What determines government spending multipliers?*

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Abstract

Theory predicts that the effect of fiscal expansion varies with the economic environment, notably the monetary and exchange rate regime, the state of public finances, and the health of the financial system. Using a panel of OECD countries, we evaluate the issue empirically, focusing on the macroeconomic effects of government consumption. Fiscal shocks are identified as residuals from an estimated government spending rule. These shocks are then interacted with conditioning variables in order to explain macroeconomic outcomes across a range of economic environments. The unconditional responses to a spending shock are in line with earlier results, featuring a positive, if relatively small output multiplier, no significant movement in consumption, and a fall in investment and the trade balance. Yet, these average results mask important differences across environments. In particular, the responses of the real exchange rate and net exports vary systematically across exchange rate regimes, with real appreciation and external deficits emerging mainly under a currency peg. Output and consumption multipliers, in turn, become quite sizeable during times of financial crisis.

Keywords: Multiplier, Fiscal policy, fiscal rules, financial crisis, public finances, exchange rate regime
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1 Introduction

The significant use of fiscal stimulus measures to counter the global financial crisis of 2007–2009 has revived the long-standing debate on the size of the fiscal multiplier. From a theoretical perspective, however, the multiplier is likely to depend on a number of factors, which vary both across countries and time. Traditional Mundell-Fleming analysis has long emphasized that the effectiveness of fiscal stabilization hinges on financial development, capital mobility, trade openness, and the exchange rate regime. In addition, the response of private demand to a fiscal intervention may well be affected by the health of public finances, as, for example, fiscal expansions at high levels of debt increase the likelihood of sharp future retrenchment. Another potential determinant is the state of the financial system, or more specifically, the extent to which the private sector has access to credit, given the greater impact of fiscal stimulus in the presence of liquidity constraints. Lastly, recent quantitative analysis predicts exceptionally large government spending multipliers during deep recessions when monetary policy is constrained by the zero lower bound on policy rates.[1]

In this paper, we provide an empirical exploration into the determinants of government spending multipliers, by studying how the fiscal transmission mechanism depends on the specific economic environment. In terms of conditioning factors, we focus on the exchange rate regime, the state of public finances, and the occurrence of a financial crisis. We conduct our analysis on a sample of 17 OECD countries for the period 1975–2008. For the classification of exchange rate regimes and financial crisis episodes we draw on Ilzetzki, Reinhart, and Rogoff (2009b), on the one hand, and Reinhart and Rogoff (2008) and Reinhart (2010), on the other hand. The state of public finances is instead proxied using the stock of outstanding public debt and/or the size of fiscal deficits.

Prior empirical work on fiscal policy transmission has mostly relied on linear time-series models estimated on U.S. data. Indeed, a fairly extensive literature has clarified key issues in identification, providing alternative benchmarks for how to quantify the effects of fiscal policy measures; see Blanchard and Perotti (2002), Mountford and Uhlig (2009), and Ramey (2009), among others. At the same time, there are only a few empirical studies examining the dependence of fiscal policy effects on economic environments, notably Perotti (1999), Giavazzi, Jappelli, and Pagano (2000) and, more recently, Tagkalakis (2008) and Ilzetzki, Mendoza, and Vegh (2009a). Even less empirical work has been devoted to the specific question of how fiscal transmission changes during times of financial crisis.[2]

Drawing on the work by Perotti (1999), we employ a flexible two-stage strategy that allows us to

[1] For an insightful theoretical analysis of the latter three aspects of fiscal policy transmission, see Bertola and Drazen (1993), Galí, López-Salido, and Vallés (2007), and Christiano, Eichenbaum, and Rebelo (2009), respectively.

[2] See International Monetary Fund (2009) for a recent investigation into how fiscal policy may mitigate deep recessions. Barro and Redlick (2009), focusing on defense spending, report a spending multiplier around 0.7 at the median unemployment rate, but a value of about unity if the unemployment rate is as high as 12 percent.

exploit variations in economic conditions across time and space in order to explore their impact on fiscal transmission. In a first step, we estimate a fiscal policy rule that is meant to describe the statistical process of government spending and provide estimates of spending shocks. The rule we consider is very similar to the structure embedded in fiscal policy VARs, linking our approach to the identification strategy most commonly found in the literature. In a second step, we use contemporaneous and lagged values of the estimated policy shocks to trace the dynamic effects of government spending on several macroeconomic variables of interest. We then explore the role of different economic environments in shaping fiscal transmission by interacting our shock measures with dummies for the exchange rate regime, the state of public finances, and financial crisis.

Reassuringly, our two-step procedure yields a series of plausible results for the *average* behavior and effects of fiscal policy, in line with earlier results from the literature. First, the estimated spending rules suggest that government spending exhibits no clear cyclical pattern, but provide significant evidence for the notion that government spending responds negatively to the level of debt, thus contributing to debt stabilization. Second, in response to an unexpected increase in government spending, we find a positive, if relatively contained increase in output, almost no response of consumption, and some crowding-out of investment and net exports. Moreover, the spending shock prompts a short-lived real appreciation, followed by a weakening of the currency.

These unconditional results, however, mask important differences in the transmission of fiscal shocks across economic environments. In fact, once we turn to our fully specified model including three sets of conditioning variables, the following picture emerges. In a country with a flexible exchange rate, no fiscal strain, and no financial crisis—our baseline scenario—we find no appreciable effects of spending shocks except on investment, which is crowded out, and the real exchange rate, which weakens over time. Relative to this baseline scenario, a pegged currency implies a larger trade deficit, but no real exchange depreciation on impact. For most specifications, we also find that spending-side stimulus is more effective in countries with a currency peg, in line with conventional wisdom. Specifically, the response of output is positive, and investment falls by less.[3] For a country under fiscal strain, in contrast, we find that the impact response of output and investment is reduced relative to the baseline scenario. At the same time, the response of consumption grows stronger in times of fiscal strain within one or two years after the shock—a pattern mirrored by an appreciating real exchange rate. Differences with the baseline scenario are however moderate, perhaps pointing to the difficulty of identifying fiscal strain from a small set of objective fiscal indicators. What turns out to affect fiscal policy transmission more strongly instead is the incidence of a financial crisis. The response of output and consumption to a government spending shock implies a multiplier of up to two during years of financial crisis. Investment shows a more muted response on impact, but then

[3] Yet, while differences across currency regimes in the response of external variables are quite robust, differences in the response of other macroeconomic aggregates are more sensitive to the specification of the model.

increases sharply over time. Similarly, we find a considerably more pronounced decline of net exports and the real exchange rate.

As the number of observations in our sample is limited and fiscal shocks may be measured with error, confidence intervals are, in general, relatively wide, and point estimates must be taken with a grain of salt. Yet our main conclusions, especially regarding the effects of government spending shocks conditional on financial crisis, appear to be robust to with respect to a number of variations of our empirical setup. These variations include estimating the model growth rates rather than in log levels, alternative versions of the first-step spending rule, and alternative definitions of our variables, including the financial crisis dummy.

Most closely related to our work is independent research by Ilzetzki et al. (2009a), who also study the transmission of fiscal policy across economic environments by estimating panel VARs for groups of countries distinguished by income level, the size of foreign debt, the exchange rate regime, and openness. In contrast to our study, these authors use a sample of both OECD and emerging market countries, with quarterly data. Their approach, however, isolates only one dimension at a time, and cannot accommodate time variation in country characteristics. Nonetheless, significant similarities in results from our different methodologies for the role of the exchange rate regime reinforce the case for studying fiscal transmission conditional on its economic, financial, and policy environment.

The remainder of the paper is organized as follows. Section 2 provides a brief theoretical discussion of why the fiscal transmission mechanism may differ across economic environments. Section 3 introduces our two-step estimation approach in detail. Sections 4 and 5 present the main results for the first and second step, respectively, and Section 6 concludes.

2 Fiscal policy in different economic environments

The recent revival of the long-standing debate on fiscal stimulus has drawn attention to a key theoretical point: there is unlikely to be such a thing as "the" fiscal multiplier. Instead, it seems plausible to expect that multipliers depend on current circumstances, as well as economic structures and policy regimes, quite aside from any variation related to specific fiscal measures. Accordingly, the likely effectiveness of fiscal stimulus cannot be assessed without proper consideration of the key factors characterizing the economic environment over time and across countries. In this paper, we emphasize, in particular, the role of exchange rate regimes, the state of public finances, and the health of the financial sector. Before assessing their relevance for fiscal transmission empirically, this section briefly reviews important theoretical contributions which guide our exploration.

2.1 A theoretical benchmark

The theoretical debate on the fiscal transmission mechanism has traditionally focused on the response of private consumption to an increase in government spending. Indeed, the consumption response not only has quantitatively important implications for the size of "the" government spending multiplier on output, but it also serves to discriminate between the (opposing) predictions of key macroeconomic models. Modern business cycle models, of both neoclassical and new Keynesian varieties, view private consumption as governed by intertemporal optimization. This generally implies that private consumption falls in response to an increase in government spending.[4] Multipliers on output are thus considerably smaller than would be stipulated by more traditional Keynesian analysis, which posits a positive consumption response. In a seminal study based on the frictionless neoclassical model, Baxter and King (1993) consider various specifications for household preferences and the duration of fiscal stimulus, but find that impact multipliers on output hardly ever exceed unity.[5] Subsequent research has further refined our understanding of the fiscal transmission mechanism. However, this research has generally been confined to standard business cycle models, abstracting from the exchange rate regime, government debt, and financial frictions (let alone financial crisis). We discuss a few exceptions below.

2.2 Exchange rate regime

In open economies with a high degree of capital mobility, the choice of the exchange rate regime determines the scope for independent monetary policy. This consideration is central to fiscal policy analysis within the traditional Mundell-Fleming framework. In the typical textbook experiment, government spending is ineffective in stimulating domestic economic activity under flexible exchange rates: assuming an unchanged money supply, a fiscal expansion completely crowds out net exports, because the exchange rate appreciates while capital inflows prevent the domestic interest rate from rising. Only under fixed exchange rates does fiscal policy become an effective stabilization tool, as any pressure toward exchange rate appreciation is immediately offset through monetary expansion. It follows that exchange rate regimes have a first-order effect on fiscal transmission.

The exchange rate regime matters for domestic fiscal multipliers also in new Keynesian business cycle models, but the sharp predictions of traditional Keynesian theory do not necessarily go through.

[4] Our discussion focuses on the transmission of government spending shocks assuming lump-sum financing and, in line with much of the literature, that government spending does not affect the marginal utility of households nor the productive capacity of the private sector.

[5] They also show that multipliers are much larger under the assumption that government spending enhances the productive capacity of the economy. By contrast, output multipliers are negative if government spending is financed by distortionary taxes, assuming balanced budgets. Linnemann and Schabert (2003) provide an early analysis within the new Keynesian baseline model. Cogan, Cwik, Taylor, and Wieland (2010), in turn, consider a richer business cycle model with a particular focus on quantifying the multiplier implied by actual fiscal policy measures implemented in the U.S. after 2009.

