A DECADE (AND A GLOBAL FINANCIAL CRISIS) AFTER BLINDER: THE INTERACTION BETWEEN RESEARCHERS AND POLICY-MAKERS IN CENTRAL BANKS *

Matthieu Bussière and Livio Stracca

Banque de France and European Central Bank

This version: 21 July, 2010

Abstract

Periods of economic and financial stress traditionally give rise to profound changes in economic theory and in the way policy decisions are taken. Motivated by the recent interest in renewing macroeconomics after the global financial crisis, we collected the views of senior central bank staff in 32 central banks by means of a special questionnaire on a number of issues related to the interaction between research and policy-making. Thereafter, the paper first surveys the existing literature on the relation between researchers and practitioners and offers some reflections on the fundamental and practical differences between research and policy work. Finally, it delves on the issue of model-based versus judgment-based approaches to economic forecasts and policy simulations, with a special emphasis on the growing role of DSGE models within central banks. We conclude with practical suggestions on how best to integrate models and research into policy making decisions.

Keywords: Economic research, policy making, central bank communication, economic crises, DSGE models.

JEL classification: A11, C1, D58.

"As a general matter, I firmly believe that monetary policy in the United States and other countries could and should become far more conceptual and less situational. This sounds a bit like saying that the central bank should become "more academic"; but [...] that is not what I mean. My experience at the Fed convinced me that central bankers are often so absorbed in the "trees" of the current economic situation that they lose sight of the "forest". They need to be constantly reminded of the latter. [...] For their part, academic researchers need to train their powerful tools on real-world issues instead of chasing intellectual will-o'-the-wisps." A. Blinder (1997), pp. 17-18.

"The ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed the world is ruled by little else. Practical men, who believe themselves to be quite exempt from any intellectual influence, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back." J. M. Keynes

1. Introduction

A bit more than ten years ago, Alan Blinder wrote an influential article on the relationship between academics and policy-makers in central banks (Blinder 1997). In a nutshell, Blinder's message was twofold. First, he advocated a more systematic approach in monetary policy making, less based on informal judgement and more on data, models and disciplined thinking. Second, he also advised academics to apply themselves to problems that are of real relevance to practical policy-making rather than to issues that are of interest merely out of intellectual curiosity. One could summarise these views by saying that policy makers and academics (at least those who are involved in monetary policy) should become more alike. The above quotation, taken from Blinder's conclusions, nicely epitomises these views.

Since the publication of Blinder's paper, the relationship between central banks and academics has if anything deepened. Not only visiting academics and central bank researchers produce top edge economic research, but also their models play an increasing role in policy-making. The kind of collaboration that has established itself between central banks and academia is something that is probably unprecedented in this scale in the history of economic policy, and arguably in politics in general; it is therefore a trend of historic significance. The kind of issues raised by Blinder, therefore, is not a minor one and warrants a deeper investigation. Furthermore, the current economic and financial crisis makes the question even more pressing: how can researchers help in this context? What advice can they give to policy makers? And to what extent should policy makers follow such advice, given that virtually no one was able to predict this crisis? This is what we set out to do in this paper. Should one agree with Willem Buiter (2009), who criticised *"the unfortunate uselessness of most 'state of the art' academic monetary economics"*?

Our general objective, in this paper, is to discuss some concrete issues that inevitably arise when researchers and policy-makers come in close contact and work together. Although the paper surveys recent developments in the literature, it is not strictly a literature review. Our analysis is to a large

extent inspired by our own work experience in policy-making departments in a major central bank. It should also be clarified that our paper does not aim to provide definitive answers but rather to raise questions and issues for further discussions. In this respect, our conclusions cannot be as sharp as those of Blinder. Nonetheless, we hope that a systematic look to issues that are often discussed just informally within central banks, and maybe at coffee breaks in academic conferences, could be of help and stimulate further discussion.

The paper starts off with a survey that we conducted amongst senior central bank staff in 32 central banks where we addressed some of the questions that we perceived to be most relevant for the relationship between monetary policy-making and economic research. We then review the existing literature and offer some personal reflections, based on our own experience and that of colleagues.

Before proceeding with this discussion, let us define more precisely what we mean by "policy makers" and "researchers". Starting with the former, what we have in mind is people in public institutions who are in a position to make decisions, or to actively contribute to them. Of course, one could further distinguish across different levels; for example, in a central bank, between the members of the Executive Board and the economists who collect information and relevant material to prepare for policy decisions. Similarly, turning now to researchers, one could distinguish between researchers who work on applied research (directly relevant for policy makers) and those who aim rather to develop new tools and concepts. One could also note that many economists have switched between research and policy making throughout their career. However, for the purpose of this paper, we consider policy makers and researchers as roles, not people, and we define policy makers as those who make economic decisions (on taxes, exchange rates, interest rates, public debt, regulations, etc) and researchers as those who develop models or empirical frameworks aiming to better understand economic mechanisms. Between the high-level policy maker and the most advanced researcher, there is therefore room for the "in-between" economists, who are called "the day-to-day economic adviser" in Chari and Kehoe (2006) and "policy adviser" in Wyplosz (1998). These are the people who need to bridge the gap between the policy makers who do make decisions and the hard-core researchers.

Policy-makers and researchers do different jobs, and differences between policy makers and researchers can be so vast that they can lead to significant misunderstandings. Taken to an extreme, such misunderstanding could lead to mutual distrust and lack of cooperation: from the policy makers' perspective, researchers may appear to waste their time looking at issues far remote from the problems that face in their day-to-day business and/or propose impractical solutions, while from the researchers' perspective, (very) applied economists and policy-makers may appear to base their decisions on shaky arguments, popular "stories" and to use imprecise language. In turn, rising

mutual distrust could lead policy makers to take decisions independently from researchers, and researchers to focus on issues of no direct relevance for policy makers. Thus, while we see the role of research as fundamental in policy-making, in our opinion the kind of fruitful and seamless collaboration envisaged by Blinder is not an inevitable and obvious outcome; as marriage counsellors use to say, researchers and policy makers need to work at their relationship.

What we propose, in this paper, is first to review the fundamental differences between policy and research work. While doing this, we emphasize what, concretely, policy makers can bring to researchers and vice versa. Specifically, we argue that researchers can bring discipline and objectivity into the policy debate and can help quantify the expected effects of policy decisions. Particularly in times of crisis, academics can help policy makers identify the mechanisms that are at play and design the most appropriate policy responses. In the other direction, we argue, policy makers can bring a lot to researchers. They can for instance identify the key policy questions of interest. In addition, policy makers generally have a very good knowledge of (and access to) data, which they can share with researchers. Another type of knowledge that policy makers commonly have relates to institutional arrangements (e.g., treaties, contracts typically agreed between economic agents, practical considerations on the feasibility of proposed solutions, for instance related to political support, etc), which researchers may incorporate in their models. Finally, policy makers often know what stylised facts can be considered as robust or, conversely, puzzling: such stylised facts often represent a promising starting point for research work. In the end, therefore, both researchers and policy makers can benefit from cooperation, provided that they are very much aware of the fundamental differences that separate them.

One important practical question is how much policy makers should trust model results (as opposed to lessons they can draw from their experience): this all depends on the quality of the model they consider. In the paper, we review prominent cases of "model failure", i.e. instances where the policy message derived from model results have provided the wrong decision (the discussion focuses on currency and financial crises). Of course, model failure in one particular instance does not represent a good reason to reject research in general; it does highlight, however, that using models requires effort, skills and care not only from the perspective of the developer but also of the user. In this connection, we review in greater detail one particular class of models that has received increasing attention in recent years, namely DSGE models and how they could and should be used by policy-makers. This discussion is particularly relevant given the sharp criticism expressed by Buiter (2009) and others.

Ultimately, the responsibility for policy decisions is with the policy makers, but the asymmetry of information between policy makers and researchers can be substantially narrowed through closer

interactions. So one of the implications is that researchers and policy-makers should talk to each other more and in particular policy-makers should question and probe the researchers, and generally be inquisitive and distrustful of pre-cooked conclusions.

The rest of the paper is organised as follows. In Section 2 we summarise the views of senior central bank staff in 32 central banks collected by means of a questionnaire. In Section 3 we describe in some detail what the main concrete implications are in the daily work of academics and researchers on the one hand, and policy-makers on the other. In Section 4 we discuss the concept of "policy-relevant" research in central banks. In Section 5 we take a close look at the role of economic models in general, and DSGE models in particular, in shaping the interaction between researchers and policy-makers. Section 6 concludes.

2. The views of central bankers

We collected views from senior central bank staff in 32 central banks (26 out of the 28 EU central banks and 6 non-EU central banks: the Federal Reserve Board, the Reserve Bank of Australia, the Reserve Bank of New Zealand, the Bank of Japan, the Swiss National Bank and the Bank of Canada).[1] Only one staff member answered for each central bank. Because the questionnaire was initially distributed amongst the members of the Eurosystem Head of Research Network, the respondents are typically the Head of Research in their central bank. In some cases, however, the position of Head of Research was not reflected in the central bank's organigram, in which case we strove to obtain the response of the manager responsible for the preparation of monetary policy briefings. The survey was conducted between December 2009 and January 2010.

It should be stated from the outset that the survey was unavoidably simplistic and provides a quite coarse reflection of the views of central bankers, which are certainly more complex and nuanced than can be reflected in a short questionnaire. Moreover, the views expressed are attributable only to the respondents and not to their institutions. Nonetheless, in spite of this cautionary premise we believe it is interesting to report the main points in quantitative terms, in order to set the stage for the rest of the paper.

Starting with the structure of economic research, we note that there is no separate research department in a majority of the central banks in the survey (24 out of 32; see Table 1). Central banks having a separate research departments mentioned historical reasons and the need to shield researchers from short-term assignments and make them focus on long-term projects as the main motivations for this organisational choice. Economists are evaluated, among other things, according to their academic publication record in 75% of the central banks with a separate research

---

[1] The full text of the questionnaire is available from the authors upon request.

department and in 54% of the others; the share of economists with a PhD is 63% in the former (research department only) and 39% in the latter. The approximate time allocation of economists also differs significantly across the two groups; while in the first group economists spend on average 46% of their time in free research and 22% in policy work, the proportions are practically inverted for economists working in central banks without a separate research department. From this evidence we might conclude that having a separate research department is effective in shielding research economists from short term assignments, however positive or negative this might be for monetary policy.

A large majority of respondents agreed or very much agreed that economic research should aim at improving policy making decisions (see Figure 1). On the question of whether policy-makers need a broader range of skills compared with academics (see later in the paper), opinions were more divided, although the number of those agreeing appears to be larger (Figure 2). Coming to the effectiveness of the peer review in the economics profession for policy making, results were (quite surprisingly for us) quite evenly balanced, with the sample almost evenly split between those who see the peer review more positively or more negatively (Figure 3). Also quite surprisingly, the significance bias and the publication lags were not seen as the main problem with the peer review process; the choice of topics that are too detached from current policy issues was seen as the most relevant by most (Figure 4). In particular, one central bank noted that *"the academic research agenda is not the same as the central bank's research agenda"*. Another central bank highlighted a *"general reliance on formal techniques and bias against more discussion-oriented papers"*.

One important element of interaction between researchers and policy makers is represented by quantitative models, in particular DSGE models. Therefore, central bank staff was asked to evaluate main benefits and risks of current generation DSGE models. Among the perceived benefits, there was wide agreement on the models' role in sharpening intuition. Significant less agreement was received by the models being a repository of knowledge on the economy, putting discipline in the forecasting process, and in particular – and quite tellingly – the models' use to fit and forecast (Figure 5). Two central banks mentioned the benefits of having a *"common language"* through the use of DSGE models, another emphasised the use of models as *"story telling device"* and still another that these models are a way to implement advances in macroeconomics. On the whole, this seems to indicate that central bank staff still see DSGE models as a tool to conceptualise problems and aid intuition, and not (yet) as a way for fitting the data and forecast well out of sample. Among the risks related to DSGE models, that of "crowding out judgement" was generally not seen as very significant. Crowding out of other forms of research, difficulty to understand the models and "illusion of controls" were somewhat more prominent, although not very much so. One central bank

mentioned that the models are a *"too narrow view of the world"*, while another highlighted the risk that practically all central banks are using the same type of models the world over. Finally, central bank staff was divided on whether DSGE models have become too complicated and intrasparent and on whether alternative approaches – such as behavioural economics – should be given more importance in central banks. One central bank representative wrote that current generation DSGE models remain *"business cycle models rather than monetary models"* and have therefore been of little use with respect to financial intermediation and as a guide to unconventional monetary policy measures during the financial crisis. The same central bank representative indicated that if anything DSGE models are too simplistic, not too complex.[2] Another central bank representative expressed however a *"preference for small DSGE models"*. Despite these differences, a clear majority answered positively to the second question, indicating that there is some appetite to expand the tool box of policy makers in central banks.

