service – the listed price actually runs at €620 for forecasts about the G7 countries and Western Europe. Or, more seriously, one may ask, why this failure? Why, as Köster (2014) called it, the "perpetual disappointment"?

Generally speaking, consumers of economic forecasts belong to two categories. Many are in the private sector, especially businesses requiring advance information for devising their plans or making financial decisions. Several end-users are in the public sector, though: policy-makers need trustworthy forecasts as a foundation for their policies. The "garbage in, garbage out" principle strikes inexorably in both cases. In the former, however, the consequences of trusting wrong forecasts fall, at least in first approximation, entirely upon the users; in the latter, the mistakes affect everyone. Regardless of the specific measures undertaken, as well as of the other problems that may affect policy-making, the simple, but scary, truth is that economic policies are all too often based upon forecasts that are no better than those exemplified above. Forecasting failures translate directly into policy failures. The credibility issue becomes a credibility crisis: why should we trust the economists in power?

The typical reply to this critique is that we live in a very complicated world, but with the help of more data and more refined models we would get better forecasts and, from there, better economic policies that would restore the discipline's authoritativeness. Alas, it is not just a matter of data availability or improving existing models. My claim is that forecasting and policy failures are related manifestations of the same fundamental problem, namely, a defective philosophy of model building. Too many economists are unaware of the intrinsic limitations of what their discipline may achieve. Practitioners should recognize that the only possible predictions of economic events are pattern predictions; that the models they use for both forecasting and policy-making are uselessly precise, but seldom accurate; that their approach to economic issues should be that of the physician, rather than the engineer; more generally, that forecasting and policy-making are always a matter of art, not science. These limitations should lead economists to revise their offer to end-users, by severely qualifying their forecasts and policy proposals. Above all, they should advice them against the adoption of too specific policies, for lack of adequate empirical grounding. Helping devise institutions capable of preventing worst-case scenarios should become the economists' more modest, but still invaluable, task.

All of the fore-mentioned are hardly original points. On the contrary, they have all been made in the past, recently and not-so-recently. Starting from the early 2000s, Austrian economics scholars such as Peter Boettke, Peter Lesson and Mark Pennington have identified systemic robustness as an essential, though often overlooked, component of political economic analysis. Performing the robustness check for any political economy argument is crucial, these authors argue, because key economic propositions are often only valid under "best-case" assumptions (like, say, full rationality and perfect information), but fail in more realistic scenarios when these assumptions do not hold. Robust systems for organizing economic activities are on the contrary those that are able to stand up to the test of less-than-ideal conditions, including the "worst-case" scenario when none of the assumptions holds but the system still performs well – hence, the name "robust political economy" given to the approach.[5] It follows that every economic forecast or policy advice should first be subjected to the robustness check and discarded if too dependent on specific assumptions.

While recognizing the validity of the robust political economy approach and its relevance for explaining the economists' dismal record vis-à-vis the financial crisis, the present paper argues that a similar lesson may be drawn borrowing from the wisdom of a short list of big names in economics. Appealing to the discipline's history to tackle so pressing an issue should not sound obscure: economics does have a useful past and evoking it often helps illuminate current controversies.[6] The economists' failure with respect to the crisis is a case in point. Accordingly, the paper builds on the lessons by the likes of Hayek (§1), Neville Keynes (§2), Mankiw, Tinbergen, Maynard Keynes and Lucas (§4). The glue to keep such an odd bunch together is provided by science historian Ted Porter (§3). A natural science field that recently managed to survive a similar credibility crisis offers a success story to imitate (§5).

## §2. Hayek and pattern predictions

The starting point is Hayek's famous critique of the possibility of economic predictions. The Austrian economist explained that a methodological divide exists between the physical and the social sciences. The former construct and test formal models and predict particular (physical) events; the latter are bound by the high complexity of their subject to explain and predict no more than the general *pattern* of (social) events, not their individual manifestation.[7] This dichotomy underlay Hayek's main critique, which he most forcefully proposed in two essays, "Degrees of explanation" (1955) and "The theory of complex phenomena" (1964), both reprinted in Hayek (1967). Following Paqué (1990), we may summarize Hayek's message with three *negative* statements. All of them are expressions of Hayek's anti-constructivism, which – as underlined by Paqué (ibid., 294) – remains the best synthetic description of his epistemology.

The first negative statement is about the impossibility of model specification in the social sciences. According to Hayek, social sciences, including economics, deal with more complex phenomena than physical sciences. Theoretical model building and testing is therefore more difficult for them and the likelihood of specification errors is always greater. It follows that empirical knowledge in social sciences, including economics, is always intrinsically fragile.

