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How large are the effects of tax changes? *

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Abstract

We use the time series of shifts in U.S. taxes constructed by Romer and Romer to estimate tax multipliers. Differently from the single-equation approach adopted by Romer and Romer, our estimation strategy (a Var that includes output, government spending and revenues, inflation and the nominal interest rate) does not rely upon the assumption that tax shocks are orthogonal to each other as well as to lagged values of other macro variables. Our estimated multiplier is much smaller: one, rather than three at a three-year horizon. When we split the sample in two sub-samples (before and after 1980) we find, before 1980, a multiplier whose size is never greater than one, after 1980 a multiplier not significantly different from zero. Following the findings in Bohn (1998), we also experiment with a model that includes debt and the non-linear government budget constraint. We find that, while in general not very important, the non-linearity that arises from the budget constraint makes a difference after 1980, when the response of fiscal variables to the level of the debt becomes stronger.

Keywords: fiscal policy, public debt, government budget constraint, VAR models

JEL Classification: H60, E62

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1 Introduction

In some recent papers the estimated effects on output (using post World War II data) of a shift in U.S. federal tax liabilities imply a multiplier significantly greater than one. Romer and Romer (forthcoming, R&R in what follows) find that tax increases are highly contractionary: according to their estimates a tax increase of 1% of U.S. GDP reduces output over the next three years by nearly 3%. The effect is highly statistically significant.

R&R use the narrative record, such as presidential speeches and Congressional reports, to identify the size, timing, and principal motivation for all major postwar tax policy actions. This analysis allows them to separate legislated changes into those taken for reasons related to prospective economic conditions and those taken for more exogenous reasons—for instance for philosophical reasons or to reduce an inherited budget deficit. Their estimates of the effects on output of shifts in taxes use only these more exogenous changes. Thus they avoid the omitted variable bias that affects regressions of output on aggregate measures of tax changes many of which are not legislated at all, but occur automatically because the tax base varies with the overall level of income, or because of changes in stock prices, inflation, and other non-policy forces. An additional advantage of the R&R "narrative" approach is that it allows to separate tax changes that are anticipated from those that caught the economy by surprise. Mertens and Ravn (2008) use the R&R measure of shifts in taxes and distinguish between those that were anticipated and those that were not. Their findings confirm the large multiplier reported by R&R and show that anticipated and unanticipated shifts in taxes have similar effects—though anticipated tax cuts, before they are implemented, tend to have a contractionary effect on output.

The size of these multipliers surprises even the authors and are much larger than those obtained in other studies. Blanchard and Perotti (2002) use U.S. data starting in 1960, thus exclude the first 15 years of the R&R sample: they estimate a multiplier for tax changes which is statistically significant, but whose size (1.3) is less than a half. [1] R&R suggest that these differences are the result of the failure of structural VAR's—the technique used in Blanchard and Perotti (2002), Perotti (2008) and in

[1]Interestingly, Perotti (2008) shows that the result for the entire sample (1960 to 2001) averages very different responses before and after 1980. In the first part of the sample tax cuts have a positive and significant effect on output, with a multiplier only slightly smaller compared with R&R (around 2.6 at a three year horizon). After 1980, however, the effect turns negative and significant with a multiplier that is similar in absolute value.

similar studies—to identify truly exogenous shifts in taxes.[2]

In this paper we assess the robustness of the evidence of a large tax multiplier using the same measure of exogenous shifts in taxes constructed by R&R but a different econometric specifications.

First, we show that the equation R&R estimate to compute the effects of a shift in taxes can be interpreted as the moving average representation of the equation for output growth in a VAR model which includes a larger set of variables (along with output growth, government revenues and spending, inflation, nominal interest rates, etc.). This representation however is truncated along two dimensions: *(i)* the number of lags is finite and *(ii)* no other shocks than tax shocks are included. Such an approach relies on the assumption that tax shocks are not only orthogonal to each other, but that they are also orthogonal to any other macro shock: productivity shocks, shifts in government spending, or in monetary policy, etc. When we relax this assumption we find multipliers whose size is much smaller than that estimated by R&R. When we split the sample in two sub-samples (before and after 1980) we find, before 1980, multipliers whose size is never greater than one per cent of GDP; after 1980 multipliers not significantly different from zero.

The model used by R&R to estimate tax multipliers, as well as our extensions, are all linear. However, there is a natural source of non-linearity among the variables included in a fiscal VAR, which arises from the government intertemporal budget constraint. Whether the government budget constraint belongs in a fiscal VAR depends on whether the level of the debt-to-GDP ratio enters the model. Bohn (1998), using a century of U.S data, finds a positive correlation between the government surplus and