3. Research and policy work: how different are they?

In a survey of the role of research for policy work, the first question that needs to be addressed is what makes them different.

At first sight, the difference between researchers and policy-makers may look very thin. One could note for example that many prominent policy makers were also prominent academics: Prof. Bernanke, Prof. Blinder and Prof. Mishkin at the Federal Reserve Board, Profs. Fischer, Rogoff and Blanchard at the IMF, Prof. Stiglitz at the World Bank, Prof. Svensson at the Swedish National Bank, Prof. Issing, Prof. González-Páramo and Prof. Papademos at the ECB, Prof. Buiter at the Bank of England and at the EBRD, Prof. Goodhart at the Bank of England, and Prof. Weber at the Deutsche Bundesbank to take but a few examples, have all been excellent academics before turning to policy making circles. Conversely, several ideas put forward by policy makers have triggered extensive analysis among academics. There are several examples of this, including Governor Bernanke's "saving glut hypothesis"[3], the "Laffer curve"[4], the "Lawson doctrine", the "Greenspan-Guidotti rule" of international reserve accumulation[5], as well as various statements by Chairman Greenspan (in particular his famous 1996 speech on "irrational exuberance").[6] Conversely, several expressions coined by academics have percolated to popular culture; one could mention for instance

---

[2] In particular, it indicated the field of expectations formation and heterogeneity as relevant in practice for central banks, e.g. to study how inflation expectations are formed.

[3] "The Global Saving Glut and the U.S. Current Account Deficit", Remarks by Governor Ben S. Bernanke at the Sandridge Lecture, Virginia Association of Economics, Richmond, Virginia, March 10, 2005.
http://www.federalreserve.gov/boarddocs/speeches/2005/200503102/.

[4] Interestingly, Arthur Laffer, who was a member of Reagan's Economic Policy Advisory Board (1981-1989), does not claim credit for what is known at the Laffer curve but says that he read it in J. M. Keynes' "General Theory" (1936).

[5] See discussion in Bussière and Mulder (2001).

[6] See in particular the review of the literature in Leroy (2004).

the expressions "zero sum game", "prisoner's dilemma" or "win-win" situation coming directly from game theory, the notions of credibility and time inconsistency, which were given prominence in articles by Kydland and Prescott (1977). Clearly, researchers already have many occasions to interact with policy makers, for instance at the occasion of international conferences where both are invited, through various consultancy and visiting fellowship programs, or, less directly, by writing op-eds in the media and giving interviews. In the other direction, most policy institutions have training programs for their economists (like summer schools) and/or allow them to take unpaid leaves to visit a faculty department. Another point to highlight is that prominent researchers and policy makers often graduated from the same universities.[7]

However, there is also a strong feeling in central bank quarters that research and policy work are somewhat different, and they are.[8] The policy-maker is essentially a multi-tasker, since it is efficient that in any institution the decisions are ultimately taken by the same people, in order to preserve the coherence of the whole, rather than by the persons who are most knowledgeable and specialised in a particular topic. The policy-maker therefore operates at the "extensive margin", jumping from one topic to the next. The researcher, on the other hand, is predominantly working on a narrower field at the "intensive margin". Another obvious difference is that policy makers are eventually responsible for their decisions and their impact on the real world; the answers to our small questionnaire clearly indicated that, for central bank staff, research should be useful for policy-making. We shall illustrate how these fundamental differences are manifested by looking at four key dimensions.

3.1. Complexity vs. simplicity

One important aspect of the difference between research and policy work is related to the complexity of the job. However, and in the same way as in the medical, scientific and other professions, it is not clear whether the job of researchers (specialists) or of the policy makers (generalists) is more complex. On the one hand, researchers seem to handle more complex material than policy makers, simply because they use more mathematics. Indeed, the Nobel Prize in economics was attributed to several researchers with a very strong background in mathematics (e.g. John Nash in 1994). Policy makers, by contrast, tend to make a less frequent use of mathematics. This is partly reflected in the type of studies that researchers and policy makers follow in many countries. Top academics typically need to study a lot of mathematics, while many central banks recruit economists based on competitive examinations including also law and accounting (however,

---

[7] A famous example is the MIT Economics Department 1977 class (Rogoff, 2002).

[8] Indeed, as we have seen, in 8 of the 32 central banks that we surveyed research and policy-making economists work in altogether different departments. This is also the case at the ECB.

as mentioned above, central banks and international organisations increasingly recruit people holding a PhD in economics).

On the other hand, the idea that researchers deal with more complex topics than policy makers is actually paradoxical. Indeed, the real world is in many respects more complex than the models designed by academics: while academics have the luxury to make simplifying assumptions, policy makers often need to confront themselves with the full complexity of the real economic world and its many unknowns. One could follow Donald Rumsfeld by stating that policy-makers have to deal with both "known unknowns" and "unknown unknowns"; academics have arguably to cope almost exclusively with the former type. Indeed, Nobel laureate Robert Solow believes that the task of researchers is to *simplify* things as much as possible.[9] Another important element to consider is the fact that researchers can specialize in a given topic, whereas policy makers often have to be knowledgeable in a wide range of topics. To take an example, central bankers recently had to learn a lot of things beyond their traditional macroeconomic skills, such as financial supervision, housing markets, the insurance sector, etc. In the corporate world, the most generalist job, the CEO, is considered the most complex. Based on this, it is therefore unclear whether complexity really is a useful criterion to disentangle research from policy making work.

3.2. Language and communication

Another important difference between policy makers and researchers is the fact that they seem to speak different languages. Policy makers, being less specialized and more open to the external world, cannot speak too much in code words. Academics even *have* to do it, in order to be precise and to increase efficiency in scientific research. This is related to the issue that policy makers and researchers have different publics. Policy makers need to communicate with other policy makers, with the general public and with business people, who may not know in detail the day-to-day intricacies of central banking. Researchers, by contrast, can exclusively communicate with other researchers if they want to. As a result, speeches pronounced by policy makers can typically be read by many people who do not have a background in economics, while advanced textbooks and research articles can sometimes only be understood by a few. Imagine, for example, if policy-makers expressed themselves like the *incipit* of an article chosen at random in the latest number of the journal *Economic Theory*:

---

[9] *"My general preference is for small, transparent, tailored models, often partial equilibrium, usually aimed at understanding some little piece of the (macro-) economic mechanism"* (Solow, 2008).

"We provide sufficient conditions in finite-horizon multi-stage games for the value function of each player, associated to extremal Markov perfect equilibria, to display strategic complementarities and for the contemporaneous equilibrium to be increasing in the state variables."[10]

Policy-makers, therefore, have to learn to speak in plain English (or German, French, and so on). This statement, again, needs to be somewhat qualified. First, policy makers can sometimes be difficult to understand too (the famous statement by Chairman Greenspan comes to mind: *"I guess I should warn you, if I turn out to be particularly clear, you've probably misunderstood what I've said"*). Second, expressions designed by researchers have made their way among the general public (a "win-win situation", the "comparative advantage", the "Greenspan put", an outlier, the "Laffer curve", etc), suggesting that economists have had some success in teaching their language to the general public. One should also say that academic economists have managed to write popular books and articles, who also contributed to the fame of their authors (one can mention for instance Prof. Stiglitz' "Globalization and its Discontents", Prof. Krugman's op-eds, Prof. Friedman's "Free to Choose", or Prof. Levitt's "Freakonomics"). Sometimes, however, differences in the terminology between researchers and policy makers can lead to strong misunderstandings. This is the case for example for the word "elasticity": the precision of the mathematical language allows to mention whether one is talking about a conditional elasticity or not; but again, policy makers holding a speech could add the adjective "conditional" or the expression "ceteris paribus" when needed; the reason why they do not always do it is likely related to the fact that they generally address a specific public, who is not used to such concepts.

3.3 Personal skills

3.3.1 General vs. specific and time horizon

In a way, policy makers are a bit like general physicians and researchers can be compared with medical specialists (say cutting-edge oncologists), who are often more involved in medical research. Policy-makers, like general physicians, need to know a lot about many fields and therefore cannot be at the cutting-edge of any specific field, which is why they need the support of the specialists. Second, policy-makers and physicians often concentrate on finding solutions to short to medium term problems: in the case of physicians, this implies curing a patient from an illness, while for policy-makers this could mean forestalling a recession or an increase in inflation. Academics and medical specialists, on the other hand, tend to concentrate on problems that could *one day* lead to the solution of practical problems, but have no immediate benefits. The time horizon, therefore, is quite different. Both academics and medical specialists have to strive for clarity and the highest

---

[10] Vives (2009). Actually, it is a very interesting article.

scientific standards; policy makers and general physicians, by contrast, do not have the luxury to wait for full clarity and need to act on incomplete information. This sense of priorities is conveyed very clearly by Issing (2008), who recalls the internal debates on the (future) monetary policy strategy at the ECB in the summer of 1998 and explains how these debates differed from academic seminars: *"Firstly, time is short and we need to reach a conclusion soon. Secondly, we need to realise that the success –and, even more so, any failure– of the ECB's monetary policy will have very real repercussions for a huge number of people"* (p. 184). Interestingly, Mankiw (2006) uses a different metaphor, which aims however at highlighting somewhat similar points: he compares policy makers to engineers (aiming at solving concrete problems) and researchers to physicists (aiming at developing theories).[11]

Partly as a result of the above differences, researchers and policy makers often tend to have different personal skills. As a general statement and with different shades according to the degree of responsibility, in our view policy makers appear to need a broader range of skills, while researchers need to excel in a particular field. To take an extreme example, "country desk" economists need to know not only standard macroeconomics and econometrics (particularly for forecasting purposes), but also extensive knowledge of law and of institutional arrangements in the country they monitor, so-called "inter-personal skills", the ability to work under tight time pressures, familiarity with databases and data providers, foreign languages, and so on. For research-oriented economists, on the other hand, breadth of awareness is normally less important than the ability to concentrate almost obsessively on a single problem and persevere in the face of setbacks. As the results of our survey of central bank staff shows (with several respondents not fully agreeing to the statement that "Policy makers need a broader range of skills"), this view is not uncontroversial and in any case differences should not be overstated in practice.

3.3.2 Communication skills

Coming back to the comparison with the medical profession, policy-makers as well as general physicians need to continuously interact with laymen, which requires special communication skills that specialists do not need to possess; on the other hand, the specialists need to develop demanding communication and persuasion skills targeted to convincing their peers, on which their career and funding ultimately depends.

We feel that the need to build consensus and convince others is more important for policy makers than for academics. It may be perfectly possible for a researcher (with tenure of course) to isolate

---

[11] The special case of monetary policy seems to give rise to yet another metaphor, comparing central banking to an art. This is the title of the book by Hawtrey (1962) and the idea defended by Niehans (1978): *"However far monetary policy may progress, central banking is likely to remain an art"* (quoted in Issing, 2008). For a discussion of the ECB monetary policy strategy see also Issing at al. (2001).

herself from the rest of the world. Policy-makers, as we already noted, need to convince a lot of people. Central bankers, for example, need to convince the general public and economic agents that their policy is credible. A good policy maker should therefore be communicative, persuasive, good at negotiations and at building consensus. Yet, again, the difference is less than clearcut; prominent researchers also had to convince the rest of the world that their theories were right. This was true in astronomy at the time of Galileo, but one can also notice that eminent economists such as John Maynard Keynes or Milton Friedman had to be particularly persuasive to gain credibility at the start of their careers.

3.3.3 Human capital

A final and important issue to address when comparing the personal skills of researchers and policy makers is that of human capital development for economists working in academic and research institutions. Clearly, the choice to work for the former or the latter is endogenous and reflects a given person's skills and preferences. But even if two identical PhD economists were randomly assigned to a policy institution and a university, it is also clear that their skills would rapidly evolve in different directions.[12] In the case of the university teacher, her economic know-how is likely to be reinforced, mostly through active research and conference participation. Teaching is also an important component of keeping up human capital, especially if it is at the graduate level. Also the economic know-how of policy-makers will be reinforced by learning by doing, but the skills will probably develop in a different direction. Let's see some of them.

