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Do fiscal rules decrease public investment? Evidence from European panel data

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This paper studies the effects of fiscal rules on public investment. Economists argue that fiscal rules decrease public investment, as it is easier for governments to lower public investment than current expenditures. This paper presents an empirical assessment of the relationship between fiscal rules and public investment using European panel data covering the 1997–2016 period. In contrast to previous work, we focus on national fiscal rules and use the European Commission's Fiscal Rules Strength Index to measure the constraints imposed on public finances. This index captures 230 national fiscal rules and reflects the annual strength of fiscal rules in each European Union member state. In line with our expectations, we find that fiscal rules decrease public investment. We run some additional models in which the results are mixed.

Keywords: public investment, fiscal rules

JEL codes: E62, H54

1 INTRODUCTION

Over recent decades, fiscal rules have become widely used tools to maintain sound public finances. More and more countries have adopted rules regarding debt, deficit, expenditures or revenues to constrain the fiscal policies of their governments. In 1992, European Union (EU) member states signed the Maastricht Treaty establishing the well-known European 3 per cent deficit and 60 per cent debt rules. In 2001, Switzerland adopted a federal debt brake in its constitutional law limiting the structural deficit. Since 2009, unexpected tax revenues in France must be used to reduce the budget deficit. These are only a few examples of fiscal rules established in recent decades.

Although fiscal rules are found to reduce government deficits effectively (Bohn/Inman 1996; Feld/Kirchgässner 2008), it has also been argued that they provide governments with the wrong policy incentives. By focusing on numerical (annual) targets, fiscal rules may induce policy-makers to prioritise the short run over the long run. For instance, fiscal rules may undermine incentives to carry out structural reforms (Beetsma/Debrun 2007),

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they may trigger inefficient privatisation (Easterly 1999), and they may decrease public investment (Balassone/Franco 2000). This paper focuses on the negative relationship between fiscal rules and public investment.

Dur et al. (1997) argue that fiscal rules lead to lower public investment given that governments prefer high current expenditures. Without fiscal rules governments would finance high current expenditures with deficits, but when deficits are capped by fiscal rules, governments compensate the high current expenditures with lower public investment spending. Turrini (2004) argues that the relationship is more ambiguous. In addition to the negative relationship, he argues that fiscal rules can also increase public investment. If fiscal rules prevent debt accumulation today, governments have more fiscal space for public investment tomorrow.

However, empirical research on the relationship between fiscal rules and public investment provides ambiguous findings. Some studies find a negative effect (Arezki/Ismail 2013; Nerlich/Reuter 2013), others do not find an effect (European Commission 2018a), or even a positive effect (Burret/Feld 2018). Several papers study the relationship between fiscal rules and public investment based on the introduction of fiscal rules under *Maastricht*. They compare public investment in EU member states either before and after the Maastricht Treaty, or with non-EU countries. The evidence is ambiguous. While some papers found that Maastricht decreased public investment (European Commission 2003; Turrini 2004), others did not find an effect (Mehrotra/Välilä 2006), or showed that the decline of public investment began well before Maastricht (Galí/Perotti 2003).

This paper introduces a novel approach to assess the relationship between public investment and fiscal rules. Using the European Commission's Fiscal Rules Strength Index (FRSI) we assess the effect of 230 national fiscal rules in 28 EU member states. The FRSI measures the extent to which governments are constrained by fiscal rules, taking into account design features of the fiscal rules. Our panel data cover the period between 1997 and 2016 and contain 473 observations. In line with our expectation, we find that fiscal rules decrease public investment.

This paper is structured as follows. Section 2 provides a literature review on the relationship between fiscal rules and public investment. In Section 3 we outline our empirical methodology and in Section 4 we present our empirical results. Section 5 provides some additional estimations and robustness checks. We conclude in Section 6.

2 FISCAL RULES AND PUBLIC INVESTMENT

It is shown that fiscal rules are effective tools to improve public finances (Bohn/Inman 1996; Feld/Kirchgässner 2008), but their working is complex, and effectiveness cannot be taken for granted. Governments may adopt fiscal rules just to signal fiscal discipline (Debrun/Kumar 2007) or to reduce their borrowing costs (Iara/Wolff 2014). By contrast, Tóth (2019) shows that fiscal rules adopted by previous governments reduce the deficit bias effectively. In addition, fiscal rules can be designed in many ways, whereas design features affect compliance and fiscal outcomes. The literature studied the effectiveness of design features and found several trade-offs. For instance, simple rules with static threshold values may not result in the optimal fiscal policies, but smart rules lead to overly complex mechanisms (Eyraud et al. 2018), while governments are less likely to comply with complex fiscal rules (Cordes et al. 2015).

Moreover, fiscal rules are accompanied by several side effects. Their downward pressure on budget balances or expenditure levels may trigger pro-cyclical fiscal policies (Alt/Lowry 1994), discourage structural reforms (Beetsma/Debrun 2007), encourage inefficient

privatisation or sale of other public assets (Easterly 1999), hinder policies to reduce income inequality (Scholz/Levine 2001) and pressure governments to report overoptimistic fiscal figures and forecasts (Pina/Venes 2011; Frankel/Schreger 2013). This paper focuses on the negative effect on public investment.

2.1 Theoretical studies

Economists have argued that fiscal rules are responsible for the suppressed public investment levels in Europe (Blanchard/Giavazzi 2004; Barbiero/Darvas 2014). The dominant line of reasoning is that public investment is politically easier to cut than current expenditures, which makes it an easy target for fiscal consolidation (Roubini/Sachs 1989; Oxley/Martin 1991). Public investment projects can be postponed or cancelled, while current expenditures are often related to obligations and commitments made by governments in the past (Turrini 2004). Fiscal rules are expected to aggravate this problem, as they trigger fiscal consolidation on a rather permanent basis.

Theoretical models provide a more detailed assessment of the relationship between public investment and fiscal rules. Using a political economy approach, Dur et al. (1997) find that binding debt rules replace strategic debt for strategic underinvestment. In their model based on Alesina/Tabellini (1990), governments can allocate the budget over public investment and two types of public consumption. Public investment is assumed to increase future tax revenues. Governments have strong preferences for one type of public consumption and are uncertain about the preference of future governments. Without a binding debt rule, debt is used to increase preference-based public consumption, as the next government may have different preferences. However, under a binding debt rule, governments decrease public investment to finance consumption. The government does not care about lost future tax revenues, since future governments may use these for public consumption of the non-preferred type.