[2] The identification strategy followed in the mixed structural VAR/event study approach of these papers uses institutional information about the tax and transfer systems and the timing of tax collections to identify the automatic response of taxes and spending to activity, and, by implication, to infer exogenous fiscal shocks. Recently, the validity of this identification approach has been questioned on the argument that it cannot take properly account of fiscal foresight. Leeper et al.(2008) point out that legislative and implementation lags provide private agents with clear signals about the tax rates they will face in the future. Paired with the forward looking behavious of agents this produces equilibrium time series with a non-invertible moving average component (see Hansen and Sargent, 1991, Lippi and Reichlin, 1994). As a consequence of the misalignament between the agents' information sets and the econometrician's information sets in the estimated VARs, economically meaningful shocks to taxes cannot be extracted from statistical innovations in the mixed structural VAR/event study approach or in any conventional structural VAR approach. The narrative approach to the identification of fiscal shocks employed in R&R is immune from the fiscal foresight problem that affects strctural VARs

the Federal debt—a result which suggests that U.S. fiscal policy reacts to the level of the debt ratio.[3] If fiscal variables respond to the level of the debt, then the analysis of the impact of fiscal shocks should be conducted by explicitly recognizing a role for debt and the stock-flow identity linking debt and deficits. We do this estimating the multiplier associated with the R&R tax shocks keeping track of the effect of such shocks on the path of the debt ratio, and allowing for a response of taxes, spending, output and interest rates to the level of the debt. We find no major difference between a non-linear model with an explicit debt dynamics equation and a VAR that excludes debt and the debt dynamics equation. We suggest that the reason why overlooking this non-linearity does not appear to be important—or at least as important as overlooking the simultaneity between tax shocks and other macro shocks—may be that the variables entering the budget constraint already enter (albeit linearly) the equation of a fiscal VAR that excludes debt. Non-linearity, however, appears to make a difference whenever–as in happens in the U.S. after 1980–the response of fiscal variables to the level of the debt is particularly strong.

2 Estimating the effects of tax changes

Having constructed a time series of exogenous shifts in taxes, u_{t-i}^{τ}—where each u_{t-i}^{τ} measures the impact of a tax change at the time it was implemented $(t-i)$ on tax liabilities at time t—R&R measure their effect on output, Y_t, estimating, using quarterly data and ordinary least squares, a single equation of the form

$$\Delta Y_t = a + \sum_{i=0}^{M} b_i u_{t-i}^{\tau} + e_t \tag{1}$$

Careful analysis of the motivation behind each u_t^{τ} allows R&R to assume that this variable is uncorrelated with the error term e_t, *i.e.* that the shifts in taxes described by $u_t^{\tau\prime} s$ are unrelated to other factors likely affect output growth (and to any other tax responses policymakers may have been making to those factors at around the same time). The effects of a tax shift on output growth can then be described by the impulse response constructed using the estimates of the b_i coefficients and allowing for three years of lags ($M = 12$).

In Figure 1 we have replicated the original results by R&R. The figure reports the effect on output between period t and period $t+i$ of a shift in taxes occurring in

[3] Corsetti et al.(2009) show that the impact of a shift in public spending depends on expectations about offsetting fiscal measures in the future.

period t and equivalent to one per cent of U.S. GDP in period t. We use quarterly data (described in the Data Appendix) and we report three impulse response functions: one based on estimates from a sample running from 1950:1 to 2006:2, and two based on estimates restricted to two sub-samples: 1950:1-1980:4 and 1981:1-2006:2. Our sample starts in 1950, rather than in 1947, the starting date of the R&R sample. The reason (as explained in the Data Appendix) is that we want to compare the results in R&R with those obtained using the $u_t^{\tau}{}'s$ in a VAR which also contains an equation for government spending—and consistent data on government spending are available only from 1950:1. We end in 2006:2, the last date for which the R&R shocks are available. The slightly shorter sample does not change the R&R result: the effect on output peaks, as in R&R, after ten quarters and implies a fall in output of about 3 per cent. The shape of the impulse response function also matches the original one. The motivation for splitting the sample is the finding by Perotti (2008) of signifcant differences in the effects of fiscal shocks before and after 1980, in the U.S. as well as in other countries. The results show a remarkable degree of stability of the effect of the R&R shocks based on the impulse responses from equation (1).

To analyse the robustness of these results we interpret equation (1) as a truncated version of the MA representation of output in a closed-economy fiscal VAR which includes output growth, inflation, the nominal rate of interest, government receipts and government expenditure. Defining these variables with the vector $\mathbf{Z}$, the Vector autoregression is

$$\mathbf{A}\mathbf{Z}_t = \mathbf{C}\mathbf{Z}_{t-1} + \mathbf{B}\boldsymbol{\varepsilon}_t \tag{2}$$

$\boldsymbol{\varepsilon}_t \equiv \left(\varepsilon_t^y, \varepsilon_t^\pi, \varepsilon_t^i, \varepsilon_t^\tau, \varepsilon_t^g\right)$ are structural shocks and their variance-covariance matrix is I.

