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Abstract

This paper develops a new measure of US fiscal policy shocks that intends to avoid the anticipation problem affecting conventional measures, being also arguably free from endogeneity. The shocks are intended to capture changes to the component of anticipated fiscal policy that is exogenous to economic developments. Key economic variables such as output and interest rates respond quickly and significantly to a realization of the estimated shock and, in the first part of the sample, 1969-1988, in a way consistent with the Keynesian prior. In contrast, over the period 1989-2008 the effects are at odds with that prior, with fiscal loosening producing contractionary impacts.

Keywords: fiscal policy, budget forecasts, macroeconomic stabilization, interest rate determination

JEL codes: E62, E43, E32

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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. Well known work by Blanchard and Perotti (2002) on the identification of fiscal policy shocks tackled endogeneity through the application of the structural VAR methodology. This approach requires that identification assumptions are made and 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 in this context has to do with anticipation (see, for instance, Leeper et al. (2008)). 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 on which policymakers base 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 current 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.

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

[1] Other contributions along these lines are, for instance, Thorbecke (1993), Quigley and Porter-Hudak (1994), Kitchen (1996) and, more recently, Laubach (2003).

the years from 1969 to 2008, and I split it into two subsamples: 1969-1988 and 1989-2008. The evidence I get in the first subsample is very much consistent with the standard Keynesian predictions. Positive deficit shocks rise simultaneously interest rates and output. Changes to anticipated exogenous taxes (net of transfers) and spending, considered separately, have also effects in line with such predictions. 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. Such results resemble the so-called «expansionary fiscal contraction» hypothesis, which emphasizes the role of agents' expectations on the impact of fiscal policy. If agents regard, say, a tax cut as unsustainable, this may revert the impact it would otherwise cause. The possibility that fiscal policy in the US had non-conventional effects in the nineties and more recently has appeared often in the popular debate, and occasionally also in the academic literature. Nevertheless, this paper probably presents the strongest evidence of a major structural break so far. In addition, results imply that perverse effects on output occur under less extreme conditions than usually associated with expansionary fiscal contractions - given the budgetary situation in the US over the 1989-2008 period.

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 the expansionary impact of fiscal policy tightening comes about through a fall in interest rates.

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, suggesting some role of the expectations channel. In view of this last point, 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 discusses features of the budget forecasting process that are relevant to reach an appropriate shock measure. Section 3 describes the derivation of the shocks and covers aspects related to the data. Section 4 provides a descriptive analysis of the estimated shocks series. The rest of the paper is devoted to the presentation of empirical results. Sections 5 and 6 analyze the reactions of output, short-term and long-term interest rates in the wake of fiscal shocks. Section 7 takes up a more detailed investigation of impacts of fiscal policy on long-term bond yields. Section 8 makes some concluding remarks.

2 The 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 1 presents the chronology of OMB announcements for which information was gathered. They start with the FY 1969 Budget (January 1968) and end with the FY 2009 Budget (February 2008). The FY 1969 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]

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

[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/.

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

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 as 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 less confidence in them, as this agency is comparatively more susceptible to political influences. Note that, even if this is the case, the precise objectives it pursues are 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 at the end of Section 3.1). 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] I conclude that the information content of OMB projections is 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 forecast-

[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.

[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.

ers 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.

Table 1: Announcements of budget projections by OMB/Bureau of the Budget 1968-2008

Date	Event	cFY	Date	Event	cFY	Date	Event	cFY
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*	06Jul,11	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) cFY refers to the current fiscal year at the time of the announcement. (d) The announcements marked with * do not have projections for a budget year.

Up to the end of the seventies, budget announcements used to include forecasts for the current fiscal year, and also for the upcoming one after the submission of the President's Budget, i.e. the budget year. A few announcements taking place between the start of a fiscal year (July, 1 prior to FY 77, and October, 1 afterwards) and the submission of a new budget - marked with an * in Table 1 - had current-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 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 current 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)[6].

My series of shocks is based on the revision to the exogenous component of the forecast for a given year, from one announcement to the other. I consider two such revisions, for the current and the budget fiscal years. Revisions for subsequent years are not taken into account. A prosaic reason for doing so is data availability: they could be computed for a limited subset of announcements in Table 1.

But there are conceptual reasons as well. The change in the exogenous component of the fiscal forecast, controlling for base-year effects and macroeconomic assumptions, is less meaningful for years not yet covered by a budget. In the absence of the latter, such effects and 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 (and capture the impact of) changes in the exogenous part of forecasts for

[6] 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.

subsequent years. 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.

3 A new measure of shocks

3.1 Derivation

My objective is to construct a measure of shocks on the basis of fiscal forecasts that is, as far as possible, free of endogeneity and anticipatory effects. Forecasts of a given fiscal variable for FY t can be modelled as being determined by the respective base-year value, for FY $t-1$, and the estimated impact of developments affecting the outcome in t, namely, changes in policy, macroeconomic scenario and a multitude of other determining factors. On the revenue side, these are factors affecting the tax base such as consumer preferences, distribution of income or the amount of capital gains. Some of them bear a relationship to the business cycle, but not a strong enough one for their impact to be predictable on the basis of the core macroeconomic assumptions. On the outlay side, those factors include demographic trends, composition of health care demand and the behavior of administrators and beneficiaries of spending programs.

The elements of the projected path of each fiscal variable that are endogenous to economic developments cannot be used to assess policy impacts and must be taken out. A first such element relates to changes in macroeconomic assumptions, which show up in the figures as forecasters seek to incorporate the effect of automatic stabilizers. 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. The second component relates to changes in discretionary systematic policy responding to useful information that policymakers (similarly to forecasters) may have about current and future macroeconomic developments.[7] The last element is the forecast component explained by previous year's figure (which may be the outurn or itself a forecast, depending on the number of steps ahead), on endogeneity grounds and to disentangle each year's specific shock.

