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FISCAL SPILLOVERS

The Importance of Macroeconomic and Policy Conditions in Transmission

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FISCAL SPILLOVERS: THE IMPORTANCE OF MACROECONOMIC AND POLICY CONDITIONS IN TRANSMISSION

Are fiscal spillovers today as large as they were during the global financial crisis? How do they depend on economic and policy conditions? This note informs the debate on the cross-border impact of fiscal policy on economic activity, shedding light on the magnitude and the factors affecting transmission, such as the fiscal instruments used, cyclical positions, monetary policy conditions, and exchange rate regimes. The note assesses spillovers from five major advanced economies (France, Germany, Japan, United Kingdom, United States) on 55 advanced and emerging market economies that represent 85 percent of global output—looking at government spending and tax revenue shocks during expansion and consolidation episodes. We find that fiscal spillovers are economically significant in the presence of slack and/or accommodative monetary policy and considerably smaller otherwise, which suggests that spillovers are large when domestic multipliers are also large. We also find that spillovers from government spending shocks are larger and more persistent than those from tax shocks and that transmission may be stronger among countries with fixed exchange rates. The evidence suggests that although spillovers from fiscal policies in the current environment may not be as large as they were during the crisis, they may still be important under certain economic circumstances.

Introduction

The global financial crisis rekindled the debate on the potential of fiscal policy to affect economic activity in other economies through cross-border spillovers. During the crisis, with substantial and persistent economic slack and monetary policy at the effective lower bound in many countries, fiscal stimulus was widely advocated, not least because the expected positive spillovers would add to the effectiveness of the effort at the multilateral level. More recently, the global effects of fiscal policy have been discussed, for example, in connection with changes—either pursued or contemplated—in the macroeconomic policy mix in Japan and the United States. There is also an ongoing debate on whether European countries with excess external surpluses should raise fiscal spending, in part to support growth elsewhere. At the same time, stronger cyclical positions—and a related easing of monetary policy constraints—in many countries raise questions about whether spillovers from fiscal stimulus today would be as large as they were during the global financial crisis.

We examine the magnitude and determinants of cross-border output spillovers from fiscal actions. We analyze the implications of fiscal policy changes in large advanced economies—France, Germany, Japan, the United Kingdom, and the United States—for economic activity in a group of advanced and emerging market economies. The analysis draws general lessons about the main factors behind the transmission of fiscal shocks.

Theory suggests that spillovers depend on the fiscal instruments involved, as well as the cyclical conditions and monetary and exchange rate policy in both shock-emitting (source) and shock-receiving (recipient) economies. First, fiscal shocks associated with expenditure measures are likely to have a direct (and relatively swift) impact on economic activity, while tax measures act indirectly through their impact on saving, consumption, and investment. The strength of these domestic effects—as captured by the domestic multipliers—will, in turn, influence the impact fiscal policy will have on other countries through trade and other channels. Second, spillovers can be stronger if there is a large amount of economic slack—which reduces the extent of crowding out of private sector activity—or if monetary policy is constrained (for example, by the effective lower bound)—since the response of monetary policy to a fiscal shock in both source and recipient countries can dampen its impact. Finally, while a fixed exchange rate between source and recipient may dampen spillovers because relative price adjustment is less pronounced, it may also amplify

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them if trade integration is stronger among a group of pegging countries. Which effect dominates is an empirical question.

The empirical literature on fiscal spillovers so far has focused on a limited set of countries and fiscal policy measures. For example, many studies focus on member countries of the Organisation for Economic Co-operation and Development (OECD) only and either just government spending shocks or only fiscal consolidation episodes. Several papers center their analysis on the euro area (Beetsma and Giuliodori 2004; Beetsma and others 2006; Blanchard, Erceg, and Lindé 2017; Hollmayr 2012) and find sizable spillovers within the currency union. Other papers analyze only fiscal consolidations (Goujard 2017; Poghosyan 2017) or focus on a small group of countries or states within a large federal union (Nicar 2015; Nakamura and Steinsson 2014). Finally, work by Auerbach and Gorodnichenko (2013) studies government spending shocks in 30 OECD economies, finding that spillovers are statistically significant only for shocks coming from a few large OECD countries; for the full sample of countries, spillovers are large and statistically significant only during recessions. Most papers use long sample periods—which makes it difficult to control for structural breaks, such as the introduction of the euro or increasing trade integration—and limit themselves to a single shock identification methodology, leaving questions about the robustness of the identification strategy.

We add to the existing literature by expanding the scope of the analysis. Specifically, we consider both government spending and tax revenue shocks during both budget expansion and consolidation episodes, for a larger set of recipient countries than has yet been studied, including advanced economies as well as emerging markets representing 85 percent of world GDP. In addition, we focus on a more recent sample period than in other studies, which is less likely to include structural breaks associated with increases in trade openness and economic integration across countries. This opens the door to examining fiscal spillovers on economic activity under a wide range of conditions and drawing policy lessons on how they depend on the type of fiscal instruments used and economic conditions in both source and recipient economies.

Our empirical approach to estimating spillovers draws on previous work by Auerbach and Gorodnichenko (2013). In our baseline specification, we identify fiscal shocks for the five systemic economies mentioned above following the structural vector autoregression (SVAR) methodology of Blanchard and Perotti (2002) and focus our analysis on the period covering the first quarter of 2000 through the second quarter of 2016. This shock identification strategy differs from that of Auerbach and Gorodnichenko (2013), but allows us to identify shocks to tax revenues in addition to expenditure shocks, at quarterly frequency. We then combine the information contained in the five source country shocks using trade weights to assess global spillovers onto output. To do this, we use the local-projections method of Jordà (2005), which allows us to estimate our baseline specification and introduce nonlinearities to analyze the role that different factors play in transmission. Our estimates of spillovers reflect all channels of transmission (broadly, trade and financial), though we are unable to disentangle their relative importance explicitly. The baseline approach is subjected to extensive robustness checks using alternative shock identification methods and estimation techniques for the spillover effects.

Our results point to a number of important takeaways. Economic slack and policy constraints can lead to large spillovers from fiscal policy, but spillovers are relatively small during normal times—that is, when economic slack is limited and monetary policy is not constrained. The fiscal policy instrument and policy frameworks also play a role in transmission. More specifically:

- *Fiscal instruments*. Changes in government expenditures have larger and more persistent spillovers than tax revenue measures, particularly over a longer horizon.
- *Cyclical positions*. Spillovers from a fiscal shock are smaller when there is less economic slack in the source or in the recipient economies.
- *Monetary policy constraints*. Policy rates near the effective lower bound amplify spillovers from fiscal policy, as monetary policy will be doing less to offset fiscal shocks. Again, this effect can be at work both in source and in recipient economies.
- *Geographical impact.* Fiscal shocks in the United States have a global impact, with a larger effect in Canada and Latin America. The global impact of shocks from Germany and France is more modest, but they are particularly relevant for Europe.
- *Exchange rate regimes.* Results suggest that spillovers may be amplified for recipient countries whose currencies are pegged with respect to the source country's currency.

The rest of the note is organized as follows. The next section explains the methodology and data used for identification of fiscal shocks. The third section assesses the evidence on fiscal spillovers under the baseline and places our results among comparable studies. The fourth section analyzes the role of critical factors in transmission—cyclical positions and monetary policy constraints. The fifth section assesses the role of exchange rate regimes and is followed by a concluding section. A number of annexes tackle technical issues and perform robustness tests for the results in the baseline.

Identification of Fiscal Shocks

Our identification strategy stresses robustness. This section describes the methodology and data used to identify fiscal shocks in the baseline specification. We rely on the SVAR methodology of Blanchard and Perotti (2002) for the baseline results and explore alternative identification strategies, such as forecast errors and comparable narrative shocks, as robustness checks.[1] We also assess whether our baseline strategy identifies relevant fiscal events by comparing the SVAR shocks with major historical fiscal policy changes documented in the literature, using the narrative method, where available (see, for example, Romer and Romer 2010; DeVries and others 2011; Cloyne 2013; Kataryniuk and Valles 2015).[2] Finally, we examine the impact of our structural shocks on the domestic economy, allowing for a comparison with the literature on domestic multipliers.

A key advantage of our baseline methodology is that it allows for comprehensive coverage and consistent joint identification of revenue and spending shocks across source countries. As noted in the introduction, one contribution of this note is that it analyzes spillovers from shocks to both government spending and tax revenue—for both consolidation and expansion episodes—across five major shock-emitting countries: France, Germany, Japan, the United Kingdom, and the United States. The SVAR methodology facilitates comprehensive coverage and, most importantly, joint identification guarantees that spending and tax shocks are orthogonal to one another, which is critical for comparing spillovers from these two fiscal variables.[3]

Methodology and Data

Blanchard and Perotti's (2002) methodology for identification of government spending and tax revenue shocks relies on two identifying assumptions. First, it assumes that *discretionary* fiscal policy (government spending or taxes) does not respond contemporaneously to unexpected changes in output, even though output can respond contemporaneously to fiscal variables. Second, it uses information from outside the model to calibrate the contemporaneous *automatic* response of tax revenues to output (the tax elasticity). The contemporaneous automatic response of government spending to changes in output is assumed to be zero. Together, these assumptions are equivalent to an ordering restriction in the SVAR, in which innovations in output are placed after innovations in the fiscal variables, with additional conditioning information given by the tax elasticity (see Annex 3 for details).

The analysis uses quarterly data, which is instrumental to the first identification assumption. The assumption capitalizes on the fiscal policy decision lags: it takes time for policymakers to assess unexpected changes in cyclical conditions and make spending and/or tax decisions, including passing new measures through the legislature and implementing them. As Blanchard and Perotti (2002) note, the assumption is more likely to hold in the short term, making the use

[1]Forecast errors are constructed as the difference between actual and projected values of the relevant fiscal variable (spending or taxes). Shocks based on forecast errors are identified as residuals from a regression of the spending- or tax-revenue-based forecast errors on GDP forecast errors and lagged macroeconomic variables.

[2]The narrative method, pioneered by Romer and Romer (2010), makes use of the narrative record, such as budget documents and speeches, to identify the size, timing, and principal motivation for fiscal actions. Romer and Romer's data set also separates fiscal policy changes into those made for reasons related to prospective economic conditions and discretionary actions (for example, to reduce public debt), thereby allowing for a causal analysis of the impact of fiscal policy on output. A shortcoming of narrative-based shocks is that, for most countries other than the United States, mostly consolidation shocks have been identified so far in the literature, making them not directly comparable to the structural shocks used in our analysis.

[3]As noted in Blanchard and Perotti's original paper, possible implementation lags imply that structurally identified shocks could be subject to a fiscal foresight problem, in which the "shocks" may be anticipated by the private sector as a result of earlier announcement of policy changes (see, for example, Forni and Gambetti 2010; Ramey 2011; Leeper, Richter, and Walker 2012; Leeper, Walker, and Yang 2013; and Ben Zeev and Pappa 2015). However, there is some evidence that fiscal foresight may not present a critical issue, especially for assessing the impact of fiscal actions on relatively slow-moving variables such as activity (Perotti 2014), as opposed to forward-looking variables such as the exchange rate (Forni and Gambetti 2016; Auerbach and Gorodnichenko 2016).

of quarterly-frequency fiscal and output data a key part of the identification strategy. In addition, the use of quarterly data—by increasing the degrees of freedom—also allows us to focus on the post-2000 period and avoid possible structural breaks related to increasing trade openness or economic integration across countries, such as the introduction of the euro.

We construct a database of quarterly government spending, tax revenues, and output for the five source countries. Our definition of government spending is the sum of government consumption and investment excluding transfers. On the revenue side, we use tax revenues where available and total government revenues in cases in which quarterly tax revenue data are absent or patchy (for example, Japan).[4] The three series—spending, tax revenues, and output—are seasonally adjusted, converted into per capita real terms, and expressed in logarithms before entering the SVAR specification. The starting point of the sample period differs across countries depending on data availability, ranging from the first quarter of 1980 for the United States to the first quarter of 1995 for Japan (see Annex 1 for details).

Fiscal Shocks

The identified structural shocks using quarterly data are relatively small in magnitude (Table 1). As a share of respective source country GDP, the absolute values of the shocks to government spending average between 0.1 and 0.2 percent and those for shocks to tax revenues between 0.2 and 0.3 percent. The range of shocks varies across source countries and across fiscal instruments, but in most cases historical fiscal shocks are small.

The shocks offer a sensible narrative of fiscal policies adopted over the most recent decades. To assess the relevance of the structural shocks with respect to historical policy records, we have reviewed narrative descriptions of fiscal measures and contrasted the SVAR shocks' order of magnitude with narrative-based shocks estimated in the literature. Figure 1 describes two examples, one for tax revenue shocks in the United States and the other for public-spending shocks in Germany.

[4]In cases in which both tax and total revenues are available, shocks identified using the two revenue measures are very highly correlated. Consistent data on specific tax instruments (for example, corporate and personal income tax, consumption tax) are generally not available.

