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# Do Fiscal Rules Constrain Fiscal Policy? A Meta-Regression-Analysis

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## Abstract

Numerical fiscal rules are implemented to counterbalance the deficit bias in budgetary policy. Over the recent years, an increasing number of studies try to test the actual effectiveness of fiscal rules. This meta-analysis condenses the existing evidence from different regional and federal contexts. It explores the study characteristics which are associated with different findings. Based on a preliminary analysis and a still incomplete sample of primary studies, the results point to a consensus that fiscal rules indeed constrain fiscal policies. This result also appears to hold in light of the criticism that rules are the endogenous reflection of fiscal preferences: even studies with a comprehensive control for fiscal preferences do not lead to systematically weaker levels of statistical significance.

**JEL classification:** E62; H50; H6; H63

**Keywords:** fiscal rules; meta-regression-analysis; public finances

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# 1 Introduction

There is a growing consensus that democratic budgetary decision making is distorted by a risky debt bias. Politicians' and voters' myopia, possibly reinforced by political instability or polarisation, may lead to a long-run unsustainable level of public debt (for a survey on the political economy of public deficits see Imbeau, 2005; Debrun et al., 2008). The most recent experience with some euro area countries close to or actually already in a state of insolvency and the subsequent forced bailouts further emphasise the relevance of these disincentives.

One seemingly obvious strategy to encounter the public debt problem is the introduction of new rules which constrain budgetary decision making. This strategy is highly popular in Europe as a reaction to the crisis of confidence on the sustainability of euro area public finances: Since the crisis' acute outbreak in the year 2010, the Stability and Growth Pact has been strengthened with stricter deficit and debt thresholds and more stringent decision making procedures. In addition, the Fiscal Compact has been signed by all euro area countries (plus most of the other European Union Member States) and obliges the signatory states to define deficit rules within their national legal systems, preferably at a constitutional level. Furthermore, already prior to the signature of this additional commitment, several EU countries had introduced new balanced budget rules in their fight for a better fiscal reputation.

One underlying assumption of this rule-setting strategy is that these rules effectively constrain politicians in their budgetary decision making. However, whether this assumption holds or not is still an open empirical question. For instance, it has been known for long that budgetary decision makers are highly creative in circumventing formal debt constraints (von Hagen, 1991). Hence, to judge the chances of the emerging new constraints, a comprehensive stock-taking of the existing evidence is necessary.

Over the last years, the scientific interest in the empirical effectiveness of fiscal rules has considerably increased and a flood of new analyses have been presented. After a first wave of studies with a focus on balanced budget rules in US states (see the literature on tax and expenditure limitations (TEL), e.g., Bails, 1982; Kenyon and Benker, 1984; Abrams and Dougan, 1986; Howard, 1989; Joyce and Mullins, 1991; Elder, 1992; Shadbegian, 1996), the literature on the impact of (sub-)national fiscal rules has increasingly covered other (sub-)national jurisdictions (e.g., Switzerland and Swiss cantons) and over the more recent years shifted towards the scrutiny of fiscal rules employing panel analyses in the international as well as in the sub-national context (for qualitative surveys see Kirchgässner, 2002; Burret and Feld, 2014).

Taken together, the literature has now reached a critical mass so that a more detailed summary is warranted. Therefore, we present the first meta-regression-analysis which explores the existing studies in a systematic way. Our screening of the literature (with the search algorithm explained in detail below) is based on a specific definition of fiscal

rules. We concentrate on *numerical fiscal rules* in a national or sub-national context. For the sake of a clear focus and comparable results we thus disregard other rules which may be relevant for the budgetary outcome such as budgetary procedures (as pioneered by von Hagen, 2002) or more general political-economic constraints such as, e.g., a supra-national fiscal rule like the Stability and Growth Pact.<sup>1</sup>

Our meta-analysis' first objective is to describe the heterogeneity of results and to identify main trends of this strand of literature with respect to its regional and jurisdictional focus, main fiscal rule indicators used, or the richness of the set of control variables covered. Here, we describe the empirical message of a "typical" study (with the characteristics of the "typical study" precisely defined along numerous study dimensions) and indicate which idiosyncratic study characteristics make a substantive difference for the result.

A more specific objective is that our meta-analysis addresses serious methodological challenges of the literature. The literature's main difficulty is the convincing identification of a causal link. The 'credibility revolution' (for an overview of the general criticism see Angrist and Pischke, 2010) has undermined the confidence that the often observable coincidence between the existence of rules and fiscal prudence can be regarded to represent a causal chain. For instance, as early as 1996, with respect to the correlation of rules and balanced budgets, Poterba (1996, p. 399) has pointed out that "it is possible that the correlations simply reflect correlations involving fiscal discipline, fiscal institutions, and an omitted third variable, voter tastes for fiscal restraint". However, only recently, studies have started to develop identification strategies coping with the challenge of endogenous institutions. In this regard, our meta-analysis tests for the relevance of this criticism by explicitly comparing the results of studies without and with some strategy to cope with omitted variable biases or other difficulties of causal identification. Equally, our meta-analysis reveals to which extent the message of a typical study is robust to several refinements of the underlying estimation model or the econometric technique.

Based on the inclusion of 25 studies which have been published in the period between 2004–2014, the following preliminary insights emerge: At first view, the literature is characterised by an overwhelming consensus that fiscal rules indeed constrain budgetary policy. This result holds particularly strong for deficits and less so for expenditures and revenues – a result hardly surprising given the strong deficit constraining element in the established rules. Further, we find evidence for the relevance of design features such as the estimation method or the calculation of standard errors in case of primary studies. In addition, we also find indication for publication biases as results obtained from working papers or books seem on average to be associated with lower levels of statistical significance. Furthermore, the constraining effects of numerical rules is more clear-cut for the national compared to the sub-national context. The more essential result, however,

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<sup>1</sup> We take account if these constraints are included in addition to numerical fiscal rules through appropriate meta-analytical control variables, however.

is that the diagnosis of effective rules appears to survive the credibility revolution: Those studies which are based on a more comprehensive account of fiscal preference proxies are not systematically characterized by less significant effects compared to our baseline.

The analysis is conducted as follows: In the next section, we describe our algorithm for detecting the studies to be included in the analysis. In Section 3, we explain the coding of the meta-variables and take a descriptive look at our sample. Based on the description of the empirical methodology including the definition of the “typical” study (section 4), the meta-analytical regression results are presented in section 5. Finally, section 6 concludes.

## 2 Selection of Studies

The validity of a meta-regression-analysis (henceforth MRA) hinges on a well-defined and reproducible algorithm guiding the literature search and the coding for the econometric analysis (see, for example, the meta-analysis of economics research reporting guidelines by Stanley et al., 2013).