The MA representation of (2) is

$$\mathbf{Z}_t = \Gamma(L)\boldsymbol{\varepsilon}_t \tag{3}$$

where $\Gamma(L) \equiv \frac{A^{-1}B}{1-A^{-1}CL}$. The MA representation is not directly estimated in the VAR approach, but can be derived by inversion after having estimated (2). To do this one needs to identify the structural shocks $\boldsymbol{\varepsilon}_t$: these can be obtained from the reduced form innovations, $\mathbf{e}_t$ using the relation $\mathbf{A}\mathbf{e}_t = \mathbf{B}\boldsymbol{\varepsilon}_t$,after having imposed a sufficient number of identifying restrictions on the matrices $\mathbf{A}$ and $\mathbf{B}$. R&R don't need to do this because their narrative approach provides a direct measure of the tax shocks ε_t^τ which are the only structural shocks they use. They then derive the impulse response by directly estimating the projection of output growth on the tax shocks. In practice they estimate one equation of the truncated MA representation (3) that can be re-written as follows:

$$\mathbf{Z}_t = \sum_{i=0}^{M} \Gamma_0 \Gamma_1^i \boldsymbol{\varepsilon}_{t-i} + \Gamma_1^{M+1} \mathbf{Z}_{t-M+1} \tag{4}$$

where $\Gamma_0 \equiv A^{-1}B,\ \Gamma_1 \equiv A^{-1}C$. A comparison between (4) and (1) reveals that the OLS estimates of the coefficients b_i obtained from (1).are consistent provided three conditions are satisfied:

- the "tax shocks" u_{t-i}^{τ} are independently distributed, otherwise the sum could not be truncated at M,
- the "tax shocks" u_{t-i}^{τ} are orthogonal to any other shock in $\boldsymbol{\varepsilon}_t$ that might influence output growth,
- the "tax shocks" u_{t-i}^{τ} are orthogonal to $\mathbf{Z}_{t-M+1}$.

The hypothesis that the u_t^{τ} are not serially correlated can be tested empirically and is satisfied in the time series constructed by R&R. Orthogonality of the tax shocks to any other shock is the identifying assumption: from an analysis of the extensive discussion in the narrative record of why each u_{t-i}^{t} action was taken, R&R conclude that *"most actions had a single predominant motivation, and that some of those motivations are unrelated to other factors likely to have important effects on output growth (and to any other tax responses policymakers may have been making to those factors at around the same time)"*.

There is an immediate way of validating the assumption that the u_{t-i}^{τ} are orthogonal to any information in the VAR dated $t - M + 1$: include u_t^{τ} in (2) and check if the impulse response of output growth to the u_t^{τ} shock obtained from the VAR is the same as that delivered by the single equation approach adopted by R&R. (Note that since we are only interested in the impulse response to a tax shock, to perform this experiment we don't need to identify any other structural shock). In fact impulses responses to the R&R tax shocks can be easily obtained by introducing them as an additional variable in (2) [4]

We have thus constructed impulse responses of output growth to the R&R tax

[4] Bagliano and Favero (1999) do this in the context of a monetary VAR to derive impulse responses to measures of monetary policy shocks costructed outside the VAR framework.

shocks by estimating the following VAR

$$\mathbf{Z}_t = \sum_{i=1}^{M} \mathbf{C}_i \mathbf{Z}_{t-i} + \delta_i u_t^{\tau} + \mathbf{e}_t \tag{5}$$

where $\mathbf{Z}_t$ includes the five variable mentioned above: the nominal rate of interest (the average cost of the Federal debt), output growth (the first difference of the log of real GDP), inflation (the first difference of the log of the price level), (the logs of) government receipts and government expenditure net of interest.(The data we use are described in the Data Appendix.). $\mathbf{e}_t$ are reduced form innovations.

Figure 2.compares the effect on output of an u_t^{τ} tax shock equivalent to one per cent of U.S. GDP estimated using, alternatively, (1) (displayed as a dotted line) and (5) (displayed as a continuos line). Estimating the effect of tax shocks using the VAR one obtains a response of output that is much smaller than that delivered by the single equation approach adopted in R&R. The impact of a tax shock on output growth estimated in a VAR never exceeds one per cent. The VAR also highlights the instability of the effects of tax shocks between the periods preceding and following 1980: the impact of tax shocks in the first sub-sample is larger and significantly different from the impact in the second sub-sample, where it is not significantly different from zero.

The results in Figure 2 show that the differences between the two impulse responses—that estimated using (1) and (5)—only appear after a few quarters, and not impact. This is a clear symptom that the single-equation framework fails to capture some significant simultaneity. This simultaneity must arise from the correlation between the tax shocks and the information included in the VAR in the periods preceeding the truncation of the MA representation directly estimated by R&R.

To see this point consider the simple case in which our VAR is of order one[5]. Then the truncated MA representation of the VAR can be re-written as

$$\mathbf{Z}_t = \sum_{i=0}^{M} \mathbf{C}_i^M \delta_i u_{t-i}^{\tau} + \mathbf{C}_i^{M+1} \mathbf{Z}_{t-M+1} + \sum_{i=0}^{M} \mathbf{C}_i^M \mathbf{e}_{t-i} \tag{6}$$

One of the equations in the VAR is the truncated MA representation of the output equation. Since by construction the u_{t-i}^{τ} are orthogonal to the $\mathbf{e}_{t-i}$, the difference in the multipliers obtained estimating (1) and (6) must depend on the correlation

[5]This implies no less of generalization as a VAR of any order can always be re-parameterized as a VAR of order 1 via the so-called "stacked representation".

