Why Do Government Spending Multipliers Differ? A Meta-Analysis*

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First draft, very preliminary and incomplete, comments welcome

Abstract

The vast literature that estimates the effects of government spending on output has not come to consensus yet. The relatively big multiplier estimated for the US is not found for other countries: a much lower or no effect is usually reported. We collect more than 800 estimates of the dynamic effects of government spending shocks from a sample of published and unpublished studies and provide the average effect implied by the literature. Next, we exploit the differences between estimates by relating them to the two sources of heterogeneity. First, we test whether there is a systematic influence of different study characteristics such as the type of identification or data characteristics. Second, we investigate how the estimates vary with differences in structural characteristics such as the level of government debt, the openness and the size of the economy, or the level of financial development. Our results suggest that the spending multipliers systematically depend on the characteristics of the economy, while the differences in study design play a less important role.

Keywords: Fiscal policy transmission; Meta-analysis

JEL Codes: C83; E62

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"One clear message emerges from (this) vast literature: estimates of multipliers are all over the map, providing empirical support for virtually any policy conclusion. The diversity of findings, often based on the same U.S. time series data, highlights the difficulties in obtaining reliable estimates of fiscal effects and points to the need for systematic analysis that confronts fiscal policy's complexities." (Leeper, 2010, Monetary Science, Fiscal Alchemy, pp. 23)

1 Introduction

To fight the global financial crisis, governments around the world adopted large fiscal stimulus packages (see Table 1). Against this background, it is of utmost importance to understand the effects that government spending has on the economy, especially for the policymakers deciding on the size of the countercyclical fiscal measures. The empirical evidence on the effects of the government spending shocks is extensive, yet policymakers rarely justify their actions by reference to such estimates.

Table 1: Fiscal packages in selected countries 2008-2010

	Net effect on fiscal balance (% of GDP)						
	US	UK	Japan	Australia	Canada	Germany	Czech Republic
Spending	-2.4	0.0	-1.5	-3.3	-1.7	-1.4	-0.5
Tax revenues	-3.2	-1.5	-0.5	-1.3	-2.4	-1.6	-2.5
Total	-5.6	-1.5	-2.0	-4.6	-4.1	-3.0	-3.0

Source: OECD

This is not surprising as the consensus on the size of the output multipliers is virtually non-existent. There are at least two distinct sources for the differences in the estimated coefficients. First, the multipliers might differ due to structural features of the different economies. The openness of the economy, the indebtedness of the country, the development of its financial markets, and the prevailing interest rates likely have the effect on the size of the multipliers.

Yet, even if there were no structural heterogeneity between countries, there are different methodological approaches to the identification of the effects of government spending shocks, and these method differences may result in the differences in estimated multipliers. The identification strategies range from assuming simple recursive scheme (Fatás & Mihov, 2001), through using institutional information and the assumption of decision lags (Blanchard & Perotti, 2002), to imposing sign restrictions on the responses of the shock (Mountford & Uhlig, 2009). Alternatively, the event study approach exploits the information about the exogenous increases in military spending to ensure that the shocks are not anticipated (Ramey & Shapiro, 1998; Ramey, 2011). Further, often overlooked importance of controlling for the government debt dynamics was brought to attention lately (Chung & Leeper, 2007; Favero & Giavazzi, 2007).

What have we learned about the effects of government spending shocks estimated within VAR framework? We attempt to shed more light on the question by reviewing the literature quantitatively, using the tools of meta-analysis. Meta-analysis is a standard method of summarizing empirical evidence in medicine and has recently been applied in economics as well

(Stanley, 2001; Disdier & Head, 2008; Card *et al.*, 2010). The existing studies surveying the fiscal multipliers literature (Hemming *et al.*, 2002; Spilimbergo *et al.*, 2009; Hebous, 2011) adopt a narrative approach, which might be more vulnerable to subjective choice of studies and their results. In such literature surveys, the weights assigned to the results of the studies and the explanation of the differences between the results is typically done without any formal testing. In contrast, meta-analysis use a battery of statistical tools and methods to explain the variation in the results and provide the average effect while controlling for methodological differences.

Recently, two studies (Cogan *et al.*, 2010; Coenen *et al.*, 2011) compared the estimates of fiscal multipliers coming from a wide range of theoretical models. The theoretical models are sometimes preferred as they are naturally more informative about the functioning of the economy as opposed to data-driven reduced-form estimates coming from VAR studies that use minimal identifying assumptions. Unfortunately, the results of these two studies stand in stark contrast to each other. This highlights the potential drawbacks of using models that might be too sensitive to the assumptions and calibration (e.g. about consumer behavior and parameters of the fiscal policy rules). Although results from theoretical models potentially possess a lot of information, they are less suitable for meta-analysis as they typically do not contain any measure of the uncertainty associated with the estimates.