Table 1. Properties of Structural Shocks
(Percent of source GDP)

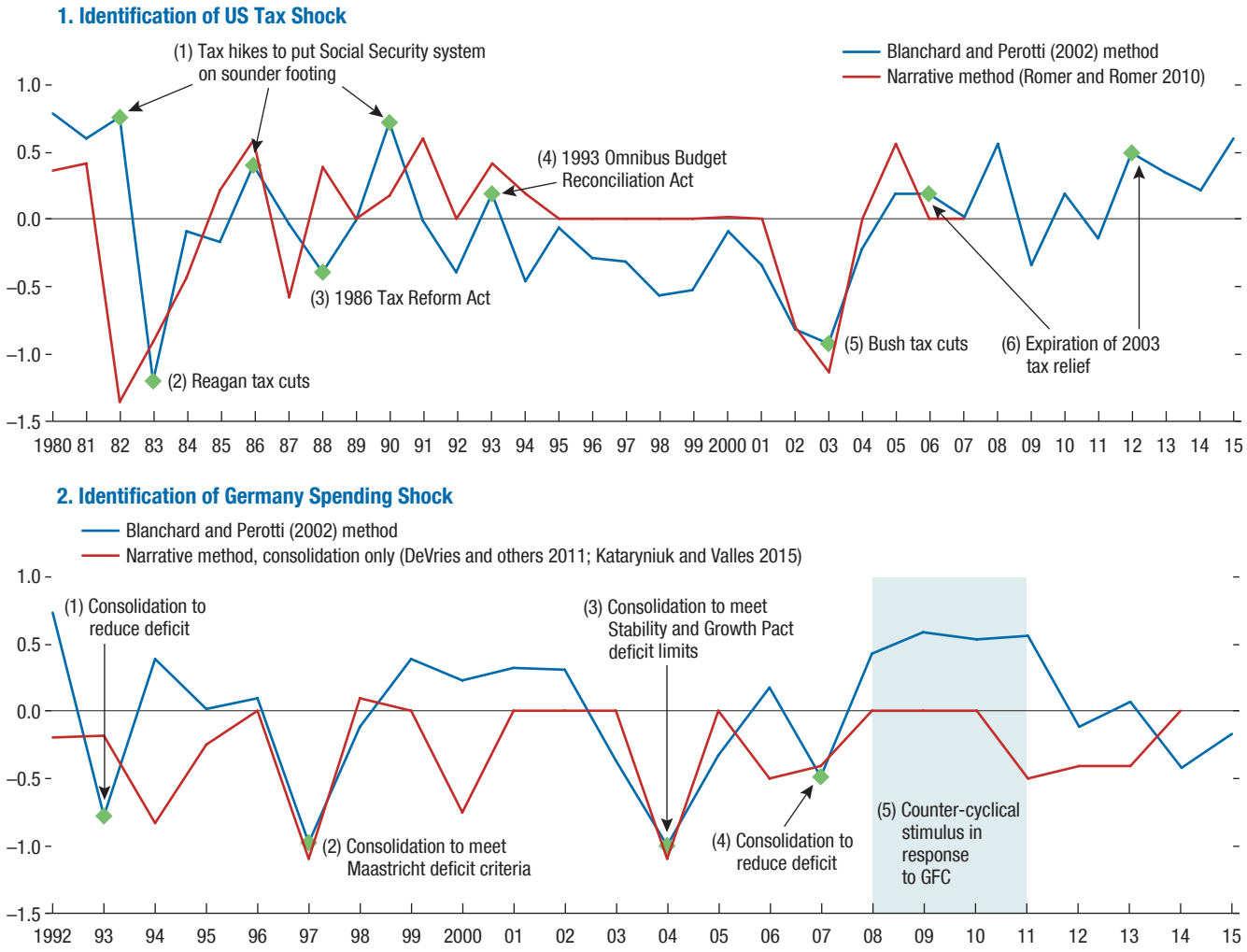
	Mean (Absolute Value)	Standard Deviation	Maximum
Government Spending Shock			
France	0.10	0.11	0.21
Germany	0.14	0.18	0.46
Japan	0.16	0.19	0.56
United Kingdom	0.14	0.17	0.49
United States	0.10	0.14	0.50
Tax Revenue Shock			
France	0.29	0.42	1.49
Germany	0.28	0.35	0.82
Japan	0.21	0.28	1.02
United Kingdom	0.23	0.29	0.70
United States	0.19	0.27	0.55

Source: IMF staff calculations.

- *Tax shocks in the United States.* The narrative shocks identified by Romer and Romer (2010) are the most comparable to the ones identified in this note, in terms of both coverage (both expansion and consolidation episodes) and frequency (quarterly). They are, however, only available until 2007. Figure 1 shows that, as in the narrative approach, the structural shocks capture the tax cuts enacted under the Reagan and Bush administrations as well as the subsequent expiration of the latter. The same is true for tax hikes during the 1980s, which were put in place following the Greenspan Commission's recommendations to shore up the financing of the Social Security system. The order of magnitude identified in the narrative approach is also similar to that identified by the structural shocks.
- *Spending shocks in Germany.* Figure 1 shows several consolidation episodes that are well captured by both our SVAR approach and the narrative database of DeVries and others (2011)—which identifies only consolidation shocks. In particular, consolidations related to the adherence to Maastricht and Stability and Growth Pact deficit criteria in 1997 and 2004, respectively, are identified clearly in both methods and show very similar magnitudes. In addition, countercyclical spending following the global financial crisis is well captured by our structural shocks—but not by the narrative record, since it features only consolidations.

Finally, the structural shocks have a statistically and economically significant impact on the domestic economy. Consistent with traditional Keynesian theory and previous empirical work, estimates of domestic

Figure 1. Tracking US Tax Shock and Germany Spending Shock
(Percent of GDP)



Sources: Romer and Romer 2010; DeVries and others 2011; Kataryniuk and Valles 2015; and IMF staff calculations.
Note: GFC = global financial crisis.

multipliers from our SVAR shocks tend to be larger for spending instruments (slightly above 1) than for tax instruments (slightly below 1).[5] There is also some heterogeneity in the size of the domestic tax multipliers across the five source countries, with the multiplier for the United States being relatively larger than that of its European peers or Japan, possibly reflecting different tax structures across countries as well as the specific tax instruments being used (see Annex 4 for details).

A plausible expectation is that larger domestic fiscal multipliers will lead to larger cross-border spillovers. Naturally, a larger increase in domestic consumption and investment in the source country following a fiscal shock will give rise to higher import demand, which directly benefits trading partners (the so-called *expenditure-shifting* effect or leakages). The marginal propensity to import in both public and private sectors plays a key role: if most of the increase in spending goes to nontradable sectors, spillovers from expendi-

[5]A dollar of government spending contributes directly to aggregate demand, whereas firms and/or households can spend or save a dollar of tax cuts. Thus, the spending multiplier would be larger than the tax multiplier if the marginal propensity to consume is less than 1. Mineshima, Poplawski-Ribeiro, and Weber (2014), based on a survey of 41 studies, document that first-year multipliers for government spending tend to be larger than those for tax revenues in advanced economies.

ture shifting will be small. In principle, however, it is possible for a fiscal shock to have spillover effects even if the domestic impact and/or the marginal propensity to import is relatively small. This can happen, for example, if despite crowding out of domestic private spending, fiscal shocks trigger an increase in import demand associated with an appreciation of the exchange rate in source countries (the so-called *expenditure-switching* effect).[6]

Fiscal Spillovers: Baseline Estimates

Baseline Specification

The local-projections method is used to estimate the response of output in recipient countries to foreign fiscal shocks, similar to the approach of Auerbach and Gorodnichenko (2013).[7] The specification at time horizon h (for $h = 0, \ldots, H$) is given by

$$\frac{Y_{i,t+h} - Y_{i,t-1}}{Y_{i,t-1}} = \alpha_h \frac{Shock_{it}}{Y_{i,t-1}} + \sum_{l=1}^{L} \beta_{hl}' \mathbf{X}_{i,t-l} + \theta_{hi} + \mu_{ht} + \varepsilon_{iht}, \quad (1)$$

in which Y_{it} is real GDP in recipient country i at quarter t; $Shock_{it}$ is the foreign fiscal shock facing country i at time t (to be specified subsequently); $\mathbf{X}_{it}$ is a vector of control variables including lags of the fiscal shock, lags of GDP growth, and lags of external demand—measured as a weighted average of trading-partner growth rates (we choose the number of lags $L = 4$; results are robust to using different lag structures); and θ_{hi} and μ_{ht} capture the country and time fixed effects. As the foreign fiscal shock is expressed in units of recipient country GDP for the panel estimation ($Shock_{it}$ scaled by lagged GDP), the coefficient α_h is analogous to a domestic multiplier of an external shock (Hall 2009; Barro and Redlick 2011). The impulse response for H periods is constructed from a sequence of estimates $\{\alpha_h\}_{h=0}^{H}$.

The recipient sample includes a broad range of countries. As a departure from the existing fiscal spillover literature, which tends to focus on a small set of advanced European or OECD economies, our estimation sample includes 55 recipient economies representing almost 85 percent of global output (on a purchasing power parity basis). The model is estimated using quarterly data for the period covering the first quarter of 2000 through the third quarter of 2016. (See Annex 2 for details on data and list of countries.)

The fiscal shock combines country-specific shocks from the five source economies, weighting their relative importance using trade links with recipient countries. Specifically, the fiscal shock facing recipient economy i in time t is given by

$$Shock_{it} = \sum_{j=1}^{5} \frac{M_{ij,t-1}}{M_{j,t-1}} \frac{s_{jt} E_{j,t-1}}{E_{i,t-1}}, \quad (2)$$

in which j denotes source country, M_{ijt} is country j's goods imports from country i at time t, M_{jt} is total goods imports by country j, s_{jt} is the identified fiscal shock in country j (in its own currency in real terms), and E_{jt} is country j's US dollar real exchange rate. Thus, the second term in the summation equals the real monetary value of the fiscal shock emanating from country j converted into units of recipient country i's currency. This is then scaled by the trade exposure between country i and country j (the first term), which captures the relative importance of recipient country i as a supplier of the source country's imports.[8] The rationale is that, all else equal, recipient countries with tighter trade linkages to the source would be expected to receive larger shocks in the form of larger changes in export demand.[9] Finally, the weighted shocks

[6]In a Mundell-Fleming-Dornbusch framework, a fiscal expansion puts upward pressure on interest rates, appreciates the nominal exchange rate, and increases domestic prices, which results in a real appreciation (see, for example, Fleming 1962; Mundell 1963; and Dornbusch 1976). Note, though, that other frameworks can deliver different exchange rate predictions (see, for example, Obstfeld and Rogoff 1995), in which case the expenditure-switching effect could go in the opposite direction.

[7]This approach was selected, in part, because it is well suited to estimating nonlinear specifications—that is, spillovers under different states of the economy—which is an issue to which we return later in the note.

[8]Bilateral trade data are available for trade in *goods* only.

[9]Implicit in this weighting scheme is the assumption that the marginal propensity to import is the same for all source countries and for both spending and tax shocks. An alternative scheme that attempts to account explicitly for the differences in marginal propensity to import across source countries and types of shocks would be to have an additional scaling factor, M_{jt}/G_{jt} for government spending shocks and M_{jt}/T_{jt} for tax revenue shocks, in which G_{jt} and T_{jt} denote total government spending and tax revenues in country j, respectively. However, these ratios would serve only as very rough proxies for the marginal propensity to import of the public and private sector in any given country. At the same time, such a specification would introduce arbitrary asymmetries into the size of spending relative to tax shocks being transmitted abroad, confounding the comparison of spillover effects from different fiscal instruments. Thus, given that results are robust to alternative weighting schemes (more on this later in the note), we keep the simple scheme in equation (2) for ease of interpretation of the relative magnitude of spillovers from spending and tax shocks.

Table 2. Top 10 Trading Partners, by Share of Total Imports, of Source Economies
(Percent)

	Asia			Europe			Americas		
	France	Germany		Japan		United Kingdom		United States	
1	Germany 18.9	Netherlands	12.6	China	18.8	Germany	13.5	Canada	16.8
2	Belgium 10.7	France	8.5	United States	11.7	United States	8.4	China	13.8
3	Italy 8.4	Belgium	6.9	Australia	5.7	Netherlands	7.3	Mexico	11.8
4	Netherlands 7.5	Italy	6.0	Korea	4.4	France	6.9	Japan	7.5
5	Spain 7.0	China	5.2	Indonesia	4.3	China	5.6	Germany	5.0
6	United Kingdom 5.7	United Kingdom	5.0	Malaysia	3.4	Belgium	5.0	United Kingdom	2.9
7	United States 4.9	United States	4.9	Germany	3.2	Norway	4.8	Korea	2.6
8	China 3.4	Austria	4.4	Thailand	3.1	Italy	4.2	France	2.0
9	Switzerland 2.7	Switzerland	4.0	Russia	1.6	Ireland	3.5	Italy	1.8
10	Russia 1.6	Czech Republic	3.7	Canada	1.6	Spain	3.1	Malaysia	1.5

Source: IMF staff calculations.
Note: Table shows top 10 partners based on trading partner's average share in respective source country's total imports for 2000–15.

are added up across the five source countries.[10] The combined shocks are relatively small in magnitude, with spending (tax) shocks averaging about 0.06 (0.1) percent of recipient country GDP. We also construct a shock to the overall fiscal balance—henceforth referred to as the *overall fiscal shock*—from shocks to government spending and tax revenues (spending minus tax), such that a positive shock implies a reduction in the source country's fiscal balance.

