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Constraining Political Budget Cycles: Media Strength and Fiscal Institutions in the Enlarged EU*

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Abstract

This article revisits institutional constraints to political budget cycles (PBCs) in the enlarged European Union (EU). Based on a panel of 25 Member States, we show that governments frequently fiscally stimulate the economy prior to elections. We argue that the occurrence of PBCs in the enlarged EU can be well explained by a peculiar interaction of two prominently discussed institutional constraints: fiscal institutions and media strength. Fiscal rules only help to limit the extent of PBCs in countries where the media is relatively weak, whereas they fail to do so in countries that host a strong press. Suggesting that this may be due to the usage of creative accounting practices in weaker media environments, we conclude that a powerful press remains key to eradicating PBCs in the EU.

Keywords: enlargement; fiscal institutions; political budget cycles; press freedom

Introduction

In the summer of 2014, the Polish administration was accused of engaging in a ‘political business cycle’ – a concept that goes back to Nordhaus (1975) – by attempting to manipulate the economy prior to elections (Chapman, 2014). The incident matched a common assumption in the literature on political business cycles, namely that these cycles are merely characteristic of younger democracies (Brender and Drazen, 2005). However, the recent literature on fiscal manipulations prior to elections, so-called political budget cycles (PBCs), also finds substantial evidence of PBCs in established democracies, among them many EU Member States (Mink and de Haan, 2006; Shi and Svensson, 2006; de Haan and Klomp, 2013). The discussion has hence shifted towards analysing specific conditions under which PBCs prevail in heterogeneous country samples (de Haan and Klomp, 2013).

Two context conditions have featured prominently in this debate and are likely to matter for the occurrence of PBCs in the enlarged EU: press freedom and the strength of national fiscal rules. The absence of a free press and the consequent presence of uninformed voters have been considered key in accounting for PBCs in many developing countries and young democracies (Shi and Svensson, 2006; Vergne, 2009). But also the EU shows substantial differences in the freedom of its media outlets: the younger central and eastern European democracies only established a free press as a form of checks and balances on governmental behaviour in the 1990s, and recent developments in Hungary, but also in old EU

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Member States such as Italy, call media freedom in the EU into question. Yet, we do not know whether PBCs are contingent on the freedom of media outlets in the EU.

Likewise, fiscal rules have been found to incentivize greater budget discipline, both in election and non-election times (Debrun *et al.*, 2008; Stanova, 2012). They have been substantially upgraded in the enlarged EU in the past decade, above all in the course of the financial and economic crisis that led to a revision of the Stability and Growth Pact and fostered the establishment of stronger fiscal rules in many EU Member States.

However, fiscal rules are considered to require a certain amount of transparency and public scrutiny in order to function without incentivizing ‘creative accounting’ practices (Milesi-Ferretti, 2004; de Haan and Klomp, 2013). While media outlets are key for creating this transparency, to the best of our knowledge the study of whether the impact of fiscal rules and institutions on PBCs is contingent on the media environment in which they operate has so far been neglected. This article addresses this gap in the literature by providing an empirical investigation of how these institutional constraints jointly impact on PBCs in the enlarged EU.

Using budget surplus data from 1996 to 2012 for estimations based on two stage least squares (2SLS), bias corrected and difference generalized method of moments (DGMM) regressions of different samples, we find that governments throughout the enlarged EU fiscally stimulate the economy prior to elections. Based on our results, we echo the findings of Shi and Svensson (2006) and Vergne (2009) that a larger share of informed voters reduces PBCs. We find that this is also the case in EU countries with a high level of freedom of the press. By employing a more detailed indicator of press freedom, however, we also suggest that there is a certain threshold of press strength that – once passed – eradicates PBCs. We additionally hypothesize and find evidence for a peculiar interaction effect of different institutional constraints: fiscal institutions are apt to formally limit the extent of opportunistic fiscal behaviour in EU Member States that lack a strong press to control unsustainable government spending, while they are seemingly irrelevant in countries with a strong press. We suggest that this may be due to higher degrees of political pressure to break rules in environments with strong media and the possibility for governments to engage in ‘creative accounting’ in countries with weaker media environments.

The article is structured as follows: Section I introduces the general concept of political budget cycles and embeds the approach of the article in the wider debate on institutional constraints to PBCs. Section II presents the methodological approach and the data used. Section III presents the empirical findings. We conclude with a discussion of their implications.

