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Using Government Documents to Assess the Influence of Academic Research on Macroeconomic Policy

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**Using government documents to
assess the influence of academic
research on macroeconomic policy**

by

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Using government documents to assess the
influence of academic research on
macroeconomic policy

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Since many factors determine the choice of economic policies it is hard to pin down the role that economic analysis plays. One possibility is to see to what extent the work of academic economists is referred to in government documents. Do academic economists provide the reasons - or at least a least a rationale - for the policies that are adopted, or are they just disregarded kibitzers? More specifically, the organizers of this conference have asked me to address the following questions: Do academic ideas enter official documents, and if so, how? What happens to them when they are used in official documents? Are they simplified? Is there any pattern to those that are accepted and those that are not? How long does it take for ideas to travel from the journals to official documents?

I. Coverage and procedure

Following the suggestion of the program committee I deal only with macroeconomic policies in developed economies. My coverage is also limited by the impossibility of reading more than a miniscule sampled of the relevant material. This sample is not a representative one because of my limited knowledge of foreign languages, and lack of knowledge about what foreign documents are available, as well as the difficulty of obtaining them. Much of my material therefore comes from U.S. documents, and it is only for the U.S. that I trace developments over time; for other countries I primarily use recent documents. This limitation is not quite as serious as may appear at first because the relative openness of the U.S. government means that United States documents provide a much richer source than do the documents of most other countries. Moreover, I am primarily trying to explain rather than enumerate the uses of academic economic research, and for that concentration on one particular country is not as serious as it would be for a more descriptive endeavor.

Within macroeconomics I look mainly at monetary policy.

This is in part due to the greater availability of monetary-policy documents and my familiarity with them, but is

motivated also by the widespread realization that fiscal policy is not an effective tool of macro policy. As Gregory Mankiw (reprinted in Snowden and Vane 1997; 448) pointed out, because persistent large deficits make it impossible to obtain the consensus that would be needed for timely fiscal policy " all attempts at stabilization are left to monetary policy." Although since Mankiw wrote this the U.S. has achieved a balanced budget, large deficits are scheduled to reappear. Other countries face a similar problem. Since the monetary authorities tend to pay more attention to economists and to be more academically oriented than the fiscal authorities this means that my discussion is, if anything, more likely to overstate than to understate the influence of academic economists. I pay little attention to international macropolicy, because this is an area in which it is often impossible for authors of government documents to speak frankly for fear of generating speculative movements.

One way one might proceed is to select specific academic contributions and see whether, and if so when, they enter official documents. The other way is to start with government documents and record the academic contributions they contain. The latter approach is more likely than the former to show that academic contributions matter, because it counts every instance where they do appear as a success, while ignoring all those that do not make it into any of these documents. Starting with academic ideas would run into the problem that one would have to decide which contributions to select. But there is so much disagreement among economists that it would be hard to find a sufficient set of ideas that are at the same time generally accepted by academic economists and sufficiently distinct from ideas held by others, or held by others only due to the work of academic economists.¹ I therefore started with the documents.

As a participant in the conference suggested, it would be desirable to quantify the influence of academic research. However, this cannot be done in a meaningful way. The appropriate quantification is the ratio of the number of times academic research is cited to the number of times it could have been cited, with both numbers weighted in some way. But to calculate the numerator would require a complete knowledge of the relevant academic literature. As another participant suggested it would also be desirable to investigate differences among countries in the role of academic research. However, that involves a

much larger

question, the differences in the status of economists, and that would require a separate paper. II. Defining academic economics and government documents

One possible definition of "academic ideas" is the ideas that originates in universities. A broader definition includes work done by economists employed in government agencies and in

the private sector who are members of the same speech community as academic economists;

that is they deal with similar problems, use similar techniques and language, sometimes publish in the same places and read each others' work. It does seem arbitrary to count as academic work a paper published in *Econometrica* if it is written by a university teacher, but not if it is written by an economist in the central bank.

The term "government documents" is also ambiguous. One could include anything a government agency publishes as "publishing" is usually defined, that is as something printed and made publicly available. But letting the definition of government documents depend on whether an item is printed or duplicated is not helpful in assessing the influence of academic research. For example, in the U.S. the "Transcripts of the Federal Open Market Committee" (FOMC) are not printed and advertised for sale, but can be obtained in certain libraries or purchased as microfilms and duplicates, and future issues will be on the internet. The corresponding minutes from the Bank of England are already on the internet. Such minutes are very close to the policy-making process, so excluding them would mean ignoring a valuable source.

But if the criterion of being printed is therefore eliminated how about working-papers from research departments? I have excluded them in part because they often have little influence on policy, and also because it is obvious that they show the imprint of academic research.

