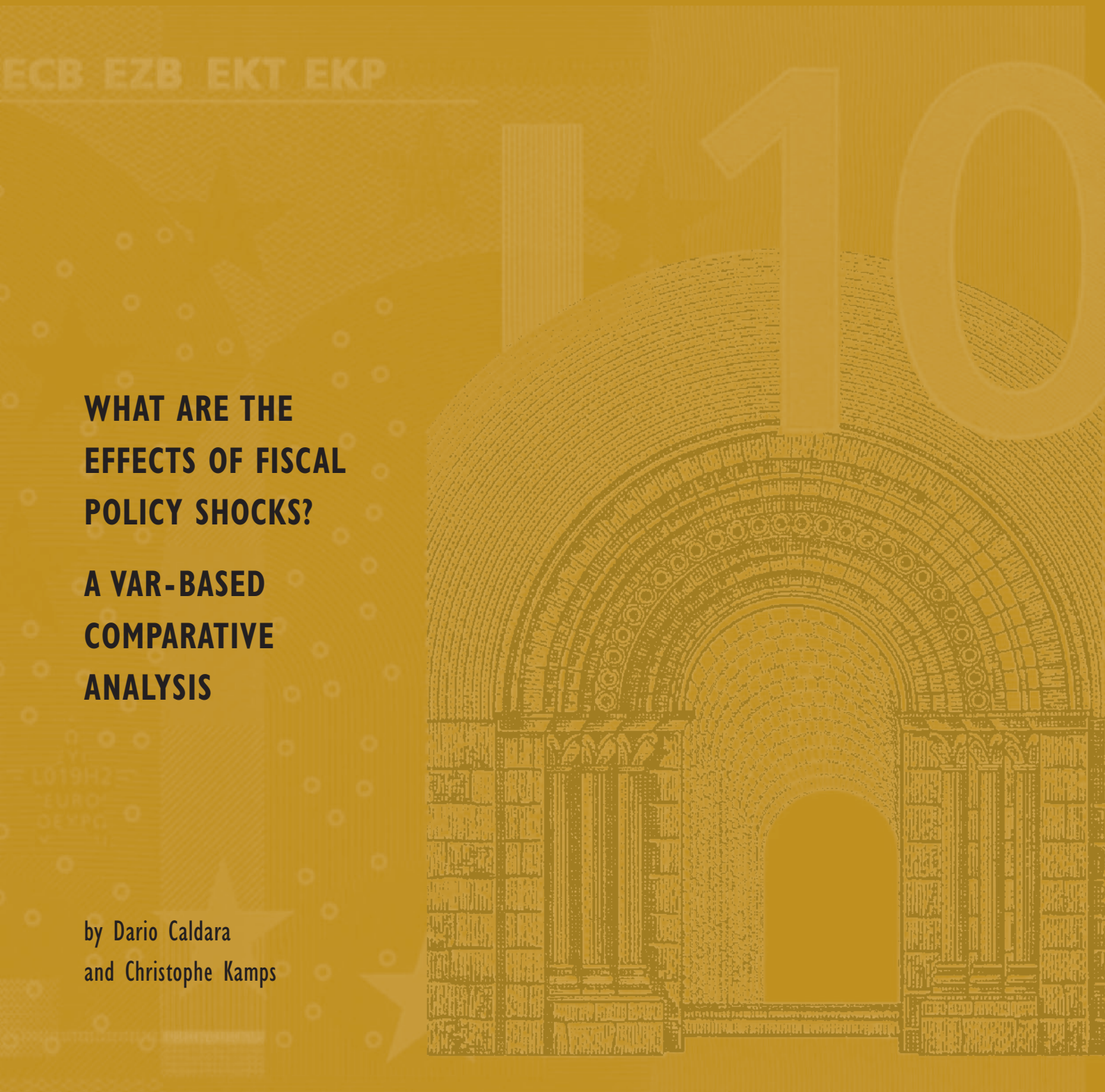
EUROPEAN CENTRAL BANK

EUROSYSTEM

WORKING PAPER SERIES

NO 877 / MARCH 2008



WHAT ARE THE EFFECTS OF FISCAL POLICY SHOCKS?

A VAR-BASED COMPARATIVE ANALYSIS

by Dario Caldara
and Christophe Kamps

EUROPEAN CENTRAL BANK

EUROSYSTEM

WORKING PAPER SERIES

NO 877 / MARCH 2008

WHAT ARE THE EFFECTS OF FISCAL POLICY SHOCKS?

A VAR-BASED COMPARATIVE ANALYSIS[1]

by Dario Caldara[2]

and Christophe Kamps[3]

In 2008 all ECB publications feature a motif taken from the €10 banknote.

This paper can be downloaded without charge from http://www.ecb.europa.eu or from the Social Science Research Network electronic library at http://ssrn.com/abstract_id=1102338.

1 We would like to thank an anonymous referee, Kai Carstensen, Efrem Castelnuovo, Andrew Mountford, Torsten Persson and Harald Uhlig for helpful comments and discussions. We also thank seminar audiences at the IIES, the ECB, the Kiel Institute for the World Economy, the Universities of Padua, Pavia and Tübingen as well as at the 2006 congresses of the EEA, the Society for Computational Economics and the IIPF and at the 2007 congress of the Royal Economic Society for helpful comments. The views expressed in this paper are those of the authors and do not necessarily reflect those of the European Central Bank.

2 Stockholm University, Institute for International Economic Studies, Universitetsvägen 10, House A, 8th Floor, SE-106 91 Stockholm, Sweden; e-mail: caldarad@iies.su.se

3 European Central Bank, Fiscal Policies Division, Kaiserstrasse 29, 60311 Frankfurt am Main, Germany; e-mail: christophe.kamps@ecb.int

Address
Kaiserstrasse 29
60311 Frankfurt am Main, Germany

Postal address
Postfach 16 03 19
60066 Frankfurt am Main, Germany

Telephone
+49 69 1344 0

Website
http://www.ecb.europa.eu

Fax
+49 69 1344 6000

ISSN 1561-0810 (print)
ISSN 1725-2806 (online)

CONTENTS

Abstract

The empirical literature using vector autoregressive models to assess the effects of fiscal policy shocks strongly disagrees on even the qualitative response of key macroeconomic variables to government spending and tax shocks. We provide new evidence for the U.S. over the period 1955-2006. We show that, controlling for differences in specification of the reduced-form model, all identification approaches used in the literature yield qualitatively and quantitatively very similar results as regards government spending shocks. In response to such shocks real GDP, real private consumption and the real wage all significantly increase following a hump-shaped pattern, while private employment does not react. In contrast, we find strongly diverging results as regards the effects of tax shocks, with the estimated effects ranging from non-distortionary to strongly distortionary. The differences in results can to a large extent be traced back to differences in the size of automatic stabilizers estimated or calibrated for alternative identification approaches. These differences also translate into uncertainty about the effects of policy experiments typically considered in theoretical models.

JEL Classification: C32, E60, E62, H20, H50.

Keywords: Fiscal policy, vector autoregression, identification, robustness.

Non-technical summary

In recent years vector autoregressive (VAR) models have become the main econometric tool to assess the effects of monetary and fiscal policy shocks. While a consensus view has emerged as regards the empirical effects of monetary policy shocks, the empirical literature has struggled so far to provide robust stylized facts on the effects of fiscal policy shocks. In particular, there is no agreement on even the qualitative effects of fiscal policy shocks on those macroeconomic variables (private consumption, real wage and private employment) which would be helpful to discriminate among competing theories.

In this paper we provide new evidence for the U.S. over the period 1955-2006. We show that, controlling for differences in specification of the reduced-form model, all identification approaches used in the literature yield qualitatively and quantitatively very similar results as regards government spending shocks. In response to such shocks real GDP, real private consumption and the real wage all significantly increase following a hump-shaped pattern, while private employment does not react. Our empirical results support theoretical models which generate an increase in private consumption and the real wage but at the same time do not support the increase in employment implied by most current-generation DSGE models. A further challenge arising from the empirical evidence is that the positive responses of private consumption and the real wage are very persistent, whereas most current-generation DSGE models consistent with an increase in these variables predict that the responses turn negative already about one year after the government spending shock occurs.

In contrast, we find strongly diverging results as regards the effects of tax shocks depending on the identification approach used, with the estimated effects of unanticipated tax increases ranging from non-distortionary to strongly distortionary. We show that the differences in results can to a large extent be traced back to differences in the automatic response of tax revenues to the business cycle (automatic stabilizers) estimated or calibrated for alternative identification approaches, with the degree of distortion associated with a given tax shock being positively related to the estimated size of automatic stabilizers. This uncertainty about the effects of tax shocks also translates into uncertainty about the effects of policy experiments. As regards the effects of balanced-budget spending increases, e.g., our results show that the sign of the fiscal multiplier depends on the degree to which taxes are estimated to be distortionary. We interpret our results as indicating a need for a better modeling of the effects of tax shocks and, in particular, for a refinement of the way taxes are adjusted for the effects of the business cycle in structural VAR models.

1 Introduction

In recent years vector autoregressive (VAR) models have become the main econometric tool to assess the effects of monetary and fiscal policy shocks. While a consensus view has emerged as regards the empirical effects of monetary policy shocks (see Christiano et al. 1999), the empirical literature has struggled so far to provide robust stylized facts on the effects of fiscal policy shocks (see Perotti 2007). In particular, there is no agreement on even the qualitative effects of fiscal policy shocks on those macroeconomic variables (private consumption, real wage and private employment) which would be helpful to discriminate among competing theories. In this paper we show that after controlling for differences in specification of the reduced-form VAR model some of the disagreement in the literature vanishes. In particular, all identification approaches used in the literature yield qualitatively and quantitatively very similar results as regards the effects of government spending shocks. In contrast, we find strongly divergent results as regards the effects of tax shocks depending on the identification approach used, with the estimated effects ranging from non-distortionary to strongly distortionary. The differences in results can to a large extent be traced back to differences in the size of automatic stabilizers estimated or calibrated for alternative identification approaches, with the estimated degree of distortion associated with a given tax shock being positively related to the size of automatic stabilizers. These differences also translate into uncertainty about the effects of policy experiments typically considered in theoretical macroeconomic models. In the case of balanced-budget spending increases, e.g., the sign of the fiscal multiplier depends on the identification approach chosen. We also provide new evidence for deficit-financed spending increases and deficit-financed tax cuts.

Apart from differences in the specification of the reduced-form VAR model (including sample period, set of endogenous variables, deterministic terms and lag length) the empirical studies in this literature distinguish themselves by the approach chosen to identify fiscal policy shocks. Four main identification approaches have been used to date: first, the recursive approach introduced by Sims (1980) and applied to study the effects of fiscal shocks by Fatas and Mihov (2001); second, the structural VAR approach proposed by Blanchard and Perotti (2002) and extended in Perotti (2005, 2007); third, the sign-restrictions approach developed by Uhlig (2005) and applied to fiscal policy analysis by Mountford and Uhlig (2005); and, fourth, the event-study approach introduced by Ramey and Shapiro (1998) to study the effects of large unexpected increases in government defense spending and also used by Edelberg et al. (1999), Eichenbaum and Fisher (2005), Perotti (2007) and Ramey (2007). In this paper we use all four identification approaches.

We first discuss the empirical evidence this literature has provided for *government spending shocks* as this evidence, conflicting as it may be, has strongly influenced recent theoretical modeling of fiscal policy. Before turning to the disagreement it is interesting to note that irrespective of the chosen identification approach all studies agree that positive government spending shocks have persistent positive output effects. This finding on its own, however, is not helpful for discriminating among competing theories because a positive output response is compatible with both Keynesian and neoclassical theories.[1] Yet, the empirical studies in this literature disagree on the effects of government spending shocks on those macroeconomic variables which are helpful in this respect. This is true, in particular, for the response of private consumption. Fatas and Mihov (2001), Blanchard and Perotti (2002) and Perotti (2005, 2007) report that private consumption significantly and persistently increases in response to a positive government spending shock, while Mountford and Uhlig (2005) and Edelberg et al. (1999) provide evidence that the response of private consumption is close to zero and statistically insignificant over the entire impulse response horizon. Ramey (2007) reports that private consumption persistently and (over short and long horizons) significantly falls in response to such a shock. As regards the responses of the real wage and employment, Perotti (2007) provides evidence that the real wage persistently and significantly increases while employment does not react, whereas Eichenbaum and Fisher (1998) and Burnside et al. (2004) provide evidence that the real wage persistently and significantly falls while employment persistently and significantly increases.

The recent theoretical literature modeling the effects of fiscal policy shocks using dynamic stochastic general equilibrium (DSGE) models has evolved along two very different lines in response to this empirical evidence. The first branch of this literature builds on the assumption that private consumption and the real wage respond negatively and employment positively to an increase in government spending. If those were the relevant stylized facts then (variants of) the prototypical real business cycle (RBC) model would be fully data-consistent. In this model an exogenous increase in government spending financed by lump-sum taxes reduces the representative agent's wealth causing the agent to consume less and to work more which in turn depresses the real wage. Examples include Edelberg et al. (1999), Burnside et al. (2004) and Eichenbaum and Fisher (2005). The second branch of this literature, instead, takes as stylized fact that private consumption responds positively to an increase in government spending. If this was a robust stylized fact then

[1] In the case of neoclassical theories a positive output response only obtains if the increase in government spending is financed by non-distortionary taxes (see Baxter and King 1993).

the standard neoclassical model would not be data-consistent. Several authors have introduced modifications to the standard model in order to make its predictions consistent with a rise in private consumption[2]: Linnemann (2006) using a modified utility function for which consumption and employment are complements shows that for empirically plausible parameter values private consumption and employment increase while the real wage falls in response to a positive government spending shock. Ravn et al (2006) incorporate good-specific habits into a model with monopolistic competition and show that for large values of the habit-persistence parameter private consumption, the real wage and employment all increase in response to a government spending shock. Galí et al (2007) incorporate rule-of-thumb consumers into a model with nominal rigidities and show that—for a sufficiently large size of the group of rule-of-thumb consumers—private consumption, the real wage and employment all increase in response to a government spending shock. The evidence presented in our paper suggests that private consumption indeed increases in response to a positive government spending shock and that the responses of labor market variables seem to be important to rationalize the consumption response. Our empirical results support models which generate an increase in the real wage but at the same time do not support the increase in employment implied by most current-generation DSGE models. A further challenge arising from the empirical evidence is that the positive responses of private consumption and the real wage are very persistent, whereas most current-generation DSGE models consistent with an increase in these variables predict that the responses turn negative already about one year after the government spending shock occurs (see e.g. Galí et al. 2007).