Our approach, that attributes part of the differences in the size of the multipliers to the structural characteristics of the economy, is related to other similar studies (Kirchner *et al.*, 2010; Ilzetzki *et al.*, 2011). These studies link the size of the spending multipliers to the openness of the economy, government debt, government investment, or financial fragility. The results, however, might be sensitive to the recursive identification assumption they use. The advantage of the meta-analytical approach we adopt is that we control for the type of the identification as well as for the other data and method characteristics.

The contribution of this paper is twofold. First, to our best knowledge, this is the first meta-analysis on the effects of government spending on output estimated within a vector autoregressions framework. We systematically review the estimates of fiscal multipliers, and provide the average effect implied by the literature. Second, we relate the sources of differences in multipliers to the structural and method heterogeneity. Our results indicate that the variation in the size of the estimated multipliers is mainly due to structural characteristics such as debt to GDP ratio, openness of the economy or the average level of interest rates. Estimation characteristics seem not to have systematic effect on the estimates.

The remainder paper is structured as follows. In Section 2 we describe the construction of the sample of fiscal multipliers. Section 3 presents the descriptive statistics of the sample, while Section 4 provides the preliminary evidence on the sources of variation in the estimates of the multipliers. Section 5 discusses further issues.

2 Assembling the Sample of the Fiscal Multipliers

Researchers typically study the dynamic effects of government spending shocks within the vector autoregression model. The underlying structural model is of the following form:

$$AY_t = B(L)Y_{t-1} + \varepsilon_t, \tag{1}$$

where Y_t is a set of endogenous variables typically consisting of tax revenues, government spending and GDP, all in real, per capita, terms. This minimal set of variables is often extended by additional variables such as consumption, inflation, and interest rate. Matrix A captures the contemporaneous relationships, while matrix polynomial $B(L)$ describes the dynamic relationships between endogenous variables, and ε_t is a vector of orthogonal structural shocks.

Due to simultaneity, following reduced form model is usually estimated:

$$Y_t = C(L)Y_{t-1} + u_t, \tag{2}$$

where $C(L)$ is a combination of the elements of A and $B(L)$ matrices and u_t is a vector of reduced form residuals. The relationship between structural and reduced form shocks is $\varepsilon_t = Au_t$, where the matrix A plays key role for the identification of the government spending shocks. Obviously, there are many different combinations of structural shocks that might generate the same reduced form residuals. Therefore, one needs to impose restrictions on A. For example, Blanchard & Perotti (2002) consider the reduced form residuals to be a linear combination of three distinct shocks - the automatic response of spending, the systematic discretionary response and the structural government spending shocks - and set the restrictions on A accordingly.

Once the shocks are identified, each endogenous variable can be rewritten into the moving average form, and the dynamic responses to the structural shocks can be thus obtained. These are usually presented in the form of impulse responses.

Fiscal multiplier is the common measure of the effect size typically reported in the literature on the government spending and tax shocks. The multiplier at horizon h after the government spending shock at time t is defined as

$$Multiplier(h) = \frac{\Delta Y(t+h)}{\Delta G(t)},$$

where $\Delta Y(t+h)$ denotes the change in output at horizon h and $\Delta G(t)$ the size of the government spending shock.[1] In order to calculate the multiplier, the impulse response are first normalized by the initial size of the shock (so that the shock is of size 1%) and then they are divided by the ratio of the responding variable and the shocked fiscal variable. For example, the output multiplier can thus be interpreted as the dollar increase in GDP that is due one dollar increase in spending.

Although, the impulse responses report the dynamic effects of the variables for many hori-

[1] Note that the most appropriate measure would be the cumulative multiplier, however, only small fraction of studies report it.

zons, typically only some horizons are discussed. We follow this convention, and gather the responses at impact, 1-, 2-, 3-, and 5-year horizons. In addition to that, we collect also the response at the peak.

To assemble the sample of the fiscal multipliers, we start by investigating the papers which cite Blanchard & Perotti (2002), the most widely cited study in the fiscal shocks literature. In addition to that, we search EconLit database. So far, we have collected 27 studies that contain 135 estimates of the impulse responses. The evidence was produced by 50 distinct authors for 20 countries. The current sample of studies receives approximately 1150 citations per year in Google Scholar, illustrating the fact that the fiscal multipliers are relatively topical area of research. The last study was added on 15 July 2011.