More precisely, I regress the fiscal forecast announced at q for fiscal year s, as a percent of GNP/GDP[8], denoted generically by $f_{q,s}$, on a constant, its value for the previous fiscal year ($f_{q,s-1}$) and key macroeconomic variables for the current and previous fiscal years. The macroeconomic dataset encompasses real GNP/GDP growth ($y_{q,s}$ and $y_{q,s-1}$), inflation measured by the GNP/GDP deflator ($p_{q,s}$ and $p_{q,s-1}$), and the 3-month Treasury bill rate ($r_{q,s}$ and $r_{q,s-1}$). I estimate

$$f_{q,s}= \alpha_0+\alpha_1 f_{q,s-1} + \alpha_2 y_{q,s} + \alpha_3 y_{q,s-1} + \alpha_4 p_{q,s} + \alpha_5 p_{q,s-1}+\alpha_6 r_{q,s} + \alpha_7 r_{q,s-1} + v_{q,s}. \qquad (1)$$

The exogenous component is to the residual of this regression.[9] This will essentially reflect the quantification by forecasters of changes relative to the previous fiscal year brought about by policy measures unrelated to macroeconomic developments, and the other determining factors. The shock referring to announcement q and fiscal year s is computed as $\hat{v}_{q,s} - \hat{v}_{q-1,s}$, the revision to the exogenous component of the projection for year s between announcements q and $q-1$. This revision may be due to, say, new policy measures announced in the interim period or pure forecast errors. Such errors are part of the shock since it is based on changes in anticipated

[7] 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.

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

[9] Each of the 114 announcements yields up to three observations for the regression. Two of these are the current- and budget-year forecasts which exist, respectively for 114 and 103 announcements. The remaining observations (10) come from the post-budget year projections in the announcements preceding the release of a new budget. All these data points are stacked into one regression with 227 observations.

figures rather than in the actual outturn. Market participants presumably make similar errors which will influence their decisions.

I am able to calculate figures for the shocks 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 current-year shocks, 113 observations in total, and for 72 announcements in the case of budget-year shocks. I can however always compute the latter shocks after taking office of a new administration. These are likely to be particularly important as they signal major policy changes.

I ran three regressions with the dependent variable in (1) given, respectively, by net taxes, spending and deficit, and computed two series of shocks from each of them. Since the predictions of competing macro theories differ particularly as to the effects of changes in taxes, it is important to consider the two sides of the budget separately. It also informative to see how the impacts of deficit shocks reflect the relative contributions stemming from each component.

The method used has similarities to calculating directly the revision to the forecast and then regressing it on changes in the base year and macroeconomic assumptions. Econometrically this corresponds to running a regression specified in differences between consecutive announcements for the same fiscal year. The dependent variable would be specified as $\Delta f_{q,s} = f_{q,s} - f_{q-1,s}$ and likewise for the dependent and residual variables. Such a regression in differences has less observations than (1) and, in principle, worse properties as far as the precision of estimates is concerned. Moreover, the methodological changes introduced over the 40 years covered by the sample (for example, the recording of interest payments to trust funds by the Treasury starting with the *FY 84 Budget*) affect revisions from one announcement to the other, but not the outcome of a regression in which each data point comes from the same announcement, since revisions have typically been retropolated.

The econometric soundness of regression (1) relies on the exogeneity of macroeconomic assumptions to the production of budget forecasts. I believe this is a reasonable hypothesis since, on the one hand, budget forecasting is a second stage in a process which starts by the elaboration of the assumptions. 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 feedback effects on economic activity of the policy proposals incorporated. The inclusion of the «lagged» forecast of the dependent variable as a regressor in (1) rests on the equally plausible hypothesis that the forecasting process is sequential, that is forecasts for FY t are determined after forecasts for FY $t-1$.

A final aspect arises when the dependent variable in (1) is net taxes or spending (i.e. not deficit). It may happen that the projections of these two variables 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. This sort of factors is likely to create a positive correlation between forecast revisions to the two sides of the budget.

A mutual 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 inclusion of one of these in the equation for the other would not be appropriate. Instead of relying, say, on an arbitrary ordering, I prefer to estimate reduced-form equations from which both variables were 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. Such correlations are further examined in Section 4, and have to be taken into account in the empirical analysis.

3.2 Variable definition and data availability

The fiscal data used in the regressions are OMB's forecasts and real-time outturn data contemporary with the announcement. I consider both on- and off-budget items, i.e. the total budget, which 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. I collected data for overall receipts and outlays by function. 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).

The nominal budget forecasts were standardized by nominal GNP/GDP, which appears to be a suitable benchmark to proxy the perception by markets of the size of fiscal shocks. The nominal GNP/GDP projection was 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.

As to availability, I have real-time outturn figures and fiscal projections for the current- and budget-years (when applicable) for all announcements. In contrast, the post-budget year projections in the last announcement before the release of a new budget (needed to compute the first budget-year shock for a given year) are available only in the last years of the sample. Availability constraints also affect the macroeconomic assumptions underlying 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 made in the *Economic Report of the President*. The scope and presentation improved much starting with the FY 1976 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 in regression (1). See the Appendix for more details on availability issues.

4 Analysis of the shock series

Net tax and expenditure shocks for the current and budget fiscal years are depicted in Figure 1. Table 2 presents some descriptive statistics, namely, the mean, the mean of absolute values and the standard deviation. Inspection of Figure 1 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 the savings and loan crisis (see contemporary budget analyses as, for instance, in Congressional Budget Office (1992)) proved very difficult to predict and gave rise to a sequence of abnormally large revisions to expenditure projections. The average absolute shock is 0.30-0.35 percent of GDP for net taxes