First, policy institutions need data collection; it is indeed crucial for decision makers to rely on up-to-date and reliable information. At first sight, this task seems to have a very low analytical content, especially compared with the daunting task of setting up a new model. However, it does take a lot of economic knowledge to do it properly. For a given concept that features a model ("the interest rate", "the exchange rate", etc), there exists a variety of underlying series (nominal or real, at which maturity/frequency, which market, which weighted average, etc). Choosing among these different series, e.g. to illustrate a given point, actually requires extensive economic knowledge and could not be done without a substantial background in economics. In turn, experience with data and databases can be seen as one important component of human capital, leading e.g. to in-depth empirical work.[13]

---

[12] To be explicit, we consider here a policy department within a policy institution (e.g., a country desk or a policy position in a functional department). The specific case of research departments is left aside for the moment. We also do not consider the case of an economist ending up in a secretarial or administrative function.

[13] We note that senior academics appear to have much less direct contact with the data collection than (even rather senior) central bank staff, though this surely reflects the more limited availability of research assistants in central banks – a problem that we feel is often underappreciated.

Second, there is another form of know-how that is the bread and butter of policy institutions and can be labeled "institutional knowledge". Two examples can illustrate this point. First, knowledge of how banks operate was (we are tempted to write: would have been) crucial in the run-up to the financial crisis. This requires, among others, frequent contacts with private sector participants, as well as the careful reading of private bank reports and of the financial press, all activities that take a lot of time and are unlikely to improve human capital (if "human capital" is understood as the knowledge that is typically taught during a PhD program). The second example considers a typical desk economist who monitors and forecasts activity in a given country. Then again, a necessary part of the job will consist in following the policy measures implemented in this country (e.g., fiscal plans or structural reforms).

A third type of assignment that typically characterises economists in policy institutions is related to macroeconomic forecasting. Of course, econometricians in the world indirectly contribute to forecasting activities when they develop models, but this is very different from regular forecasting activities, which primarily imply using, rather than developing models.

A fourth type of activity that is specific to economists in policy institutions is the drafting of speeches and official documents (like reports, bulletins or newsletters). Such documents are generally aimed at the general public, such that they require very different skills from research work. Such skills include drafting competencies and synthesizing existing work, rather than developing new models. One could therefore say that drafting speeches and reports uses a lot of human capital (such speeches are better drafted by people who know models very well) but does not always contribute to developing or even maintaining human capital, at least in the way normally emphasized in formal education.

Overall, therefore, one can conclude that policy-making and research activities rely on very different skills: researchers need essentially to have strong mathematical skills and the ability to develop models and solve them, whereas policy-makers need to have strong drafting skills, a deep knowledge of institutions, the ability to speak in public and convince an audience and a lot of experience with data work. Yet, the difference should not be exaggerated: researchers would probably do better with the skills that are traditionally attributed to policy-makers; the models they develop are also more relevant if they are built to address the concerns of policy makers. Similarly, policy makers who can build on rigorous economic knowledge are probably better equipped to tackle policy challenges. As a final comment, one could add that the skills traditionally attributed to policy-makers are best learnt on-the-job (they are not included in typical PhD programs), whereas the skills traditionally attributed to academics are more difficult to learn on-the-job. This may be

why one sees a lot of academics and researchers moving to policy institutions and more rarely the reverse.

3.4 Teamwork and career development

One main difference with other professions such as in the medical sphere is that policy-makers, unlike most general practitioners (there are exceptions), have to work in teams and even lead teams of specialists. Specialists (in economics), on the other hand, often work alone or in small groups, not organized hierarchically. This, again, is another difference with medicine and "hard" sciences, where specialists work in labs, led by a more senior researcher. One could say, therefore, that although the situation is the opposite of medicine, there is indeed an important difference between policy makers and researchers in economics, in that the former need to work in teams whereas the latter generally do not.

There are several consequences deriving from this fact. The first one is that there is a risk of 'group thinking' by central bank career staff (see Csajbok, 2008) who often share a common background and mentality. In a recent rather contentious piece, David Blanchflower (a former member of the Bank of England MPC) describes how group-think at the Bank of England has made the bank miss the financial crisis. From that perspective, the outside view of academics may be quite useful. It is not necessarily the case that academics are less subject to groupthink or other cognitive biases than central bankers proper, but it is likely that at least their biases will be *different* from those of the policy-makers and could therefore represent a useful cross-check. This is in our view an argument to keep their worlds separate, but communicating.

Another consequence that deserves attention is that the relative desirability of the two jobs may be affected by very different work habits. By this, we do not (just) mean working time, salary and dressing codes, but also concerns over the career development and the issue of external visibility. Indeed, one interesting point to note is that careers in academia and policy institutions seem to go in the opposite direction: in academia, younger people are often viewed as able to deliver more original (and therefore, the argument goes, better) research, whereas in the policy world, older economists are supposed to know better, because they are more experienced. It can also be said that policy institutions tend to provide a safer environment than academia: in the latter, especially for young researchers, an editorial decision can have huge consequences for tenure and for the rest of the career. Another important difference relates to the issue of visibility (and, therefore, marketability): research papers are published under the name of the author, whereas official reports are generally published under the name of the institution. Such differences also suggest that the thought experiment we outlined above, in which two economists were randomly assigned to

academia and to a policy making institution, is unlikely to be met in practice: the decision to go for one or the other is likely to involve a lot of self-selection.

4. What is policy relevant research?

The need to bring researchers and policy makers closer together raises the question of what constitutes *policy relevant* research. Lucas (1980) defines theoretical research as follows: *"One of the functions of theoretical economics is to provide fully articulated, artificial economic systems that can serve as laboratories in which policies that could be prohibitively expensive to experiment with in actual economies can be tested out at much lower cost"*. This question is particularly important for policy institutions such as central banks, ministries of finance or international organizations, many of which having a research department (or research units among functional departments). Crisis times suggest, both, how important it is for policy makers to rely on advances from economic research, and how difficult it is for researchers to bring a relevant message for policy makers. Few economists not only predicted, but also conceived the possibility of a financial crisis of the magnitude of the 2007-09 credit crisis; indeed mainstream economics had no model or conceptual framework ready to drive policy makers through it. We should emphasize that this sorry state of affairs was, in our opinion, not only a fault of the academic community, but arguably also of an insufficient stimulation on the side of policy-makers.

Against this background, what kind of research do policy-makers need? One popular concept that is sometimes used in policy institutions is that of "directed research". This refers to a situation in which the researcher based in a policy institution receives a request to work on a particular topic of direct relevance for the institution. The alternative approach is more incentives-based: freedom is left to individual researchers to pursue their own interest in the expectation that both career prospects and the desire to be relevant to the institution will lead them to concentrate on the questions the policy-makers are most interested in. The jury is still out on what is the best approach in order to produce policy-relevant research.

The policy-relevant work is often of an applied nature. Typical examples include designing an early warning system of currency crises, estimating forecasting models, setting up macroeconomic models for simulation purposes, etc. As a result, the work that is typically done under that framework tends to be more difficult to publish in good journals, which generally publish work that is general to *many* situations (i.e., improving the methodology) rather than specific. This often creates a dilemma for economists working in policy-making institutions, i.e. between the policy

impact of research and the internal career chances on the one hand, and their chance of success in the academic publishing market on the other hand.[14]

4.1 Does peer review provide the right incentives for policy relevant research?

In economics a two-step system has established itself in recent times, in which new research is disseminated through the publication of working papers in the internet and subsequently quality-certified, often many years later, by economics journals. Since policy-makers want to be sure about the quality of a certain analysis and often have no time to check it by themselves, what is often decisive for the acceptance of a certain idea, even in policy-making institutions, is often (though not always) the second step of the process. This creates, however, a number of problems which are usually not enough appreciated.

We have experienced very different attitudes towards top-tier publications amongst central bank staff who are not directly involved in research. On the one hand, a common attitude is to dismiss the importance of the publication outlet right away, not appreciating the amount of effort that is typically involved in bringing a paper to publishable format, which should tell something if anything in terms of perseverance and stamina of those involved in the publication process. On the other hand, we have also seen at times an uncritical acceptance of the contents of the same top journals as if what is in there is automatically true. We don't find either of these attitudes very healthy and we endeavour to set the record straight here.

We state from the outset that we believe that a peer review system should be fundamental not only for the scientific but also for the policy community. We are actually in favour of extending the concept of double-blind peer review also within policy-making institutions (whenever possible, of course). However, we are not sure that the current peer review in economies optimally serves the needs of policy-making, for four reasons.

First, by their nature, economics journals must have many professional readers and cannot be limited to a few, even if influential, policy-makers. They therefore tend to favour papers that propose general solutions to many problems, not narrowly focused analyses. In the long run, improvements in the economic methodology clearly are relevant for policy making; for example, the design of a new econometric estimator can ultimately feed into applied work that can in turn be used by policy makers. This would call for publishing primarily papers that present improvements in the methodology. Yet, in the short to medium run this editorial policy would also run the risk of

---

[14] One of the authors of the present paper was told by the editor of a good economics journal that the journal as a rule does not publish papers on individual countries (apart from the US, we assume). This already rules out the bulk of the work of 'country desk' economists in main international institutions. We also repeatedly heard the story of European and Canadian economists publishing empirical papers on US (rather than European or Canadian) data in order to publish in top tier journals.

keeping research very far from policy making. The question here is whether macroeconomics really is a science, such that one can think in terms of "fundamental" versus "applied" research (Barnett, 2006), and whether a research article can be an end in itself. Barnett (2006) mentions this quote by J. Heckman, which illustrates the issue very well:

> *"In economics there's a trend now to come up with cute papers in an effort to be cited as many times as possible. All the incentives point that way, especially for young professors who seem risk-averse rather than risk-taking after they get tenure. In some quarters of our profession, the level of discussion has sunk to the level of a New Yorker article: coffee-table articles about "cute" topics, papers using "clever" instruments. The authors of these papers are usually unclear about the economic questions they address, the data used to support their conclusions and the econometrics used to justify their estimates. This is a sad development that I hope is a passing fad. Most of this work is without substance, but it makes a short-lived splash and it's easy to do. Many young economists are going for the cute and the clever at the expense of working on hard and important foundational problems."*

It is true that there are a few academically reputable economic journals with a more policy-relevant orientation (such as Brookings Papers on Economic Activity, Economic Policy, International Finance, Oxford Review of Economic Policy), but their number is tiny compared with that of the traditional economics journals, where policy-relevant but not methodologically cutting-edge research has a hard time finding a proper space. As noted above, our survey of senior central bank staff indicated the "Topics too detached from policy" as the area with the largest room for improvement with a view to making peer review more functional for policy making.

Second, another difficulty in bringing together research and policy work relates to the publication lags, which are now too long for policy purposes (Ellison, 2007). This difficulty is inherent to other kinds of science (though peer review appears to be quicker in medical and physical sciences than in economics), where new theories need to be peered reviewed and subject to a lot of checking before being validated and used for policy purposes, but it makes applications to the field of economic policy more difficult. Nonetheless, there is a strong sense amongst many economists that the review and publication lags in economics are excessive; it is not unusual to wait for several years (sometimes even a decade) before a good paper is published. If one thinks that, for example, the four "Annus Mirabilis" papers by (the academically outsider) Albert Einstein (one of which laying the ground for the Theory of Relativity, another one later decisive for Einstein's Nobel Prize in

physics) were written, reviewed and published in the same year (1905), the oddity of this situation appears quite evident.

Third, there are also imperfections in the peer review process, but these are often under-appreciated, especially by policy-makers who have little time to carefully read scientific papers directly. First, there is evidence that many very good papers have been rejected (see Gans and Shepherd, 1994). Second, there is generally an exaggerated perception of the best journals, as if publication in a top journal were automatically a guarantee of truth. Just consider that, according to one research, many papers published in top journals do not receive any citation, while several papers published in "lesser" journals do (Oswald 2006).[15] It is useful to quote the abstract of this research in full:

"Scientific-funding bodies are increasingly under pressure to use journal rankings to measure research quality. Hiring and promotion committees routinely hear an equivalent argument: 'this is important work because it is to be published in prestigious journal X'. But how persuasive is such an argument? This paper examines data on citations to articles published 25 years ago. It finds that it is better to write the best article published in an issue of a medium quality journal such as the OBES than all four of the worst four articles published in an issue of an elite journal like the AER. Decision-makers need to understand this." Oswald (2006).