Hayek's second negative statement concerns the impossibility of single event predictions. Hayek was among the earliest scholars who recognized the fallacy of excessive testing enthusiasm – hence, his famous "pattern

[5] See Boettke & Leeson 2004, Pennington 2010, and the papers in this journal's 2006 special issue (Leeson & Subrick eds. 2006).
[6] See Boettke et al. 2010.
[7] A pattern is a regularity of physical or social phenomena, a general characteristic of events to be expected.

predictions" argument: "We are […] interested not only in individual events, and it is also not only predictions of individual events which can be empirically tested. We are equally interested in the recurrence of abstract patterns as such; and the prediction that a pattern of a certain kind will appear in defined circumstances is a falsifiable (and therefore empirical) statement. […] [Economic] theory enables us to predict or explain only certain general features of a situation which may be compatible with a great many particular circumstances" (Hayek 1967 [1964], 28-9). This is what he called explanation of the principle. Related to this second impossibility was also his idea that, contrary to naïve falsificationism, broad "frameworks of thinking", such as general equilibrium theory (GET), may still have an important epistemological (or, at least, pedagogical) role even if they cannot be properly tested.

Finally, Hayek pointed at the impossibility of using predictions to guide policy making. Economic theories can never deliver anything like the accurate forecasts that could serve as a solid basis for policy decisions. Here Hayek's critique somehow anticipated the failure of econometric models in the 1970s (see below, §5), especially on account of his previous analysis of the role of knowledge in society, including the crucial – and typically Austrian – reflexivity issue.[8]

The three negative statements originated from the above-quoted methodological divide – more specifically, when the divide was applied to Hayek's peculiar notion of economic theory.[9] Generally speaking, an economic theory is any explanation of the organizing principle of the economic system (or some part of it). For standard economics, this organizing principle is equilibrium, i.e., a situation of mutual consistency among given relationships; for Hayek, it was order, i.e., the outcome of a transformational process. In his view, economic theory should explain the real market processes conducive to order and the social rules and institutions governing them.[10] The difference explains why the Hayekian system had no room for the kind of predictions elaborated by orthodox economists, and even less so for their policy-making applications. Pattern predictions are not simply the maximum that can be achieved, but also the most proper kind of predictions within Hayek's order-based approach.

## §3. Neville Keynes and the art of economics

Among the main messages of the financial crisis, we may include an old truth that previous generations of economists knew pretty well. Applied economics is neither positive, nor normative. It is an *art*, that is to say, according to the *Merriam-Webster*, "the conscious use of skill acquired by experience, study, or observation".

[8] Morgenstern 1928 foreran much of what Hayek said about the limits of economic forecasts. In particular, he first raised the reflexivity issue. See Giocoli 2001.
[9] I thank one of the referees for raising this point.
[10] See Fleetwood 1996.

David Colander explained some time ago what understanding economics as an art entails. In his view, the art of economics performs the fundamental task of relating the lessons of positive economics to the goals determined in normative economics (Colander 1992). But Colander was not the first economist to notice that. In his classic methodological essay *The Scope and Method of Political Economy*, John Neville Keynes offered a famous articulation of the same tripartite distinction. He defined positive and normative science as systematized knowledge concerning, respectively, "what is" and "what ought to be", and art as "a system of rules for the attainment of a given end" (Keynes 1891, 34-5). Keynes noted that merging "questions of what ought to be with questions of what is" determined a confusion affecting not only economics discussions, but also economic methodology. Different methods of investigation should be credited with different value depending on whether we take an ethical and practical, or a purely scientific standpoint (ibid., 61-2).

Building on Keynes's essay, Colander claimed that positive economics suffers from the lack of an art of economics because "if a separate art is not delineated, positive economic inquiry faces pressures to have policy relevance, which is constraining to imaginative scientific enquiry" (Colander 1992, 194). Keynes had said it even better: "In dealing with practical questions, an abstract method of treatment avails less and carries us much less far than when we are dealing with theoretical questions" (Keynes 1891, 62). On the other hand, without the art of economics normative economics is forced to deal with practical policy issues, to the detriment of its proper task, namely, determining what policy goals are appropriate in the first place. Good applied work should just tell economists, and those who pay for their expertise, how to achieve the goals they want to achieve as effectively as they can, with no normative judgment about the goals themselves. Adding up, the art of economics turns out to play an essential role, namely, to take the set of goals determined in normative economics and try to achieve them in the real world using the insights of positive economics (Colander 1992, 195-6).