As a starting point for the selection of studies, we need a clear definition of what is meant by a *fiscal rule*. Thereby, we refer to the definition by Kopits and Symansky (1998, p. 2) who characterise a fiscal rule “as a permanent constraint on fiscal policy, typically defined in terms of an indicator of overall fiscal performance (...) [which covers] summary fiscal indicators, such as the government budget deficit, borrowing, debt, or major components thereof – often expressed as a numerical ceiling or target”. Applying this definition for our MRA, we focus on *numerical fiscal rules* in a national or sub-national context, i.e., we exclude analyses which exclusively ask for the impact of a supranationally set rule like it is the case with the Stability and Growth Pact for the euro area member countries.<sup>2</sup>

Following this definition, we have conducted a literature search using the *EconLit* database. The following keywords were employed to identify suitable studies: *fiscal rule*, *fiscal restraint*, *debt brake*, *debt rule*, *budget rule*, and *deficit rule*. The search was carried out in April 2014 and completed on April 16. The time frame of the search was limited to 10 years, i.e., only studies that were published between 2004 and 2014 were included.

Overall, the search resulted in 1443 studies. The next step of the process then was to select those studies that 1) use national or sub-national fiscal rules based on the definition stated above as explanatory variable and 2) employ a suitable public finance indicator as dependent variable. With respect to the latter, we decided to account for all dimensions of public finance, i.e., we look for studies which deal with the impact of fiscal rules on expenditures, revenues, deficits, or debt (overall or subcomponents, respectively). In total 41 studies remain given these selection criteria.

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<sup>2</sup> However, in case such a supranational constraint is covered in an empirical study for national or sub-national fiscal rules (see, e.g., Dahan and Strawczynski, 2013), we take account of this through an additional control variable (see the variable description).

For the variable coding of this remaining subsample of potentially suitable studies, we followed a four-eyes principle to reduce measurement errors and errors of interpretation. In a first step, each paper was read by one of the authors who had to decide on the inclusion of the paper in the final database and who had to code the ideosyncratic items of the study with respect to the variables applied in the MRA. In a second step, each paper and the variable coding was double checked by one of the remaining authors. Given this procedure, in total 25 studies (currently) form the baseline of our database.<sup>3</sup>

## 3 Data and descriptive statistics

### 3.1 Variable coding

In order to render the estimation of the meta-regression feasible, the ideosyncratic characteristics of the primary studies must be condensed into comparable variables.

A first crucial decision relates to the choice of the key variable of interest which will serve as the dependent variable of the meta-analytical regression. If a strand of empirical literature is characterised by a uniformly defined impact coefficient which is immediately comparable across studies, this coefficient would be the natural candidate. In our context, however, this is not the case. The extraction of standardised coefficients indicating the impact of a numerical fiscal rules is not feasible given the large methodological heterogeneity of that literature. This is mainly driven by two sources of variation. First, there is substantial heterogeneity with respect to the dependent variable in primary studies. While slightly below one third of our observations relate to the primary deficit as dependent variable, one quarter refers to either expenditures or revenues. Furthermore, the coding of the dependent variables differ between studies. For instance, the fiscal measures are expressed in relation to GDP, in relation to revenue, in per capita terms, or in absolute values. Second, there is substantial heterogeneity with respect to the codification of fiscal rules. As shown in section 3.2, primary studies considered in our meta-analysis capture the impact of fiscal rules either through dummy variables, discrete or continuous indices. Given these differences, comparing marginal impacts of fiscal rules across studies is not possible, as the switch from 0 to 1 in case of a dummy variable is not necessarily comparable to a change from an index value  $a$  to  $b$  in case of a continuous index. Therefore, we follow meta-analytical studies such as Card et al. (2010) which face a similar challenge and which rely on the  $t$ -value of the impact of fiscal rules rather than on the coefficient itself.<sup>4</sup> The measure is fully comparable across studies and indicates whether a fiscal rule has a statistically significant impact on the respective fiscal indicator as well its sign.

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<sup>3</sup> At the current stage of the project, 15 studies are not coded and thus not included in the database. Therefore, all results are preliminary and may be subject to change when the completed database becomes available.

<sup>4</sup> Further studies relying on  $t$ -values instead of coefficients involve Baskaran et al. (2014) or Klomp and de Haan (2010).

A further clarification with respect to the definition of the algebraic sign of our meta-analytical dependent variable is important. We want to measure to which extent fiscal rules *constrain* budgetary policy towards a more sustainable path. In order to balance the budget, fiscal rules should *reduce* deficits, debts, and expenditures and *increase* revenues. As a result, we code the *t*-values of primary studies which indicate the impact of fiscal rules in a way that a negative sign always represents a constraining impact, i.e., a fiscal rule *lowers* deficit, debt, and expenditure, or *increases* revenues, respectively. Hence, for primary studies with revenues as the dependent variable, we reverse the sign of the *t*-values for the rule's impact coefficient before we process it. Consequently, we also reverse the sign of studies with the budgetary balance (and not the deficit) as the dependent variable.

We account for the richness of different specifications through constructing numerous indicators for the following variable groups: *classification of the dependent variable*, *source of the fiscal rules data*, *type of the fiscal rule*, *codification of the fiscal rule indicator*, the primary study's *administrative level*, *estimation method*, and *econometric specification*, particular groups of *control variables* in the primary study, and indicators for a potential *publication bias*. A detailed description of all meta-analytical variables is presented in Table A.1 in the appendix.

Some coding decisions require further clarification. For instance, studies for national jurisdictions are not always precise to which extent their fiscal indicators relate to the general government or just to the central government level (like, for example, Nerlich and Reuter, 2013 or Neyapti, 2013). If there is no explicit clarification, our prior is that data refer to general government (which we sometimes can also indirectly verify through the data source information, as for example in Nerlich and Reuter (2013), who refer to the AMECO database which only provides general government data). As a result, the dummy variables indicating whether the dependent variable covers the national budget, state budget, or municipal budget are all set equal to one, respectively. In contrast, if a study only relies on subnational data, we assume that lower level jurisdictions are not included and only set the dummy for municipal budget data equal to one if there are any further information on this issue.

In general, each observation with an estimation coefficient for the impact of a fiscal rule is included in the final database. We only exclude regression results if there is no clear hypothesis how the dependent variable should react to an effective fiscal rule. Examples concern some of the results from Dahan and Strawczynski (2013) who among other things analyse the impact of fiscal rules on the structure of the budget. Because it is unclear whether and how an effective numerical fiscal rule should impact on the spending structure, we have no possibility to interpret the sign of the fiscal rules coefficient according to our key question and leave these observations out.

