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Debt and the Effects of Fiscal Policy

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Abstract:
A fiscal shock due to a shift in taxes or in government spending will, at some point in time, constrain the future path of taxes and spending, since the government's 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 arise 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 the absence of a debt feedback effect can result in incorrect estimates of the dynamic effects of fiscal shocks. In particular, omitting an effect of fiscal shocks on long-term interest rates—a frequent finding in studies that omit a debt feedback—can be explained by the misspecification of these fiscal shocks. Using data for the U.S. economy and two alternative identification assumptions, we reconsider the effects of fiscal policy shocks, correcting for these shortcomings. We close the paper by observing that the methodology described by taking into account the stock-flow relationship between debt and fiscal variables to analyze the impact of fiscal shocks could also be applied to other dynamic models that include similar identities. The inclusion of capital as a slow-moving variable in the study of the relationship between productivity shocks and hours worked is one example.

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1 Introduction

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

For example, consider 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 let the debt ratio grow in the meantime. It may even plan to use an 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 policy path the government chooses.

Yet the Vector AutoRegressive (VAR) models that are typically used to estimate the effects of fiscal shocks on various macroeconomic variables (such as output and private consumption) (i) fail to keep track of the debt dynamics that arise following a fiscal shock, and (ii) as the debt ratio evolves over time, these models overlook the possibility that future taxes and spending might respond to the debt level. In other words, following a fiscal shock, taxes and spending are assumed to shift in response to various macroeconomic variables, but not to the level of the public debt. This omission is particularly surprising for countries where the data reveal a positive correlation between the government surplus-to-GDP ratio and the government debt-to-GDP ratio, which indicates that fiscal variables do respond to the level of the debt. Bohn (1998) finds just such a correlation over a century of U.S. data.

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

One could argue that omitting the level of debt is not a problem because the VAR models typically estimated already include all the variables that enter the government's intertemporal budget constraint, thus determining the evolution of the debt over time. In other words, one might say that what is missing, at most, is an initial value for the debt. We show that this is not the case: failure to explicitly include the debt level in the estimated equation—and to keep track of its path when computing impulse responses—can result in biased estimates of how fiscal policy shocks affect macroeconomic 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 was positive and statistically significant in the 1960s and 1970s, but became insignificant in the 1980s and 1990s. 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 1980s, following a shock to spending or taxes, both fiscal policy instruments are adjusted over time in order to stabilize the debt ratio. This strategy does not appear to have been used in the 1960s and 1970s, when there is no evidence of a stabilizing fiscal policy response. This evidence suggets two reasons to explain the heterogeneity of impulse responses to fiscal shocks in the pre-1980 and the post-1980 samples. 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 a change in taxes or government spending, depending on whether they expect the government to meet its intertemporal budget constraint by adjusting taxes and/or spending in the future.

Our findings also relate to the evidence of non-linearity in the response of private consumption to fiscal shocks—documented by, among others, 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 argument we make about omitting the debt feedback effect is independent of the assumption adopted to identify fiscal shocks—whether the assumption is to impose enough constraints on a structural VAR (such as in Blanchard and Perotti (2002) or Mountford and Uhlig (2002)) or to identify shocks from the narrative record (as in 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, when 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 thesis by 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 the paper by observing that the methodology described by taking into account the stock-flow relationship between debt and fiscal variables to analyze the impact of fiscal shocks could also be applied to other dynamic models which include similar identities. The recent discussion on the importance of including capital as a slow-moving variable in order to capture the relation between productivity shocks and hours worked offers one example of this extension (for instance see Christiano, Eichenbaum, and Vigfusson (2005) and Chari, Kehoe and McGrattan (2005)).

2 Why standard fiscal policy VAR's are misspecified

Studying the dynamic response of macroeconomic variables to shifts in fiscal policy is typically done by estimating a VAR 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). However there are at least two reasons why this variable is an important factor in determining the effects of fiscal policy:

- A feedback effect from the level of the debt ratio to taxes and government spending is necessary for the debt's stability, unless the economy's growth rate is exactly equal to the average cost of financing the debt. Such a feedback effect is an observed feature of the data: Bohn (1998) finds that a century of U.S. data reveals 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 both on future expected monetary policy and on the risk premium. Each condition may be affected by debt dynamics. For instance, this may be the case 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 debt path that is stable or threatens 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 debt level. 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, which is the equation that determines how the debt ratio evolves over time. These observations naturally lead to replacing (1) with

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

where $\mathbf{Y}_t' = \left[\begin{array}{ccccc} g_t & t_t & y_t & \Delta p_t & i_t \end{array} \right]$. The debt-to-GDP ratio is d , the nominal rate of interest (the average cost of debt financing) is i, the log of real GDP is y, inflation is Δp, t and g are, respectively, (the logs of) government revenues and government expenditures net of interest [1]. Note that the presence of d_{t-i} amplifies the dynamic effect of fiscal shocks, which accumulate in (2), but not in (1); the difference between impulse responses computed using (2) versus (1) might thus increase as the time horizon increases.

Before discussing how fiscal policy shocks can be studied in the context of (2) , we pause and ask a question raised in the introduction. Since $\mathbf{Y}$

[1] We use logs because it is the log of output, taxes, and spending that generally enters into $\mathbf{Y}$.

already contains all the variables that enter the government's intertemporal budget constraint in (2), why is this not sufficient? Do we really need to insert the debt level directly? In short, 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 the short lags of g, t, Δp, Δy, and i that linearly enter (1) are unlikely to trace the evolution of the debt ratio accurately enough. To prove this claim, notice that d_t is the result of long and non-linear lag dynamics:

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

But the most convincing answer to these questions is to show that impulse responses computed using (2) differ from and produce different paths for d_t than those computed using (1). We show this using U.S. data and using two different ways to identify fiscal shocks that represent 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 the narrative approach used in Romer and Romer (2007). We start by describing our data.