The working assumption behind the construction of the shock is that fiscal policy is transmitted primarily through trade. For each recipient, the trade weight plays the key role of scaling shocks coming from different source economies based on the strength of trade linkages.[11] Combining shocks from all source economies allows us to use critical information from the variability of shocks coming from the major trading centers, as trading patterns indicate that any recipient country potentially receives shocks from more than one source country at any point in time (Table 2). In addition, among the source economies in this study, the United States tends to trade with a wide range of countries, whereas the others are more regionally focused (for example, Germany's most important trading partners are concentrated in Europe). This implies that, based on our trade-driven weighting scheme, US fiscal policy is expected to have a more global impact, while fiscal shocks from other source economies will likely have a more regional impact. Of course, whether this is indeed the case is ultimately an empirical question.

For ease of interpretation of the economic magnitude, results are presented with shocks normalized to an average 1 percent of GDP across *source* countries. Our baseline specification expresses the fiscal shocks in terms of recipient country GDP (see equation (1)), which is necessary to combine shocks from different sources. Although standard in the literature on fiscal spillovers, this transformation can make interpreting the magnitude of spillovers challenging. To facilitate the interpretation of economic magnitude, we consider how much recipient countries' GDP changes when source countries change their overall fiscal stance, spending, or taxes by 1 percent of their own GDP. This requires rescaling our panel results using relative GDP levels and trade links as follows:

$$Spill_{i,j} = S_j \frac{M_{i,j}}{M_j} \frac{Y_j}{Y_i} \alpha, \quad (3)$$

in which S_j is the source country shock as a percent of its own GDP (for this exercise, we consider a 1 percent of GDP shock). Then, this shock is weighted as in our baseline model, using the recipient country's share of the source country's total imports $\frac{M_{i,j}}{M_j}$. To apply the spillover coefficient (α) to this weighted shock, we need to express it in units of recipient country GDP, that is, to multiply by the ratio $\frac{Y_j}{Y_i}$, which captures the relative size of source and recipient country GDP—both measured in US dollars.[12]

[10]Estimated fiscal shocks are uncorrelated across countries.

[11]This does not preclude spillovers through other channels, since our estimates capture the overall response of recipient country GDP. However, the use of a trade-driven weighting scheme may result in some bias of the estimates in situations in which other channels are not proportional to trade—for example, if a recipient country's financial exposure to a source country differs markedly from its trade exposure, although these cases are likely limited.

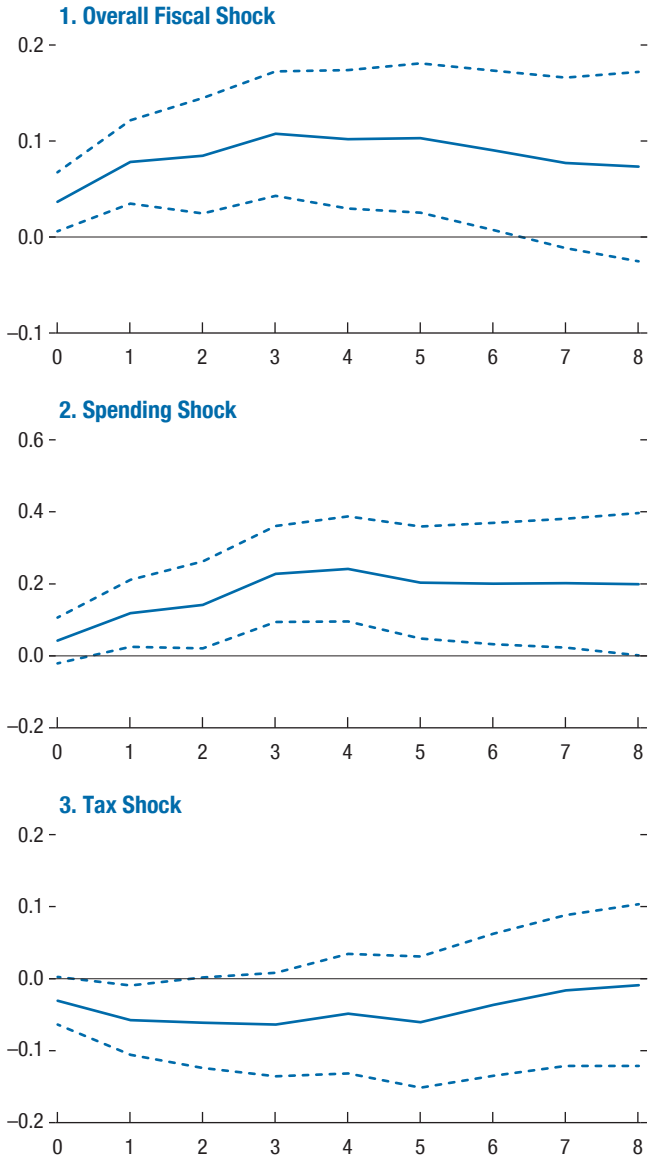
[12]Results shown in terms of source-country-GDP shocks would be unchanged under plausible alternative weighting schemes, as any

Baseline Results

We find significant spillovers from fiscal shocks in the major economies, with spending measures having a larger and more persistent effect than tax measures (Figure 2). The estimates indicate that, on average, an overall fiscal shock equal to an average 1 percent of source countries' GDP would increase recipient output by about 0.04 percent on impact, reaching 0.1 percent at the peak—around the third quarter after the shock—before starting to dissipate. Regarding the specific fiscal instrument, an increase in foreign fiscal spending of similar size is estimated to increase output by about 0.05 percent on impact, with spillovers stabilizing at about 0.2 percent over a two-year horizon—the impact is highly statistically significant.[13] The output response to a foreign tax hike of equal size is more muted and short lived, with output declining by 0.03 percent on impact and reaching a trough of about 0.05 percent by the end of the first year before starting to reverse. The average first-year impact of the tax shock is statistically significant, although it is generally less precisely estimated compared to the spending shock.[14] These results highlight the importance of the fiscal instruments at the source in determining the magnitude and persistence of cross-border spillovers and are broadly consistent with our estimates of domestic spending multipliers being generally larger than domestic tax multipliers. They are also intuitive, since spillovers from a spending shock are directly triggered by the public sector's decision to consume and/or invest, whereas spillovers from a tax shock hinge on the saving, consumption, and investment decisions of many private agents in the source economy.

Our estimates are robust to alternative specifications and shock identification strategies. The baseline results are robust to inclusion of additional control variables (for example, short-term interest rate, output gap, unemployment rate, and fiscal position in recipient countries).[15] In addition, to account for possible cross-sectional correlation among our panel

Figure 2. Dynamic Responses of Recipient Countries' Output Level to Fiscal Shocks
(Percent)



Source: IMF staff calculations.
Note: Numbers along the horizontal axes represent quarters; $t = 0$ is the quarter of respective shocks. Solid lines denote point estimates, and dashed lines denote 90 percent confidence bands. Shocks are normalized to an average 1 percent of GDP across the source countries.

alternative weighting schemes would also require us to recalculate the spillover coefficient estimated in equation (1), resulting in an equal-and-offsetting adjustment of this coefficient, since any transformation applied to the *source* shock would be constant across all *recipient* countries.

[13]We use the term "spillovers" to refer to a recipient GDP's response to the *initial* fiscal shock at the source, that is, a point estimate of the impulse-response function. Our approach does not estimate "multipliers" or "cumulative multipliers," as the aggregation of shocks across source countries makes this infeasible.

[14]The standard errors are clustered at the country level. The fiscal shock is generated outside of the local-projections model presented in equation (1), implying that confidence bands may be wider than presented in Figure 2 if uncertainty around the estimated shocks is also taken into account.

[15]As long as any omitted variable not considered in this list is uncorrelated with the fiscal shock in the source country, then its omission will not affect our spillover estimates.

of recipient countries, we conduct a robustness check by applying the Driscoll and Kraay (1998) correction to our standard errors; resulting confidence bands are nearly identical to those in the baseline. We also obtain similar spillover estimates—albeit slightly larger in magnitude—using a panel vector autoregression (VAR) estimation methodology that explicitly allows for endogenous responses of exchange rates and interest rates to fiscal shocks, which can also affect the dynamics of output. In addition, estimates using comparable fiscal shocks obtained from alternative identification strategies—namely, forecast errors and the narrative approach—also yield spillover estimates that are similar in size and dynamics, providing reassurance that our results are not driven by the SVAR methodology for identifying fiscal shocks (details on robustness tests are discussed in Annex 5).[16]

Our estimates of fiscal spillovers are also broadly in line with earlier estimates. Focusing on the one-year average impact, our estimates indicate that a 1 percent of GDP overall fiscal shock in an average major advanced economy would raise output in the recipient country by about 0.08 percent; the impact would be 0.15 percent and –0.05 percent for a spending and a tax hike of the same magnitude, respectively. Although differences in country and time samples as well as shock identification make a direct comparison challenging (for example, our sample includes several years of significant economic slack), these are of the same order of magnitude as those found in previous work. For example, Beetsma and others (2006) find that a 1 percent of German (French) GDP shock to government spending results in a European GDP response of about 0.14 (0.08) percent after two years; for a tax shock, spillovers are about –0.05 (–0.03) percent. Compared with studies that express shocks in units of recipient country GDP (Auerbach and Gorodnichenko 2013; Goujard 2017), estimates are also broadly similar (see details in Annex 6).

Estimates of spillovers differ across regions, with those from the United States having a more global impact than those from other countries. Table 3 shows approximate calculations of potential spillover effects by region, using the baseline spillover coefficient estimate, trade links, and relative size between a particular source country and the average country in different geographical regions.[17] These should be interpreted as illustrative of the relative regional impact of fiscal shocks from different source countries, rather than precise estimates of the magnitude of spillovers. The calculations suggest that fiscal shocks in the United States will likely have larger spillovers than shocks in other countries, owing to the larger size and broader trade links of the US economy. For example, a 1 percent of US GDP government spending shock would increase the average recipient country GDP by about 0.33 percent over the first year, compared to a 0.15 percent impact from a 1 percent of German GDP spending shock.[18] In addition, spillovers vary across regions, reflecting trade patterns. As expected, US shocks have a relatively global reach, with larger impact in Canada, Latin America, and Asia, where trade linkages with the US economy are tighter. By contrast, the impact of European shocks is mainly concentrated in Europe and that of Japanese shocks in Asia, given those source economies' more regional trading focus.

The Role of Cyclical Positions and Monetary Policy Constraints

The business cycle and monetary policy conditions can affect the magnitude of spillovers. In general, a larger impact of fiscal shocks in a source economy is expected to give rise to more significant spillovers. Hence, factors expected to affect the domestic impact of fiscal shocks—relevant for source countries—and/or their cross-border transmission should affect spillovers.

The literature on domestic fiscal multipliers points to the state-dependency of the results. Theoretical models indicate that the impact of fiscal shocks can be strong in the presence of significant economic slack, due to reduced crowding out of private sector activity (Michaillat 2014) or an increase in the share of liquidity-constrained households (Canzoneri and others 2016). In addition, when monetary policy is

[16]We use the narrative-based tax shocks for the United States as identified by Romer and Romer (2010) to conduct the robustness test, since these are the most comparable narrative shocks to our structural shocks (quarterly frequency, covering both expansion and consolidation episodes).

[17]For simplicity, only spending and tax shocks are presented. These simple calculations are intended to allow for comparison to other studies in the literature and may be misleading for some countries in our sample—an example of this are spillovers from Japan to Asia, which may be overstated by these calculations, since Japan is a relatively closed economy and as such accounts for a relatively small share of Asia's overall exports.

[18]Note that 1 percent (the unit we pick for illustrative purposes) represents an abnormally large shock in historical terms. To put this in context, the largest US government spending shock in our sample is 0.5 percent, with the average (absolute) value at only 0.1 percent.