### 3.2 Descriptive statistics

In the following, we take a descriptive look at specific sample characteristics. In order to avoid that the results suffer from extreme outliers, we have windsorised the data by excluding the top and bottom 1% percentiles of the  $t$ -value distribution.<sup>5</sup> In all summary tables, we present the number of observations and the percentage share in relation to the total number of 889 observations, the number of studies employing the specific variable, minimum and maximum  $t$ -values as well as unweighted and weighted mean  $t$ -values. The latter are computed because the observations are not distributed equally across studies. More precisely, only two studies out of the 25 studies forming the sample (Nerlich and Reuter, 2013: 215 observations; Tapsoba, 2012: 219 observations) account for almost half of the full sample (434 observations in relation to 889 observations). In order to avoid biased descriptive statistics or point estimates (section 5), we apply an analytical weighting procedure and weight the observations with the inverse of the share of observations per study in relation to the full sample. As a result, all studies forming the full sample have the same weight (for more information see section 4).

Table 1: Summary for the classification of the dependent variable

Variable	Obs.	% sample	Studies	Mean $t$ -value		Min.	Max.
				unweighted	weighted		
Debt	70	7.9	6	-1.227	-1.600	-4.404	0.631
Secondary deficit	117	13.2	14	-2.255	-2.273	-10.630	1.900
Primary deficit	272	30.6	10	-2.286	-2.162	-9.890	2.499
Secondary exp.	200	22.5	11	-1.967	-1.184	-11.730	3.470
Primary exp.	132	14.8	5	-1.960	-1.719	-4.043	3.971
Revenue	97	10.9	6	0.402	-0.665	-12.240	3.294

Notes: The total number of observations is 889 based on 25 studies. The statistics are based on windsorised data excluding the top and bottom 1% percentiles. Observations are weighted with the inverse of the share of observations per study in relation to the full sample. Because several studies employ more than one dependent variable, the sum of studies exceeds the total number of studies.

Table 1 gives a summary for the *classification of the dependent variable*. We distinguish six dependent variables: debt, secondary deficit, primary deficit, secondary expenditure, primary expenditure, and revenue. Most of the studies employ deficit measures (43.8 % in total) or expenditure measures (37.3 % in total) as dependent variable. On average, observations with debt as dependent variable lack statistical significance at conventional levels. The opposite holds true for observations with deficit measures. Both, observations with secondary deficit and with primary deficit as dependent variable are statistically significant on the 5%-level on average. The results are independent from the weighting procedure. In contrast, the weighting heavily affects the result for secondary expenditure and revenue. In case of the latter, even the algebraic sign changes. However,

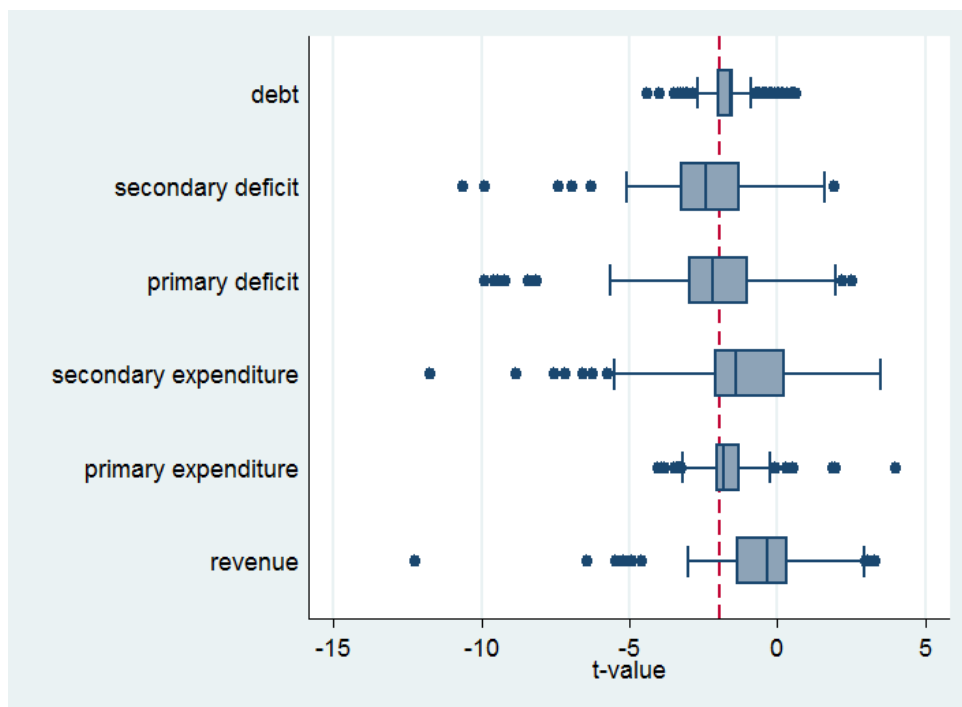
<sup>5</sup> Without windsorising the data, the minimum and maximum  $t$ -values are -19.350 and 40.710, respectively. The  $t$ -value distribution for different dependent variables without windsorising is exemplarily shown in Figure A.1 in the appendix.



the mean  $t$ -values are not statistically significant on average in both cases. With respect to secondary expenditure, the average  $t$ -value lacks statistical significance if we apply analytical weights.

To gain more insights into the distribution of the  $t$ -values for the various dependent variables, we further present a boxplot graph in Figure 1. The dashed line marks the 5%-threshold for statistical significance ( $t$ -value =  $-1.96$ ). The external borders of the boxes present the thresholds for the 25% and the 75% quartiles with the vertical line in between indicating the median  $t$ -value.<sup>6</sup> Again, studies employing deficit measures as dependent variable show statistically significant results. For both dependent variables, more than 50% of the observations are statistically significant at least at the 5%-level. For debt, secondary expenditure, and primary expenditure, this is only true for at least 25% of the observations. With respect to the skewness of the distribution of  $t$ -values,  $t$ -values from observations with debt as dependent variable are skewed to the right while the opposite holds true for observations with primary expenditure as dependent variable.

Figure 1: Boxplot for the classification of the dependent variable



Notes: The total number of observations is 889 based on 25 studies. The statistics are based on windsorised data excluding the top and bottom 1% percentiles. Observations are weighted with the inverse of the share of observations per study in relation to the full sample. The dashed line marks the 5%-threshold for statistical significance ( $t$ -value =  $-1.96$ ).