The negative consequences of disregarding the art of economics are exemplified by those areas of modern applied economics that rest upon general equilibrium theory (GET) foundations, such as modern finance and dynamic stochastic general equilibrium (DSGE) macro models. Both areas explicitly embrace the methodology of positive economics, to the total neglect of the art side of the discipline. Let's focus on DSGE models, which form the baseline for most current macro forecasts. These models employ a formalistic method of argumentation, based upon strong assumptions that warrant internal coherence. Hence, they are *rigorous* models. That consistency, or rigor, is their most important attribute reveals their purely positive (in the above mentioned sense) character.

The underlying idea of DSGE is that, by specifying, in a dynamic and stochastic framework, the agents' preferences, the technology at their disposal and (a usually very stylized set of) the institutions constraining their behavior, it is possible to solve the general equilibrium model of the economy, in order to predict what is actually produced, traded, and consumed, and how these variables would evolve over time in response to various shocks. In principle, it should also be possible to make predictions about the effects of changing the institutional framework. DSGE thus aims at building "an artificial economy that has the property that economic outcomes are clearly related to private agent objectives and constraints, generates artificial data that *resemble* macro economic data of actual economies, is useful as a laboratory to analyze the economic impact of policies, and so contributes *input* into discussions about key policy questions".[11] This is manifestly the description of what may be called a desk experiment – a procedure typical of positive inquiry in several scientific disciplines. Indeed, DSGE godfathers, Kydland and Prescott (1982), singled out the ability to *imitate* real world phenomena as the evaluation criterion for a macro model.

Starting from the late 1990s, DSGE models upgraded from academic toys to serious candidates for actual policy evaluation, but their desk experiment nature seldom received due consideration.[12] Two questions arise. First, are desk experiments any good for real life policy-making? Second, do they meet Hayek's requirement of pattern predictions? The formalized imitation of real world phenomena, or the use of models as laboratories, open big epistemological problems that I cannot handle here.[13] I will thus focus on the second question, which is of more direct interest for our problem.

By Hayekian standards, the record of DSGE has been dismal. Key macro patterns (i.e., general characteristics of events to be expected, such as 49 out of 77 countries suffering recession by 2009) went totally unforeseen. "An aphorism among macroeconomists today is that if you have a coherent story to propose, then you can do so in a suitably elaborate DSGE model", a recent authoritative account recited (Chari et al. 2009, 243). Apparently, none in the profession's mainstream ever proposed the financial meltdown story. Invoking the unpredictability of specific economic crises is no excuse here because a model unable to deliver even broad pattern predictions is simply useless for policy-making.[14] Predicting patterns is no exclusive of Hayek's order-based view, but the very least we may require even to models belonging to the orthodox, equilibrium-based approach.

Following the crisis, the DSGE approach has been subjected to extensive criticism. Even the Sub-Committee on Research and Technology of the US House of Representatives held a formal hearing on the limits not of modern macroeconomics in general, but, specifically, of DSGE![15] In his testimony, Colander argued that "the economics profession failed society […] by letting policy makers believe, and sometimes assuring policy makers, that the topography of the real-world matched the topography of the highly simplified DSGE models, even though it was obvious to anyone with a modicum of institutional knowledge and educated common sense

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[11] This short description of a DSGE model is drawn from the introduction to a 2011 macro course held at Northwestern University by prominent macroeconomist Lawrence J. Christiano (see http://faculty.wcas.northwestern.edu/~lchrist/course/syllabus.htm). Better than any reference to published research, this gives a perspective of how DSGE is presented to students, i.e., the approach's future users and, possibly, developers.

[12] Not long ago, leading macroeconomist Michael Woodford extolled the "current methodological consensus" among macroeconomists on DSGE, explaining how in the first decade of the new millennium "the rate at which ideas from the [DSGE] research literature are incorporated into modeling practice in policy institutions has accelerated, with forecast-targeting central banks often playing a leading role" and listing a series of DSGE models "developed by policy institutions for use in practical policy analysis" (Woodford 2009, 276-7). Woodford's essay – whose closing sentence read as: "the current moment is one in which prospects are unusually bright for the sort of progress [in macroeconomics] that has lasting consequences" (ibid., 277) – was rather untimely published in January 2009.

[13] A compulsory reference on this theme is Morgan 2012.

[14] On the intrinsic inability of DSGE models to account for extrinsic unpredictability (i.e., unexpected shifts in the model's underlying probability distributions), see Hendry & Mizon 2014.

[15] See http://science.house.gov/hearing/subcommittee-investigations-and-oversight-hearing-science-economics.

that the topography of the DSGE model and the topography of the real-world macro economy generally were no way near a close match" (Colander 2010).