Balassone/Franco (2000) show that fiscal rules also put pressure on public investment under a benevolent planner. As capital goods are used over multiple generations, there is an intergenerational scheme of financing public investment. The current generation pays its share of public investment made in the past in the form of interest payments. Meanwhile, the current generation shifts part of its public investment spending to future generations by having a positive deficit level. This intergenerational scheme has positive and stable interest expenditures and deficit levels. When fiscal rules, however, force governments to reach a lower deficit level, the scheme comes under pressure at the expense of the current generation. If public investment is kept constant, current expenditures must be decreased to compensate the lower deficit. When the lower deficit level is reached, however, future generations can increase current expenditures again, as interest expenditures are also lower as a result of the lower deficit level.

2.2 Empirical research

The literature studies the relationship between public investment and fiscal rules empirically. The European Commission (2018a) studies public investment levels in the 28 EU member states between 1995 and 2016, focusing on national fiscal rules. One panel regression includes the FRSI as a proxy for institutional quality. Both the direct effect on public investment and the interaction effect with debt is considered. The idea behind the interaction effect is that the negative effect of public debt on public investment can be mitigated by institutional quality. For instance, when governments adopt multi-annual fiscal frameworks,

public investment is expected to be less sensitive to consolidation efforts. No significant direct effect of the FRSI on public investment was found. Moreover, the interaction effect was only significant for member states with low values of the FRSI.

Focusing on 27 EU member states between 1990 and 2012, Nerlich/Reuter (2013) assess how different design features of national fiscal rules affect expenditure categories, including public investment. They use dummy variables (one or zero) to indicate whether a country has any fiscal rule, the type of the fiscal rules (that is, balanced budget, debt, expenditure or revenue), and whether the fiscal rules are enshrined in law or constitution. They find that only budget balance rules decrease public investment, and especially when these rules are inscribed in law or constitution.

Burret/Feld (2018) study the (unintended) effects of fiscal rules in the 26 Swiss cantons between 1980 and 2011. A dummy variable captures whether the canton adopted fiscal rules. An initial model finds surprisingly that the fiscal rule dummy is associated with significantly higher public investment expenditures. However, as some cantons exempt public investment expenditures from the fiscal rules, Burret/Feld suggest that the positive effect may result from an evasion strategy. Governments may move funds from constrained current expenditures to unconstrained public investment. They test this hypothesis by including an interaction term measuring whether public investment is exempted. They find that fiscal rules have no effect on public investment if it is constrained. Investment only increases under fiscal rules when it is exempted.

Arezki/Ismail (2013) assess public investment in 32 oil-exporting countries between 1992 and 2009. In particular, they study how oil prices affect public investment, and how fiscal rules affect this relationship. To measure fiscal rules, they split the country sample into two groups: One group of countries with fiscal rules and one group of countries without any fiscal rule in place. Arezki/Ismail find that low oil prices decrease public investment in countries with fiscal rules. By contrast, the oil price does not affect public investment in countries without fiscal rules.

2.3 The Maastricht effect

Other empirical research that focused on the relationship between public investment and fiscal rules used the introduction of the European Economic and Monetary Union (EMU) to measure fiscal rules. EU member states agreed in the Maastricht Treaty (1992) to limit deficit and debt levels to maximum 3 and 60 per cent of GDP, respectively. The Stability and Growth Pact (SGP), as introduced in 1998, adopted these thresholds and maintained the constraints after the introduction of the euro. Generally, the literature used two approaches to measure the *Maastricht effect* on public investment, but the findings of these studies are ambiguous.

First, the European Commission (2003), Turrini (2004) and Mehrotra/Välilä (2006) assess the Maastricht effect, comparing public investment in the EU before and after the Maastricht Treaty. The European Commission (2003) and Turrini (2004) use panel data sets between 1970 and 2002, and introduce an EMU dummy for the period 1994–2002 to measure the public investment effect of the fiscal rules. Both studies find a direct and indirect effect, in opposite directions. On the one hand, the EMU dummy is associated directly with higher public investment levels. The expectation of potential improvement of their public finances may have induced governments to increase public investment levels after Maastricht (European Commission 2003). The studies find, on the other hand, that Maastricht decreased public investment through the interaction effect with deficit levels. This must be interpreted as follows. Public investment decreases when deficits

become lower. As Maastricht decreased deficits, public investment decreased as well. Both papers conclude that the overall effect of Maastricht is unclear. Mehrotra/Välilä (2006) use a similar data set and EMU dummy, but do not find a significant EMU effect on public investment.

Other papers studied the Maastricht effect on public investment by comparing EU member states with non-EU countries. Heinemann (2006) studies public investment levels in 13 EU and seven OECD countries between 1961 and 2011. He includes an EMU dummy (zero or one) for EU countries after 1991, but does not find a significant effect. Bacchiocchi et al. (2011) focus on the relationship between debt accumulation and public investment, and separate low- and high-debt OECD countries. They find that both high-debt EU countries and high-debt non-EU countries decrease public investment when their debt level increases, but no significant impact of EU membership was found. For low-debt countries, an interesting result was found, although not significant. Public investment is decreasing in debt levels only in low-debt EU countries, but not in other low-debt countries. According to the authors this might be due to the budget constraint imposed by the SGP even on low-debt member states. Dahan/Strawczynski (2010) study public investment and fiscal rules in 22 OECD countries between 1960 and 2006. They include Maastricht and SGP dummies (zero or one) for the relevant countries and years. They find that the introduction of the fiscal rules under the Maastricht Treaty and the SGP had a negative effect on public investment.

However, the weakness of using EMU dummies to measure fiscal rules is shown by Galí/Perotti (2003), who study the Maastricht effect with descriptive data. They focus on 11 EMU, three EU and five OECD countries between 1978 and 2001. Despite not using regression analyses, an interesting feature is that Galí/Perotti consider three five-year periods separately: 1978–1982, 1988–1992 and 1997–2002. Five-year averages of public investment are considered to minimise the variation created by the economic cycle and elections. They draw two important conclusions. First, they show that public investment dropped in EMU countries, but that it dropped with the same magnitude in non-EMU countries. Second, the decline started well before the Maastricht Treaty, between 1978–1982 and 1988–1992.

2.4 Contribution

Our focus on national fiscal rules and use of the FRSI provides methodological advantages compared to the existing literature. First, the FRSI is a continuous measure in contrast to the discrete (EMU) dummies used in most other studies. These studies assign score one to countries with any fiscal rule in place and score zero otherwise. However, these scores do not take into account the number of fiscal rules, the institutional strength of the fiscal rule (s) in place, or the strength of the enforcement mechanism. By contrast, the scoring mechanism of the FRSI takes these elements into account.