In Table 2, we present summary statistics for the *type of the fiscal rule*, the *codification of the fiscal rule indicator*, and the *source of fiscal rule data*. With respect to the coding of the rule types, we distinguish between a debt rule, deficit rule, expenditure rule, and a

<sup>6</sup> Single dots are outliers identified by standard whisker plots using a distance of 1.5 times the inter-quartile range (IQR) above the 75% quartile and below the 25% quartile.

revenue rule. Because some primary studies employ compound indicators which comprise several rule types, the sum of the percentage shares of the rule types in relation to the full sample exceeds 100%. Again, there are differences between weighted and unweighted means. While all unweighted mean  $t$ -values of the various rule types are statistically significant at the 10%-level, only the significance of the deficit rule remains if we employ an analytical weighting. While the latter rule type is part in almost 70% of the sample estimates, revenue rules are only employed in about one third of the sample. With respect to the codification of the fiscal rule indicators, most studies employ dummy indicators. In contrast, discrete indicators are only employed in about 9% of the observations. However, the mean  $t$ -value of primary studies using discrete indicators is highly statistically significant – irrespective of the weighting procedure. Observations with dummy indicators and continuous indicators are only statistically significant at the 10%-level on average. Concerning the source of fiscal rule data, we distinguish between four main official sources (ACIR, EC, IMF, OECD) and one category for non-official sources. The latter comprises own computations based on information by the authors (see, e.g., the fiscal rule indicator for Switzerland by Feld and Kirchgässner, 2008). With respect to indicators from official sources, only the indicators from the European Commission (EC) and the OECD are statistically significant on average. Furthermore, non-official indicators are statistically significant at the 10% level on average, which, however, might be due to the high minimum  $t$ -value.

Table 2: Summary for the type, codification, and source of fiscal rules

Variable	Obs.	% sample	Studies	Mean $t$ -value		Min.	Max.
				unweighted	weighted		
<i>Type of fiscal rule</i>							
Debt rule	476	53.5	14	-1.728	-1.563	-9.890	3.457
Deficit rule	620	69.7	23	-1.958	-1.900	-12.240	3.470
Expenditure rule	470	52.9	16	-1.798	-1.577	-9.890	3.971
Revenue rule	298	33.5	11	-1.746	-1.160	-9.890	3.457
<i>Codification of fiscal rule</i>							
Dummy indicator	619	69.6	16	-1.644	-1.673	-12.240	3.971
Discrete indicator	78	8.8	4	-2.400	-2.378	-5.098	1.576
Continuous indicator	191	21.5	9	-1.991	-1.700	-9.890	2.180
<i>Source of fiscal rule data</i>							
ACIR	68	7.6	2	-0.599	-0.369	-2.965	2.174
EC	407	45.8	8	-1.706	-1.701	-9.890	3.294
IMF	448	50.4	4	-1.621	-1.232	-6.250	3.971
OECD	221	24.9	3	-1.435	-2.637	-6.250	3.294
Non-official	457	51.4	13	-1.638	-1.669	-12.240	3.470

Notes: The total number of observations is 889 based on 25 studies. The statistics are based on wind-sorised data excluding the top and bottom 1% percentiles. Observations are weighted with the inverse of the share of observations per study in relation to the full sample. Because several studies employ more than one type of a fiscal rule or various sources for fiscal rule data, the sum of studies and observations exceeds the total number of studies/observations.

Mean  $t$ -values and information on the distribution of observations for the *administrative level* are presented in Table 3. In total, 73% of the observations stem from studies for

the national level, while only about 22% and 5% of the observations are from the state or municipal level, respectively. The most statistically significant effect on average, however, is found for observations with data for the municipal level.

Table 3: Summary for the administrative level

Variable	Obs.	% sample	Studies	Mean $t$ -value		Min.	Max.
				unweighted	weighted		
National	649	73.0	14	-1.760	-1.834	-9.890	3.971
State	197	22.2	9	-1.468	-1.496	-5.098	2.174
Municipal	42	4.7	2	-3.657	-2.646	-12.240	3.470

Notes: The total number of observations is 889 based on 25 studies. The statistics are based on windsorised data excluding the top and bottom 1% percentiles. Observations are weighted with the inverse of the share of observations per study in relation to the full sample.

Finally, we present information on the coverage of control variables in Table 4. In contrast to the tables presented before, this table only presents weighted  $t$ -values and distinguishes between observations which include indicators of the respective control variable group or not. Furthermore, the number and percentage shares of observations and studies is shown. While there is more or less no difference in mean  $t$ -values for studies that include election year indicators, output gap proxies, GDP growth variables, and indicators for budgetary rules, the inclusion or exclusion of control variable groups for other variables makes a difference. For instance, the absolute mean  $t$ -values are higher and thus become more statistically significant on average if the authors control for ideology, government stability, preferences, and the presence of supranational fiscal rules. This result is quite interesting because these variable groups are highly connected to endogeneity problems such as an omitted variable bias which would support the hypothesis that the inclusion of these variables should render the mean  $t$ -values to become rather insignificant. In contrast, the exclusion of debt, openness indicators or proxies for the dependency ratio render the mean  $t$ -values more statistically significant.

## 4 Empirical Methodology

In contrast to classical meta-analyses, we rely on meta-regression analysis (MRA) techniques in our study. While the former combines individual study results to an overall or consensus estimate by assuming between-study homogeneity, the latter explicitly investigates the extent to which heterogeneity in primary results can be explained by one or several characteristics of respective studies (Harbord and Higgins, 2008).

As explained above, due to the heterogeneity in coefficients across primary studies, we employ the  $t$ -value of the fiscal rule's impact as our meta-analytical dependent variable. In line with the usual assumptions in case of MRA, we expect that heterogeneity in individual results of primary studies is driven by sampling error ( $\epsilon_{s,i}$ ), study- or design-

Table 4: Coverage of control variable groups

Variable	Obs.	% sample	Studies	% studies	Mean $t$ -value if control is	
					included	not included
Ideology	451	51%	14	56%	-2.013	-1.591
Government stability	322	36%	5	20%	-2.129	-1.682
Election year	535	60%	13	52%	-1.691	-1.828
Debt	626	70%	13	52%	-1.451	-2.069
Preferences and culture	140	16%	8	32%	-2.136	-1.659
Output gap	407	46%	8	32%	-1.691	-1.801
Growth	329	37%	11	44%	-1.781	-1.754
Openness	258	29%	4	16%	-1.035	-1.869
Dependency ratio	456	51%	3	12%	-1.527	-1.798
Supranational rule	542	61%	5	20%	-2.052	-1.692
Budgetary rules	279	31%	8	32%	-1.781	-1.759

Notes: Total number of observations is 889 based on 25 studies. Only weighted  $t$ -values are presented. The data are windsorised excluding the top and bottom 1% percentiles.

specific factors ( $\mathbf{X}$ ) and the set of estimate-specific covariates ( $\mathbf{Z}$ ). Our baseline estimation can therefore be summarised by equation 1.

$$\hat{t}_{s,i} = t_0 + \mathbf{X}_{s,i}\beta + \mathbf{Z}_{s,i}\gamma + \epsilon_{s,i} \quad (1)$$

Here,  $\hat{t}_{s,i}$  refers to the primary estimate  $t$ -value in case of specification  $i$  from study  $s$ .  $t_0$  is the intercept and captures the respectively designed baseline  $t$ -value of the 'typical' study across all primary studies considered in our MRA.