I. Political Budget Cycles and their Institutional Constraints in the Enlarged EU

Political Budget Cycles

Political budget cycles were initially considered to be a relatively wide and homogeneously spread phenomenon in democracies. Traditional political business cycle theory argues that as the primary objective of any elected government is re-election, governments have an incentive to stimulate demand before elections in order to improve a country’s macro-

economic performance and, hence, an incumbent's chances of re-election (Nordhaus, 1975). This theory builds on the assumption of short-sighted and naive voters with adaptive expectations and assumes that opportunistic cycles occur in the run-up to elections independent of the political ideology of the government. Critics of this approach have developed opportunistic models in which voting behaviour is based on rational expectations, assuming that voters and politicians have asymmetric information about the politicians' competence (see e.g. Rogoff and Sibert, 1988; Rogoff, 1990). As Mink and de Haan (2006) criticize, these models initially included the almost untestable assumption that only competent politicians engage in PBCs, relying on their competence to help them bring the economy back on track after election. Instead, they argue in favour of a moral hazard model in which an incumbent's competence is only revealed to voters and themselves after election. Hence, voters make their decision conditional on the information on economic outcomes available to them before the election that they then attribute to the incumbent's competence. This incentivizes the creation of PBCs and leads to the general expectation of higher deficits before elections.¹

The empirical evidence for PBCs in different country settings, however, has not been coherent, providing little support to the assumption that PBCs are indeed a homogeneously spread phenomenon (de Haan and Klomp, 2013). In addition, as shown by Eslava (2011) in a survey of recent literature, there is also little evidence corroborating the assumption that voters generally reward high-deficit governments because they derive direct utility out of increased pre-electoral spending or because they mistake the subsequent macro-economic performance for a signal of incumbency competence. Her review rather suggests that voters are fiscal conservatives, who either reward or at least do not punish governments for fiscal restraint, in line with the views of Peltzman (1992). Yet voters' ability to punish high-deficit governments crucially depends on whether they can accurately monitor government behaviour (Eslava, 2011).

Consequently, recent research has focused on explaining the heterogeneity between countries by investigating the conditions that stimulate or prevent the occurrence of PBCs. The capacity of voters to monitor and of governments to conceal spending is usually attributed to differences in the political and institutional environment of countries; manifested, for example, in their experience with democracy, the presence of institutional checks and balances or differences in fiscal transparency (see de Haan and Klomp, 2013 for a comprehensive overview). Brender and Drazen (2005), for example, prominently argue that evidence of PBCs in larger samples of developed and developing countries is mainly driven by new democracies and transition economies that lack institutions and actors to accurately inform and enable voters to monitor public spending decisions and punish incumbents in the case of abuses. Contradictory evidence of PBCs in the democratically experienced and developed western European member states (Buti and Noord, 2004; von Hagen, 2003; Efthyvoulou, 2012), however, suggests that these differences rather impact the strength of PBCs, but not their general occurrence (de Haan and Klomp, 2013). Others have argued that specific institutional features such as access to a free press and rent-seeking opportunities (Shi and Svensson, 2006; Vergne, 2009), or fiscal

¹ These incentives are nicely formalized by the time inconsistency model originally proposed by Kydland and Prescott (1977), where the optimal policy differs before and after elections.

institutions and transparency (Alt and Lassen, 2006; Stanova, 2012) account for differences in voter information, and hence for PBCs in different country settings.

Constraining PBCs: Media Strength and Fiscal Institutions in the Enlarged EU

Two prominent conditioning factors in this respect – media strength and fiscal institutions – are likely to be highly relevant for explaining the occurrence of PBCs in the enlarged EU.

To begin with, press freedom has been considered to limit PBCs, as better informed voters are likely to punish politicians for engaging in strategic economic manipulations prior to elections, rendering PBCs a highly risky exercise that may eventually even backfire on candidates (Peltzman, 1992; see also Alt and Lassen, 2006 for a similar argument related to fiscal transparency).² In this vein, Shi and Svensson (2006) emphasize and document the importance of voters' access to media, as well as the overall freedom of the press to provide adequate information to citizens (see also Akhmedov and Zhuravskaya, 2004; Vergne, 2009). While all EU Member States host a generally free press, data by Freedom House on laws and regulations, as well as on political and economic freedoms of print and broadcast media, suggest that there is indeed substantial variation in media strength within EU Member States, both old and new (Figure A2, online Appendix). This variation is likely to matter. As research on media's effect on political outcomes in non-election times has shown, it is not only the freedom of the press, but also its *de facto* strength in terms of media pluralism and the independence of ownership, that matters to hold governments accountable (Besley and Prat, 2006). In other words: whether incumbents will be detected and punished by the electorate when attempting to engage in PBCs does not only require a media that is free to report on government abuses without fearing legal or political punishment. It also requires strong media outlets that have the human resources and economic capacity to conduct journalistic investigations and inform voters accordingly. We thus hypothesize that a strong media environment is likely to be an effective constraint to PBCs in the enlarged EU, as it increases the risk for incumbents to be detected and punished by the electorate when attempting to engage in PBCs.

