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Fiscal Multipliers in Advanced and Developing Countries: Evidence from Military Spending

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Abstract:
Using novel data on military spending for 129 countries in the period 1988–2013, this paper provides new evidence on the effects of government spending on output in advanced and developing countries. Identifying government-spending shocks with an exogenous variation in military spending, we estimate one-year fiscal multipliers in the range 0.75–0.85. The cumulative multipliers remain significantly different from zero within three years after the shock. We find substantial heterogeneity in the multipliers across groups of countries. We then explore three potential sources leading to heterogeneous effects of fiscal policy: the state of the economy, openness to trade, and the exchange-rate regime. We find that the multipliers are especially large in recessions, in closed economies, and under a fixed exchange rate. We also discuss other potential reasons for heterogeneous effects of fiscal policy, such as its implementation and coordination with the monetary authority.

JEL Classifications: **E32, E62, F44, H56, O23**
Keywords: fiscal policy, military spending, multiplier

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1 Introduction

What are the effects of fiscal policy in different economic environments? While there exists a lot of evidence—sometimes conflicting—on this question from the United States and other developed countries,[1] such evidence from developing countries is rather scarce and based on a limited number of identification strategies. One reason, emphasized in recent literature, why the effectiveness of fiscal policy may depend on economic conditions is the amount of slack (i.e., idle resources) in the economy, or the state of the business cycle (e.g., Parker 2011; Auerbach and Gorodnichenko 2012; Michaillat 2014). Another potential reason is that some countries allow their currency's value to float freely while others choose to peg. Developing countries, in particular, often choose a fixed exchange rate to anchor inflation expectations and to reduce uncertainty in the trade sector. Moreover, a recent study by Miyamoto, Nguyen, and Sheremirov (2019) shows that the transmission mechanisms of government-spending shocks differ substantially between advanced and developing countries due to heterogeneous responses of exchange rates. Countries may also differ in the degree of openness to foreign trade and investment, independence of their central bank, and reliance on commodities, as well as in the quality and type of governance and the amount of fiscal space available to finance public spending. What do all these differences mean for the size of the fiscal multiplier?

In this paper, we provide new evidence on the output responses to government spending in advanced and developing countries. Using multiple sources, we compile data on output, government spending, military spending, unemployment rates, trade shares, and many other variables for 129 countries during the period 1988–2013. These data enable us to differentiate between the multipliers not only in advanced and developing countries but also in different states of the economy and the business cycle, under different exchange-rate regimes, and for different degrees of openness to trade. To estimate fiscal multipliers, we use a local projections method with instrumental variables (IV) wherein military-spending shocks serve as an instrument for government spending. Our main contribution is twofold: First, we provide new evidence on the effects of government spending on output from developing countries, including the least-developed countries that have not been studied enough in the previous literature. Second, we adopt the military-spending approach, which differs from the identification strategies used in the literature studying large panels of countries.

We document several empirical results. First, when advanced and developing countries are combined, the one-year multiplier is estimated in the range 0.75–0.85. For three years after the shock, the cumulative multipliers are significantly different from zero but not from one.[2] Next, when the two

[1] Studies of the effects of government spending in the United States include, among many others, Blanchard and Perotti (2002), Hall (2009), Barro and Redlick (2011), Ramey (2011b), Auerbach and Gorodnichenko (2012), and, more recently, Ramey and Zubairy (2018). See Ramey (2011a) for a detailed literature overview. The effects of taxes on U.S. output are studied, for example, in House and Shapiro (2006), Romer and Romer (2010), and Mertens and Ravn (2012). Other studies focus on the responses of employment (e.g., Chodorow-Reich et al. 2012) or consumption (e.g., Johnson, Parker, and Souleles 2006; Parker et al. 2013; Broda and Parker 2014). Still numerous other studies focus on OECD countries: Alesina and Ardagna (1998), Perotti (2008), Leigh et al. (2010), Beetsma and Giuliodori (2011), and Auerbach and Gorodnichenko (2013), among others.

[2] These are two common benchmarks, considered in the literature, to discriminate across theoretical models. For example, in neoclassical models (e.g., Barro and King 1984; Aiyagari, Christiano, and Eichenbaum 1992; Baxter and King 1993) with a

country groups are considered separately, the baseline multiplier in advanced economies is more than twice as large as it is in developing countries. Due to pronounced heterogeneities within each group, however, we cannot reject statistically the equality of the two multipliers (the *p*-value of the test is just above 10 percent); the difference is, nonetheless, economically relevant. Our further analysis indicates that the difference between advanced and developing countries is likely due to small multipliers in least-developed countries, as high-income and upper-middle-income countries are associated with multipliers of similar magnitudes.

To understand the factors behind heterogeneity in the effects of government spending, we start by examining the state of the economy and the business cycle. Our next major finding is that government spending is more effective in recessions than in expansions. The one-year response of output to a unit shock in government spending is 1.7 in recessions and 0.3 in expansions, with statistically significant differences at horizons of as long as four years. We also find larger multipliers during episodes of slack, when the unemployment rate is relatively high, than when unemployment is low. Thus, our results rationalize countercyclical fiscal policy for a large number of countries, in spite of the existing empirical evidence that fiscal policy in developing countries is procyclical (e.g., Gavin and Perotti 1997; Talvi and Végh 2005; Ilzetzki and Végh 2008).

Finally, we consider open-economy aspects of fiscal policy and document two more results: First, countries that peg the value of their currency are associated with larger multipliers than those for countries that let the exchange rate float. Second, countries with small output shares of international trade, or closed economies, have a larger multiplier than do open economies. While the confidence intervals are too wide for the difference to be statistically significant in the case of exchange-rate regimes, the difference is significant on impact in the openness case. We conclude, overall, that the effectiveness of fiscal policy likely depends on the economic environment and other policies. Therefore, multiplier estimates obtained from a large panel of countries, a long period, or both are likely less informative about potential success or failure of a particular stimulus program than multipliers obtained from homogenous subsamples.

While data limitations prevent us from fully examining many other potential channels of heterogeneity, we nevertheless provide some discussion of three important ones: the degree of monetary accommodation of fiscal policy, the type of financing, and the spending composition. For a subsample of mainly developed countries with available data, we find limited evidence that policy rates or tax rates responded significantly to spending shocks. Hence, our multipliers for advanced economies may be large, in part, because monetary policy was accommodative in the sample, and because spending was financed by debt. With monetary policy leaning strongly against the wind, or spending programs financed by immediate tax, the effects of government spending on output could be smaller. We also present some early evidence that government spending on durables may have a larger effect on output

large elasticity of intertemporal substitution and steep marginal cost, the wealth effect offsets most of the stimulus, producing multipliers close to zero. In New Keynesian models, on the contrary, sticky prices and wages allow for multipliers close to one, as long as monetary policy is accommodative (Woodford 2011). To obtain multipliers bigger than one, however, these models require additional frictions, such as rule-of-thumb consumption decisions (e.g., Galí, López-Salido, and Vallés 2007) or the zero lower bound (e.g., Eggertsson and Woodford 2003; Christiano, Eichenbaum, and Rebelo 2011; Eggertsson 2011).

than does spending on nondurables and services.

Our identification strategy is based on exogenous variation in military spending (e.g., due to geopolitical and other factors not related directly to economic activity). This approach has been widely used for the United States (e.g., Barro 1981; Hall 1986, 2009; Rotemberg and Woodford 1992; Ramey and Shapiro 1998) but not for many other countries. Because the identifying assumption may be questioned when applied to developing countries, we conduct a battery of tests wherein we exclude the cases of potential violation of the exclusion restrictions. To this end, we examine episodes of financial crises and wars, the effects of commodity prices and exports, and countries with large military imports. Our instrument also passes relevance tests, including conservative tests that allow for nonspherical disturbances. In addition, we examine cases of potential instrument weakness due to the small size of a country's military sector. While the instrument strength and the multiplier size vary across subsamples and specifications, few of our tests suggest a significant departure from the baseline estimates.

Our paper is closely related to the literature studying the effects of fiscal policy in a large panel of countries. Notable examples include Ilzetzki, Mendoza, and Végh (2013), Kraay (2012, 2014), and Miyamoto, Nguyen, and Sheremirov (2019). Relative to the first study on this list, we analyze a much larger panel of countries, which has particularly good coverage of lower-middle-income and low-income countries. We also use a different identification strategy, thereby testing that paper's conclusions with new data and a different method. While our panel of developing countries is comparable to Kraay's, our data allow us to compare developing countries with advanced economies using the same strategy—which differs from the one therein. Finally, our data and method are similar to those in Miyamoto et al.; however, this paper focuses on the effects of government spending on output and the resulting cumulative multipliers, while that paper studies the transmission mechanism of fiscal policy working through its effects on real exchange rates, current accounts, and consumption. Our paper is also related to the literature that employs cross-sectional variation in government spending (e.g., Clemens and Miran 2012; Nakamura and Steinsson 2014; Shoag 2015; Suárez Serrato and Wingender 2016; Dupor and Guerrero 2017; Chodorow-Reich 2018). However, we focus on variation across countries, rather than regions, an approach closer to the conventional notion of the fiscal multiplier.

The paper proceeds as follows. Section 2 describes the data and their sources. Section 3 presents the methodology and examines military spending as an instrument for total government spending in international data. Section 4 provides estimates of cumulative multipliers, including those for advanced and developing countries, in recessions and expansions, in closed and open economies, and under different exchange-rate regimes. We conduct numerous robustness checks and test our identifying assumptions in Section 5. In Section 6, we discuss additional factors that can potentially explain heterogeneous effects of fiscal policy. Section 7 concludes.

2 Data

To conduct our analysis, we compile data on real GDP, government spending, military spending and other relevant variables for 129 countries (36 advanced and 93 developing) during the period 1988–2013.

Table 1: Level and Variability of Government Spending

	$\frac{g}{y}$, %	$\frac{g^m}{g}$, %	$\frac{\mathbb{V}\text{ar}(\log g)}{\mathbb{V}\text{ar}(\log y)}$	$\frac{\mathbb{V}\text{ar}(\log g^m)}{\mathbb{V}\text{ar}(\log g)}$
	(1)	(2)	(3)	(4)
Full sample	16.5	16.1	1.2	2.5
	(6.5)	(14.3)	(1.4)	(4.8)
Advanced economies	19.0	11.9	0.9	1.6
	(4.6)	(9.4)	(0.9)	(2.9)
Developing countries	15.4	17.9	1.3	2.9
	(6.9)	(15.5)	(1.5)	(5.3)

Notes: Standard deviations across countries are in parentheses. The variance ratios are computed within country first and then aggregated across countries.

To obtain this dataset, we excluded countries with fewer than 15 years of observations. This approach balances the time-series and cross-sectional dimensions by focusing on countries with a relatively long history. It also provides a systematic way to exclude several war-torn countries, such as Afghanistan and Iraq, for which many data points are missing. The full list of countries in the sample can be found in Appendix Table A.1.

We obtain data from multiple sources. Spending on current military forces and activities comes from the *Military Expenditure Database*, provided by Stockholm International Peace Research Institute (SIPRI). For a subset of countries, total military spending is decomposed into spending on durables and nondurables/services. The data on real GDP and government spending are taken from the *National Accounts Main Aggregates Database*, compiled by the U.N. Statistics Division. Government spending is measured with general-government final consumption expenditure; government investment data are not readily available for a large panel of countries. Each series is in constant (real) local-currency units. The unemployment and trade data come from the World Bank's *World Development Indicators*. The development classifications are from the IMF and the World Bank, while the exchange-rate classification follows Klein and Shambaugh (2008), extended to 2013. To control for countries' military engagement, we rely on data from the Correlates of War project and the UCDP/PRIO. We supplement this core dataset with additional data on financial crises, governance, arms and commodities trade, oil prices, taxes, and monetary-policy rates, obtained from sources described in Appendix A. The scope of the data is limited to annual observations.

Table 1 presents some relevant descriptive statistics on the level and variability of government spending. In the full sample, the average share of government spending in GDP (column 1) is 16.5 percent—slightly smaller than that in the United States—while the share of military spending in total government spending (column 2) is 16.1 percent, similar to that in the U.S. data. The government-spending share in advanced countries is somewhat higher than that in developing countries, while the military-spending share is lower in advanced countries (11.9 percent versus 17.9 percent). The large share of military spending in developing countries makes it a promising candidate to be a relevant instrument.

Next, we look at the variability of key variables. Government spending is more volatile than output in the full sample and in the developing-countries subsample, but less volatile in the advanced-countries subsample (column 3 of Table 1). Military spending is, on average, 2.5 times more volatile than total government spending (column 4). Military spending in developing countries is especially volatile. A lack of variation in postwar U.S. data has often been considered an impediment to the effectiveness of the military-spending approach (e.g., Hall 2009). This appears less of a problem with international data. Figure B.1 in Appendix B shows the distribution of changes in the three main variables, providing additional evidence on the volatility of military spending in international data.

3 Methodology

3.1 Identification

To identify the effects of government spending on output, we instrument government spending with a military-spending shock. This identification strategy is based on two conditions: (1) Military spending does not correlate with the unobserved determinants of output (*instrument validity*), and (2) total government spending correlates with military spending (*instrument relevance*). While the instrument relevance can be tested statistically, no such test exists for the instrument validity. We therefore discuss this assumption in more detail.

Many studies (e.g., Barro 1981; Hall 1986, 2009; Ramey and Shapiro 1998) argue that military spending responds to geopolitical events rather than to domestic economic conditions. One concern is that this assumption is potentially more applicable to large developed economies, such as the United States, than to developing countries. Collier (2006), Miyamoto, Nguyen, and Sheremirov (2019), and others provide numerous narrative examples of specific episodes when military spending reacted to geopolitical events (e.g., the 2008 Cambodia–Thailand border dispute), thereby lending support for extending the method to international data. Such examples are common across a wide range of advanced and developing countries. To address any remaining concerns, we examine our results' sensitivity to excluding the countries where, and periods when, the identification assumption is relatively more likely to fail (episodes of civil unrest, fluctuations in commodity prices, large financial crises, etc.). These exercises provide additional support for our identifying assumption.