3 The data

We begin by using quarterly data for the U.S. economy since 1960:1, the starting sample period analyzed in Blanchard and Perotti (2002) and extended to 2005:4 by Perotti (2007). Our approach requires that the debt-dynamics equation in (2) tracks the path of d_t accurately, thus we 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 7, 2006). Specifically, y_t is (the log of) real GDP per capita, while 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 7, 2006). Our measure for g_t is the log of real per capita primary government expenditure. Nominal expenditures are obtained by subtracting net interest payments at annual rates (obtained as the difference between line 28 and line 13, NIPA table 3.2) from total Federal Government Current Expenditure (line 39 in the same Table). Real per capita expenditure is then obtained by dividing the nominal variable by population multiplied by 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 of servicing the debt, i_t, is obtained by dividing net interest payments by the federal government debt held by the public (FYGFD-PUN 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 by the Social Security Trust Fund, but also other funds, such as Trust Fund for Unemployment Insurance, the Highway Trust Fund, and the pension fund of federal employees. The assets held by these trust funds consist of non-marketable debt.[2] 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 a fraction of GDP (shown as the dotted line). We have checked the accuracy

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

of the debt-dynamics equation in (2), simulating it forward from 1970:1 (the continuous line in Figure 1). The simulated series is virtually super-imposed 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 1950s.[3]

4 Fiscal shocks identified from SVARs

We start by comparing (2) with the structural VAR (SVAR) estimated in Blanchard and Perotti (2002) and extended in Perotti (2007).

SVARs identify fiscal shocks by imposing restrictions that allow the two structural fiscal shocks in (1) to be recovered from the reduced-form residuals, $\mathbf{u}$. The innovations in the reduced form equations for taxes and government spending, u_t^g and u_t^t, contain three terms: (i) the responses of taxes and government spending to fluctuations in macroeconomic variables, such as output and inflation, that are implied by the presence of automatic stabilizers; (ii) the discretionary response of fiscal policy to news in macroeconomic variables; and (iii) truly exogenous shifts in taxes and spending, which are the shocks that we wish to identify. Blanchard and Perotti (2002) exploit the fact that it typically takes longer than a quarter for discretionary fiscal policy to respond to changes 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 that

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

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 shocks to g and t by imposing on the $\mathbf{A}$ and $\mathbf{B}$ matrices in $\mathbf{Au} = \mathbf{Be}$ the following structure [4]:

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

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,[5] 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 $b_{12} = 0$.[6]

Although we use the same identifying assumptions, our choice of variables differs slightly from those used in Blanchard and Perotti (2002), because,

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

[5]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

[6]Blanchard and Perotti (2002) 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 determining the shape of impulse-response functions.

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 bonds used in Blanchard and Perotti (2002). 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 Blanchard and Perotti (2002). 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 Blanchard and Perotti (2002). 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 sub-sample. In the second sub-sample, when an increase in public spending lowers the cost of servicing debt, the effect on interest rates is small but significant, a result that is counterintuitive.
- Fiscal shocks are consistently shown to have 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 that 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. So constructed, the path for d_t 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 and observations:

- 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 these are computed along unstable paths for the debt ratio, as these will eventually diverge. Ustable dynamics become particularly problematic when the effects of fiscal shocks are computed over relatively long horizons, or when identification is obtained by imposing long-run restrictions on the shape of impulse responses. This is not the case in the identification by Blanchard and Perotti (2002) , which is achieved by 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 difference 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. For example, consider the response, over the first sub-sample, to an expansionary fiscal shock. 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 dynamic stochastic general equlibrium (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 are unstable.

To better address these questions, 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, and 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 while 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.

In all the equations, the restriction that the two coefficients are of equal magnitude and of opposite sign cannot be rejected, which suggests 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. The gap between the actual primary surplus (as a fraction of GDP) and the surplus that would stabilize d is measured as $(d_{t-1} - d_{t-2})$. 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, that is, current expenditures are decreased when the last period's primary surplus is 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, but not before. Taxes do not respond significantly to a change in the debt ratio; however, the difference between the point estimates for the two sub-periods is close to being significant, and the response is stabilizing only after 1980. The average interest cost of debt also depends on the difference 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.

In summary, before 1980, U.S. fiscal policy does not seem to have been aimed at stabilizing the debt-to-GDP ratio. This fact probably reflects the will of the government to reduce the debt ratio from the high initial level inherited after World War II. Only after 1980 does U.S. fiscal policy become stabilizing. Using the coefficients estimated up to 1980 to simulate the effects of a current fiscal policy shock is thus inappropriate, since such a shock would put the debt ratio on a diverging path, driven by the coefficients that have been estimated on a sample characterized by a decreasing debt ratio.

The results in Table 1 prompt a question. We argue 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, which is a high-frequency variable. However, the omission of d_t is still relevant for the following reason: the first difference of d_t is itself a (non-linear) function of d_t. Taking the first difference of 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 debt stable, which is a function of the level of the debt. Hence, the change in the debt ratio depends on the level of 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.[7] Therefore, 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, which indeed is a slow moving variable.[8]

[7]This cointegrating relation is different from those experimented with in standard SVARs. 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 the Blanchard and Perotti (2002) robustness check. This could explain why estimating a cointegrated model, or a simple model specified in first differences, makes no substantial difference for the evidence they reporte. Of course, if the debt-stabilizing surplus were stationary, the data would support—up to a logarithmic transformation—their cointegrating vector, but the long-run solution of their cointegrating system would still be different from the one implied by a system in which there is a 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 on the debt level.

