

The economic consequences of fiscal rules*

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Abstract

Fiscal rules are controversial. They mitigate politicians' flexibility in responding to shocks and pursuing expansionary fiscal policy. They help, however, to handle politicians' commitment problem in fiscal policies. I portray the new and fast growing empirical literature in public economics that examines the economic consequences of fiscal rules. The survey encompasses the literature on fiscal rules at the national, sub-national and local level. The results show that fiscal rules reduce budget deficits, public spending and borrowing costs and increase GDP growth. The results do not suggest that fiscal rules decrease public investment. Future research should examine in more detail the unintended effects of fiscal rules such as how they relate to creative accounting.

Keywords: Fiscal Rules

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1 Introduction

Fiscal rules are enshrined in law to handle a trade-off between commitment and flexibility. On the one hand, fiscal rules are helpful because politicians need to handle a commitment problem in fiscal policies. On the other hand, fiscal rules have costs: they constrain politicians' flexibility. In recessions, for example, expansionary fiscal policy is needed to stabilize the economy. Politicians may want to stimulate demand, increase public expenditure and run budget deficits. How the trade-off between commitment and flexibility should be handled and how fiscal rules should be designed, is quite controversial.

Advocates and opponents of fiscal rules discuss the characteristics and economic consequences of fiscal rules - especially in times when exogenous shocks such as pandemics influence fiscal policies. Events like the COVID-19 pandemic and Russia's war against Ukraine gave rise to expansionary fiscal policies and increased public debt. Governments put together rescue packages to tackle the COVID-19 pandemic and rising energy prices. Governments also wish to increase military expenditure to address new geopolitical risks that have become visible after Russia's war against Ukraine. What is more, governments wish to spend more on environmental affairs and renewable energies to handle climate change. Opponents of fiscal rules propose that fiscal rules need to be revised if not abolished, especially in times when many extraordinary events occur. Advocates of fiscal rules maintain, by contrast, that fiscal rules are needed, in particular, in times of extraordinary events. How fiscal rules influence economic outcomes remains as an empirical question. I survey the empirical evidence on the economic consequences of fiscal rules.

Fiscal rules are in place at the supranational level, the national level and the sub-national level. A prominent example for supranational fiscal rules is the Stability and Growth Pact (SGP) in the European Union (EU). In 1992, EU member states have signed the Maastricht Treaty and agreed that national debt-to-GDP ratios should not exceed 60% and budget deficits should not exceed 3% of GDP. Many countries have implemented fiscal rules at the national level. Examples include the Swiss and the German debt brake which became effective in 2003 and 2016. Sub-national governments in federal states such as the United States, Canada, Switzerland and Germany implement fiscal rules. Local governments are also faced with fiscal rules, e.g. Italian or Colombian municipalities.

Governments may self-impose fiscal rules. For instance, the US state governments decide about introducing and designing fiscal rules. National governments impose fiscal rules on sub-national governments. International organizations such as the International Monetary Fund (IMF) or the Worldbank negotiate fiscal rules with national governments as part of the conditionality for credit support. I focus on consequences of fiscal rules at the national, sub-national and local level. It is quite difficult to investigate consequences of fiscal rules at the supranational level because there is hardly any variation across countries to be exploited. Fiscal rules at the supranational level, especially those in the EU are discussed in the scientific and public discourse (e.g. [Debrun](#)

et al., 2008; Romer and Romer, 2019; Blanchard et al., 2021; Caselli and Wingender, 2021).

Major questions are which types of fiscal rules are successful in consolidating budgets and which characteristics of the fiscal rules are useful. Types include rules on balanced budgets, debt rules, expenditure or tax revenues. Characteristics include, for example, the legal basis and escape clauses. Those characteristics describe the stringency of fiscal rules. I discuss how the individual types of fiscal rules influence the outcome variables. Budget balance rules and expenditure rules stand out as more effective than revenue rules. Stringent rules are more effective than lax rules in, for instance, reducing budget deficits.

The literature on the consequences of fiscal rules has been inspired by studies on fiscal rules in the US states. Scholars examine, for example, whether fiscal rules reduce fiscal deficits and public expenditure and whether fiscal rules help to mitigate effects of electoral motives and government ideology on fiscal outcomes (e.g. Von Hagen, 1991; Alt and Lowry, 1994; Poterba, 1994, 1995; Bohn and Inman, 1996; Rose, 2006). Many of the issues originally investigated for the US states have been translated to the national level. There is quite some evidence showing how fiscal rules relate to economic outcomes at the national level.

The results at the national and the sub-national level often report correlations between fiscal rules and the outcome variables. Governments at the national and sub-national level self-impose fiscal rules many times. There is selection into treatment. Governments in countries with electorates which prefer a small size of government are more likely to implement fiscal rules than governments in countries which prefer a large size of government. Those preferences of the electorate may be correlated with both the presence of fiscal rules and outcome variables to be examined such as budget deficits (Krogstrup and Wälti, 2008). Clearly, scholars include potential confounders such as electorates' preferences in their econometric models and discuss the role of potential confounders in their estimates. One cannot rule out, however, that unobserved third variables are correlated with both fiscal rules and outcome variables. Another source of endogeneity bias is reverse causality between fiscal rules and outcome variables such as budget deficits and expenditure: fiscal rules may be introduced or become more stringent when budget deficits are large or expenditure increased to a large extent because fiscal discipline is required. Or just the opposite: fiscal rules may be introduced or become more stringent when budget deficits are small or expenditure decreased because politicians believe that the electorate will support those fiscal rules in times when the rules are easy to comply with. Concerns about endogeneity of fiscal rules require rigorous empirical strategies. Just a few studies handle the endogeneity of fiscal rules by using instrumental variables and report causal effects. In any event, when discussing the evidence at the national and sub-national level, I use wordings such as “effect” or “influence” which is not intended to fully claim causality. I describe which studies are in my view more successful in arriving at causal results than others. Fiscal rules at the local level are often imposed by national or sub-national governments. Investigating rules that are not self-imposed helps to avoid selection into treatment

and to provide causal evidence. The caveat of the evidence based on the local level is whether results are externally valid. What could we infer for other countries when we have causally identified estimates on how fiscal rules influence budget deficits in small Italian or Colombian municipalities? My survey shows that the effects of fiscal rules on individual outcomes such as budget deficits and expenditure are quite uniform - the sample and rigor of identification notwithstanding.

Fiscal rules influence macroeconomic outcomes such as borrowing costs, fiscal and output volatility and economic growth. Borrowing costs are measured as spreads between interest rates on national governments' bonds and a reference like US treasury bonds. Markets are concerned that national governments which are pursuing non-sustainable fiscal policies would have issues in repaying debt. Those governments therefore need to pay high risk premia when issuing bonds. Empirical studies show that risk premia are more than 1.5 percentage points higher in countries with lax than stringent fiscal rules (e.g. [Iara and Wolff, 2014](#); [Heinemann et al., 2014](#); [Badinger and Reuter, 2017](#); [Afonso and Jalles, 2019](#); [Thornton and Vasilakis, 2018, 2020](#)). Stringent fiscal rules also promoted GDP growth: In the long run, real GDP was around 15% higher in countries with constitutional fiscal rules than in countries without those rules ([Gründler and Potrafke, 2023](#)). Fiscal rules also decreased fiscal and output volatility (e.g. [Sacchi and Salotti, 2015](#); [Badinger et al., 2017](#); [Larch et al., 2021](#); [Reuter et al., 2022](#)).

Political-economic determinants such as electoral motives and government ideology influence economic policies and outcomes. The political business cycle theories describe that election-motivated politicians implement expansionary fiscal policies before elections ([Nordhaus, 1975](#); [Rogoff and Sibert, 1988](#); [Rogoff, 1990](#)). Expansionary fiscal policies include increasing public spending and deficits. Fiscal rules have been shown to mitigate political business cycles ([Rose, 2006](#); [Bonfatti and Forni, 2019](#); [Gootjes et al., 2021](#); [Strong, 2022](#)). The partisan theories describe that leftwing governments implement more expansionary policies than rightwing governments ([Hibbs, 1977](#); [Chappell and Keech, 1986](#); [Alesina, 1987](#)). The hypothesis to be investigated is that fiscal rules mitigate ideology-induced policies. Scholars have not yet examined this question at the national level, but in the US states. The results for the US states are mixed. It is hard to conclude that Democratic state governments have a larger size of government than Republican state governments when fiscal rules are stringent.

The evidence at the local level corroborates many results based on data from the national and sub-national level. Findings include lower budget deficits and smaller political budget cycles when fiscal rules were present/stringent. The institutions by which fiscal rules are implemented give rise to quasi-exogenous variation in fiscal rules. An excellent laboratory is the Italian Domestic Stability Pact (DSP) - a program that was in place over the period 1999-2015. The central government imposed fiscal rules on municipalities in the year 1999 and relaxed these fiscal rules for municipalities with less than 5,000 inhabitants in the year 2001. [Grembi et al. \(2016\)](#) compare fiscal policy outcomes in municipalities with slightly less and slightly more than 5,000 inhabitants before and after the year 2001 in a difference-in-discontinuities design. The results

show that relaxing fiscal rules gave rise to higher fiscal deficits and lower taxes. Fiscal deficits increased by around 30 percent over the course of four years after the fiscal rules were relaxed. I also discuss how fiscal rules at the local level in Colombia, Germany, Norway and Japan influenced economic outcomes and policy measures.

Future research should also examine in more detail the extent to which fiscal rules give rise to creative accounting and extra budgets. A prominent example of an extra budget was Germany's *Sondervermögen* in 2022: The German debt brake requires balanced budgets. When Russia attacked Ukraine in February 2022, the German government wanted to increase military expenditure but could not because of the fiscal rule. New research on fiscal rules and creative accounting requires compiling data on extra budgets and supplementary budgets. Some studies examine late budgets and supplementary budgets in the US states (Wlezien, 1993; Andersen et al., 2012). There is no dataset yet at the national level on supplementary budgets. There are some early studies on fiscal rules and creative accounting (Von Hagen and Wolff, 2006; Canova and Pappa, 2006; Hirota and Yunoue, 2022), but more work on this issue is needed.

Previous surveys on the consequences of fiscal rules include Poterba (1995), Poterba (1996), Schaltegger (2002), Rose (2010), Wyplosz (2012), Eyraud et al. (2018), Blesse et al. (2023) and Brändle and Elsener (2023). Consequences of fiscal rules have also been portrayed by the meta-analysis of Heinemann et al. (2018). The advantage of a survey as compared to a meta-analysis is to categorize individual studies and spell out which studies are very well executed and should receive more attention than other less well executed studies. I engage with individual studies and portray the individual strengths and weaknesses. There is no other survey that describes consequences of fiscal rules at the national, sub-national and local level in such an encompassing manner.

My survey is still selective, because the empirical literature on economic consequences of fiscal rules is vast and fast growing. My choice of articles is guided by four criteria. First, I focus on the most important outcome variables. Second, I focus on studies that employ credible methods of causal inference. Third, I discuss previous studies that do not establish causality but received a great deal of attention in the literature. Fourth, I discuss individual correlational studies when they contribute to (new) research questions that I consider to be important.

I review empirical research on how fiscal rules influence observable economic outcomes, but not necessarily how fiscal rules influence unobservable social welfare. I relate to the theoretical literature that is concerned with social welfare in the next section. I discuss the evidence at the national level in section 3 and continue with the evidence at the sub-national level in section 4 and at the local level in section 5. The last section concludes.

2 Theory

A theoretical literature describes economic consequences of fiscal rules. This literature focuses on the welfare implications of fiscal rules. I discuss main results that should serve as a helpful anchor and point of reference for the empirical results.

2.1 Underlying problem

The underlying problem that should be addressed by fiscal rules is a trade-off between flexibility and commitment problems (e.g. [Athey et al., 2005](#), [Amador et al., 2006](#), [Azzimonti et al., 2016](#), [Halac and Yared, 2014, 2018, 2022a,b](#), [Felli et al., 2021](#), [Dotti and Janeba, 2023](#)). On the one hand, there are benefits from the flexibility provided by larger choice sets when agents expect to receive information (e.g. [Amador et al., 2006](#)). Fiscal rules mitigate politicians' flexibility. When recessions arise, politicians may want to enjoy flexibility and implement expansionary fiscal policies. On the other hand, agents often suffer from temptation and self-control problems. They suffer from temptation for high present consumption and are present-biased.¹ Fiscal rules therefore help politicians to control their desire to, for example, increase public expenditure before elections and issue public debt.² Politicians have a problem to commit themselves to reduce deficits and repay debt in the future. There is temptation to postpone fiscal consolidation. A fiscal rule may therefore be part of the optimal commitment policy.

The short-run and long-run costs and benefits of imposing fiscal rules are likely to differ ([Azzimonti et al., 2016](#)). Costs of imposing fiscal rules are especially high in the short-run because public good provision under fiscal rules only works through tax increases. Taxes become more volatile. Governments may also hesitate in providing public goods and when they wish to avoid tax increases. In the long-run, however, citizens benefit from fiscal rules because fiscal rules decrease public debt. An advantage of decreasing debt in the long-run is that less revenue is needed to servicing public debt.

2.2 Effects of fiscal rules on fiscal decisions

Theory proposes fiscal rules as a solution to the trade-off between the commitment problem and flexibility. An interesting question then is how fiscal rules can be expected to impact on fiscal decisions. Just a few models offer some implications on this question. [Amador et al. \(2006\)](#) consider optimal fiscal policies when a deficit rule is present. The government receives a private information shock on the value of public spending. When

¹The present-bias of politicians may lead the economy to fluctuate between regimes of fiscal responsibility and irresponsibility ([Halac and Yared, 2022b](#)). The present-bias needs to be large to induce fiscal regimes. When the present-bias is large, the threat to fiscal irresponsibility in the future sustains fiscal responsibility in the present.

²Governments often have an incentive for accumulating debt today to tie the hands of the succeeding governments ([Persson and Svensson, 1989](#); [Alesina and Tabellini, 1990](#)).

the shock to the value of public spending is low, the government borrows within the limit, that is it borrows not as much as the deficit rule would allow. When the shock to the value of public spending is higher, the government borrows at the limit, that is it borrows as much as the deficit rule allows. Optimal fiscal policies change when penalties for not fulfilling the fiscal rules are introduced (Halac and Yared, 2022a). Introducing penalties can also be viewed as considering fiscal rules with escape clauses. Having penalties (escape clauses) in place, triggers governments to fulfill the fiscal rules. Violation of fiscal rules occurs when shocks are large and unlikely and penalties are weak.

In federal states, fiscal rules influence the interplay between the central government and local governments. Dovic and Kirpalani (2020) propose a theory on the role of fiscal rules in federal states. The authors use a reputation model with two types of central government: a commitment and an optimizing type.³ The local governments have uncertainty about the type of the central government (which cannot commit to impose the fiscal rules). How fiscal rules influence overall debt depends on the reputation of the central government. The reputation of the central government is measured by the probability that local governments assign to it being a commitment type. Fiscal rules increase debt relative to the case of no rules if the central government has low reputation. The intuition for this result is as follows. There is a punishment for fiscal rule enforcement. This punishment increases the cost of the government to maintaining a good reputation. The optimizing type reveals its type earlier relative to the case with no fiscal rules. The early resolution of uncertainty increases the likelihood that local governments increase debt. By contrast, fiscal rules decrease debt if the central government enjoys a high reputation. The reason is that local governments anticipate facing a penalty for violating fiscal rules.

2.3 Effects on other macroeconomic outcomes

Another interesting question is how fiscal rules influence macroeconomic outcomes (beyond fiscal aggregates) and through which channels. The theories that are concerned about welfare implications provide very few indications on effects of fiscal rules on macroeconomic outcomes. Halac and Yared (2018) propose a theory to compare coordinated and uncoordinated rules and show how fiscal rules influence interest rates. Coordinated fiscal rules are chosen jointly by a group of countries. A good example are the supra-national fiscal rules in the EU. Uncoordinated fiscal rules are national fiscal rules. The authors are interested in the optimal design of coordinated rules and investigate whether coordinated fiscal rules should be more stringent than uncoordinated rules. Governments are present-biased and face the trade-off between flexibility and commitment. Under uncoordinated fiscal rules, national government face the trade-off between flexibility and commitment individually. Under uncoordinated fiscal rules, by

³Dovic and Kirpalani (2021) also examine the optimal design of rules in the presence of time inconsistency problems and uncertainty about whether the government can commit to follow the rule. This study does not relate, however, fiscal rules to fiscal policies.

contrast, national governments face the trade-off between flexibility and commitment jointly. There are externalities: a government's debt (and decision to impose a fiscal rule) exerts an externality on other governments through interest rates. Lowering flexibility by imposing a fiscal rule decreases interest rates. Lower interest rates give rise to redistributing resources towards governments that borrow more toward governments that borrow less. The results show that when present bias is small, coordinated rules are more stringent than uncoordinated rules. The reason is that individual countries do not internalize the redistributive effects of interest rates. By contrast, when present-bias is large, the optimal coordinated fiscal rule is less stringent than the uncoordinated fiscal rule and interest rates are higher under coordination.

2.4 Unintended effects

Fiscal rules may give rise to unintended effects. An example is how tightening fiscal rules increases entitlements (future unfunded government obligations). Such entitlements include spending obligations on pensions and health care (Bouton et al., 2020). This unintended effect of fiscal rules is based on the assumption of capital market frictions: costs of private and public borrowing differ, citizens have to pay higher interest rates than governments. The difference between interest rates for the private and the public sector crowds out private saving. The government becomes active to increase entitlements, citizens do not. The government has to consider, however, a budget constraint, especially when fiscal rules are present. Increasing spending for entitlements therefore gives rise to decreasing spending for public goods. Welfare decreases.

2.5 Important preconditions for successful rules

The success of fiscal rules is likely to depend on preconditions. An important precondition is political polarization (Piguillem and Riboni, 2021). When political polarization is high, very strict fiscal rules (such as government shutdowns or harsh spending cuts) decrease public debt because the opposition has large bargaining power (Piguillem and Riboni, 2021). The opposition would only agree to increase debt when spending for the good she prefers would increase. Fiscal rules are used as “bargaining chips”. The government also realizes that when the opposition party would win the next election, the opposition party is likely to share total resources. The opposition party considers that low debt today increases opportunities to increase spending when she might be in office in the future. When political polarization is low, by contrast, lax fiscal rules (e.g. a budget balance rule) are preferred.

