Do Fiscal Rules Reduce Public Investment? Evidence for European Regions

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Abstract

This paper analyses the impact of fiscal rules on different public spending categories, namely public expenditure and investment, at the subnational level in Europe. Building on the notion of the deficit bias, we suspect that in the presence of fiscal rules, politicians have an incentive to reduce public spending through disproportionate cuts in investments. To empirically test this hypothesis, we focus on subnational administrative levels since budget reallocations can be expected to be pronounced at these levels and because the empirical evidence here is scarce. We introduce a new index based on partially ordered set theory (POSET), using the EC's fiscal rules dataset, which allows us to analyze the stringency of fiscal rules for different levels of government. Our balanced dataset covers 179 NUTS2 regions in 14 EU member states from 1995 to 2018. The empirical analysis is based on Within, GMM, and instrumental variable estimators. Our empirical findings are highly robust. In our baseline model, a one standard-deviation increase in our fiscal rules stringency index reduces overall public expenditure by up to 1.28 percent, while investment declines by more than 4 percent. The results imply that more stringent fiscal rules lead to a disproportionate reduction in public investment as compared to overall expenditure.

JEL classification: E02, E62, H54, H60, H74,

Keywords: European regions, local government, fiscal rules, fiscal policy, expenditure, investment, deficit bias, POSET

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1 Introduction

Many countries as well as supranational organizations like the European Union (EU) have adopted numerical fiscal rules, aimed at limiting the fiscal space of policymakers, arguing that they would reduce deficits and high debt levels. One of the most dominant narratives behind this is the deficit bias whose core idea is that politicians are subject to adverse incentives, which lead to persistent overspending (Alesina and Tabellini, 1990; Persson and Tabellini, 2000). The implicit goal of fiscal rules is therefore to mitigate these adverse incentives, ultimately leading to a more efficient allocation of public resources. However, the existing literature on the effectiveness of fiscal rules is still inconclusive. While empirical studies largely suggest that fiscal rules can constrain fiscal policies (Heinemann et al., 2018), it is not well understood how this budgetary consolidation is achieved and in particular, what types of spending and revenue categories are affected most by fiscal rules. Answering these questions bears important implications for the assessment of the long-term costs and benefits of fiscal rules.

In this paper, we study the impact of fiscal rules on different spending categories, namely overall public expenditure and investment, at the subnational level in Europe. While a few recent studies link fiscal consolidation to declining investment-to-consumption ratios at the national level (Bamba et al., 2020; Ardanaz et al., 2021), little empirical evidence exists on the effects of fiscal rules on public investment. This is particularly the case for the subnational level. Yet, given the central role of regional and local entities in providing important public goods like (renewable) energy or digital infrastructures, it is important to understand how fiscal rules affect public expenditure and investment at this level. Building on the assumptions of the deficit bias, we suspect that in the context of fiscal rules, politicians have incentives to reduce public expenditure through cuts in public investment. Such incentives might be particularly pronounced at the subnational level due to common pool problems, the vertical separation of spending and taxing competencies and the associated bailout expectations of subnational sectors vis-à-vis higher levels of governments (von Hagen and Eichengreen, 1996; Alesina et al., 2015).

This paper contributes to the existing literature on the effects of fiscal rules in three main ways: First, we introduce a novel index of the stringency of fiscal rules for different levels of government, applying partially ordered set theory (POSET) to the European Commission's fiscal rules dataset (European Commission, 2022c). The created data allows for studying the effect of fiscal rules at different government levels. We thus significantly improve the quality of data on fiscal rules and contribute to an improved understanding of them. Second, to the best of our knowledge, we are the first to study the effects of fiscal rules on public investment at the subnational level in EU member states. Our analysis also emphasizes the role of different types of fiscal rules for subnational spending decisions. Third, our empirical analysis is inspired by the well-established literature on the economic effects of institutions (Acemoglu et al., 2019) and is based on dynamic Within, GMM and instrumental variable estimators. With this empirical framework, we aim to advance the identification of causal effects of fiscal rules.

Our empirical analysis is based on a balanced panel of 179 NUTS2 regions in 14 EU member states for the period 1995 to 2018[1]. We exploit data from the Annual Regional Database of the EC's Directorate for Regional and Urban Policy (ARDECO) as measures of public expenditure and investment. Specifically, we use data on gross value added (GVA) and gross factor capital formation (GFCF) of the non-market sectors. We demonstrate that these data series are suitable proxies for government expenditure and investment at the subnational level. To measure the stringency of fiscal rules, we introduce a novel index based on the EC's fiscal rules dataset. We characterize fiscal rules along four institutional properties, namely (i) the legal basis, (ii) room for setting or revising objectives, (iii) the nature of the body in charge of monitoring and enforcement, and (iv) enforcement mechanisms. Since the properties of fiscal rules are ordinally scaled and not interchangeable with each other, we apply POSET to rank the stringency of different fiscal rules in a consistent and comprehensive way. In a first step, we construct separate indices for the stringency of four types of fiscal rules: budget balance rules, debt rules, expenditure rules and revenue rules. In a second step, we combine the individual indices by the means of POSET into an aggregate index that measures the overall stringency of fiscal rules for a given level of government.

Since fiscal rules might not be distributed randomly with respect to fiscal outcomes, identfying a causal effect comes with several empirical challenges (J. M. Poterba, 1994; Asatryan et al., 2018). First, there might be a selection bias such that governments differ systematically in their likelihood to implement a fiscal rule based on past fiscal outcomes. Second, unobserved factors, such as fiscal preferences, might affect both the implementation of fiscal rules and fiscal outcomes. To account for these concerns, we employ a dynamic panel data model with region and time fixed effects. With this approach, we closely follow the literature on the economic effects of institutions (Acemoglu et al., 2019), accounting for the endogenous dynamics of fiscal rules and fiscal outcomes. We show res-

[1] We also estimate a model using unbalanced panel data of 27 EU member states with similar results.

ults based on classic Within and GMM estimators. In addition, we also employ an instrumental variable approach, instrumenting the stringency of fiscal rules with government fragmentation at the national level. The rationale is that more fragmented governments lead to higher public spending pressure at the national level (but not at subnational levels), which in turn encourages the introduction of more stringent fiscal rules for *all* administrative levels (Roubini and Sachs, 1989, Volkerink and de Haan, 2001, Perotti and Kontopoulos, 2002).

Our empirical results indicate that an increase in the stringency of fiscal rules leads to a more pronounced reduction in public investment as compared to overall expenditure cuts. In our baseline model, a one standard-deviation increase in our fiscal-rules stringency index reduces overall public expenditure by up to 1.28 percent, while investment decreases by up to 4.41 percent. After 10 years, we find on average a lasting reduction in the investment-to-expenditure ratio of over 1 percent. Our results are robust to the use of alternative estimation methods, dependent variables, country samples and government levels of the fiscal rule stringency index. When analysing the different types of fiscal rules separately, we find that our baseline result of a disproportionate reduction in public investment is mostly driven by budget balance and expenditure rules, while the empirical results for debt rules are less robust.

Overall, our results suggest that more stringent fiscal rules constrain fiscal policies by reducing public spending and that these reductions are disproportionately achieved through cuts in public investments. In this sense, our results provide tentative evidence for the notion that politicians reallocate public resources away from investments when faced with binding budget constraints. From a policy perspective, the findings imply that the currently implemented fiscal rules may not necessarily lead to a more efficient allocation of public resources, but might introduce a disinvestment bias.

The remainder of the paper is structured as follows. After placing our paper into the existing literature in Section 2, we describe the data and, in particular, the creation of our index in Section 3. Section 4 introduces the empirical framework, while section 5 presents the main results for the effects of fiscal rules on public expenditure and investment, followed by several robustness checks. Section 6 concludes.

2 Related Literature

One of the main arguments for introducing fiscal rules is the concept of the deficit bias, a tendency of governments towards deficits. Although there is no complete agreement on this tendency, the so-called deficit bias is based on a broad political economy literature that studies a set of possible incentives shaping policymakers' inclinations to persistently spend and borrow at levels that deviate from optimal fiscal policies (Alesina et al., 2015)[2]. Such distortionary spending incentives have been linked to arguments of fiscal illusion (Buchanan and Wagner, 1977), political budget cycles (Rogoff, 1990; Brender and Drazen, 2005; Shi and Svensson, 2006) and short-run political time horizons (Persson and Svensson, 1989; Lizzeri, 1999; Woo, 2005), as well as common pool problems (Ostrom, 1990;Weingast et al., 1981; Baron and Ferejohn, 1989). Common to all these arguments is the notion that the political process creates incentives for short-term expansionary fiscal policies at the expense of macroeconomic stability in the long term (Alesina and Tabellini, 1990; Alesina et al., 2015).

While in principle applying to any level of government, the aforementioned mechanisms can be expected to be particularly pronounced at subnational levels of government for two reasons. First, common pool problems are most pronounced at these levels of government (Ostrom, 1990). This is due to the separation of spending and taxing competencies between different levels of government. Substantial expenditure competencies lie at the subnational level, while most competencies for revenues lie at the national level. Thus, policymakers at subnational levels have an incentive to tie more money to one's geographical region than would be optimal for society as a whole since the costs are not fully internalised (Alesina et al., 2015). Second, it can be assumed that the subnational level could count on the national level to intervene in a supportive manner in the case of an imbalance in the region's finances or the risk of default. This creates a type of soft budget constraint (Kornai et al., 2003). This aspect is strongly related to the general literature on fiscal federalism (Oates, 2011).

One of the main policy actions aimed at counteracting the deficit bias and ensuring fiscal sustainability has been the use of fiscal rules. The most frequent definition of fiscal rules comes from Kopits and Symansky (1998) who define fiscal rules 'as a permanent constraint on fiscal policy, typically defined in terms of an indicator of overall fiscal performance' (Kopits and Symansky, 1998, p. 2). While fiscal rules might prevent the implementation of optimal fiscal policies, several theoretical papers argue that they can increase social welfare as a second-best

[2]This paper does not aim to (dis-)validate the existence of a deficit bias. Instead, it investigates one of the potentially unintended side effects of fiscal rules aimed at alleviating the alledged problem, namely reductions in public investments.

institutional check if excessive deficits arise from distortionary spending incentives (Besley and Smart, 2007; Battaglini and Coate, 2008). Yet, some models also point to the potential costs of fiscal rules, taking into account political economy considerations. In particular, fiscal rules might affect the allocation of public resources across different expenditure categories (Azzimonti et al., 2016). The general idea is that different spending components become visible at different points in time (Rogoff, 1990). As a result, the effects of cuts in public investments are often perceived much later than, for example, pension cuts. In the literature on political budget cycles, it is argued that politicians (especially in election years) prefer expenditures that directly affect their popularity with the electorate (Drazen, 2000; Woo, 2005). Furthermore, the argument that politicians discount future payoffs combined with the assumption 'that current spending provides immediate political benefits and that payoffs to public investment only materialise with a lag' (Ardanaz and Izquierdo, 2017, p. 7) leads to an incentive to disproportionately reduce public investment in relation to consumption in periods of fiscal consolidation. Assuming that fiscal rules create pressure to consolidate, it is easier for politicians to cut investment rather than consumption spending.

Whether or not fiscal rules actually constrain fiscal policy in a welfare-improving way, both at national and subnational government levels, is essentially an empirical question (J. M. Poterba, 1994; J. Poterba, 1996; Feld and Kirchgässner, 2008; Hallerberg et al., 2009). Existing studies have focused on fiscal outcomes such as fiscal balances or deficits (Hallerberg et al., 2009; Debrun et al., 2008; Dahan and Strawczynski, 2013), output volatility (Iara and Wolff, 2014) and interest rates (Fatás and Mihov, 2006). Generally, the empirical evidence points to 'a constraining and statistically significant impact of fiscal rules on fiscal aggregates on the national level' (Heinemann et al., 2018, p. 2) and an even higher level of efficacy at the municipal level (Heinemann et al., 2018). However, the empirical results are not unambiguous and often suffer from endogeneity concerns (J. M. Poterba, 1994). This is related to the observation that countries with a general tendency towards fiscal discipline are more likely to enact stricter fiscal rules. Whether a low debt ratio is due to the preference for fiscal discipline or the stringency of the fiscal rules is challenging to separate (Debrun et al., 2008). Recently, however, more sophisticated empirical strategies have been employed that address these concerns, e.g. by applying instrumental variables or difference-in-difference designs (Asatryan et al., 2018 Debrun et al., 2008; Badinger and Reuter, 2017a; Caselli et al., 2019).

Yet, even with these methodological advances, a major gap in the previous literature relates to the effects of fiscal rules on different expenditure categories. Existing studies regarding the impact of fiscal rules on the fiscal balance almost exclusively focus on the effect on public expenditure (and revenues) as a whole. Studies examining the effect on specific expenditure categories, such as investment, are much rarer. This is mainly due to suboptimal data availability. However, a few recent studies examine the link between fiscal consolidation and public investment (Bamba et al., 2020; Ardanaz et al., 2020). Bamba et al. (2020) find a negative effect of fiscal consolidation on the ratio of public investment to public consumption. Moreover, they argue that fiscal consolidation aimed at short-term stabilisation could harm the economy's potential in the long run, as it leads to a disproportionate reduction in public investment relative to public consumption. The study by Ardanaz et al. (2020) shows that fiscal rules with flexible design, for example, by treating public investment differently, lead to a smaller reduction in public investment in times of fiscal consolidation. A more recent working paper by Jürgens et al. (2022) explores the effect of fiscal rules on public investment, with a particular focus on their impact over various stages of the business cycle. She finds that unflexible fiscal rules restrain government investment in times of economic downturn.

While these studies focus on the national level, no multi-country empirical evidence exists for subnational administrative levels, where, for the aforementioned reasons, the effects of fiscal rules on public investment may also be pronounced. Also, there is no systematic evidence on how different types of fiscal rules (budget balance rules, debt rules etc.), their stringency and potential interdependencies influence public expenditure and public investment at that level. In this paper, we aim to close these gaps in the literature by empirically analyzing how fiscal rules affect public expenditure and investment at the subnational level.

3 Data

Our database covers a balanced panel of 179 NUTS2 regions in 14 member states of the European Union over a period from 1995 to 2018, resulting in a total of 4,296 region-year observations.

3.1 Dependent variables

As data basis for the dependent variables, we use the ARDECO database (European Commission, 2022b), which provides highly disaggregated data at different NUTS levels. The uniform classification into NUTS regions makes it easier to compare the regions of the member states. Another advantage of the ARDECO database is that the data is consistent with the national data of the AMECO database. The data at the NUTS2 level is of particular interest to us, as most subnational policy decisions are made at this level. The database offers a wide range of variables, including data on gross value added (GVA) and gross factor capital formation (GFCF) by sector. Unfortunately, there is no official data on public expenditure and investment at the subnational level. For this reason, we follow the approach of Brueckner et al. (2019) and Gabriel et al. (2020), who show that GVA of the non-market sector is a valid proxy for subnational government expenditure. They find that 'almost the entire variation in GVA of the non-market sector refers to activities by the general government' (Gabriel et al., 2020, p. 6). In the following, we will confirm the validity of this approach and, in addition, show that GFCF of the non-market sector is a good proxy for public investment. For this purpose, we aggregate the subnational values of GVA and GFCF within a country and compare them with the respective national time series. The comparison at hand is with the corresponding national accounts data from the AMECO database (European Commission, 2022a). Consequently, we compare the aggregated values with the GFCF and total expenditure of the general government. We also use the OECD's REGOFI Database (OECD, 2022). Unfortunately, this only covers a relatively short observation period from 2010 to 2016. However, in contrast to the AMECO database, it offers the possibility to distinguish between different levels of government. The REGOFI database differentiates between general, subnational and regional government. Combined with the data of the AMECO database, they give us a good intuition as to whether GVA and GFCF of the non-market sector are indeed good proxies for subnational government expenditure and investment. As can be seen in table 1, there is a high correlation between

		ARDECO	
		GVA non-market sector	GFCF non-market-sector
AMECO	general gov.	0.9896	0.9445
REGOFI	subnational gov.	0.89	0.9734
	regional gov.	0.71	0.9051
	general gov.	0.9944	0.9378

Table 1: Correlation between ARDECO, AMECO and REGOFI

the aggregated subnational non-market GVA and GFCF and the corresponding national statistics for public expenditure and investment. The correlation is very strong, especially for the AMECO database with values well above 0.9. The correlation is also high for the data from the REGOFI database. Since our explanatory variable within the model will be the stringency of fiscal rules affecting subnational governments, the correlation with subnational aggregates is particularly interesting. The strong correlation with these, with a correlation coefficient of 0.89 for GVA and 0.97 for GFCF, encourages us to use them as proxies for public expenditure and public investment for the rest of the paper. From now on, we will use the terms public expenditure and public investment for gross value added and gross factor capital formation of the non-market sector.