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

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 by going through the following steps:

- Generate a baseline simulation for all variables by solving (3) dynamically forward (this requires setting all shocks to zero for the same number of periods as the length of the desired impulse-responses prediction range).

- Generate an alternative simulation for all variables by setting the structural shock of interest to one for the first period of the simulation, and then solving the model dynamically forward 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.[9]

We now turn to the results on impulse responses.

Debt dynamics in a model with feedback effects

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

[9] Bootstrapping requires saving the residuals from the estimated VAR and then iterating along the following steps: i) re-sample from the saved residuals and generate a set of observations for $\mathbf{Y}_t$ and d_t, ii) estimate the VAR and identify structural shocks, iii) compute impulse responses going thorough the steps described in the text, iv) go back to the first step. By going through 1,000 iterations, we produce bootstrapped distributions for impulse responses and compute confidence intervals.

path of d_t. This is not the case in the first sub-sample, though this is not surprising, since in Table 1 we have found that the feedbacks from d_t to g_t and t_t only become significant after 1980.

Thus, omitting 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 effect is sufficient to produce stable debt paths obviously depends on the size of the feedback effects. If they are too small—as they were in the United States up to the early 1980s—unstable debt paths will not be eliminated.

The effects of fiscal shocks in a model with feedbacks

Figures 5.a and 5.b compare the impulse responses obtained from (2) with those obtained in a SVAR without debt feedback effect. In both cases we use the same identifying assumptions. Figure 5.a refers to the first sub-sample, 1960:1–1979:4; Figure 5.b refers to 1980:1–2006:2 In each figure, the left-hand panels refer to a 1 percent shock to g; while the right-hand 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 95 percent confidence bounds are for the impulse responses without debt feedback effects.

Before 1980, when U.S. fiscal policy does *not* respond to d, we observe that:

- Following a positive shck to g, allowing for a debt feedback results in a larger response of interest rates and inflation (outside the 95 percent confidence bounds). For interest rates, the divergence widens over time, as debt accumulates, though it narrows again toward the end of the period.

- 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 that includes a debt feedback effect.

After 1980, when U.S. fiscal policy is stabilizing, we find that:

- Following a positive shock to t, 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: when taxes rise, g initially falls, but eventually it rises–a feature of the stability of fiscal policy in this sub-sample.

- In the model with a feedback effect, g shocks are less persistent, and t responds by offsetting g shocks. Again this effect is a feature of stability.

- The response of interest rates to a positive g shock is still negative at the beginning of the period, but rises over time in the presence of a feedback effect.

- Following a shock to t, interest rates rise more in the presence of a feedback effect, mirroring the larger increase in y.

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

5 Fiscal shocks identified from the narrative record

Romer and Romer (2007) 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 U.S. postwar tax policy actions.[10] They then identify, among all documented tax actions, those that could be classified as "exogenous," as opposed to those that were countercyclical, meaning 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 economy's potential growth rate, and those aimed at reducing a budget deficit inherited from previous administrations.

Since 1947, U.S. federal tax laws were changed in 82 quarters. A number of these quarters had multiple types of tax changes. Among the 104 separate quarterly tax changes identified, 65 are classified as exogenous. In this section, we use these 65 exogenous tax changes identified by Romer and Romer (2007), and ask what difference it makes if the debt channel is included in the transmission mechanism.

Romer and Romer (2007) estimate the impact of tax shocks on output using a single-equation approach:

[10] Early attempts at applying the methodology Romer and Romer (2007) use in order to identify monetary policy shocks were Edelberg, Eichenbaum, and Fisher (1999); Burnside, Eichenbaum, and Fisher (2004); and Ramey (2006). These papers used a dummy variable that identifies episodes of significant and exogenous increases in government spending (typically wars).

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

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, and oil prices). All the elements of Z are assumed to be exogenous, and in particular are unaffected by the tax shocks, even with a lag. The exercise by Romer and Romer (2007) should thus be interpreted as posing the following hypothetical question: if we assume that the transmission mechanism of tax shocks is removed, and that such shocks only affect output directly (rather than, for instance, also via their effect on interest rates), then what is their effect on output? Romer and Romer (2007) find that "exogenous" tax increases have a larger negative effect on output than do countercyclical tax hikes. Among the exogenous tax increases, those motivated by the goal to rein in a budget deficit are less contractionary—in fact, the negative impact on output is statistically insignificant in this case.

To estimate the effects of the exogenous Romer and Romer (2007) 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 does not shut down the transmission mechanism. We do this using the shocks identified by Romer and Romer (2007) in the two VARs analyzed above in equations (1) and (2).[11] Therefore, we estimate the following two models:

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

[11] Romer and Romer (2007) scale their shocks by the level of GDP. We scale these shocks by taxes to allow direct comparability of the effects of these shocks with those identified in an SVAR. In an SVAR, tax shocks are extracted from a specification in the logarithms of the levels of real variables. Innovations are thus measured as a percentage change in taxes. A 1 percent change in taxes is much smaller than a 1 percent shock in the tax-to-GDP ratio. This rescaling affects the size of the effects but not the shape of the impulse responses.

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

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

Including the Romers' exogenous tax shocks in a VAR is a natural way of computing the dynamic response of macroeconomic variables to shocks identified outside the VAR, since what matters are the impulse responses generated by the different shocks, not the correlation of the shocks themselves.[12] The Romers' exogenous shocks are valid shocks to taxes because we find that they are uncorrelated with all the lags of the variables included in the VARs and are significant only in the equation for t. Thus, these satisfy the properties that exogenous shocks identified in a structural VAR should fulfill.