As regards *tax shocks*, the empirical literature is also characterized by some disagreement on their macroeconomic effects.[3] Most studies assessing the effects of tax shocks on the U.S. economy conclude that unanticipated tax increases have strong negative effects on output and other real economy variables. This is true for studies using the sign-restrictions approach (see Mountford and Uhlig 2005) or a narrative approach (similar to the event-study approach for government spending shocks) isolating those legislated tax changes which were unrelated to the state of the economy and using them to estimate the macroeconomic effects of exogenous tax changes (Romer and Romer 2007). In contrast, the structural VAR approach introduced by Blanchard and Perotti (2002) and further de-

[2] See Perotti (2007: Section 10) for a more comprehensive review of this branch of the literature.

[3] This disagreement has received much less attention in the recent theoretical literature. For simplicity nearly all theoretical studies assume that taxes are non-distortionary. Moreover, if taxes were instead assumed to be distortionary it would not only be very difficult to generate a rise in private consumption in response to a tax-financed increase in government spending but also to obtain an increase in output.

veloped by Perotti (2005) yields conflicting evidence. While Blanchard and Perotti (2002) provide evidence showing that unanticipated tax increases have strongly negative output effects, Perotti's (2005) results suggest that output does not react in the U.S. in the period when the tax shock hits the economy.[4] Our empirical results indicate that the answer to the question of whether taxes are distortionary or not depends on the identification approach chosen. While our results for the Blanchard-Perotti approach suggest that taxes are non-distortionary, our results for the sign-restrictions approach suggest that taxes are strongly distortionary. We further show that the answer depends strongly on the size of automatic stabilizers, which is lower for the Blanchard-Perotti approach (for which the size of automatic stabilizers is calibrated on the basis of extra-model evidence) than for the sign-restrictions approach (for which the size of automatic stabilizers is estimated inside the model). We show that for the Blanchard-Perotti approach there is an approximately linear relationship between the calibrated size of automatic stablizers and the estimated sign and size of the impact output response to exogenous tax shocks. We interpret our results as indicating a need for a refinement of the way taxes are adjusted for the effects of the business cycle in structural VAR models.

The uncertainty about the effects of tax shocks translates into uncertainty about the effects of *policy experiments* typically considered in the theoretical literature. We present evidence for three alternative policy experiments: a balanced-budget spending increase, a deficit-financed spending increase and a deficit-financed tax cut. We follow Mountford and Uhlig's (2005) approach to construct policy experiments by linearly combining "pure" government spending and tax shocks. Our results show that for the sign-restrictions approach the sign of the fiscal multiplier crucially depends on whether increases in government spending are tax-financed or deficit-financed. In contrast, the results for the Blanchard-Perotti approach suggest that the way government spending is financed does not matter, which is in line with the assumption of Ricardian Equivalence commonly made in recent theoretical literature. In our view the uncertainty about whether Ricardian Equivalence is a good approximation of economic reality again points to the importance of a better modeling and understanding of the effects of tax shocks.

[4] This difference in results seems to be largely due to the different definitions of taxes used by Blanchard and Perotti (2002) and Perotti (2005). While Blanchard and Perotti (2002) use cash data on federal corporate income tax receipts from the Quarterly Treasury Bulletin, Perotti (2005) uses accrual data provided with the National Income and Product Accounts. Perotti (2005: 10) argues that the accrual measure is preferable "because the cash adjustment displays a marked seasonality that is difficult to eliminate". In order to test for the importance of the different tax measures we re-estimate the 3-equation VAR used in Blanchard and Perotti (2002) using Perotti's (2005) tax measure. The results of this exercise suggest that the differences in the output response across these two studies are largely attributable to the different tax measures. Detailed results are available upon request.

The remainder of this paper is organized as follows. Section 2 describes the data used for our comparative analysis. Section 3 presents the econometric methodology, including a description of the reduced-form VAR model and the alternative identification approaches. Section 4 presents the results for the pure government spending and tax shocks. Section 5 analyzes the relationship between the estimated size of automatic stabilizers and the estimated output effects of exogenous tax shocks. Section 6 presents results for the policy experiments. Section 7 presents the results of a sensitivity analysis. Section 8 concludes.

2 Data

We use quarterly U.S. data over the period 1955:1 – 2006:4. The components of national income and various fiscal series are drawn from the NIPA tables published by the Bureau of Economic Analysis. The interest rate series is drawn from the Federal Reserve Bank of Saint Louis' ALFRED database. Our baseline measure of the real wage (real hourly compensation in the business sector) is drawn from the Bureau of Labor Statistics, while our baseline measure of employment (total economy hours worked per capita) is taken from Francis and Ramey (2005). The Data Appendix gives details on definitions and data sources for all variables used in the baseline and sensitivity analyses.

Our baseline model is a five-variable VAR model including the log of real per capita government spending, g_t, the log of real per capita net taxes, τ_t, the log of real per capita GDP, y_t, the GDP deflator inflation rate, π_t, and a short-term interest rate, r_t. This set of variables is the same as the one used by Perotti (2005). In addition we specify six-variable VAR models, adding in turn the log of real per capita private consumption, c_t, the log of real per capita private nonresidential investment, i_t^{NR}, the log of real per capita private residential investment, i_t^R, the log of per capita hours worked, n_t, and the log of the real wage, w_t, to the set of variables.

Our definition of the fiscal variables closely follows related literature. In particular, government spending and taxes are defined net of social transfers. More specifically, government spending is the sum of government consumption and investment, while net taxes are defined as government current receipts less current transfer and interest payments. Figure 1 shows the evolution of the government spending to GDP ratio and of the net tax to GDP ratio over the period 1955-2006. The figure reveals some well-known fiscal episodes. As regards the spending ratio one can discern the increase in the mid-1960s at the onset of the Vietnam war, the increase around 1980 associated with the Carter-Reagan military build-up, the drop in the 1990s associated with expenditure restraint under the

Budget Act of 1990 and the Balanced Budget Act of 1997 and, more recently, the renewed increase related to military spending in the context of the war on terrorism following 9/11. As regards the tax ratio the figure reveals the strong drops in the mid-1970s, the early 1980s and early 2000s, all related to both discretionary tax cuts and economic downswings, but also the strong increase during the stock-market boom in the late 1990s.

As one of the aims of this study is to provide evidence for those variables which are helpful to discriminate among competing theories we also check whether our baseline results are robust to alternative variable definitions. As regards private consumption we provide evidence for its durable and nondurable subcomponents. Our baseline measure of employment (total economy hours worked) includes hours worked in the government sector in order to account for the fact that government wages constitute a large fraction of government consumption (see Cavallo 2005). We also provide evidence for three alternative measures of employment: hours worked in the private business sector as well as number of persons employed in the private business sector and in the government sector. Our baseline measure of wages (real hourly compensation in the business sector) is a measure of the real product wage relevant for firms' hiring decisions. We also provide evidence for an alternative definition of the product wage as well as for two measures of the consumption wage relevant for households' labor-supply decisions. Section 7.3 presents the results.

3 Econometric methodology

This section presents the vector autoregressive methodology used in the empirical application. It first presents the benchmark reduced-form VAR model and then discusses how we implement the various identification approaches. Collecting the endogenous variables in the k-dimensional vector X_t the reduced-form VAR model can be expressed as

$$X_t = \mu_0 + \mu_1 t + A(L)X_{t-1} + u_t, \tag{1}$$

where μ_0 is a constant, t is a linear time trend, $A(L)$ is a 4th-order lag polynomial and u_t is a k-dimensional vector of reduced-form disturbances with $E[u_t] = 0$, $E[u_t u_t'] = \Sigma_u$ and $E[u_t u_s'] = 0$ for $s \neq t$. We follow Blanchard and Perotti (2002) and choose a lag length of four quarters. This seems to be a natural choice in a model with quarterly data and, moreover, using a higher lag order like, e.g., Mountford and Uhlig (2005) does not affect the results. Deterministic terms other than the constant and the linear time trend like the quadratic time trend, the seasonal dummy variables and the quarter-dependent

coefficients considered by Blanchard and Perotti (2002) turned out to be insignificant, thus we dropped them.[5] In our implementation of the event-study approach we augment our baseline VAR model with a dummy variable capturing the onset of the Vietnam war in 1965, the Carter-Reagan military buildup in 1980 and the Iraq War in 2001.

We follow Mountford and Uhlig (2005) and estimate the VAR model using Bayesian methods. The main advantage of the Bayesian approach is that it allows for a conceptually clean way of drawing error bands for impulse responses (see Sims and Zha 1999).[6] We use a Normal-Wishart prior for the coefficient matrices $A(L)$ and Σ_u, implying that the posterior also belongs to the Normal-Wishart family. We take 500 draws from the posterior of the reduced-form VAR model and, for each draw of the posterior, identify the structural shocks for the three identification approaches discussed below. In Sections 4-7 we provide results in terms of impulse responses, reporting the median of the posterior distribution of the responses as well as error bands based on the 16% and 84% fractiles of the posterior distribution.[7]

As the reduced-form disturbances will in general be correlated it is necessary to transform the reduced-form model into a structural model. Pre-multiplying the above equation by the (kxk) matrix A_0 gives the structural form

$$A_0 X_t = A_0\mu_0 + A_0\mu_1 t + A_0 A(L) X_{t-1} + Be_t, \tag{2}$$

where $Be_t = A_0 u_t$ describes the relation between the structural disturbances e_t and the reduced-form disturbances u_t. In the following, it is assumed that the structural disturbances e_t are uncorrelated with each other, i.e., the variance-covariance matrix of the structural disturbances Σ_e is diagonal. The matrix A_0 describes the contemporaneous relation among the variables collected in the vector X_t. In the literature this representation of the structural form is often called the AB model (see, e.g., Lütkepohl 2005: 364). Without restrictions on the parameters in A_0 and B the structural model is not identified.

[5] Mountford and Uhlig (2005) do not include any deterministic terms in their reduced-form VAR model. Uhlig (2005) argues that this may result in a slight misspecification, but makes for more robust results because of the interdependencies in the specification of the prior between these terms and the roots in the autoregressive coefficients. In order to test whether our results are robust to the exclusion of deterministic terms we also estimate our VAR models excluding the constant and the linear trend. The results are not affected qualitatively and there are only minor quantitative differences at longer horizons, with fiscal shocks exhibiting somewhat stronger long-run effects.

[6] The main conclusions are not affected by the choice of a Bayesian approach rather than a classical approach. As regards the empirical results presented in this paper, the median impulse responses obtained using the Bayesian approach are nearly identical to the point estimate of the responses obtained using the classical approach.

[7] See Uhlig (2005: 409-410) for technical details on the estimation approach.

In the following we present the identification approaches used in the empirical application.

3.1 The recursive approach

The first approach we consider is the recursive approach which restricts B to a k-dimensional identity matrix and A_0 to a lower triangular matrix with unit diagonal, which implies the decomposition of the variance-covariance matrix $\Sigma_u = A_0^{-1}\Sigma_e(A_0^{-1})'$.[8] This decomposition is obtained from the Cholesky decomposition $\Sigma_u = PP'$ by defining a diagonal matrix D which has the same main diagonal as P and by specifying $A_0^{-1} = PD^{-1}$ and $\Sigma_e = DD'$, i.e. the elements on the main diagonal of D and P are equal to the standard deviation of the respective structural shock. The recursive approach implies a causal ordering of the model variables. Note that there are $k!$ possible ordering in total. In this paper we order the variables as follows: spending is ordered first, output is ordered second, inflation is ordered third, tax revenue is ordered fourth and the interest rate is ordered last. This implies that the relation between the reduced-form disturbances u_t and the structural disturbances e_t takes the following form:

$$\begin{bmatrix} 1 & 0 & 0 & 0 & 0 \\ -\alpha_{yg} & 1 & 0 & 0 & 0 \\ -\alpha_{\pi g} & -\alpha_{\pi y} & 1 & 0 & 0 \\ -\alpha_{\tau g} & -\alpha_{\tau y} & -\alpha_{\tau\pi} & 1 & 0 \\ -\alpha_{rg} & -\alpha_{ry} & -\alpha_{r\pi} & -\alpha_{r\tau} & 1 \end{bmatrix} \begin{bmatrix} u_t^g \\ u_t^y \\ u_t^\pi \\ u_t^\tau \\ u_t^r \end{bmatrix} = \begin{bmatrix} 1 & 0 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 & 0 \\ 0 & 0 & 1 & 0 & 0 \\ 0 & 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 0 & 1 \end{bmatrix} \begin{bmatrix} e_t^g \\ e_t^y \\ e_t^\pi \\ e_t^\tau \\ e_t^r \end{bmatrix}. \tag{3}$$

This particular ordering of the variables has the following implications: (i) Government spending does not react contemporaneously to shocks to other variables in the system, (ii) output does not react contemporaneously to tax, inflation and interest rate shocks, but is affected contemporaneously by spending shocks, (iii) inflation does not react contemporaneously to tax and interest rate shocks, but is affected contemporaneously by government spending shocks, (iv) taxes do not react contemporaneously to interest rate shocks, but are affected contemporaneously by government spending, output and inflation shocks, and (v) the interest rate is affected contemporaneously by all shocks in the system. Note that after the initial period the variables in the system are allowed to interact freely, i.e., for example, tax shocks can affect output in all periods after the one in which the shock occurred.

[8] See, e.g., Lütkepohl (2005: 58).

The assumptions on the contemporaneous relations between the variables can be justified as follows: Movements in government spending, unlike movements in taxes, are largely unrelated to the business cycle. Therefore, it seems plausible to assume that government spending is not affected contemporaneously by shocks originating in the private sector. Ordering output and inflation before taxes can be justified on the grounds that shocks to these two variables have an immediate impact on the tax base and, thus, a contemporaneous effect on tax receipts. This particular ordering of variables, thus, captures the effects of automatic stabilizers on government revenue, while it rules out (potentially important) contemporaneous effects of discretionary tax changes on output and inflation. Ordering the interest rate last can be justified (i) on the grounds of a central bank reaction function implying that the interest rate is set as a function of the output gap and inflation, and (ii) given that spending and revenue as defined here (net of interest payments) are not sensitive to interest rate changes.