Table 2: Primary Studies Included in the Meta-Analysis

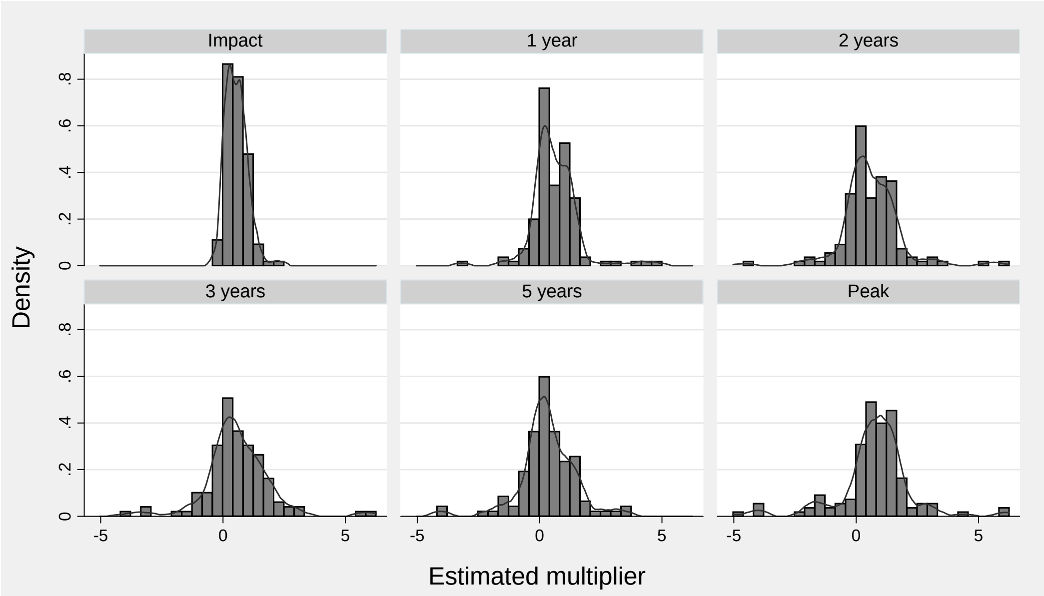
Study	Outlet	Estimates	Country
Ramey & Shapiro (1998)	Carnegie-Rochester Conference Series on Public Policy	1	US
Fatás & Mihov (2001)	CEPR DP	1	US
Blanchard & Perotti (2002)	Quarterly Journal of Economics	2	US
Perotti (2004)	IGIER WP	15	Australia, Canada, Germany, UK, US
Giuliodori & Beetsma (2005)	De Economist	3	France, Germany, Italy
Badinger (2006)	Empirica	1	Austria
Claus *et al.* (2006)	New Zealand Treasury WP	5	New Zealand
de Castro & de Cos (2006)	ECB WP	1	Spain
Wolff *et al.* (2006)	Deutsche Bundesbank Discussion Paper	1	Germany
Stikova (2006)	manuscript/IDP CNB	2	Czech Republic
Chung & Leeper (2007)	NBER WP	1	US
Favero & Giavazzi (2007)	NBER WP	4	US
Galí *et al.* (2007)	Journal of European Economic Association	1	US
Giordano *et al.* (2007)	European Journal of Political Economy	2	Italy
Perotti (2007)	NBER Macroeconomics Annual	42	Australia, Canada, UK, US
Ravn *et al.* (2007)	NBER WP	2	US
Beetsma *et al.* (2008)	Journal of European Economic Association	1	European Union
Bilbiie *et al.* (2008)	Journal of Money, Credit and Banking	2	US
Caldara & Kamps (2008)	ECB WP	12	US
Benetrix & Lane (2009)	The Economic and Social Review	5	Ireland
Mirdala (2009)	Journal of Applied Research in Finance	12	Bulgary, Czech Republic, Hungary, Poland, Slovakia, Romania
Mountford & Uhlig (2009)	Journal of Applied Econometrics	2	US
Kirchner *et al.* (2010)	ECB WP	1	Euro Area
Monacelli & Perotti (2010)	Economic Journal	4	Australia, Canada, UK, US
Pappa (2010)	manuscript	5	Canada, Euro Area, Japan, UK, US
Afonso & Sousa (2011)	Portuguese Economic Journal	2	Portugal
Ramey (2011)	Quarterly Journal of Economics	5	US
	Studies	Estimates	Countries
Total	27	135	20

Note: As of 15 July 2011.

