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**Keywords**
fiscal multiplier, regime dependance, meta regression analysis

**JEL classification**
E27, E62, H30

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# Are Fiscal Multipliers Regime-Dependent? A Meta Regression Analysis*

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September 18, 2014

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## 1. Introduction

The literature on the effect of fiscal shocks on macroeconomic variables has exploded since the outbreak of the world financial crisis, as governments across advanced economies relied on some form of fiscal stimulus to counter the crisis' effects on the real economy, accompanied by heated controversies regarding the desirability and design of such programs. More recently, the Euro area's fiscal consolidation attracted even more researchers to the field. As a result, according to our count, the number of contributions estimating the effect of fiscal policy shocks increased from 56 in 2008 to 149 in 2013.

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Getting an overview of the field has thus become a challenging task, all the more so as results vary widely. A special focus of many recent contributions has been the question as to whether some fiscal impulses are especially effective during economic downturns as compared to "normal" economic circumstances, starting with Auerbach and Gorodnichenko (2012b), because recent theoretical contributions have argued that the government expenditure multiplier is higher if monetary policy is accommodative (Woodford 2011; Christiano et al. 2011). An accommodative monetary policy stance is more likely during economic downturns due to the zero lower bound constraint on the nominal interest rate and because some central banks appear to respond less to inflation during economic downturns than in "normal" times (Bec et al. 2002; Wolters 2011).

In this paper, we therefore conduct a meta-regression analysis of estimates of fiscal multipliers from empirical reduced form models (i.e. vector autoregressive (VAR) and single equation estimation (SEE)) in order to quantify the role of the economic regime under which a multiplier was estimated. We are therefore able to investigate whether and to what extent fiscal multiplier estimates are systematically higher if an economy is classified by the respective researcher as being in an economic downturn (i.e. in the "lower regime").

The paper extends the meta-regression analysis of fiscal multipliers by Gechert (2013), which, as far as we are aware, is the first analysis applying the meta-regression technique to fiscal multiplier estimates, but does not control for the economic regime. In addition to the economic regime, we control for those variables found to significantly affect the multiplier in Gechert (2013), including the kind of fiscal impulse, the employed empirical model class, the way fiscal shocks are identified and some sample and estimation specifics.

We find that the fiscal multiplier is indeed significantly higher during economic downturns than in average economic circumstances. The multiplier of unspecified government expenditure on goods and services rises by between 0.6 to 0.8 during a downturn depending on the specification of the regression equation.

However, the magnitude of the multiplier increase during downturns varies strongly across instruments. While the public investment multiplier exceeds all others during average economic circumstances, during recessions it increases only moderately to a level similar to the public consumption multiplier. The effect of transfer changes is transformed much more dramatically in the lower regime, turning it from the second least effective expenditure type stimulus into the most effective one, followed by changes to military spending. Part of the strong increase of the transfer multiplier might be explained by an increase in the share of liquidity or credit-constrained private households, whose consumption equals their disposable income (Eggertsson and Krugman 2012). By contrast, the estimated tax multipliers are rather small in all regimes and appear to be almost unaffected by the economic situation.

During average economic situations and booms, multipliers are not only lower than in downturns but also tend to vary less across fiscal impulses. This combination is consistent with an active monetary policy during such periods which neutralizes the effect of demand shocks, and a more accommodative monetary policy during downturns, e.g. Woodford (2011); Christiano et al. (2011); Coenen et al. (2012).

We confirm a number of results obtained by Gechert (2013), including that spending multipliers tend to be larger than tax multipliers, and that identification method and model class play an important role for the multiplier estimate. More open economies have significantly lower multipliers.

After showing how the characteristics affect the multiplier, we also investigate whether cumulative multipliers exceed one during economic downturns, i.e. whether there is an overall crowding-in or crowding-out effect in an economy with average degree of openness. For that purpose, we take a simple average across estimation-technique and sample specific characteristics. We find that for all expenditure-side impulses other than unspecified increases in government spending, the cumulative multiplier robustly exceeds one in the lower regime.[1] If only VAR-based estimates are considered, all expenditure-side impulses have a multiplier exceeding one. By contrast, the tax multiplier is always far below one in all economic regimes and across all specifications we consider.

The remainder of the paper is structured as follows: The following section describes our fiscal multiplier dataset. Section 3 describes the meta-regression method while section 4 discusses the variables included in our regression equation. Section 5 and 6 present our results. The final section concludes.

## 2. Data Set and Descriptive Statistics

Our dataset is an extension of the one employed by Gechert (2013), adding studies that control for a regime dependence of the multiplier, but focussing on VAR and SEE estimates, leaving out all multipliers from structural model simulations that were of central importance to this first meta regression analysis of fiscal multipliers. The present dataset takes into account 98 studies published between 1992 to 2013, providing a sample of 1882 observations of multiplier values after controlling for some outliers outside the range of $\mu \pm 3\sigma$. A list of included papers is given in Table 6 in the appendix. The majority of papers in our sample have been published after the occurrence of the financial crisis and subsequent policy action.

In order to search for papers we used BusinessSearch, the RePEc archive, Google Scholar, and established working paper series (NBER, CEPR, IMF, Fed, ECB). As a precondition, papers had to provide calculations of multiplier effects or at least provide enough information such that we were able to calculate the multipliers. For example, some papers provided elasticities of output with respect to government spending. If these papers also provided the share of government spending to GDP, we conducted the multiplier calculations.

A methodical key question is how the multiplier is measured. In general, multiplier values are drawn from standardized fiscal impulses (e.g. one percent of GDP or one currency unit) that allow for a dimensionless comparable input-output relation. Notwithstanding some exceptions, it is generally assumed that multiplier effects are linear in scale and sign. As opposed to the comparative static textbook multiplier, dynamic and empirical approaches allow for several variants and require additional information to calculate the effect size. VAR models usually provide impulse response functions (IRF) of standard-

[1] Note that peak multipliers are consistently higher by about 0.35 units across all kinds of fiscal impulses.

ized fiscal policy shocks. Multipliers ($k$) are calculated either as the *peak* response of GDP with respect to the initial fiscal impulse ($FI_t$)

$$k = \frac{\max_h \Delta Y_{t+h}}{\Delta FI_t} \tag{1}$$

or as the *cumulative* response function of GDP divided by the cumulative fiscal impulse function

$$k = \frac{\sum_h \Delta Y_{t+h}}{\sum_h \Delta FI_{t+h}} \tag{2}$$

or as the *impact* response divided by the impact impulse

$$k = \frac{\Delta Y_t}{\Delta FI_t} \tag{3}$$

where $\Delta(\cdot)$ marks deviation from the baseline, and $h$ denotes the horizon over which the multiplier is calculated.

Since peaks are usually the maxima of response functions of GDP, we would expect peak multipliers to exceed cumulative multipliers. However, sharply declining fiscal impulse functions combined with long-lasting GDP responses can produce cumulative multipliers exceeding peak multipliers. Impact multipliers can be subsumed under cumulative multipliers with a horizon of $h = 1$.

For single equation estimations, multiplier effects are retrieved from coefficients of the exogenous fiscal variables plus the sum of lagged coefficients if lags are included. Horizons can be recorded via the number of lagged fiscal variables. The sum of coefficients condenses the information on the relation between the fiscal variable and the GDP response along a specific horizon, and are thus structurally akin to cumulative multipliers of impulse-response functions. We thus subsume them under the heading of cumulative multipliers.[2]

Table 1 provides basic statistics of reported multipliers distinguishing the fiscal impulses, the model classes and the regimes. From the studies we analyzed we can distinguish unspecified public spending impulses (SPEND) from public consumption (CONS), public investment (INVEST) or military spending (MILIT). Other impulses could be transfers to households (TRANS) or changes in taxation (TAX). We do not distinguish between the various types of taxation. Some studies only refer to unspecified public deficit shocks (DEF).

From this mono-characteristic view, multiplier values vary widely among fiscal impulses and the standard deviation of each single characteristic is wide, but keep in mind that the figures pool all kinds of economic regimes and models. The means of reported multipliers from public spending impulses are approximately twice as high as those from

[2] Some authors, such as Mountford and Uhlig (2009), refer to net-present value multipliers. Since we are interested in short horizons and both the fiscal shock and the GDP response are discounted at the same rate, present value multipliers should not deviate much from their non-discounted counterparts, such that we do not treat them separately.

tax cuts and transfers. Among the public spending categories, multipliers related to public investment seem to be highest.

With respect to model classes, mean multipliers from SEE and VAR models are close, but the medians deviate more strongly, suggesting a skewed distribution for SEE multipliers in particular. Note that the figures for the model classes comprise all kinds of fiscal impulses and regimes.