between u^{τ}_{t-i} and $\mathbf{Z}_{t-M+1}$—a result which can be easily seen re-running the R&R regression augmenting it with $\mathbf{Z}_{t-M+1}$

$$\Delta Y_t = a + \sum_{i=0}^{M} b_i u^{\tau}_{t-i} + \mathbf{C}_i^{M+1} \mathbf{Z}_{t-M+1} + e_t \tag{7}$$

This is a robustness check R&R do not perform, since the robustness checks they report only use information dated up to time M. Figure 2.1 reports the effect of tax shocks as computed originally by R&R alongwith those based on the augmented regression (7) over the full sample 1950:1-2006:2. The Figure shows that the truncation has an effect on the size of the multiplier after the 8th quarter. The multiplier estimated using the augmented equation gets very close [6] to the one delivered by the inclusion of the R&R shocks in a fiscal VAR as in (5). Interestingly, the R^2 increases from 0.09 in the original R&R specification to 0.17 in the augmented specification. Unfortunately, it is impossible to identify which variable in $\mathbf{Z}$ is responsible for these results: the $F - test$ for the joint significance of the regressors included in the augmented model rejects the null, but the $t - tests$ on the individual coefficients do not point out any coefficient on a specific variable at a specific lag as strikingly significant.

3 Debt and the effects of fiscal shocks

The models estimated so far are linear. However, there is a natural source of non-linearity among the variables included in a fiscal VAR such as (2) and which arises from the government intertemporal budget constraint.

The way in which this non-linearity enters into the model is very simple and compatible with a range of alternative theoretical models: it arises from the possibility that the macroeconomic variables included in the VAR respond to the level of the debt-to-GDP-ratio—a possibility which, as we mentioned in the introduction, Bohn (1998) has shown to be a feature of the U.S. data. A response of macroeconomic variables is also necessary for stability of the debt ratio—except in the special case in which the rate of growth of the economy is exactly equal to the average cost of debt financing. Moreover—and this point is directly relevant to the estimation of the effect of tax shocks—whether, or not, following a shift in taxes, the debt ratio will be stabilized is likely to determine the response of the economy to the fiscal shock.

[6] The small remaining difference between the impulse responses can be rationalized on the ground that, following Blanchard-Perotti (2002), we specify our fiscal VAR in the (log) levels of the macroeconimic variables.

For instance, the impact of a given tax 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

When the debt level is introduced into the VAR, this variable needs to be made endogenous, otherwise impulse response functions would be computed assuming a constant debt ratio, thus ruling out the very reason why debt is included in the first place—namely to allow macro variables to respond to the effect of the fiscal shock on the level of the debt. The way to make the debt ratio endogenous is to add to the model the equation that describes how it evolves over time as a function of the path of all other variables, *i.e.* the government's intertemporal budget constraint. [7]

The omission of a debt feedback from estimated fiscal VARs is surprising also because the equilibrium structural models used to analyse the effects of fiscal policy are typically solved by imposing the government intertemporal budget constraint and are simulated under the assumption that the real value of the debt in the hands of the public must equal the expected present value of government surpluses. It is thus natural to ask why debt has been systematically excluded from empirical investigations of the effects of fiscal policy—not only the estimates by R&R, but essentially to the entire empirical literature (Edelberg et al, 1999, Blanchard and Perotti, 2002, Mountford Uhlig, 2002, Fatàs and Mihov, 2001 among other).

One justification for omitting the debt level is that the effects of this variable are captured by all other variables included in a fiscal VAR. For instance **Z**, in the VAR estimated in the previous section, contains all the variables that enter the government intertemporal budget constraint and thus determine the dynamics of the debt ratio. The difference is that the debt dynamics equation is non-linear, while the VAR is linear.

$$\begin{aligned} d_t &= \frac{1+i_t}{(1+x_t)} d_{t-1} + \frac{\exp(g_t) - \exp(t_t)}{\exp(y_t)} \\ & x_t \equiv \pi_t + \Delta y_t + \pi_t \Delta y_t \end{aligned} \tag{8}$$

Whether or not including the debt ratio directly in the VAR makes a difference thus depends on how good an approximation the linear version of (8) is. This requires that the debt to GDP ratio is stationary and that all other conditions for the validity of linearization are met. In this case impulse responses to fiscal shocks are to be interpreted as the response of the economy computed at the mean of the stationary government debt to GDP ratio.

[7] Note that the budget constraint is an identity: it does not add new parameters to be estimated, nor new shocks to be identified.

3.1 Estimating the effects of tax changes keeping track of debt dynamics

To check the empirical importance of taking non-linearity seriously, thus including in the VAR both the debt level and the budget constraint, we have computed impulse responses to the R&R shocks using a VAR that includes a debt feedback. The model estimated thus becomes

$$\begin{aligned}\mathbf{Z}_t &= \sum_{i=1}^{k}\mathbf{C}_i\mathbf{Z}_{t-i} + \delta_i u_t^t + \boldsymbol{\gamma}_i\left(d_{t-1} - d^*\right) + \mathbf{e}_t \qquad (9)\\ d_t &= \frac{1+i_t}{\left(1+\Delta p_t\right)\left(1+\Delta y_t\right)}d_{t-1} + \frac{\exp\left(g_t\right) - \exp\left(t_t\right)}{\exp\left(y_t\right)}\end{aligned}$$

In (9) macroeconomic variables are assumed to respond not to the level of the debt-to-GDP ratio, but to its distance from a target level d^*. Although this assumption is irrelevant for the results, we make it to estimate an equation that mirrors that estimated in Bohn (1998). As in Bohn we take 0.35, as the target value for d^*. As shown in Figure A1, this is also the average debt level in our sample.