3 Descriptive Analysis

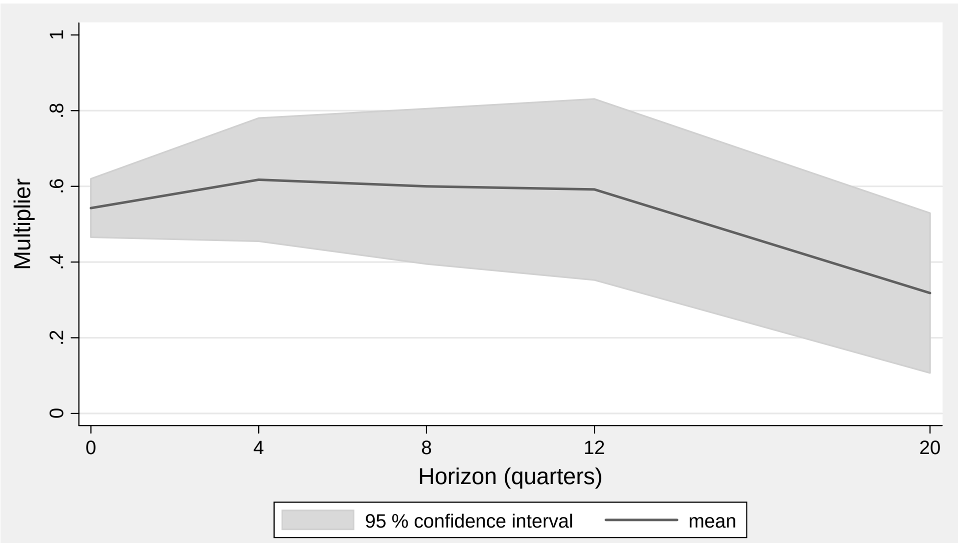
We start the descriptive analysis by looking at the histograms of the estimated multipliers at different horizons. As can be seen from the Figure 1 the estimated multipliers are quite dispersed, and range from negative to positive values.

Figure 1: Histograms of the estimated multipliers.



The next step is to compute a simple average for each horizon. Figure 2 implies that the average effect is positive, reaching value 0.6 at the peak after a year.

Figure 2: The average multiplier implied by the literature



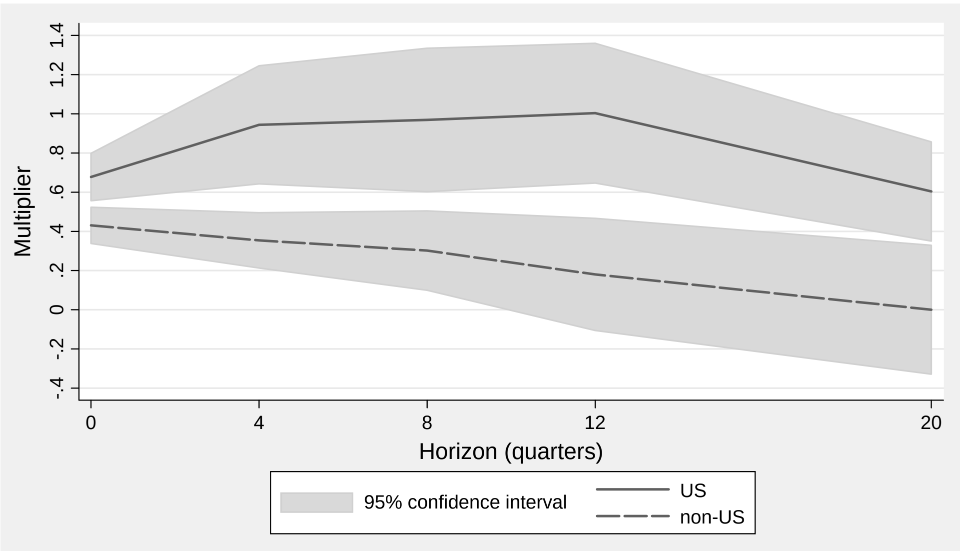
To the extent that the underlying size of the multiplier differ across countries due to structural characteristics, and to the extent that different identification strategies might systematically affect the estimated values of multipliers, the simple average might present a distorted picture about the true value of multipliers. Before we properly address these issues in our explanatory meta-regression in Section 4, we also look at the averages at more disaggregate levels. Two observations emerge. First, it seems there are no systematic differences in estimated multipliers across different identification schemes (Table 3). Second, the estimates for the US are systematically larger than those of other countries (Figure 3).