As our focus is to investigate whether the fiscal multiplier is higher during downturns or upturns, we control for the economic regime under which the recorded multiplier was estimated. We distinguish an average regime (RAV), a lower regime (RLO) and an upper regime (RUP). None of the studies on which our dataset is based allows for more than three regimes regarding the business cycle or crisis situations. The average regime comprises multiplier estimates which a.) allowed for state-dependence of the fiscal multiplier, or b.) which allow for only one regime while there is no hint in the text that the authors believed their estimates to be relevant for a specific economic situation. The lower and upper regime chiefly comprises multiplier values whose estimation allowed for the multiplier to be state dependent. Such estimates may for instance be generated by allowing for two lag-polynomials in a VAR, a "recession" and an "expansion" polynomial, as for instance in Auerbach and Gorodnichenko (2012b). However, the lower or upper regime labelling may also apply to values where the estimation method did not allow state-dependence but there is clear indication by the authors that the estimated value is relevant for a specific economic situation, e.g. Almunia et al. (2010); Acconcia et al. (2011).

Categorizing all multipliers by the respective regime suggests that multipliers are lower in upturns and higher in recession or crisis regimes. Of course, to confirm this first impression it is necessary to control for the effect of many other study characteristics, which we will discuss in section 4. Distributions in table 1 are generally not normal, as indicated by the significant Doornik-Hansen statistics (DH-p), pointing to additional influences.

## 3. Meta Regression – Method

We follow some standards in the meta-analysis literature. First, we include studies that employ the same or akin country-samples but use a different methodology to identify fiscal shocks and regimes, and to specify their model. Second, in line with Bijmolt and Pieters (2001) and Card et al. (2010) we do not only include a single multiplier value from each study, but opt to include multiple observations, since they often cover different regimes, fiscal impulses, samples, identification schemes, etc.—a variation which is useful to pick up. Study level effects are taken into account by including a dummy for each paper and by estimating with cluster-robust standard errors. In order to control for undue weight of studies with many multiplier figures we test the robustness of our results by estimating a weighted sample where each multiplier of a single study is weighted by the inverse of the number of multipliers drawn from this paper, thus giving each study an equal weight (Sethuraman 1995). Moreover, we estimate a reduced sample where only the median multiplier observation of each study is taken into account.

Table 1: Descriptive statistics of reported multiplier values for fiscal impulses, model classes and regimes

| | *fiscal impulse* | | | | |
|---|---|---|---|---|---|
| | TOTAL | SPEND | CONS | INVEST | MILIT |
| Mean | 0.83 | 0.90 | 0.89 | 1.22 | 1.12 |
| Median | 0.74 | 0.84 | 1.00 | 1.10 | 0.85 |
| Std. dev. | 1.01 | 0.80 | 1.19 | 1.37 | 1.10 |
| Max | 5.00 | 3.60 | 4.84 | 5.00 | 4.79 |
| Min | -3.14 | -2.00 | -3.06 | -2.72 | -0.43 |
| DH p | 0.00 | 0.00 | 0.00 | 0.43 | 0.00 |
| N | 1882 | 664 | 524 | 188 | 73 |
| | *fiscal impulse* | | | | |
| | TAX | TRANS | DEF | | |
| Mean | 0.44 | 0.54 | 0.35 | | |
| Median | 0.30 | 0.50 | 0.21 | | |
| Std. dev. | 0.69 | 1.16 | 0.50 | | |
| Max | 3.70 | 4.54 | 1.79 | | |
| Min | -1.50 | -3.14 | -0.40 | | |
| DH p | 0.00 | 0.00 | 0.00 | | |
| N | 318 | 36 | 79 | | |
| | *model class* | | | *regime* | |
| | SEE | VAR | RAV | RUP | RLO |
| Mean | 0.86 | 0.83 | 0.75 | 0.39 | 1.37 |
| Median | 0.67 | 0.75 | 0.68 | 0.50 | 1.38 |
| Std.Dev | 0.97 | 1.02 | 0.96 | 0.77 | 1.08 |
| Max | 4.79 | 5.00 | 4.55 | 3.20 | 5.00 |
| Min | -3.14 | -3.06 | -3.14 | -1.80 | -1.80 |
| DH p | 0.00 | 0.00 | 0.00 | 0.01 | 0.00 |
| N | 273 | 1609 | 1078 | 355 | 449 |

Third, meta analyses regularly control for a possible publication bias, i.e. the preference for statistically significant and theory-compliant results in publication selection (Stanley 2008). A standard assumption is that the observations of the effect sizes of an unbiased literature should build a symmetric funnel around its most precise estimations, such that there is no significant influence of precision on the effect size. Since most of the studies we included lack comparable standard errors of their multiplier estimations, we are unable to perform standard publication bias tests via the inverse of the standard error as a measure of precision, but rely on the number of observations used for each multiplier estimation, as a second-best proxy for precision (Stanley and Doucouliagos 2012: 73). Figure 1 reports the funnel graph of this measure of precision against the reported multiplier value.

Table 2 shows several test regressions for funnel-asymmetry (Stanley and Doucouliagos 2012: 62), where the reported multiplier is regressed on various transformations of the number of observations that was used for the respective multiplier estimation ($f(obs)$). For columns (1) and (2) significance of $f(obs)$ points to a publication selection bias. For columns (3) and (4), where the dependent variable is weighted by the log or the square root of the number of observations respectively, a publication selection bias is indicated by a significant intercept.

All tests reject the null of no asymmetry and point to a negative publication bias where higher multiplier values would be under-represented. Two qualifications apply. First, our proxy for precision is only second best so the results should be interpreted with caution. Second, the regressions in Table 2 do not take into account the whole set of relevant explanatory factors and could thus give a distorted view. For example, regime dependence of the multiplier could account for what is regarded as funnel asymmetry in the plain regressions of table 2. As will be shown in Section 5, the multivariate meta-regression model does not reconfirm a significant influence of our measure of precision on the reported multiplier. Basically, we would not expect a systematic preference for significant positive or negative multipliers, since the different approaches in multiplier theory provide arguments for a broad band of possible results. Moreover, multipliers are usually calculated irrespective of their significance levels against zero.

The significant intercepts in columns (1) and (2) as well as the significantly positive coefficients of the measure of precision in columns (3) and (4) of Table 2 point to a genuine positive underlying multiplier effect of about 0.83 – in line with the mean of multipliers for the total sample in table 1.

The next section develops the model and the set of moderator variables that capture the characteristics for the multivariate meta-regression model.

## 4. Meta Regression – Moderator Variables

For the proposed meta regression analysis we refer to Stanley and Jarrell (2005: 302). Our empirical model reads

$$k_j = \kappa + Z_j\alpha + \delta_i + \varepsilon_j \qquad j = 1, ..., N \qquad i = 1, ..., M \tag{4}$$

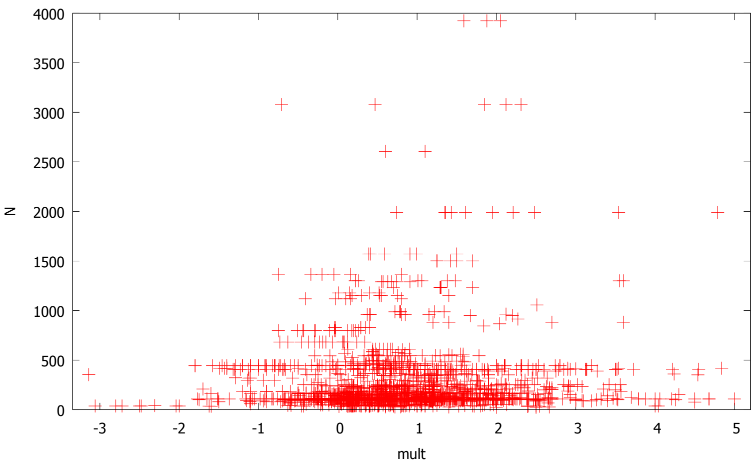


Figure 1: Funnel graph of empirical multiplier estimations

Table 2: Tests for publication selection bias

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $\kappa$ | 0.8315*** | 0.8315*** | 0.04436** | 0.3695** |
| | (0.02331) | (0.02331) | (0.02100) | (0.1808) |
| $f(obs)$ | 0.06542** | 0.006861** | 0.8331*** | 0.9931*** |
| | (0.03065) | (0.003277) | (0.04999) | (0.07924) |
| $N$ | 1882 | 1882 | 1882 | 1882 |
| $Adj.R^2$ | 0.0028 | 0.0025 | 0.3622 | 0.4635 |
| $\ell$ | -2690 | -2691 | -2494 | -6546 |

(1) Dependent: multiplier. Indep: $f(obs) = log(obs)$.
(2) Dependent: multiplier. Indep: $f(obs) = sqrt(obs)$.
(3) Dependent: mult$\cdot log(obs)$. Indep: $f(obs) = log(obs)$.
(4) Dependent: mult$\cdot sqrt(obs)$. Indep: $f(obs) = sqrt(obs)$.
*, **, *** indicate significance at the 10, 5, 1 percent level respectively, t-values in parentheses

with $k_j$ being the multiplier value of observation $j$; $\kappa$ the "underlying" or "reference" multiplier value; $Z_j$ the vector of characteristics ("moderator variables") related to observation $j$; $\alpha$ the vector of systematic effects of $Z_j$ on $k_j$; and $\delta_i$ the vector of paper-specific intercepts (paper dummies).