The necessity for a definition of a baseline is given by the way of coding of the study- or design-specific features in the context of a MRA. This is done by individual dummy variables which are then usually combined to a sub-group in order to capture a particular design feature of the primary study. In this process, dummies constituting such a sub-group are usually self-excluding. Consequently, considering all of these dummy variables at the same time in our meta-regression would result into perfect multi-collinearity. Therefore, we define one particular feature as baseline for every sub-group where self-exclusion applies. Thus, the 'typical' study is defined as an estimation of a deficit rule (expressed as dummy variable) on the primary deficit for the national level which relies on fiscal rule data from the EC. Furthermore, the statistical significance of the OLS estimation is computed with robust standard errors and the coefficient is published in a refereed journal. Taken together, all these baseline features are captured by the intercept of our MRA. Thus, all reported coefficients have to be interpreted relative to this baseline and report an impact of the deviation in this particular feature from the baseline.

Given the heterogeneity in methodological approaches taken and differing sample sizes in primary studies, the variances of individual estimates of  $\hat{t}_{s,i}$  will not be homoscedastic. Consequently, the estimation of equation 1 will be more efficient using *weighted least squares* instead of ordinary least squares (Greene, 2003).

The selection of proper weights, however, is not straightforward, as our MRA does not rely on standardised coefficients. While the literature usually uses the inverse of error term variances obtained from primary studies, this is not feasible in case of MRA using  $t$ -values. In the process of defining proper weights, another aspect has to be taken into account, namely the fact that we follow a multiple sampling approach in which information from all available estimations in primary studies are used in our MRA. Consequently, the number of observations resulting from particular studies will differ substantially. In order to avoid settings in which results and design features of particular primary studies constituting a large share in our sample drive the results of our MRA, we will use analytical weights based on the inverse of the share of observations per study in relation to the full sample, which assign an equal weight to every primary study in our estimation sample.

The circumstance that usually more than just one observation is obtained from individual studies has a further impact on the estimation of equation (1). As these individual observations are likely to be correlated due to the low within-variation in design features, error terms in the estimation of equation 1 are likely to be correlated as well. We will therefore cluster standard errors at the level of studies considered in our MRA. However, since the number of studies is equal to 25, clustering will result in further econometric issues. First, one may argue that the number of clusters is too low potentially resulting in an underestimation of the underlying serial correlation in individual observations (Angrist and Pischke, 2009). Second, given the large heterogeneity in the number of primary estimates obtained from particular studies, the size of clusters will vary accordingly. Therefore, we will also apply the so called *wild-cluster-bootstrap procedure* in order to deal with both issues (Cameron et al., 2008). This allows to implement a bootstrap procedure for the standard errors of equation (1) while taking into account the clustered structure of the data and allows re-sampling along them (Cameron and Miller, 2015).

## 5 Results

Table 5 summarises our estimation results. All four columns report estimations of equation (1) using weighted least squares. While we refer to column (1) as full specification containing all coded study- and design-specific dimensions, the remaining three columns follow a general to specific approach in which selective dimensions are sequentially excluded. As described in section 4, we report both clustered as well as wild-cluster bootstrapped  $t$ -values. While the former is noted in brackets, braces are used in case of the latter.

Table 5 is divided into subsections by headlines indicating the respective study and design dimensions as well as the baseline category where it applies to. The definition of the baseline study is constant throughout all specifications. It comprises an estimation with the primary deficit as dependent variable, captures the impact of a deficit rule coded as a dummy variable, uses information provided by the European Commission as data source, relies on national data, reports estimations using ordinary least squares as well as

robust standard errors, and is published in a refereed journal.

In case of the full specification, the  $t$ -value of this baseline study is -2.698 (see the baseline effect in column 1), i.e., studies exhibiting the particular design features explained above are found to report on average a negative and highly statistically significant impact of fiscal rules. The same holds true for the general to specific baseline effects in specifications (2) to (4), which are found to be significant at the 1% level on average and are significantly estimated in our MRA.

### Study-specific dimensions

Relative to the baseline, variation in the *dependent variable* seems to matter in case of debt, secondary expenditures and revenues. All three variables exhibit positive coefficients, hereby indicating a reduction in the level of statistical significance with which fiscal rules impact on these fiscal aggregates. More precisely, in case of debt and revenues the results of the MRA indicate that on average  $t$ -values of -1.467 or -1.261 point to a level of significance below the 10% threshold. While the statistical precision of our estimation ranges at the level of 10% when standard errors are clustered, the precision is reduced below this threshold in case of bootstrapped errors.

With respect to the type of *fiscal rules*, variation relative to the benchmark defined as deficit rule does not seem to exhibit an additional impact. This may be driven by the fact that in numerous primary studies the type of the rule is not precisely captured or lumped together with other rule types. Similarly, its *codification* as discrete or continuous index relative to the baseline as dummy variable is not found to exhibit an additional impact. This picture changes when considering the *source of fiscal rules data*. Here, the baseline is given by the data provided by the European Commission. Relying on IMF data or using non-official data sources is on average found to be associated with more negative  $t$ -values compared to the baseline. This is especially pronounced for non-official data sources. In both cases, effects are found to be statistically significant estimated when clustering and bootstrapping standard errors.

An interesting insight emerges when controlling for the *administrative level* the primary studies refer to. Our MRA indicates that the impact of fiscal rules in case of studies using data for the state level are on average associated with higher  $t$ -values relative to the baseline which refers to the national level. More precisely, studies relying on state-level data are even found to be on average associated with positive but statistically insignificant  $t$ -values (0.922). In terms of statistical precision, this effect is found to be significant at the 1%-level in case of both clustering as well as bootstrapping.

Considering the set of *control variables* usually applied in primary studies, we find that controlling for the ideology of the government is on average associated with greater  $t$ -values in absolute terms and an increase in the level of statistical significance. A similar pattern emerges for primary studies that control for openness. In contrast, a reduction in

the level of significance is found for cases when primary studies control for the dependency ratio, namely the share of the working population relative to the total population. More precisely, controlling for the dependency ratio results in a drop of the level of significance of fiscal rules below the 10% threshold (-0.574). In all three cases, these effects are found to be significantly estimated in the range between 1% and 10%.