Table 3. Average One-Year Regional Impact of Fiscal Shocks
(Percent)

	Canada + LAC5	Europe	Asia	Average
Government Spending Shock				
France	0.01	0.14	0.02	0.08
Germany	0.02	0.26	0.04	0.15
Japan	0.10	0.04	0.35	0.10
United Kingdom	0.02	0.14	0.04	0.09
United States	0.84	0.20	0.47	0.33
Tax Revenue Shock				
France	0.00	–0.05	–0.01	–0.03
Germany	–0.01	–0.09	–0.01	–0.05
Japan	–0.03	–0.01	–0.12	–0.04
United Kingdom	–0.01	–0.05	–0.01	–0.03
United States	–0.30	–0.07	–0.17	–0.12

Source: IMF staff calculations.
Note: Table shows regional responses to a 1 percent of GDP shock in the source country. LAC5 includes Brazil, Chile, Colombia, Mexico, and Peru. The impact is calculated by scaling the estimated spillover coefficient by a country-specific scaling factor, which is a function of the trade exposure (recipient country's share in source country's imports) and source country GDP relative to recipient country GDP.

constrained by the effective lower bound—such as when the economy is in a liquidity trap—the lack of monetary policy response to higher expected inflation following a fiscal shock causes real interest rates to decline, thereby crowding in domestic demand and increasing the multiplier.[19] Several empirical studies have found larger government spending multipliers when the economy is in recession and/or when monetary policy is operating at the effective lower bound.[20]

Spillovers from fiscal shocks can differ depending on the state of the economy as well, in both source and recipient countries. When the source country has substantial slack or policy rates are near the effective lower bound, the larger domestic impact of a fiscal shock implies greater demand for imports and thus may produce larger spillovers through expenditure shifting. In addition, when they are facing an external shock, recipient economies will be affected more if (1) they are unable or unwilling to counteract it with countercyclical monetary policy, (2) slack reduces the extent of crowding out from higher export demand to the rest of the economy, or (3) the share of liquidity-constrained households in the economy is substantially higher during periods of slack.[21] Consistent with these predictions, our results show that spillovers are small during normal times and much larger when either the source or recipient economy has slack or monetary policy is close to the effective lower bound. We note, however, that the estimates for slack and effective lower bound should be interpreted with caution, as it is difficult to disentangle these two states empirically, given that they often occur in tandem in our sample.[22]

Recipient Countries

We first examine how spillovers vary with the state of the recipient economy. We estimate a nonlinear version of the baseline specification, in which we partition the shock as well as the control variables according to the "state" variable of interest (that is, output gap or short-term interest rate); this allows us to consider spillovers from fiscal shocks under different economic states. Thus, following Auerbach and Gorodnichenko (2013), we adapt the baseline specification from equation (1) in the following way:

$$\frac{Y_{i,t+h}-Y_{i,t-1}}{Y_{i,t-1}} = \alpha_{1h} I_{i,t-1}\frac{Shock_{it}}{Y_{i,t-1}} + \alpha_{2h}(1-I_{i,t-1})\frac{Shock_{it}}{Y_{i,t-1}} + \sum_{l=1}^{4}\beta'_{1hl} I_{i,t-1}\mathbf{X}_{i,t-l} + \sum_{l=1}^{4}\beta'_{2hl}(1-I_{i,t-1})\mathbf{X}_{i,t-l} + \theta_{hi} + \mu_{ht} + \varepsilon_{iht}, \quad (4)$$

in which $I_{i,t}$ takes the value of either 1 or 0, indicating the state in recipient country i in period t. We consider two different states for a recipient country's cyclical

[19]See, for example, Christiano, Eichenbaum, and Rebelo 2011; Eggertsson 2011; Woodford 2011; and Blanchard, Erceg, and Lindé 2017. This can be true for expansionary fiscal shocks as well as contractionary shocks. For example, if central banks aim for a more accommodative stance than feasible, a fiscal expansion may be fully accommodated, thereby increasing domestic multipliers and spillover effects. This rationale applies with a (rather unlikely) caveat. Should the actual and shadow policy rates in the source economy be at zero at times in which the economy is closing the output gap, nothing would prevent a policymaker from hiking rates to counteract fiscal expansions, preventing an amplification of the fiscal shock.

[20]See, for example, Almunia and others 2010; Gordon and Krenn 2010; Auerbach and Gorodnichenko 2011; Auerbach and Gorodnichenko 2012; DeLong and Summers 2012; Blanchard and Leigh 2014; and Dell'Erba, Koloskova, and Poplawski-Ribiero 2014. On the other hand, there are studies that find little to no state-dependent effects in the United States (for example, Ramey and Zubairy, forthcoming, and Owyang, Ramey, and Zubairy 2013).

[21]Credit market imperfections could also play a role in increasing multipliers when a country is in a liquidity trap (see Carrillo and Poilly 2013).

[22]For example, in our post-2000 sample, about 26 percent of country-quarter observations fall under the definition of "effective lower bound," three-quarters of which coincide with economic slack; similarly, about 55 percent of observations fall under the definition of "slack," 35 percent of which coincide with the effective lower bound.

position (slack/no slack) and two different states for the ability of its monetary policy to respond to shocks (near the effective lower bound/normal times). In both cases, spillovers under the two different states can then be examined by comparing the estimated α_{1b} and α_{2b} parameters.

In the presence of economic slack, spillovers tend to be larger than in normal times. Table 4 provides estimates for economic slack corresponding to a negative output gap in the recipient, which show that spillovers are larger under this condition for an overall fiscal shock as well as separately for government spending and tax revenue shocks in the source. Specifically, over the first year, the spillover effects under slack are 0.11, 0.2 and –0.08 percent for an overall fiscal shock, a government spending shock, and a tax shock, respectively.[23] By contrast, during periods of no economic slack in the recipient, the corresponding spillover estimates are 0.06, 0.08, and –0.04 percent, respectively.[24]

Spillovers are even larger when interest rates are close to the effective lower bound. Conducting the same regime-dependent analysis as for a country's cyclical position (equation (4)), we evaluate spillovers under two different monetary policy regimes: $I_{i,t} = 1$, when a country's short-term interest rate is below the 25th percentile value of the cross-country distribution, and $I_{i,t} = 0$, otherwise.[25] Table 4 suggests that the response of recipients' output to fiscal shocks is markedly stronger and more persistent in the case of exceptionally low interest rates in the recipient economy; for example, the spillover estimate from a 1 percent of source country GDP overall fiscal shock is 0.19 percent when the effective lower bound is binding—more than four times larger than when interest rates are not near the effective lower bound. The differential effects are also observed for spending and tax instruments separately.

[23]Results are robust to using alternative definitions of slack, including using the unemployment gap or smooth-transition probability as in Auerbach and Gorodnichenko 2013.

[24]Our findings are consistent with the evidence for a sample of OECD countries in other studies in the literature (Auerbach and Gorodnichenko 2013; Goujard 2017). See Annex 6.

[25]We use different distributions for advanced economies and emerging markets. The 25th percentile value for the cross-country distribution is about 0.57 percent for advanced economies and 3.0 percent for emerging markets. Results are robust to using alternative definitions of effective lower bound, such as using absolute thresholds (common to all countries) for the short-term interest rates.

Table 4. Nonlinear Results: State in Recipient Economy
(Percent)

	Overall Fiscal Shock	Spending Shock	Tax Shock
Baseline	0.08***	0.15**	–0.05*
Economic Slack	0.11***	0.20***	–0.08**
No Economic Slack	0.06**	0.08	–0.04
Difference Statistically Significant? (p-value)	*0.27*	*0.18*	*0.43*
Interest Rate Near Effective Lower Bound	0.19***	0.3***	–0.15**
Interest Rate Not Near Effective Lower Bound	0.04	0.07	–0.02
Difference Statistically Significant? (p-value)	*0.01*	*0.02*	*0.07*

Source: IMF staff calculations.
Note: Table shows the average one-year response of recipient GDP to a shock of 1 percent of GDP across the source countries.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Source Countries

We next study how the state of the economy in *source* countries affects spillovers. To do this, we partition the shocks into two parts according to the state variable of interest—the cyclical position or the ability of monetary policy to respond to shocks, both defined as in the specification for recipient economies—as follows:

$$Shock^j_{it}: I^j_{t-1} Shock^j_{it} + (1 - I^j_{t-1}) Shock^j_{it}, \quad (5)$$

in which I^j_t indicates the relevant state in the shock-emitting country. The assumption underpinning this approach is that although shocks in the source country and its domestic response might be regime dependent, their propagation to recipient countries is not.

Spillovers are larger when there is economic slack and/or interest rates are at the effective lower bound in source countries. Table 5 shows that spillovers from both spending and tax shocks are considerably larger when the source country has economic slack. In fact, the response of recipient output to a fiscal shock when the source economy has no slack is very small, pointing to a dampening effect from countercyclical policy measures. It also suggests that while spillovers are small during normal business cycle and monetary policy conditions, they are much larger when interest rates are at the effective lower bound. Moreover, the same regime-dependent result that we documented for recipient countries holds here as well: the role of monetary policy constraints in

Table 5. Nonlinear Results: State in Source Economy
(Percent)

	Overall Fiscal Shock	Spending Shock	Tax Shock
Baseline	0.08***	0.15**	–0.05*
Economic Slack	0.09***	0.17***	–0.06**
No Economic Slack	0.02**	0.07	–0.02
Difference Statistically Significant? (p-value)	*0.35*	*0.54*	*0.58*
Interest Rate Near Effective Lower Bound	0.13***	0.25***	–0.10***
Interest Rate Not Near Effective Lower Bound	0.04	0.06	–0.01
Difference Statistically Significant? (p-value)	*0.12*	*0.15*	*0.20*

Source: IMF staff calculations.
Note: Table shows the average one-year response of recipient GDP to a shock of 1 percent of GDP across the source countries.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Table 6. Spillovers under Different Cyclical and Policy Conditions
(Percent)

	Normal Times	Slack	Near Effective Lower Bound
Government Spending Shock			
France	0.04	0.09	0.14
Germany	0.07	0.18	0.27
Japan	0.05	0.13	0.19
United Kingdom	0.04	0.11	0.17
United States	0.15	0.40	0.61
Tax Revenue Shock			
France	–0.01	–0.04	–0.06
Germany	–0.02	–0.07	–0.12
Japan	–0.02	–0.05	–0.08
United Kingdom	–0.02	–0.04	–0.07
United States	–0.05	–0.16	–0.27

Source: IMF staff calculations.
Note: Table shows average one-year impact on average country in sample from a shock of 1 percent of GDP across the source countries. Normal Times refer to an average of no slack and no effective lower bound in both source and recepient countries. Slack (Near Effective Lower Bound) estimates are averages for conditions in source and recepient economies.

boosting spillovers is more pronounced than the role of economic slack alone. Our finding is similar in spirit to the analysis of the euro area in Blanchard, Erceg, and Lindé 2017, in which a fiscal expansion in the core euro area has larger spillovers to other euro area countries under the effective lower bound, as higher inflation in the core reduces real interest rates, thereby stimulating demand in both the core and the rest of the euro area.

Finally, to compare with baseline results, we also present state-dependent spillover estimations by individual source country. Table 6 splits the figures in Table 3 for the average country by different states of the economy using the values of α from our state-dependent analysis and shows contrasting responses to shocks in normal times compared to when there is slack or interest rates are at the effective lower bound. For simplicity, "normal times" estimates are averages of estimates under no slack and those under no effective lower bound, and slack (effective lower bound) estimates are averages of estimates for slack (effective lower bound) in source and those for slack (effective lower bound) in recipient. The calculations indicate that there is a large range of spillover estimates depending on cyclical and/or policy conditions. For example, a 1 percent of US GDP government spending shock would increase the average recipient country GDP by only 0.15 percent in normal times over the first year, but about 0.4 percent if there is slack and 0.61 percent if interest rates are at the effective lower bound. A 1 percent of German GDP spending shock would do so by 0.07 percent in normal times, 0.18 percent during a period of slack, and 0.27 percent with monetary policy constraints.

Role of Exchange Rate Regimes

The exchange rate regime can affect the transmission of fiscal shocks. Fiscal shocks will have a direct impact on trade—expenditure shifting—increasing (decreasing) exports in recipient countries in the presence of fiscal expansions (consolidations) at the source. They may also—in a Mundell-Fleming-Dornbusch framework—lead to a change in interest rate spreads, triggering an appreciation (depreciation) of the source country's currency during fiscal expansions (consolidations), particularly if the change in fiscal stance is expected to be persistent and debt financed.[26] This increases incentives

[26] The empirical analysis of the exchange rate response to fiscal shocks is beyond the scope of this note. Although the empirical literature has generally found mixed results, some recent work has found evidence of exchange rate appreciation following a fiscal expansion when the anticipation of the fiscal shock is properly taken into account. The exchange rate is a forward-looking variable that reacts on announcements about future spending, that is, before spending takes place. Two recent studies (Auerbach and Gorodnichenko 2016; Forni and Gambetti 2016) isolate the announcement component of fiscal shocks and show that the exchange rate appreciates following news about future fiscal expansions. Popescu and Shibata (2017) extend Forni and Gambetti's (2016) work to a cross-country per-

for consumers to shift consumption toward relatively cheaper goods—expenditure switching—amplifying spillovers. The exchange rate regime can affect both elements of transmission.

The impact of exchange rate regimes on transmission is an empirical question; pegs can strengthen expenditure shifting between source and recipients, but lack of exchange flexibility can dampen expenditure switching. Several studies have documented a positive association between pegs and trade (for example, Rose and van Wincoop 2001; Klein and Shambaugh 2006; and Qureshi and Tsangarides 2010). They emphasize that fixed exchange rate regimes can reduce exchange rate volatility, thereby providing certainty that is helpful in forming trade relationships, which can increase spillovers from fiscal shocks.[27] On the other hand, lack of nominal exchange rate flexibility within currency pegs will likely curb exchange rate adjustments, reducing expenditure switching and, hence, spillovers.[28] Currency mismatches in balance sheets of households and corporations in recipient economies can also affect the size of spillovers, for example, by making depreciations contractionary in recipients.[29]

This section assesses the role of exchange rate regimes by looking at pegs with respect to the US dollar. The United States is a suitable country on which to conduct this exercise given its global currency and systemic trade importance. In particular, countries typically do not peg to the British pound or the Japanese yen. In the case of the euro, Germany and France's importance is mostly within Europe, where the majority of countries have a fixed regime, not allowing for enough variation in the data to identify the effect under a flexible regime.

To assess the role of exchange rate regimes in spillovers, the empirical approach is modified to allow different spillovers to countries with fixed and flexible exchange rate regimes with respect to the US dollar. The split is based on two alternative schemes: (1) Reinhart and Rogoff's (2004) de facto time-varying exchange rate regime classification, updated by Ilzetzki, Reinhart, and Rogoff (2017a, 2017b) ("Reinhart-Rogoff" classification); and (2) the IMF's *Annual Report on Exchange Arrangements and Exchange Restrictions* ("IMF" classification);[30] in both cases fixed regimes include de facto pegs and crawling pegs. Using this bilateral exchange rate arrangement, we split the shock from the United States to recipient i into two parts:

$$Shock_{it}^{US}: Fix_{i,t-1}^{US} Shock_{it}^{US} + (1 - Fix_{i,t-1}^{US}) Shock_{it}^{US}, \quad (6)$$

in which $Fix_{it}^{US} = 1$ if country i and the United States share a fixed regime in period t. Spillovers are then estimated using the local-projections method. More details are provided in Annex 2.