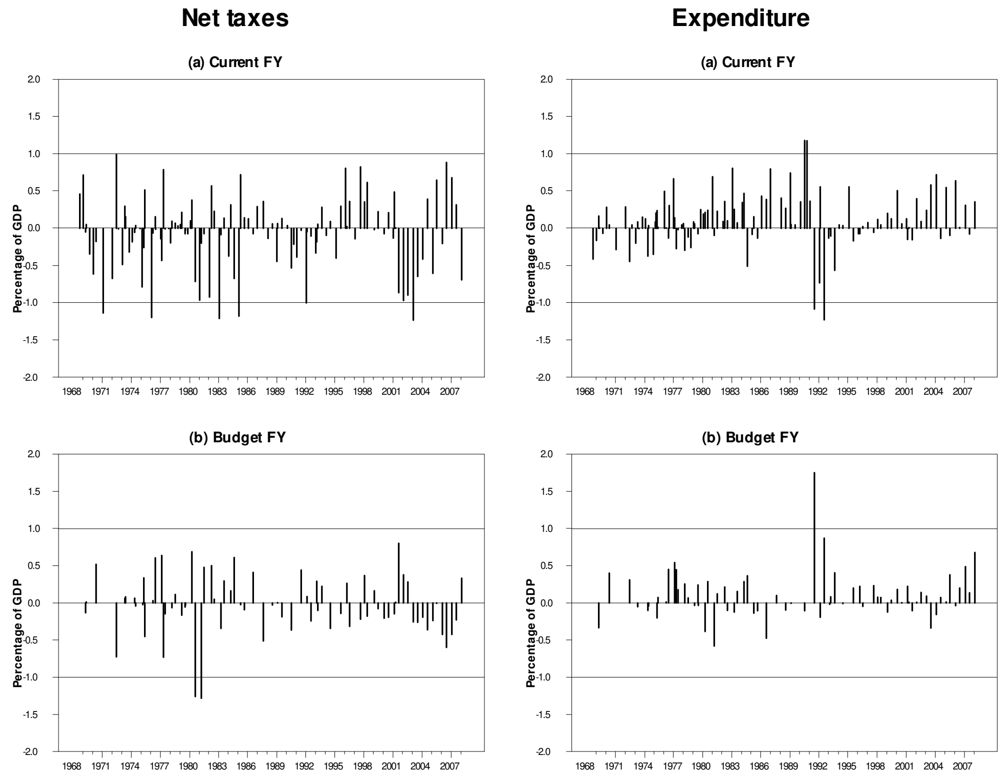

Figure 1: Estimated fiscal shocks

and 0.20-0.25 percent for spending but, excluding the years 1990-1992, this latter figure goes down to about 0.15 percent. 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.

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 (current 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 (current 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. The subsequent military episode - the Gulf War - is reflected on the large shocks of July 1991 (a recomposition of spending between current and upcoming fiscal years), its impact mixing in this occasion with that of deposit insurance spending. Some of the positive spikes of the spending shock series in the 2000s are partly related to defense outlays.

A final aspect in Table 2 is that on average current-year shocks are not smaller than their budget-year counterparts. This is the opposite of what one would expect and indicates that current-year forecasts are still surrounded by considerable uncertainty, in spite of their incorporation of more information.

Table 2: Shock series, descriptive statistics

	current 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 current- and budget-year shocks, respectively.

I mentioned above studies dealing with the properties of government agencies' budget forecasts. My shock measure should capture possible biases in the behavior of OMB as, for instance, a consistent initial over- or under-quantification of the effects of policy measures. The mean error in Table 2 indicates a slight overprediction in the case of net taxes and underprediction in the case of expenditure; but the figures are small. They are equal or less then 0.1 percent of GDP in absolute terms, and less than 1/3 of the respective standard deviations (much less for net taxes). These results are not supportive of a bias in OMB's projections and this fits in with the conclusions reached by previous literature, which mostly considered the «raw revisions» to the forecasts (i.e. the difference in the figures for the same fiscal year between consecutive announcements). The scope of such revisions is comparatively broader since base year and macroeconomic assumptions are not controlled for. The corresponding statistics calculated for the announcements in this study on the basis of raw revisions (not shown) point likewise to the absence of a bias.

I now turn to the correlations between net tax and expenditure shocks for the same fiscal year and current- and budget-year shocks for the same variable. The figures are displayed in Table 3, which also shows the corresponding correlations for the raw revisions.

Table 3: Shock series, correlations

	shocks	*raw revisions*
(deficit^{cFY}, deficit^{bFY})	-0.26	*0.76*
(net taxes^{cFY}, net taxes^{bFY})	-0.22	*0.76*
(expenditure^{cFY}, expenditure^{bFY})	-0.55	*0.40*
(net taxes^{cFY}, expenditure^{cFY})	-0.24	*-0.29*
(net taxes^{bFY}, expenditure^{bFY})	0.15	*-0.21*

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

There are negative correlations between current- 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 two very large simultaneous current-year and budget-year shocks with opposite signs in July 91 and July 92. It goes down to -0.30 excluding them, becoming close to the figure for net tax shocks. Such a negative correlation between changes in anticipated (exogenous) fiscal variables stems firstly from the temporary nature of some policy measures: their budget impact is felt only or mostly within a given fiscal year, leading to an offsetting shock in the following one. Further, particularly in the case of taxes, measures often have retroactive provisions, implying larger effects in the year of implementation. A third reason affecting the outlay side has to do with uncertainty as to the speed of implementation of programs; that is, money initially budgeted for a given year may turn out to be spent in the subsequent one or vice versa.

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 current-year counterparts which is not the case.

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

5.1 Empirical strategy

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. Given the likelihood of a quick response of financial markets and to minimize temporal aggregation which can blur the estimation of the impacts, I use weekly data. For instance, if several shocks of different sizes and even signs occur during a given quarter, quarterly averages of interest rates will not capture properly their effects. The empirical strategy followed here is similar to the one in Romer and Romer (2004, 2007): if the policy measure is approximately exogenous then its effects can be assessed on the basis of reduced-form specifications.

I start by estimating 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 *current-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 the 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 current fiscal year are:

$$r_t = \alpha + \sum_{i=1}^{52} \beta_i r_{t-i} + \sum_{i=0}^{52} \gamma_i \hat{d}_{t-i}^{cy} + \sum_{i=0}^{52} \delta_i \hat{d}_{t-i}^{by} + \varepsilon_t \tag{2}$$

$$r_t = \alpha + \sum_{i=1}^{52} \beta_i r_{t-i} + \sum_{i=0}^{52} \gamma_i \hat{n}t_{t-i}^{cy} + \sum_{i=0}^{52} \delta_i \hat{n}t_{t-i}^{by} + \sum_{i=0}^{52} \lambda_i \hat{g}_{t-i}^{cy} + \varepsilon_t \tag{3}$$

$$r_t = \alpha + \sum_{i=1}^{52} \beta_i r_{t-i} + \sum_{i=0}^{52} \gamma_i \hat{g}_{t-i}^{cy} + \sum_{i=0}^{52} \delta_i \hat{g}_{t-i}^{by} + \sum_{i=0}^{52} \lambda_i \hat{n}t_{t-i}^{cy} + \varepsilon_t \tag{4}$$

where r_t is the weekly average of the daily 10-year constant maturity rate, $\hat{d}_t^{cy}$ and $\hat{d}_t^{by}$ denote the current- and budget-year deficits shocks, and the same notation applies to net tax ($\hat{n}t_t^{cy}$ and $\hat{n}t_t^{by}$) and expenditure ($\hat{g}_t^{cy}$ 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.[10] The regression for measuring the effects of budget-year shocks is likewise (2) in the case of the deficit; in the case of net taxes it is similar to (3), except that one controls for $\hat{g}_t^{by}$ instead of $\hat{g}_t^{cy}$, and in the case of spending it is similar to (4) but controlling for $\hat{n}t_t^{by}$ instead of $\hat{n}t_t^{cy}$. All of the 6 regressions were estimated by OLS. The sample period starts in the third week of September 1968, the first one when current-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.

