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DEBT AND THE EFFECTS OF FISCAL POLICY

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ABSTRACT

A shift in taxes or in government spending (a "fiscal shock") at some point in time puts a constraint on the path of taxes and spending in the future, since the government intertemporal budget constraint will eventually have to be met. This simple fact is surprisingly overlooked in analyses of the effects of fiscal policy based on Vector AutoRegressive models. We study the effects of fiscal shocks keeping track of the debt dynamics that arises following a fiscal shock, and allowing for the possibility that taxes, spending and interest rates might respond to the level of the debt, as it evolves over time. We show that omitting a debt feedback can result in incorrect estimates of the dynamic effects of fiscal shocks. In particular, the absence of an effect of fiscal shocks on long-term interest rates -- a frequent finding in studies that omit a debt feedback -- can be explained by their mis-specification. Using data for the U.S. economy and two alternative identification assumptions we reconsider the effects of fiscal policy shocks correcting for these shortcomings.

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1 Introduction

A shift in taxes or in government spending (a "fiscal shock") at some point in time puts a constraint on the path of taxes and spending in the future, since the government intertemporal budget constraint will eventually have to be met. This simple fact is surprisingly overlooked in analyses (at least those of which we are aware) of the effects of fiscal policy based on Vector AutoRegressive models.

Consider for example a positive shock to government spending. Following the shock the government may respect its budget constraint by adjusting taxes and spending so as to keep the ratio of public debt-to-GDP stable, or it may delay the adjustment and in the meantime let the debt ratio grow. It may even plan to use the inflation tax or to default. The effects of the fiscal shock on taxes, spending, inflation and interest rates are likely to differ depending on the path the government chooses.

Another way to put this is that the Vector AutoRegressive models that are typically used to estimate the effects of fiscal shocks on various macroeconomic variables (such as output and private consumption) share two weaknesses: (i) they fail to keep track of the debt dynamics that arises following a fiscal shock, and (ii) as the debt ratio evolves over time they overlook the possibility that taxes and spending might respond to the level of the debt. In other words, following a fiscal shock, taxes and spending are assumed to respond to various macroeconomic variables but not to the level of the public debt. This omission is particularly surprising in the case of countries where the data reveal a positive correlation between the government surplus-to-GDP ratio and the government debt-to-GDP ratio and thus indicate that fiscal variables respond to the level of the debt. Bohn (1998) finds such a correlation in a century of U.S. data.

The consequence of omitting a feedback from the debt level is that the error terms in the equations that are estimated include, along with truly exogenous fiscal shocks, the responses of taxes, government spending and other variables—such as (importantly) long term interest rates—to the level of the debt ratio along the path induced by the fiscal shock. The coefficients that are estimated and then used to compute impulse responses are thus typically biased. An effect of such a bias is that impulse responses are sometimes computed along unstable debt paths, *i.e.* paths along which the debt-to-GDP ratio diverges. The omission of a feedback from the level of the debt to long-term interest rates, combined with the failure to keep track of the debt dynamics, could also be the reason why in some experiments interest rates do not appear to respond significantly to fiscal shocks.

One could argue that omitting the level of the debt is not a problem because the Vector AutoRegressive models that are typically estimated already include all the variables that enter the government intertemporal budget constraint and thus determine the evolution of the debt over time: what is missing is at most an initial value for the debt level. We show that is would not be enough: failure to explicitly include the debt level in the estimated equation—and keep track of its path when computing impulse responses—can result in biased estimates of the effects of fiscal policy shocks on macro variables.

The point we make sheds light on a common empirical finding: the effects of fiscal shocks seem to change across time. For instance, Perotti (2007) finds that the effect on U.S. consumption of an increase in government spending is positive and statistically significant in the 1960's and 1970's, but became insignificant in the 1980's and 1990's. We find a sharp difference in the way U.S. fiscal authorities responded to the accumulation of debt in the two samples: since the early 1980's, following a shock to spending or taxes, both fiscal policy instruments are adjusted over time in order to stabilize the debt ratio. This does not appear to have happened in the 1960's and 1970's, when there is no evidence of a stabilizing response of fiscal policy. This evidence can explain the heterogeneity of impulse responses to fiscal shocks in the pre-1980 and the post-1980 samples for two reasons. First, the dynamic behavior of taxes and spending following a fiscal shock depends on the importance of the debt stabilization motive in the fiscal reaction function. Second, it should not be surprising that consumers respond differently to an innovation in taxes or government spending depending on whether or not they expect the government to meet its intertemporal budget constraint by adjusting taxes and/or spending in the future.

Our findings are also related to the evidence of a non-linearity in the response of private consumption to fiscal shocks—documented among others by Giavazzi, Jappelli and Pagano (2000) for a group of OECD countries. Romer and Romer (2007) also find that the effect of a U.S. tax shock on output depends on whether the change in taxes is motivated by the government's desire to stabilize the debt, or is unrelated to the stance of fiscal policy.

The point we make is independent of the assumption adopted to identify fiscal shocks–whether imposing enough constraints on a Structural VAR (such as in Blanchard and Perotti, 2002 or Mountford and Uhlig, 2002) or identifying shocks from the narrative record, as Ramey (2006), or in Romer and Romer (2007). This paper is agnostic as to the best strategy to identify fiscal shocks: we experiment with alternative identification approaches and document the importance of omitting the debt-deficits dynamics in all cases.

The plan of the paper is as follows. In Section 2 we explain why estimating the effects of fiscal policy shocks omitting the response of taxes and spending to the level of the public debt is problematic. Section 3 describes our data. In Sections 4 and 5 we evaluate the empirical relevance of our point computing impulse responses to fiscal shocks in models in which the variables are allowed to respond to the level of the debt–whose evolution over time is determined by the intertemporal government budget constraint. We then compare these impulse responses with those obtained from models that omit the debt level. In Section 4 we use the identification technique proposed by Blanchard and Perotti (2002). In Section 5 we use the tax shocks identified by Romer and Romer (2007).

We close by observing that the methodology described in this paper to analyze the impact of fiscal shocks by taking into account the stock-flow relationship between debt and fiscal variables could be applied to other dynamic models which include similar identities. One example are the recent discussions on the importance of including capital as a slow-moving variable to capture the relation between productivity shocks and hours worked (see *e.g.* Christiano et al, 2005 and Chari et al. 2005).

2 Why standard fiscal policy VAR's are misspecified

The study of the dynamic response of macroeconomic variables to shifts in fiscal policy is typically carried out estimating a vector autoregression of the form

$$\mathbf{Y}_t = \sum_{i=1}^{k} \mathbf{C}_i \mathbf{Y}_{t-i} + \mathbf{u}_t \tag{1}$$

where $\mathbf{Y}$ includes government spending, taxes, output and other macroeconomic variables such as interest rates, consumption and inflation.

The level of the debt-to-GDP-ratio is never included in (1). This variable, however, is an important factor in determining the effects of fiscal policy for two reasons (at least):

- a feedback from the level of debt ratio to taxes and government spending is necessary for stability of the debt, unless the rate of growth of the economy is exactly equal to the average cost of financing the debt. Such a feedback is a feature of the data: Bohn (1998) finds that a century of U.S. data reveal a positive correlation between the government surplus-to-GDP ratio and the government debt-to-GDP ratio;

- interest rates, a central variable in the transmission of fiscal shocks, depend on future expected monetary policy and on the risk premium: both may be affected by the debt dynamics–for instance if a growing stock of debt raises fears of future monetization or, in the extreme case, of debt default. The impact of a given fiscal shock on interest rates will be very different depending on whether the shock produces a path of debt that is stable or tends to become explosive.

If the level of the debt ratio is significant in explaining at least some of the variables included in (1), omitting it implies that the error terms $\mathbf{u}$ will include, along with truly exogenous shocks, the responses of $\mathbf{Y}$, and in particular of taxes, spending and interest rates, to the level of the debt: this will result in biased estimates of the $\mathbf{C}_i$ coefficients. The analysis of the effects of fiscal shocks using (1) can thus be problematic.