Another potential caveat with our approach is a lack of data on expectations about future military spending. The anticipation effect proved to be important in U.S. studies, especially at high frequencies (e.g., Ramey 2011b; Auerbach and Gorodnichenko 2016; Ramey and Zubairy 2018). Using data at an annual frequency may, in part, alleviate the effects of this channel, since anticipation of military spending within a calendar year is irrelevant. In practice, there is a lot of uncertainty about government spending at longer horizons, especially in developing countries with a large degree of political uncertainty, as political instability or change in governance may lead to expectations of rising military spending. To address this possibility, in the robustness section, we employ data on political stability and governance.

We note that, along many dimensions, the properties of military spending in international data are comparable to those in U.S. data. For most countries, for example, variation in military spending (relative to its size) is similar in magnitude to, or greater than, the variation for the United States (Appendix Table B.1). In addition, military spending shocks in the United States exhibit moderate, positive correlation with the shocks in the rest of the world (Figure B.2). Such empirical observations are consistent with conjectures emphasizing common geopolitical shocks as an important driver of military

Table 2: Is the Instrument Strong?

Fixed effects	No (1)	Country (C) (2)	Time (T) (3)	C–T (4)
One year	3.6	0.3	3.5	−0.1
Two years	25.0	16.9	24.5	16.6
Three years	27.5	12.5	26.3	12.9
Four years	23.5	6.0	22.4	7.4
Five years	16.7	−1.9	15.6	−0.2

Notes: This table shows the difference between the first-stage effective F-statistic (Montiel Olea and Pflueger 2013) and the corresponding 5 percent critical value, for the cumulative responses of government spending to a military-spending shock. Positive values indicate that the null hypothesis of instrument weakness is rejected. All specifications control for wars and lags of normalized GDP, government spending, and military spending, as in the baseline second stage.

spending.

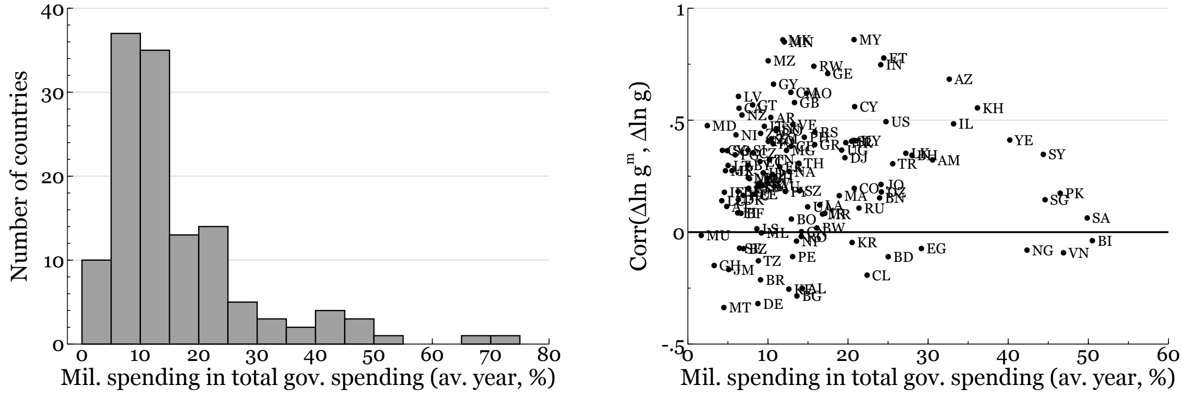
3.2 Instrument Relevance

Next, we examine the strength of military spending as an instrument for total government spending. Following Ramey and Zubairy (2018), among others, we employ the Montiel Olea and Pflueger (2013) effective F test of the first-stage instrument exclusion. Specifically, for each horizon $h = 1, 2, \ldots$, the test is based on the regression of the cumulative government spending $\sum_{j=0}^{h-1} g_{i,t+j}$ on the military-spending shock $g_{i,t}^{\mathrm{m}}$, in country i and time t, and the set of control variables from the second stage. The test is robust to the serial correlation in the error term arising from local projections, when the multiplier is estimated at longer horizons. While conventional tests require F-statistics above 10 (Staiger and Stock 1997), this test is conservative, with a threshold above 20.

Table 2 presents test results for different horizons and sets of fixed effects. We report the differences between the effective test statistic and the 5 percent threshold; that is, positive entries in the table indicate the effective F-statistic is in excess of the critical value, and thus that the instrument is relevant. Most of the differences are positive and are especially large at two-year and three-year horizons. The test statistics decline at longer horizons, which could be the case if the increase in government spending is eventually offset by fiscal consolidation in the future. The test statistics are also close to the threshold on impact, indicating that it may take government spending more than a calendar year to respond to the shock.

We investigate if the instrument strength is driven entirely by countries with large shares of military spending in total government spending. In the left panel of Figure 1, we plot these shares averaged over time. Only 10 countries have military-spending shares below 5 percent; thus, this issue affects only a small portion of our data. In the right panel of the figure, we show a scatterplot of these shares against the time-correlation between log-changes in military spending and log-changes in total government spending. The linear fit is almost horizontal, and the corresponding R^2 is below 0.01. We conduct additional checks in the appendix, reaching a conclusion that the heterogeneity in the relative size of military spending across countries, overall, plays only a limited role.

Figure 1: Relative Size of Military Spending and Correlation with Government Spending



Notes: The right panel uses ISO2 country codes. For better visibility, we drop five outliers from the right-hand-side chart: two countries with average $g^{m}/g > 60$ percent (Oman, United Arab Emirates) and three countries with $\mathbb{C}\mathrm{orr}(\Delta \ln g^{m}, \Delta \ln g) < -0.45$ (Kyrgyzstan, Lebanon, Poland). We verify that these observations do not have a material effect on the slope or the fit of the corresponding linear regression. The correlations histogram that includes the outliers is in Appendix Figure B.3.

3.3 Estimation

To estimate the cumulative government-spending multiplier, we combine the local projections method (Jordà 2005) with an IV approach. To implement this method, we regress cumulative GDP on cumulative government spending, instrumented with the military-spending shock. In our baseline specification, we capture information available at time t by controlling for lags of GDP, government spending, and military spending. We also control for country and time fixed effects. Following Gordon and Krenn (2010), Ramey and Zubairy (2018), and others, we normalize output and government spending by trend GDP. We estimate the trend by fitting real GDP on a quadratic polynomial in time. The choice of a quadratic polynomial is motivated by the annual frequency and relatively short sample period of our data. While alternative approaches exist, we verify that the transformation choice does not affect our conclusions. Since the local projections method gives rise to autocorrelated errors, we use standard errors consistent in the presence of heteroskedasticity and autocorrelation (HAC) of order h, a projection horizon. We examine alternative strategies in the appendix.

Specifically, for each horizon $h = 1,\ 2, \ldots$, the exact specification is as follows:

$$\sum_{j=0}^{h-1} y_{i,t+j} = \alpha_{i,h} + \boldsymbol{\psi}_h(L)\boldsymbol{x}_{i,t-1} + \mu_h \sum_{j=0}^{h-1} g_{i,t+j} + \boldsymbol{\gamma}_h\, \boldsymbol{z}_{i,t} + \delta_{t,h} + \epsilon_{i,t+h-1}, \tag{1}$$

where $y_{i,t}$, $g_{i,t}$, and $g^{\mathrm{m}}_{i,t}$ are normalized real GDP, total government spending, and military spending, respectively, in country i and year t. $\boldsymbol{x}_{i,t-1} \equiv (y_{i,t-1}, g_{i,t-1}, g^{\mathrm{m}}_{i,t-1})$ is a vector of the variables that control for information available at time t, and $\boldsymbol{z}_{i,t}$ is a vector of contemporaneous control variables (e.g., a war indicator). $\sum_{j=0}^{h-1} g_{i,t+j}$ is instrumented with $g^{\mathrm{m}}_{i,t}$. Parameters $\alpha_{i,h}$ represent country effects. $\boldsymbol{\psi}_h(L)$ is a lag polynomial vector of order l of loadings on $\boldsymbol{x}_{i,t-1}$. We choose the number of lags using the Akaike and Schwarz information criteria.[3] Vector $\boldsymbol{\gamma}_h$ collects loadings on $\boldsymbol{z}_{i,t}$. $\delta_{t,h}$ represent time

[3]In practice, we often need to balance the number of lags with the samples size, especially in uneven subsamples. To obtain a reasonable sample size, in such cases, we use one lag.

Table 3: Cumulative Multipliers: Full Sample

Fixed effects	No (1)	Country (C) (2)	Time (T) (3)	C–T (4)
One year	0.745***	0.749**	0.816***	0.833**
	(0.287)	(0.314)	(0.296)	(0.330)
Two years	0.805***	0.768**	0.870***	0.879**
	(0.305)	(0.349)	(0.312)	(0.366)
Three years	0.613**	0.455	0.696**	0.649*
	(0.299)	(0.366)	(0.305)	(0.390)
Four years	0.573*	0.360	0.678**	0.656
	(0.306)	(0.420)	(0.315)	(0.452)
Five years	0.456	0.142	0.574*	0.509
	(0.315)	(0.479)	(0.318)	(0.501)

Notes: This table presents estimates of the cumulative government-spending multiplier. All specifications control for the war indicator and lags of normalized GDP, government spending, and military spending. Column (1) provides pooled estimates; column (2) controls for country fixed effects; column (3) controls for time fixed effects; and column (4) controls for both country and time effects. HAC standard errors are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

effects. $\epsilon_{i,t+h-1}$ is the error term from the second-stage regression at horizon h. In this specification, coefficient μ_h measures the cumulative government-spending multiplier at an h-year horizon.

To measure the multipliers in subsamples (e.g., advanced versus developing countries), and to test the statistical difference between such multipliers, we interact the regression coefficients with a subsample dummy ($\mathbb{I}_t$), estimating the following equation:

$$\sum_{j=0}^{h-1} y_{i,t+j} = \mathbb{I}_t \times \left(\alpha^A_{i,h} + \boldsymbol{\psi}^A_h(L)\,\boldsymbol{x}_{i,t-1} + \mu^A_h \sum_{j=0}^{h-1} g_{i,t+j} + \boldsymbol{\gamma}^A_h\,\boldsymbol{z}_{i,t} + \delta^A_{t,h} \right) + (1-\mathbb{I}_t) \times \left(\alpha^B_{i,h} + \boldsymbol{\psi}^B_h(L)\,\boldsymbol{x}_{i,t-1} + \mu^B_h \sum_{j=0}^{h-1} g_{i,t+j} + \boldsymbol{\gamma}^B_h\,\boldsymbol{z}_{i,t} + \delta^B_{t,h} \right) + \epsilon_{i,t+h-1}, \quad (2)$$

with $\mathbb{I}_t \times g^{\mathrm{m}}_{i,t}$ and $(1-\mathbb{I}_t) \times g^{\mathrm{m}}_{i,t}$ as instruments for $\mathbb{I}_t \times \sum_{j=0}^{h-1} g_{i,t+j}$ and $(1-\mathbb{I}_t) \times \sum_{j=0}^{h-1} g_{i,t+j}$. In this specification, μ^A_h and μ^B_h are the horizon-h cumulative multipliers for subsample A and subsample B, respectively, and the test for the hypothesis $\mu^A_h = \mu^B_h$ is straightforward.

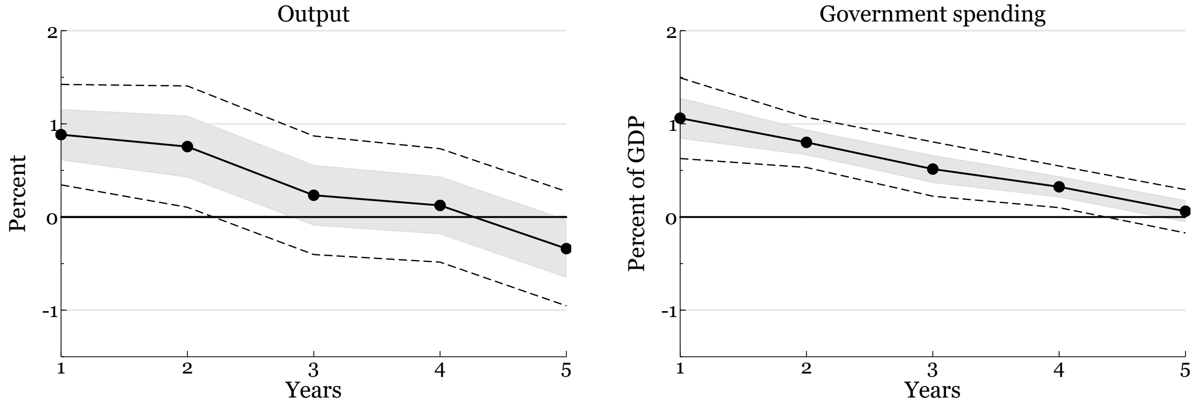
4 Results

4.1 Full Sample

Our estimates of the cumulative government-spending multiplier are presented in Table 3. In each specification, we control for the war indicator and four lags of military spending, government spending, and output. Depending on the set of fixed effects, the multiplier is in the range 0.75–0.85 on impact, remaining at this level for two years and then decreasing. The multiplier is robust to the inclusion of fixed effects; we use both country and time effects (column 4) as a baseline.