5.2 Results

Subsample sensitivity turned out to be a key finding. In particular, when the sample is broken at the midpoint, end of 1988, there is a marked difference in the responses of the economy in the first and second halves. While this breakpoint is not motivated by any precise event, it is convenient given the possibility of a major change in the effects of fiscal policy in the nineties in the US (see, for instance, Auerbach (2002)). Specifically, the tight fiscal policy implemented by the Clinton administration is hypothesed to have strengthened economic performance. Results are always split according to the subsamples 1969:09-1988:12 and 1989:01-2008:03.

Figures 2 and 3 show the dynamic multipliers for the long-term interest rate following, respectively, current- 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 4, although innovations of this magnitude did occur in a number of occasions. The responses are in percentage points (annualized). One-

[10] As explained in Section 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).

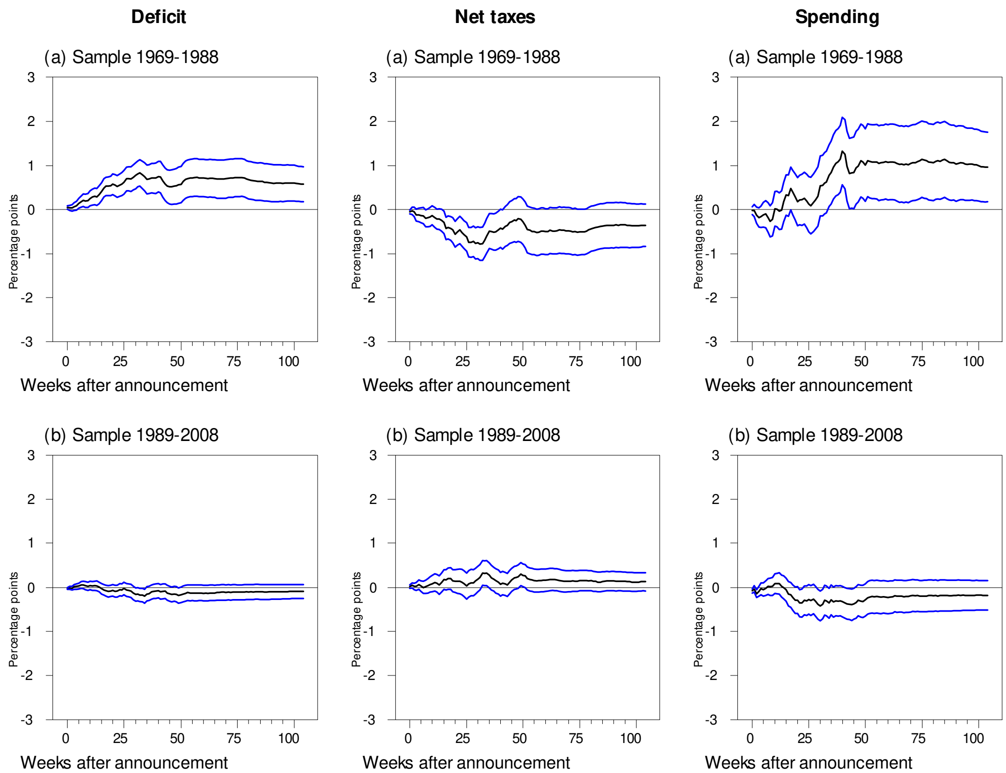

Figure 2: Estimated impact of current-year fiscal shocks on the long-term interest rate

standard-deviation bands are shown as well.[11]

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 current- 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

[11] 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.

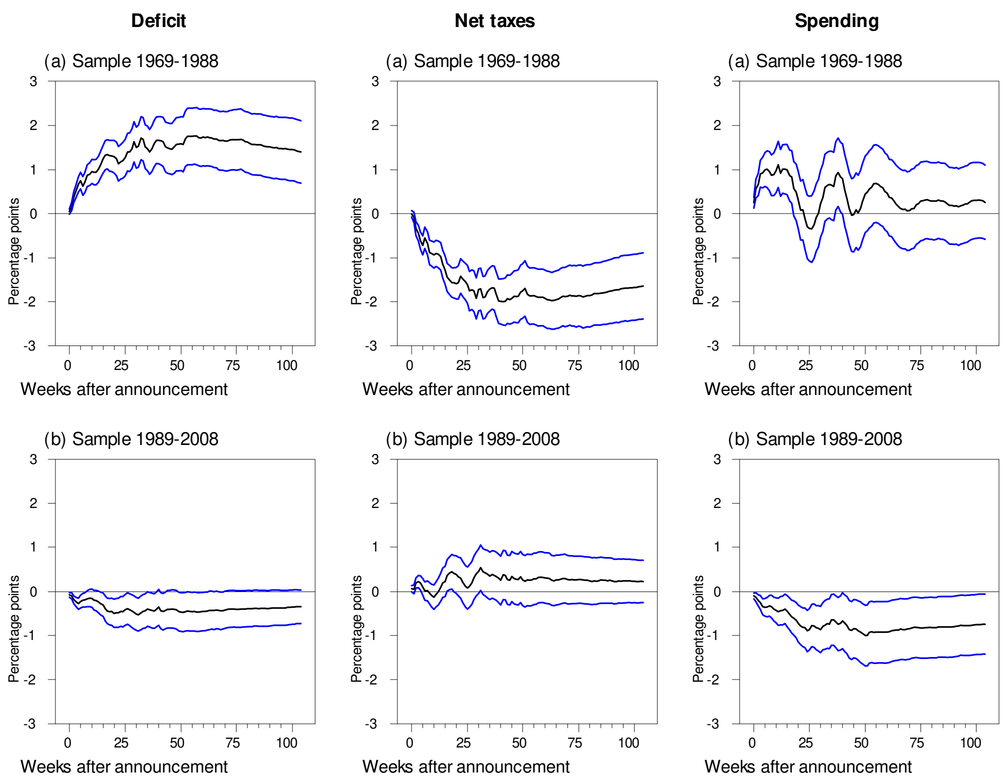

Figure 3: Estimated impact of budget-year fiscal shocks on the long-term interest rate

attained nine months out in the case of current-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 the larger size (Table 2) 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 current-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.

Changes in anticipated fiscal variables for the budget-year produce a greater impact than the corresponding changes for the current-year, particularly in the case of net taxes. There may be a number of reasons explaining this. Firstly, new policy measures are likely to be predominant as a source of budget-year shocks, while current-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 at the end of Section 2). More generally, in specific occasions such as presidential transitions, they may capture markets' beliefs about the stance of fiscal policy in the coming years.

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 (2003) 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, but his results are not clear-cut.[12]

Having documented the responses of long-term interest rates, the next step it is to place them against the full set of macroeconomic interactions following the realization of fiscal shocks. I take up this point in the next section by considering their impacts in the framework of a system of equations.