In particular, Corsetti, Meier, and Müller (2009b) show that fiscal stimulus may be either more or less effective under a fixed exchange rate, depending upon the precise assumptions about monetary policy in the alternative flexible exchange rate regime, as well as the medium-term debt consolidation framework. These same assumptions also turn out to be critical for the response of the exchange rate to fiscal expansions. Indeed, standard models predicts that government spending reduces net exports, while appreciating the exchange rate.[6] Recently, however, many contributions have questioned this result, identifying conditions under which government spending may actually depreciate the real exchange rate. Kollmann (2009), for instance, stresses that an increase in government spending may depreciate the real exchange rate if government spending shocks are very persistent and international financial markets incomplete. Ravn, Schmitt-Grohé, and Uribe (2007) show that if preferences of private households and the government are characterized by "deep habits", imperfectly competitive producers will find it optimal to lower markups and prices in the short run, so as to lock in increased public demand. In equilibrium, the price of domestic consumption falls relative to foreign consumption, and the exchange rate depreciates.

In Corsetti, Meier, and Müller (2009a) we highlight instead the importance of the medium-term debt consolidation regime for fiscal transmission in general, and the effect of government spending shocks on the real exchange rate in particular. If an increase in government spending today causes a build-up in debt which induces a systematic reduction of future government spending over time, domestic long-term real interest rates fail to increase in response to temporarily higher government spending, and the real exchange rate may depreciate. In this analysis, the exchange rate response is driven by the anticipated monetary accommodation of future spending cuts.[7]

[6] Erceg, Guerrieri, and Gust (2005) provide a detailed analysis of trade deficits triggered by fiscal policy in a modern business cycle model. On the dynamics of real exchange rate and terms of trade in the international real business cycle model, see Backus, Kehoe, and Kydland (1994). In the Keynesian textbook experiment, in turn, government spending raises domestic interest rates, triggering capital inflows and, again, an appreciation of the currency. Frenkel and Razin (1987), however, point out that this prediction may change in the case of tax finance and an exogenous money supply: lump-sum taxes lower disposable income and thus money demand, so that the exchange rate depreciates. Similarly, the model of Obstfeld and Rogoff (1995) also predicts that government spending depreciates the (nominal) exchange rate because of fiscal-monetary interactions: households lower consumption and, hence, their money demand in response to increased government spending. If the money supply is held constant, the currency must depreciate in nominal terms.

[7] Conditional on the exchange rate regime, trade openness also has potentially relevant implications for fiscal policy transmission. A well-known argument suggests that trade integration has a first-order effect on the effectiveness of fiscal stimulus, insofar as it reduces households' and firms' marginal propensity to spend on domestic goods. With a larger fraction of income spent on imported goods, some of the fiscal stimulus "leaks" abroad, increasing cross-country spillovers at the expense of domestic multiplier effects. In fact, calls for policy coordination by the IMF during the 2007–09 global financial crisis are also based on this argument, see Spilimbergo, Symansky, Blanchard, and Cottarelli (2008). Erceg, Gust, and López-Salido (2007) provide a quantitative analysis using a modern business cycle model. They confirm that increased trade integration lowers multipliers on domestic output, but the quantitative impact depends on the specific parameterization of the model. In Corsetti, Meier, and Müller (2010) we focus on cross-country spillovers, highlighting the importance of global interest rates, rather than trade, as a key channel of transmission. In the present paper we do not explore the role of trade openness, because in our data set more open countries (in terms of import shares) tend to be smaller and feature pegged exchange rates, complicating the identification of any true openness effects. We plan to explore this dimension of the data in greater detail in future work drawing on a larger data set.

2.3 State of public finances

In an influential study, Giavazzi and Pagano (1990) analyzed large-scale fiscal consolidations in Denmark and Ireland during the 1980s. These episodes were characterized by a positive comovement of the government balance and private consumption growth. While this comovement accords well with the neoclassical account of fiscal transmission, it was widely perceived as puzzling in the light of (Keynesian) received wisdom. Inspired by this contribution, a small strand of the literature has attempted to account explicitly for factors that can alter the private consumption response to "expansionary" fiscal measures (i.e., tax cuts or spending increases). Starting from the observation that fiscal consolidations are typically undertaken at exceptionally high levels of public debt, Bertola and Drazen (1993) suggest a neoclassical model where the comovement between consumption and budget balances is negative in normal times, but becomes positive once the level of debt is approaching some critical level known to trigger fiscal consolidation, hence raising the probability of a retrenchment.[8] Perotti (1999), in contrast, suggests a model where government spending, while altering the present discounted value of taxes, also raises current income through aggregate demand channels. Taxes are distortionary, and the economy is assumed to be initially away from the optimal tax smoothing path. Unconstrained households, who internalize the government budget constraint, coexist with credit-constrained households, who consume their entire disposable income in each period. In this model, the response of aggregate demand to fiscal measures depends on the initial level of debt. In normal, or "good", times, fiscal balances and consumption comove negatively; in "bad" times, with high levels of public debt, the comovement is positive. Intuitively, if initial debt is high, the distortions from a further increase in the tax rates are large, amplifying the negative wealth effect experienced by unconstrained households to such an extent that it outweighs any positive effect of fiscal expansions on income and consumption of constrained households.

2.4 Financial crisis

A distinguishing feature of financial crises is that access to credit becomes severely restricted. Although not intended as a model of fiscal policy transmission during financial crisis, the work of Galí et al. (2007) provides a useful starting point to think about this interaction. The authors extend the standard new Keynesian model to include a fraction of constrained households, who consume their disposable income in each period. By assumption, these households do not participate in asset markets. Increasing their weight in the overall population raises the expansionary effect of higher government spending. This is because government spending increases the disposable income of households,

[8] The model implies that the crowding-out of private consumption becomes smaller and smaller as debt builds up. Sutherland (1997), instead, in a related study, focuses on the effects of tax cuts in an overlapping generations framework and finds that tax cuts may have contractionary effects if debt is high and current generations of consumers expect consolidation to take place during their lifetime.

as prices are sticky and additional public demand triggers a rise in employment and wages.[9] A financial crisis, in turn, is likely to raise the share of credit-constrained agents, as lenders become more concerned about default risk and/or face capital and liquidity constraints.

However, financial crises can have another important effect, as exemplified by the most recent experience of global financial turmoil in 2007–09. Specifically, a financial crisis may exert such a pronounced recessionary impact on the economy as to lead monetary policy all the way to the zero lower bound on policy rates, impairing the central bank's ability to further stimulate the economy. As the recession takes hold, a vicious circle may set in: weak demand causes firms to cut prices and, to the extent that pricing decisions are staggered, falling prices generate expectations of lasting deflation; for a given nominal interest rate, these translate into higher real rates, which further weaken demand, thus reinforcing the deflationary dynamics, see, e.g., Gauti B. Eggertsson and Michael Woodford (2003). Under these circumstances, a sizeable fiscal stimulus can, in principle, halt the deflationary dynamics, as higher government spending is fully accommodated through unchanged (zero) policy interest rates. Indeed, Christiano et al. (2009) derive fiscal multipliers on output which easily exceed a value of two or even three, see also Hall (2009), Christopher J. Erceg and Jesper Lindé (2010) or Woodford (2010).

3 Empirical strategy

In this section, we introduce and motivate our empirical strategy to assess the role of the economic environment for the transmission of spending shocks, providing details about each of the two steps required by our estimation method.

3.1 Identification issues

Most of the existing empirical work on fiscal policy transmission employs structural vector autoregression (VAR) models to gauge the impact of spending shocks on the real economy. Following the lead of Blanchard and Perotti (2002) several authors have based identification on the assumption that discretionary government spending is subject to certain decision and/or implementation lags that prevent policymakers from responding to contemporaneous developments in the economy.[10] According to this idea, significant parts of government spending are determined by past information only. Government consumption and investment, in particular, are realistically unresponsive to current economic

[9] Bilbiie, Meier, and Müller (2008) use a similar framework to match the time-series evidence on U.S. fiscal transmission, explicitly linking household consumption to asset market participation.

[10] Recent contributions include Perotti (2004, 2007) and Galí et al. (2007), which focus on domestic-economy variables, and Canzoneri, Cumby, and Diba (2003), Kim and Roubini (2008) and Corsetti and Müller (2006), which address the international dimension. In an early contribution Rotemberg and Woodford (1992) estimate the impulse responses to a change in military spending using a VAR model and U.S. time-series data.

conditions, as, unlike transfers, they normally contain no automatic cyclical component.[11] A more detailed discussion of this identifying assumption and its interrelation with the frequency of available fiscal data is provided below.

An alternative estimation strategy is suggested by Ramey and Shapiro (1998), who consider a small number of events in postwar U.S. fiscal policy, including the military build-up for the Korean and Vietnam wars, that were arguably exogenous and thus provide natural experiments for the effect of a sudden surge in government spending. Subsequent studies have used this approach within a VAR context, see Edelberg, Eichenbaum, and Fischer (1999), Burnside, Eichenbaum, and Fisher (2004) and Ramey (2009). The latter study also considers a richer data set of military "events". In a related strand of the literature the focus has been explicitly on the multiplier for defense spending, which is estimated by regressing output growth on the change in government spending and possibly additional control variables. Identification rests again on the assumption that military spending is largely unresponsive to the state of the economy; see Barro and Redlick (2009) and Hall (2009) for recent contributions along these lines.[12]

Finally, Mountford and Uhlig (2009) have put forward an identification scheme based on sign restrictions: government spending shocks are identified within estimated VAR models by imposing the sign of the response of certain variables, for which theoretical predictions are fairly uncontroversial. While the focus of their study is on domestic variables, Enders, Müller, and Scholl (2008) derive sign restrictions on the basis of a richly specified open economy business cycle model, in order to analyze the international transmission of government spending shocks.

For the purposes of the empirical interest pursued in this paper, none of the above estimation strategies offers sufficient flexibility. Indeed, irrespectively of the specific identification scheme, the simple linear structure of standard VARs severely constrains any analysis of conditional dynamics in fiscal policy transmission. The most VAR studies allow for is to examine differences in transmission across a small number of distinct subsets of the data, through appropriate sample splits. Ilzetzki et al. (2009a), for instance, estimate panel VARs for different subgroups of countries distinguished by income level, the level of foreign debt, the exchange rate regime, and openness. In order to preserve sufficiently large data sets, however, the authors cannot isolate the importance of more than one such dimension at a time, nor does their approach accommodate time variation in country characteristics.[13] While closely related and indeed complementary to our own work, the work by Ilzetzki et al. (2009a)

[11] One possible exception is the indexation of government wages, which would lead to higher nominal outlays during times of strong economic activity and inflation. Given that budgets are fixed in nominal terms, real government spending would fall in this case. Previous work has, however, found such inflation-related cyclicality to be of very limited quantitative importance in advanced economies, see Perotti (2004).

[12] An important caveat is that military expenditure might rise systematically with command-type interventions in the economy, thus causing a downward bias in the estimated multiplier, see Hall (2009).

[13] See also Beetsma, Giuliodori, and Klaasen (2008) for a distinction of openness within a European sample.

thus leaves open a lot of questions about the marginal importance of specific country characteristics for fiscal policy transmission. At the same time, the panel VAR setup imposes significant homogeneity on the structure of fiscal policy-making across countries in a given subset of the data. Ramey and Shapiro's "event" approach, in turn, is constrained by the shortage of episodes with clear-cut exogenous fiscal policy shocks, especially once the analysis is extended beyond the U.S.