Figure 6 shows the impulse response of output to an exogenous Romer and Romer (2007) tax shock equivalent to 1 percent of taxes. Impulse responses are computed using three different models:

- (5), the equation estimated by Romer and Romer (2007), in which we have replaced $\frac{\Delta T_{t-1}^{ex}}{Y_{t-1}}$ with $\frac{\Delta T_{t-1}^{ex}}{T_{t-1}}$.
- (6), a VAR that excludes a debt feedback effect.
- (7), a model that allows the variables in the VAR to respond to the debt level.

The shocks identified in Romer and Romer (2007) 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

[12] VARs 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).

1949. As in the previous section, we split the sample into two parts: 1950:1–1979:4 and 1980:1–2006:2.

The effects on output of the exogenous tax shocks are quite different in the two sub-samples, depending on the model in which they are embedded. 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's 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 period. Thus, a tax increase did not call for a compensating change in the budget. Fiscal shocks could accumulate over time, amplifying the effect of an initial shock on output. 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 a stabilizing force, a positive shock to taxes is compensated for by a subsequent fiscal accommodation. This explains why, when analyzing the effects of shocks in a model where Z is endogenous and fiscal policy responds to the debt level, the results produce much smaller output effects compared with the single-equation model used by Romer and Romer (2007). Figure 7 shows that 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, but the effect on the budget is compensated for by increases in spending. These responses are not captured in (5) because that equation sets the dynamic response to tax shocks of all variables, with the only exception being output growth, to zero.

6 Conclusions

We have analyzed the effects of fiscal shocks by allowing taxes, government spending, and the cost of debt service to directly respond to the level of the public debt (computed as a ratio to GDP). We have shown that omitting this feedback effect can result in incorrect estimates of the dynamic effects of fiscal shocks. In particular, we suggest 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 debt feedback and do not endogenize debt dynamics—can be explained by their mis-specification, especially over samples in which the debt dynamics appear to be unstable.

The method that we use 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 that include similar identities. For instance, the recent discussion on the importance of including capital as a slow-moving variable in order to capture the relation between productivity shocks and hours worked (see, for example, Christiano, Eichenbaum and Vigfusson (2005)) and Chari, Kehoe, and McGrattan (2005)), could benefit from an estimation technique that tracks the dynamics of the capital stock, as generated by the relevant shocks. The same methodology could apply to open-economy models that study, for instance, the effects of a productivity shock on the current account (see for instance Corsetti, Dedola, and Leduc (2006)) and that typically omit feedback from the stock of external debt on macroeconomic variables.

This approach could also be used in analyzing of the effects of fiscal shocks on debt sustainability, an issue that cannot be addressed in the context of a VAR that fails to keep track of the debt dynamics. Finally, stochastic simulations of (2) could 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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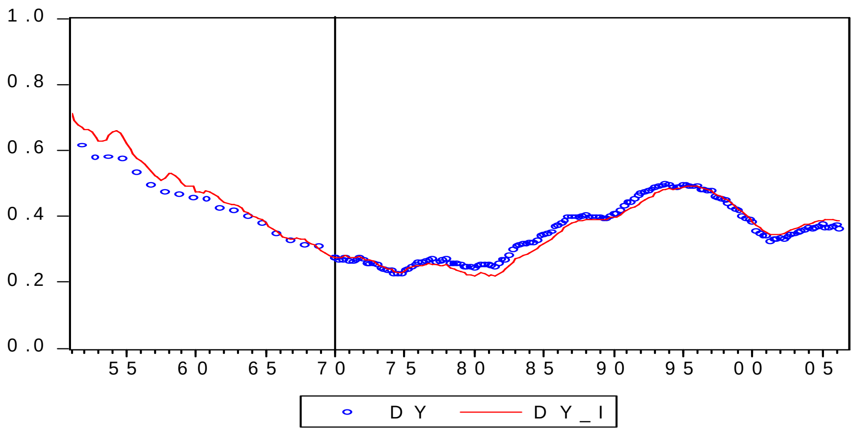

Figure 1: Actual (DY) and simulated (DY_I) (dynamically backward and forward starting in 1970:1) debt-GDP ratio. Actual data are observed at quarterly frequency from 1970 backward and at annual frequency from 1970 onward. The simulated data are constructed using the government intertemporal budget constraint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

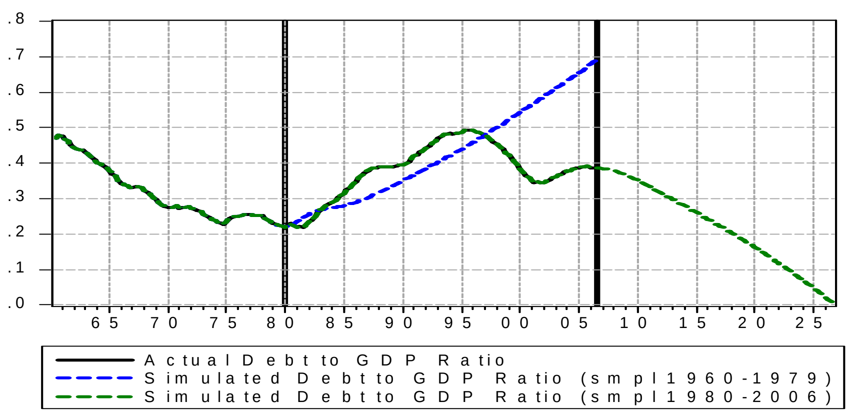

Figure 2: Actual and simulated (out-of-sample) debt-to-GDP ratio starting from conditions in 1980:1 and in 2006:2. Simulations are based on (1) .