We compare these baseline estimates with a number of alternatives and find that they are repre-

Figure 2: Impulse Responses to Military-Spending Shock



Notes: The figure shows the IRFs of output (left panel) and government spending (right panel) to a military spending shock of 1 percent of GDP, estimated using local projections. The gray area represents one standard deviation on each side of the estimates, and the dotted lines show 95 percent confidence bounds.

sentative. In Appendix Table B.2, for example, we analyze sensitivity to the number of lags included as control variables. Table B.3 presents estimates using lagged output, instead of trend output, for normalization. This transformation yields, on impact, results similar to our baseline approach and somewhat larger multipliers over longer horizons. Further, our estimates remain significant for the first two years when we allow for the errors to correlate across countries (e.g., due to trade and financial linkages or synchronized policy measures), using Driscoll and Kraay (1998) or two-way-clustered standard errors (Table B.4).

Our estimates are similar to those in the literature. For example, using military news in the United States, Ramey and Zubairy (2018) report state-invariant multipliers, cumulated over a two-year period and a four-year period, in the range 0.66–0.71. Using a structural vector autoregression (VAR) instead, Auerbach and Gorodnichenko (2012) report a 0.57 cumulative multiplier over a five-year period for the U.S., while Ilzetzki, Mendoza, and Végh (2013) report a long-run multiplier of 0.66 for a large sample of high-income countries. While our pooled multipliers look quite similar, despite using annual data for a different period and a much larger sample of countries, we later show that this similarity masks important heterogeneities across countries and episodes.

In Figure 2, we plot the impulse-response functions (IRFs) of output (left panel) and government spending (right panel) to a normalized military-spending shock. The output and government-spending responses remain positive for a few years but eventually decline to zero. Thus, the cumulative multiplier combines the persistent increase in output with the persistent increase in government spending. The timing of the output response is, in general, consistent with previous studies (e.g., Barro and Redlick 2011).

Table 4: Development, Recessions, and Slack

	Advanced (1)	Developing (2)	p-val. (3)	Recessions (4)	Expansions (5)	p-val. (6)	High U. (7)	Low U. (8)	p-val. (9)
One year	1.755***	0.804**		1.661***	0.329		1.453***	0.771*	
	(0.556)	(0.335)	0.129	(0.622)	(0.211)	0.031	(0.463)	(0.443)	0.101
	[31.2]	[21.4]		[34.6]	[16.6]		[27.4]	[15.3]	
Two years	1.964**	0.832**		1.551**	0.354		1.220**	0.958*	
	(0.791)	(0.369)	0.270	(0.607)	(0.293)	0.062	(0.505)	(0.490)	0.332
	[24.7]	[39.9]		[41.6]	[26.9]		[16.5]	[59.8]	
Three years	1.605**	0.589		1.345**	0.043		0.819	0.772	
	(0.808)	(0.403)	0.368	(0.629)	(0.311)	0.074	(0.515)	(0.733)	0.517
	[23.5]	[34.2]		[32.9]	[24.8]		[12.5]	[19.5]	
Four years	1.877**	0.608		1.408**	−0.086		0.572	0.999	
	(0.902)	(0.472)	0.260	(0.630)	(0.396)	0.040	(0.500)	(0.975)	0.774
	[11.5]	[27.9]		[27.0]	[22.0]		[11.5]	[13.6]	
Obs.	774	1,789		500	2,063		1,306	1,218	

Notes: Advanced and developing countries in columns (1) and (2) are classified as such by the IMF. Recessions in column (4) are defined at annual frequencies as a decrease in real GDP; all other episodes are considered expansions (column 5). The high (column 7) and low (column 8) unemployment regimes correspond to unemployment rates above and below the country's median, respectively. p-values in columns (3), (6), and (9) are for the differences between the point estimates in the two preceding columns. HAC standard errors are in parentheses. The effective F-statistics are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

4.2 Development

Next, we estimate the multiplier separately for developed and developing countries, as classified by the IMF.[4] The point estimates are larger for developed countries (column 1 of Table 4) than for developing countries (column 2). An additional $1 of government spending, on impact, increases GDP by $1.76 in developed countries and by $0.80 in developing countries. The multiplier in developed countries is large and statistically different from zero at all considered horizons, while in developing countries it becomes smaller and insignificant after two years. The difference in point estimates between the subsamples, however, is not statistically significant at conventional levels (column 3) due to large confidence bounds. Note also that the instrument relevance of military spending is stronger at short horizons for developed countries, and at longer horizons for developing countries (compare the effective F-statistics in brackets). The smaller multipliers could be consistent with tight fiscal space in the developing countries that have limited access to domestic debt and that rely primarily on foreign debt—including debt from the IMF, which often takes longer to secure—and taxation. Hagedorn, Manovskii, and Mitman (2019) show that, in a heterogeneous-agents New Keynesian model with incomplete markets, the fiscal multiplier is considerably above one if spending is financed by debt but below one if taxes are raised. Our empirical estimates appear consistent with the debt-financed multiplier in developed countries and the tax-financed multiplier in developing countries.

Note that the multipliers for advanced countries are larger than the ones reported in the literature, as discussed in the previous section. While the standard errors are too large to reject conventional (lower) values, one potential explanation for this discrepancy lies in the sample period. A significant

[4] As an alternative, we also use a World Bank development classification based on gross national income (Appendix Table B.5). This classification allows us to break down the sample of developing countries further, into middle-income and low-income countries. The multiplier magnitude in developing countries is pulled downward by low-income countries. Our estimates for low-income countries are similar to those of Kraay (2012), who studies the effects of World Bank aid disbursements in 29 countries, most of which are low income.

part of our sample is influenced by a deep recession and a tepid recovery across the advanced nations, which may result in larger multipliers (Auerbach and Gorodnichenko 2012). We test the hypothesis that the multipliers are larger in recessions in the next section.

While the literature provides less evidence for developing countries, a prominent study by Ilzetzki, Mendoza, and Végh (2013) documents negative but insignificant multipliers for a sample of developing countries that consists mostly of upper-middle-income countries. We, too, find smaller multipliers for developing countries than for developed countries, but in our sample the multipliers are positive and significant. The Ilzetzki et al. results can be explained by two factors: (1) The output responses quickly return to zero, and (2) increases in government spending are eventually followed by decreases. In our sample, however, the IRF for developing countries is characterized by steady increases in both variables (Appendix Figure B.4). Our estimates are also numerically larger than, but statistically not different from, the ones by Kraay (2014), who estimates a multiplier of about 0.4 in a similar sample of developing countries, but for a different period. While our results are qualitatively similar to Kraay's, this piece of evidence is based on a different identification strategy and a different period.

4.3 Recessions and Slack

To compare the multipliers in recessions and expansions, we first define a recession at an annual frequency as a decrease in real GDP relative to the previous year.[5] Because the data are observed at an annual frequency, this definition is likely to miss small recessions. We then estimate Equation (2) separately for recessionary and expansionary episodes. We find that the fiscal multipliers are larger in recessions (column 4 of Table 4) than in expansions (column 5). In recessions, a $1 increase in government spending, on impact, leads to a statistically significant increase in output of $1.66, whereas in expansions the increase is $0.33 and not statistically significant. The recession multiplier remains statistically significant and above one for at least four years after the shock. The difference between the recession and expansion multipliers is statistically significant, at least at the 10 percent level, at all horizons considered (column 6), a result consistent with Auerbach and Gorodnichenko (2012). Our recession multipliers are larger than the ones by Kraay (2014), who reports estimates in the range 0.6–0.8. Besides the differences in the identification strategies, our data end in 2013 and therefore contain the entire aftermath of the Global Financial Crisis.

One potential reasons why the fiscal multiplier may be larger in recessions than in expansions is economic slack, which makes fiscal policy particularly powerful in some models. We then estimate the multiplier for different degrees of slack in the economy. We define periods of relative economic slack as years when the unemployment rate is above the (country-specific) median unemployment rate.[6] We find that the multiplier is larger when unemployment is high (column 7 of Table 4) than when it is low (column 8). An additional $1 in government spending, on impact, increases output by $1.45 during high-slack episodes and by $0.77 during low-slack episodes. While the differences are, marginally, not

[5] Since recessions are usually defined at a quarterly frequency, we experiment with treating a year following the negative growth in GDP as a recession, too. This approach accounts for the cases when strong recovery at the end of a recessionary year offsets the negative growth at the beginning. We also try several alternative definitions and find similar results.

[6] As a threshold, we also use higher percentiles of the unemployment rate and find similar results (Appendix Table B.6).

Table 5: Exchange-Rate Regimes and Trade Openness

	Peg (1)	Float (2)	p-val. (3)	Closed (4)	Open (5)	p-val. (6)
One year	1.328**	0.434		2.024***	0.394	
	(0.668)	(0.388)	0.376	(0.726)	(0.331)	0.054
	[11.1]	[5.6]		[11.0]	[5.6]	
Two years	1.599***	0.512		1.965**	0.561	
	(0.592)	(0.527)	0.317	(0.780)	(0.484)	0.177
	[25.3]	[7.2]		[10.8]	[7.7]	
Three years	1.084*	0.358		1.918**	0.264	
	(0.591)	(0.537)	0.478	(0.905)	(0.471)	0.161
	[15.3]	[12.9]		[7.1]	[12.3]	
Four years	1.288**	0.294		1.887	0.395	
	(0.643)	(0.593)	0.387	(1.183)	(0.565)	0.353
	[11.6]	[8.6]		[4.3]	[8.2]	
Obs.	1,250	1,649		1,268	1,654	

Notes: The exchange-rate classification in columns (1) and (2) is based on Klein and Shambaugh (2008), extended to the end of the sample. The trade-openness classification in columns (4) and (5) is based on total trade (exports plus imports) relative to 60 percent of GDP. p-values in columns (3) and (6) are for the differences between the point estimates in the two preceding columns. HAC standard errors are in parentheses. The first-stage effective F-statistics are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

statistically significant (column 9), the estimates are qualitatively similar to those in columns (4) and (5).

4.4 Exchange-Rate Regimes

To evaluate the effect of the exchange-rate regime on the multiplier size, we split the sample into pegs and floats, based on the Klein and Shambaugh (2008) classification, updated through the end of our sample. Under a fixed exchange rate, the impact multiplier is 1.33 and highly significant, compared with 0.43 and insignificant under a flexible rate (columns 1 and 2 of Table 5). The difference between the pegs and the floats, however, is not statistically significant at conventional levels (column 3), despite the large quantitative difference between the estimates. This can be explained by less-pronounced differences—and more heterogeneity—between the exchange regimes in advanced countries (see Appendix Figure B.5); the confidence intervals around the output responses in advanced pegs are particularly wide. Indeed, countries in a monetary union (e.g., the euro area) are pegs with respect to other countries in the union but floats relative to currencies outside the union. The differences between the pegs and the floats in developing countries, most of which are not in a monetary union, are material.

Our empirical estimates are consistent with theoretical models emphasizing the role of central-bank constraints under a fixed exchange rate (e.g., Mundell 1963): With free capital movement, a fiscal expansion, in such models, is not offset by monetary tightening due to the central bank's commitment to maintain the peg, leading to a relatively strong response of output.[7] Our estimates are also consistent with Ilzetzki, Mendoza, and Végh (2013), who document empirically—using a VAR evidence from a smaller sample of countries—a long-run multiplier of 1.4 for pegs and a multiplier statistically indistin-

[7]While, due to limited data, we do not include capital controls in the baseline specification, including capital controls in a sample with available data, as in Fernández et al. (2016), does not materially affect the estimates.

guishable from zero for floats. Corsetti, Meier, and Müller (2012) and Born, Juessen, and Müller (2013) also find larger output responses under a fixed exchange rate, but in samples that include only developed countries. We extend those studies' results on exchange-rate regimes to a more comprehensive sample of countries and a different identification assumption.

4.5 Trade Openness

We estimate the multipliers separately for open and closed economies. Following the literature, we define an economy as open if the share of total international trade (exports plus imports) is, on average, greater than 60 percent of GDP. The results in columns (4) through (6) of Table 5 show that fiscal policy is more effective in closed economies than in open economies. A $1 increase in government spending, cumulative over three years, leads to a cumulative increase in GDP of $1.92 in closed economies and $0.26 in open economies. While the closed-economy multiplier is larger than the open-economy multiplier at all horizons considered, the difference is statistically significant, at 10 percent, only at a one-year horizon.

The relative sizes of the multipliers in closed and open economies are, again, consistent with those in Ilzetzki, Mendoza, and Végh (2013). While, quantitatively, our estimates for closed economies are larger than the ones in that study, due to wide confidence intervals, we cannot reject that the magnitudes are equal in the two studies. Our results are also consistent with the transmission mechanism documented by Miyamoto, Nguyen, and Sheremirov (2019), who find that, in open economies, a fiscal expansion leads to a statistically significant appreciation of the domestic currency and a significant decrease in the current account, whereas in closed economies the appreciation is insignificant and the current-account decline is smaller. Hence, the stimulative effects of fiscal policy in open economies may spill over to foreign producers through a higher level of imports, while in closed economies the gains are concentrated in domestic production. On the contrary, we do not lend empirical support to mechanisms emphasizing positive effects of trade linkages on consumption, thereby giving rise to multipliers that are larger in open economies than in closed economies (e.g., Cacciatore and Traum 2018).

To summarize, our empirical estimates indicate that the size of the fiscal multiplier can vary significantly. While our baseline pooled estimates are consistent with some estimates from different samples and methods reported in the literature, such similarities may be misleading because the estimated effects of fiscal policy differ materially across countries. Focusing on potential sources of such heterogeneity, we document empirical support for the multipliers being relatively large in recessions, during episodes of economic slack, under exchange-rate pegs, and in closed economies. These findings lend empirical support to prominent macroeconomic models emphasizing each of these factors. We also provide new evidence on the relevance of military spending as an instrument for government spending in international data. Military spending appears to be at least as strong an instrument in the developing-countries sample as it is in the developed-countries one. We also find that it is particularly relevant under certain economic environments (e.g., in recessions).