That still leaves two problems. One is that official agencies sometimes publish essentially academic material, such as the proceedings of academic conferences that they sponsor. Though I mention them occasionally in passing, I do not treat these as official documents, in part out of a belief that the main purpose of many conferences is at least as much to be a pay-off to one of the central banks' constituencies, academic economists and to keep them interested in the problems that concern the central bank,

as it is to help the central bank make policy decisions. Hence, such proceedings give little indication of what academic ideas the central bank takes seriously. The second problem is the treatment of international agencies, such as the IMF or OECD. Since they are not governments I exclude their documents.

III. Some other problems

Much of economics can get by with only the thinnest of psychological foundations because it deals with aggregations of individuals, so that individual idiosyncrasies usually wash out. But here this is problematic, because a particular person who happens to be in charge can sometimes make a difference. For example, the Federal Reserve Banks now publish reviews that carry many articles closely tied to academic papers. This was not so until Homer Jones became director of research at the St. Louis Federal Reserve Bank. To be sure, one can argue that if he had not been there to show what the Banks' research departments could do, someone else would have done so by now. But that is not necessarily the case, and even if it is, it might have taken much longer. Similarly, when a former academic, Robert Weintraub, was appointed staff economist of the congressional Subcommittee on Domestic Monetary Policy, he induced this Committee to interact much more closely with academic economists and to publish contributions by them in several compendia. After his untimely death that ceased. Likewise, in the 1960s and 1970s Senator Proxmire successfully insisted on the appointment of economists, many of them former academics, to the Federal Reserve Board, which surely stimulated interest in academic research at the Fed.

Another problem is that many academic ideas may not appear in any government document because they deal with an issue that is not currently alive in the political arena academic and policy-oriented researchers have different agendas (see Frey et al 1997). For example, if the Fed's independence were threatened, it would probably refer in its documents to academic publications advocating central bank independence or at least use their arguments. But absent such a threat it has nothing to gain by doing so. Hence, if an academic idea appears in an official documents only, say ten years after its publication, one should not claim that it has taken ten years for government

officials to absorb it.

An additional problem is that if a government document mentions an idea that is prominent in academic research this need not indicate causation. In 1968 Friedman (1968) persuaded many economists that raising the growth rate of money soon raises interest rates.

The Federal Reserve had been saying that for many years before. Similarly, it anticipated the current literature arguing that price stability enhances economic growth.

Still another problem is that academic research can influence government documents in an invisible way. Surely, all of us have had the experience of our research being influenced by a paper we do not cite, because what it does is to induce us not to write something that we would otherwise have written.

A related and more important problem is that many government documents use academic research not explicitly but implicitly. One reason is that most official documents are factual and avoid explicit theory. But facts are "theory-drenched", and which facts seem to deserve mention depends upon one's theoretical framework. For example, a "purely factual" survey of the current state of the economy written by a post-Keynesian would spend less time on the growth rate of money than would one written by a monetarist. But such indirect influences are hard to detect. Academic work also enters government documents in another hard to detect way; the document's forecast is likely to be based at least in part on an econometric model developed by academics.

IV. A public choice perspective on government documents

Public choice theory suggests that the relation between official documents and policy may be tenuous. The government may adopt a policy without issuing a document setting out its reasons. In other cases it may cite research that played only a minor part in its decision because it provides a convenient cover. Governments are not compelled to provide a full record of their thoughts and actions. For example, the 1982 Report of the Council of Economic Advisers discusses the weakness of fiscal policy as a stabilization tool. It seems likely that had the Democrats and not the Republicans won the previous election, the Report would not have used its limited space to make this point, though the Council might well have told the same thing to the President in

private.

From a public choice viewpoint an official document is to a substantial extent a public relations tool.² It will therefore avoid as whenever possible citing unfavorable academic research even if there is strong evidence supporting it, while citing even weakly supported favorable academic research. Hence, the sequence of events may often run - not from government documents to policy decisions - but from policy decisions to government documents. (Such a "cite what fits" procedure is also not unknown in academia.) For example, the Federal Reserve's so-called "monetarist experiment" in 1979 was in large part motivated by the failure of its prior policy. But it may well be that the Fed would not have undertaken this "monetarist experiment" at that time had it not been for the writings of monetarist economists, and the changing perception of monetary policy among academic economists in general. Yet the statement that the Fed issued to justify its policy change attributed its policy shift to changed circumstances in financial markets, and not to a recognition of errors in its previous thinking, or changing academic beliefs. All in all, It is not surprising that Edward Kane (1974; 835-36) referred to the Annual of the Report of the Federal Reserve as: 'roseate and self-serving analysis imbedded in the typical Fed pronouncements. ... The Report's goal is to put the best face possible on Fed actions of the past year. ... There are, of course, no sections Several factors ameliorate this discouraging picture. First, some government documents do not pose a potential threat to the reputation or preferred policies of a government agency. They may deal with new problems, or problems that have little political resonance, or they may be written for a narrow audience of experts. Second, doing good is one of the important motives of the government, and that it gives it an incentive to take academic advice seriously, Third, citing the support of academic economists for one's policy may be good public relations. This will differ among countries. Thus in discussing the role of the Dutch Central Planning Bureau (CPS), which is staffed by economists, Harry van Dahlen and Arjo Klammer (1997; 60) wrote that: "whatever economic policy emerges will not pass without the stamp of approval of the CPS", so that the CPS has an influence that is greater than that of comparable institutes elsewhere in

Europe, "and is inconceivable in the U.S."