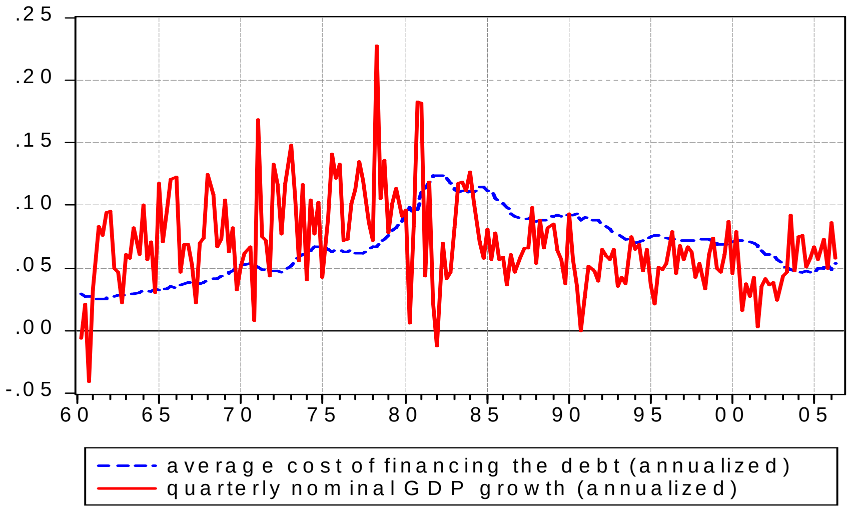

Figure 3: Average cost of debt financing and quarterly (annualized) nominal GDP growth.

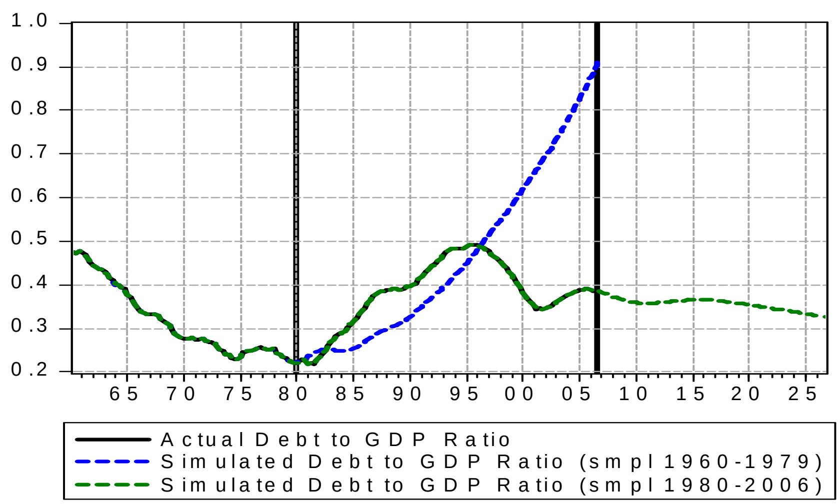

Figure 4: Actual and simulated out-of-sample debt-GDP dynamics (starting from conditions in 1980:1, and in 2006:2 respectively). Simulations are based on (2).

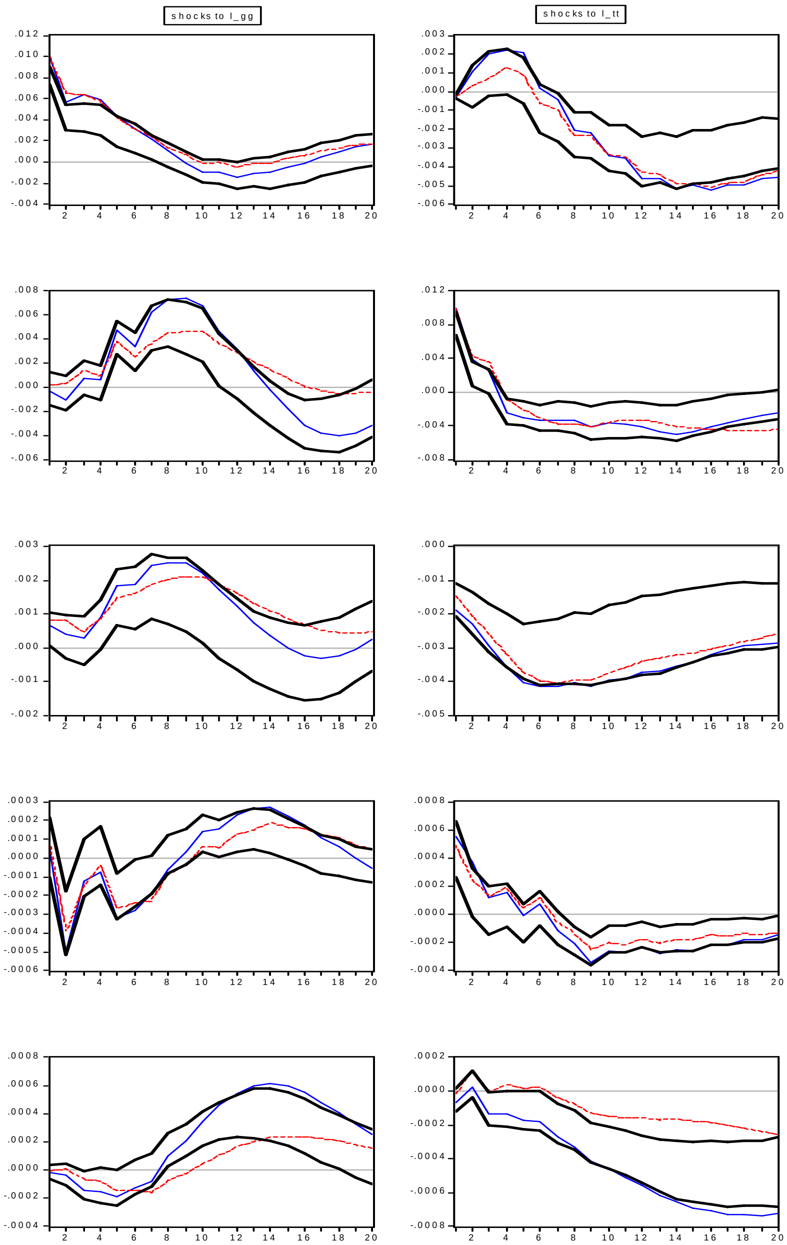

Figure 5.a: Fiscal shocks identified from a SVAR (dotted line) and in a model with feedbacks (solid line). Sample is from 1960:1 to 1979:4. The first column shows responses to shocks to g_t; the second column shows responses to shocks to t_t.The responses reported along the rows refer, respectively, to the effects on g_t, t_t, y_t, Δp_t, i_t.