In this paper, therefore, we pursue a two-stage estimation strategy similar to the one proposed by Perotti (1999). In the first step, we estimate a fiscal policy rule that is meant to describe the statistical process of government spending and provide estimates of spending shocks. The fiscal policy rule we consider is very similar to the structure embedded in fiscal policy VARs, linking our approach closely to the identification strategy most commonly found in the literature. Importantly, we estimate these fiscal policy rules for one country at a time, thus allowing for significant heterogeneity in national policy-making. In the second step, we use the estimated policy shocks as a (generated) regressor to trace the impact of government spending on key macroeconomic variables, notably output, private consumption, the trade balance, and the real effective exchange rate. A flexible specification is chosen to account for the effects of spending shocks in different economic environments, i.e., under pegged vs. flexible exchange rates, with sound vs. strained public finances, and during normal times vs. times of financial/banking crisis.

3.2 The first step: Identifying government spending shocks

The first step consists in estimating an annual time series of fiscal policy innovations for each country i in the sample. As our policy variable of interest, we consider per capita government consumption expressed in logs. Government consumption is sizeable: it accounts for a significant 21.5 percent of GDP in the average country in our sample. More important, it is held to contain virtually no automatic cyclical component, facilitating the attempt to identify government spending changes above and beyond systematic fluctuation over the cycle. Unlike public investment, government consumption also has no obvious direct link to private sector productivity, limiting the number of possible channels through which fiscal policy affects the real economy.

We posit that the process of government spending is described by a relatively simple rule that relates our fiscal variable of interest (g_t) to its own first and second lag, the first two lags of log per capita output (y_{t-1} and y_{t-2}), the lagged value of a composite leading indicator (cli_{t-1}, which proxies directly for the authorities' expectations with respect to current-year growth), and the beginning-of-period debt stock, expressed as a share of GDP (b_{t-1}). The specification also includes a trend variable and a constant. Finally, our interest in the conditional dynamics of fiscal policy motivates us to also include (in most specifications) a set of dummy variables capturing key features of the economic environment, i.e., dummies indicating an exchange rate peg (peg_{t-1}), strained public finances ($strain_t$), and

a financial crisis (fc_{t-1}). Note that the information captured by each of the three dummies is lagged by one period, consistent with our general identifying assumption. In the case of the fiscal strain dummy, this is achieved by defining a period of fiscal strain as a function of high beginning-of-period debt and/or a high deficit in the preceding year. The resulting equation reads as follows:

$$\begin{aligned} g_{t,i} &= \phi_i + \eta_i trend_t + \beta_{i,1} g_{t-1,i} + \beta_{i,2} g_{t-2,i} + \gamma_{i,1} y_{t-1,i} + \gamma_{i,2} y_{t-2,i} + \theta_i cli_{t-1,i} + \delta_i b_{t-1,i} \\ &+ \rho_{i,1} fc_{t-1,i} + \rho_{i,2} strain_{t,i} + \rho_{i,3} fc_{t-1,i} + \varepsilon_{t,i}. \end{aligned} \quad (1)$$

The rule posits stable parameters ($\phi_i, \eta_i, \beta_i, \gamma_i, \theta_i, \delta_i$ and ρ_i) over time for each country in the sample but allows the parameters to be different across countries. The additive shock term ($\varepsilon_{t,i}$) is meant to capture unexpected discretionary policy changes, whose impact on the real economy is the ultimate object of our study. Note that the policy rule also allows for the desirable property of automatic debt stabilization, namely when $\delta_i < 0$.[14]

The key assumption, however, relates to the contemporaneous relationship between government spending and its determinants, notably output. Identification requires that there be no two-way contemporaneous interdependence. This is achieved by assuming, in line with the identifying assumption of most structural VARs, that spending cannot respond to simultaneous output developments.[15] Instead, spending is assumed to respond to past growth developments as well as expectations about economic activity formed one period in advance. Specifically, we include the OECD's composite leading indicator (CLI) as a proxy for near-term growth expectations. The CLI is a real-time measure with a proven track record of predicting changes in economic activity, especially cyclical turning points, several months in advance. As such, it seems well suited to capture expectations about the growth outlook held by both policymakers and the public.

In principle, our identifying assumption could be violated for two reasons. First, fiscal policy in most countries contains nondiscretionary cyclical elements, or automatic stabilizers. For our study, however, these automatic stabilizers should not pose a problem, as they operate essentially through (tax) revenue and transfer payments, such as unemployment benefits, but not through higher or lower outlays for government consumption.

A second potential problem is discretionary fiscal policy action in response to contemporaneous output developments. The relevance of this concern obviously hinges on the precise definition of "contemporaneous". Blanchard and Perotti (2002), for instance, argue that government spending policy could not realistically respond to output shocks within the same quarter. Indeed, fiscal authorities are subject not only to constraints on data availability about real-time developments but also to usually

[14] Favero and Giavazzi (2007) explore the importance of explicitly allowing for a feedback of fiscal policy variables to debt in a VAR framework. Corsetti et al. (2009a) study the impact of debt-responsive government spending on fiscal multipliers and provide evidence for spending reversals in U.S. fiscal data.

[15] This assumption carries over to the effect of economic environment variables on fiscal policy choices, as signalled by the use of lagged-information dummies in the policy equation.

significant time lags between budget formulation and execution. Whether or not these constraints prevent discretionary policy responses for significantly more than one quarter, is less obvious. For example, the fiscal stimulus packages adopted by the U.S. Congress in early 2008 and 2009, respectively, may suggest that the time lag between the arrival of new economic data and the implementation of a fiscal response can be shortened to about 5-8 months, at least under exceptional circumstances. However, it is worth noting in this context that the swiftest element in U.S. policy-makers' response to the crisis—in both decision-making and implementation—was a set of tax rebates, which would not be included in our concept of government spending.

With these caveats in mind, it is worth noting that the Blanchard-Perotti identification has been previously employed on annual data by several authors, including Beetsma, Giuliodori, and Klaasen (2006). In part, this may simply reflect practical constraints, as reliable quarterly fiscal data are not available for more than a handful of advanced economies. In part, it reflects the sense that quick-response fiscal policy is a very rare exception, and perhaps mostly focused on tax measures that can be implemented swiftly. Indeed, the above-mentioned stimulus packages adopted by the U.S. Congress can be viewed as closely related to the very exceptional circumstances created by the unfolding financial crisis. In order to capture the unusual dynamics of fiscal policy during such exceptional times, we include a lagged financial crisis dummy in the specification above. Given the start date of the financial crisis in 2007, the dummy should adequately capture any systematic fiscal policy response to the crisis during 2008–09, when indeed the two consecutive stimulus packages were agreed.

Aside from these considerations, there is another more substantive argument for using annual data even if they might at times give rise to endogeneity issues under the Blanchard/Perotti identification strategy. Indeed, using annual data is likely to attenuate a separate possible concern about identification, namely the notion that identified spending shocks might actually be foreseeable. The recent U.S. stimulus packages again provide a good case in point. The tax rebate measures announced in January 2008, for instance, were only starting to be implemented toward the end of the second quarter of 2008. Treating the measure as an unanticipated shock in the second and third quarter would therefore be incorrect, possibly inducing a severe bias in estimates of its effect on the real economy. The same is true for the extra spending legislated in the early 2009 stimulus package, which only started coming on stream several months later. This anticipation problem has gained prominence through the recent work of Ramey (2009) and Mountford and Uhlig (2009). Although the issue is likely to affect fiscal policy studies in general, it is arguably a greater concern for high-frequency (such as quarterly) data.

Note, finally, that policy rules similar to (1) have been considered in a range of recent quantitative studies of fiscal policy. One example of a single-estimation approach like ours is Galí and Perotti (2003). However, equation (1) also mimics many of the government spending equations contained in VAR-based studies, such as Blanchard and Perotti (2002). Although relatively simple, these rules

appear to capture quite well the macroeconomic essence of fiscal policy, thus providing us with useful measures of surprise fiscal policy innovations.

3.3 The second step: Tracing the effects of government spending in different economic environments

In the second step, we use the estimated fiscal shocks ($\widehat{\varepsilon}_{t,i}$) to gauge the dynamic impact of government spending on aggregate output, its key components, as well as international prices. We begin this exercise by describing the economy's average, or unconditional, response to a spending shock, abstracting from the role of specific economic environments. Subsequently, however, we allow the response to be affected by the set of conditioning factors introduced above, namely exchange rate regimes, the state of public finances, and financial crises.

Accordingly, we specify the following prototype second-step equation, to be estimated in a fixed-effects panel regression:

$$\begin{aligned} x_{t,i} &= \alpha_i + \mu_i trend_t + \chi x_{t-1,i} + \sigma_1 \widehat{\varepsilon}_{t,i} + \sigma_2 \widehat{\varepsilon}_{t-1,i} + \sigma_3 \widehat{\varepsilon}_{t-2,i} + \sigma_4 \widehat{\varepsilon}_{t-3,i} \\ &+ \kappa_1 \left(\widehat{\varepsilon}_{t,i} * d_{t,i}\right) + \kappa_2 \left(\widehat{\varepsilon}_{t-1,i} * d_{t-1,i}\right) + \kappa_3 \left(\widehat{\varepsilon}_{t-2,i} * d_{t-2,i}\right) + \kappa_4 \left(\widehat{\varepsilon}_{t-3,i} * d_{t-3,i}\right) \\ &+ \lambda_1 d_{t,i} + \lambda_2 d_{t-1,i} + \lambda_3 d_{t-2,i} + \lambda_4 d_{t-3,i} + u_{t,i} \end{aligned} \tag{2}$$

where x_t denotes one of our macroeconomic variables of interest (consumption, output and so on); $d_{t,i}$ is a dummy variable indicating a particular feature of the economic environment in a particular year, such as a currency peg or a financial crisis; and σ and κ are the key parameters of interest. Specifically, for $d_{t,i}$ indexing a currency peg, the σ parameters capture the dynamic effect (up to three years after the impact) of a government spending shock in economies with a floating currency, while the κ parameters indicate the marginal effect of the spending shock under a peg. Lastly, the λ parameters account for the direct effect on economic performance (even in the absence of government spending shocks) of that same economic feature.

Apart from the direct inclusion of the dummy variables, we stress that no additional control variable should be required. Provided that our first-step identification strategy delivers accurate estimates of fiscal policy shocks, these innovations are orthogonal to all other contemporaneous information, thus assuring consistent second-step estimates, without any need to control for other potential determinants of the macroeconomic variables of interest.[16] Nonetheless, we include several control variables in our specifications, including the lagged dependent variable and the (un-interacted) economic environment dummies, as detailed below.

[16] As a technical matter, note that the estimated government spending shocks are a generated regressor in the second-step equation. While this points to the possibility of measurement error affecting our results (notably through a downward bias in the absolute values of our estimates), the estimates are consistent as long as the specification of the first-step equation is fundamentally correct, i.e., accurately captures the process of government spending. However, standard errors of the second-step estimates would still have to be corrected for the use of a generated regressor.

3.4 The data

As foreshadowed above, we consider annual data, covering a maximum sample period from 1975 through 2008. We initially aim to include the same 19 OECD countries studied by Perotti (1999), but due to data limitations (we require at least 20 consecutive annual observations to obtain reliable estimates for the fiscal policy rule in the first step) wind up with a sample of 17 countries: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Ireland, Italy, Japan, Netherlands, Norway, Portugal, Spain, Sweden, the United Kingdom, and the United States. Table 1 provides further details. The variables used in our estimation are detailed in Table 2. Our primary data sources are the IMF and OECD. The real exchange rate as well as most expenditure aggregates are expressed in logs. Only the trade balance is expressed in percentage points of GDP. As regards our dummies, the classifications of exchange rate regimes is based on Ilzetzki et al. (2009b), while the financial crisis dates are provided by Reinhart and Rogoff (2008) and Reinhart (2010). An economy has strained public finances in a particular year if its beginning-of-period gross government debt exceeds 100 percent of GDP or if lagged net government borrowing exceeds 6 percent of GDP. These definitions are varied below to verify the robustness of our findings.