Once the level of the debt ratio is included in (1), one must allow for the fact that taxes, government spending, output, inflation and the rate of interest–in other words the variables entering $\mathbf{Y}$–are linked by an identity, the equation that determines how the debt ratio evolves over time. These observations naturally lead to replacing (1) with

$$\begin{aligned}\mathbf{Y}_t &= \sum_{i=1}^{k}\mathbf{C}_i\mathbf{Y}_{t-i} + \sum_{i=1}^{k}\boldsymbol{\gamma}_i d_{t-i} + \mathbf{u}_t \qquad (2)\\ d_t &= \frac{1+i_t}{(1+\Delta p_t)(1+\Delta y_t)}d_{t-1} + \frac{\exp(g_t)-\exp(t_t)}{\exp(y_t)}\end{aligned}$$

where $\mathbf{Y}_t' = \begin{bmatrix} g_t & t_t & y_t & \Delta p_t & i_t \end{bmatrix}$. d is the debt-to-GDP ratio, i is the nominal rate of interest (the average cost of debt financing), Δy is real GDP growth, Δp is inflation, t and g are, respectively, (the logs of) government revenues and government expenditure net of interest. (We use logs because it is the log of output, taxes and spending that enters $\mathbf{Y}$). Note that the presence of d_{t-i} amplifies the dynamic effect of fiscal shocks, which cumulate in (2), while they do not in (1): the difference between impulse responses computed using (2) and (1) might thus diverge as the horizon increases.

Before discussing how fiscal policy shocks can be studied in the context of (2) we pause and ask a question. $\mathbf{Y}$ already contains all the variables that enter the government intertemporal budget constraint in (2): isn't this good enough ? Do we need to insert the debt level directly? Why are the impulse responses biased if the model does not explicitly include d and the identity describing debt accumulation? The reason why d cannot be dropped is that it is unlikely that the short lags of g, t, Δp, Δy and i that enter (linearly) (1) can trace the evolution of the debt ratio accurately enough. To convince yourself notice that d_t is the result of a long and non-linear lag dynamics

$$\begin{aligned} d_t &= \sum_{i=0}^{K} \left(\frac{\exp\left(g_{t-i}\right) - \exp\left(t_{t-i}\right)}{\exp\left(y_{t-i}\right)} \right)^i \prod_{i=o}^{K} \left(\frac{1 + i_{t-i}}{\left(1 + \Delta p_{t-i}\right)\left(1 + \Delta y_{t-i}\right)} \right) + \\ &\quad + \prod_{i=o}^{K} \left(\frac{1 + i_{t-i}}{\left(1 + \Delta p_{t-i}\right)\left(1 + \Delta y_{t-i}\right)} \right) d_{t-i-1} \end{aligned}$$

But the best way to convince the reader is to show that impulse responses computed using (2) differ from those computed using (1) and produce different paths for d_t. We show this using U.S. data and two different ways to identify fiscal shocks, that are representatives of alternative paths researchers have followed (in this paper we remain agnostic as to the preferred identification strategy): the technique proposed by Blanchard and Perotti (2002) and the "exogenous" tax shocks identified by Romer and Romer (2007) with a narrative approach. We start by describing our data.

3 The data

We begin using quarterly data for the U.S. economy since 1960:1, the sample analyzed in Blanchard and Perotti (2002) and extended to 2005:4 in Perotti (2007). Our approach requires that the debt-dynamics equation in (2) tracks the path of d_t accurately: we thus need to define the variables in this equation with some care.

The source for the different components of the budget deficit and for all macroeconomic variables are the NIPA accounts (available on the Bureau of Economic Analysis website, downloaded on December 7th 2006). y_t is (the log of) real GDP per capita, Δp_t is the log difference of the GDP deflator. Data for the stock of U.S. public debt and for population are from the FRED database (available on the Federal Reserve of St.Louis website,also downloaded on December 7th 2006). Our measure for g_t is (the log of) real per capita primary government expenditure: nominal expenditure is obtained subtracting from total Federal Government Current Expenditure (line 39, NIPA Table 3.2) net interest payments at annual rates (obtained as the difference between line 28 and line 13 on the same table). Real per capita expenditure is then obtained by dividing the nominal variable by population times the GDP chain deflator. Our measure for t_t is (the log of) real per capita government receipts at annual rates (the nominal variable is reported on line 36 of the same NIPA Table).

The average cost servicing the debt, i_t, is obtained by dividing net interest payments by the federal government debt held by the public (FYGFDPUN in the Fred database) at time $t-1$. The federal government debt held by the public is smaller than the gross federal debt, which is the broadest definition of the U.S. public debt. However, not all gross debt represents past borrowing in the credit markets since a portion of the gross federal debt is held by trust funds–primarily the Social Security Trust Fund, but also other funds: the Trust Fund for Unemployment Insurance, the Highway Trust Fund, the pension fund of federal employees, etc.. The assets held by these funds consist of non-marketable debt.[1] We thus exclude it from our definition of federal public debt.

Figure 1 reports, starting in 1970:1 (the first quarter for which the debt data are available in FRED), this measure of the debt held by the public as

[1] Cashell (2006) notes that "this debt exists only as a book-keeping entry, and does not reflect past borrowing in credit markets."

a fraction of GDP (this is the dotted line). We have checked the accuracy of the debt dynamics equation in (2) simulating it forward from 1970:1 (this is the continuous line in Figure 1). The simulated series is virtually superimposed to the actual one: the small differences are due to approximation errors in computing inflation and growth rates as logarithmic differences, and to the fact that the simulated series are obtained by using seasonally adjusted measures of expenditures and revenues. Based on this evidence we have used the debt dynamics equation to extend d_t back to 1950:1. (A quarterly series for d_t extending back to 1950:1 will become necessary when we compare our results with those in Romer and Romer (2007) whose sample starts just after World War II.) Figure 1 shows that this series tracks the annual debt level accurately, at least up to the early 1950's. [2]

4 Fiscal shocks identified from SVAR's

We start by comparing (2) with the Structural VAR (SVAR) estimated in Blanchard and Perotti (2002) and extended in Perotti (2007) (B&P in what follows).

SVAR's identify fiscal shocks imposing restrictions that allow the two structural fiscal shocks in (1) to be recovered from the reduced form residuals, **u**. The innovations in the reduced form equations for taxes and government spending, u_t^g and u_t^t, contain three terms: *(i)* the response of taxes and government spending to fluctuations in macroeconomic variables, such as output and inflation, that is implied by the presence of automatic stabilizers; *(ii)* the discretionary response of fiscal policy to news in macro variables and *(iii)* truly exogenous shifts in taxes and spending, the shocks we wish to identify. B&P exploit the fact that it typically takes longer than a quarter for discretionary fiscal policy to respond to news in macroeconomic variables: at quarterly frequency the contemporaneous discretionary response of fiscal policy to macroeconomic data can thus be assumed to be zero. To identify the component of u_t^g and u_t^t which corresponds to automatic stabilizers they use institutional information on the elasticities of tax revenues and government spending to macroeconomic variables. They thus identify the structural

[2] We are unable to build the debt series back to 1947:1, the start of the Romer and Romer sample, because data for total governemnt spending, needed to buld the debt series, are available on a consistent basis only from 1950:1.

shocks to g and t by imposing on the $\mathbf{A}$ and $\mathbf{B}$ matrices in $\mathbf{Au} = \mathbf{Be}$ the following structure [3]:

$$\begin{bmatrix} 1 & 0 & a_{gy} & a_{g\Delta p} & a_{gi} \\ 0 & 1 & a_{ty} & a_{t\Delta p} & a_{ti} \\ a_{31} & a_{32} & 1 & 0 & 0 \\ a_{41} & a_{42} & a_{43} & 1 & 0 \\ a_{51} & a_{52} & a_{53} & a_{54} & 1 \end{bmatrix} \begin{bmatrix} u_t^g \\ u_t^t \\ u_t^y \\ u_t^{\Delta p_t} \\ u_t^i \end{bmatrix} = \begin{bmatrix} b_{11} & 0 & 0 & 0 & 0 \\ b_{21} & b_{22} & 0 & 0 & 0 \\ 0 & 0 & b_{33} & 0 & 0 \\ 0 & 0 & 0 & b_{44} & 0 \\ 0 & 0 & 0 & 0 & b_{55} \end{bmatrix} \begin{bmatrix} e_t^g \\ e_t^t \\ e_t^1 \\ e_t^2 \\ e_t^3 \end{bmatrix}$$

where e_t^i $(i = 1, 2, 3)$ are non-fiscal shocks and have no structural interpretation. Since a_{gy}, $a_{g\Delta p}$, a_{gi}, a_{ty}, $a_{t\Delta p}$ and a_{ti} are identified using external information [4], there are only 15 parameters to be estimated. As there are also 15 different elements in the variance-covariance matrix of the 5-equation VAR innovations, the model is just identified. The e_t^i $(i = 1, 2, 3)$ are derived by imposing a recursive scheme on the bottom three rows of $\mathbf{A}$ and $\mathbf{B}$; however, the identification of the two fiscal shocks–the only ones that we shall use to compute impulse responses–is independent of this assumption. Finally, the identification assumption imposes $b_{12} = 0$. [5]

Although we use the same identifying assumptions, our choice of variables differs slightly from those used in B&P, because, as discussed above, we need to use variables that allow the debt dynamics equation to track the path of d_t accurately. In particular, our measure of i is the average cost of debt financing rather than the yield to maturity on long-term government

[3] Mountford and Uhlig (2002) identify government spending and revenue shocks by imposing restrictions on the sign of impulse responses. Fatas and Mihov (2001) rely on a simple Choleski ordering.