3.2 The Blanchard-Perotti approach

The identification approach due to Blanchard and Perotti (2002) relies on institutional information about tax and transfer systems and about the timing of tax collections in order to identify the automatic response of taxes and government spending to economic activity. This identification scheme relies on a two-step procedure: In a first step, the institutional information is used to estimate cyclically adjusted taxes and government expenditures. In a second step, estimates of fiscal policy shocks are obtained. Blanchard and Perotti (2002) and Perotti (2005) applied this approach to estimate the effects of government spending and tax shocks for the United States. This subsection relies on the identification scheme used by Perotti (2005) as he also used a five-variable VAR model while Blanchard and Perotti's (2002) analysis built on a three-variable system. Adapting Perotti's (2005) starting point to our context, the relationship between the reduced-form disturbances u_t and the structural disturbances e_t can be written as

$$u_t^g = \alpha_{gy}u_t^y + \alpha_{g\pi}u_t^\pi + \alpha_{gr}u_t^r + \beta_{g\tau}e_t^\tau + e_t^g, \quad (4)$$

$$u_t^\tau = \alpha_{\tau y}u_t^y + \alpha_{\tau\pi}u_t^\pi + \alpha_{\tau r}u_t^r + \beta_{\tau g}e_t^g + e_t^\tau, \quad (5)$$

$$u_t^y = \alpha_{yg}u_t^g + \alpha_{y\tau}u_t^\tau + e_t^y, \quad (6)$$

$$u_t^\pi = \alpha_{\pi g}u_t^g + \alpha_{\pi y}u_t^y + \alpha_{\pi\tau}u_t^\tau + e_t^\pi, \quad (7)$$

$$u_t^r = \alpha_{rg}u_t^g + \alpha_{ry}u_t^y + \alpha_{r\pi}u_t^\pi + \alpha_{r\tau}u_t^\tau + e_t^r, \quad (8)$$

Note that the above system of equations is not identified. The variance-covariance matrix of the reduced-form disturbances has ten distinct elements whereas the above system of equations has 17 free parameters. Unlike the recursive approach the Blanchard-Perotti approach does not involve imposing (only) zero restrictions on seven parameters in order to achieve identification. The first step of the estimation strategy consists in an adjustment of government spending and revenue for the automatic response of these variables to the business cycle and inflation. For this purpose, Perotti (2005) regresses individual revenue items on their respective tax base, obtaining an aggregate value for the output elasticity of government revenue ($\alpha_{\tau y}$) of 1.85 and an aggregate value for the inflation elasticity of government revenue ($\alpha_{\tau\pi}$) of 1.25. Since government spending is defined net of transfers and, thus, acyclical, Perotti (2005) sets the output elasticity of government spending (α_{gy}) equal to zero. He sets the inflation elasticity of government spending ($\alpha_{g\pi}$) equal to -0.5, arguing that nominal wages of government employees, which account for a large part of government consumption, do not react contemporaneously to changes in inflation implying that the government wage bill declines in real terms if there is an unanticipated increase in inflation. In addition, he sets the interest rate elasticities of government spending (α_{gi}) and net taxes ($\alpha_{\tau i}$) equal to zero, respectively, because interest payments paid and received by the government are excluded from the definition of spending and net taxes. Finally, he sets the parameter $\beta_{g\tau}$ equal to zero, which is equivalent to saying that government decisions on spending are taken before decisions on revenue. Imposing these restrictions on the parameter values the relation between the reduced-form and the structural disturbances can be written in matrix form as[9]

$$\begin{bmatrix} 1 & 0 & 0.5 & 0 & 0 \\ -\alpha_{yg} & 1 & 0 & -\alpha_{y\tau} & 0 \\ -\alpha_{\pi g} & -\alpha_{\pi y} & 1 & -\alpha_{\pi\tau} & 0 \\ 0 & -1.85 & -1.25 & 1 & 0 \\ -\alpha_{rg} & -\alpha_{ry} & -\alpha_{r\pi} & -\alpha_{r\tau} & 1 \end{bmatrix} \begin{bmatrix} u_t^g \\ u_t^y \\ u_t^\pi \\ u_t^\tau \\ u_t^r \end{bmatrix} = \begin{bmatrix} 1 & 0 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 & 0 \\ 0 & 0 & 1 & 0 & 0 \\ \beta_{\tau g} & 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 0 & 1 \end{bmatrix} \begin{bmatrix} e_t^g \\ e_t^y \\ e_t^\pi \\ e_t^\tau \\ e_t^r \end{bmatrix}. \quad (9)$$

Comparing this system of equations with the system for the recursive approach reveals the following differences between the two identification approaches: Whereas in the recursive approach all elements of A_0 above the principal diagonal are restricted to zero, there are three exceptions in Perotti's identification approach. These exceptions are potentially

[9] Since the structural parameters collected in A_0 and B are nonlinearly related to the reduced-form parameters, a closed form of the maximum likelihood estimates does not exist, necessitating the use of an iterative optimizing algorithm to compute the estimates. We use the Broyden-Fletcher-Goldfarb-Shanno algorithm implemented in RATS (see Doan 2004).

important when the responses to a tax shock are considered. By fixing the size of automatic stabilizers Perotti (2005) is able to freely estimate the contemporaneous effect of taxes on output and inflation whereas the recursive approach freely estimates the size of automatic stabilizers while imposing a zero restriction on the contemporaneous effect of taxes on output and inflation. Surprisingly, the empirical analysis suggests that the conceptual differences between the recursive approach and the Blanchard-Perotti approach have little effect on the results—for the benchmark value of the output elasticity of net taxes imposed for the Blanchard-Perotti approach.

3.3 The sign-restrictions approach

The third approach identifies fiscal policy shocks via sign restrictions on the impulse responses. Unlike the recursive approach and the Blanchard-Perotti approach, the sign-restrictions approach does not require the number of shocks to be equal to the number of variables and it does not impose linear restrictions on the contemporaneous relation between reduced-form and structural disturbances. Rather, Mountford and Uhlig (2005) impose restrictions directly on the shape of the impulse responses and identify four shocks: a business cycle shock, a monetary policy shock, a government spending shock and a tax shock. In our application we identify a business cycle shock, a government spending shock and a tax shock. We disregard the monetary policy shock because it is not the focus of this paper and because the results are not sensitive to the (non)identification of this shock. We impose the following sign restrictions on the impulse responses: The business cycle shock is identified by the requirement that the impulse responses of output and taxes are positive for at least the four quarters following the shock. This turns out to be the crucial identifying assumption, having implications also for the identification of the fiscal policy shocks. The tax shock is identified by the requirements that the impulse responses of taxes are positive for at least the four quarters following the shock, while the government spending shock is identified by the requirements that the impulse responses of government spending are positive for at least the four quarters following the shock. In addition, both shocks are required to be orthogonal to the business cycle shock identified in the first step. The assumption that the business cycle shock comes first rules out that the responses of the model variables to a fiscal policy shock all have the same sign as those to a business cycle shock. In practice, this assumption brings about that whenever taxes and output move in the same direction, this is attributed to a change in the business cycle. It is, thus, unlikely that an increase (fall) in taxes generates an increase (fall) in output, a phenomenon which has received some attention in the recent literature on the effects of

fiscal policy under the label "expansionary fiscal contractions" (see, e.g., Giavazzi et al. 2000). As a consequence it might be that the sign-restrictions approach overstates the (negative) output effects of a tax shock.

Following Uhlig (2005) we write the relationship between the reduced-form disturbances u_t and the structural shocks e_t as $u_t = Be_t$, with $E[u_t u_t'] = \Sigma_u$ and $E[e_t e_t'] = I$. Note that e_t is a m-dimensional vector with $m \leq k$, i.e. unlike in the two approaches discussed above it is not necessary to identify as many shocks as there are variables. In our setup, for example, we identify three shocks using the sign-restrictions approach while there are five or six variables in the estimated VAR models. For the implementation of the sign-restrictions approach Mountford and Uhlig (2005) decompose the matrix B into two components, $B = PQ$, where P is the lower triangular Cholesky factor of Σ_u and Q is an orthonormal matrix with $QQ' = I$. Note that the matrix P, which serves to identify the structural shocks in the recursive approach, here merely serves a useful computational tool without affecting the results. Instead, the matrix Q plays the crucial role in the sign-restrictions approach because it collects the identifying weights with each column of Q corresponding to a particular structural shock. We use the penalty function approach described in detail in Mountford and Uhlig (2005: Appendix A) to compute the individual elements of Q. The penalty function approach consists in minimizing a criterion function, which penalizes impulse responses violating the sign restrictions, with respect to the identifying weights. We take a number of draws from the posterior of the VAR coefficients and the variance-covariance matrix of the reduced-form residuals. For each draw we identify the three structural shocks. In all estimations we take as many draws as are necessary to obtain 500 draws satisfying the sign restrictions.

3.4 The event-study approach

Following the work of Ramey and Shapiro (1998) parts of the literature have tried to avoid the identification problem inherent in structural VAR analysis and have instead looked for fiscal episodes which can be seen as exogenous with respect to the state of the economy. Ramey and Shapiro (1998) have argued that the large increases in military spending associated with the onset of the Korean war, the Vietnam war and the Reagan military buildup can be seen as such exogenous events. Later, Eichenbaum and Fisher (2005) have argued that the expansion of defense spending in the aftermath of 9/11 can also be viewed as such an exogenous event. We follow the literature and define a dummy variable, D_t, which takes on the value of 1 in the first quarter of 1965, i.e. at the onset of the Vietnam war, in the first quarter of 1980, i.e. at the onset of the Reagan military

buildup, and in the third quarter of 2001, i.e. at the onset of the war on terrorism following 9/11. Our sample excludes the Korean war, which occured in the early 1950s.[10] Including the dummy variable in the empirical model, our baseline reduced-form VAR model given by equation (1) is replaced by the following reduced form:

$$X_t = \mu_0 + \mu_1 t + A(L)X_{t-1} + \Phi(L)D_t + u_t, \tag{10}$$

where $\Phi(L)$ is the 4th-order lag polynomial associated with the dummy variable capturing the above-mentioned fiscal episodes.

4 Results for the pure fiscal shocks

This section presents empirical results for pure government spending and tax shocks, i.e. for shocks to one fiscal variable at a time without constraining the response of the respective other fiscal variable. Instead, Section 6 presents results for selected policy experiments. The impulse responses presented in this section are scaled as follows: As regards the responses of output and its components as well as the fiscal variables, the original impulse responses are transformed such as to give the dollar response of each variable to a dollar shock in one of the fiscal variables.[11] For this purpose we follow the procedure of Blanchard and Perotti (2002) and first divide the original impulse responses by the standard deviation of the respective fiscal shock in order to have shocks of size one percent. These impulse responses are then divided by the ratio of the respective variable and the shocked fiscal variable, where the ratio is evaluated at the sample mean. The major advantage of this transformation is that the responses of output to the fiscal shocks can be interpreted as (non-accumulated) multipliers. As regards the responses of inflation, wages and employment, they give the percentage change of each variable in response to a one-percent fiscal shock. Finally, the responses of the interest rate are expressed as change in percentage points for a one-percent fiscal shock. For each variable we report the median

[10] This omission affects the results for the event-study approach. Perotti (2007) shows that the consumption response to a spending increase is negative if the Korean war is included in the analysis, while it is positive if it is excluded. We opted for the sample starting in 1955 for two reasons. First, it avoids our results being affected by the lagged effects of World War II. Second, Perotti (2007) shows that the military build-up associated with the Korean War was very different in nature from the later episodes in that it was entirely tax-financed.

[11] For the event-study approach the impulse responses are not transformed using this method because the impact change in government spending is close to zero for this approach. Instead, we report the percentage change in all variables in response to a unit increase in the dummy variable capturing the Ramey-Shapiro episodes.

as well as the 16% and 84% fractiles of the posterior distribution of the impulse responses.

4.1 The pure spending shock

The impulse responses for a pure spending shock are shown in Figure 2, with the individual columns displaying the results for the alternative identification approaches.[12] The figure reveals a number of interesting findings. Firstly, the identified government spending shocks are the same for all identification approaches except for the event-study approach.[13] According to the latter approach government spending does not change much at the onset of a fiscal episode whereas according to the other approaches the increase in government spending is close to its peak on impact. Secondly, the results show that taxes on impact at most partly offset the increase in government spending. For the sign-restrictions approach, e.g., taxes hardly change at all in the first year suggesting that in this case the pure spending shock can be interpreted as a deficit-financed spending shock. In contrast, taxes increase by about 50 cents on impact for the recursive and the Blanchard-Perotti approaches. These differences in the tax responses potentially limits the comparability of the results across approaches. We tackle this issue in Section 6. Thirdly, despite the differences in tax responses for alternative identification approaches the responses of non-fiscal variables show striking similarities. For all approaches real GDP persistently increases in response to a government spending shock, following a hump-shaped pattern. Moreover, for all approaches but the event-study approach the spending multiplier peaks after three to four years at a value of around 2. A persistent and hump-shaped increase also obtains for the response of private consumption.[14] The results also show that hours worked do not change significantly for all approaches considered, while the real product wage strongly and persistently increases according to all approaches but the event-study approach. According to the latter approach the real wage falls somewhat but the responses are not statistically significant at any horizon. As regards private residential and nonresidential investment, the responses are small in general and not statistically significant. Inflation

[12] In all figures we use the following acronyms: RA for the recursive approach, BP for the Blanchard-Perotti approach, SR for the sign-restrictions approach and ES for the event-study approach. The acronyms used for the variables are explained in the Data Appendix.

[13] In the case of the recursive approach and the Blanchard-Perotti approach not only the responses of government spending but also all other responses are virtually identical. This is not surprising given that the spending shock is identified in the same way for both approaches, namely by ordering government spending first (compare the first row of matrix A in equations (3) and (9)).

[14] In the case of the sign-restrictions approach the output and consumption responses are not statistically significant at horizons up to one year. In the case of the event-study approach the consumption and output responses die out more quickly than for the approaches and are statistically significant only at the one to three years horizons.

and the short-term interest rate increase with a lag of around two years.

As discussed in the Introduction one branch of the theoretical literature has taken as stylized fact that private consumption increases in response to a government spending shock. The evidence presented here suggests that private consumption indeed increases in response to a positive government spending shock and that the responses of labor market variables seem to be important to rationalize the consumption response. Our empirical results support models which generate an increase in the real wage but at the same time do not support the increase in employment implied by most current-generation DSGE models.[15] A further challenge arising from the empirical evidence is that the positive responses of private consumption and the real wage are very persistent, whereas most current-generation DSGE models consistent with an increase in these variables predict that the responses turn negative already about one year after the government spending shock occurs (see e.g. Galí et al. 2007).

4.2 The pure tax shock

The impulse responses for a pure tax shock are shown in Figure 3. Results are presented for all approaches but the event-study approach, which is only suitable for the analysis of spending shocks. The results shown in the figure reveal that while all approaches agree on the responses of fiscal variables there is a strong discrepancy as regards the responses of non-fiscal variables. Turning first to the fiscal variables, the tax response peaks in the quarter when the shock occurs and then monotonically declines to die out after about three years while government spending does not react at all. The pure tax shock can, thus, be interpreted as a "deficit-reducing tax increase" policy experiment. As regards the responses of the non-fiscal variables, there is disagreement on the effects of pure tax shocks between the recursive and Blanchard-Perotti approaches on the one hand and the sign-restrictions approach on the other hand. While the results for the latter approach suggest that unanticipated tax increases have strong distortionary effects, the results for the two other approaches suggest that tax shocks hardly have any effects on the real economy.[16] In the case of the sign-restrictions the decline in GDP peaks at about 1.2 dollars after around one year whereas the GDP response is never significantly different from zero according to

[15] Our results regarding the consumption, employment and wage responses to government spending shocks confirm the evidence presented in Perotti (2007).