2.6 Types of fiscal rules

Do different types of rules have different effects on economic outcomes? The theoretical literature on welfare implications does not yet provide answers to this question. It is interesting to observe, however, which types of fiscal rules the theories consider.

Theories on fiscal rules often consider deficit or budget balance rules (Amador et al., 2006, Halac and Yared, 2014, Azzimonti et al., 2016, Halac and Yared, 2018, Doviš and Kirpalani, 2021, Halac and Yared, 2022a, Dotti and Janeba, 2023), some consider debt brakes (Hatchondo et al., 2022), and spending rules (Piguillem and Riboni, 2021). This pattern is quite in line with the frequency of types of fiscal rules in practice to which I now turn in the next section.

3 Empirical evidence: the national level

3.1 Measuring fiscal rules

The gold standard to measure fiscal rules at the national level is the data of Schaechter et al. (2012) which was updated of Davoodi et al. (2022). The dataset of Davoodi et al. (2022) encompasses 106 countries over the period 1985-2021. It includes information about national and supranational fiscal rules. The authors distinguish between budget balance rules (BBR), debt rules (DR), expenditure rules (ER), and revenue rules (RR). These rules apply to the general or central government or the public sector. BBR require the budget to be balanced. DR often set a ceiling on debt or debt-to-GDP ratios. In a similar vein, ER and RR typically set a ceiling on the ratio between public expenditure and tax revenue and GDP or on the growth of those ratios. Governments frequently use BBR and less so RR. In 2021, for example, 93 countries used BBR, 85 countries used DR, 55 countries used ER and 17 countries used RR. Many times, governments use combinations of the individual types of rules. A prime example is combining a debt rule together with operational limits on expenditures and/or the budget balance.

Fiscal rules may well differ by individual characteristics such as the legal basis and escape clauses. The dataset of Davoodi et al. (2022) includes information about those individual characteristics which help to measure stringency of fiscal rules. The *legal basis* may be constitutional, an international treaty, statutory, a coalition agreement or a political agreement. Having a fiscal rule based in the constitution is the strongest legal commitment, having a fiscal rule confirmed by a political agreement is the weakest legal commitment. *Escape clauses* also describe fiscal rules. When natural disasters such as earthquakes or pandemics hit, escape clauses provide governments leeway to not comply with fiscal rules in individual years. Before the COVID-pandemic hit, two thirds of the countries with fiscal rules had included escape clauses. *Formal enforcement* mechanisms require “integrating fiscal rules in an annual budget preparation and medium-term fiscal framework, as well as holding the government accountable for ex-post compliance” (page 9). *Stabilization* means that fiscal rules are adjusted for cyclical fluctuations. An example is that BBR allow for small budget deficits (in Germany 0.35% of GDP) because of business cycle fluctuations. Davoodi et al. (2022) provide a stringency index based on the four criteria (i) legal basis, (ii) presence of a monitoring mechanism, (iii) enforcement and correction mechanism in place, (iv)

flexibility and resilience against shocks.

3.2 Deficits and debt

Fiscal rules are implemented to encourage fiscal policies to be sustainable. Fiscal sustainability is difficult to measure, though. Fiscal sustainability often improves when budget deficits and debt-to-GDP ratios decrease over time. Budget deficits and debt-to-GDP ratios are among the main outcome variables when scholars investigate whether fiscal rules are effective. Many studies examining consequences of fiscal rules use budget deficits (or primary surpluses or debt-to-GDP ratios) as the dependent variable (Table 1). The results suggest that fiscal rules are effective in decreasing budget deficits (see, for example, [Heinemann et al., 2018](#), [Caselli and Reynaud, 2020](#)).

An early and well recognized study is [Hallerberg et al. \(2007\)](#). The authors have self-compiled data on fiscal rules based on survey evidence and interviews among economic experts. Fiscal rules are coded “according to their stringency and also budgetary processes according to the degree of delegation” (p. 347). The sample includes 15 EU countries over the period 1985-2004. The authors regress the first difference in the public debt-to-GDP ratio (general government) on the fiscal rules index, the lagged dependent variable and other control variables. The rules show that stricter fiscal rules were associated with lower deficits.

[Bergman et al. \(2016\)](#) use data for 27 EU countries over the period 1990-2012 and investigate how fiscal rules and government efficiency interact. Fiscal rules decrease the structural budget deficit and tend to be more effective at lower levels of government efficiency. The authors conclude: “A key finding of this paper is that a budget balance budget rule is clearly the most effective rule in reducing the deficit bias” (p. 17f.).

In the early studies, scholars regressed the fiscal outcome variable such as primary surpluses or debt-to-GDP ratios on a fiscal rule variable and control variables. When doing so, the fiscal rule variable is likely to be endogenous. First, it is conceivable that there is reverse causality between the public deficit and the fiscal rule variable: politicians have implemented fiscal rules because deficits or debt-to-GDP ratios were pronounced. Second, there may well be third variables that are likely to be correlated with both the public deficit and the fiscal rule variables. When those third variables are not included in the econometric model, the parameter estimate of the fiscal rule variable is prone to omitted variable bias. Clearly, scholars made quite some attempts to control for potential confounders such as political preferences ([Krogstrup and Wälti, 2008](#)): when electorates are fiscally conservative, for example, they prefer both low public deficits and fiscal rules which prevent public deficits and debt in the future. There may, however, also be unobserved variables that are correlated with the fiscal outcome variables and fiscal rules. Scholars did not yet employ the tests by [Oster \(2019\)](#), [Diegert et al. \(2022\)](#) and [Masten and Poirier \(2022\)](#) to investigate whether unobserved variables explain away or change the sign of the estimated effect of fiscal rules on fiscal outcomes.

Scholars employ identification strategies to estimate causal effects of fiscal rules

on budget deficits. An example is the study of [Caselli and Reynaud \(2020\)](#) who use the presence of fiscal rules in neighboring countries as an instrumental variable for fiscal rules in the considered country. Employing the presence of (fiscal) institutions in neighboring countries was promoted, for example, by [Acemoglu et al. \(2019\)](#) when investigating how democracy influences economic growth. I believe that using the presence of fiscal rules in neighboring countries as an instrumental variable for fiscal rules is suitable. The sample of [Caselli and Reynaud \(2020\)](#) includes 142 countries over the period 1985-2015. The results show that the two-stage-least squares (2SLS) estimates based on the instrumental variable changes the inferences of the OLS model. The OLS results indicate that overall fiscal rules were associated with smaller public deficits. When employing 2SLS, the parameter estimate of the fiscal rules variable lacks statistical significance. This specification considers the presence of any fiscal rule. When the authors consider the stringency of fiscal rules (as measured by the IMF's continuous index of fiscal rules' design)⁴, however, the 2SLS show that fiscal rules decreased public deficits. The public deficit as a share of GDP in a country with a badly designed fiscal rule was around 0.7 percentage points higher than the budget balance in a country with a well designed fiscal rule. [Caselli and Reynaud \(2020\)](#) make a strong case for fiscal rules being effective in reducing budget deficits.

An important study examining how fiscal rules relate to deficits, public debt and expenditure is [Asatryan et al. \(2018\)](#). The authors use constitutional BBR to measure fiscal rules. Constitutional BBR are included in the constitutions of the individual countries. As compared to BBR that are not included in the countries' constitutions, constitutional BBR are often hard to be circumvented by politicians. The sample includes 132 countries over the period 1945 to 2015. The results show that constitutional BBR were associated with lower public debt and public expenditure. For example, the debt-to-GDP ratio was around 11 percentage points lower in countries with constitutional BBR than in countries without constitutional fiscal rules. [Asatryan et al. \(2018\)](#) estimate generalized diff-in-diff models and discuss issues regarding identifying causal effects when investigating how fiscal rules translate into lower debt and deficits. The authors also present case study evidence. They focus on individual countries which have included BBR in their constitutions. The purpose is to compare the development of fiscal policy measures when a constitutional fiscal rule was implemented with a hypothetical scenario (counterfactual) in which the fiscal rule was not implemented. This counterfactual cannot be observed and needs to be estimated. Employing the synthetic control method ([Abadie and Gardeazabal, 2003](#); [Abadie et al., 2010, 2015](#); [Abadie, 2021](#)) is useful to measure such a counterfactual. A prime example is Switzerland which implemented a BBR in its constitution in the year 2003 (see also [Salvi et al., 2020](#) and [Pfeil and Feld, 2023](#)). [Asatryan et al. \(2018\)](#) estimate that the debt-to-GDP ratio decreased by around 30 percentage points as compared to the counterfactual

⁴The strength index by [Schaechter et al. \(2012\)](#) considers the dimensions: broad institutional coverage, independence of the monitoring and enforcement bodies, legal base, flexibility to respond to shocks, existence of correction mechanisms and sanctions.

when the BBR was introduced. Clearly, treatment was not random. The BBR was introduced on purpose to consolidate public budgets.

[Badinger and Reuter \(2017\)](#) corroborate that fiscal rules reduced fiscal deficits. The authors use data for 47 countries over the period 1985-2012. The dependent variable is the structural fiscal balance as a share of potential GDP. Fiscal rules are measured by three indices on the stringency of fiscal rules. The indices are taken from [Badinger and Reuter \(2015\)](#) who employ partially ordered set (POSET) theory to arrive at indices measuring the stringency of fiscal rules. Two indices measure the stringency of BBR and DR. The third index measures general fiscal institutions considering, for example, laws on monitoring performance. The authors address potential endogeneity of the fiscal rule indices by using instrumental variables for fiscal rules. The instrumental variables are measures of checks and balances, government fragmentation and inflation targeting. The authors estimate models including the three instrumental variables together and using the instrumental variables individually. One may well discuss whether these instrumental variables are excludable. Political fragmentation is likely, for example, to directly influence the fiscal balance. In any event, the OLS and 2SLS results show that countries with stringent fiscal rules had smaller fiscal deficits than countries with lax fiscal rules. The baseline result for BBR is, for example, that increasing the BBR fiscal rule index by one standard deviation was associated with an increase of the fiscal balance by around 1.5 percentage points.

Fiscal rules influenced deficits also in relation with other variables. For example, they promoted the effectiveness of fiscal decentralization in lowering budget deficits ([Neyapti, 2013](#)) and reduced deficits and made fiscal adjustments more likely when fiscal transparency was pronounced ([Gootjes and de Haan, 2022](#)). The results of [Maltritz and Wüste \(2015\)](#) show that countries with stringent fiscal rules had larger primary surpluses than countries with lax fiscal rules. The effects of fiscal rules become stronger when the individual countries had fiscal councils that had encompassing tasks to comment on the countries' fiscal policies. Fiscal rules have also influenced fiscal performance in relation with inflation targeting. [Combes et al. \(2018\)](#) show that the primary budget balance as measured as a share of GDP was around 1.5 percentage points higher when countries had fiscal rules as compared to when they did not. The effect of fiscal rules is amplified, however, when countries also implement inflation targeting: the primary budget balance was around 2.3 percentage points higher in countries implementing both fiscal rules and inflation targeting as compared to countries which did not implement fiscal rules and inflation targeting. Clearly, these results portray conditional correlations and no causal effects; [Combes et al. \(2018\)](#) estimate GMM models using internal instrumental variables that are relevant but hardly excludable. The effects are especially pronounced for BBR and ER rules and lack statistical significance for DR rules.

Those empirical studies examining effects of fiscal rules in relation with other variables take up the theoretical literature on the role of preconditions (see [Piguillem and Riboni, 2021](#) in section 2 who propose polarization as an important precondition). There is no empirical study yet which considers political polarization –be it polariza-

tion between political parties or electorates— as a precondition for the success of fiscal rules.

3.2.1 Fiscal rules and fiscal consolidation

Fiscal rules have also been effective in reducing deficits during periods of fiscal consolidation. [Aaskoven and Wiese \(2022\)](#) use data for 19 OECD countries over the period 1967-2013. Periods of fiscal consolidation are measured by employing structural break point tests on the cyclically adjusted primary budget balance. The authors’ sample includes 108 country-year observations of fiscal consolidation periods. The authors regress the change in the debt-to-GDP ratio on the fiscal rule indicators by [Schaechter et al. \(2012\)](#) and find that national fiscal rules were associated with decreasing changes in the debt-to-GDP ratio.

During periods of fiscal consolidation, flexible fiscal rules were effective and prevented politicians from decreasing public investments. [Ardanaz et al. \(2021\)](#) employ data for 75 countries over the period 1990-2018. Fiscal rules are defined to be flexible “with at least one of three features present: (i) provisions that exclude public investment from the perimeter of the rule; (ii) the rule includes cyclically adjusted fiscal targets; or (iii) the rule contains well-defined escape clauses to accommodate exogenous shocks of various sorts, such as natural disasters” (p. 4). Fiscal consolidation is defined “as a two-year period in which the cyclically adjusted primary balance-to-GDP ratio improves each year and the cumulative improvement is at least 2 percentage points of GDP” (p. 4). The authors regress real public investment on a flexible fiscal rule variable, a fiscal consolidation variable and the interaction between them. Marginal effects show that public investment is only reduced during periods of fiscal consolidation when fiscal rules were not flexible.

3.3 Public spending

3.3.1 Total spending and budget composition

Fiscal rules influence public spending and budget composition (Tables 2 and 3). In particular, fiscal rules are expected to decrease overall public spending and to increase consumption as compared to investment expenditure. This notion is present in the public discourse and it relates to the theory suggesting that fiscal rules may increase entitlements and decrease spending on public goods (see [Bouton et al., 2020](#) in section 2). Politicians are often election-motivated and spend public money on issues that are visible to the voters. Transfers such as unemployment benefits, subsidies to public pensions and health systems and subsidies to firms are visible to voters. Investment expenditure is often less visible to voters. Investment expenditure includes spending for public infrastructure such as roads and railways and expenditure for Research & Development (R&D).

The empirical evidence does not suggest that fiscal rules decrease public investment. By contrast, the early study of [Dahan and Strawczynski \(2013\)](#) suggests fiscal rules

increased public investment. The authors employ data for 22 OECD countries over the period 1960-2010 and self-compile data on BBR and ER. Their results show that the ratio of social transfers to government consumption decreased when governments implemented national fiscal rules.

Vinturis (2023) uses data for 185 developing and developed countries over the period 1985-2015. The author uses a matching approach that is intended to compare a country with a fiscal rule with a statistically identical country without a fiscal rule. First, she finds that fiscal rules decrease overall spending. Second, she disentangles the effects of fiscal rules on consumption and investment expenditure. The results show that fiscal rules decrease consumption expenditure. The parameter estimates of the fiscal rule variables are positive and lack statistical significance when public investment expenditure is used as the dependent variable. In a similar vein, fiscal rules are positively associated with the ratio between public investment and consumption expenditure. The effects are driven by BBR and DR and they are more likely to occur in developing than in developed countries. Vinturis (2023) implements a plethora of heterogeneity tests. An important result among the heterogeneity analyses is that fiscal rules are especially effective when they are based on a hard legal basis.

The results of Jürgens (2022) suggest that fiscal rules are negatively correlated with public investment. The author uses data for 23 EU countries over the period 1985-2019. The effect is especially pronounced in recessions. The author makes a strong case for fiscal rules decreasing public investment. I was not convinced. For example, she proposes an IV strategy for robustness tests. However, she does not instrument fiscal rules - her main explanatory variable - but the business cycle variable. Jürgens (2022) also uses the Classification of Functions of Government (COFOG) data and disentangles the types of expenditures. These results suggest that fiscal rules (in recessions, we do not know about the overall effect) were associated with lower spending on Economic Affairs, Housing, Health Care and Social Protection. In a similar vein, the results of Schakel et al. (2018) suggest that fiscal rules were negatively correlated with public health expenditure in OECD countries over the period 1985-2014. Delgado-Téllez et al. (2022) use data for 22 OECD countries over the period 1960-2015 and employ social expenditure and public investment expenditure as their dependent variable. The parameter estimates of the fiscal rules variables often have a negative sign but lack statistical significance in many specifications when public investments are used as the dependent variable. By contrast, the parameter estimates of the fiscal rules variable are numerically large, have a negative sign and are statistically significant when social expenditures are used as the dependent variable. I believe that the strong negative correlation between fiscal rules and spending on Health Care and Social Protection calls into question whether implementing (strict) fiscal rules gives rise to lower public good provision.

Government expenditure was lower in countries with deficit rules embedded in their constitutions than in countries which did not have deficit rules in their constitutions: Blume and Voigt (2013) self-compile data on “constitutional deficit rules”: They use a binary indicator assuming the value one “if there was for a constitutional rule limiting

either deficits or spending” (p. 240). The authors estimate a cross-sectional model using many time invariant explanatory variables measured in the 1990s. Total government expenditure as a share of GDP was around 4.6. percentage points lower in countries with constitutional deficit rules than in countries without constitutional deficit rules.

3.3.2 (Pro)cyclical spending

Governments use public expenditure to respond to the business cycle (stabilization policy): in recessions, governments may want to implement expansionary fiscal policies and increase public spending. In booms, by contrast, governments may want to implement restrictive fiscal policies and decrease public spending. Such counter-cyclical fiscal policies have been advocated, for example, by John Maynard Keynes (Keynes, 1936) and Richard Musgrave (Musgrave, 1959). In practice, fiscal policies were not counter-cyclical. By contrast, fiscal policies were often pro-cyclical.

An important question is how fiscal rules influence the cyclicity of fiscal policies (e.g. Clemens and Miran, 2012). Empirical evidence shows that fiscal rules dampened the pro-cyclicity of fiscal policies: Bergman and Hutchison (2015) use data for 81 countries over the period 1985-2012. The authors regress cyclically adjusted government expenditure on GDP, a measure for fiscal rules and the interaction between GDP and fiscal rules. The results show that fiscal rules mitigated the pro-cyclicity in government spending. Government efficiency also determined the effects of fiscal rules on pro-cyclicity in government spending: when governments were efficient, the effect of fiscal rules on mitigating pro-cyclicity was stronger than when governments were not efficient. In a similar vein, the presence of supranational EU fiscal rules has also been shown to mitigate pro-cyclical fiscal policies (Larch et al., 2021).

