UNIVERSIDADE TÉCNICA DE LISBOA

INSTITUTO SUPERIOR DE ECONOMIA E GESTÃO

DOUTORAMENTO EM ECONOMIA

EFFECTS OF FISCAL POLICY: MEASUREMENT ISSUES AND STRUCTURAL CHANGE

MANUEL BERNARDO VIDEIRA COUTINHO RODRIGUES PEREIRA

Orientação:

Prof. Doutor Artur Silva Lopes e Prof. Doutor Luís F. Costa

Presidente do Júri:

Reitor da Universidade Técnica de Lisboa

Vogais:

Professor Doutor Huw David Dixon (Univ. de Cardiff)

Professora Doutora Maria Leonor M. Ribeiro Modesto (Univ. Católica Portuguesa)

Professor Doutor Miguel Pedro Brito St.Aubyn (Univ. Técnica de Lisboa)

Professor Doutor Artur Carlos Barros da Silva Lopes (Univ. Técnica de Lisboa)

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Abstract

Considerable uncertainty surrounds the macroeconomic effects of fiscal policy. The research presented in this dissertation firstly aims at improving on the methods used to measure such effects - which feature vector autoregressions (VARs) as the basic tool. The investigation is partly carried out using structural VARs. The methodological innovations in that part concern the joint identification of fiscal shocks vis-a-vis monetary policy shocks and the estimation of a model with time-varying parameters using a non-recursive identification scheme. I also use reduced-form VARs to assess the effects of a novel shock measure, derived from budget forecasts, that is arguably free of anticipatory movements. The second aim of the dissertation is to present empirical results for the US, focusing on the way the impacts of the government budget on the economy have changed over time.

The thesis is divided into three essays. In the first one, I present evidence that taxes and transfers were the most important force attenuating the severity of recessions up to the eighties, surpassing the role of monetary policy. Fiscal policy has, however, become less effective in stimulating output in the course of the last decades. The findings in the second and the third essays corroborate this conclusion. Such a change in effectiveness is particularly marked for the shock measure that is relatively unaffected by anticipation, which features multipliers with non-conventional signs in the recent period.

In general, these findings call for more research on the factors that intervene in the transmission mechanism of fiscal policy and can bring about important variation in its impacts.

Key Words: fiscal policy, macroeconomic stabilization, structural change, anticipated policy, vector autoregressions, time-varying parameter models

JEL codes: E62, E63, E32

Contents

List of Tables

List of Figures

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Lastly, this thesis is dedicated to *Carmen*, *Bubu* and *Francis*.

Introductory remarks

General motivation

The macroeconomic effects of fiscal policy have been one of the most debated issues in macroeconomics and remain one of the most controversial ones. As far as the impact on real variables is concerned, over the last years theoretical efforts have focused on altering the neoclassical benchmark model, in order to come to larger positive output multipliers of government expenditure (and less negative or non-negative consumption multipliers), which are more in accordance with the common reading of the empirical evidence.[1] Typical features of these New Keynesian models are sticky wages, sticky prices and a negative relationship between the markup ratio and output (see Hall (2009) for a review). In particular, a declining markup ratio in the course of business cycle expansions allows the real wage to rise at the same time the work volume expands. Other New Keynesian models consider a share of consumers that do not follow the life-cycle principle, and are immune to the wealth shock caused by the change in the present discounted value of taxes. More recently, attention has been devoted to the case of fiscal loosening when an economy hits the zero nominal interest bound. One can mention, among the contributions in this vein, Galí et al. (2007), Ravn et al. (2007), Monacelli and Perotti (2008) and Christiano et al. (2009). Some of this work and other more empirically oriented (e.g. Barro and Redlick (2009), Cogan et al. (2010) and Costa and Afonso (2010)) was spurred by the revival in the use of discretionary budgetary policy to stabilize the economy in the wake of the 2008-09 recession. This area of research had been relatively neglected in the years before, since

[1] Although such a reading is not fully accurate, as discussed in the next section.

something close to a consensus had emerged that countercyclical action by governments ought be confined to automatic stabilization.

New Keynesian models are generally consistent with positive output multipliers of government spending but, depending on the precise assumptions and values of the parameters, these can assume a wide range of values. At the same time, it is unknown which assumptions and parameter values adhere better to reality and should be favoured. Therefore, in spite of the progress, they leave room for considerable uncertainty.[2] This came to light in the recent debate surrounding the effects of the American Recovery and Reinvestment Act of 2009 (particularly in the controversy between Romer and Bernstein (2009) and Cogan et al. (2010)). It adds to this that other theoretical ideas outside the main strand of the literature are sometimes brought into the debate. For instance, very recently the European Central Bank has revived the «expansionary fiscal contraction» hypothesis in connection with the adoption of a tight fiscal policy in Europe despite of the incipient stage of recovery (see European Central Bank (2010) and Krugman (2010)).

One would expect empirical work to play an important role in steering this debate. Several issues contribute, however, to making the measurement of the effects of fiscal policy a particularly hard task. The first one is the simultaneity issue, pervasive in empirical macroeconomics, and affecting particularly the budget categories reactive to the business cycle. Indeed, taxes and certain categories of transfers move with output, due to the action of the automatic stabilizers, and this effect has to be isolated in order to estimate the contemporaneous impact they may have on output. A similar question holds for prices. As a consequence, recursive identification schemes, common in monetary policy, cannot be applied in such cases. Discretionary policy also reacts to the economy but, at relatively high frequencies as with quarterly data, it seems reasonable to assume that such a reaction is not contemporaneous.

[2] Most of the recent theoretical work has focused on the effects of spending, but such uncertainty extends to the effects of taxes (and transfers). The theoretical benchmark is the complete discounting case, with lump-sum taxes and infinite horizons, where changes in taxes produce no effect on output. Deviations from this case, imply different outcomes. For instance, if the horizons are finite, households will face a change in their net lifetime income, implying a movement in consumption and output akin to the traditional Keynesian model.

A second major issue has to do with anticipation. It is well known that, while the implementation lags help with the exogeneity of fiscal shocks, they create, at the same time, an anticipation problem. Whenever changes in taxes and spending have to go through a legislative process, as it happens with the measures encompassed in the annual budget, they become known ahead of their implementation. This raises the issue of the right timing of fiscal shocks. Indeed, models estimated on the basis of outturn data will capture policy measures at time of implementation, but one may argue that this is too late and that the right timing is when the information about future measures becomes public.

In this dissertation I will also deal with the impact of fiscal policy on interest rates. The empirical issues to address are even trickier in this case. On the one hand, anticipation is believed to be particularly important for financial markets which are supposed to react promptly to news. In particular, there is a strand of literature (originating in Wachtel and Young (1987)) that has gathered evidence of a positive relationship between deficit announcements and interest rates in the United States (US). On the other hand, there is an aggregation issue: financial market variables move at very high frequencies and fiscal shocks are typically derived from lower frequency data. This may blur the estimated relationships. Perhaps partly reflecting these difficulties, the empirical results in the field do not point to clear-cut conclusions (Elmendorf and Mankiw (1999) and Gale and Orszag (2002)). Some support the conventional wisdom positing that fiscal loosening raises interest rates, but many others could not find evidence of such an impact.

Notwithstanding all these difficulties, the debate about the effects of fiscal policy cannot be settled on purely theoretical grounds, and further empirical work may be the most fruitful way to move forward. The aim of the research presented in the thesis is to contribute to the methods used in the field and present results with a focus on structural change, as there is a growing body of evidence that the effects of fiscal policy over time have been unstable (see the survey in the next section). The dissertation is divided into three essays. The first one contributes to the literature by considering fiscal and monetary policies jointly in a structural vector autoregression (SVAR). More specifically, it adresses the identification of shocks and an assessment of the relative roles of both policies in terms of (de)stabilizing output. This latter issue has been relatively neglected as far as empirical work is concerned. The second essay puts forward a new measure of fiscal shocks that intends to avoid the abovementioned problem of anticipation, being at the same time arguably free from endogeneity. In these two essays, structural change is accommodated through split- and rolling-sample estimates. The third essay formally addresses the change in the effects of budget variables over time. It considers a structural fiscal policy VAR with coefficients that vary throughout the sample with a non-recursive identification system, which is a novelty in the literature. These models can capture many forms of time variation, particularly gradual changes in the parameters.

Evidence from vector autoregressions about the effects of fiscal policy

In the course of the dissertation, I will make extensive use of VARs that have become the main analytical tool to assess the effects of the government budget on real variables. It is thus appropriate to survey the literature in the field, which has pursued two main lines of investigation: the SVAR and the event-study approaches (see the discussion in Perotti (2007)).

The first approach uses SVAR models which combine institutional information about the tax and transfer systems with other restrictions on the contemporaneous relationships among the macroeconomic variables. This strand of the literature originated in the seminal work of Blanchard and Perotti (2002), later extended in Perotti (2004), that appeared in the wake of the SVAR literature for monetary policy (Bernanke and Blinder (1992), Bernanke and Mihov (1998) and Christiano et al. (1999)). However, while monetary policy VARs exploited extensively the block recursiveness assumption of policy variables vis-a-vis macroeconomic variables, this assumption is less useful for fiscal policy, as already pointed out.[3] In the Blanchard and Perotti's identification methodology, institutional information

[3] Note that if one is focusing on purchases of goods and services *only*, a recursive scheme can be assumed.

is used to calibrate the contemporaneous effects of the economy on the fiscal variables. The effects of fiscal policy on the economy are, by contrast, estimated. In doing this, a sluggish reaction of fiscal authorities to movements in economic activity is assumed, and the identification of shocks to each side of the budget, to spending and taxes net of transfers, is achieved through an arbitrary ordering of these variables.

SVAR shock measures are computed on the basis of outturn data and have been criticized for capturing anticipated policy (see Ramey (2009)). That is, for that part of the shock anticipated by agents, the right timing is missed. Nevertheless, certain type of agents, such as those facing liquidity constraints, may respond to fiscal measures at time of implementation. There is much evidence in this direction, as discussed at the beginning of the first essay. On the other hand, notwithstanding all the information available, some uncertainty remains as to the exact impact of fiscal measures. In this context, one may usefully distinguish between current-policy shocks, where the SVAR shocks fit, and expected-policy shocks. Shocks of the first type will convey additional information relative to those of the second type - the second essay offers a discussion of this point.

The other main strand of empirical literature on the effects of fiscal policy is the so-called event-study approach of Edelberg et al. (1999) and Burnside et al. (2003), building on earlier work in Ramey and Shapiro (1998). This focuses on the response of the economy to three major shocks to military spending in the postwar US history, the Korean War, the Vietnam War and the Carter-Reagan buildup, which Ramey and Shapiro date according with their assessment of the moment when the media began to forecast the surge in spending.[4] These episodes have an added claim to exogeneity and their effects can be studied on the basis of a reduced-form VAR system, with the inclusion of dummy variables at the relevant dates. The timing of the Ramey-Shapiro episodes is chosen so that shocks capture unanticipated policy; this is nevertheless based on the

But one cannot order both output and prices either before or after expenditure. On the one hand, one must allow a contemporaneous impact of prices on government spending because the latter is defined in real (often also per capita) terms. On the other hand, one must allow an impact of spending on GDP. A possibility is then to order prices first, followed by government spending and lastly by output.

[4] More recently, Eichenbaum and Fisher (2004) applied the same methodology to study the impact of the September 11, 2001 on the US economy.

authors' reading of history and is not undisputable. Indeed, considerable uncertainty remains when the news about the likely military build-up first come up, for instance, as to its actual size, the weapon systems government will purchase, who among competing contractors will be chosen as the supplier, and so on. Another drawback to note is that inference is based on a small number of shocks.

In my second essay, the effects of the shocks are measured by embedding them into a reduced-form VAR, which is essentially the strategy employed in the event-study approach. Another contribution following this method is Romer and Romer (2010), who construct a shock series for taxes on the basis of quantitative and qualitative information about the legislated tax changes in the post-WW II period. They classify tax changes into endogenous and exogenous according to policymakers' intentions and statements. The first category includes, for instance, countercyclical actions, and the second one actions to foster long term growth. The Romers present the estimated impact of the various types of tax changes on the economy.

It is worth mentioning a third approach to the identification of fiscal policy shocks that imposes restrictions on the signs of the impulse-responses, represented by Mountford and Uhlig (2009). This does not rely either on identification restrictions in an SVAR sense, i.e. on the contemporaneous relationships between the variables. It is nevertheless less appealing because identification is achieved in such a way that it may strongly condition the findings. For instance, revenue shocks correspond to situations in which revenue and output do not covary positively, when they covary positively the event is identified as a business cycle shock. A negative impact of tax shocks on activity is thus assumed from the beginning.

I now give an overview of the main findings in the cited empirical work, which refers almost exclusively to the US.[5] I will come back at several points in the dissertation to comparisons between other researchers' findings and my own results which are as well for the US economy. A first aspect to stress is that part of this literature deals only with

[5] An exception is Perotti (2004) who considers a number of OECD countries: Australia, Canada, Germany, the United Kingdom and the US. It is worth noting that his results differ somewhat across countries.

the effects of expenditure shocks. The output multiplier following spending shocks in Blanchard and Perotti (2002) is 0.9 or 1.3, depending on the specification, and the private consumption multiplier is positive as well. The multiplier in the Ramey-Shapiro episodes is smaller, around 0.5, and the effect on private consumption is negative. Therefore, surveys typically associate SVAR evidence with comparatively larger output multipliers, supporting a Keynesian or New Keynesian reading of the results. This sort of reading of the SVAR evidence, as opposed to the event-study approach deemed to back up the neoclassical prior, has been put forward by Ramey (2009). Note, however, that subsequent SVAR evidence showed considerable subsample instability in the output multiplier. In particular, Perotti (2004) considers two subsamples: 1960:1-1979:4 and 1980:1-2001:4 and, in the second one, obtains a multiplier that is essentially zero from the second quarter on. The event-study approach started to consider subsample instability only recently on the basis of improved measures of defense shocks (see Ramey (2009)).[6] In one of these measures, when the sample starts in 1955 instead of 1947 (ending in 2008), the sign of the output response reverts from positive to negative from the second period on.

Concerning net tax shocks, the multiplier in Blanchard and Perotti's paper assumes sizeable negative values (-0.8 or -1.3, depending on the specification). Again, this evidence is played down by Perotti (2004) who finds a marked change in responses over time. In effect, a negative multiplier is confined to the first subsample; in the second one, positive tax shocks raise output instead of contracting it. Romer and Romer (2010) find large negative tax multipliers, which depend on the precise specification as well but are generally below -2.0. They explain this result by the fact that their shock measure is comparatively less affected by endogeneity with GDP, which is expected to bias the results toward finding less negative multipliers. They also consider subsample sensitivity - the response of output after 1980 is clearly weaker although the negative sign is not reversed.

[6] Note that the Ramey-Shapiro episodes gave raise to three shocks only, which precluded to address the issue on the basis of them.

Plan of the dissertation and main findings

The thesis has three chapters corresponding to three separate but interrelated essays in that they propose improvements in the way the effects of fiscal policy are measured and focus on structural change.

The first essay is entitled «Empirical evidence on the stabilizing role of fiscal and monetary policies in the US». It considers fiscal and monetary policy in conjunction, in the framework of an SVAR, generalizing the Blanchard and Perotti's identification scheme. In contrast to previous studies, such as Perotti (2004) and Canzoneri et al. (2002), a nonzero semi-elasticity of taxes to changes in the short-term rate is assumed. The sample consists of quarterly national accounts data from 1955:1 to 2005:4 for the US. I consider two separate subsamples, 1955:1-1979:4 and 1980:1-2005:4, and for some exercises rolling samples of 25 years. The definition of budget variables is the usual one in the SVAR context, taxes net of transfers on the revenue side, and purchases of goods and services on the expenditure side. Evidence is obtained both on the basis of SVAR tools, such as impulse-reponses and variance decompositions, and counterfactual simulations. The essay addresses the relative roles of fiscal and monetary policies in terms of stabilizing output, as well as the potential destabilizing role of exogenous policies, that is, the contribution of the respective disturbances to the volatility of output.

Monetary and fiscal disturbances contributed much less to output volatility in the second part of the sample, starting in 1980, than in the first one. This result stems from their smaller impact and, to a lesser extent, from a decline in their variance. Systematic taxes net of transfers were the most important stabilizing force in the course of postwar recessions until the eighties. Monetary policy had a comparatively smaller role in offsetting the downturns in activity at those episodes. Net taxes have, however, suffered a marked lost of effectiveness in recent decades. The feedback between the two budget variables also appears to have changed in the course of the sample. While a budget-balancing movement is captured in the first subsample, results for the subsequent period show the two budget variables diverging in the short-run.

The second essay is entitled «A new measure of fiscal shocks based on budget forecasts and its implications». This new shock measure is for the US and is meant to capture changes in anticipated fiscal policy that are exogenous to economic developments. The information about anticipated policy is taken from the budget projections regularly released by the Office for Management and Budget. Fiscal variables now follow the budget definitions and relate to federal government, but are defined similarly to the first essay, i.e. taxes less social outlays and budget expenditure excluding such outlays. The forecast of a fiscal variable for a given fiscal year and announcement is regressed on an information set including the base-year figure and macroeconomic data, to get the exogenous component of the forecast. The shock measure is based on the revision in that component between two consecutive announcements, for the same fiscal year. As releases can be precisely dated, generally to the day, I am able to investigate the responses using high-frequency data. The announcements are for the period 1968-2008.

Key economic variables such as output and interest rates respond quickly and significantly to a realization of the estimated shock, and the responses have changed substantially over the last decades. I differentiate between the impacts in two subsamples: 1968-1988 and 1989-2008. The evidence in the first one is very much consistent with conventional predictions: positive deficit shocks raise interest rates and output simultaneously. In the second half of the sample, the impacts are the opposite: revisions that signal loosening have a contractionary impact on economic activity and reduce interest rates. I also gather evidence against the view that revisions to anticipated fiscal policy affect aggregate demand only indirectly, via the impact on interest rates: both the financial markets and agents appear to behave in a forward-looking manner.

The third essay is entitled «Fiscal policy and time variation in the US». It takes up a Blanchard and Perotti-like identification scheme in the framework of a VAR with time-varying parameters. Such models are not estimated but simulated with the aid of Bayesian tools such as the Gibbs sampler as applied to state-space models. The data used are, as in the first essay, from quarterly national accounts and the variables are defined in the same way. A number of initial years is now lost due to the technicalities of the simulation procedure, and the relevant sample period starts in 1965:2, going through 2009:2.

I conclude that fiscal policy has lost capacity to stimulate output in the course of the sample, this trend being more pronounced for taxes net of transfers than for government expenditure. Despite such an evolution, the multipliers keep conventional signs throughout. There is moderate support for an increase in policy effectiveness in the course of recessionary episodes. An investigation of changes in fiscal policy conduct indicates that the countercyclical activism of net taxes became stronger over time, and appears to have reached a maximum during the 2008-09 recession.

To sum up, the evidence presented in the dissertation fully justifies the emphasis placed on structural change in the impact of fiscal policy on the economy. While the three essays consistently suggest a weakening of that impact, its intensity is open to further investigation. The split-sample estimates (first essay) point to more a pronounced change in effectiveness than the model with time-varying coefficients (third essay), in an otherwise similar structural VAR. This raises the possibility that split-sample estimates exacerbate the magnitude of the break, and speaks for a more generalized use of models allowing gradual time-variation in the coefficients. Such models have, however, the drawback of requiring heavy simulation techniques, still not widely used. On the other hand, structural change was at its strongest for the shock measure developed in the second essay, featuring a change in the sign of the responses over the last decades. Since this shock measure is less plagued by the shortcomings affecting SVAR measures, it is expectable that it triggers sharper responses.

The research presented has also some implications for the theoretical and empirical work. In general, theoretical literature should consider more the issue of variation in policy effectiveness, including the possibility of unconventional multipliers. Indeed, although the possibility of negative output multipliers of spending has been occasionally noted (see Basu and Kimball (2003) and Hall (2009)), as said, most of the effort has been put on modifying the neoclassical benchmark in the direction of positive and larger figures. At the same time, there is a need to investigate how (and which) underlying conditions in the economy interfere with the way fiscal policy operates, and the empirical literature should accompany this effort by gathering evidence. In other words, we need to learn more about the transmission mechanism of fiscal policy.

Chapter 1

Empirical evidence on the stabilizing role of fiscal and monetary policies in the US

1.1 Introduction

The (de)stabilizing role of fiscal and monetary policies can be assessed by considering the role of exogenous policies as a source of business cycle fluctuations and also the contribution of endogenous policies to dampen them. These aspects depend in turn on how active policies have been and the impact on output they have had. The goal of this paper is to present evidence about such questions for the US taking as a reference data for 1955 to 2005. Structural change over the period is accounted for on the basis of split-sample (separating pre- and post-1980 periods) and rolling-sample estimates. There is a great deal of literature seeking to determine changes in the way monetary policy was conducted and its effect on the economy, including Boivin (2006), Boivin and Giannoni (2006), Primiceri (2005) and Sims and Zha (2006) among others. Such an idea has been much less explored for the fiscal side.[1] This paper takes up the task and focuses, in addition, on aspects

[1] Two exceptions are Auerbach (2002) and Taylor (2000), but they differ substantially from the approach followed here, among other things in that they estimate single-equation relationships.

arising from the joint consideration of fiscal and monetary policies. From the empirical viewpoint, it also relates to the literature on the great moderation (see, for instance, Stock and Watson (2002), Ahmed et al. (2004), Canova (2009) and references therein), as far as the role played by policymakers in it is concerned.

The analysis is made in the framework of a simple, textbook-like macroeconomic system with five equations: three of them are structural - a monetary policy rule and equations for government revenue and expenditure, the latter capturing both the reaction function of fiscal authorities and automatic responses to macroeconomic variables. There are two additional equations which can be seen as solved out versions, respectively, for GDP and inflation, of standard IS and aggregate supply curves. The disturbances in these last equations do not have, contrary to the policy disturbances, a structural interpretation (that is, I do not disentangle aggregate supply and private aggregate demand innovations). This set-up is described in Section 1.3.1 and has some common points with that in Blanchard and Watson (1984), one of the earliest contributions to the SVAR literature.

The macroeconomic system is cast and estimated in the form of an identified VAR. Thus I have to tackle joint identification of monetary and fiscal policy innovations and this links with a few studies that dealt with the same question, such as Perotti (2004) and Canzoneri et al. (2002). The most prominent simultaneity issue arising in this context - the co-movement between taxes and the monetary policy instrument, the federal funds rate - has, however, not received much attention before. I model this carefully by allowing a contemporaneous nonzero semi-elasticity of taxes to the short-term interest rate. Some of the contemporaneous coefficients in the equations for the fiscal variables are calibrated using non-sample information, following Blanchard and Perotti (2002). This requires that I generalize the OECD method to derive the elasticity of personal income taxes to GDP that they use, to encompass the elasticities to prices and the short-term interest rate.

A general remark about the approach followed is that I take it as given that endogenous and exogenous policies have real effects and attempt to assess them. Also as preliminary point, Section 1.2 addresses the ability of identified VARs to estimate the effects of fiscal policy on GDP, which has been questioned (see Ramey (2009)) on the grounds that SVAR fiscal disturbances are anticipated by agents.

Section 1.4 addresses the first question above, that is, the contribution of exogenous policies to the volatility of output. The key finding is that policy disturbances both on the fiscal and monetary sides were much less destabilizing in the second part of the sample. Such a result was to an important extent brought about by a smaller impact of those disturbances on output. In fact, there is evidence of a generalized weakening of exogenous policies' effectiveness - particularly marked for taxes and transfers which feature a unconventional multiplier in the more recent period. Improved policy in the form of a smaller variance of the shocks is also found to have contributed to the decline in volatility in the case of the federal funds rate and government spending.

Section 1.5 presents additional empirical results concerning the behavior of monetary and fiscal policies, in particular their responsiveness to the economy. Changes in the federal funds rate and taxes net of transfers are dominated by the respective systematic components. By contrast, the exogenous component dominates fluctuations in government expenditure. As far as structural change is concerned, the sensitiveness of net taxes to economic developments is found to have increased in recent decades. A similar analysis for the funds rate was not conclusive. Another issue addressed is the feedback between the two budget variables. The results in the first subsample, ending in 1980, indicate that changes in expenditure lead changes in taxes, and capture a budget-balancing movement in the short-run. In contrast, results for the subsequent period show a long-lasting divergence between the two budget variables. I interpret this latter result as reflecting the conduct of debt stabilization policies from early to mid-eighties on and, toward the end of the sample, «spending the surplus» policies.

Section 1.6 attempts to quantify the stabilizing role played by endogenous policies. This is done by means of counterfactual simulations. Specifically, I simulate the system under counterfactual assumptions which are, respectively, absence of the *exogenous* component and of the *endogenous* component of policy. By comparing the historical behavior of the variables with the implied behavior, I am able to break down actual changes in policy variables during contractions into the endogenous and exogenous components, and measure the output loss avoided at trough for each of them. I do this for the eight NBER business cycle contractions between 1955 and 2005. There is evidence that taxes and transfers were the most important force attenuating the severity of recessions up to the eighties. They have markedly lost effectiveness over time, however, in parallel with the same phenomenon for the respective exogenous shocks. The offsetting effect of systematic monetary policy was comparatively smaller in the past and this appears to be accounted for by a slow buildup of the output response against the length of the average recession. Except for more protracted recessions, full impact tends be felt already at the initial stages of the recovery. Government spending has played a minor stabilizing role throughout the whole sample period.

1.2 On the ability of SVARs to capture fiscal policy shocks

A correct measurement of the effects of fiscal policy in an SVAR context requires, in the first place, that the shocks are exogenous in relation to the variable, say GDP, on which the impact is being determined. The portion of the fiscal variables labelled as the «shock» must not respond to GDP nor, more generally, to variables correlated with it, such as interest rates and prices. As a first point, it is important to ascertain whether there are fiscal policy actions meeting such requirements in practice. Romer and Romer (2010) investigated the legislated tax changes in the US since World War II and distinguished among four types of motivations behind them: to react to the business cycle, to finance changes in spending, to raise long-run growth and to cope with an inherited deficit (which could be also stated as to cope with growing debt). The Romers classify the last two as exogenous with respect to output fluctuations, and show that they have been clearly more prevalent than their endogenous counterparts throughout the postwar period. Turning to budget outlays, examples of exogenous, or at least party exogenous, interventions are also not difficult to find. These include, for instance, build-ups in defense spending and the creation and extension of certain social programs largely unrelated to the business cycle (like Medicaid). Another fiscal intervention concerns the annual across-the-board adjustments to the pay of government employees. Such adjustments are partly endogenous to past inflation to the extent that they make up for it (adding to the other increases in pay related to the advancement of employees), but they are also determined by exogenous policy goals as, for instance, expenditure restraint or achieving wage rates comparable with those in the private sector. The last kind of goals can be very important in practice. This can be seen by analyzing the pay adjustments in the General Schedule which covers most Federal government civilian employees, in the years spanning since mid-fifties.[2] Until the beginning of the seventies, a time when the comparability principle ranked high on the political agenda (see Smith (1982)), the cumulative increase stood over 70 p.p. above the variation in the CPI. By contrast, during the high inflation period from 1973 to 1981 that followed, pay updates fell systematically short of the rise in prices (more than 50 p.p. below, in cumulative terms). Since 1982 the adjustments have been more in line with inflation (negative difference of 19 p.p. in relation to the CPI from 1983 to 2005). Changes in social transfers and purchases of goods and services undertaken in response to business cycle conditions have been infrequent and small over the last decades (Romer and Romer (1994)). Hence, contrary to monetary policy for which the existence of exogenous interventions has been a matter of debate, in the case of fiscal policy many actions fall within this category, even if identification assumptions are generally needed to isolate them.

A second requirement for a correct measurement of the effects of fiscal policy is that the timing of the shocks corresponds to the moment in which they actually impacted economic activity. If fiscal shocks, albeit exogenous, can be anticipated by agents *and* if agents modify their behavior accordingly, identified VARs will still not estimate properly their effects on GDP. This issue is clearly of importance in the case of fiscal policy because changes to taxes and spending typically go through a legislative process. It is appropriate to put forward some considerations about how serious this problem might be (see also the discussion in Perotti (2004)).

[2] The Federal Civilian Workforce Statistics (US Office of Personnel Management) present a chronology of the General Schedule Pay Legislation since 1945.

A way to get evidence about the importance of anticipation effects is through micro studies addressing the actual behavior of agents in the wake of implementation of fiscal measures. There is a large body of empirical evidence about the way households react to changes in taxes (also some about the reaction to changes in social benefits and, in any case, one might expect the same type of behavior to apply). This has been gathered by the literature documenting the so-called «natural tax experiments» (see Johnston et al. (2006) and the references they cite), and provides support to the hypothesis that tax changes do affect households' behavior at the time revenue is collected. For instance, predictable tax liabilities and refunds have significant contemporaneous impacts on consumption. It is illustrative in this respect that although Romer and Romer (2010) follow a non-SVAR methodology, they date their benchmark tax shocks according to when legislated changes impacted revenue. In the same vein, one can assume that households do not smooth consumption in anticipation of small changes in disposable income resulting from shocks to compensation of government employees. No comparable micro evidence as to the behavior of firms in face of information about pending fiscal changes is (to my knowledge) available.

An issue that admittedly may disturb the measurement of fiscal shocks is the way purchases of durable goods are recorded in NIPAs. NIPAs mostly record such purchases on a cash disbursements basis (see BEA (2005)) while the full amount of the acquisition (known by the supplier from the moment the contract is signed) is likely to be the relevant fact from the private sector's viewpoint. Thus National Accounts will typically record an initial payment which does not reflect the full size of the «true» shock. Still, an important part of purchases of goods and services is not affected by the issue.

1.3 Methodology

1.3.1 Equations and identifying restrictions

The results presented in this paper are based on the following system:

$$g_t = a_0^{g,p} p_t + a_0^{g,ff} ff_t + \sum_{i=1}^{4} \mathbf{a}_i^g \mathbf{x}_{t-i} + b_0^{g,nt} e_t^{nt} + e_t^g, \tag{1.1}$$

$$nt_t = a_0^{nt,y} y_t + a_0^{nt,p} p_t + a_0^{nt,ff} ff_t + \sum_{i=1}^4 \mathbf{a}_i^{nt} \mathbf{x}_{t-i} + b_0^{nt,g} e_t^g + e_t^{nt}, \tag{1.2}$$

$$ff_t = a_0^{ff,y} y_t + a_0^{ff,p} p_t + \sum_{i=1}^4 \mathbf{a}_i^{ff} \mathbf{x}_{t-i} + e_t^{ff}, \tag{1.3}$$

$$y_t = a_0^{y,g} g_t + a_0^{y,nt} nt_t + \sum_{i=1}^4 \mathbf{a}_i^{y} \mathbf{x}_{t-i} + w_t^y, \tag{1.4}$$

$$p_t = a_0^{p,g} g_t + a_0^{p,nt} nt_t + \sum_{i=1}^4 \mathbf{a}_i^{p} \mathbf{x}_{t-i} + w_t^p. \tag{1.5}$$

Purchases of goods and services (including of capital goods) are denoted by g_t, taxes net of transfers by nt_t, the federal funds rate by ff_t, detrended GDP by y_t and inflation by p_t. The vector $\mathbf{x}_t$ includes the variables in the system: $\mathbf{x}_t = [g_t, nt_t, ff_t, y_t, p_t]'$. The structural policy innovations (e_t^g, e_t^{nt} and e_t^{ff}) are orthogonal to each other and also to w_t^y and w_t^p, while these two innovations will be in general correlated. As usual in the SVAR methodology, the identification restrictions are imposed on the contemporaneous coefficients, while the lag structure of the model (the $\mathbf{a}_i$'s) is left unrestricted. As explained below, I assume that either $b_0^{g,nt} = 0$ or $b_0^{nt,g} = 0$ in equations (1.1) and (1.2). I did not include deterministic terms in the equations; a discussion of the assumptions about the low-frequency properties of series is offered below.

The system was estimated with quarterly data, which were seasonally adjusted (except for the funds rate) at source. The lag length was set to 4. The fiscal variables and output are the logarithms of the levels measured in real and *per capita* terms. Inflation is calculated from the GDP deflator and, like the federal funds rate, is measured at annual rates. I give more details about the definition of the fiscal variables and sources in Appendix 1.B. Throughout the paper, nt_t is also sometimes called simply «taxes», and g_t «expenditure» or «spending». The reference sample is 1955:1-2005:4. Since I want to explore the changes in the behavior and effects of policies over time, I generally present results for two subsamples, splitting the main sample into two parts: 1955:1-1979:4 and 1980:1-2005:4.

The counterfactual exercises are carried out on the basis of rolling subsamples, spanning as well over 25 years, so that the recessions approximately coincide with the middle of them.

The first two equations above are those for government expenditure and net taxes.[3] If one assumes, following Blanchard and Perotti (2002), that any government reaction to macroeconomic conditions takes more than one quarter to be implemented, the a_0's in (1.1) and (1.2) can be interpreted as the *automatic* contemporaneous response of the fiscal variables to macroeconomic conditions. Such a response may be brought about, in particular, by mechanisms built in the tax code, transfer programs and budgeting procedures. Since the fiscal variables are in real terms, deflated by the GDP deflator, this also induces a contemporaneous co-movement between them and prices (these points are detailed in the discussion of the calibration of the parameters). The parameters $a_0^{nt,y}$ and $a_0^{nt,p}$ will capture the automatic responses of net taxes to activity and prices within the quarter, and $a_0^{g,p}$ of government spending to prices. It appears relatively undisputable that spending does not react to contemporaneous movements in activity, and therefore current GDP is absent from equation (1.1). Turning to the semi-elasticity of taxes to the short-term interest rate, can $a_0^{nt,ff}$ be set to zero? I argue it cannot. This point deserves special attention since it lies at the very heart of the joint identification of monetary and fiscal policy, and has hardly been dealt with by the literature. It is therefore addressed separately below. As to the corresponding parameter in the expenditure equation, $a_0^{g,ff}$, one expects it to be indeed equal to zero, since there is no obvious mechanism linking purchases of goods and services and interest rates within the quarter. However, once $a_0^{nt,ff} \neq 0$, the estimation of $a_0^{g,ff}$ comes at no additional cost. Hence I estimate this coefficient rather than impose a zero restriction, in order to have exact identification (see Section 1.3.5). Note further that I allow either the structural innovation to net taxes to enter the equation for g_t, or the structural innovation to expenditure to enter the

[3] To consider each side of the budget separately, rather than the deficit, allows us to investigate potential differentiated behavior and impacts. The definition of revenue as taxes net of transfers is in line with their impact operating through the standard aggregate demand channel. Such definition has the practical advantage of lumping together in the revenue variable the budget categories that respond automatically to the business cycle.

equation for nt_t (borrowing from Blanchard and Perotti). It makes sense to do so because when setting fiscal policy, government takes into consideration both sides of the budget. Identification of the respective parameters ($b_0^{g,nt}$ and $b_0^{nt,g}$) requires that one of them is set to zero or, equivalently, that net taxes and spending are ordered one after the other. Given that such an identification restriction is arbitrary, the results have to be checked under both possibilities.

The coefficients in $\mathbf{a}^g$ and $\mathbf{a}^{nt}$ will reflect any systematic response of government to macroeconomic developments (the fiscal policy rule), the lagged automatic reaction to the economy, and the persistence in budget variables brought about by the way fiscal policy is set, since the government budget and tax laws are not designed from scratch each year.[4] Non-systematic policy is captured by the structural fiscal shocks (e_t^g and e_t^{nt}) whose effects one endeavours to trace using the SVAR methodology.

Equations (1.1) and (1.2) are supposed to capture fiscal policy rules. Literature on this issue for the US such as Bohn (1998) and, more recently, Favero and Giavazzi (2007) argued that fiscal authorities have acted according to a government debt stabilization motive besides an output stabilization one. I did not take debt on board in the system, nevertheless. The reason is that the fiscal actions to cope with growing debt or protracted deficits approximately qualify as exogenous, for they are unrelated to current economic developments. It is, thus, acceptable that they are part of the shocks that will be used to measure the macroeconomic impact of fiscal policy. Note that such debt stabilization motive can be distinguished from the short-term interaction between the sides of the budget, say, when taxes are raised simultaneously with measures that increase spending. In this case there may be endogeneity, and the current and lagged values of net taxes in (1.1) or of expenditure in (1.2) ensure that the estimated shocks will not be «polluted» by it. In any case, the evidence as to whether debt enters significantly the fiscal equations is weak. Estimating the reduced-form of the system with lags of the variables in $\mathbf{x}_t$ and the

[4] Here it is interesting to draw a parallel with monetary policy rules based on interest rate targeting, in which the Federal Reserve is, in principle, freer to set the interest rate at a given level. Nevertheless, the literature has assumed that the Fed smooths the changes in interest rates, implying that the rule includes lags of the policy variable (see, for instance, Clarida et al. (2000)). In the case of fiscal policy there are even more reasons to follow such a specification.

lagged debt to GDP ratio (lags 1 to 4 in turn), the latter regressor was not significant at standard levels in the spending and net tax equations (though the coefficient signs were the expected ones, that is, negative and positive, respectively). My reading of these results is that fiscal variables may have responded to government debt mostly in an nonlinear fashion: for example, corrective action was triggered only upon the cumulative imbalance reaching a certain threshold (as in the period of sharp growth in the government debt to GDP ratio, from 1982 to 1995).

Equation (1.3) is the monetary policy rule and builds on well known literature showing that (i) the federal funds rate provides a good measure of the monetary policy instrument, and (ii) the rule can be modelled as the federal funds rate responding to output gap and to the deviation of inflation from a target (see e.g. Bernanke and Blinder (1992), Taylor (1993) and Christiano et al. (1999)). In this context it is common to assume that monetary authorities observe the developments in activity and inflation and react accordingly within the quarter, whereas GDP and inflation are slow-moving variables that respond with a certain delay to changes in the interest rates. I follow this assumption.[5] A systematic response of monetary authorities to contemporaneous fiscal developments is ruled out, that is, the current values of government budget variables do not enter the monetary policy rule. As it is well known, monetary policy VARs usually include a commodity price indicator in order to eliminate the so-called «price puzzle». I do not follow this practice because, on the one hand, the issue matters essentially for the impact on inflation, while the focus here is a narrow one, on activity. Moreover, since estimation is based on short time periods, it is important to keep the system as small as possible.