The evidence points to larger spillovers from government spending shocks under fixed exchange rate regimes. Figure 3 shows persistent spillovers from US spending shocks to recipients whose currency is pegged to the US dollar (solid green line) and smaller and shorter-lived spillovers to countries with flexible exchange rates. This is the case regardless of which exchange rate regime classification is used. The difference in the output responses between fixed and flexible regimes is statistically significant on impact under both classifications and also during the second year under the Reinhart-Rogoff classification. We find no difference in spillovers from an overall fiscal shock or a tax revenue shock under different exchange rate regimes.

Larger spillovers under fixed exchange rate regimes seem to suggest relatively weak expenditure-switching effects in transmission. This weakness could reflect that US monetary policy was constrained by the effective lower bound for a large part of the sample, limiting interest rate and exchange rate movements. At the same time, direct trade channels may be larger under pegs as a result of stronger trade integration. Further work is required to disentangle these effects. The case of a currency union—such as the euro area—is particularly interesting, as in such a case, a common monetary policy dampens expenditure switching, while long-standing economic and institutional integration and the use of a common currency strengthen trade and hence expenditure-shifting effects (Rose and van Wincoop 2001; Berger and Nitsch 2008).

spective to study the impact of US government spending shocks on external positions.

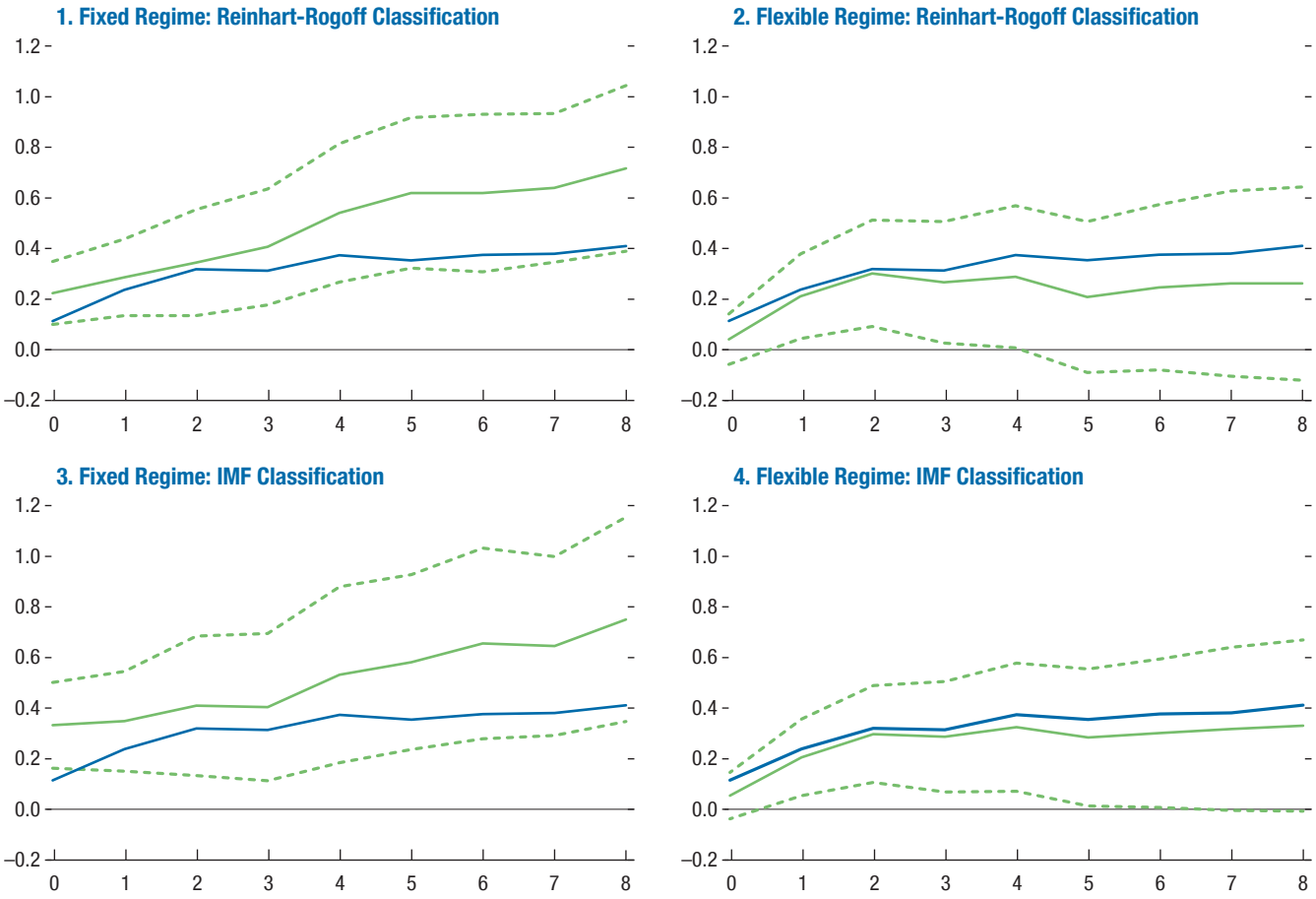
[27]Qureshi and Tsangarides (2010) point to comparable effects in conventional and hard pegs (currency unions).

[28]This mechanism is especially relevant in the short term, because real exchange rates will adjust over the medium term, mitigating the differences in spillovers between the two regimes.

[29]For a discussion on contractionary devaluations, see Diaz Alejandro 1966 and Edwards 1987.

[30]In 2015, for example, the Reinhart-Rogoff classification yields seven recipient countries with "fixed" exchange rates, while the IMF classification yields two "fixed" countries. However, the number of fixed-rate countries varies over time (and there tend to be more of these in earlier years).

Figure 3. Dynamic Responses of Recipient Countries' Output to US Spending Shock under Different Exchange Rate Regimes
(Percent)



Source: IMF staff calculations.
Note: The figure depicts the impact on output level. Numbers on horizontal axes represent quarters; $t = 0$ is the quarter of respective shocks. Solid green lines denote point estimates conditional on exchange rate regime; dashed green lines denote 90 percent confidence bands; and solid blue lines represent unconditional estimates. Shocks are normalized to an average 1 percent of GDP across the source countries (note that this will represent a shock of less than 1 percent of US GDP).

Conclusions

This note informs the debate regarding cross-border spillovers from fiscal policy on economic activity. It sheds light on their magnitude and the factors affecting their transmission, such as the fiscal instruments used, cyclical positions, monetary policy conditions, and exchange rate regimes. We assess spillovers from five economies—France, Germany, Japan, the United Kingdom, and the United States—on 55 advanced and emerging market economies that represent 85 percent of global GDP—a larger sample than any other study in the literature. Using information on bilateral trade links to inform the patterns of global and regional spillovers from the source economies, we draw general lessons about the transmission of fiscal shocks. We consider both government spending and tax revenue shocks during both expansion and consolidation episodes, extending the range of conditions considered in the literature.

The analysis in this note offers lessons on the magnitude and transmission of fiscal shocks. In general, they indicate that the magnitude of spillovers from fiscal shocks depends critically on conditions that also affect their domestic impact. The findings can be summarized as follows:

- *The fiscal instrument matters.* There is evidence of larger and more persistent spillovers from changes in government spending, relative to those from changes

in tax revenues. For example, on average, a 1 percent of GDP spending hike in a major advanced economy can raise output in recipient countries by 0.15 percent over the first year, against 0.05 percent for a tax cut of equal size.

- *Relatively weak cyclical positions imply larger spillovers.* This is true for source countries—suggesting that slack in the source may increase spillovers through a larger *domestic* impact of the fiscal impetus—as well as for recipient countries, suggesting stronger *transmission* in the presence of slack. When economies have little or no slack, estimated spillovers are small.
- *Monetary policy constraints can also increase spillovers.* When monetary policy in either the source or recipient country is unable or unwilling to counteract the fiscal shocks, spillovers can be amplified. For example, compared to average baseline results, spillovers from spending shocks under monetary policy constraints in source (recipient) countries can reach 0.25 percent (0.30 percent) over the first year, while those from tax cuts can reach 0.1 percent (0.15 percent).
- *Currency pegs between source and recipient countries may amplify fiscal spillovers*. The note suggests that fiscal spending shocks from the United States have somewhat larger spillovers on recipient economies whose currencies are pegged to the US dollar compared to those with flexible exchange rates, although this does not seem to be the case for tax revenue shocks.
- Finally, while fiscal actions in the United States have farther-reaching spillovers, those in European countries and Japan have a more regional impact. Fiscal shocks in the United States can entail larger cross-border impact than shocks in other countries—especially onto Canada and Latin America. The global impact of euro area shocks is more modest but particularly relevant for countries in Europe.

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Annex 1. Data for Shock Identification

Quarterly fiscal data used in shock identification for five shock-emitting (source) countries (France, Germany, Japan, United Kingdom, United States) are collected from respective national statistical bureaus, either directly or via Haver Analytics. The following sections describe the data, their limitations, and calculations underlying identified fiscal shocks.

Data Description

Quarterly real government spending and tax revenue data used in constructing fiscal shocks are all expressed in local currency units, seasonally adjusted, and annualized for the sample period, which covers the first quarter of 2000 through the second quarter of 2016. Government spending is calculated as the sum of quarterly general government consumption and general government gross fixed capital formation from the national accounts. For tax revenue, quarterly general government total tax income is used, except in the case of Japan. Annex Table 1.1 provides detail on data sources for each country.

Data Limitations

For Japan, quarterly government total revenue is used instead of tax revenue because of data limitations. Government total revenue data are estimated using both monthly Treasury receipts data from Japan's Ministry of Finance and annual general government revenue data from the IMF's *World Economic Outlook*. Higher-frequency fiscal data cannot be used directly for our analysis owing to definitional differences. Treasury data cover receipts and payments of the private sector only, while official government budget data cover all receipts and payments (Ministry of Finance, *Japan Statistical Yearbook*). To reconcile this difference, we extrapolate quarterly data from the annual government revenue using information from Treasury receipts using the Denton proportional benchmarking method (Di Fonzo and Marini 2014). This method both preserves the seasonality observed from higher-frequency Treasury receipts data and matches the data published in the *World Economic Outlook* when converted to an annual basis.

Annex 2. Data for Spillover Analysis

The quarterly database of 55 recipient countries for the sample period (first quarter of 2000 to second quarter of 2016) includes series on real output, external demand, short-term interest rate, output gap, and exchange rate regime, collected from multiple data sources. The following sections explain how each data series is estimated. Annex Table 2.1 provides details on data sources for each series, and Annex Table 2.2 lists countries in our sample.

Data Description

- *Real GDP.* Quarterly real output levels are rebased to 2010 prices, expressed in local currency units, seasonally adjusted, and annualized.
- *Bilateral goods exports/imports.* Bilateral weights are calculated using the ratio of bilateral exports to imports of goods between 55 countries in the sample and 5 source countries (55 × 5 = 275 pairs). For each country pair, the average between reported values for both countries is taken.

Annex Table 1.1. Sources for Quarterly Fiscal Data for Source Countries

Country	Fiscal Data	Data Source	Seasonal Adjustment	Note
France	Government spending	Eurostat[1]	SWDA by source	Sum of government final consumption and GFCF
	Tax revenue	Eurostat[1]	SWDA by source	Current taxes on income and wealth, excluding social contributions
Germany	Government spending	Bundesbank	SWDA by source	Sum of government final consumption and GFCF
	Tax revenue	Eurostat[1]		
Japan	Government spending	Cabinet Office	SAAR by source	Sum of government final consumption and GFCF
	Government total revenue	Ministry of Finance and Cabinet Office	X-12-ARIMA by IMF staff	Extrapolated using Denton method (Di Fonzo and Marini 2014)
United Kingdom	Government spending	Office for National Statistics	Seasonally adjusted by source	Sum of government final consumption and GFCF
	Tax revenue	Eurostat[1]	X-12-ARIMA by IMF staff	
United States	Government spending	Bureau of Economic Analysis	Seasonally adjusted by source	Sum of government final consumption and GFCF
	Tax revenue	Bureau of Economic Analysis	Seasonally adjusted by source	

Source: IMF staff compilation.

Note: For government spending, nominal levels are deflated using GDP deflator if real levels are not directly available from the source. For tax revenue (total revenue for Japan), real levels are calculated by deflating nominal levels using GDP deflator for each country, respectively. ARIMA = autoregressive integrated moving average; GFCF = gross fixed capital formation; SAAR = seasonally adjusted and annualized data; SWDA = seasonally and working days–adjusted data.

[1]Quarterly nonfinancial accounts for general government database.