It is also the case that empirical work reported in prestigious journals has subsequently not been validated, showing that journal publications cannot by any means be considered as free of error.[16]

Finally, the issue of "non-results" and the bias of significant results should not be neglected, and it is quite surprising that it was not considered more prominent in our survey of senior central bank staff. In many cases, studies conclude that the results they show are not clear-cut. In these cases, one may raise the questions whether (i) such studies really make a contribution and (ii) they are policy relevant. Concerning both (i) and (ii), one would be tempted to answer "yes". In the first case, a non-result is ethical and should be reported similarly as a clear-cut result; however, one may fear that such papers are not given the exact same chance for publication in academic journals, and in fact there is a widespread perception that this is so, although this is clearly an example of bad science.[17] Concerning (ii), a non-result should be interpreted as word of caution for policy makers and should be important to them. Yet, again, one might fear that this word of caution is not always heard or appreciated. Certainly, the good policy-maker should be well aware of the fact that positive results tend to get more published in economics journals, and that (should he/she use research

---

[15] Wall (2009) raised a similar point more recently, providing additional evidence.

[16] See Dewald et al. (1986), on the duplication of empirical work in the *Journal of Money, Credit and Banking*. They conclude "Our findings suggest that inadvertent errors in published empirical articles are a commonplace rather than a rare occurrence".

[17] See Roberts and Stanley (2006).

findings in policy) there is a higher risk of 'Type II' errors as a result. This problem is of course not specific to economics – it has been recognised and discussed since long in medicine, for example – but it is often not appreciated enough in economic policy-making, where influential academic papers may at times carry an important weight.

Our conclusion, therefore, is that although peer review should generally be seen in a positive light policy makers should also form their own opinion on the research input that is presented to them and maintain a healthy scepticism vis-à-vis academic publications.

4.2 The case *for* research in policy institutions

In spite of these difficulties, it is striking how much research is done within policy institutions. Central banks and international organisations produce indeed a lot of research output. Aside from quantity, one can also note that many papers produced within the central banking community were published in excellent journals and are very often quoted in the same journals.[18] One explanation for this is that some questions really need a lot of research to be answered. Wyplosz (1998) lists competence among the qualities a "policy adviser" should have and says *"All advisers know that first year textbook economics is the most that is needed in nearly all instances. Equally important is to know what is not in textbooks: what are the controversies, what previously popular idea has been proven wrong and why, which ideas are currently under study and whom to ask to know where to stand. The closer are advisers to frontline research, the more they know these things."*

We can think of at least five reasons why research is indispensable for policy work. The first one is when policy decisions involve mechanisms that are too complex to be handled by any human brain: in that case, models –a simplified representation of reality- can help think through the complexity of the real world. In addition, one can say that models can act as an efficient way to discipline policy arguments, by using a more precise language and clearer assumptions.

A second, related, advantage of economic models is that they allow quantifying economic effects, which a more literary form of reasoning does not allow. This is the case, for example, of the effect of a given interest rate increase on the economy, which would be impossible to gauge in the absence of a model. This feature of model is particularly useful when different effects may take place: without a quantification of these effects, it is difficult to see which one would prevail, such that even the direction of the response to a given shock is uncertain.

The third reason why sophisticated models cannot be simply ignored relates to the Lucas critique, which states that the parameters that policy makers sometimes take as given are not policy

---

[18] One could also mention prominent papers that were written by academics while they were visiting policy institutions, such as the paper by Krugman (1979) on currency crises while he was an intern at the Fed.

invariant, i.e. they may change depending on what policy is being conducted. A corollary of the Lucas critique is Goodhart's law, which states that once an economic variable is chosen as a policy target, it will no longer be useful as an indicator. If the Lucas critique and its corollaries are true, policy makers cannot just act based on rules-of-thumb or by tracking stylised facts and recent indicators, they need a model sophisticated model that takes into account the purposeful reaction of economic agents to policy decisions. There is evidence that policy-makers are fully aware of the Lucas critique and take it into account when they make policy decisions. For example, Issing (2006) specifically mentions the Lucas critique as one of the key elements that he took from academia when he moved to policy: *Like all of us, as an academic I was deeply impressed by the convincing logic of the famous Lucas Critique. As a central banker, it influenced my reflections on the potential implications of structural breaks in the context of German unification, and even more so at the start of European Monetary Union. The monetary policy strategy of the ECB – among many other results of research – reflects this important contribution to assessing the high degree of uncertainty under which policy has to be conducted*".

The fourth reason why economic research within policy-making institutions may be worthwhile is that central bank researchers, as argued earlier, typically posses a very good knowledge of data and institutions, which is not easy to obtain for an outsider and which may make a crucial difference to the results, especially in empirical work. For an illuminating example of this type of applied knowledge, see Thornton (2001)'s rebuttal of Hamilton (1997)'s celebrated analysis of the liquidity effect in the fed funds market. Hence, it can be argued that the economics profession benefits from the participation of researchers who are particularly strong on data and institutions.[19]

Fifth, both academics and policy-makers may be subject to the excessive influence of "stories", e.g. popular stories that often come up but have only an anecdotal basis. To the extent that the stories are not the same for the two groups, the interaction between academics and policy makers can foster a healthy scepticism towards them. Researchers are particularly well positioned to "deflate" popular stories of the economy, and policy-makers should listen to them. For example, many such stories have circulated in recent years in relation to globalisation (e.g. global liquidity, the impact of globalisation on inflation, and so forth) and academics have played a very useful role in "separating the wheat from the chaff".

Aside from the above cases for the use of economic models in policy making, there are less direct – but powerful- benefits of using research within policy institutions. One is the issue of credibility: often, institutions need to establish a track record of publications to show that their policy rests on

---

[19] The authors frequently encounter empirical studies and even more press articles (even by academics) which mis-understand crucial aspects of the institutional set-up in the euro area.

sound grounds. Another relates to the human capital of economists working in central banks, which may deteriorate quickly if economists are not given incentives to publish. As discussed above, some of the tasks performed on a daily basis in a central bank may seem unrelated to research activity, but require in fact advanced knowledge in economics. This is the case of speech writing, of the interpretation of data releases, of macroeconomic projections, and even of the computation of official statistics. Giving economists in policy institutions the incentive to publish economic papers ensures that their human capital does not fade out; it is also likely to attract excellent economists (as discussed above, career considerations naturally self-select PhD students into choosing academia or policy institutions). Above all, without active participation in research there is no enough incentive for economists to *know* cutting edge research, which may imply that the institution as a whole relies more and more on outdated knowledge.

4.3 Is there a case *against* research in policy institutions?

One of the authors was asked several years ago by a colleague, coming originally from the Deutsche Bundesbank, how it is possible that the Banca d'Italia is a beacon of economic research but failed to maintain price stability before EMU, in contrast with the Deutsche Bundesbank which, at the time at least, had invested much less in economic research and modelling. Is our colleague right? Should one be suspicious about the use of economic research as is done nowadays, based on models?[20]

There are indeed several examples of "model failures", i.e. instances where models that were used to take policy decisions have been rejected by the data; Colander et al. (2009) even speak of 'the use of models as a source of risk'. It is difficult of course to evaluate models, but instances where models made a clear prediction about something that proved to be untrue are often used as a way to question their relevance. The use of models could arguably create the same 'illusion of control' in central banks that the use of mathematical models has encouraged within private financial institutions in the run up to the financial crisis. It is therefore important that the policy maker is a sophisticated and not a naïve user.

In a recent contribution reflecting on his career, Bruce Benson writes that *"I quickly became aware of the fact that mathematical theories can be manipulated to predict practically anything that the theorist wants to predict, simply by changing some assumptions. [...] policy recommendations based on such mathematically complex but economically-simplistic model-building abound."* (Benson 2009). Later, by describing his shift from mathematical game-playing to more empirical research, he writes that *"while this anti-policy research was quite interesting, it also was very*

---

[20] One answer to our colleague is that price stability may have not featured very high among the Banca d'Italia's objectives due to political reasons. Even so, this demonstrates that the ability to create and manipulate models is not a warranty of good macroeconomic outcomes.

frustrating. First, it revealed that econometric analysis is just as easy to manipulate as mathematical model-building. (…) Clearly, policy advocacy based on statistical analysis is just as suspect as policy implications drawn from math models."

There are several examples of areas where using models may have created more harm than good. For instance, one area that seems to create particular difficulties for researchers is that of exchange rates. First, exchange rates are very difficult to predict.[21] Second, the notion of equilibrium exchange rates is very controversial among economists. This notion is not only used for forecasting purposes, but also in the case of countries that decide to peg their exchange rates or join a currency union. A wrong evaluation of equilibrium exchange rates can lead to serious economic distress, as can be seen from the example of the 1992 EMS crisis that led to UK to devalue very sharply. In fact, many empirical models also seem unable to predict currency crises out-of-sample.[22] Another example of "model failure" relates to the effect of free trade agreements (FTA). In particular, existing models have underestimated the trade creation generated by NAFTA.[23] Such failure is disturbing, because it considerably affects the cost/benefit analysis that prevails when negotiating free trade agreements: if model results are biased downwards, this may artificially tilt policy makers and the public opinion towards rejecting such agreements.

One particularly striking example of "model failure" that is worth considering is related to the recent financial crisis. This crisis, like the above mentioned currency crises, was not predicted by a majority of economists. Not only this, but models also seem unable to explain the suddenness and the magnitude of the crisis. This, in turn, raises questions about the ability of current research to bring relevant information to policy makers. Although we do not necessarily concur with him on all his views, Buiter (2009) has a point when he states that *"Research tended to be motivated by the internal logic, intellectual sunk capital and esthetic puzzles of established research programmes rather than by a powerful desire to understand how the economy works - let alone how the economy works during times of stress and financial instability. So the economics profession was caught unprepared when the crisis struck"*.

An obvious counterargument to these considerations is that almost *no one* really predicted the crisis, be it based on models or not, and there is to date no alternative to models for addressing such questions. As argued in Krugman's book *The Accidental Theorist*, economic modelers and rule-of-thumb policy makers may be equally surprised by a crisis, because there will always be some elements of surprises in the economy that will be overlooked in economic models. However,

---

[21] The classic reference here is Meese and Rogoff (1983).

[22] See Berg and Pattillo (1999) for a critical assessment; Bussiere and Fratzscher (2006) provide however a more optimistic view, based on a different model.

[23] See the informal discussion by Tim Kehoe: http://www.econ.umn.edu/magazine/MinnesotaEconomics1105.pdf

Krugman argues, models allow those who operate them to understand more quickly what is going on, if a particular model failed to predict a crisis, this may suggest that the main mechanism that is featured in this model is not the one that triggered the crisis.

4.4 An assessment

In the end, the answer to the question of what constitutes policy relevant research is largely subjective: even though one can discuss the pros and cons of models –or of a particular class of model- this leaves open the question of the extent to which policy makers really base their decisions on models. In other words, how can one claim that research had any influence, good or bad, on policy? One could argue that policy makers do not really need models to take decisions; in many situations, common sense suffices. Very often, in fact, researchers formalized ideas that policy makers had already thought about. This is the case, for instance, of endogenous growth (policy makers had the sense that they could stimulate growth by building up capital long before Paul Romer developed his models). The area of central banking also offers examples where the true influence of research over policy making is heavily debated. Thus, Blinder (1998) argued that central bankers did not really follow prescriptions by neo-classical economists *"Instead, they brought inflation down dramatically by purely discretionary policy decisions. As in the Nike commercial, they just did it."* [24] There is therefore reason to doubt that the key achievements of central banks worldwide (the Great Moderation and the Great Disinflation) were the result of economic research *per se*, much less of current generation macro models.[25] Arguably one of the best performers among central banks, the Deutsche Bundesbank, was *not* an avid user of DSGE models, and perhaps of models in general, though it certainly espoused the ideas of the rational expectations revolution about the neutrality of money.

Others, such as Chari and Kehoe (2006), bring a more optimistic assessment on the usefulness of research for policy making. Chari and Kehoe mention a number of areas where economic theory undoubtedly influenced policy, including central bank independence, the widespread adoption of inflation targeting as a preferred monetary strategy, tax policy (especially the switch towards consumption and income taxes, over capital income taxes), and the increasing perception of the costs of labour policies that distort labor markets. Their conclusions are unambiguous: *"The day-to-day economic adviser is useful to the extent that the adviser can educate policymakers about trade-offs, but is largely irrelevant otherwise. It is easy to see why those economists caught up in the*

---

[24] This is also confirmed by the empirical analysis in Cecchetti et al. (2007). It could be interpreted as a triumph of the 'Type B' approach of Faust (2005).