Consider, finally, equations (1.4) and (1.5). I do not identify non-policy innovations, and these equations may be seen as solved-out versions for output and prices of the IS and aggregate supply relationships. Since current fiscal variables enter the former relationship,

[5] To check the practical implications of this assumption, I experimented with ff_t ordered before y_t and p_t as well. Switching the ordering does not matter much for the estimated parameters in the fiscal equations, nor for the effects of fiscal innovations on output over time. It matters for the effects of monetary policy shocks on GDP, in particular, in the initial quarters (this point is analyzed in Christiano et al. (1999)). One gets the counter-intuitive result that a tightening in monetary policy causes a *positive* initial reaction on GDP.

they will enter both equations as well. Moreover, the disturbances w_t^y and w_t^p will be correlated, and a function of the underlying structural private aggregate spending and aggregate supply innovations.[6]

1.3.2 The semi-elasticity of net taxes to the short-term rate

I address first a preliminary issue concerning the definition of net taxes which has a direct implication for the way this variable responds to the interest rate. Net taxes are equal to taxes minus transfers and the latter can be computed either *including* or *excluding* interest paid (there are examples of both treatments in the literature). The first definition implicitly assumes that the fiscal structural shocks originate in the full budget, and the second one that they originate in the primary budget. I argue that the latter is the appropriate definition. SVARs are supposed to identify and trace the effects of discretionary non-systematic fiscal policy. However, the direct determinants of interest outlays are the interest rates and the stock of debt and not (except in very particular cases) discretionary fiscal policy actions. In other words, the structural fiscal innovations do not enter an equation (actually, rather an identity) explaining government interest outlays. From the point of view of empirical work, sticking to the primary budget implies that the econometrician has to deal with only one channel through which the unexpected movements in interest rates may impact movements in net taxes - the tax base - ruling out an additional impact via the interest bill. Thus the precise issue is whether $a_0^{nt,ff}$ can be set to zero, when net taxes are defined *without* considering interest paid, as in this paper.

The correlation between the residuals of the reduced-form equations for net taxes and the funds rate is around 0.19 and 0.42, respectively, in the first and second subsamples. It is thus reasonably high. Naturally that correlation is partly caused by a common response of the two variables to the business cycle, in the first case reflecting the action of the automatic stabilizers, in the second one due to the action of the Federal Reserve (and a

[6] Let the contemporaneous part of the IS curve be given by $y_t = fiscal\ variables + \alpha p_t + \varepsilon_t^d$ and aggregate supply by $y_t = \beta p_t + \varepsilon_t^s$. The respective structural innovations are ε_t^d and ε_t^s. Equation (1.4) obtains solving out this system for y_t and (1.5) solving it out for p_t. Note that only the lagged interest rate, not the contemporaneous, enters the IS curve, which follows from ordering this variable after activity and prices.

similar argument applies to inflation). Nevertheless, the preliminary evidence is clearly against setting $a_0^{nt,ff}$ to zero. Note also that the opposite causality - a contemporaneous response of monetary policy to fiscal variables - seems less plausible and should imply an important correlation between the reduced-form residuals of federal funds and expenditure equations. The latter is, however, negligible (0.04 and -0.03 in the first and second subsamples, respectively).

1.3.3 Assumptions regarding the low-frequency properties of the data

Although the analysis in this paper is confined to the short-term effects of policies and does not rely on long-run identification restrictions, the sample spans over 50 years and, hence, some discussion of the assumptions about the low-frequency properties of the data is in order. There is no point in entering here the debate about unit root behavior versus stationarity around a deterministic linear trend of GDP for the US. In addition, both hypotheses might not be fully adequate as they do not accommodate the observed decline in the long-run GDP growth over the last decades (as noted by Blanchard (1989)). Note that the evolution of the fiscal variables throughout the sample (Figure 1.1) is also well characterized by a decreasing long-run growth rate. Therefore, I formalize the trends in GDP and budget variables as deterministic, but allow for a quadratic term in order to capture the change in average growth over time. This specification was used in Blanchard and Perotti (2002) and is also one of the measures of the output gap considered by Clarida et al. (2000) in the estimation of a monetary policy rule for the US. As the system also includes an interest rate and inflation for which it does not make sense to assume a trending behavior, the deterministic trends in GDP and fiscal variables are removed by OLS regression prior to estimation of the system.

If the time-series properties of GDP are controversial, those of the short-term interest rate and inflation are hardly less. Stationarity of both series follows from a great deal of theoretical models that rationalize the use of monetary policy rules. Visual inspection of the respective charts in Figure 1.1, however, indicates a long-run path difficult to square with stationarity around a single long-run mean - a driftless random walk appearing more

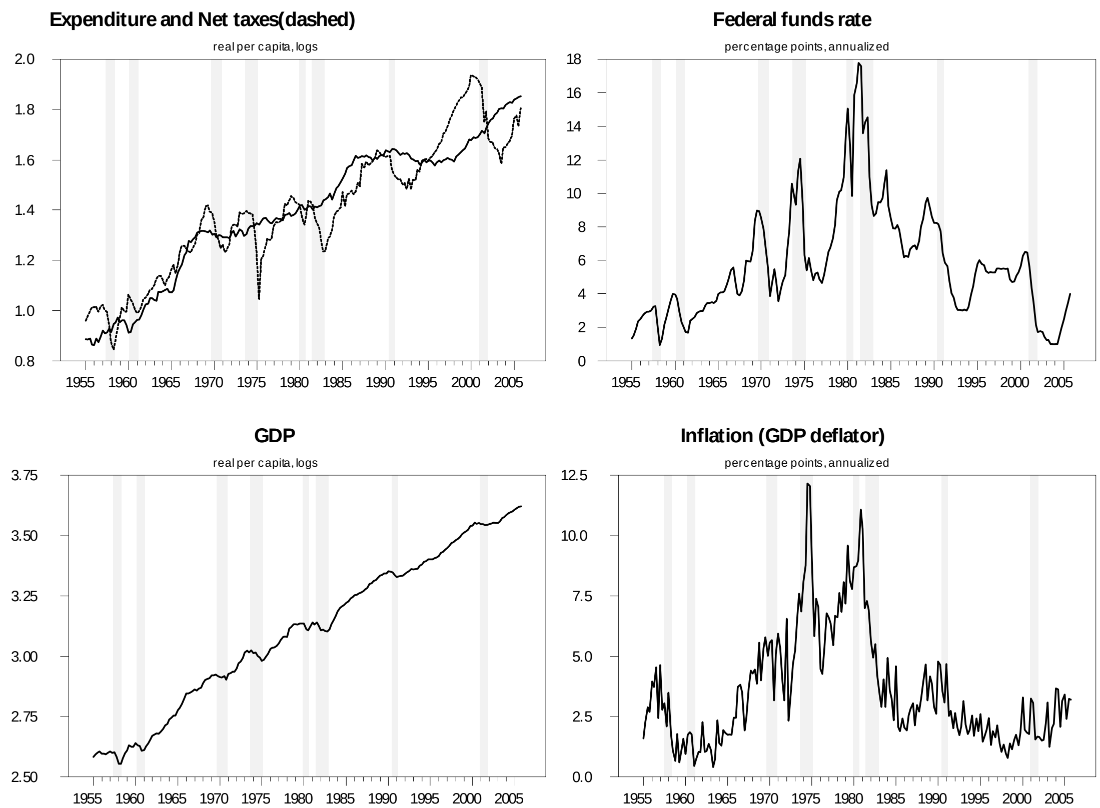

Figure 1.1: Macroeconomic variables, 1955:1-2005:4, and NBER recession dates

appropriate. However, alternative stationary characterizations would be equally plausible, such as around a long-run mean with an upward shift in the period from mid-seventies to mid-eighties. This assumption could be rationalized as a temporary increase in expected inflation implicit in the monetary policy rule, brought about by the inflationary process in the seventies. Nevertheless, as it would have some degree of arbitrariness - in particular, as to the moment of the upward shift in the mean - a conventional specification was chosen, including only a constant.

1.3.4 Calibration of elasticities of the government budget items

Before one looks into the identification and estimation of the system, it is appropriate to consider the possibility of calibrating some of the parameters in net tax and expenditure equations on the basis of institutional information, following Blanchard and Perotti (2002). They relied on the framework developed by the OECD (Giorno et al. (1995), updated in van den Noord (2000) and Girouard and André (2005)) to compute the elasticity of personal income taxes to GDP. In Appendix 1.A, I extend this by deriving analytical expressions for the elasticity of personal income taxes to prices and the semi-elasticity to the short-term interest rate. As discussed there, however, this latter parameter cannot be calibrated on the basis of the data made available by the OECD. I give in the appendix, in addition, the details underlying the calculation of the elasticities of the remaining taxes and transfers to activity and prices. Summing up, one is able to obtain $a_0^{nt,y}$, $a_0^{nt,p}$ and $a_0^{g,p}$ from non-sample information, but not $a_0^{nt,ff}$ which has to be estimated along with the other elements of the matrix of the contemporaneous coefficients.

Note that Perotti (2004) studied the effects of fiscal policy in a system with the interest rate and prices, but imposing a zero semi-elasticity of net taxes to the sort-term interest rate (and also using assumptions different from the ones used here in order to derive the elasticity to prices). This simplifies the identification task but, as seen, is not adequate in the US context (Perotti's study deals with the US in the framework of a group of OECD countries).[7]

[7] Canzoneri et al. (2002) also consider a system with the federal funds rate and prices, but concentrated

1.3.5 Identification and estimation

It is useful to write down the matrices with the contemporaneous structural coefficients, denoted by $\mathbf{A}_0$ and $\mathbf{B}_0$:

$$\mathbf{A}_0 = \begin{bmatrix} 1 & 0 & -a_0^{g,ff} & 0 & -(a_0^{g,p}) \\ 0 & 1 & -a_0^{nt,ff} & -(a_0^{nt,y}) & -(a_0^{nt,p}) \\ 0 & 0 & 1 & -a_0^{ff,y} & -a_0^{ff,p} \\ -a_0^{y,g} & -a_0^{y,nt} & 0 & 1 & 0 \\ -a_0^{p,g} & -a_0^{p,nt} & 0 & 0 & 1 \end{bmatrix} \mathbf{B}_0 = \begin{bmatrix} 1 & b_0^{g,nt} & 0 & 0 & 0 \\ b_0^{nt,g} & 1 & 0 & 0 & 0 \\ 0 & 0 & 1 & 0 & 0 \\ 0 & 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 0 & 1 \end{bmatrix}, \quad (1.6)$$

in which the calibrated parameters are in parentheses and it is assumed that either $b_0^{nt,g} = 0$ or $b_0^{g,nt} = 0$.

I estimated first the reduced-form system. There are 15 independent moments in the reduced-form covariance matrix and, excluding the information needed to obtain the 5 variances of the disturbances plus the covariance between w_t^y and w_t^p, one is left with 9 usable moments. Given the restrictions I impose on the contemporaneous coefficients and as I am able to compute $a_0^{nt,y}$, $a_0^{nt,p}$ and $a_0^{g,p}$ on the basis of non-sample information, there are 9 parameters to estimate. Therefore, the order condition is satisfied for exact identification. Contrary to Blanchard and Perotti (2002), the system cannot be estimated by instrumental variables (this would be, for instance, possible if the federal funds rate was predetermined with respect to all the other variables). I estimated the decomposition by maximum likelihood, but note that the case at hand differs slightly from standard structural decompositions in that the covariance matrix of the system 1.1 to 1.5 is not diagonal (as the covariance between w_t^y and w_t^p is nonzero).[8] Also note that the information

on modelling the impact of the short-term rate on government interest outlays. The definition of variables adopted here rules out this sort of co-movement, as already explained.

[8] This feature complicates the maximization process: as a strategy I took as initial values for the parameters in $\mathbf{A}_0$ and $\mathbf{B}_0$ the estimates obtained when a diagonal covariance matrix is imposed (i.e. corresponding to an overidentified system). Then, I reestimated allowing a non-diagonal covariance matrix and searching over a grid of initial values for the variances. The final results were very close both to the ones in the overidentification case and also to the ones where exact identification is obtained in a standard way, by imposing an arbitrary ordering between prices and output (i.e. if either prices entered equation

about the calibrated parameters is incorporated into $\mathbf{A}_0$ and $\mathbf{B}_0$ as average values over the subsamples.

1.4 The destabilizing role of exogenous policies

Variance decompositions are the natural starting point for assessing the effect of exogenous policy disturbances on the volatility of output. Table 1.1 shows the breakdown of the variance of the n-quarter ahead forecast error for output into the proportion accounted for by each of the three identified policy disturbances, and the macroeconomic disturbances as a whole.[9] I present the point estimates and one-standard error bands in brackets computed on the basis of Monte Carlo simulations[10], separately for the subsamples 1955:1-1979:4 and 1980:1-2005:4. As memo items are shown the point estimate for the long-run error variance, and the respective decomposition in absolute terms. This quantity is of interest because it theoretically matches the unconditional variance (whose estimate is also shown), helping explain the change between periods. The figures for the two statistics differ in practice, among other things, because they are small-sample estimates and the autoregressive representation assumed does not exactly hold. Nevertheless, the unconditional variance of output is well approximated, and the procedure is informative about how it was accounted for by the source disturbances in each of the subsamples considered.

(1.4) or output entered equation (1.5)).

[9] The latter is equal to the contribution associated with the variances and covariance of the disturbances in output and price equations. As it turns out, the role of the covariance term is very small in the case of GDP. It represents around ± 1 to ± 2 percent of the total long-run forecast error variance in both subsamples.

[10] The simulations were computed as follows. The OLS estimates of the reduced-form coefficients and covariance matrix were used to draw for the vector of coefficients (assuming normality). The covariance matrix and its structural factorization, obtained as described in Section 1.3.5, remains unchanged throughout. I found that a sizeable proportion of the replications (for instance, almost one half in the first sample) implied non-stationary systems, for which the long-run forecast error is not finite. I disregarded them. The one-standard error bands are computed as the percentiles 0.16 and 0.84 of the simulated distribution on the basis of 1000 draws.

Table 1.1: Variance decomposition for output

Proportion due to	Sample 1955-1979: Policy sh. e^g	e^{nt}	e^{ff}	Macroec. sh.	Sample 1980-2005: Policy sh. e^g	e^{nt}	e^{ff}	Macroec. sh.
1Q ahead	12.5	10.0	0.3	77.2	4.1	0.5	0.2	95.2
4Q ahead	12.4	16.0	2.2	69.4	2.5	1.6	2.5	93.4
	(7.5,18.8)	(9.2,21.9)	(0.6,6.7)	(59.7,75.7)	(1.0,5.6)	(0.5,4.8)	(1.0,6.2)	(85.4,95.1)
12Q ahead	21.1	19.5	8.7	50.6	5.7	2.9	1.9	89.6
	(10.0,30.8)	(7.5,27.6)	(2.8,26.5)	(35.5,60.8)	(1.8,14.0)	(1.4,12.4)	(1.4,7.1)	(71.0,90.9)
Long-run	23.9	19.9	7.2	49.0	5.7	4.2	3.8	86.3
	(9.7,32.7)	(6.1,26.2)	(4.8,28.5)	(33.2,60.6)	(5.4,25.2)	(3.2,17.7)	(2.5,10.3)	(54.9,81.6)
Memo:								
uncd. var.		*13.3*					*4.7*	
long-run FEV		*13.0*					*3.4*	
decomp.	*3.1*	*2.6*	*0.9*	*6.4*	*0.2*	*0.1*	*0.1*	*3.0*
shock var.	*1.7*	*5.1*	*0.3*		*0.9*	*4.9*	*0.5*	

Notes: Rows 1st to 4th: percentage of the forecast error variance for GDP accounted for by structural policy disturbances (government spending, net taxes and funds rate) and macroeconomic disturbances, point estimates with one-standard error bands in parenthesis. Rows 5th: unconditional variance of output. Row 6th and 7th: long-run forecast error variance and contribution of each disturbance (absolute figures, point estimates). Row 8th: standard error of policy shocks.

According to the point estimates, in the first half of the sample policy shocks jointly accounted for slightly more than half of long-run movements in output gap, not far from the corresponding figure of 44 per cent presented in Blanchard and Watson (1984) (using a sample from 47:1 to 82:4). In the period 1980-2005, in contrast, only around 15 percent of long-run GDP variance is attributable to them. Such point estimates in the second half of the sample are, however, close to the lower limit of the confidence bands for the policy disturbances and beyond the upper limit for the macroeconomic ones. Hence, this appears to overstate somewhat the loss of importance of the policy disturbances over time. If one takes instead the average of the simulated distributions (not shown), the share of long-run variance becomes about 1/3 and 2/3 for policy and non-policy disturbances, respectively, in the post-1980 period, against 1/2 for each group in the pre-1980 years. These figures still support a reduction in the relative role of exogenous policies as a source of output volatility in recent decades.

As said, the point estimate of the long-term forecast error variance mimics well the unconditional variance of output, including its well known decline in recent decades (note that the 2008-09 recession is beyond my sample period). That indicator goes down from 13.3 in the period 1955:1-1979:4 to 4.7 in 1980:1-2005:4, the phenomenon known as the great moderation.[11] Looking at the decomposition of the long-run variance in *absolute terms*, there is a generalized fall of the contribution across all disturbances in the post-1980 years. Such movement was sharper in the case of policy shocks leading to their mentioned loss of importance vis-a-vis their macroeconomic counterparts. On balance, evidence in Table 1.1 indicates that most of the decline in output volatility can ultimately be ascribed to the effect of exogenous policies.

In order to explore this result further, note that the contribution to a variable's variance of primary shocks depends both on the own variance and the impact on that variable (i.e. shock propagation). Over the two subsamples, the variance of policy shocks (last line in Table 1.1) remained broadly stable for net taxes, went down by about 50 percent for spending and up by a similar percentage for the federal funds rate. It is worth noting that the results for this last variable hinge on the inclusion of the early eighties in the second subsample, corresponding to the Volcker desinflation period, characterized by high volatility of the estimated shocks. In fact, when the second subsample is restricted to 1982:4-2005:5, the variance of monetary policy disturbances is around 0.1, less than half than in the pre-1980 period. Considering these latter figures, one concludes that improved exogenous monetary policy played some role in the decline of output variance. The same holds for government expenditure.[12] Nevertheless, the results also suggest a dampening of the effect of policy shocks on GDP not only in the case of net taxes but, given the magnitude of the decrease in the absolute contributions documented in Table 1.1, also in

[11] Recall that the paper uses detrended log real and *per capita* output. Other studies though using alternative volatility measures - for instance, defined on the basis of growth rates - and slightly different sample periods present reductions in the range from 40 to 50 percent in terms of standard deviation (see Ahmed et al. (2004)) which are similar to the one I get.

[12] In what concerns this variable, one may conjecture that the smaller deviation of pay updates from average inflation in the more recent period (see Section 1.2) contributed importantly to the reduced volatility.

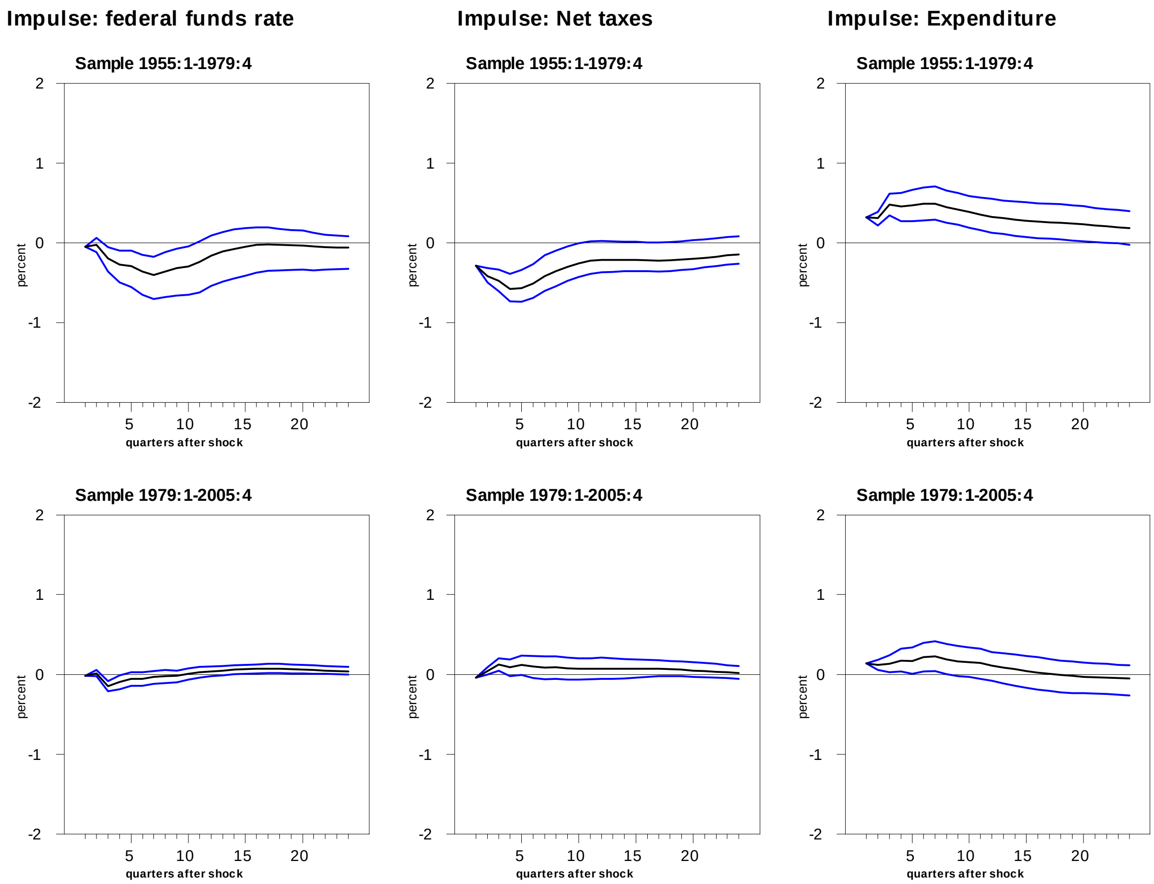

Figure 1.2: Impact of policy shocks on ouput

the case of the federal funds rate and spending.

Figure 1.2 depicts the effects of policy shocks of the *same* size in both subsamples (equal to the standard error in the first one) on output: point estimates and one-standard error confidence bands computed using the same methodology as for the variance decompositions. The charts show a marked subsample sensitivity with respect to the impact of exogenous fiscal policy on real activity. In the pre-1980 period the evidence is consistent with the Keynesian prior.[13] That impact becomes much smaller in the recent decades for

[13] It can also be reconciled with neoclassical models, since a distinction between macro theories could only be made by considering the effects on output components. This is not the objective of the study. Note, however, that the definition of the revenue variable is more suited for investigating the effects of fiscal policy in a Keynesian framework.

expenditure, while for net taxes there is even a perverse effect on output.

The impact multiplier of spending shocks on output in the first subsample is significant and stands at 1.3. It builds up subsequently to a peak multiplier around 2.0, reached around the third quarter. In the post-1980 years, in contrast, the corresponding peak impact figure is 1.0 only and the response stands overall on the brink of non-significance. Structural net tax innovations trigger a fall of output before 1980, the multiplier being equal to -0.7 on impact and -1.4 at trough - attained three quarters out. Note that the magnitude of the response depicted in Figure 1.2 is nevertheless similar to that for spending shocks, because the size of net tax shocks in currency is about twice larger. When the estimation period starts in 1980, the point estimate changes to a positive very small effect on output (maximum impact equal to 0.4), albeit barely significant.

Such break in the effectiveness of exogenous fiscal policy is in line with the evidence presented in Perotti (2004), also as regards the reversion of the sign of the impact of net taxes in recent decades (he considered two subsamples approximately coinciding with mine: 1960:1-1979:4 and 1980:1-2001:4). Blanchard and Perotti (2002) obtained relative large Keynesian effects for the two sides of the budget using data from 1960:1 to 1997:4. The specification they follow has important differences in comparison to the one followed here. For instance, it does not control for the monetary policy variable (nor for prices) and this may amplify the depressing effects of net tax shocks.[14] Nonetheless, the measured effectiveness of fiscal policy seems to depend more on the sample period than on the inclusion of the monetary policy instrument in the system. In particular, Blanchard and Perotti's sample does not comprise the years between end-1990s and mid-2000s, and their inclusion contributes to the measured decrease in fiscal policy effectiveness. For example, when I take the full sample but ending in 1997 instead of 2005, the spending multiplier goes down from 1.9 to 1.3. More on the time profile of policy effectiveness is given in Section 1.6.

There is also a weakening of the impact of exogenous monetary policy in recent decades.

[14] Assuming that net tax shocks respond positively to the funds rate, when the latter variable is omitted from net tax equation.

In the pre-1980 sample, the dynamics of GDP take more time to build up following monetary policy shocks, by comparison with their fiscal counterparts. I compute an indicator of relative policy effectiveness (analogous to the fiscal multiplier). The maximum impact on output is attained about seven quarters out and stands at about 0.7 percent per p.p. of change in the funds rate. In the second subsample, the profile of the response changes in that the peak impact is reached quicker. The relative effectiveness goes down to less than half of the figure for the years prior to 1980. Such findings are consistent with those presented elsewhere (for instance, Boivin and Giannoni (2006))

Different explanations have been put forward for the lost of influence of exogenous policies on output which, for the purposes of this paper, is useful to divide into two groups. The first one includes explanations coming from the behavior of the private sector, say, financial innovation may have allowed households and firms to protect themselves better against fluctuations in interest rates and budget aggregates. The second group includes explanations related to the conduct of *endogenous* policies. For instance, it has been argued that the weakening of the effect of fiscal policy shocks stems from the more powerful stabilizing role of monetary policy in recent decades. Such explanation has been put forward also to justify the smaller impact of monetary policy shocks. Similarly, if automatic stabilizers had become more effective in the post-1980 period, this would mitigate the effect of exogenous policies. In the subsequent sections, some evidence bearing on this second type of explanations is presented and does not favour it. The reaction of the federal funds rate following budget shocks (Section 1.5.3 below) is not consistent with a stronger dampening impact in the second subsample. At the same time, the counterfactual simulations carried out in the last part of the paper point to a smaller stabilizing effect of fiscal policy (the results for monetary policy being not informative).

In comparison to previous work dealing with the great moderation, the findings here presented are novel particularly as regards the role of exogenous fiscal policy in the moderation of GDP fluctuations. This possibility has been generally overlooked as studies centered on monetary side as far as policy explanations for the phenomenon were concerned.[15] Actually part of what these studies assigned to good luck may be accounted for by fiscal shocks, whose effect is captured by the general demand shock when they are omitted.

1.5 Some aspects about the behavior of monetary and fiscal policies

1.5.1 Responsiveness to the economy

This section deals with aspects concerning the behavior of monetary and fiscal policy that can be inferred still using standard VAR tools. The first one is the responsiveness of endogenous policies to economic developments. One way to assess this is by looking at the joint contribution of macroeconomic disturbances to the variance of the error in forecasting the policy variables (shown in Table 1.2). In order to compare the figures before and after 1980, I present as previously the long-run forecast error and absolute contributions, as well as the unconditional variance. Given that, as said, the behavior of the funds rate was markedly different at the beginning of the eighties in comparison to subsequently, I also present the estimates for the period 1982:4-2005:4 in square brackets.

Subsample sensitivity questions apart, there is a clear difference between the role of non-policy disturbances for the fluctuations in net taxes and spending. They explain about 1/2 of the long-run variation in the first case, but only around 1/4 in the second. A great deal of movements in net taxes are thus endogenous reflecting the reaction of both automatic and discretionary policies to output. While our methodology does not allow to distinguish between them, analyses typically indicate a much more important role of automatic responses, and the difference vis-a-vis the behavior of spending is consistent with this conclusion. In fact, own innovations to government expenditure are the most important source for the respective variance decomposition. Most movements in it

[15] An exception in this regard is Stock and Watson (2002) who in one of their exercises considered the role of fiscal shocks but concluded that they had played a negligible role. The approach they follow differs from the one here in that they take directly the structural shocks, say, monetary, fiscal, and so on from different studies. These shocks are not orthogonal by construction and cannot be used to decompose the variance of output as I do here.

pursued policy goals that cannot be traced back - and hence are exogenous - to macroeconomic conditions. Among these goals feature, as alluded to in Section 1.2, national security, expenditure restraint and wage comparability with the private sector. Finally, the important endogenous content of the monetary policy instrument reflects the conduct of stabilization actions by the Federal Reserve.

Table 1.2: Variance of policy variables accounted for by macroeconomic shocks

	Sample 1955-1979			Sample 1980-2005		
Proportion	Exp.	Net taxes	Fed. funds	Exp.	Net taxes	Fed. funds
1Q ahead	3.3	47.9	3.7	3.8	27.5	15.0 [11.8]
4Q ahead	5.9	70.3	39.8	4.7	53.7	73.6 [49.4]
	(3.6,12.5)	(62.0,74.6)	(30.3,47.9)	(3.2,10.4)	(44.5,60.4)	(66.9,76.2)
12Q ahead	16.8	52.1	56.2	8.2	60.3	84.1 [53.8]
	(10.7,30.7)	(39.5,60.3)	(38.2,64.9)	(5.1,22.4)	(43.0,67.8)	(67.5,84.7)
Long-run	27.7	54.9	61.6	12.2	47.8	78.3 [39.0]
	(18.6,47.0)	(37.9,61.6)	(35.6,64.8)	(10.4,37.9)	(30.2,58.7)	(43.4,76.8)
Memo:						
uncd. var.	*23.4*	*60.9*	*7.2*	*27.7*	*97.1*	*14.0 [6.2]*
long-run FEV	*27.5*	*79.8*	*12.8*	*34.1*	*88.1*	*7.9 [4.7]*
macroec. sh.	*7.6*	*43.8*	*7.9*	*3.9*	*42.1*	*6.1 [1.8]*
var. w_t^y		*1.2*			*0.3 [0.2]*	
var. w_t^p		*0.9*			*0.5 [0.4]*	

Notes: Rows 1st to 4th: percentage of the forecast error variance for policy variables accounted for by macroeconomic disturbances, point estimates with one-standard error bands in parenthesis. Rows 5th: unconditional variance of expenditure, net taxes and the federal funds rate. Row 6th and 7th: long-run forecast error variance and contribution of macroeconomic shocks (absolute figures, point estimates). Rows 8th and 9th: variance of each macroeconomic disturbance. In square brackets are figures computed restricting the second subsample to the period 1982:4-2005:4.

The proportion of the long-run variance of net taxes accounted for by the non-policy shocks slightly declined between the pre- and post-1980 periods, while the contribution measured in absolute terms remained stable. Note that there was a large rise of the unconditional variance which the statistic computed on the basis of the long-run forecast error does not fully replicate. In any case, the variance of the macroeconomic disturbances went down considerably between the two periods, as also shown in Table 1.2 (last two lines), particularly that of GDP which accounts for the bulk of the long run net tax fluctuations.[16] Hence, an increase in responsiveness has most likely occurred. The question arises whether this is accounted for by automatic or discretionary responses. Auerbach (2002) studied the sensitivity of economic stabilizers to the business cycle concluded that it has fluctuated over time but without a defined trend. The results I get are thus likely to be accounted for by discretionary responses, as suggested by Taylor (2000). This may be seen as surprising since legislated tax changes responding to cyclical developments were approximately confined to the period covered by the first subsample (see, for instance, Romer and Romer (2010)). Bush II tax cuts build possibly the only exception of a measure whose motivation was partly anti-recessionary in the post-1980 period until the end of my sample. Several factors may nevertheless contribute to an apparent increase in the anti-cyclical nature of discretionary policy. On the one hand, poor timing of countercyclical policy may blur the estimation of its pattern in the first subsample, and in some occasions after 1979 policy was countercyclical by coincidence: Reagan tax cuts, albeit not aiming at stimulating demand, were implemented in the course of the 1981-82 recession. On the other hand, the growth of revenue in the nineties was quicker than justified by the boom, since the incomes of people in higher tax brackets rose particularly fast. This may be captured in the estimation as a countercyclical response.

I now turn to the responsiveness of the federal funds rate to economic conditions. This issue has been intensively debated and a number of studies (see, for instance, Boivin (2006) and Primiceri (2005) and references therein) have found that the reaction of monetary authorities to the economy gained strength in recent decades, although this conclusion is not fully consensual. Unfortunately the unconditional variance of the funds rate is poorly approximated in both subsamples by the procedure I have been using. In the second subsample, this is perhaps due to the much higher volatility of the series in the early eighties (total variance decreases from 14.0 to 6.2 when the period 1980:1-1982:3 is excluded from the sample), which is not captured in the estimation with constant

[16] This quantity depends also on the change in the covariance between the two macroeconomic disturbances, as they are not orthogonal. Like for GDP, however, for net taxes the contribution of the covariance term is rather small.

coefficients throughout the subsample as a whole. The approximation improves a bit when the post-1982:3 sample is taken, but some underestimation of the unconditional variance by the long-run forecast error remains. In the first subsample, in contrast, there is an overestimation. Therefore, although the figures suggest a reduction of the contribution of macroeconomic disturbances in relative and absolute terms, given the uncertainty about the respective magnitudes and the fall in macroeconomic shocks' volatility, it is difficult to draw conclusions.

1.5.2 The feedback between net taxes and spending

A question of interest in this context is the feedback between the two sides of the budget. Figures in Table 1.3 indicate that expenditure shocks account for a sizeable proportion of the long-run movement in net taxes, about 1/5 in the first subsample and 1/4 in the second - though the confidence bands are wide. In contrast, innovations to net taxes explain a small amount of the forecast error variance for spending in both subsamples whatever the horizon taken (this is particularly pronounced if point estimates are taken, but note that the average of the simulated distribution also indicates a share of only 10 percent).

Table 1.3: Variance of fiscal variables accounted for by fiscal shocks

	Sample 1955-1979				Sample 1980-2005			
Proportion	Expenditure		Net taxes		Expenditure		Net taxes	
due to	e^g	e^{nt}	e^g	e^{nt}	e^g	e^{nt}	e^g	e^{nt}
1Q ahead	96.6	0.0	10.1	41.2	94.8	0.0	0.3	66.0
4Q ahead	90.8	0.8	10.0	11.1	90.3	0.2	1.3	42.7
	(80.6,91.7)	(0.3,3.5)	(6.0,15.4)	(9.3,14.3)	(81.7,92.3)	(0.2,2.5)	(0.5,4.3)	(33.9,49.7)
12Q ahead	77.8	2.5	23.4	8.5	87.9	0.4	5.9	32.4
	(54.2,77.8)	(0.9,9.7)	(12.9,31.4)	(5.9,14.0)	(69.5,88.0)	(0.5,6.7)	(1.6,17.6)	(19.7,44.0)
Long-run	60.2	5.2	21.1	9.5	84.4	2.4	25.6	23.3
	(26.0,61.4)	(2.7,17.2)	(10.4,29.9)	(5.5,17.1)	(46.5,79.2)	(1.8,17.4)	(9.4,41.7)	(12.9,35.0)

Notes: Percentage of the forecast error variance for expenditure and net taxes accounted for by structural fiscal disturbances, point estimates with one-standard error bands in parenthesis.

In order to complement this evidence, it is useful to look at the impact of shocks to each fiscal variable on the opposite side of the budget (Figure 1.3). Net tax shocks

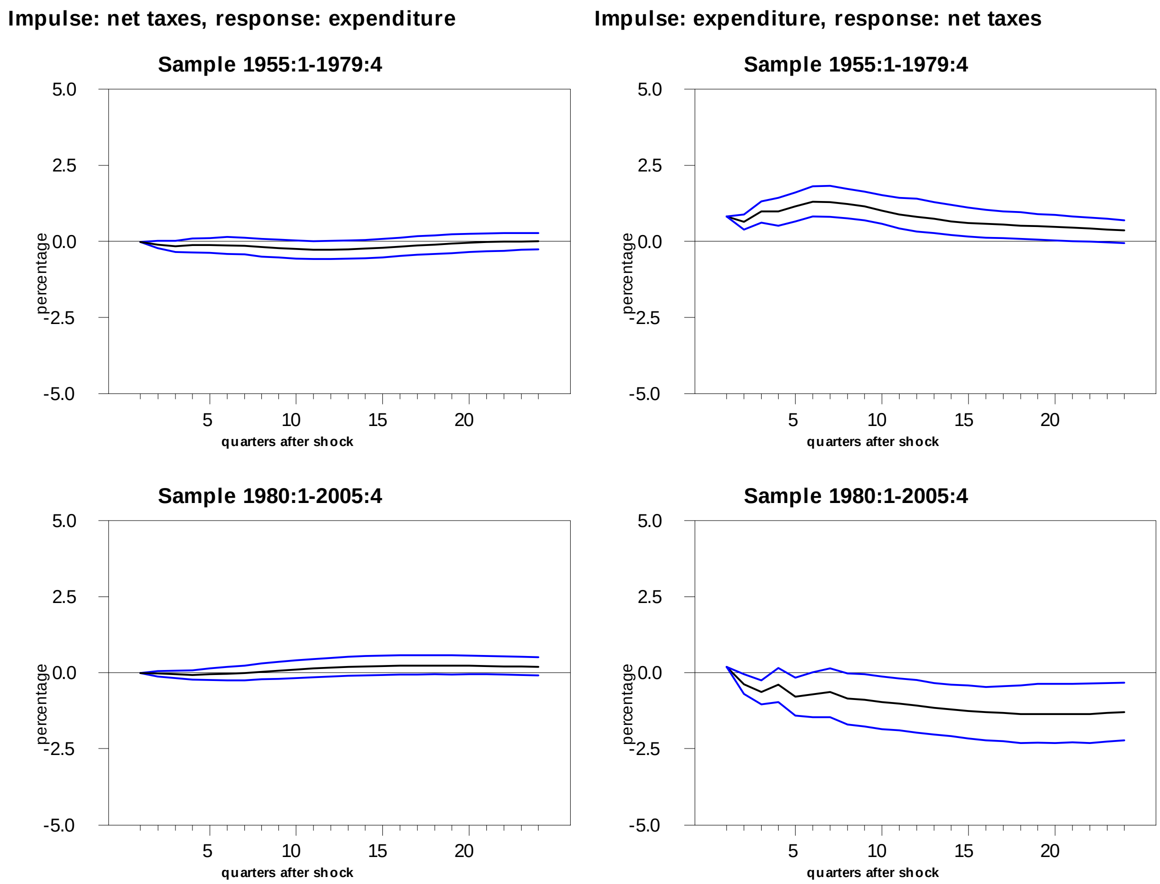

Figure 1.3: Responses of fiscal variables to fiscal shocks

have essentially no impact irrespective of the sample period. On the contrary, spending shocks trigger a significant effect in the two subsamples, but the respective sign changes from positive in the pre-1980 data to negative in the subsequent period. The magnitudes of these effects are similar and thus nearly cancel out in the full-sample responses (not shown), and the same occurs for the variance decomposition of spending. The maximum impact stands at about 1.3 percent in the first subsample and -1.0 percent in the second one. The figure for the period before 1980 matches the initial shock, which has a size of 1.3 percent as well, given that the levels of the two fiscal variables are close. The results thus capture a short-term budget-balancing movement in the pre-1980 period, but not subsequently.

These results are robust to a reversal of the ordering, i.e. to placing expenditure after taxes. When this is done, impulse-responses hardly move in comparison to Figure 1.3. Similarly, net tax innovations continue to be unimportant for spending fluctuations and spending innovations to account for a sizeable part of net tax unconditional variance (almost 25 percent in each of the subsamples). What conclusions can be drawn from this? Firstly, given that the results hold under both possible orderings, there is evidence of causality going from spending to taxes and not the other way around. Secondly, the mechanism underlying the respective relationship changed from one subsample to the other. Political economy offers multiple explanations for casual links between the sides of the budget, going in both directions. The results in the first subsample indicate that changes in expenditure lead changes in taxes. They are consistent with the main findings of older studies such as von Furstenberg et al. (1986), whose sample period roughly corresponds to my first subsample, and may reflect the way important spending programs (e.g. the interstate highway system) were financed during the fifties and sixties.