[16] In the case of the recursive and Blanchard-Perotti approaches only the labor market variables show statistically significant responses, with hours worked declining and the real product wage increasing in response to a tax shock. The increase in the product wage can be attributed to the fact that this is a gross wage including labour taxes and social contributions.

the other two approaches.

A surprising finding is that the results for the recursive approach and the Blanchard-Perotti approach are nearly identical also for the pure tax shock. A priori, it could be expected that the results for these approaches differ because the recursive approach restricts the short-run output effect of a pure tax shock to be zero while the Blanchard-Perotti approach does not. Our baseline results suggest that this conceptual difference does not matter much given that the impact response of output is close to zero for the Blanchard-Perotti approach. This result is in line with the results reported by Perotti (2005: Figure 3) but stands in contrast to the results of Blanchard and Perotti (2002: Figure III) who report that output decreases by around 70 cents on impact in response to a pure revenue shock.[17] In contrast, our results for the sign-restrictions approach are similar in magnitude to those reported by Blanchard and Perotti (2002). Before turning to the implications of the tax shock results for selected policy experiments (see Section 6) the next section provides evidence explaining the differences in results obtained for the pure tax shock.

5 The size of automatic stabilizers and the output effects of exogenous tax shocks

This section shows that the striking coincidence of results for the recursive and Blanchard-Perotti approaches as well as the strong disagreement between these two approaches and the sign-restrictions approach can be traced back to the same underlying source: the size of automatic stabilizers estimated or calibrated for alternative identification approaches. At first sight it may seem surprising that the size of automatic stabilizers matters for the size and even sign of the macroeconomic effects of discretionary tax changes. Yet, there is a simple reason why they must be closely related: What identification of tax shocks does is to separate the correlation between the residuals in the GDP equation and the residuals in the tax equation into two components—the automatic response of taxes to unexpected changes in GDP (automatic stabilizers) and the response of GDP to unexpected changes in taxes not related to the business cycle (the output effects of discretionary tax changes). For example, the estimation results for the reduced-form 5-equation VAR model show that the correlation coefficient between the tax residuals and

[17] Perotti (2005: Figure 3) presents empirical evidence for five OECD countries. The impact output response to an unanticipated tax increase is zero in four cases and even positive in the case of Australia (note that Perotti presents results for a tax cut). For two countries (Australia and the United Kingdom) the output response is positive at horizons larger than 1.

the GDP residuals is positive and equal to 0.42.[18] This positive correlation could a priori be compatible with very different views about the relation between taxes and output: firstly, it could be exclusively due to automatic stabilizers, implying that discretionary tax shocks do not have any effects (this is suggested by the results for the baseline recursive and Blanchard-Perotti approaches); secondly, it could be that automatic stabilizers taken on their own would suggest an even larger positive correlation, partly offset by negative effects of discretionary tax increases (this is what the results for the sign-restrictions approach suggest); and, thirdly, it could be that automatic stabilizers are not large enough to explain all of the positive correlation but that the remainder is explained by discretionary tax changes not having negative but instead positive output effects in line with the literature on "expansionary fiscal contractions".

Figure 4 presents evidence underlining the empirical relevance of the explanations given above. The figure shows the impact response of GDP to a pure revenue shock, i.e. the GDP response in the period when the tax shock occurs, for alternative values of the output elasticity of net taxes ($\alpha_{\tau y}$) using the Blanchard-Perotti approach. Recall that $\alpha_{\tau y}$ measures the size of automatic stabilizers in the Blanchard-Perotti approach. For the baseline results presented in Section 4.2 we calibrate the output elasticity of net taxes using the same value as Perotti (2005), i.e. we set $\alpha_{\tau y}$ equal to 1.85. For this value the impact response of GDP to a pure revenue shock is close to zero. The figure also shows the impact response of GDP for other calibrated values of $\alpha_{\tau y}$ over the range from 0 to 4. As can be seen the relationship between the output elasticity of net taxes and the impact response of GDP is almost linear. In the absence of automatic stabilizers ($\alpha_{\tau y} = 0$) the impact response of GDP is positive and amounts to around 65 cents. The impact response remains positive but gets successively smaller as the output elasticity of net taxes increases until the latter reaches a value around 1.9. The impact response of GDP becomes negative as the output elasticity exceeds 2. These results show how sensitive the results for the Blanchard-Perotti approach are to the calibrated value of the output elasticity of net taxes.

Now it is also easy to understand why the baseline results for the recursive approach and the Blanchard-Perotti approach are nearly identical. Recall that in the recursive approach the output elasticity of net taxes is treated as free parameter in the estimation. We obtain a point estimate of 1.93, which is nearly identical to the value imposed for the Blanchard-Perotti approach. This explains the striking similarity of the results for these two approaches. Also, it is easy to see why the recursive approach is not well-suited for

[18] Blanchard and Perotti (2002) report a very similar value (0.38) for their 3-variable VAR model estimated over the period 1960-1997.

the analysis of tax shocks. By ordering the variables the recursive approach either sets the impact response of GDP equal to zero (GDP ordered before taxes) or sets the size of automatic stabilizers equal to zero (taxes ordered before GDP). It is because we have chosen the first of these two options as our baseline that the results for the recursive and Blanchard-Perotti approaches are so strikingly similar.

The figure also shows that it would be possible to obtain results for the Blanchard-Perotti approach which resemble those obtained for the sign-restrictions approach. Recall that Blanchard and Perotti (2002) and Perotti (2005) calibrate the value of $\alpha_{\tau y}$ based on a first-step cyclical-adjustment procedure applied outside the VAR model. Instead, it is possible to estimate rather than to impose the output elasticity of net taxes inside the structural VAR model for the Blanchard-Perotti approach. For this purpose it is necessary to restrict another parameter in the estimation and we choose to set $\beta_{\tau g}$ equal to zero in order to ensure identification.[19] If the output elasticity of net taxes is freely estimated a point estimate of 2.98 obtains, which translates into a negative impact response of GDP to a revenue shock of almost 50 cents.[20] The results provided by the Blanchard-Perotti approach would, thus, appear not to differ significantly from those obtained for the sign-restrictions approach for the pure tax shock and, consequently, for the policy experiments. Moreover, the results for the sign-restrictions approach suggest that a value for the output elasticity of net taxes of 2.98 is not implausibly large. Figure 5 shows the responses of GDP and government revenue for the business cycle shock scaled such that the impact response of GDP is equal to 1 percent. The impact response of government revenue to the business cycle shock can be interpreted as measuring the size of automatic stabilizers and is, thus, comparable to the output elasticity of net taxes in the Blanchard-Perotti approach. The results suggest that government revenue increases by 3.49 percent on impact in response to the business cycle shock, with a 68% confidence band ranging from 2.97 to 3.90.

All in all, the results provided in this section suggest that there is considerable uncertainty regarding the size of automatic stabilizers. The recursive approach and the "calibrated" version of the Blanchard-Perotti approach suggest that automatic stabilizers are relatively small compared to the sign-restrictions approach and the "estimated" version of the Blanchard-Perotti approach. The uncertainty about the magnitude of automatic stabilizers translates into uncertainty about the degree of distortion associated

[19] Setting this parameter to zero does not affect the results. This statement is based on the benchmark Blanchard-Perotti structural VAR model where setting this parameter to zero gives an overidentifying restriction which can be tested and in our context cannot be rejected.

[20] The standard deviation of the estimate of $\alpha_{\tau y}$ is 1.01, revealing that the uncertainty surrounding this parameter is quite large.

with a given tax shock and, as is shown in Section 6 below, also about the effects of policy experiments. We interpret our results as indicating a need for a refinement of the way taxes are adjusted for the effects of the business cycle in structural VAR models.

6 Results for the policy experiments

Most studies in the literature report the effects of pure fiscal shocks only. However, as Mountford and Uhlig (2005) pointed out, pure fiscal shocks are not connected to the policy experiments considered in the theoretical literature or by policymakers. The reason is that pure fiscal shocks do not restrict the time paths of both fiscal variables. As a consequence it is not possible to answer questions such as "What are the effects of a tax-financed compared to a deficit-financed spending increase?" on the basis of the results for the pure spending shock because the identification of this shock does not restrict the response of taxes. Yet, there is an easy way to construct meaningful policy experiments on the basis of the results for pure spending and tax shocks. Following Mountford and Uhlig (2005) such policy experiments can be constructed as linear combinations of the two pure fiscal shocks. This section presents the results for three alternative policy experiments: a deficit-financed spending increase, a balanced-budget spending increase and a deficit-financed tax cut.

6.1 The deficit-financed spending increase

The deficit-financed spending increase is defined as an increase in government spending by 1$ for four quarters while taxes remain unchanged. This policy experiment is obtained by linearly combining the sequence of the two pure fiscal policy shocks that causes these responses in the two fiscal variables. The impulse responses for this policy experiment are shown in Figure 6. As can be expected from the discussion in Section 4.1 the dynamics of the non-fiscal variables are very similar to those reported for the pure government spending shock. As regards the recursive and the Blanchard-Perotti approaches, this similarity stems from the results obtained for the pure tax shock indicating that tax shocks do not have any significant effects on non-fiscal variables. For the sign-restrictions approach, this similarity is due to the fact that taxes do not react to pure government spending shocks, implying that pure spending shocks are deficit-financed. The main message from Figure 6 is that output, private consumption and the real product wage all increase in response to a deficit-financed spending shock, while employment remains unchanged. Except for the output response these results are inconsistent with the standard neoclassical model. Yet, except for the employment response and the persistence of the other responses, they are

consistent with the recent theoretical literature discussed in the Introduction.

6.2 The balanced-budget spending increase

The balanced-budget spending increase is defined as an increase in both government spending and taxes by 1$ for four quarters. The impulse responses for this policy experiment are shown in Figure 7. As regards the recursive approach and the Blanchard-Perotti approach, the results for the balanced-budget spending increase are again very similar to those reported in Figure 2 for the pure spending shock. For these approaches it makes little difference whether a spending increase is deficit-financed or tax-financed because as shown in Section 4.2 tax shocks hardly have any effect on the non-fiscal variables. In contrast, as regards the sign-restrictions approach, the results for the balanced-budget spending increase are markedly different from those reported for the pure spending shock (Figure 2) and for the deficit-financed spending increase (Figure 6). Output, consumption, investment and hours worked all significantly fall for about two to three years in response to a balanced-budget spending increase. One possible interpretation of this finding is that the rise in distortionary taxes necessary to match the spending increase has strong disincentive effects which entail a decline in output. For example, the standard neoclassical growth model analyzed by Baxter and King (1993) predicts that output and employment decrease if a spending increase is financed with distortionary taxes while they increase if the increase is financed with lump-sum taxes (which is equivalent to deficit-finance in their model). All in all, the results for this policy experiment suggest that if one trusts the results for the recursive and Blanchard-Perotti approaches the theoretical literature's modeling choice of lump-sum taxes over distortionary taxes is innocuous whereas if one trusts the results for the sign-restrictions approach this choice is problematic. Most importantly, according to the results for the latter approach the sign of the fiscal multiplier depends on the financing alternative.

6.3 The deficit-financed tax cut

The deficit-financed tax cut is defined as a fall in taxes by 1$ for four quarters while government spending remains unchanged. The impulse responses for this policy experiment are shown in Figure 8. The impulse responses are the mirror image of the responses depicted in Figures 3 for the pure tax shock. The main reason for this similarity is that government spending does not respond very strongly to a pure tax shock, implying that the pure tax shock can be interpreted as a deficit-reducing tax increase. Thus, for the

deficit-financed tax cut the results for the recursive and Blanchard-Perotti approach indicate that none of the non-fiscal variables shows any significant response, whereas the results for the sign-restrictions approach suggest that output, consumption, investment, employment, inflation and the interest rate increase in the short to medium run, while the real product wage falls. Put differently, the recursive and Blanchard-Perotti approaches suggest that Ricardian Equivalence is a good approximation of economic reality, while the sign-restrictions approach suggests that taxes are strongly distortionary. The uncertainty about whether Ricardian Equivalence is supported by the data again points to the importance of a better modeling and understanding of the effects of tax shocks.

7 Robustness

This section presents the results of various sensitivity analyses regarding the specification of the reduced-form VAR model, subsample stability and the use of alternative definitions for key variables.

7.1 Reduced-form VAR specification

As concerns the specification of the reduced-form VAR model, the results presented for the benchmark specification are robust to the following alternative specifications[21]: (i) use of a 6-th order lag polynomial instead of a 4-th order lag polynomial, (ii) inclusion of a quadratic time trend among the deterministic terms, (iii) inclusion of a dummy variable capturing the Ramey and Shapiro (1998) episodes also in the baseline VAR model, (iv) inclusion of a dummy variable capturing the tax rebate in the second quarter of 1975, as in BP (2002), and (v) trend break in 1973:2 as in Burnside et al. (2004). The fact that different approaches agree under different specifications of the reduced-form VAR does not necessarily mean that specification issues are not relevant. We change one component of the reduced-form VAR at a time, while in the literature the specification of the reduced-form VAR models sometimes substantially differs. In particular, differences can arise because of the inclusion of a different number of variables, different definitions of the time series meant to capture the same concept[22], different data sources as well as different sample sizes and periods. For example, as mentioned earlier the results for the event-study approach are sensitive to the starting date of the sample.

[21] Only minor quantitative deviations from the baseline results are recorded. Detailed results are available on request.

[22] As an important example consider the implications of the different definitions of net tax series used by Blanchard and Perotti (2002) and Perotti (2005) discussed in the Introduction.

7.2 Subsample stability

Perotti (2005) presents evidence suggesting that the transmission of fiscal policy shocks has changed over time. In particular, the responses of GDP and its components appear to have become weaker in the post-1980 period. Bilbiie et al. (2006) show that the same holds for the responses of the real wage. In order to check whether our main results are stable we split our sample into two subperiods: 1955:1 - 1979:3 (99 observations) and 1983:1-2006:4 (96 observations). We follow the literature and exclude the period 1979:4 to 1982:4 in order to avoid that the shorter samples are affected by the substantial changes in the monetary policy framework that occurred around that time.[23] Figure 9 reports the results for a pure spending shock for the two subsamples. The results suggest that for both subsamples the pure spending shocks can be interpreted as deficit-financed spending increases. There is some evidence that the effects of spending shocks have become somewhat weaker and less persistent over time but the results do not suggest that the changes were dramatic. Most importantly, the qualitative effects of government spending shocks do not seem to have changed much over time. The responses of output, private consumption and the real product wage are significantly positive for both subsamples, with the response of consumption showing the familiar hump-shaped pattern. The only response that significantly changes is the one of hours worked. The response is positive in the first subsample but negative in the second subsample, which explains why the full sample results suggest that hours worked do not react significantly. In any case, the current-generation DSGE literature is inconsistent with either unchanged hours worked or declining hours worked.