However, the effect of media strength on constraining PBCs is unlikely to be linear. Rather, we assume that there is a certain ceiling-effect or threshold that – once passed – makes the media an effective watchdog for government behaviour. Once this strength has been reached, the costs for governments to engage in PBCs are considered prohibitively high and PBCs are hence effectively deterred. A similar assumption also underlies Shi and Svensson's (2006) distinction between free and unfree media environments. We adjust it to account for variation in press strength in developed countries to test whether it is indeed 'the case that even the evidence for Europe fits the hypothesis that electoral changes in fiscal aggregates concentrate in countries with less well informed voters' (Eslava, 2011, p. 650).

Unlike press freedom, which figures as an informal constraint to PBCs, fiscal rules such as the Stability and Growth Pact in the EU or national numerical fiscal rules set

² As Drazen and Eslava (2010) suggest, governments may still engage in PBCs under these conditions, but are more likely to manipulate the composition of government spending, instead of increasing the overall budget.

formal budget targets and thereby arguably limit the extent to which incumbents can engage in (pre-electoral) spending. A sizeable strand of the literature is devoted to the effect of fiscal institutions on budgets in non-election times. The general conclusion within this work is that fiscal institutions are successful in promoting fiscal discipline (Beetsma *et al.*, 2009). This is shown in particular by empirical studies in the European context, where the effect is generally robust, significant (Debrun *et al.*, 2008) and particularly relevant in times of revenue shortfalls (Wierdsma, 2008a, 2008b). While the independent effect of fiscal institutions on pre-election spending has also been well documented in the literature on PBCs (de Haan and Klomp, 2013; Stanova, 2012), it has rarely been considered as being contingent on press freedom.

The literature on fiscal institutions in non-election times, however, argues that the functioning of fiscal rules crucially depends on the transparency of a budget in order not to incentivize creative accounting practices (Milesi-Ferretti, 2004). As argued by Milesi-Ferretti (2004), creative accounting is less attractive for incumbents if budget transparency, and thus the costs of detection, are higher. Strong media outlets are key in creating this transparency by critically reporting on spending and accounting decisions and thus allowing the larger public to monitor how fiscal rules are being implemented.

In environments that host a weak press, we thus first hypothesize that strong fiscal rules are more likely to limit the degree to which PBCs occur in the reported budget. We assume incumbent governments to seek adherence to formal fiscal rules in order to signal competence to voters. At the same time, as argued above, incumbents who do not have to fear rigorous media coverage have a greater incentive to engage in opportunistic spending in order to increase their chances of re-election. In the presence of strong fiscal rules and a weak media, they can thus do so to an extent that either remains within the limits of the fiscal rule or can be concealed by creative accounting. Numerous studies document that the latter approach is a widespread reaction to deal with tight fiscal rules in the EU: governments frequently circumvent formal fiscal constraints, among other methods by using stock-flow adjustments that affect debt but leave deficits unchanged or by shifting debt or deficits to non-constrained areas (Beetsma *et al.*, 2009; Benito *et al.*, 2013; von Hagen and Wolff, 2006). In a detailed case study on infrastructure financing in Spain, for example, Benito *et al.* (2008) document how the Spanish government incorrectly accounts for publicly financed infrastructure to comply with the EU's SGP. We thus expect to see that strong fiscal institutions limit pre-electoral spending under these conditions, either because they constrain the magnitude of spending or because they incentivize creative ways of concealing PBCs in the budget.

We also hypothesize that in contexts where pressure from a strong and free press is high, fiscal rules may be largely irrelevant, but also unnecessary for constraining PBCs. First, there is little prospect of success for incumbents engaging in creative accounting practices. Governments that are confronted with a strong and free press face greater risks of being detected if they try to cover excessive pre-election spending by falsely pretending to adhere to strong fiscal rules. Second, whether or not to adhere to fiscal rules is likely to be more disputed in strong media environments – also in non-electoral times. Schick (2003, p. 27) argues that 'budget rules are political rules' and that whether they are respected ultimately depends on the will of politicians, who are – as a side effect of

Table 1: The Interplay of Institutional Constraints and Their Predicted Impact on the Occurrence of PBCs

		<i>Media</i>	
		<i>Strong</i>	<i>Weak</i>
Fiscal Institutions	<i>Strong</i>	<i>no incentive for incumbents to engage in PBCs</i> <ul style="list-style-type: none"> - formal and informal constraints high: creative accounting penalized - political pressure is high: fiscal rules less relevant 	<i>incentive for incumbents to engage in limited PBCs</i> <ul style="list-style-type: none"> - formal constraints high: creative accounting is incentivized - political pressure low: fiscal rules limit extent of PBCs
	<i>Weak</i>	<i>no incentive for incumbents to engage in PBCs</i> <ul style="list-style-type: none"> - informal constraints high, no formal constraints 	<i>incentive for incumbents to engage in full PBCs</i> <ul style="list-style-type: none"> - no formal or informal constraints - no limits to PBCs

increased public scrutiny to the budget process – increasingly exposed to political pressures from outside. In this regard, a strong and independent press may also increase pressure on politicians not to adhere to rules. More importantly, however, as argued above, a strong media environment is likely to increase the risk of being detected, and hence turns PBCs into highly risky endeavours for incumbents. It is thus likely to function as a sufficient deterrent for incumbents not to engage in fiscal manipulations prior to elections (see Table 1 for an overview of all hypotheses).