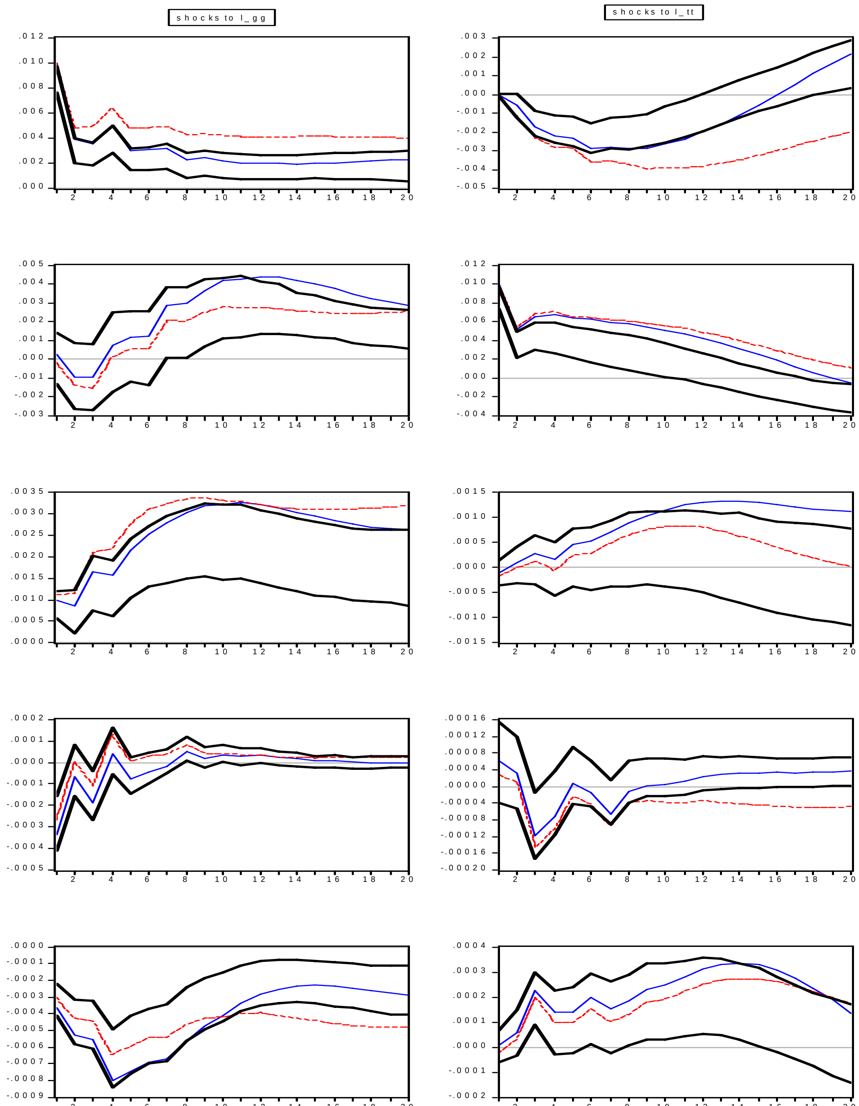

Figure 5.b: Fiscal shocks identified from a SVAR (dotted line) and in model with feedbacks (solid line). Sample is from 1960:1 to 1979:4. The first column shows responses to shocks to g_t; the second column shows responses to shocks to t_t..The responses reported along the rows refer, respectively, to the effects on g_t, t_t, y_t, Δp_t, i_t.