[4] The elasticities of taxes and government spending with respect to output, inflation and interest rates used in the identification have been updated in Perotti (2007) and are

Elasticities of government revenues and expenditures

	a_{gy}	$a_{g\Delta p}$	a_{gi}	a_{ty}	$a_{t\Delta p}$	a_{ti}
Entire sample	0	-0.5	0	1.85	1.25	0
1960:1-1979:4	0	-0.5	0	1.75	1.09	0
1980:1-2006:2	0	-0.5	0	1.97	1.40	0

[5] B&P provide robustness checks for this assumption by setting $b_{21} = 0$ and estimating b_{12}. We have also experimented with this alternative option. In practice, as the top left corner of the $\mathbf{B}$ matrix is not statistically different from a diagonal matrix, the assumption $b_{12} = 0$ is irrelevant to determine the shape of impulse response functions.

bonds used in B&P. Our definitions of g and t are also slightly different: we follow the NIPA definitions by considering net transfers as part of government expenditure, rather than subtracting them from taxes.

To check that our slight differences in data definitions do not change the results we have first estimated (1) as in B&P. Following Perotti (2007) who finds differences in the impulse response functions before and after 1980, the sample is split in two sub-samples 1960:1-1979:4 and 1980:1-2006:2. The impulse responses are reported in Figures A1 and A2 in the Appendix and are consistent with those reported in B&P. In particular:

- an (exogenous) increase in public expenditure has an expansionary effect on output, while an (exogenous) increase in revenues is contractionary. The impact of fiscal policy weakens in the second sub-sample, in particular the effects of tax shocks become insignificant;
- after 1980 fiscal shocks become less persistent;
- the effect of fiscal shocks on interest rates is insignificant in the first subsample; it is small, significant but counterintuitive in the second subsample when an increase in public spending lowers the cost of servicing the debt;
- fiscal shocks have consistently no significant effect on inflation.

4.1 The debt dynamics implied by a standard SVAR

To assess the importance of omitting d, we start with a simple exercise. After having estimated the parameters $\mathbf{C}_i$ in (1) we use the identity which describes debt accumulation to simulate the system out-sample for 80 quarters starting from the conditions prevailing in the last observation of the estimation period. The path for d_t so constructed reveals the steady state properties of the estimated empirical model.

When (1) is estimated over the first sub-sample (1960:1-1979:4) the simulated out-of-sample path for d_t diverges (Figure 2). When (1) is estimated over the second sub-sample (1980:1-2006:2) the simulated debt ratio tends, eventually, to fall below zero.

This exercise naturally raises a number of questions:

- does the apparent instability depend on the underlying behavior of the government, or is it simply the result of a mis-specified model? Debt stabilization requires that the primary budget surplus reacts to the accumulation of debt, but such a reaction–if it were in the data–would not be captured by (1). Hence the simulated path may very well be the result of a mis-specification of the empirical model rather than a description of the actual behavior of the government;

- it is obviously difficult to interpret impulse response functions when they are computed along unstable paths for the debt ratio, as they would eventually diverge. An unstable dynamics becomes particularly problematic when the effects of fiscal shocks are computed over relatively long horizons, or when identification is obtained imposing long run restrictions on the shape of impulse responses. This is not the case in the B&P identification, that is achieved imposing restrictions on the simultaneous effects of fiscal policy shocks. However, the interpretation of the responses to shocks along an unstable debt path remains problematic;

- impulse response functions appear to differ over the two sub-samples. Does this depend on the different dynamics for the debt-to-GDP ratio implied by the SVAR estimated over the two sub-periods? In particular (1) often produces a puzzling response of interest rates to a fiscal shock. Consider for example the response to an expansionary fiscal shock over the first sub-sample. The path of the debt ratio eventually becomes explosive: how can this be reconciled with the evidence that the estimated response of i_t is small and insignificant?

- impulse responses are often used to discriminate between competing DSGE models, or to provide evidence on the stylized facts to include in theoretical models used for policy analysis. It is obviously impossible to compare the empirical evidence from a model that delivers an explosive path for the debt, with the paths of variables generated by forward looking models, since such models do not have a solution when the debt dynamics is unstable.

We now turn to the model described in (2).

4.2 Estimating the effects of fiscal shocks in a SVAR with debt dynamics

The identification problem does not change when the debt level is included in the model. Since we treat the debt-deficit relationship as an identity, the number of shocks remains the same, so that the identification assumptions discussed in the previous section remain valid. Also, since there are no parameters to be estimated in the debt-dynamics equation, (2) can be estimated excluding that equation. The identified system is therefore

$$\begin{aligned} \mathbf{Y}_t &= \sum_{i=1}^{k} \mathbf{C}_i \mathbf{Y}_{t-i} + \sum_{i=1}^{k} \boldsymbol{\gamma}_i d_{t-i} + \mathbf{A}^{-1}\mathbf{B}\mathbf{e}_t \\ d_t &= \frac{1+i_t}{(1+\Delta p_t)(1+\Delta y_t)} d_{t-1} + \frac{\exp(g_t) - \exp(t_t)}{\exp(y_t)} \end{aligned} \tag{3}$$

Table 1 reports the estimated coefficients of the first and the second lags of d_t in the five equations (taxes, spending, output, inflation and the cost of debt service) in the two sub-samples.

[INSERT TABLE 1 HERE]

In all equations the restriction that the two coefficients are of equal magnitude and of opposite sign cannot be rejected, suggesting that the five variables respond to the lagged change in the debt ratio. The last two rows in the Table report the coefficients (and their standard errors) when this restriction is imposed. For instance, government spending is reduced when the lagged change in the debt ratio is positive. $(d_{t-1} - d_{t-2})$ measures the gap between the actual primary surplus (as a fraction of GDP) and the surplus that would stabilize d: the magnitude of the coefficient indicates that the gap between the surplus that would stabilize the debt ratio and the actual surplus acts as an error correction mechanism in the fiscal reaction function: current expenditures are decreased when last period's primary surplus was below the level that would have kept the debt ratio stable.

The response of g_t to a change in the debt-ratio is significant after 1980, not before. Taxes do not respond significantly to a change in the debt ratio, however the difference between the point estimates between the two subperiods is close to being significant and the response is stabilizing only after 1980. The average interest cost of the debt also depends on the gap between the actual surplus and the debt stabilizing surplus. This result is particularly strong in the second sub-sample. Finally, the direct effect of lags in d_t on inflation and output is never significant in any of the samples.

Summing up. Before 1980 U.S. fiscal policy does not seem to have been aimed at stabilizing the debt-to-GDP ratio: this probably reflects the will of the fiscal authorities to reduce the debt ratio from the high initial level inherited after World War II. Only after 1980 does fiscal policy become stabilizing. Using the coefficients estimated up to 1980 to simulate the effects of a fiscal policy shock is thus inappropriate, since such a shock would put the debt ratio on a diverging path, while the coefficients have been estimated on a sample characterized by a decreasing debt ratio.