[25] Part of the explanation may be related to the fact that, even in the most sophisticated DSGE models, just applying the very intuitive principle of adjusting the nominal interest rate to inflation in a more than proportional way usually delivers welfare outcomes that are close to the first best; see e.g. Schmitt-Grohe and Uribe (2006).

whirlwind of day-to-day policy making miss the dramatic changes in policy that result from slow, secular changes in institutions, practices and mind-sets. The toilers in academe are uniquely placed to develop analyses of institutions and to educate the public and policymakers about economic trade-offs. The essence of our argument is that, at least in macroeconomics, these toilers have delivered large returns to society over the last several decades." Similarly, Walsh (1986) presents a positive assessment of the relation between theory and practice in the field of monetary policy, mentioning progress in the theory and increased convergence in recent decades ("monetary theory has again become relevant for monetary policy").

All in all, the most important element is that the body of economic knowledge is used in the policy-making process. This is where the "policy advisers", i.e. those who bridge the gap between theory and praxis, should intervene. They can do so by formulating messages from economic research in a way that is directly relevant for policy makers. This requires a good training in economics to be able to understand what models are really suggesting. It also takes additional qualities, which are reviewed in Wyplosz (1998). One of them is *"clarity in reasoning and presentation of ideas"*, to be sure that the policy message is not lost in translation. A second quality is *"to recognize the policy maker's need for solutions"*, i.e. the fundamental difference between research and policy work highlighted in the introduction of this paper. Another is honesty: the advice should also highlight the pitfalls in the theory. Wyplosz (1998) mentions this superb quote by US Representative Hamilton *"An economist who wants to contribute to the policy-making process needs to be a good salesman -- but not a snake-oil salesman"*.

5. The use of economic models in policy-making

Our review would be incomplete if we did not address the use of models, in particular what has been emerging as a key tool within central banks, namely Dynamic Stochastic General Equilibrium (DSGE) models, developed by a relatively limited group of specialists with strong skills in computer programming and used by our "general practitioners", the policy-makers. It is increasingly the ground on which researchers and policy makers meet more and more often.

As always, there are those who defend the use of models in policy-making (Paul Krugman's book "The Accidental Theorist" makes the case in favour of them, though he seems to have reconsidered his views more recently) and those who criticize models for being too far from reality, intractable and distracting (as argued in Paul Ormerod's book "The Death of economics"). The terms of the debate are effectively couched by Krugman. On the one hand, the experienced policy makers knows a lot more stylized facts than the typical modeler, who often was absorbed in her model for a long period. This gives an edge to the experienced policy maker "in normal times", because she can rely on the experienced accumulated over the years to predict how the economy will react to a given

stimulus. However, Krugman argues, things change dramatically in crisis times. Indeed, the structural parameters behind the stylized facts memorized by the policy makers may change, thus affecting the relation between variables. By contrast, in Krugman's opinion, the researcher can make the appropriate changes to her model and derive more quickly new policy prescriptions. One key challenge however is that there is sometimes not enough time to do the latter. Ideally, in such circumstances, policy makers should learn from researchers. In the other direction, researchers may need to learn from policy makers what, from their perspective, has changed.

Against this background, it is notable that nowadays most major central banks now use some sort of DSGE model in their policy making process.[26] At the same time, no central bank uses a single model in its monetary policy decisions without exercising some judgment. This raises the question of what the actual role of models in the policy making process is. In this section, we will first review the use of economic models in general, and will then turn more in detail to the New Keynesian models that are currently mostly in vogue.

5.1 What is the purpose of an economic model?

In the traditional view, an economic model is essentially an aid to thinking about problems with economic content. It is conceived as a strategic simplification of the real world in order to shed some light on a particular problem. This is the view expounded, also recently and among others, by some Nobel prize winning economists such as Robert Solow and Paul Krugman. It is perhaps most useful to compare this use of economic models to telling a story, which can be both about the real world as well as about a hypothetical world in which counterfactuals can be made (see e.g. Morgan 2001). Whether a certain story or model is more or less successful depends a lot on whether they strike the right chord in the audience. It can be argued that a good model is not very different from a good movie; neither has to be true, but both have to be believable. As an example which that is quite relevant to the financial crisis, consider the Diamond-Dybvig bank run model, which has even an entry in Wikipedia stating: *"The model shows how banks' mix of illiquid assets (such as business or mortgage loans) and liquid liabilities (deposits which may be withdrawn at any time) may give rise to self-fulfilling panics among depositors."* The Wikipedia summary is, in a sense, exactly what is needed to policy makers to act. The mathematical model is certainly important to check the logical consistency of the argument, but what is important is the end-result in the form of a memorable story. Less memorable models are instead easily forgotten.

If models are to be conceived and used this way, they should have three desirable characteristics. First, they should tell remarkable, easy to remember stories, in the limit being paradoxical or

---

[26] We don't review this in detail; see Tovar (2008) for more details. In the case of the ECB, the main reference model is the New Area Wide Model, which is a DSGE model at its core.

extreme. Second, they should be clearly understandable, if not by laymen, certainly by non-specialist trained economists and policy-makers. Third, and as a consequence to the first two previous characteristics, they should be simple and 'minimalist', stripping reality to the bare minimum. The use of economic models in central banks is moving somewhat away from this traditional view. Models are becoming more complex and less logically cogent, and are increasingly used to fit and forecast, more than to explain and conceptualise.

The shift towards more complex models is mainly a result of stronger computing power and the development of efficient software, in particular DYNARE.[27] Using DYNARE, anybody with a basic knowledge of DSGE models can specify, calibrate and estimate a model with relatively limited effort. The software requires to type in the key equations of the model as they appear on paper, and then to initialise some approximate steady state values. The program is then able to compute impulse responses, counterfactuals, optimal policy, and so forth. The shift from pencil and paper to computer implies that the model developer does not need to understand the inner workings of his model, as he learns about the model behaviour mainly from impulse responses. In modern DSGE models, especially large ones, it is often impossible to say ex ante whether the effect of shock X on variable Y is positive or negative; the researcher needs to look at the impulse response produced by the program to find out. So not only can models quantify effects, but without a proper model it is even difficult to know qualitatively the relation between key variables when different effects play in the opposite directions. The shift towards using models not as an aid for thought but for fitting and forecasting real data is also mainly due to recent developments in software, which allows in particular the Bayesian estimation of DSGE models (more on that later). The need for these models to explain real world data has in turn an impact on their structure, which has to become not only more complex, but also less rigorous in terms of intuition and explicit microfoundations, including a large number of rather ad hoc shocks and frictions.

One aspect that is not often mentioned and is quite unexplored, but that in our view is quite important, is the potential of the DYNARE software as a thinking aid in the traditional way. In the past, economic ideas had either to be expressed in informal terms or derived more formally, and quite painstakingly, with pencil and paper. With the development of DYNARE, even the policy maker who is not at the frontier of research and cannot spend months on a particular idea or project could still find it useful to test a particular idea in a general equilibrium framework, which provides some rigour to thinking. In other words, DYNARE could lower the price of developing minimalist models in the old ways too, and make them more effective and therefore more used. Whether DYNARE will be used in this way, however, remains to be seen.

---

[27] http://www.cepremap.cnrs.fr/dynare/ .

The development of DSGE models is certainly a positive and promising trend in central banks, and the profession has a lot to gain from models that are not only logically consistent but also quantitatively relevant, especially (as will be discussed later) if they will be refined so as to include financial frictions and intermediaries and bounded rationality. However, their widespread use does raise some concerns. One of the most pressing of those concerns may be that DSGE modelling, especially if aimed at fitting and forecasting real world data, 'crowds out' the minimalist traditional modelling that we see as still very useful in a central banking context. This kind of stripped-down, easy to grasp models are still useful to "train the intuition" of policy makers, as Robert Solow put it. This may be an example of Faust (2005)'s 'Type B' analysis – i.e. based on an intelligent and coherent rule of thumb and not on formal optimisation – but we believe that it is still very useful in policy-making.

This is particularly true when there are new phenomena or new aspects of reality that cannot be easily and quickly embedded in the DSGE framework - and the current financial crisis provides plenty of such examples - should require that policy-makers are equipped with the ability and the tools (hence the models) to think rigorously about any issue of interest. At present the development of a fully-fledged DSGE model takes too much investment and time in order to be effective in policy-makers' 'thinking on their feet' (Blanchard 2008). As a consequence, policy-makers' thinking on the issues that DSGE models cannot cover is left without the guidance and rigour of formal economic modelling. It should be clarified that this does not mean in any way that DSGE modelling as currently done in central banks should be discontinued - rather that it should be complemented by more minimalist and flexible forms of modelling. Indeed, central bank staff indicated, in our survey, that "crowding out of other forms of research" is the largest risk of DSGE models; and that "sharpening intuition" is their biggest asset.

There are other concerns surrounding the DSGE models in policy-making that have already been raised in the literature and that are relevant for their use within central banks. In the following sub-sections we describe some of them, in particular (i) whether DSGE models are as structural as they claim to be, (ii) whether they can be meaningfully estimated, (iii) whether they are really preferable to Structural Vector Autoregression (SVAR) models, and (iv) whether and how they can usefully brought at the heart of the policy-making process.

5.2 Are DSGE models really structural?

One major claim of the Real Business Cycle literature first, and of the New Neoclassical Synthesis later on, is that unlike alternative and more reduced form models, models built on explicit microfoundations (i.e. optimising agents and clearing markets) are structural. The term 'structural' is

generally referred to the invariance of the model to policy shifts, namely being immune from the Lucas critique, and more generally being stable over time (or at least not systematically unstable). It is often argued that this is a key advantage of DSGE models over VAR models, in particular, and this feature should make them more interesting for policy-makers.

Despite these strong statements, it is surprising how little evidence there is that DSGE models are 'more structural' than their possible alternatives. Given the difficulties in estimating DSGE models (as we will discuss below), it is still very difficult to conduct a stability analysis on them, or check whether they are subject to the Lucas critique. The few examples that are available in the literature are not very encouraging (Juselius and Franchi 2007; Fernandez-Villaverde and Rubio-Ramirez 2007).

Another (but related) possible meaning of 'structural' is where a model is linked to deep, unchangeable characteristics of human behaviour and activity. A good structural model should therefore go deeper and beyond the description of actual behaviour and explain the inner motivations. Do DSGE models pass this test? It appears that some of them do, at least if they are interpreted in the traditional, minimalist way, namely being used to explain narrowly and single-mindedly a certain phenomenon of interest from a qualitative perspective. If instead they are used to match the data, they can hardly be considered as structural. As argued by Sims (2008) and Blanchard (2008), there is no 'true model of the economy' in which there is a single representative agent, a single good, CARA utility function, and so on. Particularly "un-structural" is the assumption of a representative agent (or of a couple of representative agents, say borrowers and lenders), as emphasised most forcefully by Kirman (2009). The assumption of a representative agent is certainly useful in terms of story-telling (at least to the extent that the policy maker can identify himself or herself with him), but there is simply just too much heterogeneity and incomplete markets in the real world to make the assumption minimally realistic.

There is therefore a measurement error is for the most part in the models, not in the data. In fact, Chari et al. (2008) claim that in current DSGE models the typical structural shocks are not structural at all, and this may be a price to be paid for bringing these models closer to the data.[28] Gordon (2009) goes even further by saying that "*modern DSGE models are littered with contradictions*". Obviously, all models have limitations and contradictions but one should nevertheless be very cautious in drawing the conclusion, as is often made in central banks, that current generation DSGE models are "derived from first principles".

---

[28] In private conversation with the authors, V. Chari clarified that theirs is not a criticism to DSGE models in general and to the aim of fitting the data, but just to the way DSGE models are commonly specified, by introducing new or different assumptions without bringing adequate (new) evidence in their support.

5.3 Can DSGE models be identified and estimated?

The second question which has been addressed in the literature and that is of paramount importance for their use in the policy process is whether DSGE models can be meaningfully identified and estimated from the data. This is an important question given the need to have models that can be falsified by the data. Failing this 'Popperian test' would put the analysis on flimsier, less scientific foundations. This would in turn make the application of these models to the policy-making context significantly more problematic.

A seminal contribution to this literature is Canova and Sala (2009), who argue that typical DSGE models are weakly identified and their parameters can hardly be estimated using standard estimation techniques. It is therefore necessary to follow a Bayesian approach and impose strict priors in order to obtain meaningful estimates. There is a lively debate in the literature on the optimal source of identification restrictions and priors in DSGE models (see e.g. Levin et al. 2008).