In addition, compared to studies focusing on Maastricht, we adopt a broader scope. As Maastricht represents a specific set of fiscal rules and a particular political environment and economic context, the results of previous studies must be treated with caution. Using the FRSI we consider 230 fiscal rules used in 28 EU member states between 1997 and 2016. Moreover, the fiscal rules under Maastricht cannot be considered in isolation. These are imposed on general government finances, which are the sum of central, state, and local governments, and social security. Therefore, the Maastricht rules are often transformed into fiscal rules and frameworks that target the individual government levels. For instance, the European Investment Bank (2017) found that 71 per cent of the municipalities in the EU consider debt ceilings as obstacles for public investment, while there is no European

fiscal rule targeting municipalities directly. Nevertheless, sub-national governments were on average responsible for two-thirds of total government investment in EU member states in 2013–2014 (European Commission 2016). The FRSI also covers 46 fiscal rules targeting local governments.

3 MODEL SPECIFICATION AND METHODOLOGY

We set up a regression model that explains public investment levels in Europe. We use panel data of the EU28 between 1997 and 2016. The model specification is:

$$\begin{aligned} GFCF_{i,t} = & \beta_0 GFCF_{i,t-1} + \beta_1 FRSI_{i,t-1} + \beta_2 NCS + \beta_3 GDP_{i,t-1} + \beta_4 interest_{i,t-1} + \\ & \beta_5 debt_{i,t-1} + \beta_6 CAPB_{i,t-1} + \beta_7 EDP_{i,t} + \\ & \beta_8 election_{i,t} + \beta_9 ideology_{i,t} + \beta_{10} fragmentation_{i,t} + \eta_i + \mu_t + \varepsilon_{i,t}. \end{aligned}$$

Our dependent variable is gross fixed capital formation of general government (*GFCF*) as a percentage of GDP. It is commonly used in the literature to measure public investment. Following De Haan et al. (1996), Galí/Perotti (2003), Bacchiocchi et al. (2011) and Burret/Feld (2018), we also run models using *GFCF* as a percentage of total government primary expenditures. This provides a robustness check and prevents endogeneity between our economic cycle control variable and the dependent variable. Moreover, using *GFCF* relative to total government primary expenditure controls for Wagner's law (De Haan et al. 1996). Wagner's Law refers to the increase of government expenditures as a percentage of GDP under persistent economic development.

Our independent variable of interest is the FRSI. This index measures the strength of fiscal rules present in a member state. The European Commission (2018b) constructed the FRSI, which scores fiscal rules taking into account five specific design features: the rule's legal base, binding character, bodies monitoring compliance, the correction mechanism, and the rule's resilience to shocks. Based on the scores and government coverage, the European Commission gave each EU member state an annual index score for the strength of its fiscal rules. These scores range from -0.93 to 3.26 , whereas negative values result from normalisation. We refer to European Commission (2018b) for a detailed explanation of the scoring mechanism. We are not the first to use the FRSI for the measurement of fiscal rules. For instance, Debrun et al. (2008) and Ayuso-i-Casals et al. (2009) use the FRSI to study the effectiveness of fiscal rules at improving budget balances.

3.1 Control variables

We include a range of control variables that affect public investment, as proposed by the literature. First, we control for the net capital stock (NCS) of the total economy. The NCS is the sum of the net values of all the fixed assets still in use (Eurostat 2013: 175). It can be described as the gross fixed capital stock minus the consumption of fixed capital (that is, depreciation). When the NCS is higher, governments may be less inclined to invest in new fixed capital.¹ Heinemann (2006) provides evidence for this proposition.

1. As public investment affects the public NCS, we would prefer to include public NCS in the models, instead of the NCS of the total economy. However, due to data limitations we are not able to control for the public NCS only.

We therefore include NCS per capita at constant prices in our models and we expect a negative regression coefficient.²

Second, we control for the economic cycle. The state of the economy is found to affect public investment spending, although there is ambiguous evidence on the direction. On the one hand, economists found that governments use public investment counter-cyclically (De Haan et al. 1996; European Commission 2003; Turrini 2004). Governments increase public investment during low economic growth to compensate lower aggregate demand and to boost growth. On the other hand, governments are found to pursue pro-cyclical public investment policies and increase public investment during upswings (Hallerberg/Strauch 2002; Galí/Perotti 2003; Mehrotra/Välilä 2006; Bacchiocchi et al. 2011). Due to higher tax revenues during upswings, interest groups will intensify their lobby for funds. This is referred to as the voracity effect (Tornell/Lane 1999).

To control for the economic cycle, we follow mainstream research and include real GDP growth (De Haan et al. 1996; Mehrotra/Välilä 2006; Bacchiocchi et al. 2011). We take various measures to accommodate the endogeneity between this measure and our first measure for public investment, which is denoted relative to GDP. We use lagged values of our GDP measure and we use instruments, as we will explain below. Moreover, our second dependent variable, public investment as a percentage of total primary expenditures, erases the endogeneity problem, as it is not denoted relative to GDP. Since previous research found evidence for both pro- and counter-cyclicality, we do not expect a specific direction for the relationship between public investment and real growth.

Third, we include the country-specific real long-term interest rate to control for the price to attract funds on financial markets. When interest rates are higher, it is more expensive for governments to borrow funds to finance public investment. The European Commission (2003) and Turrini (2004) find a negative relationship between interest rates and public investment levels. However, Heinemann (2006) does not find this interest-rate effect and Mehrotra/Välilä (2006) even find a slightly positive effect. Nevertheless, we expect a negative coefficient sign in line with the theory that governments invest less when interest rates are higher.

Fourth, we control for gross public debt. When public debt is high and governments need to consolidate, public investment is often decreased to reach budget surpluses. Turrini (2004) and Mehrotra/Välilä (2006) find evidence of this relationship. We include gross public debt as a percentage of GDP and expect a negative coefficient sign.

Fifth, we include the cyclically adjusted primary balance (CAPB) to control for fiscal austerity. Several papers find that governments cut public investment along with other expenditures during times of fiscal stringency (European Commission 2003; Turrini 2004). Based on these findings we expect negative regression coefficients under our first dependent variable. If governments decrease public investment proportionally to current expenditures, we should not find any effect on our second dependent variable. During austerity, however, governments may treat public investment differently from current expenditures. The empirical literature finds two opposing relationships. On the one hand, governments decrease public investment more proportionately than current expenditures, as they are politically easier to cut (De Haan et al. 1996; Breunig/Busemeyer 2012). This means that we should also find negative regression coefficients under our second dependent variable. On the other hand, Stančík/Välilä (2012) find that current expenditures are decreased

2. We also run a set of models including an interaction variable of the FRSI and NCS. When the NCS is lower (the need for public investment is higher), the negative effect of fiscal rules on public investment may be lower. We expected a negative interaction coefficient, but the coefficients were zero and not significant at a meaningful level.

more proportionately, but they do not provide an explanation. This latter finding suggests that we should find positive correlation coefficients under the second dependent variable.