3.2 Fiscal rules stringency index

One of the main problems when studying the effects of fiscal rules is how to measure their stringency. This paper builds on the approach of Badinger and Reuter (2015), who were the first to create an index for fiscal rules using partially ordered set theory (POSET). Unlike Badinger and Reuter (2015), our index in this paper is based on the dataset of the European Commission (2022c) and not on that of the IMF (Schaechter et al., 2012)[3]. However, the advantages of using POSET also apply to the dataset of the European Commission, as their coding of fiscal rules contains several properties that are ordinally scaled and not interchangeable with each other. In contrast to a composite index, the POSET approach respects the ordinal structure and the non-interchangeability of the variables. With composed indices, it is necessary to assign numerical values to the different observations of one property in order to subsequently aggregate the different properties. By doing so, one must necessarily make subjective assumptions regarding the relationship of different observations within one property and between the properties.
To understand the logic of the index, it is helpful to first use a simplified example. For this purpose, imagine that we have two properties defining the stringency of fiscal rules – legal basis (lb) and monitoring (m) – with the following structure:

$$lb : \text{constitutional law} > \text{ordinary law} > \text{coalition agreement}$$
$$m : \text{monitoring} > \text{no monitoring}$$

Consider the following rules:

$$fr_1 = (\text{constitutional law}, \text{monitoring})$$
$$fr_2 = (\text{ordinary law}, \text{monitoring})$$
$$fr_3 = (\text{constitutional law}, \text{no monitoring})$$

A fiscal rule with constitutional status is stronger than the same rule based on ordinary law ($fr_1 > fr_2$). A rule with monitoring is - all other things being equal - more stringent than a rule without ($fr_1 > fr_3$). It becomes more problematic when we want to compare fr_2 and fr_3. The profile fr_3 has a stronger legal basis, whereas fr_2 is more stringent in the monitoring dimension. Which of these two

[3] New version: Medas et al., 2022

rules is stronger? To answer this question, we could assign numerical values. This is the procedure for composite indices. Intuitive values would be the following:

$$lb = \begin{cases} 1 & \text{coal. agr.} \\ 2 & \text{ord. law} \\ 3 & \text{const. law} \end{cases} \qquad m = \begin{cases} 1 & \text{no mon.} \\ 2 & \text{mon.} \end{cases}$$

This is only one exemplary combination among many others and, like all other possibilities, a subjective assignment. By setting the values, we make some questionable assumptions. For example, we stipulate that constitutional law is three times stronger than a coalition agreement. We also assume that *one unit* of legal basis is worth as much as *one unit* of monitoring. fr_2 and fr_3 would consequently be equally stringent if the individual dimensions were united with an arithmetic mean. Of course, one could use a weighted mean. Unfortunately, we would have the same problem that the weights must be determined subjectively. To avoid making these subjective assumptions while still getting an overall index for the stringency of the fiscal rules, one can use partially ordered set theory. In the following, we will explain the basic idea of this method, using the same example.

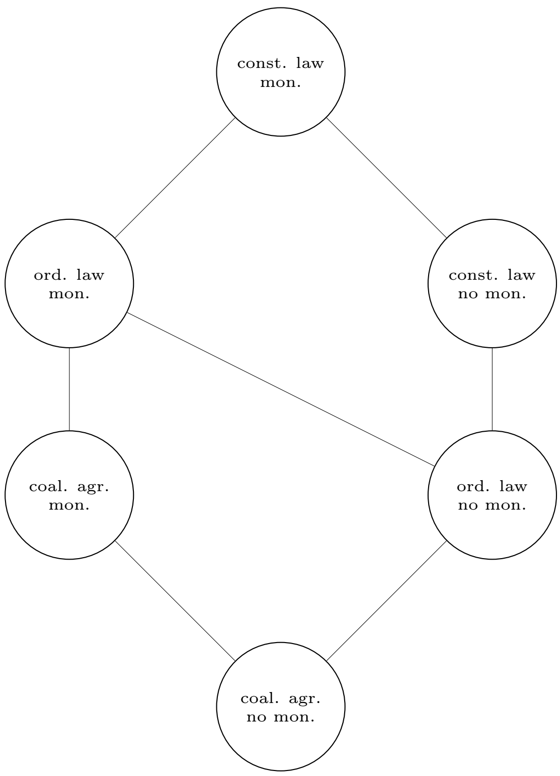


Figure 1: Hasse diagram

Formally speaking, we have a set O of N fiscal rules fr_i with $i = 1, \ldots, N$, and K properties $(v_1(fr_i), \ldots, v_k(fr_i))$ which are each ordinally scaled with $m_1, \ldots, m_j$ degrees. In our example, we have two properties with three and two degrees, respectively. A fiscal rule is equally or more stringent than another if it is equally or more stringent for all properties. Formally speaking:

$$fr_i \trianglerighteq fr_{i'} \iff v_j(fr_i) \geq v_j(fr_{i'}) \qquad \forall j = 1, \ldots, k$$

In such a case, fr_i and $fr_{i'}$ are comparable. Otherwise they are incomparable $(fr_i || fr_{i'})$. If $fr_i \trianglerighteq fr_{i'}$ and $v_j(fr_i) > v_j(fr_{i'})$ for at least one j, fr_i is strictly more stringent than $fr_{i'}$ $(fr_i \triangleright fr_{i'})$. Now, we can express our earlier observations formally. We know that: $fr_1 \triangleright fr_2$, $fr_1 \triangleright fr_3$, and $fr_2 || fr_3$.
The partial order of all possible property combinations can be visualised in a Hasse diagram (see figure 1). If $fr_i \triangleright fr_{i'}$, there is a sequence of downwards connected objects from fr_i to $fr_{i'}$ in the diagram. If two objects are not connected in that way, they are incomparable. The next step is to ask how to turn the incomparable objects into comparable ones. This is where the so-called linear extensions come into play. One can form a linear extension of a POSET by converting all incomparabilities into comparabilities. If we consider only one single

Figure 2: Linear Extensions

linear extension, we ultimately assume one possible ranked order of all possible achievement profiles under the restriction that if $fr_i \rhd fr_{i'}$ in the poset, it has to be $fr_i \rhd fr_{i'}$ in the linear extension. To create our index, intuitively speaking, we form all possible ranked orders that respect the partial order of the partially ordered set. Using the linear extensions, shown in figure 2, we can calculate the average ranks of the individual achievement profiles. Considering the examples fr_1, fr_2, and fr_3, the average rank of fr_1 is 1 since it is always at the top. The average rank of fr_2 is $\frac{3*2+2*3}{5} = 2.4$ and the average rank of fr_3 is $\frac{2*2+2*3+1*4}{5} = 2.8$. Based on this, we conclude that fr_2 is more stringent than fr_3. Obviously, this is a very simple example with only a few possible achievement profiles, but the general logic holds for more complex versions like our actual fiscal rules stringency index.

Property	**Interpretation values**
Legal basis	4 = constitutional level 3 = legal act 2 = coalition agreement 1 = political commitment
Room for setting/revising objectives	3 = no margin for adjusting objectives 2 = some but constrained margin 1 = no restriction in setting objectives
Nature of the body in charge of monitoring respect and enforcement of the rule	3 = monitoring and enforce (at least one independent) 2 = monitoring and/or enforcement (not independent) 1 = no monitoring, no enforcement
Enforcement mechanisms of the rule	3 = triggered automatically and/or scope is predefined 2 = actions must be taken or presented to parliament 1 = no ex-ante defined actions in case of non-compliance

Table 2: Coding of the index components

In the following, we present our fiscal rules stringency index based on POSET, using the European Commission's fiscal rules database. The index components are the *legal basis, the possibility of setting/revising objectives, the nature of the body in charge of monitoring respect and the enforcement, and the nature of the enforcement mechanisms* of the rule. The coding of the variable can be found in table 2. It is important to note that the variables, although represented by numerical values, are ordinally scaled. One of the properties obtains four attributes, while the three others have three. Consequently, we have a POSET with 108 possible property combinations (achievement profiles). One constraint when working with POSETs is that the number of linear extensions increases quadratically. This leads to the fact that with larger POSETs, we quickly reach the limits of what is computationally feasible. This is also the main reason why we have combined the properties *nature of the body in charge of monitoring respect of the rule* and *nature of the body in charge of enforcement of the rule.* Besides the computational limitations, these variables are also very closely related and are subcategories of a common category in the European Commission's fiscal rules database. For this reason, we consider merging the two subcategories to be fully justified. Criteria 1 and 2 are exactly congruent in definition to the European Commission's fiscal rules database. For the fourth property, it was necessary to combine two attributes. This is due to a change in the definitions of the database between the new and old methodology of the Commission. Here we face a trade-off between preferring a higher level of detail concerning this property or a larger time span. We opted for the latter, especially since the characteristics of the variable are still more detailed than in the IMF database (Medas et al., 2022). Having established the properties defining fiscal rules, we continue with the actual estimation of the fiscal rules stringency index.

Separate indices are formed for different levels of government, namely local government, regional government, central government, general government and social security. In addition, we construct separate indices for different types of fiscal rules, namely budget balance rules (BBR), deficit rules (DR), expenditure rules (ER), and revenue rules (RR). The same achievement profile will have the same value regardless of the level. The index is calculated over the full POSET. Consequently, achievement profiles that do not appear in the data are also taken into account. For the actual calculation of the index, we use the R package PARSEC (Fattore and Arcagni, 2014), which uses the algorithm of Bubley and Dyer (1999) to sample the linear extensions. The average ranks are normalised so that the index takes on a value of 0 if there is no fiscal rule and a value of 10 in the case of the most stringent fiscal rule possible. In the rare case in which one level of government is affected by several national fiscal rules simultaneously, we choose the maximum value. By doing so, we assume that if there are several rules, the most stringent rule dominates the others. In other words, introducing a new rule weaker than the previous rule cannot lead to a reduction in the overall stringency. This decision is necessary to avoid counting individual regions more than once. The maximum value is the most convincing option and due to the very small number of cases, one can assume that it does not affect the overall results of the analysis.

As we are interested in the overall effect of fiscal rules, we aggregate the indices of the individual rule types for a given level of government into a joint index, which we label FR_{full}[4]. In many countries, different fiscal rule types are simultaneously in force at the same level. If we consider the types separately, we disregard that they might work together. For this reason, we create a composed index that includes all fiscal rule types. To do so, we also use POSET but instead

[4]In section 5.2.4, we also investigate the effects of different fiscal rule types separately.

of the different properties we use different fiscal rule types. The main difference is that the composed index is not computed on the basis of the full POSET but that of the realised achievement profiles. Using the full POSET is not feasible, as this would go beyond what is computationally possible. The POSET of the realised profiles is a subposet of the full poset. The average ranks based on the realised profiles are basically an estimate of the average ranks based on the full poset (Badinger and Reuter, 2015). In our main analysis, we focus on this full fiscal rules stringency index for subnational governments (frd_sng_full), which covers local and regional fiscal rules[5].

Before looking at the descriptive statistics of the index, one cautious note on what this index captures and, more importantly, what it does not. The index creates a very good picture of the institutional characteristics of fiscal rules. However, it is crucial to understand that the technical properties of fiscal rules, such as numerical thresholds, are not covered by the index. If the technical but not the institutional aspect of a rule were changed, this would not affect the index, although it would probably have an impact on the actual strictness of the fiscal rule. For example, if the limit on net borrowing in a budget balance rule were to be raised from 0.35 to 1 percent without changing anything else, this would be a pretty extraordinary change in the stringency of the rule. However, the current version of the index does not capture such technical changes and would not be affected by it. This is a major shortcoming throughout the entire literature. There is great potential for research on the classification of technical aspects of fiscal rules. This might not be an easy task and is definitely beyond the scope of this paper but should be addressed in future projects. Even if the aspect of technical properties of fiscal rules cannot be solved by this paper, it nevertheless contributes strongly to an improvement of the data availability regarding fiscal rules. In particular, the application of POSET to the European Commission's dataset for the first time and the resulting possibility to look at the effects of fiscal rules on different levels of government is an important contribution to the literature.

Figure 3 shows the density of the combined fiscal rules stringency index at the subnational level for the years 1995, 2009 and 2018. Interestingly, over time, more countries implemented fiscal rules at the subnational level. In 1995 and 2009, most countries had a fiscal rule for the subnational level with a fiscal rules stringency index of around 6. From 1995 to 2009, more countries that previously had no or a weak fiscal rule shifted into this area. Between 2009 and 2018, the density shifted to the right. Most likely affected by the global financial crisis and

[5]The underlying argument is that both local and regional fiscal rules affect spending and investment decisions at the NUTS2 level.

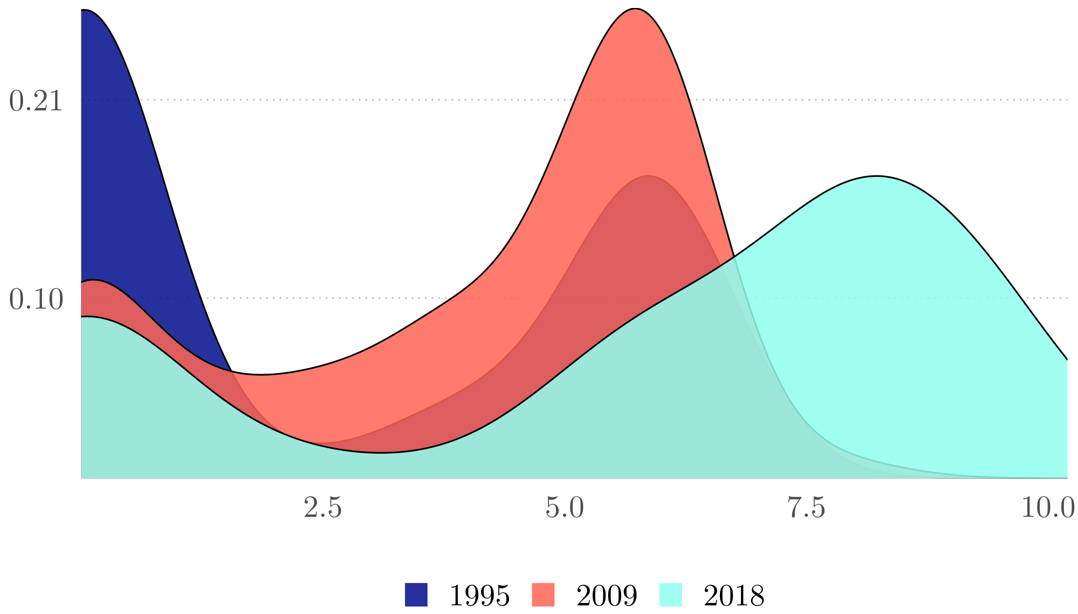


Figure 3: Density of fiscal rules stringency index (subnational government, full)

the subsequent euro and debt crisis, countries that already had a fiscal rule in place tightened their fiscal rules. A large number of countries now has fiscal rules with an index value of around 8.

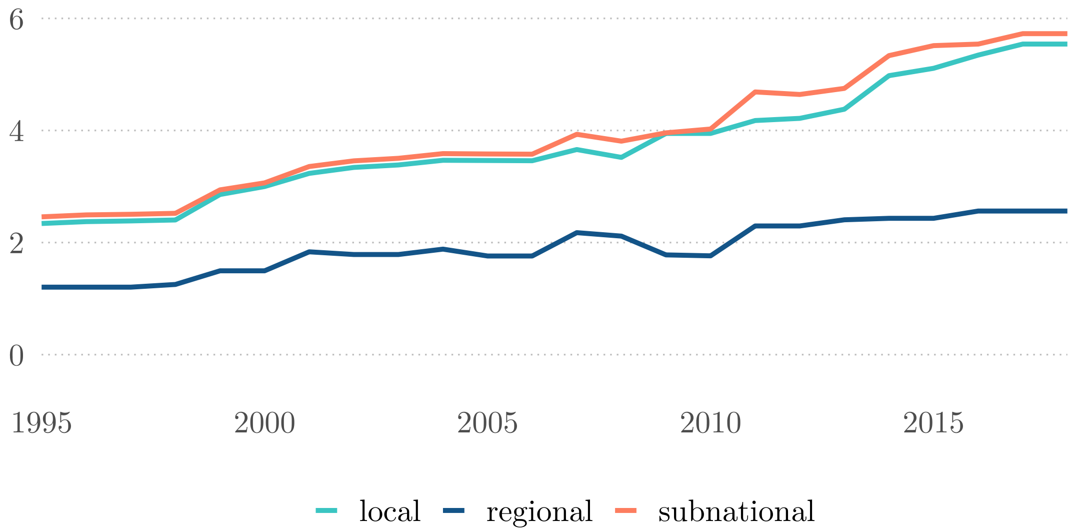


Figure 4: Trend of the fiscal rules stringency index (full, year average)

Figure 4 also confirms that the average strength of fiscal rules increases over time 4. For all three levels of government - local, regional and subnational - there is a clear upward trend.

3.3 Instruments and other variables

Our empirical analysis incorporates a bundle of control variables. These control variables can be divided into two blocks: institutional and macroeconomic control variables. The sources for the variables are the already known AMECO database (European Commission, 2022a) for the macroeconomic variables and the Database of Political Institutions by Cruz et al. (2021) for the institutional controls. The inclusion of the control variables aims to reduce the risk of a possible omitted variable bias due to time-varying variables whose effect varies differently for each region. The macroeconomic control variables are gross public debt of the general government (ameco_udgg), gross factor capital formation at the national level (ameco_uigg0), total current expenditure excluding interest of the general government (ameco_uucgi), national GDP (ameco_uvgd), and the output gap which is the gap between actual GDP and potential GDP (ameco_avgdgp). Furthermore, we control for the subnational share of general government revenue (eurostat_taxauton). The institutional control variables are: whether a legislative election (dpi_legelec) or presidential election (dpi_exelec) was held in a given year, whether the subnational governments are locally elected (dpi_state), and whether the regions of the respective country are autonomous (dpi_auton).

4 Empirical strategy

4.1 Baseline model

The baseline empirical approach is based on a full dynamic model of the following form:

$$y_{rct} = \beta FR_{ct-h} + \sum_{j=1}^{p+h} \gamma_j y_{rct-j} + X_{rct}\lambda + \alpha_r + \delta_t + \varepsilon_{rct}, \tag{1}$$

where r is the region index, c is the country index, and t is the time index. The dependent variable, y_{rct}, is the natural logarithm of either subnational public expenditure or public investment in million PPS (purchasing power standard) (section 3.1). FR_{ct-h} is the full fiscal rules stringency index combining all fiscal rule types for subnational government levels at time $t-h$ (section 3.2). X_{rct} is a vector of control variables at the national level, α_r and δ_t are region and time fixed effects, and ε_{rct} is the error term. Furthermore, h is the lag of the explanatory variables. The number of included lags of the dependent variable is given by $p+h$.