Annex Table 2.1. Data Sources for Recipient Countries

Series	Data Sources	Estimation	Countries Missing Data	Note
Real Output	Haver Analytics; IMF, *World Economic Outlook*	Rebased to 2010; deflated using GDP deflator	None in our sample	Seasonally adjusted, annualized, in national currency
Bilateral Goods Exports/Imports	IMF, *Direction of Trade Statistics*	Average between values reported by the reporter and partner countries	None in our sample	Original data in monthly frequency, aggregated by sum
External Demand	Haver Analytics; IMF, *Direction of Trade Statistics*; IMF, *World Economic Outlook*;	Export-weighted sum of partner countries' real GDP growth	None in our sample	Seasonally adjusted, quarter-over quarter growth, in percent
Short-Term Monetary Policy Rate	Bloomberg Finance L.P.; Haver Analytics	Three-Month London interbank offered rate (LIBOR), three-month Treasury bill rate where available	Cyprus, Estonia, Luxembourg, Slovak Republic, Uruguay	Policy rate, deposit rate, target rate used where LIBOR and Treasury bill rate are not available
Output Gap	Haver Analytics; IMF, *World Economic Outlook*	Gap between real output and potential output estimated by Hodrick-Prescott filter	None in our sample	Denton method (Di Fonzo and Marini 2014) used to match annual output gap numbers in *World Economic Outlook*

Source: IMF staff compilation.

Annex Table 2.2. Recipient Countries in Sample

Region	Countries (55 total)
Africa	South Africa
Americas	Argentina, Brazil, Canada, Chile, Colombia, Costa Rica, Mexico, Peru, *United States*, Uruguay
Asia	Australia, China, India, Indonesia, *Japan*, Korea, Malaysia, New Zealand, Philippines, Thailand, Vietnam
Europe	Austria, Belgium, Bulgaria, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, *France*, *Germany*, Greece, Hungary, Ireland, Israel, Italy, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Norway, Poland, Portugal, Romania, Russia, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, Turkey, *United Kingdom*

Source: IMF staff compilation.

Note: Shock-emitting (source) countries are shown in italics.

- *External demand.* This is calculated as a weighted sum of partner countries' real growth based on bilateral export weights.
- *Short-term interest rate.* The three-month London interbank offered rate (LIBOR) and three-month Treasury bill rate are used. For better country coverage and historical coverage, policy, deposit, and target rates are used where three-month LIBOR and Treasury bill data are not available.
- *Output gap.* The quarterly output gap is first calculated as the gap between real output and potential output, estimated by the Hodrick-Prescott (HP) filter. Then, to reconcile any potential difference between our estimated output gap and the annual output gap numbers published in IMF's *World Economic Outlook* (WEO), the Denton proportional benchmarking method is used. This method both preserves the seasonality observed from quarterly estimated output gap series and matches the WEO data when converted to an annual basis.

Before entering the regressions, variables with notable trends over the sample period are detrended using country-specific linear trends. In addition, outliers—that is, observations showing quarter-over-quarter GDP growth rates above 10 percent or below –10 percent in any given quarter—are removed (there are very few of these observations).

Exchange Rate Regime Classification

We construct a measure of bilateral exchange rate arrangement with respect to the US dollar to estimate spillovers for different exchange rate regimes.

For the Reinhart-Rogoff classification, the exchange rate regime is expressed as a time-varying index based on the annual coarse de facto classification from Ilzetzki, Reinhart, and Rogoff (2017a, 2017b), ranging from 1 (most rigid) to 6 (most flexible). For each period, if a country is assigned a value of 1 (de facto peg) or 2 (de facto crawling peg), it is deemed a "fixed regime." The quarterly index is interpolated from annual data, assigning the same value for all four quarters within a year. For example, in 2015, this classification yields 7 "fixed"-rate countries out of the sample of 55 countries (Argentina, China, Costa Rica, India, Peru, Philippines, Vietnam).[31]

For the IMF classification, the pre-2008 (coarse) scheme consists of six categories, with 1 being most rigid and 6 being most flexible. Data for regime classification before 2008 are obtained from Carmen Reinhart's website.[32] The classification changed in 2008, and post-2008 data are obtained from the IMF's website.[33] Similarly to the Reinhart-Rogoff classification, a country is generally classified as having a "fixed" exchange rate with respect to the US dollar if it is assigned a value of 1 (de facto peg) or 2 (de facto crawling peg or crawling band that is narrower than or equal to +/–2 percent). Again, the quarterly index is interpolated from annual data. For example, in 2015, this classification yields two "fixed"-rate countries out of the sample of 55 countries (China, Vietnam), although there are more "fixed"-rate countries in earlier periods.

Annex 3. Blanchard and Perotti Methodology

This annex provides a brief overview of the SVAR shock identification methodology of Blanchard and Perotti (2002) as applied in this note.

VAR Specification

The identification of shocks under this methodology involves estimating the following VAR specification:

$$\mathbf{Y}_t = A(L,q)\mathbf{Y}_{t-1} + \mathbf{U}_t, \qquad \text{(A.3.1)}$$

in which $\mathbf{Y}_t \equiv [T_t, G_t, X_t]'$ is a vector containing the values of quarterly taxes, spending, and GDP (all in logs of real, per capita terms), $A(L,q)$ is a four-quarter distributed lag polynomial, and $\mathbf{U}_t \equiv [t_t, g_t, x_t]'$ is the corresponding vector of reduced-form residuals. We can write

$$t_t = a_1 x_t + a_2 e^g_t + e^t_t, \qquad \text{(A.3.2)}$$

$$g_t = b_1 x_t + b_2 e^t_t + e^g_t, \qquad \text{(A.3.3)}$$

$$x_t = c_1 t_t + c_2 g_t + e^x_t, \qquad \text{(A.3.4)}$$

in which e^t_t, e^g_t, e^x_t are the mutually uncorrelated structural shocks that we want to recover. For example, equation (A.3.2) says that unexpected movements in taxes can be due to a response to unexpected movements in GDP and a response to structural shocks to spending or taxes.

[31]The numbers of countries classified as having "fixed"-rate regimes can generally vary over time, since the exchange rate regime classification is time varying.

[32]http://www.carmenreinhart.com/data/browse-by-topic/topics/12/

[33]http://www.elibrary-areaer.imf.org/Pages/Home.aspx

Identification

The identification follows three steps:

- The effects of activity on taxes and government spending—captured by the coefficients a_1 and b_1—consist of two channels: (1) the *automatic* responses of these fiscal variables to activity under existing fiscal policy rules and (2) *discretionary* policy changes in response to unexpected shocks to activity. The key identifying assumption is that the second channel does not operate with the use of quarterly data because of decision lags (that is, it takes time for policymakers to realize a shock to GDP and make spending/tax decisions in response). In addition, there is no evidence of any automatic response of spending to activity, and thus $b_1 = 0$. For taxes, the automatic response of tax revenues to activity can be calibrated using the empirically estimated elasticity of tax revenues with respect to output (or "tax elasticity"; see discussion later in the annex), pinning down the a_1 coefficient.
- With a_1 and b_1 pinned down, the cyclically adjusted reduced-form tax and spending residuals, $t'_t \equiv t_t - a_1 x_t$ and $g'_t \equiv g_t - b_1 x_t = g_t$, can be constructed and can then be used as instruments to estimate c_1 and c_2 in a regression of x_t on t_t and g_t since they are not correlated with e^x_t.
- The remaining parameters, a_2 and b_2, can be estimated under two alternative assumptions: (1) assuming $a_2 = 0$ (taxes do not respond to spending) and estimating b_2, or (2) assuming $b_2 = 0$ (spending does not respond to taxes) and estimating a_2. Both assumptions give similar results.

While the identified structural shocks are not very sensitive to the value of tax elasticity used, the domestic tax multiplier is. Blanchard and Perotti (2002) use data on institutional characteristics of the US tax system to estimate the elasticity at quarterly frequency, obtaining the number 2.08. Their estimate of the domestic tax multiplier after eight quarters is 0.72 or 1.32 depending on the VAR specification. Caldara and Kamps (2012) show that the size of the fiscal multiplier increases in the size of the elasticity, suggesting that careful calibration of this value is important to correctly estimate the size of the multiplier. Mertens and Ravn (2014) propose a new methodology—proxy SVAR, which integrates shocks identified from a narrative approach, such as, for example, those of Romer and Romer (2010), into the standard SVAR framework—that allows estimating the size of the elasticity rather than directly assuming it, and find that the underlying value of the elasticity is 3.13 rather than 2.08 for the United States. This higher elasticity value reconciles the size of the domestic multiplier typically obtained from SVARs with the estimates obtained using narrative shocks, the latter of which are typically higher.

To estimate the tax elasticities in the five source countries, we follow Mertens and Ravn (2014) and use information on other measures of tax shocks:

- *United States*. We use the value of 3.13, which comes from Mertens and Ravn's (2014) analysis based on Romer and Romer's (2010) shocks and quarterly data.
- *United Kingdom*. Cloyne (2013) estimates this elasticity for the United Kingdom using a new quarterly data set of narrative tax shocks and arrives at the value of 1.61, which we use in our analysis.
- *Germany, France, Japan*. Elasticity estimates for these countries are not readily available from the literature; therefore, we estimate the elasticity values ourselves. Data on narrative shocks, which could be used in a proxy SVAR, for these countries are scarce. The only available narrative data set, that of DeVries and others (2011), has annual frequency and includes only fiscal consolidations, thus not fully capturing all possible tax shocks. Instead, we use forecast error shocks[34] to complement the SVAR and recover the elasticity estimates. These shocks capture unanticipated tax changes based on OECD forecasts.[35] The sample for each country is based on availability of forecast error shocks. The resulting values of elasticities vary depending on the exact VAR specification (trend, dummies), and we choose a specific value within the obtained range: 0.7 for Germany, 1.8 for France, and 1.3 for Japan.

[34]See Annex 5 for details on how the forecast error shocks are constructed.

[35]One potential drawback of using these shocks is that they are available only at annual frequency, meaning that the elasticity should be recovered from a VAR specified on annual data and might not be a good measure for quarterly elasticity. Another potential problem is that forecast error shocks can capture only unanticipated changes in fiscal variables, while anticipated changes can play an important role as well. However, there is no quarterly measure of shocks available for these three countries, nor is there a measure of anticipated shocks, that we could use in the estimation.

Annex 4. Domestic Fiscal Multipliers

This annex discusses the results in regard to domestic multipliers and how they relate to the literature. We find that spending multipliers tend to be larger than tax multipliers in all source countries, except the United States. These results are broadly in line with the findings in the vast empirical literature on the size of domestic fiscal multipliers.

Since changes in a source country's demand for recipient country exports is an important channel through which spillovers are propagated, a brief discussion of domestic (that is, source country) fiscal multipliers is warranted.

The "Baseline Results" section in the note text shows that government spending shocks have larger spillovers onto recipient country output than tax shocks. If trade is the main channel for international transmission of fiscal shocks, one would expect that domestic fiscal multipliers are also larger for government spending shocks. Indeed, we find this is almost universally the case. Annex Table 4.1 shows our estimated domestic fiscal multipliers for tax and expenditure shocks for our set of five source countries. Government spending multipliers tend to be slightly above 1 and are relatively tightly grouped between values of 1.12 (France) and 1.49 (United States). By comparison, tax multipliers are generally well below 1, with the notable exception of the United States, which is discussed later in this annex.

The finding that government spending shocks have larger spillovers is consistent with traditional Keynesian theory. Consider two changes to fiscal policy: an increase in government spending and a cut in taxes, each with a budgetary cost of a dollar. The increase in government spending immediately contributes a dollar to aggregate demand, but the tax cut could contribute less than a dollar because it can be either spent or saved, since the marginal propensity to consume is typically less than 1. There is also considerable empirical evidence that suggests multipliers are larger for spending than for tax shocks: based on a survey of 41 studies, Mineshima, Poplawski-Ribeiro, and Weber (2014) show that first-year multipliers amount on average to 0.75 for government spending and 0.25 for government revenue in advanced economies.

The heterogeneity in domestic tax multipliers across the United States and Europe presents an apparent puzzle. One possible explanation for this result may rely on the differences between the tax systems in the United States and in Europe. The US tax system relies more on personal and corporate income taxes and less on consumption taxes relative to the European system. The literature using dynamic stochastic general equilibrium models (for example, Coenen, Straub, and Trabandt 2012; Kilponen and others 2015) tends to find that multipliers for personal and corporate income taxes are higher than those for consumption taxes, reflecting their more distortionary effects on labor supply and investment decisions. These findings suggest a higher tax multiplier in the United States, given its tax system structure.

The empirical literature on this topic also tends to find larger tax multipliers for the United States than for countries in Europe. For example, Romer and Romer (2010) find that the output response to a narrative-based tax shock peaks at –2.93 after 10 quarters in the United States. Similarly, Mertens and Ravn (2014) find a large multiplier (–2.5 after three quarters) from narrative-based personal income tax changes for the United States. By contrast, most estimates of tax multipliers for European countries lie below 1 (Kilponen and others 2015).