Reuter et al. (2022) employ data for the EU28 member states over the period 1996-2015 and show that fiscal rules decrease fiscal volatility. The authors use four measures based on which they compute fiscal volatility: primary expenditure, public consumption, the sum of public consumption and investment and the primary balance. The authors use these four measures as dependent variables, regress their growth rates on the growth rates of GDP plus control variables and compute fiscal volatility based on the residual of the individual regressions. Fiscal rules are measured by the data of Schaechter et al. (2012). The results show that the BBR index was negatively correlated with fiscal volatility, the ER index did not turn out to be a significant predictor of fiscal volatility. The authors also condition the effects of fiscal rules on compliance with fiscal rules and cyclical properties. Complying with fiscal rules does not influence how fiscal rules affect fiscal volatility, but the cyclical properties do: fiscal rules should be “a-cyclical” to reduce fiscal volatility. See also Table 5.

3.4 Macroeconomic outcomes

3.4.1 Borrowing costs

Fiscal rules influence governments' borrowing costs. Large deficits and debt-to-GDP ratios increase the risk of governments' bankruptcy. Fiscal rules are designed and have been shown to decrease budget deficits and debt-to-GDP ratios. Consequently, having fiscal rules in place is expected to decrease risk-premia on government bonds. Empirical evidence corroborates this expectation. The empirical studies arrive at very similar numerical estimates: when fiscal rules are present, interest rates decrease by around 1.5 percentage points ([Heinemann et al., 2014](#); [Iara and Wolff, 2014](#); [Badinger and Reuter, 2017](#); [Thornton and Vasilakis, 2018](#); [Afonso and Jalles, 2019](#); [Thornton and Vasilakis, 2020](#)). What is more, the empirical strategies of the studies are very similar. Scholars regress a country's government bond spread against a (risk-free) bond such as US government bonds on fiscal rule variables. Clearly, those studies do not provide innovative identification strategies. The reported results are, however, strong and suggest that fiscal rules, especially stringent ones, reduce borrowing costs. See [Table 6](#).

[Iara and Wolff \(2014\)](#) use data for 11 EU countries over the period 1999-2009. The dependent variable is the logarithm of government bond spreads against the German Bund based on the yield of their 10-year on-the-run fixed coupon bonds. Fiscal rules are measured by the fiscal rule indicators from the fiscal governance unit of the European Commission's Directorate-General for Economic and Financial Affairs. This index considers the presence of fiscal rules at the local, sub-national and national level. The index is based on five dimensions: 'the statutory base of the rule, room for revising objectives, mechanisms of monitoring compliance with and enforcement of the rule, the existence of predefined enforcement mechanisms, and media visibility of the rule (see [Iara and Wolff \(2014\)](#) page 226). The index assumes values between 0 (very lax rules) and 10 (very stringent rules). The results show that increasing the fiscal rules index by one unit decreases the risk premium by around 23 %.

[Heinemann et al. \(2014\)](#) extend the sample of [Iara and Wolff \(2014\)](#) by also considering some non-EU countries over the period 1993-2008. Their dependent variable is the 10-year spot rate yield differential to Germany. The stringency of fiscal rules are measured the Fiscal Rule Index (FRI) of the European Commission. The results show that strict fiscal rules are correlated with lower bond spreads. The innovation of [Heinemann et al. \(2014\)](#) is to control for stability culture in the individual countries because stability culture is correlated with the stringency of fiscal rules. Countries with strict fiscal rules had low inflation, governments preferring low taxes and high public expenditure and high social trust. The correlation between fiscal rules and bond spreads remains negative and significant when the authors control for the three cultural stability measures. The results also show, however, that fiscal rules were only significantly related to low bond spreads when social trust was low.

[Badinger and Reuter \(2017\)](#) use data for 30(36) countries on short-run(long-run) government bond spreads. Fiscal rules are measured by their indicators on BBR, DR and overall stringency of fiscal rules. The authors estimate their models by OLS and 2SLS (instrumenting fiscal rules by checks and balances, political fragmentation and inflation targeting). The results show that countries with stringent fiscal rules had smaller government bond spreads.

[Thornton and Vasilakis \(2018\)](#) employ a sample of 101 developing and developed countries over the period 1985-2010. The dependent variable is the spread between an individual country's government bonds and the yield on 10-year U.S. Treasury bonds. Fiscal rules are measured by the data from [Schaechter et al. \(2012\)](#). The authors employ propensity score matching to compare a country which has fiscal rules in place with a country that is statistically identical except having a fiscal rule in place. The results show that the borrowing spread decreases by around 1.5% to 1.8% percent when a fiscal balance rule was in place and by around 1.1% to 1.2% when a debt rule was in place.

[Thornton and Vasilakis \(2020\)](#) use a sample of low and middle-income countries over the period 1985-2017. The authors use two measures of borrowing costs: First, the domestic interest rate spread, "which is the average interest rate on bank lending to the private sector minus the treasury bill rate, and the treasury bill rate itself" (p. 501). Second, the interest rate on treasury bills. Fiscal rules are measured by the data of [Schaechter et al. \(2012\)](#). The authors find quite strong effects showing that fiscal rules decreased borrowing costs. When fiscal rules were present, the domestic interest rate spread increased around 1.8 to 2.6 percentage points indicating that borrowing costs of the public sector decreased relative to the private sector. The interest rate on treasury bills decreased by around 1.1 to 1.7 percentage points. The authors also find that more stringent fiscal rules give rise to larger decreases in borrowing costs than lax fiscal rules.

[Afonso and Jalles \(2019\)](#) use data for 34 advanced and 19 emerging countries over the period 1980-2016. Fiscal rules are measured by the data of [Schaechter et al. \(2012\)](#). The dependent variable is the sovereign bond yield spread against an US risk-free bond. The results show that fiscal rules reduced borrowing costs by around 1.2 to 1.8 percentage points. The effects are driven by expenditure rules and the sample of the advanced countries.

3.4.2 Economic growth and GDP's volatility

The effects of fiscal rules on public finances and borrowing costs are likely to translate into effects on economic growth. GDP growth was higher in countries with fiscal rules as compared to countries without fiscal rules (Table 4). The first study on the nexus between GDP growth and national fiscal rules has been conducted by [Afonso and Jalles \(2013\)](#). The authors regress the log of GDP per capita on self-compiled data of national fiscal rules in 25 EU countries. The model also considers GDP dynamics which, in turn, transforms the model into a growth regression. The results show that

countries with national ER had higher GDP growth than countries without such rules (Afonso and Jalles, 2013). The fiscal rules measure based on both BBR and DR is less strongly correlated with GDP growth than the measure based on ER rules.

The study of Gründler and Potrafke (2023) provides causal evidence and shows that fiscal rules increase economic growth. Gründler and Potrafke (2023) employ an historical sample including 54 countries over the period 1789-1954, a modern sample including 106 countries over the period 1985-2019 and a sample for sub-national jurisdictions such as the US states or Swiss cantons in 10 federal states over the period 1992-2012. The results show that constitutional fiscal rules are positively correlated with GDP growth in the historical, modern and sub-national sample. Causality is established in the modern sample by using an instrumental variable. The instrument is a new measure for attitudes towards fiscal rules in the individual countries (time-invariant) times the presence of fiscal rules in neighboring countries (time-variant). Attitudes towards fiscal rules are measured based on experts' assessments. Experts from more than 130 countries have been asked by the ifo institutes' Economic Experts Survey in 2020. Three questions on the perceived consequences of rules are used and consolidated into an index of a country's propensity to adopt fiscal rules. By focusing on attitudes towards fiscal rules, the instrumental variable is distinguished from other dimensions of fiscal or political conservatism that may influence economic growth. The results show that, in the long-run, GDP was around 15 % higher in countries with constitutional fiscal rules as compared to countries without constitutional fiscal rules.

Output volatility was lower in countries with stringent fiscal rules than in countries with lax fiscal rules. Badinger and Reuter (2017) use data for 74 countries over the period 1985-2012. Output volatility is measured by the standard deviation of GDP per capita growth. Fiscal rules are measured by their indicators on BBR, DR and overall stringency of fiscal rules. The authors estimate their models by OLS and 2SLS (instrumenting fiscal rules by checks and balance, political fragmentation and inflation targeting). The results show that countries with more stringent fiscal rules had smaller output volatility than countries with lax fiscal rules.

Fiscal rules mitigated the effect of discretionary fiscal policy on output volatility. Sacchi and Salotti (2015) employ a procedure with two stages to arrive at this result. First, the authors regress the growth rate of government spending on GDP growth and control variables. The residual of this regression is used as the measure of discretionary fiscal policy. Second, the authors regress the standard deviation of real GDP per capita growth on the fiscal policy measure. The results show that discretionary fiscal policy increases output volatility. When interacting discretionary fiscal policy with fiscal rules, the results show that discretionary fiscal policy only increases output volatility when fiscal rules are lax. These effects are especially pronounced for BBR.

3.4.3 Twin deficits

The twin deficit hypothesis describes that budget deficits increase external deficits. In open economies, the current account balance is equal to net private saving (saving

private saving minus private investments) plus net public saving (tax revenues minus government expenditure). Budget deficits are associated with current account deficits (when private savings do not overcompensate the public deficit). Fiscal rules are likely to influence current account deficits through three channels (Badinger et al., 2017): First, fiscal rules decrease uncertainty about future fiscal policies and, in turn, decrease precautionary private savings. Consequently, with lower private savings, the current account deficit increases. Second, fiscal rules decrease interest rates. Private spending and net exports increase. The effect on the current account balance is unclear. Third, fiscal rules influence budget deficits which mitigates the effect of the budget balance on the current account balance.

The empirical evidence on the extent to which fiscal rules reduce twin deficits is mixed. The results of Badinger et al. (2017) do not suggest that fiscal rules were directly associated with the current account balance. The presence of fiscal rules is found, however, to mitigate the effect of the budget balance on the current account balance. The sample of Badinger et al. (2017) includes 73 countries over the period 1985-2012. Fiscal rules are measured by the fiscal stringency indicator of Badinger and Reuter (2015) that covers both national and supranational fiscal rules.

Afonso et al. (2022) revisit how fiscal rules influence twin deficits. The authors' sample includes 65 countries over the period 1985-2015. Fiscal rules are measured based on the data by Schaechter et al. (2012). The results show that the presence of fiscal rules overall improves the current account balance. The authors disentangle the types of fiscal rules (BBR, DR, ER, RR) and interact the individual fiscal rule dummy variables with the budget balance: ER and BBR increase and DR and RR decrease the impact of the budget balance on the current account balance.

3.5 Mitigating political business cycles

Fiscal rules mitigate political business cycles. The political business cycle theories describe that election-motivated politicians implement expansionary fiscal policies before elections (see Nordhaus, 1975, Rogoff and Sibert, 1988, Rogoff, 1990 and De Haan and Klomp, 2013 and Dubois, 2016 for surveys). Expansionary fiscal policies include increasing public expenditure and deficits. Implementing expansionary fiscal policies just before elections are likely to be inefficient. Governments are supposed to use public expenditure, for example, to provide public goods when private provision does not give rise to an efficient provision of public goods and free-riding occurs. Governments also use public expenditure to provide transfers: subsidies to social security, unemployment benefits etc. and subsidies to industries when the production of goods and services gives rise to positive externalities (e.g., knowledge spillovers). Fiscal rules may help to prevent that election-motivated politicians pursue their own interest in spending public expenditure.

Implementing expansionary policies to manipulate voters is difficult when fiscal rules are in place. Empirical studies show that fiscal rules dampen political business cycles. To do so, scholars regress outcome variables such as budget deficits on a fiscal

rule variable, election year dummy variables and the interaction terms between the fiscal rule variable and the election year dummy variables. Marginal effects are computed to measure how the fiscal outcome variables developed depending on the presence or strictness of fiscal rules. See Table 7.

Gootjes et al. (2021) use data for 77 democracies over the period 1984-2015. Fiscal rules are measured by the data of Schaechter et al. (2012). The dependent variable is the primary budget balance as a share of GDP. The results show that the primary deficit was around 0.6 % of GDP in election years when fiscal rules were absent or lax. These effects are numerically important and statistically significant at the 1 % level: they suggest that election-motivated politicians - when not restricted by fiscal rules - implemented expansionary fiscal policies before elections. By contrast, the authors do not find any evidence for political business cycles when fiscal rules become more stringent. The marginal effects lack statistical significance once evaluated at the mean and above of the fiscal rule variable. The results hold for an overall fiscal rule index as well as for ER, BBR, RR and DR. Using data for 25 EU countries over the period 1996-2012, the results of Ademmer and Dreher (2016) also show that fiscal rules dampened political business cycles. The budget balance (in % of GDP) was around 2 percentage points lower in election than in non-election years when fiscal rules were lax. By contrast, when fiscal rules (measured by the stringency index of the EU Commission) were stringent, the budget balance did not differ in election and non-election years. What is more, fiscal rules were effective in mitigating political business cycles when media freedom was low (and politicians had a chance to manipulate voters).⁵

4 The state level

Sub-national governments in federal states such as the United States or Switzerland also impose fiscal rules. There are many studies investigating consequences of fiscal rules at the state level. The literature on the consequences of fiscal rules has been inspired by studies at the US state level. Exploiting variation in fiscal rules across sub-national governments within an individual country has an advantage: Institutions within an individual state are more homogeneous than among nations in different continents such as Europe and Africa.⁶

⁵Governments were also less likely to comply with fiscal rules in election years as compared to non-election years (Reuter, 2019). Doing so corroborates the political business cycle theories proposing that incumbents implement expansionary fiscal policies before elections. In Latin American and Caribbean countries, leftwing governments were less likely to comply with fiscal rules than rightwing governments (Ulloa-Suárez, 2023).

⁶One may also pool information about sub-national governments across nations. Foremny (2014) examines, for example, the effect of sub-national rules on deficits in 15 EU countries over the period 1995-2008. The results show that fiscal rules decrease deficits only in non-federal countries.

4.1 US states

4.1.1 Background

The United States have 50 states. The first fiscal rule, a DR, was introduced in Rhode Island in 1842. Many US states have followed introducing fiscal rules in their constitution. Only one US state, Vermont, has no BBR. The BBRs in the US states have different stages of requirements. The weakest form of BBR is that the governor needs to submit a balanced budget. This rule is in place in 44 US states. A more stringent form of BBR is that the legislature (State House and State Senate) needs to enact a balanced budget. This rule is in place in 37 US states. Those BBR may still give rise to budget deficits in individual years when actual revenues and expenditures deviate from expectations (forecasts). The question then arises whether state governments are allowed to carry-over deficits to future years. The strictest form of BBR prohibits this carry-over. 24 of the 37 US states that require the legislature to enact balanced budgets do not allow to carry-over budgets. BBR in the individual states also differ regarding the type of individual state spending that is covered by the BBR (Poterba, 1995).

4.1.2 Deficits, debt, expenditure and tax revenues

The early studies used cross-sectional data. A fiscal outcome variable such as states' expenditure is regressed on a fiscal rule dummy variable. Abrams and Dougan (1986) use, for example, states' and local governments spending per capita and regress it on dummy variables assuming the value one when states had ER or RR or any constitutional rules limiting spending and debt and do not find evidence that those dummy variables predict public spending. Those cross-sectional studies ignore, of course, variation over time and do not consider that the US states differ in a systematic manner. The next generation of studies therefore used panel data. Those studies exploit variation across states and time while taking into account the systematic differences across the US states and idiosyncratic events that affected all US states by including fixed state effects and fixed time effects. See Tables 8 and 9.

Von Hagen (1991) is one of the first and influential studies describing how fiscal rules relate to fiscal policy outcomes. The author prepares descriptive statistics and non-parametric tests on fiscal policy outcomes depending on the states' fiscal rules. He disentangles states with nominal debt limits or percentage debt limits, states that have a referendum requirement to create debt and states ranked according to the strength of BBR based on the index of the Advisory Council on Intergovernmental Relations (ACIR). Fiscal outcome variables are: state debt as measured by the amount of debt outstanding per capita, the ratio of state debt in 1985 to state debt in 1975, the ratio of nonguaranteed state debt and local debt to fully guaranteed state and local debt in 1985, and the ratio of state debt to personal income in the state. An important result is that state debt per capita in states with lax BBR (1,576.6 US-Dollar) was higher than in states with stringent BBR (919.2 US-Dollar). Clearly, Von Hagen (1991) presents unconditional correlations between fiscal rules and fiscal policy outcomes.

Those unconditional correlations do not help to make any inferences regarding the causal effects of fiscal rules on fiscal policy outcomes, but they helped a great deal to initiate the debate about the consequences of fiscal rules.

[Poterba \(1994\)](#) regresses changes in spending and tax revenue on unexpected deficit shocks interacted with dummy variables for weak fiscal rules. Weak fiscal rules are measured based on the ACIR index and assume the value one when the ACIR index assumes values smaller than one. Unexpected deficit shocks are measured as the difference between expenditure shocks and revenue shocks. Expenditure shocks are measured as the difference between actual outlays, spending cuts enacted after the initial budget but during the fiscal year and forecast outlays. Revenue shocks are measured as the difference between actual revenues, the change in revenue during the fiscal year that results from tax changes enacted during that fiscal year and forecast outlays. The sample includes 27 states over the period 1988-1992. The results show that changes in spending were positive when unexpected deficits occurred and fiscal rules were lax. [Poterba \(1994\)](#) also uses dummy variables for tax and expenditure limits and interacts those dummy variables with unexpected deficit shocks. The results suggest that the change in tax revenue was small when unexpected deficits occurred and tax and spending limits were in place.

An encompassing study on the effects of fiscal rules on fiscal outcomes is [Bohn and Inman \(1996\)](#). The study shows that stringent BBR are effective in reducing budget deficits. The sample includes 47 states over the period 1970-1991. The main dependent variable is the general fund deficit. Fiscal rules are measured by dummy variables and based on stringency of the fiscal rules. The authors include dummy variables for when the governor needs to submit a balanced budget, the legislature needs to pass a balanced budget, a requirement that a carried-over deficit needs to be corrected in the subsequent year and a no carry-over rule. The results show that general fund deficits were significantly smaller (in the baseline model by around 100 USD per capita, around 75 % of a standard deviation in the dependent variable) in states with no carry-over rules than in states without such rules. The authors also note that the eleven states that never had a general fund deficit are all states with a no carry-over rule. When examining the channels through which the deficits are reduced under no carry-over rules, the result suggests that governments reduced spending to a larger extent than they increased tax revenues. An interesting result is that increased general fund surpluses were used to augment rainy-day reserve balances.