II. Data and Methods

In order to investigate to what extent we observe PBCs in the enlarged EU, and whether and how they are constrained by institutional features, our baseline equation takes the form of

$$surplus_{i,t} = \alpha_0 + \alpha_1 election_{i,t} + \alpha_2 fiscalinst_{i,t} + \alpha_3 election_{i,t} * fiscalinst_{i,t} + \alpha_4 press_{i,t} + \alpha_5 surplus_{i,t-1} + \alpha_6 growth_{i,t} + \alpha_7 debt_{i,t-1} + \alpha_i + u_{i,t}$$

where $surplus_{i,t}$ is the dependent variable defined as the budget surplus as a percentage of GDP of government i in year t , following Hallerberg *et al.*'s (2002) assessment of fiscal cycles. Following Franzese (2000), we code the $election_{i,t}$ variable in such a way that the 12 months preceding an election are coded as the share of the 12 months falling into the relevant calendar year. The variable thus displays values between 0 and 1. As controlling for the endogeneity of elections would result in a substantial loss of observations in our already limited sample, we rely on Brender and Drazen (2005), who argue that there is neither a clear theoretical argument nor empirical evidence for endogenous election dates to substantially alter the results. The fiscal institutions variable $fiscalinst_{i,t}$ is based on data provided by the European Commission's (2014) Fiscal Rule Index. The index captures the characteristics of fiscal rules based on the legal base of the rule, the room for revising objectives, the nature of the body that monitors and enforces the rule, the existence of enforcement mechanisms and the rule's

visibility in the media.³ Due to high degrees of multicollinearity with $election_{i,t}$, the inclusion of an additional interaction term of press freedom ($press_{i,t}$) with $election_{i,t}$ strongly blurs the results.⁴ We thus decided to include $press_{i,t}$ as a control variable and work with sample splits later on to investigate whether different degrees of press freedom trigger differences in PBCs and constraints by fiscal institutions. This is common practice in the literature on political budget cycles (see e.g. Klomp and De Haan, 2013; Brender and Drazen, 2005). We measure media strength with the help of Freedom House's Press Freedom indicator (Freedom House, 2014) that captures the freedom of print and broadcast media. The index's categorical assessment of free, partly free and unfree media, jointly with the amount of radios per capita, has been used in studies on PBCs previously (Shi and Svensson, 2006). However, since its 1994 edition, the index has also awarded countries more detailed scores to assess their media environment in terms of laws and regulations, political pressure, economic influences and repressive actions against media representatives and outlets (Freedom House, 2014). To control for the role of modern media, we multiply the index with the inverse of internet usage population shares as provided by the World Bank's World Development Indicators, so that greater values represent a weaker press (in line with the setup of the Freedom House index), and include regression results for the full sample as a robustness check in the online Appendix (Table A2.5). Our results remain largely unchanged in terms of both coefficient size and significance.⁵

We also control for factors that impact on the budget surplus irrespective of elections. In line with Hallerberg *et al.* (2002), we include the one-year lag of the budget surplus ($surplus_{i,t-1}$) to measure path dependence. We assume that voters in high-debt countries may eye excessive spending more critically (see Redžepagić and Llorca, 2007 and Staehr, 2008 for related arguments and empirical evidence) and control for general government consolidated gross debt ($debt_{i,t-1}$). We include the GDP growth rate ($growth_{i,t}$) to control for business cycle dynamics in the respective economy that are not related to PBCs.⁶ We also report a number of robustness checks in the online Appendix. We show that there are no systematic differences in PBCs in younger and established democracies in the enlarged EU by providing regression results for different subsamples of old and new EU Member States in Table A4.4 of the online Appendix. In order to prevent biases stemming from the choice of control variables, we also run a robustness check that uses data on the output gap instead of growth. Further robustness checks do justice to alternative explanations discussed in the literature. Based on the Mundell-Fleming Model, Hallerberg *et al.* (2002) argue that it is the exchange rate regime that

³ The fact that the European Commission includes 'fiscal rule visibility in the media' as one component in its index does not pose a problem for the inclusion of press freedom as a further explanatory variable. The Commission's questionnaire asks specifically about the degree of media coverage and the likelihood of public debate regarding non-compliance with the fiscal rule. It does not make any statement about the quality and independence of this coverage, as measured by Freedom House's press freedom indicator.