6 Macroeconomic interactions in the wake of fiscal shocks

The results in this section are based on a set of key macroeconomic variables 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 the output gap measured as the detrended log of the IPI[13], 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.[14] The regressions include 12 lags, and correlated

[12] Perotti estimates a structural VAR on the basis of quaterly data, considering two subsamples: 1960:1-1979:4 and 1980:1-2001:4. He gets impacts on interest rates that are akwardly negative for both net taxes and spending (and very small, in the range of -0.1 to -0.2 p.p.). Such results hold for the two subsamples considered. They seem to underline the difficulties in using fiscal innovations derived from structural VAR to assess impacts on interest rates.

[13] 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 gap measure.

[14] Throughout the whole sample I have only one case of two shocks occuring 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.

shocks are controlled for, as in the previous section. For instance, the multivariate regressions estimated to assess, respectively, the effects of current-year deficit, net tax and spending shocks are:

$$\mathbf{x}_t = \boldsymbol{\alpha} + \sum_{i=1}^{12} \mathbf{B}_i \mathbf{x}_{t-i} + \sum_{i=0}^{12} \gamma_i \hat{d}_{t-i}^{cy} + \sum_{i=0}^{12} \boldsymbol{\delta}_i \hat{d}_{t-i}^{by} + \boldsymbol{\varepsilon}_t, \tag{5}$$

$$\mathbf{x}_t = \boldsymbol{\alpha} + \sum_{i=1}^{12} \mathbf{B}_i \mathbf{x}_{t-i} + \sum_{i=0}^{12} \boldsymbol{\gamma}_i \hat{n}t_{t-i}^{cy} + \sum_{i=0}^{12} \boldsymbol{\delta}_i \hat{n}t_{t-i}^{by} + \sum_{i=0}^{12} \boldsymbol{\lambda}_i \hat{g}_{t-i}^{cy} + \boldsymbol{\varepsilon}_t, \tag{6}$$

$$\mathbf{x}_t = \boldsymbol{\alpha} + \sum_{i=1}^{12} \mathbf{B}_i \mathbf{x}_{t-i} + \sum_{i=0}^{12} \boldsymbol{\gamma}_i \hat{g}_{t-i}^{cy} + \sum_{i=0}^{12} \boldsymbol{\delta}_i \hat{g}_{t-i}^{by} + \sum_{i=0}^{12} \boldsymbol{\lambda}_i \hat{n}t_{t-i}^{cy} + \boldsymbol{\varepsilon}_t. \tag{7}$$

where, as before, $\hat{d}_t^{cy}$ and $\hat{d}_t^{by}$ denote current- and budget-year deficits shocks, and the same notation applies to net tax ($\hat{n}t_t^{cy}$ and $\hat{n}t_t^{by}$) and expenditure ($\hat{g}_t^{cy}$ and $\hat{g}_t^{by}$) ones. This empirical approach is similar to the one followed in Edelberg et al. (1999) and related literature.

Figure 4 shows the impacts of 1 percent of GDP deficit shocks according to the sample split 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 an indication of essentially 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 of the long rate. The results in the two approaches are very consistent. The maximum impacts of the realization of deficit shocks in the period prior to 1988:12 are now about 0.6 and 1.8 p.p. for current- and budget-year horizons, respectively, being 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 current-year shocks. For their budget-year counterparts, the maximum impact goes down a bit

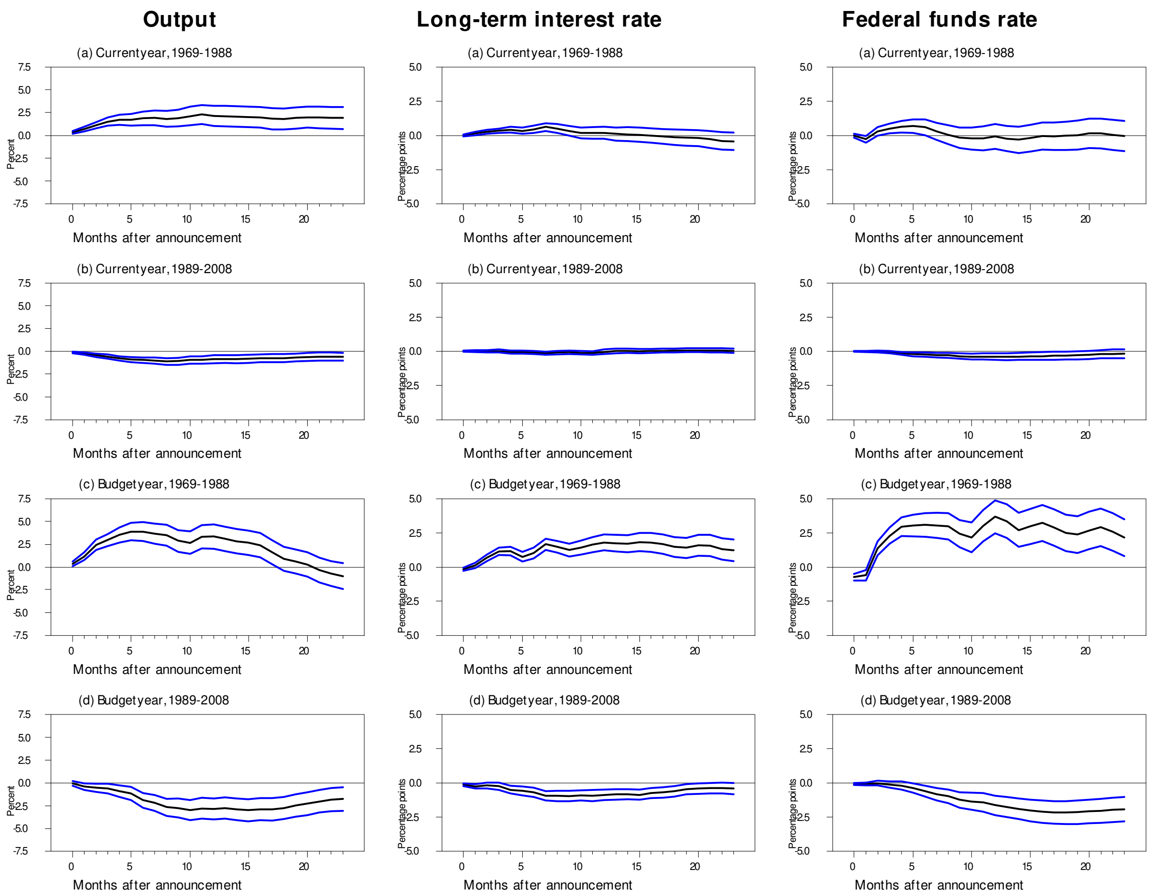

Figure 4: Estimated macroeconomic responses to deficit shocks (VAR based results)

to -0.9 p.p. against -0.5 p.p. previously.