For each of the $M = 98$ studies, we include a dummy $\delta_i$, termed study-level effect in meta regression analysis (Stanley and Doucouliagos 2012: 113), in order to control for possible cluster effects. We also use heteroskedasticity-robust and cluster-robust standard errors, clustered by studies. In the regression, one of the dummies would be omitted due to exact collinearity and its influence on the dependent variable would thus be reflected in the reference value $\kappa$. In order to eliminate the bias arising from arbitrarily removing a study dummy on $\kappa$, we run two stages of each regression. In the first step, we include all study dummies, let the econometric software randomly choose the one to drop and calculate the mean coefficient of the remaining study dummies. In the second step, we deliberately drop the dummy closest to this mean and therefore get a reference value with a minimized bias from study dummies. Note that only second step regressions are shown and that the choice of the omitted dummy in no way influences any of the other coefficients, but only shifts the reference value $\kappa$.

A multiplier observation in a study comes with specific characteristics, captured in the vector $Z_j$ including the regimes, the different kinds of fiscal impulses as well as study-design characteristics.

Most characteristics, such as the kind of fiscal impulse itself, are measurable on a nominal scale only, i. e. there is no possible ranking order and we have to capture them as a group of dummies, which are mutually exclusive. A reported multiplier value must exclusively belong to one value in the group 'fiscal impulse', which incorporates the values (SPEND, CONS, INVEST, MILIT, TRANS, TAX, DEF).

Regarding regimes, reported multipliers exclusively belong to one value of the group (RAV, RUP, RLO).

Coenen et al. (2012) not only find that in a range of structural macroeconomic models used in central banks and other policymaking institutions, the multiplier of government demand exceeds the multiplier of the various tax changes they consider, but also that the former increases more in downturns as monetary policy becomes more accommodative. Therefore we allow the fiscal impulses to interact with the economic regimes.

The method with which fiscal shocks are identified has been argued to be of paramount importance for the size of the estimated fiscal multiplier. In our dataset, identification is always linked to a model class, which can be a VAR or a single equation estimation, and we combine each model class-identification pair in one variable.

For VAR studies, there are five established approaches of identification of exogenous fiscal shocks, two of which rely on additional historical information, and three of which try to identify exogenous fiscal shocks directly from the time series (Caldara and Kamps 2008). (1) The war episodes approach (VARWAR) focuses on a few periods of extraordinary US military spending hikes, which are deemed to be orthogonal to business cycle fluctuations (Ramey and Shapiro 1998). (2) The narrative record (VARNAR), established in the fiscal policy literature by Romer and Romer (2010), follows a similar idea, but employs historical information such as government announcements or economic forecasts, and is not limited to military spending hikes; Romer and Romer (2010) focus on discretionary tax changes. (3) The recursive VAR (VARRA) approach (Fatás and Mihov 2001) uses unfiltered time series of public spending and revenue series (both net of transfers) and a Choleski decomposition that imposes a causal ordering of the variables of the VAR with zero restrictions to the factorization matrix to rule out contemporaneous reactions of the fiscal variable to business cycle variations. (4) The Blanchard and Perotti (2002) SVAR approach (VARBP) builds on the recursive VAR approach, but additionally allows for non-zero restrictions such as imposing estimated elasticities of automatic stabilizers (5) The sign-restriction VAR (VARSR) approach (Mountford and Uhlig 2009) achieves identification by imposing restrictions on the signs of impulse-response functions for a given horizon and then distinguishing fiscal shocks from business cycle shocks. The underlying priors are that both the elasticity of revenues to GDP and revenue multipliers are always positive for some periods, while spending multipliers and elasticities of spending with respect to GDP could have any sign.

Single equation estimations deal with the identification problem by employing the war episodes (SEEWAR) or narrative (SEENAR) approaches as described above, instrumented variables (SEEIV) or event studies using cyclically-adjusted fiscal time series (SEECA). An observation must belong exclusively to one value in this group.

We record the multiplier calculation method with a dummy for peak vs. cumulative calculation (PEAK, CUM). As pointed out, multiplier calculations also differ concerning the time horizon of measurement, so we record the horizon of the number of quarters after the shock (HOR) on which the multiplier calculation is based. We also add a quadratic term of the horizon to allow for the usual hump-shaped behavior of impulse-responses.

Another natural control variable is the share of imports in GDP as a country specific effect, as this variable would be expected to be positively related to the degree of import leakage and thus negatively to the fiscal multiplier (OECD 2009). Using the World Bank World Development Indicators data set, we recorded the average import quota (M/GDP) of the time series and country (or group of countries) that the reported multiplier relates to.

We include the log of the number of observations the multiplier estimation is based upon (LOGOBS) such that, again, we can control for the influence of precision and a possible publication selection bias in the presence of our moderator variables. The log is preferred to the square root since it has more explanatory power in Table 2 as shown by t-stats.

A list of all variables can be found in Table 3 (excluding the numerous interaction terms). All non-dummy variables are demeaned in order to leave the intercept of the meta regressions, the reference value, unaffected by inclusion or exclusion of these variables.

## 5. Meta Regression – Results

We start by regressing reported multipliers of the total sample on characteristics as shown in Table 4. Groups of variables that are measured on a nominal scale, such as the type of impulse or the regime, are necessarily multicollinear because they are mutually exclusive. That is why one variable from each of these groups is always omitted and its

Table 3: List of variables for meta regression

| variable | explanation | scale |
|---|---|---|
| *economic regime* | | |
| RAV | average or unspecified regime | dummy |
| RUP | upswing regime | dummy |
| RLO | downswing or crisis regime | dummy |
| *fiscal impulse* | | |
| SPEND | unspecified public spending | dummy |
| CONS | public consumption | dummy |
| INVEST | public investment | dummy |
| MILIT | public military spending | dummy |
| TAX | tax reliefs to private sector | dummy |
| TRANS | transfers to private sector | dummy |
| DEF | unspecified tax relief or spending increase | dummy |
| *model class and identification strategy* | | |
| SEEIV | SEE with instrument variable approach | dummy |
| SEECA | SEE with prior cyclical adjustment of public budget | dummy |
| SEEWAR | SEE with war episodes approach | dummy |
| SEENAR | SEE with narrative record / action-based approach | dummy |
| VARWAR | VAR with war episodes approach | dummy |
| VARNAR | VAR with narrative record / action-based approach | dummy |
| VARRA | VAR with recursive approach | dummy |
| VARBP | VAR with Blanchard-Perotti approach | dummy |
| VARSR | VAR with sign restrictions approach | dummy |
| *further controls* | | |
| PEAK | calculated as peak multiplier | dummy |
| CUM | calculated as cumulative multiplier | dummy |
| HOR | horizon of the multiplier calculation | quarters |
| M/GDP | import quota of the surveyed country sample | percentage points |
| LOGOBS | log of number of obs. used | continuous |

influence is reflected in the constant ($\kappa$), which is called *reference value*. Thus, $\kappa$ should not be interpreted as the *true multiplier* since it depends on the reference specification. We identified best practice specifications and take them as a reference, yet, this choice is still subjective. The reference specification is a cumulative multiplier value (CUM) from an unspecified public spending impulse (SPEND) taking place in average economic circumstances (RAV); the multiplier stems from a VAR model with Blanchard-Perotti identification (BP), with mean import quota and mean horizon. Such an observation on average reports a multiplier of 0.59 when controlling for other influences, which is significantly different from zero.