Table 3: The average effect does not vary across different identification approaches.

Identification	Impact	1 year	2 years	3 years	5 years	Peak	No. of Estimates	% of Total
Recursive	0.60	0.67	0.68	0.54	0.19	0.70	58	43.0
	(0.05)	(0.10)	(0.15)	(0.19)	(0.21)	(0.19)		
Blanchard-Perotti	0.47	0.43	0.48	0.49	0.37	0.70	49	36.3
	(0.05)	(0.07)	(0.09)	(0.12)	(0.12)	(0.11)		
Sign Restrictions	0.41	0.39	0.51	0.35	-0.10	0.34	10	7.4
	(0.21)	(0.16)	(0.32)	(0.31)	(0.29)	(0.45)		
Event	0.71	1.31	0.87	1.23	0.80	1.32	18	13.3
	(0.17)	(0.57)	(0.69)	(0.65)	(0.37)	(0.83)		
Total							135	100

Note: Standard errors in parentheses.

Figure 3: The average multiplier in non-US countries is much smaller than in the US.



In the next section, we attempt to explain the variation in estimates by relating them simultaneosly to structural characteristics, data characteristics, estimation characteristics, and publication characteristics.

4 Explaining the heterogeneity - Preliminary results

In order to explain the differences in estimates in our sample of studies, we estimate the following regression suggested by Disdier & Head (2008):

$$b_{ij} = \beta + \sum_{k=1}^{K} \gamma_k Z_{ijk} + \epsilon_{ij}, \quad (3)$$

where b_{ij} is the i-th reported estimate of a fiscal multiplier in study j, β is the underlying effect, Z_{ijk} denotes explanatory variables that are assumed to affect the estimate. Z_{ijk} includes explanatory variables described below, and ϵ_{ij} is the disturbance term. To account for the fact that in some cases we have more estimates from the same study we apply clustering on the study level.

Structural heterogeneity The variables capturing structural heterogeneity are constructed as an average for the period corresponding to the sample on which the multiplier was estimated. For example, in the case of the interest rate variable: when the estimate of the fiscal multiplier comes from a VAR model estimated on the US sample spanning 1960:1 - 1997:4, we use the average value of interest rate in US for the period 1960-1997.[2] A simpler approach is often used: meta-analyses use the data at the median year of the data used by primary studies (Havranek & Irsova, 2011). Nevertheless, our approach increases the variability in the explanatory variables, and describes the estimates more precisely.

- Debt to GDP ratio - Literature on expansionary fiscal contractions. Substantial decrease in debt associated with increase in consumption. (Giavazzi & Pagano, 1990; Giavazzi *et al.*, 2000; Perotti, 1999)
- Openness - If large part of stimulus leads to higher imports, domestic output might not be affected, effects of fiscal shocks on trade balance (Beetsma *et al.*, 2008)
- Financial development - In countries with less developed financial markets the probability that the consumers are credit constrained is higher, the effects of government spending might be larger.
- Interest rate - High interest rates decrease multipliers through contractionary effects on investment and consumption, also cite some literature on fiscal policy, monetary policy interactions, moreover multipliers at the interest rate zero lower bound are thought to be larger (Christiano *et al.*, 2011).

Source of the data are World Bank's World Development Indicators for the debt to GDP ratio, openness and financial development, while the short term interest rates are extracted from International Monetary Fund's International Financial Statistics.

Data characteristics We include the number of observations as a control variable to test whether studies using smaller samples deliver systematically different estimates. We also add a dummy variable indicating whether the data used were at annual frequency. Using annual data has implications for the identification of the shocks as well as for the dynamics of estimated impulse responses (Perotti, 2007).

[2]In the cases when the data were not available for the required time period, we used the data for the nearest year that was available.

Estimation characteristics Identification strategies – recursive (Fatás & Mihov, 2001), non-recursive (Blanchard & Perotti, 2002), sign-restrictions (Mountford & Uhlig, 2009), event studies (Ramey & Shapiro, 1998; Ramey, 2011), over-parametrization – need to use BVAR (Banbura *et al.*, 2010), omitted variables bias – small number of variables included in VAR can result in confounding the endogenous esponses with shocks (examples from monetary VARs), importance of debt feedback (Chung & Leeper, 2007; Favero & Giavazzi, 2007).