5 Robustness

We now address concerns about our identification strategy and the robustness of our results. Many of these exercises follow the approach of Miyamoto, Nguyen, and Sheremirov (2019), who use military-spending shocks to study the effects of fiscal policy on exchange rates. This approach is based on a battery of checks wherein countries with a suspected violation of the identifying assumption are dropped from the baseline sample.

5.1 Financial Crises and Wars

We start by considering the effects of financial crises. Besides causing negative output effects, financial crises may prompt a decrease in military spending through budget cuts, leading to an omitted-variable bias. To check the potential extent of this issue, we drop observations with financial crises, as defined in Reinhart and Rogoff (2011). The multiplier estimates from this subsample are presented in columns (1) through (3) of Table 6. While the estimates for developing countries are slightly larger than the baseline estimates, they are quite similar to the baseline for advanced economies. In both subsamples, the new estimates are well within the original confidence intervals. We conclude that episodes of financial crisis have only a minor effect on our results.

We also investigate the effect of wars on our estimates. While the identification assumption exploits the shocks due to wars fought for exogenous reasons, large wars on domestic soil could have consequences for domestic output that are very different from the consequences of small wars fought overseas. Large wars destroy output but also call for military spending. An ongoing military threat may also have an effect on expectations about spending and output in the future, thereby altering present economic decisions.

To account for these effects, we run a series of exercises and relegate the results to Appendix Table B.7 due to space constraints. In one exercise, we account for countries that are not at war per se but located in hotbeds of geopolitical instability by controlling for a regional war indicator. The indicator assigns a value of one to all countries located in a narrowly defined region in years with any war in that region. In another exercise, we drop countries with long civil wars, because such wars take place on domestic soil and often have a devastating effect on output. In these two exercises, we obtain multipliers only slightly smaller than the baseline: in the range 0.67–0.78 at a one-year horizon. Finally, as a more restrictive test, we exclude countries with any war incidence (i.e., countries that were at war of any type at least one year during our sample period). While this test leads to significant changes in the sample composition, the resulting multipliers rise modestly to about one on impact. This increase is explained, in part, by the omission of a relatively larger number of developing countries.

5.2 Anticipation of Military Shocks and Governance

Changes in military spending may be anticipated; some military programs are announced in advance and implemented over a longer period of time. The timing of the shock is therefore crucial for proper

Table 6: Robustness to Sample Composition

	No Financial Crises			No Autocracies			No Large Arms Importers		
	All	Adv.	Dev.	All	Adv.	Dev.	All	Adv.	Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
One year	1.169***	1.801***	1.123**	1.020***	1.994***	0.900**	0.986**	1.848***	0.953**
	(0.411)	(0.606)	(0.444)	(0.364)	(0.553)	(0.412)	(0.427)	(0.556)	(0.438)
	[22.8]	[34.9]	[17.3]	[67.8]	[39.9]	[65.0]	[15.0]	[33.7]	[13.6]
Two years	1.170***	1.853**	1.110**	0.958***	2.314***	0.782**	1.038**	2.104***	0.968**
	(0.433)	(0.735)	(0.458)	(0.356)	(0.824)	(0.385)	(0.449)	(0.806)	(0.455)
	[26.2]	[20.2]	[23.3]	[56.4]	[24.6]	[74.8]	[29.0]	[24.4]	[28.2]
Three years	0.800*	1.458**	0.721	0.814**	1.809**	0.662*	0.802	1.768**	0.711
	(0.474)	(0.696)	(0.507)	(0.338)	(0.805)	(0.379)	(0.501)	(0.802)	(0.527)
	[16.5]	[15.9]	[14.2]	[52.5]	[19.0]	[55.8]	[25.7]	[18.9]	[23.1]
Obs.	1,955	546	1,409	1,805	706	1,099	2,226	752	1,474

Notes: HAC standard errors are in parentheses. The first-stage effective F-statistics are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

accounting of its effects (Ramey 2011b). While we do not have data on military-spending announcements, using annual data allows us to overcome the mismatch between the news and implementation, as long as both take place within a calendar year.

To address the issue arising from longer-term programs, we follow Miyamoto, Nguyen, and Sheremirov (2019) and examine the effects of governance and political instability on our results. A higher degree of political uncertainty or worsening in the governance practices may lead to an increase in expectations of military spending, due to potential civil unrest. Less-democratic governments may also be associated with a higher degree of political uncertainty and a higher probability of war, since it is easier for them to engage in hostilities, due to insufficient checks and balances.

To this end, we employ the democracy and autocracy indices from Polity IV Project. Each index varies from zero to ten, and we define a country as an autocracy in a given year if the democracy index is smaller than the autocracy index. We then drop autocracies from the sample, and re-estimate the multipliers. Columns (4) through (6) of Table 6 report estimates qualitatively similar to the baseline.[8]

5.3 Arms Imports

Our next concern is arms imports. Most countries do not have significant arms production and therefore import arms. In countries where arms imports make up an especially large share of military spending, such spending may have a smaller effect on output due to a smaller increase in absorption. If arms imports, however, are large enough to depreciate the local currency, they may lead to future increases in net exports, a channel working in the opposite direction. To understand how large arms importers differ from other countries in the sample, we exclude countries for which the average share of arms imports in total military spending is larger than 30 percent, which results in dropping more than 10 percent of the observations. As columns (7) through (9) of Table 6 suggest, excluding large arms importers does not materially affect our results.

[8] In another exercise, we use the World Bank's *Worldwide Governance Indicators* data, which contain a country's percentile rank of political stability and absence of violence and terrorism. We consider a country to be politically stable if it is ranked above the median. While the data are available for only a small subsample, controlling for political instability does not materially affect the multipliers in that sample.

Table 7: Commodity Exports and Prices

	No Commodity Exporters			Oil Prices			No Giant Oil Discoveries		
	All	Adv.	Dev.	All	Adv.	Dev.	All	Adv.	Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
One year	0.686**	1.578***	0.669**	0.782**	1.918***	0.748**	0.809**	1.802***	0.781**
	(0.317)	(0.572)	(0.323)	(0.336)	(0.592)	(0.339)	(0.340)	(0.566)	(0.345)
	[33.7]	[35.4]	[38.4]	[20.2]	[33.6]	[19.0]	[21.4]	[34.4]	[20.2]
Two years	0.624*	1.703**	0.570*	0.809**	1.957**	0.763**	0.848**	2.053***	0.803**
	(0.330)	(0.741)	(0.334)	(0.355)	(0.836)	(0.359)	(0.357)	(0.786)	(0.360)
	[63.8]	[24.7]	[80.5]	[40.0]	[24.5]	[39.9]	[47.9]	[24.3]	[49.3]
Three years	0.462	1.287*	0.390	0.531	1.450*	0.470	0.615	1.715**	0.556
	(0.341)	(0.688)	(0.358)	(0.372)	(0.798)	(0.390)	(0.376)	(0.763)	(0.391)
	[78.2]	[19.3]	[81.5]	[39.2]	[18.7]	[36.4]	[48.3]	[19.1]	[47.0]
Obs.	1,873	708	1,165	2,563	774	1,789	2,485	755	1,730

Notes: HAC standard errors are in parentheses. The first-stage effective F-statistics are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

5.4 Commodity Exports and Prices

The military-spending budget may be linked to commodity exports. In some cases, such a relation is legally binding. For example, Chile's Copper Law allocates 10 percent of copper export revenues to the military. In large commodity-exporting countries, an increase in commodity prices may therefore lead to an increase in GDP and in military spending. To address this concern, columns (1) through (3) of Table 7 exclude from our baseline sample large commodity exporters, which are defined as countries with average commodity exports above 50 percent of GDP. The results appear similar to our baseline. This conclusion also holds when we exclude large oil exporters (Appendix Table B.8, columns 1–3).

In a separate exercise, we control for commodity prices. While large commodity exporters may increase their military spending when commodity prices are high and decrease it when they are low, commodity importers may exhibit the opposite pattern. Because oil is a major commodity that has been given disproportionate attention in the literature on supply shocks, we focus on oil prices. Oil is also a major commodity import in many countries with scarce natural resources. To control for oil prices, we use data on West Texas Intermediate crude oil. The estimates from this specification (columns 4–6 of Table 7) are very similar to our baseline estimates, in the pooled sample and in the advanced- and developing-countries subsamples.

Finally, giant oil discoveries have been discussed in the literature as another factor that affects both military spending and output. For example, Lei and Michaels (2014) find that giant oil discoveries increase the probability of armed conflict. As a robustness check, we exclude episodes of large oil discovery identified in that study. The resulting estimates, again, are similar (columns 7–9 of Table 7). This conclusion also holds when we drop from the sample countries with at least one giant oil discovery during the sample period (Appendix Table B.8, columns 4–6), with somewhat larger (than in the baseline) multipliers in developing countries at short horizons.

5.5 Size of the Military Sector

Our next concern is that military spending is relatively small in some countries and the passthrough to total government spending is low. To address this concern, we conduct two robustness exercises. In one

Table 8: OLS Estimates

	Baseline (1)	Adv. (2)	Dev. (3)	Rec. (4)	Exp. (5)	Peg (6)	Float (7)	Closed (8)	Open (9)
One year	0.494***	0.989***	0.482***	0.667***	0.236**	0.763***	0.383**	1.317***	0.342**
	(0.155)	(0.378)	(0.156)	(0.188)	(0.118)	(0.122)	(0.160)	(0.353)	(0.136)
Two years	0.612***	1.029**	0.602***	0.699***	0.294*	0.935***	0.418**	1.382***	0.442**
	(0.170)	(0.455)	(0.174)	(0.192)	(0.152)	(0.156)	(0.179)	(0.351)	(0.174)
Three years	0.558***	0.899*	0.555***	0.621***	0.246	0.926***	0.322	1.375***	0.379*
	(0.196)	(0.505)	(0.202)	(0.195)	(0.171)	(0.170)	(0.198)	(0.356)	(0.209)

Notes: HAC standard errors are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

exercise, we restrict the sample to countries with high shares of military spending (e.g., more than 5 or 10 percent of total spending). In the other, we drop from the sample countries for which an increase in government spending, on average, did not lead to an increase in total spending (i.e., we focus on cases with a positive correlation of changes in g^{m} and g). To save space, the results are relegated to the appendix (Tables B.9 and B.10). In both cases, there is little material effect on the multiplier.

5.6 OLS Estimates

Finally, it is instructive to compare our baseline IV estimates with those from ordinary least squares (OLS). If government spending is used as a means of countercyclical fiscal policy, using total government spending as a fiscal shock may result in attenuation bias. To the extent that military spending responds only to geopolitical events unrelated to domestic economic conditions, and fiscal policy is effective, we expect the OLS estimates to be smaller than the baseline IV estimates. As Table 8 suggests, this is indeed the case, and in many samples the attenuation is quite large. The OLS estimates can also be interpreted as the multipliers obtained from an annual bivariate VAR, with Cholesky decomposition and government spending ordered last (i.e., not responding to output contemporaneously). While this approach has been used widely with quarterly data (e.g., Blanchard and Perotti 2002; Ilzetzki, Mendoza, and Végh 2013), it is hard to justify with annual data, since it is unlikely that government spending takes more than a year to react to changes in output. This exercise, however, enhances our understanding of the baseline results.

6 Discussion

As our paper studies a large panel of countries, our ability to extend the analysis to other variables and methods is limited by available data. For example, in order to compare our identification with other alternatives, such as the Blanchard and Perotti (2002) identification, we need quarterly data. We would also like to gain a better understanding of how the implementation of fiscal policy influences its effectiveness. In this section, we discuss these issues, focusing on small subsamples of countries with more data.

6.1 Comparison with Ilzetzki et al.

Because Ilzetzki, Mendoza, and Végh (2013) use a different sample and different method, we strive to explain the differences between their results and ours. While our results are, overall, qualitatively similar to theirs, our point estimates are larger for both developed and developing countries. The differences could be due to two factors: sample composition and identification.

To differentiate between these factors, we apply our method to a sample as close to theirs as possible. We match their sample fairly well, with two exceptions: (1) our data for Australia, France, the United Kingdom, and the United States start in 1988, while their data for France go back to 1978, and to 1960 for the other three countries; and (2) our sample does not include Iceland. In this exercise (Appendix Table B.11), we obtain point estimates larger than in the baseline, but within the confidence intervals. While these results point to the differences between the methodologies as a likely explanation, we are cautious about this conclusion, due to remaining differences in the samples and, importantly, the frequency of observations.

This exercise, in addition, confirms our earlier conjecture that the differences between advanced and developing countries come mostly from low-income countries. Our multiplier estimates for developing countries in the Ilzetzki et al. sample of mainly upper-middle-income countries are larger than the baseline estimates and similar to those for developed countries, at least on impact.