Fourth, government economists themselves are a significant constituency. They care about the opinions of other economists, particularly if there is significant migration between government service and academia. So, when asked to endorse obviously bad reasoning they will tend to drag their feet. They will also try to enhance their status with academics by using the academic literature. Fifth, the authors of government documents need to care about whether academic economists respect their work because journalists sometimes talk to academic economists when they write their stories. And a good way to achieve such respectability is to refer either explicitly or implicitly to academic work. This does not mean that academic research is used only as a decorative rationalization in government documents with no influence on actual policy. The rationalizations that one gives affect one's thinking and hence one's actions.

V. Other determinants of the role of academic economics

Suppose that, as I have argued, the major function of many government documents is to persuade the public. Then, except in the rare situation in which the public is willing to accept academic analyses on faith, the role played by the work of academic economists depends in part on its plausibility to an audience not trained in economics. Since the general public is not likely to read government documents their main audience consists of journalists. Much therefore depends upon their training in economics and their willingness to take the time and effort to understand serious economic reasoning, as well as on its difficulty. Academic research is likely to play a much larger role in government documents if they can present it in a simple, intuitive way. For example, research that shows that raising the minimum wage will cause unemployment is more likely to find its way to the general public than is, say a demonstration of the uniqueness of a general equilibrium solution. Authors of government documents know this, and hence are more likely to make more use of the former type of research. And they are likely to avoid anything that seems implausible to the average person. ..

A related determinant is the willingness of

academic economists to do the type of work that government officials find useful. That is the sophisticated use of simple tools that are mostly taught in introductory economics (see Allen 1977; Frey et al 1997; Harberger 1998). It is only in academia that the term "technical" is considered high praise. In other words, the supply as well as the demand for usable economic analysis determines the amount bought."

VI. Some American documents

I will look mainly at the Report of the Council of Economic Advisers (CEA), at the Transcripts of the Federal Open Market Committee, (Transcripts)³, and more briefly at congressional Hearings. In the U.S. Treasury and Commerce Department documents are generally uninformative about macro policy.

1. The CEA reports

The CEA Report is written in non-technical language and is addressed in the first instance mainly to the media and the informed public. It contains a convenient and useful summary of economic events during the previous year, a justification for the President's proposals to Congress, and a set of chapters on specific topics reflecting the interest of the Administration or of the CEA Chairperson. It is obviously a political document. Unless it chooses to preserve a discreet silence on some issues - and it may not be allowed to do even that - it must defend the Administration and its proposals.⁴ At the same time the Council members and senior staff, most of whom are academics on leave from their universities, need to look to their professional reputations and to the reputation of the Council in general, and hence must avoid seeming too partisan. The CEA is therefore much more open to the influence of academic economics than other U.S. government agencies and its Reports come closest to fulfilling an educational mission.

An examination of the Reports over the last thirty years shows a strong shift in the evaluation of counter-cyclical policy. Although the 1968 Report conceded that stabilization policy cannot offset very minor fluctuations, the (then Democratic) CEA believed in counter-cyclical policy in other situations. It stated that fiscal policy has "contributed to the improved record of economic stability" (63), while monetary policy "made a major contribution to the advance of the economy" (71). In its 1969 Report it advocated annual changes in tax rates to

make stabilization policy even more effective.

Subsequent Reports took a different line. The 1970 Report (66-67) recognized that monetary and fiscal policy may be destabilizing "if moves are not made in the right amounts

and at the right time. ... There is now abundant experience with the obstacles to the effective and flexible use of tax changes" for stabilization." Given the long and variable lags of monetary policy, it is prudent not to let monetary policy "stray widely from the steady posture" called for by longer run policy (68).

In 1982 another Report (now by a Republican CEA) advocated a stable growth rate of money instead of countercyclical monetary policy. It argued that fluctuations induced by productivity changes and price shocks cannot be avoided, but what is avoidable are "the procyclical changes in the growth of the money supply that have occurred in the past." (77)5. And with respect to fiscal policy the 1985 Report stated that, quite apart from the problem of

lags and forecast errors "there is increasing doubt about the effectiveness of discretionary fiscal policy" (57) Subsequently the 1989 Report declared that: "Discretionary fiscal policies ... have as often" been destabilizing as stabilizing. (77). The 1990 Report (68) added that counter-cyclical fiscal policy is "fraught with so many difficulties that ... [it] becomes inconsistent with ambitious goals for long-run growth.