7.3 Alternative measures of consumption, employment and the real wage

Figure 10 presents impulse responses to a pure spending shock for alternative definitions of key variables using the baseline 6-variable VAR model estimated over the full sample. Firstly, we split private consumption into its durable and nondurable subcomponents. The results show that consumption of both durable and nondurable goods increase in response to a pure spending shock, with the hump-shaped pattern being more pronounced in the case of durable goods. Secondly, we show results for three alternative definitions of employments. The baseline model uses total economy hours worked, which do not react

[23] Note that this choice of subsamples excludes the Carter-Reagan military build-up. For the event-study approach the first subsample includes the Vietnam war while the second subsample includes the military operations following 9/11.

significantly to a spending shock. The same is true for hours worked (the intensive margin) and the number of employees (the extensive margin) in the business sector. In contrast, the number of government employees significantly increases in response to a spending shock, which is in line with the empirical findings documented in the literature (Cavallo 2005). Finally, we show results for three alternative definitions of the real wage. The baseline model uses the real product wage in the business sector, which strongly increases in response to a spending shock. The same is true for the real product wage in the manufacturing sector. In contrast, the increase in real consumption wages in the business and manufacturing sectors is less pronounced and in general statistically insignificant. All in all, we interpret the evidence provided in this section as showing that the baseline findings presented in this paper are quite robust.

8 Conclusions

This paper presents an extensive comparative study on the empirical literature using vector autoregressive models to assess the effects of fiscal policy shocks. The starting point of our analysis is that there is strong disagreement in the literature not only on the quantitative but also on the qualitative effects of fiscal policy shocks. We provide new evidence for the U.S. over the period 1955-2006. We show that, controlling for differences in specification of the reduced-form model, all identification approaches used in the literature yield qualitatively and quantitatively very similar results as regards government spending shocks. In response to such shocks real GDP, real private consumption and the real wage all significantly increase following a hump-shaped pattern, while private employment does not react. Our empirical results support theoretical models which generate an increase in private consumption and the real wage but at the same time do not support the increase in employment implied by most current-generation DSGE models. A further challenge arising from the empirical evidence is that the positive responses of private consumption and the real wage are very persistent, whereas most current-generation DSGE models consistent with an increase in these variables predict that the responses turn negative already about one year after the government spending shock occurs.

In contrast, we find strongly diverging results as regards the effects of tax shocks depending on the identification approach used, with the estimated effects of unanticipated tax increases ranging from non-distortionary to strongly distortionary. We show that the differences in results can to a large extent be traced back to differences in the automatic response of tax revenues to the business cycle (automatic stabilizers) estimated or calibrated

for alternative identification approaches, with the degree of distortion associated with a given tax shock being positively related to the estimated size of automatic stabilizers. This uncertainty about the effects of tax shocks also translates into uncertainty about the effects of policy experiments. As regards the effects of balanced-budget spending increases, e.g., our results show that the sign of the fiscal multiplier depends on the degree to which taxes are estimated to be distortionary. We interpret our results as indicating a need for a better modeling of the effects of tax shocks and, in particular, for a refinement of the way taxes are adjusted for the effects of the business cycle in structural VAR models.

Latest studies in this literature have pointed out two interesting extensions to the baseline VAR models used to assess the effects of fiscal policy shocks. Firstly, since fiscal policy measures are in general announced in advance of their implementation the standard fiscal VAR model—assuming that fiscal policy shocks are unanticipated—might be misspecified. Two recent VAR-based studies accounting for announcement effects disagree on whether these effects matter. Tenhofen and Wolff (2007) provide evidence showing that the response of private consumption to a government spending shock turns negative once the empirical model accounts for announcement effects, whereas Mountford and Uhlig (2005) find that the response of private consumption is positive if announcement effects are accounted for. Yet, it is questionable whether VAR models are the appropriate tool to gauge the importance of announcement effects. Introducing fiscal policy foresight into an otherwise standard DSGE model Yang (2005) shows that the data-generating process is not invertible and, as a consequence, does not have a VAR representation but instead a VARMA (vector autoregressive moving average) representation. Secondly, the standard VAR model does not explicitly take into account fiscal solvency considerations and it can, thus, not be ruled out a priori that the estimated fiscal shocks and their transmission imply explosive debt dynamics. Chung and Leeper (2007) address this issue by imposing a debt-stabilizing condition derived from the intertemporal government budget constraint on the estimated VAR model. Their results suggest that imposing fiscal solvency has quantitatively important implications at very long horizons, whereas responses at the horizons considered here (up to ten years) are not strongly affected. Similarly, Favero and Giavazzi (2006) show that including government debt in the set of observable variables has important implications for the response of interest rates to fiscal policy shocks, whereas the responses of other macroeconomic variables—which are the focus of our analysis—are not strongly affected by the inclusion of this variable. We leave the detailed exploration of these issues for future research.

Data Appendix

The data were taken from four sources. The components of national income, government receipts and the GDP deflator were taken from the NIPA tables of the Bureau of Economic Analysis (vintage date: April 27, 2007). Of the remaining series, the interest rate series were taken from the ALFRED database of the Federal Reserve Bank of Saint Louis (vintage dates: May 8, 2007) while the labor market variables—with one exception—were taken from the Bureau of Labor Statistics (BLS) (download date: May 17, 2007). The Francis and Ramey (2005) measure of total hours worked per capita was downloaded from Ramey's homepage http://econ.ucsd.edu/~vramey/ on 16 May 2007. The other series can be obtained free of charge from http://www.bea.gov/histdata/NIyear.asp, http://alfred.stlouisfed.org/ and http://www.bls.gov/, respectively. The components of national income and net taxes are in real per capita terms and were transformed from their nominal values by dividing them by the price index for GDP (NIPA Table 1.1.4, Line 1) and by civilian noninstitutional population (ALFRED Series ID: CNP16OV). All series are seasonally adjusted by the source. For all series except for the interest rates, we took the natural logarithm and multiplied the resulting series by 100, yielding the series used in the estimation. Where necessary we take the arithmetic average of monthly figures to obtain quarterly series.

- **GDP**: 'Gross domestic product'; NIPA Table 1.1.5, Line 1.
- **Private consumption (P_CONS)**: 'Personal consumption expenditures'; NIPA Table 1.1.5, Line 2. 'Durable goods'; NIPA Table 1.1.5, Line 3. 'Nondurable goods'; NIPA Table 1.1.5, Line 4.
- **Government spending (G_SPEN)**: 'Government consumption expenditures and gross investment'; NIPA Table 1.1.5, Line 20).
- **Net taxes (TAX)**: 'Government current receipts' (NIPA Table 3.1 Line 1) minus 'Current transfer payments' (NIPA Table 3.1 Line 17) minus 'Government interest payments' (NIPA Table 3.1, Line 22).
- **Residential investment (R_INV)**: 'Private Fixed Investment - Residential'; NIPA Table 1.1.5, Line 11.
- **Nonresidential investment (NR_INV)**: 'Gross Private Domestic Investment' (NIPA Table 1.1.5, Line 6) minus 'Private Fixed Investment - Residential' (NIPA Table 1.1.5, Line 11).
- **Inflation (INFL)**: Log difference of the price index for GDP (NIPA Table 1.1.4, Line 1).
- **Interest rate (INT)**: '3-Month Treasury Bill: Secondary Market Rate' (ALFRED Series ID: TB3MS).

- **Hours worked (HOURS)**: 'Total Economy Weekly Hours per Capita' from Francis and Ramey (2005).
- **Real compensation (W)**: 'Real Hourly Compensation, Business Sector, Index 1992=100' (BLS Series ID: PRS84006153), deflated by the source using the Consumer Price Index for all urban consumers (CPI-U).

The following series were used for the sensitivity analyses reported in Section 7:

- **Long-term interest rate**: '10-Year Treasury Constant Maturity Rate' (ALFRED Series ID: GS10).
- **Hours worked - Business**: 'Private Business Sector Weekly Hours' devided by noninstitutional population aged 16+, both taken from Francis and Ramey (2005).
- **Employment - Business**: 'Total Private Employees' (BLS Series ID: CES0500000001).
- **Employment - Government**: 'Government Employees' (ALFRED Series ID: USGOVT).
- **Real compensation - Manufacturing**: 'Real Hourly Compensation, Manufacturing Sector, Index 1992=100' (ALFRED Series ID: COMPRMS).
- **Real wage - Business**: 'Average Hourly Earnings of Production Workers, Private Sector' (BLS Series ID: CES3000000008) divided by the CPI (BLS Series ID: CUSR0000SA0).
- **Real wage - Manufacturing**: 'Average Hourly Earnings of Production Workers, Manufacturing Sector' (BLS Series ID: CES3000000008) divided by the CPI (BLS Series ID: CUSR0000SA0).

References

[1] Baxter, M., and R.G. King (1993). Fiscal Policy in General Equilibrium. *American Economic Review* 83 (3): 315–333.

[2] Bilbiie, F. O., A, Meier, and G. J. Müller (2006). What Accounts for the Changes in U.S. Fiscal Policy Transmission? ECB Working Paper 582. Frankfurt am Main.

[3] Blanchard, O.J., and R. Perotti (2002). An Empirical Characterization of the Dynamic Effects of Changes in Government Spending and Taxes on Output. *Quarterly Journal of Economics* 117 (4): 1329–1368.

[4] Burnside, C., M. Eichenbaum and J.D.M. Fisher (2004). Fiscal Shocks and Their Consequences. *Journal of Economic Theory* 115 (1): 89–117.

[5] Cavallo, M. (2005). Government Employment Expenditure and the Effects of Fiscal Policy Shocks. FRB of San Francisco Working Paper 2005-16. San Francisco.

[6] Christiano, L.J., M. Eichenbaum, and C.L. Evans (1999). Monetary Policy Shocks: What Have We Learned and to What End? In J.B. Taylor and M. Woodford (eds.), *Handbook of Macroeconomics*. Vol. 1A. Amsterdam: Elsevier.

[7] Chung, H., and E.M. Leeper (2007). What Has Financed Government Debt? NBER Working Paper 13425. Cambridge, MA.

[8] Doan, T. (2004). RATS Version 6 User's Guide. Evanston, IL.

[9] Edelberg, W., M. Eichenbaum, and J.D.M. Fisher (1999). Understanding the Effects of a Shock to Government Purchases. *Review of Economic Dynamics* 2 (1): 166–206.

[10] Eichenbaum, M., and J.D.M. Fisher (2005). Fiscal Policy in the Aftermath of 9/11. *Journal of Money, Credit and Banking* 37 (1): 1–22.

[11] Fatás, A., and I. Mihov (2001). The Effects of Fiscal Policy on Consumption and Employment: Theory and Evidence. CEPR Discussion Paper 2760. London.

[12] Favero, C.A., and F. Giavazzi (2007). Debt and the Effects of Fiscal Policy. NBER Working Paper 12822. Cambridge, MA.

[13] Francis, N., and V. A. Ramey (2005). Measures of Per Capita Hours and their Implications for the Technology-Hours Debate. NBER Working Paper 11694. Cambridge, MA.

[14] Galí, J., D. Lopéz-Salido, and J. Vallés (2007). Understanding the Effects of Government Spending on Consumption. *Journal of the European Economic Association* 5 (1): 227-270.

[15] Giavazzi, F., T. Jappelli, and M. Pagano (2000). Searching for Non-linear Effects of Fiscal Policy: Evidence from Industrial and Developing Countries. *European Economic Review* 44 (7): 1259–1289.

[16] Linnemann, L (2006). The Effect of Government Spending on Private Consumption: A Puzzle? *Journal of Money, Credit, and Banking* 38 (7): 1715-1736.

[17] Lütkepohl, H. (2005). *New Introduction to Multiple Time Series Analysis*. Berlin: Springer.

[18] Mountford, A., and H. Uhlig (2005). What Are the Effects of Fiscal Policy Shocks? SFB 649 Discussion Paper 2005-039. Humboldt University, Berlin.

[19] Perotti, R. (2005). Estimating the Effects of Fiscal Policy in OECD Countries. CEPR Discussion Paper 168. Center for Economic Policy Research, London.

[20] Perotti, R. (2007). In Search of the Transmission Mechanism of Fiscal Policy. In D. Acemoglu, K. Rogoff, and M. Woodford (eds.), *NBER Macroeconomics Annual 2007*. Forthcoming: Paper version of 30 July 2007.

[21] Ramey, V.A., and M.D. Shapiro (1998). Costly Capital Reallocation and the Effects of Government Spending. *Carnegie-Rochester Conference Series on Public Policy* 48 (June): 145–194.

[22] Ramey, V.A. (2007). Identifying Government Spending Shocks: It's All in the Timing. University of California, San Diego (mimeo).

[23] Ravn, M., S. Schmitt-Grohé, and M. Uribe (2006). Deep Habits. *Review of Economic Studies* 73 (1), 195-218.

[24] Romer, C.D., and D.H. Romer (2007). The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks. NBER Working Paper 13264. Cambridge, MA.

[25] Sims, C.A. (1980). Macroeconomics and Reality. *Econometrica* 48 (1): 1–48.