4 Systematic and non-systematic changes in government spending

The primary focus of our study is on the economy's response to government spending shocks, i.e., changes in government spending that are not systematically related to the state of the economy. We estimate these responses in the second step of our estimation strategy. Nevertheless, the results obtained for the first step are also of interest in their own right, insofar as they capture the *systematic* response of government spending to the state of the economy. For example, estimates of the parameters for the empirical fiscal rule can shed some light on whether and how spending policy responds to cyclical developments, on the one hand, and debt levels, on the other. We discuss some of the key relevant findings in the following.

Table 3 provides a summary of results from the first-step estimation of the spending rule for each country included in our sample. A few observations stand out. To start with, the fit obtained by our simple empirical fiscal rule is very high, reflecting the inclusion of autoregressive terms in our specification for log levels. Nonetheless, the fit remains quite good for most countries even when we re-estimate the model in growth rates, as one of our robustness checks.

Turning to the parameter estimates, the most general qualitative finding is for government spending to respond negatively to the outstanding stock of public debt. The corresponding coefficient is estimated to be negative for all but two countries (Austria and Finland), and significantly so for about half of them. This finding aligns well with the argument that government spending has a greater role to play

in debt consolidation strategies than standard theoretical models assume—a point stressed in Corsetti et al. (2009a). The parameter estimates relating to lagged output and the composite leading indicator are somewhat harder to translate into a clear statement about the cyclical properties of government spending. In particular, the relevant coefficient should not be regarded in isolation, as the other two related coefficients capture the cyclical properties of spending as well.

Another result worth noting relates to the sign of the estimated financial crisis dummy for those countries where financial crises occurred during the sample period. Counter to the experience from stimulus policies in advanced countries during the most recent crisis, the relevant coefficient is estimated to be negative, and sometimes significantly so, in seven out of thirteen countries. This implies that government spending would slow down, rather than accelerate, during financial crises (holding everything else fixed). While perhaps surprising, this points to the fact that what has been considered desirable in the latest global financial crisis, i.e., disproportionately strong countercyclical fiscal stimulus, is not necessarily what countries have found opportune during previous crises. A key reason for this may be financing constraints, especially when banking sector bailouts and falling tax revenue already put a significant dent into the public finances of the country concerned.

Next, we turn to the primary output of interest provided by the first-step regression, i.e., the estimated fiscal policy shocks. Although our choice of an empirical policy rule is motivated by theoretical considerations as well as previous contributions in the literature, its appropriateness needs to be subject to statistical tests. Specifically, if the residuals are supposed to be reliable measures of unanticipated spending shocks, one obvious requirement is that they exhibit no serial correlation. We test for this property using Arellano-Bond tests for autoregression (for one, two, and three lags) for each of the country-specific residual series in our sample. As the last column of Table 3 shows, the null hypothesis of no autocorrelation cannot be rejected at conventional levels for any of the countries in our sample.

Additional information on the shock series retained for the second-stage estimation is provided in Tables 4 and 5. Table 4 describes the composition of the final sample. It is somewhat reduced from the initial sample described in Table 1, now comprising 435 country-year observations. This reflects data gaps for some of the variables included in the second-stage regressions. For example, whenever a country's exchange rate regime changed within a given year, that observation is deleted from the sample, implying a gap for each country-year observation that requires this data point as a contemporaneous or lagged regressor in the second step.

The shocks contained in the final sample exhibit a mean and median of essentially zero and a standard deviation of 0.99 percent of government spending. The minimum and maximum values, ranging from -3.57 to 5.16 percentage points of government spending, are also indicated in Table 5. Finally, the correlation of the estimated shocks with the raw growth rate of government spending is just

below 0.7. This suggests that the first-step estimation clearly removes some systematic component of government spending changes, while producing a shock series that still bears a resemblance with the raw data, facilitating an intuitive interpretation of the identified fiscal changes.

5 The effects of government spending shocks

As detailed in the previous sections, our model is meant to capture the dynamic response of key macroeconomic variables to an identified government spending shock. Specifically, we consider the responses of six variables of interest: private consumption, fixed investment, and output, the trade balance, the real effective exchange rate, and, lastly, government spending itself. While our second-step regression is specified in log levels (or ratio to GDP in the case of the trade balance), we transform the results so as to allow a simple interpretation in terms of percentage points of GDP for expenditure aggregates, and percent for the price variables. The behavior of each variable is traced for six years after the impact. Before taking up the main issue of the paper, namely the economy's response to a spending shock conditional on different economic environments, we first illustrate that our approach produces estimates for the average (or unconditional) effects of government spending shocks that align well with those typically reported in the literature.

5.1 Unconditional effects

In order to estimate how government spending shocks affect the economy on average across countries and time, we begin with a parsimonious specification that excludes from our regressions, both in the first and the second step, all of the specific economic environment dummies described in Table 6.[17] Figure 1 provides a graphical representation of the results in terms of impulse response functions. The solid lines in Figure 1 represent the point estimate, while the shaded areas denote one-standard deviation confidence intervals.[18] The horizontal axis measures the time after the shock in years, the vertical axis measures deviations from trend. GDP and its components are measured in output units, while the real exchange rate is measured in percent. The size of the shock is normalized to one percent of GDP.[19]

As shown in the first row of Figure 1, we find a persistent increase in government spending, and a sizeable increase in aggregate output by about 0.7 on impact, while consumption does not respond

[17] To save space, we do not report results from our first-stage estimation omitting conditioning dummies. These results are essentially in line with those shown in Table 3. Namely, government spending exhibits no clear cyclical pattern, but responds negatively to public debt accumulation.

[18] We compute standard errors by drawing 1,000 realizations of the coefficient vector assuming a multivariate normal distribution with variance-covariance matrix corresponding to that of the regression coefficients; the mean is set equal to the point estimate.

[19] We normalize the shock and express GDP and its components in output units using the average expenditures shares in our sample period.

at all. As the responses of output and consumption are measured in the same units as government spending, the responses represent estimates of the government spending multipliers on output and consumption, respectively. These multipliers have been extensively researched over the last two or three decades on the basis of various time-series techniques.

Reassuringly, our results for the average multipliers are well within the range of results reported in this literature, which has to a large extent focused on U.S. time-series. As regards the consumption multiplier, for instance, studies adopting the Blanchard-Perotti identification typically report a positive estimate, but more recently evidence based on this approach suggests a decline in its size, see Perotti (2004) and Bilbiie et al. (2008). Conversely, studies drawing on the Ramey-Shapiro approach often report a negative multiplier for consumption, but quantitatively the effect is typically quite contained. Also, using different identification strategies, neither Mountford and Uhlig (2009) nor Barro and Redlick (2009) or Hall (2009) find a significant response of consumption to a deficit-financed government spending shock or an increase in defense spending, respectively. As recently summarized by Hall (2009): "Empirical work using simple regressions or more elaborate VARs finds output multipliers in the range from 0.5 to 1.0, with a few exceptions, and consumption multipliers in the range from somewhat negative to 0.5." Our unconditional estimates fall roughly into the middle of these ranges.

Turning to the second row, we find that government spending reduces investment, a result documented in various studies, e.g., Blanchard and Perotti (2002), Mountford and Uhlig (2009) or Ramey (2009). Our estimated response of the trade balance, suggesting that a positive spending shock triggers a decline in net exports, also squares well with conventional wisdom and earlier empirical studies, supporting the notion of "twin deficits"—defined as a conditional positive correlation between budget and current account deficits. For the U.S., however, Kim and Roubini (2008) find that, in response to fiscal shocks, the trade balance and the government budget actually move in opposite directions, a phenomenon that these authors dub "twin divergence". Subsequent work by Corsetti and Müller (2006) and Monacelli and Perotti (2006) extends the analysis to countries other than the U.S. Overall, it appears that expansionary fiscal measures tend to worsen the trade balance, though not strongly so. In this regard, our estimation methodology may help shed light on whether there is any systematic pattern linking "twin deficits" or "twin divergence" to particular features of the economy.

Finally, we find that the real effective exchange rate appreciates slightly on impact, but then depreciates over time. While the initial appreciation conforms well with conventional wisdom, it contrasts with the evidence from a few recent studies which document a fall in the real exchange rate after a rise in government spending.[20] However, the studies reaching this conclusion typically focus on countries featuring flexible exchange rate regimes. Conversely, studies focusing on European countries tend

[20] Kim and Roubini (2008) and Enders et al. (2008), for instance, report a depreciation of the REER for the U.S. Monacelli and Perotti (2006) find a depreciation for Australia, the U.S., and the U.K.

to document real appreciation in response to positive spending shocks.[21] These contrasting findings suggest that the dynamic response of the real exchange rate may differ systematically across currency regimes. Our methodology is well suited to examine this type of question more closely.

Indeed, the main message of our study is that findings based on simple unconditional models may hide large differences in the response to fiscal stimulus across diverse economic environments, as suggested by theory.[22] With this in mind, we turn next to the results for our specification which attempts to account simultaneously for specific dimensions of the economic environment.

5.2 Accounting for the economic environment

We now turn on three dimensions of the economic environment which, on the basis of the theoretical considerations discussed above, are likely to have a bearing on government spending multipliers: the exchange rate regime, the state of public finances, and the health of the financial sector. While the definitions of these characteristics are provided in Table 2, Table 6 gives a summary of which country-year observations fit under each of these categories. We thus estimate equation (2) for our entire final sample, including the appropriate interaction terms, that is, shocks interacted with a dummy for a currency peg, fiscal stress, and financial/banking crises.[23]

Figures 2 to 4 represent the results graphically. For each dimension of the economic environment, we report impulse responses for all variables of interest, comparing dynamics relative to a common baseline scenario. Although this focuses the discussion on only one conditioning factor (or environmental aspect) at a time, it is worth stressing that our results all pertain to a comprehensive specification that simultaneously controls for the three different dimensions that we are interested in. This is important insofar as variations along these dimensions are not necessarily orthogonal to each other. For the purposes of our charts, we thus condition on an identical economic environment in the two dimensions that are not the focus of the respective chart.

Baseline scenario: flexible exchange rates, sound public finances, no financial crisis In Figures 2 through 4, we report results for the marginal effects of a government spending shock, i.e., impulse response functions for a baseline scenario defined by an economy that does not peg its currency, suffers no fiscal stress, and does not experience a financial crisis. In each figure, the impulse responses for this baseline scenario are denoted by a solid line, the confidence interval by a shaded area. As apparent from these figures, in such a scenario there are no clear effects of spending shocks on other

[21] See, e.g., Beetsma et al. (2008) and Bénétrix and Lane (2009).

[22] Another case in point is the finding of Perotti (2004) that effects of government spending on the economy tend to be more muted after 1980 in the U.S.