The results for the post-1980 years, causality apart, imply a negative correlation between the budget variables. This was a period of larger and long-lasting budget imbalances of both signs, as depicted in Figure 1.1, characterized by debt stabilization policies (during the Clinton years and before) and «spending the surplus» policies (during the Bush II years). Both entail changes in the two sides of the budget going in the opposite direction in the short-run.[17] More difficult to explain is the direction of the causality, running from spending to taxes; this may be just chance causality given that we are looking at small samples.

A potential intertemporal link between the two sides of the budget that received attention recently is the «starve the beast hypothesis» which predicts that tax cuts lead to spending reductions. The results here are against this hypothesis for the US (consistently with Romer and Romer (2009)).

[17] Given that transfers are netted out against taxes in the definition of variables followed, my results cannot capture a possible feedback between revenue and mandatory outlays. Such a feedback could particularly originate in the «pay-as-you-go» budget rules in place during the nineties, which required that changes in one of those be matched by changes in the other.

1.5.3 Interaction between fiscal and monetary policies

To start with I consider the reaction of the budget variables to monetary policy shocks. Net taxes go up following a tightening in monetary policy[18] (Figure 1.4), a result presumably brought about by the reaction within the quarter of the tax base of the personal income tax to movements in short-term rates. Christiano et al. (1996), working with flow of funds data, also report an initial contraction of government borrowing following a tightening in monetary policy. In the period before 1980, the response weakens quickly and becomes negative after about one year as recession takes hold, in line with the depressing effect of the monetary shock on output. In the second subsample, there is simply a rapid decay toward zero. The response of expenditure in the wake of a funds rate shock, albeit small and on the brink of non-significance, has a negative sign that is difficult to interpret.

I now turn to the pattern displayed by the funds rate following government budget shocks (Figure 1.5). The evidence for the first subsample appears consistent with the operation of the policy rule, given that net tax and spending innovations work, respectively, as negative and positive aggregate demand shocks (also as far as the responses of inflation - not shown - are concerned). In the post-1980 years the negative trajectory of the short-term rate following spending shocks is - barring an accommodating behavior - difficult to explain, as those shocks are still expansionary (and the effect on inflation still positive) albeit much less effective than in the first subsample. As far as net taxes are concerned, the initial rise in the funds rate may be triggered by the perverse effect on output, while subsequently the response to declining inflation takes hold. In any case, the evidence is clearly not consistent with the weakening of fiscal shocks' effectiveness being explained by the behavior of monetary policy, for the kind of response I get would magnify their effects rather than mute them.

[18] The contemporaneous semi-elasticity of net taxes to the federal funds rate is estimated at 0.6 and 1.0 p.p., respectively, in the pre- and post-1980 periods. This implies that a 1 p.p. increase in the funds rate leads to a rise in net taxes from 0.5 to 1 percent, on impact.

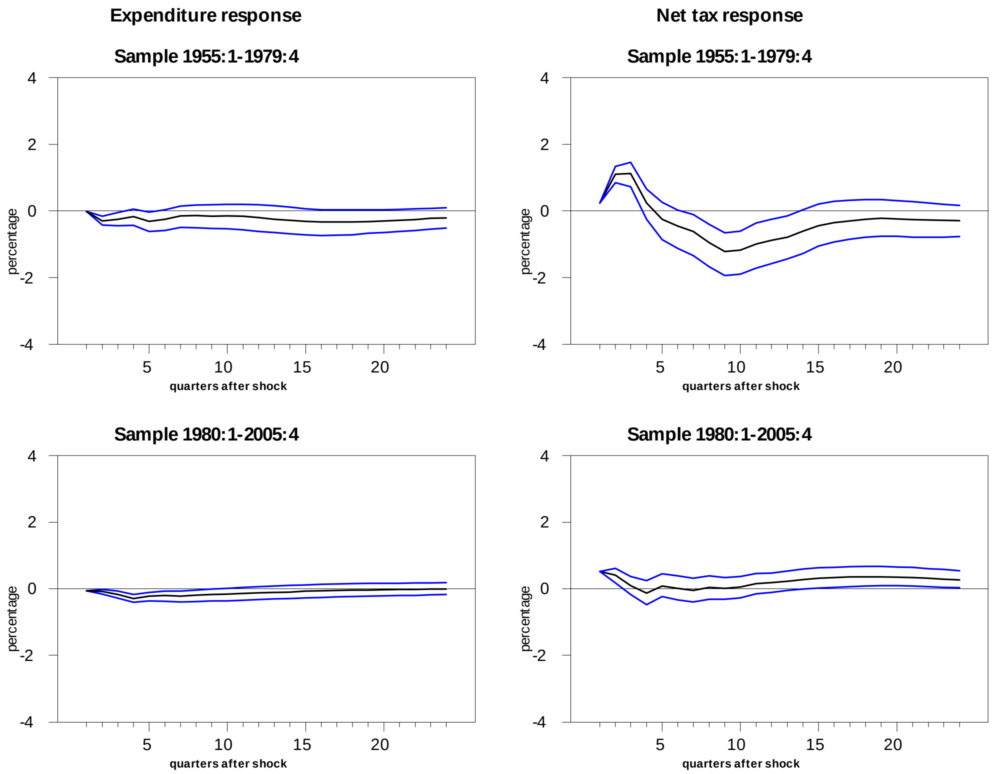

Figure 1.4: Responses of fiscal variables to monetary policy shocks

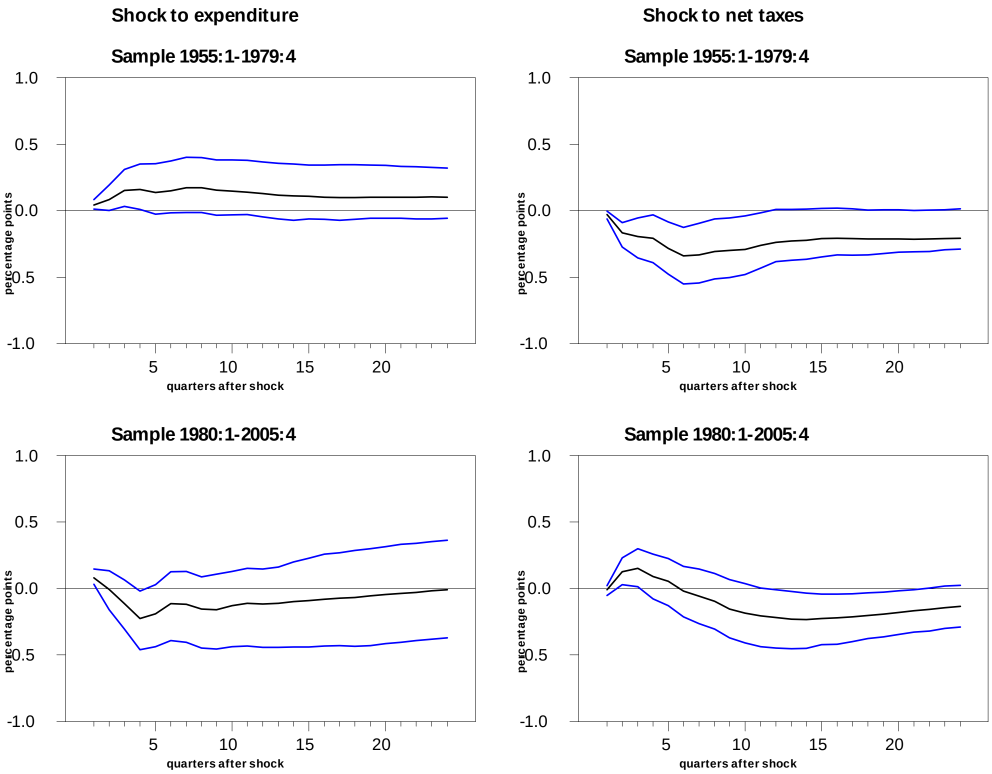

Figure 1.5: Response of the federal funds rate to fiscal shocks

1.6 The stabilizing role of endogenous policies during contractions: a counterfactual exercise

In this section, the identified VAR estimated previously is used to shed some light on the effects of endogenous monetary and fiscal policies during postwar business cycle contractions. In order to do so, I carry out a counterfactual exercise in the spirit of Sims and Zha (1998) and Bernanke et al. (1997). The basic idea behind it is to compare the historical behavior of the variables of interest with the implied behavior when the system is simulated under counterfactual assumptions, which here concern modifications in the policy responses and paths of exogenous policy shocks. I undertake this exercise for each of the eight business cycle contractions - as given by the NBER dates - from 1955 to 2005. Analyses like the one carried out below have been pursued by previous literature using different methodologies - a particularly well-known example being Romer and Romer (1994), who nevertheless did not differentiate between endogenous and exogenous policies.

The detailed methodology of this counterfactual exercise is as follows. For each contraction and each policy variable, I simulate the system (1.1) to (1.5) under two scenarios: (i) absence of the *exogenous* component of policy and (ii) absence of the *endogenous* component of policy. The simulation period starts at the first quarter after the peak and ends at the quarter of the trough. More precisely, taking expenditure -g_t - as an example, exercise (i) is carried out with the parameters in all equations at their estimated values and the shocks set to their estimated paths during the simulation period, except for $\hat{e}_t^g$ which is set to zero. Exercise (ii) shuts down any systematic reaction of expenditure so that during the simulation period the variable in driven only by exogenous shocks (i.e. the variable follows a random walk). This is done by setting all parameters in (1.1) to zero, except for the first lag of expenditure which is set to one. Otherwise the shocks to all variables, including $\hat{e}_t^g$, are set to their estimated paths and the parameters in the remaining equations are at their estimated values. As a first step I split the actual change in the policy variable into the exogenous and endogenous components. These obtain as the difference between the actual level and the simulated level of the policy variable at trough in each of the exercises. Similarly, the effect on GDP is measured as the difference between the actual level of output gap and the level implied by the simulations.

Given the evidence of structural change presented above, the exercise is carried out on the basis of 25-year rolling subsamples whose mid-points coincide roughly with the start of each recession. For the recessions taking place close to the beginning and the end of the sample, I take respectively the extreme subsamples 1955:1-1979:4 and 1980:1-2005:4 (the ones used in the preceding sections).

It is well known that the implementation of such policy analyses in a VAR context is not without caveats given the issues raised by the Lucas critique: one can argue that if endogenous policy had been different from the historical path, agents could have reacted differently. In defense of this approach, one can put forward the argument of Sims and Zha that it may provide acceptable results if the deviation of policy from its historical path is not too protracted. The episodes considered lasted on average less than 4 quarters. Beyond that issue of a more theoretical nature, another caveat to be made concerns the reliance on the identification assumptions.

1.6.1 Breaking down the change in policy variables into the endogenous and exogenous components

Table 1.4 breaks down the actual peak-to-trough change in expenditure, net taxes and the federal funds rate into the systematic and exogenous components. This is measured in percentage points also in the case of the fiscal variables, as these are taken in percent deviation from trend. Note that the actual change in each policy variable is not exactly matched by the sum of the two components, because the structural shock interacts with the endogenous structure of the system after it has impacted the respective policy variable. The simulation exercise by definition does not capture such an interaction, but the approximation generally works well. There are however exceptions, for instance, the endogenous component of net taxes is overestimated in the 1960-61 recession and, to a lesser extent, in the 1973-75 and 1981-82 ones.

Table 1.4: Decomposition of changes in the policy variables during contractions

Business cycle contractions	Expenditure (p.p., cumulative)			Net taxes (p.p., cumulative)			Fed. funds rate (p.p., cumulative)		
	actual	cf. decomp.		actual	cf. decomp.		actual	cf. decomp.	
	change	exog.	endog.	change	exog.	endog.	change	exog.	endog.
57:03-58:02	1.2	0.2	1.0	-16.0	-2.2	-12.0	-2.3	0.4	-2.9
60:02-61:01	2.9	1.3	1.5	-6.9	2.1	-16.1	-1.7	0.3	-2.0
69:04-70:04	-2.5	-1.8	-0.7	-15.8	0.8	-14.3	-3.4	-1.5	-1.7
73:04-75:01	1.8	3.1	-2.2	-18.4	2.3	-28.3	-3.7	-2.9	-1.4
80:01-80:03	-2.5	-2.6	0.5	-8.7	1.1	-9.4	-5.2	-1.2	-3.9
81:03-82:04	1.8	-0.9	3.7	-20.5	0.4	-27.4	-8.3	-1.0	-7.2
90:03-91:01	0.6	1.0	-0.6	-8.6	-1.6	-6.3	-1.7	0.0	-1.8
01:01-01:04	1.9	0.7	1.2	-12.6	-0.1	-9.5	-3.5	-1.4	-1.0

Notes: The dates indicate the peak and trough quarters. Actual change in the variable is measured as the variation peak-to-trough. The components are equal to the difference, at the trough, between the actual figure for the policy variable and the simulated figure shutting down the exogenous or the endogenous response, respectively. The simulation period starts in the first quarter after the peak. The sample periods are: 1955:1-1979:4 - 1st and 2nd recessions, 1957:3-1982:2 -3rd, 1961:3-1986:2 - 4th, 1967:3-1992:2 - 5th, 1968:3-1993:2 - 6th, 1977:3-2002:2 - 7th and 1980:1-2005:4 - 8th.

Figures in Table 1.4 indicate a consistent pattern of anti-recessionary endogenous movements in the federal funds rate and net taxes, in line with the evidence presented above about the responsiveness of these variables to the economy. Nothing comparable happens for government expenditure whose endogenous variation is not even uniformly countercyclical (i.e. positive). In this case the exogenous component dominates, documenting the importance of own innovations for spending fluctuations.

The exogenous component of net taxes is relatively unimportant against the overall change. It will capture, for instance, the impact of factors unrelated to the economy causing changes in social transfers (e.g. aging populations): recessions coinciding with periods of particularly high growth will tend to have smaller such components.[19] Another factor that might be present in the results - prior to 1980 - is «bracket creeping». Personal

[19] Visual inspection of the chart with the growth rate of (real and per capita) transfers not related to unemployment indicates that this may have been the case of the recessions at the beginning of the 1990s and 2000s.

income tax brackets used to remain unchanged for some time, which happened in the years overlapping with all recessions during that period (see Tax Foundation (2007)). This amounted to a tax increase even without legislation passed, and may explain the sign and particularly large size of the exogenous component in the 1973-75 recession, given its length and high level of inflation (although this phenomenon may have been also partly captured as an endogenous response to inflation).

The figures do not indicate a noticeable difference in the relative importance of the endogenous and exogenous components for the funds rate before and after 1980. In some recessions, notably the 1973-75 one, an important part of the reduction in the funds rate was captured by the exogenous component, that is, the actual loosening was larger than implied by the estimated rule. This fits in with the reading of the Fed's behavior during this episode in Romer and Romer (1994), in that, the Fed recognized at an early stage the downturn in activity but hesitated to take action (in what can be seen as acting in accordance with the rule) due to concerns about inflation. However, in view of the unfavorable output developments, decided subsequently to cut the funds rate more sharply.

Movements in government expenditure during contractions have been much smaller than for the other variables: they averaged 1.5 standard deviations[20] against almost 5 in the case of the funds rate, and almost 6 in the case of net taxes. The most important spending item is compensation of employes which reacts negatively to current inflation (as calibrated above), given that all variables are in real terms and, one would expect, on average positively to lagged inflation. This mechanism should reduce the endogenous component in periods of rising inflation and the opposite in times of declining inflation, and this is consistent with the magnitudes observed for the recessions of 1973-75 and 1981-82 that coincided with such periods. Note also that great deviations from inflation of pay updates of government employees, as it used to happen until the beginning of the 80s, will be reflected on the exogenous component.

[20] Considering only the positive (i.e. countercyclical) changes.

1.6.2 Impact of endogenous policies on GDP

Table 1.5 shows the impact on GDP of the outlined pattern of endogenous changes in policy variables during contractions. The stabilizing role is computed as the output loss avoided at trough, i.e. the difference between the actual level and the simulated level without the operation of endogenous policies. By comparing this figure and the actual contraction of output (also shown), it is possible to have a measure of the relative dampening effect at that point. The counterfactual multiplier/relative effectiveness figure intends to capture the effectiveness of endogenous policies, and is obtained as the relationship between the stabilizing impact and the change in the policy variable. In parenthesis appear the indicators for the maximum impact of *exogenous* policy, as computed in Section 1.4, taking the same rolling samples. These are shown in order to give a rough indication about effectiveness of endogenous *vs* exogenous policies (note, however, that in the first case effectiveness is assessed at trough of the recession while, in the second case, it is measured at the point where it is highest).

Table 1.5: Impact of fiscal and monetary policies on output

Business cycle contractions	actual output change (p.p.)	Impact of endogenous change in:					
		Expenditure		Net taxes		Fed. funds rate	
		cf. stabil. role (p.p.)	cf. multiplier	cf. stabil. role (p.p.)	cf. multiplier	cf. stabil. role (p.p.)	cf. relative effectiv.
57:03-58:02	-6.6	0.3	1.9 (2.1)	6.1	-2.5 (-1.4)	0.3	-0.1 (-0.7)
60:02-61:01	-3.6	0.5	1.7 (2.1)	6.5	-2.0 (-1.4)	0.2	-0.1 (-0.7)
69:04-70:04	-3.6	-0.2	- (1.5)	7.2	-2.3 (-1.3)	0.8	-0.4 (-0.9)
73:04-75:01	-7.2	-0.3	- (1.1)	7.7	-1.4 (-1.1)	0.1	-0.1 (-0.7)
80:01-80:03	-3.8	0.1	1.7 (1.4)	0.7	-0.4 (-0.5)	0.2	-0.0 (-0.4)
81:03-82:04	-6.4	1.2	1.8 (1.6)	3.5	-0.7 (-0.5)	2.0	-0.3 (-0.4)
90:03-91:01	-2.9	-0.1	- (1.8)	-0.4	0.3 (0.7)	0.0	-0.0 (-0.3)
01:01-01:04	-1.8	0.1	0.7 (1.0)	-0.4	0.2 (0.3)	0.1	-0.1 (-0.3)

Notes: The dates indicate the peak and trough quarters. Actual change in output is measured as the variation peak-to-trough. The stabilizing role is equal to the difference at trough between the actual GDP level and the simulated level, shutting down the endogenous response. The multiplier/relative effectiveness indicator is the ratio between the output loss avoided and the change in policy variable; in parenthesis is shown the maximum effect of exogenous policy shocks on GDP relative to the impulse. The simulation period starts in the first quarter after the peak. The rolling sample periods are: 1955:1-1979:4 - 1st and 2nd recessions, 1957:3-1982:2 -3rd, 1961:3-1986:2 - 4th, 1967:3-1992:2 - 5th, 1968:3-1993:2 - 6th, 1977:3-2002:2 - 7th and 1980:1-2005:4 - 8th.

Taxes net of transfers played a key stabilizing role in the recessions during the sixties and seventies. This resulted from the important countercyclical movements in the variable coupled with its great effectiveness to stimulate activity. In effect, the multiplier of endogenous net taxes is estimated in the range -2.0 to -2.5 (and below that of exogenous policy) in the course of that period. The effectiveness of endogenous net taxes has weakened over time and in the last two recessions they had even a small destabilizing role. Given that this variable is chiefly associated with automatic movements, it follows that not only discretionary but also automatic policy seems to have lost capacity to stimulate activity. An important caveat about these conclusions is that the last two recessions considered were particularly short and mild, and this may bias the results toward finding smaller effects of policy.

The figures imply a very large dampening impact of net taxes on economic fluctuations in the sixties and seventies, around 50 percent or more. However, these values have to be seen with caution because when the negative endogenous component is overestimated - as in the 1960-61 and 1973-75 recessions - the same will happen with the mentioned impact (note that the multiplier, as a relative indicator, is not affected by this problem). At the same time, it is natural that I get figures larger than in previous studies, such as Auerbach and Feenberg (2000) and Cohen and Follete (2000)[21], because the scope of the policy measure I use is much broader than theirs. On the one hand, it includes transfers and state and local government taxes (these are about 40 per cent of total taxes during the sample period, mostly indirect taxes). On the other hand, it also reflects the contribution of discretionary policy. Those studies came to benchmark dampening impacts of about 8 per cent (for aggregate demand shocks).

Systematic monetary policy seems to have had a more modest stabilizing role than fiscal policy in the past, even taking into account a certain degree of overestimation of the latter's role. On average the dampening effect is estimated at around 10 percent. This appears to be due to its comparatively delayed full impact which takes more time to build up than the length of the average contraction (note that the effectiveness indicator for endogenous policy is consistently much smaller than the one for exogenous policy). In the 1981-82 recession, which was longer than average, the stabilizing role of monetary policy was more evident (this did not happen for the 1973-75 episode, but note that the endogenous variation was particularly small in the course of it). This suggests that - except for more protracted recessions - monetary policy has contributed particularly to strengthen recoveries. It is worth noting that the identification assumption that monetary policy has no impact on output within the quarter may contribute to this result. Moreover, the exercise does not suggest any obvious pattern in effectiveness of endogenous monetary policy over time.

Figures in Table 1.5 indicate that government spending has played a minor role as a stabilizing tool since mid-fifties, with the exception of the 1981-82 recession. The large

[21] Auerbach and Feenberg (2000) report simulations based on the NBER TAXSIM model. Cohen and Follete (2000) also present the results of simulations, using a large-scale macroeconometric model for the US (FRB/US).

multiplier of endogenous policy suggests, however, that it *could* have if it had been more used for that purpose. Results also indicate that the reduction in effectiveness was less marked and more concentrated toward the end of the sample than in the case of net taxes.

1.7 Conclusions

In this paper an SVAR system was estimated, identifying monetary and fiscal policy disturbances. Standard SVAR tools and counterfactual simulations were used to gauge the (de)stabilizing impact of systematic and non-systematic policies, using data from 1955 to 2005. The following main conclusions were reached:

- Policy disturbances were much less destabilizing in the post-1980 years both on the fiscal and monetary sides. This result is mainly explained by a smaller impact of those disturbances on output and, to a lesser extent, by a smaller variance of policy shocks (in the cases of the federal funds rate and government spending). The impact of exogenous policies on output has weakened in the recent decades, this trend being particularly evident for net taxes.

- Net taxes have a large endogenous content featuring a high degree of responsiveness to output, and there has been an increase in such responsiveness over time (possibly reflecting discretionary policy). In contrast, government expenditure is mostly driven by own shocks. The federal funds rate responds strongly to the economy as well, in line with the operation of the monetary policy rule. An analysis of the variation in the strength of that response over time was inconclusive.

- The main stabilizing force during the activity contractions since the beginning of the sample until the eighties was taxes net of transfers, as measured by the reduction in output foregone at the trough of recessions. However, a marked lost of effectiveness appears to have occurred in the recent period. Government spending played a small stabilizing role over the whole sample.

- Monetary policy has contributed comparatively less to offset the downturns in activity during postwar contractions, due to the slower build-up of the impact on output. This suggests a particularly important contribution to enhance growth at the initial stage of the recoveries.

Appendices

1.A Detailed computation of the contemporaneous fiscal elasticities

1.A.1 Personal income taxes

The derivation of theoretical expressions for the elasticity to GDP, prices and the interest rate of personal income taxes (which also applies with small changes to the elasticity of social contributions to activity and prices) is a bit more involved than for the remaining types of taxes. I assume that the personal income tax base reacts to prices, as nominal wages adjust to it to some degree, and also to the short-term interest rate, as the latter affects asset income earned by households. Each individual in the population (assumed to be equal to the labor force) earns labour income and/or asset income. Let the real personal income tax revenue be given by $T = [t((W(L,P) + A(FF))(W(L,P) + A(FF))L(Y)]/P$ where $t(.)$ is the average tax rate, W the nominal wage, A individual income on assets, P prices, L employment, Y GDP and FF the federal funds rate.[1] The nominal tax base per worker is $B = W + A$. I assume that the income on assets reacts contemporaneously only to the federal funds rate because, as regards personal interest income, the underlying stock is mostly determined by past economic conditions, while dividends are also linked to past profits.

The elasticity of real personal income tax revenue to output is given by

[1] I assume in the computation of the elasticities of purchases of goods and services that the wage bill in government sector does not respond to macroeconomic developments (see below). One would have to consider a separate elasticity for government's wage bill, as a component of the tax base, to be fully consistent. I have not done so, in order to simplify matters.

$$a_{PIT,Y}=\frac{\partial \ln T}{\partial \ln Y}=\frac{\partial \ln t}{\partial \ln W}\frac{\partial \ln W}{\partial \ln L}\frac{d \ln L}{d \ln Y}+\frac{\partial \ln B}{\partial \ln W}\frac{\partial \ln W}{\partial \ln L}\frac{d \ln L}{d \ln Y}+\frac{d \ln L}{d \ln Y} \tag{1.A1}$$
$$= a_{W,L}a_{L,Y}(a_{t,W}+s_W)+a_{L,Y},$$

where $a_{W,L}$ the elasticity of wages to employment, $a_{L,Y}$ the elasticity of employment to output, $a_{t,W}$ is the elasticity of the (average) tax rate to the wage and $s_W = \frac{W}{W+A}$ is the share of labour income in total income. Note that the expression for $a_{PIT,Y}$ appearing in OECD's work (in Giorno et al. (1995)) corresponds to the one above but with s_W is equal to 1, as they consider labor income only.

The elasticity of the real tax revenue to prices is given by

$$a_{PIT,P}=\frac{\partial \ln T}{\partial \ln P}=\frac{\partial \ln t}{\partial \ln W}\frac{\partial \ln W}{\partial \ln P}+\frac{\partial \ln B}{\partial \ln W}\frac{\partial \ln W}{\partial \ln P}-1/4=a_{W,P}(a_{t,W}+s_W)-1/4, \tag{1.A2}$$

in which $a_{W,P}$ is the elasticity of wages to prices and the changes in prices are measured at annual rates.

The semi-elasticity of real tax revenue to the short-term interest rate is given by

$$a_{PIT,FF}=\frac{\partial \ln T}{\partial FF}=\frac{\partial \ln t}{\partial \ln A}\frac{d \ln A}{dFF}+\frac{\partial \ln B}{\partial \ln A}\frac{d \ln A}{dFF}=a_{A,FF}(a_{t,A}+s_A), \tag{1.A3}$$

where $a_{A,FF}$ is the semi-elasticity of asset income to the interest rate and $s_A = \frac{A}{W+A}$ is the share of asset income in total income.

The expressions above are based on the partial derivatives of the real income tax revenue with respect to each one of the variables of interest which assume, by definition, that the *other* variables in the expressions remain constant. This assumption does not raise problems because such partial effect is exactly what the contemporaneous coefficients in the structural equations are supposed to measure.[2] I now examine the assumptions

[2] That is, the derivative of real direct taxes with respect to Y assumes that FF and P are unchanged

underlying the computation of the elasticities of the average tax rate to the wage and asset income per worker, $a_{t,W}$ and $a_{t,A}$ (the remaining parameters are estimated by means of econometric regressions - see below). It is clear that these elasticities will not be constant throughout the wage and asset income distribution. Nevertheless, one needs a summary measure in order to compute the figures using the expressions given above. The OECD approach copes with this, for the labor income case, by computing the average and the marginal tax rates of a representative family with certain characteristics, at different points of the wage distribution. Afterwards a weighted average of each of the two tax rates is computed on the basis of the weight of wage income at each point in total. The ratio of the two weighted averages yields the summary elasticity measure. This procedure is carried out for several years so to incorporate modifications in the tax code.

In order to describe precisely how to extend this procedure to the case of labor and asset income, and to illustrate the difficulties to compute $a_{t,A}$, I now denote *with ij* the magnitudes above evaluated at the arbitrary cohort (W^i, A^j) of the wage and individual asset income distribution, and *without ij* the corresponding aggregate magnitudes. Assuming that the elasticity to the base at a given cohort (W^i, A^j) is the same irrespective of whether there is a marginal variation in the wage or individual asset income[3], and denoting that elasticity by $a^{ij}_{t,B}$, then one can write $a^{ij}_{t,W}=s^{ij}_W a^{ij}_{t,B}$ and $a^{ij}_{t,A}=s^{ij}_A a^{ij}_{t,B}$. The corresponding aggregate elasticities are given by

$$a_{t,W}=\sum_i \sum_j \phi^{ij} s^{ij}_W a^{ij}_{t,B} \qquad \text{and} \qquad a_{t,A}=\sum_i \sum_j \phi^{ij} s^{ij}_A a^{ij}_{t,B}, \tag{1.A4}$$

where the ϕ^{ij}'s are the weights computed as the share of wage and asset income associated with the cohort (W^i, A^j) in total income from both sources ($\phi^{ij} = L^{ij}B^{ij}/\sum_i \sum_j L^{ij}B^{ij}$ with B^{ij} equal to $W^i + A^j$ and L^{ij} equal to the number of individuals associated with the cohort (W^i, A^j)). The computation of precise figures for $a_{t,W}$ and $a_{t,A}$ would thus require

when Y varies. Of course, when GDP changes, the federal funds rate and prices may change as well, but this is captured by other contemporaneous coefficients than $a_0^{nt,y}$.

[3] This may not happen for every (W^i, A^j). For instance, if there are tax deductions applying only to labor income, say the first \$X dollars of employment income are exempt from tax, then for wage levels below \$X the marginal change in tax revenue is zero when the wage changes but positive when asset income changes.

information about the distribution of (W, A) and the corresponding values for $a_{t,B}^{ij}$, for several years, which is not available.

Nevertheless, the OECD figure should provide a good basis to compute $a_{t,W}$. Note that, if $a_{t,B}^{ij}$ was constant for a given wage level W^i (i.e. it did not depend upon j because all individuals would concentrate in a given cohort A), then $a_{t,W}{=}s_W \sum_i \psi_W^i a_{t,B}^i$ would hold, with the weights ψ_W^i given by the share of wage income associated with the cohort W^i in total, according to the marginal distribution of W. This relationship should provide a reasonable approximation in practice, as there is a higher concentration of individuals (at lower cohorts) for individual asset income than for wages. Further, as labor income represents the bulk of personal income, the elasticities calculated considering only labor income as the tax base (as in OECD) should not be too far from $a_{t,B}^i$. By contrast, such elasticities and information about the the marginal distribution of W would not be suitable for the calibration of $a_{t,A}$.

The OECD figures correspond to $\sum_i \psi_W^i a_{t,B}^i + 1$ (as they refer to the elasticity of the tax revenue not of the tax rate) and vary considerably over time, ranging from 1.3 to 3.9 over the last three decades. The computation of aggregate figures for the shares of labor and asset income - s_W and s_A - does not raise problems since they are just the shares of wage and asset income for the economy as a whole[4] (see Appendix 1.B for the series used). The figure for s_W ranges from 0.75 to 0.85 over the period 1955:1-2005:4.

The remaining parameters in (1.A1) and (1.A2) are computed through econometric regressions, following the method in Blanchard and Perotti (2002). Specifically, $\hat{a}_{W,L} = 0.33[\hat{t} = 4.0]$ and $\hat{a}_{W,P} = 0.09[\hat{t} = 1.6]$ are the lag 0 coefficients of a regression of log change in wages on the first lead and lags 0 to 4 of log change in employment and change in annualized inflation (sample 1955:1-2005:4).[5] Note that I take as the price variable inflation measured at annual rates. Likewise $\hat{a}_{L,Y}$ $=0.68[\hat{t} = 12.1]$ is the lag 0 coefficient of a regression of log change in employment on the first lead and lags 0 to 4 of log change in GDP. The average

[4] As $s_W = \sum_i \sum_j \phi^{ij} s_W^{ij}$ and $s_A{=}\sum_i \sum_j \phi^{ij} s_A^{ij}$.

[5] One could raise the issue of simultaneity in relation to the regressions used to compute some of the parameters in (1.A1) and analogous expressions. I checked the results of corresponding regressions excluding the leads and using lags of the right-hand side variables as instruments and they differed by little.

figures for $\hat{a}_{PIT,Y}$ and $\hat{a}_{PIT,P}$ are equal, respectively, to 1.1 and -0.09.

1.A.2 Social security contributions

The responses of social contributions are based on the corresponding expression for the real revenue $T = [t((W(L,P))W(L,P)L(Y)]/P$, where $t(.)$ is the average tax rate and the other variables are as above. The elasticities of real social contributions revenue to output and prices are, respectively,

$$a_{SC,Y} = \frac{\partial \ln t}{\partial \ln W}\frac{\partial \ln W}{\partial \ln L}\frac{d \ln L}{d \ln Y} + \frac{\partial \ln W}{\partial \ln L}\frac{d \ln L}{d \ln Y} + \frac{d \ln L}{d \ln Y} = a_{W,L}a_{L,Y}(a_{t,W} + 1) + a_{L,Y}, \tag{1.A5}$$

$$a_{SC,P} = \frac{\partial \ln t}{\partial \ln W}\frac{\partial \ln W}{\partial \ln P} + \frac{\partial \ln W}{\partial \ln P} - 1/4 = a_{W,P}(1 + a_{t,W}) - 1/4. \tag{1.A6}$$

The average figures for $\hat{a}_{SC,Y}$ and $\hat{a}_{SC,P}$ are equal, respectively, to 0.88 and -0.17.

1.A.3 Corporate income taxes

The tax base of the corporate income tax, corporate profits, is supposed to react to GDP and prices. I assume that the tax is proportional (note further that the corporate income tax is recorded on an accrual basis by NIPAs, which should approximately undo the lag between the earning of profits and the payment of the tax). Therefore, real corporate income tax revenue is given by $T = tPR(Y,P)/P$, where t is the tax rate and PR are corporate profits. The elasticities of corporate income taxes to GDP and prices are, respectively,

$$a_{CIT,Y} = \frac{\partial \ln PR}{\partial \ln Y} = a_{PR,Y}, \tag{1.A7}$$

$$a_{CIT,P} = \frac{\partial \ln PR}{\partial \ln P} - 1 = a_{PR,P} - 1/4, \tag{1.A8}$$

where $a_{PR,Y}$ and $a_{PR,P}$ are the elasticities of profits to GDP and prices. These parameters were computed as the coefficients for lag 0 of a regression of the first differences of log profits on the first lead and lags 0 to 4 of the change in log GDP and the change in annualized inflation. This yielded $\hat{a}_{PR,Y} = 4.6[\hat{t} = 10.4]$ and $\hat{a}_{PR,P} = 1.8[\hat{t} = 4.7]$. Accordingly, $\hat{a}_{CIT,Y} = 4.6$ and $\hat{a}_{CIT,P} = 1.6$.

1.A.4 Indirect taxes

The tax base of indirect taxes is assumed to be nominal GDP and the tax to be proportional. The revenue of indirect taxes in real terms is given by $T = tY$, where t is the tax rate, implying a 1.0 elasticity to activity and a 0.0 elasticity to prices.

1.A.5 Transfers to households

Transfers to households are expected to only to activity mainly through unemployment insurance payments. Such payments have represented on average only about 3 percent of social benefits over the last decade, though at the beginning of the sample they represented a bit more than that, averaging 5 to10 percent. Let real transfers to households be equal to $T = (\bar{T} + UB(Y))/P$, where $\bar{T}$ is the component of transfers that does not react to activity and $UB(Y)$ is the amount of unemployment benefits. The elasticity of transfers to households to GDP is approximately (ignoring the term related to the response of labor force to the business cycle) given by

$$a_{TH,Y} = s_{UB}\frac{d\ln UB}{d\ln Y} = s_{UB}\frac{du}{d\ln Y}\frac{1}{u} = s_{UB}a_{u,Y}\frac{1}{u}, \tag{1.A9}$$

where s_{UB} is the share of unemployment benefits in total transfers, $a_{u,Y}$ is the unit variation of the unemployment rate in response to a 1 percent increase in GDP and u is the unemployment rate. I set $a_{u,Y}$ equal to -0.24 from Blanchard (1989). The average figure for $\hat{a}_{TH,Y}$ is -0.26.

As to the contemporaneous response to prices, many categories of social benefits such as old-age and unemployment benefits are not indexed within the quarter, and thus a

-1.0 elasticity for real outlays seems adequate. By contrast payments related to health programs are likely to be sensitive to change in prices. I assume for them a zero elasticity in real terms. These payments were rather small in the fifties and sixties, but they have become one of the most important components of social benefits, weighting currently over 40 percent. The elasticity of transfers to households to prices is based on an expression analogous to the one above, but picking out the part of transfers that reacts to prices, i.e. health benefits. That is,

$$a_{TH,P} = \frac{d \ln T}{d \ln P} = (s_{HB} - 1)/4, \tag{1.A10}$$

where s_{HB} is the share of health benefits in total. The average figure for $\hat{a}_{TH,P}$ is -0.19.

1.A.6 Purchases of goods and services

Purchases of goods and services are composed of compensation of government employees and intermediate consumption and investment (one does not have to consider here the consumption of fixed capital since it is excluded from the measure of purchases used - see Appendix 1.B). The share of compensation of employees in total was slightly below 50 per cent in the initial years of the sample, but it has represented a bit more than half of the total since mid-sixties. In general one expects intermediate consumption and investment spending to be determined by the nominal amount budgeted, implying a -1.0 elasticity of real purchases to contemporaneous inflation. Also the wage updating process in the government sector is such that price developments typically affect wages with some lag. There may be indexation but with a certain delay, for instance, pay adjustments for the blue-collar occupations in the Federal government (Federal Wage System) are indexed to lagged changes in private sector wages, according to the areas where the services are located (see Office for Personnel Management (2002)). The semi-elasticity of real purchases of goods and services to annualized changes in prices is assumed to be constant:

$$\hat{a}_{G,P} = -1/4. \tag{1.A11}$$

1.B Variable definition and data sources

Fiscal data are from NIPAs Table 3.1. Government Current Receipts and Expenditures; data on the components of government consumption, including the breakdown defense/non-defense, are from NIPAs Table 3.10.5 Government Consumption Expenditures and General Government Gross Output; data on social benefits including unemployment and health-related benefits are from NIPAs Table 3.12. Government social benefits (annual data, the share for the year as a whole was assumed for the quarter).

Taxes = Personal current taxes + Taxes on production and imports + Taxes on corporate income + Contributions for government social insurance + Capital transfer receipts (the latter item is composed mostly by gift and inheritance taxes).

Transfers = Subsidies + Government social benefits to persons + capital transfers paid - Current transfer receipts (from business and persons).

Net taxes = Taxes - Transfers.

Purchases of goods and services = Government consumption - Consumption of fixed capital[1] + Government investment.

Gross domestic product is from NIPAs Table 1.1.5. Gross Domestic Product.

Gross domestic product deflator is from NIPAs Table 1.1.4. Price Indexes for Gross Domestic Product.

Federal funds rate (quarterly averages of daily data) is from the FRED database (Federal Reserve Bank of St. Louis).

Population is from NIPAs Table 2.1. Personal income and its Disposition.

Federal debt held by the public (Section 1.3.1) is from the FRED database (Federal Reserve Bank of St. Louis).