[Smith and Hou \(2013\)](#) use data for 48 states over the period 1950-2004. The authors use three dependent variables: total expenditures, general expenditures and operating expenditures (each measured in per capita terms). BBR are measured by nine dummy variables that assume the value one when the following institutions are in place: the governor must submit a balanced budget, own-source revenue must meet or exceed expenditures, own-source revenue plus debt must meet or exceed expenditures, the legislature must pass a balanced budget, a limit on debt that may be assumed for deficit reduction, controls on supplemental appropriations, within-fiscal-year controls, and no deficit carry-over. The results suggest that all three types of expenditure decreased

when the governor needed to submit a balanced budget, controls on supplemental appropriations were present and no deficit carry-over was implemented.

Debt levels were correlated with fiscal rules. [Kiewiet and Szakaty \(1996\)](#) use data for 50 states over the period 1961-1990. The dependent variables are the amount of outstanding state guaranteed debt, nonguaranteed debt and total long-term local debt. Each debt observation is divided by the average amount of such debt carried out by all states in this year. The authors distinguish between four types of fiscal rules: legislative supermajority requirements, referendum approval requirements, prohibitions against guaranteed long-term debt and revenue-based limitations. They include the four dummy variables measuring fiscal rules in one specification. I was wondering about the extent to which the four fiscal rule variables are correlated with each other and the results are driven by multicollinearity issues. The results suggest that referendum approval requirements for public debt were negatively correlated with guaranteed debt and total state debt. Legislative supermajority rules were positively correlated with any type of debt. Prohibitions against guaranteed long-term debt was negatively correlated with guaranteed debt and positively correlated with total state debt. The authors propose that politicians may circumvent constitutional limitations by issuing nonguaranteed debt. I believe it is hard to make any inferences based on the authors' results. The study includes one table with regression results. We do not know whether the results are robust.

The Colorado's Taxpayer Bill of Rights (TABOR) includes very strict tax and expenditure limitations. The first expenditure limitation was introduced in 1977. In 1991, however, more restrictive rules were introduced. [Eliason and Lutz \(2018\)](#) examine how the TABOR 1991 limitations influence tax revenues and expenditure. The authors employ the SC method and conclude that there is no evidence showing that TABOR influences tax revenues and expenditure. To measure the synthetic Colorado, the authors consider many predictors except the lagged outcomes. I was wondering how inferences would change when the synthetic Colorado would be measured by lagged outcomes only. What is more, the baseline model includes 47 states in the donor pool. Many US states in the donor pool also implemented fiscal rules. I would have excluded all US states from the donor pool that had fiscal rules similar to Colorado in the pre-treatment period.

4.1.3 Fiscal sustainability

An alternative approach to examine whether fiscal rules promote fiscal sustainability is using panel data techniques testing fiscal sustainability. Fiscal sustainability is tested by whether the debt-to-GDP ratio contains a unit root, revenues and expenditures are cointegrated and by estimating Bohn's fiscal reaction function, that is investigating whether the debt-to-GDP ratio predicts in period $t - 1$ predicts the primary surplus in period t ([Bohn, 1998](#)). Those fiscal sustainability tests may well be used for US states with lax and stringent fiscal rules. [Mahdavi and Westerlund \(2011\)](#) employ cointegration tests. The authors use data for 47 states over the period 1961-2006

and individual types of revenues and expenditures (total, general, current etc.). The stringency of fiscal rules are measured based on ACIR. The results show that the extent of cointegration between revenues and expenditures is quite similar in states with stringent and lax fiscal rules. The presence of some (stringent) BBR, especially no-carry over rules helps to improve sustainability of public finances.

4.1.4 Fiscal and macroeconomic volatility

Volatility of government spending was higher in US states which did not have no-carryover rules (BBR) or less stringent rules as compared to states with carryover and stringent fiscal rules: [Fatás and Mihov \(2006\)](#) use data for 48 states over the period 1963-2000. For each state in their sample, the authors regress the log government spending on GDP growth. Based on this regression, the authors compute volatility measures which they, in turn, regress on fiscal rule variables. [Fatás and Mihov \(2006\)](#) also use the elasticity of government spending (a measure for pro-cyclicality) as the dependent variable. The results suggest that fiscal policy was more pro-cyclical in states with stringent rules as compared to states with less stringent rules.

[Bayoumi and Eichengreen \(1995\)](#) conclude that strict fiscal rules were associated with less fiscal volatility. The authors regress the difference in the fiscal balance (as a share of GDP) on GDP growth. They do so for every individual US state and use the parameter estimate of the GDP growth rate as a measure for fiscal volatility. In the next step, the authors estimate a cross-sectional model and regress the fiscal volatility measure on the ACIR index. The results show that the cyclical variance of the fiscal balance was around 0.055 points (40 percent) smaller in an US state with very strict rules (ACIR index of 10) as compared to an US state with very lax rules (ACIR index of 0). [Bohn and Inman \(1996\)](#) confirm the results of [Bayoumi and Eichengreen \(1995\)](#) for a sample of 47 US states over the period 1970-1991. When using a cyclical measure based on state unemployment, however, [Bohn and Inman \(1996\)](#) do not conclude that fiscal rules reduce cyclical volatility.

The results of [Alesina and Bayoumi \(1996\)](#) suggest that strict fiscal rules as measured by the ACIR index were positively correlated with budget surpluses and negatively correlated with the standard deviation of the surplus. Strict fiscal rules were also negatively correlated with the fiscal cyclical volatility (as measured as the parameter estimate of GDP growth when the change in the budget surplus (in % of GDP) is the dependent variable).

The results of [Sørensen et al. \(2001\)](#) also suggest that strict fiscal rules were associated with smaller fiscal cyclical volatility. The authors arrive at this result by two strategies. First, the authors regress the budget surplus (in % of GSP) on the change in GSP interacted with a dummy variable for stringent and not stringent fiscal rules. Fiscal rules are measured as stringent when the ACIR index assumes values larger than 8. The results show that the positive correlation between changes in GSP and the budget surplus are significantly larger when fiscal rules are not stringent than when fiscal rules are stringent. Second, [Sørensen et al. \(2001\)](#) follow previous studies such as [Alesina](#)

and Bayoumi (1996) and regress the budget surplus on GSP growth in every individual US state, obtain parameter estimates of GSP growth and regress those parameter estimates on the ACIR index. The parameter estimates of the ACIR index are negative - indicating that more stringent fiscal rules were associated with lower fiscal cyclical-ity - but lack statistical significance. The sample includes 48 states over the period 1978-1994.

Canova and Pappa (2006) focus on unconditional correlations between fiscal rules and macroeconomic outcomes. The sample includes 48 US states over the period 1969-2000. The authors consider six types of fiscal rules by including dummy variables: the governor must submit or the legislature must pass a balanced budget, no carry-over rules, a requirement to balance the budget within the current fiscal cycle, some form of debt restriction, prohibition of guaranteed (full faith and credit) debt or allow a nominal amount below 200,000 US-Dollars, prohibition the issue of short-term debt. The authors also use the ACIR index. Nine macroeconomic outcome variables are used: the volatility of the log of state real per-capita expenditure, the volatilities of the log of per-capita real state output, prices and employment; their correlation with the log of per-capita real state consumption expenditure; the mean of the consumption expenditure to output ratio and the mean of real per-capita output. The authors conduct two tests: asymptotic and rank tests for the differences in the average moments of states with and without fiscal rules. The results do not suggest that these macroeconomic outcome variables differ a great deal in states with and without fiscal rules.

Levinson (1998) uses two dependent variables to measure output volatility: the standard deviation and the maximum difference of linearly detrended quarterly real state per capita personal income. The sample includes 50 states over the period 1969 to 1995. He estimates a cross-sectional model because fiscal rules have not changed within his sample (Tennessee being the only exception). The main explanatory variable is a dummy variable that assumes the value of one for states that allow deficit carryover from one year to the next. He calls these “lenient” states and interacts this “lenient” dummy variable with a dummy variable for large states based on population. He describes his results showing that lenient states (especially large ones) have smaller business cycle fluctuations than non-lenient states. I would interpret his results in a different manner: the parameter estimates of the lenient “lenient” dummy variable are positive and numerically larger than the negative parameter estimates of the interaction terms between the “lenient” dummy variable and the large state dummy variable.

Krol and Svorny (2007) take issue with the results of Levinson (1998) and show that altering the econometric specification changes the inferences. They detrend the data, use an alternate measure of state activity and measure population size by the log of state population. These results show that states with no-carryover rules have smaller business cycle fluctuations than states with carryover rules.

4.1.5 Borrowing costs

Borrowing costs were lower in states with stringent fiscal rules than in states with lax fiscal rules. [Bayoumi et al. \(1995\)](#) use data for 38 states over the period 1981-1990. The dependent variable is the basis point spread for twenty-year state general obligation bonds (GO) relative to a New Jersey twenty-year GO. The authors regress this bond spread on the ACIR index and find that the bond spreads are smaller in states with stringent BBR than in states with lax BBR.

[Poterba and Rueben \(1999\)](#) use data for 40 states over the period 1973-1995. The dependent variable is the interest rate on 20-year general-obligation debt issued by an individual state (against the interest rate of New Jersey). Fiscal rules are measured by eight variables: the ACIR index, an indicator for restrictions on debt issue, a binding expenditure limit, a binding revenue limit, the legislature must pass a balanced budget, a requirement to correct the deficit in the next budget cycle, a requirement to correct the deficit in the next two-year cycle, a requirement to correct the deficit in the current one-year cycle. The authors regress their borrowing cost measure on the individual fiscal rules measures (and interactions with variables such as the unemployment rate to capture economic performance). The results show that states with strict fiscal rules had lower borrowing costs. For example, borrowing costs were around 7 basis points lower in states with binding expenditure limits than in states without binding expenditure limits.

Fiscal rules mitigated the effects of unexpected deficits on borrowing costs. [Poterba and Rueben \(2001\)](#) use data for 39 states over the period 1988-1998. The results show that unexpected fiscal deficits increased bond yields. These effects were smaller when no-deficit rules were in place. Deficit shocks are measured by the difference between expenditure shocks and revenue shocks. Revenue shocks and expenditure shocks are based on actual revenues and expenditure, changes in revenue and expenditure and revenue and expenditure forecasts. The dependent variable is the difference in the average value of state bond yields relative to yields for New Jersey (20-year general obligation debt as reported in the Chubb Insurance Company “Relative Value Survey”). Fiscal rules are measured by “antideficit rules, limits on the ability of state legislatures to issue debt, and limits on state taxes or expenditures” (p. 547). The budget balance rules are measured based on the ACIR index. The authors use a dummy variable that assumes the value one when the ACIR index assumes values below 6. They also include a dummy variable that assumes the value one for states that consider any debt restriction, a limit on state taxes, a limit on state expenditure and a supermajority provision for the enactment of new taxes. The results suggest that especially BBR mitigate the effect of unexpected deficits. For example, a deficit equal of 5 % of projected revenue increased borrowing costs by 3.3 basis points in a state without BBR as compared to a state with BBR.

[Kelemen and Teo \(2014\)](#) examine the extent to which fiscal rules should be strict or transparent to be effective in reducing borrowing costs. They propose that sovereign markets are more likely to enforce politicians to comply with fiscal rules than the

judiciary. The authors regress the general obligation bond credit ranking on a variable measuring fiscal rule strictness and transparency. Strictness is measured by the index compiled by the ACIR. Transparency is measured by the fiscal transparency index of [Alt et al. \(2006\)](#). The sample includes data for 47 US states over the period 1990-1999. The results show that both strictness and transparency increases the probability of achieving a higher credit ranking. The authors also include the interaction between strictness and clarity and show that strictness only increases the probability of achieving a higher credit ranking when fiscal rules are transparent to a sufficient degree.

[Deng and Liu \(2022\)](#) corroborate that government bond spreads and Credit Default Swap (CDS) spreads were smaller in states which had BBR than in states without BBR. The authors self-compile data on BBR, considering variation over time in a panel over the period 1976-2020. The BBR data show that 20-30 % of the states have not imposed any deficit carryover restrictions. There is also quite some variation among those states having BBR. For example, some states have constitutional BBR and other states statutory BBR. The authors address endogeneity of the BBR variable by using past state government surpluses as an instrumental variable. The past state government surpluses are a relevant instrumental variable because state governments introduced BBR when they had fiscal deficits. The exclusion restriction requires that past state government surpluses do not directly predict government bond spreads other than through the BBR. The exclusion restriction is likely to hold because bond prices are forward-looking. I found this IV strategy clever.

4.1.6 Creative accounting

Descriptive evidence indicates that fiscal rules in the US states gave rise to creative accounting. Public authorities may help, for example, to circumvent fiscal rules. Public authorities are responsible for highways, public buildings, housing or nongovernmental hospital facilities. Constitutional debt rules were associated with the size and scope of public authorities ([Bunch, 1991](#)). The dependent variables are “the number of state authorities, the scope of functions addressed by authorities, the existence of a public building authority, and the state’s reliance on authorities to issue public infrastructure debt” (page 60). [Bunch \(1991\)](#) compiles the data on public authorities overall for the period 1982-1986. Constitutional debt limits are measured by two dummy variables: One dummy variable assuming the value one when the constitutional debt limit only applies to general obligation (GO) debt (bonds that are backed by the taxing authority of the state). Another dummy variable that assumes the value one when constitutional debt limits apply to GO and “at least some forms of revenue debt” (page 60), that is “bonds that legally are secured solely by an earmarked revenue source” (page 57). The analysis is based on cross-sectional regressions for 48 states. The results show that states which have both a GO and a revenue debt limit had more state authorities than states that have no constitutional debt limit.

[Canova and Pappa \(2006\)](#) describe that fiscal rules tend to give rise to creative accounting. The authors use the ratio of state non-guaranteed to guaranteed debt,

the ratio of state to local expenditure and average stock-flow adjustments (SFA - the difference between (current account) deficit and growth of debt) as outcome variables. The results show that states with strict fiscal rules have more non-guaranteed debt and delegate more expenditure to the local level than states with lax fiscal rules.

Future research should examine in more detail how fiscal rules influence creative accounting. Employing panel data models is useful because the panel data models help to control for systematic differences across the US states and being exposed to idiosyncratic shocks over time.

4.1.7 Political-economic factors

Fiscal rules may well mitigate effects of political-economic determinants on fiscal outcomes. The prime example are political business cycles. [Rose \(2006\)](#) shows that no-carryover rules have dampened political business cycles in deficits and expenditure. The sample includes data for 43 states over the period 1974-1999. The dependent variables are the real per capita general fund surplus, real per capita general revenue from taxes and real per capita general expenditure. The author includes dummy variables for cycles in gubernatorial elections (election year, pre-election year etc.). The election year variables are interacted with a dummy variable for strict no-carry over rules. The results show that electoral cycles occurred in deficits and spending when fiscal rules were lax. In election years, for example, the fiscal deficit was 31.78 USD when fiscal rules were lax and 4.15 USD when fiscal rules were strict.

The effect of fiscal rules on fiscal policy outcomes may also depend on other political economic determinants than electoral motives. Examples include divided government and government ideology ([Alt and Lowry, 1994](#); [Primo, 2006](#); [Hong, 2015](#)). Divided government occurs when a governor in an US state belongs to party A and the legislature is dominated by party B. Divided government is likely to give rise to a large size and scope of government when the Democrats and Republicans need to make compromises (common pool problem). The empirical evidence on how divided government influences the size of government is mixed though (see, for example, [Bjørnskov and Potrafke, 2013](#) and [Bernecker, 2016](#)). In any event, fiscal rules may well mitigate the effect of divided government on size of government.

The partisan theories propose that Democratic governments have a larger size of government than Republican governments (see [Hibbs, 1977](#), [Chappell and Keech, 1986](#) and [Alesina, 1987](#)). The empirical evidence shows that Democratic governments had higher taxes, higher expenditure and higher public debt than Republican governments in the US states (see [Potrafke, 2018](#) for a survey of the empirical literature on partisan politics in the United States). The effects of fiscal rules on fiscal policy outcomes will be smaller under divided governments than unified governments and under Democratic than Republican governments.

An influential study is [Alt and Lowry \(1994\)](#). The sample includes 48 states over the period 1968-1987. It is quite hard to understand, however, how the authors arrive at their estimates on how Democratic and Republican governments influence revenues

and expenditure when no-carry over rules are in place and deficit shocks occur.

[Primo \(2006\)](#) examines how fiscal rules relate to government spending under divided government and Democratic and Republican state governments. His sample includes data for 47 US states over the period 1969-2000. The dependent variable is per capita real direct general expenditure. Fiscal rules are measured by a spending limit that “is operationalized as the presence of an elected high court and a no-carryover rule for deficits” (page 292). Government ideology is measured by the party affiliation of the governor and party majorities in the state House and state Senate. The main result regarding the fiscal rules is that the spending limit decreases public expenditure by 127 USD per capita (around 11 percent of a standard deviation of public expenditure). He concludes that the nexus between fiscal rules and spending is robust. The results do not suggest, in contrast to many other studies on partisan politics and divided government, that partisan politics and divided government influences public expenditure. One finding stands out, however: When spending limits are present, Republican governors spent more than Democratic governors.

[Hong \(2015\)](#) uses data for 50 states over the period 2004-2008. The dependent variable is budget cuts as measured by “the amount cut from the budget after the budget of that fiscal year has passed the legislature” (page 510). Fiscal rules are measured by BBR stringency based on ACIR and the US General Accounting Office (GAO). The fiscal rule dummy variables enter the model in levels and are interacted with unexpected deficit shocks ([Poterba and Rueben, 2001](#)), and triple interactions with divided government and government ideology variables (party identity of the governor and share of Democrats and Republicans in the state House and Senate). The author concludes that that BBR give rise to budget cuts when unexpected deficit shocks occur, especially when Republican governors are in office. The questions posed by [Hong \(2015\)](#) on how fiscal rules mitigate effects of partisan politics and divided government are interesting. I was not convinced by the analysis based on triple interaction terms, however. Future research should examine in more detail the extent to which fiscal rules mitigate partisan politics.