Coefficients of the moderator variables show deviations from the reference value, which allows us to make unconditional relative statements about the effectiveness of fiscal policy in a given setting as compared to an alternative setting. Interaction terms apply between the groups of mutually exclusive dummy variables (for example each regime interacted with each kind of fiscal impulse), and thus are dummies by themselves. Stand-alone coefficients of the interacted variables represent the deviation from the reference value. Coefficients of the interaction terms are deviations from the stand-alone coefficients. For example, while INVEST marks the difference of the public investment multiplier to unspecified spending multipliers in the average regime, and RLO is the difference of spending multipliers as compared to the average regime, RLO*INVEST+INVEST represents the specific impact of investment multipliers in the lower regime as compared to spending multipliers in the very same regime.

We interpret the coefficients in our regressions in the following way: Coefficients of any dummy variable show the estimated difference of the multiplier value from the reference specification when the specification of the dummy applies. Coefficients of continuous variables, such as horizon and the import-to-GDP ratio, show derivatives of the multiplier with respect to these independent variables.

The different columns of Table 4 represent alternative specifications of the regression model. Column (1) represents our preferred specification, where we take into account the interaction between fiscal impulses and regimes. Column (2) additionally interacts all other groups of variables to show the robustness of the selective choice of interactions in column (1);[3] there are some issues with this specification since not all combinations have a representation in the data set and the respective coefficients of the interactions are naturally omitted in such a case. By contrast, column (3) provides a specification without any interaction terms. The model in column (4) repeats the exercise of column (1), this time without controlling for the fixed effects of the paper dummies. Column (5) uses the baseline specification for a reduced sample where only the cumulative multipliers are taken into account, leaving out the peak multipliers.

It turns out that multipliers of general government spending in the average regime vary between 0.4 and 0.7 across the various specifications and are significantly different from zero. Spending multipliers stemming from circumstances where the economy is running well are generally close to the average regime multipliers or slightly below. In

[3]In order not to overburden the table, the large set of coefficients is not reported here, but is available on request.

recessions or crisis situations, however, they exceed the multipliers of the average regime by between 0.6 to 0.8.

When looking at the relative effectiveness of fiscal impulses for the average regime, it turns out that public consumption multipliers are largely in line with unspecified public spending multipliers. Public investment multipliers are significantly higher for almost all specifications in a range of 0.5 to 0.7 units. Tax and transfer multipliers are about 0.3 units lower than unspecified spending multipliers, and significantly so. Military spending shocks induce GDP effects which are by and large insignificantly lower than general spending multipliers. Multipliers from studies that look only at the broad measure of the public deficit do not provide a clear picture. The positive and significant coefficient in column (2) might be due to some small sample issues in the presence of a large set of regressors in this specification that includes all possible interactions. The multipliers of the spending impulses in the average regime are largely in line with Gechert (2013).

Interesting results can be derived from the interaction terms between regimes and fiscal impulse categories. Most strikingly, averages of investment multipliers turn out to be much lower in upswings and their strong relative magnitude is in average situations is muted in downturns, because other spending impulses produce high GDP effects in these circumstances as well. As compared to unspecified spending, military build-ups have smaller effects in booms, but much stronger multipliers in recessions. Interestingly, tax multipliers show no specific behavior in upturns as compared to spending multipliers, but they are much lower in the downturn, such that, as compared to spending hikes, tax reliefs are a less efficient option to counter a recession. Transfers, however, are much more efficient in a downswing, which is plausible in case they are targeted to credit or liquidity-constrained households. Note that no upper regime multipliers for transfers have been reported in our sample. For the case of the broad measure of public deficit, results are not robust in the case of inclusion of all possible interaction terms. We interpret this as a small sample issue.

With respect to modeling and identification strategies, we can tag some groups that produce similar results. First of all, the BP, RA and NAR identifications in VAR models by and large report the same multipliers.[4] The VARSR and VARWAR identifications generally produce lower multiplier effects. In the case of SEE models, results fluctuate more widely. Their level is generally higher for the case where identifications are used that are also applied to VAR models, namely the narrative and the war dummy identification. The multipliers of the CA and IV identification are comparably lower, more in line with the effects found for the VAR models. The extraordinary effects found in column (2) again could reflect the problem of trying to control for all possible interactions while for some of them a representation in the data set is missing.

The lower rows of Table 4 show the coefficients of the other control variables. We find very robust coefficients throughout columns (1) to (5). Peak multipliers are significantly

[4] In the case of column (2), where we also interact models and identification schemes with fiscal impulses, we find tax multipliers to be significantly higher for the narrative approach, both for the VARNAR and the SEENAR case (results not shown). This finding reproduces the well-known issue in the literature that the top-down and bottom-up identifications do not agree upon the size tax multipliers. See for example Perotti (2012).

higher by about 0.35 units. The horizon of measurement and its quadratic term show plausible, if insignificant coefficients, reflecting a slight inverse U-shape of the multiplier effects with growing multipliers for shorter horizons turning somewhat lower on longer horizons. The import quota of the country-sample under investigation of the studies in our data set has a plausible and significant negative coefficient. An increase of the import quota of one percentage point lowers the multiplier by approximately 0.025 units. As opposed to the plain test for publication selection bias in table 2, The log of the number of observations of a study has an insignificant impact on the multiplier, giving no hints for a publication bias when controlling for our set of characteristics.

Since we find some differences regarding the impact of VAR and SEE models, we repeat the exercise of Table 4 in Table 5 with a subsample of VAR models only, in order to investigate as to whether the results of other coefficients are driven by model classes. Since there are only three studies that employ VAR models to report multipliers of unspecified public deficit impulses, we leave them out in order not to run into multicollinearity issues with the study dummies. Thus, there is no regressor for DEF in the specifications of Table 5.

The results are largely confirmed when looking at VAR models only. Reference multipliers are slightly higher now. Lower regime multipliers on average are still 0.6 to 0.8 units larger than their normal regime counterparts. Investment multipliers are exceptionally high on average but do not show a particularly high multiplier effect in downturns, where all spending multipliers are large. The effects of military spending shocks are muted in the average and upper regime case, but turn out to be exceptionally strong in downturns. Tax multipliers are significantly lower (approximately 0.4 units) and particularly so in the downturn, where spending multipliers increase. Thus, they exhibit rather linear effects irrespective of economic circumstances. Transfer multipliers for the average regime are also significantly lower than those for spending; note that no regime-dependent transfer multipliers are present in this subsample so no interaction term applies. Regarding model class and identification scheme as well as the further control variables, results of the subsample are very much in line with those of the full sample.

Table 7 in the appendix contains several robustness tests concerning a possible overweighting of comprehensive studies and a binary representation of the dependent variable. In column (1) we test a weighted version of the prime specification of the full sample by weighting each multiplier observation of a paper and the respective characteristics by the number of observations in the paper. Note that interpreting the magnitude of coefficients is not straightforward in this case. Generally, results are confirmed by and large, but significance levels are somewhat lower, whereas $R^2$ values are large. This is due to the paper-specific intercepts which now carry the bulk of information as each paper has its specific weight.

In column (2) of table 7 we test a model using only the median multiplier of each study of the total sample. Most results are confirmed, but public investment multipliers are now lower, peak multipliers are not significantly different from cumulative ones and the significance level of the downturn regime is somewhat lower. It should, however, be pointed out that this sample eats away a lot of information and certainly biases

Table 4: Total sample (Dep. Var.: multiplier)