The results in Table 1 raise a question. We argued that (1) is mis-specified because it overlooks the possibility that fiscal policy reacts to the level of the debt ratio. In other words, the mis-specification would arise from the omission of a low-frequency variable. But according to Table 1 what matters is the change in the debt ratio, thus a high-frequency variable. Does this make the omission of d_t irrelevant? No, for the following reason. The first difference of d_t is itself a (non-linear) function of d_t. Differencing the debt dynamics equation we obtain

$$\Delta d_t = \frac{(i_t - \Delta p_t - \Delta y_t - \Delta y_t \Delta p_t)}{(1 + \Delta p_t)(1 + \Delta y_t)} d_{t-1} + \frac{\exp(g_t) - \exp(t_t)}{\exp(y_t)} \tag{4}$$

the change in the debt ratio is equal to the difference between the actual surplus-to-GDP-ratio and the ratio that would keep the debt stable–which is a function of the level of the debt. Hence, the change in debt ratio depends on the level of the debt via a time-varying relationship–because the first term on the right hand side of (4)–the ratio of the average cost of debt financing to nominal GDP growth–varies over time. Figure 3 shows that this time variation is empirically relevant over the sample we consider. In other words, our empirical model is an error correction model consistent with cointegration between the primary surplus and the debt-stabilizing surplus.[6] Therefore,

[6] This cointegrating relation is different from those experimented in standard SVAR's. In particular, the cointegrating relation implied by (4) is different from the cointegrating relation between g_t and t_t, with a cointegrating vector $(1, -1)$, proposed in their robustness check by B&P. This could explain why estimating a cointegrated model, or a simple model specified in first differences, makes no substantial difference for the evidence reported by

including the change in d in a VAR is virtually equivalent to augmenting the VAR with a time-varying function of the level of the debt-to-GDP ratio, that is indeed a slow moving variable[7].

Computing impulse responses

The presence of the intertemporal budget constraint makes computing the responses of the variables in $\mathbf{Y}_t$ to innovations in $\mathbf{e}_t$ different from computing impulse responses in a standard VAR. Impulse responses comparable to those obtained from the traditional moving average representation of a VAR can be constructed going through the following steps:

- generate a baseline simulation for all variables by solving (3) dynamically forward (this requires setting to zero all shocks for a number of periods equal to the horizon up to which impulse responses are needed),
- generate an alternative simulation for all variables by setting to one–just for the first period of the simulation–the structural shock of interest, and then solve dynamically forward the model up to the same horizon used in the baseline simulation,
- compute impulse responses to the structural shocks as the difference between the simulated values in the two steps above. (Note that these steps, if applied to a standard VAR, would produce standard impulse responses. In our case they produce impulse responses that allow for both the feedback from d_{t-i} to $\mathbf{Y}_t$ and for the debt dynamics),
- compute confidence intervals.[8]

B&P. Of course, if the debt stabilizing surplus were stationary, the data would support–up to a logarithmic transformation–the cointegrating vector in B&P, but the long-run solution of their cointegrating system would still be different from the one implied by a system in which there is tight relation between the actual surplus and the debt stabilizing surplus. The cointegrating relation implied by (4) is also different from the error correction model proposed in Bohn (1988): Bohn includes the level of the debt ratio in the fiscal reaction function but does so without allowing for the time variation of the coefficient multiplying the debt level.

[7]As a robustness check we have re-run our SVAR augmenting it with the debt stabilizing surplus-to-GDP ratio lagged once and twice. The coefficients on the two lags where equaly signed and their sum was not statistically different from the coefficient on the first difference of d, our proposed model.

[8]Bootstrapping requires saving the residuals from the estimated VAR and then iterating

We now turn to the results.

Debt dynamics in a model with feedbacks

Figure 4 reports out-sample simulations of d_t obtained from (2). In the second sub-sample, allowing $\mathbf{Y}_t$ to respond to past debt growth stabilizes the path of d_t. This is not the case in the first sub-sample–not surprisingly, since we have found, in Table 1, that the feedbacks from d_t to g_t and t_t only start being significant after 1980.

Thus omitting a feedback from the debt level to fiscal policy can result in impulse responses to fiscal shocks that are based on biased estimates and are computed along implausible paths for the debt ratio. Whether including such a feedback is sufficient to produce stable debt paths obviously depends on the size of the feedbacks. If they are too small–as they were in the U.S. up to the early 1980's–unstable paths will not be eliminated.

The effects of fiscal shocks in a model with feedbacks

Figures 5.1 and 5.2 compare the impulse responses obtained from (2) with those obtained in a SVAR without a debt feedback. In both cases we use the same identifying assumptions. Figure 5.1 refers to the first sub-sample, 1960:1-1979:4; Figure 5.2 to the later period. In each figure the left-hand panels refer to a one percent shock to g; the right-hand side panels refer to an equivalent shock to t. In each column the graphs show, from top to bottom, the impulse response of g, t, y, inflation, and the average cost of debt service. The reported confidence bounds are for the impulse responses without a debt feedback.

Pre-1980, when fiscal policy does *not* respond to d

- following a positive shock to g, allowing for a debt feedback results in a larger response of interest rates and inflation (outside the 95% confidence bounds). For interest rates the divergence widens over time, as debt accumulates,

the following steps: a) re-sample from the saved residuals and generate a set of observation for $\mathbf{Y}_t$ and d_t, b) estimate the VAR and identify strucutral shocks, c) compute impulse responses going thorough the steps described in the text, d) go back to step 1. By going thorugh 1,000 iterations we produce bootstrapped distributions for impulse responses and compute confidence intervals.

- following a positive shock to t, interest rates fall more in the model with feedbacks and the difference also widens over time,
- the output effects of shocks to g and t are larger in the model with a debt feedback.

Post-1980, when fiscal policy is stabilizing

- following positive t shock output rises. In the model without a debt feedback the effect on output of a shock to t is never statistically significant. The larger increase in output in the model with a debt feedback is partly explained by the response of spending to a tax shock: g initially falls as taxes rise, but eventually it rises–a feature of the stability of fiscal policy in this sub-sample,
- g shocks are less persistent in the model with a feedback and t responds offsetting g shocks–again a feature of stability,
- the response of interest rates to a positive g shock is still negative at the beginning, but rises over time in the presence of a feedback,
- following a shock to t interest rates rise more in the presence of a feedback, mirroring the larger increase in y.

Table 2 complements the result in Figures 5 by computing the cumulative response of interest rates and output to a fiscal shock over three horizons, (4, 12 and 20 quarters) and comparing them with the responses in the absence of a debt feedback. The effect of a 1% g shock on interest rates, cumulated over 20 quarters, in the first sub-sample, is 0.118 in the model with feedback, 0.032 without: the larger reaction of interest rates to a fiscal shock is consistent with the finding that in the first sub-sample fiscal policy is not stabilizing. This is confirmed by the observation that the differences in the cumulated responses of interest rates vanish in the second sub-sample where fiscal policy is stabilizing. The expansionary effect of a tax increase in the second-subsample is confirmed by the cumulated responses. Following a 1% increase in taxes output rises (over a 20 quarters horizon) by 0.288 in the model with feedback, as opposed to 0.170 in the model without a feedback.

[INSERT TABLE 2 HERE]

5 Fiscal shocks identified from the narrative record

Romer and Romer (2007) (R&R in what follows) use the U.S. narrative record–presidential speeches, executive-branch documents, and Congressional reports–to classify the size (defined as the estimated revenue effect of a new tax bill), timing, and principal motivation for all major postwar tax policy actions.[9] They then identify, among all documented tax actions, those that could be classified as "exogenous", as opposed to those that were countercyclical, *i.e.* motivated by a desire to return output growth to normal. Exogenous tax changes are further divided into two groups: those that appear to be motivated by a desire to raise the potential growth rate of the economy, and those aimed at reducing a budget deficit inherited from previous administrations.

Since 1947 U.S. Federal laws changed taxes in 82 quarters. A number of these quarters had tax changes of multiple types. Among the 104 separate quarterly tax changes identified, 65 are classified as exogenous. In this Section we use these 65 tax changes (the R&R exogenous tax shocks) and ask what difference it makes if the debt channel is, or is not, included in the transmission mechanism.

R&R estimate the impact of tax shocks on output using a single-equation approach:

$$\Delta y_t = \beta_0 + \sum_{i=1}^{12} \beta_i \frac{\Delta T_{t-1}^{ex}}{Y_{t-1}} + \sum_{j=1}^{k} \gamma_j Z_{t-j} + e_t \tag{5}$$

where Δy_t is real quarterly output growth, $\frac{\Delta T_{t-1}^{ex}}{Y_{t-1}}$ are the tax shocks, measured as a percent of nominal GDP, and Z_{t-j} are controls (lags of Δy_t, monetary policy shocks, government spending, oil prices). The $Z's$ are assumed to be exogenous, and in particular unaffected by the tax shocks, not even with a lag. The R&R exercise should thus be interpreted as asking the following (hypothetical) question: Assume that the transmission mechanism

[9] Early attempts at applying to fiscal policy the methodology proposed by Romer and Romer (1989) to identify monetary policy shocks were Edelberg, Eichenbaum and Fisher (1999), Burnside, Eichenbaum and Fisher (2004), Ramey (2006). These papers used a dummy variable which identifies characterizes episodes of significant and exogenous increases in government spending (typically wars).

of tax shocks is shut down and that such shocks only affect output directly, rather than, for instance, also via their effect on interest rates. What is their effect on output under this hypothesis? R&R find that "exogenous" tax increases have a larger negative effect on output than countercyclical tax hikes. Among the exogenous tax increases, those motivated by the aim to rein in a budget deficit are less contractionary.–in fact the negative impact on output is statistically insignificant.