All of the citations in the previous paragraph come from Republican CEA's. After the Democratic regained the White House in 1992 their CEA's have kept a judicious silence on the efficacy of counter-cyclical policy. Perhaps they shared the Republicans' view; that is hard to say.

Because so little information is available on academic economists' evaluation of stabilization policy only an crude comparison of their evaluation with the CEA's is possible. It seems likely that the optimistic 1968 Report mirrored the predominant view of academic economists. The CEA's shift to a more skeptical view of fiscal policy was probably shared by the majority of academic economists, though I suspect that the CEA shifted faster. On monetary policy academic economists also shifted in the same direction as the CEA, but only a minority would go all the way with the monetarist CEA's of the 1980s. Yet the CEA's positions certainly did not lack support from some eminent academics, and seem based on their work. It would be hard

to argue that the Republican CEA in the 1980s ignored what was going on in the academic journals. And the subsequent Democratic CEA's should not be blamed for keeping silent, it is not required to cover all aspects of a particular subjects.

The CEA's treatment of the specifics of monetary policy, too, shows the influence of academia. Thus the 1968 Report argued that uncertainty reduces the optimal size of the

response of monetary policy to fluctuations, a finding that Brainard (1967) had published only the previous year. The (Democratic) 1969 Report warned against an accommodative monetary

policy and stated that at turning points the interest rate can be a misleading indicator of the stance of monetary policy; points that monetarists had been making for many years, but which the Fed would not absorb for many more years. Several Reports showed concern with the choice of a monetary target versus an interest rate target, a hotly debated issue in academia, and one of them seems to refer implicitly to the seminal Poole model. By 1983 the Report already dealt with GDP targeting. Money demand functions and velocity were also discussed in informed ways in various Reports.

The Reports also show the influence of academic work in their discussions of interest rates as well as saving. Thus, while the 1968 Report, like the academic literature at the time, ignored the Fisher effect, the 1970 Report, written about two year after Friedman's (1968) presidential address, discussed it. The 1993 Report discussed Ricardian equivalence.

The academic literature has also influenced discussions of inflation. Thus while the 1968 Report still employed the loose formulation of the wage-price spiral that was then standard in

the academic literature, the 1971 Report advanced to the expectations-augmented Phillips curve. The 1975 Report talked in terms of the natural rate of unemployment and a long-run vertical supply curve. This was in the year when Lipsey and Steiner (1975), as had Samuelson (1973) two years earlier, told beginning students about a long-run trade-off.⁶ The 1982 Report rejected the policy of not lowering the inflation rate but holding it stable and letting expectations adjust, because as monetarist academics had argued: "Once a positive rate of inflation is accepted it becomes difficult to argue against a slightly higher rate." (56) It also pointed to the

positive correlation between the level and the variance of the inflation rate, a much discussed topic in academic journals.

The shift from the old macroeconomics to the new also showed up in a discussion of the importance of reputational effects for monetary policy, with the 1972 Report pointing to the possibility that easing monetary policy might have restrictive effects because it could raise the expected inflation rate, and hence long interest rates. Time inconsistency was discussed in the 1990 Report, while the 1996 Report took up the effects of deficit reduction in a model with forward looking agents.

The contributions of academic economists also appears in discussions of international macroeconomics, for example in the explanation exchange-rate overshooting in the 1977

Report. The 1979 Report presented a good discussion of exchange rate flexibility, while the 1987 Report took up the absorption approach. Other discussions that academic readers will find familiar deal with implicit contract theory (1978 and 1981), the coordination problem stressed by the new Keynesians (1981), Lucas' island model (1982), the effect of the Smoot-Hawley tariff on the Great Depression (1989), the role of fiscal discipline in terminating hyper-inflations (1993), and hysteresis (1997).

With respect to methods, the 1968 and 1969 Reports mentioned econometric models as being used in the Administration's forecasts and in calculating the effects of a tax cut. But shortly after that the CEA may have become disillusioned with these models. The 1973 Report (61) complained about their "poor record" in predicting the inflation rate.

The 1982 Report is of particular interest. Its discussion of monetary policy ranged from the choice of regimes (the gold standard, and a monetary growth rate rule) to some specifics, such as tying the discount rate to open-market rates. It has an unusual number of references to the academic literature ranging from 1930 (Cassel) to 1981 (Bordo). A plausible explanation is that this was the first Report issued by the Reagan Administration, which tried to institute substantial changes in economic policy. To justify such changes it helps to discuss economics in a more fundamental way than is usual in Reports that recommend minor adjustments to current policies. It is for such fundamental rethinking that the work of academics is most relevant.

All in all, academic research has found its way

into the Reports. It is obvious that their authors have read the academic literature, and their arguments are professionally respectable. But the Reports are intended to be political documents, not scholarly endeavors. This shows up in the choice of problems discussed. The extent to which a Report uses the academic literature therefore depends in part on the availability of academic work on the particular problems that concern the Administration. And it depends also on whether this work supports the case that the Report wants to make.