⁴ The correlation between these two variables is 0.87. Correlation of the host of explanatory variables mentioned in the literature is a challenge in general, as shown by de Haan and Klomp (2013), and prevents the inclusion of large numbers of explanatory variables in any study of political business cycles.

⁵ We do not reproduce the high/low press freedom split regressions with internet access data, as a sensible definition of a threshold is impeded by the consistent upward trend in internet access across all sampled countries.

⁶ We also tested for specific crisis effects by including a crisis dummy constructed on the basis of Laeven and Valencia's (2013) indicators for banking, currency and sovereign debt crises. This dummy variable is predominantly not statistically significant, and does not qualitatively or quantitatively affect our central results.

determines whether electoral cycles appear in monetary or fiscal policy. Hence, we include the exchange rate regime classification as put forward by Reinhart and Rogoff (2004). This classification is fine-grained enough to also control for different steps towards and the eventual accession to EMU, which has been considered to matter for PBCs (Mink and de Haan, 2006; Efthyvoulou, 2012). Likewise, we include government ideology and government type as suggested, for example, by Hallerberg and Yläoutinen (2010) using the Party Government Data Set (Woldendorp *et al.*, 2011).⁷ Finally, we control for further macro-economic and demographic variables (including inflation and old-age dependency). The inclusion of all five variables leaves our central results largely unaltered.

Our sample covers all current EU countries except for Malta, Cyprus and Croatia. Its time frame ranges from 1996 – the year in which all CEECs (central and eastern European countries) formally applied for EU membership (Cameron, 2009) – to 2012. Table A1 in the online Appendix gives an overview of variables, descriptive statistics and data sources.

Following previous studies on PBCs (see e.g. Efthyvoulou, 2012), we conduct fixed effects regressions to control for further country-level heterogeneity. As we assume that the budget surplus may affect the growth rate of a given year, we deal with this potential endogeneity of growth by running 2SLS regressions in which we use two lags of growth as instruments in the first stage regression. In order to cope with the inconsistency of LSDV (least-squares dummy variable) estimators arising in autoregressive panel data models (Nickell, 1981) and better deal with our unbalanced panels, especially for the resulting rather small sample sizes (where both the cross-sectional and time dimensions i and t are small), we also estimate our results based on bias corrected LSDV (LSDVC) regressions⁸ (Bruno, 2005; Bun and Kiviet, 2003). In addition, we report the results of a DGMM (difference generalized method of moments) estimation (Arellano and Bond, 1991) to show the validity of our 2SLS and bias correction results when we account for both the dynamic panel bias and the endogeneity of growth.

III. Empirical Results

The empirical results are shown in Tables 2 and 3, starting with the full sample in Table 2, for which we apply 2SLS fixed effects, bias corrected and DGMM estimations. We then proceed with a sample split based on a threshold for press freedom in Table 3.

Fiscal Institutions as Constraints to PBCs

Table 2 suggests a very significant electoral cycle in fiscal policy for the full sample of states from 1996 to 2012 and for EU Member States only, including the CEECs after 2004 and 2007, respectively. The highly negative and statistically significant coefficient of *election* suggests that the budget will typically worsen considerably in the run-up to

⁷ We transformed the data to fit our panel by calculating yearly averages in cases of intra-year changes in government. We used updates to the dataset as provided by Seki and Williams (2014).

⁸ The bias correction is initialized by the Arellano–Bond estimator. This method requires the assumption of strict exogeneity to hold for all variables besides the dynamic variable (surplus). Simple pooled OLS and fixed effects regressions yield very similar results (although magnitudes differ somewhat) but are not reported due to the econometric issues of these methods discussed previously.

Table 2: Results for the Full Sample

Variables	(1) 2SLS Full	(2) 2SLS EU	(3) LSDVC Full	(4) LSDVC EU	(5) DGMM Full	(6) DGMM EU
election_t	-1.12*** (0.36)	-1.32*** (0.41)	-1.12*** (0.25)	-1.31*** (0.48)	-1.43** (0.56)	-1.51** (0.68)
fiscalinst_t	0.15 (0.22)	-0.00 (0.25)	-0.05 (0.21)	-0.21 (0.26)	0.75 (0.53)	1.54 (1.15)
$\text{election}_t * \text{fiscalinst}_t$	0.97*** (0.34)	1.14*** (0.38)	1.06*** (0.23)	1.23*** (0.39)	1.14** (0.49)	1.21** (0.55)
surplus_{t-1}	0.58*** (0.04)	0.59*** (0.05)	0.69*** (0.04)	0.70*** (0.05)	0.62*** (0.06)	0.62*** (0.05)
growth_t	0.52*** (0.07)	0.54*** (0.08)	0.33*** (0.02)	0.35*** (0.03)	0.51*** (0.11)	0.51*** (0.12)
debt_{t-1}	0.02* (0.01)	0.02 (0.01)	0.02** (0.01)	0.03*** (0.01)	0.04 (0.03)	0.03 (0.04)
press_t	0.06 (0.04)	0.08 (0.05)	0.04 (0.04)	0.05 (0.04)	-0.05 (0.08)	-0.10 (0.11)
constant	-4.20*** (0.92)	-4.45*** (1.07)				
Observations	393	335	374	321	368	319
Countries	25	25	25	25	25	25
Time Periods	17	17			16	16
Instruments					19	19
AB stat.(p)					0.817	0.760
Sargan(p)					0.978	0.908
Hansen(p)					0.901	0.922