Several objections against DSGE models may, and have been, raised.[16] But I agree with Colander that what dooms them as policy tools are their positive economics foundations. A formalistic approach leads to "needlessly precise" results (Colander 1992, 194), at the price of losing the interconnections between the various dimensions (institutional, social, political, etc.) of the problem under scrutiny. As Neville Keynes underlined, the art of economics keeps these dimensions always at center stage; positive economics – by its own methodological requirements (say, rigor) – ignores them.

Colander (ibid., 195) adds à la Hayek that, because of the central role of institutional, sociological and political dimensions, precise tests are impossible in the art of economics. One may then ask what the role of empirical validation for "economic artisans" should be. Colander's answer is that empirical work in the art of economics has a different purpose than in positive economics. Application, rather than testing, should be the driver. Empirical work in positive economics tests whether a theory should be accepted or rejected. This kind of test, and its underlying question, is irrelevant when a theory has to be applied to real world problems – that is, when a degree of judgment dependent on institutional and historical information is required. Hence, empirical work in the art of economics must be of a nature other than statistical testing. It must be expressly designed to apply a theory by adding back the contextual reality.[17] The two types of empirical work are fundamentally different. The problem, Colander concludes (ibid.), is that current practices, especially in predicting business cycles, do not differentiate between them. A reborn Neville Keynes could then legitimately ask, why?

## §4. The precision trap

As we said before, DSGE results are "needlessly precise". A different way to say that is that, though precise, they are *not* accurate.[18] By *precision* we mean the quality of a number, or scientific result, of being definite and unambiguous; by *accuracy* the quality of it being valid to identify a thing or event. For example, very precise directions for traveling between two points or for assembling a piece of furniture may well be inaccurate and lead their user astray. Accurate instructions are those that correctly indicate their object – i.e., how to get to destination or build your IKEA bookshelves.

The previous definitions are taken from a 2006 essay by science historian Ted Porter. His main thesis in that essay is that in every instance of public and administrative use of a social science, first of all economics,

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[16] For a popular summary of these critiques, see Quiggin 2012, Ch.3.

[17] A possible example of this kind of empirical validation may be found in market design experiments, on which see e.g. Guala 2007.

[18] Writing about "Macro after the crisis", MIT economist Ricardo Caballero has made the same point: "What does concern me about my discipline, however, is that its current core – by which I mainly mean the so-called dynamic stochastic general equilibrium – has become so mesmerized with its own internal logic that it has begun to confuse the precision it has achieved about its own world with the precision that it has about the real one. This is dangerous for both methodological and policy reasons (Caballero 2010, 85).

numbers serve, and have always served, as a *technology of distance*, and, by implication, of trust or distrust. Such a technology is necessary to satisfy the leading historical preoccupation of social science, namely, that of appearing "neutral" and "objective" (like the natural sciences), despite its inevitable connection with political decisions – a connection that make its results and prescriptions perennially "suspect" vis-à-vis the public opinion's request for *truth* (Porter 2006, 1282).

According to Porter, the 20th-century conception of social science as an independent academic practice, rather than as a political or administrative branch of government, has further increased the pressure to ground the social scientists' applied work in neutral, impersonal methods. Only rigorous quantitative methods may guarantee the distance between the analyst and her object of study or policy advice. The point is that, by definition, these are methods that shun the recourse to wisdom and experience – that is, to the very features that make the social scientist's work akin to a subjective *art* (ibid., 1284). The two most typical phenomena in the evolution of social sciences are, therefore, "the transformation of knowledge into information, with the attendant obscuring of subtleties that would demand interpretation" (ibid., 1282), on the one side, and the circumstance that "the idealization of impersonal objectivity as the model of public rationality may happen at the expense of accuracy", on the other (ibid., 1288).

In this view, the development of empirical quantitative methods, like those employed to forecast business cycles,[19] has been at least as much an adaptation to these "distance" requirements of applied social science as an achievement of fundamental research. To use Porter's effective terminology (ibid., 1288), empirical methods cater to the necessity of modern social scientists of "speaking *precision* to power". This necessity has replaced, for the above mentioned reasons, the old Enlightenment ideal of "speaking *truth* to power *and* society".[20] Predicting business cycles nicely exemplifies the precision drift.

When the emphasis is on the "objectivity" of information, what social scientists are asked for is not truth, but constraint, i.e., the minimization of the researcher's subjectivity. But objectivity means precision, not, or not necessarily, accuracy. Hence, the economists' work generating "objective" business cycle forecasts is deemed scientific if and only if it satisfies high standards of precision – say, those warranted by formal mathematical requirements – that transcend any pretension to accuracy. Alas, it turns out that precision is *the most* that can be achieved when economists apply the conventions of quantitative methods – that is, of the positive side of their discipline – to the necessarily simplified description of a very complex socio-economic reality they may never grasp in its entirety. This puts economists in an awful situation. Their professional exigency to "speak precision to power" collides with the public opinion's unchanged request for *truth*. Economists are, in short, subjected to opposing pressures they can in no way reconcile.