[23] Note that the regression also includes each of these dummies and its lagged values as a direct control variable. This avoids the risk of attributing effects to the government spending shock that are actually accounted for by the respective economic characteristic per se. For example, a financial crisis is likely to affect output, exchange rates, public debt etc. even in the absence of sudden changes to government spending.

variables, with the exception of investment, which is crowded out, and the real exchange rate, which weakens over time. Compared to the unconditional results discussed above, these results support the notion that, in an economy with flexible exchange rates in normal times, the macroeconomic effects of government spending are generally quite contained.

Also in contrast to our unconditional findings above, the response of net exports is no longer negative, and hence more in line with the evidence reported for the U.S. by Kim and Roubini (2008). Conversely, real international prices weaken over time, broadly in accord with the unconditional evidence displayed in Figure 1. As discussed above, this finding matches a growing body of evidence, but conflicts with the transmission mechanism embedded in many traditional and modern models. Specifically, conventional wisdom holds that government spending causes real appreciation and thus crowds out net exports; this in turn explains a muted response of output. Our empirical results are at odds with such a mechanism. While there is virtually no response of output, net exports do not deteriorate, and the real exchange rate tends to depreciate. At the same time, we should stress that a muted, or even negative response of output is indeed reported by several recent VAR studies on the basis of U.S. time series, see Mountford and Uhlig (2009), as well as the results by Ramey (2009) and Perotti (2004) for a more recent sample period. Finally, Ilzetzki et al. (2009a) also find no significant output effect of government spending under flexible exchange rates.

Exchange rate regime According to conventional wisdom, rooted in the Mundell-Fleming and more recent macro models, fiscal policy transmission varies systematically across exchange rate regimes. Together with results for the baseline scenario, Figure 2 also displays the estimated impulse responses and confidence intervals for the case of a currency peg (but no fiscal and financial stress), denoted by dashed-dotted and the dashed lines, respectively.

As shown by Figure 2, the impact response of output and investment demand is larger under a peg than under a more flexible exchange rate arrangement, consistent with the conventional wisdom. Correspondingly, there is a deterioration of net exports. The real exchange rate tends to appreciate, rather than weakening. These results arguably reflect a higher degree of monetary accommodation under a peg. Together with an inflexible nominal rate, this may translate into higher demand for investment and some inflation, appreciating the currency in real terms over time. We note, however, that the response of consumption is lower relative to the baseline scenario, counter to the predictions of the Mundell-Fleming model. Ilzetzki et al. (2009a) also find that the output effects of government spending shocks are significant and sizeable under fixed exchange rates. They do not report results for output components or international relative prices, however.

Our results on the real exchange rate shed light on the seemingly contradictory conclusions of earlier empirical studies, some suggesting a real depreciation, some a real appreciation, in response to

positive government spending shocks. Much of this divergence in the literature could be explained in light of our result that the sign of the real exchange rate response appears to be conditional on the exchange rate regime.[24] Similarly, as the impact response of net exports appears to vary across currency regimes, differences across VAR studies might be attributable to differences in exchange rate regimes, pointing to interesting directions for future research in the interaction of fiscal and monetary policy. Before turning to the next "dimension", we should note that the implications of a peg on external variables—real appreciation and trade deficit—are quite robust to alternative specifications of the model, as discussed below.

State of public finances Recall that we proxy the strength of a country's public finances by a dummy inspired by Perotti (1999): weak fiscal conditions, i.e., "bad times", are identified with beginning-of-period gross government debt in excess of 100 percent of GDP, or lagged net government borrowing in excess of 6 percent of GDP. The effects of government spending shocks for an economy under fiscal strain so defined (but no financial crisis, and not pursuing a peg) are shown in Figure 3, once again together with the responses under the baseline scenario.
We find that when government spending expands in a state of fiscal strain, the impact response of output, consumption, and investment is lower relative to the baseline scenario, while there is no difference in the impact response of consumption. However, conditional on fiscal strain, output and consumption tend to grow stronger, rather than weaker, over time. This pattern is mirrored by the exchange rate, which does not respond on impact, but depreciates over time. As overall differences relative to the baseline scenario are moderate, our results might reflect difficulties in capturing fiscal stress through a small set of indicators for which comparable cross-country data are available. Japan's long experience of living with very high stock of liabilities, and yet very low financing costs testifies to the empirical challenge facing research in this area.

Financial crisis The last of our set of results pertains to the distinction between normal times and times of financial crisis, thus bringing our study closer to the core of the debate on fiscal policy since the eruption of the global financial crisis in 2007–2009. The possibility that a financial crisis affects fiscal policymaking is already captured in our first-stage specification. Results here focus on the effect of a given fiscal expansion (above and beyond what is explained by the systematic response of spending to cyclical conditions) during times of financial crisis.

[24] In addition to Bénétrix and Lane (2009), a number of studies have documented real appreciation in countries/regions with a fixed exchange rate. In their analysis of U.S. states and EMU member countries, Canova and Pappa (2007) also document that government spending shocks raise the price level relative to the price level in the rest of the union. Relying on a static regression model estimated on data for OCED countries, Lane and Perotti (2003) find that an increase in the government wage bill appreciates the nominal exchange rate. Ricci, Milesi-Ferretti, and Lee (2008) also document a positive association between government consumption and the real exchange rate for a panel of 48 countries.

Our results, shown in Figure 4, suggest that the response of consumption and output to a fiscal expansion is positive and large, once we condition on the occurrence of a financial crisis: consumption and output rises about twice as much as the rise in spending. Correspondingly, the trade balance deteriorates significantly and persistently. The response of investment is initially muted, but appears to grow over time.[25] In addition, the real exchange rate depreciates strongly.[26]

Our estimates may be interpreted as evidence in support of the argument, reviewed above, that higher fiscal outlays are particularly effective as a stabilization tool in a financial crisis, when many agents may be liquidity-constrained and/or when monetary policy is at the lower bound. For the U.S., a multiplier as large as 2 has been often claimed (and disputed) as a possibility in the recent policy debate about fiscal stabilization. Our analysis would lend support to a multiplier of this order of magnitude, even though our results in general do not point to very large fiscal multipliers at all.

It is worth stressing, however, that not all historical financial crises in our sample have induced policymakers to increase government spending on final goods and services. As shown above, the crisis dummy in our first-step regression takes on a negative sign for several countries, probably reflecting concerns about financing or fiscal sustainability. Indeed, one may suspect that fiscal sustainability is a precondition for obtaining high multipliers in times of financial crisis. In any case, our findings also contain a warning that a financial crisis can be even more damaging if it forces the government to retrench spending in the midst of the downturn. This clearly underscores the case for preserving and strengthening fiscal buffers in good times.

A final caveat is in order: the number of observations for a financial crisis is necessarily limited in our sample, and although we follow the classification of Reinhart and Rogoff (2008), not all of these financial crisis episodes are identical or even very similar in their features and depth. Nevertheless, as we show in the following, our main results appear quite robust to changes in the definitions of our dummies, including a more narrow definition of financial crisis that focuses on truly big crisis episodes only.

Sensitivity analysis We explore the robustness of our results with respect to alternative definitions of the dummy variables and model specifications. Results are shown in Figure 5 through 7, where each column displays results for one of three possible departures from the baseline scenario. Starting

[25] We are not aware of empirical studies attempting to quantify the impact of financial crisis on fiscal policy transmission—perhaps with the exception of Tagkalakis (2008) who, in a related study, finds that recessions, more broadly defined, tend to raise the government spending multiplier on consumption.

[26] Observe that the real exchange rate is linked through the interest rate parity condition to the entire path of future real short-term interest rates, i.e., to the long-term real interest rate. To the extent that monetary policy is constrained in lowering short-term nominal interest rates by the zero lower bound in a severe recession, the impact of government spending on expected price dynamics may greatly reduce short-term real rates. This may account for its marginal effect on long-term real rates and, hence, the real exchange rate. This mechanism may however fall short of explaining the magnitude of the response.

with Figure 5, we display results obtained under a narrower definition of financial crisis, including only the so called "big five" banking crises (Finland 1991, Japan 1992–97, Norway 1988–93, Spain 1984–85, and Sweden 1991–94) plus the global financial crisis starting in 2007. Clearly, a narrow definition of crisis has the advantage of selecting crises with a comparably large magnitude, with plausibly large macro consequences, albeit at the price of reducing the number of observations considerably.

In all columns we report (solid line, shaded area) results for the baseline scenario (no peg, no stress, no crisis): our findings are not much altered relative to those obtained under the wider banking/financial crisis definitions. The impulse responses conditional on a financial crisis are reported by the panels in the left column. While the overall results of Figure 4 are confirmed, there are also two notable differences. The first concerns the dynamics of spending itself, which is less persistent. The second concerns the dynamics of output and consumption: while the impact response remains close to 2, overall activity tends to revert to the trend over time more quickly.

A narrower definition of the crisis dummy does not affect multipliers conditional on fiscal stress, shown in the second column of Figure 5. It does however appear to strengthen our results concerning the impact of fiscal shocks under a peg. As apparent from the right column of the same figure, the difference in the impact responses of output and investment to a positive fiscal shock with and without a currency peg is larger than in Figure 2.

Figure 6 reports impulse responses when we adopt a stricter definition of fiscal stress, raising the thresholds of debt and deficits to 120 and 7 percent of GDP, respectively. In this case, the results conditional on financial and fiscal stress are unaffected, although the differences in impulse responses relative to the baseline tend to be smaller. The most apparent consequence concerns the currency regime. As shown by the right column of Figure 6, our main result regarding output and investment is reversed: the impact responses in these quantities are actually lower under a peg, relative to a regime of exchange rate flexibility. Conversely, the response of external variables, i.e. the real exchange rate and the trade balance, are similar, if not stronger, than in Figure 2. The real exchange rate strengthens on impact, the trade deficit worsens.

Figure 7 displays results for the model specified in differences, rather than levels. To allow a comparison with the results obtained above, we consider the cumulative effect of government spending shocks and express variables in units of GDP. Overall, the results from the level specification turn out to be quite robust with respect to this alternative specification. Unexpected spending expansions are clearly more effective conditional on a financial crisis than in the absence of it. The output and consumption multipliers remain around 2, investment rises over time, net exports deteriorate, and the real exchange rate depreciates. Similarly, Figure 7 confirms our previous conclusions regarding the effects of fiscal policy conditional on fiscal stress and under a peg. If anything, results under a peg,

especially those concerning the output multiplier, are more clearly in line with the received wisdom.

6 Conclusion

Economic theory suggests that the transmission of fiscal policies may vary across economic environments—there is no single multiplier describing the effectiveness of fiscal stimulus. However, empirical evidence on the determinants of fiscal transmission is patchy. In this paper we take a step toward a systematic empirical analysis of how different economic conditions affect the transmission of government spending shocks. Specifically, we propose a two-step procedure to estimate impulse response functions for economies that differ in terms of their exchange rate regime, the state of their public finances, and the health of their financial systems. Our findings suggest that standard estimates of average fiscal multipliers mask substantial differences in fiscal transmission across economic environments.

Our main results have implications for both theory and policy. First, we find that the real exchange rate response to a spending shock varies systematically with the exchange rate regime. This underscores the importance of interactions between fiscal and monetary policy—an interesting avenue for future research especially in an open economy context. Our finding also sheds new light on topical debates about fiscal transmission via international prices. Specifically, several recent studies have documented exchange rate depreciation after a spending increase, contrary to the prediction of standard theory. These studies tend to focus on samples including Australia, the U.S., and the U.K., i.e., countries with floating exchange rate regimes, whereas other studies finding real appreciation tend to focus on samples dominated by Euro area countries. Our analysis indicates that these differences could be systematically related to distinct exchange rate regimes.