Note: Columns 1, 3, and 5 use the full range of data available from 1996 to 2012 for all current EU member states (except Malta, Cyprus and Croatia, but including CEECs before their accession to the EU), while columns 2, 4, and 6 contain only those years in which countries have been members of the EU. Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; 2SLS results take account of potential endogeneity of growth through a first stage regression using growth_{t-1} and growth_{t-2} as instruments. LSDVC results apply a bias correction to the LSDV estimator based on the consistent Arellano-Bond estimator. The bootstrap variance-covariance matrix to estimate standard errors was calculated using 20 repetitions. The Difference GMM estimation uses up to 4 lags. In the choice of instruments, press_t , debt_{t-1} , and election_t are treated as exogenous, while fiscalinst_t and $\text{election}_t * \text{fiscalinst}_t$ are treated as predetermined, but potentially endogenous. Growth_t is treated as an endogenous variable. AB stat refers to the Arellano-Bond test for second order autocorrelation in differences. When $p < 0.1$, we would assume autocorrelation, i.e. endogeneity, and would be required to switch to a higher lag order of instruments. The Sargan and Hansen test are tests for over-identifying restrictions. When $p < 0.1$, the set of instruments would not be exogenous and should be reduced. The Hansen test (unlike the Sargan test) is robust to heteroskedasticity and autocorrelation, but is weakened by instrument proliferation, hence 'perfect' scores of 1 should be treated with caution (Bruno, 2005).

an election, hence hinting at a deliberate attempt by the incumbent government to gain votes by increasing spending or reducing tax revenues. This effect is robust to all model alterations and additional controls described above. The highly positive $\text{election}_t * \text{fiscalinst}_t$ coefficient in all regression results shown in Table 2 suggests that fiscal institutions have a substantial positive effect on budget balances especially in election times. It is consistently significant below or around the 10 per cent level throughout all models. Its significance vanishes, however, if we control for government ideology, government size and the exchange rate regime in 2SLS and DGMM regressions. The estimation results of the 2SLS, bias corrected and DGMM models, however, overall strongly suggest that fiscal institutions are a powerful tool to constrain governments in the extent to which they fiscally manipulate the economy prior to elections.

Note that due to the interaction model we employ, the output tables present the size of the coefficient of *election* only if fiscal institutions are held constant at a value of 0. As suggested by Brambor *et al.* (2006), we therefore also present a marginal effect plot to show the coefficient of *election* of the DGMM estimates at different values of fiscal institutions in Figure 1. This plot reveals the size of electoral cycles at different strengths of fiscal institutions. For countries and time periods with laxer fiscal rules below a value of approximately 0.5, PBCs are statistically significant, with elections increasing the deficit by over 2 percent if fiscal rules are maximally weak. Under conditions of stronger fiscal rules, namely above a value of 0.5, negative effects on the budget are eradicated and eventually turned into net positive effects, although these are statistically insignificant within the observed range of fiscal institutions.

Interestingly, though, while fiscal institutions are effective in constraining PBCs in the enlarged EU, they seemingly fail to significantly positively affect the budget balance in ‘normal’ times. This result is in contrast to findings of large parts of the existing literature (see e.g. Beetsma *et al.*, 2009; Debrun *et al.*, 2008) and is not trivial: after all, fiscal rules and institutions have been designed to secure a healthy budget in the first place, not to prevent manipulative spending prior to elections.

While fiscal institutions seemingly effectively constrain cycles, they have trouble explaining why cycles cannot be fully eradicated, as suggested by Figure 1. One potential explanation may be, as hypothesized above, that the election effect on the budget, as well

Figure 1: Effect of Fiscal Institutions on PBCs in Full Sample.

Note: Figure 1 (based on ‘Detailed Explanation of Stata Code for Marginal Effect Plot’ provided with Brambor *et al.*, 2006) shows the marginal effect of elections for different strengths of fiscal institutions within our full sample. The coefficients and standard errors are taken from the DGMM regression in Table 2 (Column 5). The solid line indicates the size of the election coefficient at different strengths of fiscal institutions. The dashed line indicates the confidence interval associated with the respective coefficient. The bars in the back indicate the distribution of observations. The figure shows that significant political budget cycles exist in countries with weak fiscal institutions. As our indicator of fiscal institutions increases in strength (along the horizontal axis), the effect of an election on the surplus (vertical axis, left-hand side) becomes positive but insignificant.

as the constraining effect of formal fiscal institutions on these PBCs, is contingent on a weak press.