Furthermore, we test for the existence of a *publication bias*. Relative to the baseline which is set by a publication in a refereed journal, we find that on average the level of significance is reduced below the 10% threshold (-0.755) for publications as working papers. A comparable pattern in case of a book publication is not found in our MRA.

### **Design-specific dimensions**

With respect to the design-specific dimensions of our MRA, we control for the *estimation approach* taken in primary studies and further features of the *econometric specification*. Relative to the benchmark of ordinary least squares, studies using a 2-stage-least-squares instrumental variables approach are on average associated with smaller *t*-values in absolute terms, resulting in a loss of statistical significance. This result holds both in case of clustering as well as bootstrapped standard errors in the MRA. In case of GMM estimations or other estimation methods, such an effect can not be found through our MRA.

With respect to the handling of standard errors in primary studies, our analysis indicates that relative to the benchmark of robust standard errors, studies relying on plain standard errors report on average greater *t*-values in absolute terms. Put differently, these studies find on average more frequently a negative and statistically significant impact of fiscal rules at the 1% level. This result is confirmed at the 1% level by both estimations with clustered and bootstrapped standard errors. Surprisingly, the same pattern emerges in case of the application of dedicated identification strategies. While this dummy variable captures whether primary studies apply credible identification strategies with respect to fiscal rules, it is found to increase the level of significance on average. In contrast, the inclusion of jurisdiction-, time-fixed effects, or the estimation of a dynamic specification do not seem to exhibit an additional impact beyond the baseline-impact.

### **General to specific approach**

Given the rich set of dimensions and the amount of control variables considered in our MRA, we follow a general to specific approach in the remaining three columns of Table 5 and sequentially exclude particular dimensions from our estimation specification. This is mainly motivated by the circumstance that these particular dimensions are closely associated with each other. Dimensions we sequentially exclude involve the type of fiscal rules, their source, their way of codification, the administrative level the data of primary studies refers to as well as several control variables considered in primary studies. While the exclusion of the type of fiscal rules is mainly driven by the fact that several primary

studies do not explicitly distinguish between the type of rules and solely capture the existence of any rule, the exclusion of the data source and the codification is driven by the circumstance that particular sources are mainly used or only provide information in terms of dummies or continuous indices. Further, some sources usually only provide information about rules at the national level and are therefore closely related to administrative level of data in primary studies. The latter is also closely related to some control variables in primary studies which usually only refer to the national level such as the growth rate or the output gap.

Looking at the results for the baseline effect in case of specification (2) to (4), we obtain results comparable to the full specification indicating on average negative and statistically significant impacts of fiscal rules on the 1% level. While the baseline effect is larger in absolute terms in specifications (2) and (3), specification (4) roughly exhibits the same size as specification (1).

Compared to the full specification, column (2) excludes the type as well as the source of fiscal rules data from the estimation of equation (1). While the results are comparable in case of the classification of the dependent variable, the codification of fiscal rules provides a new insight. Here, studies relying on discrete indices rather than dummies are on average associated with greater  $t$ -values in absolute terms. Further, the application of an instrumental variables approach does not create a statistically significant impact beyond the baseline effect and leaves the average level of significance reported in primary studies unchanged. In addition, the meta-regression finds that publications in books are on average associated with smaller  $t$ -values in absolute terms. However, taken together with the baseline effect, this still results in a  $t$ -value indicating a statistically significant impact of fiscal rules at the 1% level.

In case of column (3), the type of fiscal rules, their codification as well as the administrative level of underlying data are excluded. Compared to the full specification however, the source of fiscal rules data does not exhibit an effect beyond the baseline. Also, the effect for book-publications found in column (2) is not present anymore. In case of column (4), the type of fiscal rules, their source and the administrative level the data are excluded. While again the majority of results obtained is comparable to the full specification, we do not find a statistically significant impact of applying an instrumental variables approach or publications as working papers or books beyond the baseline-effect.

### **Implications for endogeneity concerns**

Although these results are still of a preliminary nature, our meta-analytical finding point to a strong consensus in the literature that fiscal rules indeed constrain budgetary policy. The results are also informative with a perspective on the criticism that the diagnosed effectiveness of rules may simply reflect fiscal preferences. Those studies which account for preference and culture proxies among their controls show no systematically lower



significancies compared to our benchmark regression. For government ideology, also close to issues of fiscal preferences, we have even the reversed finding that studies which include that variable tend to reveal constraints of higher significance. Only the non-robust result for the importance of instrumental regressions points towards the relevance of endogeneity issues.

Table 5: Meta-Analysis: Results using weighted least squares

	(1) Full specification	(2) Specific 1	(3) Specific 2	(4) Specific 3
<i>Baseline effect</i>	-2.698 [-3.66]*** {-2.46}**	-3.964 [-5.22]*** {-3.10}***	-3.048 [-3.62]*** {-2.12}**	-2.591 [-2.81]*** {-1.93}*
<i>Classification of dependent variable, Baseline: Primary deficit</i>				
Debt	1.231 [1.85]* {1.33}	1.04 [1.83]* {1.35}	0.418 [0.60] {0.47}	0.747 [1.18] {0.96}
Secondary deficit	-0.047 [-0.08] {-0.08}	-0.147 [-0.25] {-0.27}	-0.243 [-0.36] {-0.33}	-0.0738 [-0.11] {-0.11}
Secondary expenditure	0.972 [1.74]* {1.42}	0.899 [1.65] {1.16}	0.438 [0.66] {0.39}	0.889 [1.22] {0.65}
Primary expenditure	0.659 [1.35] {1.05}	0.592 [1.26] {1.03}	0.0753 [0.13] {0.06}	0.359 [0.62] {0.45}
Revenue	1.437 [1.89]* {1.46}	1.24 [1.57] {1.16}	1.087 [1.17] {0.92}	1.235 [1.30] {0.93}
<i>Type of fiscal rule, Baseline: Deficit rule</i>				
Debt rule	-0.311 [-0.78] {-0.74}			
Expenditure rule	-0.291 [-0.50] {-0.32}			
Revenue rule	0.302 [0.40] {0.16}			
<i>Source of fiscal rules data, Baseline: EC</i>				
ACIR	-0.527 [-0.64] {-0.54}		1.347 [1.53] {0.84}	
IMF	-1.359 [-1.79]* {-1.78}*		0.282 [0.24] {0.13}	
OECD	-0.707 [-1.18] {-0.97}		-0.894 [-1.34] {-0.81}	
Non-official	-2.614 [-3.14]*** {-3.10}***		0.675 [0.96] {0.60}	
<i>Codification of fiscal rule, Baseline: Dummy indicator</i>				
Continuous indicator	0.0836 [0.21] {0.24}	-0.0539 [-0.13] {-0.06}		-0.87 [-1.04] {-0.63}
Discrete indicator	-0.496 [-1.27] {-1.17}	-0.802 [-2.37]** {-2.12}**		0.016 [0.03] {0.04}
<i>Administrative level, Baseline: National</i>				
State	3.62 [3.35]*** {2.89}***	2.318 [7.65]*** {3.02e+19}***		
Municipal	-0.261 [-0.16] {-0.11}	-1.082 [-1.30] {-0.79}		