Note that because (9) is non linear, constructing an MA representation of $\mathbf{Z}_t$ is no longer possible. This might therefore be an additional source of mis-specification of the single equation estimated by R&R.[8]

3.2 Computing impulse responses with stocks and flows

After all the parameters in (9) have been estimated, we are left with the problem of constructing impulse responses. The special nature of (9) poses an interesting (and solvable) problem for the computation of impulse responses, which requires going through the following steps:

[8] Also notice that the estimation of (9) differs from the procedure followed by Chung and Leeper (2007). These authors recognize the importance of including the government budget constraint because the present value condition implied by the linearized budget constraint—that the real value of the debt must equal the expected present-value of surpluses—generates a set of cross-equation restrictions on traditional fiscal VARs. And their results show that imposing such restrictions makes some difference for impulse response analysis. Our procedure doesn't need to assume that the conditions for linearization are satisfied. By augmenting the traditional fiscal VARs with the debt deficit dynamics our impulse responses satisfy by construction period-by-period the debt-deficit stock-flow relationship. We can therefore directly evaluate the validity of the tranversality condition by considering the long-run response of d_t to fiscal shocks and by checking if it converges to zero.

- generate a baseline simulation for all variables by solving (9) 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 $\mathbf{Z}_t$ and for the endogeneity of d_t modelled via (8),
- compute confidence intervals via bootstrap methods.[9]

3.3 Is non-linearity empirically important?

We illustrate the empirical relevance of including debt and the government intertemporal budget constraint by directly comparing impulse responses obtained using (9) with those shown in Section 2. The results are in Figure 3, both for the entire sample and for the two sub-samples considered separately.

The two sets of impulse responses illustrate that the model augmented with debt and the non-linear debt dynamics equation produces results which are very similar to those obtained by including the R&R shocks in a traditional fiscal VAR .Figure 3 confirms that when the R&R measure of tax shocks is considered within a multiple equation model, rather than in a single equation framework, the estimated multpliers are much smaller. However, while simultaneity is important, we find no major empirical difference between a non-linear model with an explicit debt dynamics equantion and a linearized model where the effect of debt is captured by its components.

Interestingly, the impulse responses based on the linearized model and on the non-linear model with debt differ in the second subsample where the effect of an

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

exogenous increase in taxes affects negatively and significantly output growth (with a peak effect of about 0.5 per cent), while the same effect is never significantly different from zero in the model without debt. This is because the feedback from the debt ratio to government spending and revenues is stronger in the second sub-sample—but the linearized model computes impulse responses at the mean of the stationary government debt ratio and thus fails to fully capture this feedback..

4 Conclusions

We have estimated the multiplier associated with the narrative shifts in taxes constructed by R&R (forthcoming) without imposing that tax shocks are uncorrelated with past macroeconomic outcomes. We find a much smaller multiplier: 1, rather than 3 at a three-year horizon. We also find that the multplier changes significantly before and after 1980, when the impact of tax shocks becomes not significantly different from zero

We have also estimated the multiplier keeping track of the effect of tax shocks on the level of the debt-GDP ratio. We have done this allowing for the non-linearity which arises from the government budget constraint. We find that, while in general not very important, this non-linearity makes a difference after 1980, when the response of fiscal variables to the level of the debt becomes stronger.

The methodology we have developed to analyze the impact of tax shocks by keeping track of the non-linear budget constraint, could be used in other settings. For instance, the 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 and that typically omit a feedback from the stock of external debt to macroeconomic variables.

This approach could also be used in the analysis of the effects of tax 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.

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6 Data Appendix

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 R&R tax shocks start in 1947, while our data only start in 1950:1 because data for total governemnt spending are available on a consistent basis only from 1950:1. We thus exclude the exogenous shocks that occurred between January 1947 and December 1949.

Our approach requires that the debt-dynamics equation in (9) 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). 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.[10] We thus exclude it from our definition of federal public debt. We are unable to build the debt series back to 1947:1, the start of the Romer and Romer sample, because, as mentioned above, data for total governemnt spending, needed to buld the debt series, are available on a consistent basis only from 1950:1

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

Figure A-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 (this is the dotted line). We have checked the accuracy of the debt dynamics equation in (9) simulating it forward from 1970:1 (this is the continuous line in Figure A-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.