2. The FOMC transcripts

The FOMC transcripts are much more closely related to actual policy decisions than are the CEA Reports since they record the arguments that various FOMC participants advance during the decision-making process. Both the five year delay in their publication and the excruciating amount of technical detail they contain ensure that they have only a small scholarly audience and no influence on public opinion.

They show that the FOMC's thinking was slow to reflect the great improvement in the quality and volume of both academic research and the Federal Reserve's own research that began in the 1960s. Even now the Transcripts do not show much direct influence of academic research. In the 1960s they indicated that the FOMC did not value highly the analytic work of its own economic departments, not to speak of the work of academic economists. Chairman William McChesney Martin, who is quoted as saying: "no more economists" (cited in Matusow, 1998 25), preferred to rely on his own intuition. As Robert Hetzel (1995; 2) explained: "Martin valued [the] Research ... [department] for an ability to organize information rather than an ability to think analytically about policy. He valued individuals who could offer anecdotal information about economic activity more highly than economists." Thus in the early 1960's the staff was actually forbidden to make any forecasts. Then, when the Board of Governors acquired its own econometric model it did not, at first, take the forecasts based in part on this model seriously. Only very gradually did it learn to trust them.

An example of how the FOMC ignored academic research is its treatment in the lag in the effect of monetary policy. Though this is clearly critical in deciding when and how to change policy, for a

long time the FOMC paid it little attention. In the late 1960s the median estimate of FOMC members was probably around 6 to 9 months. While this was in line with some of the academic estimates, it was much shorter than the lag shown by the Board's own econometric model, as well as by most other econometric models. Although subsequently, FOMC members seem on the whole to have lengthened their estimates of the lag, for a long time they did not seem to catch up with the long lags that appeared in more and more of the academic literature.

This discrepancy is probably due to several factors. One may be that Chairman Martin focused his attention predominantly on the immediate money-market effect of monetary policy rather than on its effect on GDP. Another is the FOMC's distrust of forecasts, particularly forecasts extending for a year or more. If it had conceded that the lag is long it might then have had to admit that could not operate an effective counter-cyclical policy. It was less disturbing to ignore the long lags shown by its own and other econometric models (see Mayer, 1990).

One might expect that in the 1960s and 1970s as more and more academic economists (including several eminent ones) were appointed to the Board of Governors and to Reserve Bank presidencies (and thus became FOMC members) its discussions would have become more hospitable to the work of academics. But this did not happen, even when one of the world's leading economists, Arthur Burns, became chairman. Burns, according to some reports, did make extensive use of the expertise of the academically oriented National Bureau of Economic Research. But he made little use of other types of academic research. For example, by 1974 many money demand functions had been successfully fitted (see Laidler 1974) But Burns did not refer to any them when discussing velocity. Instead, he commented that velocity: "depended on confidence in economic prospects. When confidence was weak, a large addition to the money stock might lie idle, but when confidence strengthened the existing stock of money could finance an enormous expansion." (FOMC, December 1974; 103-4)

The neglect of the academic literature is probably related to the FOMC's reluctance to discuss the basic issues on which the academic literature focuses on, such as the existence of a long-run unemployment - inflation trade off.⁷ With so many good economists on the

FOMC that seems surprising. One likely explanation is that the FOMC is afraid that confronting such issues might generate ideological or paradigmatic splits that would not only make it hard to agree on a specific policy at each meeting, but would also politicize the Fed. Another possibility is that if FOMC members were to make the basic ideas underlying their decisions explicit, then they would suffer feelings of regret and guilt if any of these ideas were later disconfirmed (see Mayer 1990). Furthermore, by being silent on basic issues the Fed presents less of a target to its critics. Another possibility is that not explicating even to themselves the theoretical framework that underlies their policy decisions gives FOMC members great flexibility in deciding what policy to adopt at each meeting. This flexibility permits them to make the necessary compromises between sound policy and the policy dictated by political pressures without feeling embarrassed (see Hetzel 1990).

FOMC discussions therefore focus on the current state of the economy and on how that is likely to change, as well as on the effect that a small change in the federal funds rate at this particular time would have. The academic literature has almost nothing to say directly about the former, and little to say about the latter.