| | (1) base[a] | (2) all[a] | (3) no inter[a] | (4) no dum[a] | (5) cumulative[a] |
|---|---|---|---|---|---|
| $\kappa$ | 0.587(0.279)** | 0.406(0.155)** | 0.636(0.33)* | 0.697(0.091)*** | 0.717(0.221)*** |
| *regime* | | | | | |
| RUP | -0.049(0.097) | -0.006(0.141) | -0.197(0.091)** | -0.166(0.083)** | 0.005(0.093) |
| RLO | 0.769(0.143)*** | 0.653(0.126)*** | 0.755(0.12)*** | 0.633(0.158)*** | 0.723(0.155)*** |
| *fiscal impulse* | | | | | |
| CONS | -0.159(0.196) | 0.312(0.238) | -0.122(0.171) | 0.081(0.199) | -0.114(0.195) |
| INVEST | 0.788(0.263)*** | 0.471(0.28)* | 0.474(0.305) | 0.704(0.28)** | 0.728(0.233)*** |
| MILIT | -0.467(0.328) | -0.678(0.358)* | -0.122(0.293) | -0.068(0.136) | -0.641(0.449) |
| TAX | -0.328(0.126)*** | -0.297(0.131)** | -0.46(0.112)*** | -0.368(0.106)*** | -0.266(0.145)* |
| TRANS | -0.287(0.136)** | -0.348(0.234) | -0.239(0.154) | -0.443(0.212)** | -0.147(0.085)* |
| DEF | -0.087(0.086) | 0.315(0.138)** | -0.176(0.089)** | -0.532(0.179)*** | -0.071(0.093) |
| *interaction of impulse and regime* | | | | | |
| RUP*CONS | 0.005(0.266) | -0.163(0.244) | | -0.37(0.324) | -0.029(0.269) |
| RLO*CONS | 0.484(0.248)* | 0.024(0.233) | | 0.109(0.241) | 0.51(0.249)** |
| RUP*INVEST | -1.166(0.25)*** | -0.948(0.21)*** | | -1.061(0.278)*** | -1.225(0.217)*** |
| RLO*INVEST | -0.364(0.243) | -0.008(0.168) | | -0.45(0.341) | -0.466(0.218)** |
| RUP*MILIT | -0.343(0.322) | -0.768(0.441)* | | -0.834(0.203)*** | -0.38(0.349) |
| RLO*MILIT | 1.059(0.429)** | 1.048(0.221)*** | | 0.469(0.275)* | 1.048(0.483)** |
| RUP*TAX | 0.023(0.154) | 0.037(0.198) | | 0.000(0.157) | -0.073(0.155) |
| RLO*TAX | -0.744(0.235)*** | -0.664(0.224)*** | | -0.756(0.214)*** | -0.753(0.255)*** |
| RLO*TRANS | 1.378(0.138)*** | 1.196(0.13)*** | | 1.002(0.279)*** | 1.205(0.094)*** |
| RUP*DEF | 0.023(0.107) | 1.03(0.234)*** | | -0.168(0.228) | |
| RLO*DEF | -0.677(0.15)*** | -0.365(0.126)*** | | -0.495(0.438) | -0.563(0.16)*** |
| *model and identification* | | | | | |
| VARRA | -0.029(0.079) | 0.26(0.175) | -0.052(0.074) | 0.028(0.101) | -0.155(0.108) |
| VARSR | -0.409(0.031)*** | -0.553(0.204)*** | -0.425(0.028)*** | 0.011(0.112) | -0.434(0.079)*** |
| VARNAR | -0.01(0.117) | -0.101(0.275) | 0.058(0.106) | 0.221(0.298) | -0.092(0.11) |
| VARWAR | -0.542(0.107)*** | -0.287(0.231) | -0.609(0.115)*** | -0.634(0.137)*** | -0.806(0.102)*** |
| SEENAR | 0.833(0.232)*** | -0.231(0.413) | 0.888(0.229)*** | 0.403(0.146)*** | 0.804(0.208)*** |
| SEEWAR | 0.956(0.357)*** | 0.815(0.89) | 0.891(0.256)*** | -0.295(0.203) | 1.025(0.401)** |
| SEECA | -0.07(0.244) | -1.089(0.282)*** | 0.015(0.236) | -0.481(0.176)*** | -0.065(0.211) |
| SEEIV | 0.232(0.27) | 1.44(0.227)*** | 0.222(0.273) | 0.165(0.156) | -0.192(0.156) |
| *further controls* | | | | | |
| PEAK | 0.379(0.103)*** | 0.389(0.115)*** | 0.376(0.099)*** | 0.31(0.099)*** | |
| HOR | 0.019(0.012) | 0.019(0.012) | 0.02(0.012)* | 0.017(0.011) | 0.017(0.014) |
| $HOR^2$ | -0.0002(0.0003) | -0.0002(0.0003) | -0.0003(0.0003) | -0.0003(0.0003) | -0.0001(0.0003) |
| M/GDP | -0.026(0.007)*** | -0.028(0.007)*** | -0.025(0.007)*** | -0.02(0.004)*** | -0.029(0.008)*** |
| LOGOBS | 0.013(0.053) | 0.049(0.051) | 0.028(0.051) | 0.012(0.066) | -0.002(0.059) |
| N | 1882 | 1882 | 1882 | 1882 | 1432 |
| DF | 1752 | 1709 | 1763 | 1849 | 1309 |
| $R^2$ | 0.394 | 0.448 | 0.350 | 0.269 | 0.383 |
| AIC | 4703.1 | 4614.3 | 4812.5 | 4862.6 | 3606.9 |

[a] reference: RAV, SPEND, VARBP, CUM
*, **, *** indicate significance at the 10, 5, 1 percent level, std. ers. in parentheses

Table 5: VAR models only (Dep. Var.: multiplier)

| | (1) base[a] | (2) all[a] | (3) no inter[a] | (4) no dum[a] | (5) cumulative[a] |
|---|---|---|---|---|---|
| $\kappa$ | 0.809(0.083)*** | 0.66(0.234)*** | 0.858(0.134)*** | 0.722(0.095)*** | 0.957(0.172)*** |
| *regime* | | | | | |
| RUP | -0.031(0.107) | -0.026(0.142) | -0.209(0.095)** | -0.226(0.092)** | 0.018(0.102) |
| RLO | 0.808(0.168)*** | 0.618(0.117)*** | 0.811(0.13)*** | 0.642(0.182)*** | 0.752(0.18)*** |
| *fiscal impulse* | | | | | |
| CONS | -0.143(0.216) | 0.28(0.235) | -0.101(0.173) | 0.077(0.211) | -0.111(0.215) |
| INVEST | 0.837(0.292)*** | 0.453(0.279) | 0.498(0.34) | 0.719(0.302)** | 0.767(0.262)*** |
| MILIT | -0.704(0.082)*** | -0.901(0.22)*** | -0.311(0.074)*** | -0.228(0.178) | -1.41(0.08)*** |
| TAX | -0.443(0.125)*** | -0.309(0.129)** | -0.542(0.11)*** | -0.514(0.104)*** | -0.417(0.142)*** |
| TRANS | -0.506(0.151)*** | -0.36(0.228) | -0.546(0.158)*** | -0.879(0.1)*** | -0.281(0.116)** |
| *interaction terms* | | | | | |
| RUP*CONS | 0.018(0.269) | -0.117(0.229) | | -0.354(0.327) | -0.007(0.272) |
| RLO*CONS | 0.492(0.262)* | 0.101(0.209) | | 0.077(0.263) | 0.539(0.262)** |
| RUP*INVEST | -1.203(0.264)*** | -0.926(0.21)*** | | -1.053(0.291)*** | -1.265(0.238)*** |
| RLO*INVEST | -0.43(0.274) | 0.026(0.167) | | -0.39(0.337) | -0.554(0.258)** |
| RUP*MILIT | -0.242(0.112)** | -0.324(0.168)* | | -0.549(0.201)*** | -0.238(0.102)** |
| RLO*MILIT | 1.316(0.167)*** | 1.273(0.135)*** | | 0.826(0.301)*** | 1.128(0.18)*** |
| RUP*TAX | 0.123(0.165) | 0.042(0.191) | | 0.18(0.173) | 0.068(0.172) |
| RLO*TAX | -0.621(0.229)*** | -0.626(0.221)*** | | -0.602(0.219)*** | -0.58(0.238)** |
| *model and identification* | | | | | |
| VARRA | -0.073(0.072) | 0.252(0.172) | -0.092(0.068) | 0.039(0.096) | -0.183(0.103)* |
| VARSR | -0.43(0.028)*** | -0.552(0.203)*** | -0.443(0.026)*** | -0.027(0.114) | -0.453(0.08)*** |
| VARNAR | 0.02(0.124) | -0.046(0.132) | 0.069(0.111) | 0.289(0.33) | -0.04(0.114) |
| VARWAR | -0.615(0.109)*** | -0.3(0.226) | -0.667(0.117)*** | -0.581(0.163)*** | -0.883(0.103)*** |
| *further controls* | | | | | |
| PEAK | 0.376(0.105)*** | 0.374(0.117)*** | 0.372(0.101)*** | 0.34(0.098)*** | |
| HOR | 0.018(0.013) | 0.018(0.013) | 0.018(0.013) | 0.014(0.013) | 0.013(0.015) |
| $HOR^2$ | -0.0002(0.0003) | -0.0003(0.0003) | -0.0003(0.0003) | -0.0002(0.0003) | -0.0001(0.0004) |
| M/GDP | -0.028(0.007)*** | -0.029(0.007)*** | -0.028(0.007)*** | -0.022(0.004)*** | -0.03(0.009)*** |
| LOGOBS | 0.13(0.081) | 0.16(0.086)* | 0.139(0.081)* | -0.001(0.087) | 0.115(0.079) |
| N | 1560 | 1560 | 1560 | 1560 | 1154 |
| DF | 1467 | 1446 | 1475 | 1535 | 1068 |
| $R^2$ | 0.37063 | 0.42109 | 0.33138 | 0.26061 | 0.35204 |
| AIC | 3976.8 | 3888.4 | 4055.1 | 4092.1 | 2949.6 |

[a] reference: SPEND, VARBP, RAV, CUM

*, **, *** indicate significance at the 10, 5, 1 percent level, standard errors in parentheses

extraordinary coefficients towards zero. This is especially true for those characteristics that are usually run against each other in one study, such as the impact of different fiscal impulses or the business cycle regime. An observation can only become the median if it is not among the higher or lower values in a study. Given that, for example, public investment multipliers and lower regime multipliers are usually among the largest in a paper, they do not become the median observation unless they are not different from the average. The same applies to peak multipliers.