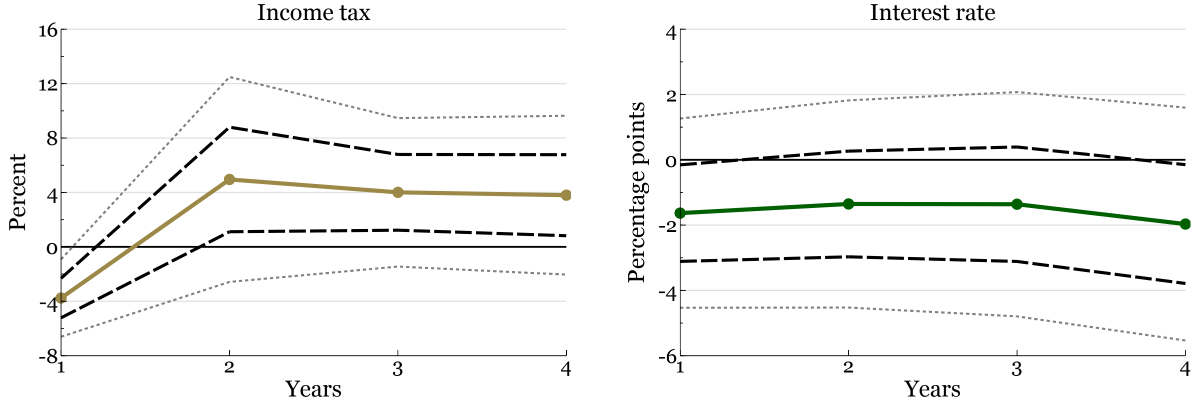
6.2 Taxes

The effects of government spending on output may depend on, among other factors, whether the government spending is financed by taxes or debt. In the baseline, we estimate the size of the multiplier for the average response of taxes in the sample. Distinguishing debt-financed government spending from tax-financed spending is beyond the scope of this paper, due in part to a lack of available data. We therefore address a more limited question: What is the response of tax rates to government-spending shocks in countries with available tax data?

To this end, we collect data on tax rates from two sources. First, we use individual income marginal tax rates from the OECD. These data provide information on the entire tax scale and personal allowances for the member states. Second, to extend the coverage to developing countries, we use individual income marginal tax rates of top earners from KPMG, an auditor. The KPMG data, however, provide marginal tax rates for only the top income earners and are not available before 2006. While this measure ignores variation in tax rates for low-income brackets as well as variation in personal allowance, it is not affected by changes in total income or its distribution.

The left panel of Figure 3 shows that, while there is limited evidence that tax rates increase in response to a spending shock at longer horizons, the increases are not statistically significant. When we split the sample into advanced and developing countries (Appendix Figure B.6, Panel A), we find that, in response to a spending shock, tax rates are more likely to increase in developing countries than in developed ones. One potential explanation could be that developed countries have more capacity to issue debt, whereas many developing-country governments operate in tight fiscal space. While confi-

Figure 3: Responses of Tax and Interest Rates to Military-Spending Shock



Notes: The figure shows the IRFs of the income marginal tax rate (left panel) and the monetary-policy rate (right panel) to a military-spending shock of 1 percent of GDP, estimated using the local projections method. The dashed lines represent one-standard-deviation bands, and the dotted lines show 95 percent confidence intervals.

dence intervals are large due to the small samples with available data, the difference in tax responses is consistent with larger multipliers in advanced economies.

6.3 Monetary Policy

The size of the fiscal multiplier may also depend on whether the monetary authority leans against the wind—and, if so, to what extent—as well as on the monetary-fiscal coordination. To get a sense of whether monetary policy leaned against the wind in our sample, we estimate the responses of policy rates to government-spending shocks. We obtain end-of-year policy rates for 25 countries and the euro area, provided by the corresponding central banks. When official policy rates are not available, we use discount rates from the IMF's *International Financial Statistics*.

The right panel of Figure 3 suggests no statistically significant increases in interest rates at all horizons considered. We also find little qualitative difference between the interest-rate responses in advanced and developing countries; both are negative and insignificant (Appendix Figure B.6, Panel B). The responses in advanced countries are especially small quantitatively, which could be explained, in part, by a prolonged period of the zero lower bound in our sample.

We provide additional evidence on the responses of output, policy rates, and taxes from a four-variable panel VAR with shocks identified by ordering military spending last (and output first) in the Cholesky decomposition (Appendix Figure B.7). The responses are consistent with our baseline evidence. Since the data on policy rates and taxes are available mostly for advanced and upper-middle-income countries, we re-estimate cumulative multipliers in the samples with data for each of the two variables (Table B.12). Since we find multipliers larger than in the baseline, it is conceivable that the responses of tax rates and policy rates in countries with no data were different.

Table 9: **Sectoral Military Spending**

	(1)	(2)	(3)	(4)
Military nondurables	1.62**			
	(0.66)			
Military durables		4.50***	2.91**	0.96
		(1.11)	(1.37)	(1.76)
Total military spending			1.29**	1.26
			(0.66)	(0.99)
Recession × Military durables				4.86**
				(2.43)
Recession × Total military spending				−0.82
				(1.20)
Obs.	548	548	548	548

Notes: This table presents estimates of the one-year output response to military spending on durables and on nondurables/services. Estimates of Equation (3) are in column (3). Standard errors are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

6.4 Spending Composition

The effectiveness of government spending may also depend on spending composition (Ramey and Shapiro 1998). One particular characteristic of spending composition discussed in the recent literature is product durability (e.g., Boehm 2016). Using data on spending composition in, mostly, advanced economies, we provide some evidence on the effects of spending on military durables and nondurables. Following Gartzke (2001), we use data from NATO press releases and other sources, and define military equipment and infrastructure as durables, and spending on personnel and other expenditures (typically, operations costs) as spending on nondurables and services.

Since we do not have data on total government spending on durables and nondurables, we deviate from our baseline methodology by instead estimating the direct contemporaneous response of output to military spending, by type. Specifically, we estimate the following specification:

$$\frac{\Delta y_{i,t}}{y_{i,t-1}} = \alpha_i + \beta \frac{\Delta g^{\mathrm{m}}_{i,t}}{y_{i,t-1}} + \zeta \frac{\Delta g^{\mathrm{d}}_{i,t}}{y_{i,t-1}} + \boldsymbol{\gamma} \boldsymbol{z}_{i,t} + \delta_t + \epsilon_{i,t}, \tag{3}$$

where $g^{\mathrm{d}}_{i,t}$ is government spending on military durables, and other variables and parameters are defined as before. In this specification, a \$1 increase in military spending on nondurables raises output by β dollars, while \$1 spent on durables raises it by $(\beta + \zeta)$ dollars. If there is no difference between the two effects (i.e., $\zeta = 0$), then $\beta = \mu_1 (\partial g / \partial g^{\mathrm{m}})$; in other words, β is close to the one-year multiplier as long as the passthrough from military spending into total spending is close to one.

Table 9 shows estimates of the output response to military spending on durables and on nondurables/services. In columns (1) and (2), we include one type of spending at a time, while column (3) presents estimates of Equation (3) as a whole. The output effect of spending on military durables is substantially larger than that on military nondurables. Our benchmark estimates in column (3) indicate that a \$1 increase in government spending on military nondurables and services is associated with a \$1.29 increase in output, while \$1 spent on durables leads to an additional increase in output of \$2.91. While this difference is statistically significant, small coefficients are possible, and the effects of both types of spending dissipate over longer horizons (Appendix Figure B.8).

Why is the output response to durables spending so large? In column (4), we interact the two variables with the recession dummy and find that the differences comes mostly from recessionary episodes. Another potential reason is that large spending programs are often focused on durables, and large shocks could be disproportionately more effective. We leave analyses of potential nonlinearities in the effects of fiscal shocks to future research.

7 Conclusion

Using data on military spending for more than a hundred countries, we estimate an annual government-spending multiplier, pooled across the sample, in the range 0.75–0.85. The multiplier estimates remain significant over longer horizons. We also find that the multiplier is larger in developed countries than in developing countries, under a fixed exchange rate than under a floating regime, in recessions than in expansions, and in closed economies than in open economies. Hence, there is a wide range of economic conditions for which expansionary fiscal policy can be a particularly effective stabilization tool.

These findings have important implications for policymakers, as the decisions on whether and how to use fiscal policy should account for particular economic circumstances and environments. Our findings also have implications for the identification of government-spending shocks. We find that the military-shocks approach produces multipliers comparable to those from other methods and robust to tests involving various controls and samples that could potentially violate the identifying assumption.

This paper also touches on three important issues that need further investigation but require additional data. While, for a subset of countries with available data, we do not find a strong response of tax rates to government-spending shocks, we hope that, in future work, tax-financed multipliers will be given further assessment. Next, we find only mild evidence of coordination between the fiscal and monetary authorities, which may point to some institutional constraints. It remains a question if such coordination can affect the multiplier size in a quantitatively important manner. Finally, the role of spending composition as well as other aspects of fiscal programs can be explored further when more data become available.

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Appendix

A Data Sources

Annual data on real GDP, government spending, and military spending are available for 160 countries during the period 1988–2013: 3,298 observations in total. We use the number of years for which these variables are available to proxy for the reliability of the data for a particular country. For this reason, we exclude 31 countries that have fewer than 15 years of observations.[1] These inclusion criteria also weed out countries that had significant wars on domestic soil, such as Afghanistan and Iraq, leaving us with a sample of relatively stable countries.

Our final sample contains 129 countries (36 advanced and 93 developing): 3,001 observations in total. Table A.1 contains information on the countries available in the entire sample, as well as the number of observations available per country. In what follows, we provide a short summary of the data and sources used in our analysis.

Real GDP and Total Government Spending: We obtain annual data on real GDP and general-government final consumption expenditure, at constant 2005 prices in national-currency units, from the *National Accounts Main Aggregates Database*, provided by the U.N. Statistics Division. The dataset contains time-series from 1970 onward for more than 200 countries, which report to the United Nations in the form of the National Accounts Questionnaires. We use the December 2014 version of the dataset, which has data available until 2013. For additional information and detailed methodology, see unstats.un.org/unsd/snaama/index.

Total Military Spending: Stockholm International Peace Research Institute (SIPRI) provides data on total military expenditure for 171 countries in the period 1988–2013. SIPRI collects data from three sources: (1) national governments and statistical agencies; (2) international organizations (e.g., NATO press releases, IMF country reports); and (3) specialist newspapers, journals, and other publications (e.g., Economist Intelligence Unit, Europa World). We calculate total military spending by using the ratios of military spending to GDP. More specifically, we multiply these ratios by real GDP, obtained from the United Nations, to calculate total military spending at constant 2005 prices in national-currency units. For more details, see sipri.org/research/armaments/milex/milex_database.

Development Classification: The development classifications are taken from the IMF and World Bank. The World Bank classification is based on gross national income. More information can be found at datahelpdesk.worldbank.org/knowledgebase/articles/378834-how-does-the-world-bank-classify-countries.

Unemployment Rates: We obtain unemployment data from the World Bank's *World Development Indicators* (WDI) database. We use the October 2015 edition of the database. The coverage starts in 1991. The World Bank collects the unemployment series from the International Labor Organization. More information can be found at datacatalog.worldbank.org/dataset/world-development-indicators.

Exchange-Rate Regimes: The exchange-rate classification follows Klein and Shambaugh (2008), updated to 2013. A country is considered to have a fixed exchange rate if the end-of-month exchange rate stays within the 2 percent bands for the entire year. The updated classification can be found at gwu.edu/∼iiep/about/faculty/jshambaugh/data.cfm.

Trade: We use data on exports and imports of goods and services as a percentage of GDP from the WDI. The exports and imports series are collected from the World Bank and the OECD national accounts data. These series exclude compensation of employees, investment income, and transfer payments. We use these data to create an

[1] The countries excluded are Afghanistan, Benin, Bosnia and Herzegovina, Central African Republic, Congo-Brazzaville, Democratic Republic of the Congo, Equatorial Guinea, Eritrea, Gabon, Gambia, Guinea, Guinea-Bissau, Haiti, Honduras, Iceland, Iraq, Kuwait, Liberia, Libya, Montenegro, Niger, Panama, Qatar, South Sudan, Tajikistan, Timor Leste, Togo, Trinidad and Tobago, Turkmenistan, Uzbekistan, and Zimbabwe.

Table A.1: Countries in the Sample

Country	Obs.	cont.	
Albania	21		
Algeria	25	Lebanon	23
Angola	20	Lesotho	25
Argentina	25	Lithuania	20
Armenia	19	Luxembourg	25
Australia	25	Macedonia	17
Austria	25	Madagascar	25
Azerbaijan	21	Malawi	23
Bahrain	25	Malaysia	25
Bangladesh	25	Mali	22
Belarus	21	Malta	25
Belgium	25	Mauritania	19
Belize	23	Mauritius	25
Bolivia	24	Mexico	25
Botswana	25	Moldova	20
Brazil	25	Mongolia	22
Brunei	25	Morocco	25
Bulgaria	24	Mozambique	22
Burkina Faso	25	Namibia	23
Burundi	21	Nepal	25
Cambodia	25	Netherlands	25
Cameroon	25	New Zealand	25
Canada	25	Nicaragua	23
Cape Verde	19	Nigeria	25
Chad	16	Norway	25
Chile	25	Oman	25
China	24	Pakistan	25
Colombia	25	Papua New Guinea	25
Côte d'Ivoire	16	Paraguay	24
Croatia	21	Peru	24
Cyprus	25	Philippines	25
Czech Republic	20	Poland	25
Denmark	25	Portugal	25
Djibouti	20	Romania	25
Dominican Republic	25	Russia	21
Ecuador	25	Rwanda	25
Egypt	25	Saudi Arabia	25
El Salvador	25	Senegal	22
Estonia	21	Serbia	16
Ethiopia	23	Seychelles	25
Fiji	25	Sierra Leone	22
Finland	25	Singapore	25
France	25	Slovakia	20
Georgia	17	Slovenia	21
Germany	25	South Africa	25
Ghana	25	Spain	25
Greece	25	Sri Lanka	25
Guatemala	25	Swaziland	25
Guyana	21	Sweden	25
Hungary	25	Switzerland	25
India	25	Syria	22
Indonesia	23	Tanzania	25
Iran	24	Thailand	25
Ireland	25	Tunisia	25
Israel	25	Turkey	25
Italy	25	United Arab Emirates	15
Jamaica	24	Uganda	25
Japan	25	Ukraine	20
Jordan	25	United Kingdom	25
Kazakhstan	20	United States	25
Kenya	25	Uruguay	25
Korea	25	Venezuela	22
Kyrgyzstan	21	Vietnam	16
Laos	20	Yemen	20
Latvia	20	Zambia	18

indicator for open and closed economies. We define an economy as open if its combined exports and imports shares of GDP are greater than 60 percent.