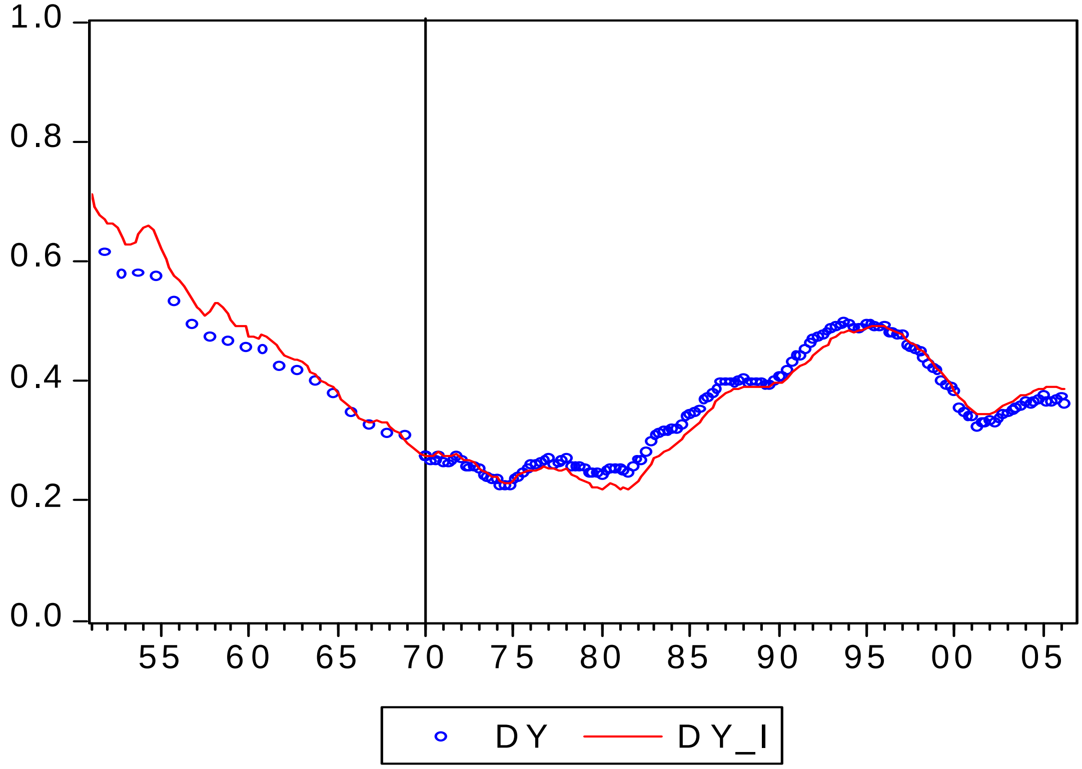


Figure A1: Actual (DY) and simulated (DY_I) (dynamically backward and forward starting in 1970:1) debt-GDP ratio. Actual data are observed at quarterly frequency from 1970 onwards and at annual frequency from 1970 backward. The simulated data are constructed using the government intertemporal budget constraint (2) with observed data and initial conditions given by the debt-to-GDP ratio in 1970:1.