Labor income and personal asset income (Appendix 1.A) are equal, respectively, to wages and salaries and to the sum of interest income, dividend income and rental income, all from NIPAs Table 2.1. Personal income and its Disposition. Proprietors' income was not

[1] Consumption of fixed capital is excluded on two grounds. Firstly, there are no shocks to this variable which is fully determined by the existing capital stock and depreciation rules. Secondly, from the viewpoint of the impact on aggregate demand, it is the cost of capital goods *at time of acquisition* (already recorded in government investment) that matters and not *at time of consumption*.

considered, since there is no obvious way to allocate it between labor and asset income. Employment in the manufacturing and Average hourly earnings in the manufacturing (Appendix 1.A) are from the FRED database (Federal Reserve Bank of St. Louis). Corporate profits (Appendix 1.A) is from NIPAs Table 1.10. Gross domestic income, by type of income (the inventory valuation and capital consumption adjustments were undone).

Chapter 2

A new measure of fiscal shocks based on budget forecasts and its implications

2.1 Introduction

The empirical investigation of the effects of fiscal policy shocks has to cope with two well known issues: endogeneity and anticipation. The first one is not specific to fiscal policy; it also arises, for instance, in the identification of monetary policy shocks. The structural VAR methodology tackled endogeneity through identification assumptions, including the calibration of fiscal elasticities to macroeconomic variables. While these assumptions and calibrated figures are by their very nature debatable, the key objection one can raise in relation to structural VARs has to do with anticipation (see, for instance, Leeper et al. (2008) and Ramey (2009)). This issue is largely specific to the way fiscal policy in conducted. Important changes to taxes and spending have to pass a legislative process before they are signed into law and often more time elapses until they are actually implemented. Markets and agents get information about future fiscal policy and it is plausible that they react to this information.

Potential anticipation and/or endogeneity problems have prevented empirical analyses to come to widely accepted conclusions about the impact of fiscal policy on the economy. As a result such analyses have given an insufficient contribution to reduce the uncertainty stemming from the divergent theoretical views in the field. The objective of this paper is to develop a shock measure that is relatively less affected by these shortcomings, so that it can be more credibly used to assess the impacts of government budget on the economy.

The shock measure put forward takes advantage of the information about anticipated fiscal policy contained in the budget projections regularly announced by the Office of Management and Budget (OMB). However, not the full information content of these projections is suitable to be used to assess the macroeconomic impacts of policy. Budget projections respond to the information that forecasters have about current and future economic developments, embodied in the macroeconomic assumptions. The same holds for similar information that policymakers use to make their decisions. Another source of endogeneity comes from the fact that budget forecasts are anchored on the outturn figures for a base year.

In a first step, I purge the fiscal forecasts from these endogenous elements by regressing them on an information set including real time data and macroeconomic assumptions. The residual of this regression yields the exogenous component of the forecast. This quantity can be computed throughout the sequence of forecast announcements for a given fiscal year, and my shock measure is based on its revision between two such consecutive announcements. Typically releases include at least projections for the ongoing and budget fiscal years, and I am able to compute two corresponding shock series. The methodology followed has similarities to the one used in Romer and Romer (2004) to derive monetary policy shocks.

I collected information about all releases of budget projections made by OMB I could track down over the period 1968-2008. For each of them, I further collected information about the underlying macroeconomic assumptions and real-time contemporary data. The fact that most of the releases can be precisely dated, generally to the day, allows me to investigate the impact of the shocks using data at a higher frequency (monthly and weekly) than usually in this context. This study is not the first one to use budget forecasts to capture anticipated fiscal policy, but it is the first one to derive from them a measure of shocks that can be broadly employed to assess its effects. Previous literature initiated by Wachtel and Young (1987)[1] considered simply the overall revision to the forecast between announcements and mostly cared only about their immediate (daily) impact on interest rates.

Once series of estimated shocks are obtained, their effects are measured by plugging them into reduced-form univariate and vector autoregressions. The key findings can be summarized as follows: revisions to anticipated fiscal policy, as measured by the change in the exogenous component of the forecast, matter for the economy and their effects have changed substantially over the last decades. The usable sample includes the years from 1969 to 2008, and I consider two subsamples: 1969-1988 and 1989-2008. In the first subsample, positive deficit shocks raise simultaneously interest rates and output. Positive variations in anticipated exogenous taxes (net of transfers) reduce output and in spending increase it. In the second half of the sample the impacts are quite the opposite. In particular, revisions to anticipated fiscal policy which signal loosening have a contractionary impact on economic activity and reduce interest rates.

This paper adds strongly to the evidence that the effects of fiscal policy in the US have been unstable over time. These findings present a great challenge to the theory, suggesting that more attention should be paid to such instability, and particularly to deviations from the neoclassical model that may explain unconventional multipliers.

My findings do not support the view that revisions to anticipated fiscal policy affect aggregate demand only indirectly, via the impact on long-term interest rates. Positive deficit shocks work in the first and second subsamples, respectively, as positive and negative aggregate demand shocks. This is evidence against the argument - very common in the popular debate - that an expansionary impact of fiscal policy tightening comes about through a fall in interest rates.

[1] Other contributions along these lines are, for instance, Thorbecke (1993), Quigley and Porter-Hudak (1994), Kitchen (1996) and, more recently, Laubach (2009).

The response of the federal funds rate to fiscal shocks appears generally in line with the endogenous reaction of monetary authorities to the ensuing deviation of output from trend. No indication of an accommodating behavior is found. The long-term interest rate accompanies the short rate in a muted way, possibly reflecting the role of the expectations channel. I carry out a deeper investigation of the long interest rate response. In particular, I search for an impact of fiscal policy on the risk premium - which the evidence does not support, although this sort of investigation is contingent on the difficulties in estimating unobservable components of the long rate.

The paper is organized as follows. Section 2.2 describes the budget forecast data used, focusing on aspects relevant from the viewpoint of reaching an appropriate shock measure. Section 2.3 explains the derivation of the shocks. Section 2.4 provides a descriptive analysis of the estimated shocks series. The rest of the paper is devoted to the presentation of the empirical results. Sections 2.5 to 2.8 analyze the reactions of output, short-term and long-term interest rates and the fiscal variables in the wake of the shocks. Section 2.9 takes up a more detailed investigation of the impacts on long-term bond yields. Section 2.10 makes some concluding remarks.

2.2 Announcements of budget forecasts

This paper proxies anticipated fiscal policy through the projections released by OMB. There are two main releases of budget forecasts by this agency throughout the year: at the time of the submission of the *President's Budget* in January or February, and around July or August in the *Mid-Session Review*. The Congressional Budget Office (CBO) releases its own forecast shortly after OMB, respectively, in the *Economic and Budget Outlook* and *Economic and Budget Outlook: An Update*. Table 2.1 presents the chronology of OMB announcements for which information was gathered. They start with the FY 69 Budget (January 1968) and end with the FY 09 Budget (February 2008). The FY 69 Budget was the first one employing the so-called «new budget concepts» which defined the methodology used in the compilation of budget data that is, by and large, still in place today. Prior to mid-eighties there used to be additional releases of forecasts (this still occurs occasionally nowadays, as at times of presidential transition). In the earlier years of the sample, some of these releases were not backed by a formal document. As to the sources used, beyond the budgetary documents and the *Economic Report of the President*, the *Economic Indicators* prepared on a monthly basis by the Council of Economic Advisers was a valuable source to keep track and collect information of OMB releases over the time span considered.[2]

[2] This study was made solely on the basis of resources available on the web. The US Budgets for FYs 1963-1986, the Economic Report of the President since 1947, and the Economic Indicators since 1948 are available from the Federal Reserve Archival System for Economic Research, FRASER (http://fraser.stlouisfed.org/). The US Budgets since FY 1996 and the Mid-Session Review since FY 1998 are available at http://www.gpoaccess.gov/usbudget/. CBO documents relating to the budget published over the years can be found at http://www.cbo.gov/publications/.

Table 2.1: Announcements of budget projections 1968-2008

Date	Event	oFY	Date	Event	oFY	Date	Event	oFY
68Jan29	FY69 Budget	68	79Jan22	FY80 Budget	79	92Feb18	FY93 Budget, Sup.	92
68Sep	Summer Review	69*	79Mar	Curr. Bud. Est.	79	92Jul24	Mid-Sess. Review	92
69Jan15	FY70 Budget	69	79Jul12	Mid-Sess. Review	79	93Jan6	Budget baselines	93*
69Apr15	Review FY70B	69	79Jul31	Mid-Sess.Rev., rev.	79	93Feb17	Prel. FY94 Budget	93
69May		69	79Oct25	Treas./OMB Stat.	80*	93Apr8	FY94 Budget	93
69Sep17	Summer Review	70*	80Jan28	FY81 Budget	80	93Sep1	Mid-Sess. Review	93
70Feb2	FY71 Budget	70	80Mar31	FY81 Budget rev.	80	94Feb7	FY95 Budget	94
70May19		70	80Jul21	Mid-Sess. Review	80	94Jul14	Mid-Sess. Review	94
71Jan29	FY72 Budget	71	81Jan15	FY82 Budget	81	95Feb6	FY96 Budget	95
72Jan24	FY73 Budget	72	81Mar10	FY82 Budget Rev.	81	95Jul31	Mid-Sess. Review	95
72Jun	Mid-Sess. Review	72	81Jul15	Mid-Sess. Review	81	96Feb5	FY97 Budget	96
72Sep		73*	82Feb8	FY83 Budget	82	96Mar19	FY97 Budget Rev.	96
73Jan29	FY74 Budget	73	82Apr	Curr. Budget Est.	82	96Jul16	Mid-Sess. Review	96
73May1		73	82Jul30	Mid-Session Review	82	97Feb6	FY98 Budget	97
73Jun1		73	83Jan31	FY84 Budget	83	97Sep5	Mid-Sess. Review	97
73Oct18		74*	83Apr	Curr. Bud. Est.	83	98Feb2	FY99 Budget	98
73Nov15		74*	83Jul25	Mid-Sess. Review	83	98May26	Mid-Sess. Review	98
74Feb4	FY75 Budget	74	84Feb1	FY85 Budget	84	99Feb1	FY00 Budget	99
74May13		74	84Apr	Curr. Bud. Est.	84	99June28	Mid-Sess. Review	99
74Jun12		74	84Aug15	Mid-Sess. Review	84	00Feb7	FY01 Budget	00
74Nov26		75*	85Feb4	FY86 Budget	85	00Jun26	Mid-Sess. Review	00
75Feb3	FY76 Budget	75	85Apr15	Curr. Budget Est.	85	01Jan16	Budget baselines	01*
75Mar12		75	85Aug30	Mid-Sess. Review	85	01Feb28	Prel. FY02 Budget	01
75Apr18		75	86Feb5	FY87 Budget	86	01Apr9	FY02 Budget	01
75May30		75	86Aug6	Mid-Sess. Review	86	01Aug22	Mid-Sess. Review	01
76Jan21	FY77 Budget	76	87Jan5	FY88 Budget	87	02Feb4	FY03 Budget	02
76Mar25	Spring Update	76	87Aug17	Mid-Sess. Review	87	02Jul15	Mid-Sess. Review	02
76Jun24		76	88Feb18	FY89 Budget	88	03Feb3	FY04 Budget	03
76Jul16	Mid-Sess. Review	77*	88Jul28	Mid-Sess. Review	87	03Jul15	Mid-Sess. Review	03
77Jan17	FY78 Budget	77	89Jab9	FY90 Budget	89	04Feb2	FY05 Budget	04
77Feb22	FY78 Budget Rev.	77	89Feb9	FY90 Budget rev.	89	04Jul30	Mid-Sess. Review	04
77Apr	Curr. Bud. Est.	77	89Jul18	Mid-Sess. Review	89	05Feb7	FY06 Budget	05
77Jul1	Mid-Sess. Review	77	90Jan29	FY91 Budget	90	05Jul13	Mid-Sess. Review	05
77Nov11	Rev. Outlay Est.	78*	90July16	Mid-Sess. Review	90	06Feb6	FY07 Budget	06
78Jan20	FY79 Budget	78	90Sep30	Budget Summit	91*	06Jul11	Mid-Sess. Review	06
78Mar	Curr. Bud. Est.	78	91Feb4	FY92 Budget	91	07Feb5	FY08 Budget	07
78Jul6	Mid-Sess. Review	78	91Jul15	Mid-Sess. Review	91	07Jul11	Mid-Sess. Review	07
78Oct27	Treas./OMB Stat.	79*	92Jan29	FY93 Budget	92	08Feb4	FY09 Budget	08

Notes: (a) Prior to 1971 the budget was prepared by the Bureau of the Budget. (b) Before FY 77 the fiscal year ended on June, 30; it ends on September, 30, since then. (c) oFY refers to the ongoing fiscal year at the time of the announcement. (d) The announcements marked with * do not have projections for a budget year.

2.2.1 Why the OMB forecasts are preferred

I work with OMB's projections for a number of reasons. A key reason is that, while these reflect the proposals of the administration before they have been signed into law, CBO's projections are usually «current-services» estimates taking current law as a benchmark.[3] Since the emphasis of the shock measure put forward is precisely to capture unanticipated policy, it is crucial that the forecasts on which it is based embody policy proposals at the earliest stage possible. At the end of the day not all proposals are enacted, this depending on aspects such as the White House and the Congress being controlled by the same party. Nevertheless, it is preferable to be protected against the risk of missing the right timing, even at the cost of taking on board some intentions that did not survive the legislative process. Moreover, the President's proposals subsequently dropped may still have influenced the behavior of market participants who basically face the same uncertainty as forecasters do.

A second reason for preferring OMB's projections is that the respective series of announcements is longer than the one by CBO, which starts in the second half of the seventies. The length of the sample is important from the viewpoint of documenting structural changes in the effects of fiscal policy. A third argument is that the releases by OMB come first. Assuming that both agencies' projections have a similar information content (in particular, abstracting from the current law *vs* proposed law issue mentioned above), one may expect a more precise estimate of impacts on the basis of OMB data. Nevertheless, as they are made public only with a couple of weeks difference and given the persistence of the shocks estimated below, one set of announcements is likely to pick up the effects of the other anyway.[4] Finally, working with OMB's projections is also convenient in that one can pinpoint the respective release date very precisely.

A possible argument against using OMB data is that market participants may have

[3] Although CBO typically presents an own re-estimation of President's proposals in the documents produced concurrently with the submission of the budget.

[4] Indeed, studies such as Wachtel and Young (1987) and Thorbecke (1993) that worked with current-year announcements by the two agencies reported that one could not include both sets in the same regression on colinearity grounds.

less confidence in them, as this agency is comparatively more susceptible to political influences. Note that, even if this was the case, the precise objectives it pursues would be open to debate. Blackley and DeBoer (1993) put forward a number of models that may govern the behavior of the agency, which imply different outcomes in terms of a possible bias in the projections (see a brief discussion in Section 2.3.2). In practice, studies that examined assumptions and budget projections of OMB and CBO as to accuracy and other properties (e.g. Plesko (1988), Auerbach (1999) and Cohen and Follete (2003)) could not find significant differences. The regular assessments published by CBO of its own macroeconomic forecasting record vis-a-vis that of OMB and *blue chip* consensus (an average of private-sector forecasters) also point to the same conclusions.[5] Therefore, the information content of OMB projections seems to be essentially as good as that of the competitors, in spite of the institutional constraints affecting its activity. The picture that emerges from the analysis in Auerbach (dealing with budget receipts) is one of «consensus» estimates of the two government agencies, from which even private-sector forecasters tend not to deviate much. Consistently with this, Foster and Miller III (2000) point out that forecasters in both agencies often maintain a behind-the-scenes dialog in order to minimize public disagreement, reducing the scope for pure partisanship.

On the basis of the data collected, I present in Section 2.4 some evidence concerning the properties of the shock series and the underlying OMB forecasts. Specifically, I test for unbiasedness and use of all available information (efficiency), and none of these properties is rejected.

2.2.2 Data collected for each announcement

The fiscal data used from each announcement are OMB's forecasts and contemporary real-time outturn data. I consider both on- and off-budget items, i.e. the total budget, which

[5] CBO computes simple indicators of accuracy (root mean square error, RMSE) and bias (mean error, ME), considering the results for the forecasts made early in a given calendar year for that year and the following one. Taking as an example the period 1982-2004, Congressional Budget Office (2006), the RMSE for real GDP growth is 1.2, 1.2 and 1.3 percent, and the ME -0.4, -0.5 and -0.3 percent, respectively, for CBO, *Blue Chip* and OMB. For CPI inflation, the RMSE is 0.9 percent for all sources, and the ME 0.4, 0.4 and 0.2 percent, respectively.

agencies and analysts usually consider to be the most meaningful for economic purposes - see, for example, Congressional Budget Office (1992). This was also the definition adopted by earlier studies. The fiscal variables I use are taxes net of transfers and expenditure plus the deficit calculated as the difference between the two. Net taxes are computed as total receipts minus outlays related to social transfers. This class of outlays corresponds, in terms of breakdown by function, to the item «health and income security» in the initial years of the sample. It has been further broken down over time and includes currently the items «health», «medicare», «income security» and «social security». Expenditure comprises the entries «defense», «international» and «other (domestic)». Note that these entries roughly coincide in budget terminology with «discretionary spending», and those that are netted out from receipts with «mandatory spending». I did not consider interest outlay projections because they are basically determined by the past stock of debt and interest rate assumptions. Therefore, it does not make sense to extract an exogenous component from them (much in the same way as exogenous shocks in a structural VAR sense cannot originate in interest expenditure, see Section 1.3.2 in the first essay).

The nominal budget forecasts are standardized by nominal GNP/GDP[6], which appears to be a suitable benchmark to proxy the perception by markets of the size of fiscal shocks. The nominal GNP/GDP projection is calculated as the real-time figure at the date of the announcement, projected according to the real and price growth assumptions for the fiscal years ahead.

The budget forecasts released by government agencies take as a reference the fiscal year, which ended on June, 30 prior to FY 77, and ends on September, 30 since then. Up to the end of the seventies, each announcement used to include forecasts for the *ongoing fiscal year*, and also for the upcoming one after the submission of the President's Budget, i.e. the *budget fiscal year*. A few announcements taking place between the start of a fiscal year and the submission of a new budget - marked with an * in Table 2.1 - had ongoing-year projections only. From the beginning of the eighties on, longer-term forecasts started to be reported including years not yet covered by a budget, on a current law basis (the

[6]From the FY 1993 Budget on, GDP replaced GNP as the central output measure.

so-called budget baselines). This was initially done only for the main releases, at time of the presentation of the budget. Currently the forecasting horizon stretches over a five-year period beyond the ongoing year. Announcements after the beginning of a fiscal year and before a new budget submission have become infrequent, being more or less restricted to the budget baselines released by outgoing Presidents (see the January 1993 and January 2001 announcements)[7].

Fiscal projections for the ongoing and budget years (when applicable), and real-time outturn figures, are available for all announcements in the Table. In contrast, post-budget year projections can be used only in the final years of the sample period, i.e. from FY 98 on. This is because during part of it they were not produced at all, as said, and for some years more my sources do not comprise them (Appendix 2.A gives more details about data availability). The way the information for different fiscal years is utilized to construct the shock measures is explained in the next section.

The second set of data needed concerns the macroeconomic assumptions underlying the fiscal forecasts. For the initial years, information about these assumptions was scant and not presented in a systematic way: for instance, the breakdown of nominal GNP growth projections by price and volume has to be taken from the discussion about economic prospects in the *Economic Report of the President*. The scope and presentation improved much starting with the FY 76 Budget, after the enactment of the Budget and Impoundment Control Act of 1974. Nevertheless, even for the subsequent period, a number of difficulties have to be overcome in order to come to a macroeconomic dataset usable to control for in the derivation of the shocks (see the appendix).

[7] This became possible only since 1990. Before that, the outgoing presidents had to submit a budget (see Congressional Research Service (2008)) and the incoming administration typically issued a revised budget.

2.3 Methodology

2.3.1 Derivation of the expected-policy shock

My objective is to derive shock series suitable to measure the effects of expected policy - thus not affected by anticipatory effects - and that at the same time are relatively free from endogeneity. In order to motivate the proposed approach, it is useful to describe it by reference to the well known SVAR framework.

The starting point in the SVAR framework is the estimation of reduced-form errors that coincide with the 1-period-ahead forecast errors. For instance, taking a fiscal policy variable, denoted by f_t, and its forecast given by a linear projection on the lagged endogenous variables included in the system up to $t-1$, denoted by $\hat{f}_{t|t-1}$, such forecast error is given by (f_t- $\hat{f}_{t|t-1}$). In this study, in contrast, the fiscal forecasts come from an external source, the OMB announcements.[8] In fiscal VARs the forecasts have by definition the same frequency as the underlying data - usually on a quarterly basis; the budget forecasts released by agencies take the fiscal year as a reference.

I denote by $\hat{f}_{t|q}$ the forecast for the FY t released at announcement q, where q may refer to a point in time during FY t (i.e. FY t is the ongoing fiscal year) or before the beginning of it. If forecasts until FY $t+i$ are available for two consecutive announcements, q and $q-1$, *expected-policy shocks* relate to the forecast revision between the two announcements: $(\hat{f}_{t|q}-\hat{f}_{t|q-1})$, $(\hat{f}_{t+1|q}-\hat{f}_{t+1|q-1})$, ..., $(\hat{f}_{t+i|q}-\hat{f}_{t+i|q-1})$. There may be several such shocks - relating to different fiscal years - for the same announcement. They should be distinguished from the *current-policy shock* which is based on the final forecast error, $(f_t-\hat{f}_{t|q*})$, the difference between the outturn for FY t and the last forecast for that year, released at announcement q^*. This corresponds to the 1-period-ahead forecast error on which the SVAR shocks - that can be considered current-policy shocks as well - are based. Note that in a VAR context, the revisions of the i-period-ahead forecasts , $(\hat{f}_{t+i|t}-\hat{f}_{t+i|t-1})$, $i=1,2,...$, convey no additional information relative to 1-period-ahead forecast error.[9]

[8] See Thapar (2008) for a study that has similarities with mine in that market forecasts are used to derive the monetary policy shock.

[9] To see this formally , let's consider the moving average representation of a VAR as given, for instance,

Therefore, there is no point in computing expected-policy shocks in that context.

The fact that the information content of the shock has to do with expected-policy or current-policy determines a difference in the respective dating. For expected policy what matters is the *point in time* when the information is released to the markets, i.e. the date of the announcement; for current policy what matters is a *time interval.* For instance, for the current-policy shock $(f_t - \hat{f}_{t|q*})$, this time interval is the period between the last announcement in the course of the fiscal year, at q^*, and the end of that year (in a VAR, this time interval is the whole period t). In this respect, it is worth noting that my ongoing-year shocks are not pure expected-policy shocks. Besides the expected-policy content on which I focus, they have a current-policy content relating to the time interval back to the proceeding announcement or to the beginning of the fiscal year, as applicable. Evidence presented below on the response of fiscal variables to these shocks, however, suggests that their macroeconomic impacts estimated in the paper are essentially driven by expected policy, rather than by current policy.

A first issue to address is the number of fiscal years to take. I consider shocks for the ongoing and budget fiscal years. A prosaic reason for doing so is data availability: shocks for subsequent years could be computed for a limited subset of announcements, as they require availability of post-budget year projections (see the previous section). But there are conceptual reasons as well. As explained shortly, my shocks are based on the revision to the exogenous component of the forecast for a given fiscal year - controlling for the base-year and macroeconomic assumptions - from one announcement to the other. This revision is less meaningful for years not yet covered by a budget. In the absence of the latter, such assumptions are precisely the key factors driving the projections. Actually, as the forecasting horizon moves into the future, they become more mechanical, approaching paths of fiscal variables consistent with an equilibrium trajectory of the economy. Note also that budget-year shocks are likely to be correlated with changes in the exogenous part of forecasts for subsequent years, and capture their impact when these are omitted.

in Hamilton (1994, ch.11), $\mathbf{y}_t = \boldsymbol{\mu} + \mathbf{u}_t + \Psi_1 \mathbf{u}_{t-1} + \Psi_2 \mathbf{u}_{t-2} + ...$ The following holds: $(\hat{\mathbf{y}}_{t+i|t} - \hat{\mathbf{y}}_{t+i|t-1}) = \Psi_i \mathbf{u}_t = \Psi_i(\hat{\mathbf{y}}_t - \hat{\mathbf{y}}_{t|t-1})$. That is, the revision of the forecast for $\mathbf{y}_{t+i}$, between period $t-1$ and t, is a linear function of the current forecast error, with the coefficients coming from the moving average representation.

This happens because in the case of measures gradually implemented, for instance a tax cut phased-in over a number of years, the initial (budget-year) variation in fiscal variables is usually a smaller version of the overall multi-year variation.

In the SVAR methodology, the reduced-form errors for the policy variables have to be orthogonalized, in order to obtain the respective structural shocks, which are then used to measure the effects of policies on the economy. For instance, in the Cholesky decomposition this is achieved through a regression of the 1-period-ahead forecast errors for the policy variable, $(f_t - \hat{f}_{t|t-1})$, on the corresponding quantities for the variables ordered before it. A similar issue applies when external forecasts are used, as in this study. In this case the revision to the forecast must be purged from elements that are endogenous to the state of the economy. I now describe the methodology used for doing this.

Budget forecasts for FY t can be modelled in the following way. They are determined, in the first place, by the respective base-year figure, for FY $t-1$, which may be the outturn or itself a forecast, depending on the number of steps ahead. Secondly, they reflect the estimated impact of changes in the macroeconomic scenario affecting the outcome in t, via two channels. On the one hand, forecasters seek to incorporate the effect of automatic stabilizers into the forecasts. My revenue-side variable is taxes net of transfers, thus including the items for which cyclical sensitivity is normally taken into account when forecasts are drawn. On the other hand, discretionary systematic policy responds to useful information that policymakers (similarly to forecasters) may have about current and future macroeconomic developments.[10]

The components of the projected path of the fiscal variables that relate to the base year or economic developments cannot be used to assess policy impacts and must be taken out. This can be done through regressions orthogonalizing the forecasts. I regress the fiscal forecast $\hat{f}_{t|q}$, as a percent of GNP/GDP, on a constant, its value for the previous fiscal year ($\hat{f}_{t-1|q}$) and core macroeconomic assumptions for the current and previous fiscal years. This macroeconomic dataset encompasses real GNP/GDP growth ($\hat{g}_{t|q}$ and $\hat{g}_{t-1|q}$),

[10] As explained shortly, budget projections are regressed on the forecasters' information set. However, this should roughly coincide with the policymakers' one for measures taken around the budget, and give an acceptable approximation in the remaining cases.

inflation measured by the GNP/GDP deflator ($\hat{\pi}_{t|q}$ and $\hat{\pi}_{t-1|q}$), and the 3-month Treasury bill rate ($\hat{r}_{t|q}$ and $\hat{r}_{t-1|q}$). This definition of the regressors is the one that matches best the way how main assumptions were presented throughout the years. Note that, when the dependent variable relates to the ongoing fiscal year, the lagged regressors are not forecasts, but actual data. I estimate

$$\hat{f}_{t|q}= \alpha_0+\alpha_1\hat{f}_{t-1|q} + \alpha_2\hat{g}_{t|q} + \alpha_3\hat{g}_{t-1|q} + \alpha_4\hat{\pi}_{t|q} + \alpha_5\hat{\pi}_{t-1|q}+\alpha_6\hat{r}_{t|q} + \alpha_7\hat{r}_{t-1|q} + v_{t|q} \quad (2.1)$$

by OLS - the results are presented in Section 2.3.4.[11] The residual of this regression, $\hat{v}_{t|q}$, is the exogenous component of the forecast. This will reflect, for instance, the quantification by forecasters of the impact on budget outcomes of factors that bear no relationship to the macroeconomic assumptions or, at least, not a strong enough one for their impact to be predictable on the basis of those assumptions. Examples on the revenue side include factors affecting the tax base such as consumer preferences, distribution of income or the amount of capital gains. On the outlay side, one can mention demographic trends, composition of health care demand and the behavior of administrators and beneficiaries of spending programs. It is worth noting the contrast to VARs, where quantities corresponding to $\hat{v}_{t|q}$ are by definition equal to zero, as forecasts incorporate no more information than the set of lagged endogenous variables.

The shock for fiscal year t at the announcement q is computed as $(\hat{v}_{t|q} - \hat{v}_{t|q-1})$ - see the diagram presented in Table 2.2 for an illustration. This is the revision to the exogenous component of the forecast between consecutive announcements, i.e. the exogenous component of $(\hat{f}_{t|q} - \hat{f}_{t|q-1})$. Such quantity can reflect, for instance, new policy measures announced in the interim period or pure forecast errors, which are part of the shock as this is based on changes in anticipated figures rather than in the actual outturn. Market participants presumably make similar errors which will influence their decisions.

[11] Note that the results of this regression will be approximately unaffected by methodological changes introduced over time in the recording of budget items (e.g. the recording of interest payments made to trust funds, starting with the *FY 84 Budget*). This is because I control for the base year and methodological changes have typically been retropolated within each announcement.

Table 2.2: Computation of fiscal shocks

	beginning of the sample									
FY	FY68		FY69							FY70
Anns.	68Jan29		68Sep	69Jan15		69Apr15		69May		69Sep17
Resids.	$\hat{v}_{FY68}$	$\hat{v}_{FY69}$	$\hat{v}_{FY69}$	$\hat{v}_{FY69}$	$\hat{v}_{FY70}$	$\hat{v}_{FY69}$	$\hat{v}_{FY70}$	$\hat{v}_{FY69}$	$\hat{v}_{FY70}$	$\hat{v}_{FY70}$
oFY sh.		↑	$\underbrace{sh1}$ ↑ $\underbrace{sh2}$ ↑	$\underbrace{sh3}$		↑	$\underbrace{sh4}$	↑	↑ $\underbrace{sh5}$	↑...
bFY sh.					↑	$\underbrace{sh1}$	↑	$\underbrace{sh2}$	↑...	

	end of the sample									
FY	FY06			FY07					FY08	
Anns.	06Jul11			07Feb5		07Jul11			08Feb4	
Resids.	$\hat{v}_{FY06}$	$\hat{v}_{FY07}$	$\hat{v}_{FY08}$	$\hat{v}_{FY07}$	$\hat{v}_{FY08}$	$\hat{v}_{FY07}$	$\hat{v}_{FY08}$	$\hat{v}_{FY09}$	$\hat{v}_{FY08}$	$\hat{v}_{FY09}$
oFY sh.		...↑	$\underbrace{sh111}$	↑	$\underbrace{sh112}$	↑	↑	$\underbrace{sh113}$	↑	
bFY sh.		...	↑	$\underbrace{sh70}$	↑	$\underbrace{sh71}$	↑	↑	$\underbrace{sh72}$	↑

Notes: oFY and bFY refer to the ongoing and budget fiscal years, respectively. Shocks can be computed whenever two consecutive releases have projections for the same fiscal year. This is possible for all announcements (except the first one) in the case of ongoing-year shocks, 113 in total, and for 72 announcements in the case of budget-year shocks. I can always compute the latter shocks after taking office of a new administration. These are likely to be particularly important when major policy changes take place.

The procedure I follow is equivalent to regressing directly the revision in forecasts between consecutive announcements, $(\hat{f}_{t|q}-\hat{f}_{t|q-1})$, on the revision in the base year figures, $(\hat{f}_{t-1|q}-\hat{f}_{t-1|q-1})$ and likewise for the macroeconomic regressors $((\hat{g}_{t|q}-\hat{g}_{t|q-1})$, $(\hat{g}_{t-1|q}-\hat{g}_{t-1|q-1})$, and so on). The residuals of this regression correspond to $(\hat{v}_{t|q}-\hat{v}_{t|q-1})$. Such an alternative regression is akin to that arising in panel data framework, where one can take the data in differences, i.e. with the variables defined as $(x_{i,t}-x_{i,t-1})$, instead of $x_{i,t}$, where i indexes the individual and t the year. This differencing is done in order to eliminate individual unobservable effects (see Wooldridge (2002), ch. 10), but it has negative implications as far as the properties of the regression are concerned, for instance, in terms of measurement error bias.

The econometric soundness of regression (2.1) relies on the exogeneity of the macroeconomic scenario to the production of budget forecasts. This is a tenable assumption since, on the one hand, budget forecasting is a second stage in a process which starts by the elaboration of the scenario. Usually different people intervene at each stage (Auerbach (1999)). On the other hand, Foster and Miller III (2000) make the point that budget scoring is «static» rather than «dynamic», in the sense that it tends to disregard effects on economic activity of the policy proposals incorporated. The inclusion of the «lagged» forecast of the dependent variable as a regressor in (2.1) rests on the equally plausible assumption that the forecasting process is sequential, that is, forecasts for FY t are determined after forecasts for FY $t-1$.

2.3.2 Correlation between shocks for a given announcement

I ran three regressions as in (2.1), with the dependent variable given by net taxes, spending and deficit. One may expect that projections of net taxes and spending for the same fiscal year react to each other. This may derive firstly from the use of the two sides of the budget for the conduct of fiscal policy. For instance, spending programs may be financed by the enactment of revenue-raising measures or, conversely, unexpected revenue windfalls may trigger spending. Moreover, budget rules as those stemming from the Gramm-Rudman-Hollings Act may induce such behavior by policymakers.

A feedback may also originate in the behavior and objectives of forecasters. As pointed out by Blackley and DeBoer (1993), OMB may act as a budget cutter and produce forecasts on the pessimistic side, or it may be optimistically biased so as to make the president's budget to look balanced. Both types of behavior could induce, in contrast to above, a negative correlation between revisions. But forecasters can pursue other objectives, such as minimizing the revision of key figures - maybe the deficit target in this case. If so, they may tend to compensate changes in one side of the budget with changes in the other, in particular if uncertainty is still considerable.

In view of the simultaneous determination of spending and net tax forecasts, the estimation of equation (2.1) for each of these variables including the other would not be appropriate. Instead of relying, say, on an arbitrary ordering, I rather estimate reduced-form equations from which the other side of the budget is excluded. By implication, the residuals computed from the net tax and spending equations will be correlated and so will be the shock measures based on them. This is further examined in Section 2.4, where I also consider correlation between current- and budget-year shocks for the same variable. These correlations have to be taken into account in the empirical analysis.

2.3.3 The current-policy shock

In this paper, I concentrate on the effects of expected-policy, and do not to attempt to compute current-policy shocks. This computation is difficult given that forecasts are drawn taking the fiscal year (instead of a shorter period, say, the quarter) as a reference. Recall that the shock relating to current policy is given by $(\hat{v}_t - \hat{v}_{t|q*})$. The error term, $\hat{v}_{t|q*}$, presents no difficulties as it is simply taken from regression (2.1) - considering the last announcement in which FY t is forecasted, typically the Mid-Session Review for the FY $t+1$ Budget. The computation of $\hat{v}_t$, in contrast, requires that one specifies a structural equation for the policy variable of the form

$$f_t = \alpha'_0 + \alpha'_1 f_{t-1} + \alpha'_2 g_t + \alpha'_3 g_{t-1} + \alpha'_4 \pi_t + \alpha'_5 \pi_{t-1} + \alpha'_6 r_t + \alpha'_7 r_{t-1} + v_t, \qquad (2.2)$$

similar to the ones appearing in fiscal SVARs. Equation (2.2) concerns actual economic relationships, not the replication of the procedure underlying budget forecasting as in (2.1), and its estimation requires that one makes identification assumptions. However, in the context of fiscal-year data, it is hard to find credible ones. Recall that identification in SVARs takes precisely advantage of the use of quarterly data, i.e. that fiscal authorities do not react within the quarter to economic developments. Note that the omission of current-policy shocks is unlikely to interfere significantly with the measurement of the effects of their expected-policy counterparts. Indeed, if forecasts are efficient, the final forecast error, $(f_t - \hat{f}_{t|q*})$, should be approximately uncorrelated with previous forecast revisions for the same fiscal year, $(\hat{f}_{t|q*} - \hat{f}_{t|q*-1})$,... (see Nordhaus (1987)). The same will hold for the shocks related to these quantities.

2.3.4 Results of the regression orthogonalizing the forecasts

This section reports the results of estimating equation (2.1). In addition to the regressors noted above, a dummy variable interacting with the 3-month Treasury bill rate forecast, for the announcements during the period 1968-74, is also included in this regression (see the end of Appendix 2.A). Table 2.3 presents the estimation results with the dependent variable given, respectively, by net taxes, spending and the deficit.

The first thing to note is the high degree of persistence of the fiscal forecasts, with the coefficient of the base year being in the range 0.85 to 0.95 and precisely estimated. The coefficient of contemporaneous GDP growth is particularly significant in the case of net taxes, reflecting the replication by forecasters of the working of economic stabilizers. Another issue to note is the high value of the R^2, around 0.80 for the deficit and 0.95 for net taxes and expenditure, meaning that most of the forecasts' variability is explained by the base year plus macroeconomic assumptions. The mechanism underlying the production of forecasts is thus well captured by the regressions. One may wonder whether the information content that remains in the residual is sufficient to identify policy effects. Note, however, coefficients of determination of this magnitude, or higher, also arise in the context of VARs, whose errors serve the same purpose.

Table 2.3: Determinants of the fiscal forecasts

	net taxes	expenditure	deficit
constant	-0.0040	0.0041	0.0094
	(-1.7)	(2.8)	(4.3)
dependent var.(FY-1)	0.9006	0.9465	0.8380
	(30.1)	(42.3)	(25.0)
GDP growth	0.0021	-0.0005	-0.0020
	(7.4)	(-2.0)	(-4.4)
GDP growth(FY-1)	0.0001	0.0003	-0.0002
	(0.7)	(1.2)	(-0.4)
price growth	0.0016	0.0007	-0.0005
	(2.7)	(1.4)	(-0.6)
price growth(FY-1)	-0.0013	0.0003	0.0016
	(-2.2)	(0.6)	(1.9)
st interest rate	0.0009	-0.0026	-0.0038
	(0.8)	(-3.0)	(-2.3)
x dummy68-75	0.0011	0.0012	0.0001
	(1.0)	(1.5)	(0.1)
st interest rate(FY-1)	0.0003	0.0017	0.0017
	(0.2)	(1.8)	(2.0)
x dummy68-75	-0.0015	-0.0010	0.0003
	(-1.2)	(-1.2)	(0.2)
N	227	227	227
R^2	0.94	0.96	0.81
DW	1.85	2.30	2.19

Notes: The table shows the coefficients and, in parenthesis, the t-ratios. The regressions are based on 227 observations, of which 114 are ongoing-year forecasts (available for all announcements in Table 2.1), 103 are budget-year forecasts, and 10 are post-budget-year forecasts from announcements preceding the release of a new budget.