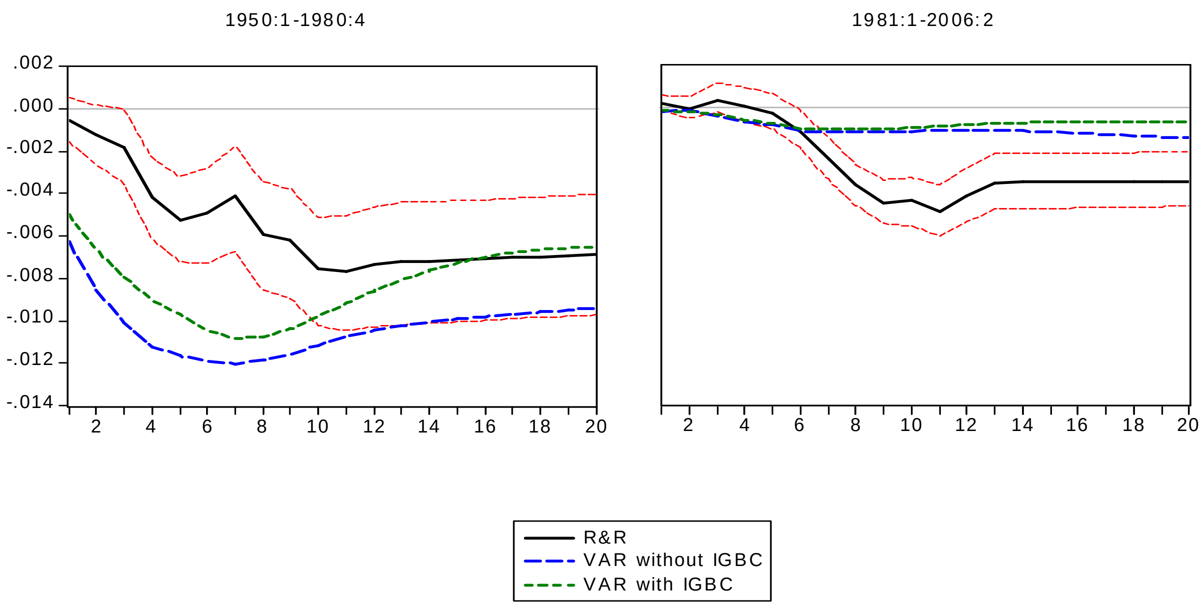

Figure 6: Using the Romer and Romer (2007) exogenous tax shocks. The effect on output in different models.

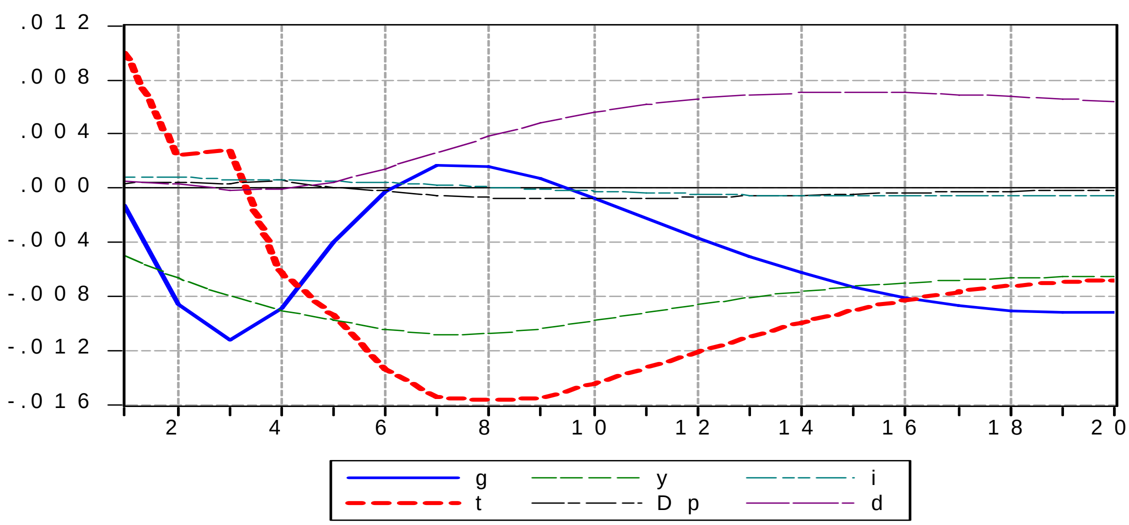

Figure 7: Dynamic response of all variables to an R&R tax shock, in a VAR with debt feedback estimated over the sample (1950:1–1980:1).

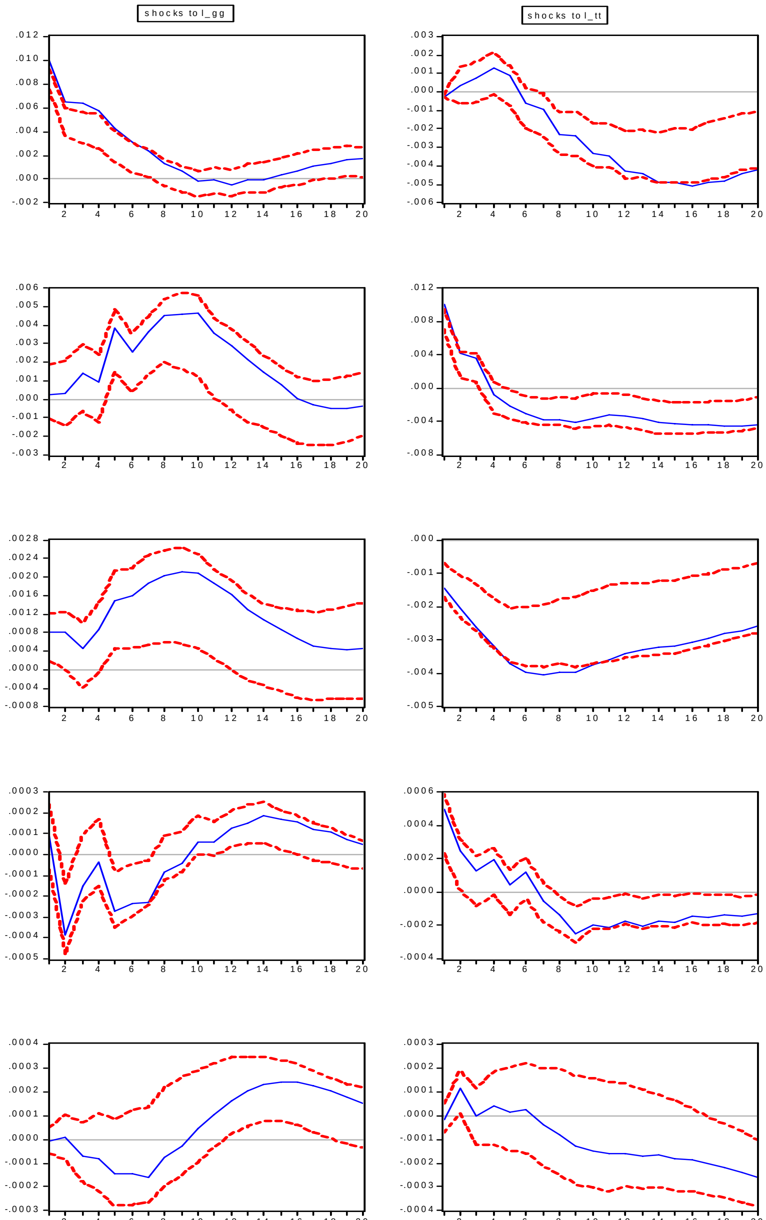

Figure A.1: Fiscal shocks identified from an SVAR:1960:1–1979:4. The first column shows responses to shocks to g_t, the second column to shocks to t_t.The responses reported along the rows refer, respectively, to the effects on g_t, t_t, y_t, Δp_t, i_t.