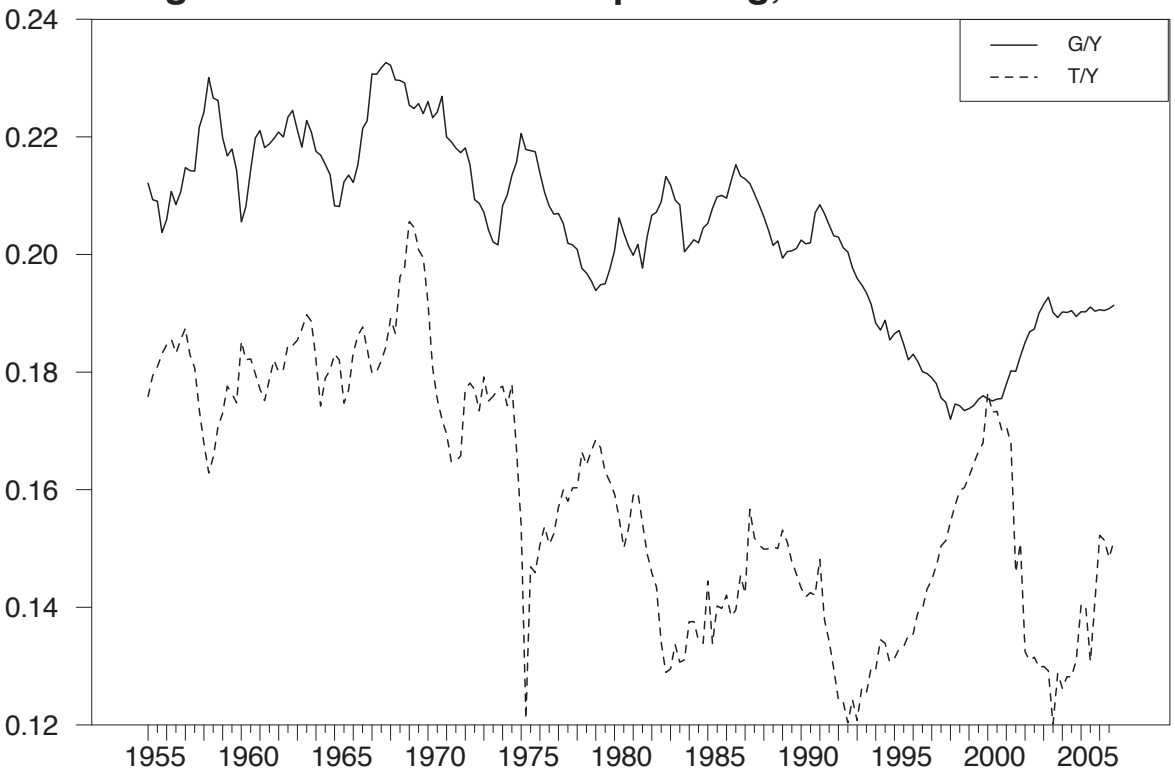
[26] Sims, C.A., and T. Zha (1999). Error Bands for Impulse Responses. *Econometrica* 67 (5): 1113–1155.

[27] Tenhofen, J., and G.B. Wolff (2007). Does Anticipation of Government Spending Matter? Evidence from an Expectation Augmented VAR. Deutsche Bundesbank Discussion Paper 14/2007. Frankfurt am Main.

[28] Uhlig, H. (2005). What Are the Effects of Monetary Policy on Output? Results from an Agnostic Identification Procedure. *Journal of Monetary Economics* 52 (2): 381–419.

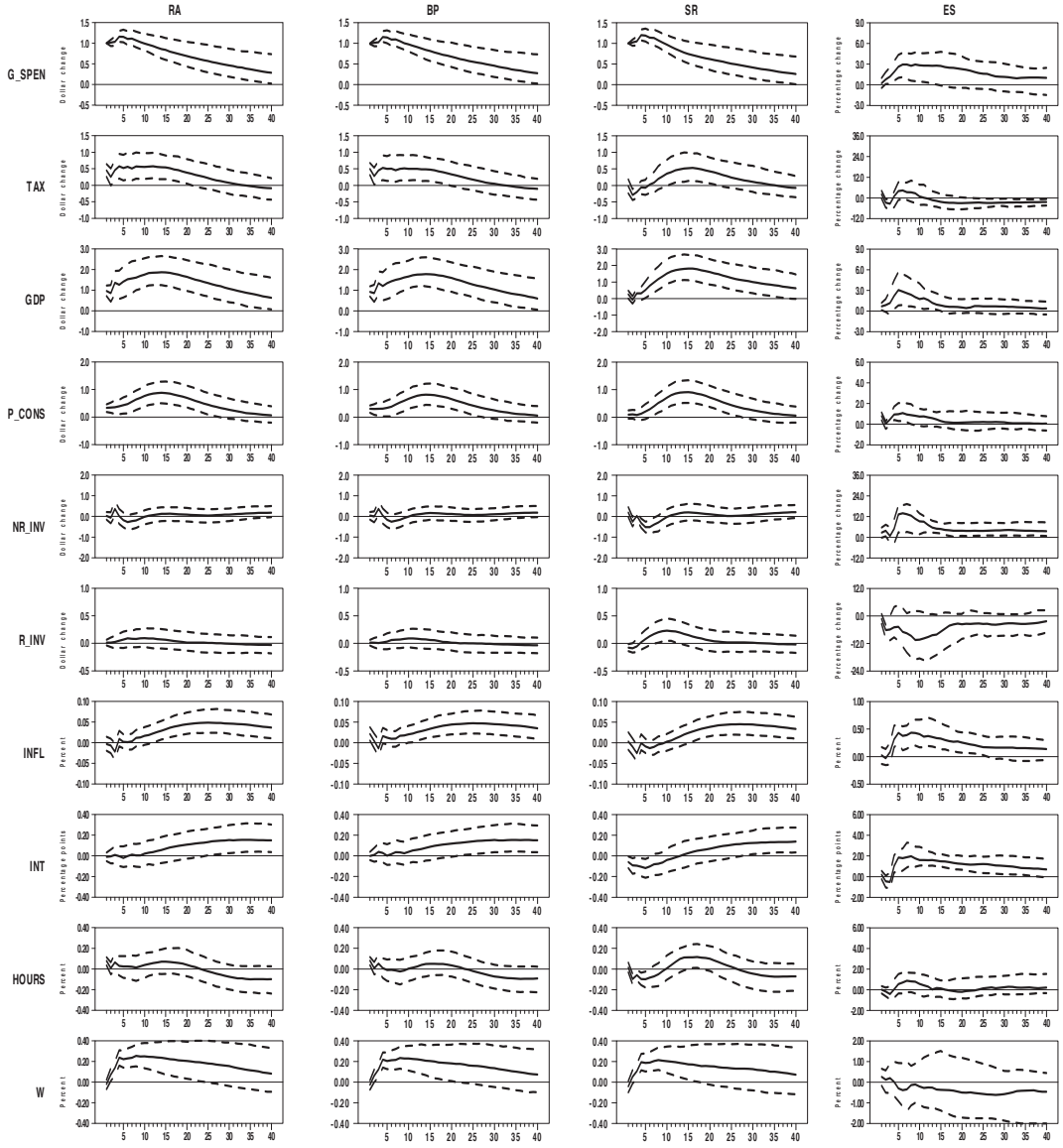
[29] Yang, S.-C. (2005). Quantifying Tax Effects under Policy Foresight. *Journal of Monetary Economics* 52 (8): 1557-1568.

Figure 1: Net Taxes and Spending, Share of GDP



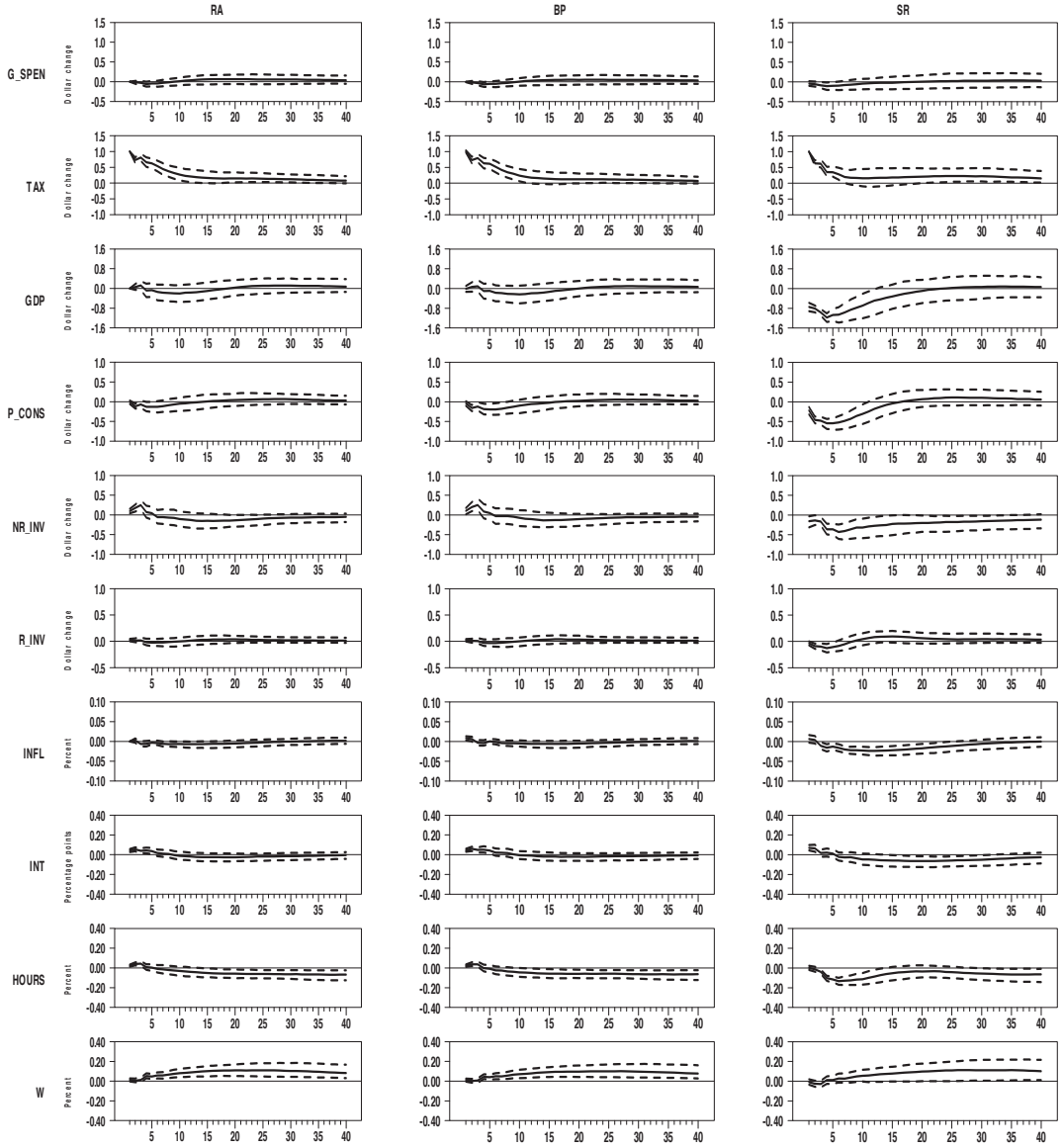
Note: The solid line plots the ratio of government spending to GDP, the dotted line the ratio of net taxes to GDP over the period 1955-2006.

Figure 2: Responses to a Pure Spending Shock



Note: The solid lines plot the median, the dotted lines the 16% and 84% fractiles of the posterior distribution of the impulse responses for the recursive identification approach (column 1), the Blanchard-Perotti approach (column 2), the sign-restrictions approach (column 3), and the event-study approach (column 4). The responses are shown for a horizon of 40 quarters. The responses of government spending, net taxes, GDP, inflation and the interest rate are based on the 5-variable VAR model, the responses of the other variables are based on 6-variable VAR models adding each of the other variables in turn to the benchmark set of variables. For the first three approaches the responses of GDP, its components and the fiscal variables are scaled such that they depict the dollar change in these variables in response to a pure government spending shock of size one dollar. For the event study approach the responses depict the percent change of these variables to a pure government spending shock of size one percent. For inflation, hours worked, and real hourly compensation, the responses are scaled such that they depict the percentage change in response to a pure government spending shock of size one percent. For the real interest rate the responses are scaled such that they depict the change in percentage points in response to a pure government spending shock of size one percent.

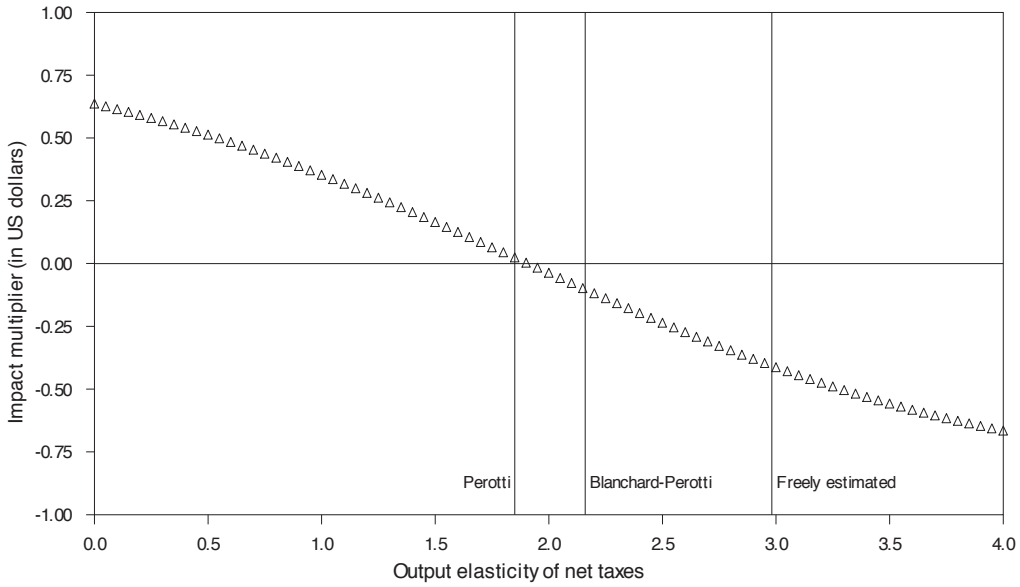
Figure 3: Responses to a Pure Tax Shock



Note: The solid lines plot the median, the dotted lines the 16% and 84% fractiles of the posterior distribution of the impulse responses for the recursive identification approach (column 1), the Blanchard-Perotti approach (column 2), and the sign-restrictions approach (column 3). The responses are shown for a horizon of 40 quarters. The responses of government spending, net taxes, GDP, inflation and the interest rate are based on the 5-variable VAR model, the responses of the other variables are based on 6-variable VAR models adding each of the other variables in turn to the benchmark set of variables. The responses of GDP, its components and the fiscal variables are scaled such that they depict the dollar change in these variables in response to a pure government spending shock of size one dollar. For inflation, hours worked, and real hourly compensation, the responses are scaled such that they depict the percentage change in response to a pure government spending shock of size one percent. For the real interest rate the responses are scaled such that they depict the change in percentage points in response to a pure government spending shock of size one percent.