In column (3) and (4) a probit and logit model are tested for the full sample. The dependent variable is binary, signaling whether the multiplier is greater than or equal to one or whether it is less than one. The value of coefficients are not interpretable as deviations. Signs and significance levels are similar to baseline estimations.

As a further robustness test, there are hardly any changes to the coefficients when dropping single papers with many observations ($N > 30$) from the sample (results are available on request).

## 6. Multiplier Magnitudes

After showing how the individual moderator variables affect the multiplier, we now focus on how the absolute magnitude of the multiplier varies across different economic regimes, and whether it tends to exceed one in the lower regime, i.e. during economic downturns. We do so by adding up the relevant coefficients of fiscal impulses, regimes, interaction terms and further controls respectively. Regarding the values of the control variables, we consider the cumulative multiplier for an average import quota, an average number of observations employed and the average horizon in the sample which is 8 quarters. This horizon is relevant for a short term stimulus. All other moderator variables with a nominal scale are kept at their sample averages as well. Furthermore, as no model class or identification strategy is intrinsically superior to the others a priori, we take the simple average across the nine identification strategy / model class pairs we consider.

Apart from the results of our baseline specification (column (1) of Table 4), we also consider a specification including all possible interactions between the independent variables featuring in our regression (column (2) of Table 4).

Figure 2 plots the cumulative multipliers of the various impulses under the baseline specification and the all-interactions specification. As expected, for most impulses, the multiplier increases as the economy moves from the upper to the lower regime. The only exception is the tax multiplier. Furthermore, the multiplier differs less across instruments in the upper regime than in the lower regime. Under the plausible conjecture that the lower regime typically coincides with a more accommodative monetary policy, while the upper regime is associated with a more restrictive monetary policy able and willing to neutralize the effect of demand shocks, these results are in line with simulations of fiscal stimuli in standard monetary macroeconomic models, e.g. Coenen et al. (2012).

Furthermore, for all impulses other than tax changes, the multiplier is smaller than one in the upper regime but strongly exceeds one in the lower regime. Among the various types of expenditure, transfers have the highest lower-regime multiplier, followed by military spending, investment, consumption and general spending. This result is

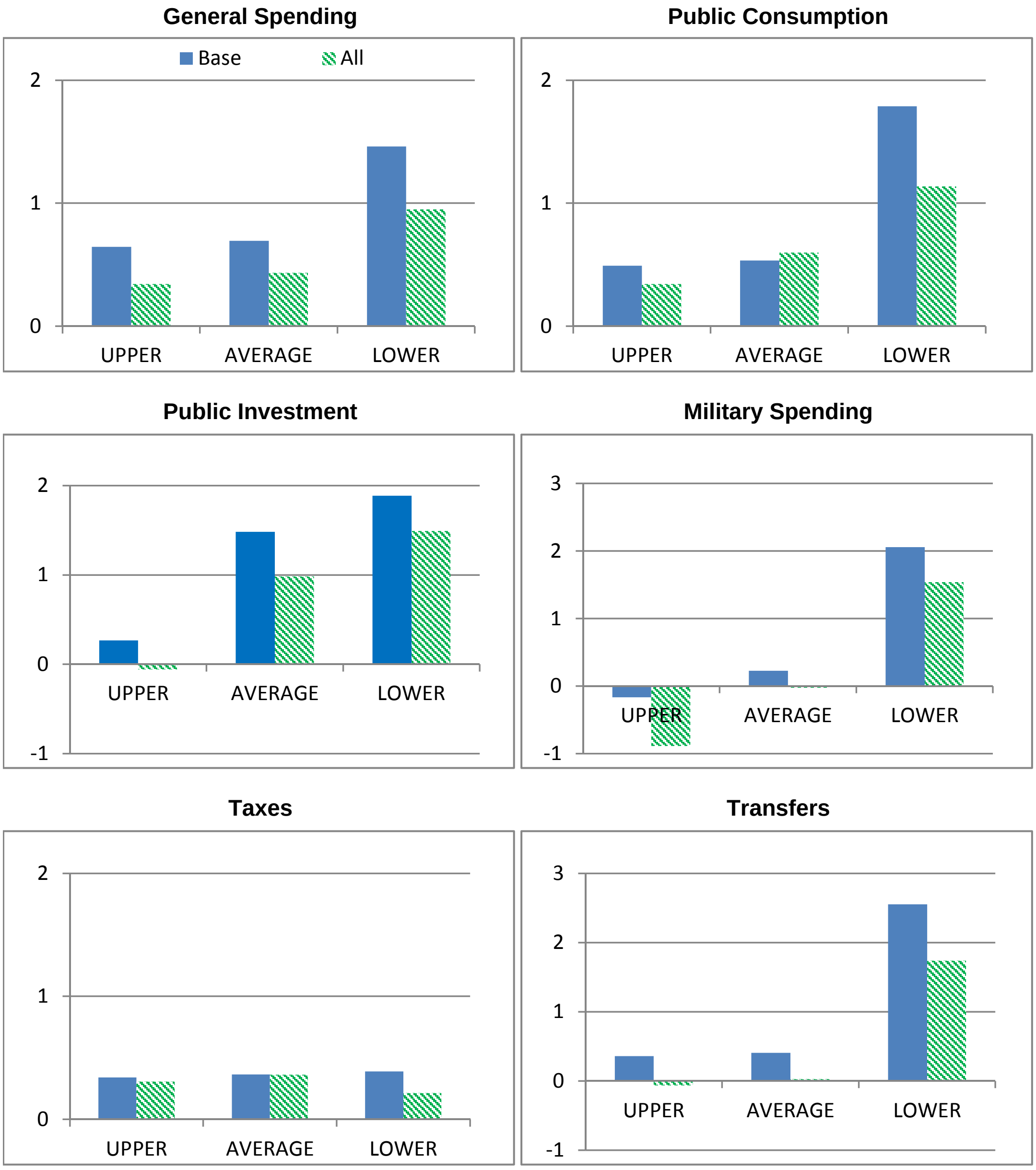


Figure 2: Compound cumulative multipliers of fiscal impulses for different regimes – Full sample. (Baseline specification: blue-bold bars, based on column (1) of Table 4. Specification with all possible interactions: green-striped bars, based on column (2) of Table 4)

surprising, as a fraction of the increase in transfers would be expected to be saved by households, suggesting a lower multiplier than increases of government demand for goods and services. Part of the explanation might be that the share of liquidity-constrained or credit-constrained consumers rises strongly during downturns (Eggertsson and Krugman 2012) and/or that in our sample, the transfer increases occurring during downturns tend to be especially well-targeted. Furthermore, by alleviating situations of poverty arising during downturns, transfer increases may also lift consumer sentiment, thus inducing first round spending increases exceeding the size of the impulse (Bachmann and Sims 2012). As illustrated in Figure 2, under the alternative specifications, the multiplier of each impulse is lower in each regime, except for a tax change. Moreover, multipliers are now slightly less than one for an increase in general spending during downturns. Note that peak multipliers are generally larger by approximately 0.3 to 0.4 units.

However, the sensitivity against adding interactions is mostly driven by the single equation estimations (SEE) of the multiplier. As Figure 3 shows, if one restricts attention to the VAR based estimates, the sensitivity of the multiplier to including all possible interactions decreases. Note that we do not report the size of the transfer multiplier as in the VAR only sample, there are no regime dependent estimates of the transfer multiplier in our fiscal multiplier database.

## 7. Conclusions

This paper attempts to quantify the effect of the economic situation in which a fiscal shock occurs on the estimated fiscal multiplier by conducting a meta-regression analysis of fiscal multiplier estimates. We find that the fiscal multiplier is indeed significantly higher during economic downturns than in average economic circumstances, both in an econometric and an economic sense. Furthermore, during average economic situations and booms, multipliers are not only lower than in downturns but also tend to vary less across instruments. This combination is consistent with an active monetary policy during such periods which neutralizes the effect of demand shocks, and a more accommodative monetary policy during downturns, e.g. Woodford (2011); Christiano et al. (2011); Coenen et al. (2012).