A second key finding relates to the marked increase in fiscal multipliers during times of financial crisis. On the one hand, this may be taken as evidence in support of fiscal stimulus during financial crises. On the other hand, our empirical results also suggest that many countries have historically cut back government spending during financial crises, presumably out of concern over debt sustainability. In this sense, a large conditional multiplier also provides a stark warning about the costs of financial turmoil, and an argument in favor of building up fiscal buffers in normal times so as to avoid fiscal retrenchment when it is most painful.

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Table 1. Composition of Initial Sample 1/

Country	Time Period
Australia	1989-2008
Austria	1975-2008
Belgium	1975-2008
Canada	1975-2002, 2004-08
Denmark	1975-2008
Finland	1986-92, 1995-2008
France	1979-2008
Ireland	1980-2008
Italy	1975, 1977-1992, 1995-2008
Japan	1975-2008
Netherlands	1975-2008
Norway	1975-2008
Portugal	1987-2008
Spain	1981-2008
Sweden	1975-2008
UK	1975-90, 1992, 1994-2008
USA	1975-78, 1980-2008
Total no. of observations:	517

1/ Initial sample comprises the same set of OECD countries considered in Perotti (1999), with maximum time period 1975-2008, depending on data availability. Missing observations for individual years within a given time series accounted for by within-year changes in the exchange rate regime, which preclude an unequivocal coding.

Table 2. Data Sources and Definitions

Variable	Definition	Data Sources
Government spending	Log of real per capita government consumption	OECD Economic Outlook Database: volume of final government consumption expenditure (CGV); OECD Analytic Database: population size (POP).
GDP	Log of per capita GDP	OECD Economic Outlook Database: value of gross domestic product (GDP), GDP deflator (PGDP); OECD Analytic Database: population size (POP).
CLI	Composite leading indicator	OECD Monthly Economic Indicators database: CLI amplitude-adjusted; normalized by subracting 100, and dividing by 100.
Public debt	General government gross debt (in percent of GDP)	Primary source: IMF World Economic Outlook: General government gross debt (GGD), nominal GDP (NGDP); where unavailable: OECD Analytic Database: General government gross financial liabilities as a percentage of GDP (GGFLQ).
Financial crisis dummy	Takes on value of 1 during financial crises, and 0 otherwise	Reinhart and Rogoff (2008, pp. 65 ff.) and Reinhart and Rogoff (2010): the relevant crisis episodes are: Austria 2008, Belgium 2008, Denmark 2008, Finland 1991-94, Germany 2007-08, Ireland 2007-08, Italy 2008, Japan 1992-97, Netherland 2008, Norway 1988-93, Spain 1978-85 and 2008, Sweden 1991-94, United Kingdom 2007-08, and United States 2007-08.
Bad fiscal times dummy	Takes on value of 1 when lagged public debt exceeds 100 percent of GDP or lagged government net borrowing exceeds 6 percent of GDP, and 0 otherwise	OECD Analytic Database: Government net lending as a percentage of GDP (NLGQ); see also entry for public debt above.
Peg dummy	Takes on value of 1 when exchange rate regime defined as peg, and 0 otherwise	Ilzetzki, Reinhart, and Rogoff (2008): categories 1-8 of the authors' fine classification scheme are defined as "peg"; category 14 ("freely falling") coded as "not available"; within-year changes in regime category also lead to coding as "not available"; time series updated for 2008 (identical classification as for 2007) by the authors.
Private consumption	Log per capita real private consumption	OECD Economic Outlook Database: volume of final private consumption expenditure (CPV); OECD Analytic Database: population size (POP).
Private investment	Log per capita real fixed investment	OECD Economic Outlook Database: volume of private total fixed capital formation (IPV); OECD Analytic Database: population size (POP).
Trade balance	Ratio of net exports to GDP	IMF World Economic Outlook: exports of goods and services at current prices (NX), imports of goods and services at current prices (NM), nominal GDP (NGDP).
REER	CPI-based real effective exchange rate (in percent)	OECD Monthly Economic Indicators Database (CCRETT01.IXOB).
Terms of trade	Terms of trade for goods and services (in percent)	OECD Analytic Database: terms of trade for goods and services (TTRADE).

Table 3. Results of First-Step Regression 1/

Dependent variable: Log of per capita government consumption	Regressor									F-test of joint significance (p-value)	R squared	Arellano-Bond test of autocorrelation (p-value)
Country	Government spending growth (-1)	Government spending growth (-2)	GDP growth (-1)	GDP growth (-2)	CLI (-1)	Bad fiscal times dummy	Lagged fin. crisis dummy	Lagged peg dummy	Beginning-of-period govt. debt			
Australia	-0.210	-0.315	0.159	0.240	0.055	.	0.000	.	-0.139 *	0.00	1.00	0.28
	(0.300)	(0.218)	(0.153)	(0.230)	(0.094)	.	(0.009)	.	(0.071)			
Austria	1.344 ***	-0.507 ***	-0.320 **	0.245	0.020	.	.	.	0.009	0.00	1.00	0.56
	(0.176)	(0.169)	(0.148)	(0.144)	(0.069)	.	.	.	(0.033)			
Belgium	0.554 ***	0.194	0.056	-0.103	-0.054	0.014	.	.	-0.041 ***	0.00	0.99	0.11
	(0.197)	(0.175)	(0.154)	(0.166)	(0.097)	(0.009)	.	.	(0.014)			
Canada	0.916 ***	-0.004	0.190	-0.069	-0.141 *	-0.016	0.015	0.001	-0.020	0.00	0.98	0.38
	(0.192)	(0.162)	(0.171)	(0.195)	(0.077)	(0.010)	(0.011)	(0.016)	(0.043)			
Denmark	1.007 ***	-0.084	-0.024	-0.111	0.076	-0.015	-0.010	.	-0.005	0.00	..993	0.97
	(0.199)	(0.205)	(0.210)	(0.216)	(0.088)	(0.017)	(0.009)	.	(0.017)			
Finland	1.060 **	-0.432	0.431 *	-0.077	-0.020	0.012	0.000	.	0.082	0.00	0.99	0.74
	(0.363)	(0.371)	(0.220)	(0.249)	(0.084)	(0.011)	(0.019)	.	(0.115)			
France	0.610 ***	0.277	0.085	0.092	-0.089	-0.008	0.000	.	-0.071 *	0.00	1.00	0.43
	(0.200)	(0.183)	(0.221)	(0.267)	(0.142)	(0.011)	(0.007)	.	(0.036)			
Ireland	0.709 ***	-0.075	0.002	-0.008	0.466 **	-0.011	-0.046 *	.	-0.188 **	0.00	1.00	0.42
	(0.181)	(0.171)	(0.233)	(0.238)	(0.162)	(0.028)	(0.024)	.	(0.087)			
Italy	1.099 ***	-0.235	0.299	0.013	-0.008	.	-0.015	-0.006	-0.070 *	0.00	0.99	0.95
	(0.208)	(0.200)	(0.225)	(0.258)	(0.105)	.	(0.009)	(0.010)	(0.046)			
Japan	0.620 ***	0.205	-0.519 ***	0.602 ***	0.059	-0.009	-0.018 **	-0.002	-0.010	0.00	1.00	0.34
	(0.114)	(0.129)	(0.155)	(0.168)	(0.057)	(0.009)	(0.007)	(0.012)	(0.058)			
Netherlands	0.784 ***	-0.210	-0.154	-0.050	0.114	-0.019	.	.	-0.026	0.00	0.99	0.08
	(0.189)	(0.221)	(0.286)	(0.321)	(0.218)	(0.013)	.	.	(0.042)			
Norway	1.015 ***	-0.305 *	0.147	-0.043	0.035	.	0.014 *	.	-0.011	0.00	1.00	0.07
	(0.165)	(0.151)	(0.212)	(0.163)	(0.141)	.	(0.008)	.	(0.035)			
Portugal	-0.075	0.148	1.192 **	-0.387	-0.180	0.024	.	.	-0.136	0.00	0.99	0.35
	(0.351)	(0.296)	(0.474)	(0.550)	(0.215)	(0.016)	.	.	(0.198)			
Spain	0.533 **	0.161	0.458	-0.336	0.018	0.003	-0.054 ***	.	-0.090 **	0.00	1.00	0.19
	(0.227)	(0.223)	(0.295)	(0.294)	(0.136)	(0.008)	(0.016)	.	(0.037)			
Sweden	0.768 ***	0.065	-0.154	-0.168	0.054	-0.014 *	-0.007	0.033 *	-0.042	0.00	0.99	0.44
	(0.203)	(0.201)	(0.175)	(0.176)	(0.070)	(0.008)	(0.010)	(0.018)	(0.038)			
UK	0.980 ***	-0.147	0.050	0.168	-0.168	0.005	0.011	-0.017	0.005	0.00	0.99	0.28
	(0.181)	(0.188)	(0.252)	(0.245)	(0.166)	(0.010)	(0.015)	(0.018)	(0.061)			
USA	0.998 ***	-0.257 *	0.240 **	-0.286 **	-0.155 **	.	0.023 ***	-0.003	-0.069 **	0.00	0.99	0.42
	(0.129)	(0.126)	(0.106)	(0.117)	(0.071)	.	(0.005)	(0.006)	(0.028)			

1/ Separate regression for each country in the sample; regression relates log of government spending to its own first two lags, two lags of real GDP, the lagged (October of the preceding year) OECD composite leading indicator, beginning-of-period gross public debt, a lagged financial crisis dummy, a lagged exchange rate peg dummy, a 'bad fiscal times' dummy, a time trend and a constant (not reported here). Point estimate reported in top row, standard error in parentheses below. Asterisks indicate significance at the 10 percent (*), 5 percent (**), or 1 percent (***) level. Arellano-Bond test tests for autocorrelation of residuals up to three lags under the null hypothesis of no autocorrelation.

Table 4. Composition of Final Sample 1/

Country	Time Period
Australia	1992-2008
Austria	1978-2008
Belgium	1978-2008
Canada	1978-2001, 2007-08
Denmark	1978-2008
Finland	1989-91, 1998-2008
France	1982-2008
Ireland	1983-2008
Italy	1980-1991, 1998-2008
Japan	1978-2008
Netherlands	1978-2008
Norway	1978-2008
Portugal	1990-2008
Spain	1984-2008
Sweden	1978-2008
UK	1978-1989,1997-2008
USA	1983-2008
Total no. of observations:	444

1/ Final sample determined by availability of all regressors included in the most comprehensive specification for second-step regression.

Table 5. Summary Statistics for Estimated Government Spending Shocks 1/
(Percent)

No. of observations	444
Mean	0.04
Median	0.00
Standard deviation	1.02
Minimum	-3.57
Maximum	5.16
Correlation with simple growth rate of government spending	0.64
Five largest negative and positive shocks:	
Portugal, 1993	-3.57
Netherlands, 1984	-3.33
Netherland, 2005	-3.18
Norway, 1988	-2.97
Spain, 1988	-2.67
Portugal, 1991	2.60
Portugal, 2005	2.68
Denmark, 1993	2.85
Ireland, 1986	3.83
Netherlands, 2006	5.16

1/ All statistics refer to final sample described in Table 4.