Wars: The war data come from two sources: the Correlates of War project and the UCDP/PRIO *Armed Conflict Dataset*. The war data contain information on participating countries, start and end dates, and the number of battle deaths for each conflict. For more information on the first source, see Sarkees and Wayman (2010). The dataset and its description are available at correlatesofwar.org/data-sets/COW-war. For more information on the second source, see Gleditsch et al. (2002) and prio.org/Data/Armed-Conflict/UCDP-PRIO.

Financial Crises: Annual data on financial crises come from Reinhart and Rogoff (2011). These data are available from 1800 to 2010 for 70 countries and include information on crises pertaining to currency, inflation, stock market, sovereign debt, and banking. We consider a country to be in a financial crisis if it experiences any of these crises. See carmenreinhart.com/data/browse-by-topic/topics/7.

Democracy and Political Risk: The Polity IV Project provides classification of political regimes for 167 countries since 1800. The dataset includes indicators for democracy, autocracy, regime durability, and polity persistency, as well as other component variables related to the executive recruitment processes and the independence of executive authority. The democracy and autocracy indicators range from zero to ten, with a score of ten indicating a full democracy or full autocracy, respectively. These indicators are calculated as the sum of the various authority characteristic component variables. See systemicpeace.org/polityproject.html for more detail. We define a country as an autocracy in a given year if the democracy index is smaller than the autocracy index. According to this definition, a country can have a positive autocracy score and still be considered a democracy. We observe a maximum autocracy score of three among the countries we label as democratic.

We also use annual data on political stability and good governance from the World Bank's *Worldwide Governance Indicators* (WGI). WGI reports governance indicators for over 200 countries since 1996. There are six indicators expressed in standard normal units or percentile ranks: (1) voice and accountability, (2) political stability and absence of violence/terrorism, (3) government effectiveness, (4) regulatory quality, (5) rule of law, and (6) control of corruption. In our robustness checks, we use percentile ranks for political stability and government effectiveness. See info.worldbank.org/governance/wgi/#home. Note that while these two measures are different from those in Miyamoto, Nguyen, and Sheremirov (2019), those data are proprietary and therefore cannot be used freely to compare the two measures.

Arms Imports: We collect annual data on arms trade from the U.S. Department of State's *World Military Expenditures and Arms Transfers*. The data are available from 1964 onward for over 180 countries and provide information on military expenditures, armed forces, arms imports, arms exports, and the number of weapons delivered. See state.gov/t/avc/rls/rpt/wmeat.

Commodities: Data on commodity exports are collected from the United Nations' UNCTAD statistics and *Comtrade* database. The data can be found at unctad.org/en/pages/statistics.aspx and comtrade.un.org, respectively. We also use West Texas Intermediate crude oil prices. Data on giant oil discoveries come from Lei and Michaels (2014), who use data compiled by Myron Horn and the American Association of Petroleum Geologists. These data are available for 193 countries from 1946 through 2008. A giant oil discovery is defined as an oilfield that contained ultimate recoverable reserves of at least 500 million barrels of oil.

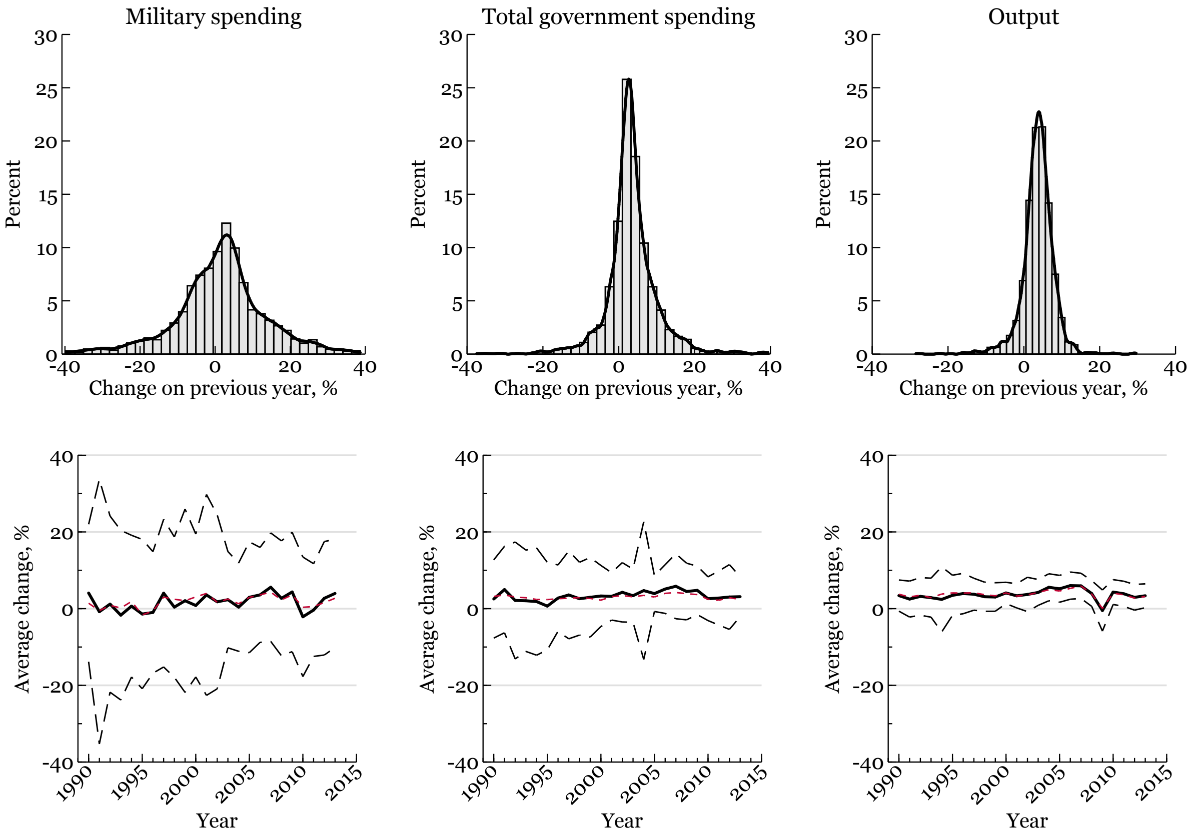
Marginal Tax Rates: The OECD *Central Government Personal Income Tax Rates and Thresholds* dataset provides annual data on marginal income tax rates for 33 member countries for the period 1981–2014. We use the top marginal income tax rate. See oecd.org/tax/tax-policy/tax-database.htm. In addition, we use marginal income tax rates provided by KPMG, an auditor. The KPMG data include tax rates for both advanced and developing countries for the period 2006–2014. While the KPMG data also include tax rates for developed countries, the period covered is shorter than that in the OECD data. See home.kpmg/mm/en/home/services/tax/tax-tools-and-resources/tax-rates-online/individual-income-tax-rates-table.html.

Monetary Policy Rates: We collect end-of-period interest rate data for 75 countries (36 advanced and 39 developing). The data, starting in 1960 for some countries, were obtained from the IMF's *International Financial Statistics* (IFS) and national central banks. See data.imf.org/IFS.

Disaggregated Military Spending: Data on the composition of military spending come from Gartzke (2001) and NATO. Gartzke's data are available from 1950 through 1997 for 99 countries, but the coverage is incomplete. The data are split into capital and operating costs in constant U.S. dollars, which proxy for durable and nondurable spending. The data come from four sources: (1) NATO press releases, (2) *The UN Report on Military Expenditures*, (3) *SIPRI Yearbook*, and (4) Ball (1988). For several countries, there are two observations for the same year. In such cases, our preferred sources are SIPRI and NATO, and our third preferred source is the United Nations. Because the Gartzke (2001) data end in 1997, we supplement them with those from NATO for the period 1998–2013, using his method. See nato.int/cps/en/natohq/topics_49198.htm for data details. To calculate durable and nondurable spending in constant 2005 prices in local currency units, we multiply the durable and nondurable shares by total military spending. The resulting series covers 53 countries, mostly NATO members.

B Additional Results

Figure B.1: Time and Space Distributions of Main Variables



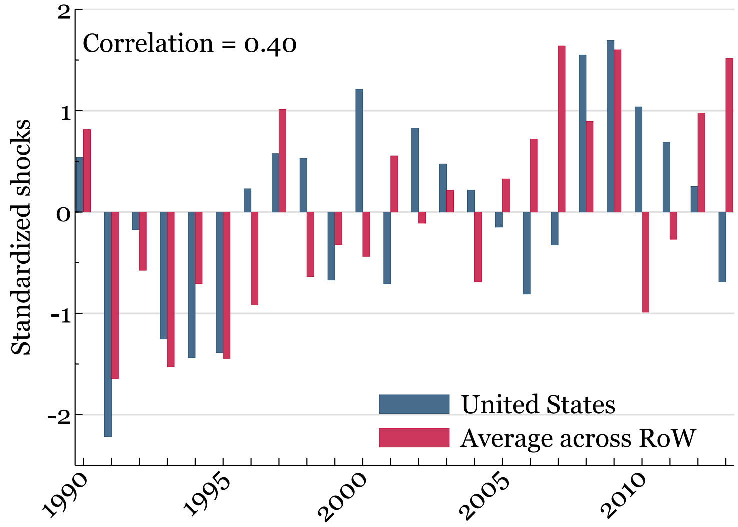
Notes: The top three panels show the distribution of changes, in percent, in military spending, total government spending, and output, respectively. For readability, changes over 40 percent in absolute value are cut off. The bottom three panels show the averages (black solid line) and medians (red broken line, virtually indistinguishable) of corresponding changes by year. The dashed lines depict one standard deviation around the averages.

Table B.1: Distribution of $|\Delta \ln g^{\mathrm{m}}|$ over Countries

	Average year (1)	Median year (2)
Mean	0.115	0.081
Standard deviation	0.063	0.044
Minimum	0.028	0.018
Maximum	0.425	0.250
Percentiles:		
10th	0.044	0.035
25th	0.069	0.050
50th	0.104	0.071
75th	0.148	0.103
90th	0.192	0.136
United States	0.053	0.044
Larger than U.S. share	0.853	0.814

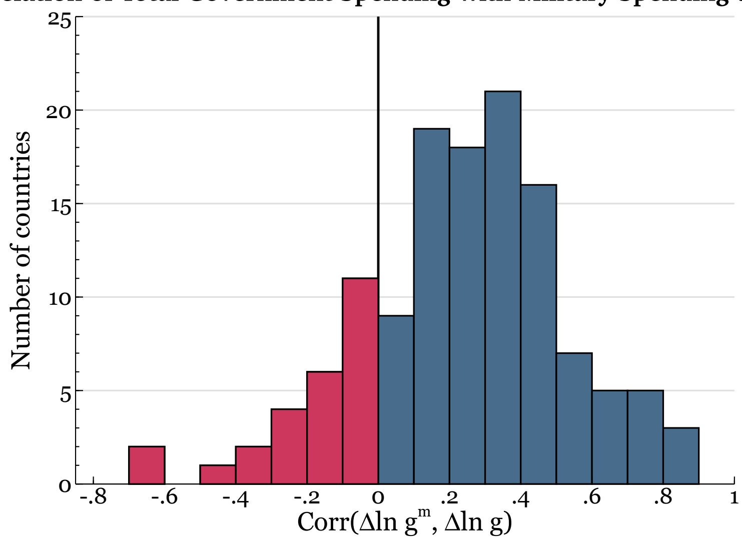
Notes: We compute annual log-changes in military spending, $\Delta \ln g^{\mathrm{m}}_{i,t} = \ln g^{\mathrm{m}}_{i,t} - \ln g^{\mathrm{m}}_{i,t-1}$ and then, in column (1), report the mean, standard deviation, and percentiles of its average (over time) absolute value, $|\Delta \ln g^{\mathrm{m}}_{i}| = \sum_t |\Delta \ln g^{\mathrm{m}}_{i,t}|/T$, across countries ($i$). In column (2), we report the moments and percentiles for medians rather than averages over time. The penultimate row reports the corresponding measure for the U.S., and the last row reports the share of countries (between 0 and 1) with $|\Delta \ln g^{\mathrm{m}}_{i}| > |\Delta \ln g^{\mathrm{m}}_{\mathrm{U.S.}}|$.

Figure B.2: Comovement of Military Spending Shocks in the United States with the Rest of the World (RoW)



Notes: The figure depicts military spending shocks obtained as the fitted values from the regression of the log-difference of total government spending on the log-difference of military spending. To enhance visibility, the shocks are standardized (demeaned and divided by the standard deviation over time).

Figure B.3: Correlation of Total Government Spending with Military Spending across Countries



Notes: For each country i, we compute time correlation of log changes in military spending and log changes in total government spending, $\mathbb{C}\text{orr}_i(\Delta \ln g^{\text{m}}_{i,t}, \Delta \ln g_{i,t})$. This figure shows the histogram of $\mathbb{C}\text{orr}_i$ over i. The blue and red colors indicate positive and negative values of $\mathbb{C}\text{orr}_i$, respectively. In 103 out of 129 countries, the sample correlation is positive.