Sixth, we control for the European rules under the SGP, as these are assumed to affect public investment levels as well (Blanchard/Giavazzi 2004). We differentiate country years based on performance under the SGP, as governments at risk of non-compliance will be more inclined to decrease public investment than compliant governments running sound public finances. We create a dummy variable for member states subject to an excessive deficit procedure (EDP). Under an EDP, member state governments receive Council of Ministers recommendations to improve public finances within a specific timeframe. The Council of Ministers can adopt sanctions if the member state concerned does not take effective action. We expect that member states subject to an EDP have lower public investment levels than other member states.

We also include some political control variables. We control for the impact of parliamentary elections. Vergne (2009) and Gupta et al. (2016) find that governments decrease public investment during election years to compensate for higher current expenditures on wages and subsidies, for example. This is in line with Rogoff (1990), who shows that governments prefer spending on visible projects during election years to signal competence and to increase re-election chances. We include a dummy variable in parliamentary election years, and we expect a negative coefficient.

In addition, we control for government ideology, limiting ourselves to the left–right dimension. The literature studies the relationship between government ideology and public investment, but finds ambiguous evidence. Van Dalen/Swank (1996) and Rodrik (1998) conclude that right-wing governments spend more on public investment compared to left-wing governments. As left-wing governments spend more on health care and social transfers, right-wing governments have more fiscal space for public investment.³ However, Gupta et al. (2016) find that left-wing governments are associated with higher public investment levels. Left-wing governments prefer higher expenditures in general and pursue higher public investment levels accordingly. De Haan et al. (1996) and Heinemann (2006) do not find an effect of government ideology on public investment.

To measure ideology we use the ParlGov database, which assigns index scores to political parties to indicate their left–right preference (Döring/Manow 2018). We calculate ideology scores for governments and use the weighted average scores of the coalition parties according to their relative parliamentary seat shares. Governments with ideology scores closer to ten are more right-wing. Based on the mixed findings in the literature, we do not expect a certain direction for the relationship between ideology and public investment.

Finally, we control for government fragmentation, as this is found to increase public expenditures. When the number of coalition parties increases, more parties aim to grab part of the budget to favour their constituencies, whereas finance ministers have more difficulties coordinating fiscal decision-making (Hallerberg/Von Hagen 1999). In this line, Gupta et al. (2016) find that the number of coalition parties increases public investment. On the other hand, De Haan et al. (1996) and Heinemann (2006) do not find an effect of the number of coalition parties on public investment. To control for fragmentation, we include the Herfindahl index, as used by Fabrizio/Mody (2006). This is the sum of the squared seat shares of all coalition partners as a percentage of the coalition's seat share, and accordingly captures to what extent power is centralised to one party. Single-party governments have a score of one and more fragmented governments have scores closer to zero. We expect a negative coefficient.

3. Van Dalen/Swank (1996) focus on infrastructure and defence investment.

All fiscal variables are lagged with one year, since most fiscal decisions are taken prior to the fiscal year (Kappeler/Välilä 2008; European Commission 2018a). This also helps to tackle the possible endogeneity between the control variables and the dependent variable. We include time-fixed effects (μ_t) to capture time-specific effects, such as the financial crisis that hit economies all over Europe in 2009.

3.2 Estimation methodology

We run dynamic panel regressions, as governments commonly base their (public investment) budgets on previous values. Including the lagged dependent variable is common practice in studies focusing on public investment (see for example De Haan et al. 1996; Heinemann 2006; Kappeler/Välilä 2008). However, ordinary least squares (OLS) regression models assume no autocorrelation of the error term, and ignoring it might lead to biased results (see for example Keele/Kelly 2005). We therefore use the generalised method of moments (GMM) estimator to account for the dynamic panel bias. The bias is small, however, when the number of periods increases, as is the case in our case (Nickell 1981). To avoid some caveats of GMM, we therefore also use the least square dummy variable estimator. We elaborate on these models in more detail below.

First, we run a set of models using the bias-corrected least square dummy variable (LSDVC) estimator as introduced by Bruno (2005). The LSDVC estimator has a smaller bias when the number of cross-sectional units is low compared to both difference and system GMM estimators (Kiviet 1995). Another reason we use the LSDVC estimator is that it allows us to include more independent variables. When we run the GMM models with all independent variables, the instruments overfit the variables, while using LSDVC estimator prevents this problem. As we have an unbalanced data set, we use Kiviet/Bun's (2001) bootstrap procedure. A limitation of the LSDVC estimator is that it is not robust to endogenous variables. To address the endogeneity between the real GDP growth and public investment as a percentage of GDP, we instrument the former with the weighted average output gap of the member state's three largest export countries, following Ayuso-i-Casals et al. (2009). We include country (η_i) and time-fixed effects (μ_t), and a country- and time-specific error term ($\varepsilon_{i,t}$).

Second, we use GMM to account for the endogeneity between the dependent variable and its lagged value. We run both one-step difference and system GMM (see Arellano/Bond 1991; Blundell/Bond 1998). The latter is found to perform better when the number of cross-sectional units is small (Blundell and Bond 1998). GMM uses lags as instruments to address the endogeneity, which in our models concerns the lagged dependent and the lagged real GDP growth. A potential problem, however, is overfitting the instruments. As a rule of thumb, the instruments should not outnumber the cross-sectional units (Bruno 2005). As we have 28 individuals and 29 variables, including time dummies, overfitting is likely when we include all variables. Following the suggestions by Roodman (2009), we limit the number of lags and collapse the instrument matrix. We use and report the Hansen J test statistic to monitor overfitting while including different numbers of lags. As we will show, changing the number of lags does not affect the results. We use robust standard errors against heteroscedasticity, and we include time-fixed effects (μ_t) and a country- and time-specific error term ($\varepsilon_{i,t}$).

Table 1 shows the data sources and summary statistics. Table 2 depicts the correlation coefficients between our dependent and independent variables. There are no problems with multicollinearity. There is only an unsurprising high correlation (0.899) between our two dependent variables. As these will not be included in the same models, the high correlation is unproblematic. We have an unbalanced panel since (older) data are missing for some member states.

Table 1 Data sources and summary statistics

Variable	Source	Mean	Std dev.	Min.	Max.
GFCF as % of GDP	AMECO	3.703	1.093	1.327	7.728
GFCF as % of primary expenditures	AMECO	8.927	2.919	3.626	17.902
FRSI	European Commission (2018b)	0.237	1.029	-0.926	3.581
Net capital stock	AMECO	65 228.110	45 648.040	6 957.593	195 527.400
Real GDP growth	AMECO	2.557	3.566	-14.814	25.557
Interest rate	AMECO	2.189	3.054	-12.351	24.402
Debt	AMECO	55.580	32.272	3.664	180.833
CAPB	AMECO	-5.214	4.118	-31.640	4.599
EDP (dummy)	-	0.361	0.481	0.000	1.000
Election (dummy)	Cruz et al. (2016)	0.257	0.437	0.000	1.000
Fragmentation	ParlGov; own calculations	0.635	0.260	0.181	1.000
Ideology	ParlGov; own calculations	5.419	1.533	1.053	8.684

Notes: $N = 28$, $T = 20$ (1997–2016), $n = 560$.