Implicitly, however, academic research does make an important contribution to FOMC discussions by influencing staff research at the Board and at each of the Banks. How much of this FOMC members read is hard to say. but research by academics underlies some remarks at FOMC meetings. Thus when Greenspan (FOMC 1987, February; 34) stated that we "ought to take M1 seriously in a sense," one can see the quantity theory at work at least indirectly. Such influences may well be greater now than before, because FOMC discussions now seem much more sophisticated than they were in the early 1970s. Not only do technical terms occasionally appear, but the whole tone of the discussion seems different. Before, even though they might have thought like economists, on the whole, FOMC members did not sound like economists. Now they do. When reading the discussions during the 1970s I frequently felt that FOMC members were making arguments that someone familiar with the literature would not have

made. The more recent discussions do not give this impression. Academic research now also plays an important role through the econometric models that the staff uses along with other information in preparing its forecasts, and in presenting policy simulations. And as Edison and Marquez (forthcoming) have shown, these forecasts and simulations do play an important role in FOMC deliberations.

Does the failure of academic research to play an explicit role in FOMC discussions signal a serious shortcoming of academic research or in the FOMC's procedures? One might well argue that it is an appropriate division of labor, that research on what policy should be followed on a month-to-month basis should be left to central-bank technicians. But one can also make a case that although academics can hardly be as knowledgeable as central bankers about the details of central banking, they should work on actual policy-making, as for example Karl Brunner and Allan Meltzer have done (see Brunner and Meltzer 1989).

3. Some other federal reserve documents. Some of the Federal Reserve Banks and occasionally the Board publish papers by their staffs (see for instance, Board of Governors 1981) or papers given at conferences they sponsor. Many of these papers are essentially academic papers. All the Banks also issues a Review that contains papers interacting with the academic literature, some through literature surveys and others through original research. However, their articles are not official documents, since they do not necessarily represent the views of the Federal Reserve.

4. Congressional documents Congress plays an important role in the determining of U.S. macro policy because of its power over fiscal policy, and also to a much lesser extent because of its influence over the Fed. Its documents consist primarily of transcripts of the Hearings and Reports of its committees. Some of these committees have also commissioned and published compendia of studies and other material by academics.

The Joint Committee on the Economic Report, on which sit the chairpersons of all committees with primary responsibility for macro-policy, is unusually receptive to academic work. Thus at its Hearing on "Monetarism in the United States and the United Kingdom" (U.S. Congress, Joint Economic Committee 1981) two eminent

academics, David Laidler and Allen Meltzer, were the only witnesses. At a subsequent Hearing on "The Future of Monetary Policy," four of the fifteen witnesses were academics (U.S. Congr., Joint Economic Committee 1982), and so were five of twenty-one witnesses at a 1988 Hearings. (U.S.

Congr., Joint Economic Committee 1988). At its Hearings on the 1992 Report the Committee heard from Paul Krugman, Robert Gordon, Paul Samuelson, James Tobin and George Perry (U.S. Congr. Joint Economic Committee 1992). How much influence such Hearings have is hard to say. Usually only a few committee members, sometimes only a single one, attend, but one can hope that at least some of the other members have their staff read the testimony and summarize it for them.

VII. Europe and Japan

In Britain the Bank of England publishes the "Minutes" of its Monetary Policy Committee (MPC). These focus on the current and future states of the economy. Though its members may discuss more general and fundamental issues of macroeconomic theory elsewhere, in its official minutes it, too, avoids fundamental discussions.⁸ Even so, academic research has an indirect effect by setting the framework for the discussion, and sometimes it also shows up in the details. For example, the MPC has used the life cycle hypothesis to estimate the effect of windfall gains on consumption, and it has discussed a buffer-stock money demand function (1997, June; December). It also referred to its own simulations showing that its credibility affects the Phillips curve (1997, December). While such a conclusion is impressionistic and cannot be properly documented, it seems that the MPC makes efficient use of the relevant academic research.

Since the Bank of England has been given an inflation target and since it aims for transparency, it also issues a quarterly Inflation Report. This is a factual document that uses the available economic and econometric work where it is relevant. For example, its discussion of the monetary aggregates includes a section on Divisia money (a measure which weighs various monetary components by the relative opportunity cost of holding them), and in discussing unemployment it takes up the hysteresis hypothesis (Bank of England 1994 May; 34).

The Bank's Quarterly Bulletin carries some articles that are more technical. For example, one calculates

implicit forward rates from the Black-Scholes model (Bank of England, February, 1997), while others deal with the optimal rate of disinflation (November 1996), the debate about monetary policy rules (August, 1996), and the concept of broad money (May 1996). These articles, which supplement less technical discussions of current policy, often cite the academic literature. Some are written by academics. The important Hendry-Ericsson critique of Friedman and Schwartz's book on monetary trends that appeared in the American Economic Review (Hendry and Ericsson 1991) is a condensation of a version first published by the Bank in a set of papers by its academic consultants (Bank of England 1983).

The Deputy Governor described to an academic audience the use the use the Bank makes of academic research: 'A considerable amount of research ... [on how monetary policy operates] has been undertaken within the Bank for many years. And in doing so, we have drawn heavily on the ideas and techniques developed by academics outside the Bank.' (Harold Davies 1996; 464) He also mentioned that the Bank includes "within a wide range of information variables" policy rules developed by two academics, Bennett McCalum. and John Taylor. They "provide useful reference points", though they are not used as an automatic pilot. (Bank of England, 1996; 464-5). The Bank has come a long way from the time of Governor Norman, who when asked for the reason for a certain decision responded: "Reasons, Mr. Chairman? I don't have reasons, I have instincts." (cited in Boyle 1967; 327).