2.4 Analysis of the shock series

Net tax and expenditure shocks for the ongoing and budget fiscal years are depicted in Figure 2.1. Inspection of this figure shows that the computed net tax shocks have been most of the time larger than expenditure ones, with the exception of the period 1990-1992. In this period, the considerable and highly volatile outlays in the framework of

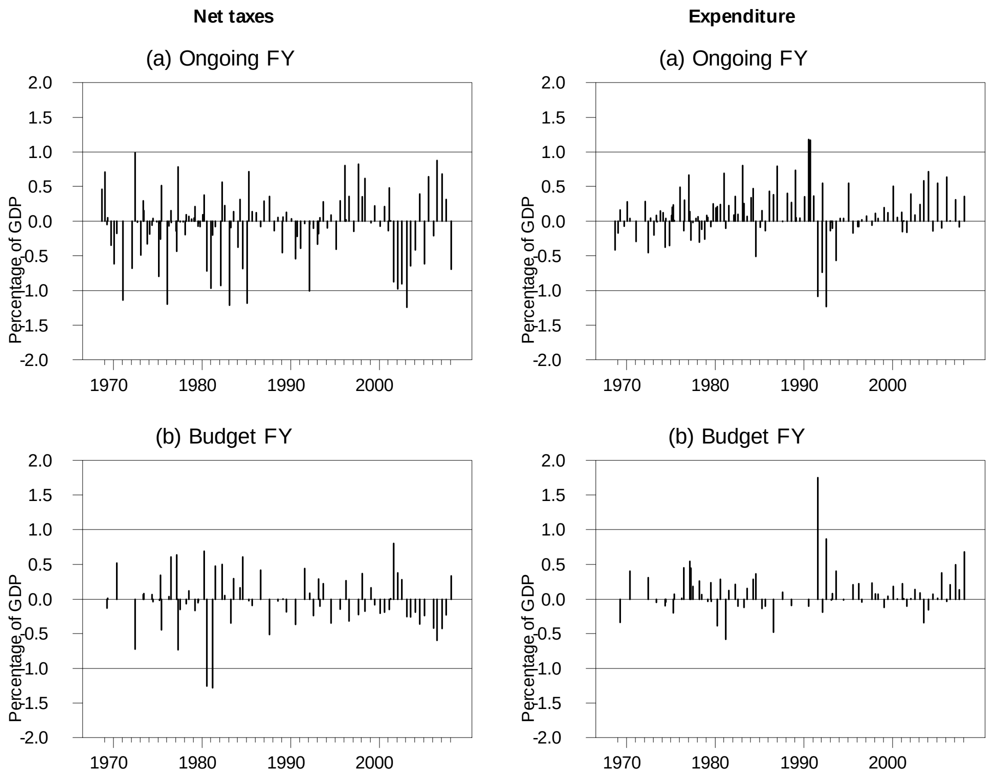

Figure 2.1: Estimated fiscal shocks

the savings and loan crisis proved very difficult to predict and gave rise to a sequence of abnormally large revisions to expenditure projections. There is, in particular, one outlier related to this event which is the positive budget-year shock of 1.7 percent of GDP dated July 1991. This is a shift of spending between consecutive years, and thus there is a large negative simultaneous ongoing-year shock. Note, however, that contemporary budget analyses considered that «these ups and downs [in expenditure] do not faze credit markets or alter interest rates because deposit insurance spending is an exchange of assets that does not affect total demand in the economy» (Congressional Budget Office (1992, p.xiii)). Given the nature of the spending at issue and the particularly large size of these shocks in July 1991, I exclude them (for spending and deficit) from the sample used to study the macroeconomic effects of fiscal policy.

In some occasions it is possible to pinpoint concrete legislative changes «behind» the estimated net tax shocks. This is the case of the Tax reduction Act of 1975 (ongoing year, February 1975), the Economic Recovery Tax Act of 1981 (budget year, March 1981), and the Jobs and Growth Tax Relief Reconciliation Act of 2003 (ongoing year, February 2003). It is to a certain extent surprising that the defense buildup at the beginning of the eighties hardly shows up in spending shocks. This is partly due to the gradual nature of the military spending increase; for instance, the Reagan budget for FY 1982 entailed an upward revision in the defense function of only 0.15 percent of GDP for that year, in comparison to the budget previously submitted by Carter.

Table 2.4 presents some descriptive statistics for the shock series, namely, the mean, the mean of absolute values and the standard deviation. The average absolute shock is 0.30-0.35 percent of GDP for net taxes and 0.20-0.25 percent for spending. This reflects a more active use of the revenue side of the budget to conduct fiscal policy in the US, together with greater difficulties in predicting budget receipts in comparison to outlays. For example, the impact of factors such as capital gains and the distribution of income on the outturn of the personal income tax is very difficult to anticipate. On average ongoing-year shocks are not smaller than their budget-year counterparts. This is the opposite of what one would expect and indicates that forecasts for the ongoing year are still surrounded by considerable uncertainty, in spite of their incorporation of more information.

Table 2.4: Shock series, descriptive statistics

	ongoing FY			budget FY		
	mean	mean abs.	st.dev.	mean	mean abs.	st.dev.
deficit	0.18	0.52	0.68	0.14	0.33	0.45
net taxes	-0.08	0.36	0.49	-0.03	0.29	0.39
spending	0.10	0.26	0.36	0.10	0.21	0.32

Notes: Based on 113 and 72 observations for ongoing- and budget-year shocks, respectively.

According to the theory of optimal forecasts, forecasts should be unbiased and use all the information available when they are drawn, implying that successive revisions for a given fiscal year must have zero mean and be uncorrelated. This can be as well applied to my shock measure, which captures such revisions after the changes in the base year and macroeconomic assumptions have been controlled for.[12] Figures in Table 2.4 indicate a positive mean of the shocks in the case of net taxes and a negative one in the case of expenditure, but the magnitudes are small: equal to or less than 0.1 percent of GDP in absolute terms, and less than 1/3 of the respective standard deviations (much less for net taxes). Table 2.5 presents the results of a formal testing of the mentioned properties. These are significance tests for the constant and slopes in a regression of the last announcement for a given fiscal year on the previous three plus constant. I did not include more announcements back in the regression because this would restrict much the number of usable fiscal years. I also present the corresponding results for the raw revisions to the forecasts (i.e. the difference in the forecasts for the same fiscal year between consecutive announcements). The findings presented in Table 2.5 support an efficient use of the information, both for the shock series and the raw revisions.

Table 2.5: Shock series and budget forecasts, efficiency tests

	shocks		raw revisions	
(p-values)	unbias.	uncorrel.	unbias.	uncorrel.
deficit	0.806	0.441	0.702	0.485
net taxes	0.810	0.402	0.543	0.080
spending	0.324	0.052	0.784	0.330

Notes: Regression of the shock related to the last announcement for a given fiscal year on a constant and the shocks related to the three preceding announcements for the same year (these can be computed for 33 out of the 40 FYs considered). The unbiasedness test is a test for the significance of the constant and the uncorrelation test is a test for the significance of the slopes.

I now turn to the correlations between net tax and expenditure shocks for the same fiscal year and ongoing- and budget-year shocks for the same variable. Such correlations are relevant as far as the measurement of the effect of the shocks is concerned. The figures are displayed in Table 2.6, which also shows the corresponding correlations for the raw revisions.

[12] Here one is considering successive forecast for the *same fiscal year*, not to be mixed with correlation between contemporaneous forecasts for consecutive fiscal years, considered below.

Table 2.6: Shock series, correlations

	shocks	raw revisions
(deficit^{oFY}, deficit^{bFY})	-0.26	0.76
(net taxes^{oFY}, net taxes^{bFY})	-0.22	0.76
(expenditure^{oFY}, expenditure^{bFY})	-0.55	0.40
(net taxes^{oFY}, expenditure^{oFY})	-0.24	-0.20
(net taxes^{bFY}, expenditure^{bFY})	0.15	-0.21

Notes: (a) oFY and bFY refer to ongoing and budget fiscal years, respectively. (b) The correlations between ongoing- and budget-year shocks were calculated on the basis of 72 observations.

There are negative correlations between ongoing- and budget-year shocks for deficit, net taxes and spending, in contrast with large positive correlations between raw revisions. These positive correlations are easily explained by base-year effects and revision to macroeconomic assumptions that typically go in the same direction throughout the forecasting horizon, thus being particularly large for net taxes which are affected by the two effects. When such effects are controlled for, a negative correlation emerges, in particular as far as spending is concerned (-0.55). Note that this latter figure is driven upward by the mentioned large ongoing- and budget-year shocks with opposite signs in July 91 - it goes up to -0.43 excluding them. Such a negative correlation between changes in anticipated (exogenous) fiscal variables apparently stems from uncertainty about the fiscal year in which the impact of policy measures is felt - money initially budgeted for a given year may turn out to be received or spent in the subsequent one or vice versa. In particular as far as spending programs are concerned, their speed of implementation is difficult to anticipate.

The correlation between raw revisions to net taxes and expenditure for the same fiscal year is negative. This suggests that whenever forecasts have been optimistic or pessimistic, this has extended to the two sides of the budget, though there is no evidence of a repeated behavior of either kind. The sign of the correlation is reverted when budget-year shocks are considered. The reason could be that these reflect comparatively more the behavior of policymakers than the behavior of forecasters. One would expect, however, the same to happen also for their ongoing-year counterparts which is not the case.

2.5 A first set of results: effects on long-term interest rates

2.5.1 Econometric specification

The general empirical strategy followed in this paper is akin to Edelberg et al. (1999) and Romer and Romer (2010), in that the shocks are derived in a first step, and their effects assessed using a reduced-form specification. Identification assumptions as in SVARs are not needed because the policy measure is already exogenous. This measure is embedded into a univariate or multivariate autoregression and the responses are obtained as the dynamic multiplier for the variable at issue. Since the shocks can be dated mostly to the day, I am able to work with data up to a weekly frequency. Naturally, it is also possible to work at lower frequencies adding up the shocks over longer time spans. Nevertheless, since temporal aggregation of the shocks can blur the estimation of the impacts, it is desirable to work at the highest frequency, particularly for variables that are supposed to react quickly such as interest rates. For instance, if several shocks of different sizes and signs occur during a given quarter, quarterly averages of interest rates may not capture properly their effects.

As a first step I study how the long-term interest rate behaves following realizations of the shock measure, in keeping with the traditional emphasis of empirical studies based on budget forecasts. This is done on the basis of *weekly data*. I estimate a univariate specification in which I regress the long-term rate on a constant, own lags, and current and lagged values both of the shock whose effects are being measured and correlated shocks. It is necessary to control for the latter since they take place at the same time as the shock whose effects are being assessed. Specifically, when measuring the effects of *ongoing-year* shocks to net taxes, expenditure or deficit, I control for *budget-year* shocks to the same variable - and vice versa. In the case of net taxes and spending, in addition, I control for same year's shocks to each other. The long rate own lags are meant to control for the normal dynamics of the variable. I include one year of lags, that is 52 weeks, in the regressions. For example, the regressions estimated to assess, respectively, the effects of changes in anticipated deficit, net taxes and spending for the ongoing fiscal year are:

$$r_t = \alpha + \sum_{i=1}^{52} \beta_i r_{t-i} + \sum_{i=0}^{52} \gamma_i \hat{d}_{t-i}^{oy} + \sum_{i=0}^{52} \delta_i \hat{d}_{t-i}^{by} + \varepsilon_t \tag{2.3}$$

$$r_t = \alpha + \sum_{i=1}^{52} \beta_i r_{t-i} + \sum_{i=0}^{52} \gamma_i \hat{n}t_{t-i}^{oy} + \sum_{i=0}^{52} \delta_i \hat{n}t_{t-i}^{by} + \sum_{i=0}^{52} \lambda_i \hat{g}_{t-i}^{oy} + \varepsilon_t \tag{2.4}$$

$$r_t = \alpha + \sum_{i=1}^{52} \beta_i r_{t-i} + \sum_{i=0}^{52} \gamma_i \hat{g}_{t-i}^{oy} + \sum_{i=0}^{52} \delta_i \hat{g}_{t-i}^{by} + \sum_{i=0}^{52} \lambda_i \hat{n}t_{t-i}^{oy} + \varepsilon_t \tag{2.5}$$

where r_t is the weekly average of the daily 10-year constant maturity rate, $\hat{d}_t^{oy}$ and $\hat{d}_t^{by}$ denote the ongoing- and budget-year deficits shocks, and the same notation applies to net tax ($\hat{n}t_t^{oy}$ and $\hat{n}t_t^{by}$) and expenditure ($\hat{g}_t^{oy}$ and $\hat{g}_t^{by}$) ones. When there is one announcement during week t these shocks are computed, as explained, as the revision to the exogenous part of the projected fiscal variable; they are equal to 0 otherwise.[13] The regression for measuring the effects of budget-year shocks is also (2.3) in the case of the deficit; in the case of net taxes it is similar to (2.4), except that one controls for $\hat{g}_t^{by}$ instead of $\hat{g}_t^{oy}$, and in the case of spending it is similar to (2.5) but controlling for $\hat{n}t_t^{by}$ instead of $\hat{n}t_t^{oy}$. All of the 6 regressions were estimated by OLS. The sample period starts in the third week of September 1968, the first one when ongoing-year shocks assume a nonzero value, and ends in the last week of March 2008 (the last announcement considered is at the beginning of February 2008). This sample period is likewise taken in the regressions measuring the effects of budget-year shocks. Note that given the inclusion of 52 lags of the series, the span of usable observations is one year shorter.

[13] As explained in Section 2.3, budget-year shocks cannot be computed for all announcements, in which case they are set to zero. I am able to identify the week when the shock occurred in all but eight announcements. When only the month could be identified, I assumed that the shock had occurred in the middle of it (i.e. during the 5 day-week including or following the 15th).

2.5.2 Results

Figures 2.2 and 2.3 show the dynamic multipliers for the long-term interest rate following, respectively, ongoing- and budget-year shocks with the size of 1 percent of GDP. This size is about three times (in the case of net taxes) to four times (in the case of spending) bigger than the average absolute shock presented in Section 2.4, although innovations of this magnitude did occur in a number of occasions. The responses are in percentage points (annualized). One-standard-deviation bands are shown as well.[14]

Results are shown for the full sample and, breaking the sample at the midpoint, for the subsamples 1969:09-1988:12 and 1989:01-2008:03. This breakpoint is chosen for convenience, and is not motivated by any precise event. Nevertheless, as it turned out, it seems well placed to capture a change in the effects of fiscal policy, as there is a marked difference in the responses of the economy in the first and second halves of the sample.

In the first half of the sample positive deficit shocks raise interest rates in line with conventional wisdom while, in the second half, the effects are the opposite. Such change in the sign of responses is observed both for ongoing- and budget-year horizons. When the subsample prior to 1988:12 is taken, the effect on the long-term rate builds up to a significantly positive one over the months following the revision to anticipated deficit. A peak effect of around 0.8 p.p. is attained nine months out in the case of ongoing-year shocks while, for their budget-year counterparts; the maximum effect stands at about 1.8 p.p. and is reached after slightly more than one year. Net tax and spending shocks have broadly symmetrical effects, but the precise figures vary depending on the forecasting horizon underlying them. Budget-year net tax innovations are particularly powerful, with a peak impact of almost -2.0 p.p., which compares with around 1.0 p.p. for their spending counterparts. The trajectory of the long interest rate following the latter is, in addition, a bit awkward - being initially positive, then reverting to zero and becoming positive again. The response to deficit innovations is comparatively more determined by net taxes, given

[14] The bands were obtained by a standard Monte-Carlo procedure, drawing 500 vectors of coefficients from a multivariate normal with mean and variance-covariance given by the OLS point estimates. A response for each draw was computed, and then the standard deviation across all responses for each week after the shock, up to the horizon considered.

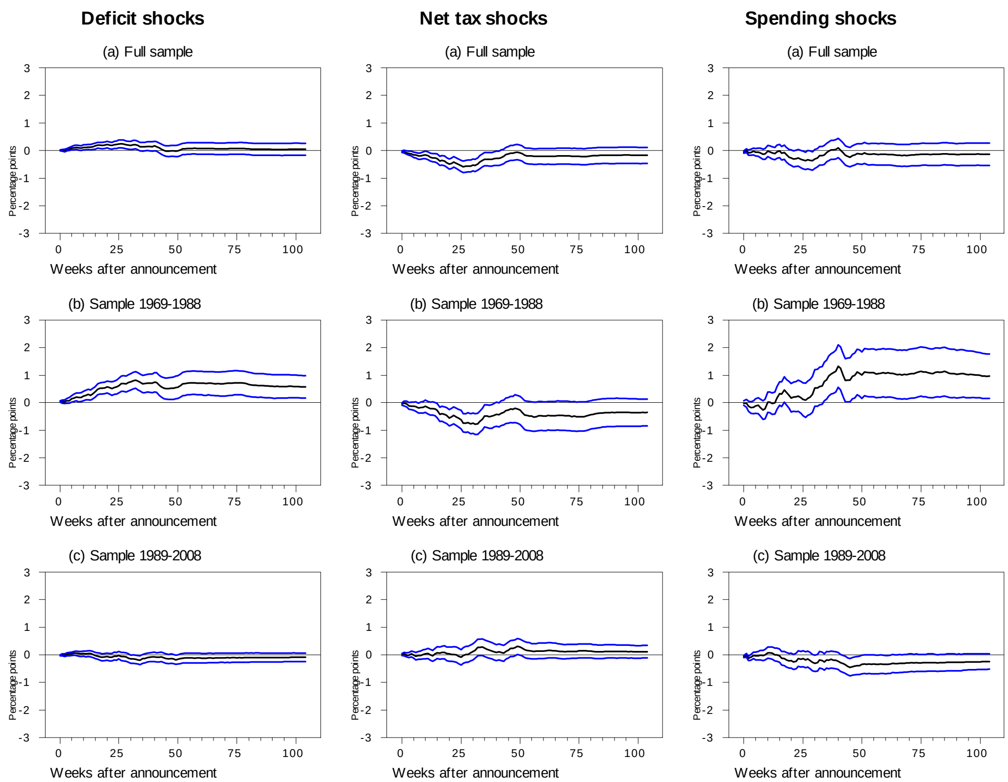

Figure 2.2: Impact of ongoing-year shocks on the long-term rate

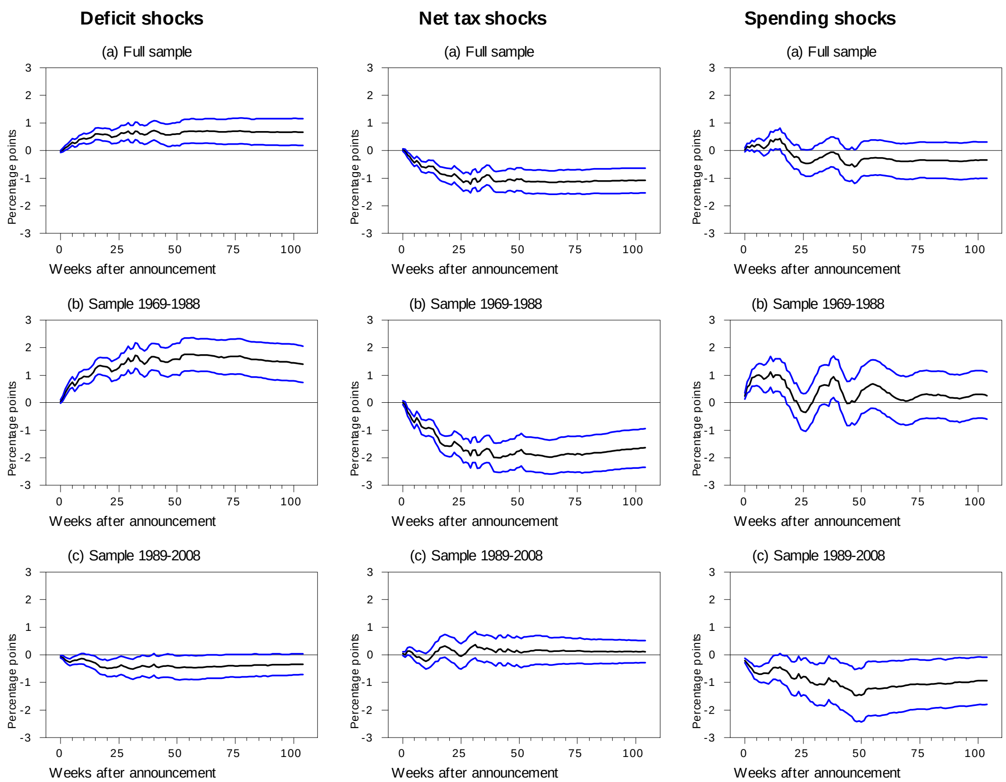

Figure 2.3: Impact of budget-year shocks on the long-term rate

the larger size (Table 2.4) and, at least for budget-year shocks, the larger response for this variable.

When the subsample after 1988:12 is considered, changes in anticipated net taxes have a positive impact on the long-term rate, and in anticipated spending a negative one. The magnitudes are smaller in absolute terms than in the first half of the sample. Actually, in the case of ongoing-year shocks the responses are not significant, since the horizontal axis is within the one-standard-deviation bands. In the case of budget-year shocks, the impacts are a bit more prominent, the peak effects being around -1 p.p. for spending and 0.5 p.p. for net taxes.

The full-sample results are, as one would expect, an average of those in both subsamples. For net taxes and deficit, the responses in the first subsample are stronger and determine those for the whole sample; for expenditure, in contrast, it is the post-1988 response that dominates.

Changes in anticipated fiscal variables for the budget year produce a greater impact than the corresponding changes for the ongoing year, particularly in the case of net taxes. There may be a number of explanations for this. Firstly, new policy measures are likely to be predominant as a source of budget-year shocks, while ongoing-year ones should chiefly originate in «ordinary» forecast revisions due to additional information. One may conjecture that markets are more sensitive to modifications in policy. Secondly, budget-year shocks may be picking up the overall impact of measures gradually implemented over a number of years (see discussion in Section 2.3.1). More generally, in specific occasions such as presidential transitions, they may capture markets' beliefs about the stance of fiscal policy in the coming years.

2.5.3 Relationship with other studies

How do these results compare with the previous literature on the effects of fiscal policy on interest rates? The literature on this topic is voluminous and studies surveying it such as Elmendorf and Mankiw (1999) or, more recently, Gale and Orszag (2002) show that taken as a whole it is rather inconclusive too. Older papers supported views ranging from significant positive effects (e.g. Feldstein (1986)) to insignificant ones (e.g. Evans (1985, 1987)), though Gale and Orszag stress that a large proportion of the latter use either current deficits or a mechanical measure of future deficits (as derived from a reduced-form VAR) which is an important shortcoming. Studies that take into account anticipated policy through a measure of budget forecasts (like in this paper) tend to find a positive impact. This feature extends to more recent papers not included in the aforementioned surveys: Laubach (2009) using OMB's and CBO's deficit projections concludes that higher anticipated deficits increase interest rates, while Evans and Marshall (2001) using a shock measure from a structural VAR get negligible effects.

Among the papers documenting that fiscal policy affects interest rates, Gale and Orszag indicate, as a benchmark figure, an increase of around 0.5 p.p. in interest rates for 1 percent of GDP deficit shocks, and report that simulations of macroeconometric models yield average effects of a similar size. Since the majority of the papers surveyed are relatively old, using samples ending in the early nineties at the latest, their findings compare with my pre-1988:12 results and, to this extent, are broadly consistent with them. My estimated impacts in the first half of the sample are larger, and particularly so (by more than 1.0 p.p.) in the case of budget-year shocks. This may be due to the fact that the methodology in this paper is freer from a number of drawbacks that may have blurred the estimation of impacts in other studies. Notably, the measure of fiscal policy used is arguably purged from endogeneity and anticipation, estimation is carried out on the basis of high-frequency data, and the effects on impact and over time are clearly differentiated. The most important piece of evidence emerging from the present study is, however, that the effects of fiscal policy on interest rates have undergone a major structural change. The possibility of such a change has not been much emphasized, even in more recent papers (Perotti (2004) is an exception in this respect)[15].

[15] As mentioned in the first essay, Perotti estimates a structural VAR on the basis of quarterly data, considering two subsamples: 1960:1-1979:4 and 1980:1-2001:4. He gets a negative impact of net taxes on the long-term rate in both subsamples. The impact of spending is initially positive but decays rapidly to zero.

2.6 Macroeconomic interactions in the wake of fiscal shocks

2.6.1 Econometric specification

I now study the full set of macroeconomic interactions following the realization of fiscal shocks, in the framework of a system of equations including output, inflation and short- and long-term interest rates. Multivariate analogues of the univariate regressions in the previous section are estimated. The analysis is now based on *monthly data*, the highest frequency at which all series are available. Specifically, I use industrial production for output and the PPI of finished goods for prices, the variables typically showing up in monetary policy VARs estimated at this frequency. The interest rates are the federal funds rate and the 10-year constant maturity rate.

Let $\mathbf{x}_t = [y_t, p_t, ff_t, r_t]'$ be a vector where y_t is output measured as the detrended log of the IPI[16], p_t is inflation measured as the change in the log of the PPI for finished goods, and ff_t and r_t are the monthly averages of, respectively, the short- and long-term interest rates. Shocks are assigned to month t if there was one announcement in the course of it; they are equal to 0 otherwise.[17] The regressions include 12 lags, and correlated shocks are controlled for, as above. For instance, the multivariate regressions estimated to assess the effects of ongoing-year deficit, net tax and spending shocks are:

$$\mathbf{x}_t = \boldsymbol{\alpha} + \textstyle\sum_{i=1}^{12} \mathbf{B}_i \mathbf{x}_{t-i} + \sum_{i=0}^{12} \boldsymbol{\gamma}_i \hat{d}_{t-i}^{oy} + \sum_{i=0}^{12} \boldsymbol{\delta}_i \hat{d}_{t-i}^{by} + \boldsymbol{\varepsilon}_t, \tag{2.6}$$

$$\mathbf{x}_t = \boldsymbol{\alpha} + \textstyle\sum_{i=1}^{12} \mathbf{B}_i \mathbf{x}_{t-i} + \sum_{i=0}^{12} \boldsymbol{\gamma}_i \hat{n}t_{t-i}^{oy} + \sum_{i=0}^{12} \boldsymbol{\delta}_i \hat{n}t_{t-i}^{by} + \sum_{i=0}^{12} \boldsymbol{\lambda}_i \hat{g}_{t-i}^{oy} + \boldsymbol{\varepsilon}_t, \tag{2.7}$$

$$\mathbf{x}_t = \boldsymbol{\alpha} + \textstyle\sum_{i=1}^{12} \mathbf{B}_i \mathbf{x}_{t-i} + \sum_{i=0}^{12} \boldsymbol{\gamma}_i \hat{g}_{t-i}^{oy} + \sum_{i=0}^{12} \boldsymbol{\delta}_i \hat{g}_{t-i}^{by} + \sum_{i=0}^{12} \boldsymbol{\lambda}_i \hat{n}t_{t-i}^{oy} + \boldsymbol{\varepsilon}_t. \tag{2.8}$$

where, as before, $\hat{d}_t^{oy}$ and $\hat{d}_t^{by}$ denote ongoing- and budget-year deficits shocks, and the same notation applies to net tax ($\hat{n}t_t^{oy}$ and $\hat{n}t_t^{by}$) and expenditure ($\hat{g}_t^{oy}$ and $\hat{g}_t^{by}$) ones.

[16] The log of the IPI was detrended by regressing it on a constant, a linear time trend and a squared time trend (sample: 1950:01-2008:03). The residuals of this regression were taken as the output measure.

[17] Throughout the whole sample I have only one case of two shocks occurring during the same month: July 1979, on the 12 and 31. As the second shock was on the last day of July, it was assigned to August 1979.

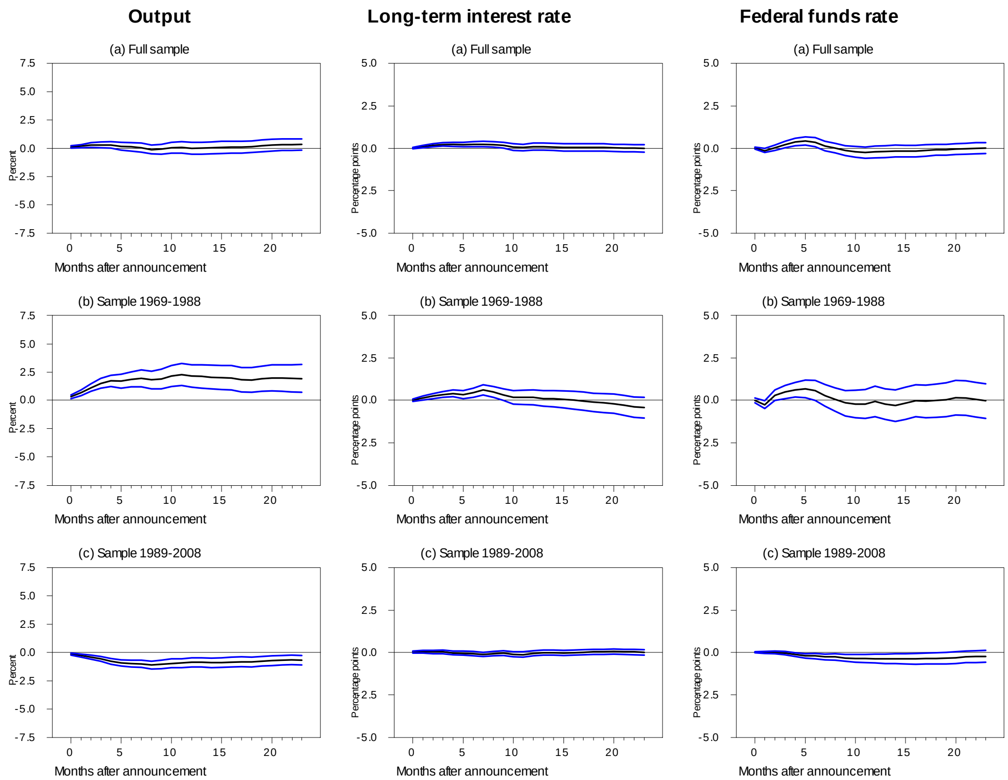

Figure 2.4: Macroeconomic responses to deficit shocks, ongoing fiscal year (VAR-based results)

2.6.2 Results

Figures 2.4 and 2.5 show the impacts of 1 percent of GDP deficit shocks for the full sample and the two subsamples considered before. The responses of output, short-term and long-term interest rates are presented. The response of inflation (not shown) fluctuates irregularly around zero giving essentially an indication of no impact.

To start with it is appropriate to check the consistency of the results for the long rate with those obtained on the basis of the univariate regressions. Since a monthly frequency is still a reasonably high one, the VAR procedure should lead to very similar findings as when weekly data are used. Note that the VAR procedure is somewhat more robust, in that it controls for the past behavior of all variables in the system, and not only for that of the long rate. The results in the two approaches are very consistent. The maximum

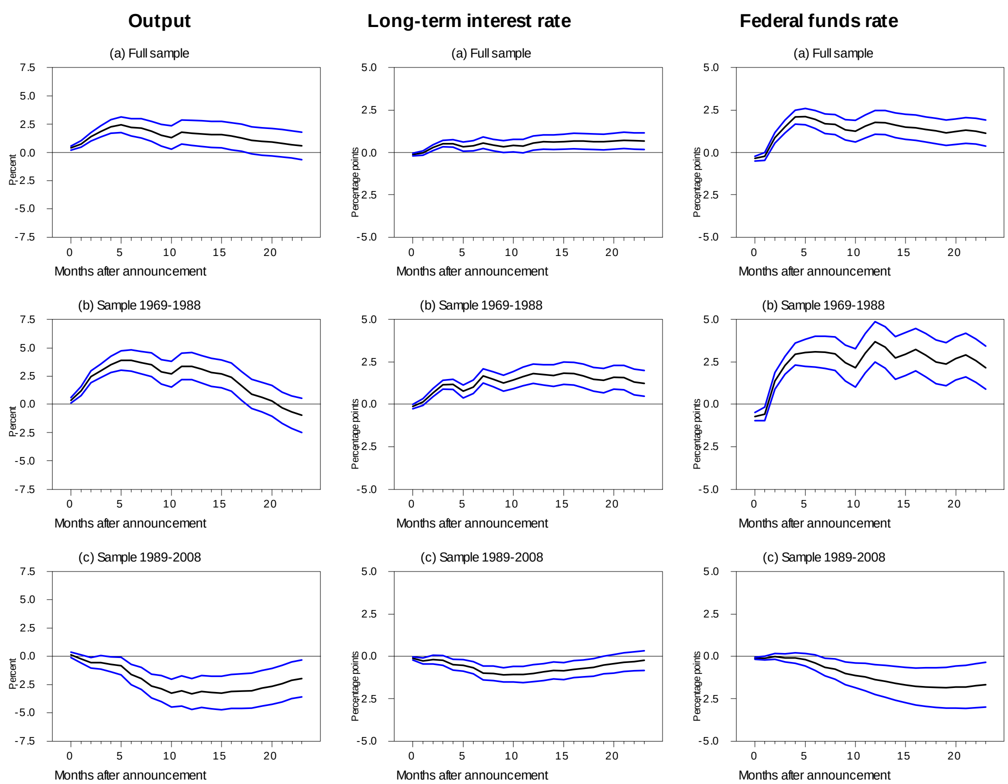

Figure 2.5: Macroeconomic responses to deficit shocks, budget fiscal year (VAR-based results)

impacts of ongoing- and budget-year deficit shocks prior to 1988:12 are now about 0.6 and 1.8 p.p., respectively, close to the results on the basis of weekly data (0.8 and 1.8 p.p.). In the post-1988:12 period, the figures are not far from zero in both procedures in the case of ongoing-year shocks. For their budget-year counterparts, the maximum impact goes down a bit to -0.9 p.p. against -0.5 p.p. previously.

Fiscal shocks and the behavior of output

Figures 2.4 and 2.5 indicate that output reacts quickly to revisions to anticipated fiscal policy and the responses seem to have undergone pretty much the same structural change as for interest rates. This holds as well for net tax and spending shocks taken separately (1 percent of GDP size, as before), whose impacts are shown in Figures 2.6 and 2.7.

In the first half of the sample, positive deficits shock raise output. In calculating the multipliers, one has to take into account that the amplitude of economic fluctuations is exacerbated by the use of the IPI instead of GDP as the output indicator. A scale factor of 2 seems to measure fairly well the size of this effect.[18] The multipliers - measured as the peak effect - for ongoing- and budget-year deficit shocks are thus around 1.0 and 2.0, respectively. This is consistent with the conjecture that markets respond more strongly to the latter shocks. Output moves in the opposite direction following changes in anticipated net taxes and spending, the trajectory being more precisely estimated in the first case. The multipliers depend a bit on the forecasting horizon underlying the shocks, but they are within the 1.5 to 2.0 range in absolute values in the period before 1988:12. The multiplier for ongoing-year deficit shocks is smaller than those for the respective components because these attain the maximum impact at different points in time.

The deficit multipliers are negative in the second half of the sample, with figures of about -0.5 for ongoing-year shocks and a surprisingly one of -1.5 for their budget-year

[18] The following procedure was used in order to come to this figure. GDP and the IPI in logs were first detrended by regressing them on a second-degree polynomial in time. Then, I took the values of the detrended variables at all turning points of the NBER cycles contemporary with the sample period. I started at the December 1969 activity peak and ended in the December 2007 one. The average absolute change between each two consecutive turning points was calculated. This yields 0.093 for the IPI and 0.042 for GDP.

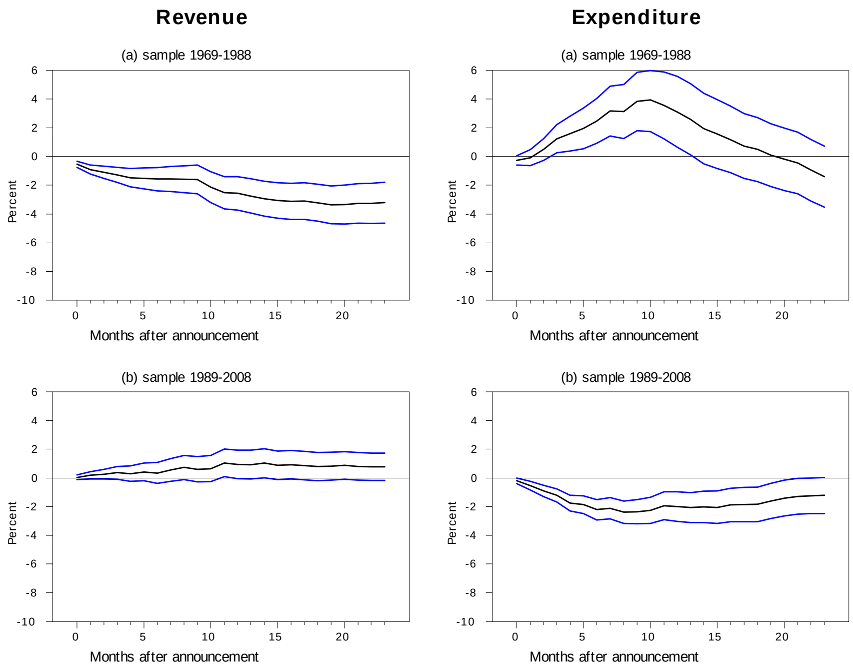

Figure 2.6: Output responses to ongoing-year shocks

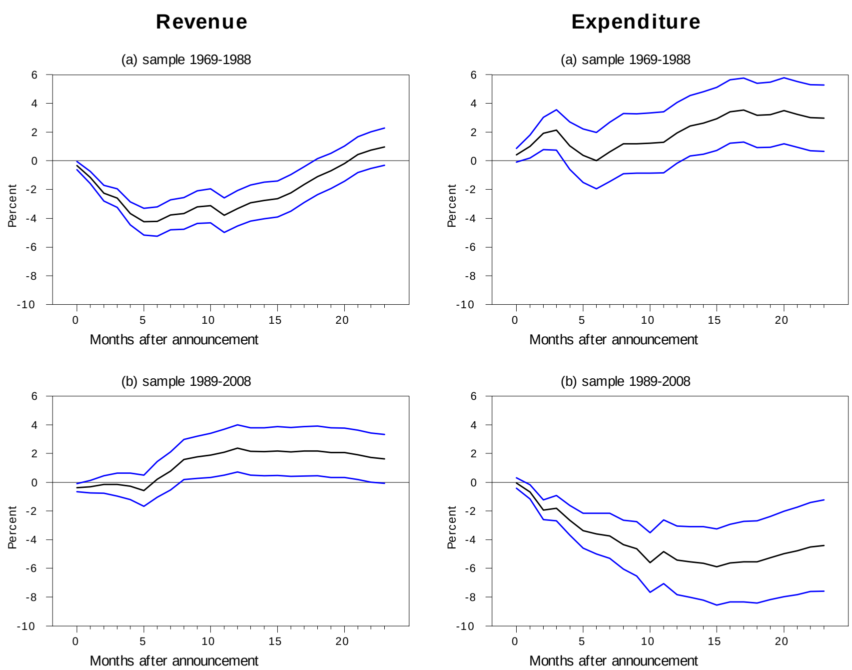

Figure 2.7: Output responses to budget-year shocks

counterparts. These estimates are statistically significant. The large negative output response to changes in anticipated deficits for the budget year is driven by the depressing effects of the spending component, featuring a multiplier of around -3.0. Budget-year net tax shocks are expansionary but have milder effects by comparison, the multiplier being below 1. A greater impact of spending than net taxes is also visible for impulses derived from ongoing-year forecasts.