Table 2 Correlation matrix

	GFCF	GFCF/Prim.	FRSI	NCS	GDP growth	Interest rate	Debt	CAPB	EDP	Election	Fragm.	Ideology
GFCF	1.000	–	–	–	–	–	–	–	–	–	–	–
GFCF/Prim.	0.899	1.000	–	–	–	–	–	–	–	–	–	–
FRSI	-0.171	-0.189	1.000	–	–	–	–	–	–	–	–	–
NCS	-0.267	-0.427	0.145	1.000	–	–	–	–	–	–	–	–
GDP growth	0.033	0.237	-0.037	-0.123	1.000	–	–	–	–	–	–	–
Interest rate	-0.156	-0.239	-0.108	-0.047	-0.479	1.000	–	–	–	–	–	–
Debt	-0.411	-0.513	0.072	0.139	-0.316	0.324	1.000	–	–	–	–	–
CAPB	-0.072	0.014	0.352	0.266	0.250	-0.248	-0.509	1.000	–	–	–	–
EDP	-0.084	-0.156	-0.039	-0.093	-0.273	0.205	0.384	-0.336	1.000	–	–	–
Election	0.046	0.022	0.001	-0.019	0.019	0.015	0.001	-0.063	0.053	1.000	–	–
Fragm.	0.033	0.022	-0.105	-0.049	-0.026	0.137	0.179	-0.277	0.137	0.026	1.000	–
Ideology	-0.000	0.037	0.054	0.012	-0.012	-0.025	-0.054	0.080	-0.020	0.034	-0.105	1.000

Notes: NCS = net capital stock per capita; GDP growth = real GDP growth; fragm. = fragmentation.

4 EMPIRICAL RESULTS

4.1 LSDVC results

Table 3 reports the results of the LSDVC regressions. Regressions 1–5 include GFCF as a percentage of GDP as the dependent variable, whereas regressions 6–10 have GFCF as a percentage of primary expenditures as the dependent variable. The regressions vary furthermore with respect to the sets of control variables. Regressions 3–5 and 8–10 include political variables. The lagged dependent is statistically significant at the 1 per cent level in all models, which confirms the serial correlation and justifies the dynamic specification.

The estimations provide evidence that fiscal rules decrease public investment. In eight models, the coefficients of the FRSI are negative and statistically significant at the 10 per cent level. For our first dependent variable, the coefficients range between -0.09 and -0.10 , and for our second dependent variable between -0.20 and -0.22 . To illustrate the effect of fiscal rules on public investment as a percentage of GDP, consider what happens when the FRSI increases. The FRSI values range between -0.93 and 3.58 . A regression coefficient of -0.09 means that if the FRSI is increased by one, public investment drops by 0.09 per cent of GDP. This means that when a government strengthens its fiscal rules and moves from the 25th to the 75th percentile of the FRSI, public investment will drop by 0.16 percent of GDP.

The coefficients of most of our control variables are stable and have the expected direction, but not all are statistically significant. First, we find that public investment decreases in the NCS per capita, as expected. The coefficients are negative and statistically significant at the 10, 5 or 1 per cent level. The positive coefficients of our GDP measure indicate pro-cyclical public investment behaviour, but the coefficients are not significant.

The results show an interest and debt effect on public investment. In line with our expectations, we find that higher debt and interest levels decrease public investment. In all models under our first (second) dependent variable, the interest coefficient is -0.02 (-0.08). The interest effect is particularly clear under the second dependent variable, where all interest coefficients are statistically significant at the 1 per cent level. Also, the debt coefficient (-0.01) is stable and statistically significant at a meaningful level in all models. Under the first dependent variable, the debt coefficient is statistically significant at the 1 per cent level in four models. By contrast, the CAPB coefficients do not provide clear results, as they are not significant at a meaningful level.

The results related to the dummy variables are ambiguous. The EDP coefficients are negative, as expected, but none of them is statistically significant. Also the election coefficients are not statistically significant at a meaningful level. Moreover, we do not find evidence that government fragmentation or ideology affect public investment. This may indicate that these variables have no effect on public investment, or that positive and negative effects cancel each other out.

4.2 GMM results

Table 4 shows the estimation results of the GMM models. For each dependent variable we run two difference and three system GMM models, including different numbers of lags and different sets of control variables. We report the number of lags, number of instruments, and the p -value of Hansen's J test for overfitting the variables. As too many lags may overfit the instrumented variables, failing to remove the endogeneity, we choose the number of lags in accordance with Hansen's p -value (Roodman 2009). Under the null hypothesis of Hansen's J test the instruments are valid and the test should accordingly

Table 3 Estimation results: LSDVC (EU28, 1997–2016)

Dep. variable	GFCF as % of GDP					GFCF as % of primary expenditures				
	1	2	3	4	5	6	7	8	9	10
Lagged dependent	0.59*** (13.72)	0.59*** (14.03)	0.59*** (13.94)	0.59*** (10.93)	0.59*** (13.55)	0.59*** (14.35)	0.59*** (14.68)	0.59*** (14.61)	0.60*** (11.45)	0.59*** (14.72)
Lagged FRSI	-0.09* (-1.87)	-0.10* (-1.95)	-0.09* (-1.89)	-0.09* (-1.67)	-0.09 (-1.57)	-0.22* (-1.92)	-0.22* (-1.94)	-0.22* (-1.91)	-0.21* (-1.64)	-0.20 (-1.56)
Lagged NCS	-0.00** (-1.99)	-0.00** (-2.20)	-0.00** (-2.20)	-0.00** (-1.95)	-0.00** (-2.01)	-0.00** (-2.49)	-0.00*** (-2.61)	-0.00*** (-2.61)	-0.00** (-2.39)	-0.00** (-2.41)
Lagged real GDP growth	0.01 (0.54)	0.01 (0.51)	0.02 (0.55)	0.02 (0.71)	0.02 (0.66)	0.04 (0.63)	0.04 (0.61)	0.04 (0.63)	0.04 (0.78)	0.04 (0.66)
Lagged interest	-0.02** (-2.16)	-0.02** (-2.11)	-0.02** (-2.14)	-0.02* (-1.93)	-0.02** (-2.12)	-0.08*** (-3.83)	-0.08*** (-3.80)	-0.08*** (-3.79)	-0.08*** (-3.30)	-0.08*** (-3.70)
Lagged debt	-0.01*** (-3.40)	-0.01*** (-3.27)	-0.01*** (-3.27)	-0.01*** (-2.51)	-0.01*** (-3.00)	-0.02*** (-2.49)	-0.01** (-2.42)	-0.01** (-2.41)	-0.01* (-1.83)	-0.02** (-2.28)
Lagged CAPB	0.00 (0.13)	-0.00 (-0.07)	-0.00 (-0.06)	-0.00 (-0.02)	-0.00 (-0.03)	0.03 (1.51)	0.03 (1.41)	0.03 (1.41)	0.03 (1.25)	0.03 (1.28)
EDP (dummy)	-	-0.05 (-0.60)	-0.06 (-0.62)	-0.06 (-0.63)	-0.05 (-0.52)	-	-0.06 (-0.29)	-0.06 (-0.30)	-0.07 (-0.32)	-0.05 (-0.25)
Election (dummy)	-	-	0.06 (0.96)	0.06 (0.96)	0.06 (1.35)	-	-	0.06 (0.42)	0.07 (0.49)	0.07 (0.70)
Fragmentation	-	-	-	0.23 (1.15)	0.12 (0.59)	-	-	-	0.29 (0.64)	0.12 (0.26)
Ideology	-	-	-	-	-0.02 (-1.09)	-	-	-	-	-0.04 (-0.86)
Observations	473	473	473	472	468	473	473	473	472	468