[19] Another prominent example being cost-benefit analysis: see Porter 2006, 1285.

[20] Porter 2006, 1274-8, explains how satisfying the demand for truth arising from both government quarters and society at large was part of the Enlightenment project.

If Porter's reconstruction is correct, the disaster of macroeconomic forecasts stems as the logical outcome of a sort of syllogism. Hayek's message is that, on account of the higher complexity of social sciences, you may at most predict social (and thus economic) patterns, never single events. Neville Keynes message is that good applied economics, of the kind required to make predictions, requires art, not merely science. With the right dose of art, prediction is possible, though, Hayek would add, only in the form of patterns. Porter's message is that when called to make predictions, economists are forced by their exigency to achieve objectivity, to "speak precision"; to do so, they must neglect the art side of their discipline and only make recourse to positive methods capable of generating sharp quantitative forecasts. It follows that, by Hayekian and "Nevillian" standards, their efforts are inevitably destined to fail: economic forecasts will be precise, but never accurate and, therefore, totally useless. Worse, to public opinion's eyes, the predictive failure casts discredit on the whole discipline.

In his Nobel Prize lecture, Hayek evoked exactly this risk. "[C]ompared with the precise predictions we have learnt to expect in the physical sciences", he wrote, "this sort of mere pattern predictions is a second best with which one does not like to have to be content. Yet the danger of which I want to warn is precisely the belief that in order to have a claim be accepted as scientific is necessary to achieve more. This way lies charlatanism and worse. To act on the belief that we possess the knowledge and the power which enable us to shape the process of society entirely to our liking, knowledge which in fact we do not possess, is likely to make us do much harm" (Hayek 1974).

## §5. Of dentists and engineers

Predictions are central to the *engineering view* of macroeconomic policy. The expression refers to the distinction between macroeconomics as science and as engineering drawn by Gregory Mankiw in an essay written just before the financial crisis (Mankiw 2006). According to Mankiw, the field of macroeconomics "has evolved through the efforts of two types of macroeconomist – those who understand the field as a type of engineering and those who would like it to be more of a science. Engineers are, first and foremost, problem solvers. By contrast, the goal of scientists is to understand how the world works" (ibid., 29-30). The engineering approach is the key to applied work: "If God put macroeconomists on earth to solve practical problems, then Saint Peter will ultimately judge us by our contributions to economic engineering" (ibid., 40).

As if he were prescient of the mayhem that was about to explode in the world economy, Mankiw asked a crucial question: "Have the developments in business cycle theory over the past several decades improved the making of economic policy?" (ibid.). The theoretical developments he had in mind are, of course, DSGE and, more generally, the rational expectations revolution of the late 1970s-early 1980s. Mankiw's answer was surprisingly negative – surprisingly, I mean, because it came from one of the most prominent macroeconomist of the world. It deserves to be quoted in full: "The real world of macroeconomic policymaking can be disheartening for those of us who have spent most of our careers in academia. The sad truth is that the macroeconomic research of the past three decades has had only minor impact on the practical analysis of monetary or fiscal policy. The explanation is not that economists in the policy arena are ignorant of recent developments. Quite the contrary. […] The fact that modern macroeconomic research is not widely used in practical policymaking is prima facie evidence that it is of little use for this purpose. The research may have been successful as a matter of science, but it has not contributed significantly to macroeconomic engineering" (ibid., 42-3). A substantial disconnect thus exists between the science and the engineering of macroeconomics. This, to Mankiw, "should be *a humbling fact* for all of us working in the field" (ibid., 30; emphasis added).[21]

The engineering view is the intellectual backbone of modern macroeconomic policy. This, note well, even in market economies where, of evidence, none plays the social engineer's role. The view's underlying metaphor is Jan Tinbergen's targets-and-instruments approach. In a series of mid-1950s works, Tinbergen was the first to introduce the distinction between the objects of policy (*targets*) and the means of influencing the economy's path towards them (*instruments*).[22] While he conceived of it as a way to support his favored organization of economic activity, centralized planning, the distinction has inspired all subsequent policy-making. The idea is that an economist should, first, observe the economy's behavior, trying to capture it with a macroeconometric model; having done that, she can vary the policy instruments (say, the interest rate), using the model to predict the results of her actions with respect to the targets. Theoretically speaking, the method would allow the planned management of the whole economy, but its actual application has been to perform specific economic policies.