Used in the best way, the Bayesian approach is a transparent and increasingly practical approach that has a lot to be commended, especially as it has the potential to convey estimation and model uncertainty at the same time. This is only true, however, if the model builder is completely honest and unbiased, and the model user is able to understand and assess the priors that have been imposed on the model. In practice, it is doubtful that these stringent requirements are satisfied; according to many, the Bayesian approach is dubious as the researcher could get out just what he gets in, without policy-makers really having a clue. Suppose, for example, that a researcher has some strong priors or, even worse, believes that a certain result of the model will be more appealing for policy-makers; if this is the situation, the Bayesian approach could enhance 'confirmatory bias', a tendency that the human mind already appears to have irrespective of background and education (so even economists may not be immune to it). Theoretically, there is a way to check the practical importance of this problem by analyzing the sensitivity of posterior estimates to different priors; as a matter of fact, however, most papers estimating DSGE models do not carry out this type of analysis and it is anyway doubtful that policy-makers would have the time to read the fine print of scientific papers. It could be argued that this should become best practice in the literature, especially if models are to be used in the policy process. On their part, the policy-makers need to be aware of the risk of confirmatory bias and do not send signals to researchers that a certain result is expected or appreciated. Moreover, they need to cultivate a healthy and constructive suspicion of the policy prescriptions stemming from DSGE models estimated with the Bayesian approach.

5.4 DSGE models vs. SVAR models

Macroeconomists nowadays use predominantly DSGE or SVAR models in their work whey they need to address issues from a structural perspective. From a cursory look at the output of major academic journals it would seem that DSGE models clearly have the upper hand, but this conclusion could be premature based on three considerations. First, it is not a foregone conclusion that SVAR (and more generally autoregressive) models are more subject to the Lucas critique than DSGE models; Rudebusch (2005) actually argues that the opposite appears to be true in practice. Because it is still easier to estimate SVAR models than typical DSGE models, it is also easier to assess their stability and susceptibility to the Lucas critique, although as in the case of DSGE models this is seldom done.[29] Second, SVAR models are better able to match the long term behaviour of the data than DSGE models, which are designed to work in deviation from a long term steady state (see e.g. Sims 2008); results of the estimation of DSGE models are particularly sensitive to the detrending assumptions (Delle Chiaie 2009).[30] Third, and most pertinent from the standpoint of this paper, because of their smaller size it is easier to understand what is going on in SVAR than in typical DSGE models, not least for policy-makers with limited time at their disposal. At the same time, SVAR models cannot be used in the same way as DSGE models to learn about the transmission channels and the mechanisms through which certain behaviour takes place or a certain effect is produced. So, there is certainly a value added in DSGE models even if they do not fit the data, especially the long term trends, any better than SVAR models; they can provide an understanding and a 'story telling' that most SVAR models don't. In addition, some authors have proposed to use DSGE models as a basis for the identifying restrictions in SVAR models (e.g., Canova 2002). We think, however, that SVAR should continue to be used extensively if anything as a minimum useful cross check of the story telling arising from the DSGE models.[31]

5.5 Using DSGE models in practice in the policy process

There are several practical issues surrounding the use of DSGE models in a policy making context. We will review three of them there: (i) the core / non-core distinction; (ii) the communication problems with the (non-specialist) decision-makers; and in particular (iii) the intuition problem.

On the first issue, in many central banks where DSGE models are taken directly to the data there is some sort of core and non-core parts of the models (see among others Alvarez-Lois et al. 2008, Reichlin 2008). This is related to the problem, to which we have already hinted before, that models that are beautiful, compact and logically consistent have a hard time matching the data. The model builders therefore need to complement the "core" part of the model with a number of "non core"

---

[29] Assessing the stability of SVAR models is neither easy nor commonly done either, however.

[30] See, however, Ferroni (2009).

[31] On the other hand, Del Negro and Schorfeide (2004) propose to use general equilibrium models to form priors for Bayesian VAR (BVAR) models.

elements before taking the model to the data. Some of these elements are accounting identities and definition of auxiliary variables, and this should represent no problem, but others include new shocks and frictions that are more difficult to justify from a theoretical perspective. If the policy-maker is given a simulation or a forecast based on the full model, it is very hard for him to distinguish what is driven by the core, story-telling and intellectually appealing part and what is instead the consequence of some ad-hoc element in the non-core part of the model. Especially for short-term forecasting it is likely that the latter elements play the most important role; but then this raises the question of whether it would be more appropriate, efficient and intellectually honest to just use an ad hoc time series model for this purpose.

These considerations lead us to the second point, that of the communication with the policy-makers. One key difficulty is indeed that it is not easy to understand what is going on in current generation DSGE models (the model developer himself has to play with impulse responses and structural parameters in order to get a good grasp of the model's workings). It is neither realistic nor efficient that policy makers invest the time to learn about the model in depth, for two reasons. First, the policy makers might not be skilled in DSGE modelling altogether, and, as discussed in Section 2, this again might be efficient, since there are many other important qualities that a good policy-maker should have beyond being a proficient user of DYNARE. Second, any model could be mis-specified or turn out not to be useful; in that case, the policy-maker's investment would have been wasted. Therefore, the situation is (and will be for quite some time) that the policy-maker can at most have a broad idea of the features of a particular DSGE model. In that context, it is not easy for the model developer to give policy prescriptions based on the model given the information asymmetry. The fact that the end-users do not understand everything is, by itself, not a major impediment. To come back to our medical example, many doctors prescribe medications without having a full grasp of their working at a chemical and physiological level (in many cases, even the specialists don't). However, the DSGE models should be useful exactly because they allow a story telling. Therefore, the current generation models are torn between two competing forces. On the one hand, the story has to be simple and appealing if it has to be understood and acted upon. On the other hand, it has to be complex and rigorous if it has to match the real world. It is not easy to determine where the optimal position between these two competing objectives lies.

In this context, and this is our third and last point, we believe that economic intuition plays and will always play an important role in how the prescriptions from DSGE models are incorporated in the policy process.[32] Given the information asymmetry, the risk is that only the prescriptions of the DSGE models that accord well with the policy-makers' previous knowledge and economic intuition

---

[32] On economic intuition see the work of Colander (2000, 2003).

will be acted upon in the actual policy making. While this accords well with common sense, it can also create a dangerous form of confirmatory bias among policy-makers, and puts the burden on the model builders (normally in the staff rather than in the decision making bodies) to communicate particularly clearly on those issues that do not conform with the decision makers' priors.

Looking ahead, it will be very interesting to see how the financial crisis will affect the incentives to use DSGE models in policy-making. Again, there appear to be two competing forces at stake.

On the one hand, current generation DSGE models do a lousy job at matching asset prices (Fernandez-Villaverde 2009); the word 'equity premium puzzle' should be sufficient to illustrate this point. Although there are by now many DSGE models with some type of financial frictions, current generation DSGE models still have a hard time even coming close to tackle phenomena such as financial crises or the build-up of financial imbalances. One important feature of current generation DSGE models is, in fact, the existence of no-Ponzi and transversality conditions which generally rule out insolvency and other non-regular or explosive states. Relaxing this type of restriction will not be easy from a technical point of view, without changing the very nature of current-generation DSGE models, since this would leave the door open to discontinuties, non-convexities and multiple equilibria of the type that macroeconomists typically do not like.

On the other hand, there is a great demand for a structural explanation of the financial crisis in a coherent and logic setting, an analysis that has been sorely lacking in the current conjuncture, and general equilibrium models are particularly good on that dimension. Moreover, although certainly difficult, it is not inconceivable that second or third generation DSGE models will eventually be successful in integrating highly non-linear phenomena such as financial imbalances, crises and defaults. Advances in computing power and software could significantly lower the computational cost in modelling "second generation" financial frictions.

We will have to see whether the DSGE modelling community will be up to the challenge. So far, it appears that mainstream, DSGE-oriented academics have generally not been very influential in shaping the debate on the financial crisis, and we will have to see whether the situation will change in a few years time.

An important element that has been missing from current generation DSGE models (though there are exceptions in the burgenoning literature on learning) is the psychological element of human and social behaviour, which was instead central in the Keynesian tradition and which is still very much present in the mind of policy makers, who for example often mention words such as confidence and 'changes in risk aversion' in their interventions. We see this as an area where researchers have to

learn from policy-makers, rather than the other way round.[33] It is also true, however, that policy-makers have made very little use of the enormous academic literature on behavioural economics and bounded rationality. This is a literature which has tried to provide evidence of *systematic* deviations from the standard rationality assumptions, it is not in any way an 'heterodox' field (it has reached the upper echelons of the academia in the US and elsewhere) and some of its conclusions appear to be of direct relevance for policy-making, but for some reason it has not found its way inside central banks. Indeed, our survey indicated that there is some interest in these alternative approaches, but it is not very widely shared.

6. Conclusions

In Plato's *Republic*, philosophers are the rulers because they are the only ones to know the world of ideas and rise beyond the world in its apparent and practical manifestations. Are we ready for the modelers to take the reins of central banks?

Motivated by a small survey of senior central bank staff, this paper has looked at the interaction between economic research and policy work, with a particular focus on central banking issues. It reviewed the relevant literature on the subject, but also presented more personal reflections, motivated in particular by the current financial crisis. We have argued that policy-makers and researchers have two different jobs, the first being more multi-tasker, the second concentrating deeper in a narrower field. Nonetheless, it is essential to make researchers and policy-makers work more productively together and we think that the difficulties are manageable once one becomes aware of them.

These are the four conclusions that we have to offer:

First, that models represent an irreplaceable tool to guide policy decisions, but that they should be complemented by judgment and they need to be well understood (in terms of clear and memorable stories) by policy-makers, otherwise their impact is going to be limited, a point that is not often appreciated enough by research-oriented economists.

Second, models should be subject to (even) close(r) scrutiny by independent researchers: there is clearly scope for refining the reviewing process in economic journals, and reduce publication lags so as to make research more directly relevant for policy-making. If long publication lags are not acceptable in medicine or physics, nor they should be in economics.

Third, policy makers should have wider horizons, be curious about different approaches and not rely only on DSGE models but also on other strands of economic research. For instance behavioural economics seems to be a promising research avenue with potentially relevant implications for central banks.

---

[33] The literature on learning is a promising start, as is the recent focus on "animal spirits" and "news" shocks.

Finally, although researchers and policy-makers have different roles, they should talk closely to each other, cultivate a healthy skepticism (especially on the side of policy-makers) and strive for a better mutual understanding. It is not enough for policy-makers to tell researchers that their models are not (or are) useful, they should tell them where and why.

References

Adolfson, M., Andersson, M., Linde, J., Villani, M. and A. Vredin (2007): "Modern Forecasting Methods in Action: Improving Macroeconomic Analyses at Central Banks", *International Journal of Central Banking*, vol. 3(4), pp. 111-144, December.

Alvarez-Lois, P., Harrison, R., Piscitelli, L. and A. Scott (2008): "On the Application and Use of DSGE Models", *Journal of Economic Dynamics and Control*, Elsevier, vol. 32(8), pp. 2428-2452, August.

Barnett, W. A. (2006): "Is Macroeconomics a Science?", MPRA Paper No. 415, University Library of Munich, Germany, April 2006.

Benson, B. L. (2009): "Economic dissociative identity disorder: the math gamer, the anti-policy econometrician and the narrative political economist", Econ Journal Watch, 6, 3.

Berg, A. and C. Pattillo, (1999): "Are Currency Crises Predictable? A Test," *IMF Staff Papers*, Palgrave Macmillan Journals, vol. 46(2), p. 107-138.

Blanchard, O. (2008): "The State of Macro", NBER Working Paper No. 14259.

Blinder, A. (1997): "What Central Bankers Could Learn from Academics -- And Vice Versa", *Journal of Economic Perspectives*, vol. 11(2), pp. 3-19.

Blinder, A. (1998): *Central Banking in Theory and Practice*, MIT Press.

Buiter, W. (2009): "The unfortunate uselessness of most 'state of the art' academic monetary economics", March 3, 2009, http://blogs.ft.com/maverecon/2009/03/the-unfortunate-uselessness-of-most-state-of-the-art-academic-monetary-economics/#more-667.

Bussière, M. and M. Fratzscher (2006): "Towards a New Early Warning System of Financial Crises", *Journal of International Money and Finance*, Elsevier, vol. 25(6), pp. 953-973, October.

Bussiere, M. and C. B. Mulder (2001): "Which Short-term Debt Reserve Ratio Works Best? Operationalising the Greenspan-Guidotti Rule", with C. Mulder, 2001, ch. 6, Part 2, pp. 73-82. In *"Capital Flows Without Crisis? Reconciling Capital Mobility and Economic Stability"*, ed. By D. Dasgupta, M. Uzan and D. Wilson.