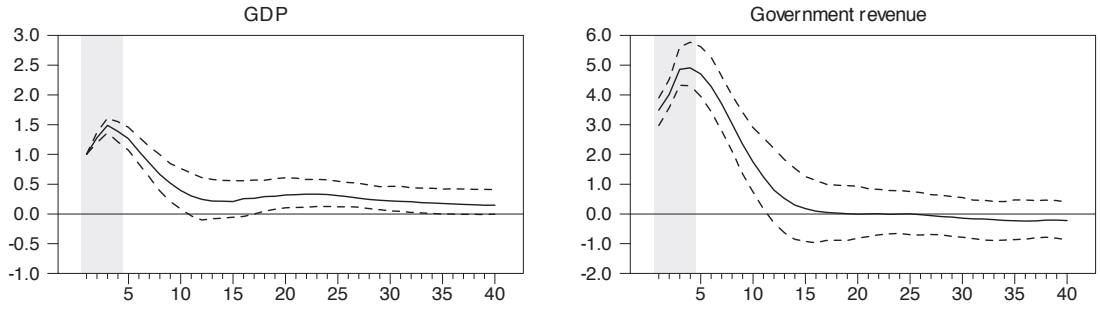
Figure 4: Impact Response of GDP to a Pure Tax Shock – BP Approach

Response for alternative values of the output elasticity of net taxes



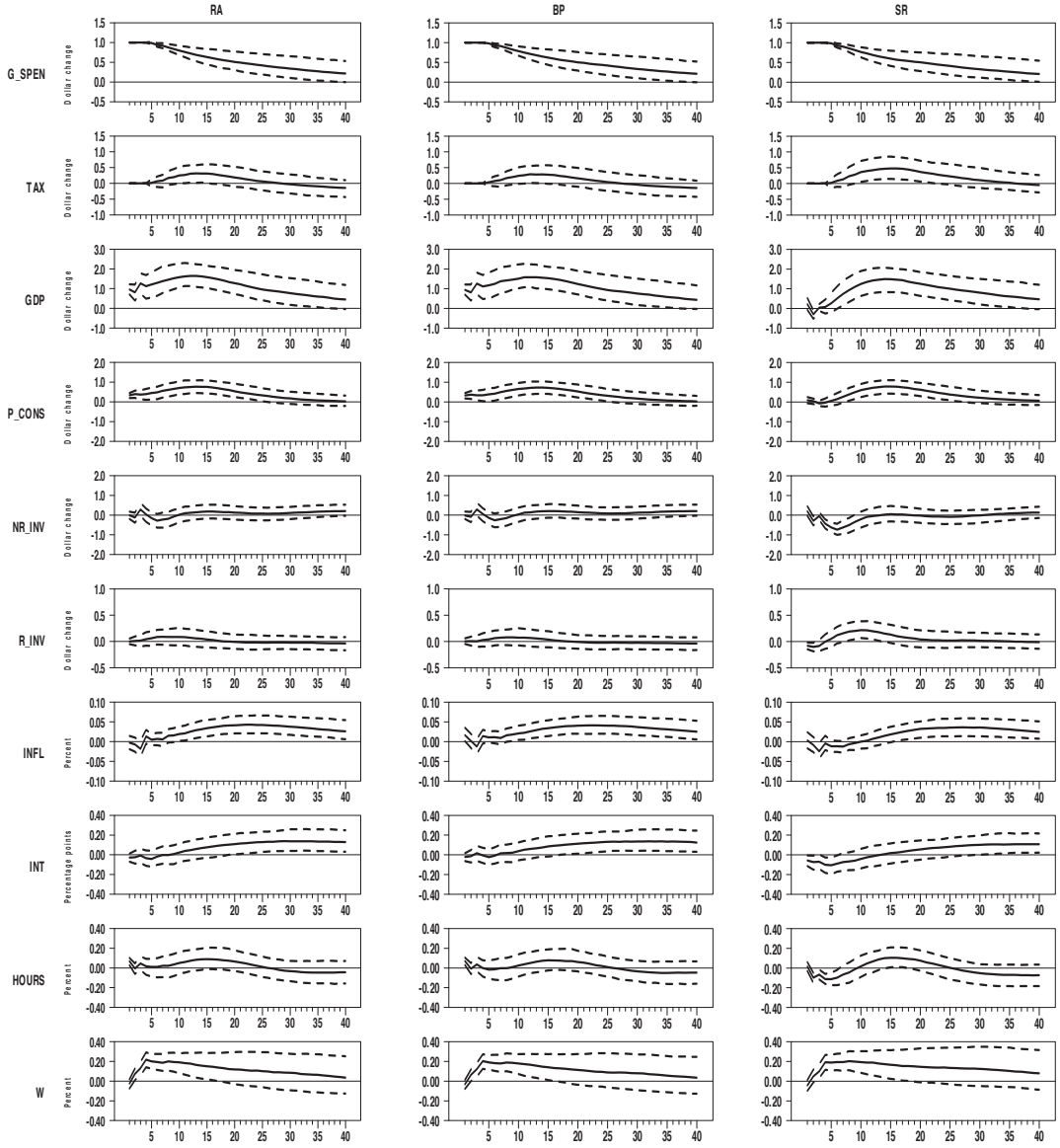
Note: The symbols depict the impact response of GDP in US dollars to a pure revenue shock of size one dollar for the Blanchard-Perotti identification approach for alternative values of the output elasticity of net taxes. The vertical lines indicate the values of the output elasticity of net taxes imposed by Perotti (2006), Blanchard and Perotti (2002) and the value that obtains if this elasticity is treated as a free parameter in the estimation, respectively. The responses are based on the 5-variable VAR model.

Figure 5: Responses to a Business Cycle Shock – Sign-Restrictions Approach



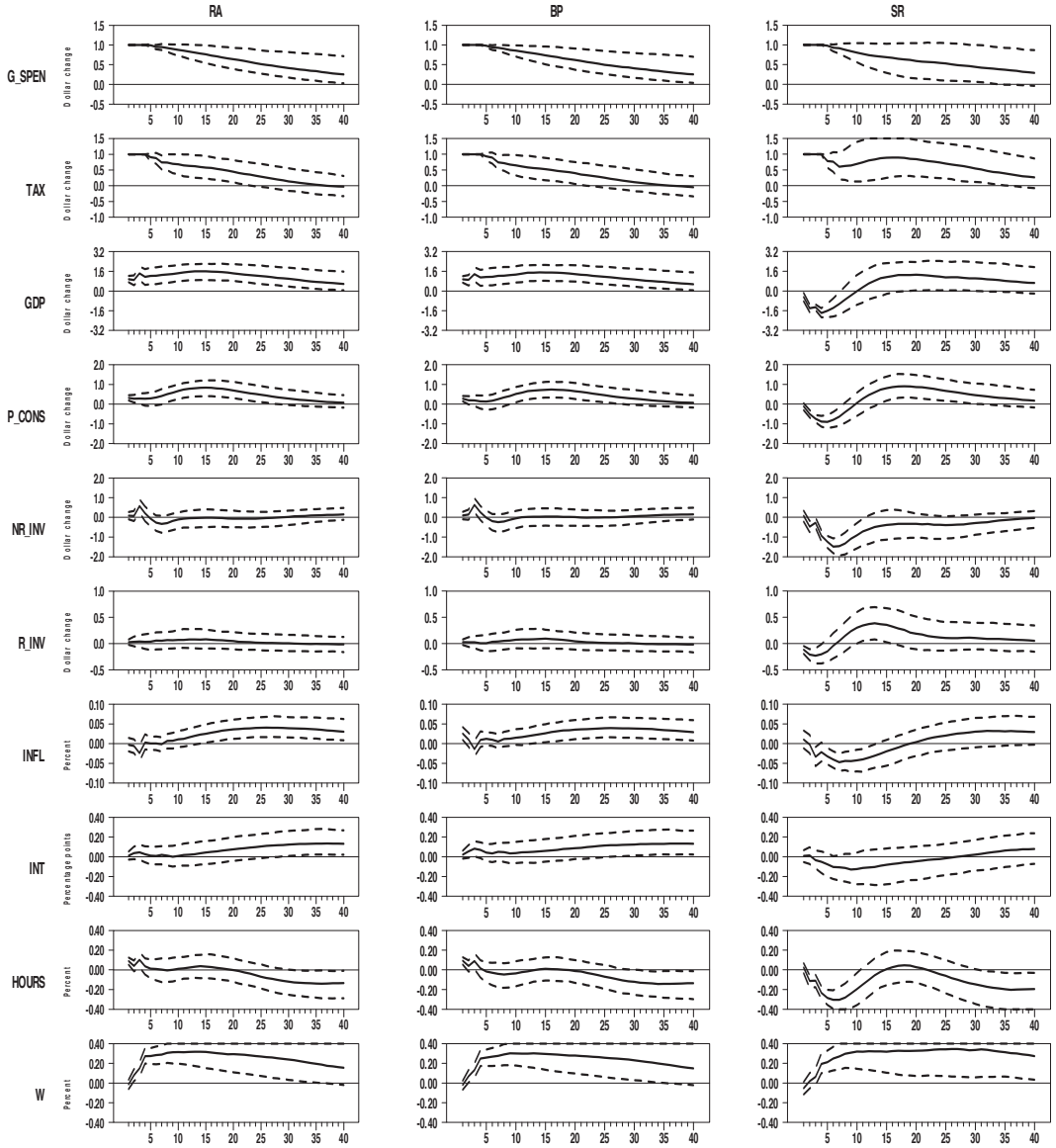
Note: The solid lines plot the median, the dotted lines the 16% and 84% fractiles of the posterior distribution of the impulse responses for the sign-restrictions identification approach. The responses are shown for a horizon of 40 quarters. They depict the percentage change in the plotted variables in response to a business cycle shock standardized such that the impact response of GDP is equal to 1 percent. The sign restrictions on the impulse responses are indicated by the shaded areas. The responses are based on the 5-variable VAR model.

Figure 6: Responses to a Deficit-Financed Spending Increase



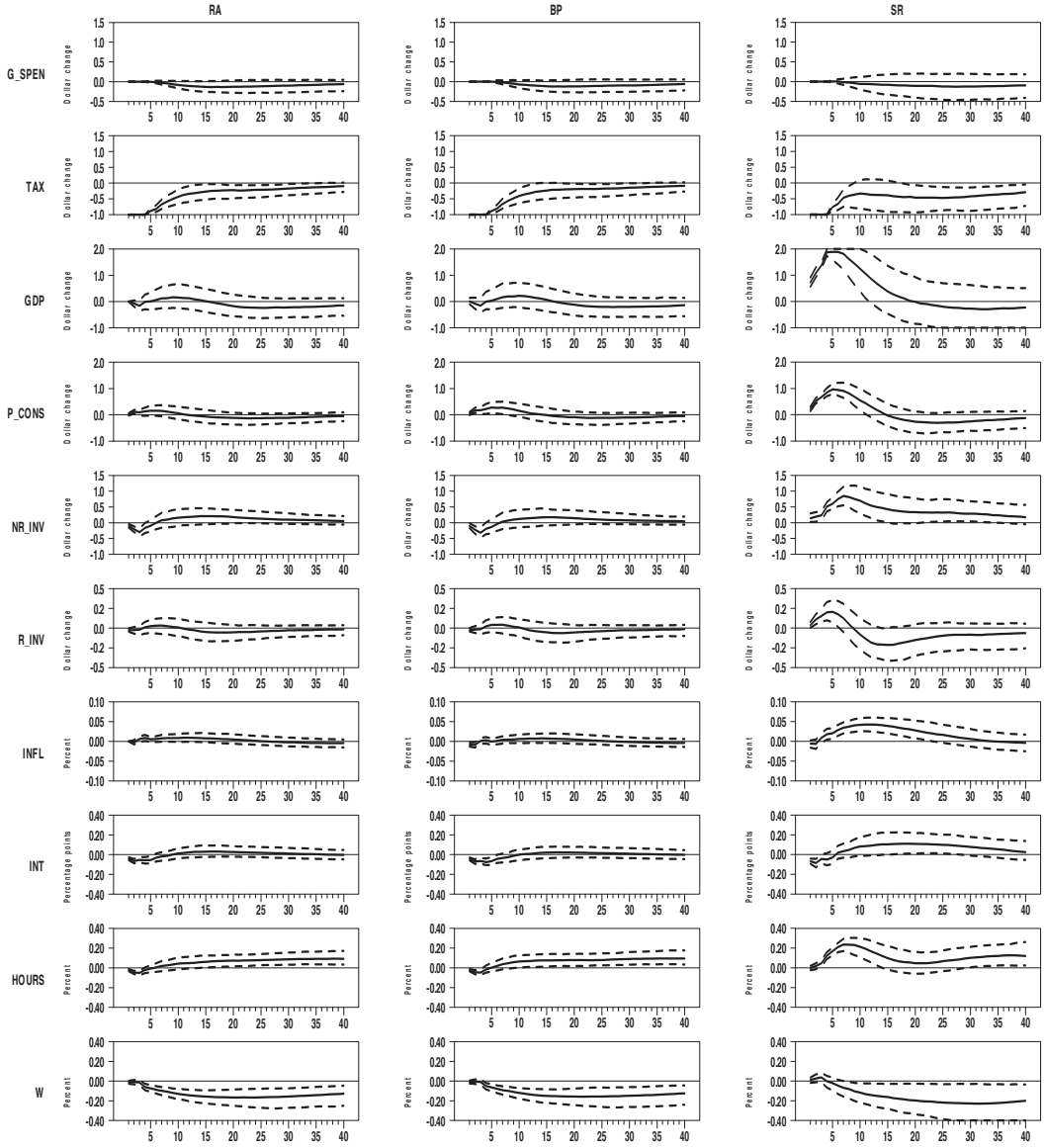
Note: The solid lines plot the median, the dotted lines the 16% and 84% fractiles of the posterior distribution of the impulse responses for the recursive identification approach (column 1), the Blanchard-Perotti approach (column 2), and the sign-restrictions approach (column 3). The responses are shown for a horizon of 40 quarters. The responses of government spending, net taxes, GDP, inflation and the interest rate are based on the 5-variable VAR model, the responses of the other variables are based on 6-variable VAR models adding each of the other variables in turn to the benchmark set of variables. This policy experiment is defined as follows: The pure spending and revenue shocks are linearly combined such that the responses of both government spending are equal to one dollar for four quarters and the responses of net taxes are equal to zero for four quarters. The responses of GDP, its components and the fiscal variables are scaled such that they depict the dollar change in these variables in response to a pure government spending shock of size one dollar. For inflation, hours worked, and real hourly compensation, the responses are scaled such that they depict the percentage change in response to a pure government spending shock of size one percent. For the real interest rate the responses are scaled such that they depict the change in percentage points in response to a pure government spending shock of size one percent.

Figure 7: Responses to a Balanced-Budget Spending Increase



Note: The solid lines plot the median, the dotted lines the 16% and 84% fractiles of the posterior distribution of the impulse responses for the recursive identification approach (column 1), the Blanchard-Perotti approach (column 2), and the sign-restrictions approach (column 3). The responses are shown for a horizon of 40 quarters. The responses of government spending, net taxes, GDP, inflation and the interest rate are based on the 5-variable VAR model, the responses of the other variables are based on 6-variable VAR models adding each of the other variables in turn to the benchmark set of variables. This policy experiment is defined as follows: The pure spending and revenue shocks are linearly combined such that the responses of both government spending and government revenue are equal to one dollar for four quarters. The responses of GDP, its components and the fiscal variables are scaled such that they depict the dollar change in these variables in response to a pure government spending shock of size one dollar. For inflation, hours worked, and real hourly compensation, the responses are scaled such that they depict the percentage change in response to a pure government spending shock of size one percent. For the real interest rate the responses are scaled such that they depict the change in percentage points in response to a pure government spending shock of size one percent.