As noted by Kevin Hoover, this approach is typical of engineers. Like an engineer, the economist is viewed as standing outside "the machine" of the economy, trying to discover how to fix, or boost, it by looking at the different solutions' impact upon a stylized model of "the machine". Out of the metaphor, Tinbergen characterized the economist as an advisor who employs policy variables to perform various kinds of counterfactual exercises that may possibly lead to sound policy advice (Hoover 2012, 19). This approach is fully positive, or scientific, in the above-mentioned sense. Economic art can find no place here, because quantitative precision in the relation between instruments and targets is key to the credibility of the whole endeavor. Engineers are not supposed to make their expertise conditional to fuzzy environmental variables, such as social, political, and institutional features. They are supposed to give precise – and possibly accurate – advice.

At most, engineering-oriented economists may view real world institutions as obstacles. This, as Peter Boettke and Steven Horwitz explain, was the prevailing attitude between the late 19th century and the early 20th century, when economists first tried to apply the engineering method, "which had been apparently so successful in taming nature", in order "to rein in the forces of the social world" and put them to "serve the cause of human betterment" as a result of "human reason rather than blind evolution" (Boettke & Horwitz 2005, 11-2). New

[21] In the essay mentioned above (fn.12), Woodford replied to Mankiw's thesis praising the diffusion of DSGE models in contemporary policy-making. See Woodford 2009, 275-7.
[22] See for all Tinbergen 1956. The rest of this § follows Hoover 2012.

institutions had therefore to be designed to replace those that were deemed responsible for the economic problems of the day. Disappointment with the outcome of their engineering efforts led post-WWII economists to disregard institutions altogether and focus on the optimal allocation of income and resources – the quintessential precise task – in a sort of institutional vacuum. Tinbergen's approach exemplifies this later attitude.

Hoover underlines that the engineering view did not match John *Maynard* Keynes's idea of the economist's role as a policy advisor. Keynes famously despised macroeconometric modeling à la Tinbergen precisely because he rejected the mechanical metaphor underlying it: "The object of our analysis", he wrote in the *General Theory*, "is, not to provide a machine, or method of blind manipulation, which will furnish an infallible answer, but to provide ourselves with an organized and orderly method of thinking out particular problems" (Keynes 1936, 297). Accordingly, when dealing with monetary policy in the *Treatise on Money*, he famously endorsed "the reasonable doubts of practical men towards the idea that the Federal Reserve System has the power to raise or lower the price level by some automatic method, by some magic mathematical formula" (Keynes 1971 [1930], 305). Keynes's role model was not the engineer, but a less heralded professional: "If economists could manage to get themselves thought of as *humble*, competent people on a level with dentists, that would be splendid" (Keynes 1972 [1931], 332; emphasis added). Indeed, we usually approach dentistry differently than engineering. Building on Keynes's words, *FT Magazine* Harford remarks that you do not expect a dentist to be able to forecast the pattern of tooth decay. What you do expect of her is that she will offer good practical advice on dental health and will intervene when your tooth begins to ache (Harford 2014).

Of course, the boundary between dentistry and engineering may not be that sharp: engineers will often offer good practical, problem-solving advice, with no ambition to re-shape the environment that originated the problem in the first place; at the same time, dentists may abuse of the tools at their disposal. Indeed, the key in Keynes's passage above is the adjective "humble". Humility – or, as Boettke & Horwitz (2005, 11) calls it, "epistemic modesty" – with respect to the power of one's own knowledge to predict and control the environment is what distinguishes the dentist's from the engineer's approach.

Among the reasons why Keynes could not buy into Tinbergen's method was his belief that predictions were always affected by irreducible uncertainty. This basic fact made economists' knowledge for policy use always *partial* – that is, unsuited for application to engineering exercises. Yet, Keynes did not suggest inertia or surrender. On the contrary, he thought economists should acquire valuable pieces of that partial knowledge by "playing the game" of policy-making – that is to say, by being involved, with their art *and* their science, in the working of the economic machine (Hoover 2012, 19-20). In short, the Keynesian economic advisor should operate *within* the economy, as an "artisan" dirtying her hands with the real thing, not externally of it, as an engineer providing abstract instructions about what levers to pull.

Going back to Mankiw, he recognizes that real macroeconomic policy sticks to a problem-solving, practical approach that seemingly does not (or does not immediately) incorporate the discipline's scientific advancements. At a minimum, this demonstrates an awareness by economic advisers of the limits of a purely positive approach.