We also confirm a number of results obtained by Gechert (2013), including that spending multipliers tend to be larger than tax multipliers, and that identification method and model class play an important role for the multiplier estimate. More open economies have significantly lower multipliers.

After showing how the characteristics affect the multiplier, we also investigate whether cumulative multipliers exceed one during economic downturns, i.e. whether there is an overall crowding-in or crowding-out effect for a sample with an average import quota. For that purpose, we take a simple average across study characteristics and country-sample specifics. We find that for all expenditure-side impulses other than unspecified increases in government spending, the cumulative multiplier robustly exceeds one in the lower regime. If attention is restricted to VAR-based estimates, the general government spending multiplier exceeds one, too. By contrast, the tax multiplier is far below one in all economic regimes and across specifications of the regression equation we estimate.

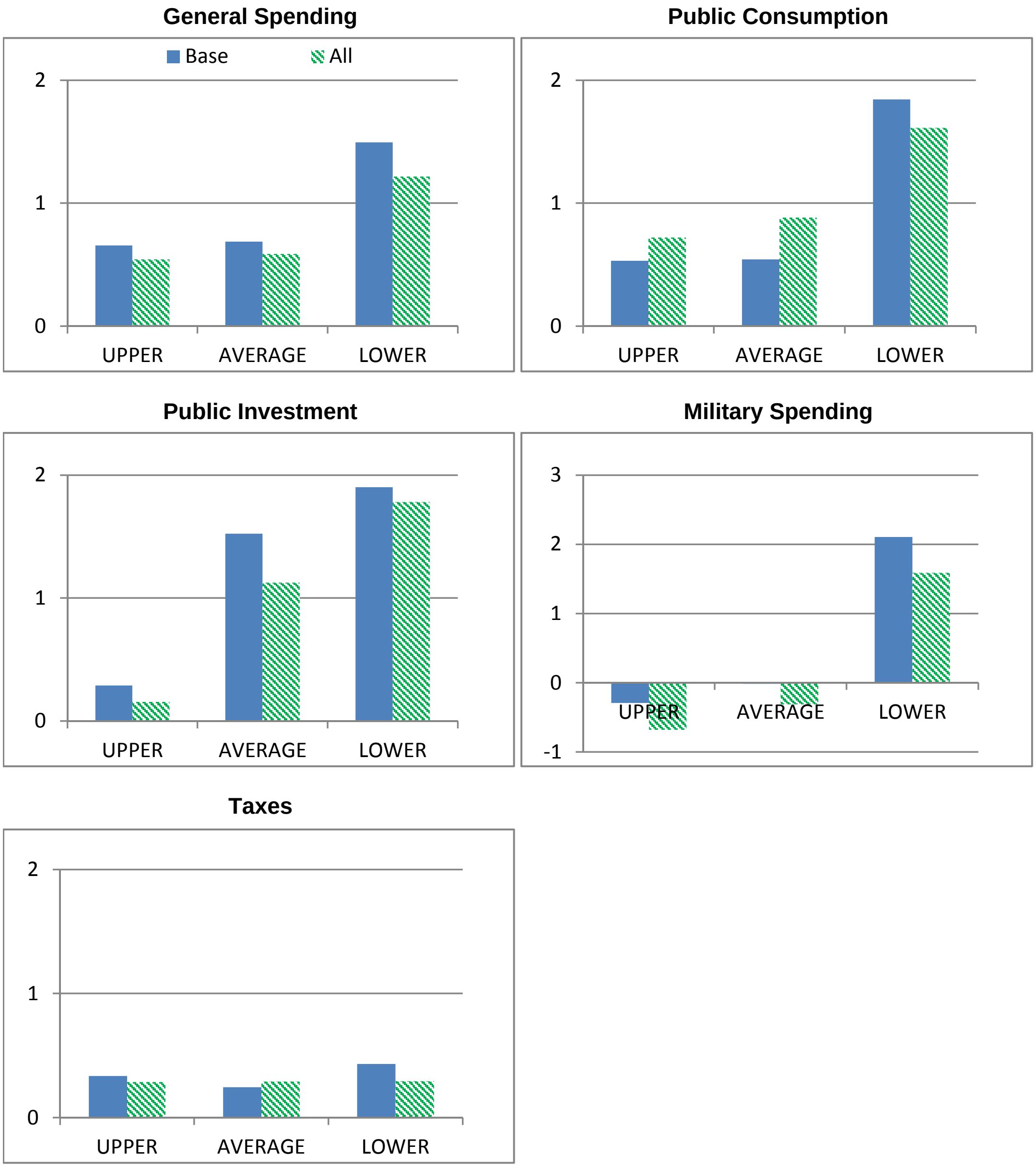


Figure 3: Compound cumulative multipliers of fiscal impulses for different regimes – VAR sample only. (Baseline specification: blue-bold bars, based on column (1) of Table 5. Specification with all possible interactions: green-striped bars, based on column (2) of Table 5)

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## A. Appendix

Table 6: List of included studies

| Study | Model Class(es) | # of mult. | Country Sample |
|---|---|---|---|
| Acconcia et al. (2011) | SEE | 3 | ITA municip |
| Afonso and Aubyn (2009) | VAR | 17 | 17 OECD countries |
| Afonso et al. (2010) | SEE | 2 | 127 countries |
| Afonso et al. (2011) | VAR | 40 | US+UK+GER+ITA |
| Alesina and Ardagna (2010) | SEE | 14 | 21 OECD countries |
| Alesina and Ardagna (2013) | SEE | 24 | 21 OECD countries |
| Almunia et al. (2010) | SEE+VAR | 3 | 27 countries |
| Arcangelis and Lamartina (2003) | VAR | 8 | FR+GER+ITA+US |
| Arin et al. (2012) | SEE | 16 | US |
| Auerbach and Gorodnichenko (2012b) | VAR | 30 | US |
| Auerbach and Gorodnichenko (2012a) | VAR | 9 | 25 OECD countries |
| Bachmann and Sims (2012) | VAR | 18 | EMU countr.+JAP+US |
| Barro and Redlick (2011) | SEE | 8 | US |

Continued on next page

Table 6 – cont'd

| Study | Model Class(es) | # of mult. | Country Sample |
|---|---|---|---|
| Batini et al. (2012) | VAR | 60 | EMU countr.+JAP+US |
| Baum and Koester (2011) | VAR | 36 | GER |
| Baum et al. (2012) | VAR | 132 | G7 |
| Bayoumi and Sgherri (2006) | SEE | 13 | US |
| Beetsma et al. (2006) | VAR | 4 | 14 EU-countries |
| Beetsma and Giuliodori (2011) | VAR | 10 | 7 EU-countries |
| Bénassy-Quéré and Cimadomo (2006) | VAR | 12 | GER+UK+US |
| Bénétrix and Lane (2009a) | VAR | 64 | EMU |
| Bénétrix and Lane (2009c) | VAR | 33 | IRE |
| Bénétrix and Lane (2009b) | VAR | 56 | EMU countries |
| Biau and Girard (2005) | VAR | 10 | FRA |
| Bilbiie et al. (2008) | VAR | 4 | US |
| Blanchard and Perotti (2002) | VAR | 4 | US |
| Born et al. (2013) | VAR | 27 | 13 OECD countries |
| Bouakez et al. (2013) | VAR | 30 | US |
| Broadbent and Daly (2010) | SEE | 6 | 23 OECD countries |
| Brückner and Tuladhar (2010) | SEE+VAR | 8 | 47 JAP prefectures |
| Burriel et al. (2010) | VAR | 8 | EMU+US |
| Caldara and Kamps (2006) | VAR | 10 | US |
| Caldara and Kamps (2008) | VAR | 20 | US |
| Caldara and Kamps (2012) | VAR | 16 | US |
| Candelon and Lieb (2011) | VAR | 12 | US |
| Canova and Pappa (2007) | VAR | 4 | EMU+US |
| Canzoneri et al. (2002) | VAR | 8 | US |
| Caprioli and Momigliano (2011) | VAR | 47 | ITA |
| Caprioli and Momigliano (2013) | VAR | 38 | ITA |
| Castro Fernández and Hernández de Cos (2008) | VAR | 17 | ESP |
| Cavallo (2005) | SEE | 2 | US |
| Chodorow-Reich et al. (2012) | SEE | 1 | US |
| Chung and Leeper (2007) | VAR | 24 | US |
| Cimadomo et al. (2011) | VAR | 20 | US |
| Claus et al. (2006) | VAR | 27 | NZ |
| Clemens and Miran (2010) | SEE | 7 | US states |
| Clemens and Miran (2012) | SEE | 2 | US states |
| Cloyne (2011) | SEE+VAR | 11 | UK |
| Corsetti et al. (2012b) | VAR | 81 | 17 OECD countries |
| Corsetti et al. (2012a) | VAR | 12 | US |
| Crafts and Mills (2012) | SEE | 4 | UK |
| Creel et al. (2005) | VAR | 8 | FRA |
| Hernández de Cos and Moral-Benito (2013) | VAR | 249 | ESP |
| Edelberg et al. (1999) | VAR | 2 | US |
| Eichenbaum and Fisher (2005) | VAR | 2 | US |
| European Commission (2012) | VAR | 32 | EMU countries |
| Fatás and Mihov (2001) | VAR | 4 | US |
| Favero and Giavazzi (2010) | SEE+VAR | 18 | US |
| Fazzari et al. (2012) | VAR | 12 | US |
| Ferraresi et al. (2013) | VAR | 120 | US |
| Feyrer and Sacerdote (2011) | SEE | 11 | US states |
| Fisher and Peters (2010) | VAR | 1 | US |
| Furceri and Zdzienicka (2012) | SEE | 43 | 23 OECD countries |
| Galí et al. (2007) | VAR | 13 | US |