To estimate the effects of the R&R tax shocks when fiscal policy is allowed to respond to the level of the debt we first need to embed these shocks in a model that doesn't shut down the transmission mechanism. We do this using the R&R shocks in the two VAR's analyzed above: (1) and (2).[10] Therefore, we estimate the following two models:

$$\mathbf{Y}_t = \sum_{i=1}^{k} \mathbf{C}_i \mathbf{Y}_{t-i} + \delta_i \frac{\Delta T_t^{ex}}{T_t} + \mathbf{u}_t \tag{6}$$

$$\begin{aligned} \mathbf{Y}_t &= \sum_{i=1}^{k} \mathbf{C}_i \mathbf{Y}_{t-i} + \sum_{i=1}^{k} \boldsymbol{\gamma}_i d_{t-i} + \delta_i \frac{\Delta T_t^{ex}}{T_t} + \mathbf{u}_t \\ d_t &= \frac{1+i_t}{(1+\Delta p_t)(1+\Delta y_t)} d_{t-1} + \frac{\exp(g_t) - \exp(t_t)}{\exp(y_t)} \end{aligned} \tag{7}$$

where the variables in $\mathbf{Y}$ are, as before, taxes, government spending, output, inflation and interest rates.

Including the R&R tax shocks in a VAR is a natural way of computing the dynamic response of macro variables to shocks identified outside the VAR because what matters are the impulse responses generated by the different shocks, not the correlation of the shocks themselves. [11] The R&R shocks are valid shocks to taxes because we find that they are uncorrelated with all lags of the variables included in the VAR's and are significant only in the equation

[10] R&R scale their shocks by the level of GDP. We scale them by taxes to allow direct comparability of the effects of these shocks with those identified in a SVAR. In a SVAR tax shocks are extracted from a specification in the logarithms of the levels of real variables. Innovations thus have the dimension of a percentage change in taxes. A one per cent change in taxes is much smaller than a one per cent shock in the tax-to-GDP ratio. The re-scaling affects the size of the effects but not the shape of the impulse responses.

[11] VAR's have been used to compute impulse responses to shocks identified outside the VAR in the analysis of the effects of monetary shocks in Bagliano and Favero (1999).

for t. Thus they satisfy the properties that exogenous shocks identified in a structural VAR should fulfill.

Figure 6 shows the impulse response of output to an exogenous R&R tax shock equivalent to 1% of taxes. Impulse responses are computed using three different models:

- (5), the equation estimated by R&R where we have replaced $\frac{\Delta T^{ex}_{t-1}}{Y_{t-1}}$ with $\frac{\Delta T^{ex}_{t-1}}{T_{t-1}}$,
- (6), a VAR that excludes a debt feedback
- (7), a model that allows the variables in the VAR to respond to the level of the debt.

The R&R shocks start in 1947, while our data, for the reasons noted in footnote 2, only start in 1950:1: we thus miss the exogenous shocks that occurred between January 1947 and December 1949. As in the previous Section we split the sample in two parts: 1950:1-1979:4 and 1980:1-2006:2.

The effects on output of the exogenous R&R tax shocks are quite different in the two sub-samples and depending on the model they are embedded in. In the first sub-sample (1950:1-1979:4) the contractionary. effect of a tax hike is larger when Z is endogenized in a model that includes the level of the debt and the government intertemporal budget constraint. This probably happens because–as documented in the previous Section–debt stabilization does not appear to have been a concern for the U.S. fiscal authorities in the first part of the sample. A tax increase thus did not call for a compensating change in the budget. Fiscal shocks could cumulate over time amplifying the effect on output of an initial shock. This may explain why tax hikes have larger effects in the models that allow the variables in Z to respond to the shock.

In the second sub-sample, when fiscal policy becomes stabilizing—-a positive shock to taxes is compensated by a subsequent fiscal accommodation. This explains why, analyzing the effects of shocks in a model where Z is endogenous and fiscal policy responds to the debt level, produces much smaller output effects compared with the R&R single equation model. Figure 7 shows that in fact, in the second sub-sample, an initial positive tax shock is accompanied by further tax changes in the opposite direction. Following the initial shock taxes fall: when this happens the effect on the budget is compensated by increases in spending. These responses are not captured in (5) because the equation sets to zero the dynamic response of all variables, with the only exception of output growth, to tax shocks.

6 Conclusions

We have analyzed the effects of fiscal shocks allowing for a direct response of taxes, government spending and the cost of debt service to the level of the public debt (as a ratio to GDP). We have shown that omitting such a feedback can result in incorrect estimates of the dynamic effects of fiscal shocks. We suggested in particular that the absence of an effect of fiscal shocks on long-term interest rates–a frequent finding in research based on Vector Autoregressions that omit a debt feedback and do not endogenize debt dynamics–can be explained by their mis-specification, especially over samples in which the debt dynamics appears to be unstable.

The methodology described in this paper to analyze the impact of fiscal shocks by taking into account the stock-flow relationship between debt and fiscal variables could be extended to other dynamic models which include similar identities. For instance, the recent discussions on the importance of including capital as a slow-moving variable to capture the relation between productivity shocks and hours worked (see e.g. Christiano et al, 2005 and Chari et al. 2005) could benefit from an estimation technique that tracks the dynamics of the capital stock generated by the relevant shocks. The same applies to open economy models that study, for instance, the effects of a productivity shock on the current account (see e.g. Corsetti et al., 2006) and that typically omit a feedback from the stock of external debt on macroeconomic variables.

This approach could also be used in the analysis of the effects of fiscal shocks on debt sustainability, an issue which cannot be addressed in the context of a VAR that fails to keep track of the debt dynamics. Stochastic simulations of (2) could also be used to evaluate the sustainability of current systematic fiscal policy and to compute the risk of an unstable debt dynamics implied by the current policy regime.

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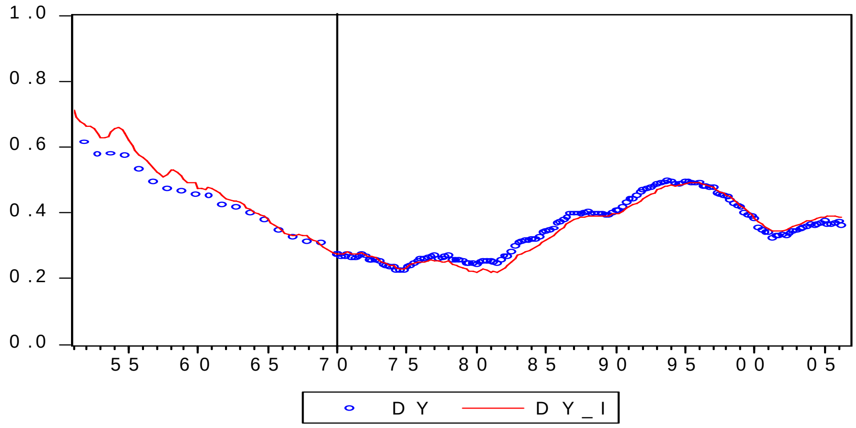

Figure 1: Actual (DY) and simulated (DY_I) (dynamically backward and forward starting in 1970:1) debt-GDP ratio. Actual data are observed at quarterly frequency from 1970 onwards and at annual frequency from 1970 backward. The simulated data are constructed using the government intertemporal budget constraint (2) with observed data and initial conditions given by the debt-to-GDP ratio in 1970:1.