Publication characteristics We need to control for the quality of studies. A dummy variable for published study might not be a good indicator of quality: some working paper series such as ECB WP, NBER WP are arguably better than some low ranked journals. As for the impact factor, there are issues which one to use. The number of citations per year seems to be the most appropriate.

Table 4 presents the definitions and summary statistics for the variables used in the explanatory meta-regression.

Table 4: Description and Summary Statistics of Regression Variables.

Variable	Description	Mean	Std. Dev.	Min	Max
Multiplier (Impact)	The size of the multiplier at impact.	0.543	0.448	-0.397	2.413
Multiplier (1 year)	The size of the multiplier at 1 year-horizon.	0.618	0.952	-3.190	4.883
Multiplier (2 years)	The size of the multiplier at 2 year-horizon.	0.600	1.201	-4.576	5.879
Multiplier (3 years)	The size of the multiplier at 3 year-horizon.	0.592	1.323	-3.962	5.861
Multiplier (5 years)	The size of the multiplier at 5-year horizon.	0.318	1.139	-4.149	3.539
Multiplier (Peak)	The size of the multiplier at the peak.	0.743	1.524	-5.032	6.251
Structural heterogeneity					
Debt to GDP ratio	The debt to GDP ratio defined as: central government debt/GDP.	0.383	0.155	0.080	0.950
Openness	The openness of the economy: imports/GDP	0.224	0.181	0.061	0.811
Financial development	The financial development of the economy: (domestic credit to private sector)/GDP	0.882	0.314	0.203	1.682
Interest rate	The short term interest rate.	7.567	2.289	3.299	21.323
Data characteristics					
Observations	The logarithm of the number of observations.	4.680	0.602	3.555	5.513
Annual	=1 if the data at annual frequency are used.	0.163	0.370	0.000	1.000
Estimation characteristics					
BVAR	=1 if Bayesian VAR is used.	0.163	0.370	0.000	1.000
Blanchard-Perotti	=1 if Blanchard & Perotti (2002) identification is used.	0.430	0.495	0.000	1.000
Sign restrictions	=1 if sign restrictions are used.	0.074	0.262	0.000	1.000
Event study	=1 if event study approach is used.	0.133	0.340	0.000	1.000
Size of VAR	Number of endogenous variables included in VAR.	5.496	1.647	1.000	10.000
Debt	=1 if debt feedback is controlled for.	0.059	0.236	0.000	1.000
Publication characteristics					
Study citations	The logarithm of [(the Google Scholar citations)/(age of the study)+1].	2.676	1.493	0.000	5.493

Table 5 presents the preliminary results of the explanatory meta-regression. Results suggest that structural heterogeneity seem to be more important for the differences in the estimates, while the effect estimation characteristics is imprecisely estimated. The signs and the magnitude of the coefficients for the structural characteristics are rather intuitive.

Table 5: Explanatory meta-regression, OLS with study-clustered standard errors

	Horizon					
	Impact	1 Year	2 Years	3 Years	5 Years	Peak
Constant	0.835	2.551**	2.479*	2.479	1.427	5.037***
	(0.570)	(0.988)	(1.323)	(1.840)	(2.165)	(1.793)
Structural heterogeneity						
Debt to GDP ratio	-0.096	-0.568	-1.994***	-1.692***	-2.897**	-1.658*
	(0.204)	(0.376)	(0.458)	(0.595)	(1.323)	(0.947)
Openness	-0.333	-2.511**	-2.738**	-4.752***	-2.676	-4.914**
	(0.418)	(0.949)	(1.228)	(1.406)	(1.978)	(1.929)
Financial development	0.174	-0.388	-0.163	-0.777*	-0.560	-0.971
	(0.107)	(0.294)	(0.420)	(0.437)	(0.568)	(0.572)
Interest rate	-0.016	-0.035**	-0.028	-0.143**	-0.223***	-0.095
	(0.022)	(0.015)	(0.033)	(0.069)	(0.076)	(0.057)
Data characteristics						
Observations	-0.161*	-0.121	-0.001	0.407	0.804*	0.073
	(0.082)	(0.231)	(0.310)	(0.360)	(0.452)	(0.299)
Annual	0.283**	0.404*	0.574*	0.888***	0.467*	-0.032
	(0.109)	(0.211)	(0.289)	(0.293)	(0.271)	(0.436)
Estimation characteristics						
BVAR	0.381***	0.294*	0.159	0.227	-0.003	0.044
	(0.131)	(0.148)	(0.163)	(0.155)	(0.290)	(0.182)
Blanchard-Perotti	0.029	-0.087	-0.056	0.284	0.196	-0.108
	(0.110)	(0.098)	(0.127)	(0.176)	(0.233)	(0.141)
Sign restrictions	-0.473**	-0.682**	-0.565	-0.694	-0.732	-0.830
	(0.222)	(0.271)	(0.480)	(0.578)	(0.504)	(0.585)
Event study	0.125	0.716***	0.222	0.661	0.236	0.492***
	(0.142)	(0.163)	(0.193)	(0.436)	(0.432)	(0.163)
Size of VAR	0.037	0.042	0.061	0.032	-0.037	-0.049
	(0.031)	(0.078)	(0.071)	(0.088)	(0.090)	(0.102)
Debt	-0.134	-0.255	-0.023	0.091	0.132	-0.167
	(0.126)	(0.181)	(0.296)	(0.366)	(0.424)	(0.462)
Publication characteristics						
Study citations	0.093*	-0.106	-0.202*	-0.346***	-0.376***	-0.364***
	(0.050)	(0.080)	(0.106)	(0.096)	(0.084)	(0.128)
R^2	0.27	0.20	0.18	0.28	0.39	0.19
Observations	129	132	132	118	112	132