4.2 Canadian provinces

Canada is a federal state with 10 provinces. The first fiscal rule was introduced in Manitoba in the year 1989, many of the other provinces followed soon by introducing fiscal rules. Provinces with no or lax fiscal rules were Newfoundland and Prince Edward Island. In Manitoba, by contrast, fiscal rules have been stringent (e.g. [Kennedy et al., 2003](#)). Fiscal rules have been very effective in reducing budget deficits and public debt (Table 10). Budget surpluses (in % of GDP) were larger and debt-to-GDP ratios were smaller in Canadian provinces with stringent fiscal rules than lax fiscal rules ([Tellier and Imbeau, 2004](#); [Tapp, 2013](#); [Mou et al., 2018](#); [Mou and Hing, 2021](#)). The first empirical study on consequences of fiscal rules is [Tellier and Imbeau \(2004\)](#): the authors use the budget balance as a share of total spending as their dependent variable. Fiscal rules are measured by the stringency index of [Millar \(1997\)](#). This index assumes values

between 0 (very lax or no fiscal rules) and 6 (very stringent fiscal) rules. Provinces with no fiscal rules were British Columbia, Newfoundland, Ontario and Prince Edward Island. Manitoba had the most stringent fiscal rules (the index assumes the value 6). The authors' sample covered the period 1968-2000. This index turned out to be a statistically significant correlate of the budget balance: surpluses were large in provinces with stringent fiscal rules.

[Tapp \(2013\)](#) employs data for the period 1981-2007 and estimates that fiscal rules reduced deficits by around 0.8 percentage points of GDP and debt-to-GDP ratios by around 1.5 percentage points. The author is well aware of potential endogeneity threats, controls for obvious potential confounders in a common panel data model, and also offers IV-estimates using the presence of fiscal rules in neighboring provinces as IV. The estimates also show that BBR and DR are effective, ER and RR are less effective.

[Mou et al. \(2018\)](#) use data over the period 1981-2013. Fiscal rules are measured by a stringency index for BBR that assumes values between 0 (very lax) and 10 (very stringent). The authors measure stringency based on an examination of the pertinent laws. Stringency of fiscal rules increases, for example, when "the government has to report an actual, ex post budget surplus instead of a forecasted, ex ante surplus...; the budget has to be balanced each year instead of within a more extended time line...; the budget has to be balanced even in extreme economic conditions...; failure to comply with requirements results in a penalty to the salaries of executive council members" (page 32). The authors' stringency index is positively correlated with budget surpluses and negatively correlated with public debt. [Mou et al. \(2018\)](#) also examine how fiscal rules relate to budget deficits and debt in booms and recessions. Clearly, budget deficits are larger and public debt is higher in recessions than in booms. A major question is how stringent fiscal rules and recessions jointly relate to budget deficits and public debt. The authors include interaction terms between the fiscal stringency index and their dummy variable for recessions. The interaction terms lack statistical significance and, in turn, the authors conclude that the stringency of fiscal rules relates to budget deficits in public debt in constant manner - booms or recessions notwithstanding. We do not quite know, however, because the authors do not compute marginal effects of the stringency index on the outcomes and do not report the standard errors of the interaction terms.⁷

The results of [Mou and Hing \(2021\)](#) show that the budget surplus as a share of GDP was around 0.38 percentage points higher (around 0.2 standard deviations) when their BBR stringency index increased by one standard deviation. The authors' sample covers the period 1980-2018. The authors also consider an index of fiscal transparency which is positively correlated with the budget surpluses. The parameter estimate of the interaction term between BBR stringency and fiscal transparency has a negative sign and is numerically small. This estimate does not indicate that stringent fiscal rules and fiscal transparency invigorate each other. What is more, the results do not

⁷A controversial issue is how fiscal rules were related to fiscal outcomes before and after the Great Recession in 2008 and 2009 ([Simpson and Wesley, 2012](#); [Atkinson et al., 2016](#)).

show that the stringency of BBR is correlated with SFA.

Costs of government were related to fiscal rules. The price index for the public sector was lower in provinces with stringent fiscal rules than in provinces with lax fiscal rules over the period 1997-2007 (Mou et al., 2014).

4.3 Swiss cantons

Switzerland is also an excellent laboratory to examine how fiscal rules influence fiscal outcomes at the sub-national level. Switzerland has 26 cantons (states) which enjoy a great deal of fiscal autonomy. The first fiscal rule was introduced in the canton of *St. Gallen* in 1929. The canton of *Fribourg* introduced a fiscal rule in 1960. By the end of the 1990s, only three other cantons (*Appenzell a. Rh.*, *Graubünden* and *Solothurn*) had fiscal rules. In 2022, 20 out of the 26 cantons had fiscal rules.

There is overwhelming evidence showing that fiscal rules in Swiss cantons were strongly associated with low public deficits: Feld and Kirchgässner (2001); Schaltegger (2002); Feld and Kirchgässner (2008); Krogstrup and Wälti (2008); Luechinger and Schaltegger (2013); Burret and Feld (2018a,b). See Table 11. Early studies by Feld and Kirchgässner (2001), Schaltegger (2002) and Feld and Kirchgässner (2008) use data for the periods 1986-1997 and 1980-1998. The authors use a fiscal rule index that assumes the value 0 when no fiscal rules are in place (21 out of 26 cantons), and the values 1, 2 and 3 when fiscal rules are in place (1 lax, 3 stringent). Schaltegger (2002) addresses the potential endogeneity of fiscal rules and uses the number of referenda and voter turnout in referenda as instrumental variables. The results show that stringent fiscal rules decreased expenditures, deficits and debt.

An encompassing study on the effects of cantonal fiscal rules on public finances is Burret and Feld (2018a). The authors use data for the period 1980-2011. The dependent variables include expenditure, revenues, deficits, debt and investment spending. Fiscal rules are measured by dummy variables in the baseline model. The results show, for example, that fiscal rules influence public finances: cantonal revenue was around 411 Swiss francs higher, cantonal debt and deficit were around 1035 and 225 Swiss francs lower when debt brakes were in place. The authors also disentangle types of public expenditure to investigate how fiscal rules influence budget composition: there is no evidence that fiscal rules influenced consumption spending, current spending and spending by functional category (except spending on transportation which increased when fiscal rules were present). By contrast, Burret and Feld (2018a) find that fiscal rules increased cantonal investment spending and conclude: “Thus, evidence clearly rejects the common claim that debt brakes hurt investments” (p. 175). The authors also use the “balance of funds and special financing” as dependent variables and conclude that “we have no evidence that cantonal constraints are associated with an evasion into funds and special financing” (p. 176).

Cantonal debt brakes decreased cantonal yield spreads (Feld et al., 2017). The yield spread of cantonal bonds against Swiss federal bonds (maturity of 10 years) was, for example, around 17 basis points lower in cantons that had fiscal rules as compared to

cantons that did not have fiscal rules. [Feld et al. \(2017\)](#) also consider the strictness of fiscal rules and find that the yield spread became smaller, the stricter fiscal rules were.

The fiscal rules also helped to improve the accuracy of budgetary forecasting ([Luechinger and Schaltegger, 2013](#)). Tax revenue projections errors were higher under rightwing than leftwing finance ministers in the Swiss cantons ([Chatagny, 2015](#)). It is conceivable that leftwing finance ministers needed to signal fiscal competence to the voters by producing more conservative fiscal estimates than rightwing finance ministers (leftwing politicians often increase spending and deficits to a larger extent than rightwing politicians). Fiscal rules mitigated, however, the effect of the finance ministers' ideology on tax revenue forecast errors.

The fiscal rules at the cantonal level hardly influenced fiscal outcomes of the Swiss municipalities. To examine vertical effects of cantonal fiscal rules on municipalities' public finances [Burret and Feld \(2018b\)](#) employ aggregated data of the municipalities at the cantonal level (1980-2011) and fine-grained data at the local level for 139 large municipalities (1982-2007). Fiscal rules are measured by dummy variables and the fiscal rules stringency index. The results do not suggest that the fiscal rule dummy variable and the fiscal rules stringency index are correlated with expenditure, revenues, debt and deficits. The authors also disentangle expenditure categories and find that stringent fiscal rules were positively correlated with spending on environment protection and negatively correlated with transportation expenditure.

4.4 German states

Germany is a federal state with 16 states. All the 16 states have a constitutional fiscal rule since the year 2020. The national government imposed this rule. The state governments had the opportunity to design individual characteristics of the fiscal rules. An example are escape clauses. When a catastrophe such as a natural disaster hits, members of parliament decide whether the catastrophe is so severe that the fiscal rule is suspended for an individual year. The required majorities differ across the state parliaments.

The national government decided in 2009 to introduce a more stringent debt rule in the German constitution than the debt rule which was in place since the year 1969. The new debt brake was effective since 2016 at the national level and since 2020 in the German states. The state governments could introduce debt brakes in their constitutions already before the year 2020. Clearly, some state governments decided on purpose to introduce the fiscal rule in their constitution. There is no exogenous variation on introducing fiscal rules to be exploited in an econometric model.⁸ Eight German states introduced the fiscal rule in their constitution before 2020. The debt-to-GDP ratio

⁸Conservative politicians were more likely to support the fiscal rule in the German states than leftwing politicians ([Potrafke et al., 2016](#)). German state parliamentarians were more likely to comply with the German debt brake when their party was in government than in opposition ([Heinemann et al., 2022](#)). The German debt brake influenced politicians' expectations about public deficits ([Heinemann et al., 2016](#)).

decreased in seven of these eight states once they introduced the debt brake in their constitution (Fuest et al., 2019).

The fiscal rules in the German Laender are quite stringent. A major question therefore is whether governments made attempts to circumvent the fiscal rules. Evidence suggests that German state governments increased the number of state-owned companies that could issue debt on their own (Heinemann and Nover, 2023). Core budgets of the German states do not consider debt of state-owned companies.

5 The local level

Using institutions and data at the local level has two advantages as compared to examining institutions and data at the national level: first, identifying causal effects of fiscal rules on economic outcomes is easier at the local level because empirical techniques such as difference-in-discontinuity designs can be used. Doing so helps to exploit quasi-random variation in the presence of fiscal rules. Second, scholars also examine effects of fiscal rules on outcomes that are quite difficult to measure in a cross-country setting. An example is quality of politicians.

5.1 Italy

Italy is an excellent laboratory to investigate consequences of fiscal rules at the local level. The pioneering study examining effects of fiscal rules at the local level is the one by Grembi et al. (2016). The authors exploit an encompassing reform: the Domestic Stability Pact (DSP) - a program that was in place over the period 1999-2015. The central government imposed fiscal rules on municipalities in the year 1999 and relaxed these fiscal rules for municipalities with less than 5,000 inhabitants in the year 2001. Italy has around 8,000 municipalities. Around 60 % of the municipalities have less than 5,000 inhabitants. The median population size is around 2,500 inhabitants. Grembi et al. (2016) compare fiscal policy outcomes in municipalities with slightly less and slightly more than 5,000 inhabitants before and after the year 2001 in a difference-in-discontinuities design. This empirical strategy makes sure to disentangle the effect of fiscal rules on fiscal outcomes from the effect of mayors' salaries on fiscal outcomes: the threshold of 5,000 inhabitants also influences mayors' salaries. Mayors in municipalities with more than 5,000 inhabitants receive much higher salaries than mayors in municipalities with less than 5,000 inhabitants. The salary scheme did, however, not change in the year 2001. The baseline sample includes 1,050 municipalities from 1999 to 2004: 555 municipalities are treated after the year 2001, 495 municipalities are in the control group. The results show that relaxing fiscal rules gave rise to higher fiscal deficits and lower taxes. Fiscal deficits increased by around 30 percent over the course of four years after the fiscal rules were relaxed.

Many empirical studies exploit the empirical design proposed by Grembi et al. (2016) to examine consequences of fiscal rules (Tables 12, 13 and 14). They take

advantage, however, of many individual reforms of the DSP by the national government. There have been further reforms after the year 2001. [Venturini \(2020\)](#) describes the individual reforms in an excellent manner. 2003, for example, the BBR was extended to apply to the cash and accrual basis instead of only to the cash basis. In 2003, an expenditure ceiling on overall expenditure was applied to all municipalities that have more than 3,000 inhabitants. In 2005, two distinct expenditure ceilings on current and capital spending were applied - they replaced the initial BBR. In 2007, the BBR was reintroduced and there were no exemptions anymore to not comply with the BBR. In 2012, the BBR was extended to be effective for municipalities with more than 1,000 instead of 5,000 inhabitants. A major question is whether choosing individual reforms of the DSP changes inferences of the empirical studies. There is indication that it does. [Venturini \(2020\)](#) shows, for example, that the reform in 2007 influenced the composition of public expenditure. The municipalities being subject to stricter fiscal rules spent less on investments than municipalities with hardly less inhabitants that were not subject to the fiscal rule. However, when focusing on the 2001 reform which [Grembi et al. \(2016\)](#) used, [Venturini \(2020\)](#) does not find any effect (rather a positive parameter estimate which lacks statistical significance) of fiscal rules on investment expenditure. In a similar vein, when examining the 2005 reform, [Mancini and Tommasino \(2023\)](#) do not find a consistent effect of fiscal rules on investment expenditures.

The DSP introduced a fiscal rule for municipalities with more than 1,000 inhabitants in the year 2013. [Alpino et al. \(2022\)](#) use this reform to investigate how the fiscal rules influenced income tax rates. The dependent variables are the average tax rate at the first and at the ninth income deciles, the average rate progression and a dummy variable that assumes the value one when the municipality has progressive tax. The authors find that the fiscal rule increased overall tax revenues and income redistribution: the fiscal rule increased, in particular, the marginal income tax rates on top earners. Investigating the mechanisms in more detail, the authors show that mayors with college degrees were active in increasing the marginal income tax rates on top earners when fiscal rules were present. By contrast, mayors without a college degree did not do so. [Alpino et al. \(2022\)](#) also examine the extent to which having fiscal rules influenced the mayors' reelection chances. The results show that voters rewarded mayors with college degree: these mayors responded by introducing progressive taxation when the fiscal rule was in place.

The laxer budgetary restrictions for municipalities with fewer than 5,000 inhabitants also deteriorated budgetary forecasts after the year 2002 ([Picchio and Santolini, 2020](#)): revenue (expenditure) forecast errors increased by 22% (26%). In a similar vein, the cap on capital expenditures in municipalities with more than 5,000 inhabitants after the year 2005 improved budgetary forecasts. Capital and investment expenditure forecast errors decreased by around one third ([Mancini and Tommasino, 2023](#)).

The DSP-induced fiscal rules have also been used to examine whether fiscal rules mitigate political business cycles. [Bonfatti and Forni \(2019\)](#) employ the 5,000 inhabitant threshold over the period 2005-2012. The authors test whether mayors in municipalities with less than 5,000 inhabitants implemented more expansionary fis-

cal policies than mayors in municipalities with 5,000 to 15,000 inhabitants (who were restricted by the fiscal rule). The authors estimate a panel data model both for subsamples (up to 5,000 inhabitants versus 5,000 to 15,000 inhabitants) and for the full sample including election year dummy variables interacted with a dummy variable for the 5,000 inhabitant threshold. The results show that capital expenditure increased before elections: around 10-20 percent on average in the full sample. In municipalities subject to the fiscal rule, the increase in capital expenditure was around 25% of the increase in capital expenditure of the municipalities which were not subject to the rule.

The DSP-induced fiscal rules decreased public expenditure and, in turn, corruption. [Daniele and Giommoni \(2021\)](#) exploit the 2013-reform of the DSP that extended the effectiveness of fiscal rules to municipalities with 1,000 to 5,000 inhabitants. The authors focus on the threshold of 5,000 inhabitants (recall that [Alpino et al., 2022](#) also use the 2013 reform but focus on the 1,000 inhabitant threshold to examine the consequences of the 2013 DSP reform). An issue with exploiting the threshold of 5,000 inhabitants is that the rules for assigning auditors to supervise municipalities' budgets were also changed beginning at the year 2013 ([Vanmutelli, 2022](#)). Corruption is measured by the number of initiated procedures against corruption in an individual municipality. The baseline estimate suggests that the number of investigations against corruption was around 6 to 12 percent of a standard deviation smaller when the fiscal rule was in place. The effect of public expenditure on corruption operated through discretionary spending, that is capital expenditure and expenditure on public procurement. The authors also consider whether municipalities received transfers from the EU. Those transfers were excluded from the DSP restrictions and dampened the effects of the fiscal rules - the conclusions regarding the negative effect of fiscal rules on corruption are based on municipalities which did not receive any EU transfers.

The evidence shows that the DSP-induced fiscal rules decreased public expenditure, including public procurement spending. [Coviello et al. \(2022\)](#) use data over the period 2004-2011 and describe that "only municipalities with populations greater than 5,000 were subject to it [the DSP] during our sample period." (page 585). The authors focus on the reform that took place in the year 2008: "In August 2008, a law was unexpectedly passed that made enforcement of the Patto much stricter. For the first time, non-compliant municipalities suffered substantive cuts in government transfers, and restrictions to borrowing for investment; moreover, mayors and councillors in non-compliant municipalities received a 30 % salary cut. This new law aimed to permanently curb municipal spending. These penalties persisted throughout our sample period and beyond. Following this law, municipalities with populations exceeding 5,000 expected to see a drop in procurement due to the increased enforcement of the Patto" (p. 585). The authors use this reform to examine how this exogenous demand shock for firms in the procurement sector influenced the firms' factor utilization. The authors find that firms' revenue from procurement decreased in the course of the reform. Firms responded by cutting capital (as measured by total annual physical assets) rather than labor (as measured by total annual wages). [Coviello et al. \(2022\)](#) do not discuss the other reforms of the DSP.

Public expenditure was also increased in municipalities with lax fiscal rules after the 2005 reform: [Pavese and Rubolino \(2022\)](#) employ data on individual expenditure categories to examine how the spending cuts influenced student performance in standardized national tests. The sample includes municipalities up to 30,000 inhabitants. I would have liked to see the baseline model focusing on inhabitants around the 5,000 inhabitants threshold.