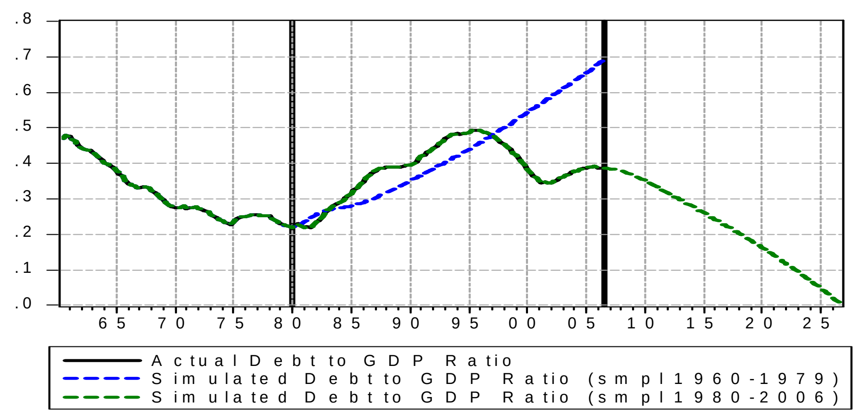

Figure 2: Actual and simulated (out of sample) debt-to-GDP ratio starting from conditions in 1980:1, and in 2006:2 respectively. Simulations are based on (1) .

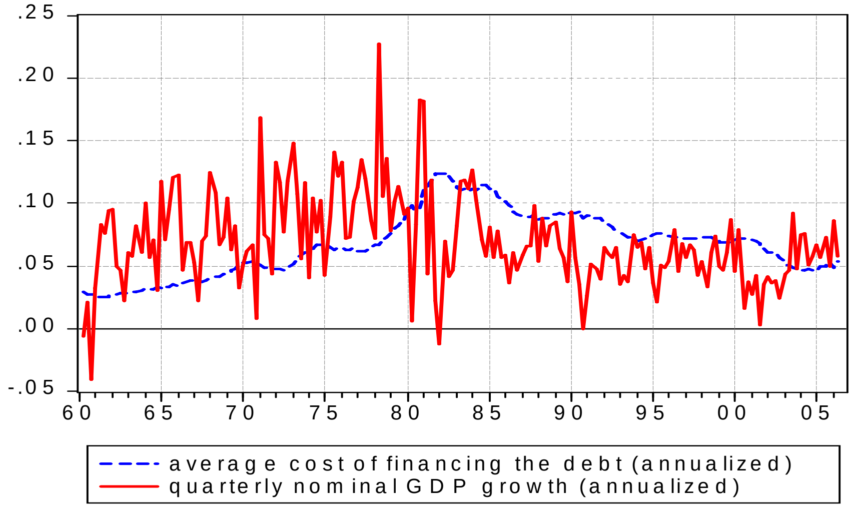

Figure 3: Average cost of debt financing and quarterly (annualized) nominal GDP growth

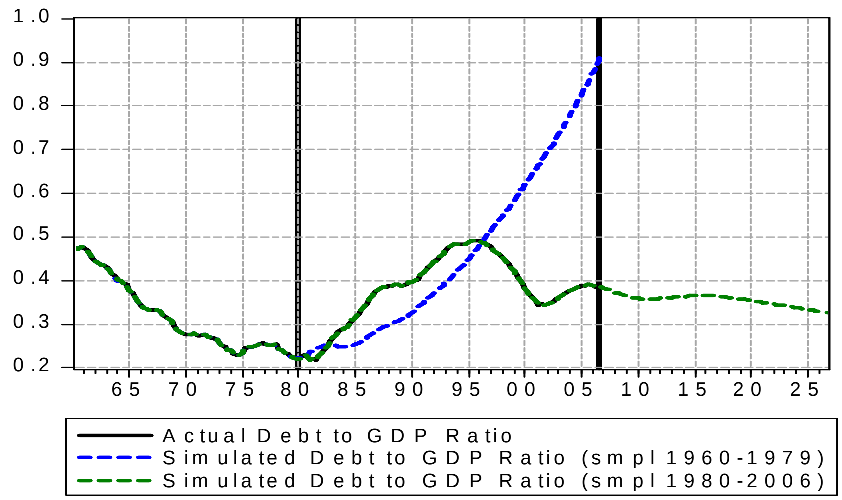

Figure 4: Actual and simulated out-of sample debt-GDP dynamics (starting from conditions in 1980:1, and in 2006:2 respectively). Simulations are based on (2).

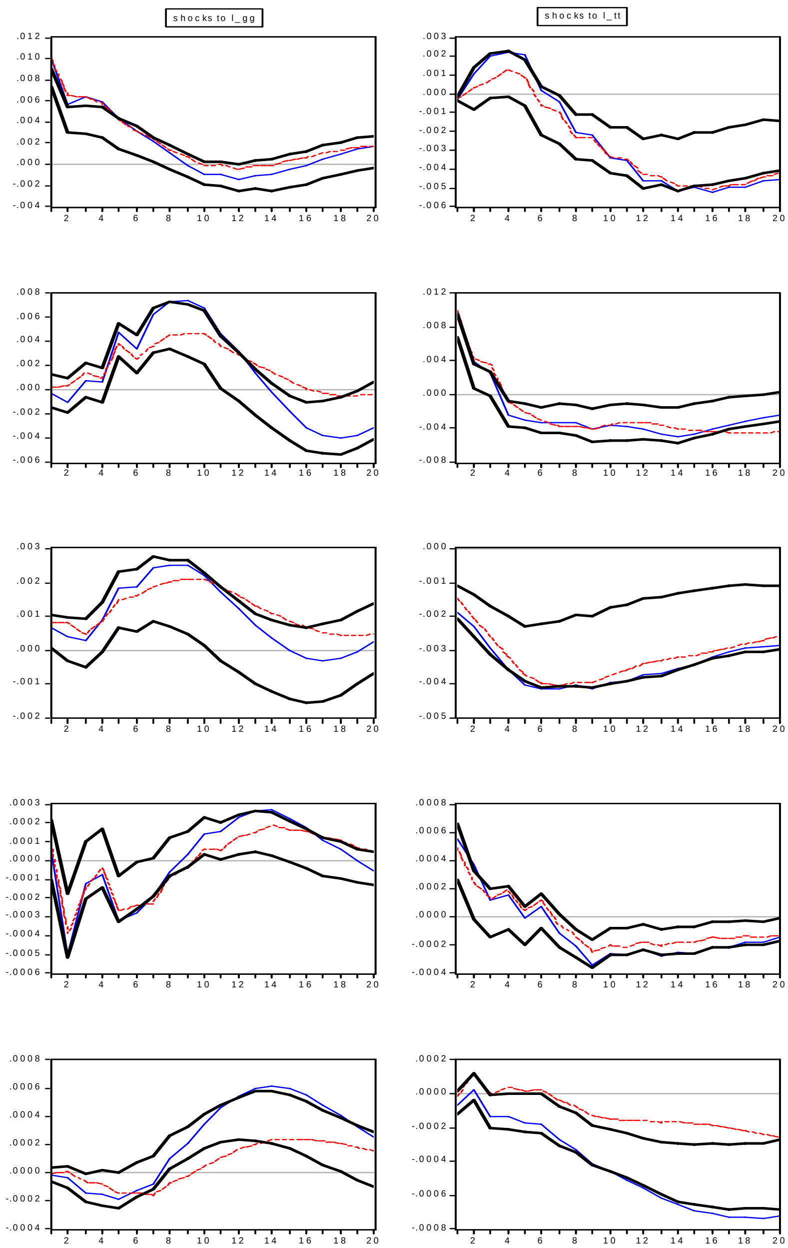

Figure 5.1: Fiscal shocks identified from a SVAR (dotted line) and in model with feedbacks (solid line). Sample 1960:1 1979:4. The first column shows responses to shocks to g_t; the second column to shocks to t_t.The responses reported along the rows refer, respectively, to the effects on g_t, t_t, y_t, Δp_t, i_t.