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In France the central bank issues a Bulletin (Banque de France). Although it is mainly concerned with elucidating current developments it has also published some articles on issues on which academic research has something to contribute directly.⁹ And these articles do make use of academic research, some carrying explicit citations to the academic literature. In addition, the Banque de France has run joint conferences with universities.

In Germany relevant documents are published by the Sachverständigenrat (Council of Economic Experts) and the Bundesbank. The Sachverständigenrat has only advisory responsibilities, and individual members can dissent from the majority's recommendations. It is essentially a government think tank, and does not speak for the Administration. That raises the question of how much influence it has on policy, and hence whether its reports

give much of a clue about the influence of academic research on policy. They are technically well informed, provide a ready home for academic ideas, and are not reluctant to discuss basic issues, such as the choice of targets for monetary policy (see Sachverständigenrat 1988; 171-73).

The Bundesbank's Annual Reports contain little economic analysis, and focus more on surveying recent events and trends, topics on which the academic literature makes more of an indirect than a direct contribution. But there are points where academic influence is direct. Two examples are a sophisticated and up-to-date discussion of the costs of inflation, and concern that, by raising the expected inflation rate, an easing of monetary policy might raise rather than lower long-term interest rates (Bundesbank 1993; 61-62). There are references to a paper by Akerlof and Perry in the Brookings Papers on Economic Activity and to an NBER working-paper by Feldstein (Bundesbank 1996; 81, 85). Overall, the Annual Reports give the impression that any lack of reference to the academic literature reflects neither ignorance of, nor disdain for the current macro-economic literature, but primarily that this literature does not relate directly to the issues under discussion.

The situation is similar at the Bank of Italy. Again, the Annual Report is largely a factual survey. A section called "The Governor's Concluding Remarks", does go beyond that and evaluates various policy options, but at least in the ones I have been able to obtain (1981-83), it is written in such general terms, that there is usually little opportunity for a delectable academic influence to show up. However, the 1981 Report (16) does have a sophisticated discussion of the marginal propensity to consume and the wealth effect.

The Bank of Japan publishes a Quarterly Bulletin that very occasionally has articles employing academic research. Summaries of speeches by the Bank's officials posted on the internet also use academic research in the few cases where it that is relevant, and give the impression of familiarity with academic research. The Bank of Japan also publishes a scholarly journal, Monetary and Economic Studies, that carries papers given by outstanding academic economists at a conference sponsored by the Bank.

The Swedish central bank has an official inflation target and issues a quarterly Inflation Report. Only

occasionally does this Report make explicit contact with academic economics, though indirectly its forecasts are based on the work of academic modelers. The Bank also publishes a Quarterly Review that contains articles on topics such as monetary policy and unemployment, the management of short-term interest rates, and electronic money. In one article the Governor explained the vertical Phillips curve, and also cited publications by Krugman, Lindbeck and Snower that had appeared only a few years earlier (Bäckström 1997).

The Swiss National Bank issues an annual volume of essays. These contain what are essentially academic papers. Thus the 1989 volume (Swiss National Bank 1989) contains essays testing the rational expectations hypothesis, a popularly written, but technically sound discussion of the role of monetary policy in a small economy, and a paper that uses an error-correction model to measure the effect of the real exchange rate on exports.

Finally, a report by the deputies to the Group of Ten (Group of Deputies 1995) provides an interesting contrast. It draws on the academic literature much more than any of the above-discussed documents. Even the most demanding academic could not ask for more. One likely reason for this is that the Deputies were asked to address technical questions, i.e., the future levels of saving, investment and interest rates. Another reason is that it is not a political document either in the sense of being a program for action, or an attempt to influence public opinion. Instead, it seems intended for an audience of economists, both in the national bureaucracies and in academia.

VIII. The specific questions.

What do these documents tell us about the three questions I was asked to address? The answer to the first, how academic research enters official documents, is that presumably even though few policy-makers themselves read academic journals, their staff does, and also those policymakers who were trained as economists may remember what they were taught.

To the second question whether academic ideas enter in simplified form, the answer is normally, yes. That is not surprising because important policy ideas can generally be presented in simple, intuitive language. I found no evidence that such translations distort the ideas.

The third question asks whether there is a pattern in the ideas that are accepted. Yes,

those that fit into policy-makers' beliefs and wishes are accepted. For example, a number of central bank documents have picked up the fashionable idea that central banks should have substantial independence. If papers were to appear that provide strong econometric evidence that central bank independence is undesirable, I doubt that they would get nearly as much attention in these documents. Similarly, it is hardly surprising that the case against counter-cyclical policy appeared in Reports of Republican rather than Democratic CEA's.