Press Freedom

The overall results for the control of *press* are, unsurprisingly, relatively inconsistent (see Table 2). As we argued above, instead of a linear effect of press freedom, it is more intuitive to assume that there may be a certain threshold that needs to be passed for countries to have a sufficiently strong press to constrain PBCs. In order to test this assumption, we divide the enlarged EU into strong versus weak press environments, which additionally allows us to investigate differences in the effects of fiscal institutions in sub-samples.⁹ Freedom House defines the threshold of free media to lie at a press freedom score of 30. We hence draw a new divide at a score of 15, assuming that countries below this threshold host a much stronger press than those above. As Table 3 shows, we observe that splitting our sample accordingly changes the results into the expected direction: PBCs only occur in the sample with a weaker press, where they are substantial and significant. In the sample with stronger press, there is no significant evidence for PBCs. At the same time, fiscal institutions constrain PBCs in the sample with a weaker press, while the coefficient of *election*fiscalinst* is negative and insignificant in the sample that hosts a stronger press.

Figures 2 and 3 provide marginal effect plots to show how the *election* coefficient changes at different values of fiscal institutions in strong versus weak press environments. They suggest that in strong press environments, coefficients are either close to zero or slightly positive for most observations in this sample and, importantly, that there are no significant PBCs independent of the type of fiscal institutions in place. In weak press environments, however, we find that fiscal institutions significantly limit PBCs, but only eradicate them at high and rarely observed levels of institutional strength. In weak press environments, the budget in an election year decreases by about 3 per cent when fiscal institutions are at their weakest, while it increases by roughly 4 per cent when the score for our fiscal institutions index is at its maximum. These results suggest that while increasing the capacity of a free press to actually hold governments accountable seems to be the most promising way to contain PBCs also in developed countries, fiscal institutions seemingly help to reduce the – formally noticeable – extent to which PBCs occur in settings where the press is relatively weak. Based on the widespread evidence of those practices in the literature and the comparably weaker monitoring capacity of the press, this may yet be driven by an increasingly pronounced usage of creative accounting practices.

Having set the threshold at a value of 15, we split our generally free press sample arbitrarily into freer and less free halves. Therefore, Figure 4 displays the DGMM regression results for the size of the election coefficient and its p-value range if we relax or tighten the threshold value that qualifies countries as having a strong or weak press.

⁹ Our full sample in Table 2 includes almost 400 observations. While splitting the sample along the press freedom threshold leads to a high press freedom sample of 98 observations, this nonetheless incorporates 26 elections (for nine countries). Furthermore, the finer nature of the election variable spanning the 12 months preceding an election (as opposed to a dummy) and resulting variance in the data (at least 52 observations in which election is > 0) gives us confidence that the small size of the panel is not a problem.

Table 3: Results for Press Freedom Sample Splits

Variables	(1) 2SLS High Press	(2) 2SLS Low Press	(3) LSDVC High Press	(4) LSDVC Low Press	(5) DGMM High Press	(6) DGMM Low Press
election _t	−0.80 (1.05)	−1.03** (0.41)	−0.41 (0.80)	−1.04** (0.42)	0.06 (0.95)	−1.44** (0.59)
fiscalinst _t	0.00 (0.47)	0.04 (0.26)	0.12 (0.36)	−0.15 (0.23)	−3.73 (3.29)	0.23 (0.66)
election _t *fiscalinst _t	−0.01 (0.85)	1.39*** (0.42)	−0.26 (0.65)	1.53*** (0.45)	−0.43 (0.49)	1.71*** (0.46)
surplus _{t-1}	0.66*** (0.09)	0.59*** (0.05)	0.72*** (0.09)	0.71*** (0.06)	0.74*** (0.15)	0.62*** (0.09)
growth _t	1.03*** (0.30)	0.46*** (0.08)	0.47*** (0.06)	0.31*** (0.03)	0.67*** (0.20)	0.46*** (0.12)
debt _{t-1}	−0.03 (0.03)	0.02** (0.01)	0.01 (0.02)	0.03** (0.01)	−0.05 (0.07)	0.05* (0.03)
constant	−0.30 (1.59)	−3.57*** (0.64)				
Observations	105	288	99	275	98	270
Countries	9	21	9	21	9	21
Time Periods	17	17			16	16
Instruments					10	10
AB stat.(p)					0.320	0.929
Sargan(p)					0.286	0.833
Hansen(p)					0.250	0.812

Note: Columns 1, 3, and 5 use only observations in which the country has a high press freedom. Columns 2, 4, and 6 include the observations with the corresponding weak press freedom. The threshold along which these samples are split is a value of 15 in Freedom House's Press Freedom indicator. Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; Difference GMM estimations use up to 2 lags, as further lags violate the over-identifying restrictions tests. For further notes, see Table 2.