Table B.2: Sensitivity to the Number of Lags

	One lag (1)	Two lags (2)	Three lags (3)	Four lags (4)
One year	0.643	0.725**	0.841***	0.833**
	(0.399)	(0.356)	(0.322)	(0.330)
	[6.5]	[6.3]	[27.6]	[23.0]
Two years	0.861*	0.832**	0.987**	0.879**
	(0.511)	(0.401)	(0.390)	(0.366)
	[10.6]	[9.4]	[37.8]	[39.7]
Three years	0.616	0.588	0.825**	0.649*
	(0.512)	(0.378)	(0.420)	(0.390)
	[15.5]	[12.2]	[38.4]	[36.0]
Four years	0.675	0.665	0.843*	0.656
	(0.611)	(0.444)	(0.487)	(0.452)
	[10.2]	[8.2]	[32.3]	[30.5]
Five years	0.474	0.466	0.750	0.509
	(0.619)	(0.436)	(0.555)	(0.501)
	[15.1]	[12.1]	[25.0]	[22.9]
Obs.	2,989	2,843	2,701	2,563

Notes: This table presents multipliers for alternative numbers of lags; the baseline includes four lags. HAC standard errors are in parentheses. First-stage effective F-statistics are in brackets. The war dummy, country fixed effects, and time effects are included. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table B.3: Alternative Normalization: Lag Output

Fixed effects	No (1)	Country (C) (2)	Time (T) (3)	C–T (4)
One year	0.790**	0.795**	0.863**	0.889**
	(0.340)	(0.383)	(0.350)	(0.396)
	[28.4]	[23.6]	[28.5]	[23.5]
Two years	0.877**	0.881*	0.952**	0.990**
	(0.413)	(0.472)	(0.422)	(0.482)
	[45.4]	[35.5]	[45.9]	[36.0]
Three years	0.947*	0.963	1.042**	1.102*
	(0.507)	(0.603)	(0.520)	(0.620)
	[51.8]	[35.8]	[52.6]	[36.5]
Four years	1.051*	1.156	1.160**	1.359*
	(0.567)	(0.721)	(0.581)	(0.737)
	[48.3]	[31.0]	[49.7]	[32.3]
Five years	1.045	1.073	1.157*	1.354
	(0.648)	(0.909)	(0.657)	(0.912)
	[42.3]	[22.8]	[43.5]	[24.1]

Notes: This table presents multipliers based on an alternative variable transformation. In this exercise, we normalize output, government spending, and military spending by lag output, as opposed to trend output. HAC standard errors are in parentheses. First-stage effective F-statistics are in brackets. The war dummy, country fixed effects, and time effects are included. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table B.4: Alternative Standard Errors

	One year (1)	Two years (2)	Three years (3)	Four years (4)	Five years (5)
Cumulative multiplier	0.833	0.879	0.649	0.656	0.509
Errors:					
Classical	(0.122)***	(0.160)***	(0.220)***	(0.274)**	(0.337)
Robust	(0.322)***	(0.341)**	(0.377)*	(0.436)	(0.499)
Heteroskedastic and autocorrelated (HAC)	(0.330)**	(0.366)**	(0.390)*	(0.452)	(0.501)
Clustered by country	(0.380)**	(0.399)**	(0.409)	(0.496)	(0.540)
Driscoll and Kraay (1998)	(0.406)*	(0.440)*	(0.468)	(0.521)	(0.551)
Two-way clustered by country and year	(0.419)*	(0.472)*	(0.500)	(0.594)	(0.663)

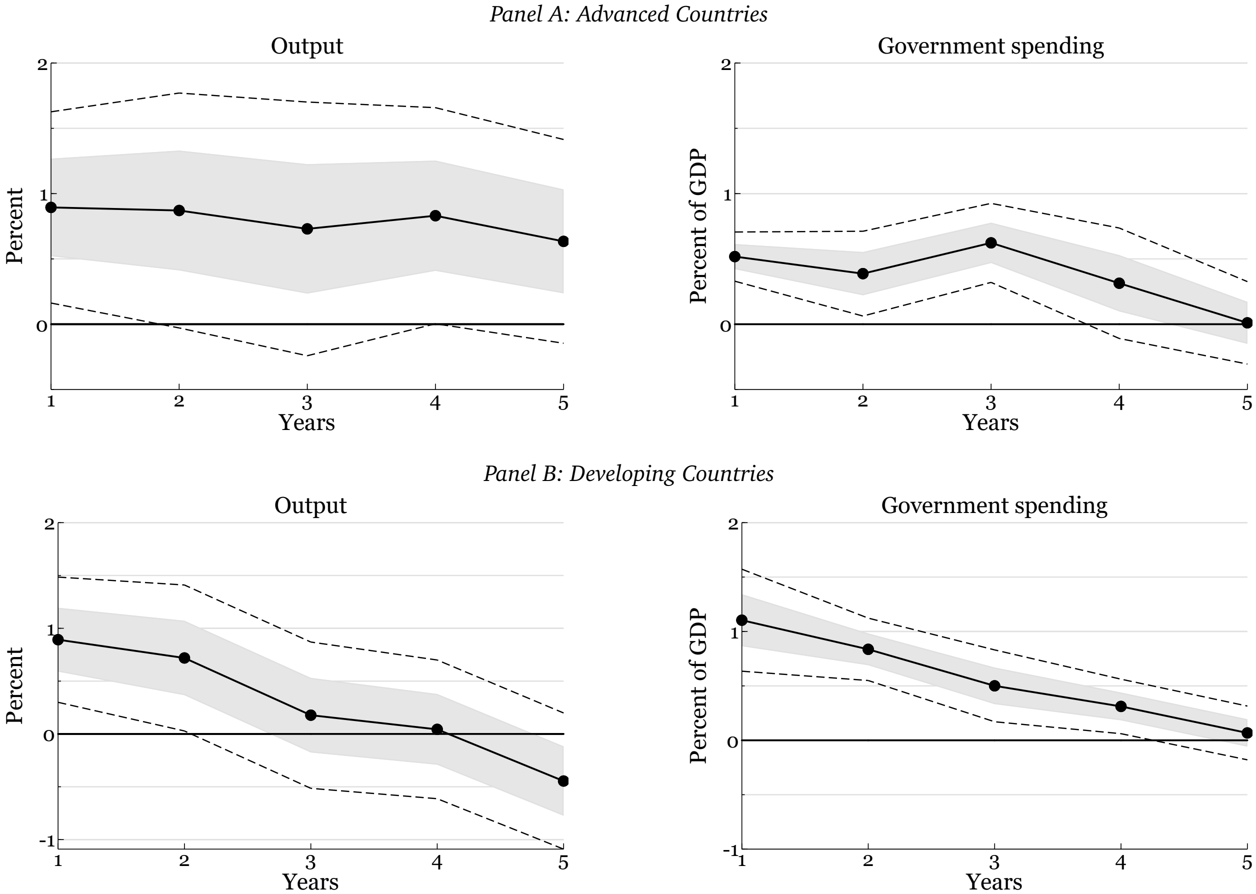
Notes: This table presents the sensitivity of standard-errors estimates; the baseline uses HAC standard errors. The war dummy, country fixed effects, and time effects are included. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table B.5: Breakdown of Developing Sample

	High income	Less developed		
		All	Middle income	Low income
	(1)	(2)	(3)	(4)
One year	2.022**	0.802**	1.885***	0.406
	(0.938)	(0.348)	(0.715)	(0.388)
	[4.7]	[29.0]	[19.7]	[41.5]
Two years	1.778**	0.809**	1.886**	0.275
	(0.738)	(0.374)	(0.747)	(0.438)
	[17.4]	[44.9]	[15.1]	[53.6]
Three years	0.072	0.676*	1.575**	0.086
	(1.469)	(0.395)	(0.674)	(0.547)
	[3.6]	[47.5]	[14.9]	[39.9]
Obs.	701	1,861	1,293	566

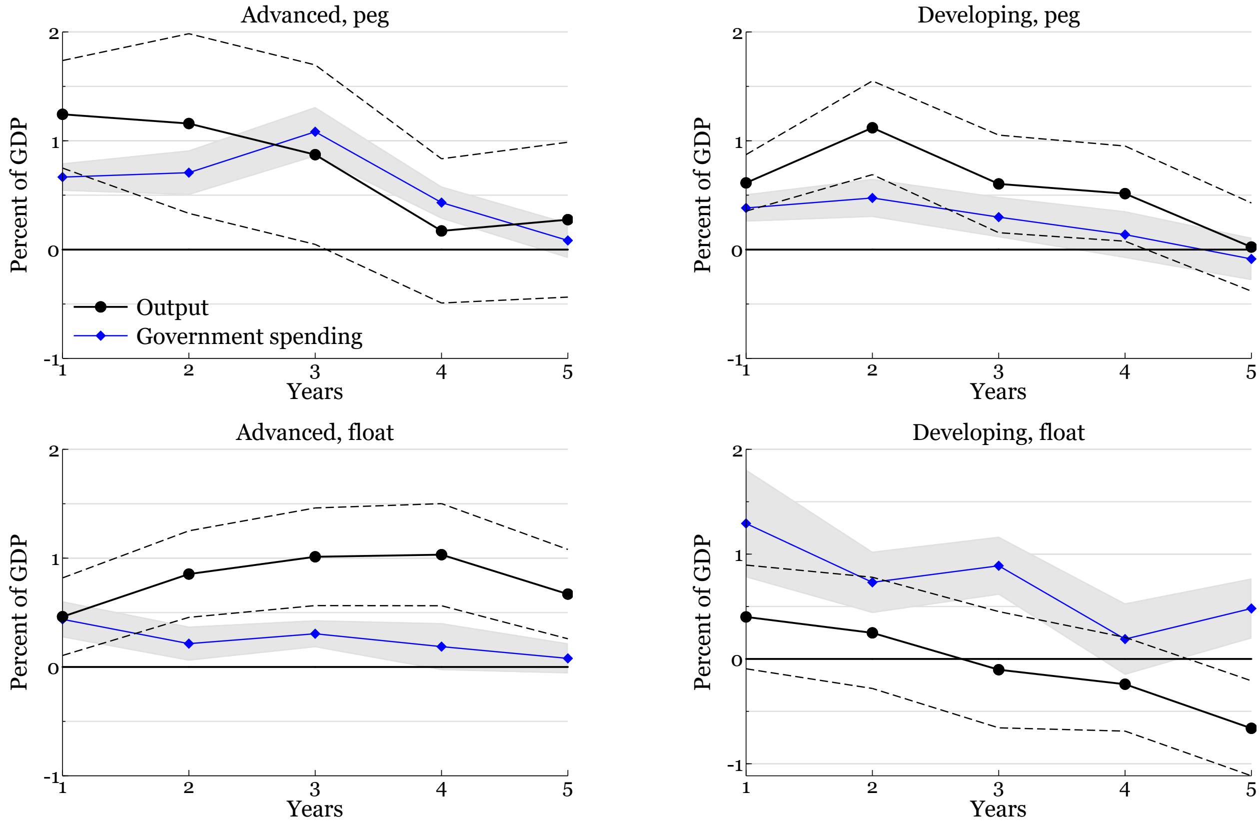
Notes: This table uses the World Bank development classification, based on gross national income, which allows breaking down the developing sample further. Column (1) shows the results for high-income (advanced) countries, and column (2) for less developed countries (low- and middle-income countries together). Column (3) shows the results for upper-middle- and lower-middle-income countries (called middle-income for our purposes), while column (4) shows results for low-income countries.

Figure B.4: IRFs to Military-Spending Shock in Advanced and Developing Countries



Notes: The figure presents the impulse-response function (IRF) separately for advanced and developing countries. See notes to Figure 2.

Figure B.5: IRFs to Military-Spending Shock by Development–Exchange Regime Sample



Notes: The figure presents the IRF by sample. The bands show one standard deviation around the estimates.

Table B.6: Multipliers during Slack Episodes: Alternative Thresholds

	Unemployment threshold: percentile					
	50th		75th		90th	
	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
One year	1.453***	0.771*	1.412**	0.893***	1.686**	0.754**
	(0.463)	(0.443)	(0.583)	(0.339)	(0.819)	(0.309)
	[27.4]	[15.3]	[15.0]	[23.0]	[9.2]	[27.1]
Two years	1.220**	0.958*	0.974	1.077***	1.205	0.846**
	(0.505)	(0.490)	(0.608)	(0.380)	(0.801)	(0.357)
	[16.5]	[59.8]	[13.0]	[52.9]	[8.0]	[48.4]
Three years	0.819	0.772	0.348	1.072**	0.671	0.672
	(0.515)	(0.733)	(0.671)	(0.507)	(0.831)	(0.445)
	[12.5]	[19.5]	[13.0]	[29.6]	[7.5]	[28.2]
Obs.	1,306	1,218	659	1,865	325	2,199

Notes: The tables presents multipliers for alternative definitions of slack. The first two columns are the baseline, defined as the unemployment rate above the country's median. The other four columns experiment with larger thresholds.

Table B.7: Multipliers and Wars

	Military threat indicator (1)	Excluding countries with long civil wars (2)	Excluding countries with any war incidence (3)
One year	0.779**	0.667**	1.068***
	(0.338)	(0.308)	(0.404)
	[19.6]	[16.0]	[48.7]
Two years	0.798**	0.728**	1.034***
	(0.359)	(0.355)	(0.393)
	[38.8]	[32.3]	[33.4]
Three years	0.529	0.478	0.836**
	(0.376)	(0.372)	(0.366)
	[37.5]	[32.0]	[30.5]
Obs.	2,563	2,175	1,377

Notes: In column (1), we control for a military-threat indicator based on the incidence of war in a narrowly defined geographical region. In column (2), we exclude countries with long civil wars (fought on domestic soil). In column (3), we exclude countries with any incidence of war during the sample period.