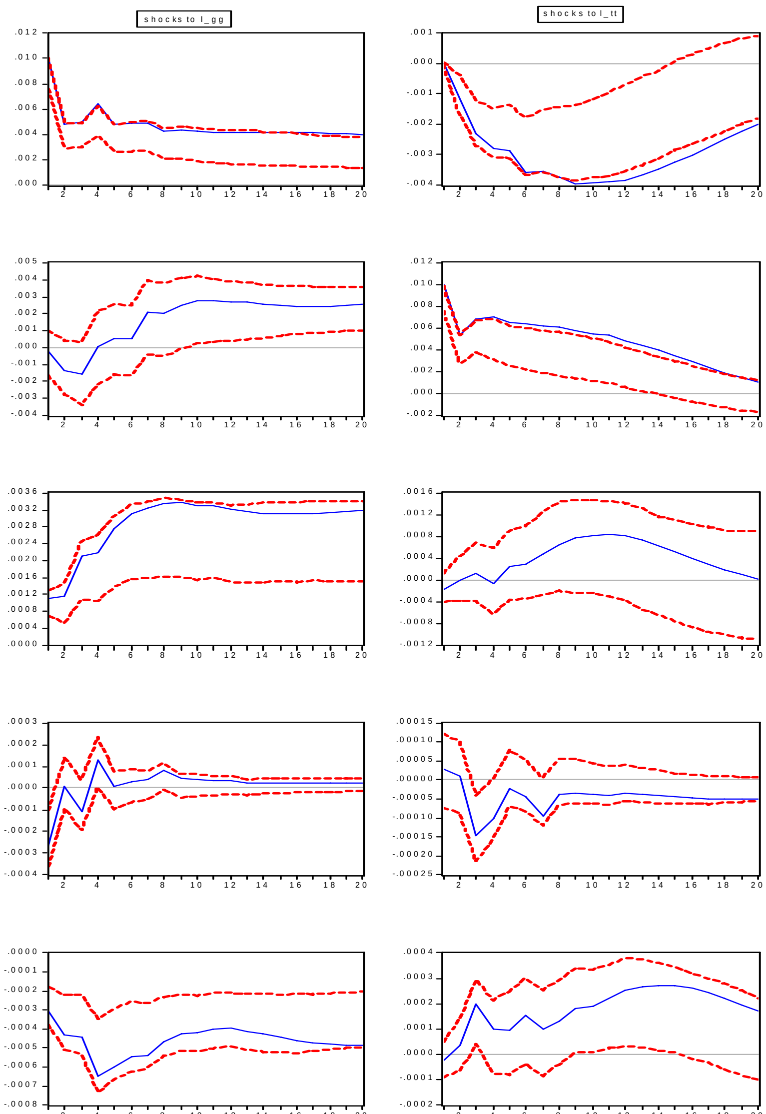

Figure A.2: Fiscal shocks identified from an SVAR: 1980:1–2006:2. The first column shows responses to shocks to g_t, the second column to shocks to t_t.The responses reported along the rows refer, respectively, to the effects on g_t, t_t, y_t, Δp_t, i_t.

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 (0.005 0.12)	0.164 (0.12 0.19)	−0.231 (−0.32 −0.14)	−0.004 (−0.08 0.06)	0.056 (−0.013 0.11)	0.127 (0.077 0.16)	−0.249 (−0.35 −0.16)	0.016 (−0.07 0.06)
	12	0.440 (0.17 0.60)	0.805 (0.55 0.84)	−0.987 (−1.25 −0.55)	0.170 (−0.13 0.38)	0.463 (0.10 0.58)	0.712 (0.48 0.75)	−0.994 (−1.31 −0.59)	0.288 (−0.18 0.34)
	20	0.585 (0.06 0.85)	1.431 (0.95 1.50)	−1.577 (−2.03 −0.83)	0.272 (−0.46 0.65)	0.475 (−0.12 0.73)	1.280 (0.77 .1.31)	−1.590 (−2.11 −0.86)	0.654 (−0.48 0.57)
i_t	4	−0.004 (−0.02 0.007)	−0.045 (−0.07 −.0.02)	0.003 (−0.01 0.013)	0.011 (−0.005 0.02)	−0.009 (−0.02 0.001)	−0.056 (−0.07 −.0.04)	−0.007 (−0.02 0.002)	0.016 (0.002 0.02)
	12	−0.010 (−0.05 0.05)	−0.141 (−0.20 −0.08)	−0.013 (−0.06 0.05)	0.058 (0.004 0.10)	0.022 (0.001 0.52)	−0.161 (−0.20 −0.09)	−0.075 (−0.10 −0.34)	0.081 (0.02 0.11)
	20	0.032 (−0.02 −0.10)	−0.232 (−0.32 −0.14)	−0.054 (−0.13 0.03)	0.125 (0.03 0.15)	0.118 (0.04 0.13)	−0.212 (−0.29 −0.13)	−0.205 (−0.26 −0.11)	0.160 (0.03 0.18)