Mayors' education decreased when municipalities were forced to implement fiscal rules. [Gamalerio and Trombetta \(2022\)](#) propose that the limited flexibility under fiscal rules attracted less educated candidates to become a mayor. Highly educated citizens wish to enjoy flexibility when operating in office. What is more, highly educated citizens are more likely to handle flexibility (when no fiscal rules are enforced) in a more efficient manner than less educated citizens. I believe that this argument is plausible. I was not convinced, however, by the empirical estimates that are intended to corroborate this argument. The authors focus on the 2001 reform of the DSP and use the educational degrees of the mayors as the dependent variable. Their baseline estimate shows that the share of mayoral candidates decreased by 10 percentage points when municipalities were forced to introduce fiscal rules. The authors use, however, data for the period 1993-2012 for municipalities of up to 15,000 inhabitants. I would have liked to see an estimate based on data for the period 1999-2003 for municipalities around the threshold of 5,000 inhabitants. The authors relate to the 2013 reform and write: "we extend the data to 2015 and exploit the 2013 variation in the application of fiscal rules. The results of this exercise indicate similar trends, providing further evidence for the negative effect of fiscal rules on politician quality" (page 2). There is, however, no evidence in the paper corroborating this.

An issue with the studies based on the DSP is that they seem to cherry-pick individual reforms over the period 1999-2015. We would like to compare effects of every individual research question based on the reform from 2001 which [Grembi et al. \(2016\)](#) used.

5.2 Colombia

In Colombia, a reform on fiscal rules influenced the public finances of the 1.100 municipalities. In the year 2000, a fiscal rule was introduced that limited the operating expenditures to 80 % of current revenue. Operating expenditures basically cover the payroll and procurement of the municipalities' administrative apparatus ([Carreri and Martinez, 2022](#)). They covered around 30% of the municipalities' total expenditure before the reform. Clearly, the reform affected all Colombian municipalities. The exogenous variation to be exploited comes from differences in treatment intensity: only municipalities that spent a higher share than 80 % of current revenues on operating expenditures were affected by the reform, they were exposed to a new fiscal rule. [Carreri and Martinez \(2022\)](#) examine how the new fiscal rule influenced public finances, welfare and political outcomes in an encompassing manner. The results show that the fiscal rule was effective in improving the municipalities' public finances. The overspending

ratio of operating expenditures decreased by around 30 % in municipalities affected by the fiscal rule. Consequently, the probability of a current deficit decreased by around 31 percentage points.

There is no evidence showing that the fiscal rule decreased capital expenditure. This result indicates that the fiscal rule did not mitigate the provision of local public goods. The authors examine the provision of local public goods in more detail by measures of education and health, and the provision of clean water and sanitation. There is no indication showing that the fiscal rule influenced the provision of any local public good. What is more, there is also no evidence showing that the fiscal rule influenced corruption of local public officials, civil conflict and or the cultivation of coca. I was quite convinced by the authors' empirical analysis and their conclusion that the fiscal rules reduced public spending without compromising the provision of local public services. These results indicate that administrative expenditure was wasted before the reform.

The fiscal rule gave rise to compelling electoral consequences: the political party of the municipalities' mayors benefitted from the fiscal rule. Voters have been convinced of the benefits of the reform. The authors estimate that the vote share of the incumbent party in the next mayoral election increased by around 8 percentage points in municipalities exposed to the reform. The study by [Carreri and Martinez \(2022\)](#) is very well prepared and advances the literature portraying the consequences of fiscal rules.

5.3 Germany

Local governments in individual German states have been exposed to fiscal rules. The state government in North Rhine-Westphalia, Germany's largest state with around 18 million inhabitants, imposed a fiscal rule on 61 of its 396 municipalities in the year 2011 (*Stärkungspakt Stadtfinanzen*). The program encompassed both intergovernmental transfers to the municipalities enrolled in the program and consolidation requirements. The municipalities' budgets had to be consolidated in the year 2017 (2021 net of the intergovernmental transfers). [Fremerey et al. \(2022\)](#) exploit this reform to examine how this fiscal rule influenced the municipalities' public finances and public good provision. Quasi-exogenous variation of being treated is achieved by comparing the 61 treated municipalities with 76 municipalities that were also likely to be treated before the program started. The state government considered 137 (61 in the treatment group and 76 in the control group) municipalities to be suitable for being part of the program. I was wondering based on which characteristics the state government chose municipalities to be treated. The authors describe that the state government chose municipalities to be treated based on municipalities' equity. They have contacted the state government to learn about the running variable, for example, the state government's forecast of municipalities' equity in 2016, but did not receive any official information on the running variable. Individual observable characteristics of the treated and non-treated municipalities do not differ before treatment. Examples

include total debt, loans expenses, working-age population, number of plants etc. I was also wondering whether treated and non-treated municipalities differed regarding political preferences of the electorates. The authors could have compared party vote shares between treated and non-treated municipalities. Did the state government wish to (not) consider individual municipalities?

[Fremerey et al. \(2022\)](#) compare the outcomes for a period of up to eight years after treatment. I am concerned about confounding events. Treatment occurred in the year 2011. There were local elections in the year 2014. We do not know about the extent to which being treated by the *Stärkungspakt Stadtfinanzen* influenced the outcomes of the local elections in 2014 that, in turn, may well have influenced the dependent variables the authors examine. New mayors and members of municipal councils are likely to have influenced the outcomes which the authors investigate. What is more, a reform of the fiscal equalization scheme influenced public finances and is likely to have confounded the effects of fiscal rules.⁹ Monitoring rules also changed and may have confounded the fiscal rule treatment.

The results show that treated municipalities were successful in consolidating their budgets. Six to eight years after treatment, treated municipalities were around 44 percentage points more likely to have balanced budgets or surpluses than non-treated municipalities. Consolidation strategies differed across small and large municipalities: small municipalities decreased public expenditure. Large municipalities increased the business and property tax rates and, in turn, tax revenues. There is no evidence that the fiscal rule influenced (economic) outcomes such as the number of establishments or working-age population.

[Christofzik and Kessing \(2018\)](#) investigate the consequences of withdrawing fiscal oversight on debt in municipalities in NRW. The results show that withdraw of fiscal oversight increased debt per capita by around 205 Euros (around 30 % of a standard deviation). This effect is numerically substantial - the authors conclude that their “analysis provides strong evidence for causal effects of fiscal rules on policy outcomes” (p. 83). Since 1991, the municipalities need to present their budgets to a supervisory authority. When the municipality is in fiscal distress, it needs to present a budget consolidation plan. This plan needs to show how the municipality will achieve a balanced budget within a period of four years. If the supervisory authority is not convinced by the plan, the municipality is put under supervision. The municipality then needs approval for changes in local tax rates and designing the budget. The supervision authority may even fully be in charge of the municipalities’ financial policies.

Over the period 2004-2009, cameralistic public sector accounting was replaced by accrual accounting. To smooth the transition from the cameralistic to accrual accounting, municipalities had a one time opportunity to create an equalization reserve (*Ausgleichsrücklage*) that is issuing debt.¹⁰ The state government provided this opportunity

⁹[Christofzik and Schneider \(2019\)](#) describe how the reform of the fiscal equalization scheme influenced municipalities’ budgets.

¹⁰On how changes in accounting standards influenced fiscal outcomes in Germany see also [Christofzik \(2019\)](#) and [Dorn et al. \(2021\)](#).

to handle potential uncertainties during the transition from cameralistic to accrual accounting. Municipalities de facto switched from cameralistic to accrual accounting over the period 2006-2009. [Christofzik and Kessing \(2018\)](#) focus on municipalities that were supervised by the authority and use the timing of switching from cameralistic to accrual accounting - and having the one time opportunity to issue new debt - as treatment. The authors are aware that treatment was not exogenous; the municipalities decided themselves when to switch from cameralistic to accrual accounting. The authors argue, for example, that treatment was “largely determined by operational considerations within the local administration” (p. 76). Balance tests show that many observable characteristics of the municipalities (population etc.) do differ between municipalities which switched accounting standards in individual years. [Christofzik and Kessing \(2018\)](#) also compare municipalities in NRW with municipalities in the neighboring state of Hesse. In Hesse, municipalities also needed to switch from cameralistic to accrual accounting but did not have the opportunity to issue new debt (withdraw of the fiscal rule). When comparing municipalities in NRW and Hesse, the results show that withdrawing the fiscal rule increased debt by 327 Euro per capita.

The authors elaborate on the mechanisms through which public debt increased in treated municipalities. Treated municipalities increased debt by increasing operating expenditure. Effects are strong in electorally contested districts, indicating that politicians increased spending to become re-elected.

5.4 Norway

In Norway, local governments also face BBR. Budgets generally need to be balanced. They can be carried over to, but must be balanced within two years. The national government approved the local governments’ budgets and borrowing plans till 2001 when a reform took place. The reform granted those municipalities leeway which complied with the BBR: the municipalities complying with the BBR did not need to ask for approval of their budgets and borrowing plans. In other words, this reform loosened fiscal rules. Municipalities reduced deficits after the reform ([Borge and Hopland, 2020](#)). There was, however, another policy accompanied with the loosening of fiscal rules that makes it impossible to disentangle the effect of loosening fiscal rules: the national government also introduced a register listing all governments violating the BBR. The register was called Register for Governmental Approval of Financial Obligations (Robek), received a great deal of media attention and operated as a “list of shame”. Being listed in Robek has been shown to influence electoral outcomes. Voters punished the incumbent party when their municipality was listed in Robek ([Hopland, 2014](#)). We do not know whether decreasing fiscal deficits in the course of the 2001 reform was based on granting local governments more autonomy in designing their budgets or incumbents’ concerns to be punished at the ballot box when they run high deficits.

5.5 Japan

Japan had 1,741 municipalities in the year 2020. The local governments are faced with fiscal rules since the 1950s: when deficits and debt are pronounced and exceed thresholds, local governments have to consolidate their budgets. Local governments also need to prepare consolidation strategies which need to be approved by the national government and are not allowed to issue local bonds. The national government may also supervise the municipalities' budgets. In 2008, fiscal rules were reformed. A major aspect of the reform was considering budgets of extra-governmental organizations such as public enterprises to prevent that local governments shift deficits. The fiscal performance of the municipalities was evaluated based on four fiscal indicators instead of two fiscal indicators before the reform. [Hirota and Yunoue \(2022\)](#) examine how this reform influenced the municipalities' fiscal outcomes. The authors evaluate the period 2007-2010, it thus includes just one pre-treatment period. There are two reasons for using just one pre-treatment year: first, municipal mergers took place in 2005. Second, there is a lack of data for the year 2006. The authors thus cannot compare (parallel?) pre-trends in the outcome variables between treated and non-treated municipalities. Balance tests of observable characteristics such as population, political majorities etc. would have been useful to be discussed.

Those municipalities which had sound public finances before the reform were less likely to be affected by the reform in fiscal rules and its consequences (e. g. being supervised by the national government) than municipalities with unsound public finances. The results show that municipalities with stricter fiscal rules had lower deficits and debt after the reform than municipalities which did not experience stricter fiscal rules. The authors also arrive at one quite counter-intuitive result: treated municipalities were more active in increasing stock-flow adjustments (the difference in the change of the debt stock and the deficit) than non-treated municipalities. The reform was intended to reduce and not to increase stock-flow adjustments. The 2008 reform and the effects of fiscal rules on public finances in Japanese municipalities need to be examined in a more rigorous manner. In any event, the study of [Hirota and Yunoue \(2022\)](#) is useful in drawing attention to the issue of how fiscal rules influence creative accounting.

6 Conclusion

Fiscal rules are implemented to handle a trade-off between flexibility and commitment. The theories on welfare implications of fiscal rules portray this trade-off in an excellent manner. Those theories provide, however, only very few implications about how fiscal rules influence fiscal policies and other economic outcomes. By contrast, the empirical literature on the effects of fiscal rules on observable economic outcomes is rich. Core results of this empirical literature include that fiscal rules help to decrease budget deficits and debt. Fiscal rules decrease governments' borrowing costs and promote

economic growth. Strict rules are more effective than lax rules, and features such as escape clauses help to sustain some flexibility. Clearly, those effects do not help to make inferences about unobservable welfare.

An important avenue for future empirical research is to investigate unintended effects of fiscal rules in more detail. Theoretical studies may well guide the future empirical research on unintended effects. An example is creative accounting. Fiscal rules are likely to give rise to creative accounting (Von Hagen and Harden, 1995; Milesi-Ferretti, 2004; Von Hagen and Wolff, 2006): When politicians are constrained in running deficits in the core budget, they issue debt by, for example, issuing extra budgets. There are just a few studies investigating the extent to which fiscal rules promote creative accounting. We need causal evidence in this area. If fiscal rules would give rise to creative accounting and public debt hidden in extra budgets, reforms of fiscal rules may well be discussed that prevent creative accounting and extra budgets.

Another avenue for future research on fiscal rules relates to compliance with the rules. Empirical research describes determinants of compliance with fiscal rules (e.g. Reuter, 2015, Reuter, 2019, Larch et al., 2023). The results show, for example, that the probability to comply with national rules was positively associated with the presence of independent monitoring and enforcement bodies (issuing real-time alerts). The probability to comply with supra-national EU rules was positively associated with government efficiency. This evidence is based on conditional correlations. Causal evidence on compliance with fiscal rules would be helpful. What is more, compliance with fiscal rules is important for designing fiscal rules in practice. We need measures to make sure that governments do indeed comply with the fiscal rules. Compliance requires (i) the willingness of citizens and politicians and (ii) incentive schemes and institutions to follow the rules.

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Table 1 CROSSNATIONAL STUDIES - DEFICITS AND DEBT

| Study | Dependent variable | FR type | Result | Sample | FR data | Method |
|-------------------------------|--|--------------------|--------|-----------------|--------------------------|--------------------|
| Aaskoven and Wiese (2022) | D debt (% of GDP) | Any | - | 1967-2013 (19) | self-compiled | GLS random effects |
| Caselli and Reynaud (2020) | budget balance (% of GDP) | Any | + | 1985-2015 (142) | Schaechter | OLS |
| Caselli and Reynaud (2020) | budget balance (% of GDP) | Any | 0 | 1985-2015 (142) | Schaechter | IV |
| Caselli and Reynaud (2020) | budget balance (% of GDP) | Strength index | + | 1985-2015 (142) | Schaechter | IV |
| Asatryan et al. (2018) | Public debt (% of GDP) | Constitutional BBR | - | 1945-2015 (132) | Schaechter | OLS |
| Asatryan et al. (2018) | Growth in Public debt (% of GDP) | Constitutional BBR | - | 1945-2015 (132) | Schaechter | OLS |
| Combes et al. (2018) | Primary budget balance (% of GDP) | Any | + | 1990-2009 (104) | Bova et al. | GMM |
| Combes et al. (2018) | Primary budget balance (% of GDP) | BBR | + | 1990-2009 (104) | Bova et al. | GMM |
| Combes et al. (2018) | Primary budget balance (% of GDP) | ER | + | 1990-2009 (104) | Bova et al. | GMM |
| Combes et al. (2018) | Primary budget balance (% of GDP) | DR | 0 | 1990-2009 (104) | Bova et al. | GMM |
| Combes et al. (2018) | Overall budget balance (% of GDP) | Any | + | 1990-2009 (104) | Bova et al. | GMM |
| Combes et al. (2018) | Overall budget balance (% of GDP) | BBR | + | 1990-2009 (104) | Bova et al. | GMM |
| Combes et al. (2018) | Overall budget balance (% of GDP) | ER | + | 1990-2009 (104) | Bova et al. | GMM |
| Combes et al. (2018) | Overall budget balance (% of GDP) | DR | 0 | 1990-2009 (104) | Bova et al. | GMM |
| Badinger and Reuter (2017) | structural fiscal balance (% of potential GDP) | BBR | + | 1985-2012 (47) | Own measure (Schaechter) | OLS/IV |
| Badinger and Reuter (2017) | structural fiscal balance (% of potential GDP) | DR | + | 1985-2012 (47) | Own measure (Schaechter) | OLS/IV |
| Badinger and Reuter (2017) | structural fiscal balance (% of potential GDP) | overall stringency | + | 1985-2012 (47) | Own measure (Schaechter) | OLS/IV |
| Bergman et al. (2016) | cyclically-adjusted primary balance (% of GDP) | Any | + | 1990-2012 (27) | Schaechter | OLS/GMM |
| Bergman et al. (2016) | cyclically-adjusted primary balance (% of GDP) | BBR | + | 1990-2012 (27) | Schaechter | OLS/GMM |
| Bergman et al. (2016) | cyclically-adjusted primary balance (% of GDP) | ER | + | 1990-2012 (27) | Schaechter | OLS/GMM |
| Bergman et al. (2016) | cyclically-adjusted primary balance (% of GDP) | RR | 0 | 1990-2012 (27) | Schaechter | OLS/GMM |
| Bergman et al. (2016) | cyclically-adjusted primary balance (% of GDP) | DR | + | 1990-2012 (27) | Schaechter | OLS/GMM |
| Malritz and Wüste (2015) | primary budget balance | FR index | + | 1991-2011 (27) | self-compiled | None |
| Dahan and Strawczynski (2013) | D deficit (% of GDP) | BBR | - | 1960-2010 (22) | self-compiled | None |
| Dahan and Strawczynski (2013) | D deficit (% of GDP) | ER | 0 | 1960-2010 (22) | self-compiled | None |
| Hallerberg et al. (2007) | Change in Public debt (% of GDP) | Any | - | 1985-2004 (15) | Self compiled | OLS |