The core assumption of the Within estimator is sequential exogeneity:

$$E[\varepsilon_{rct}|y_{rct-1},\ldots,y_{rct_0},FR_{ct},\ldots,FR_{ct_0},X_{rct},\alpha_r,\delta_t]=0 \text{ for all} \quad (2)$$
$$y_{rct-1},\ldots,y_{rct_0},FR_{ct},\ldots,FR_{ct_0},X_{rct},\alpha_r, \text{ and } \delta_t \text{ and for all r, c, and } t\geq t_0$$

Accordingly, the fiscal rule stringency index, past levels of the dependent variable, and the control variables, including the fixed effects, must be orthogonal to the error term ε_{rct} which in turn must not be serially correlated. Furthermore, the model specification assumes a stationary process when controlling for region and time fixed effects[6]. Intuitively, sequential exogeneity implies that regions where the fiscal rule stringency changes are not on a different trend of the dependent variable relative to regions with a similar level of the dependent variable in past years and similar conditions in the long-term. The latter results from including region fixed effects, while we control for the dynamics of the dependent variable in recent years by including lags of the dependent variable.

We use a dynamic approach for two reasons. First, it eliminates a possible residual serial correlation of the error term. Secondly, it is plausible to assume that, for example, public expenditure is above average in the years before the introduction or strengthening of fiscal rules. In addition, including lagged values of the dependent variable controls for a variety of possibly omitted variables that develop only sluggishly. This should, for example, be the case for most institutional aspects.

Intuitively, there are two different channels through which we suspect an effect of the stringency of fiscal rules on public spending and public investment. The first channel is the direct effect of FR_{ct-h} on y_{rct}. This channel, for example, explains the entire effect of fiscal rules on the dependent variable in the case of $h=0$. For $h>0$, however, there is also an indirect channel via the lagged dependent variables y_{rct-1} to y_{rct-h}. Consider, for example, the reinforcement of a fiscal rule at time t-1 and its effect on public expenditure. A change of the stringency of the fiscal rule at time t-1 has a direct effect on y_{rct} but also an effect on public expenditure at time t-1, which in turn has an effect on public expenditure at time t. Within the model, however, this indirect effect is not attributed to the effect of FR_{ct-1}, as y_{rct-1} is directly included on the right-hand side. Consequently, it can be assumed that the effect of the stringency of fiscal rules on the depend-

[6]We test whether the process is stationary by using a test for the presence of a unit root proposed by Levin et al. (2002). For both public investment and public expenditure, we clearly reject the null hypothesis of a unit root at every common significance level. The test statistic z is equal to -13.76 for public investment and -84.32 for public expenditure, resulting in p-values<0.000.

ent variable is underestimated[7]. This generally applies to all lagged dependent variables between 1 and h. For this reason, we consider the specification given in equation 1 as the lower bound estimate for the effect of FR_{ct-h} on y_{rct}.

If we exclude the lagged dependent variables y_{rct-1} to y_{rct-h} from the model, the indirect effect is attributed to the change in FR_{ct-h}. Hence, the effect is no longer underestimated. However, this version of the empirical model is more susceptible to possible omitted variables, whose effect on y_{rct} is time delayed, and serial correlation. We, therefore, regard this specification as the upper bound and prefer the more conservative specification given in equation 1 which likely underestimates the effect. Nevertheless, taken together, the two specifications give a fairly good picture of the range in which the actual effect of fiscal rules on public expenditure and public investment should lie.

The results of the Within estimator have an asymptotic bias of the order $\frac{1}{T}$ due to a lack of strict exogeneity in the case of a dynamic panel model (Nickell, 1981, Alvarez and Arellano, 2003). The bias tends towards zero as the number of included time periods increases and should thus be small given our large temporal dimension of up to $T = 21$. Nevertheless, we present alternative estimation methods to test the robustness of our baseline model in the following subsection.

4.2 Arellano-Bond

First, we turn to a GMM estimator developed by Arellano and Bond (1991) which is based on the following moment conditions:

$$E[(y_{rcs}, FR_{cs-h+1})'\Delta\varepsilon_{rct}] = 0 \tag{3}$$
$$\text{for all } s \leq t-2,\ t \geq 2, \text{ and } h \geq 0$$

The basic idea of the Arellano-Bond estimator is to use lagged values of the dependent variable and other predetermined variables as instruments. Since it can be assumed that our explanatory variable is predetermined, we also use past values of FR_{ct-h} as instruments. The core assumption of our Arellano-Bond estimator, therefore, is that y_{rct-2}, FR_{ct-h-1}, and more distant lags are uncorrelated with the error term of the first-differenced model, $\varepsilon_{rct} - \varepsilon_{rct-1}$. The instruments are, per definition, relevant and the exclusion assumption follows directly from the sequential exogeneity assumption that past values are not correlated with future error terms.

One problem of the Arellano-Bond estimator is that the number of instruments increases quadratically with growing time dimension. This leads to a bias of $\frac{1}{N}$

[7] This applies under the assumption that the effect of the lagged dependent variables is positive in total, which we confirm in section 5.

for panels with large T. The bias is, however, smaller than the Nickel bias of the Within estimator for N>T (Alvarez and Arellano, 2003). This is the case for our application. To further reduce the problem of too many instruments, we limit the number of lags used as instruments to 5.
The biggest threat to the validity of the results of the Arellano-Bond estimator are omitted variables, that abruptly change over time, affecting regions in a heterogeneous way. Time-varying variables whose change is similar for all regions are captured by year fixed effects and time-invariant variables by using region fixed effects. Furthermore, variables, which only change sluggishly, are likely to be captured by the dynamic nature of our model. Nevertheless, a problem with the aforementioned possibly omitted variables cannot be completely ruled out. Another potential problem that could bias our estimates is reverse causality. Although we control for lagged values of the dependent variable, it cannot be ruled out entirely that y_{rct} might affect the stringency of the fiscal rules in the same year.

4.3 Instrumental variable

To test the robustness of our results regarding these problems, we turn to an instrumental variable strategy. The instrument selection builds on existing literature, identifying determinants of fiscal rules (Badinger and Reuter, 2017b). In a subsequent paper, Badinger and Reuter (2017a) use checks and balances, government fragmentation, and inflation targeting as instruments. Unlike their paper, we only consider EU member states, most of which are in a monetary union. Therefore, we cannot use inflation targeting since it has too little variation. The variable checks and balances is subject to similar problems. Therefore, we only use government fragmentation as an instrument[8]. The model, including the instrumental variable, is given by

$$y_{rct} = \beta FR_{ct-h} + \sum_{j=1}^{p+h} \gamma_j y_{rct-j} + X_{rct}\lambda + \alpha_r + \delta_t + \varepsilon_{rct}$$

$$FR_{ct-h} = \pi Z_{ct-h-2} + \sum_{j=1}^{p+h} \phi_j y_{rct-j} + \theta_r + \eta_t + \nu_{rct}$$

The model is identical to the baseline model, with the exception that we treat the stringency of fiscal rules as endogenous. To get consistent estimates, the instrument must be relevant and exogenous.
The relevance of governmental fragmentation as an instrument is based on a broad

[8] We use the second lag of government fragmentation as it has the greatest explanatory power.

theoretical basis, suggesting that higher political fragmentation on the national level leads to public spending pressure, which in turn encourages the introduction of more stringent fiscal rules for the national level and, potentially, for all other vertical government levels (Roubini and Sachs, 1989, Volkerink and de Haan, 2001, Perotti and Kontopoulos, 2002).
To be valid an instrument must be uncorrelated with the error term ε_{rct}. An important difference between our study and that of Badinger and Reuter (2017b) is that our dependent variable is at a different level of government than the instrument. This makes it easier to argue that the instrument is indeed valid. The logical reasoning is similar to that of Foremny (2014) who argues 'that the characteristics of central governments, which impose rules on the sub-national one, are unlikely to be correlated with their budgetary outcomes, but describe well the prevalence of rules.' (Foremny, 2014, p. 90). Furthermore, the two-way fixed effects approach, in addition to the dynamic nature of our model, helps building the argument that the instrument is valid. Even if there were initially back doors between the instrument and the dependent variable, they are most likely closed by the lagged dependent variable and the region and year fixed effects.

5 Results

5.1 Baseline Analysis

Table 3 reports the effects of FR_{ct-h} on public expenditure and public investment at the subnational level, using our baseline model (Section 4.1). Columns 1-4 show the impact of fiscal rules on public expenditure, while columns 5-8 report the effect on public investment. The individual columns of the two groups represent different lags of the explanatory variable, from a lag of 0 to 3. The specifications include all lagged values of the dependent variable up to three years before the observed change of FR_{ct-h}. Therefore, the results can be interpreted as the lower bound of the expected effect, as described in section 4.1. For comparison, Figure 5 shows the results of the lower and upper bound together, where the upper bound specification only includes the lags $t-h$ to $t-h-3$ of the dependent variable. All results include the full set of controls (Section 3.3) as well as regions and time-fixed effects. Standard errors are clustered at the Nuts2-level. Since we use a log-lin model, the magnitude of the effect is interpreted as $FR_{t-h} * 100\%$.

The effect of fiscal rules on overall public expenditure ranges from -0.18% (-0.52%)

	Expenditure				Investment			
	h=0	h=1	h=2	h=3	h=0	h=1	h=2	h=3
FR_{t-h}	$-.0044^{***}$	$-.0037^{***}$	$-.0031^{***}$	$-.0018^{***}$	$-.0095^{***}$	$-.0102^{***}$	$-.0147^{***}$	$-.0152^{***}$
	(.0005)	(.0005)	(.0005)	(.0005)	(.0025)	(.0025)	(.0028)	(.0025)
dep var$_{t-1}$	1.0396^{***}	1.0124^{***}	$.9886^{***}$	$.9295^{***}$	$.5493^{***}$	$.5316^{***}$	$.5125^{***}$	$.5088^{***}$
	(.0408)	(.0401)	(.0408)	(.0384)	(.0347)	(.0352)	(.0358)	(.0371)
dep var$_{t-2}$	$-.1232^{***}$	$-.1081^{***}$	$-.1075^{***}$	$-.0738^{**}$	$-.0044$	$-.0138$	$-.0233$	$-.0197$
	(.0316)	(.0314)	(.0349)	(.0328)	(.0588)	(.0555)	(.0544)	(.0561)
dep var$_{t-3}$	$-.0322$	$-.0345$	$-.0147$	$-.0080$	$.0951^{**}$	.0557	.0466	.0451
	(.0219)	(.0377)	(.0435)	(.0426)	(.0434)	(.0464)	(.0453)	(.0450)
dep var$_{t-4}$		.0127	$-.0492$	$-.0185$		.0571	.0655	.0619
		(.0234)	(.0440)	(.0429)		(.0346)	(.0419)	(.0421)
dep var$_{t-5}$			$.0655^{***}$	.0479			$-.0175$	$-.0240$
			(.0239)	(.0391)			(.0320)	(.0359)
dep var$_{t-6}$				.0211				.0217
				(.0222)				(.0221)
N	3759	3580	3401	3222	3759	3580	3401	3222
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓
Within adj. R^2	.9035	.8976	.8955	.9066	.6234	.6152	.6060	.6095
Effect after 10 years (t+10)	$-.0314^{***}$	$-.0255^{***}$	$-.0200^{***}$	$-.0114^{***}$	$-.0257^{***}$	$-.0268^{***}$	$-.0347^{***}$	$-.0365^{***}$
	(.0049)	(.0036)	(.0029)	(.0025)	(.0070)	(.0068)	(.0070)	(.0063)
P-value for H_0: $\lvert\beta_{exp}\rvert \geq \lvert\beta_{inv}\rvert$	–	–	–	–	.0234	.0050	.0000	.0000

Notes: Coefficient estimates for Fixed-Effects Least-Squares (FELS) estimations of equation (1). Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. Effect after 10 years and the corresponding standard error based on dynamic effects and deltamethod. The bottom row of the table reports p-values for a t-test with the null hypothesis that $|\beta_{exp}| \geq |\beta_{inv}|$ in the corresponding specification with lag size h. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 3: Baseline model (lower bound)

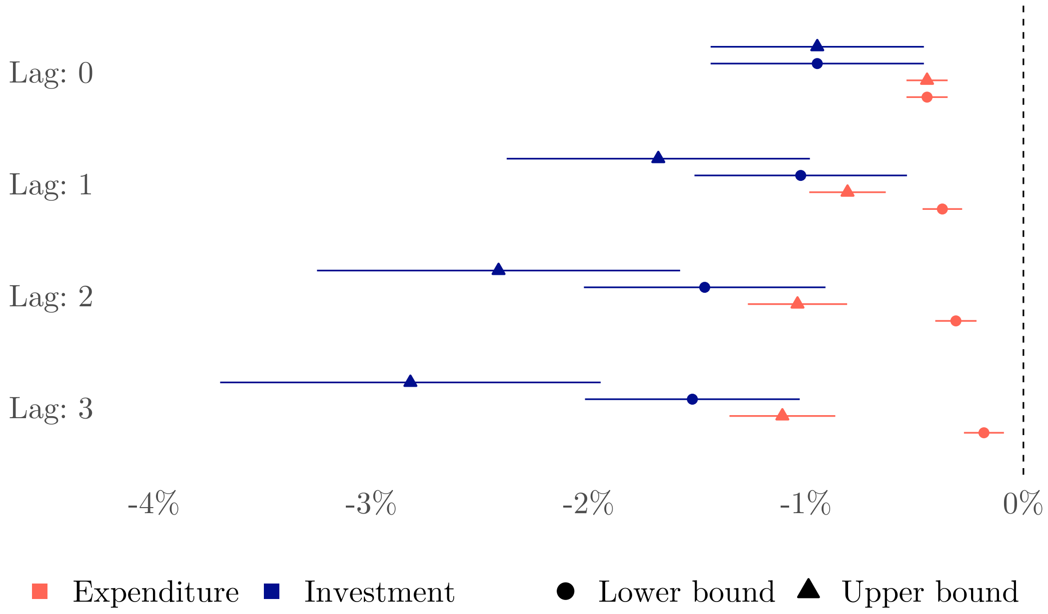
to -0.44% (-1.28%) for a one-unit (standard deviation) change[9] of FR_{ct-h} and is significant throughout all specifications. Regarding the effects of fiscal rules on public investment, the magnitude ranges from -0.95% to -1.52% for a one-unit increase in the fiscal rules stringency index or from -2.76% to -4.41% for a change by one standard deviation[10].

For each specification, the effect on public investment is highly significant and substantially larger than that on public expenditure. Unlike public expenditure, we find that the effects on public investment become stronger over time. A possible explanation relates to the idea that cuts in public investment are only possible with a certain time lag and are at least partly achieved by refraining from future investment projects, which reduces investment spending in subsequent years. In other words, cuts in public investment might be more difficult in the short term but become much easier over time. To test wether the effect of fiscal rules on public investment is indeed larger than the effect on overall public expenditure, we apply a t-test with the null hypothesis that the effect of FR_{ct-h} on expendit-

[9] FR_{ct-h} is normalised to the interval from 0 to 10 and has a standard deviation of 2.90.

[10] The effect for an average fiscal rule change (2.40) lies between 0.43% and 1.06% for public expenditure and 2.28% and 3.35% for public investment.

ure is larger (in absolute terms) than the effect on investment ($|\beta_{epx.}| \geq |\beta_{inv.}|$) in the respective specification. The last goodness of fit measure in table 3 contains the p-values of this test. The hypothesis is rejected for each specification at a significance level of 5%.



Note: Figure displays coefficient estimates and 95% confidence intervals for lower and upper bound estimations of the baseline model (within, two-way fixed effects), see equation (1). *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$; *upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at NUTS2 level. The coefficients are scaled by 100 to simplify the interpretation. Coefficient estimates presented in tables 3 and B.1.

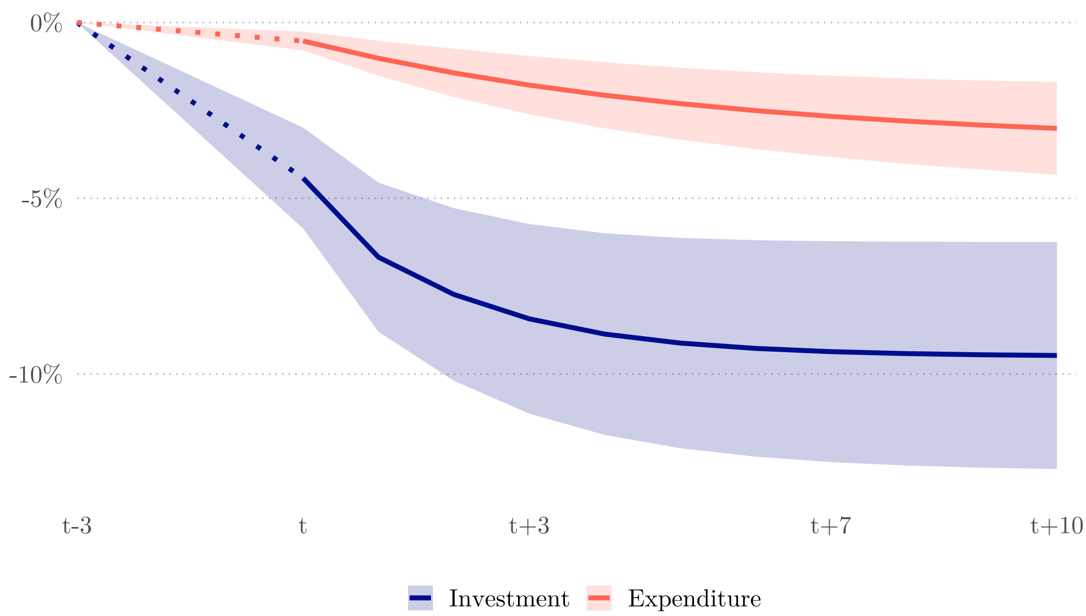
Figure 5: Effect plot - baseline model

In Figure 5, we compare the effect sizes of our baseline estimates for $h = 0$ to $h = 3$ from the lower and upper bound estimations. For all specifications, the effect on public investment is consistently stronger than the effect on overall expenditure. For the upper bound results, as with the lower bound, we clearly reject the hypothesis that the effect on expenditure is larger than the effect on investment[11].