A possibility worth investigating is whether non-conventional effects of fiscal policy were already at work in the Reagan era. This issue was raised in relation to the 1981 tax cuts and spending increases that coincided with the 1981-82 recession (see Blanchard (1984)), although the policy stance in this period is complicated by the enactment of counteracting measures in 1982. I am able to investigate indirectly the effects of fiscal policy around the 1981-82 recession, for instance, by recomputing the multipliers in the first subsample excluding these two years - the initial ones of Reagan's first term. When this is done, the output response (not shown) shifts downward, suggesting a conventional behavior of policy during the episode at issue.

A final word on the inflation response. In principle, one would expect to find significant impacts on inflation accompanying the sizeable ones on output gap. This is not the case, however, and experiments with the CPI as an alternative inflation measure led to similar findings. Such an evidence can be seen as surprising, but it may just reflect the sluggishness of price adjustment. Below I address shortly the reaction of *expected* inflation to my shock series.

Relationship with other empirical studies

The findings of the two main methodologies used to assess the empirical effects of fiscal policy, the SVAR and the event-study approaches, are usually summarized as being consistent with conventional output multipliers - the disagreement between them rests on the consumption multiplier. Nevertheless, even as far as the output multiplier is concerned, the evidence is less clear-cut once subsample sensitivity is carefully examined. On the SVAR side, the evidence in the first essay (particularly, Sections 1.4 and 1.6.2) indicates much instability, corroborating Perotti (2004). Moreover, in the third essay (Section 3.5.2) I estimate an SVAR with a Blanchard and Perotti-like identification scheme on the basis of a 25-year rolling sample. The 1-year-ahead spending multiplier fluctuates a lot and assumes negative values for samples ending approximately between mid-90s to mid-00s. At the same time, the tax multiplier weakens over time and becomes negative for samples ending toward 2009. The most recent paper of the event-study approach, Ramey (2009), presents results based on two new shock measures: one derived from an improved series of defense news and another one from the survey of professional forecasters. The output multiplier of military purchases in the first measure, albeit positive when the WW II is included in the sample, becomes negative when the sample periods starts just a bit later, in 1955. The second measure (for the period 1968-2008) indicates similarly a negative multiplier.[19] My paper adds forcefully to this evidence of important changes in the effects of fiscal policy over time, and the possibility of unconventional multipliers.

It is worth noting that the possibility of a major change in the effects of fiscal policy in the nineties in the US has been pointed out in the popular debate (see Auerbach (2002)). Specifically, the tight fiscal policy implemented by the Clinton administration is hypothesized to have strengthened economic performance. The findings of this paper provide some backing to this argument.

Relationship with the theory

In the original neoclassical model, full employment combined with intertemporally optimizing households and infinite planing horizons implies a positive but small spending multiplier. Hall (2009) shows that departures from this benchmark, as sticky wages or sticky prices and a variable markup, can generate multipliers that are positive and large but, under certain circumstances, negative multipliers can arise as well. For instance, this may happen with sticky wages in a competitive economy with a constant markup.

[19] Romer and Romer (2010) find a conventional negative tax multiplier for the whole sample (1947-2005) and, when the main sample is split into the period before and, after 1980, there is a weakening but not a reversal of the effects of the shocks. However, increases in taxes to cope with an inherited deficit, as opposed to increases motivated by long-run growth considerations, have positive - albeit very imprecisely estimated - effects on output.

When the firms have market power and the markup varies across the business cycle in a countercyclical manner, a positive multiplier arises only if the elasticity of the markup to output is lower than a certain threshold. Basu and Kimball (2003) show that assumptions about investment adjustment costs can change the sign of the multiplier. Nevertheless, the bulk of the theoretical research challenging the pure neoclassical model has endeavoured to modify the basic assumptions in such a way that positive and larger output multipliers are obtained, since these are believed to match reality better. The evidence in this paper questions this belief.

There is one feature in my results that has a traditional Keynesian flavour: net tax and expenditure shocks trigger opposite effects on output, and this holds even in the second half of the sample. One important distinction vis-a-vis the traditional Keynesian model is, of course, that we are dealing with anticipated, rather than implemented fiscal policy. Older literature introducing anticipated policy in this framework (Blanchard (1984) and Branson et al. (1985)) typically assumed that only the financial markets, not agents, were forward-looking and that revisions to anticipated fiscal policy affected aggregate demand only indirectly, via interest rates. My findings do not support this view; otherwise one would expect long-term interest rates and output to move temporarily in opposite directions, in the wake of deficit shocks.[20]

A theoretical view compatible with non-conventional multipliers (albeit a relatively less well articulated one) is the «expansionary fiscal contraction» hypothesis, which emphasizes the role of agents expectations on the impact of fiscal policy (see e.g. Giavazzi and Pagano (1990) and Giavazzi et al. (2000)). This view emphasizes the effects of fiscal measures, such as tax cuts or spending increases, taken in situations of very large budget imbalances, which render more likely the need for major and disruptive fiscal adjustments (or conversely, in the case of policy tightening, eliminating or postponing this need). For instance, if the path of the current policy is already seen as unsustainable, further loos-

[20] For instance, in the case of fiscal loosening, the long rate would rise, depressing output, in line with the anticipation by financial markets of an increase in the short rate. This «perverse» effect on output would be temporary because, in principle, the actual implementation of the fiscal stimulus later on would reverse it.

ening will be seen as particularly bad news. When the adjustment is expected to come from the revenue side, Blanchard (1990) puts forward the idea that agents may believe it will bring the tax rate above a certain threshold that implies a jump in the respective deadweight loss. More generally, a major fiscal consolidation on either side of the budget will cause important variations in future income when implemented. An increased probability that it occurs also means added uncertainty about such income, which may have depressing effects through precautionary savings and postponement of spending decisions.

This last channel would conceivably influence directly long-term interest rates as well, as market participants demanded a higher risk premium when buying bonds to make up for added uncertainty. Actually, it is sometimes suggested that *this is* the mechanism behind expansionary fiscal contractions. Credibility or reduced uncertainty effects of fiscal tightening lower long interest rates which, in turn, stimulate real activity (Alesina and Ardagna (1998)) - and the opposite holds for fiscal loosening. This possibility builds on the aforementioned idea that financial markets react first to changes in anticipated policy. Nevertheless, Figures 2.4 and 2.5 indicate that in the post 1988:12 period, even in the presence of non-conventional effects, output and the long-term rate continue to move in the same direction. Fiscal loosening is accompanied by a decrease in both variables, working as a negative aggregate demand shock. However, as the response of the long rate is presumably partly determined by that of the short-term rate via the expectations channel, perhaps this obscures a positive effect, for instance, at the level of the risk premium. I come back to this issue below.

The empirical testing of theories resting on long-run expectations about the course of fiscal policy - largely impossible to proxy by observable variables - is difficult. It is appropriate at this point to distinguish them from anticipated fiscal policy as it can be inferred with reasonable certainty from the budgetary documents for one or two years ahead (precisely the point explored in this study in order to construct a measure of fiscal shocks).[21] For sure the political debate in the US came to reflect growing concerns about

[21] As it is known, longer-term budget projections are available but its usefulness in this respect is doubtful (see the discussion in Section 2.3).

fiscal sustainability from the early- to mid-eighties on. The Balanced Budget and Emergency Deficit control Act was enacted in 1985 and in the following years budget imbalances were often invoked to object to expansionary policies. This was a change in comparison to some years before, and may signal a parallel change in the beliefs of agents. At the same time, the easing of liquidity constraints over time has possibly made agents more responsive to expected rather than current policy. Still, the fiscal situation in the US over, say, the last two decades would hardly fit into the picture of a major crisis as required by the expansionary fiscal contraction hypothesis. For instance, Federal US debt grew very fast in the period between 1982 and 1993, reaching a peak around 50 per cent of GDP, a figure nevertheless below the levels prevailing for instance during the fifties. The personal income tax marginal rates were lower in the second half of the sample.[22]

2.7 Response of fiscal variables

I now address the response of the fiscal variables to expected-policy shocks. In order to analyze this response in a framework close to that utilized in the previous sections, I take up taxes net of transfers and expenditure for the Federal government in turn, in a system including the same set of core variables as above. The fiscal data are from NIPA (see Appendix 1.B in the first essay for the precise definition)[23] and are available on a quarterly basis, seasonally adjusted; these are the data normally used in the estimation of fiscal policy VARs. They are defined as a percentage of GDP to match the definition of the shocks. The core variables are output (GDP)[24], inflation (change in the log of the GDP

[22] Some literature (e.g. Alesina and Ardagna (1998) and Perotti (1999)) has associated shocks on the outlay side with unconventional effects. The larger negative spending multiplier in the second subsample appears consistent with this. I also considered the effects of transfer outlays in isolation which, as it turned out, are much more predictable than taxes and spending. The variability of the transfer shock in the post-1988 period is much smaller than for the other two budget items - about 1/4 to 1/5 (budget-year shocks). The output response to it (not shown) is indeed negative, but the confidence bands are extremely wide, being difficult to draw conclusions.

[23] The budget outturn data are published on a cumulative basis, adding up to the fiscal year, so monthly or quarterly data are not directly available. Although the methodological definitions for budget data do not completely coincide with those for NIPA, one expects the results to be close for the two data sources.

[24] More precisely, the log of real GDP detrended by regressing it on a constant, a linear time trend and a squared time trend (sample: 1950:01-2008:03). The residuals of this regression were taken as the output measure.

deflator), and the federal funds rate and the 10-year constant maturity rate (quarterly averages). Systems analogous to (2.7) and (2.8) are estimated with 4 lags, and the shocks are now summed over the quarter.

The fact that one is considering expected-policy shocks determines an important difference in their impacts on the policy variables in comparison to ordinary SVAR shocks. Let's exemplify this generically, with the aid of a system with only two variables: a fiscal policy variable, say net taxes, and a macro variable, say output. SVAR shocks affect the policy variable first and, through it, the macro variable. Therefore, the impact on the policy variable is typically one-to-one or close to this (in case there are feedback effects). With expected-policy shocks it is the other way around: the shock impacts the macro variables first, and the policy variable indirectly, through it, at the initial stages. Thus, the initial policy response is driven by the macroeconomic effects of the shock; its role as a predictor of actual policy will become apparent only with a delay. One expects this to be more marked for budget-year shocks than ongoing-year ones. Another issue to take into account is that, since the shocks concern the fiscal year as a whole, this may blur the measured relationship with fiscal data on a quarterly basis.

Figure 2.8 shows the impact of the shock measures on federal taxes net of social benefits and expenditure. The shocks have, as before, the size of 1 percent of GDP and the response is measured in p.p. of GDP as well. The trajectory of net taxes seems essentially a reaction to the expansionary or recessive effects of the shocks (except for budget-year shocks in the second subsample where it is close to zero from the first quarter on), i.e. induced by their impact on the business cycle that, as seen above, is rather strong. This appears to override all the other effects. In the case of expenditure, where the role of the economic stabilizers is not present, the responses are around zero throughout, which corroborates the idea that the shocks trigger almost no direct response of fiscal variables at a quarterly frequency. This may be explained, on the one hand, by the mentioned fact that the shocks are derived from data at a lower frequency. In addition, not the full information content of the shock is relevant as a predictor of actual policy due to the forecast errors (in contrast to current-policy shocks as those in SVARs).

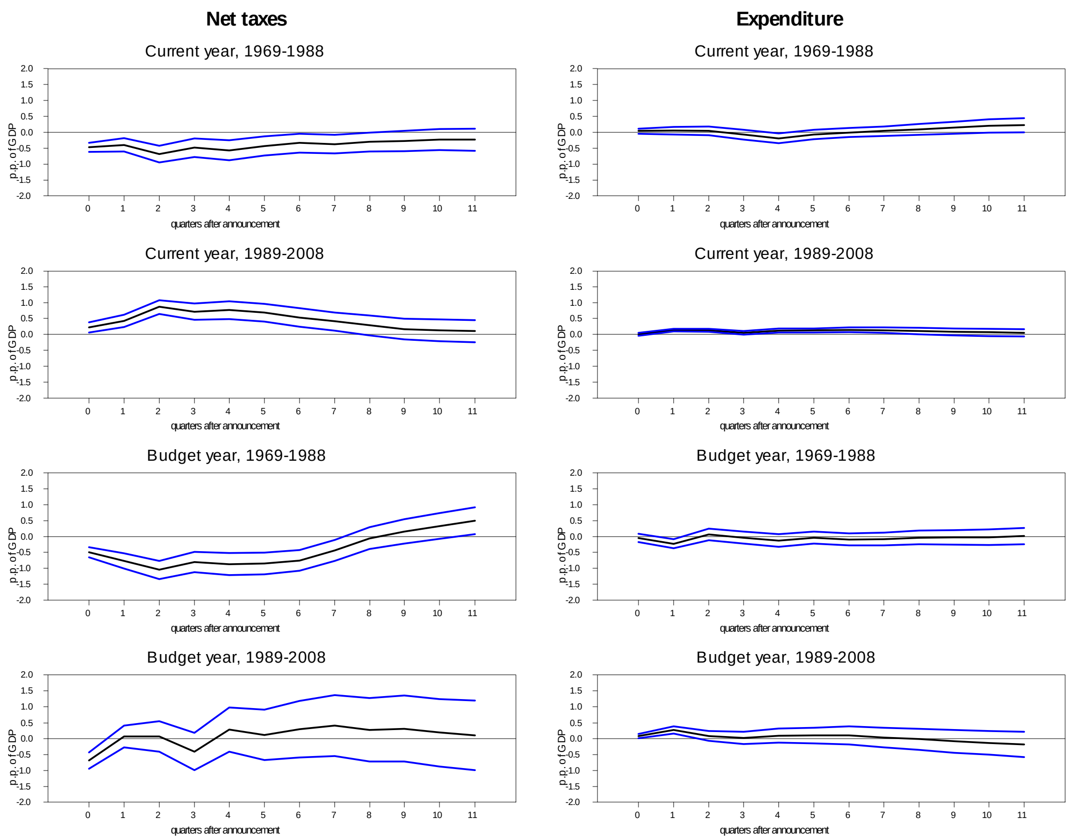

Figure 2.8: Responses of fiscal variables to the shock measures

The fact that the shocks appear approximated uncorrelated with implemented policy, at high frequencies, reinforces the reading that the effects presented in the previous sections are driven by changes in expected policy. One expects nevertheless the shocks to have information content in relation to implemented policy. In order to have a better specification to assess this, I now take budget outturn data *on a fiscal year basis*, so to match the frequency of the shocks. I add up all the ongoing-year shocks for the same fiscal year, and similarly for budget-year shocks. I run a regression of the outturn data on a constant, first lag of the dependent variable, and the aggregated ongoing- and budget-year shocks, for each fiscal year. Note that, maintaining the sample-split considered before, I have only 18 to 19 observations in each subsample.

Table 2.7: Fiscal shocks and prediction of actual policy

	FYs 1970-1988		FYs 1989-2007	
	ongoing-year shocks	budget-year shocks	ongoing-year shocks	budget-year shocks
Net taxes	0.941	-0.347	1.07	0.444
	(2.4)	(-0.8)	(5.4)	(1.1)
Expenditure	-0.194	-0.091	0.784	0.536
	(-0.6)	(-0.3)	(4.8)	(2.2)
Deficit	0.758	-0.136	1.06	1.03
	(2.1)	(-0.3)	(5.9)	(2.0)

Notes: Regression of outturn data on a constant, first lag of the dependent variable, and aggregated ongoing- and budget-year shocks, for a given fiscal year. Coefficients and t-ratios (in parenthesis) of the latter variables.

Table 2.7 presents the coefficients of the aggregated shocks in this regression, and the respective t-ratios. In the majority of cases they are statistically significant and positive, indicating that the shocks help predict actual policy. Some coefficients, notably for the budget-year shocks in the first subsample, are however negative and clearly not significant, indicating no impact. This may be due to particularly poor forecasting accuracy and the small size of the sample.

2.8 Impact on the funds rate and monetary authorities' behavior

The precise transmission of aggregate demands shocks and, in particular, fiscal shocks to the short-term rate obviously depends on the way monetary policy is conducted. For most of the sample period the behavior of the Federal Reserve is well described as having followed an interest rate targeting procedure or a borrowed reserves one, implying similar consequences for the funds rate in the presence of aggregate demand shocks. The short-term rate changes only as the Fed becomes aware of the new developments in the economy and reacts to them. In view of this, the movement in the same direction of the funds rate and output following the realization of positive deficit shocks in Figures 2.4 and 2.5 can generally be interpreted as reflecting the endogenous response of the policymaker to the deviation of output from trend. Considering the issue in more detail, however, one may investigate whether there is any hint of a direct reaction by monetary authorities to fiscal news, and an accommodation of what they forecast to be the impact on output of fiscal measures. In particular, this could weaken or perhaps reverse the standard response of the funds rate following deviations of output from trend (in the case of conventional impacts of fiscal shocks). Another issue to consider is that the period from October 1979 to October 1982 marks a temporary change in the Fed behavior, toward allowing the short-term rate to be determined by market forces. It is thus appropriate to complement the evidence in Figures 2.4 and 2.5 by presenting the same responses when the period 1979:10 to 1982:10 is excluded from the first subsample (Figure 2.9).

I first compare the responses of the federal funds rate to budget-year shocks in Figure 2.9 and in panels (b) of Figures 2.4 and 2.5. They are markedly different and consistent with the change in the Fed operating procedures. In the wake of positive aggregate demand shocks, if the Fed does not adjust non-borrowed reserves, there will be a quicker and possibly sharper rise in the money market rate than otherwise. Indeed, in Figure 2.5 the funds rate begins to rise about one month after impact, up to around 3 p.p. four months out (the peak impact is 3.7 p.p.). In contrast, in Figure 2.9 the money market

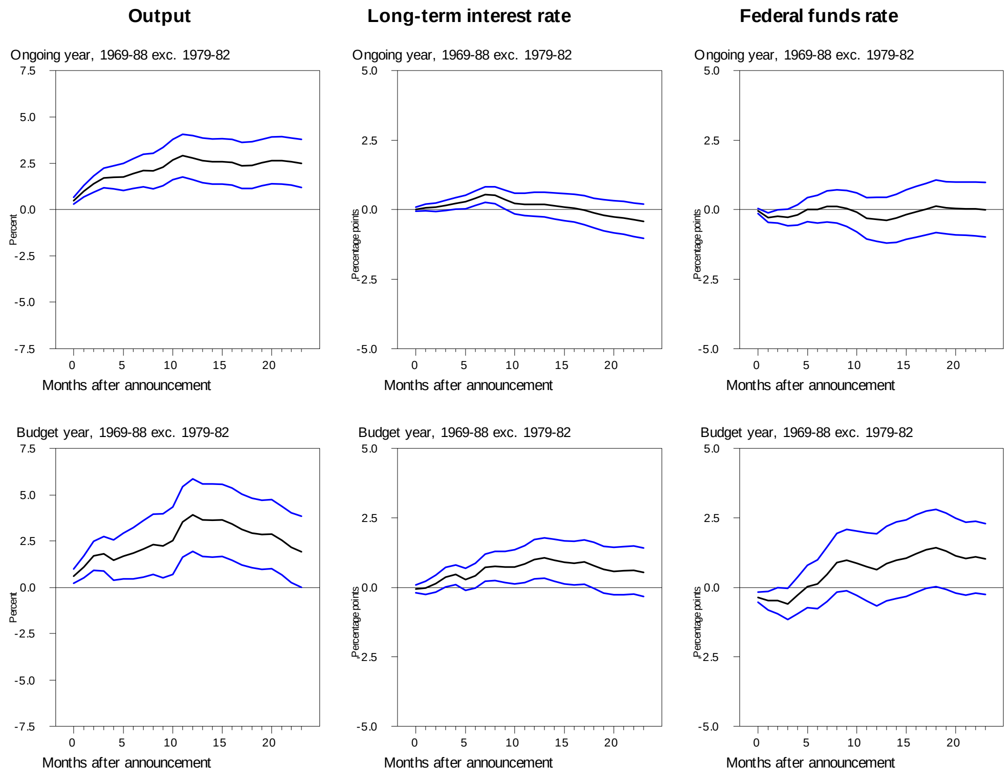

Figure 2.9: Macroeconomic responses to deficit shocks excluding the Volcker experiment

rate falls slightly during the first three months and only then starts to increase. The peak impact is only about 1.4 p.p. . Given that the years of the Volcker experiment span only a fraction of the first subsample, but its exclusion implies a substantial modification of the short-term rate response, this implicitly indicates a huge upward movement in the variable during the period. These results give a measure of the role played by the Federal Reserve as far as stabilization of interest rates is concerned. Note that the rise in the funds rate is considerably muted following ongoing-year shocks in the pre-1988:12 data (when the years 1979-1982 are excluded there is no rise at all). This difference in the behavior vis-a-vis their budget-year counterparts is difficult to explain, since ongoing-year shocks have a sizeable positive impact on output.

The negative trajectory of the funds rate in the initial months after the budget-year shocks in Figure 2.9, along with a rise in output approximately since impact, could signal some accommodation of fiscal shocks by monetary authorities in the first subsample. But the initial fall is small, being difficult to draw firm conclusions. Moreover, the magnitude of the peak change in output gap (around 2.0 percent)[25] and in the funds rate (1.4 p.p.) imply a sensitivity to the business cycle somewhat over 0.5. This appears to be a sensible figure in the light of previous studies (see Clarida et al. (2000)).

In the period post-1988:12, there is an initial stickiness in the funds rate in the wake of budget-year shocks, lasting for about five months before it goes down in line with the widening of the negative output gap. This response is consistent with the trajectory of output, rather subdued as well for those initial months. The degree of sensitivity to the cyclical variable implied by the results is now greater than 1.0. Although an increase in this parameter in the second subsample is consistent with what other studies have found, the figure is a bit above those usually computed. It is worth noting that in either of the subsamples there might be other factors at work, such as a positive reaction of the short rate to the long nominal rate, a possibility raised by Mehra (1997).

[25] Considering, as before, a factor of 1/2 to scale the output gap, from industrial production to GDP.

2.9 More on the impact on long-term bond yields

2.9.1 Impact on the risk premium

Figures 2.4, 2.5 and 2.9 indicate that the 10-year note rate accompanies the trajectory of the funds rate in a muted way in both subsamples. This sort of profile appears to reflect the dampening impact of the expectations channel, given the temporary nature of the federal funds rate response against the duration of the long-term bond. Simulations of simple macroeconomic models including, in particular, a monetary policy rule and a term structure relationship (such as in Walsh (1995, Ch. 10)) also predict a muted behavior of the long rate following changes in the federal funds rate. This suggests that uncertainty or credibility effects as stressed by the literature on expansionary fiscal contractions are best searched at the level of the risk premium. Market's expectations of the nominal short-term rate and risk premia are unobservable. The method I use to disentangle these two components is to proxy expectations through projections drawn from a reduced-form VAR. The risk premium is computed as the spread between the actual yield of the long bond and the yield implied by the pure expectations theory.

I denote by $\hat{r}_t$ the expectations component of the long-term interest rate, equal to the weighted average of market's expectation of the federal funds rate ($E_t ff_{t+j}$) over the holding period of the long bond: $\hat{r}_t = \sum_{j=0}^{N-1} \omega_j E_t ff_{t+j}$, where the ω_j are weights. I posit further that agents' expectations are formed on the basis of a reduced-form system comprising the variables in $\mathbf{x}_t = [y_t, p_t, ff_t, r_t]'$. They are thus obtained on a pure forecasting exercise basis. The federal funds rate is taken as a proxy for 1-month maturity, meaning that N is equal to 120 months, in order to span the life of the 10-year note. Then $\hat{r}_t$ can be written as a linear projection on a constant and current values and lags (the original number of lags in the system minus 1, 11 in this case) of the variables in $\mathbf{x}_t$. The coefficients of the projection are complicated functions of the reduced-form VAR coefficients and the weights, but they can be easily retrieved in practice from a regression of $\hat{r}_t$ on a constant and current and lagged $\mathbf{x}_t$ (which yields an exact fit).

The term premium, denoted by s_t, is obtained as a residual from the identity $r_t \equiv$

$\hat{r}_t + s_t$. The empirical strategy is to use this identity and the expression of $\hat{r}_t$ as a linear projection to write r_t as a function of the variables in $\mathbf{x}_t^* = [y_t, p_t, ff_t, s_t]'$. Then, in the original VAR equations used to compute the effects of fiscal policy shocks - in (2.6) above - to replace r_t by the derived expression, and rewrite as a system in $\mathbf{x}_t^*$. The latter is then used to compute the reaction of the term premium to shocks. The general approach followed here borrows from Bernanke et al. (1997), and I use their method for computing the weights. These are given by $\omega_j = \frac{\beta^j}{\sum_{j=0}^{N-1} \beta^j}$, and the monthly discount factor by $\beta = 0.997$.

A technicality arises at this point. When the long-rate is replaced by its expression in terms of the variables in $\mathbf{x}_t^*$, the resulting system corresponding to (2.6) has a different structure, in that it has a longer lag length for the first three variables in $\mathbf{x}_t^*$ and the fiscal shocks, and the disturbances are autocorrelated.[26] In order to save degrees of freedom and not to complicate the estimation, I impose the necessary restrictions (e.g. the lags beyond the 12th are excluded) on the system used to assess the effects of the shocks on $\mathbf{x}_t^*$, so that it has the same structure as (2.6). The results below confirm that this is a good approximation since the responses for the two components roughly add up to the overall response of the long rate. Note that the impact on the expectations component is simply calculated by replacing the original impacts on the variables in $\mathbf{x}_t$, for each period ahead, into the expression for $\hat{r}_t$. In order to account for the possibility of a structural change in the way expectations were formed over time, I estimated separately the underlying reduced-form VAR for each of the two subsamples that are being considered throughout the paper.

Figure 2.10 presents the impacts of budget-year deficit shocks of 1 percent of GDP broken down by the two components of the long rate. The first thing to note is that the trajectories of the long rate consistent with the pure expectations hypothesis are even more muted than the actual responses. This reflects the smoothing effect induced by the

[26] Illustrating this point more formally: let the projection yielding $\hat{r}_t$ be given by $\hat{r}_t = \sum_{i=0}^{11} \pi_i^1 L^i r_t + \sum_{i=0}^{11} \boldsymbol{\pi}_i^2 L^i \mathbf{x}_t^+$, where $\mathbf{x}_t^+ = [y_t, p_t, ff_t]$. Then, from the relationship $r_t \equiv \hat{r}_t + s_t$, $r_t = (1 - \pi_0^1 - \sum_{i=1}^{11} \pi_i^1 L^i)^{-1} (\sum_{i=0}^{11} \boldsymbol{\pi}_i^2 L^i \mathbf{x}_t^+ + s_t)$. Substituting this expression into, say, the first equation in the system (1.3) for y_t, one gets the corresponding equation in the new system which has the form $y_t = a + \sum_{i=1}^{23} \mathbf{b}_i \mathbf{x}_{t-i}^+ + \sum_{i=1}^{12} c_i s_{t-i} + \sum_{i=0}^{23} d_i \hat{d}_{t-i}^{cy} + \sum_{i=0}^{23} e_i \hat{d}_{t-i}^{ny} + \sum_{i=0}^{11} f_i \boldsymbol{\varepsilon}_{t-i}^y$.

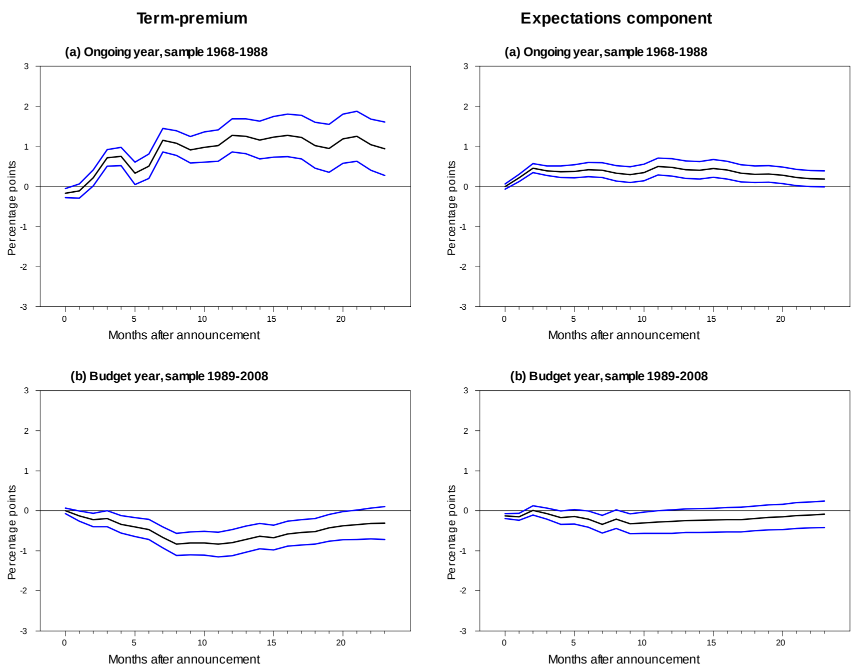

Figure 2.10: Response of the long-term rate to deficit shocks: term premium and expectations component

stationarity of the VAR which brings the forecast of the funds rate close to its unconditional mean for an important part of the lifespan of the long bond. This unconditional mean differs substantially in the two subsamples, being around is 8.5 percent in the first one and 2.9 percent in the second.

As far as the impact of deficit shocks is concerned, both the expectations component and the term premium rise in the first subsample. They account for, respectively, about 1/3 and 2/3 of the total movement in the long rate. The opposite happens in the second subsample for which both variables fall in the wake of the same shock, each justifying about 1/2 of the overall response. If investors were sensitive to the increased uncertainty brought about by fiscal loosening in a context of concern about budget sustainability, then a particularly large positive reaction of the risk premium would be expected in the second subsample. This is, however, contradicted by the response depicted in Figure 2.10. Other standard justifications for term premia as, for instance, that deficits put pressure on the demand for long-term bonds, pushing the respective interest rate upward relative to the short rate, would lead to positive responses in both periods. In short, the results for the term premium seem indirectly driven by the impact of budget shocks on aggregate demand, not to aspects specifically linked to fiscal policy.[27] Naturally these findings are conditional on the ability of reduced-form VARs to capture properly market's expectations of the short-term rate, which has been questioned (see Rudebusch et al. (2007)).

2.9.2 Impact on expected inflation

Older literature used to emphasize a related (and observationally equivalent) mechanism as far as the response of the long-term interest rate to fiscal policy was concerned. This was the hypothesis that monetary policy would ultimately bear the burden of protracted fiscal imbalances through deficit monetization. The argument is in its essence similar to the one underlying the expansionary fiscal contraction hypothesis. In the first case, the adjustment is expected to take place through accommodating monetary policy. In the

[27] It is beyond the scope of this paper to interpret the connection between the movements in aggregate demand and those in the term premium. This is a controversial issue, for which it is not even established whether there should be a positive or a negative association between them.

second case, it comes by a disruptive change in the course of fiscal policy. The fact that deficit monetization became a less considered possibility may reflect the added credibility that monetary authorities gained in terms of their commitment to fight inflation. Studies in that vein such as Brunner (1986) argued similarly to above that larger deficits would lead to a rise in the risk premium of long bonds, reflecting market's uncertainty about the profile of future inflation. They considered in addition an effect going in the same direction on the *level* of future inflation. I now investigate this last point. One could proxy inflation expectations through a VAR procedure as the one used to obtain the term premium (splitting short-term nominal rate expectations into expected inflation and a residual supposed to reflect short-term real rate expectations). I prefer, however, to bring in additional independent information about expected inflation coming from a survey.

Among surveys that ask responders to quantify their inflation expectations, the one most useful in our context is the Michigan Survey of Households because it has higher frequency data, namely, on monthly basis. Unfortunately the series starts only in 1978 and thus I restrict the investigation of the impacts to the second subsample (on which anyway interest focus now). A drawback of the Michigan Survey is that people are asked about the expected change in prices during the coming 12 months not, say, up to ten years ahead. It seems nevertheless reasonable to think that if there is an effect on expected inflation, this will emerge in the responses whatever the horizon asked. The impact of deficit shocks on expected inflation, measured in percentage points (annualized), are shown in Figure 2.11. They were obtained on the basis of univariate regressions, analogous to (2.3) but with monthly data (thus 12 lags of the regressors were included).

Expected inflation rises following an upward revision to anticipated deficits, the response being very small for ongoing-year shocks but reasonably large for their budget-year counterparts (the peak is close to 1 p.p.). These responses contrast with the imprecisely estimated and essentially zero ones of current inflation and thus do not appear to be induced by them. On balance, this is the only piece of evidence I get suggesting a positive effect on nominal long-term interest rates of policy loosening in the post-1988 period.

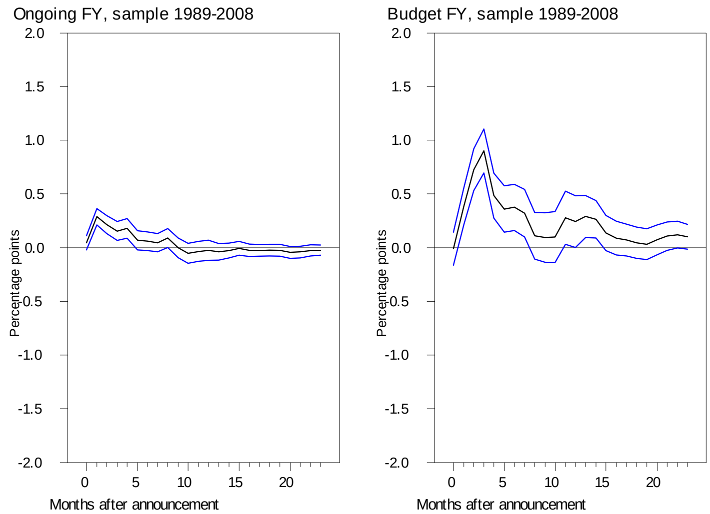

Figure 2.11: Response of expected inflation to deficit shocks

2.10 Concluding remarks

This study developed a new measure of fiscal shocks based on changes to anticipated fiscal policy and drew inferences about its impact on the economy. Splitting the sample into to periods, one finds opposite effects of fiscal policy in the two subsamples considered: 1969-1988 and 1989-2008. Policy tightening is contractionary in the first subsample but expansionary in the second. The findings in this more recent period put clearly a question mark on the use of discretionary fiscal policy as a stabilizing tool. They are troubling against the background of the recent recession where fiscal policy has been called to play an important anti-recessionary role, and governments in the US and elsewhere have implemented stimulus packages of considerable sizes. The evidence presented in this paper suggests that research should consider more explicitly instability in the effects of fiscal shocks, and unconventional responses to them.

Appendices

2.A Data availability

Concerning fiscal data, the only availability gap concerned the post-budget year projections in the last announcement before the submission of a new budget, which could be used only from FY 1998 on. On the one hand, these started to be published only toward the middle of the sample. On the other hand, I had only partial access to the elements in the Mid-Session Reviews prior to FY 1998 (note that this study was carried out on the basis of resources on the web), not including such projections. This latter aspect, however, precluded the computation of only about 10 observations in the series of budget-year shocks.

Concerning the macroeconomic assumptions, three main availability shortcomings had to be tackled. The first one stemmed from the fact that the macroeconomic scenario underlying budget forecasts is not presented on a fiscal year basis. It takes instead the calendar year as a reference or, more often, in the case of real and price growth, the change fourth quarter to fourth quarter. Real and price GNP/GDP growth on a fiscal year basis were derived using the following strategy. I considered real-time quarterly data, up to the time the projection was drawn (taken from the relevant issue of the *Economic Indicators*) and drew quarterly forecasts for the periods ahead in such a way to be consistent with the administration's yearly (or fourth quarter over fourth quarter) growth rate. More specifically, I took the growth rate (year-on-year) of the last quarter available and assumed a constant increment of this rate from one quarter to the other within each calendar year. A similar procedure was followed for the 3-month Treasury bill rate, but taking the level of the variable.

A second issue was that, while I always had the macroeconomic forecasts underlying budget submissions, for the remaining announcements this was not the case before 1992 (except for the years 1988-89). This was partly due to the aforementioned lack of access to the full text of the Mid-Session Reviews during an important part of the sample. However, for some announcements in the initial years, in particular those not backed by documents, the underlying macroeconomic assumptions may not be retrievable anymore. For the announcements in which the assumptions were missing, they were proxied by considering firstly the real-time quarterly data contemporary with the release. Then, real and price GNP/GDP growth (and the level of the short-term interest rate) for a given quarter ahead were calculated as a weighted average of the figures for the same quarter in the announcements immediately before and after for which assumptions were available.

Additional difficulties were faced in the period prior to FY 1976, for which only assumptions for the current calendar year were given in the budget documents. Moreover, no projection for the 3-month Treasury bill rate was given at all. Note that, in this period, the current calendar year ran until the middle of the upcoming fiscal year which started in July, 1. Thus I had to extend the procedures just described in order to obtain figures for the two missing quarters of the budget fiscal year. In the case of GNP real growth and deflator, I simply assumed the same growth rate (year-on-year) as obtained for the fourth quarter of the current calendar year. In the case of the short-term rate, given the absence of a projection, I set all quarters ahead equal to the average of the last two quarters known at the time of the announcement. For the announcements during the period 1968-74, I then included in regression (2.1) a dummy variable interacting with the short-term interest rate projection, in order to allow it not to have an impact on the fiscal forecast.

Chapter 3

Fiscal policy and time variation in the US

3.1 Introduction

Effectiveness of fiscal policy to stimulate activity remains a highly controversial topic, as it resurfaced in the discussion of the stimulus packages implemented in the awake of the 2008-09 recession. This controversy stems firstly from the differences between the predictions of neoclassical and some New Keynesian macromodels. Secondly, on the empirical side, different measurement strategies lead to different estimated shock series and measured impacts. On the top of this, *even under the same methodology*, results may vary substantially when the sample period varies - as documented in the first and the second essays.

The issue of time variation must be given careful consideration if one is to determine precisely what the existing identification methodologies imply in terms of the impacts of fiscal policy. The paper takes up this issue in the framework of the Blanchard and Perotti identification methodology, by embedding it into a VAR with time-varying parameters (TVP). Such models have great flexibility in terms of capturing time variation and are free from the shortcomings of less formal alternative approaches such as split- or rolling-sample estimates. Indeed, the latter have the disadvantage that, on the one hand, the number and timing of the possible breaks are unknown and, on the other hand, they do not accommodate more general forms of time variation such as a gradual change in the parameters. It is worth noting that TVP-VAR models have been already used in a relatively large number of papers focusing on monetary policy (e.g. Cogley and Sargent (2001), Cogley and Sargent (2005), Primiceri (2005)). Applications to fiscal policy are still scarce; it is worth mentioning Kirchner et al. (2010) who implement a model of this kind for the euro area.

The methodology for estimating reduced-form VARs with time-varying coefficients and covariance matrices is well established by now. However, its application to the case of identified VARs, particularly with non-recursive identification schemes, as the one I use, poses some questions insufficiently covered in the literature. The contribution of this paper is thus twofold. At the theoretical level, I extend the TVP-SVAR field to more general identification schemes like the one of Blanchard and Perotti. In this framework, at the empirical level, I document changes in the effects and the conduct of fiscal policy in the US over time.