The starting sample to the very left applies a highly restrictive threshold in which only countries that display a press freedom score lower than 10 are included. As we move to the right along the horizontal axis of Figure 4, we include countries with declining press freedom. As a general trend, the election effect becomes negative (shown by the size of bars) and more significant (shown by the darkness of bars), exhibiting how a weaker press is less able to constrain PBCs. More interestingly, Figure 4 additionally shows that the tighter the threshold for having a high press freedom is set towards the restrictive threshold of 10, the more positive the election coefficient gets, hence even suggesting that these contexts are marked by 'reversed PBCs'.

The highly positive election coefficients for thresholds set at a value of 10 to 14 suggest that the surplus even increases prior to elections, even though this effect is only statistically significant at a threshold of 12. This provides some evidence that in contexts of well-informed voters, politicians eye their budgets prior to elections, as sound finances may themselves become an election asset. Put differently, this may suggest that voters are indeed fiscally conservative and that their preference actually translates into pre-electoral budgets in a strong media environment in which governments run a high risk of being punished for engaging in (classic) PBCs.

Figure 2: Effect of Fiscal Institutions on PBCs in Strong Media Environments.

Note: Figure 2 shows the marginal effect of elections for different strengths of fiscal institutions within our subsample of countries with a strong media environment. The coefficients and standard errors are taken from the DGMM regression in Table 3 (Column 5). The figure shows that election effects are close to 0 and insignificant across all levels of our fiscal institutions indicator.

Figure 3: Effect of Fiscal Institutions on PBCs in Weak Media Environments.

Note: Figure 3 shows the marginal effect of elections for different strengths of fiscal institutions within our subsample of countries with a weak media environment. The coefficients and standard errors are taken from the DGMM regression in Table 3 (Column 6). The figure shows that budget deficits before elections are significant when both the media environment and fiscal institutions are weak. As our indicator of fiscal institutions strength rises, the election effect on the budget quickly becomes positive (and significant).

Figure 4: Press Strength Thresholds and PBCs.

Note: Figure 4 shows the size and significance of the election effect on the budget surplus at different press freedom thresholds. The height of the bars represents the size of the coefficient (vertical axis), while the darkness of the bars represents the statistical significance. As the threshold is set to include countries with an increasingly weaker press (moving rightward along the horizontal axis) in the sample, the election effect becomes more negative and more significant.

Controls

As to our control variables, the results of 2SLS, bias corrected and DGMM estimations provide substantial evidence for a certain path dependency of the budget, as measured by the lagged dependent variable, and its response to general economic cycles, represented by a positive and highly significant coefficient for growth. Likewise, the debt level in the previous year also significantly and positively affects current budgets.

Conclusion

This article investigated the occurrence of political budget cycles in the enlarged EU. We asked to what extent fiscal institutions and press freedom constrain governments in their attempts to fiscally stimulate the economy prior to elections. Evidence provided by estimations based on 2SLS, bias corrected and DGMM regressions of 25 new and old EU Member States from 1996 to 2012 suggests that while fiscal institutions seemingly fail to guarantee a balanced budget in non-election times, they help to reduce the extent of opportunistic fiscal policies in election years throughout the EU. Unlike previous studies, our regression results also hint at a peculiar interaction effect of fiscal institutions and media freedom in the enlarged EU. Countries with a powerful media environment do not display significant electoral cycles, whereas those with weaker, albeit still free, media witness substantial and highly significant PBCs. Fiscal institutions only help to limit the extent of PBCs in the sample with lower press freedom, whereas they fail to have a significantly positive effect on the budget in election times of highly press-free countries.

This leads us to the conclusion that fiscal institutions may help to limit PBCs in countries with a weak media environment, but this effect may be triggered by incumbents' increased usage of creative accounting practices. A strong and powerful media capable of holding governments accountable and deterring PBCs is seemingly a more accurate recipe to fully eradicate the occurrence of PBCs, also in developed countries in the EU.

These findings are highly relevant for recent developments in the enlarged EU. First, our findings call into question whether the current focus on strengthening fiscal rules is indeed an accurate recipe to improve fiscal governance in Europe, or whether this merely provides increasingly high incentives for Member States' governments to engage in creative accounting. Further research certainly has to show whether our hypothesized creative accounting effect can be substantiated. Second, we are currently witnessing the weakening of media outlets in many EU Member States. Hungary, Bulgaria and Romania, but also old Member States such as Italy and Spain, are either on the verge of losing their status of having a free press, as measured by Freedom House, or have already done so. This is not only an alarming development with regard to the overall status of democratic checks and balances in these Member States. Our results also suggest that these developments may substantially impede the improvement of fiscal governance throughout crisis-ridden Europe.

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