Continued on next page

Table 6 – cont'd

| Study | Model Class(es) | # of mult. | Country Sample |
|---|---|---|---|
| Giordano et al. (2007) | VAR | 23 | ITA |
| Gordon and Krenn (2010) | VAR | 4 | US |
| Guajardo et al. (2011) | SEE+VAR | 10 | 17 OECD countries |
| Hall (2009) | SEE | 3 | US |
| Heppke-Falk et al. (2006) | VAR | 10 | GER |
| Höppner (2001) | VAR | 6 | GER |
| Ilzetzki and Végh (2008) | VAR | 12 | 49 countries |
| Ilzetzki et al. (2011) | VAR | 34 | 44 countries |
| IMF (2008) | SEE | 16 | 31 countries |
| IMF (2010) | SEE | 10 | 15 OECD countries |
| Kirchner et al. (2010) | VAR | 2 | EMU |
| Kraay (2012) | SEE | 19 | 29 developing countr. |
| Kuttner and Posen (2002) | VAR | 2 | JAP |
| Leduc and Wilson (2012) | SEE | 5 | US states |
| Melina and Villa (2012) | VAR | 2 | US |
| Monacelli and Perotti (2008) | VAR | 6 | US |
| Monacelli et al. (2010) | VAR | 3 | US |
| Mountford and Uhlig (2009) | VAR | 6 | US |
| Nakamura and Steinsson (2011) | SEE | 20 | 10 US regions |
| Owyang et al. (2013) | SEE | 17 | US+CAN |
| Pappa (2009) | VAR | 15 | G7 |
| Parkyn and Vehbi (2013) | VAR | 25 | NZ |
| Perotti (2004) | VAR | 10 | AUS+CAN+GER+UK+US |
| Perotti (2005) | VAR | 30 | AUS+CAN+GER+UK+US |
| Ramey (2011) | VAR | 6 | US |
| Ravn et al. (2007) | VAR | 2 | AUS+CAN+UK+US |
| Romer and Romer (1994) | SEE | 1 | US |
| Romer and Romer (2010) | SEE+VAR | 6 | US |
| Rotemberg and Woodford (1992) | VAR | 3 | US |
| Shoag (2011) | SEE | 7 | US states |
| Suárez Serrato and Wingender (2011) | SEE | 3 | US counties |
| Tenhofen et al. (2010) | VAR | 5 | GER |
| Weber (1999) | SEE | 11 | US |
| Zangari (2007) | VAR | 12 | US |

Table 7: Robustness checks

| | (1) weighted[a] | (2) median[b] | (3) probit[c] | (4) logit[c] |
|---|---|---|---|---|
| $\kappa$ | 0.146(0.033) | 0.894(0.108)*** | 0.045(0.421) | 0.035(0.761) |
| *regime* | | | | |
| RUP | -0.264(0.192) | -0.441(0.23)* | -0.084(0.564) | -0.123(0.983) |
| RLO | 0.991(0.169)*** | 0.458(0.247)* | 1.732(0.371)*** | 2.885(0.658)*** |
| *fiscal impulse* | | | | |
| CONS | 0.56(0.382) | 0.019(0.155) | 0.096(0.314) | 0.16(0.543) |
| INVEST | 1.279(0.428)*** | -0.213(0.196) | 0.92(0.301)*** | 1.537(0.501)*** |
| MILIT | -0.871(0.625) | 0.265(0.142)* | -1.599(0.909)* | -2.767(1.744) |
| TAX | 0.045(0.178) | -0.35(0.161)** | -0.913(0.324)*** | -1.681(0.581)*** |
| TRANS | -0.011(0.149) | -0.639(0.119)*** | -0.036(0.571) | -0.084(0.97) |
| DEF | 0.215(0.185) | -0.769(0.292)** | -1.157(0.786) | -1.884(1.842) |
| *interaction of impulse and regime* | | | | |
| RUP*CONS | -0.974(0.648) | | 0.151(0.591) | 0.211(1.016) |
| RLO*CONS | -1.455(0.696)** | 0.129(0.457) | 0.393(0.511) | 0.738(0.902) |
| RUP*INVEST | 0.307(0.651) | | -1.627(0.531)*** | -2.683(0.927)*** |
| RLO*INVEST | -0.975(0.475)** | 1.05(0.503)** | -1.047(0.358)*** | -1.754(0.61)*** |
| RUP*MILIT | 0.667(0.68) | | | |
| RLO*MILIT | 1.505(0.772)* | -0.181(0.29) | 11.275(1.151)*** | 40.341(2.761)*** |
| RUP*TAX | 0.209(0.332) | 0.606(0.266)** | -0.834(0.701) | -1.609(1.408) |
| RLO*TAX | -0.827(0.328)** | -0.892(0.419)** | -1.826(0.677)*** | -3.322(1.273)*** |
| RLO*TRANS | 0.919(0.171)*** | | | |
| RUP*DEF | 0.215(0.2) | 0.48(0.376) | | |
| RLO*DEF | -0.922(0.179)*** | 1.037(0.438)** | -6.455(0.468)*** | -22.467(1.026)*** |
| *model and identification* | | | | |
| VARRA | 0.161(0.168) | 0.06(0.115) | | |
| VARSR | -0.327(0.06)*** | 0.173(0.211) | | |
| VARNAR | -0.103(0.139) | -0.164(0.19) | | |
| VARWAR | -0.356(0.134)*** | -0.904(0.206)*** | | |
| SEENAR | 0.661(0.216)*** | 0.337(0.245) | | |
| SEEWAR | 1.504(0.735)** | -0.732(0.16)*** | | |
| SEECA | -0.291(0.222) | -0.093(0.255) | | |
| SEEIV | 0.059(0.258) | -0.086(0.201) | | |
| *further controls* | | | | |
| PEAK | 0.425(0.106)*** | 0.035(0.119) | 0.669(0.078)*** | 1.153(0.131)*** |
| HOR | 0.018(0.014) | 0.041(0.017)** | 0.055(0.017)*** | 0.093(0.029)*** |
| $HOR^2$ | 0(0) | -0.001(0.001)* | -0.001(0)** | -0.002(0.001)** |
| M/GDP | -0.029(0.009)*** | -0.013(0.005)*** | -0.043(0.019)** | -0.073(0.035)** |
| logN | 0.015(0.053) | -0.002(0.059) | -0.108(0.116) | -0.148(0.193) |
| $N$ | 1882 | 98 | 1650 | 1650 |
| $DF$ | 1752 | 69 | 1562 | 1562 |
| Adj. $R^2$ | 0.89412 | 0.55743 | 0.35741 | 0.35705 |
| AIC | -6537.8 | 144.22 | 1621.4 | 1622.2 |

[a] Values weighted by number of reported multipliers in each paper. Reference: SPEND, VARBP, RAV, CUM

[b] Only median observation from each paper. Reference: SPEND, VARBP, RAV, CUM

[c] Dependent variable is binary, signalling whether multiplier is $\gtreqless 1$. Reference: SPEND, VARBP, RAV, CUM

*, **, *** indicate significance at the 10, 5, 1 percent level, Std. ers. in parentheses

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