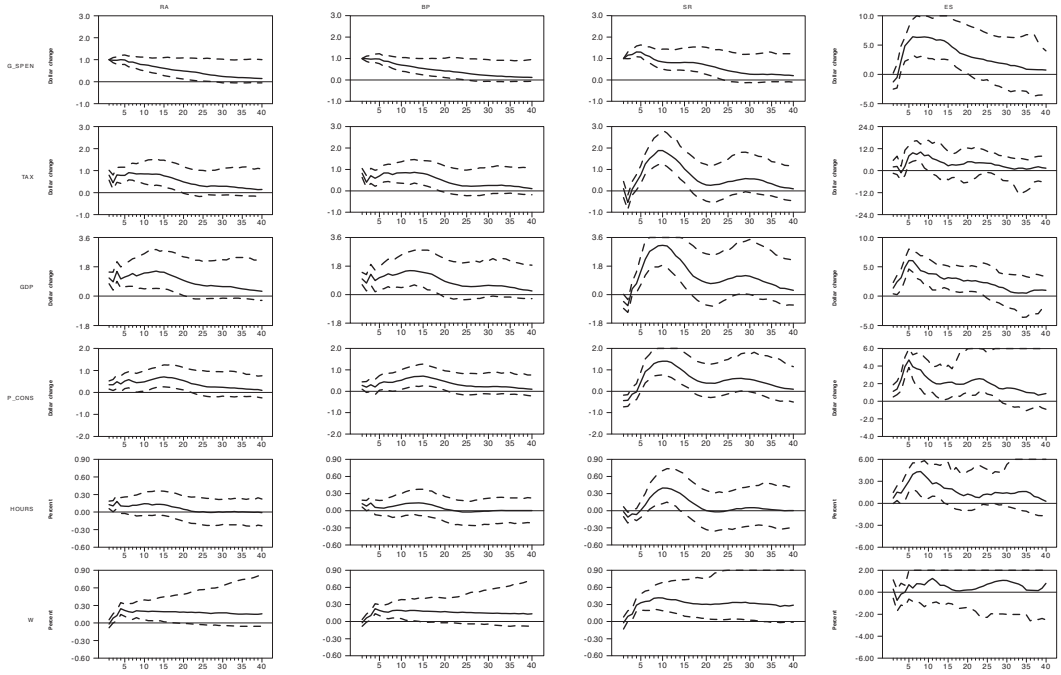
Figure 8: Responses to a Deficit-Financed Tax Cut



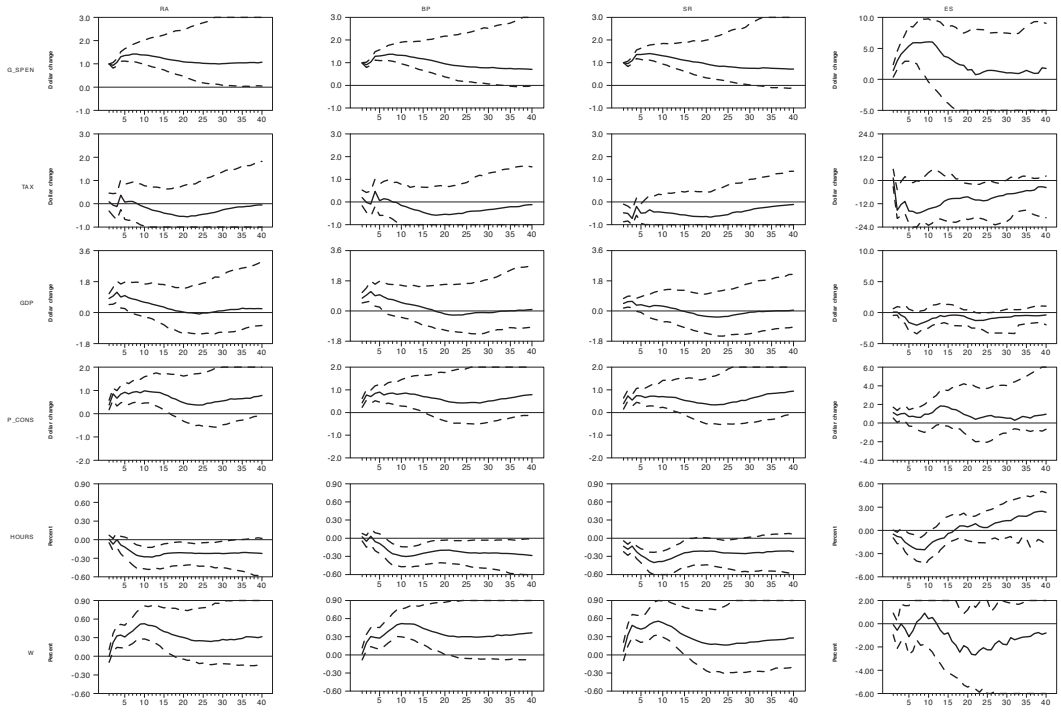
Note: The solid lines plot the median, the dotted lines the 16% and 84% fractiles of the posterior distribution of the impulse responses for the recursive identification approach (column 1), the Blanchard-Perotti approach (column 2), and the sign-restrictions approach (column 3). The responses are shown for a horizon of 40 quarters. The responses of government spending, net taxes, GDP, inflation and the interest rate are based on the 5-variable VAR model, the responses of the other variables are based on 6-variable VAR models adding each of the other variables in turn to the benchmark set of variables. This policy experiment is defined as follows: The pure spending and revenue shocks are linearly combined such that the response of net taxes is equal to minus one dollar for four quarters while government spending remains unchanged. The responses of GDP, its components and the fiscal variables are scaled such that they depict the dollar change in these variables in response to a pure government spending shock of size one dollar. For inflation, hours worked, and real hourly compensation, the responses are scaled such that they depict the percentage change in response to a pure government spending shock of size one percent. For the real interest rate the responses are scaled such that they depict the change in percentage points in response to a pure government spending shock of size one percent.

Figure 9: Responses to a Pure Spending Shock — Subsamples

1955:1 - 1979:3

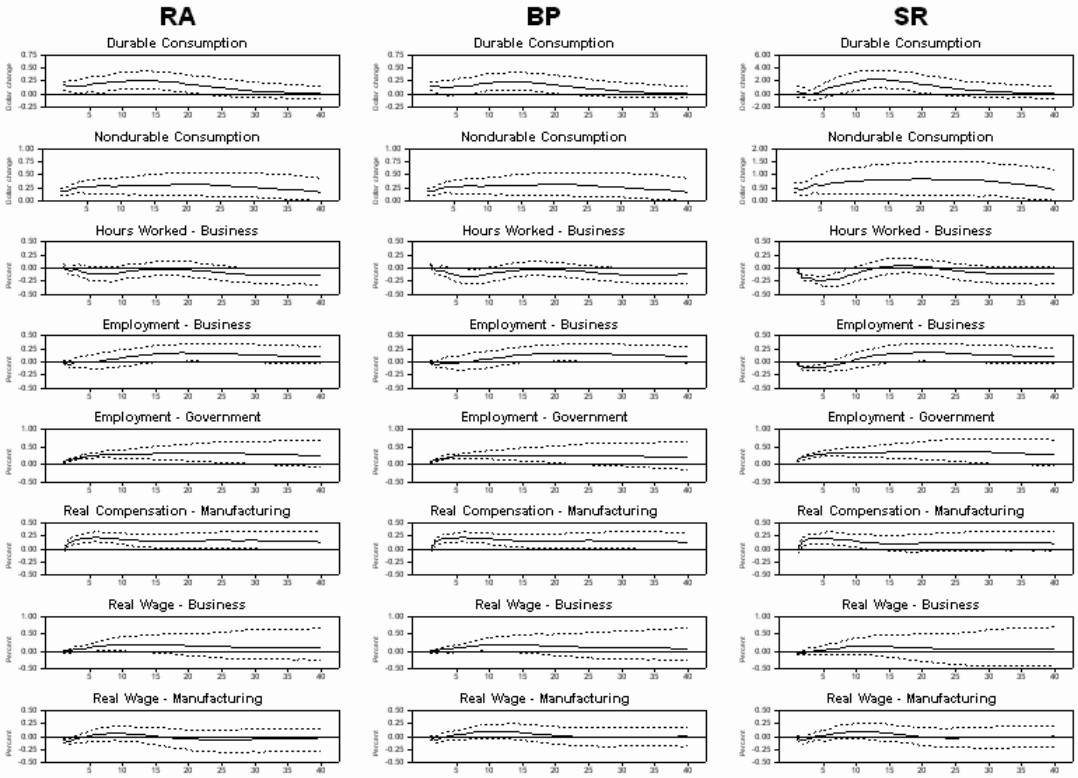


1983:1 - 2006:4



Note: See notes to Figure 2.

Figure 10: Responses to a Pure Spending Shock — Alternative Measures of Consumption, Employment and the Real Wage



Note: The solid lines plot the median, the dotted lines the 16% and 84% fractiles of the posterior distribution of the impulse responses for the recursive identification approach (column 1), the Blanchard-Perotti approach (column 2), and the sign-restrictions approach (column 3). The responses are shown for a horizon of 40 quarters. The responses are based on the 6-variable VAR model, adding each of the variables in turn to the benchmark set of variables. The responses of durable and nondurable consumption are scaled such that they depict the dollar change in these variables in response to a pure government spending shock of size one dollar. For the alternative measures of hours worked, employment and the real wage, the responses are scaled such that they depict the percentage change in response to a pure government spending shock of size one percent.

European Central Bank Working Paper Series

For a complete list of Working Papers published by the ECB, please visit the ECB's website (http://www.ecb.europa.eu).

836 "Reporting biases and survey results: evidence from European professional forecasters" by J. A. García and A. Manzanares, December 2007.

837 "Monetary policy and core inflation" by M. Lenza, December 2007.

838 "Securitisation and the bank lending channel" by Y. Altunbas, L. Gambacorta and D. Marqués, December 2007.

839 "Are there oil currencies? The real exchange rate of oil exporting countries" by M. M. Habib and M. Manolova Kalamova, December 2007.

840 "Downward wage rigidity for different workers and firms: an evaluation for Belgium using the IWFP procedure" by P. Du Caju, C. Fuss and L. Wintr, December 2007.

841 "Should we take inside money seriously?" by L. Stracca, December 2007.

842 "Saving behaviour and global imbalances: the role of emerging market economies" by G. Ferrucci and C. Miralles, December 2007.

843 "Fiscal forecasting: lessons from the literature and challenges" by T. Leal, J. J. Pérez, M. Tujula and J.-P. Vidal, December 2007.

844 "Business cycle synchronization and insurance mechanisms in the EU" by A. Afonso and D. Furceri, December 2007.

845 "Run-prone banking and asset markets" by M. Hoerova, December 2007.

846 "Information combination and forecast (st)ability. Evidence from vintages of time-series data" by C. Altavilla and M. Ciccarelli, December 2007.

847 "Deeper, wider and more competitive? Monetary integration, Eastern enlargement and competitiveness in the European Union" by G. Ottaviano, D. Taglioni and F. di Mauro, December 2007.

848 "Economic growth and budgetary components: a panel assessment for the EU" by A. Afonso and J. González Alegre, January 2008.

849 "Government size, composition, volatility and economic growth" by A. Afonso and D. Furceri, January 2008.

850 "Statistical tests and estimators of the rank of a matrix and their applications in econometric modelling" by G. Camba-Méndez and G. Kapetanios, January 2008.

851 "Investigating inflation persistence across monetary regimes" by L. Benati, January 2008.

852 "Determinants of economic growth: will data tell?" by A. Ciccone and M. Jarocinski, January 2008.

853 "The cyclical behavior of equilibrium unemployment and vacancies revisited" by M. Hagedorn and I. Manovskii, January 2008.

854 "How do firms adjust their wage bill in Belgium? A decomposition along the intensive and extensive margins" by C. Fuss, January 2008.

855 "Assessing the factors behind oil price changes" by S. Dées, A. Gasteuil, R. K. Kaufmann and M. Mann, January 2008.

856 "Markups in the euro area and the US over the period 1981-2004: a comparison of 50 sectors" by R. Christopoulou and P. Vermeulen, January 2008.

857 "Housing and equity wealth effects of Italian households" by C. Grant and T. Peltonen, January 2008.

858 "International transmission and monetary policy cooperation" by G. Coenen, G. Lombardo, F. Smets and R. Straub, January 2008.

859 "Assessing the compensation for volatility risk implicit in interest rate derivatives" by F. Fornari, January 2008.

860 "Oil shocks and endogenous markups: results from an estimated euro area DSGE model" by M. Sánchez, January 2008.

861 "Income distribution determinants and public spending efficiency" by A. Afonso, L. Schuknecht and V. Tanzi, January 2008.

862 "Stock market volatility and learning" by K. Adam, A. Marcet and J. P. Nicolini, February 2008.

863 "Population ageing and public pension reforms in a small open economy" by C. Nickel, P. Rother and A. Theophilopoulou, February 2008.

864 "Macroeconomic rates of return of public and private investment: crowding-in and crowding-out effects" by A. Afonso and M. St. Aubyn, February 2008.

865 "Explaining the Great Moderation: it is not the shocks" by D. Giannone, M. Lenza and L. Reichlin, February 2008.

866 "VAR analysis and the Great Moderation" by L. Benati and P. Surico, February 2008.

867 "Do monetary indicators lead euro area inflation?" by B. Hofmann, February 2008.

868 "Purdah: on the rationale for central bank silence around policy meetings" by M. Ehrmann and M. Fratzscher, February 2008.

869 "The reserve fulfilment path of euro area commercial banks: empirical testing using panel data" by N. Cassola, February 2008.

870 "Risk management in action: robust monetary policy rules under structured uncertainty" by P. Levine, P. McAdam, J. Pearlman and R. Pierse, February 2008.

871 "The impact of capital flows on domestic investment in transition economies" by E. Mileva, February 2008.

872 "Why do Europeans work part-time? A cross-country panel analysis" by H. Buddelmeyer, G. Mourre and M. Ward, February 2008.

873 "The Feldstein-Horioka fact" by D. Giannone and M. Lenza, February 2008.

874 "How arbitrage-free is the Nelson-Siegel model?" by L. Coroneo, K. Nyholm and R. Vidova-Koleva, February 2008.

875 "Global macro-financial shocks and expected default frequencies in the euro area" by O. Castrén, S. Dées and F. Zaher, February 2008.

876 "Are sectoral stock prices useful for predicting euro area GDP?" by M. Andersson and A. D'Agostino, February 2008.

877 "What are the effects of fiscal policy shocks? A VAR-based comparative analysis" by D. Caldara and C. Kamps, March 2008.

ISSN 1561-0810