The structure and key results of the paper are as follows. Sections 3.2 and 3.3 deal with methodological issues. TVP-VARs are usually simulated with the aid of Bayesian tools. More precisely, I use the Gibbs sampler as applied to the analysis of state-space models (see Kim and Nelson (1999)). An overview of the simulation procedure is given in the text, but the full details are left to an appendix. These sections also describe the identification strategy and the way how it is embedded into the simulation procedure. In Section 3.4 I adduce some evidence about parameter instability when the model is estimated with a traditional fixed-parameter specification. The outcome of the stability tests provides backing to the use of a model where both coefficients and the covariance matrix can vary with time, i.e. the so-called heteroskedastic TVP model. The remaining sections of the paper present and discuss the results.

I identify shocks to the two fiscal variables, taxes net of transfers and government spending, and the simulation period stretches from 1965 to 2009 (using quarterly data). I find a drop in the effects of net taxes on output around mid-seventies, and then a further gradual weakening until the end of the sample. The effects of expenditure shocks have faded over time as well. These findings generally accord with the belief that fiscal policy has lost power to stimulate activity in the last decades. A particular hypothesis investigated is whether there has been an increase in policy effectiveness in the course of recessionary episodes, and I find moderate support for it. The amount of time variation I get is more modest than the one suggested by the estimation of the time-invariant parameter version of the model over a rolling sample, which I also present to have a bridge to previous studies.

I then go on to investigate the impacts of fiscal policy on consumption. Positive shocks to net taxes bring private consumption down, and the respective multiplier remains stable throughout. On the expenditure side, I find evidence of a negative and small multiplier within the quarter and, in recent decades, essentially zero multipliers for longer horizons. The evidence is not consistent with a sizeable Keynesian impact of expenditure shocks on consumption that SVARs are normally believed to corroborate, though it could square with some New Keynesian models.

A final issue I address are patterns of time-variation in the conduct of fiscal policy. As regards systematic policy, there has been an overall increase in the countercyclical responsiveness of net taxes to output over time. In particular, there was a jump in fiscal activism during the 1973-75 recession and this indicator appears to have reached a peak in the course of the 2008-09 recession. I get procyclical expenditure responses, featuring a decreasing trend throughout the simulation period.

3.2 System definition and identification

In the time-varying parameter context, it is convenient to write the VAR in such a way that the reduced-form coefficients are stacked into a single vector. Following this convention, the model I consider throughout the paper can be written as:

$$\mathbf{x}_t = X_t \boldsymbol{\theta}_t + \mathbf{u}_t, \tag{3.1}$$

$$A_t\mathbf{u}_t = B_t\mathbf{e}_t, \tag{3.2}$$

$$\mathbf{e}_t = D_t\boldsymbol{\varepsilon}_t, \tag{3.3}$$

where $\mathbf{x}_t$ is a $n \times 1$ vector of macroeconomic variables and $X_t = I_n \otimes [1, \mathbf{x}'_{t-1}, ...\mathbf{x}'_{t-p}]$; $\boldsymbol{\theta}_t$ is a $n(np+1) \times 1$ vector that stacks the reduced-form coefficients, equation by equation ($\boldsymbol{\theta}_t = vec([\boldsymbol{\mu}_t, \Theta_{1,t}, ..., \Theta_{p,t}]')$ with $\boldsymbol{\mu}_t$ a $n \times 1$ vector of constants and $\Theta_{j,t}(j = 1, ..., p)$ $n \times n$ matrices including the coefficients for the lag j of the endogenous variables); A_t and B_t are $n \times n$ matrices with the contemporaneous coefficients and D_t is a $n \times n$ diagonal matrix that includes the standard deviations of the orthogonalized shocks. All parameters are allowed to vary stochastically over time. It is assumed that $\boldsymbol{\varepsilon}_t$ is a $n \times 1$ Gaussian vector with $E[\boldsymbol{\varepsilon}_t] = \mathbf{0}$ and $E[\boldsymbol{\varepsilon}_t\boldsymbol{\varepsilon}'_t] = I_n$, implying that $\mathbf{u}_t$ and $\mathbf{e}_t$ are vectors of Gaussian heteroskedastic disturbances such that $E[\mathbf{u}_t|A_t, B_t, D_t] = E[\mathbf{e}_t|D_t] = 0$, $E[\mathbf{u}_t\mathbf{u}'_t|A_t, B_t, D_t] = A_t^{-1}B_tD_tD'_tB'_t(A_t^{-1})' = \Sigma_t$ and $E[\mathbf{e}_t\mathbf{e}'_t|D_t] = D_tD'_t$. System (3.1) corresponds to a reduced-form system, system (3.2) specifies the structural decomposition of the covariance matrix Σ_t and system (3.3) the volatility of the structural disturbances.

My baseline specification has four variables: net taxes (nt_t), government expenditure (g_t), inflation (p_t) and output (y_t) (see Section 3.5.1 for more on the definition of the variables). Let $\mathbf{x}_t$ be equal to $[nt_t, g_t, p_t, y_t]'$, $\mathbf{u}_t$ to $[u_{nt,t}, u_{g,t}, u_{p,t}, u_{y,t}]'$ and $\mathbf{e}_t$ to $[e_{nt,t}, e_{g,t}, e_{p,t}, e_{y,t}]'$. A first formulation of the identification scheme, useful in order to motivate it, is one such that matrices A_t and B_t in (3.2) are given, respectively, by (time subscripts indexing the elements omitted):

$$A_t = \begin{bmatrix} 1 & 0 & -a_{13} & -a^*_{14} \\ 0 & 1 & -a^*_{23} & 0 \\ 0 & -a_{32} & 1 & 0 \\ -a_{41} & -a_{42} & -a_{43} & 1 \end{bmatrix}, B_t = \begin{bmatrix} 1 & b_{12} & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 1 \end{bmatrix}. \tag{3.4}$$

I identify the shocks to net taxes and expenditure, and impose a convenient orthogonalization between price and output shocks ordering the latter variable in the second place.

Net taxes respond contemporaneously to prices and output, but expenditure responds only to the first of these variables. This latter restriction is a common one in fiscal VARs identified by restrictions in the matrices of contemporaneous coefficients. Output is allowed to react within the quarter both to net taxes and expenditure, but prices can react to expenditure only. Further, government expenditure is ordered before net taxes. This identification scheme is a simplified version of the one in the first essay, in that there is no contemporaneous reaction of prices to net taxes. The elasticities of net taxes to output and expenditure to prices, a^*_{14} and a^*_{23}, are calibrated according to the formulas given in Appendix 1.A, which is in turn based on the procedure introduced by Blanchard and Perotti (2002) (the price elasticity of taxes, a_{13}, is estimated). The calibrated figure for the first elasticity varies over time, while that for the second one is constant. The order condition is met with exact identification in (3.4), given that the number of free parameters (six) is equal to the number of free elements of Σ_t less the four standard deviations in D_t.

In the time-invariant parameter version of system (3.2), with matrices A and B as in (3.4), the resulting equations would include endogenous regressors: u^g_t would be endogenous in the price equation, u^p_t in the net tax equation, and u^{nt}_t in the output equation. Hence in this setting, the structural decomposition in (3.4) would have to be estimated by 2SLS[1] (or a more general method, such as maximum likelihood). When one moves to a time-varying context, it is convenient that matrices A_t and B_t are such that the equations following from (3.2) include predetermined variables only. As explained in the next section, in this case the identification scheme can be easily embedded into the algorithms for normal linear state space models used to draw for the matrix Σ_t. This prerequisite is met in the alternative definition of matrices A_t and B_t as:

[1] It would be estimated sequentially using the residuals of previous steps as instruments for the endogenous regressors. Specifically, $\hat{e}^g_t$ as an instrument for $\hat{u}^g_t$ in the price equation, $\hat{e}^p_t$ as an instrument for $\hat{u}^p_t$ in the net tax equation, and $\hat{e}^{nt}_t$ as an instrument for $\hat{u}^{nt}_t$ in output equation.

$$A_t = \begin{bmatrix} 1 & 0 & 0 & -a_{14}^* \\ 0 & 1 & -a_{23}^* & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 1 \end{bmatrix}, \ B_t = \begin{bmatrix} 1 & \beta_{12} & \beta_{13} & 0 \\ 0 & 1 & 0 & 0 \\ 0 & \beta_{32} & 1 & 0 \\ \beta_{41} & \beta_{42} & \beta_{43} & 1 \end{bmatrix}, \tag{3.4'}$$

which form an identification scheme equivalent to (3.4), in the sense that it yields the same impulse-responses in a time-invariant setting.[2] As shown in Appendix 3.B, there is a one-to-one correspondence between the parameters in both schemes - in particular, the calibrated parameters coincide. The Bayesian simulations in the paper take as a reference the definition of matrices A_t and B_t as given in (3.4').

I consider a generalization of the baseline system including private consumption when studying the effects of fiscal policy on this variable. The latter variable is ordered last in the system, and a convenient orthogonalization of it in relation to output and prices is imposed. This should be innocuous for the object of interest, the effects of the fiscal policy shocks. It is straightforward to modify the identification methodology for the baseline specification to accommodate such an extension.

3.3 Formalizing time variation and Bayesian simulations

Three blocks of time-varying parameters (or states) are considered. The first one includes the «coefficient states», i.e. the coefficients of the reduced form in vector $\boldsymbol{\theta}_t$. The second block has the «covariance states», the non-zero and non-one elements of B_t in (3.4') - recall that matrix A_t has no unknown elements. Let $\mathbf{b}_{i,t}$ denote vectors collecting the states corresponding to row i; there are three such vectors: $\mathbf{b}_{1,t}$, $\mathbf{b}_{3,t}$ and $\mathbf{b}_{4,t}$. The third block includes the «volatility states» which are the elements in the main diagonal of D_t. These are taken in logarithms and collected in the vector $\log \mathbf{d}_t$. The coefficient and covariance states are assumed to follow random walks and the volatility states geometric random walks, as it is common in the empirical applications of this sort of models. That

[2] The estimated structural shocks ($\hat{\mathbf{e}}_t$) resulting from (3.4) and (3.4') coincide for net taxes and expenditure, and coincide except for a scale factor for output and prices - see Appendix 3.B.

is,

$$\boldsymbol{\theta}_t = \boldsymbol{\theta}_{t-1} + \boldsymbol{\epsilon}_t^{\theta}, \tag{3.5}$$

$$\mathbf{b}_{i,t} = \mathbf{b}_{i,t-1} + \boldsymbol{\epsilon}_t^{bi}(i = 1, 3, 4), \tag{3.6}$$

$$\log \mathbf{d}_t = \log \mathbf{d}_{t-1} + \boldsymbol{\epsilon}_t^{d}. \tag{3.7}$$

where it is assumed that $\boldsymbol{\epsilon}_t^{\theta} \sim i.i.d.N(0, Q^{\theta})$, $\boldsymbol{\epsilon}_t^{bi} \sim i.i.d.N(0, Q^{b_i})$, and $\boldsymbol{\epsilon}_t^{d} \sim i.i.d.N(0, Q^{d})$, and that the disturbances $\boldsymbol{\epsilon}_t^{\theta}$, $\boldsymbol{\epsilon}_{i,t}^{b}$, and $\boldsymbol{\epsilon}_t^{d}$ are orthogonal to each other and also to $\boldsymbol{\varepsilon}_t$. The elements of matrices Q^{θ}, Q^{b_i} and Q^{d} are usually called the hyperparameters. Apart from the block-diagonality of the covariance of the innovations relating to covariance states, I impose no other restrictions on the matrices of the hyperparameters.

The simulation of the heteroskedastic TVP-VAR using Bayesian methods is by now fairly standard, so I outline here the main steps and give the full details in Appendix 3.A. The algorithm employed iterates on a number of blocks using the conditioning feature of the Gibbs sampler. The time-varying parameters are treated as unobserved state variables whose dynamics is governed by the transition equations (3.5), (3.6) or (3.7) which, together with the measurement equations relating the state variables to the data, form a normal linear state-space model in each block. A Bayesian algorithm for this model, as proposed in Carter and Kohn (1994) (see Kim and Nelson (1999) for a description), is run sequentially, sampling the state vectors from the posterior Gaussian distributions with mean and covariance matrix obtained from running the ordinary Kalman filter followed by a backward recursion.

More precisely, the Gibbs simulation algorithm consists of going through the following steps at each iteration.

Step 1: The measurement equation in this block is given by (3.1) and the state equation by (3.5). A history of $\boldsymbol{\theta}_t$'s is generated conditional on the data, histories of covariance and volatility states (which yield a history of Σ_t's) and the covariance of innovations in the state equation (Q^{θ}).

Step 2: The normal linear state space algorithm is applied sequentially equation by equation, conditional on the data, histories of coefficient and volatility states, and the covariance of innovations in state equations (Q^{b_i}). The measurement equations come from (3.4') and the state equations from (3.6). A history of $\mathbf{b}_3$'s is generated firstly, then, conditional on it, a history of $\mathbf{b}_1$'s and, finally, conditional on both, a history of $\mathbf{b}_4$'s.

Step 3: The measurement equation is based on a transformed version of (3.3) and the state equation is (3.7). A history of $\log \mathbf{d}$'s is generated conditional on the data, histories of coefficient and covariance states, and the covariance of innovations in state equation (Q^d).

Step 4: The model's hyperparameters, Q^θ, Q^{b_i} and Q^d, are generated conditional on histories of the corresponding state vectors ($\boldsymbol{\theta}_t$, $\mathbf{b}_{i,t}$ and $\log \mathbf{d}_t$).

There is one aspect that merits discussion in the application of Bayesian methods in the context of the multivariate stochastic volatility model. The methods that have been used in empirical macroeconomics to estimate a time-varying matrix Σ_t, notably in Cogley and Sargent (2005) and Primiceri (2005), require a decomposition of this matrix of the form $\Sigma_t = L_t H_t L_t'$, with L_t lower triangular and H_t diagonal. Under this assumption it is possible to draw blockwise from the distribution of the covariance states (L_t), and from the distribution of the volatility states (H_t). The measurement equations are in this case given by $L_t \mathbf{u}_t = \mathbf{e}_t$ and $\mathbf{e}_t = H_t \boldsymbol{\varepsilon}_t$, which correspond to (3.2) and (3.3) above. Note that the variables in the i-th measurement equation following from $L_t \mathbf{u}_t = \mathbf{e}_t$, that is u_{jt} with $j < i$, are predetermined. On the basis of this result, the normal linear state space algorithm can be applied equation by equation, once independence between the states belonging to different equations is assumed. This latter assumption is equivalent to a block-diagonal covariance matrix of the respective innovations, each block relating to a given equation.

The estimate of Σ_t obtained as just described depends on the ordering of the variables underlying the triangular structure of L_t. This is, in general, a undesirable feature of the impulse-responses coming from TVP-SVARs with stochastic volatility. They will depend on the identification scheme applied to the draws of Σ_t, but the draws themselves already depend on a previous orthogonalization scheme. When the identification restrictions assume the form of a triangular factorization, as it is often the case in monetary policy VARs, a straightforward way to circumvent this is to draw for Σ_t already using that factorization. That is, one embeds the identification scheme into the simulation procedure.[3] It is possible to do the same in our case, when formulation (3.4') of the identification scheme is used because, as seen, this gives raise to a system of equations where all regressors are predetermined (in contrast to formulation (3.4)). The normal linear state space algorithm can be applied equationwise, as long as independence between the parameters belonging to different rows of B_t is assumed.

3.3.1 Priors and practical issues

In order to make the whole procedure operational, prior distributions need to be specified both for the initial states and the hyperparameters. I follow the previous TVP-VAR literature in this regard. The priors for the initial states are Gaussian, with means given by the point estimates, $\hat{\boldsymbol{\theta}}_t$, $\hat{\mathbf{b}}_{i,t}$ and $\log \hat{\mathbf{d}}_t$, from estimating a time-invariant VAR over the training subsample 1947:1-1959:4, and covariance matrices equal to multiples of the corresponding asymptotic covariances[4] (see Appendix 3.A). It is worth noting that the calibration of the priors for the initial states has typically almost no influence on *a posteriori* inference. The hyperparameters have conjugate inverse-Wishart priors, with scale matrices equal to a constant fraction of the aforementioned asymptotic variances of the parameters estimated over the training subsample (multiplied by the respective degrees of freedom). This constant fraction summarizes the prior beliefs about the amount of time variation. In the prior for the covariance matrix of the innovations relating to coefficient states , Q^{θ}, this was set to the benchmark value of $(0.01)^2$, used by Cogley and Sargent (2001) and virtually all subsequent TVP studies.[5] This is a conservative figure, as it can

[3] Primiceri (2005) suggests a more general procedure in case several factorizations i.e. orderings of the variables appear plausible. This is to impose a prior on each of them, and then average the results obtained on the basis of posterior probabilities.

[4] Except for the initial state of $\log \mathbf{d}_t$ whose covariance matrix is set to a multiple of the identity.

[5] The corresponding value for Q^d was set to $(0.01)^2$ and the ones for Q^{b_i} to $(0.1)^2$, following Primiceri (2005).

be interpreted as time variation accounting for 1 percent of the standard deviation of each coefficient. As discussed below, however, using larger values for that constant - implying more prior volatility of the states - changes little in the pattern of posteriori time variation in the effects of fiscal policy.

One issue arising in the simulation of TVP-VARs is whether to impose a stability condition that discards the draws of $\boldsymbol{\theta}_t$ that imply non-stable systems.[6] As one might expect, this condition makes more of a difference for the impulse responses at longer horizons (according to the experience in the current application, say, longer that 4 steps ahead), since the stability properties of the system become apparent as one projects it into the future. In Cogley and Sargent (2001) the variable of concern was inflation, and they imposed the stability condition on the grounds that Fed's behavior rules out explosive paths of this variable. In the context of fiscal policy, as noted in Kirchner et al. (2010), there might not be such a compelling theoretical reason for imposing this condition, because fiscal policy may have not been on a sustainable paths at some points in time. I chose to report results without the stability condition, and signal in the text how they change when it is imposed. A practical aspect about the stability condition is that it makes the simulation procedure more time consuming, given that part of the draws are thrown out. In the application at hand, approximately two out of three draws were unstable.

In this paper, a «filtered» variant of the simulation algorithm is used (as in Cogley and Sargent (2001) and Gambetti et al. (2008)). Full sets of iterations of the Gibbs sampler are sequentially implemented, with the simulation period being extended by one year at a time. The starting date is always 1960:1; the first ending date is 1965:2, and the last one 2009:2 (the full set iterations is thus repeated 45 times). For each ending date, 30,000 iterations of the Gibbs sampler are run, after a burn-in period of 5,000, and every 5th iteration kept. The implied impulse-responses for each of the kept draws (6,000) are computed, and I report statistics of the distribution of those responses.[7] I also

[6] This is implemented in such a way that the whole history of $\boldsymbol{\theta}_t$'s generated at step 1 is discarded, in case the condition is not met, at least, for one t.

[7] The simulations were implemented in RATS.

report at the end of Appendix 3.A results concerning the autocorrelation functions of the draws, which give an indication about the convergence properties of the algorithm. These autocorrelations are generally low, indicating that the chain mixes well.

3.4 Some preliminary evidence about parameter instability

In this section I apply parameter instability tests to the fixed-parameter version of the fiscal VAR, in order to back up more formally the assumption that time-variation is indeed preferable as a modelling alternative. This sort of tests has been employed, for instance, by the literature investigating regime changes in macroeconomic relationships, as in Stock and Watson (2002) and Ahmed et al. (2004) who focus on the moderation in GDP growth volatility in recent decades. I perform two such tests. The first one is the Nyblom-Hansen test presented in Hansen (1992) which has the random-walk TVP model as the alternative hypothesis. This is the model I simulate in the next sections using Bayesian techniques. The stability tests were implemented by estimating directly the structural form of the system, that is, in the notation of Section 3.2: $A\mathbf{x}_t = A\boldsymbol{\mu} + A\Theta_1\mathbf{x}_{t-1} + ... + A\Theta_p\mathbf{x}_{t-p} + B\mathbf{e}_t$. Given that the 2SLS, equation by equation, is used, the test statistic was computed according to the particular formulation for this estimator in Hansen (1990).

The second test is based on the Quandt likelihood-ratio statistic in Wald form (QLR), that is, the maximum of the Chow statistic calculated for a sequence of breakdates over a portion of the sample. This is a test of parameter constancy against the alternative of a single break of unknown timing, although it has also power against the randomly TVP alternative. The sequential breakdates were defined considering a symmetric trimming of 25%: they start at 1963:2 and end at 1994:3 (note that the usable sample is from 1948:2 to 2009:2). At each break date all coefficients in each equation were allowed to change by means of interacting dummies. The Wald statistic for joint exclusion of these dummies was then computed taking the White heteroskedasticity-consistent covariance matrix (p-values obtained as described in Hansen (1997)). The display of the values of the test statistic over time is interesting as it gives an indication about the occasion(s) where a structural break is more likely to have taken place. After testing for a change in the coefficients, I tested for a break in the variances using a simple procedure from Stock and Watson (2002). I took the residuals from estimating each equation allowing for a break in the coefficients at the date selected by the QLR test. I then repeated this test in regressions of each series of residuals in absolute value on a constant and a dummy, in order to test for a change in the respective mean (that is, the results of the variance stability test are computed assuming a break in the regression coefficients).

The distributions of the Nyblom-Hansen and Quandt likelihood ratio statistics are derived under the assumption of stationary regressors. Non-stationarity biases the results of the tests toward showing instability. This should not interfere with my results, because I detrend GDP, net taxes and expenditure prior to estimation (see Section 3.5.1), and the price variable is measured as the first differences of the log GDP deflator.

Table 3.1: Results of parameter stability tests

Equation (p-values)	Nyblom joint	Nyblom variance	QLR coeffs.	QLR variance
Net taxes	0.16	0.07	0.00	0.73
Expenditure	0.01	0.00	0.00	0.00
GDP deflator	0.00	0.00	0.00	0.00
GDP	0.01	0.00	0.00	0.00

Note: p-values of the Nyblom-Hansen test for driftless random-walk coefficients and variance (1st column) and variance only (2nd), and p-values of the QLR test for a single break of unknown timing in the coefficients (3rd) and variance (4th). The usable sample is 1948:2 to 2009:2 and the break search dates for the QLR test are between 1963:2 and 1994:3.

Table 3.1 shows the p-values for the Hansen-Nyblom and QLR tests, and Figure 3.1 plots the full sequences of QLR statistics. The p-values point to widespread parameter instability in the system. As regards the expenditure equation, the sequence of QLR statistics suggests a break in the coefficients (more strongly than one in the variance), occurring toward the beginning of the sample. This might be accounted for by the Korea War that made the stochastic process followed by expenditure in the early fifties very

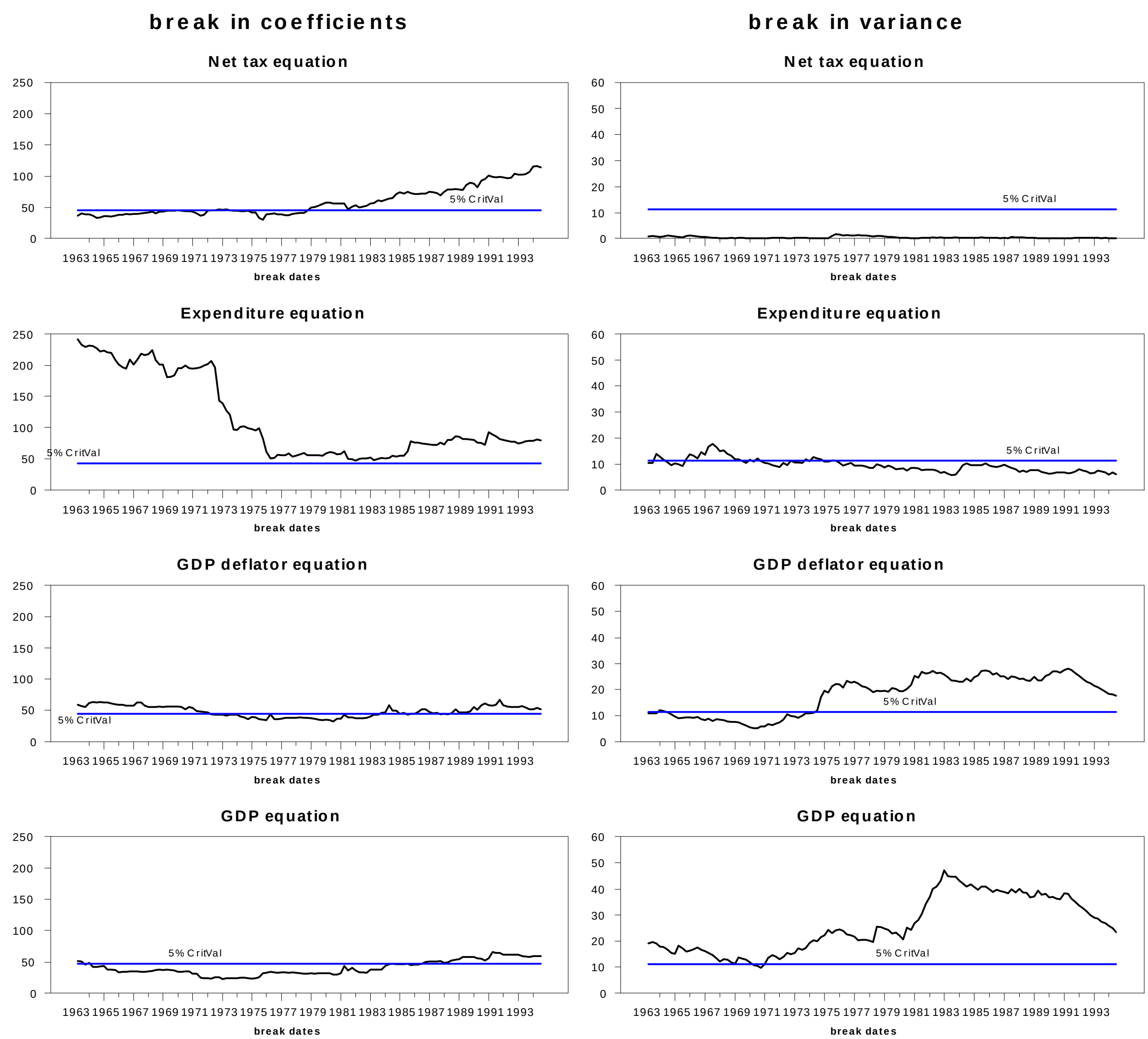

Figure 3.1: Sequencies of QLR statistics

different from subsequently. As far as the output equation is concerned, in contrast, there is much stronger evidence of a break in the variance than in the coefficients (same happens for the price equation). This is consistent with the findings of the literature on the great moderation that regime changes affected first and foremost the volatility of the shocks (see Stock and Watson (2002)).

The Hansen-Nyblom test does not reject the parameter constancy hypothesis for the net tax equation (at the 5 per cent level). The results from the QLR test are partly contradictory with this, since they do reject the null of constant coefficients, with the evidence cumulating in the second half of the sample. It might be that instability in the coefficients of this equation is more of the single break type, and thus best captured by the QLR statistic. With the variance is the other way around, only the Hansen-Nyblom test signals instability.

As a whole, the results of the tests clearly back up the use of a specification with time-varying parameters, against a fixed-parameter one. Moreover, they call for a model which accommodates stochastic volatility. At the same time, the results of the QLR statistic indicate different break timings depending on specific equations and parameters (not a generalized regime change affecting all equations at the same point in time). In this context, a model with time-varying parameters appears superior to the traditional split- or rolling-sample estimates of a fixed-parameter model.

3.5 Results

3.5.1 Data

The baseline specification includes four variables: taxes net of transfers, government expenditure (consumption plus investment)[8], GDP and inflation. I also estimate a specification including private consumption. Taxes net of transfers, government expenditure, output, and private consumption are in loglevels, in real and *per capita* terms. I detrend all these variables prior to estimation by regressing them on a polynomial of second degree

[8] For the precise way how fiscal variables are computed, see the Appendix 1.B in the first essay.

in time. Inflation is calculated as the change in the log GDP deflator at annual rates. The data are on a quarterly basis and the lag length of the system is set to 2, the same value as in previous studies with TVP-VARs. A short lag length prevents the simulation procedure from becoming too heavy, as it reduces considerably the size of the vector of coefficient states (for instance in the benchmark system, from 68 elements with 4 lags to 36 elements with 2 lags). It is worth noting that, in a time-invariant setting, SVARs estimated with quarterly data normally include 4 lags. For the sake of comparison with previous studies, I also estimate such a version of the model over a rolling-sample, and adopt a lag length of 4 in that instance.

3.5.2 Responses of output to fiscal shocks

Figure 3.2 presents the percentage responses of output to fiscal shocks in the model with driftless random-walk parameters. The shocks have the size of 1 percent of GDP, and so the figures have the interpretation of multipliers. The charts show for date t the simulated impulse-responses with the parameters indexed to that date[9] for four horizons: within the quarter and 1,2 and 3 years ahead. I present both the median response (darker line) and the average response (lighter line), as they differ somewhat for longer horizons, plus confidence bands corresponding to the 16 and 84 percentiles. The shaded areas in the charts are the NBER recessions.

I comment on the median response which is less sensitive to the «extreme» responses brought about by unstable draws. There is a weakening of the effects of net tax shocks throughout the simulation period. The multiplier within the quarter evolves from around -0.8 in the mid-sixties to -0.4 toward 2009. Such an effect is, however, more visible for longer horizons. For instance, 1 year ahead, the multiplier fluctuates around -2.0 until mid-seventies, then there is a peak of effectiveness in 1975, with a figure of -2.5. This is followed by a drop (in absolute terms) to about -1.5, and a further decrease to -1.0 by the end of the simulation period. On the expenditure side, the amount of time variation

[9] I follow the usual practice of presenting a simplified version of the impulse-responses, in which the response for shocks at t is a function of the parameters estimated for that date all steps ahead.

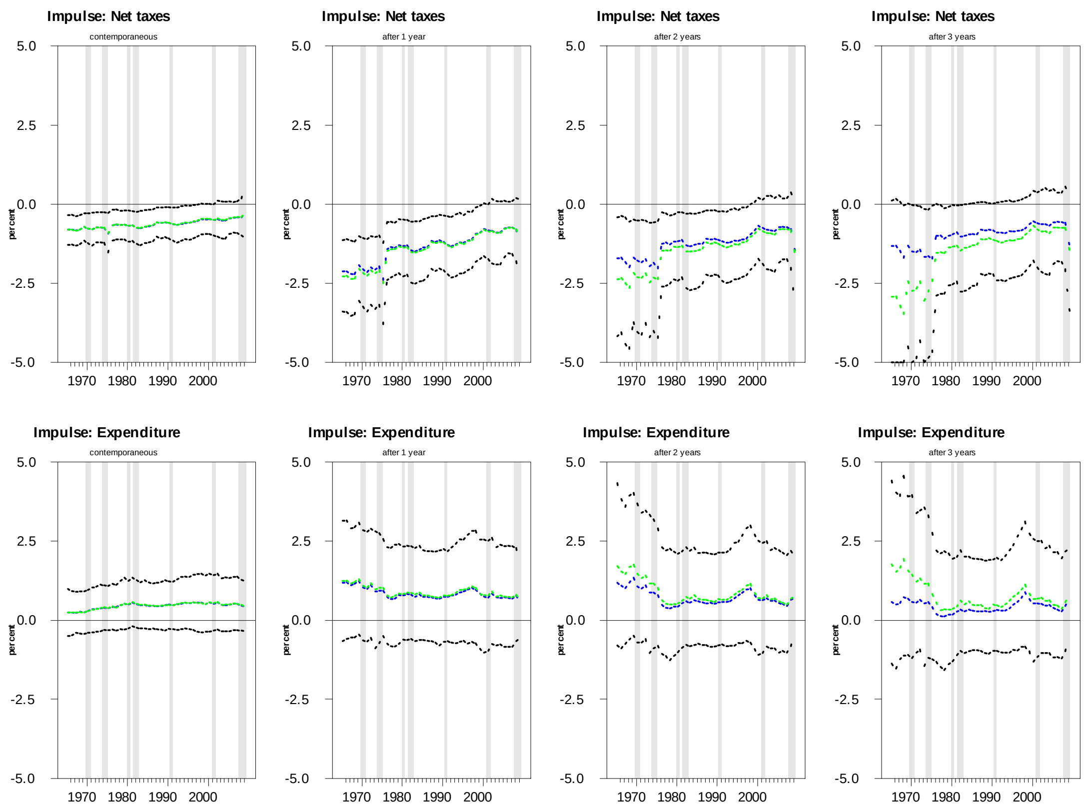

Figure 3.2: Time-profile of output responses, Bayesian simulation of a model with time-varying parameters

captured by the TVP specification is more limited. In the responses one year ahead and longer, a slight weakening of the impacts occurs initially (approximately until 1977), from 1.25 to 0.75-0.5. Subsequently, the response essentially stabilizes around this latter figure. The profile of contemporaneous impacts is the opposite in the initial years, featuring a slightly increase from 0.25 to 0.50. There is as well a stabilization thereafter.

Results in Figure 3.2 indicate a fading of the effects of fiscal policy over time, this being much more evident for net taxes than for expenditure. Such a pattern corroborates the belief that the effectiveness of fiscal policy in the US has lost strength in recent decades. In general, the responses evolve in a way that is well described by the gradual change hypothesis, although for net taxes there is evidence of a sizeable one-off break in the mid-seventies. It is worth noting that, in spite of the observed time variation, the multipliers keep conventional signs and reasonable sizes throughout.

One issue to note is that the confidence bands in Figure 3.2 are rather wide, and particularly so in the case of expenditure shocks for which they comprise the x-axis at all horizons considered. Even for net tax shocks, one cannot reject the hypothesis of constant effects throughout the period, since a horizontal line always fits within the area delimited by the two bands.

When the stability condition is imposed, the pattern of the responses over time (not shown) is qualitatively similar, but those 2 years after the shock and longer become noticeably more compressed. The median net tax multiplier 2 years ahead is in the range -1.4 to -0.5 with the stability condition, and -2.0 to -0.7 without it; similarly the expenditure multiplier is in the interval 0.25 to 0.9 instead of 0.4 to 1.3. When the average response instead of the median response is taken and/or responses for longer horizons are considered these discrepancies widen.

I present the NBER recessions in the charts with the impulse-responses, so as to provide informal evidence whether there has been a peak in policy effectiveness around such episodes. This hypothesis is sometimes mentioned in the literature (recently, for instance, in Hall (2009)). There is some support for it in my results, as far as net tax shocks are concerned. We saw that the maximum impact of these shocks occurs in 1975, when the slack in the economy was very large.[10] Moreover toward the end of longer recessions, such as the ones of 1969-70 and 1981-82, there is as well a hint of increase in effectiveness, and this happens even more strongly in the current contraction (the multiplier changes from -0.8 in 2008 to -1.1 in 2009). On the side of expenditure shocks, the responses remain more or less flat during recessionary episodes.

I now compare my findings with those presented in Kirchner et al. (2010) using the same type of model for the euro area. They identify shocks to spending only, ordering them before all the other variables (an identification assumption I also make in relation to GDP), and report responses from 1980 on. Concerning the amount of time variation captured, their results are equally compressed as mine, or even somewhat more.[11] Otherwise both the level and profile of their responses differ from the ones in this paper. They get a decrease in the size of the spending multiplier starting from late eighties, a period in which I get a stable response. Furthermore, their one-year-ahead multiplier is below mine: marginally positive (always lower than 0.5) until 2000 and slightly negative thereafter.

3.5.3 Comparison with rolling-sample estimates

The amount of time variation in Figure 3.2 falls short of that encountered in the first essay using a similar SVAR (and in the second essay, too, but there using a completely different shock measure). In order to pursue this issue, I consider now an SVAR with exactly the same variables and identification scheme as in the previous section, but in a time-invariant specification estimated over rolling samples of 25 years. The impact of fiscal shocks on GDP for this exercise is depicted in Figure 3.3, where the responses in t are those based on the sample ending at that date. Note that the first year for which such estimates can be calculated is 1973, and therefore the time-span covered differs from the one in Figure 3.2, which starts in 1965. Median responses and 16- and 84-percentile

[10] Note that the effects depicted in Figure 3.2 refer to the second quarter of each year, and the trough of the 1973-75 recession was in the first quarter.

[11] The reason may be that, although Kirchner et al. (2010) do not impose the stability condition, they use a smoothed variant of the simulation procedure (I use a filtered variant, instead).

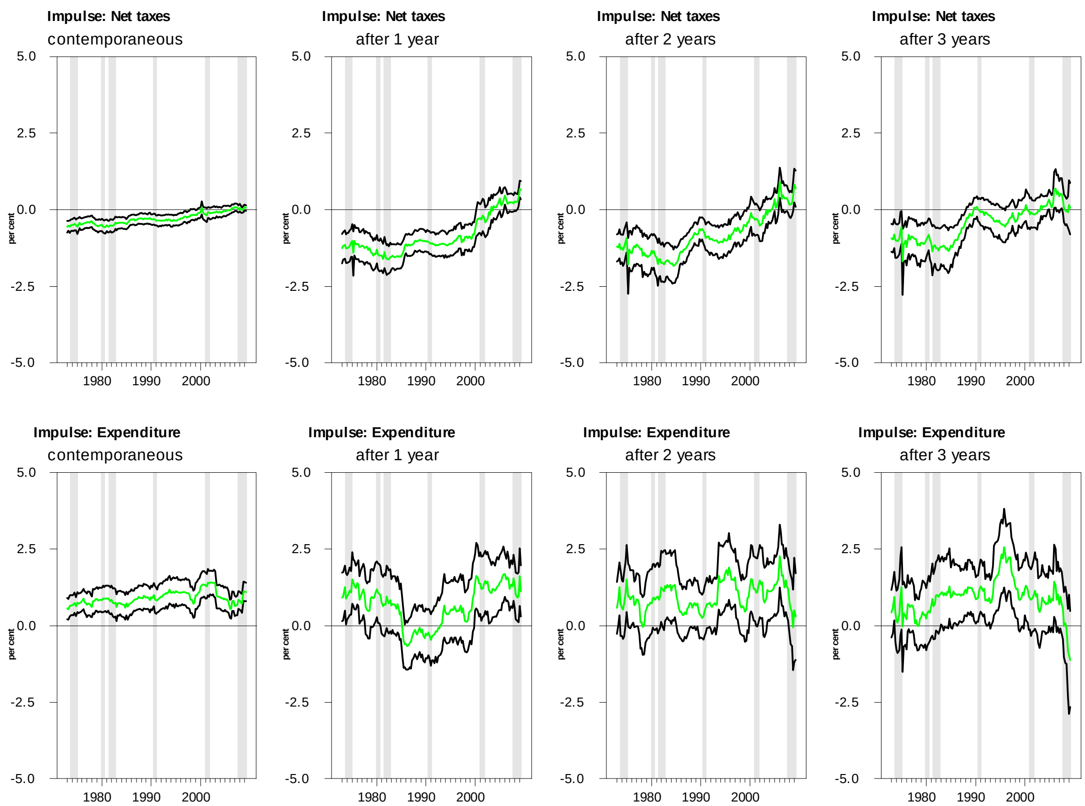

Figure 3.3: Time-profile of output responses, rolling-sample estimates of a model with fixed parameters

confidence bands are shown.[12] The profiles of net tax responses are broadly consistent in the two methodologies, in that the response fades progressively. However, rolling the model with time-invariant parameters yields a much sharper weakening toward the end of the simulation period, in such a way that perverse positive multipliers (up to about 0.5) arise from 2003 on. Turning to expenditure shocks, the results in Figure 3.3 are much more volatile than under the TVP specification. The multiplier one year ahead assumes values ranging from a maximum of around 1.5 to small negative (between the mid-eighties and the mid-nineties, although a zero multiplier is also encompassed by the confidence bands during this period).