Annex Table 4.1. Domestive Fiscal Multipliers

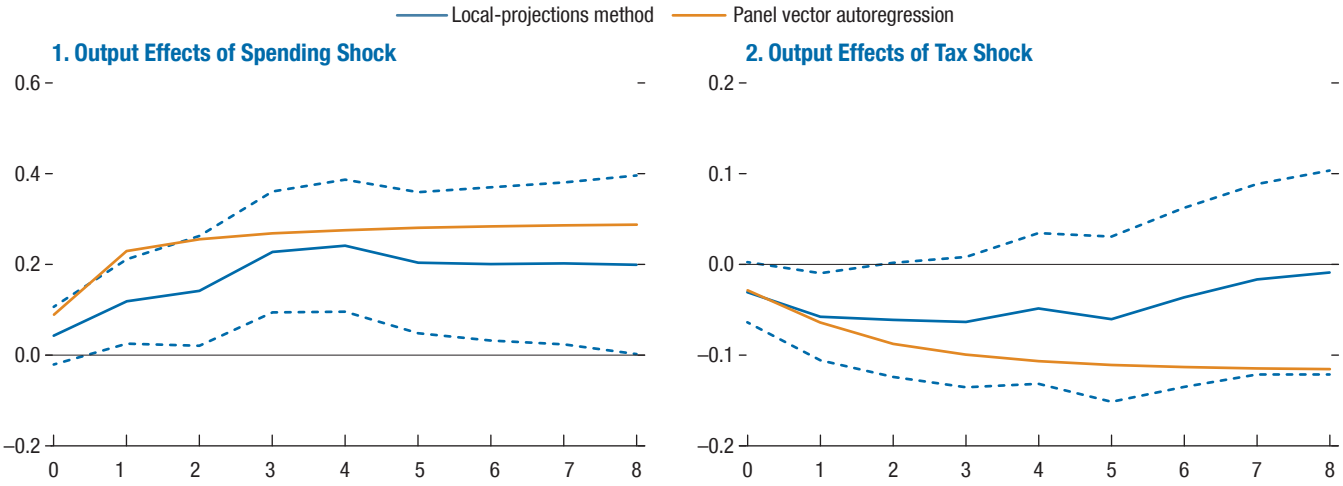
Country	Estimated Multiplier[1]		Sample
	Spending Shock	Tax Shock	
France	1.12*	–0.33*	2000:Q1–2016:Q2
Germany	1.47*	–0.73*	2000:Q1–2016:Q3
Japan	1.18*	–0.56*	1995:Q1–2016:Q3
United Kingdom	1.14	–0.24*	2000:Q1–2016:Q3
United States	1.49*	–2.24*	1980:Q1–2016:Q3

Source: IMF staff estimates.
Note: Table shows effect of one-dollar increase in spending/tax on real GDP level.
[1]Peak impact or largest significant impact, Blanchard-Perotti methodology.
* $p < 0.1$.

Annex 5. Robustness Tests

To ensure that our baseline results are not solely a function of our shock identification scheme, estimation approach, or various assumptions made during the analysis, in this annex we conduct numerous robustness checks. We find that our findings are robust to (1) estimation of spillovers in a panel VAR environment, which accounts for the endogenous response of exchange rates and monetary policy in recipient countries, (2) the use of alternative fiscal shocks based on both forecast error and narrative approaches, and (3) controlling for additional recipient country variables. These are explored in turn.

Annex Figure 5.1. Effects of Spending and Tax Shocks on Recipient Countries' Output: Comparison with Panel Vector Autoregression
(Percent)



Source: IMF staff calculations.
Note: Numbers on horizontal axes represent quarters; $t = 0$ is the quarter of the respective shocks. Solid blue lines denote the baseline response to respective shocks using local-projections method; dashed lines denote 90 percent confidence bands; and solid orange lines represent the response to respective shocks using panel vector autoregressions. Shocks are normalized to an average 1 percent of GDP across the source countries.

Spillover Estimates Using a Panel VAR

We conduct our spillover analysis in the context of a panel VAR (PVAR) to ensure that our results are not driven by the use of the local-projections method. The main goal here is to explicitly take into account the endogenous response of key macro variables when estimating spillovers to a fiscal shock. Consistent with this goal, we specify a six-variable PVAR, according to the following equation:

$$\mathbf{Y}_{i,t} = \mathbf{c}_i + \sum_{p=0}^{1} \mathbf{A}_p \mathbf{Y}_{i,t-p} + \mu_{i,t}, \qquad \text{(A.5.1)}$$

in which $\mathbf{c}_i$ is a vector of country-specific fixed effects, $\mathbf{A}_p$ is a reduced-form coefficient matrix, $\mu_{i,t}$ is a vector of shock terms, and $\mathbf{Y}_{i,t}$ is a vector of six endogenous variables:

$$\mathbf{Y} = \{\frac{Shock^{G}_{it}}{Y_{i,t-1}}; \frac{Shock^{T}_{it}}{Y_{i,t-1}}; \textit{effective external demand}; \textit{GDP growth}; \textit{interest rate}; \textit{REER}\},$$

in which *REER* is the real effective exchange rate.

With the exceptions of $\frac{Shock^{G}_{it}}{Y_{i,t-1}}$ and $\frac{Shock^{T}_{it}}{Y_{i,t-1}}$, which are identical to the government spending and tax shocks used in the baseline analysis (see "Fiscal Spillovers: Baseline Analysis"), each variable is in (detrended) quarter-over-quarter growth rates and relates to recipient country *i*'s domestic economy.[36] The analysis is conducted for the same sample period as the baseline local-projections analysis.

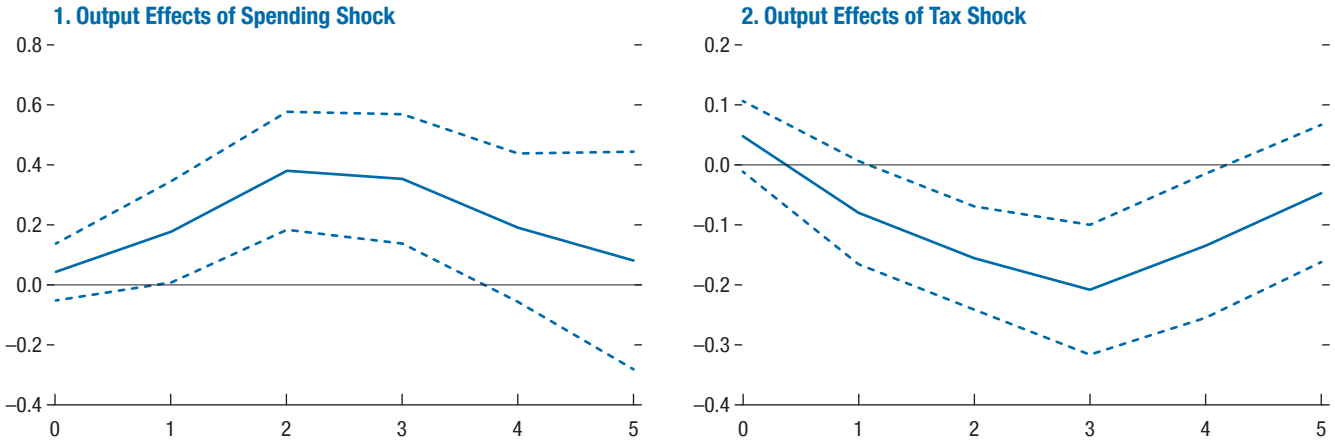
The results from the PVAR analysis are closely aligned with the findings from the baseline local-projections model. Shown in Annex Figure 5.1, the spillover effects from a shock to government spending in source countries are larger than those for identically sized shocks to tax revenues. These results, shown by the orange lines in the figure—expressed in terms of the cumulative impulse-response functions from the PVAR following a 1 percent of source country GDP shock to government spending or tax revenues—are different from zero at the 5 percent level of statistical significance, according to simulations conducted using standard (Monte Carlo) resampling methods.

Robustness to Identification Using Forecast Errors

The second robustness check focuses on the identification of fiscal shocks as forecast errors in the growth rates of government spending or tax revenues.

[36]Results from the PVAR are robust to several alternative specifications, including not detrending the data.

Annex Figure 5.2. Effects of Spending and Tax Shocks on Recipient Countries' Output: Forecast Errors
(Percent)



Source: IMF staff calculations.
Note: Numbers on horizontal axes represent years; $t = 0$ is the year of the respective shocks. Solid lines denote the response to respective shocks, and dashed lines denote 90 percent confidence bands. Effects are estimated based on shocks derived from forecast errors. Shocks are normalized to an average 1 percent of GDP across the source countries.

The method has been previously used in the literature (Auerbach and Gorodnichenko 2013; Ramey 2011) and identifies fiscal shocks by exploiting the difference between actual government purchases (tax revenues) and their forecast from the previous period. This approach captures only unanticipated changes in spending and revenues, as opposed to SVAR shocks, which are based on actual changes in fiscal variables and can be anticipated by agents if they have been announced earlier. The presence of such anticipated shocks in theory could bias the estimates, because the econometrician's information set is different from the agents' information set. Since forecast errors capture unexpected changes, this approach reduces the problem with fiscal foresight, as the econometrician's and agents' information sets are more aligned.

We rely on real-time OECD fiscal projections to construct the forecast error shocks. The data are at annual frequency, and the sample covers the period from 2000 to 2012 (after 2012, the forecasts data are not continuous). The forecast errors are constructed based on real-time information about expectations and actual data. The forecast error for each variable $X = \{G, T, Y\}$ is constructed as

$$FE_t^X = X_t - X_{t|t-1}^f, \qquad \text{(A.5.2)}$$

in which X_t is the growth rate of the variable from contemporaneous data release and $X_{t|t-1}^f$ is the forecast made one period earlier. A positive forecast error therefore implies an expansionary spending and a contractionary tax shock. Following Auerbach and Gorodnichenko (2013), we regress the forecast errors of spending and taxes on the forecast errors of output to take into account any changes due to surprises in the business cycle and also on lagged macroeconomic variables growth (GDP, deflator, investment, government spending or tax revenues) to account for the part of the innovation that can be predicted from past observations. The forecast error shocks are then constructed as residuals from this regression, converted to levels using base year (2010) levels of expenditures or revenues, and substituted in the baseline regression equation instead of the SVAR shocks.

Spillover analysis using forecast error shocks confirms the baseline results—that spending shocks have larger spillovers than tax shocks—and provides a strong robustness check (Annex Figure 5.2). These shocks are constructed using a very different methodology, which relies on a different database, and they are estimated at a different frequency than the shocks used in our baseline specification. Obtaining similar spillovers using forecast error shocks is reassuring and suggests that problems related to fiscal foresight seem not to affect our main results. The size of the spillovers is somewhat larger than that of those obtained using structural shocks. In part this can be explained by a

larger response of government spending and tax revenue to forecast error shocks than to structural shocks, especially for the United States (although in the former case, these impulse responses are imprecisely estimated because of the small sample).

Robustness to Identification with Narrative Approach

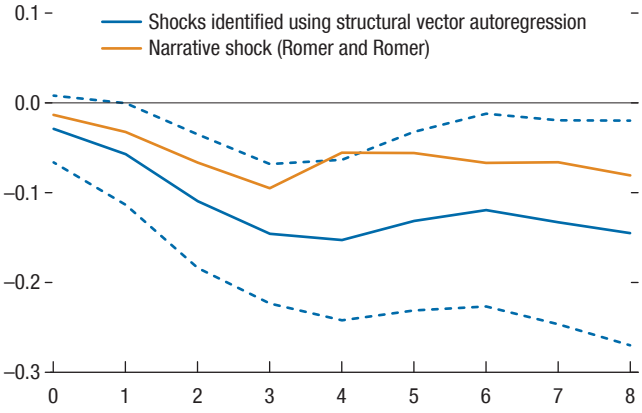
To further establish the robustness of our results, we consider spillovers from tax shocks given by the narrative shock of Romer and Romer (2010). Although some other studies construct narrative fiscal shocks (for example, DeVries and others 2011), the data set of Romer and Romer (2010) is the most suitable for our purposes, since it covers both expansion and consolidation episodes, making it most comparable to our baseline shock specification.[37] To obtain spillover results using this type of shock, we simply replace each source country shock (*sjt*) from equation (2) with the narrative shock; this analysis is performed over the period from the first quarter of 1995 to the fourth quarter of 2007.[38] A more comparable set of baseline results using our SVAR shocks is then constructed by restricting our baseline analysis to the same time period.

Analysis using narrative tax shocks for the United States shows similar spillovers onto partner countries. Despite their being derived from a very different identification scheme, the broad similarity between the estimated US tax shock spillovers from the narrative approach and those from our (time-sample-modified) baseline approach is notable. Results presented in Annex Figure 5.3 indicate that although spillovers identified by the narrative approach are somewhat smaller than those in our baseline, they are similar and fall comfortably within the confidence bands of our baseline estimates.

Robustness to Additional Control Variables

Baseline results are also robust to the inclusion of additional control variables. First, we use the short-term interest rate to control for the stance of recipient country monetary policy and the output gap and unemployment rate as measures of slack in recipients. Dynamic responses are presented in Annex

Annex Figure 5.3. Effects of US Tax Shock on Recipient Countries' Output: Comparison with US Narrative Tax Shock, 1995–2007
(Percent)



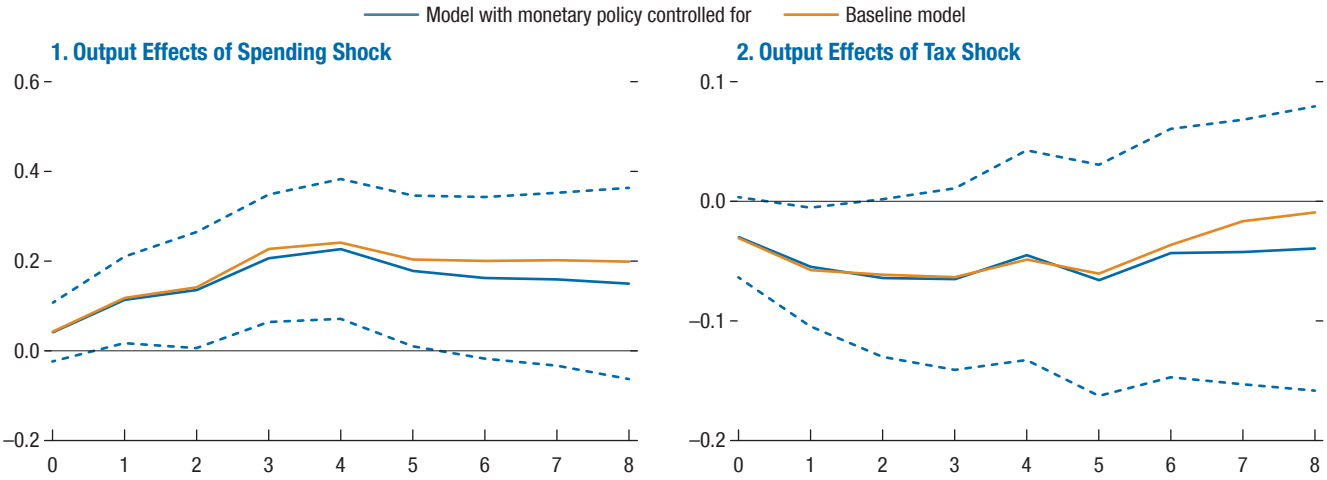
Sources: Romer and Romer (2010); and IMF staff calculations.
Note: Numbers on horizontal axes represent quarters; t = 0 is the quarter of the US tax shock. Solid blue line denotes the response to US tax shock using structural vector autoregression; dashed blue lines denote 90 percent confidence bands; and solid orange line represents the response to US narrative tax shock based on Romer and Romer (2010). Shocks are normalized to an average 1 percent of GDP across the source countries (note that this will represent a less than 1 percent of US GDP shock).