Table 6. Overview of Dummy Characteristics

Dummy	Countries and Time Periods Fulfilling the Respective Criterion in the Final Sample
Peg	Austria, 1978-2008 Belgium, 1978-2008 Canada, 1978-2001 Denmark, 1978-2008 Finland, 1989-91, 1998-2008 France, 1982-2008 Ireland, 1983-2008 Italy, 1983-91, 1998-2008 Netherlands, 1978-2008 Portugal, 1990-2008 Spain, 1984-2008 Sweden, 1978-92
Bad fiscal times	Belgium, 1978-2003 Canada, 1983-87, 1992-97 Denmark, 1982-84 France, 1994 Ireland, 1983-89 Italy, 1980-91, 1998-2008 Japan, 1997-2008 Netherlands, 1983, 1996 Portugal, 1991-92, 1994-95, 2006 Spain, 1986-87, 1994-96 Sweden, 1983, 1993-96
Financial crisis	Australia, 1992 Austria, 2008 Belgium, 2008 Canada, 1983-85 Denmark, 1987-92, 2008 Finland, 1991 France, 1994-95 Ireland, 2007-08 Italy, 1990-91, 2008 Japan, 1992-97 Netherlands, 2008 Norway, 1988-93 Spain, 1984-85, 2008 Sweden, 1991-94 United Kingdom, 2007-08 United States, 1984-91, 2007-08

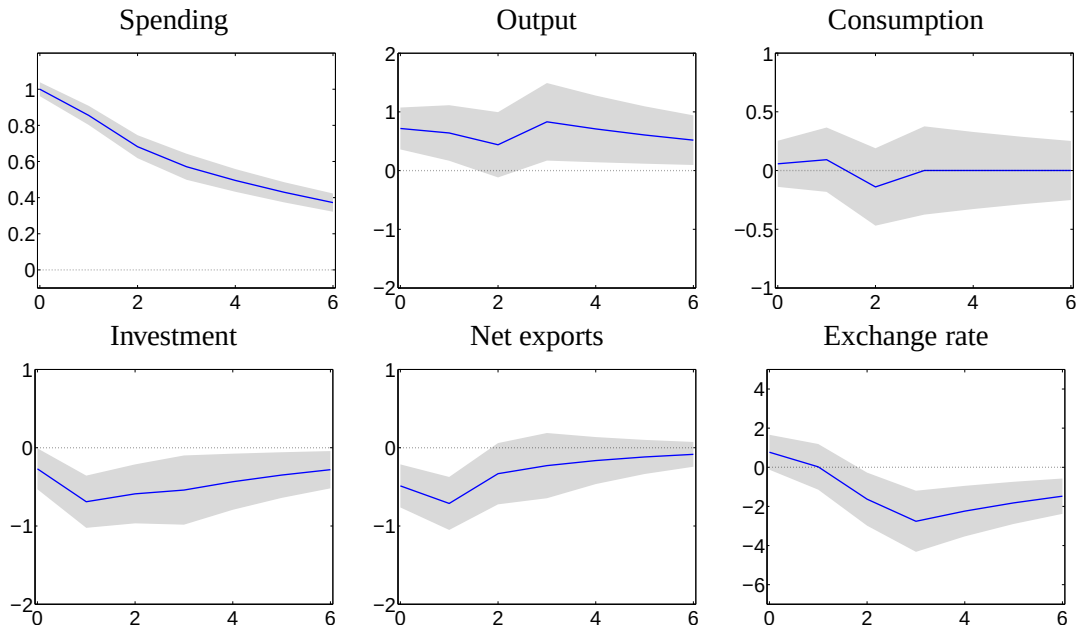

Figure 1: Impulse responses to government spending shock: unconditional effects. Notes: quantities measured in output units; solid line: point estimate (shaded area: ± one standard error); computation 1000 simulations of estimated second stage model (assuming normality).

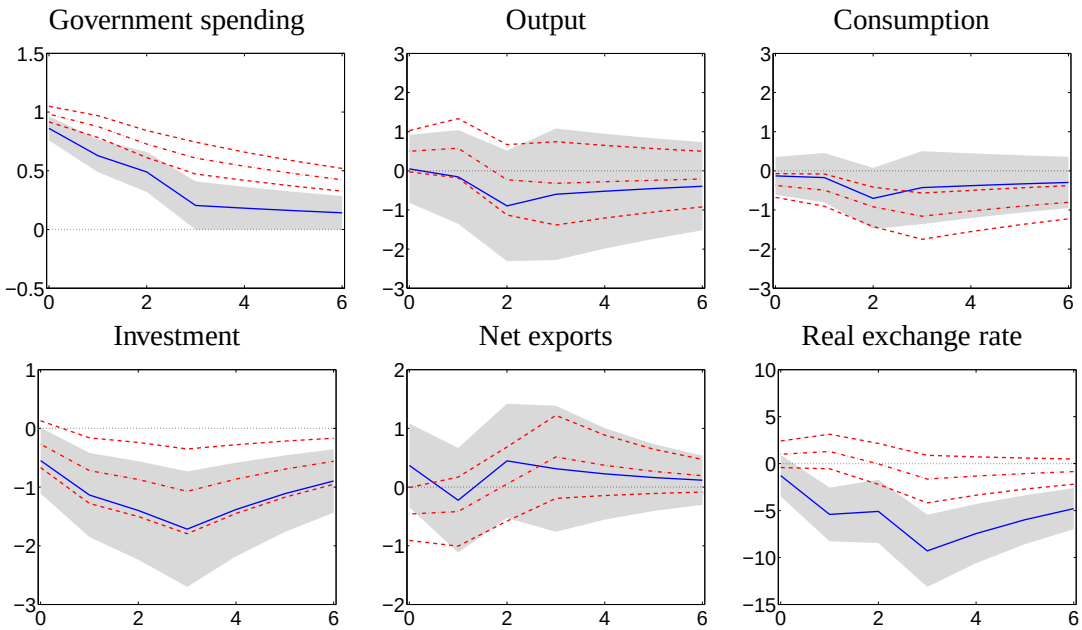

Figure 2: Impulse responses to government spending shock: baseline scenario vs peg. Notes: quantities measured in output units; solid line: point estimate for baseline scenario (shaded area: ± one standard-error); dashed dotted line (dashed lines: ± one standard error): deviation from baseline due to peg; computation 1000 simulations of estimated second stage model (assuming normality).

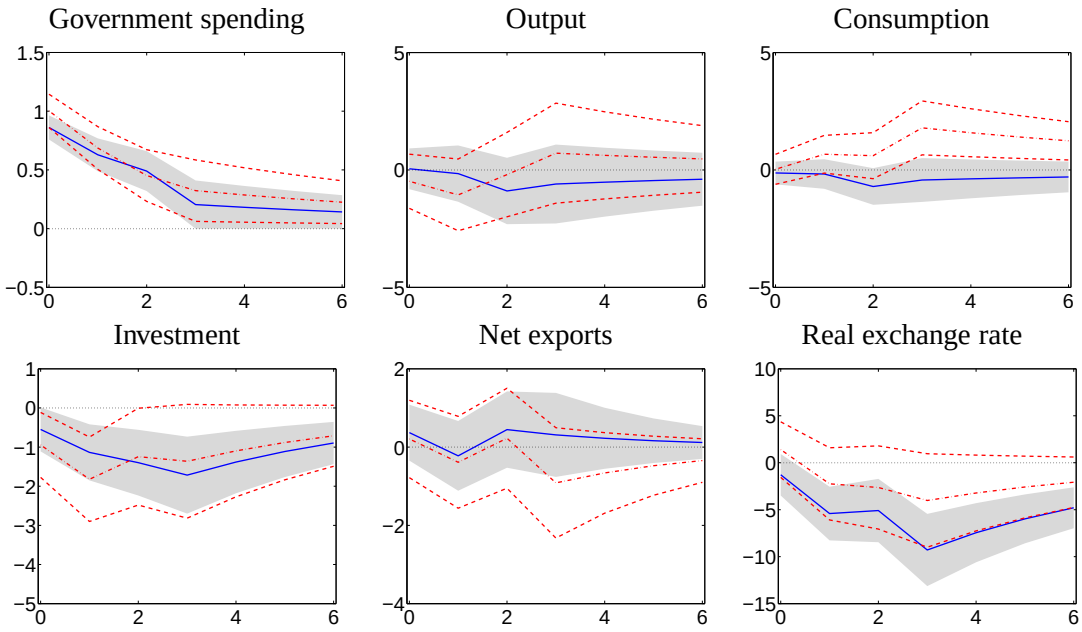

Figure 3: Impulse responses to government spending shock: baseline scenario vs fiscal stress. Notes: see figure 2.

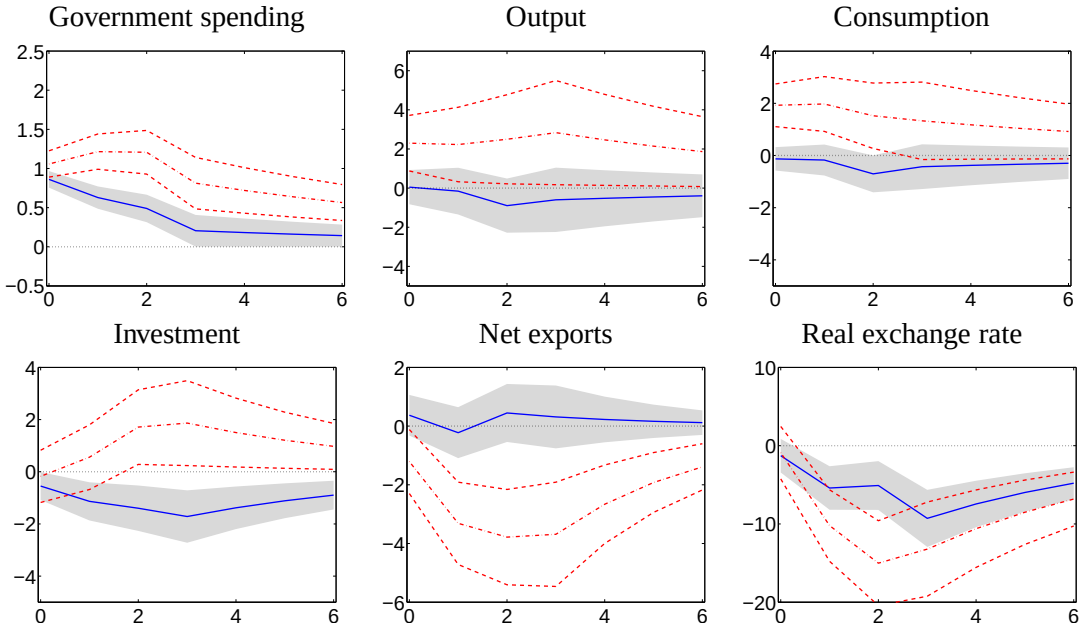

Figure 4: Impulse responses to government spending shock: baseline scenario vs banking/financial crisis. Notes: see figure 2.