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Table 5: Meta-Analysis: Results using weighted least squares (continued)

	(1) Full specification	(2) Specific 1	(3) Specific 2	(4) Specific 3
<i>Control variables</i>				
Ideology	-1.132 [-2.59]** {-2.15}**	-1.235 [-3.64]*** {-2.89}***	-1.077 [-1.80]* {-1.21}	-0.836 [-1.32] {-0.85}
Government stability	-0.232 [-0.41] {-0.40}	0.928 [1.68] {1.11}	0.967 [1.18] {0.69}	0.218 [0.31] {0.22}
Election year	0.778 [1.57] {1.31}	-0.316 [-0.78] {-0.65}	-0.868 [-1.47] {-0.96}	0.0586 [0.11] {0.05}
Debt	-0.269 [-0.50] {-0.36}	-0.265 [-0.57] {-0.43}	-0.663 [-1.06] {-0.69}	-0.272 [-0.49] {-0.37}
Preferences and culture	-0.273 [-0.85] {-0.81}	-0.0846 [-0.26] {-0.21}	-0.337 [-1.14] {-0.87}	-0.244 [-0.77] {-0.56}
Output gap	-1.443 [-1.88]* {-1.41}			
Growth	0.224 [0.54] {0.44}			
Openness	-0.885 [-1.93]* {-3.10}***			
Dependency ratio	2.124 [2.22]** {1.85}*	1.492 [1.77]* {1.39}	-0.0905 [-0.08] {-0.04}	0.291 [0.25] {0.16}
Supranational rule	-0.0219 [-0.04] {-0.02}			
Budgetary rules	0.591 [0.82] {0.63}	0.176 [0.46] {0.30}	-0.00588 [-0.01] {-0.02}	-0.681 [-1.34] {-0.88}
Number of covariates	0.0972 [0.95] {0.92}	0.0708 [0.93] {0.86}	0.111 [0.88] {0.64}	0.145 [1.22] {0.82}
<i>Publication bias, Baseline: Refereed journal</i>				
Working paper	1.943 [2.30]** {1.23}	1.783 [4.01]*** {2.08}**	1.619 [1.99]* {0.85}	0.997 [1.69] {0.94}
Book	0.847 [1.62] {1.32}	0.665 [3.38]*** {8.68e+18}***	0.191 [0.56] {0.40}	0.226 [0.49] {0.38}
<i>Estimation method, Baseline: OLS</i>				
2SLS IV	2.789 [3.19]*** {2.89}***	0.97 [0.52] {0.44}	1.296 [0.83] {0.69}	0.0788 [0.04] {0.07}
GMM	0.0266 [0.05] {0.10}	0.309 [0.60] {0.58}	-0.398 [-0.51] {-0.28}	-1.21 [-1.21] {-0.75}
Other	-0.0503 [-0.16] {-0.16}	0.0787 [0.24] {0.19}	0.0167 [0.04] {0.01}	-0.211 [-0.55] {-0.49}

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Table 5: Meta-Analysis: Results using weighted least squares (continued)

	(1) Full specification	(2) Specific 1	(3) Specific 2	(4) Specific 3
<i>Econometric specification, Baseline: Robust S.E.</i>				
Dynamic specification	0.545 [1.02] {0.78}	0.728 [1.72]* {1.12}	0.746 [1.12] {0.82}	0.783 [0.93] {0.59}
Time fixed effects	-0.918 [-1.83]* {-1.54}	-0.548 [-1.29] {-0.80}	0.0976 [0.25] {0.23}	-0.122 [-0.28] {-0.19}
Jurisdiction fixed effects	0.644 [1.29] {1.08}	0.57 [1.36] {1.14}	0.584 [1.23] {1.11}	0.778 [1.58] {1.23}
Identification strategy	-2.239 [-3.58]*** {-3.10}***	-0.136 [-0.09] {-0.09}	-0.658 [-0.47] {-0.37}	0.816 [0.46] {0.26}
Plain S.E.	-1.163 [-2.22]** {-1.98}**	-0.527 [-1.34] {-0.98}	-1.228 [-1.98]* {-1.24}	-1.998 [-2.96]*** {-1.58}
Clustered S.E.	0.0525 [0.09] {0.03}	0.078 [0.25] {0.20}	0.0254 [0.05] {0.00}	-0.441 [-1.03] {-0.76}
Bootstrapped S.E.	0.921 [0.88] {0.74}	-1.995 [-1.66] {-1.10}	-0.746 [-0.33] {-0.12}	-2.675 [-1.58] {-1.11}
<i>N</i>	889	889	889	889
<i>R</i> <sup>2</sup>	0.368	0.343	0.285	0.253

Notes: Clustered *t*-statistics in brackets, wild-cluster-bootstrapped *t*-statistics in braces. All specifications estimated using weighted least squares. Data are winsorised excluding the top and bottom 1% percentiles. Analytical weights equalising the weight for every study are included.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 6 Conclusions

The empirical literature on the budgetary implications of numerical fiscal rules has developed over the recent years. It now has reached a critical mass so that meta-analytical analyses become meaningful for taking stock of the overall message. According to our preliminary results which are still based on a incomplete coverage of primary studies the following picture emerges: Overwhelmingly, the existing studies point to significant effective constraints associated with the existence of fiscal rules. This finding holds independent from the variation of study characteristics over the type of fiscal rule indicators or its source, the precise definition of the fiscal variable in focus, and without a major importance of the wealth of other control variables employed. There are some interesting differences, however: Studies dealing with regional jurisdictions (like states or cantons) tend to show a lower effect compared to rules at the national level. Furthermore, the old experience that editors like significant findings is supported: journal articles report more significant results compared to working papers. A particularly interesting finding is that the effectiveness result also appears to survive the criticism that rules are just the endogenous reflection of fiscal preferences: even studies with a comprehensive control for fiscal preferences do not lead to systematically weaker results in terms of statistical significance.