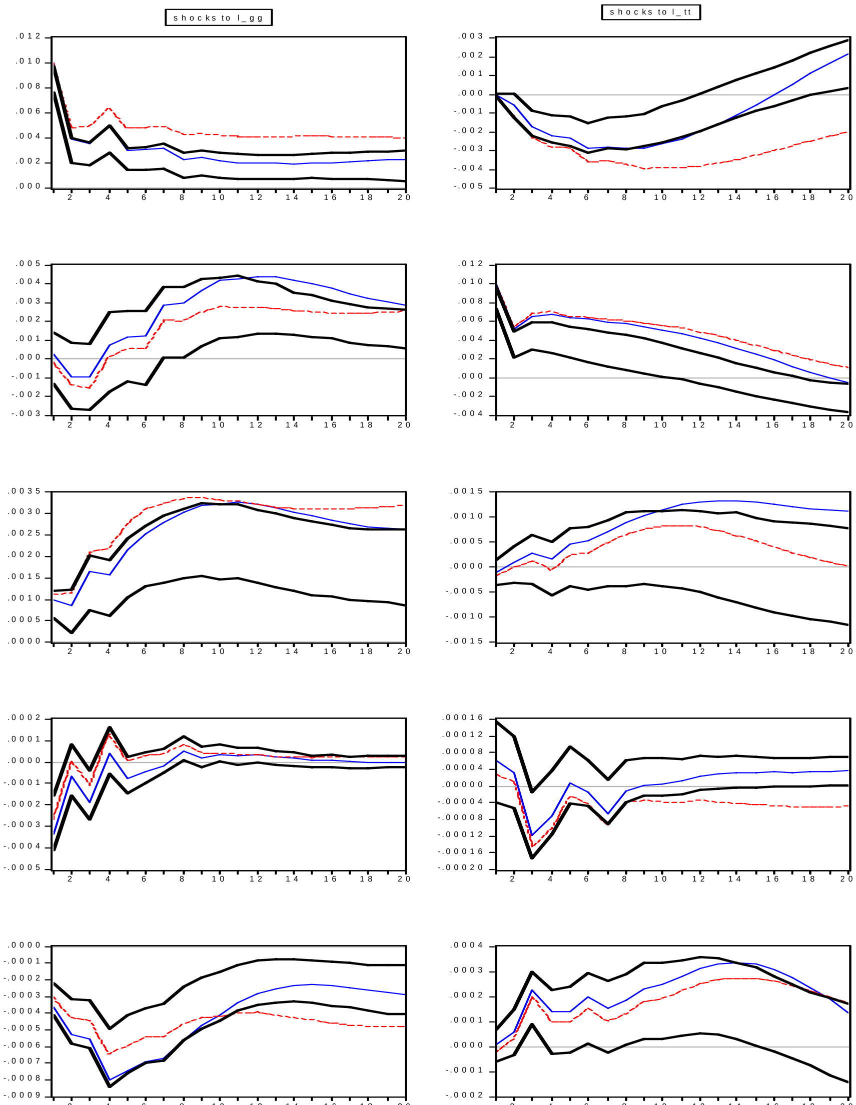

Figure 5.2: Fiscal shocks identified from a SVAR (dotted line) and in model with feedbacks (solid line). Sample 1980:1 2006:2. The first column shows responses to shocks to g_t; the second column to shocks to t_t.The responses reported along the rows refer, respectively, to the effects on g_t, t_t, y_t, Δp_t, i_t.

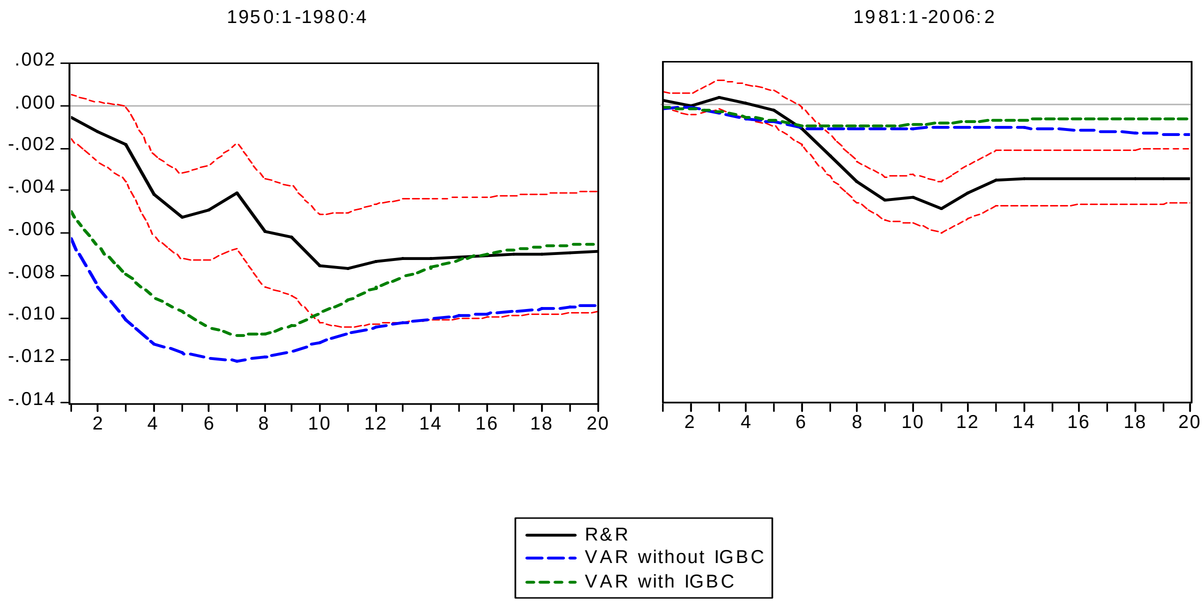

Figure 6: Using the Romer and Romer (2007) tax shocks. Effect on output in different models

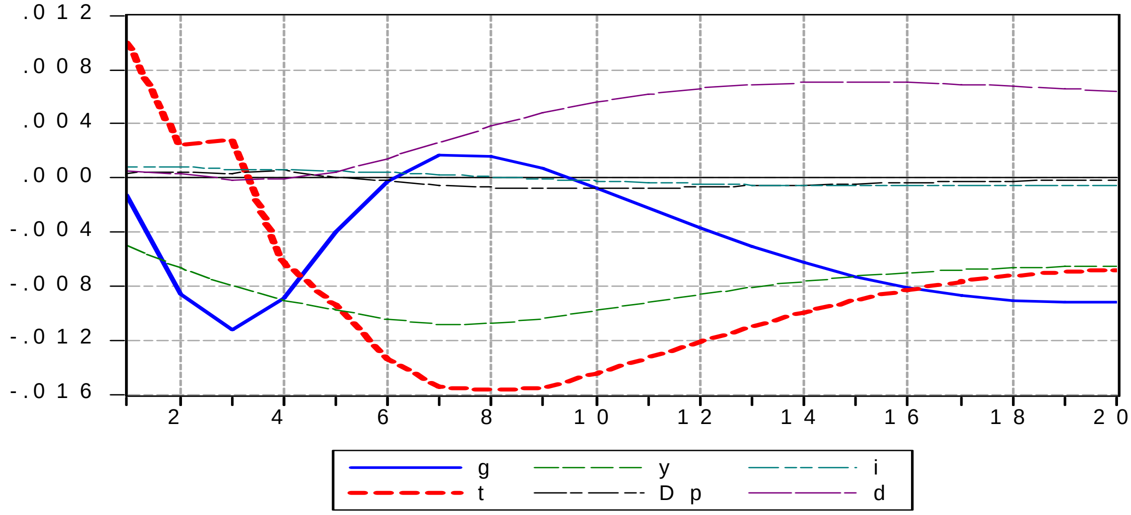

Figure 7: Dynamic response of all variables to an R&R tax shock, in a VAR with a debt feedback estimated over the sample 1950:1-1980:1.