6.1 Fiscal shocks and the behavior of output

Figure 4 indicates 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 5 and 6. 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.[15] The multipliers - measured as the peak effect - for current- 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. The trajectories of output following changes in anticipated net taxes and spending are also in line with the Keynesian prior in the period before 1988:12, 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. The multiplier for current-year deficit shocks is smaller than those for the respective components because these attain the maximum impact at different points in time.

A point to stress is that output and long-term interest rates move in the same direction. This is evidence against the hypothesis that only the financial markets, not agents, are forward-looking and that revisions to anticipated fiscal policy affect aggregate demand only indirectly, via interest rates. If this was the case, one would expect long-term interest rates to go up,

[15] 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.

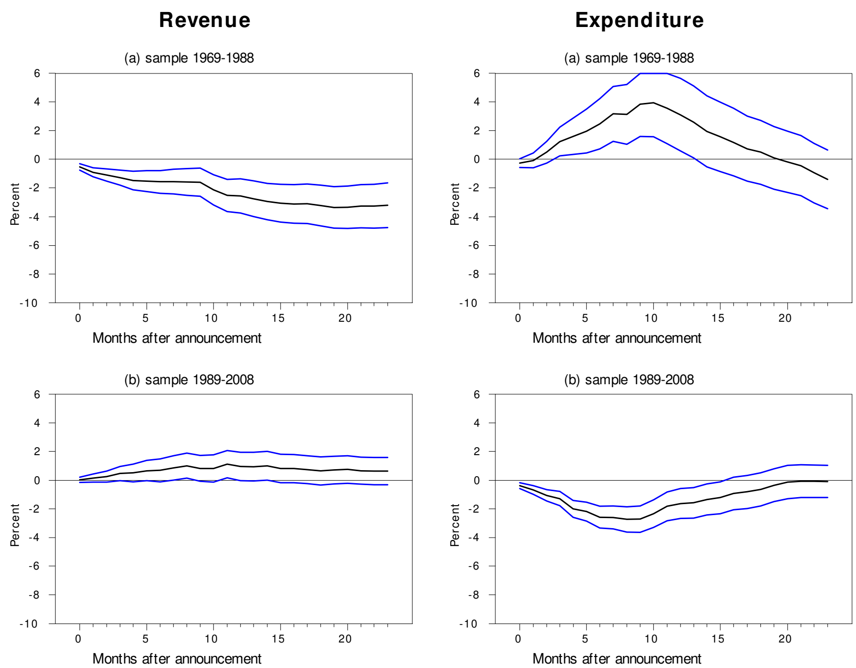

Figure 5: Estimated output responses to current-year fiscal shocks

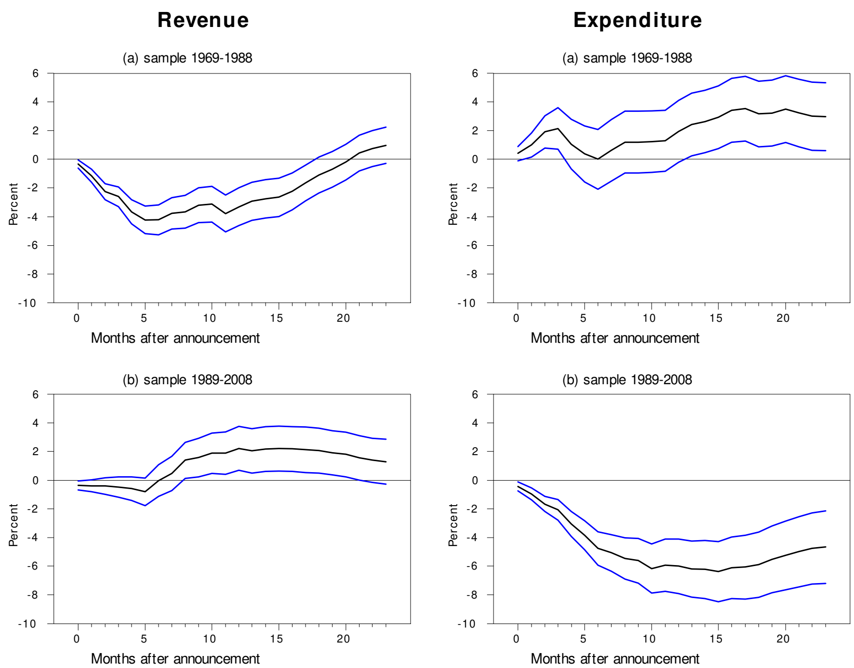

Figure 6: Estimated output responses to budget-year fiscal shocks

depressing aggregate demand and output, at least temporarily, in the wake of positive deficit shocks.[16] Contrary to these predictions, in the first subsample deficit shocks trigger positive immediate effects on output much as they do on interest rates.

The deficit multipliers are negative in the second half of the sample, with figures of about -0.5 for current-year shocks and a surprisingly one of -1.5 for their budget-year 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 -3.5. Since these inferences are based on a reasonable but not very large number of observations, in particular for budget-year horizons, estimates are sensitive to influential ones. In particular, the very large budget-year spending shock in July 1991 (1.7 percent of GDP), occurring in a period of sluggish growth shortly after the end of a recession, is «inflating» the estimated decrease in output. When this particular value is removed (simply by setting it to 0), the output multiplier becomes approximately -2.8, still a rather large figure nevertheless. 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 current-year forecasts. Precise magnitudes apart, these results lend strong credibility to the expansionary fiscal contraction hypothesis in the US over the last two decades.

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

[16] This would happen because financial markets would anticipate a rise in the short rate in line with the effects of fiscal loosening in a standard IS-LM framework (as formalized in Blanchard (1984) and Branson et al. (1985)) or, alternatively, in line with an expected response of the Federal Reserve offsetting the expansion of output. The «perverse» effects on output would be temporary because, in principle, the actual implementation of the fiscal stimulus later on would reverse them.

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, indicating a Keynesian behavior of policy.

This is not the first paper to indicate that the effects of fiscal policy in the US may have been, at least partially, at odds with the Keynesian prior in recent decades. Romer and Romer (2007) show that 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. SVAR measures of shocks document a weakening or a reversion (particularly for net taxes) of the response of activity when the sample is broken around the eighties. This possibility has been associated with fiscal policy in the Clinton era and, more recently, with Bush II tax cuts.