The final question deals with the lag in the transmission of ideas. That can be very short, even working-papers can be cited - or if the idea is unwelcome - very long. One cannot be more specific because the date at which an idea is accepted by the profession is unclear, and the date of first publication is irrelevant. For example, would it be fair to say that it took the Fed more two hundred years to accept the idea that the quantity of money is the main determinant of the price level because David Hume stated this in the 1750s?

IX. A summing up

The glass is half full and half empty. On the one hand one can safely say that there are no major dominant views among macro-economists that fail to find a resonance in government documents - but on the other hand there are few such (non-trivial) views. The authors of government documents are familiar with academic research and tend to employ it - but mainly when it supports, or at least does not conflict, with what they would like to say. The sequence "academic research -> government documents -> policy decisions" is a seriously incomplete way to describe the situation - but the alternative sequence: "policy decisions -> government documents" does not deny any influence to academic research, because academic research can influence policy without first going through government documents. The contrasts between the publications of the Sachverständigenrat, the CEA Reports and the FOMC Transcripts suggests that the further removed a document is from policy-making the more likely is it to explicitly use academic research. But by inspiring both the underlying analytic framework and the econometric models used in forecasting and in policy simulations, academic research has a strong implicit influence even on documents close to policy-making.

If the present role of academic research in

government documents indicates that we academics do not have as much influence on policy as we think appropriate, we should remember that our record is hardly unblemished. In the U.S. in the 1960s and early 1970s leading economists advocated a policy that generated stagflation. Then, shortly after monetarism achieved substantial influence in academia velocity became unstable, and monetarist advice was no longer so useful. Would the world have been better off if subsequently policy-makers had paid much attention to the rise of new classical theory? In the 1950s, Fed chairman Martin sounded naive to economists when he stressed the dangers of inflation, and when he said that interest rates were determined by the grass roots of the country, not by the central bank. Now it no longer sounds so naive.

But perhaps we finally do have it right and policy-makers should pay more attention to us. What can we do to further this? One answer might be to reduce the extent of our disagreement, but that is hard to do.¹⁰ Also, paying more attention to the day-to-day, hands-on problems that policymakers face would help. More contact with them and their staffs

would facilitate that. It all too easy now for central bankers to feel that academic economists do not grasp the vital details of the problems that central banks face. Estrella and Mishkin (1998) provide a persuasive example of how from the viewpoint of central bankers academic discussions of the NAIRU miss the mark. There is much to be said for delineating speech communities by common problems, rather than by the source of employment or methods used.

Endnotes

* I am indebted for helpful comments to participants in the Conference.

1. For surveys of economist's opinions see Kearl et al (1979); Alston, Kearl and Vaughan (1982); Frey et al (1984), Ricketts and Shoesmith (1990). Not all the economists in these surveys were academics, but the overall results probably apply to academics.

2. This does not necessarily mean that the reasons cited in government documents are unimportant. By influencing public opinion they can expand (or contract) the area of admissible policy discussions, and they can legitimize the positions that officials in various government agencies can safely take.

3. Up to March 1976 they are called "Minutes", but do not

differ much from the later Transcripts, so I will call both Transcripts. They report what each member said, unlike the "Minutes" in the Federal Reserve Bulletin. They are voluminous and I have not read them all. I did read - rather cursorily - the January 1965 -March 1976 Transcripts in connection with another study (Mayer, 1999), but only sampled the those for 1985-1992. They are issued with a five year lag, and the latest set available to me was 1992.

4. An indication of the limits to the Council's independence is an incident during the Nixon Administration, when one of Nixon's political aides persuaded CEA Chairperson McCracken to change the estimates of GNP and unemployment he presented in congressional testimony (Matusow 1998; 179)

5. The reference to technology shocks is a rare, perhaps unique, bow to real business cycle theory. It appeared several months prior to the Kydland-Prescott (1982) paper, and thus shows that even radical ideas can get into government documents rapidly.

6. Even in the next edition Samuelson (1976), while conceding that the short-run Phillips curve will change in the long run, did not warn about it becoming vertical.

7. This statement is subject to the caveat that for the years 1985-92 I read only one Transcript per year, and had neither the April 1976-84 nor the 1993-98 Transcripts available. Moreover, there were probably some discussions of fundamental issues among many FOMC members outside of FOMC meetings.

8. I looked only at the Minutes from June 1997 to January 1998. It is possible, but unlikely, that more academic discussions occurred at other meetings.

9. Because of the difficulty of getting access in time to more recent issues my discussion is based on 1992 and 1993 issues.

10. Paradoxically, lack of disagreement among academic economists might reduce the frequency with which they are cited in official documents, since those on the other side of the argument would no longer be able to cite some academic economists in support.