Good news then for those who long for a return to policy-as-art? Alas, no. Hoover's interpretation of Tinbergen's target-and-instrument method shows that this is just an illusion. Even when disconnected from the discipline's theoretical frontier, macroeconomic policy still partakes of an engineering approach that inevitably disdains art and privileges precision over accuracy.

The solution may not come from new classical macroeconomics (NCM) either. The approach has had the unquestionable merit of fusing together the insider and outsider views of economic advising. The well-known Lucas critique to macroeconometric modeling (Lucas 1976) may indeed be considered a sophisticated way of taking into account the economist's "internal role" in the economic machine – that is, of answering Keynes's objection to Tinbergen while preserving the gist of the engineering approach.

Indeed, it is fairly well known that Lucas's main polemic target was Tinbergen, rather than Keynes. Lucas famously argued that if a macroeconometric model characterizes the time-series behavior of variables without explicitly accounting for the underlying decision problems of the rational agents who make up the economy, then when the situation those agents find themselves changes (because of, say, a policy change), their optimal decisions will change too, and so will the time-series behavior of the aggregate variables upon which the model has been built. In other words, a macroeconometric model's fit to the aggregate data will *not* remain stable with respect to a shift in the policy rule. This in turn will make it useless any evaluation exercise à la Tinbergen of the macro effects of policy measures. Lucas's solution called for explicit microfoundations of macroeconometric models: to overcome his 1976 critique, all macro relations must be grounded in the optimal (i.e., equilibrium) micro decisions of rational agents. Consistency required that the microfoundations enterprise could find its building blocks in nothing else than GET and rational expectations. DSGE models are just the most mature fruit of the project.

Yet, even post-Lucas models cannot escape the engineering trap. They are thoroughly quantitative and mathematically closed. They are, in Porter's sense, still precise, but not necessarily accurate. Under this respect, and notwithstanding the Lucas critique, these models still belong to Tinbergen's mechanical world (Hoover 2012, 22). While Lucas offered a brilliant way out from the problems created by the economist's inevitable embedment in the object of her study, he could not fully answer Keynes's objection against mechanical policy-making. Retaining this objection, we may thus conclude that it is in the engineering side of DSGE models – in their quest for that absolute consistency that represents their very *raison d'être* – that we may find the true reason for their dismal predictive performances.

## 6. Conclusion: accepting contamination

We can think of the testable implications of economic models as inevitably suffering from several kinds of "contamination": irrationality, uncertainty, disequilibrium, socio-political contingencies, cultural biases, and so on.[23] Moreover, as in any social science, a further element of contamination is provided by the reflexivity effect that agents' expectations have on the structure of the models themselves. It were these contaminations that made the mechanical approach to economic forecasts – itself an expression of a control-oriented culture (i.e., the engineering view) – simply untenable.

The NCM revolution of the late 1970s recognized the problem and tried to rescue the control-oriented approach by applying the rational expectations hypothesis and the Lucas critique as elements of a rigorous "anti-contamination protocol".[24] Indeed, the entire microfoundation approach to macroeconometric modeling has been developed as a guarantee of scientific *precision*. The mantra became: if a model's microfoundations are ok – if the NCM protocol is rigorously obeyed – its results can be trusted. Unfortunately, reality has disconfirmed the mantra. Predictions based upon DSGE models – the NCM's most mature and rigorous offspring – have been a disaster. In Porter's terminology, precision has come at the price of severe accuracy loss.

It may thus be argued that economists would better follow the lead of other scientific disciplines that recently faced the same difficulty. The endless search for the perfect anti-contamination protocol, aimed at abstracting away from disturbing features such as irrationality, uncertainty, or real-world institutional constraints, should be replaced by a complexity-oriented culture of "non-knowledge". This would entail, first, accepting that contamination exists and no protocol can get rid of it, and, second, shifting the focus of research from statistical certainty to merely plausible patterns – from precision to accuracy, that is to say.

Economists would not have to start from scratch in this endeavor, because the basic ingredients already exist. Pattern predictions à la Hayek, the art of economics à la Neville Keynes, and the "insider work" of economic advisers à la Maynard Keynes all entail acceptance of the fact that, due to unavoidable contamination, non-knowledge is inevitable in economics, while plausibility is the highest degree of accuracy that a complexity-oriented approach can achieve. This admission should not sound completely alien to modern economists. Entire areas of the discipline already exist that, when dispassionately considered, consist of mere plausibility (i.e., "is it possible that?") statements – most notably, game theory.[25] Even historically speaking, recognizing non-knowledge would be no novelty. It would entail a return to the economists' original (and truly Smithian) role as "cautionary prophets", i.e., as experts in the art of advising about the limits of what can and cannot be done with respect to complex economic phenomena (see Boettke & Horwitz 2005, 18-9).