Table 2 CROSSNATIONAL STUDIES - PUBLIC EXPENDITURE

| Study | Dependent variable | FR type | Result | Sample | FR data | Method |
|------------------------------|---|----------------|------------------------------|-----------------|----------------|---------------|
| Vinturis (2022) | Total public spending (% of GDP) | Any | - | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Public consumption (% of GDP) | Any | - | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Public investment (% of GDP) | Any | 0 | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Public investment/public consumption | Any | + | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Total public spending (% of GDP) | ER | 0 | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Public consumption (% of GDP) | ER | - | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Public investment (% of GDP) | ER | 0 | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Public investment/public consumption | ER | 0 | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Total public spending (% of GDP) | DR | - | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Public consumption (% of GDP) | DR | - | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Public investment (% of GDP) | DR | 0 | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Public investment/public consumption | DR | + | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Total public spending (% of GDP) | BBR | - | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Public consumption (% of GDP) | BBR | - | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Public investment (% of GDP) | BBR | + | 1985-2015 (184) | Schaechter | Matching |
| Vinturis (2022) | Public investment/public consumption | BBR | + | 1985-2015 (184) | Schaechter | Matching |
| Jürgens (2022) | Public investment/(logs) | Any | - (especially in recessions) | 1985-2019 (23) | Schaechter | OLS/GMM |
| Jürgens (2022) | Government investment/(logs) | Any | - (especially in recessions) | 1990-2019 (23) | Schaechter | OLS |
| Jürgens (2022) | Public investment/(logs) | Any | - | 1985-2019 (23) | EU | OLS |
| Delgado-Tellez et al. (2022) | social expenditure FD in (% of GDP) | Any | - | 1960-2015 (22) | Schaechter | OLS |
| Delgado-Tellez et al. (2022) | investment expenditure FD in (% of GDP) | Any | -/0 | 1960-2015 (22) | Schaechter | OLS |
| Delgado-Tellez et al. (2022) | investment expenditure growth rates | Any | 0 | 1960-2015 (22) | Schaechter | OLS |

Table 3 CROSSNATIONAL STUDIES - PUBLIC EXPENDITURE CONTINUED

| Study | Dependent variable | FR type | Result | Sample | FR data | Method |
|-------------------------------|---|----------------------------------|---------------|-----------------|----------------|---------------|
| Ardanaz et al. (2021) | Investment expenditure | Flexible FR cond. fiscal cons | 0 | 1990-2018 (75) | Guerguil | OLS/IV |
| Asatryan et al. (2018) | Total expenditure (% of GDP) | Constitutional BBR | - | 1945-2015 (132) | Schaechter | OLS |
| Asatryan et al. (2018) | Growth in Total expenditure (% of GDP) | Constitutional BBR | - | 1945-2015 (132) | Schaechter | OLS |
| Schakel et al. (2018) | Public health care expenditure (logs) | Any | - | 1985-2014 (32) | Schaechter | OLS |
| Schakel et al. (2018) | Public health care expenditure (logs) | ER | - | 1985-2014 (32) | Schaechter | OLS |
| Schakel et al. (2018) | Public health care expenditure (logs) | BBR | - | 1985-2014 (32) | Schaechter | OLS |
| Dahan and Strawczynski (2013) | D log (total expenditure) | BBR | -/0 | 1960-2010 (22) | self-compiled | None |
| Dahan and Strawczynski (2013) | D log (total expenditure) | ER | - | 1960-2010 (22) | self-compiled | None |
| Dahan and Strawczynski (2013) | D log (transfer payments) | BBR | 0 | 1960-2010 (22) | self-compiled | None |
| Dahan and Strawczynski (2013) | - D log (government consumption) | ER | - | 1960-2010 (22) | self-compiled | None |
| Dahan and Strawczynski (2013) | D log (transfer payments) | BBR | 0 | 1960-2010 (22) | self-compiled | None |
| Dahan and Strawczynski (2013) | - D log (government consumption) | ER | - | 1960-2010 (22) | self-compiled | None |
| Dahan and Strawczynski (2013) | D log (public investments) | BBR | 0 | 1960-2010 (22) | self-compiled | None |
| Dahan and Strawczynski (2013) | - D log (government consumption) | ER | 0 | 1960-2010 (22) | self-compiled | None |
| Dahan and Strawczynski (2013) | D log (public investments) | BBR | 0 | 1960-2010 (22) | self-compiled | None |
| Blume and Voigt (2013) | - D log (government consumption) | Any | - | 1990s (47) | self-compiled | OLS |
| | Total government expenditure (% of GDP) | | | | | |

Table 4 CROSSNATIONAL STUDIES - GROWTH, VOLATILITY, INFLATION

| Study | Dependent variable | FR type | Result | Sample | FR data | Method |
|-----------------------------|---|----------------|---|-----------------|--------------------------|---------------|
| Gründer and Potrafke (2023) | GDP per capita | BBR | + | 1985-2019 (106) | Schaechter | OLS/IV |
| Combes et al. (2018) | Inflation | Any | - (amp with IT) | 1990-2009 (106) | Bova et al. | GMM |
| Badinger and Reuter (2017) | standard deviation of GDP per capita growth | BBR | 0 | 1985-2012 (74) | Own measure (Schaechter) | OLS/IV |
| Badinger and Reuter (2017) | standard deviation of GDP per capita growth | BBR | 0 | 1985-2012 (74) | Own measure (Schaechter) | OLS/IV |
| Badinger and Reuter (2017) | standard deviation of GDP per capita growth | BBR | 0 | 1985-2012 (74) | Own measure (Schaechter) | OLS/IV |
| Sacchi and Salotti (2015) | ln standard deviation of inflation | overall | 0 (fiscal rules mitigate discretionary fiscal policy) | 1985-2012 (21) | Schaechter | OLS/GMM |
| Sacchi and Salotti (2015) | ln standard deviation of inflation | BBR | 0 (fiscal rules mitigate discretionary fiscal policy) | 1985-2012 (21) | Schaechter | OLS/GMM |
| Sacchi and Salotti (2015) | ln standard deviation of inflation | ER | 0 (fiscal rules mitigate discretionary fiscal policy) | 1985-2012 (21) | Schaechter | OLS/GMM |
| Sacchi and Salotti (2015) | ln standard deviation of inflation | RR | 0 (fiscal rules mitigate discretionary fiscal policy) | 1985-2012 (21) | Schaechter | OLS/GMM |
| Sacchi and Salotti (2015) | ln standard deviation of inflation | DR | 0 (fiscal rules mitigate discretionary fiscal policy) | 1985-2012 (21) | Schaechter | OLS/GMM |
| Afonso and Jalles (2013) | GDP per capita | Any | + | 1990-2008 (25) | self-compiled | GMM |
| Afonso and Jalles (2013) | GDP per capita | ER | + | 1990-2008 (25) | self-compiled | GMM |
| Afonso and Jalles (2013) | GDP per capita | BBR + DR | 0/+ | 1990-2008 (25) | self-compiled | GMM |

Table 5 CROSSNATIONAL STUDIES – TAX REVENUE AND FISCAL VOLATILITY

| Study | Dependent variable | FR type | Result | Sample | FR data | Method |
|------------------------------|---|--------------------|---|-----------------|--------------------------|---------|
| <i>Tax revenue</i> | | | | | | |
| Asatryan et al. (2018) | Tax revenue (% of GDP) | Constitutional BBR | 0 | 1945-2015 (132) | Schaechter | OLS |
| Asatryan et al. (2018) | Growth in Tax revenue (% of GDP) | Constitutional BBR | 0 | 1945-2015 (132) | Schaechter | OLS |
| <i>Fiscal volatility</i> | | | | | | |
| Reuter et al. (2022) | primary expenditure | BBR Index | 0 | 1996-2015 (28) | Schaechter | OLS/GMM |
| Reuter et al. (2022) | public consumption | BBR Index | - | 1996-2015 (28) | Schaechter | OLS/GMM |
| Reuter et al. (2022) | public consumption + investment | BBR Index | - | 1996-2015 (28) | Schaechter | OLS/GMM |
| Reuter et al. (2022) | primary balance (% of GDP) | BBR Index | - | 1996-2015 (28) | Schaechter | OLS/GMM |
| Reuter et al. (2022) | primary expenditure | ER Index | 0 | 1996-2015 (28) | Schaechter | OLS/GMM |
| Reuter et al. (2022) | public consumption | ER Index | 0 | 1996-2015 (28) | Schaechter | OLS/GMM |
| Reuter et al. (2022) | public consumption + investment | ER Index | 0 | 1996-2015 (28) | Schaechter | OLS/GMM |
| Reuter et al. (2022) | primary balance (% of GDP) | ER Index | 0 | 1996-2015 (28) | Schaechter | OLS/GMM |
| Badinger and Reuter (2017) | standard deviation of primary balance | BBR | - | 1985-2012 (74) | Own measure (Schaechter) | OLS/IV |
| Badinger and Reuter (2017) | real expenditure growth | DR | 0/- | 1985-2012 (74) | Own measure (Schaechter) | OLS/IV |
| Badinger and Reuter (2017) | standard deviation of real expenditure growth | DR | 0/- | 1985-2012 (74) | Own measure (Schaechter) | OLS/IV |
| Badinger and Reuter (2017) | real expenditure growth | overall stringency | - | 1985-2012 (74) | Own measure (Schaechter) | OLS/IV |
| Sacchi and Salotti (2015) | ln standard deviation of real GDP per capita growth | overall | - (fiscal rules mitigate discretionary fiscal policy) | 1985-2012 (21) | Schaechter | OLS/GMM |
| Sacchi and Salotti (2015) | ln standard deviation of real GDP per capita growth | BBR | - (fiscal rules mitigate discretionary fiscal policy) | 1985-2012 (21) | Schaechter | OLS/GMM |
| Sacchi and Salotti (2015) | ln standard deviation of real GDP per capita growth | ER | - (fiscal rules mitigate discretionary fiscal policy) | 1985-2012 (21) | Schaechter | OLS/GMM |
| Sacchi and Salotti (2015) | ln standard deviation of real GDP per capita growth | RR | 0 (fiscal rules mitigate discretionary fiscal policy) | 1985-2012 (21) | Schaechter | OLS/GMM |
| Sacchi and Salotti (2015) | ln standard deviation of real GDP per capita growth | DR | - (fiscal rules mitigate discretionary fiscal policy) | 1985-2012 (21) | Schaechter | OLS/GMM |
| Bergman and Hutchison (2015) | real GDP per capita growth cyclically adjusted government expenditure | own measure | - | 1985-2012 (21) | Schaechter | OLS/GMM |

Table 6 CROSSNATIONAL STUDIES – BORROWING COSTS

| Study | Dependent variable | FR type | Result | Sample | FR data | Method |
|-------------------------------|---|--------------------|--------|-----------------|--------------------------|----------|
| Thornton and Vasilakis (2020) | domestic borrowing spread | Any | - | 1985-2017 (61) | Schaechter | Matching |
| Thornton and Vasilakis (2020) | private/public treasury bill yield | Any | - | 1985-2017 (61) | Schaechter | Matching |
| Afonso and Jalles (2019) | sovereign bond yield spreads against US | Any | - | 1980-2016 (53) | Schaechter (Schaechter) | GMM |
| Afonso and Jalles (2019) | sovereign bond yield spreads against US | BBR | 0 | 1980-2016 (53) | Schaechter (Schaechter) | GMM |
| Afonso and Jalles (2019) | sovereign bond yield spreads against US | DR | 0 | 1980-2016 (53) | Schaechter (Schaechter) | GMM |
| Afonso and Jalles (2019) | sovereign bond yield spreads against US | ER | - | 1980-2016 (53) | Schaechter (Schaechter) | GMM |
| Afonso and Jalles (2019) | sovereign bond yield spreads against US | RR | 0 | 1980-2016 (53) | Schaechter (Schaechter) | GMM |
| Thornton and Vasilakis (2018) | risk premium | Any | - | 1985-2015 (184) | Schaechter | Matching |
| Badinger and Reuter (2017) | government bond spread against US (long-run) | BBR | - | 1985-2012 (36) | Own measure (Schaechter) | OLS/IV |
| Badinger and Reuter (2017) | government bond spread against US (long-run) | DR | - | 1985-2012 (36) | Own measure (Schaechter) | OLS/IV |
| Badinger and Reuter (2017) | government bond spread against US (long-run) | overall stringency | - | 1985-2012 (36) | Own measure (Schaechter) | OLS/IV |
| Badinger and Reuter (2017) | government bond spread against US (short-run) | BBR | - | 1985-2012 (30) | Own measure (Schaechter) | OLS/IV |
| Badinger and Reuter (2017) | government bond spread against US (short-run) | DR | - | 1985-2012 (30) | Own measure (Schaechter) | OLS/IV |
| Badinger and Reuter (2017) | government bond spread against US (short-run) | overall stringency | - | 1985-2012 (30) | Own measure (Schaechter) | OLS/IV |
| Iara and Wolff (2014) | log government bond spread against German Bund (10 years) | Any | - | 1999-2009 (10) | Own measure (Schaechter) | OLS/IV |
| Heinemann et al. (2014) | government bond spread against German Bund (10 years) | Any | - | 1993-2009 (15) | FEVD | |

Table 7 CROSSNATIONAL STUDIES — MITIGATING POLITICAL BUSINESS CYCLES

| Study | Dependent variable | FR type | Result | Sample | FR data | Method |
|---------------------------|-----------------------------------|------------------|--|----------------|------------|---------|
| Strong (2022) | debt (% of GDP) | BBR | - (higher debt) in election years! no PBC | 2000-2020 (32) | Schaechter | OLS |
| Strong (2022) | expenditure (% of GDP) | BBR | | 2000-2020 (32) | Schaechter | OLS |
| Strong (2022) | M2 growth (% of GDP) | BBR | no PBC | 2000-2020 (32) | Schaechter | OLS |
| Strong (2022) | debt (% of GDP) | BBR | no PBC | 2000-2020 (32) | Schaechter | GMM |
| Strong (2022) | expenditure (% of GDP) | BBR | no PBC | 2000-2020 (32) | Schaechter | GMM |
| Strong (2022) | M2 growth (% of GDP) | BBR | - | 2000-2020 (32) | Schaechter | GMM |
| Gootjes et al. (2021) | primary budget balance (% of GDP) | Any | - | 1984-2015 (77) | Schaechter | OLS/GMM |
| Gootjes et al. (2021) | primary budget balance (% of GDP) | ER | - | 1984-2015 (77) | Schaechter | OLS/GMM |
| Gootjes et al. (2021) | primary budget balance (% of GDP) | BBR | - | 1984-2015 (77) | Schaechter | OLS/GMM |
| Gootjes et al. (2021) | primary budget balance (% of GDP) | RR | - | 1984-2015 (77) | Schaechter | OLS/GMM |
| Gootjes et al. (2021) | primary budget balance (% of GDP) | DR | - | 1984-2015 (77) | Schaechter | OLS/GMM |
| Ademmer and Dreher (2016) | budget surplus (% of GDP) | overall strength | - in election years! | 1996-2012 (25) | EU Comm. | DGMM |

Table 8 US STATE LEVEL

| Study | Dependent variable | FR type | Result | Sample | Method |
|---------------------------|-----------------------------------|---|-------------------|---------------------------------|--|
| Deng and Liu (2022) | government bond spreads | BBR | - | 1976-2020 (48) | OLS/IV |
| Deng and Liu (2022) | CDS spreads | BBR | - | 1976-2020 (48) | OLS/IV |
| Eliason and Lutz (2018) | Total taxes (per capita) | Any | 0 | 1977-2013 (47) | SC |
| Eliason and Lutz (2018) | Total expenditures (per capita) | Any | 0 | 1977-2013 (47) | SC |
| Eliason and Lutz (2018) | Education spending (per capita) | Any | 0 | 1977-2013 (47) | SC |
| Eliason and Lutz (2018) | Health spending (per capita) | Any | 0 | 1977-2013 (47) | SC |
| Eliason and Lutz (2018) | Debt (per capita) | Any | 0 | 1977-2013 (47) | SC |
| Hong (2015) | budget cuts | BBR | - | 2004-2010 (?) | OLS |
| Smith and Hou (2013) | Total spending (p.c.) | BBR dummies | - (strict ones) | 1950-2004 (48) | OLS |
| Smith and Hou (2013) | General spending (p.c.) | BBR dummies | - (strict ones) | 1950-2004 (48) | OLS |
| Smith and Hou (2013) | Operating spending (p.c.) | BBR dummies | - (strict ones) | 1950-2004 (48) | OLS |
| Fatás and Mihov (2006) | volatility of government spending | no-carry over, ACIR | - | 1963-2000 (48) | OLS |
| Fatás and Mihov (2006) | pro-cyclicality of spending | no-carry over, ACIR | + | 1963-2000 (48) | OLS |
| Krol and Svorny (2007) | standard deviation of real GDP | no carryover | - | 1969-1995 (50) | OLS |
| Canova and Pappa (2006) | 12 macro variables | 7 FR dummies | weak corr | 1969-2000 (48) | asymptotic test |
| Primo (2006) | general expenditure | Spending limit | - | 1969-2020 (47) | rank sum test FGLS/ Error correction |
| Rose (2006) | Surplus (p.c.) | No-carryover | Mitigate PBC | 1974-1999 (43) | GMM |
| Rose (2006) | Spending (p.c.) | No-carryover | Mitigate PBC | 1974-1999 (43) | GMM |
| Rose (2006) | Tax revenue (p.c.) | No-carryover | 0 (PBC) | 1974-1999 (43) | GMM |
| Sørensen et al. (2001) | Cyclical coefficient surplus | ACIR | -/0 | 1978-1994 (48) | OLS |
| Sørensen et al. (2001) | Budget Surplus (in % of GSP) | BBR dummies $\times GSP_{growth}$ | - | cross-section 1978-1994 (48) | OLS |
| Poterba and Rueben (2001) | 20-year bond spread | BBR (ACIR) | - (deficit shock) | 1988-1999 (39) | OLS |
| Poterba and Rueben (2001) | 20-year bond spread | DR | - (deficit shock) | 1988-1999 (39) | OLS |
| Poterba and Rueben (2001) | 20-year bond spread | RR | 0 (deficit shock) | 1988-1999 (39) | OLS |
| Poterba and Rueben (2001) | 20-year bond spread | RR | + | 1988-1999 (39) | OLS |
| Poterba and Rueben (2001) | 20-year bond spread | supermajority | + | 1988-1999 (39) | OLS |
| Poterba and Rueben (1999) | 20-year bond spread | ACIR | tax provision | 1973-1995 (40) | OLS |
| Poterba and Rueben (1999) | 20-year bond spread | restrict debt issue | + | 1973-1995 (40) | OLS |
| Poterba and Rueben (1999) | 20-year bond spread | expenditure limit | - | 1973-1995 (40) | OLS |
| Poterba and Rueben (1999) | 20-year bond spread | revenue limit | - | 1973-1995 (40) | OLS |
| Poterba and Rueben (1999) | 20-year bond spread | legislature must pass | 0 | 1973-1995 (40) | OLS |
| Poterba and Rueben (1999) | 20-year bond spread | balanced budget | 0 | 1973-1995 (40) | OLS |
| Poterba and Rueben (1999) | 20-year bond spread | correct deficit in the next year cycle | 0 | 1973-1995 (40) | OLS |
| Poterba and Rueben (1999) | 20-year bond spread | correct deficit in the next two-year cycle | 0 | 1973-1995 (40) | OLS |
| Poterba and Rueben (1999) | 20-year bond spread | correct deficit in the current one-year cycle | - | 1973-1995 (40) | OLS |