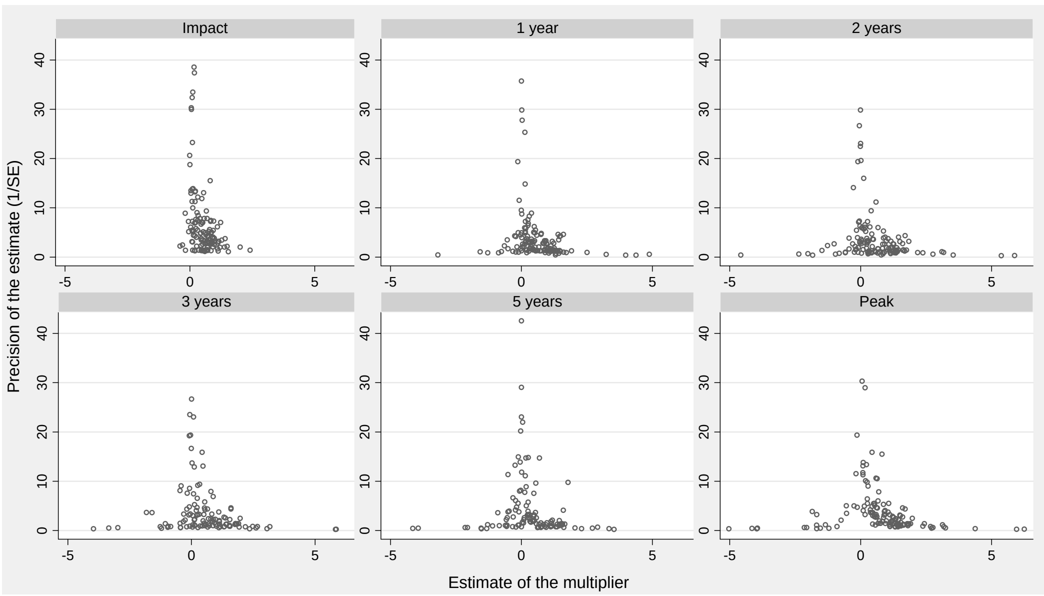
Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5 Further issues

Weights How to account for the fact that some studies provide more estimates? Recent trend is to use all estimates (Disdier & Head, 2008; Cipollina & Salvatici, 2010). Clustering might not be enough. If each estimate gets the same weight, studies with more estimates get more weight. Two solutions. First, weight each estimate by the number of estimates in a given study. Second, we might want to weigh estimates/studies by how precise they are. But standard errors estimated from VARs might be misleading, the impulse responses are non-linear function of estimated parameters from reduced form VAR model. It is well known that one needs to use Monte Carlo standard errors (Sims & Zha, 1999). Most studies do that. Alternatively, follow Smith & Huang (1995) who use sample size as an explanatory variable instead of weighting estimates by their variance. Finally, Nelson & Kennedy (2009) suggest to use heteroscedasticity-consistent estimators, if primary study variances are believed to be poorly estimated.