To understand how the effects evolve over time, consider a fiscal reform that leads to an increase in the stringency of fiscal rules by one unit for a given region r in year t. Our results imply that such a reform leads to a reduction in the level of public expenditure by 0.44% in the same year (compared to the region's average expenditure) and to a reduction by 0.18% three years after the reform. For subsequent years (i.e. $h > 3$), we continue to find statistically significant values of a similar magnitude (see Appendix Tables C.4 and C.5).

Regarding the impact of the same fiscal reform on public investment, our results imply a reduction in investment levels by 0.95% in the same year and by 1.52%

[11]The p-values of the t-test with the null that $|\beta_{exp.}| \geq |\beta_{inv.}|$ are 0.0234, 0.0087, 0.0009, 0.0001.



Note: Figure displays the long-term effect of an increase in FR_{t-3} by one-standard deviation and its 95% confidence intervals for the lower bound estimation of the baseline model. Since we do not observe the excact development within the years between t-3 and t, this intervall is shown in dotted lines. Standard errors clustered at NUTS2 level. The coefficients are scaled by 100 to simplify the interpretation.

Figure 6: Long-run effects on expenditure and investment, baseline model

three years after the reform has been implemented. For subsequent years, we even find slightly stronger statistically significant effects but in general of a similar magnitude. In other words, an increase in our FR stringency index by one unit leads to persistent reductions in the levels of public expenditure and investment which are considerably stable after three years.

Based on our dynamic estimation specification we can also calculate the long-term effect of a change in our FR stringency index that unfolds over time[12]. After 10 years, the overall effects on public expenditure and investment are -1.04% (-3.01%) and -3.65% (-9.47%) for a one-unit (one-standard deviation) increase, respectively. Figure 6 presents the time path of the effects on expenditure and investment for a one-standard deviation increase in the FR stringency index at $h = 3$.

Overall, the results of the baseline model indicate that an increase in the stringency of fiscal rules leads to a persistent and pronounced reduction in public investments which is up to eight times larger than the effect on overall public

[12]The effect at time t+i, E_i, can be derived through the following recursive function:

$$E_i = \begin{cases} \beta & \text{if } i = 0 \\ \beta + \sum_{j=1}^{i} E_{j-1} \cdot \gamma_{1+i-j} & \text{if } 1 \leq i \leq ||\gamma||, \\ \beta + \sum_{j=1}^{||\gamma||} E_{i-j} \cdot \gamma_j & \text{if } i > ||\gamma|| \end{cases}$$

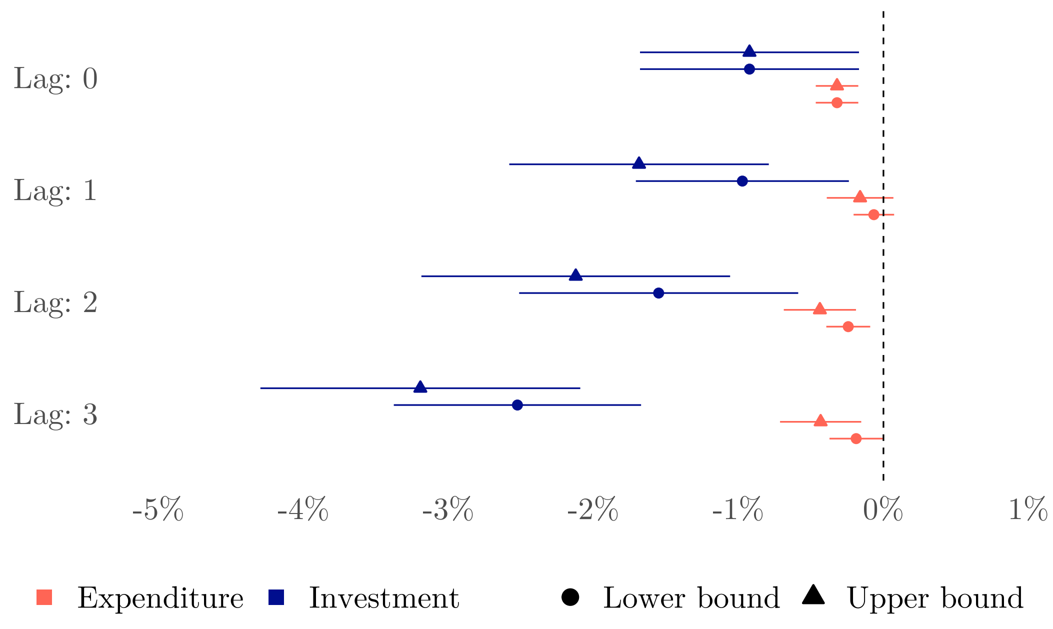
where β is the coefficient of FR_{t-h}, γ is a vector with the coefficients of the lagged dependent variables and $||\gamma||$ is the length of that vector. The process is identical to the method used in Acemoglu et al. (2019). The standard errors are computed through a similar iteration using the delta method.

expenditure (in percentage terms, for $h = 3$ in the lower bound specifications). These findings suggest that fiscal rules induce disproportionate cuts in public investments vis-à-vis other spending categories, with potential long-term consequences for capital formation and economic growth.

Our findings are robust to several alternative specifications presented in the supplmentary material. First, Table C.1 and Figure C.1 show the results for the indices of fiscal rules stringency at the regional and local level separately. Second, Table C.2 and Figure C.2 additionally contain the results for an extended panel that includes all current EU member states with a total of 5,736 region-year observations. In contrast to the baseline this panel is unbalenced. Third, Table C.3 and Figure C.3 display the results for twoway-clustered standard errors and clustering at the countrycode level. In the following, we present a number of additional analyses aimed at validating and extending our baseline findings.

5.2 Sensitivity Analysis

5.2.1 Arellano Bond



Note: Figure displays coefficient estimates and 95% confidence intervals for Arellano-Bond first-differenced GMM estimations of equation (1). *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$; *upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at NUTS2 level. The coefficients are scaled by 100 to simplify the interpretation. Coefficient estimates presented in tables B.2 and B.3.

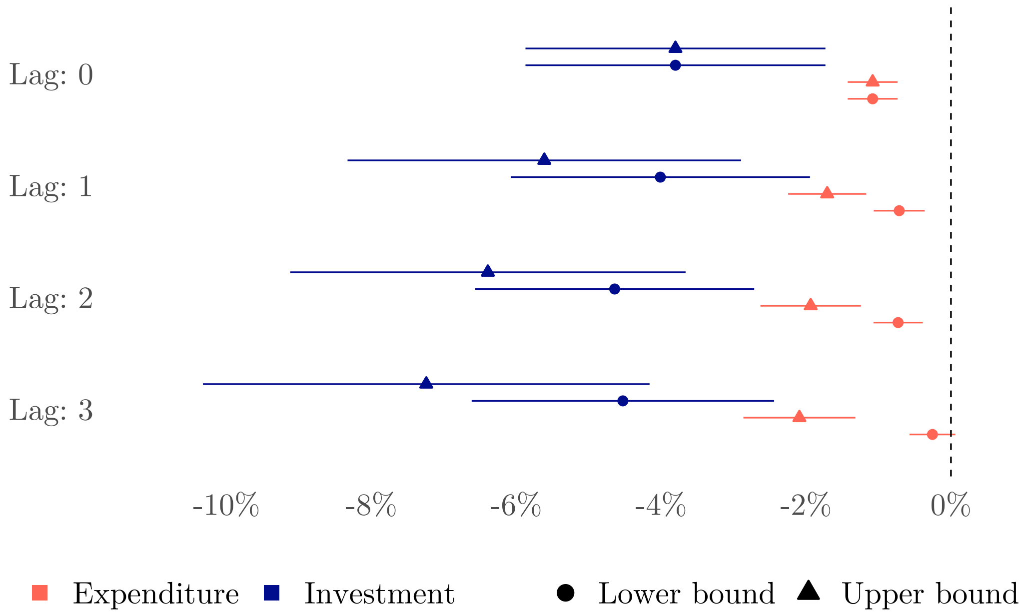
Figure 7: Effect plot - Arellano-Bond

Figure 7 displays the results of the Arellano-Bond estimator. The corresponding tables B.2 and B.3 can be found in the Appendix. The tables also report the p-values of the AR2 test for serial correlation in the first-differenced residuals proposed by Arellano and Bond (1991) and the p-value of the Sargan test[13]. We cannot reject the null of absence of serial correlation for the lower bound. The upper bound is more susceptible to serial correlation as desribed in section 4.1. Furthermore, for all specifications, we cannot reject the hypothesis that the instruments are valid.

Using the Arellano-Bond estimator, the effect of a one-unit (standard deviation) increase in FR_{t-h} on public expenditure lies between -0.06% (-0.17%) and -0.32% (-0.93%), and hence in a similar range compared to the baseline model. The effects are mostly statistically significant, pointing to a lasting decline in public expenditure after an increase in the stringency of fiscal rules.

The effect on public investment ranges from -0.91% (-2.64%) to -2.45% (-7.11%) and increases noticeably with larger lags of FR_{t-h}. The increasing strength of the effect over time supports our interpretation that cuts in public investment become easier with a certain lag. Overall, the results of the Arellano-Bond estimations are in line with the findings of the baseline analysis.

5.2.2 Instrumental Variable



Note: Figure displays coefficient estimates and 95% confidence intervals for Two-Stage Least-Squares estimations of equation (1). *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$; *upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at NUTS2 level. The coefficients are scaled by 100 to simplify the interpretation. Coefficient estimates presented in tables B.4 and B.5.

Figure 8: Effect plot - Instrumental variable

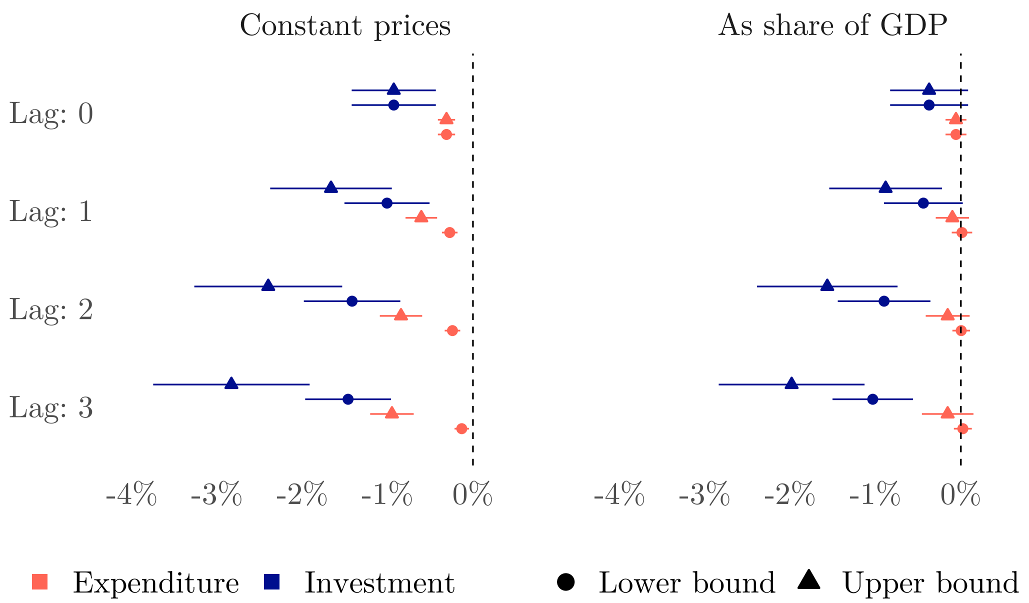
The results for the instrumental variable estimator can be found in Figure 8. In

[13]One should proceed with caution when assessing the results of this test, as it loses significance with an increasing number of instruments and tends towards 1 (Roodman, 2009). To aliviate the risk of overidentification, we restrict the number of lags used as instruments to 5.

the respective tables B.4 and B.5, we report the first stage results as well as an F-test and the Kleinbergen-Paap statistic to test for instrument relevance. Both statistics underline the relevance of government fractionalisation at the national level as instrument for FR_{t-h}.

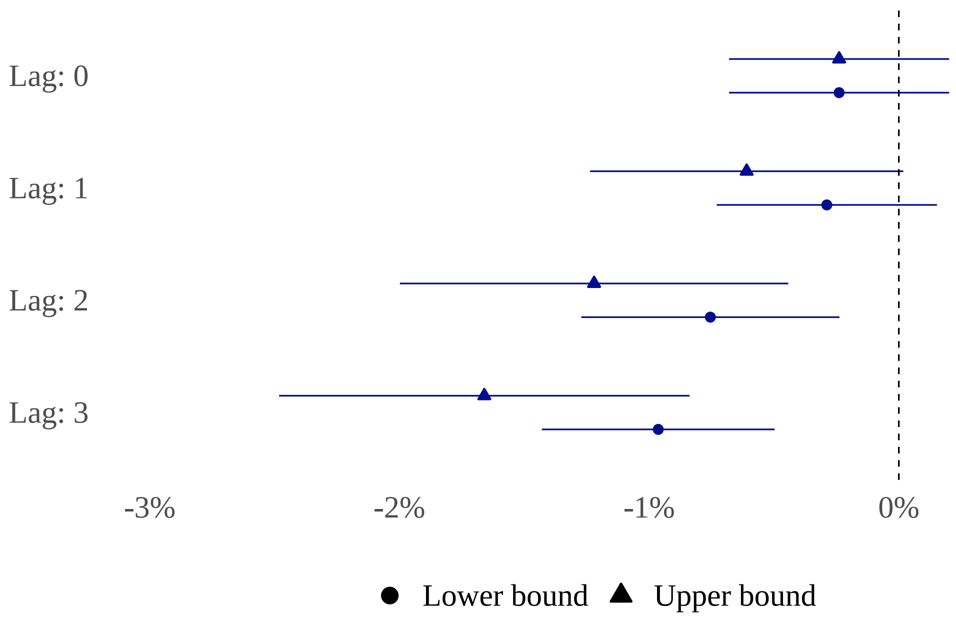
The IV estimates presented in Figure 8 support our baseline findings. We find significant negative effects on both public expenditure and investment with a greater effect on public investment. The coefficients are quantitatively larger, pointing to a potential downward bias of the Within and GMM estimator. However, we interpret the absolute size of the IV results with caution due to potential violations of the exclusion restriction. For instance, national and sub-national government fractionalization might correlate if voters do not differentiate between government levels in terms of voting preferences. Taken together, the IV and Arellano-Bond estimates strengthen the general empirical pattern found in our baseline analysis: An increase in the stringency of fiscal rules has a negative effect on public investment and this effect is disproportionately stronger than the effect on total public expenditure.

5.2.3 Alternative dependent variables



Note: Figure displays coefficient estimates and 95% confidence intervals for lower and upper bound estimations of the baseline model (within, two-way fixed effects) with alternative dependent variables. In the left panel, the dependent variables are expressed in million constant prices; in the right panel, the dependent variables as expressed as share of GDP. *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$; *upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at NUTS2 level. The coefficients are scaled by 100 to simplify the interpretation. Coefficient estimates presented in table B.6.

Figure 9: Effect plot - Alternative dependent variables



Note: Figure displays coefficient estimates and 95% confidence intervals for lower and upper bound estimations of the baseline model (within, two-way fixed effects) with the investment-to-expenditure ratio as dependent variable. *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$; *upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at NUTS2 level. The coefficients are scaled by 100 to simplify the interpretation. Coefficient estimates presented in table B.6.

Figure 10: Effect plot - Investment-Expenditure Ratio

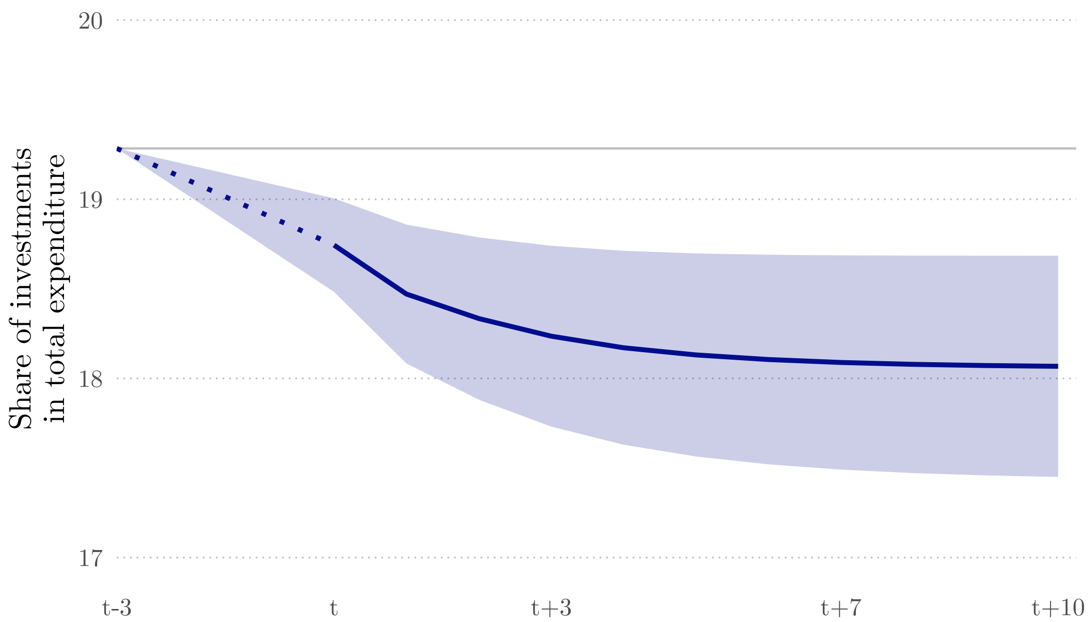
So far we have used gross value added and gross factor capital formation of the non-market sector in million PPS (purchasing power standard) as proxies for public expenditure and public investment. In Figure 9 we show the results for two alternative specifications of the dependent variable.