Canova, F. (2002): "Validating Monetary DSGE Models through VARs", CEPR Discussion Papers 3442.

Canova, F. and L. Sala (2009): "Back to Square One: Identification Issues in DSGE Models", Journal of Monetary Economics, 56, 4, pp. 431-449.

Chari, V. V., Kehoe, P. J. and E. R. McGrattan (2008): "New Keynesian Models: Not Yet Useful for Policy Analysis", *American Economic Journal, Macroeconomics*, January 2009, volume 1, number 1, pp. 242-266.

Chari, V. V. and P. J. Kehoe (2006): "Modern Macroeconomics in Practice: How Theory is Shaping Policy", *Journal of Economic Perspectives*, vol. 20(4), Fall 2006, pp. 3-28.

Colander, D. (2000): "New Millennium Economics: How Did It Get This Way, and What Way is It?", *Journal of Economic Perspectives*, vol. 14(1), Winter, pp. 121-132.

Colander, D. (2003): "Muddling Through and Policy Analysis", New Zealand Economic Papers 37, pp. 197-215.

Colander, D., Howit, P., Kirman, A., Leijonhufvud, A. and P. Mehrling (2008): "Beyond DSGE Models: Toward an Empirically Based Macroeconomics", *American Economic Review Papers & Proceedings*, Vol. 98(2), pp. 236-240.

Colander, D., Föllner, H., Goldberg, M., Haas, A., Juselius, K., Kirman, A., Lux, T. and B. Sloth (2009): "The Financial Crisis and the Systemic Failure of Academic Economists", working paper.

Csajbok, A. (2008): "The Use of Staff Policy Recommendations in Central Banks", MNB Working Paper No. 2008/4.

Del Negro, M. and F. Schorfeide (2004): "Priors for equilibrium models for VARs", *International Economic Review*, 45, pp. 643-673.

Del Negro, M., Schorfeide, F., Smets, F. and R. Wouters (2007): "On the Fit and Forecasting Performance of New Keynesian Models", *Journal of Business and Economic Statistics*, Vol. 25(2), pp. 123-162.

Delle Chiaie, S. (2009): "The sensitivity of DSGE models' results to data detrending", OENB Working Paper No. 157.

Dewald, W. G., J. G. Thursby and R. G. Anderson (1986): "Replication in Empirical Economics: The Journal of Money, Credit and Banking Project", *The American Economic Review*, Vol. 76, No. 4 (Sep., 1986), pp. 587-603.

Ellison, G. (2007): "Is Peer-Review in Decline?", NBER Working Paper No. 13272, July 2007.

Faust, J. (2005): "Is Applied Monetary Policy Analysis Hard?", mimeo, Federal Reserve Board.

Fernandez-Villaverde, J. and J. Rubio-Ramirez (2007): "How Structural are the Structural Parameters", NBER Working Paper No. 13166.

Fernandez-Villaverde, J., Rubio-Ramirez, J., Sargent, T. and M. Watson (2007): "A, B, C (and D's) for Understanding VARs", *American Economic Review*, vol. 97(3), pp. 1021-1026.

Fernandez-Villaverde, J. (2009): "The Econometrics of DSGE models", NBER Working Paper No. 14677.

Fukac, M. and A. Pagan (2006): "Issues in Adopting DSGE Models for Use in the Policy Process", Czech National Bank Working Paper No. 06/2006.

Gali, J. and M. Gertler (2007): "Macroeconomic Modelling for Monetary Policy Evaluation", *Journal of Economic Perspectives*, Vol. 21(4), pp. 25-46.

Gans, J. S. and G. B. Shepherd (1994): "How are the Mighty Fallen: Rejected Classic Articles by Leading Economists", *The Journal of Economic Perspectives*, Vol. 8(1), Winter, pp. 165-179.

Gordon, R. (2009): "Is modern macro or 1978-era macro more relevant to the understanding of the current economic crisis?", paper presented to the International Colloquium on the History of Economic Thought, San Paulo, August 3.

Groopman, J. (2007): *How Doctors Think*, Houghton Mifflin Company.

Hawtrey, R. G. (1962): *The Art of Central Banking*, 2nd Edition, London, 1962.

Issing, O. (2006): Dinner speech at "Monetary Policy: a Journey from Theory to Practice - an ECB Colloquium Held in Honour of Otmar Issing"; Frankfurt am Main, 16 March 2006.

Issing, O. (2008): *The Birth of the Euro*, Cambridge University Press.

Issing, O., V. Gaspar, I. Angeloni and O. Tristani (2001), *Monetary Policy in the Euro Area: Strategy and Decision Making at the European Central Bank*, Cambridge University Press, Cambridge.

Juselius, K. and M. Franchi (2007): "Taking a DSGE Model to the Data Meaningfully", Economics Discussion Papers.

Keynes, J. M. (1936): *The General Theory of Employment, Interest and Money*, Palgrave Macmillan.

Kirman, A. (2009): *Complex Economics: Individual and Collective Rationality*, Routledge, Abingdon.

Krugman, P. (1979): "A Model of Balance-of-Payments Crises," *Journal of Money, Credit and Banking*, Blackwell Publishing, vol. 11(3), pp. 311-25, August.

Krugman, P. (1998): *The Accidental Theorist: And Other Dispatches from the Dismal Science*, W. W. Norton & Co Inc; 1st edition (May 1998).

Kydland, F.E. and E.C. Prescott, "Rules Rather than Discretion: The Inconsistency of Optimal Plans", *The Journal of Political Economy*, Vol. 85, No. 3. (1977), pp. 473-492.

Leroy, S. F. (2004): "Rational Exuberance", *Journal of Economic Literature*, Vol. 42, September, pp. 783–804.

Levin, A. T., Lopez-Salido, J. D., Nelson, E. and T. Yun (2008): "Macroeconometric Equivalence, Microeconomic Dissonance, and the Design of Monetary Policy", Federal Reserve Bank of St. Louis, Working Paper No. 2008/035A.

Lucas, R. E, Jr, (1980): "Methods and Problems in Business Cycle Theory", *Journal of Money, Credit and Banking*, Blackwell Publishing, vol. 12(4), pages 696-715, November.

Lucas, R. Jr, (1976): "Econometric Policy Evaluation: A Critique", *Carnegie-Rochester Conference Series on Public Policy*, Elsevier, vol. 1(1), pages 19-46, January.

Morgan, M. S. (2001): "Models, Stories and the Economic World", *Journal of Economic Methodology*, Vol. 8(3), pp. 361-384.

Nelson, W. (2008): "Monetary Policy Decisions: Preparing the Inputs and Communicating the Outcomes", BIS Paper No. 37.

Mankiw, G. (2006): "The Macroeconomist as a Scientist and Engineer", *Journal of Economic Perspectives*, Vol. 20(4), pp. 29-46.

McIntosh, B. S., Seaton, R. A. F., and P. Jeffrey (2007): "Tools to Think With? Towards Understanding the Use of Computer-Based Support Tools in Policy Relevant Research", Environmental Modelling & Software, 22, pp. 640-648.

Meese, R. and K. Rogoff (1983): "Empirical Exchange Rate Models of the Seventies : Do They Fit Out of Sample?", *Journal of International Economics*, Elsevier, vol. 14(1-2), pp. 3-24, February

Onatski, A. and N. Williams (2003): "Modeling Model Uncertainty", *Journal of the European Economic Association*, MIT Press, vol. 1(5), pp. 1087-1122.

Ormerod, P. (1994): *The Death of Economics*, St. Martin's Press.

Oswald, A. (2006): "An Examination of the Reliability of Prestigious Scholarly Journals: Evidence and Implications for Decision-Makers", *Economica*, vol. 74, pp. 21–31.

Reichlin, L. (2008): "Discussion on 'Taking DSGE Models to the Policy Environment", *Journal of Economic Dynamics and Control*, vol. 32, pp. 2453-2459.

Roberts C. J. and T. D. Stanley eds. (2006): *Meta-Regression Analysis: Issues of Publication Bias in Economics Surveys of Recent Research in Economics*, Blackwell.

Rogoff, K. (2002): "Dornbusch's Overshooting Model after Twenty-five Years", *IMF Staff Papers*, Vol. 49, Special Issue, pp. 1-35.

Romer, C. D. and D. Romer (2008): "The FOMC versus the Staff: where can Monetary Policymakers Add Value?", NBER Working Papers No. 13751.

Rudebusch, G. (2005): "Assessing the Lucas Critique in Monetary Policy Models", *Journal of Money, Credit, and Banking*, vol. 37(2), pp. 245-272.

Simon, H. (1969), *The Sciences of the Artificial*, Cambridge, Mass., MIT Press.

Sims, C. A. (2008): "Improving Monetary Policy Models", *Journal of Economic Dynamics and Control*, 32, pp. 2460-2475.

Sims, A. A. (2008): "Making Macro Models Behave Reasonably", Working Paper, Princeton University; http://sims.princeton.edu/yftp/ReasonableModels/MakingReasonable.pdf

Smets, F. and R. Wouters (2007): "Shocks and Frictions in US Business Cycles: A Bayesian DSGE Approach", *American Economic Review*, vol. 97(3), pp. 586-606, June.

Solow, R. (2008): "The State of Macroeconomics", *Journal of Economic Perspectives*, forthcoming.

Spaventa, L. (2009): "Economists and economics: what does the crisis tell us?", CEPR Policy Insight No. 38.

Tovar, C. E. (2008): "DSGE Models and Central Banks", BIS Working Paper No. 258.

Wall, H. J. (2009): "Journal rankings in economics: handle with care", Federal Reserve Bank of St. Louis Working Paper No. 2009-014A.

Walsh, C. E. (2006): "The Contribution of Theory to Practice in Monetary Policy: Recent Developments", Prepared for *Monetary Policy: A Journey from Theory to Practice,* An ECB Colloquium held in honour of Prof. Otmar Issing, Frankfurt, 16-17 March 2006.

Wyplosz, C. (1998): "The Culture of Economic Policy Advice: an International Comparison with Special Emphasis on Europe", http://hei.unige.ch/~wyplosz/stgallen.pdf

Table 1 – Structure of economic research in central banks

	yes	no
CB with separate research dept. (*)	8	24
Economists evaluated according to publication record (%) (**)	75	54
Share of PhD economists (%) (***)	63	39
Time allocation: free research (%)	46	22
Time allocation: directed research (%)	31	36
Time allocation: policy work (%)	22	42

Note: Results from questionnaire, answers received from 32 central banks. Results refer to all economists in central banks without a separate research department, to research economists in central banks with a separate research department.
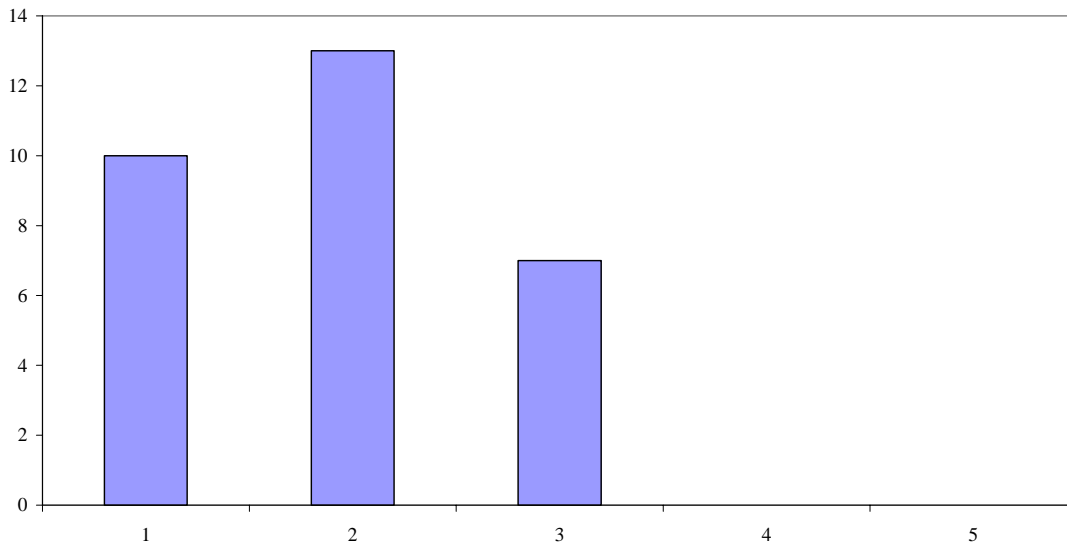
(*) Includes the Deutsche Bundesbank where the allocation to a separate research department is on a rotation basis. It excludes the Bank of Japan where there is a separate institute called Institute for Monetary and Economic Studies that is associated to the central bank.