Publication bias The presence of publication selection bias (preference for statistically significant results and/or intuitive signs of the estimated coefficients) has been recognized as a major issue in economics (Card & Krueger, 1995; Ashenfelter & Greenstone, 2004; Stanley, 2008). Informal tool to assess the presence of publication bias is the scatterplot of the estimate versus its precision (the inverse of the standard error). Asymmetry of the funnel plot indicates the publication bias. Figure 4 suggests that bias should not be a major concern, funnel plots are symmetric, although right tails seem heavier.

Figure 4: Funnel plots of the estimated multipliers.



Formal test requires adding standard error into explanatory meta-regression (to account for publication selection), and dividing regression by standard error (to address heteroscedasticity), use of mixed effects estimation (to account for multiple estimates from one study). We adress these issues by estimating the following equation:

$$t_{ij} = \frac{b_{ij}}{SE_{ij}} = \beta_0 + \frac{\beta}{SE_{ij}} + \sum_{k=1}^{K} \frac{\gamma_k Z_{ijk}}{SE_{ij}} + \alpha_j + \epsilon_{ij}, \tag{4}$$

where SE_{ij} denotes the standard error of the estimate, β_0 captures the extent of the publication bias, and α_j is a study-level random effect.

Table 6 presents the results. On the whole, the results from the baseline model (Table 5) are confirmed. The extent of the publication bias is rather moderate. The sign for the financial development is reversed.

Table 6: Explanatory meta-regression, Mixed-effects multilevel estimation

	Horizon					
	Impact	1 Year	2 Years	3 Years	5 Years	Peak
Intercept (Publication bias)	1.874***	0.587**	0.198	0.134	-0.229	0.698**
	(0.515)	(0.255)	(0.291)	(0.274)	(0.303)	(0.346)
$1/SE$	0.339	0.662	0.060	1.035	0.062	2.308***
	(0.424)	(0.452)	(0.582)	(0.669)	(0.666)	(0.657)
Structural heterogeneity						
Debt to GDP ratio	-0.257*	-0.328*	-1.006***	-1.048***	-1.797***	-0.322
	(0.149)	(0.183)	(0.201)	(0.249)	(0.452)	(0.311)
Openness	-0.113	-0.841**	-0.079	-1.679***	-0.025	-2.007***
	(0.203)	(0.347)	(0.449)	(0.611)	(0.736)	(0.506)
Financial development	0.280**	-0.062	0.512***	-0.160	0.274	-0.493*
	(0.122)	(0.166)	(0.197)	(0.209)	(0.255)	(0.260)
Interest rate	-0.007	-0.009	-0.012	-0.098***	-0.150***	-0.032
	(0.007)	(0.015)	(0.024)	(0.030)	(0.030)	(0.021)
Data characteristics						
Observations	-0.042	-0.050	-0.077	0.095	0.333**	0.026
	(0.094)	(0.070)	(0.104)	(0.115)	(0.130)	(0.120)
Annual	0.354**	0.810***	0.902***	0.972***	0.608**	0.543**
	(0.138)	(0.144)	(0.201)	(0.249)	(0.293)	(0.216)
Estimation characteristics						
BVAR	0.399	-0.063	0.051	0.465**	0.517**	-0.025
	(0.266)	(0.172)	(0.202)	(0.223)	(0.228)	(0.231)
Blanchard-Perotti	0.115***	0.073	0.219**	0.362**	0.332**	-0.002
	(0.039)	(0.086)	(0.108)	(0.144)	(0.141)	(0.105)
Sign restrictions	-0.539**	-0.185	-0.331	-1.055***	-1.433***	-0.321
	(0.247)	(0.212)	(0.278)	(0.332)	(0.332)	(0.298)
Event study	-0.307	0.399	0.268	0.348	-0.150	0.481
	(0.191)	(0.287)	(0.386)	(0.370)	(0.307)	(0.507)
Size of VAR	-0.029	0.041	0.075	0.097*	0.103*	-0.064
	(0.030)	(0.035)	(0.047)	(0.056)	(0.056)	(0.043)
Debt	-0.206	-0.136	-0.130	-0.159	-0.288	-0.312
	(0.183)	(0.162)	(0.188)	(0.198)	(0.196)	(0.220)
Publication characteristics						
Study citations	0.017	-0.026	0.037	-0.125**	-0.173***	-0.180***
	(0.046)	(0.045)	(0.060)	(0.054)	(0.050)	(0.059)
Within-study correlation	0.66	0.22	0.18	0.00	0.00	0.19
Observations	127	130	130	116	108	130

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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