First, we specify the dependent variable in million constant prices.[14] The results using constant prices are qualitatively unchanged compared to the baseline model. Second, we set the dependent variable in relation to GDP. Investigating how fiscal rules affect public expenditure and investment in terms of GDP further strengthens our main finding of a disproportionate reduction in public investment: More stringent fiscal rules significantly reduce public investment as a share of GDP, while the reductions in overall expenditure as a share of GDP are much less pronounced and estimated with less precision (see Appendix Table B.6).

To further analyze the extent to which public investment decreases compared to overall expenditure following a strengthening of fiscal rules, we use the ratio of public investment to total public expenditure as dependent variable. Figure 10 shows the results for the investment-to-expenditure ratio. In fact, we find significant negative effects for $h > 1$ that amount to a reduction of the share of investments in total expenditure of up to 0.96 (1.66) percent for the lower bound (upper

[14] In the baseline model, we used PPS as this eliminates the difference in price levels between different regions that are not captured by the region fixed effect.

bound). The average investment to expenditure ratio in the sample is around 19 percent. An one-standard-deviation (2.9) increase of FR_{ct-3} would therefore lead to a reduction of the investment-to-expenditure ratio by 0.54 (0.95) percentage points. Figure 11 shows the long-term effect of a one-standard deviation increase in FR_{ct-3} on the share of investment in total expenditure for the lower bound and h=3, starting from the average share in the data sample. After 10 years, the overall effect on the share of investment in total expenditure is -1.22% for an one-standard deviation increase. On average, a tightening of fiscal rules leads to a permanent reduction of the share of investment in total expenditure of over 1 percent within 10 years.

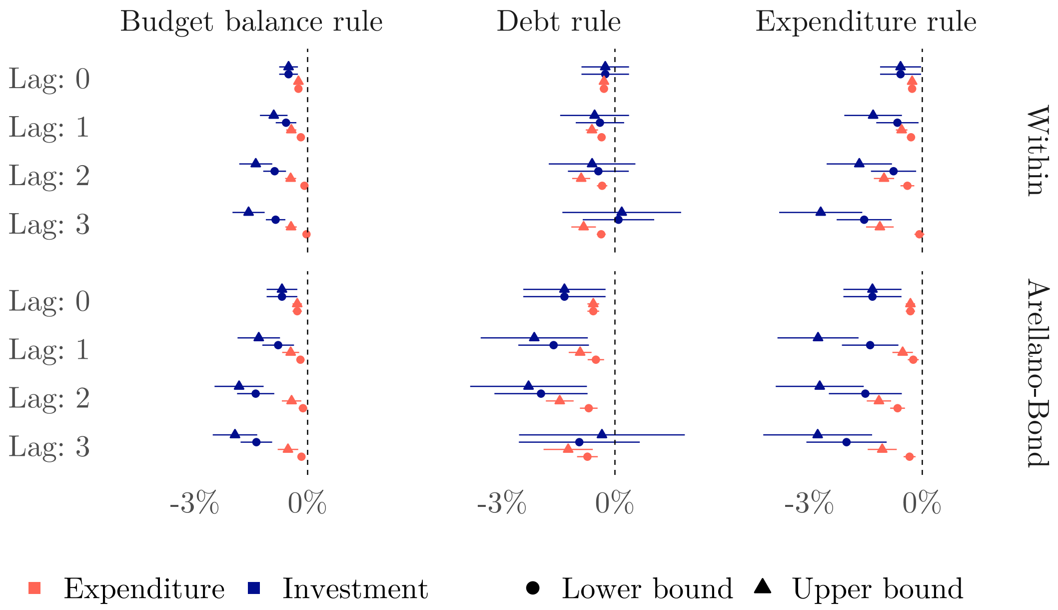


Note: Figure displays the longrun effect of an increase in FR_{t-3} by one standard-deviation on the investment-to-expenditure ratio and its 95% confidence interval for the lower bound estimation of the baseline model. The initial share of 19.28 percent is the average share in the data sample. Since we do not observe the excact development within the years between t-3 and t, this intervall is shown in dotted lines. Standard errors clustered at NUTS2 level.

Figure 11: Long-term effect on share of investment in total expenditure

5.2.4 Different types of fiscal rules

In the analysis presented so far, we have combined the variation from changes in different fiscal rules in one index. Consequently, we have considered the joint effect of a set of fiscal rules. Yet, while often implemented and changed as a package, different types of fiscal rules might impose different fiscal constraints on policymakers and induce heterogeneous fiscal adjustments. To analyze this potential heterogeneity more closely, we simultaneously include in our specification three different types of fiscal rules: budget balance rules (BBR), debt rules



Note: Figure displays coefficient estimates and 95% confidence intervals for different fiscal rule types from Within and Arellano-Bond estimations (lower and upper bound). *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$; *upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at NUTS2 level. The coefficients are scaled by 100 to simplify the interpretation. Coefficient estimates presented in table B.8.

Figure 12: Effect plot - Different rule types

(DR) and expenditure rules (ER)[15]. Results for the Within and Arellano-Bond estimator are illustrated in Figure 12.

For budget balance and expenditure rules, we observe a pattern that is highly consistent with the results on the composite index for both public expenditure and investment across specifications as well as in terms of effect sizes. Regarding the impact of debt rules, the results are less robust, especially with respect to public investment. One potential reason for this relates to the fact that debt rules have the lowest coverage, change least frequently and are, on average, the least stringent rule type in our dataset.[16]

6 Conclusion

In this paper, we estimate the effects of fiscal rules on public expenditure and public investment in 179 European NUTS2 regions for the period 1995 to 2018. We construct a new index of fiscal rules and identify subnational government spending and investment by help of the ARDECO database. Using a dynamic panel model that isolates variation over time within regions and accounts for

[15]There are no revenue rules (RR) at the subnational level in Europe.

[16]The sample mean of DR is 1.04 compared to 5.39 (BR) and 1.29 (ER) (see Tables A.2 and A.3 in the appendix). In fact, debt rules at the subnational level are only in place in Spain and Portugal, limiting the external validity of our results for this subset of fiscal rules

the non-random distribution of fiscal rules by including lagged values of the dependent variable, we find that an increase in the stringency of fiscal rules leads to a reduction in both public expenditure and public investment. The effect on public investment is, however, much more substantial than that on overall public expenditure. Overall, our results suggest a disproportional reduction in public investment due to fiscal rules. The findings are highly robust for a wide range of methods and different administrative levels. Considering the different types of fiscal rules, the baseline findings are confirmed for budget balance and expenditure rules, while the results are less robust for debt rules.

Our paper makes two main contributions to the study of fiscal policies. First, with our index, we create a new dataset that allows studying the effect of fiscal rules at different government levels. The potential applications of the data have by no means been exhausted in this paper. By applying the POSET procedure, we also contribute to further establishing this method. Second, we contribute to a relatively small but growing literature on the link between fiscal rules and public investment. Zooming in to specific spending categories like public investment is particularly relevant to get a better understanding of the potential welfare effects of fiscal rules.

In addition, the results have important policy implications. While our analysis suggests that fiscal rules can effectively constrain fiscal policies by reducing public spending, the induced fiscal consolidation might come with potential welfare costs if it is achieved through excessive budget cuts in investments. Our findings imply that the currently implemented fiscal rules do not necessarily offset the underlying distortionary spending incentives related to the deficit bias but might introduce a disinvestment bias. Thus, the design of fiscal rules should take these political-economy considerations into account, for instance by incorporating more flexible regulations for public investments. In this sense, our paper also lays the ground for future research that investigates the effectivness of golden rules in light of the trade-off between the deficit and disinvestment bias at the national level.

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Appendix

A Summary Statistics

Variable Definitions

Database	Variable	Description
	nuts_id	id for nuts2 area
	nuts_name	name of nuts2 area
	countrycode	countrycode
Fiscal rules stringency index (Self-generated with POSET using the European Commission's fiscal rules database.)	frd_sng_bbr	subnational government, budget balance rule
	frd_sng_dr	subnational government, deficit rule
	frd_sng_er	subnational government, expenditure rule
	frd_sng_rr	subnational government, revenue rule
	frd_sng_full	subnational government, all fiscal rule types combined
	frd_lg_bbr	local government, budget balance rule
	frd_lg_dr	local government, deficit rule
	frd_lg_er	local government, expenditure rule
	frd_lg_rr	local government, revenue rule
	frd_lg_full	local government, all fiscal rule types combined
	frd_rg_bbr	regional government, budget balance rule
	frd_rg_dr	regional government, deficit rule
	frd_rg_er	regional government, expenditure rule
	frd_rg_rr	regional government, revenue rule
	frd_rg_full	regional government, all fiscal rule types combined
	frd_cg_bbr	central government, budget balance rule
	frd_cg_dr	central government, deficit rule
	frd_cg_er	central government, expenditure rule
	frd_cg_rr	central government, revenue rule
	frd_cg_full	central government, all fiscal rule types combined
	frd_gg_bbr	general government, budget balance rule
	frd_gg_dr	general government, deficit rule
	frd_gg_er	general government, expenditure rule
	frd_gg_rr	general government, revenue rule
	frd_gg_full	general government, all fiscal rule types combined
	frd_ss_bbr	social security, budget balance rule
	frd_ss_dr	social security, deficit rule
	frd_ss_er	social security, expenditure rule
	frd_ss_rr	social security, revenue rule
	frd_ss_full	social security, all fiscal rule types combined
Ardeco database	gva_pps	gross value added in pps
	gdp_pps	gross domestic product in pps
	gdp_pps_pp	gdp per inhabitant in pps
	gva_nms_pps	gva by sector in pps -non-market services (o-u)
	gva_nms_cp	gva by sector at constant prices -non-market services (o-u)
	gva_nms_bp	gva by sector at basic prices -non-market services (o-u)
	gfcf_nms_pps	gfcf by sector in pps - non-market services (o-u)
	gfcf_nms_cp	gfcf by sector at constant prices - non-market services (o-u)
	gfcf_nms_bp	gfcf by sector at constant prices - non-market services (o-u)
The Database of Political Institutions 2020	dpi_legelec	legislative election held
	dpi_exelec	presidential election held
	dpi_auton	autonomous regions (federalism)
	dpi_state	state government
	dpi_govfrac	government fractionalization index
Ameco database	ameco_udgg	gross public debt of general government
	ameco_uigg0	gross factor capital formation (national level)
	ameco_uucgi	total current expenditure, excluding interest (general government)
	ameco_uvgd	national GDP
	ameco_avgdgp	output gap
Eurostat (GOV_10A_MAIN)	eurostat_taxauton	share of the subnational government of general government revenue

Table A.1: Variables within the database

Descriptive Statistics

Table A.2: Descriptive statistics

	Mean	Median	SD	Q_1	Q_3	IQR
Explanatory Variables						
frd-sng-full (baseline)	4.37	5.59	2.90	1.19	6.09	4.90
frd-sng-bbr	5.39	7.67	4.28	0.00	8.97	8.97
frd-sng-dr	1.04	0.00	2.86	0.00	0.00	0.00
frd-sng-er	1.29	0.00	2.73	0.00	0.58	0.58
frd-sng-rr	0.00	0.00	0.00	0.00	0.00	0.00
frd-lg-bbr	5.28	7.67	4.19	0.00	8.97	8.97
frd-lg-dr	0.74	0.00	2.06	0.00	0.00	0.00
frd-lg-er	0.77	0.00	2.46	0.00	0.00	0.00
frd-lg-rr	0.00	0.00	0.00	0.00	0.00	0.00
frd-lg-full	4.05	4.84	2.83	1.33	6.05	4.72
frd-rg-bbr	3.10	0.00	4.18	0.00	8.92	8.92
frd-rg-dr	1.02	0.00	2.85	0.00	0.00	0.00
frd-rg-er	1.26	0.00	2.72	0.00	0.00	0.00
frd-rg-rr	0.00	0.00	0.00	0.00	0.00	0.00
frd-rg-full	2.54	0.80	2.90	0.00	5.22	5.22
frd-cg-bbr	1.55	0.00	2.93	0.00	3.60	3.60
frd-cg-dr	0.04	0.00	0.30	0.00	0.00	0.00
frd-cg-er	1.49	0.00	2.59	0.00	2.33	2.33
frd-cg-rr	1.06	0.00	2.61	0.00	0.00	0.00
frd-cg-full	3.30	2.98	3.27	0.00	6.18	6.18
frd-gg-bbr	2.70	0.00	4.11	0.00	7.67	7.67
frd-gg-dr	0.85	0.00	2.67	0.00	0.00	0.00
frd-gg-er	1.16	0.00	2.82	0.00	0.00	0.00
frd-gg-rr	0.44	0.00	1.59	0.00	0.00	0.00
frd-gg-full	2.01	0.00	3.00	0.00	4.45	4.45
Dependent Variables						
ardeco-gva-nms-pps	9393.98	6800.35	9874.06	3157.53	11963.30	8805.77
ardeco-gfcf-nms-pps	1631.33	1198.38	1650.49	598.89	2117.72	1518.83
ardeco-gva-nms-cp	11193.94	8511.50	11332.53	3999.48	14570.88	10571.40
ardeco-gfcf-nms-cp	1903.76	1466.24	1893.06	712.98	2482.53	1769.55
ardeco-gva-gdp-cp	0.25	0.24	0.07	0.21	0.28	0.07
ardeco-gfcf-gdp-cp	0.05	0.04	0.02	0.03	0.05	0.02
ardeco-gfcf-gva-ratio	19.28	17.13	8.48	14.39	21.78	7.39
Control Variables						
dpi-legelec	0.26	0.00	0.44	0.00	1.00	1.00
dpi-auton	0.51	1.00	0.50	0.00	1.00	1.00
dpi-state	1.51	2.00	0.62	1.00	2.00	1.00
dpi-exelec	0.06	0.00	0.23	0.00	0.00	0.00
ameco-udgg	79.24	67.68	30.21	59.53	101.34	41.80
ameco-uigg0	3.25	3.21	0.95	2.35	3.95	1.60
ameco-uvgd	1106.29	1110.94	840.81	256.46	1730.20	1473.75
ameco-avgdgp	-0.71	-0.29	3.26	-1.66	1.34	3.01
ameco-uucgi	40.79	40.98	5.19	37.66	44.56	6.90