Notes: *T*-statistics are in parentheses, and *, **, *** denote significance at the 10, 5 and 1 per cent level respectively. Time dummies are not shown due to space limitations.

Table 4 Estimation results: GMM (EU28, 1997–2016)

Dep. variable	GFCF as % of GDP			GFCF as % of primary expenditures		
	Difference	Difference	System	Difference	Difference	System
Lagged dependent	1.05*** (5.21)	1.01*** (5.31)	0.55*** (4.74)	0.54*** (5.11)	1.05*** (3.74)	0.51*** (4.71)
Lagged FRSI	0.02 (0.28)	0.02 (0.32)	-0.09** (-2.05)	-0.09** (-1.97)	0.03 (0.16)	-0.20* (-1.84)
Lagged NCS	-0.00 (-1.56)	-0.00* (-1.75)	-0.00** (-2.02)	-0.00* (-1.85)	-0.00** (-2.01)	-0.00*** (-2.73)
Lagged real GDP growth	0.04 (1.57)	0.04 (1.58)	0.01 (0.30)	0.00 (0.28)	0.16*** (2.67)	0.08** (2.20)
Lagged interest	0.03 (1.37)	0.03 (1.26)	-0.00 (-0.05)	-0.00 (-0.06)	0.11 (1.56)	0.01 (0.23)
Lagged debt	0.00 (0.33)	0.00 (0.23)	-0.01 (-1.44)	-0.01* (-1.83)	0.03 (1.52)	-0.02** (-2.09)
Lagged CAPB	0.05* (1.90)	0.04* (1.79)	0.00 (0.25)	— (-)	-0.02 (-0.30)	— (-0.01)
EDP (dummy)	-0.20 (-1.49)	-0.19 (-1.46)	-0.16** (-2.03)	-0.17** (-2.13)	-0.24 (-0.78)	-0.28 (-1.51)
Election (dummy)	-0.02 (-0.29)	-0.01 (-0.23)	— (-)	0.03 (0.54)	-0.10 (-0.63)	-0.02 (-0.17)
Observations	445	445	473	473	445	473
Lags	3	5	2	3	5	2
No of instruments	31	35	31	33	31	31
AR(1) (<i>p</i> -value)	0.003	0.002	0.001	0.003	0.005	0.001
AR(2) (<i>p</i> -value)	0.611	0.561	0.104	0.126	0.891	0.458
Hansen (<i>p</i> -value)	0.527	0.904	0.978	0.929	0.745	0.985
						0.864
						0.837

Notes: *T*-statistics are in parentheses, and *, **, *** denote significance at the 10, 5 and 1 per cent level respectively. Constants and time dummies are not shown due to space limitations. The AR(1) and AR(2) results confirm the validity to use GMM, as residuals are autocorrelated in the first but not in the second lag. To avoid overspecification in the GMM models, we have collapsed the matrix of instruments and restricted the set of internal instruments. The Hansen test confirms that the instruments as groups are exogenous.

not reject the null hypothesis. The columns show that altering the number of lags, and thus the number of instruments, does not change the results significantly.

Since including more variables increases the number of instruments and the likelihood of overfitting, we limit the set of control variables. Specifically, we omit the government fragmentation and ideology in the GMM models. The latter were not statistically significant under the LSDVC models and did not affect the coefficients of our variables of interest significantly. The system GMM models also exclude the CAPB, whose coefficients were zero and not significant at a meaningful level. The other results do not change after omitting these variables.

The GMM estimation results also show that fiscal rules decrease public investment. In the system GMM models, the coefficients of the FRSI are negative, stable, and statistically significant at the 5 or 10 per cent level. The coefficients of the system GMM under the first dependent variable are -0.09 , which is the same as the coefficients under the LSDVC models. Under the second dependent variable, the coefficients are -0.20 , which is similar to the coefficients under the LSDVC models. In the difference GMM models, the FRSI coefficients are not significant at a meaningful level.

Using the LSDVC results, we explained that public investment drops with 0.16 per cent of GDP when the FRSI increases from the 25th to the 75th percentile. The GMM models confirm this interpretation as the system GMM coefficients are similar to the ones under the LSDVC. Using the coefficients of the lagged dependent variable and the FRSI, we also test the long-run effect of fiscal rules on public investment. The long-run coefficient equals $\beta_1/(1 - \beta_0)$, which becomes -0.20 when we use the results of the system GMM models under the first dependent variable. A simple post-estimation test for combinations of parameter estimates finds that this long-run coefficient is significant at the 5 per cent level.

The GMM results associated with the control variables are in line with the results under the LSDVC models, although the effects are sometimes stronger or weaker. The GMM models confirm the negative effect of NCS per capita on public investment. Nine of the models find negative coefficients that are significant at the 10, 5 or 1 per cent level. This is in line with our expectation that public investment may be less prioritised by governments when the NCS is higher.

The GMM models suggest that governments implement pro-cyclical public investment policies. Under the second dependent variable, all coefficients of real GDP growth are statistically significant at the 5 or 1 per cent level. Although the coefficients under the first dependent variable are also positive, they are not significant at a meaningful level. We do not find the strong interest and debt effects as we found under the LSDVC models. The coefficients of the interest variable vary and are not statistically significant. Regarding public debt, only the system GMM models find a negative effect on public investment.