Such findings are consistent with a large amount of time variation in the responses, generally matching the evidence presented in the first essay, and in studies that considered subsample sensitivity such as Perotti (2004).[13] The fact that the TVP specification shows comparatively much less instability in the effects of fiscal policy raises the issue whether the prior for the hyperparameters in the latter specification, in particular that for the covariance of the innovations relating to coefficient states, is compressing posterior time variation. I fed more prior volatility into the system by setting to $(0.1)^2$ the constant fraction of the parameters' asymptotic variances in calibrating the inverse-Wisharts for all the hyperparameters.[14] The results remained, however, very similar to those in Figure 3.2. These findings suggest that the fixed-parameter specification may be overestimating the actual drift in the coefficients, particularly for the responses to expenditure shocks. This specification lacks the flexibility of the TVP model to smoothly accommodate new observations, which bring about large changes in the estimated coefficients.

On balance, the TVP specification reconciles, to a certain extent, the SVAR evidence

[12] These are computed as follows. A reduced form VAR is estimated for each of the rolling-samples. On the basis of the point estimate for the covariance matrix, one draws firstly for this matrix, assuming a inverse-Wishart distribution. The structural decomposition is applied to each draw. At the same time, one draws for the vector of coefficients, assuming a Gaussian distribution, conditional on the covariance matrix previously drawn. The implied impulse-responses are obtained, on the basis of 1000 draws, and the relevant statistics computed.

[13] One could speculate that instability is partly caused by the shortness of the sample (25 years). Note, however, that the uncertainty surrounding the point estimates in Figure 3.3 is not unusually large for VAR standards.

[14] The benchmark value of this constant is $(0.1)^2$ for calibrating Q^{θ} and Q^{d} - see Section 3.3.1 and Appendix 3.A.

with output multipliers of spending with more conventional sizes and signs. Hall (2009) summarizes the empirical evidence on the spending multiplier as in the interval from 0.5 to 1.0. The magnitudes in Figure 3.2 broadly conform to this, being a bit above it in the initial years and slightly below toward the end of the period.

3.5.4 Time-varying responses of private consumption

A key disagreement between the predictions of the some new Keynesian models and Neoclassical models concerns the impact of government expenditure on private consumption. The former generally predict a positive effect on this variable of a rise in government purchases, while the latter posit a negative effect. I now investigate this question on the basis of the simulation of a identified TVP-VAR including private consumption, in addition to output, prices, net taxes and government expenditure. The responses of private consumption to fiscal shocks are presented in Figure 3.4. They can be again interpreted as multipliers since fiscal shocks are now normalized to have the size of 1 percent of that variable.

I find that positive shocks to net taxes consistently reduce private consumption. The effects are smaller (in absolute terms) than for output: the multipliers one year ahead and longer remain not far from -0.5 throughout the whole period. The results for expenditure shocks have the feature that the contemporaneous consumption multiplier is slightly negative, thus having the opposite sign of the output multiplier. For longer horizons, the indicator generally assumes small positive values (maximum of about 0.3) in the initial years, until mid-seventies, and then essentially decays to zero. Such evidence is clearly not compatible with a large Keynesian impact of expenditure shocks on consumption, particularly in the more recent decades. It could fit with in New Keynesian models that may yield slightly positive or zero consumption multipliers, depending on the extent of deviation from the neoclassical benchmark assumptions.[15] It is worth noting that the responses of consumption on the basis of the time-invariant rolling sample (not shown)

[15] The size of the multipliers in these models depends, for instance, on the intensity of the (negative) relationship between the markup ratio and output and the (positive) elasticity of labour supply (Hall (2009)), or the proportion of non-Ricardian consumers (Galí et al. (2007)).

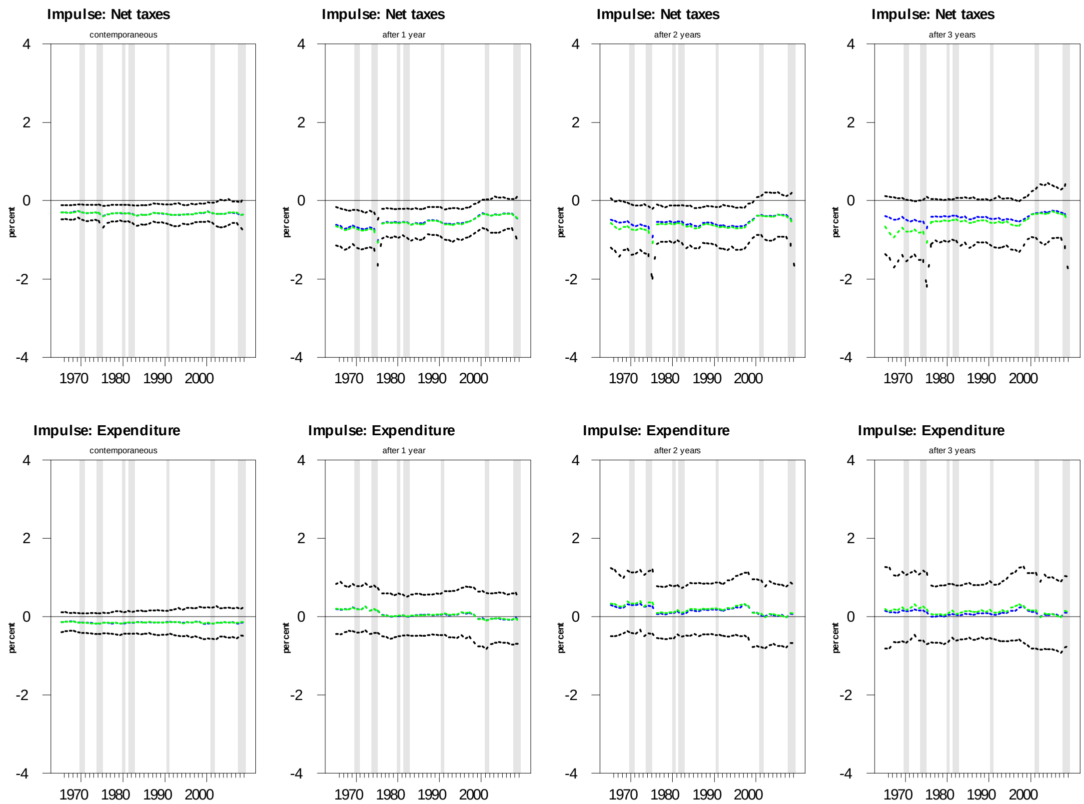

Figure 3.4: Time-profile of private consumption responses, Bayesian simulation of a model with time-varying parameters

parallel those for output in Figure 3.3. In the case of expenditure shocks, they fluctuate a lot, being generally positive, but assuming negative values between mid-eighties and mid-nineties.

3.5.5 Some evidence on time variation in the conduct of fiscal policy

I finalize this paper by using the framework developed to address questions such as time variation in exogenous fiscal policy and the responsiveness of endogenous policy to output. Relatively little attention has been devoted to them in contrast to monetary policy, where there has been much debate over, for instance, the existence of a drift in the coefficients of the reaction function versus in the variance of the exogenous disturbances (see Cogley and Sargent (2005) and references therein).

In an SVAR framework it is natural to distinguish between non-systematic and systematic policy. Given that my model incorporates stochastic volatility, I have direct evidence on the former coming from the time-varying figure for the standard errors of the structural fiscal shocks, which is a by-product of the simulation exercise. Things are more complicated for systematic policy. In the first place, SVARs do not allow to differentiate between the respective discretionary and automatic components. Therefore, if one is to analyze how fiscal policy activism has changed over time, the two components must be considered together. An additional issue is that such an analysis is carried out by looking at the response of fiscal variables to output shocks.[16] However, as explained in Section 3.2, the identification of output shocks vis-a-vis price shocks is based on an arbitrary ordering (incidentally, a limitation that also applies to similar analyses for monetary policy, as in Primiceri (2005)). Notwithstanding the issues mentioned, I believe this is a worthwhile exercise to pursue.

I consider systematic policy first. Figure 3.5 shows the one-year-ahead responses of fiscal variables to output shocks, with the size of 1 percent. Note that the contemporaneous

[16] The the size of output (and price) shocks in the identification scheme (3.4'), which I use in the simulations, does not coincide with that in (3.4) - see Appendix 3.B. The difference is however small (the standard deviation of the shocks is about 4 percent bigger in the first scheme in a fixed-parameter setting) and I ignore this issue.

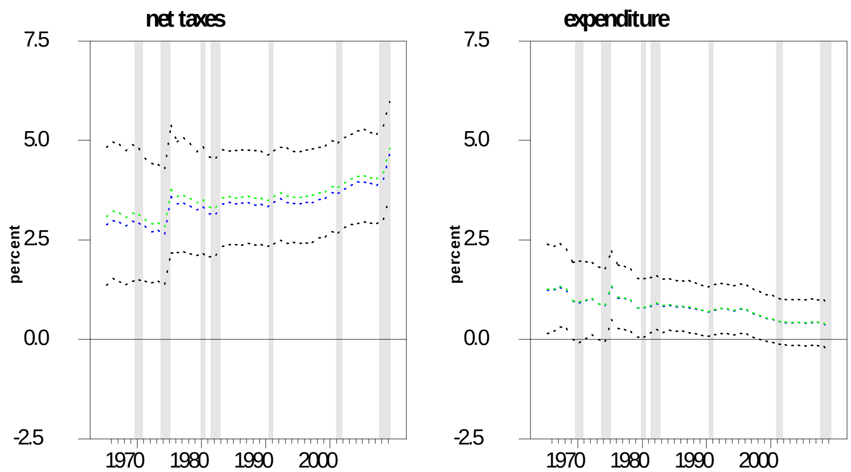

Figure 3.5: Time-profile of the one-year-ahead responses of fiscal variables to output shocks

responses in my system are determined by the identification assumptions, i.e. a zero response in the case of expenditure and the calibrated elasticity in the case of net taxes. These assumptions also influence the responses for longer horizons, but the latter are increasingly determined by the remaining dynamics of the system, as one projects into the future. It is worth noting that the calibrated elasticity of net taxes to output fluctuates without a defined trend for almost the whole period, in the interval from 2.0 to 2.5, but rise sharply to 3.5 in the two quarters of 2009.[17]

Net taxes respond positively to shocks to GDP, as one would expect, in line with the operation of the automatic stabilizers and the conduct of stabilization actions. A one percent shock to GDP triggers initially a rise close to 3 percent in net taxes, then there is a shift to responses around 3.5 percent from mid-seventies on, and further to around 4 percent toward the end of the simulation period. In the last time period considered, the second quarter of 2009, there is a jump in the response to a figure of 4.5. On the expenditure side, the responses are procyclical: they start with figures slightly over 1 percent and essentially show a decreasing trend throughout the period considered, to a

[17] The evolution in 2009 is explained as follows. In course of the recession there is a simultaneous fall in taxes and rise in social benefits, which cause a large decrease in net taxes. Therefore, the weight of taxes in total goes up and that of transfers, which is negative, becomes more negative - note that both weights add up to 1. Since the elasticity of taxes to output is positive and the elasticity of transfers is negative, this leads by itself to an increase in the overall elasticity.

value of about 0.4. In order to put these figures in context, I first calculate the implied semi-elasticity of deficit (as a percentage of output) to output gap, a common indicator of fiscal policy responsiveness.[18] Such semi-elasticity fluctuates in the range from 0.3 to 0.5 until the eighties and from 0.5 to 0.6 in the last two decades. The overall increase in responsiveness is consistent with the findings of others, such as in Taylor (2000) or Auerbach (2002), and in particular the figures broadly match the response of the surplus to output gap presented in the first of these studies (0.32 for the sample 1960-1982 and 0.68 for the sample 1983-1999).

Figure 3.5 shows in particular two jumps in the strength of net tax responses which coincide, respectively, with the 1973-75 and the 2008-09 recessions. The countercyclical action around these recessionary episodes is likely to contribute to the measured increase in responsiveness. Moreover, as seen, in the course of the 2008-09 recession there has been a large increase in the calibrated elasticity.

The behavior of expenditure is procyclical. The respective responses are generally significant; the lower confidence band becomes slightly below the x-axis from 1999 on, but by little. Auerbach (2002) in a regression of discretionary Federal expenditure on output gap finds evidence of countercyclicality (albeit statistically not significant). The difference comparing to my results may be due to the inclusion of the spending of state and local government which has been found to follow a procyclical pattern.

I now move on to non-systematic policy. Figure 3.6 presents the evolution of the volatility of structural fiscal shocks since mid-sixties. As far as net taxes are concerned, there was a rise in that volatility from early to mid-seventies (with a peak around 1975). Factors such as bracket creeping in the Personal Income Tax in a period of rising inflation[19], and large countercyclical one-off measures around the 1973-75 recession (notably the Nixon tax rebate), despite partly captured by the systematic part of the VAR, may «pass on» to the shocks to some extent. Volatility goes progressively down, to a minimum around

[18] This is obtained as the difference between the products of the response of each fiscal variable and the ratio of that variable to GDP. Note that the semi-elasticity actually refers to the primary deficit, since the definition of fiscal variables I adopt excludes interest outlays.

[19] The rates and brackets of the Personal Income Tax remained unchanged between the Tax Reform Act of 1969 and the Tax Reform Act of 1976 (Tax Foundation (2007)).

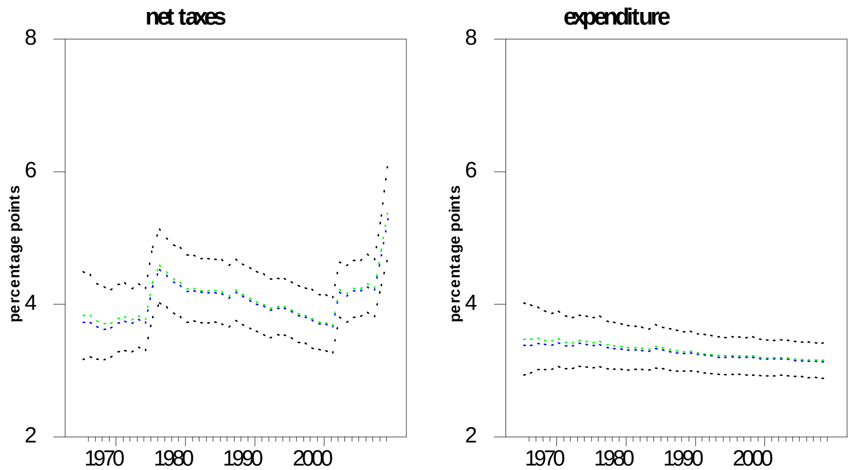

Figure 3.6: Time-profile of the standard deviation of structural fiscal shocks

2000, and subsequently there a large increase toward the end of the sample. The evolution in the last years should reflect firstly the tax cuts enacted by the Bush II administration and, more recently, the tax and benefit measures included in the stimulus packages of 2008-2009 although, similarly to above, these are also accommodated by the systematic reaction to the recession reinforced by the enhanced responsiveness. As a matter of fact, the fall in net taxes in the course of the 2008-09 recession, about 50 per cent, was the largest one during such episodes throughout the simulation period. The corresponding figure for the 1973-75 recession (including the Nixon tax rebate) was around 30 percent, and the one for the 1982-83 recession (contemporary with Reagan's tax cuts) around 20 percent. The standard deviation of spending shocks remained comparatively more stable, featuring a minor decrease throughout the period.

3.6 Conclusions

This paper presents the results of the simulation of a fiscal policy VAR with time-varying parameters, embedding a Blanchard and Perotti-like identification scheme into a Bayesian simulation procedure. The simulation period ranges from 1965 to 2009. I conclude that fiscal policy effectiveness has come down substantially over the period considered, particularly as far as net taxes are concerned. On the expenditure side, a fading of the effects of policy shocks is detected as well, but of a smaller magnitude. Private consumption responds negatively to net tax shocks and very little to expenditure shocks. The effects remained stable over time in this case. I also address time-variation in the conduct of fiscal policy, where I found that endogenous net taxes have increasingly reacted to output, while the respective exogenous component has fluctuated much and been particularly volatile in the recent years.

I do not do an exercise of relating the documented time-profile of the fiscal multipliers to possible underlying factors in the paper, with the exception of the stance of the business cycle. Many other hypotheses have been put forward in this context, as it is well known, such as the degree of openness of the economy or the easing of liquidity constraints. In order to investigate them in a rigorous manner, one would have to set up a non-linear system whose specification and simulation pose open questions that are left to further research.

Appendices

3.A Detailed simulation procedure

The simulation procedure uses the Gibbs sampler, iterating on four steps. Histories of states are sequentially generated and in the last step the model's hyperparameters, conditional on the results for the other steps. Throughout this appendix I follow the usual convention of denoting the history of a vector $\mathbf{w}_t$ up to time s, $\{\mathbf{w}_t\}_{t=1}^{s}$, by $\mathbf{w}^s$. The description of the procedure is for the baseline system with four variables, i.e. n equal to 4 and $\mathbf{x}_t$ to $[nt_t, g_t, p_t, y_t]'$.

3.A.1 Step 1 - drawing for the coefficient states

The measurement equation in this step is given by (3.1). The state-space model is thus

$$\mathbf{x}_t = \mathbf{X}_t \boldsymbol{\theta}_t + \mathbf{u}_t, \tag{3.A1}$$

$$\boldsymbol{\theta}_t = \boldsymbol{\theta}_{t-1} + \boldsymbol{\epsilon}_t^{\theta}, \tag{3.A2}$$

where $\mathbf{u}_t \sim i.i.d.N(0, \Sigma_t)$, $\boldsymbol{\epsilon}_t^{\theta} \sim i.i.d.N(0, Q^{\theta})$, and $\mathbf{u}_t$ and $\boldsymbol{\epsilon}_t^{\theta}$ are independent. The full history of coefficient states $\boldsymbol{\theta}^T$ is drawn conditional on the data, $\mathbf{x}^T$, a history of covariance and volatility states summarized in Σ^T, and the hyperparameters in Q^{θ}. The posteriori distributions are (see Kim and Nelson (1999), Ch.8):

$$\boldsymbol{\theta}_T \mid \mathbf{y}^T, \Sigma^T, Q^{\theta} \sim N(\boldsymbol{\theta}_{T|T}, P_{T|T}^{\theta}) \tag{3.A3}$$

$$\boldsymbol{\theta}_t \mid \mathbf{y}^T, \boldsymbol{\theta}_{t+1}, \Sigma^T, Q^{\theta} \sim N(\boldsymbol{\theta}_{t|t,\boldsymbol{\theta}_{t+1}}, P_{t|t,\boldsymbol{\theta}_{t+1}}^{\boldsymbol{\theta}}), t = 1, ..., T-1, \tag{3.A4}$$

where the conditional mean and variance in expression (3.A3), $\boldsymbol{\theta}_{T|T}$ and $P_{T|T}^{\boldsymbol{\theta}}$, can be obtained as the last iteration of the usual Kalman filter, going forward from

$\boldsymbol{\theta}_{t|t} = \boldsymbol{\theta}_{t|t-1} + P_{t|t-1}^{\theta} X_t (X_t' P_{t|t-1}^{\theta} X_t + \Sigma_t)^{-1} (\mathbf{y}_t - X_t' \boldsymbol{\theta}_{t|t-1}),$

$P_{t|t}^{\theta} = P_{t|t-1}^{\theta} - P_{t|t-1}^{\theta} X_t (X_t' P_{t|t-1}^{\theta} X_t + \Sigma_t)^{-1} X_t' P_{t|t-1}^{\theta},$

$\boldsymbol{\theta}_{t|t-1} = \boldsymbol{\theta}_{t-1|t-1},$

$P_{t|t-1}^{\boldsymbol{\theta}} = P_{t-1|t-1}^{\boldsymbol{\theta}} + Q^{\theta},$

starting from the initial values $\boldsymbol{\theta}_{0|0}$ and $P_{0|0}^{\theta}$. These initial values are given by the mean and covariance matrix of the prior, $\boldsymbol{\theta}_0 \sim N(\hat{\boldsymbol{\theta}}, 4V(\hat{\boldsymbol{\theta}}))$, obtained as coefficient vector and covariance matrix from the estimation by OLS of the reduced-form system (3.1) for a training subsample 1947:1-1959:4. The elements in $\boldsymbol{\theta}^{T-1}$ are drawn from (3.A4) going backward. That is, $\boldsymbol{\theta}_{T-1}$ is drawn conditional on the realization of $\boldsymbol{\theta}_T$, $\boldsymbol{\theta}_{T-2}$ conditional on the realization of $\boldsymbol{\theta}_{T-1}$ and so on up to $\boldsymbol{\theta}_1$. The conditional mean and variance in (3.A4) are given by

$\boldsymbol{\theta}_{t|t,\boldsymbol{\phi}_{t+1}} = \boldsymbol{\theta}_{t|t} + P_{t|t}^{\theta} (P_{t|t}^{\theta} + Q^{\theta})^{-1} (\boldsymbol{\theta}_{t+1} - \boldsymbol{\theta}_{t|t}),$

$P_{t|t,\boldsymbol{\theta}_{t+1}}^{\theta} = P_{t|t}^{\theta} - P_{t|t}^{\theta} (P_{t|t}^{\theta} + Q^{\theta})^{-1} P_{t|t}^{\theta}.$

3.A.2 Step 2 - drawing for the covariance states

The system of measurement equations is now based on (3.2), i.e. $A_t\mathbf{u}_t = (B_t - I_n)\mathbf{e}_t + \mathbf{e}_t$, with matrices A_t and B_t as given in (3.4'). As explained in the text, it is assumed that there is independence between the states in B_t belonging to different equations, that is, the covariance matrix of the state innovations is block-diagonal, with the block for equation i given by Q^{bi} $(i = 1, 3, 4)$. The simulations in this step are conditional on $\mathbf{x}^T$ and $\boldsymbol{\theta}^T$, which makes $\mathbf{u}^T$ observable, a history of volatility states, D^T, and the the hyperparameters in Q^{bi}. Note also that the elements of A_t are known. Since there is independence among states in different equations and, at the same time, the covariance matrix of the error term in the measurement equation (D_tD_t') is diagonal, the state-space problem can be tackled equation by equation. Moreover, the structure of matrix B_t is such that the elements of $\mathbf{e}_t$ entering each equation as regressors are predetermined, so the assumptions of the linear state-space model are met. The simulations proceed in the following sequence. Firstly, given $\mathbf{u}^T$ and A^T, e_g^T is observable. The first state-space problem is

$$u_{p,t} = e_{g,t}\mathbf{b}_{3,t} + e_{p,t}, \tag{3.A5}$$

$$\mathbf{b}_{3,t} = \mathbf{b}_{3,t-1} + \boldsymbol{\epsilon}_t^{b3}, \tag{3.A6}$$

where $\mathbf{b}_{3,t} = [\beta_{32,t}]$, $e_{p,t} \sim i.i.d.N(0, d_{33}^2)$, d_{33} being the third element in the main diagonal of $\mathbf{D}_t$, $\epsilon_t^{b3} \sim i.i.d.N(0, Q^{b3})$, and $e_{p,t}$ and $\boldsymbol{\epsilon}_{i,t}^{b3}$ are independent. This simulation yields a history b_3^T and, conditional on it, a history e_p^T.

The next state-space model is

$$u_{nt,t} - a_{14}^* u_{y,t} = [e_{g,t}e_{p,t}]\mathbf{b}_{1,t} + e_{nt,t}, \tag{3.A7}$$

$$\mathbf{b}_{1,t} = \mathbf{b}_{1,t-1} + \boldsymbol{\epsilon}_t^{b1}, \tag{3.A8}$$

where $\mathbf{b}_{1,t} = [\beta_{12,t}\beta_{13,t}]$, $e_{p,t} \sim i.i.d.N(0, d_{11}^2)$, d_{11} being the first element in the main diagonal of $\mathbf{D}_t$, $\boldsymbol{\epsilon}_t^{b1} \sim i.i.d.N(0, Q^{b1})$, and $e_{nt,t}$ and $\boldsymbol{\epsilon}_t^{b1}$ are independent. This simulation yields a history $\mathbf{b}_1^T$ and, conditional on it, a history e_{nt}^T.

The third state-space problem is

$$u_{y,t} = [e_{nt,t}e_{g,t}e_{p,t}]\mathbf{b}_{4,t} + e_{y,t}, \tag{3.A9}$$

$$\mathbf{b}_{4,t} = \mathbf{b}_{4,t-1} + \boldsymbol{\epsilon}_t^{b4}, \tag{3.A10}$$

where $\mathbf{b}_{4,t} = [\beta_{41,t}\beta_{42,t}\beta_{43,t}]$, $e_{y,t} \sim i.i.d.N(0, d_{44}^2)$, d_{44} being the fourth element in the main diagonal of D_t, $\boldsymbol{\epsilon}_t^{b4} \sim i.i.d.N(0, Q^{b4})$ and $e_{y,t}$ and $\boldsymbol{\epsilon}_t^{b4}$ are independent. This simulation yields $\mathbf{b}_4^T$ and, conditional on it, a history e_y^T.

The simulations for each of the three state-space models are conducted precisely in the same way as described for Step 1, on the basis of the distributions corresponding to (3.A3) and (3.A4) above. The initial values for the Kalman filter, $\mathbf{b}_{i,0|0}$ and $P_{0|0}^{b_i}$, are from the mean and covariance matrix of the priors: $\mathbf{b}_{i,0} \sim N(\hat{\mathbf{b}}_i, 4V(\hat{\mathbf{b}}_i))$. These parameters are obtained from estimating by OLS the structural decomposition (3.4') for the training subsample 1947:1-1959:4.

3.A.3 Step 3 - drawing for the volatility states

The system of measurement equations is now based on (3.3), i.e. $\mathbf{e}_t = \mathbf{D}_t\boldsymbol{\varepsilon}_t$. Squaring and taking logarithms on both sides of each measurement equation, the state-space model becomes:

$$\mathbf{e}_t^+ = 2\log\mathbf{d}_t + \log\boldsymbol{\varepsilon}_t^2, \tag{3.A11}$$

$$\log\mathbf{d}_t = \log\mathbf{d}_{t-1} + \boldsymbol{\epsilon}_t^d, \tag{3.A12}$$

where $\mathbf{e}_t^+ = \log(\mathbf{e}_t^2 + 0.001)$ denotes the logarithm of the square of each element of $\mathbf{e}_t$ plus a offsetting constant equal to 0.001, $\log \mathbf{d}_t$ denotes the elementwise logarithm of the vector $\mathbf{d}_t$ and $\log(\boldsymbol{\varepsilon}_t^2)$ the elementwise logarithm of the vector $\boldsymbol{\varepsilon}_t$. Furthermore, $\epsilon_t^d \sim i.i.d.N(0, Q^d)$ and, since $\boldsymbol{\varepsilon}_t$ and $\boldsymbol{\epsilon}_t^d$ are independent, the same applies to $\log \boldsymbol{\varepsilon}_t^2$ and $\boldsymbol{\epsilon}_t^d$.[20]

The algorithms for the Gaussian linear state space model cannot be directly applied in this case, because the disturbances $\log \boldsymbol{\varepsilon}_{i,t}^2, i = 1, ...4$, are not Gaussian. The distribution of these disturbances can, however, be approximated using a mixture of seven Gaussian densities (see Kim et al. (1998) for the details):

$$f(\log \boldsymbol{\varepsilon}_{i,t}^2) \approx \textstyle\sum_{j=1}^{7} q_j f_N(\log \boldsymbol{\varepsilon}_{i,t}^2; m_j - 1.2704, v_j^2), \tag{3.A13}$$

where q_j, m_j and v_j^2 are known constants which depend on j. Then, conditioning on the realization of an indicator random variable $s_{i,t}, i = 1, ...4$, taking on values in $\{1, 2, 3, 4, 5, 6, 7\}$, one element of the family of normals is selected:

$$\log \boldsymbol{\varepsilon}_{i,t}^2 \mid s_{i,t} = j \sim N(m_j - 1.2704, v_j^2). \tag{3.A14}$$

Therefore, a history $\log \mathbf{d}^T$ can be drawn conditional on $\mathbf{s}^T$, in addition to $\mathbf{x}^T$, $\boldsymbol{\theta}^T$, $\mathbf{B}^T$(making $\mathbf{e}^T$ or $\mathbf{e}_t^{+T}$ observable) and the hyperparameters in Q^d. It is straightforward to adapt the formulae in Step 1 to this end. The initial values for the Kalman filter are, as previously, from the mean and covariance matrix of the prior which is given by $\log \mathbf{d}_0 \sim N(\log \hat{\mathbf{d}}, I_n)$. The figures in $\log \hat{\mathbf{d}}$ are the log standard deviations of the structural shocks from the abovementioned estimation of the system in the training subsample.

Step 3A: drawing for s_t

A history $\mathbf{s}^T$ is sampled independently for $i = 1, ..., 4$ and $t = 1, ...T$, given $\mathbf{e}_t^{+T}$ and $\log \mathbf{d}^T$, using the following result

[20] This description of the simulation procedure assumes that the covariance matrix of the state innovations, Q^d, is unrestricted and thus the volatility states are drawn jointly. One could alternatively assume a diagonal Q^d matrix (independent state innovations), in which case the simulations would carried out equation by equation. We experimented with both possibilities and the results were similar.

$$Pr(s_{i,t} = j \mid \mathbf{e}^{+}_{i,t}, \log d_{i,t}) \propto q_j f_N(\mathbf{e}^{+}_{i,t}; 2\log d_{i,t} + m_j - 1.2704, v_j^2), \tag{3.A15}$$

with j defined in $\{1, 2, 3, 4, 5, 6, 7\}$ and q_j, m_j and v_j^2 known constants.

3.A.4 Step 4: Drawing for the hyperparameters

The prior and posterior distributions of the hyperparameters are conjugate inverse-Wishart. The hyperparameters are drawn conditioning on the data and histories of coefficient, covariance and volatility states, which makes the innovations in all state equations (i.e. $\boldsymbol{\epsilon}^{\theta T}$, $\boldsymbol{\epsilon}^{b1T}$, $\boldsymbol{\epsilon}^{b3T}$, $\boldsymbol{\epsilon}^{b4T}$ and $\boldsymbol{\epsilon}^{dT}$) observable.

The prior distribution of Q^{θ} is $IW(\bar{Q}^{\theta}, T_0)$, with $\bar{Q}^{\theta} = k_{\theta}^2 T_0 V(\hat{\boldsymbol{\theta}})$, where $V(\hat{\boldsymbol{\theta}})$ is the covariance matrix of the reduced-form coefficients (used to calibrate the prior for $\boldsymbol{\theta}_0$ above), T_0 is the number of observations in the training sample[21] and k_{θ}^2 is a chosen parameter. I set k_{θ} to 0.01. The posteriori distribution of Q^{θ} is $IW((\bar{Q}^{\theta} + \sum_{t=1}^{T} \boldsymbol{\epsilon}_t^{\theta} \boldsymbol{\epsilon}_t^{\theta\prime})^{-1}, T_0 + T)$.

The prior distribution for Q^{b3} is $IW(\bar{Q}^{b3}, 2)$, with $\bar{Q}^{b3} = 2k_b^2 V(\hat{\mathbf{b}}_3)$, where $V(\hat{\mathbf{b}}_3)$ is the covariance matrix of the coefficients of the structural decomposition (used to calibrate the prior for $\mathbf{b}_{3,0}$ above) and k_b^2 is a chosen parameter. This parameter is set to 0.1. The posterior for Q^{b3} is given by $IW((\bar{Q}^{b3} + \sum_{t=1}^{T} \boldsymbol{\epsilon}^{b3} \boldsymbol{\epsilon}^{b3\prime})^{-1}, 2 + T)$.

The prior distribution for Q^{b1} is $IW(\bar{Q}^{b1}, 3)$, with $\bar{Q}^{b1} = 3k_b^2 V(\hat{\mathbf{b}}_1)$, where $V(\hat{\mathbf{b}}_1)$ is the covariance matrix of the coefficients of the structural decomposition (used to calibrate the prior for $\mathbf{b}_{1,0}$ above) and k_b^2 equal to 0.1. The posterior for Q^{b1} is given by $IW((\bar{Q}^{b1} + \sum_{t=1}^{T} \boldsymbol{\epsilon}^{b1} \boldsymbol{\epsilon}^{b1\prime})^{-1}, 3 + T)$.

The prior distribution for Q^{b4} is $IW(\bar{Q}^{b4}, 4)$, with $\bar{Q}^{b4} = 4k_b^2 V(\hat{\mathbf{b}}_4)$, where $V(\hat{\mathbf{b}}_4)$ is the covariance matrix of the coefficients of the structural decomposition (used to calibrate the prior for $\mathbf{b}_{4,0}$ above) and k_b^2 equal to to 0.1. The posterior for Q^{b4} is given by $IW((\bar{Q}^{b4} + \sum_{t=1}^{T} \boldsymbol{\epsilon}^{b4} \boldsymbol{\epsilon}^{b4\prime})^{-1}, 4 + T)$.

The prior distribution for Q^{d} is $IW(\bar{Q}^{d}, 5)$, with $\bar{Q}^{d} = 5k_d^2 I_4$, where k_d^2 is a chosen para-

[21] In the 5-variable system including private consumption, T_0 is set to 56. This is equal to the size of the vector $\boldsymbol{\theta}_t$ plus 1, the minimum number of degrees of freedom for the prior to be proper (and exceeds the number of observations in the training sample).

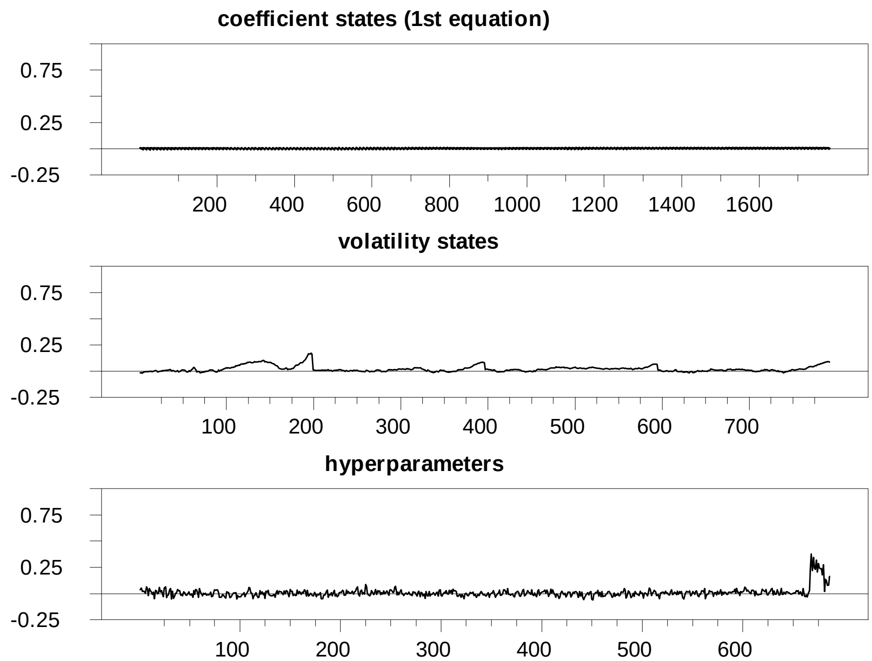

Figure 3.7: Autocorrelation of the draws for selected sets of parameters

meter. This is set to 0.01. The posterior for Q^d is given by $IW((\bar{Q}^d + \sum_{t=1}^{T} \boldsymbol{\epsilon}^d \boldsymbol{\epsilon}^{d\prime})^{-1}, 5+T)$.

3.A.5 Convergence diagnostics for the simulation procedure

I conclude this appendix by reporting a set of results concerning autocorrelations of the draws. The convergence of the Gibbs sampler is known to be faster when the draws are approximately independent. I report the 20th sample autocorrelation of the kept draws, following Primiceri (2005), for last iteration of the Gibbs sampler which corresponds to the simulation period 1960:1-2009:2. The number of parameters is very large and I thus present that statistic for a selection of them comprising the coefficient states in the first equation ($1782 = 9 \times 198$), the volatility states ($792 = 4 \times 98$) and the hyperparameters (686). Figure 3.7 shows that the correlations are close to zero in most cases and, when they are higher, remain nevertheless below 0.2. The only exception is for the hyperparameters in Q^{bi}, featuring autocorrelations in the range from 0.2 to 0.3 (end of the third panel).

3.B Mapping between the identification schemes (3.4) and (3.4') in Section 3.2

The system of equations implied by scheme (3.4) in Section 3.2 is

$$u_t^{nt} = a_{13}u_t^p + a_{14}^* u_t^y + b_{12}e_t^g + e_t^{nt}, \tag{3.B1}$$

$$u_t^g = a_{23}^* u_t^p + e_t^g, \tag{3.B2}$$

$$u_t^p = a_{32}u_t^g + e_t^p, \tag{3.B3}$$

$$u_t^y = a_{41}u_t^{nt} + a_{42}u_t^g + a_{43}u_t^p + e_t^y. \tag{3.B4}$$

Note that equation (3.B2) has no unknown parameters. In order to reparameterize equation (3.B3), one has to replace u_t^g as given by (3.B2) in it, yielding

$$u_t^p = \beta_{32}e_t^g + e_t^{+p}, \tag{3.B3'}$$

where $\beta_{32} = a_{32}/(1 - a_{32}a_{23}^*)$ and $e_t^{+p} = e_t^p/(1 - a_{32}a_{23}^*)$.

Consider now equation (3.B1): replacing u_t^p as given by (3.B3') in it and simplifying yields

$$u_t^{nt} - a_{14}^* u_t^y = \beta_{12}e_t^g + \beta_{13}e_t^{+p} + e_t^{nt}, \tag{3.B1'}$$

where $\beta_{12} = b_{12} + \beta_{32}a_{13}$ and $\beta_{13} = a_{13}$.

Finally, equation (3.B4) can be rewritten as

$$u_t^y = \beta_{41}e_t^{nt} + \beta_{42}e_t^g + \beta_{43}e_t^{+p} + e_t^{+y} \tag{3.B4'}$$

where β_{41} $=(1-a_{41}a^*_{14})^{-1}a_{41}$, $\beta_{42}=(1-a_{41}a^*_{14})^{-1}[(a_{41}a_{13}+a_{42}a^*_{23}+a_{43})\beta_{32}+a_{41}b_{12}+a_{42}]$, $\beta_{43}=(1-a_{41}a^*_{14})^{-1}(a_{41}a_{13}+a_{42}a^*_{23}+a_{43})$ and $e^{+y}_t=e^y_t/(1-a_{41}a^*_{14})$.

It is easy to check that the set of equations implied by scheme (3.4') in Section 3.2 consists of (3.B1'), (3.B2), (3.B3') and (3.B4').

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