Figures 5.4–5.6 and confirm that additional control variables do not materially change the baseline results. Controlling for domestic fiscal policies in the baseline specification is another important robustness check, however, estimating fiscal shocks for 55 recipient economies at quarterly frequency is infeasible, because quarterly fiscal data are unavailable for many countries. Since Eurostat provides fiscal data at quarterly frequency for European countries, we conduct a robustness check for this subsample in which we control for changes in primary balances (as a percent of GDP) to proxy for the stance of recipient country fiscal policy. Since this robustness check is conducted on a limited sample (European Union), we select Germany and France as source countries for this exercise, since shocks from these countries are most relevant for Europe. We find that the results of this robustness check are almost identical to those in the regression that omits the stance of recipient country fiscal policy (Annex Figure 5.7).

[37]Narrative shock databases for government spending are much less common in the literature, which precludes a robustness check of spillovers from spending shocks based on narrative shocks.

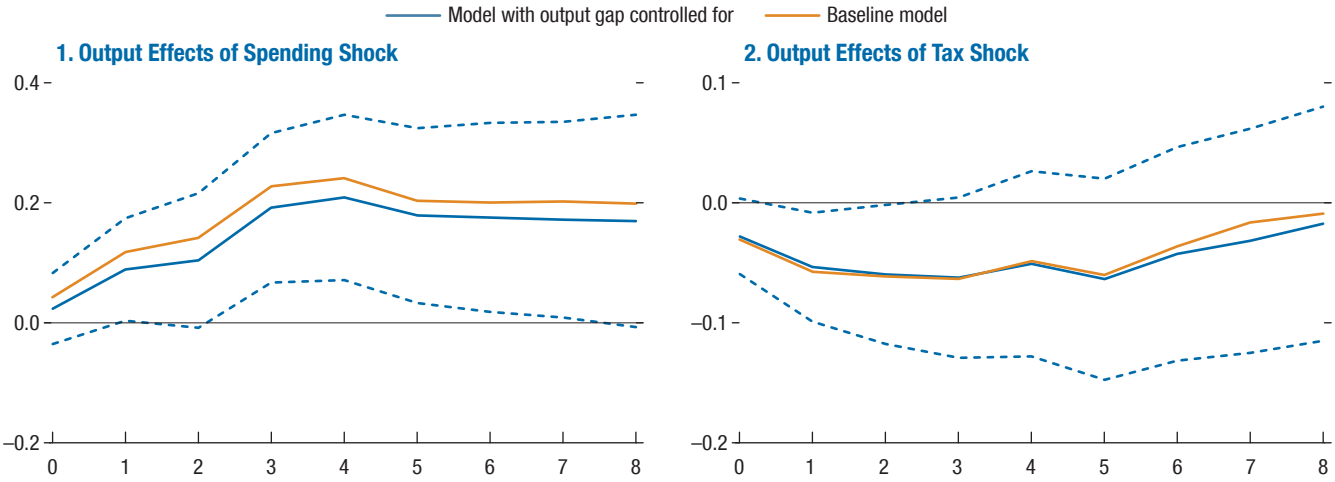
[38]The fourth quarter of 2007 is the last period for which data on these shocks are available.

Annex Figure 5.4. Dynamic Responses of Recipient Output to Fiscal Shocks, with Monetary Policy Controlled For
(Percent)



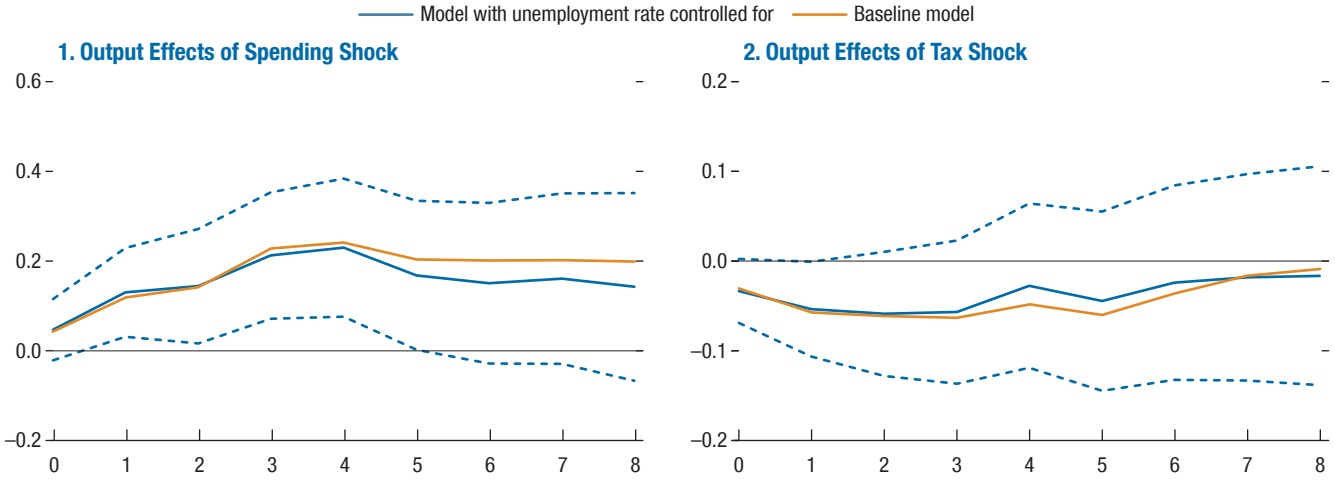
Source: IMF staff estimates.
Note: Numbers on horizontal axes represent quarters; $t = 0$ is the quarter of respective shocks. Solid blue lines denote the response to respective shocks, controlling for monetary policy; dashed blue lines denote 90 percent confidence bands; and solid orange lines represent the baseline response to respective shocks. Shocks are normalized to an average 1 percent of GDP across the source countries.

Annex Figure 5.5. Dynamic Responses of Recipient Output to Fiscal Shocks, with Output Gap Controlled For
(Percent)



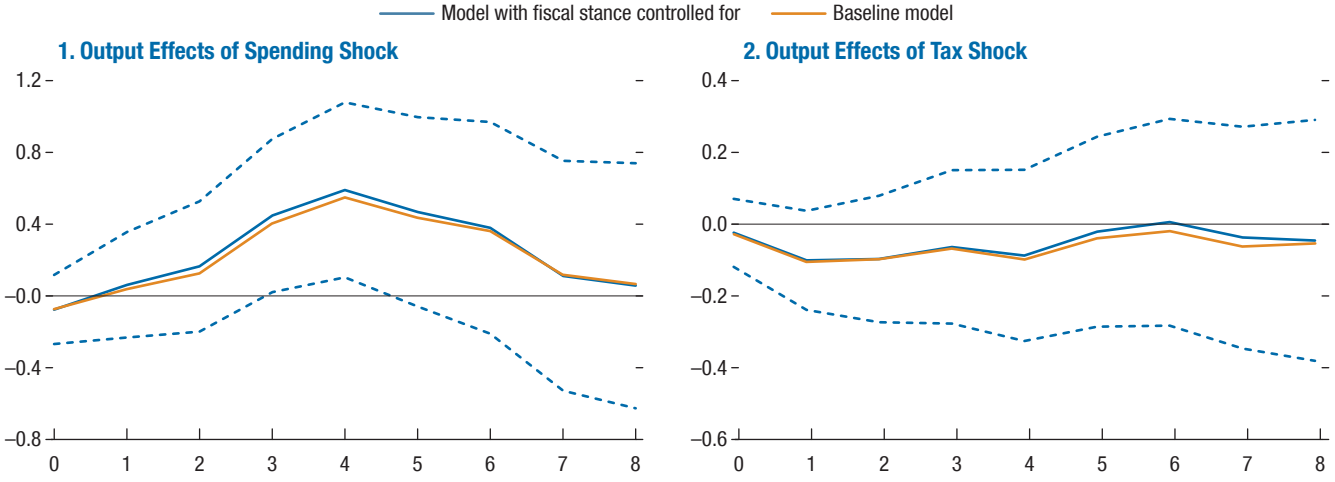
Source: IMF staff estimates.
Note: Numbers on horizontal axes represent quarters; $t = 0$ is the quarter of respective shocks. Solid blue lines denote the response to respective shocks, controlling for output gap; dashed blue lines denote 90 percent confidence bands; and solid orange lines represent the baseline response to respective shocks. Shocks are normalized to an average 1 percent of GDP across the source countries.

Annex Figure 5.6. Dynamic Responses of Recipient Output to Fiscal Shocks, with Unemployment Rate Controlled For
(Percent)



Source: IMF staff estimates.
Note: Numbers on horizontal axes represent quarters; $t = 0$ is the quarter of respective shocks. Solid blue lines denote the response to respective shocks, controlling for unemployment rate; dashed blue lines denote 90 percent confidence bands; and solid orange lines represent the baseline response to respective shocks. Shocks are normalized to an average 1 percent of GDP across the source countries.

Annex Figure 5.7. Dynamic Responses of Recipient Countries' Output to France and Germany Fiscal Shocks, with Recipient Countries' Fiscal Stance Controlled For
(Percent)



Source: IMF staff estimates.
Note: Numbers on horizontal axes represent quarters; $t = 0$ is the quarter of respective shocks. Solid blue lines denote the response to respective shocks, controlling for recipients' primary balance estimated on a (time-varying) European Union sample; dashed blue lines denote 90 percent confidence bands; and solid orange lines represent the response to respective shocks from the baseline model estimated on a (time-varying) European Union sample. Shocks are normalized to an average 1 percent of GDP across the source countries.

Annex Table 6.1. Comparison to Empirical Literature: Response of Recipient Country GDP
(Percent)

	Impact	Average One-Year	Average Three-Year
Government Spending Shock			
Local Projections with SVAR–Blanchard-Perotti Shocks (baseline)	0.30	1.05**	1.35**
PVAR with SVAR–Blanchard-Perotti Shocks	0.62**	1.55**	1.82**
Local Projections with Forecast Error Shocks[1]	—	0.78	1.65***
Auerbach and Gorodnichenko (2013)[1]	—	—	1.94*
Goujard (2017)[1]	—	—	1.98
Tax Revenue Shock			
Local Projections with SVAR–Blanchard-Perotti Shocks (baseline)	–0.21	–0.37*	0.04
PVAR with SVAR–Blanchard-Perotti Shock	–0.20**	–0.54**	–0.70**
Local Projections with Forecast Error Shocks[1]	—	–0.10	–0.68**
Goujard (2017)[1]	—	—	0.63

Source: IMF staff calculations.
Note: Table shows response to a shock normalized to 1 percent of recipient country GDP.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Annex 6. Comparison of Spillover Estimates with Previous Literature

While we choose to present results in terms of source country GDP shocks for ease of interpretation, several previous studies on fiscal spillovers normalize shocks to recipient country GDP. Key among these studies are Auerbach and Gorodnichenko 2013 and Goujard 2017. Auerbach and Gorodnichenko (2013) estimate spillovers from shocks to government spending for a set of 30 OECD countries, using forecast error shocks constructed from the OECD's Economic Outlook: Statistics and Projections database. Goujard (2017) considers spillovers on 34 recipient countries from both spending and tax shocks in 17 OECD countries using annual data for 1978–2011; shocks are taken from the narrative database of DeVries and others (2011) and pertain only to consolidation episodes.

Our estimates of fiscal spillovers are broadly similar to those obtained in these studies. Annex Table 6.1 compares the results from our baseline and alternative specifications—that is, panel VAR using structural shocks and local projections using forecast error shocks—to estimates reported in Auerbach and Gorodnichenko 2013 and Goujard 2017, focusing on separate estimates for spending and tax shocks. The comparison shows that

- For government spending shocks, our average spillovers over the first three years are comparable to those in both studies and are statistically significant. The average first-year effects, which are not reported in comparable studies, are also statistically significant.
- For tax revenue shocks, our estimates of spillovers are statistically significant over the first year, with more mixed results over the longer horizon. Meanwhile, Goujard (2017) finds no statistically significant effect from tax shocks.

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