The expansionary fiscal contraction hypothesis is linked to the so-called expectations view of fiscal policy (see Giavazzi and Pagano (1990)). A key ingredient is an increased awareness of the government's long-run budget constraint, leading agents to expect that measures such as tax cuts or spending increases be counteracted in the future. However, from the Ricardian equivalence debate one may expect this awareness to weaken, or fully offset in the limit, the impact of fiscal loosening on output, but not to reverse it. Theoretical contributions in the area (see Giavazzi et al. (2000) for a survey) have emphasized that such effects should be associated with fiscal decisions taken in situations of very large budget imbalances, rendering more likely the need for major and disruptive fiscal adjustments (or conversely, in the case of policy tightening, eliminating or postponing this need). If the path of the current policy is already seen as unsustainable, further loosening 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 that 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, Figure 4 indicates 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 relevant question is to what extent the fiscal situation in the US over, say, the last two decades was such that agents felt that a major adjustment was necessary in the near future. This is a tricky question primarily because such expectations are largely impossible to proxy by observable variables. 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).[17]

[17] As it is known, longer-term budget projections are available but its usefulness in this respect is doubtfull (see the discussion at the end of Section 2).

It is beyond doubt that political debate came to reflect growing concerns about fiscal sustainability from the early- to mid-eighties. 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 back, and may signal a parallel change in the beliefs of agents. When during the 1980 presidential campaign Reagan put forward the proposal of a major tax cut, his opponent Carter objected on the grounds it was inflationary, not on budgetary grounds (Romer and Romer (2008)).

It seems reasonable to assume that consumers became more Ricardian over the time spanned by the sample. On the one hand, liquidity constraints have eased in line with the development of the capital market. Further, one may conjecture that recent experiences showing that government's fiscal position can change rapidly (also because it depends crucially on economic growth) led agents to reckon with offsetting measures not only within their lifespan but in the near future. The seemingly comfortable fiscal position achieved in the late nineties in the US gave way in not many years to large imbalances and revived concerns about budget sustainability. In short, in the second half of the sample one can pinpoint conditions comparatively more propitious to the occurrence of non-Keynesian effects.

Looking at the evolution of budget indicators, Federal US debt grew very fast in the period between 1982 and 1993, by about 25 p.p. of GDP (considering the debt held by the private sector), reaching a peak around 50 per cent of GDP. This value was nevertheless below the levels prevailing a couple of decades before, during the fifties. The picture is thus one of a sharp budget deterioration but hardly of a major crisis. And then there was an improvement over the subsequent period that brought the debt ratio down by around 15 p.p.. The personal income tax marginal rates were comparatively lower in the second half of the sample. Therefore, my results document that fiscal policy can have non-conventional effects under less extreme budgetary positions than previously thought. But further research is needed to determine the precise mechanism at work.

Some literature dealing with the expansionary fiscal contraction hypothesis (e.g. Alesina and Ardagna (1998) and Perotti (1999)) has suggested that larger shocks and shocks on the outlay side are more likely to have non-Keynesian effects. The larger negative spending multiplier in the second subsample appears consistent with the second possibility. I can dig a bit further into it since I have separate information about transfers. Accordingly I split net taxes into its two components and consider the effects separately. As it turns out, transfers are much more predictable than taxes and spending, in particular in the second part of the sample on which interest focus now. Consequently, the average absolute transfer shock in the post-1988 period is much smaller than for the other two budget items - by about one half - and the variability is only 1/4 to 1/5 (budget-year shocks). The respective impulse-responses have extremely wide confidence bands (not shown), although the point estimate for positive transfer impulses is negative. The response of taxes alone is very similar to that of *net* taxes, since tax dominate transfer shocks in terms of magnitude and variability. On balance, in my results non-conventional effects are more prominent for spending impulses than for their tax counterparts, while the evidence for transfers is not conclusive.

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.

6.2 The impact on the federal funds rate and the behavior of monetary authorities

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 Figure 4 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, two possibilities arise as to the reaction of monetary authorities to fiscal shocks. They may not react to fiscal news as such, but only to the ensuing output response. Alternatively they may react directly to fiscal news, including what they forecast to be the impact on output gap, and for instance tend to accommodate that impact to some extent. In the latter case, one would expect this to weaken or perhaps reverse the standard upward response of the funds rate following a positive deviation of output from trend in the first subsample. 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 Figure 4 by presenting the same responses when the period 1979:10 to 1982:10 is excluded from the first subsample (Figure 7).

I first compare the responses of the federal funds rate to budget-year shocks in Figure 7 and in panels (a) and (c) of Figure 4. 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

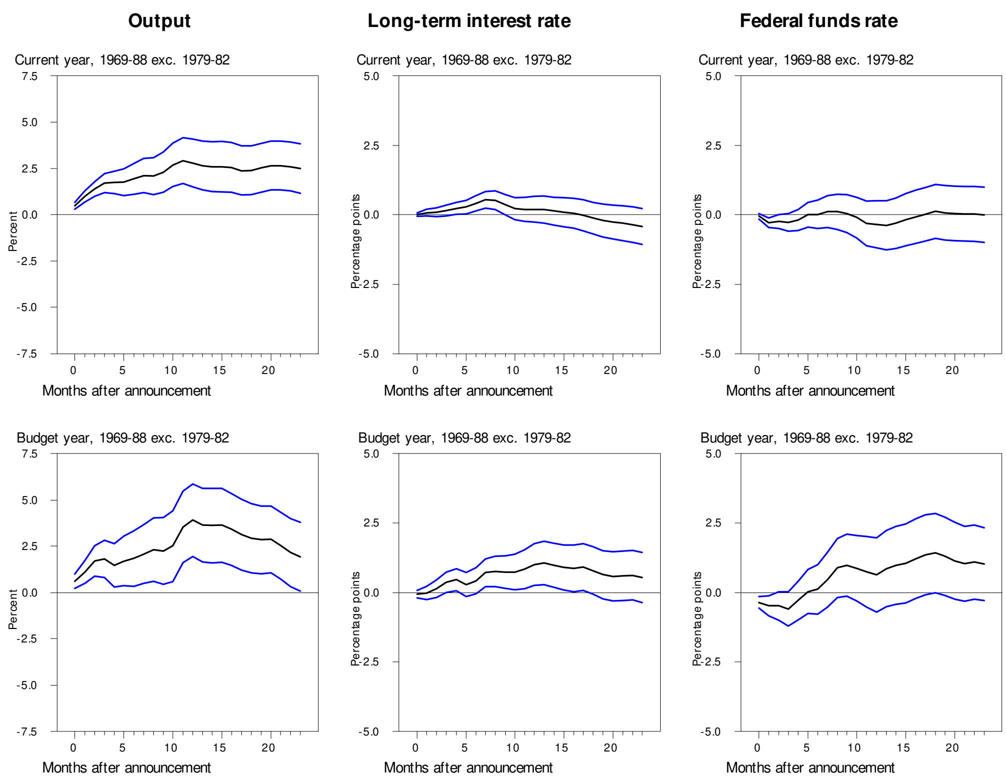

Figure 7: Estimated macroeconomic responses to deficit shocks excluding the years of the Volcker experiment

money market rate than otherwise. In the first case, the funds rate begins to go up 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 the second case the money market 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 current-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 current-year shocks have a sizeable positive impact on output gap.