Table 9 US STATE LEVEL - CONTINUED

| Study | Dependent variable | FR type | Result | Sample | Method |
|--------------------------------|--|--|--------|---------------------------------|---------|
| Levinson (1998) | standard deviation of real GDP | no carryover | - | 1969-1995 (50) | OLS |
| Levinson (1998) | maximum difference of real GDP | no carryover | - | 1969-1995 (50) | OLS |
| Alesina and Bayoumi (1996) | primary surplus (% of GDP) | ACIR | + | 1988-1992 (48) cross-section | OLS |
| Alesina and Bayoumi (1996) | average surplus (% of GDP) | ACIR | + | 1965-1992 (48) cross-section | OLS |
| Alesina and Bayoumi (1996) | St. dev. of average surplus (% of GDP) | ACIR | - | 1965-1992 (48) cross-section | OLS |
| Alesina and Bayoumi (1996) | Cyclical coefficient surplus | ACIR | - | 1965-1992 (48) cross-section | OLS |
| Alesina and Bayoumi (1996) | St. dev. of state product | ACIR | 0 | 1965-1992 (48) cross-section | OLS |
| Bohn and Inman (1996) | states' general funds surplus (real, p.c.) | governor to submit a BB | + / 0 | 1970-1991 (47) cross-section | OLS/GLS |
| Bohn and Inman (1996) | states' general funds surplus (real, p.c.) | legislature to pass a BB | + / 0 | 1970-1991 (47) | OLS/GLS |
| Bohn and Inman (1996) | states' general funds surplus (real, p.c.) | carried-over deficit corrected next year | + / 0 | 1970-1991 (47) | OLS/GLS |
| Bohn and Inman (1996) | states' general funds surplus (real, p.c.) | no carry-over | + | 1970-1991 (47) | OLS/GLS |
| Bayoumi and Eichengreen (1995) | Cyclical responsiveness | ACIR | - | 1971-1990 (50) cross-section | OLS |
| Bayoumi et al. (1995) | 25-year bond spread | ACIR | - | 1981-1990 (38) | OLS |
| Poterba (1994) | Delta expenditure | Weak BBR | + | 1988-1992 (27) | OLS |
| Poterba (1994) | Delta tax revenue | Weak BBR | 0 | 1988-1992 (27) | OLS |
| Poterba (1994) | Delta expenditure | Tax limit | 0 | 1988-1992 (27) | OLS |
| Poterba (1994) | Delta tax revenue | Tax limit | - | 1988-1992 (27) | OLS |
| Elder (1992) | states' tax revenues | ER | - | 1950-1985 (19) | OLS |
| Elder (1992) | states' tax revenues | RR | 0 | 1950-1985 (19) | OLS |
| Abrams and Dougan (1986) | expenditure p.c. | ER & RR | 0 | 1980 (50) | OLS |
| Abrams and Dougan (1986) | expenditure p.c. | Constitutional limits | 0 | 1980 (50) | OLS |

Table 10 THE EFFECT OF FISCAL RULES — CANADIAN PROVINCES

| Study | Dependent variable | FR type | Result | Sample | Method |
|---------------------------|--|----------------|---------------|----------------|---------------|
| Mou et al. (2021) | budget balance (% of GDP) | BBL stringency | + | 1980-2018 (10) | GLS/IV |
| Mou et al. (2021) | debt (% of GDP) | BBL stringency | - | 1980-2018 (10) | GLS/IV |
| Mou et al. (2021) | SFA (% of GDP) | BBL stringency | - | 1980-2018 (10) | GLS/IV |
| Mou et al. (2018) | budget balance (p. c.) | BBL stringency | + | 1981-2013 (10) | GLS |
| Mou et al. (2018) | budget balanced (dummy) | BBL stringency | + | 1981-2013 (10) | Logit |
| Mou et al. (2018) | budget balance (% of GDP) | BBL stringency | + | 1981-2013 (10) | GLS/IV |
| Mou et al. (2018) | debt (levels) | BBL stringency | - | 1981-2013 (10) | GLS |
| Mou et al. (2018) | debt (% of GDP) | BBL stringency | - | 1981-2013 (10) | GLS |
| Mou et al. (2014) | price index net government expenditure | BBL stringency | 0/- | 1981-2007 (10) | GLS |
| Tapp (2013) | budget balance (% of GDP) | Any | + | 1981-2007 (10) | OLS/GMM/IV |
| Tapp (2013) | net debt (% of GDP) | Any | - | 1981-2007 (10) | OLS/GMM/IV |
| Tapp (2013) | spending (% of GDP) | Any | 0 | 1981-2007 (10) | OLS/GMM/IV |
| Tapp (2013) | revenue (% of GDP) | Any | 0 | 1981-2007 (10) | OLS/GMM/IV |
| Tellier and Imbeau (2004) | budget balance (% of spending) | BBL stringency | + | 1968-2000 (10) | SUR |

Table 11 THE EFFECTS OF FISCAL RULES—SWISS CANTONS

| Study | Dependent variable | FR type | Result | Sample | Method |
|----------------------------------|--|------------------|--|----------------|------------|
| Burret and Feld (2018a) | expenditures (p.c.) | Any dummy | 0 | 1980-2011 (26) | OLS |
| Burret and Feld (2018a) | revenues (p.c.) | Any dummy | + | 1980-2011 (26) | OLS |
| Burret and Feld (2018a) | debt (p.c.) | Any dummy | - | 1980-2011 (26) | OLS |
| Burret and Feld (2018a) | deficit (p.c.) | Any dummy | - | 1980-2011 (26) | OLS |
| Burret and Feld (2018a) | combined deficit (p.c.) | Any dummy | - | 1980-2011 (26) | OLS |
| Burret and Feld (2018a) | cantonal + local investment | Any dummy | + | 1980-2011 (26) | OLS |
| Burret and Feld (2018b) | local expenditures (p.c.) | Any dummy | 0 | 1980-2011 (25) | OLS |
| Burret and Feld (2018b) | cantonal level | dummy, index | | | |
| Burret and Feld (2018b) | local expenditures (p.c.) | Any dummy, index | 0 | 1982-2007 (26) | OLS |
| Burret and Feld (2018b) | municipal level | dummy, index | | | |
| Burret and Feld (2018b) | local revenues (p.c.) | Any dummy, index | 0 | 1980-2011 (25) | OLS |
| Burret and Feld (2018b) | cantonal level | dummy, index | | | |
| Burret and Feld (2018b) | local revenues (p.c.) | Any dummy, index | 0 | 1982-2007 (26) | OLS |
| Burret and Feld (2018b) | municipal level | dummy, index | | | |
| Burret and Feld (2018b) | local debt (p.c.) | Any dummy, index | 0 | 1980-2011 (25) | OLS |
| Burret and Feld (2018b) | cantonal level | dummy, index | | | |
| Burret and Feld (2018b) | local debt (p.c.) | Any dummy, index | 0 | 1982-2007 (26) | OLS |
| Burret and Feld (2018b) | municipal level | dummy, index | | | |
| Burret and Feld (2018b) | local deficit (p.c.) | Any dummy, index | 0 | 1980-2011 (25) | OLS |
| Burret and Feld (2018b) | cantonal level | dummy, index | | | |
| Burret and Feld (2018b) | local deficit (p.c.) | Any dummy, index | 0 | 1982-2007 (26) | OLS |
| Burret and Feld (2018b) | municipal level | dummy, index | | | |
| Feld et al. (2017) | yield spread cantonal bonds against federal bonds (10 years) | Any | - | 1981-2007 (18) | OLS/GMM/IV |
| Chatagny (2015) | Tax revenue projection error | Any | 0/+ mitigating the finance minister's ideology | 1980-2007 (26) | OLS/GMM |
| Lüchinger and Schaltegger (2013) | Deficit forecasted (dummy variable) | Any | - | 1984-2005 (26) | OLS/Logit |
| Lüchinger and Schaltegger (2013) | Deficit realized (dummy variable) | Any | - | 1984-2005 (26) | OLS/Logit |
| Lüchinger and Schaltegger (2013) | Deficit forecast correct (dummy variable) | Any | + | 1984-2005 (26) | OLS |
| Krogstrup and Wälti (2008) | real budget balance per capita | Any | + | 1955-1999 (25) | OLS/VECD |
| Feld and Kirchgässner (2001) | expenditures (log) | Any | 0 | 1986-1997 (26) | OLS |
| Feld and Kirchgässner (2001) | revenues (log) | Any | 0 | 1986-1997 (26) | OLS |
| Feld and Kirchgässner (2001) | debt (log) | Any | - | 1986-1997 (26) | OLS |
| Feld and Kirchgässner (2001) | deficit (log) | Any | - | 1986-1997 (26) | OLS |
| Feld and Kirchgässner (2001) | local expenditures (p.c.) | Any | 0 | 1986-1997 (26) | OLS |
| Feld and Kirchgässner (2001) | local revenues (p.c.) | Any | 0 | 1986-1997 (26) | OLS |
| Feld and Kirchgässner (2001) | local debt (p.c.) | Any | 0 | 1986-1997 (26) | OLS |
| Feld and Kirchgässner (2001) | local deficit (p.c.) | Any | - | 1986-1997 (26) | OLS |
| Schaltegger (2002) | log expenditures | Any | - | 1980-1997 (26) | OLS/IV |
| Schaltegger (2002) | log revenues | Any | 0 | 1980-1997 (26) | OLS/IV |
| Schaltegger (2002) | log debt | Any | - | 1980-1997 (26) | OLS/IV |
| Schaltegger (2002) | log expenditures | Any | - | 1980-1997 (26) | OLS/IV |

Table 12 THE EFFECTS OF THE DSP – ITALIAN MUNICIPALITIES

| Study | Dependent variable | Reform year | Population threshold | Result |
|--------------------------------|---|--------------------|-----------------------------|----------------------------|
| Mancini and Tommasino (2023) | capital expenditure forecast errors | 2005 | 5,000 | - |
| Mancini and Tommasino (2023) | current expenditure forecast errors | 2005 | 5,000 | 0 |
| Mancini and Tommasino (2023) | capital revenue forecast errors | 2005 | 5,000 | - |
| Mancini and Tommasino (2023) | current revenue forecast errors | 2005 | 5,000 | - |
| Mancini and Tommasino (2023) | investment forecast errors | 2005 | 5,000 | - |
| Mancini and Tommasino (2023) | deficit (revenue-costs) investment forecasted | 2005 | 5,000 | - |
| Mancini and Tommasino (2023) | investment realized | 2005 | 5,000 | 0/- |
| Mancini and Tommasino (2023) | capital revenue forecasted | 2005 | 5,000 | 0/- |
| Mancini and Tommasino (2023) | capital revenue realized | 2005 | 5,000 | 0 |
| Pavese and Rubolino (2022) | current expenditure | 2005 | 5,000 | - |
| Pavese and Rubolino (2022) | capital expenditure | 2005 | 5,000 | - |
| Gamalerio and Trombetta (2022) | Mayors' higher education | 2001 | 5,000 | - |
| Gamalerio and Trombetta (2022) | Mayoral candidates' higher education procurement expenditure | 2001 | 5,000 | - |
| Coviello et al. (2022) | revenues from procurement | 2008 | 5,000 | - |
| Coviello et al. (2022) | firms' total annual wages | 2008 | 5,000 | 0 |
| Coviello et al. (2022) | firms' total annual physical assets | 2008 | 5,000 | - |
| Alpino et al. (2022) | income tax rate | 2013 | 1,000 | + (top incomes) |
| Alpino et al. (2022) | tax revenues (per taxpayer) | 2013 | 1,000 | + (top incomes) |
| Alpino et al. (2022) | total tax revenues | 2013 | 1,000 | + |
| Alpino et al. (2022) | Mayors' reelection probability | 2013 | 1,000 | 0 (+ if college degree) |
| Alpino et al. (2022) | Mayors' re-run probability | 2013 | 1,000 | 0 |
| Daniele and Giommoni (2021) | corruption (initiated procedures) | 2013 | 5,000 | - |
| Daniele and Giommoni (2021) | capital expenditure | 2013 | 5,000 | - |
| Daniele and Giommoni (2021) | corruption / capital expenditure | 2013 | 5,000 | - |
| Daniele and Giommoni (2021) | corruption / total expenditure | 2013 | 5,000 | - |
| Daniele and Giommoni (2021) | property tax rate | 2013 | 5,000 | -/0 |
| Daniele and Giommoni (2021) | procurement expenditure | 2013 | 5,000 | - |
| Daniele and Giommoni (2021) | GDP per capita | 2013 | 5,000 | 0 |
| Daniele and Giommoni (2021) | income inequality | 2013 | 5,000 | 0 |
| Daniele and Giommoni (2021) | school canteens | 2013 | 5,000 | 0 |
| Daniele and Giommoni (2021) | kindergartens | 2013 | 5,000 | 0 |
| Daniele and Giommoni (2021) | waste collection | 2013 | 5,000 | 0 |
| Daniele and Giommoni (2021) | street lighting | 2013 | 5,000 | 0 |

Table 13 THE EFFECTS OF THE DSP – ITALIAN MUNICIPALITIES CONTINUED

| Study | Dependent variable | Reform year | Population threshold | Result |
|------------------------------|--|-------------|----------------------|--------|
| Venturini (2020) | Total expenditure (accrual basis of accounting) | 2001 | 5,000 | 0 |
| Venturini (2020) | Total expenditure (cash basis of accounting) | 2001 | 5,000 | 0 |
| Venturini (2020) | investment expenditure (accrual basis of accounting) | 2001 | 5,000 | 0 |
| Venturini (2020) | investment expenditure (cash basis of accounting) | 2001 | 5,000 | 0/+ |
| Venturini (2020) | Total expenditure (accrual basis of accounting) | 2005 | 3,000 | 0 |
| Venturini (2020) | Total expenditure (cash basis of accounting) | 2005 | 3,000 | 0 |
| Venturini (2020) | investment expenditure (accrual basis of accounting) | 2005 | 3,000 | + |
| Venturini (2020) | investment expenditure (cash basis of accounting) | 2005 | 3,000 | 0/+ |
| Venturini (2020) | Total expenditure (accrual basis of accounting) | 2007 | 5,000 | - |
| Venturini (2020) | Total expenditure (cash basis of accounting) | 2007 | 5,000 | - |
| Venturini (2020) | investment expenditure (accrual basis of accounting) | 2007 | 5,000 | - |
| Venturini (2020) | investment expenditure (cash basis of accounting) | 2007 | 5,000 | - |
| Venturini (2020) | human capital (accrual basis of accounting) | 2007 | 5,000 | - |
| Venturini (2020) | infrastructure (accrual basis of accounting) | 2007 | 5,000 | - |
| Venturini (2020) | pure public goods (accrual basis of accounting) | 2007 | 5,000 | - |
| Picchio and Santolini (2020) | revenues (accrual basis of accounting) | 2001, 2002 | 5,000 | - |
| Picchio and Santolini (2020) | forecast errors | 2001, 2002 | 5,000 | - |
| Picchio and Santolini (2020) | Taxes | 2001, 2002 | 5,000 | - |
| Picchio and Santolini (2020) | forecast errors | 2001, 2002 | 5,000 | - |
| Picchio and Santolini (2020) | Fees and tariffs | 2001, 2002 | 5,000 | - |
| Picchio and Santolini (2020) | forecast errors | 2001, 2002 | 5,000 | 0 |
| Picchio and Santolini (2020) | Other revenues | 2001, 2002 | 5,000 | - |
| Picchio and Santolini (2020) | forecast errors | 2001, 2002 | 5,000 | - |
| Picchio and Santolini (2020) | expenditure | 2001, 2002 | 5,000 | 0 |
| Picchio and Santolini (2020) | forecast errors | 2001, 2002 | 5,000 | - |
| Picchio and Santolini (2020) | current outlays | 2001, 2002 | 5,000 | 0 |
| Picchio and Santolini (2020) | forecast errors | 2001, 2002 | 5,000 | - |
| Picchio and Santolini (2020) | capital outlays | 2001, 2002 | 5,000 | 0 |
| Picchio and Santolini (2020) | forecast errors | 2001, 2002 | 5,000 | - |
| Picchio and Santolini (2020) | other expenditures | 2001, 2002 | 5,000 | 0 |
| Picchio and Santolini (2020) | forecast errors | 2001, 2002 | 5,000 | - |

Table 14 THE EFFECTS OF THE DSP – ITALIAN MUNICIPALITIES FURTHER CONTINUED

| Study | Dependent variable | Reform year | Population threshold | Result |
|---------------------|---------------------------|--------------------|-----------------------------|---------------|
| Grembi et al.(2016) | deficit | 2001 | 5,000 | - |
| Grembi et al.(2016) | fiscal gap | 2001 | 5,000 | - |
| Grembi et al.(2016) | current outlays | 2001 | 5,000 | 0 |
| Grembi et al.(2016) | capital outlays | 2001 | 5,000 | 0 |
| Grembi et al.(2016) | debt service | 2001 | 5,000 | 0 |
| Grembi et al.(2016) | Taxes | 2001 | 5,000 | + |
| Grembi et al.(2016) | Fees and tariffs | 2001 | 5,000 | 0 |
| Grembi et al.(2016) | Central transfers | 2001 | 5,000 | 0 |
| Grembi et al.(2016) | other revenues | 2001 | 5,000 | 0 |
| Grembi et al.(2016) | real estate tax rate | 2001 | 5,000 | + |
| Grembi et al.(2016) | Income surcharge | 2001 | 5,000 | + |