Table A.3: Descriptive statistics

	1995		2000		2005		2010		2016		2017		2018	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Explanatory Variables														
frd-sng-full (baseline)	3.13	2.83	3.73	2.40	4.10	2.29	4.21	2.60	5.72	3.47	5.72	3.47	5.72	3.47
frd-sng-bbr	3.98	4.36	4.70	4.27	5.63	3.90	5.39	4.17	6.33	4.61	6.33	4.61	6.33	4.61
frd-sng-dr	0.95	2.77	0.95	2.77	1.10	2.82	1.30	3.17	0.39	1.93	0.39	1.93	0.39	1.93
frd-sng-er	0.49	0.95	1.39	2.48	1.39	2.48	1.05	2.89	2.10	3.94	2.10	3.94	2.10	3.94
frd-sng-rr	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
frd-lg-bbr	3.98	4.36	4.70	4.27	5.61	3.90	5.23	4.05	6.13	4.47	6.13	4.47	6.13	4.47
frd-lg-dr	0.62	1.80	0.62	1.80	0.77	1.90	0.92	2.26	0.39	1.93	0.39	1.93	0.39	1.93
frd-lg-er	0.00	0.00	0.90	2.48	0.90	2.48	1.05	2.89	1.05	3.06	1.05	3.06	1.05	3.06
frd-lg-rr	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
frd-lg-full	2.96	2.86	3.61	2.48	3.90	2.35	4.00	2.61	5.41	3.33	5.41	3.33	5.41	3.33
frd-rg-bbr	2.15	3.66	2.53	3.82	3.44	4.01	3.20	4.15	3.27	4.67	3.27	4.67	3.27	4.67
frd-rg-dr	0.95	2.77	0.95	2.77	0.95	2.77	1.30	3.17	0.39	1.93	0.39	1.93	0.39	1.93
frd-rg-er	0.49	0.95	1.39	2.48	1.39	2.48	1.05	2.89	2.10	3.94	2.10	3.94	2.10	3.94
frd-rg-rr	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
frd-rg-full	1.61	2.28	1.94	2.17	2.23	2.33	2.36	2.65	3.44	3.70	3.44	3.70	3.44	3.70
frd-cg-bbr	0.76	1.48	1.22	2.10	1.33	2.14	0.91	1.58	2.48	4.23	2.48	4.23	2.48	4.23
frd-cg-dr	0.07	0.41	0.07	0.41	0.07	0.41	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
frd-cg-er	0.75	1.29	1.07	1.59	1.97	2.59	0.73	2.00	1.48	3.45	1.48	3.45	1.48	3.45
frd-cg-rr	0.14	0.56	0.51	1.92	0.51	1.92	1.68	3.18	1.54	2.95	1.54	2.95	1.54	2.95
frd-cg-full	1.67	2.67	2.61	2.76	3.18	2.51	2.06	2.70	4.39	3.73	4.39	3.73	4.39	3.73
frd-gg-bbr	0.06	0.38	0.17	0.61	0.98	2.39	1.14	2.42	9.17	2.57	9.17	2.57	9.17	2.57
frd-gg-dr	0.00	0.00	0.00	0.00	0.01	0.08	0.01	0.08	4.02	4.72	4.52	4.79	4.52	4.79
frd-gg-er	0.45	1.56	0.45	1.56	0.45	1.56	0.98	2.48	2.52	4.24	2.52	4.24	2.52	4.24
frd-gg-rr	0.42	1.55	0.42	1.56	0.59	1.83	0.59	1.83	0.00	0.00	0.00	0.00	0.00	0.00
frd-gg-full	0.28	0.90	0.48	1.55	0.79	1.76	0.94	1.91	7.03	2.13	7.08	2.18	7.08	2.18
Dependent Variables														
ardeco-gva-nms-pps	5637.75	5609.57	7407.02	7522.82	9134.80	9138.24	10807.88	10987.15	11953.10	11997.62	12330.75	12373.43	12660.17	12667.28
ardeco-gfcf-nms-pps	1109.92	1075.70	1393.74	1301.64	1664.93	1674.33	1934.05	1999.13	1773.27	1700.26	1840.21	1755.61	1928.12	1866.54
ardeco-gva-nms-cp	8434.37	8495.40	10116.42	10218.95	11082.22	11004.80	12178.32	12230.03	12764.49	12803.60	13008.52	13078.22	13178.07	13222.49
ardeco-gfcf-nms-cp	1529.69	1529.25	1797.89	1735.65	1976.19	1981.64	2129.85	2150.59	1913.38	1866.95	1949.59	1891.91	2007.42	1967.58
ardeco-gva-gdp-cp	0.29	0.09	0.27	0.07	0.25	0.06	0.24	0.06	0.22	0.06	0.22	0.06	0.22	0.06
ardeco-gfcf-gdp-cp	0.06	0.03	0.05	0.02	0.05	0.02	0.05	0.02	0.04	0.01	0.04	0.02	0.03	0.01
ardeco-gfcf-gva-ratio	21.23	8.87	21.25	9.13	19.36	7.02	19.36	8.76	16.20	6.40	16.73	8.03	16.42	6.31
Control Variables														
dpi-legelec	0.18	0.38	0.18	0.38	0.28	0.45	0.17	0.38	0.12	0.33	0.48	0.50	0.32	0.47
dpi-auton	0.50	0.50	0.54	0.50	0.54	0.50	0.40	0.49	0.50	0.50	0.50	0.50	0.54	0.50
dpi-state	1.52	0.63	1.49	0.58	1.52	0.63	1.55	0.58	1.46	0.63	1.52	0.63	1.49	0.58
dpi-exelec	0.15	0.36	0.03	0.17	0.00	0.00	0.12	0.33	0.09	0.29	0.15	0.36	0.15	0.36
ameco-udgg	74.64	26.00	69.25	23.34	69.59	23.23	85.35	27.79	95.20	36.06	92.85	36.86	91.55	38.98
ameco-uigg0	3.47	0.85	3.32	0.85	3.29	0.93	3.53	0.92	2.78	0.76	2.85	0.87	2.83	0.74
ameco-uvgd	749.52	575.83	910.76	663.61	1075.62	760.42	1204.46	851.76	1374.51	1014.42	1426.18	1053.88	1467.97	1084.93
ameco-avgdgp	-1.22	1.03	2.09	1.07	0.05	1.80	-2.46	2.02	-1.90	3.65	-0.52	3.38	0.20	2.78
ameco-uucgi	40.03	6.14	38.42	5.09	39.81	4.75	43.15	3.77	42.51	5.08	41.83	5.11	41.79	5.07

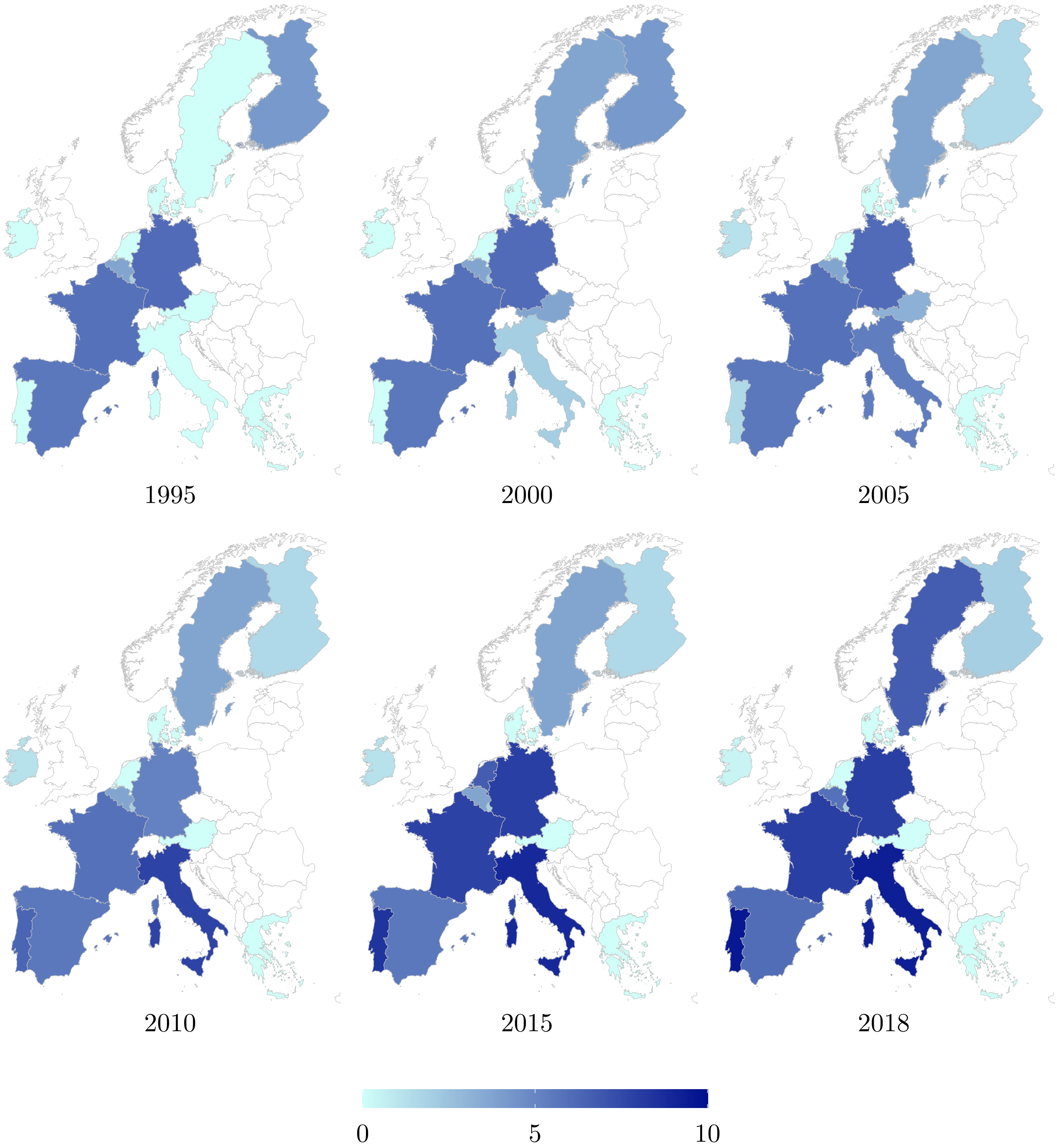


Figure A.1: Map fiscal rule stringency index (subnational government)

B Additional Tables

Baseline

	Expenditure				Investment			
	h=0	h=1	h=2	h=3	h=0	h=1	h=2	h=3
FR_{t-h}	$-.0044^{***}$	$-.0081^{***}$	$-.0104^{***}$	$-.0111^{***}$	$-.0095^{***}$	$-.0168^{***}$	$-.0242^{***}$	$-.0282^{***}$
	(.0005)	(.0009)	(.0012)	(.0012)	(.0025)	(.0035)	(.0042)	(.0044)
dep var$_{t-1}$	1.0396^{***}				$.5493^{***}$			
	(.0408)				(.0347)			
dep var$_{t-2}$	$-.1232^{***}$	$.9159^{***}$			$-.0044$	$.2919^{***}$		
	(.0316)	(.0744)			(.0588)	(.0494)		
dep var$_{t-3}$	$-.0322$	$-.1578^{***}$	$.7745^{***}$		$.0951^{**}$	$.0559^{*}$	$.2006^{***}$	
	(.0219)	(.0308)	(.1024)		(.0434)	(.0320)	(.0380)	
dep var$_{t-4}$		.0019	$-.1716^{***}$	$.6316^{***}$		$.1052^{**}$	$.0992^{**}$	$.2050^{***}$
		(.0278)	(.0401)	(.1038)		(.0443)	(.0405)	(.0385)
dep var$_{t-5}$			$.0552^{**}$	$-.0983^{**}$			.0115	$-.0022$
			(.0239)	(.0431)			(.0342)	(.0261)
dep var$_{t-6}$				.0480				.0253
				(.0303)				(.0283)
N	3759	3580	3401	3222	3759	3580	3401	3222
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓
Within adj. R^2	.9035	.7897	.7008	.6629	.6234	.5046	.4705	.4601
P-value for H_0: $\|\beta_{exp}\| \geq \|\beta_{inv}\|$	–	–	–	–	0.0234	0.0087	0.0009	0.0001

Notes: Coefficient estimates for Fixed-Effects Least-Squares (FELS) estimations of equation (1), excluding y_{rct-1} to y_{rct-h}. Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. The bottom row of the table reports p-values for a t-test with the null hypothesis that $|\beta_{exp}| \geq |\beta_{inv}|$ in the corresponding specification with lag size h. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.1: Baseline model (upper bound)

Arellano-Bond estimates

	Expenditure				Investment			
	h=0	h=1	h=2	h=3	h=0	h=1	h=2	h=3
FR_{t-h}	$-.0032^{***}$	$-.0007$	$-.0024^{***}$	$-.0019^{**}$	$-.0092^{**}$	$-.0097^{***}$	$-.0155^{***}$	$-.0253^{***}$
	(.0007)	(.0007)	(.0008)	(.0009)	(.0039)	(.0037)	(.0049)	(.0044)
dep var_{t-1}	$.7781^{***}$	$.7054^{***}$	$.6720^{***}$	$.6254^{***}$	$.3516^{***}$	$.3310^{***}$	$.3155^{***}$	$.3215^{***}$
	(.0342)	(.0341)	(.0434)	(.0529)	(.0547)	(.0559)	(.0542)	(.0534)
dep var_{t-2}	$-.0874^{***}$	$-.0614$	$-.1230^{***}$	$-.0530$	$-.0570$	$-.0603$	$-.0724$	$-.0622$
	(.0339)	(.0394)	(.0445)	(.0454)	(.0538)	(.0497)	(.0496)	(.0506)
dep var_{t-3}	$-.0024$	$-.0137$	.0114	.0004	.0573	.0272	.0211	.0221
	(.0309)	(.0496)	(.0537)	(.0528)	(.0380)	(.0391)	(.0380)	(.0372)
dep var_{t-4}		.0211	.0529	.0136		.0511	.0455	.0583
		(.0327)	(.0441)	(.0417)		(.0333)	(.0372)	(.0372)
dep var_{t-5}			$-.0063$	$.1193^{***}$			$-.0097$	$-.0165$
			(.0310)	(.0338)			(.0301)	(.0297)
dep var_{t-6}				$-.1377^{***}$				$.1037^{***}$
				(.0394)				(.0372)
N	3580	3401	3222	3043	3580	3401	3222	3043
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓
AR(2)	.2521	.2948	.0503	.1404	.1414	.8681	.6046	.1397
Sargan (GMM)	.8042	.6797	.5166	.3206	.7785	.6222	.5227	.2445
Effect after 10 years (t+10)	$-.0102$	$-.0019$	$-.0061$	$-.0045$	$-.0143^{***}$	$-.0149^{***}$	$-.0222^{***}$	$-.0426^{***}$
	(.0068)	(.0058)	(.0056)	(.0063)	(.0030)	(.0029)	(.0029)	(.0044)
P-value for H_0: $\lvert\beta_{exp}\rvert \geq \lvert\beta_{inv}\rvert$	–	–	–	–	0.0624	0.009	0.0045	0.000

Notes: Coefficient estimates for Arellano-Bond First-Differenced GMM estimations of equation (1). *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$. Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. Table reports p-values for the AR(2) test on the the null hypothesis of absence of serial correlation in the first-differenced residuals, and for the Hansen-Sargan J Test on the null of instrument validity. Effect after 10 years and the corresponding standard error based on dynamic effects and deltamethod. The bottom row of the table reports p-values for a t-test with the null hypothesis that $|\beta_{exp}| \geq |\beta_{inv}|$ in the corresponding specification with lag size h. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.2: Arellano-Bond (lower bound)

	Expenditure				Investment			
	h=0	h=1	h=2	h=3	h=0	h=1	h=2	h=3
FR_{t-h}	$-.0032^{***}$	$-.0016$	$-.0044^{***}$	$-.0043^{***}$	$-.0092^{**}$	$-.0169^{***}$	$-.0212^{***}$	$-.0320^{***}$
	(.0007)	(.0012)	(.0013)	(.0014)	(.0039)	(.0046)	(.0054)	(.0056)
dep var_{t-1}	$.7781^{***}$				$.3516^{***}$			
	(.0342)				(.0547)			
dep var_{t-2}	$-.0874^{***}$	$.4022^{***}$			$-.0570$	$-.0571$		
	(.0339)	(.0416)			(.0538)	(.0466)		
dep var_{t-3}	$-.0024$	$-.1235^{***}$	$.1458^{***}$		.0573	$-.0106$	$-.0391$	
	(.0309)	(.0447)	(.0454)		(.0380)	(.0283)	(.0269)	
dep var_{t-4}		.0385	$.0691^{*}$	$.1244^{***}$		.0454	.0443	.0384
		(.0403)	(.0394)	(.0412)		(.0336)	(.0388)	(.0359)
dep var_{t-5}			$-.0200$	$.1653^{***}$			$-.0050$	$-.0105$
			(.0369)	(.0443)			(.0264)	(.0255)
dep var_{t-6}				$-.2032^{***}$				$.1041^{***}$
				(.0722)				(.0388)
N	3580	3401	3222	3043	3580	3401	3222	3043
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓
AR(2)	.2521	.0000	.0026	.0014	.1414	.0002	.0227	.0652
Sargan (GMM)	.8042	.6898	.5086	.3130	.7785	.6650	.6393	.3065
P-value for H_0: $\lvert\beta_{exp}\rvert \geq \lvert\beta_{inv}\rvert$	–	–	–	–	0.0624	0.0007	0.0014	0.0000

Notes: Coefficient estimates for Arellano-Bond First-Differenced GMM estimations of equation (1). *Upper bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$. *Upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. Table reports p-values for the AR(2) test on the the null hypothesis of absence of serial correlation in the first-differenced residuals, and for the Hansen-Sargan J Test on the null of instrument validity. The bottom row of the table reports p-values for a t-test with the null hypothesis that $|\beta_{exp}| \geq |\beta_{inv}|$ in the corresponding specification with lag size h. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.3: Arellano-Bond (upper bound)

Instrumental Variable estimates

	Expenditure				Investment			
	h=0	h=1	h=2	h=3	h=0	h=1	h=2	h=3
FR_{t-h}	$-.0108^{***}$	$-.0071^{***}$	$-.0073^{***}$	$-.0026$	$-.0380^{***}$	$-.0401^{***}$	$-.0464^{***}$	$-.0453^{***}$
	(.0017)	(.0018)	(.0017)	(.0016)	(.0105)	(.0105)	(.0098)	(.0106)
dep var$_{t-1}$	$.9938^{***}$	$.9801^{***}$	$.9547^{***}$	$.9248^{***}$	$.5226^{***}$	$.5012^{***}$	$.4823^{***}$	$.4752^{***}$
	(.0434)	(.0469)	(.0473)	(.0430)	(.0370)	(.0361)	(.0360)	(.0398)
dep var$_{t-2}$	$-.1042^{***}$	$-.0946^{***}$	$-.1084^{***}$	$-.0739^{**}$	$-.0104$	$-.0246$	$-.0374$	$-.0264$
	(.0327)	(.0322)	(.0353)	(.0327)	(.0579)	(.0543)	(.0530)	(.0549)
dep var$_{t-3}$	$-.0154$	$-.0349$	$-.0108$	$-.0107$	$.0934^{**}$	.0525	.0395	.0354
	(.0216)	(.0369)	(.0431)	(.0437)	(.0431)	(.0455)	(.0442)	(.0445)
dep var$_{t-4}$		.0257	$-.0364$	$-.0161$		$.0605^{*}$	.0655	.0550
		(.0237)	(.0440)	(.0437)		(.0338)	(.0406)	(.0406)
dep var$_{t-5}$			$.0743^{***}$	.0496			$-.0129$	$-.0183$
			(.0245)	(.0388)			(.0319)	(.0354)
dep var$_{t-6}$				.0225				.0198
				(.0225)				(.0236)
First Stage								
gov. frac.$_{t-h-2}$	2.415^{***}	2.659^{***}	3.200^{***}	2.873^{***}	2.373^{***}	2.737^{***}	3.362^{***}	3.103^{***}
	(0.3114)	(0.2915)	(0.2724)	(0.2868)	(0.3243)	(0.3274)	(0.3078)	(0.3087)
N	3759	3580	3401	3222	3759	3580	3401	3222
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓
F-test 1^{st} stage	174.8128	190.6243	260.6907	204.1090	165.8409	195.8259	272.6551	226.7540
Kleinbergen-Paap	60.1527	83.1993	136.1666	100.2384	53.5420	69.8738	119.3767	101.0507
Wu-Hausman	.0001	.0289	.0018	.6122	.0010	.0002	.0000	.0001
Effect after 10 years (t+10)	$-.0710^{***}$	$-.0462^{***}$	$-.0433^{***}$	$-.0158^{*}$	$-.0948^{***}$	$-.0955^{***}$	$-.0993^{***}$	$-.0971^{***}$
	(.0138)	(.0103)	(.0092)	(.0087)	(.0231)	(.0221)	(.0187)	(.0187)
P-value for H_0: $\|\beta_{exp}\| \geq \|\beta_{inv}\|$	–	–	–	–	0.0054	0.001	0.000	0.000