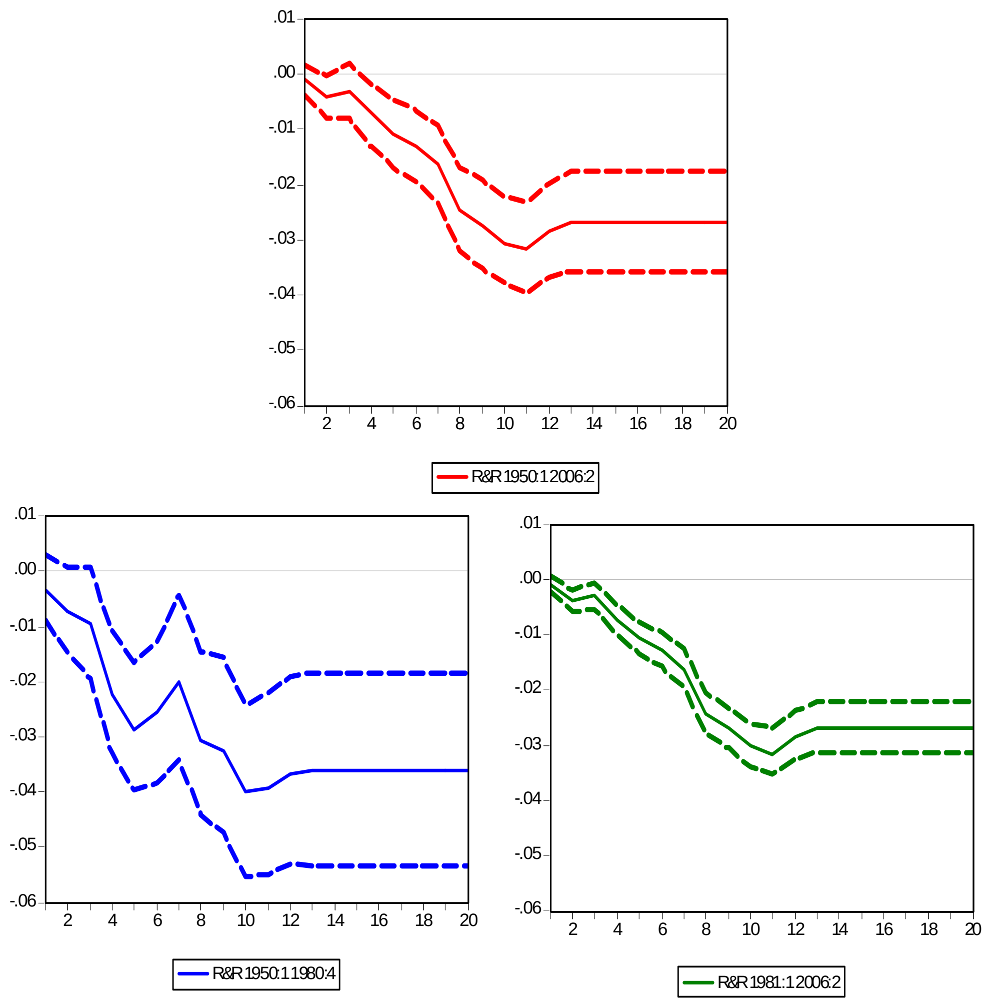


Figure 1: Estimated Impact of an Exogenous Tax Increase of 1% of GDP on GDP: R&R single equation approach

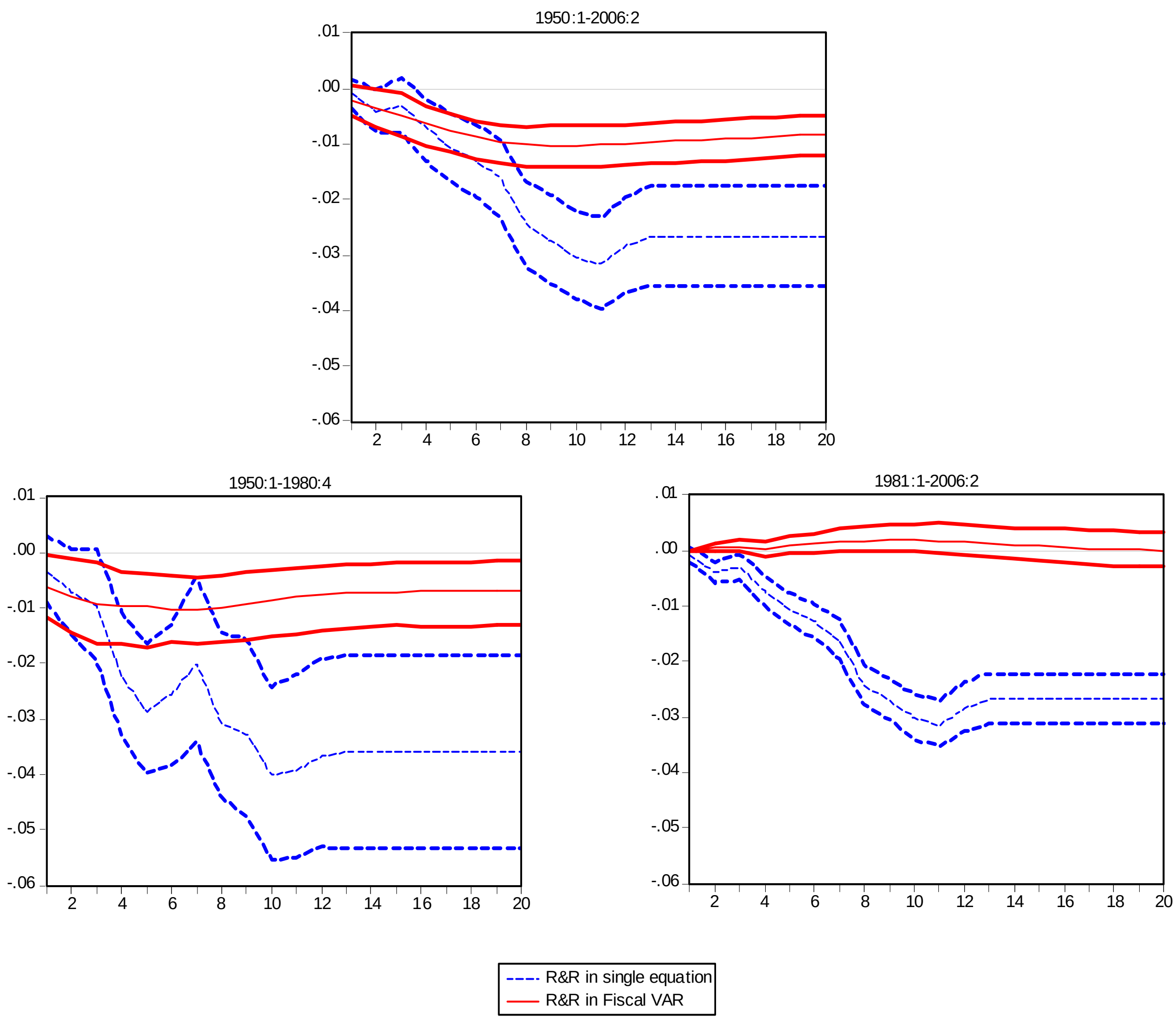
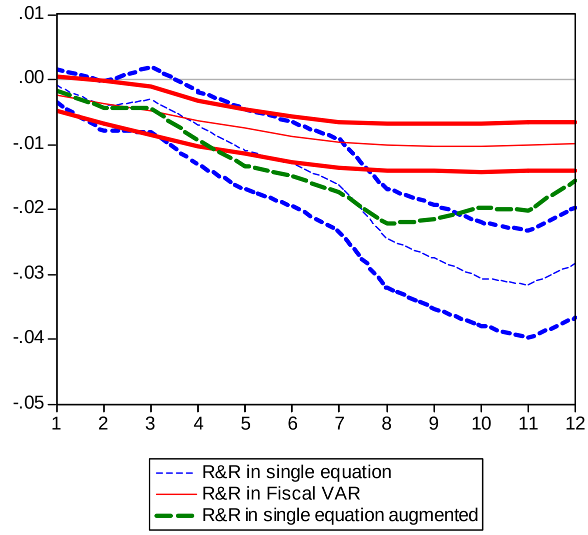