The GMM models do not provide clear results regarding the CAPB. Only the difference GMM models under the first dependent variable find positive coefficients that are statistically significant at the 10 per cent level. The EDP coefficients are negative in all models, as expected, and statistically significant at the 5 per cent level in three system GMM models. This indicates that public investment is lower during EDP years, as expected. Finally, the coefficients of the election dummies do not show a clear direction and are not significant in any of the models.

5 ADDITIONAL MODELS AND ROBUSTNESS CHECKS

5.1 Six-Pack and Fiscal Compact

The FRSI, our independent variable of interest, increased strongly in 2013 and 2014 as a result of the Six-Pack (2011) and Fiscal Compact (2012). EU member states agreed under

these international agreements to incorporate the European fiscal rules into national law. As a result, the member states adopted national fiscal rules that already applied under EU law. This affects the value of the FRSI without strengthening the constraint on fiscal policies. Therefore, we control for this in an additional model by excluding the years 2013–2016 from our data. Although the steep increase in the FRSI took place in 2013 and 2014, we also exclude 2015 and 2016 as we use lagged values of the index in both the LSDVC and GMM models. We expect to find more negative FRSI coefficients, as we dropped years in which the FRSI increased without strengthening the constraint on public finances.

Table 5 reports the results. Because of space limitations we reduced the number of models compared to our basic specification. The set of control variables under the LSDVC model is similar to columns (3) and (8) in the basic specification and under the difference GMM it remains the same. Under the system GMM we were able to add more control variables compared to the basic specification without overfitting, as the time dummies for 2013–2016 were dropped. For the GMM models we again report the number of lags, number of instruments, and the p -value of Hansen's J test.

Table 5 confirms that fiscal rules decrease public investment. Compared to our basic specification, the FRSI coefficients are lower in all six models, in line with our expectations. Especially under the difference GMM models, the coefficients changed from close to zero to -0.27 and -0.51 . Although the FRSI coefficients remain significant at the 10 or 5 per cent level in three of our models, the significance levels worsened in four models compared to the basic specification. This may be due to the decrease in the number of observations from 473 to 365. Regarding the control variables, the results are broadly similar compared to the basic specification. Again we find that public investment decreases in the NCS per capita, and that interest rates lower public investment under the LSDVC models. The debt coefficients, by contrast, are not significant at a meaningful level.

5.2 EU cohesion funds

EU cohesion funds represent a significant share of public investment in some member states. Cohesion funds are transferred from the EU treasury to national accounts to finance public investment. The funds increase public investment levels, but do not contribute to debt or deficit, and fall accordingly outside the reach of national debt and deficit rules. The empirical literature on public investment does not always account for cohesion funds or exclude or separate cohesion member states. Bacchiocchi et al. (2011) and the European Commission (2018a) ignore the impact of cohesion funds. Kappeler/Välilä (2008) exclude cohesion member states from their data set, and Mehrotra/Välilä (2006) consider cohesion member states in a separate model. This latter solution still contains biased public investment levels, as some cohesion member states received considerably more than others.

By contrast, we subtract received cohesion funds from the public investment spending and consider all member states. As a result, we include exclusively public investment paid by national governments. To measure the annual cohesion funds per member state, we use the annual average disbursement of the full programming period concerned. For 2007, for instance, we use the member state's total disbursement under programming period 2006–2013 and divide this by seven. A disbursement made in 2014 but falling under programming period 2006–2013 is attributed to 2006–2013. We use averages instead of actual disbursements as the timing of disbursements is not necessarily aligned with the timing of public investment projects. For data on cohesion funds we use the ESIF Open Data Platform (European Commission 2018c). Under this regression, we expect more negative coefficients of the FRSI, as fiscal rules do not have an effect on public investment projects financed by EU cohesion funds.

Table 5 Estimation results: controlling for the Six-Pack and Fiscal Compact (EU28, 1997–2012)

Dep. variable	GFCF as % of GDP			GFCF as % of primary expenditures		
	LSDVC	Difference GMM	System GMM	LSDVC	Difference GMM	System GMM
Lagged dependent	0.67*** (15.04)	0.65*** (6.57)	0.72*** (6.75)	0.68*** (16.69)	0.58*** (5.01)	0.73*** (8.21)
Lagged FRSI	-0.16 (-1.63)	-0.27* (-1.69)	-0.12* (-1.72)	-0.28 (-1.25)	-0.51** (-1.96)	-0.22 (-1.53)
Lagged NCS	-0.00 (-1.34)	-0.00* (-1.95)	-0.00** (-2.45)	-0.00** (-2.20)	-0.00*** (-2.92)	-0.00*** (-3.20)
Lagged real GDP growth	0.02 (0.60)	0.01 (0.56)	0.02 (1.28)	0.02 (0.28)	0.07* (1.86)	0.10** (2.30)
Lagged interest	-0.02** (-2.00)	0.00 (0.20)	0.00 (0.02)	-0.07*** (-2.67)	0.04 (0.75)	0.02 (0.52)
Lagged debt	-0.01 (-1.47)	-0.01 (-1.22)	-0.00 (-0.43)	-0.01 (-0.76)	-0.01 (-0.89)	-0.01 (-0.90)
Lagged CAPB	0.01 (0.58)	0.04* (1.94)	0.02** (2.07)	0.05 (1.54)	0.03 (0.65)	0.05** (2.26)
EDP (dummy)	-0.11 (-1.03)	-0.15 (-1.31)	-0.16* (-1.74)	-0.27 (-1.10)	-0.25 (-0.87)	-0.24 (-1.19)
Election (dummy)	0.04 (0.54)	-0.02 (-0.21)	0.01 (0.16)	0.01 (0.06)	-0.06 (-0.52)	-0.04 (-0.36)
Constant	—	—	1.33** (2.26)	—	—	3.35*** (2.72)
Observations	365	337	365	365	337	365
Lags	—	8	6	—	6	9
No of instruments	—	37	36	—	33	42
AR(1) (<i>p</i> -value)	—	0.009	0.004	—	0.009	0.002
AR(2) (<i>p</i> -value)	—	0.703	0.938	—	0.948	0.992
Hansen (<i>p</i> -value)	—	0.990	0.987	—	0.977	0.998

Notes: *T*-statistics are in parentheses, and *, **, *** denote significance at the 10, 5 and 1 per cent level respectively. Time dummies are not shown due to space limitations. The AR(1) and AR(2) results confirm the validity to use GMM, as residuals are autocorrelated in the first but not in the second lag. To avoid overspecification in the GMM models, we have collapsed the matrix of instruments and restricted the set of internal instruments. The Hansen test confirms that the instruments as groups are exogenous.