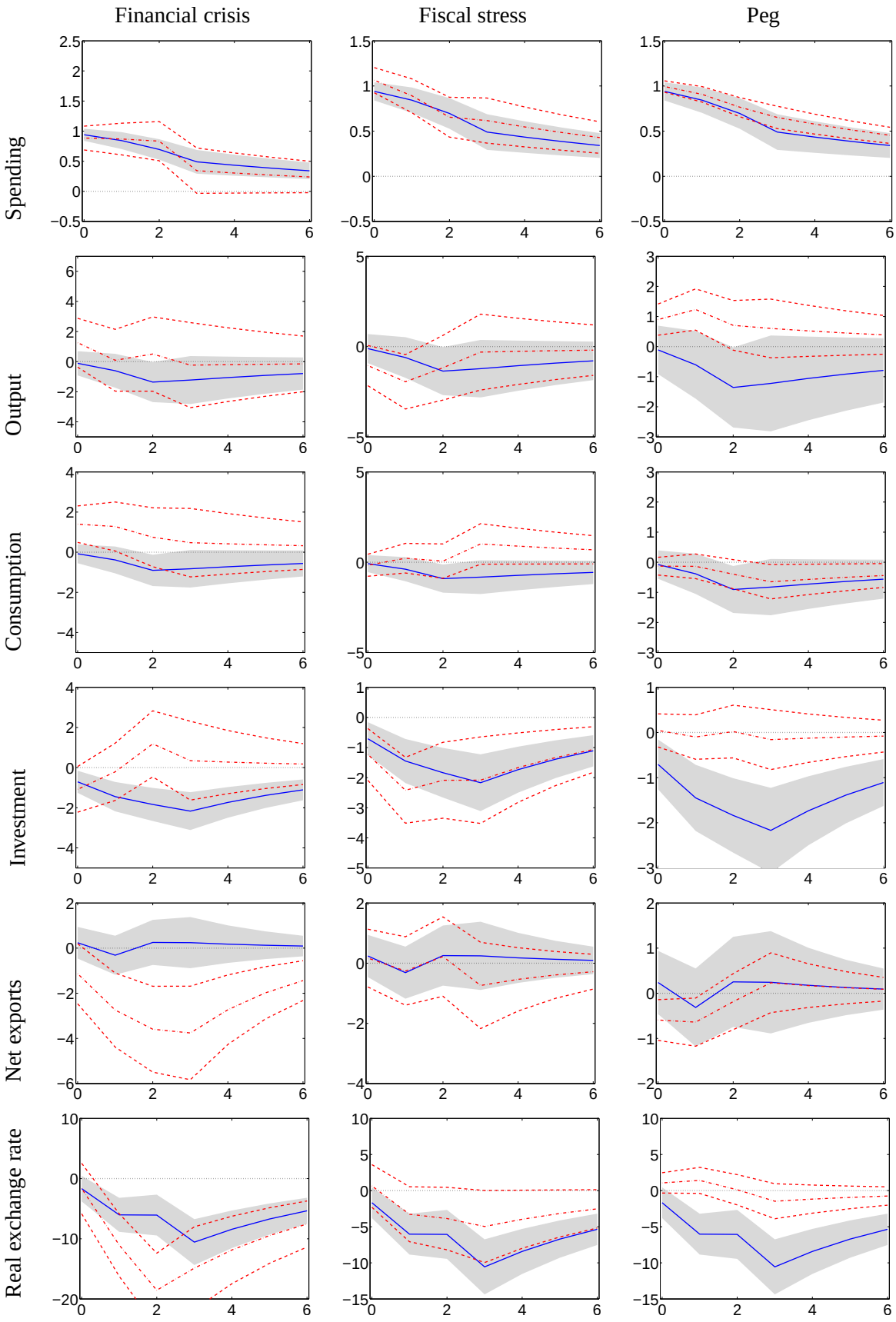

Figure 5: Results for narrow definition of financial crisis. Notes: quantities measured in output units; solid line: point estimate for baseline scenario (shaded area: $\pm$ one standard-error); dashed dotted line (dashed lines: $\pm$ one standard error): deviation from baseline indicated on top of column; computation 1000 simulations of estimated second stage model (assuming normality).

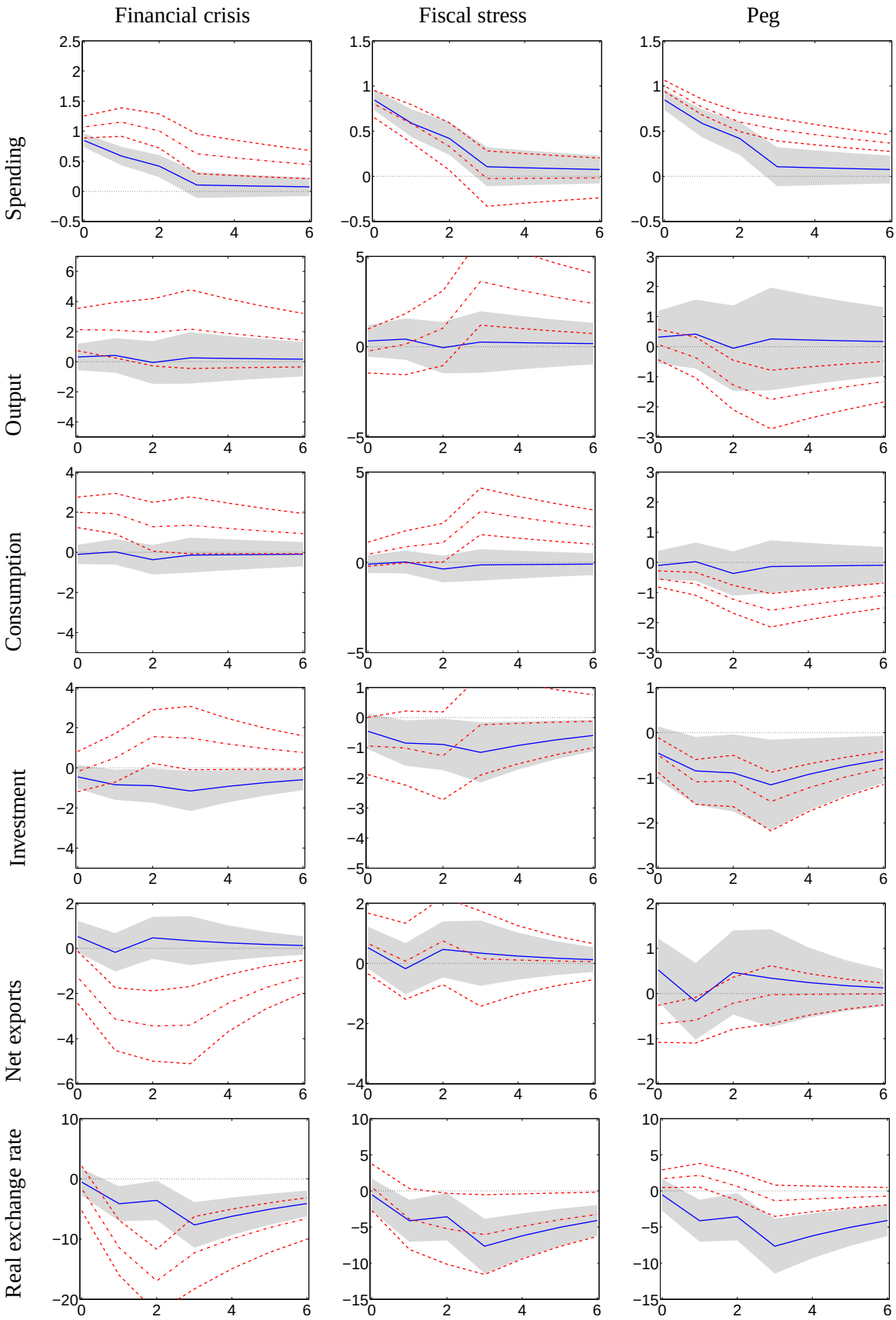

Figure 6: Results for restrictive definition of fiscal stress. Notes: see figure 5.

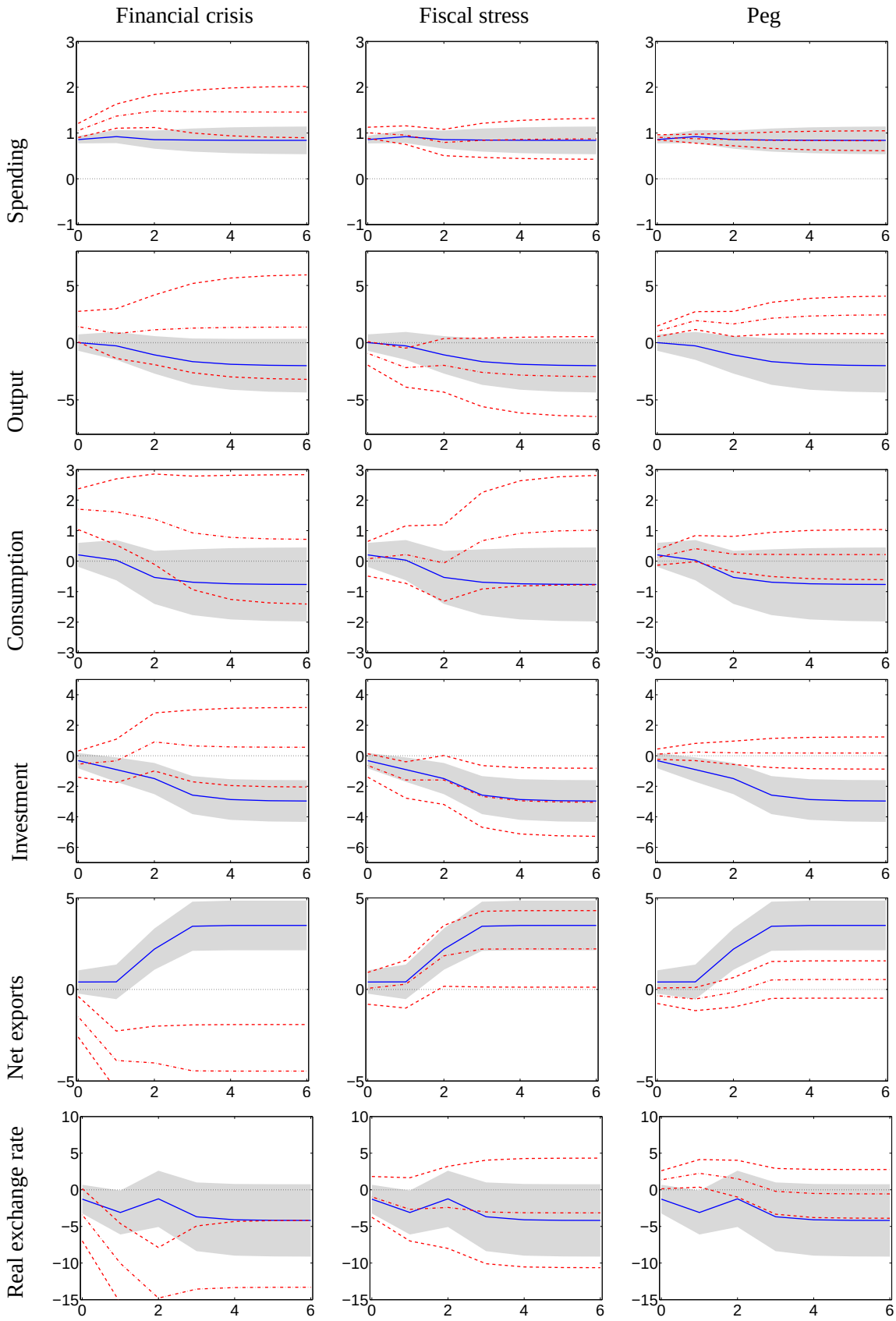

Figure 7: Results for difference specification. Notes: cumulative responses.