Figure 2: Estimated Impact of an Exogenous Tax Increase of 1% of GDP on GDP: R&R single equation approach *vs.* Fiscal VAR approach


.01
.00
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1
2
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R&R in single equation
R&R in Fiscal VAR
R&R in single equation augmented

Figure 2.1

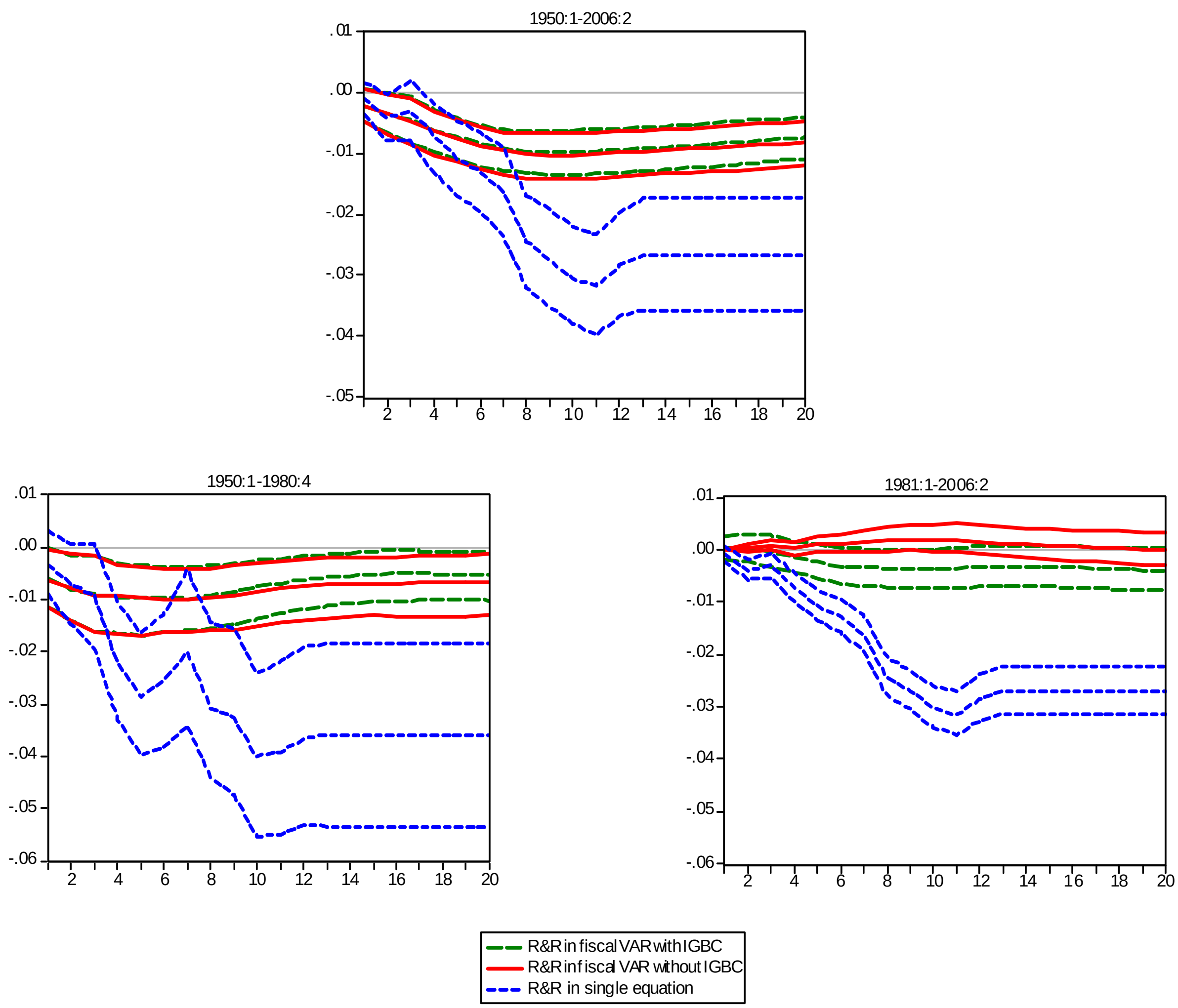

Figure 3: Estimated Impact of an Exogenous Tax Increase of 1% of GDP on GDP: R&R single equation approach *vs.* Fiscal VAR with and without the intertemporal budget constraint