The negative trajectory of the funds rate in the initial months after the budget-year shocks in Figure 7, 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)[18] 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. In this case, however, the response is very 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

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

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).

7 More on the impact of fiscal policy on long-term bond yields

7.1 Impact on the risk premium

Figures 4 and 7 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 (5) 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 (5) 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.[19] 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 (5). 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

[19] 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 (5) for y_t, one gets the corresponding equation in the new system which has the form $y_t = a + \sum_{i=1}^{23} \mathbf{b}_i \mathbf{x}_{t-i}^+ + \sum_{i=1}^{12} c_i s_{t-i} + \sum_{i=0}^{23} d_i \hat{d}_{t-i}^{cy} + \sum_{i=0}^{23} e_i \hat{d}_{t-i}^{ny} + \sum_{i=0}^{11} f_i \boldsymbol{\varepsilon}_{t-i}^{y}$.

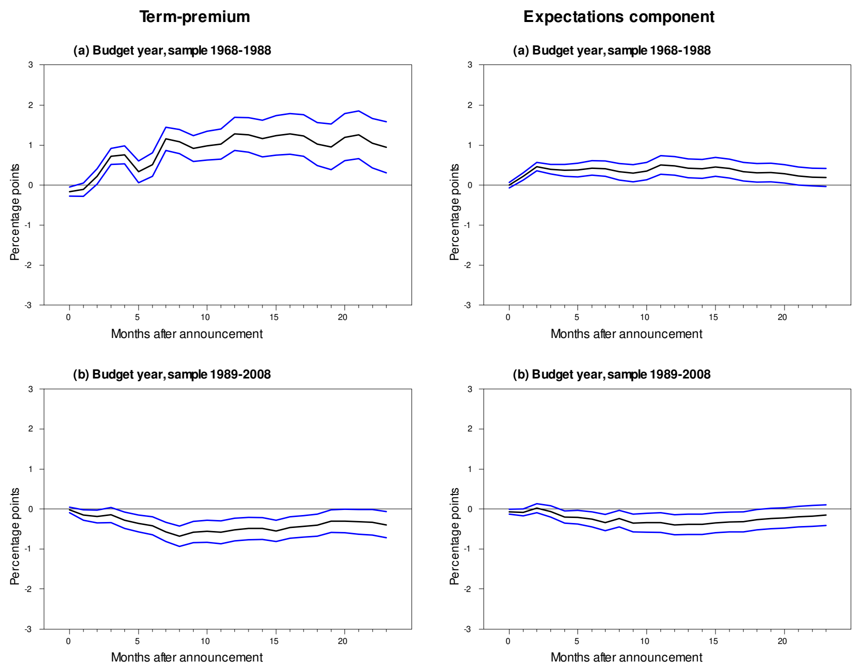

Figure 8: Response of the long-term rate following deficit shocks: term premium and expectations component

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 8 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 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 8. 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.[20] 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)).

7.2 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

[20] 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.

expansionary fiscal contraction hypothesis. In the first case, the adjustment is expected to take place through accommodating monetary policy. In the 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 9. They were obtained on the basis of univariate regressions, analogous to (2) 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 current-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

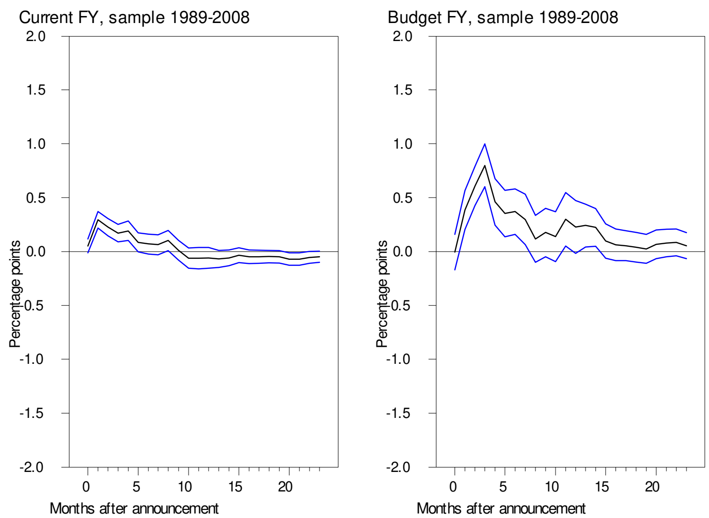

Figure 9: Response of expected inflation to deficit shocks

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.

8 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. While in the first subsample running from 1969 to 1988 the results are quite consistent with Keynesian textbook predictions, they change substantially in the two subsequent decades ending in 2008, during which fiscal policy is found to have sizeable perverse effects on output. The findings in this more recent period put clearly a question mark on the use of discretionary fiscal policy as a stabilizing toll. They are troubling against the background of the recent recession where fiscal policy has been called to play an important anti-recessionary role. Indeed, governments in the US and elsewhere have implemented stimulus packages of considerable sizes, but the evidence presented here generally questions their effectiveness.

The key question arising is: are such perverse effects inherent to the use of fiscal policy in the current macroeconomic setting or do they have to do with specific aspects of the policy design? Indeed, in contrast with monetary policy, fiscal policy can be designed in multiple ways, in terms of which and how budget items are affected. Nevertheless, as Auerbach (2009) points out, little progress has been made in recent years toward improving such design. This is explained by the fact that almost a consensus had emerged that the countercyclical role of the government budget should be left to automatic stabilizers rather than discretionary measures. Research should be directed to the mechanisms at work behind non-conventional impacts of fiscal policy on output in recent decades and whether specific features of the way it is conducted are bringing them about.

9 Appendix on 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, the 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 before and after for which the assumptions were known.

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 (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.

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