Table B.8: Large Oil Exporters and Discoveries

	No Large Oil Exporters			No Countries with Giant Oil Discoveries		
	All	Adv.	Dev.	All	Adv.	Dev.
	(1)	(2)	(3)	(4)	(5)	(6)
One year	0.792**	1.569***	0.775**	1.315***	1.794***	1.270***
	(0.345)	(0.570)	(0.351)	(0.449)	(0.632)	(0.492)
	[31.0]	[36.0]	[33.7]	[22.9]	[32.5]	[18.8]
Two years	0.768**	1.693**	0.728*	1.228***	1.921**	1.167**
	(0.372)	(0.740)	(0.377)	(0.436)	(0.868)	(0.472)
	[50.4]	[25.2]	[57.8]	[36.8]	[20.1]	[36.5]
Three years	0.597	1.301*	0.546	0.905**	1.589*	0.834*
	(0.390)	(0.695)	(0.409)	(0.440)	(0.832)	(0.474)
	[53.0]	[19.5]	[52.8]	[34.8]	[16.0]	[35.9]
Obs.	2,407	752	1,655	1,780	519	1,261

Notes: In columns (1) through (3), we exclude countries with a share of oil exports in total exports above 15 percent of GDP: Algeria, Chile, Mauritania, Nigeria, Oman, Russia, Saudi Arabia, United Arab Emirates, and Zambia. In columns (4) through (6), we drop countries with at least one giant oil discovery in the sample.

Table B.9: Relative Size of Military Spending

	Baseline	Share of military spending > 5%	Share of military spending > 10%
	(1)	(2)	(3)
One year	0.833**	0.864***	0.773**
	(0.330)	(0.329)	(0.318)
	[23.0]	[23.9]	[24.7]
Two years	0.879**	0.917**	0.799**
	(0.366)	(0.363)	(0.349)
	[39.7]	[40.0]	[42.6]
Three years	0.649*	0.697*	0.495
	(0.390)	(0.388)	(0.373)
	[36.0]	[34.2]	[34.5]
Four years	0.656	0.702	0.470
	(0.452)	(0.450)	(0.442)
	[30.5]	[28.6]	[27.6]
Five years	0.509	0.585	0.308
	(0.501)	(0.496)	(0.495)
	[22.9]	[22.1]	[20.3]
Obs.	2,563	2,354	1,612

Notes: This table reproduces the baseline results for the cases when we drop countries with an average share of military spending in total government spending below 5 percent (column 2) or below 10 percent (column 3).

Table B.10: Excluding Countries with $\mathbb{C}\text{orr}(\Delta \ln g^{\mathbf{m}}, \Delta \ln g) \leq 0$

Fixed effects	No	Country (C)	Time (T)	C–T
	(1)	(2)	(3)	(4)
One year	0.702***	0.715**	0.759***	0.785**
	(0.266)	(0.294)	(0.271)	(0.305)
	[30.5]	[26.1]	[30.4]	[26.2]
Two years	0.736***	0.713**	0.785***	0.810**
	(0.278)	(0.324)	(0.280)	(0.334)
	[59.1]	[48.0]	[58.7]	[49.0]
Three years	0.557**	0.412	0.622**	0.598*
	(0.271)	(0.335)	(0.272)	(0.349)
	[68.6]	[48.4]	[67.2]	[50.6]
Four years	0.550*	0.378	0.633**	0.661
	(0.283)	(0.396)	(0.285)	(0.412)
	[62.6]	[39.0]	[61.9]	[42.6]
Five years	0.453	0.217	0.548*	0.578
	(0.295)	(0.453)	(0.292)	(0.459)
	[52.9]	[27.4]	[52.4]	[30.9]
Obs.	2,022	2,022	2,022	2,022

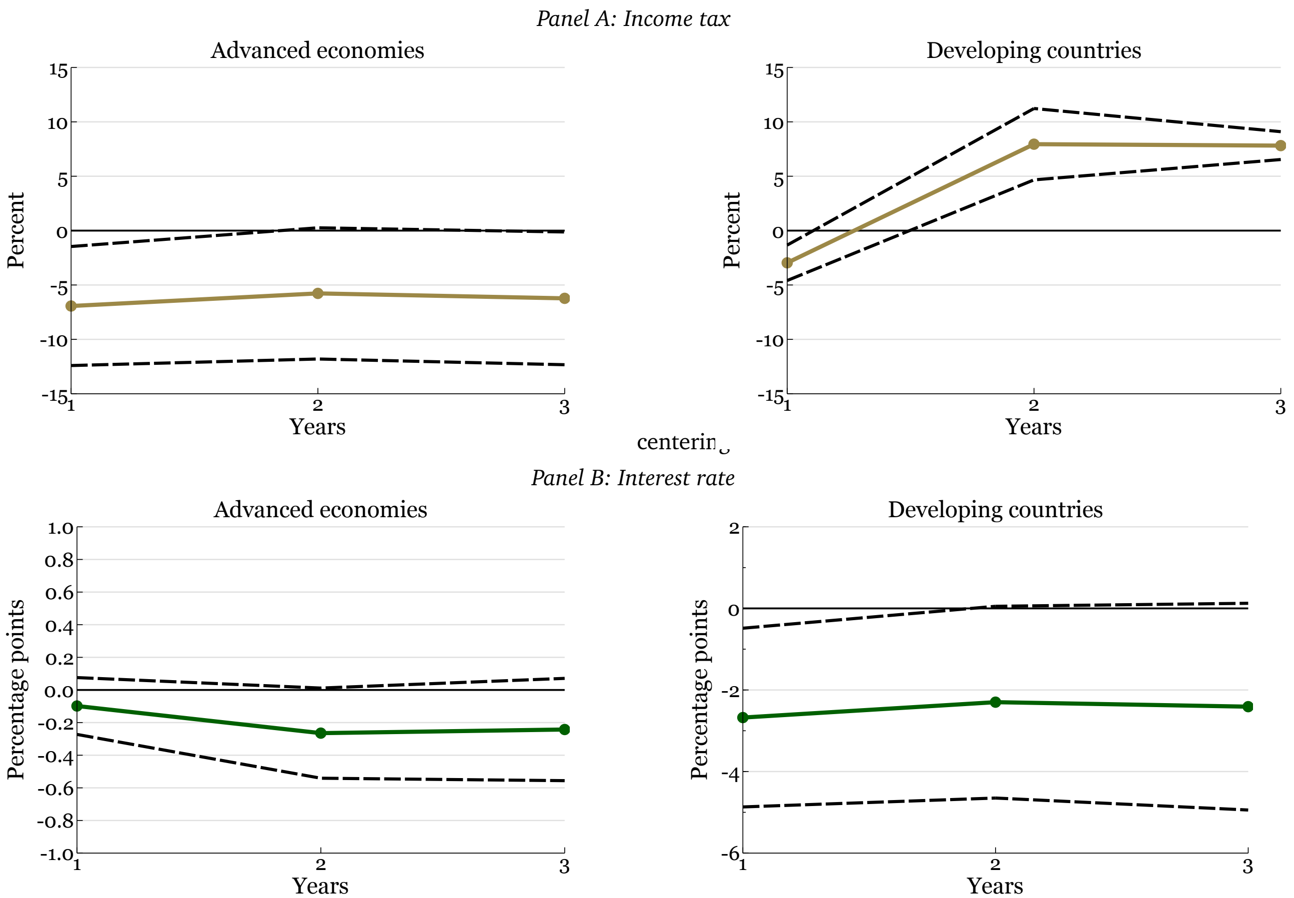
Notes: To construct this table, we drop countries with a negative sample correlation of changes in military spending and changes in total government spending. The distribution of these correlations across countries can be found in Figure B.3.

Table B.11: Ilzetzki et al. Sample

	All	Adv.	Dev.
	(1)	(2)	(3)
One year	2.179**	2.155	2.519*
	(1.002)	(1.434)	(1.463)
	[40.0]	[27.3]	[14.7]
Two years	2.050**	2.654*	1.733
	(1.018)	(1.570)	(1.350)
	[33.2]	[18.8]	[18.2]
Three years	1.991*	2.633	1.720
	(1.128)	(1.628)	(1.518)
	[19.7]	[10.3]	[11.7]
Obs.	823	524	299

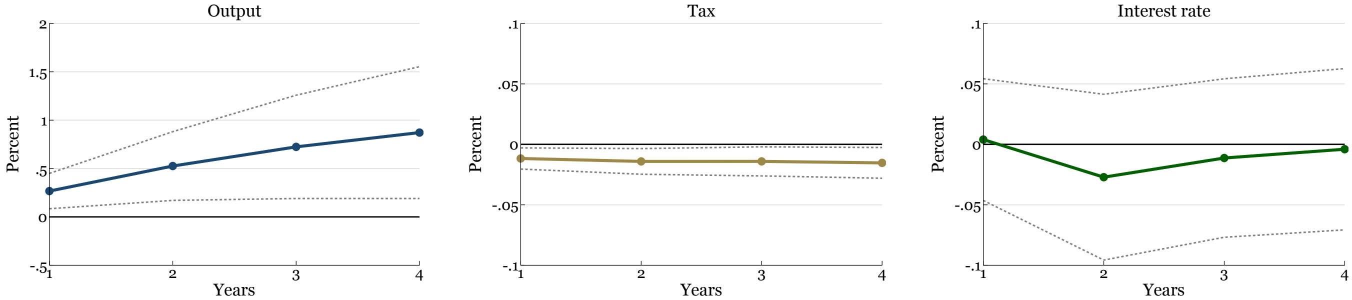
Notes: This table presents multipliers estimated in the sample restricted to countries considered in Ilzetzki, Mendoza, and Végh (2013), except Iceland (not in our baseline sample). The data for Australia, France, the United Kingdom, and the United States start in 1988.

Figure B.6: Income Tax and Interest Rate Response to Government-Spending Shock by Country Sample



Notes: The figure reproduces Figure 3 in the text, splitting the country sample into developed and developing countries. The dashed lines represent one standard deviation bands.

Figure B.7: IRFs to Military-Spending Shocks: VAR Estimates



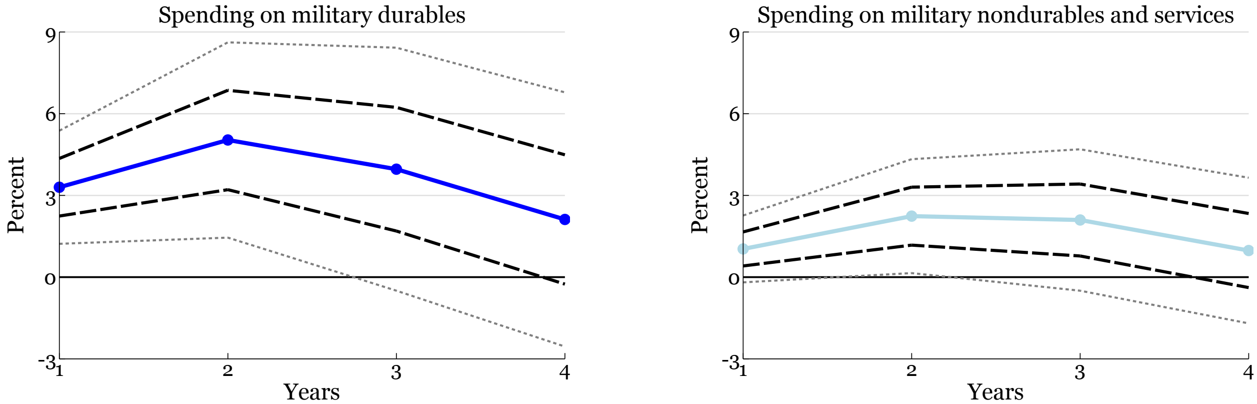
Notes: The figure presents impulse responses of output, marginal income tax rates, and policy rates to a military spending shock of 1 percent of GDP, estimated from a vector autoregression (VAR) with two lags. The system is identified using Cholesky decomposition, with military spending ordered last and output first (i.e., military spending does not respond to any variable within a year). Dotted lines represent 95 percent confidence bands from Monte Carlo simulations.

Table B.12: Multipliers in Subsamples with Extra Data

Fixed effects	No (1)	Country (C) (2)	Time (T) (3)	C–T (4)
	Panel A: Tax-rate sample			
One year	1.296***	1.376***	1.404***	1.509***
	(0.298)	(0.301)	(0.422)	(0.391)
	[9.2]	[8.5]	[9.0]	[8.1]
Two years	1.441***	1.353***	1.434***	1.421***
	(0.268)	(0.298)	(0.367)	(0.399)
	[21.2]	[17.2]	[21.0]	[18.0]
Three years	0.840***	0.639**	0.762**	0.739**
	(0.282)	(0.286)	(0.315)	(0.311)
	[20.5]	[38.3]	[19.3]	[38.8]
Obs.	1,009	1,008	1,009	1,008
	Panel B: Policy-rate sample			
One year	1.829**	1.930***	2.342***	2.382***
	(0.717)	(0.690)	(0.903)	(0.803)
	[10.2]	[9.7]	[8.8]	[8.8]
Two years	1.799*	2.080*	2.856*	3.099**
	(0.987)	(1.110)	(1.469)	(1.420)
	[4.8]	[3.5]	[3.9]	[3.3]
Three years	1.263	1.659	2.591	2.982*
	(1.058)	(1.260)	(1.697)	(1.583)
	[3.6]	[2.3]	[2.8]	[2.4]
Obs.	1,355	1,354	1,355	1,354

Notes: This table presents estimates of the baseline specification for two subsamples: one for which tax-rate data are available (Panel A) and the other for the countries with policy-rate data (Panel B).

Figure B.8: Output Response to Military Spending



Notes: The left panel shows the output response, at horizon h, to military spending on durables, estimated using the local projections method. The right panel shows the corresponding output response to military spending on nondurables and services. The dashed lines represent one-standard-deviation bands, and the dotted lines show 95 percent confidence intervals.