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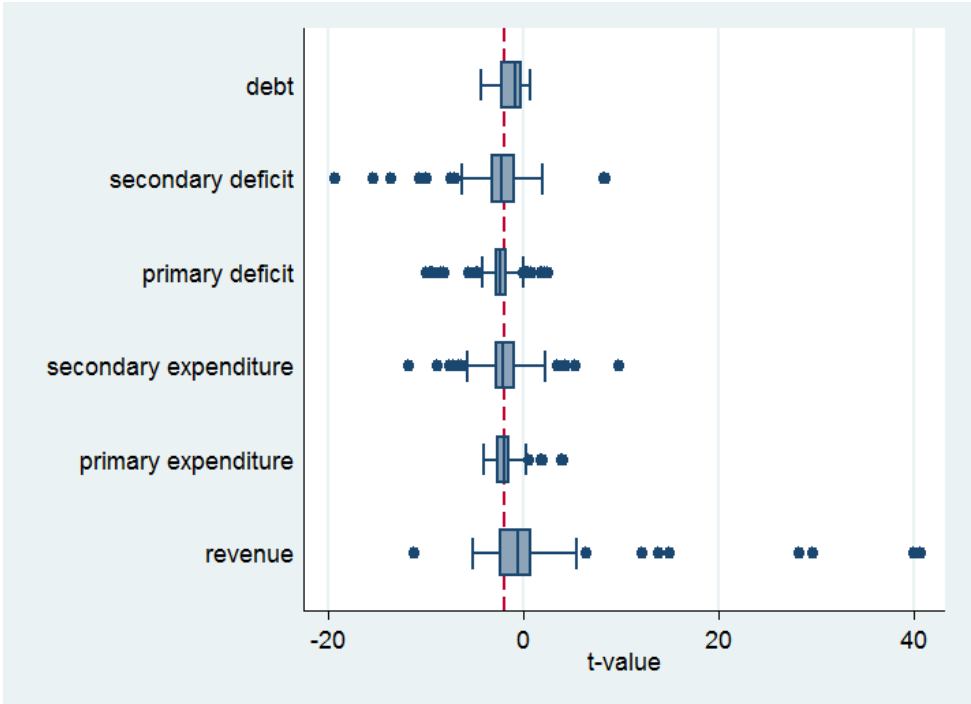
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# A Appendix

Figure A.1: Boxplot for the classification of the dependent variable based on the full sample without windsorising



Notes: Data of the full sample without windsorising the top and bottom 1% percentiles and without analytical weights.

Table A.1: Variable Description

Variable	Description
	<i>Dummy = 1 if the primary study's</i>
<i>Classification of dependent variable</i>	
Debt	Dependent variable is debt.
Secondary deficit	Dependent variable is secondary deficit.
Primary deficit	Dependent variable is primary deficit.
Secondary expenditure	Dependent variable is secondary expenditure, including subcategories of spending, e.g., expenditure for interest payments.
Primary expenditure	Dependent variable is primary expenditure.
Revenue	Dependent variable is revenue, including subcategories, e.g., tax revenue or social security contributions.
<i>Source of fiscal rules data</i>	
ACIR	Fiscal rules data are from the Advisory Commission on Intergovernmental Relations (ACIR).
EC	Fiscal rules data are from the European Commission (EC).
IMF	Fiscal rules data are from the International Monetary Fund (IMF).
OECD	Fiscal rules data are from the Organization for Economic Cooperation and Development (OECD).
Non-official	Fiscal rules data are from sources other than ACIR, EC, IMF, or OECD, including own computations by the author(s), e.g., Feld and Kirchgässner (2008).
<i>Type of fiscal rule</i>	
Deficit rule	Fiscal rule indicator comprises a deficit rule.
Debt rule	Fiscal rule indicator comprises a debt rule.
Expenditure rule	Fiscal rule indicator comprises an expenditure rule.
Revenue rule	Fiscal rule indicator comprises a revenue rule.
<i>Codification of fiscal rule</i>	
Dummy indicator	Fiscal rule indicator is coded as dummy variable.
Discrete indicator	Fiscal rule indicator is coded as discrete variable.
Continuous indicator	Fiscal rule indicator is coded as continuous variable.
<i>Administrative level</i>	
National	Administrative level is the national level.
State	Administrative level is the state level.
Municipality	Administrative level is the municipal level.
<i>Publication bias</i>	
Refereed journal	Observations are published in a refereed journal.
Book/book section	Observations are published in a book or book section.
Working paper	Observations are published in a working paper.

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Table A.1: Variable Description (continued)

Variable	Description <i>Dummy = 1 if the primary study's</i>
<i>Estimation method</i>	
OLS	Estimation method is OLS including estimations applying a within transformation or least squares dummy variables (LSDV).
2SLS (IV)	Estimation method is an instrumental variables approach (2SLS) with the fiscal rule indicator as the instrumented variable.
GMM	Estimation method is GMM.
Other method	Estimation method is not OLS, 2SLS (IV), or GMM. The category comprises, e.g., matching, logit estimations, or fixed effects vector decomposition (FEVD) estimations.
<i>Econometric specification</i>	
Time fixed effects	Econometric specification includes time fixed effects.
Jurisd. fixed effects	Econometric specification includes jurisdiction fixed effects, e.g., country fixed effects.
Dynamic specification	Econometric specification includes the lagged dependent variable.
Identification strategy	Econometric specification is based on a credible identification strategy for the fiscal rule indicator, e.g., instrumental variables (Luechinger and Schaltegger, 2013; Debrun et al., 2008).
Plain S.E.	Econometric specification includes plain standard errors.
Robust S.E.	Econometric specification includes robust standard errors.
Clustered S.E.	Econometric specification includes clustered standard errors.
Bootstrapped S.E.	Econometric specification includes bootstrapped standard errors.
<i>Control variables</i>	
Ideology	Control variables comprise government ideology indicators.
Government stability	Control variables comprise government stability indicators, e.g., tenure of the government or specific index values.
Election year	Control variables comprise variables indicating election years.
Debt	Control variables comprise debt.
Preferences and culture	Control variables comprise preferences and cultural proxies, e.g., language dummies, regional dummies, or fiscal preferences dummies.
Dependency ratio	Control variables comprise dependency ratio indicators, e.g., share of working population in total population.
Output gap	Control variables comprise the output gap.
Growth	Control variables comprise GDP growth indicators.
Openness	Control variables comprise openness indicators, e.g., sum of exports and imports to GDP.
Budgetary rules	Control variables comprise indicators for budgetary rules, e.g., delegation vs. contract approach.
Supranational rule	Control variables comprise an indicator for a supranational fiscal rule, e.g., the Stability and Growth Pact.