Notes: Coefficient estimates for Two-Stage Least-Squares estimations of equation (1). *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$. Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. Table reports the first-stage F statistic and Kleibergen-Paap Wald statistic. P-values reported for the Wu-Hausmann test on the null of no differences between the IV and baseline Within estimates. Effect after 10 years and the corresponding standard error based on dynamic effects and deltamethod. The bottom row of the table reports p-values for a t-test with the null hypothesis that $|\beta_{exp}| \geq |\beta_{inv}|$ in the corresponding specification with lag size h. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.4: Instrumental variable (lower bound)

	Expenditure				Investment			
	h=0	h=1	h=2	h=3	h=0	h=1	h=2	h=3
FR_{t-h}	$-.0108^{***}$	$-.0171^{***}$	$-.0194^{***}$	$-.0209^{***}$	$-.0380^{***}$	$-.0561^{***}$	$-.0639^{***}$	$-.0724^{***}$
	(.0017)	(.0027)	(.0035)	(.0039)	(.0105)	(.0138)	(.0138)	(.0156)
dep var$_{t-1}$	$.9938^{***}$				$.5226^{***}$			
	(.0434)				(.0370)			
dep var$_{t-2}$	$-.1042^{***}$	$.8630^{***}$			$-.0104$	$.2541^{***}$		
	(.0327)	(.0755)			(.0579)	(.0489)		
dep var$_{t-3}$	$-.0154$	$-.1482^{***}$	$.7104^{***}$		$.0934^{**}$	.0517	$.1681^{***}$	
	(.0216)	(.0301)	(.1063)		(.0431)	(.0328)	(.0374)	
dep var$_{t-4}$		.0388	$-.1313^{***}$	$.5715^{***}$		$.1060^{**}$	$.0968^{**}$	$.1694^{***}$
		(.0278)	(.0440)	(.1072)		(.0429)	(.0394)	(.0394)
dep var$_{t-5}$			$.0764^{***}$	$-.0586$			.0141	.0045
			(.0249)	(.0468)			(.0343)	(.0265)
dep var$_{t-6}$				$.0686^{**}$				.0205
				(.0316)				(.0302)
First Stage								
gov. frac.$_{t-h-2}$	2.415^{***}	2.903^{***}	3.541^{***}	3.398^{***}	2.373^{***}	2.816^{***}	3.507^{***}	3.340^{***}
	(0.3114)	(0.3153)	(0.3056)	(0.3231)	(0.3243)	(0.3392)	(0.3290)	(0.3390)
N	3759	3580	3401	3222	3759	3580	3401	3222
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓
F-test 1^{st} stage	174.8128	222.8063	307.8644	274.6814	165.8409	206.2747	293.8118	260.0255
Kleinbergen-Paap	60.1527	84.7716	134.2971	110.5947	53.5420	68.8920	113.5857	97.0828
Wu-Hausman	.0001	.0000	.0000	.0000	.0010	.0000	.0000	.0000
P-value for H_0: $\|\beta_{exp}\| \geq \|\beta_{inv}\|$	–	–	–	–	0.0054	0.0028	0.001	0.0008

Notes: Coefficient estimates for Two-Stage Least-Squares estimations of equation (1). *Upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. Table reports the first-stage F statistic and Kleibergen-Paap Wald statistic. P-values reported for the Wu-Hausmann test on the null of no differences between the IV and baseline Within estimates. The bottom row of the table reports p-values for a t-test with the null hypothesis that $|\beta_{exp}| \geq |\beta_{inv}|$ in the corresponding specification with lag size h. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.5: Instrumental variable (upper bound)

Different dependent variables

	Expenditure				Investment			
	h=0	h=1	h=2	h=3	h=0	h=1	h=2	h=3
	Panel A: Constant prices							
	Lower bound							
FR_{t-h}	$-.0031^{***}$	$-.0027^{***}$	$-.0024^{***}$	$-.0013^{***}$	$-.0093^{***}$	$-.0101^{***}$	$-.0142^{***}$	$-.0147^{***}$
	(.0005)	(.0005)	(.0005)	(.0004)	(.0025)	(.0025)	(.0029)	(.0026)
	Upper bound							
FR_{t-h}	$-.0031^{***}$	$-.0061^{***}$	$-.0084^{***}$	$-.0095^{***}$	$-.0093^{***}$	$-.0167^{***}$	$-.0240^{***}$	$-.0283^{***}$
	(.0005)	(.0009)	(.0013)	(.0013)	(.0025)	(.0036)	(.0044)	(.0047)
	Panel B: Share of GDP							
	Lower bound							
FR_{t-h}	$-.0014^{**}$	$-.0006$	$-.0004$	$-.0001$	$-.0043^{*}$	$-.0049^{**}$	$-.0091^{***}$	$-.0107^{***}$
	(.0005)	(.0005)	(.0006)	(.0006)	(.0023)	(.0023)	(.0027)	(.0024)
	Upper bound							
FR_{t-h}	$-.0014^{**}$	$-.0017^{*}$	$-.0018$	$-.0018$	$-.0043^{*}$	$-.0092^{***}$	$-.0152^{***}$	$-.0192^{***}$
	(.0005)	(.0009)	(.0012)	(.0014)	(.0023)	(.0033)	(.0040)	(.0041)
N	3759	3580	3401	3222	3759	3580	3401	3222
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Coefficient estimates for Fixed-Effects Least-Squares (FELS) estimations of equation (1). In Panel A, the dependent variables are expressed in million constant prices; in Panel B, the dependent variables are expressed as share of GDP in PPS. *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$. *Upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.6: Alternative dependent variables

	Lower bound				Upper bound			
	h=0	h=1	h=2	h=3	h=0	h=1	h=2	h=3
FR_{t-h}	$-.0024$	$-.0029$	$-.0075^{***}$	$-.0096^{***}$	$-.0024$	$-.0061^{*}$	$-.0122^{***}$	$-.0166^{***}$
	(.0022)	(.0022)	(.0026)	(.0024)	(.0022)	(.0032)	(.0039)	(.0042)
N	3580	3401	3222	3043	3580	3401	3222	3043
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓
Within adj. R^2	.5676	.5576	.5460	.5470	.5676	.4318	.3876	.3713
Effect after 10 years (t+10)	$-.0064$	$-.0074$	$-.0172^{***}$	$-.0222^{***}$	–	–	–	–
	(.0060)	(.0058)	(.0060)	(.0056)	–	–	–	–

Notes: Coefficient estimates for Fixed-Effects Least-Squares (FELS) estimations of equation (1). The dependent variable is the investment-to-expenditure ratio. *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$. *Upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. Effect after 10 years and the corresponding standard error based on dynamic effects and deltamethod. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.7: Investment-to-expenditure ratio

Different types of fiscal rules

	Expenditure				Investment			
	h=0	h=1	h=2	h=3	h=0	h=1	h=2	h=3
				Panel A: Within Estimates				
				Lower bound				
FR^{bbr}_{t-h}	$-.0025^{***}$	$-.0018^{***}$	$-.0008^{***}$	$-.0003$	$-.0051^{***}$	$-.0058^{***}$	$-.0088^{***}$	$-.0085^{***}$
	(.0004)	(.0003)	(.0003)	(.0003)	(.0013)	(.0014)	(.0015)	(.0013)
FR^{dr}_{t-h}	$-.0029^{***}$	$-.0036^{***}$	$-.0034^{***}$	$-.0037^{***}$	$-.0026$	$-.0040$	$-.0044$	$.0009$
	(.0005)	(.0005)	(.0007)	(.0006)	(.0032)	(.0033)	(.0041)	(.0048)
FR^{er}_{t-h}	$-.0027^{***}$	$-.0030^{***}$	$-.0040^{***}$	$-.0008$	$-.0058^{**}$	$-.0066^{**}$	$-.0077^{**}$	$-.0155^{***}$
	(.0004)	(.0005)	(.0009)	(.0007)	(.0028)	(.0029)	(.0031)	(.0037)
				Upper bound				
FR^{bbr}_{t-h}	$-.0025^{***}$	$-.0044^{***}$	$-.0045^{***}$	$-.0044^{***}$	$-.0051^{***}$	$-.0090^{***}$	$-.0138^{***}$	$-.0157^{***}$
	(.0004)	(.0007)	(.0007)	(.0008)	(.0013)	(.0019)	(.0022)	(.0022)
FR^{dr}_{t-h}	$-.0029^{***}$	$-.0061^{***}$	$-.0090^{***}$	$-.0084^{***}$	$-.0026$	$-.0054$	$-.0061$	$.0018$
	(.0005)	(.0008)	(.0012)	(.0017)	(.0032)	(.0047)	(.0059)	(.0080)
FR^{er}_{t-h}	$-.0027^{***}$	$-.0055^{***}$	$-.0102^{***}$	$-.0113^{***}$	$-.0058^{**}$	$-.0131^{***}$	$-.0168^{***}$	$-.0271^{***}$
	(.0004)	(.0008)	(.0014)	(.0019)	(.0028)	(.0039)	(.0044)	(.0056)
N	3759	3580	3401	3222	3759	3580	3401	3222
				Panel B: Arellano-Bond estimates				
				Lower bound				
FR^{bbr}_{t-h}	$-.0028^{***}$	$-.0019^{***}$	$-.0012^{**}$	$-.0016^{***}$	$-.0068^{***}$	$-.0079^{***}$	$-.0139^{***}$	$-.0137^{***}$
	(.0006)	(.0006)	(.0006)	(.0005)	(.0021)	(.0022)	(.0025)	(.0021)
FR^{dr}_{t-h}	$-.0058^{***}$	$-.0051^{***}$	$-.0070^{***}$	$-.0073^{***}$	$-.0135^{**}$	$-.0164^{***}$	$-.0197^{***}$	$-.0095$
	(.0008)	(.0011)	(.0012)	(.0014)	(.0056)	(.0048)	(.0063)	(.0082)
FR^{er}_{t-h}	$-.0032^{***}$	$-.0024^{***}$	$-.0066^{***}$	$-.0034^{***}$	$-.0133^{***}$	$-.0139^{***}$	$-.0152^{***}$	$-.0202^{***}$
	(.0006)	(.0007)	(.0010)	(.0008)	(.0040)	(.0038)	(.0050)	(.0055)
				Upper bound				
FR^{bbr}_{t-h}	$-.0028^{***}$	$-.0045^{***}$	$-.0043^{***}$	$-.0052^{***}$	$-.0068^{***}$	$-.0130^{***}$	$-.0183^{***}$	$-.0194^{***}$
	(.0006)	(.0012)	(.0013)	(.0014)	(.0021)	(.0029)	(.0033)	(.0030)
FR^{dr}_{t-h}	$-.0058^{***}$	$-.0093^{***}$	$-.0147^{***}$	$-.0125^{***}$	$-.0135^{**}$	$-.0215^{***}$	$-.0230^{***}$	$-.0035$
	(.0008)	(.0016)	(.0019)	(.0034)	(.0056)	(.0073)	(.0079)	(.0113)
FR^{er}_{t-h}	$-.0032^{***}$	$-.0052^{***}$	$-.0116^{***}$	$-.0107^{***}$	$-.0133^{***}$	$-.0278^{***}$	$-.0274^{***}$	$-.0279^{***}$
	(.0006)	(.0014)	(.0017)	(.0020)	(.0040)	(.0055)	(.0060)	(.0074)
N	3580	3401	3222	3043	3580	3401	3222	3043
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Coefficient estimates for Fixed-Effects Least-Squares (FELS) (Panel A) and Arellano-Bond first-differenced GMM estimations of equation (1), simultaneously including separate FR indices for each fiscal rule type as explanatory variables. BBR = budget balance rules; DR = debt rules; ER = expenditure rules. *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$. *Upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.8: Different rule types

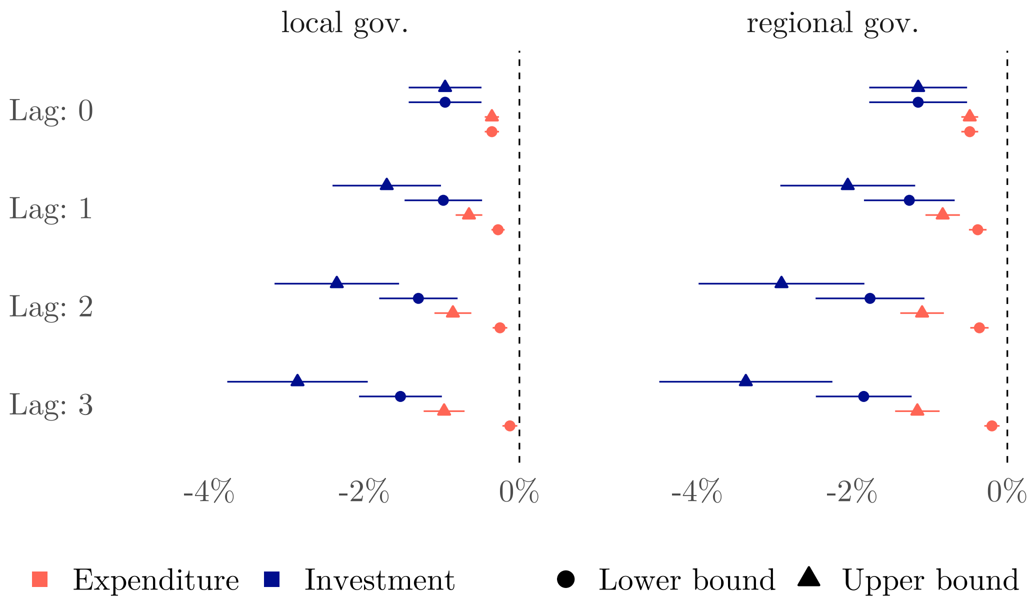
C Supplementary Material

Alternative levels of government

	Expenditure				Investment			
	h=0	h=1	h=2	h=3	h=0	h=1	h=2	h=3
				Local government				
				Lower bound				
FR^{lg}_{t-h}	$-.0036^{***}$	$-.0028^{***}$	$-.0025^{***}$	$-.0012^{**}$	$-.0096^{***}$	$-.0098^{***}$	$-.0130^{***}$	$-.0154^{***}$
	(.0005)	(.0004)	(.0005)	(.0005)	(.0024)	(.0025)	(.0026)	(.0027)
				Upper bound				
FR^{lg}_{t-h}	$-.0036^{***}$	$-.0065^{***}$	$-.0086^{***}$	$-.0097^{***}$	$-.0096^{***}$	$-.0171^{***}$	$-.0236^{***}$	$-.0287^{***}$
	(.0005)	(.0009)	(.0012)	(.0013)	(.0024)	(.0035)	(.0041)	(.0046)
				Regional government				
				Lower bound				
FR^{rg}_{t-h}	$-.0049^{***}$	$-.0038^{***}$	$-.0036^{***}$	$-.0020^{***}$	$-.0115^{***}$	$-.0127^{***}$	$-.0177^{***}$	$-.0185^{***}$
	(.0006)	(.0006)	(.0006)	(.0005)	(.0032)	(.0030)	(.0036)	(.0031)
				Upper bound				
FR^{rg}_{t-h}	$-.0049^{***}$	$-.0083^{***}$	$-.0110^{***}$	$-.0116^{***}$	$-.0115^{***}$	$-.0206^{***}$	$-.0292^{***}$	$-.0338^{***}$
	(.0006)	(.0011)	(.0014)	(.0015)	(.0032)	(.0044)	(.0054)	(.0057)
N	3759	3580	3401	3222	3759	3580	3401	3222
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Coefficient estimates for Fixed-Effects Least-Squares (FELS) estimations of equation (1). *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$. *Upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table C.1: Alternative government levels



Note: Figure displays coefficient estimates and 95% confidence intervals for different levels of government from Within estimations (lower and upper bound), see equation (1). *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$; *upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at NUTS2 level. The coefficients are scaled by 100 to simplify the interpretation. Coefficient estimates presented in table C.1.