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[23] I use the term "contamination" in the same sense of paleo-population genetics, where the term refers to anything that may affect the purity of so-called ancient DNA, i.e., DNA recovered from biological samples (like fossils or mummified tissues) not specifically preserved for genetic analysis. Contamination by multiple external factors severely undermines the results obtainable by existing genetic techniques, which have been designed for application to pure DNA specimens. See Bösl 2014.

[24] Continuing with the previous footnote's analogy, an "anti-contamination protocol" is a procedure devised to guarantee the purity of ancient DNA samples and, therefore, their employability for genetic analysis. Regardless of the exact conditions of DNA recovery, respect of a properly designed protocol should suffice to trust the information obtained by laboratory analysis – or so it seems. On the intrinsic limits of the protocol-based approach to ancient DNA studies and the alternative, broader methodology now endorsed by many practitioners of the field, see again Bösl 2014.

[25] See Rubinstein 2006. For the argument that complexity is the missing ingredient in current economic policy discussions and that when complexity is added into the framework policy advice reduces to no more than educated common sense, see Colander & Kupers 2014.

Plausibility statements may sometimes suffice in policy-making – for example, to warn against possible catastrophic outcomes, like a global financial collapse – though not always. The biggest obstacle is that many economists, despite the manifest failure of their forecasts, still cannot accept to confine their predictions and policy advice to mere plausibility. We know they have reasons for that.

Natural scientists may have less trouble in admitting the ubiquity of contamination and the ensuing limits of their results. This is because theirs are not social sciences, so they do not have to "speak precision to power" and are relatively free to confess to the public opinion their inability to "speak truth".[26] Economists just cannot do that. They must produce sharp predictions. Their precision duty (dogma? hubris?) forces them to *pretend* authenticity even when none exists – worse, even when discovering plausible patterns would suffice to at least avoid the worst scenarios. It is time for economists to join the footsteps of Hayek and Neville Keynes and stop speaking precision or pretending truth. As Maynard Keynes and Mankiw wished for, the time has come that they start speaking humility to power.

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[26] Note however that a huge literature exists about the distortions that the ever increasing role of big money may have in the research attitude of natural scientists. Even in their fields precision and (fake) authenticity may be instrumental to justify the request of enormous research budgets.

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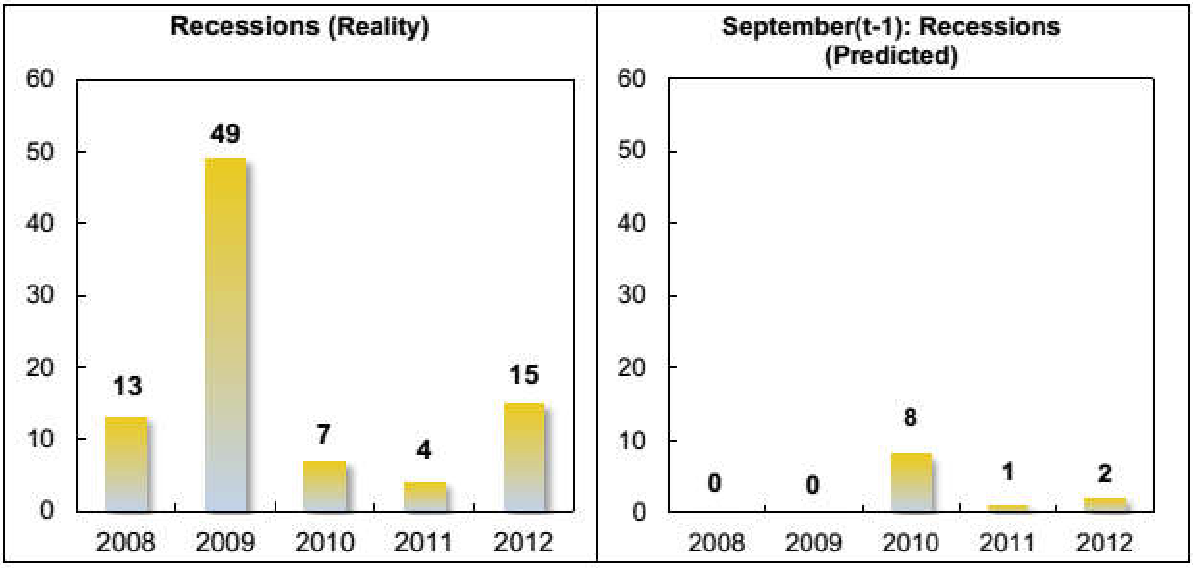
Figure 1: Number of Recessions Predicted by September of the Previous Year




(Source: Ahir & Loungani 2014)