(**) Several central banks mentioned that the publication record is only one of the evaluation criteria.

(***) For France this represents new recruitment only.

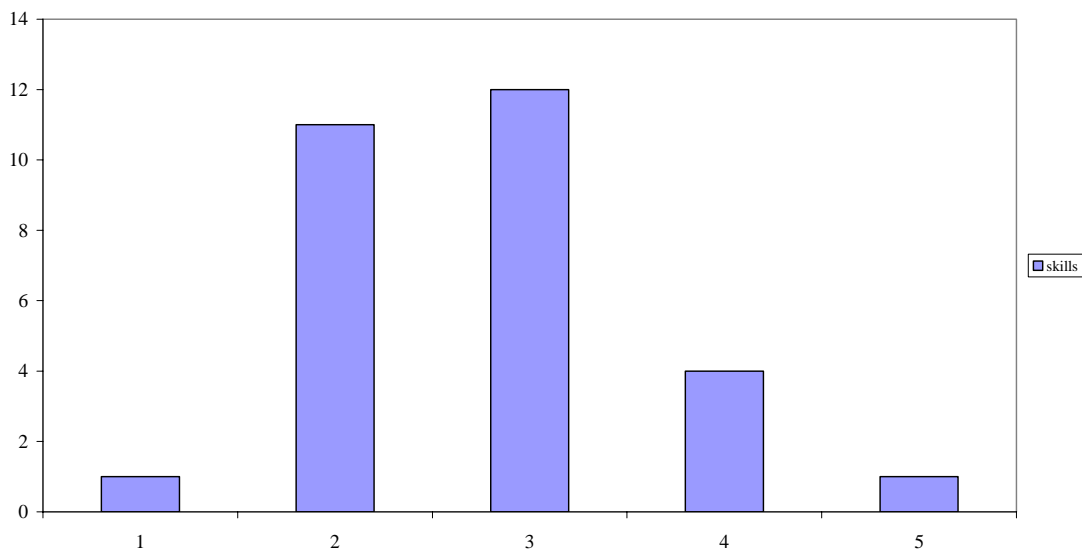**Figure 1: Economic research and policy decisions**

Economic research should aim at improving policy making decisions
(From 1=very much agree to 5=disagree)

Note: based on a questionnaire sent to central banks. Data reported is the number of central banks responding. The total may not add up to 32 since some central banks have not answered all questions.
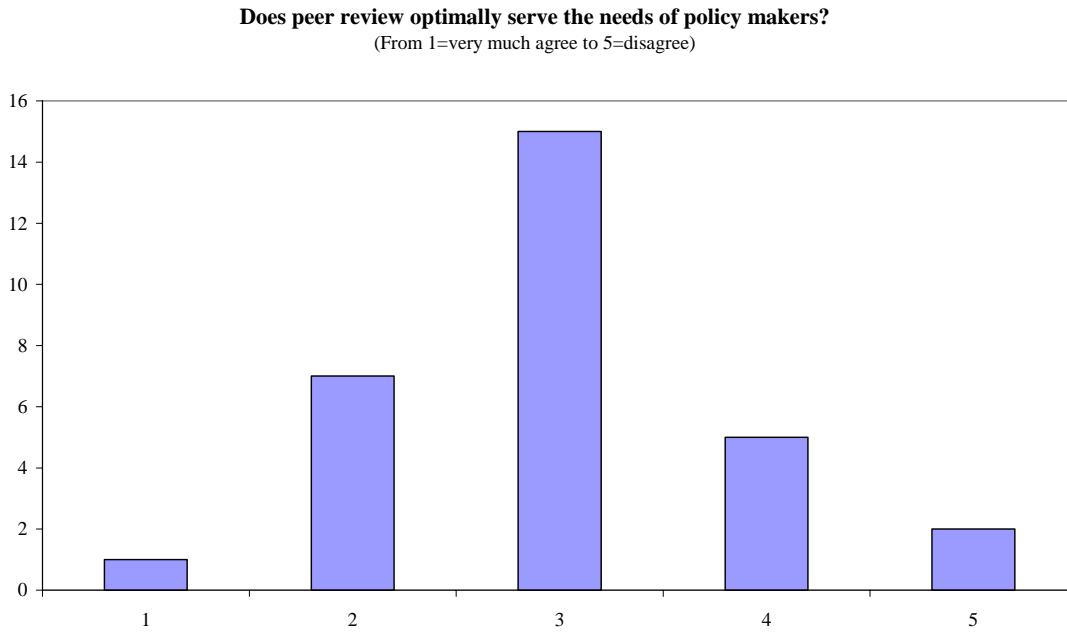
Figure 2: Skills

Do policy-makers need a broader range of skills?
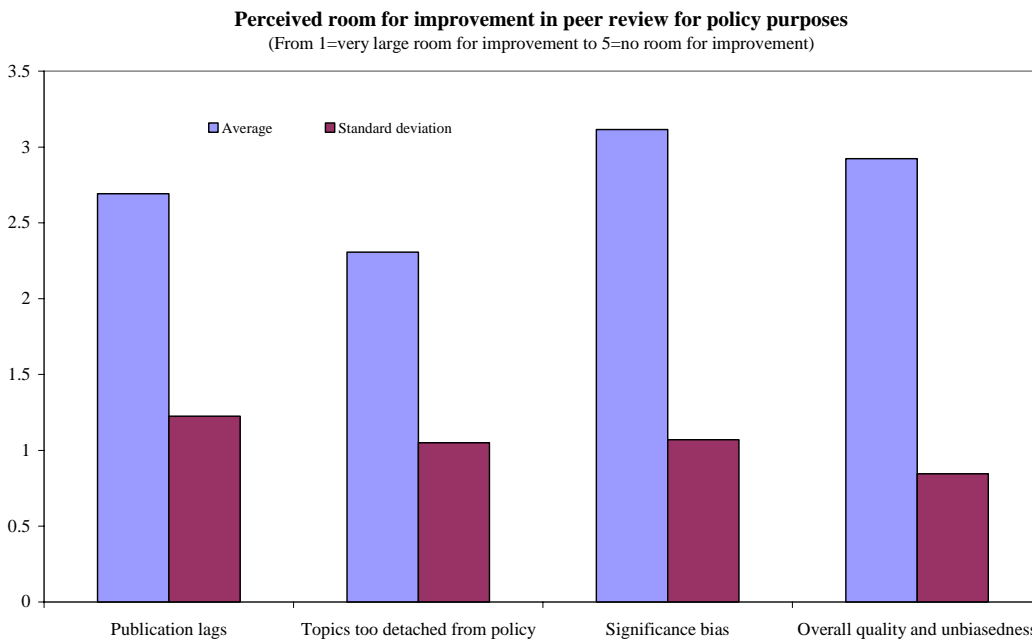(From 1=very much agree to 5=disagree)

Note: based on a questionnaire sent to central banks. Data reported is the number of central banks responding. The total may not add up to 32 since some central banks have not answered all questions.

Figure 3: Peer review

Does peer review optimally serve the needs of policy makers?
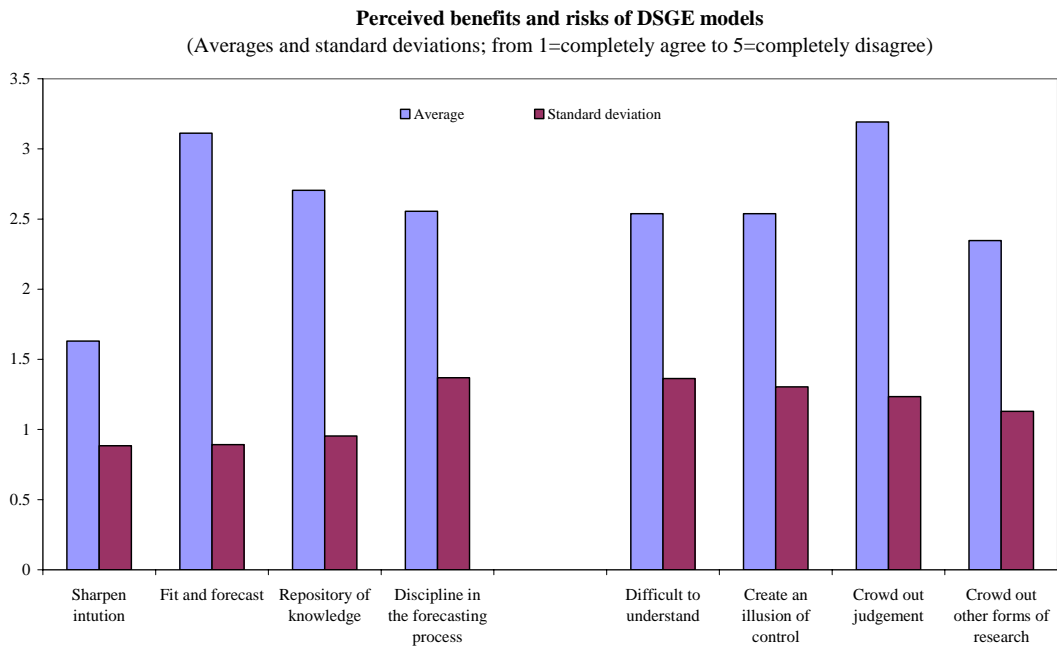(From 1=very much agree to 5=disagree)

Note: based on a questionnaire sent to central banks. Data reported is the number of central banks responding. The total may not add up to 32 since some central banks have not answered all questions.

Figure 4: Room for improvement in the peer review process

Perceived room for improvement in peer review for policy purposes
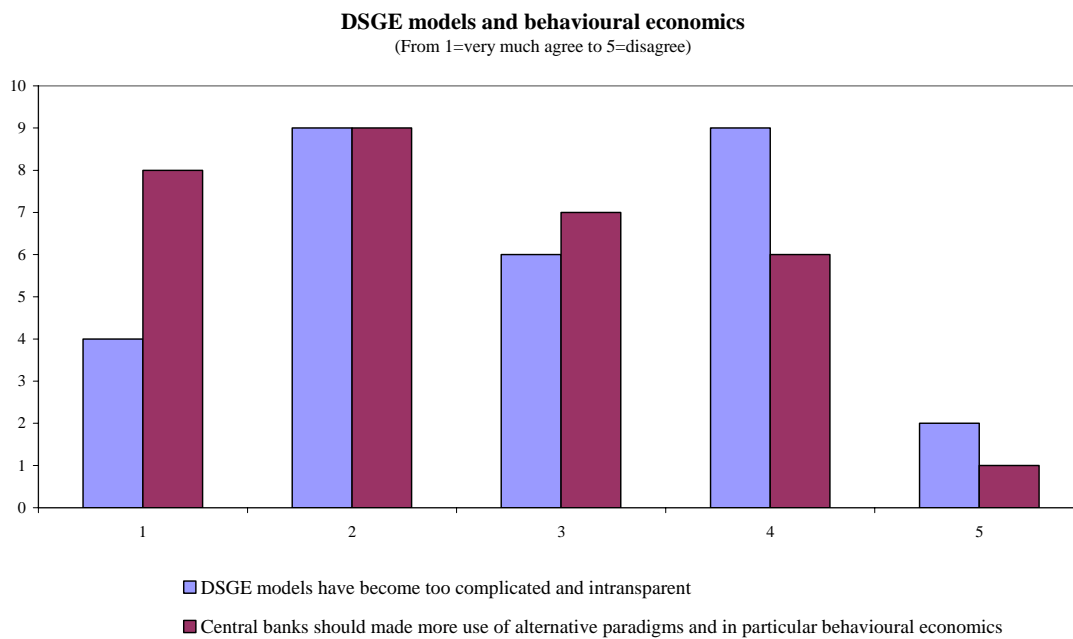(From 1=very large room for improvement to 5=no room for improvement)

Note: based on a questionnaire sent to central banks. Data are averages and standard deviations across respondents.

Figure 5: Benefits and risks of DSGE models

Perceived benefits and risks of DSGE models

(Averages and standard deviations; from 1=completely agree to 5=completely disagree)

Note: based on a questionnaire sent to central banks. Data are averages and standard deviations across respondents.

Figure 6: DSGE models and alternative paradigms

DSGE models and behavioural economics

(From 1=very much agree to 5=disagree)

Note: based on a questionnaire sent to central banks. Data reported is the number of central banks responding. The total may not add up to 32 since some central banks have not answered all questions.