Table 6 depicts the estimation results. The LSDVC and difference GMM models are similar to the ones in the basic specification. Under the system GMM we have omitted two insignificant control variables to avoid overfitting. The system GMM model under the first dependent variable contained second-order autocorrelation and we have therefore added the second lagged dependent variable. The rest of the models are kept unchanged. We again report the number of lags, number of instruments, and Hansen's p -value to monitor the validity of our instruments.

In line with our expectation, we find slightly lower FRSI coefficients compared to the basic specification. This is an intuitive finding as fiscal rules have no impact on public investment projects financed using EU cohesion funds. The significance levels of the FRSI coefficients improve slightly compared to the basic specification while the difference GMM models still do not provide significant FRSI results. The findings regarding the control variables under the LSDVC and GMM models are broadly similar compared to the basic specification.

6 CONCLUSION

This paper builds on the literature that studies the relationship between public investment and fiscal rules. It is often argued that fiscal rules decrease public investment, as it is easier for politicians to lower public investment than current expenditures. Empirical research studied the relationship, but evidence remained ambiguous and focused mainly on the effects of the Maastricht Treaty. To measure the effect of fiscal rules on public investment, we focused on national fiscal rules, and constructed a panel regression model of the EU28 between 1997 and 2016.

In line with our expectations, we found empirical evidence that fiscal rules decrease public investment. Under the LSDVC and system GMM models, we found statistically significant FRSI coefficients of -0.09 and -0.10 . If the FRSI moves from the 25th to the 75th percentile, public investment drops by 0.16 per cent of GDP. However, under the difference GMM models, we found coefficients close to zero, which were not significant at a meaningful level. In addition, we run models to exclude the effect of the Six-Pack and Fiscal Compact on our measure of fiscal rules. As expected, the coefficients were lower in all these models. However, under the LSDVC models, the coefficients were not significant any more, but the difference GMM models improved significantly. Moreover, we ran an additional set of models in which we excluded the EU cohesion funds from the public investment levels. Under all LSDVC and system GMM models, the coefficients decreased and significance levels improved, as expected. The (insignificant) difference GMM results did not improve.

The contribution of this paper is twofold. First, we provide evidence that fiscal rules decrease public investment. We run various models to assess the relationship and conduct several robustness checks. Second, while most literature focuses on European fiscal rules under the Maastricht Treaty, this paper has a broader scope and focuses on national fiscal rules. Therefore, our findings contribute to the understanding of fiscal rules in a broader context than the EU's supranational fiscal rules. Instead of using a Maastricht dummy variable, our measure of fiscal rules is a continuous index, which also provides more robust results. Finally, compared to analyses based on the Maastricht Treaty, our analysis takes fiscal rules on all government levels into account. This is important as subnational governments account for about two-thirds of public investment (European Commission 2016).

The negative effect of fiscal rules on public investment is problematic. Although fiscal rules improve budget balances, public investment is needed for long-run economic

Table 6 Estimation results: controlling for EU cohesion funds (EU28, 1997–2016)

Dep. variable	GFCF as % of GDP			GFCF as % of primary expenditures		
	LSDVC	Difference GMM	System GMM	LSDVC	Difference GMM	System GMM
Lagged dependent	0.59*** (14.12)	0.80*** (6.13)	0.60*** (5.50)	0.59*** (14.53)	0.86*** (5.57)	0.51*** (4.38)
Second lagged dependent	—	—	—0.04 (-0.57)	—	—	—
Lagged FRSI	-0.11** (-2.07)	0.02 (0.35)	-0.10** (-2.52)	-0.24** (-2.05)	0.05 (0.33)	-0.24** (-2.28)
Lagged NCS	-0.00* (-1.76)	-0.00* (-1.84)	0.00 (0.87)	-0.00** (-2.23)	-0.00** (-2.51)	-0.00 (-1.22)
Lagged real GDP growth	0.01 (0.36)	0.04* (1.72)	0.01 (0.55)	0.03 (0.48)	0.15*** (2.89)	0.08** (2.17)
Lagged interest	-0.02** (-2.43)	0.02 (1.23)	-0.00 (-0.21)	-0.09*** (-4.02)	0.09 (1.48)	0.00 (0.01)
Lagged debt	-0.01*** (-3.09)	-0.00 (-0.38)	-0.00 (-1.43)	-0.01** (-2.35)	0.02 (1.47)	-0.01* (-1.80)
Lagged CAPB	-0.00 (-0.16)	0.04** (1.96)	—	0.02 (1.09)	-0.00 (-0.03)	—
EDP (dummy)	-0.09 (-1.02)	-0.18 (-1.53)	-0.15** (-2.00)	-0.16 (-0.76)	-0.24 (-0.93)	-0.30* (-1.69)
Election (dummy)	0.07 (1.16)	0.02 (0.42)	—	0.10 (0.68)	-0.01 (-0.06)	—
Constant	—	—	1.66** (2.46)	—	—	4.82*** (3.28)
Observations	473	445	458	473	445	473
Lags	—	4	4	—	4	3
No of instruments	—	33	35	—	33	32
AR(1) (<i>p</i> -value)	—	0.001	0.001	—	0.001	0.001
AR(2) (<i>p</i> -value)	—	0.245	0.385	—	0.774	0.315
Hansen (<i>p</i> -value)	—	0.780	0.998	—	0.986	0.968

Notes: *T*-statistics are in parentheses, and *, **, *** denote significance at the 10, 5 and 1 per cent level respectively. Time dummies are not shown due to space limitations. The AR(1) and AR(2) results confirm the validity to use GMM, as residuals are autocorrelated in the first but not in the second lag. To avoid overspecification in the GMM models, we have collapsed the matrix of instruments and restricted the set of internal instruments. The Hansen test confirms that the instruments as groups are exogenous.

development. Various economists argued therefore in favour of ‘golden rules’, which exempt public investment from the fiscal rules (Blanchard/Giavazzi 2004; Barbiero/Darvas 2014). For instance, a golden rule may imply that current expenditures must be covered by government revenues and that borrowing is only allowed to finance public investment. When public investment is exempted from the fiscal rules, the disciplining effect of fiscal rules may be maintained without causing underinvestment. Studying the 26 Swiss cantons, Burret/Feld (2018) found that public investment increases under fiscal rules that exempt public investment expenditures. Using the FRSI, we were unfortunately unable to account for golden rules or public investment clauses.

Golden rules and alternative solutions for the negative relationship are suitable topics for future research. Our analysis may also be extended by focusing on different types or specific design features of fiscal rules. Is the effect on public investment different under budget balance rules compared to expenditure rules or debt rules? Does the time horizon of fiscal rules affect the impact on public investment? Is an enforcer with substantial discretion able to mitigate the effect?

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