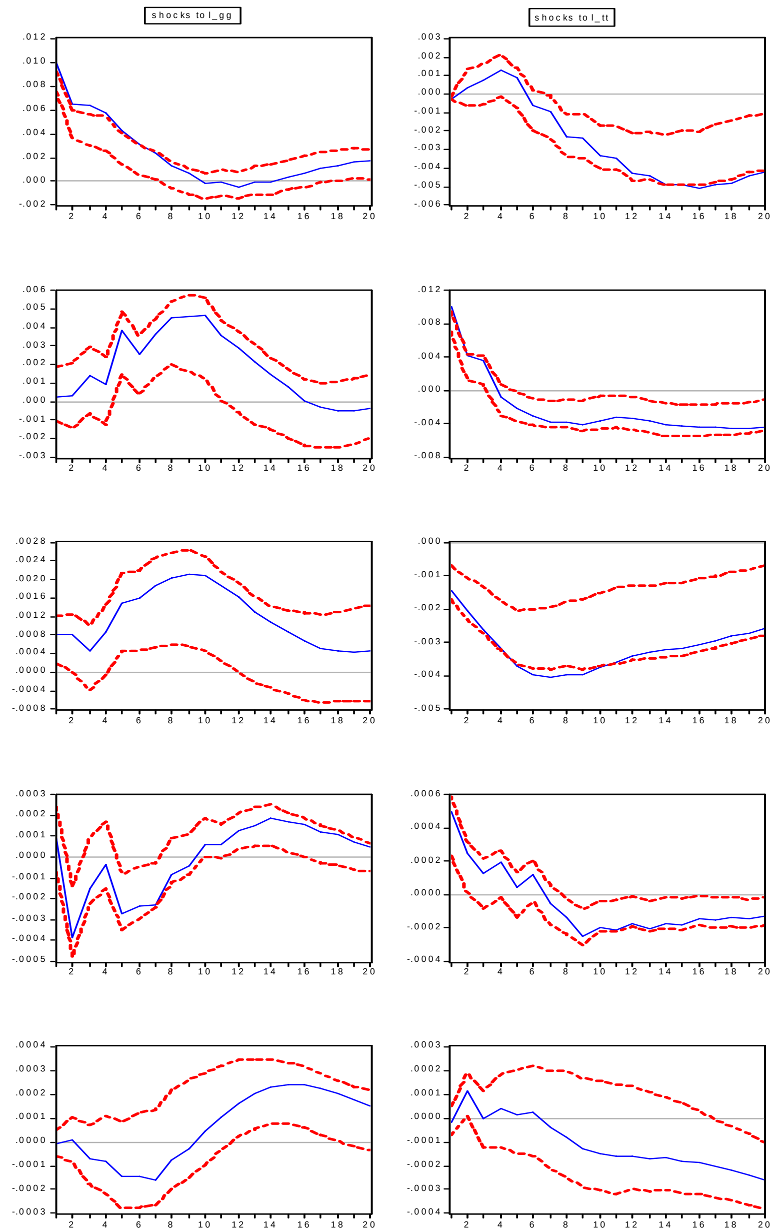

Figure A.1: Fiscal shocks identified from a SVAR::1960:1-1979:4. The first column shows responses to shocks to g_t; the second column to shocks to t_t.The responses reported along the rows refer, respectively, to the effects on g_t, t_t, y_t, Δp_t, i_t.

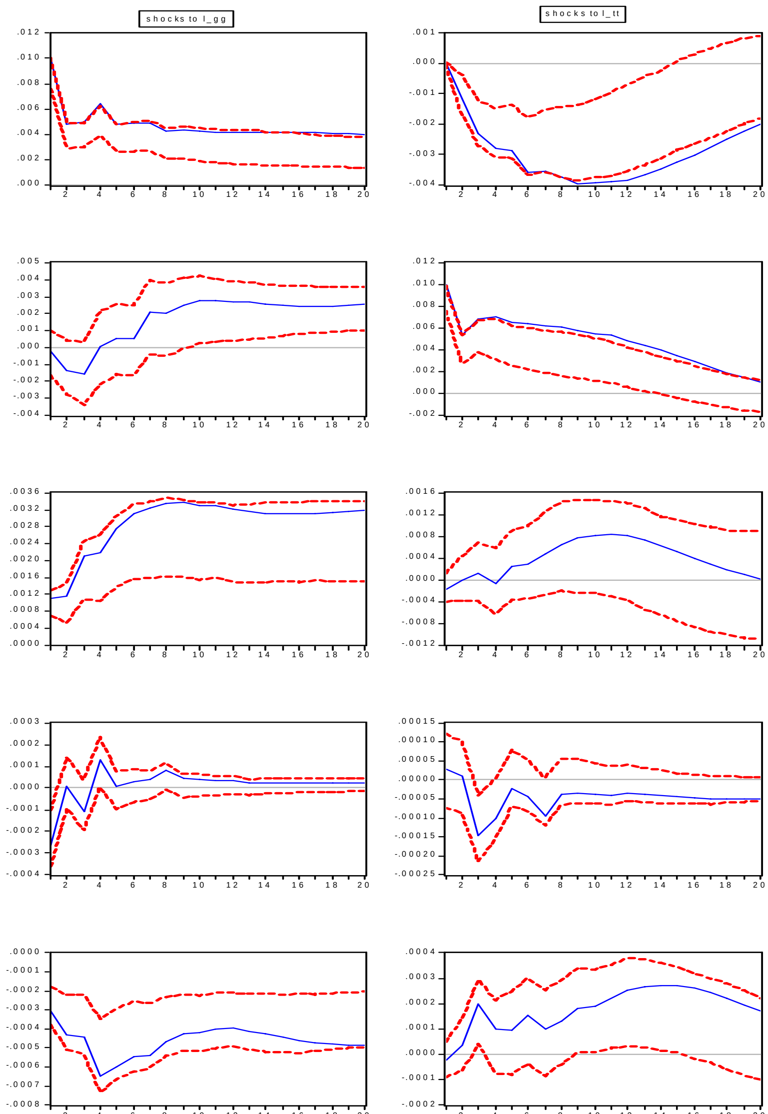

Figure A.2: Fiscal shocks identified from a SVAR: 1980:1 2006:2. The first column shows responses to shocks to g_t; the second column to shocks to t_t.The responses reported along the rows refer, respectively, to the effects on g_t, t_t, y_t, Δp_t, i_t.

Table 1	*Feedbacks from* d_{t-i} *(st. errors in parenthesis)*					
		g_t	t_t	y_t	Δp_t	i_t
d_{t-1}	*1960:1-1979:4*	-5.83 (5.14)	-3.55 (2.17)	-1.59 (2.17)	-0.88 (0.71)	0.079 (0.25)
	1980:1-2006:2	-3.94 (2.58)	1.63 (4.27)	0.83 (1.06)	0.13 (0.34)	0.62 (0.32)
d_{t-2}	*1960:1-1979:4*	5.90 (5.11)	4.18 (5.89)	1.75 (2.16)	0.87 (0.72)	-0.049 (0.25)
	1980:1-2006:2	3.82 (2.60)	-1.59 (4.30)	-0.85 (1.06)	-0.14 (0.34)	-0.63 (0.33)
$d_{t-1} - d_{t-2}$	*1960:1-1979:4*	-6.12 (5.04)	-6.07 (6.22)	-2.21 (2.19)	-0.84 (0.70)	-0.038 (0.27)
	1980:1-2006:2	-6.48 (2.50)	2.44 (3.97)	0.25 (0.99)	-0.12 (0.32)	0.56 (0.30)

Table 2

Cumulative responses of y and i to a g and a t shock

Cumulative responses (annualized) to g and t shocks equal to 1 per cent (annualized). Bootstrapped confidence intervals in brackets

	Horizon	*without debt feedback*				*with debt feedback*			
	quarters	*60:1-79:4*	*80:1-06:2*	*60:1-79:4*	*80:1-06:2*	*60:1-79:4*	*80:1-06:2*	*60:1-79:4*	*80:1-06:2*
		g *shock*		t *shock*		g *shock*		t *shock*	
y_t	4	0.073	0.164	−0.231	−0.004	0.056	0.127	−0.249	0.016
		(0.005 0.12)	(0.12 0.19)	(−0.32 −0.14)	(−0.08 0.06)	(−0.013 0.11)	(0.077 0.16)	(−0.35 −0.16)	(−0.07 0.06)
	12	0.440	0.805	−0.987	0.170	0.463	0.712	−0.994	0.288
		(0.17 0.60)	(0.55 0.84)	(−1.25 −0.55)	(−0.13 0.38)	(0.10 0.58)	(0.48 0.75)	(−1.31 −0.59)	(−0.18 0.34)
	20	0.585	1.431	−1.577	0.272	0.475	1.280	−1.590	0.654
		(0.06 0.85)	(0.95 1.50)	(−2.03 −0.83)	(−0.46 0.65)	(−0.12 0.73)	(0.77 .1.31)	(−2.11 −0.86)	(−0.48 0.57)
i_t	4	−0.004	−0.045	0.003	0.011	−0.009	−0.056	−0.007	0.016
		(−0.02 0.007)	(−0.07 −.0.02)	(−0.01 0.013)	(−0.005 0.02)	(−0.02 0.001)	(−0.07 −.0.04)	(−0.02 0.002)	(0.002 0.02)
	12	−0.010	−0.141	−0.013	0.058	0.022	−0.161	−0.075	0.081
		(−0.05 0.05)	(−0.20 −0.08)	(−0.06 0.05)	(0.004 0.10)	(0.001 0.52)	(−0.20 −0.09)	(−0.10 −0.34)	(0.02 0.11)
	20	0.032	−0.232	−0.054	0.125	0.118	−0.212	−0.205	0.160
		(−0.02 −0.10)	(−0.32 −0.14)	(−0.13 0.03)	(0.03 0.15)	(0.04 0.13)	(−0.29 −0.13)	(−0.26 −0.11)	(0.03 0.18)