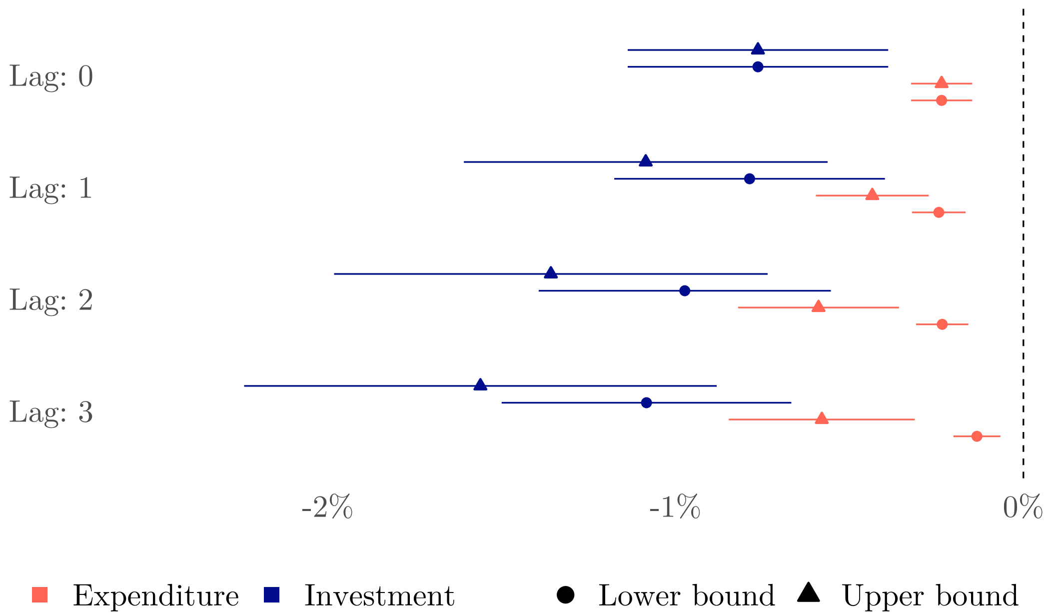
Figure C.1: Effect plot - Alternative government levels

Alternative sample

	Expenditure				Investment			
	h=0	h=1	h=2	h=3	h=0	h=1	h=2	h=3
				Lower bound				
FR_{t-h}	$-.0024^{***}$	$-.0024^{***}$	$-.0023^{***}$	$-.0013^{***}$	$-.0076^{***}$	$-.0079^{***}$	$-.0097^{***}$	$-.0108^{***}$
	(.0004)	(.0004)	(.0004)	(.0003)	(.0019)	(.0020)	(.0021)	(.0021)
				Upper bound				
FR_{t-h}	$-.0024^{***}$	$-.0043^{***}$	$-.0059^{***}$	$-.0058^{***}$	$-.0076^{***}$	$-.0109^{***}$	$-.0136^{***}$	$-.0156^{***}$
	(.0004)	(.0008)	(.0012)	(.0014)	(.0019)	(.0027)	(.0032)	(.0034)
N	3580	3401	3222	3043	3580	3401	3222	3043
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Coefficient estimates for Fixed-Effects Least-Squares (FELS) estimations of specification (1). Sample includes 239 regions in 27 countries for the period 1995 to 2018 (unbalanced). *Upper bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$. *Lower bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table C.2: Alternative sample



Note: Figure displays coefficient estimates and 95% confidence intervals for an extended sample from Within estimations (lower and upper bound), see equation (1). *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$; *upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at NUTS2 level. The coefficients are scaled by 100 to simplify the interpretation. Coefficient estimates presented in table C.2.

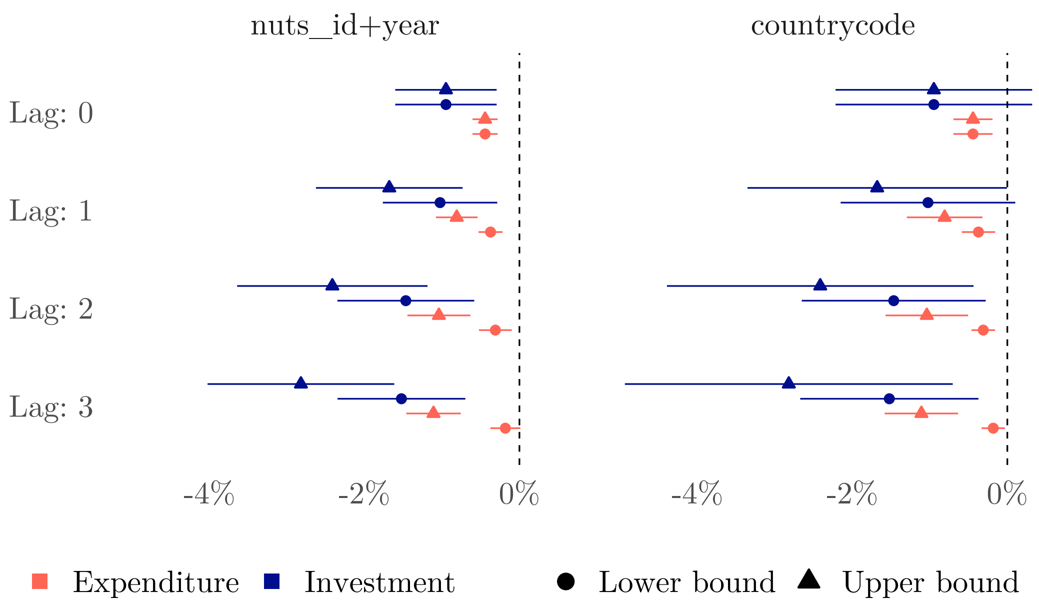
Figure C.2: Effect plot - Alternative Sample

Alternative standard errors

	Expenditure				Investment			
	h=0	h=1	h=2	h=3	h=0	h=1	h=2	h=3
	Panel A: SE clustering by NUTS2+year							
	Lower bound							
FR_{t-h}	$-.0044^{***}$	$-.0037^{***}$	$-.0031^{***}$	$-.0018^{*}$	$-.0095^{***}$	$-.0102^{**}$	$-.0147^{***}$	$-.0152^{***}$
	(.0008)	(.0008)	(.0011)	(.0010)	(.0033)	(.0037)	(.0045)	(.0042)
	Upper bound							
FR_{t-h}	$-.0044^{***}$	$-.0081^{***}$	$-.0104^{***}$	$-.0111^{***}$	$-.0095^{***}$	$-.0168^{***}$	$-.0242^{***}$	$-.0282^{***}$
	(.0008)	(.0014)	(.0021)	(.0018)	(.0033)	(.0048)	(.0062)	(.0061)
	Panel B: SE clustering by country							
	Lower bound							
FR_{t-h}	$-.0044^{***}$	$-.0037^{***}$	$-.0031^{***}$	$-.0018^{**}$	$-.0095$	$-.0102^{*}$	$-.0147^{**}$	$-.0152^{**}$
	(.0013)	(.0011)	(.0008)	(.0008)	(.0064)	(.0057)	(.0060)	(.0058)
	Upper bound							
FR_{t-h}	$-.0044^{***}$	$-.0081^{***}$	$-.0104^{***}$	$-.0111^{***}$	$-.0095$	$-.0168^{*}$	$-.0242^{**}$	$-.0282^{**}$
	(.0013)	(.0025)	(.0027)	(.0024)	(.0064)	(.0085)	(.0100)	(.0107)
N	3580	3401	3222	3043	3580	3401	3222	3043
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Coefficient estimates for Fixed-Effects Least-Squares (FELS) estimations of specification (1). *Upper bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$. *Lower bound* estimations do not include y_{rct-1} to y_{rct-h}. In Panel A, tow-way clustered standard errors (SE) at the NUTS2 + year level are reported in parentheses. In Panel B, standard errors are clustered at the country level. Macroeconomic and institutional controls as explained in section 3.3. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table C.3: Alternative standard errors



Note: Figure displays coefficient estimates and 95% confidence intervals Within estimations (lower and upper bound), see equation (1). In the left panel, twoway clustered standard errors at NUTS2 + year level included; in the right panel, standard errors clustered at the country level. *Lower bound* estimations include all y_{rct-1} to $y_{rct-(p+h)}$; *upper bound* estimations do not include y_{rct-1} to y_{rct-h}. Standard errors clustered at NUTS2 level. The coefficients are scaled by 100 to simplify the interpretation. Coefficient estimates presented in table C.3.

Figure C.3: Effect plot - Alternative Standard errors

	Expenditure											Investment										
	h=0	h=1	h=2	h=3	h=4	h=5	h=6	h=7	h=8	h=9	h=10	h=0	h=1	h=2	h=3	h=4	h=5	h=6	h=7	h=8	h=9	h=10
FR_{t-h}	−.0044***	−.0037***	−.0031***	−.0018***	−.0018***	−.0026***	−.0009*	−.0016***	−.0021***	−.0002	−.0008	−.0095***	−.0102***	−.0147***	−.0152***	−.0178***	−.0181***	−.0207***	−.0198***	−.0197***	−.0177***	−.0170***
	(.0005)	(.0005)	(.0005)	(.0005)	(.0005)	(.0005)	(.0005)	(.0006)	(.0007)	(.0009)	(.0011)	(.0025)	(.0025)	(.0028)	(.0025)	(.0025)	(.0034)	(.0030)	(.0034)	(.0038)	(.0050)	(.0061)
dep var$_{t-1}$	1.0396***	1.0124***	.9886***	.9295***	.9056***	.8652***	.8693***	.8419***	.7864***	.7683***	.7370***	.5493***	.5316***	.5125***	.5088***	.4859***	.4633***	.4466***	.4237***	.4042***	.3821***	.3570***
	(.0408)	(.0401)	(.0408)	(.0384)	(.0344)	(.0362)	(.0372)	(.0370)	(.0461)	(.0538)	(.0558)	(.0347)	(.0352)	(.0358)	(.0371)	(.0373)	(.0387)	(.0403)	(.0404)	(.0430)	(.0489)	(.0553)
dep var$_{t-2}$	−.1232***	−.1081***	−.1075***	−.0738**	−.0626*	−.0398	−.0545	−.0498	−.0361	−.0317	−.0300	−.0044	−.0138	−.0233	−.0197	−.0226	−.0278	−.0421	−.0512	−.0551	−.0563	−.0616
	(.0316)	(.0314)	(.0349)	(.0328)	(.0347)	(.0373)	(.0416)	(.0435)	(.0476)	(.0486)	(.0560)	(.0588)	(.0555)	(.0544)	(.0561)	(.0580)	(.0584)	(.0593)	(.0589)	(.0597)	(.0633)	(.0675)
dep var$_{t-3}$	−.0322	−.0345	−.0147	−.0080	−.0021	.0035	.0047	.0013	.0042	−.0210	−.0134	.0951**	.0557	.0466	.0451	.0509	.0450	.0377	.0319	.0274	.0230	.0066
	(.0219)	(.0377)	(.0435)	(.0426)	(.0415)	(.0425)	(.0463)	(.0444)	(.0500)	(.0516)	(.0576)	(.0434)	(.0464)	(.0453)	(.0450)	(.0436)	(.0436)	(.0445)	(.0440)	(.0452)	(.0456)	(.0490)
dep var$_{t-4}$		.0127	−.0492	−.0185	−.0490	−.0494	−.0553	−.0248	−.0362	−.0085	.0014		.0571	.0655	.0619	.0570	.0641	.0623	.0600	.0594	.0487	.0458
		(.0234)	(.0440)	(.0429)	(.0420)	(.0375)	(.0373)	(.0397)	(.0404)	(.0414)	(.0427)		(.0346)	(.0419)	(.0421)	(.0417)	(.0444)	(.0458)	(.0464)	(.0466)	(.0483)	(.0478)
dep var$_{t-5}$			.0655***	.0479	.0798**	.0587	.0772**	.0582	.0864***	.0768***	.0696**			−.0175	−.0240	−.0294	−.0343	−.0343	−.0401	−.0420	−.0490	−.0536
			(.0239)	(.0391)	(.0369)	(.0361)	(.0368)	(.0381)	(.0294)	(.0272)	(.0274)			(.0320)	(.0359)	(.0359)	(.0342)	(.0346)	(.0355)	(.0358)	(.0366)	(.0381)
dep var$_{t-6}$				.0211	−.0064	.0242	.0141	.0163	.0137	−.0078	−.0020				.0217	.0256	.0231	.0212	.0222	.0184	.0163	.0028
				(.0222)	(.0244)	(.0238)	(.0254)	(.0272)	(.0222)	(.0271)	(.0282)				(.0221)	(.0341)	(.0343)	(.0338)	(.0342)	(.0336)	(.0355)	(.0352)
dep var$_{t-7}$					.0198	−.0197	−.0227	−.0425	−.0447*	−.0069	−.0223					.0075	.0363	.0307	.0311	.0402	.0365	.0298
					(.0154)	(.0255)	(.0263)	(.0270)	(.0240)	(.0273)	(.0308)					(.0415)	(.0465)	(.0477)	(.0475)	(.0466)	(.0507)	(.0511)
dep var$_{t-8}$						.0295	.0416	.0499	.0346	.0254	.0493						−.0541**	−.0603*	−.0676**	−.0775**	−.0563*	−.0564
						(.0197)	(.0324)	(.0331)	(.0332)	(.0329)	(.0372)						(.0233)	(.0313)	(.0318)	(.0315)	(.0320)	(.0344)
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Within adj. R^2	.9035	.8976	.8955	.9066	.9051	.9034	.9027	.9043	.9043	.8997	.8925	.6234	.6152	.6060	.6095	.6057	.6018	.5924	.5836	.5748	.5605	.5295
P-value: $\beta_{inv} < \beta_{exp}$	–	–	–	–	–	–	–	–	–	–	–	.0234	.0050	.0000	.0000	.0000	.0000	.0000	.0000	.0000	.0003	.0047

Notes: Coefficient estimates for Fixed-Effects Least-Squares (FELS) estimations of equation (1). Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. The bottom row of the table reports p-values for a t-test with the null hypothesis that $|\beta_{exp}| \geq |\beta_{inv}|$ in the corresponding specification with lag size h. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table C.4: Baseline (lower bound, additional lags)

	Expenditure											Investment										
	h=0	h=1	h=2	h=3	h=4	h=5	h=6	h=7	h=8	h=9	h=10	h=0	h=1	h=2	h=3	h=4	h=5	h=6	h=7	h=8	h=9	h=10
FR_{t-h}	$-.0044^{***}$	$-.0081^{***}$	$-.0104^{***}$	$-.0111^{***}$	$-.0108^{***}$	$-.0108^{***}$	$-.0096^{***}$	$-.0086^{***}$	$-.0106^{***}$	$-.0112^{***}$	$-.0098^{***}$	$-.0095^{***}$	$-.0168^{***}$	$-.0242^{***}$	$-.0282^{***}$	$-.0332^{***}$	$-.0343^{***}$	$-.0362^{***}$	$-.0360^{***}$	$-.0362^{***}$	$-.0303^{***}$	$-.0280^{***}$
	(.0005)	(.0009)	(.0012)	(.0012)	(.0014)	(.0016)	(.0017)	(.0017)	(.0017)	(.0016)	(.0016)	(.0025)	(.0035)	(.0042)	(.0044)	(.0049)	(.0056)	(.0052)	(.0051)	(.0053)	(.0057)	(.0072)
dep var_{t-1}	1.0396^{***}											$.5493^{***}$										
	(.0408)											(.0347)										
dep var_{t-2}	$-.1232^{***}$	$.9159^{***}$										$-.0044$	$.2919^{***}$									
	(.0316)	(.0744)										(.0588)	(.0494)									
dep var_{t-3}	$-.0322$	$-.1578^{***}$	$.7745^{***}$									$.0951^{**}$	$.0559^{*}$	$.2006^{***}$								
	(.0219)	(.0308)	(.1024)									(.0434)	(.0320)	(.0380)								
dep var_{t-4}		.0019	$-.1716^{***}$	$.6316^{***}$									$.1052^{**}$	$.0992^{**}$	$.2050^{***}$							
		(.0278)	(.0401)	(.1038)									(.0443)	(.0405)	(.0385)							
dep var_{t-5}			$.0552^{**}$	$-.0983^{**}$	$.5715^{***}$									.0115	$-.0022$	$.1045^{***}$						
			(.0239)	(.0431)	(.0923)									(.0342)	(.0261)	(.0304)						
dep var_{t-6}				.0480	$-.1166^{***}$	$.5280^{***}$									.0253	.0181	$.0621^{*}$					
				(.0303)	(.0335)	(.0886)									(.0283)	(.0353)	(.0362)					
dep var_{t-7}					$.0653^{*}$	$-.1129^{***}$	$.4664^{***}$									.0219	.0487	$.0679^{**}$				
					(.0354)	(.0336)	(.0951)									(.0508)	(.0395)	(.0323)				
dep var_{t-8}						.0480	$-.0629^{*}$	$.3980^{***}$									$-.0434$	$-.0194$	$-.0066$			
						(.0447)	(.0355)	(.1004)									(.0346)	(.0339)	(.0503)			
F.E.(Nuts2)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
F.E.(Year)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Macroeconomic controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Institutional controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Within adj. R^2	.9035	.7897	.7008	.6629	.6440	.6281	.6201	.6239	.6399	.6664	.6651	.6234	.5046	.4705	.4601	.4533	.4620	.4678	.4737	.4798	.4774	.4585
P-value: $\beta_{inv} < \beta_{exp}$	–	–	–	–	–	–	–	–	–	–	–	.0234	.0087	.0009	.0001	.0000	.0000	.0000	.0000	.0000	.0007	.0071

Notes: Coefficient estimates for Fixed-Effects Least-Squares (FELS) estimations of equation (1). Standard errors clustered at the NUTS2-level in parentheses. Macroeconomic and institutional controls as explained in section 3.3. The bottom row of the table reports p-values for a t-test with the null hypothesis that $|\beta_{exp}| \geq |\beta_{inv}|$ in the corresponding specification with lag size h. Significance levels: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table C.5: Baseline (upper bound, additional lags)