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**Working Paper**

## Institutional constraints to political budget cycles in the enlarged EU

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## **Working Papers**

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### **Institutional Constraints to Political Budget Cycles in the Enlarged EU**

by

**Esther Ademmer and Ferdinand Dreher**

**No. 1964** | September 2014

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## **Institutional Constraints to Political Budget Cycles in the Enlarged EU\***

Esther Ademmer & Ferdinand Dreher

### **Abstract**

This paper revisits Political Budget Cycles (PBCs) in the enlarged European Union (EU). Based on a panel of 25 current EU member states from 1996 to 2012, we show that governments frequently fiscally stimulate the economy prior to elections; a phenomenon that is seemingly not only an ‘Eastern problem’ of the EU’s new members, as has been suggested in the literature. We argue that fiscal institutions are apt to reduce the extent of opportunistic fiscal behaviour both in these younger democracies as well as in other EU member states that lack a strong press to hold governments accountable. Yet, we conclude that in order to eradicate PBCs in the enlarged EU, a powerful press remains key.

**Keywords:** Central and Eastern Europe, Enlargement, Fiscal Institutions, Political Budget Cycles, Press Freedom

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## 1. Introduction

In a recent scandal, the Polish administration has been 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. While the alleged undue economic pre-election stimulus has sparked immense protest and led to a confidence vote in the Polish parliament (EurActiv, 2014), some of the scholarly literature finds evidence for a much wider spread of Political Business Cycles throughout the EU. Initially, Political Business Cycles were considered to be merely characteristic of younger democracies and developing countries (Brender and Drazen, 2005). In this vein, Hallerberg et al. (2002) detected Political Business Cycles in Central and Eastern European Countries (CEECs) prior to their accession to the EU. They hypothesized that these new EU member states would display even stronger fiscal cycles, so-called Political Budget Cycles (PBCs) after accession, given the increasing fixing of their exchange rates to the Euro and a toothless Stability and Growth Pact (Hallerberg et al., 2002).

However, since the accession of 10 CEECs states to the EU in 2004 and 2007, respectively, both the enlarged EU and its member states have engaged in reforms to improve their economic governance. The financial and economic crisis that started in 2008 has led to a revision of the Stability and Growth Pact and fostered the establishment of strong fiscal institutions in the EU's member states; a move that has been found to limit the government's discretion over fiscal policy and hence, over creating Political Budget Cycles, especially in Eastern Europe (Stanova, 2012). Likewise, the CEECs have engaged in a thorough transformation of their domestic institutions, mostly transformed into full-fledged democracies, and established checks and balances on discretionary governmental behaviour, including a free press that has been considered key in constraining Political Budget Cycles in developing countries and young democracies (Shi and Svensson, 2006; Vergne, 2009; see also Stanova, 2012 for the effect of democratic learning in CEECs).

Yet, to the best of our knowledge, we still lack a comprehensive empirical investigation of how these new institutional constraints impact on the enlarged EU in a comparative perspective. We know relatively little about whether there is indeed an East-West divide when it comes to PBCs in the enlarged EU - as some studies have shown that PBCs are apparently also widespread in the democratically consolidated old EU member states (see e.g. Efthyvoulou, 2012; Mink and de Haan, 2006) - or whether there has been a deterioration of the CEECs' performance after accession, as hypothesized by Hallerberg et al. (2002). This is surprising as many studies have indeed been devoted to assessing potential post-accession backlashes in terms of democratic standards and comparative compliance with EU law (Levitz and Pop-Eleches, 2009; Sedelmeier, 2014, 2008), or

the CEECs' commitment to eventually entering the Economic and Monetary Union (EMU) (Epstein and Johnson, 2010; Dyson, 2007).

This paper addresses these gaps in the literature and asks two interrelated questions: first, is there a patterned occurrence of PBCs in the enlarged EU along the lines of East versus West or pre- versus post-accession times? And second, how do new institutional constraints change the occurrence of PBCs in both younger and established democracies 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 Generalised Method of Moments (GMM) regressions of different samples, we find that governments throughout the enlarged EU fiscally stimulate the economy prior to elections. Based on our results, we suggest that the divide between countries in the EU engaging in PBCs runs less between the East and the West than between countries with strong versus weak media. In this vein, we echo the findings of Shi and Svensson (2006) and Vergne (2009) that a larger share of informed voters reduces the degree of PBCs. By employing a more detailed indicator of press freedom, however, we also suggest that there is a certain threshold of press freedom that - once passed - eradicates PBCs. We additionally find evidence that fiscal institutions are apt to limit the extent of opportunistic fiscal behaviour in EU member states that lack a strong press to control unsustainable government spending. Yet, we conclude that in order to eliminate PBCs in the enlarged EU, the functioning of a powerful press remains key. The paper hence seeks to add to a more recent debate about the contextual features of PBCs (de Haan and Klomp, 2013).

The paper is structured as follows: Sections 2 and 3 discuss existing theoretical and empirical literature on PBCs and develop hypotheses for the conditions under which we expect PBCs to emerge in the enlarged EU. Section 4 explains the methodological approach and presents the data used. Section 5 presents the empirical findings. Section 6 concludes with a discussion of the implications of our findings.

## 2. Political Budget Cycles: East versus West?

For a long time, PBCs have been considered not to differ across countries (de Haan and Klomp, 2013). Traditional Political Business Cycle theory argues that the primary objective of any elected government is re-election, giving governments the incentive to stimulate demand before parliamentary elections in order to increase their 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.

Authors criticizing the idea of adaptive expectations have developed opportunistic PBC models in which voting behaviour is based on rational expectations. These models assume 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 Political Budget Cycles in order to boost macroeconomic performance and leads to the expectation of higher deficits before elections.<sup>1</sup>

More recent research has focused on the heterogeneity between countries and investigated the conditions that stimulate the occurrence of PBCs (de Haan and Klomp, 2013). PBCs have initially been assumed to occur in less developed countries (Brender and Drazen, 2005). Yet, the empirical evidence for PBCs in developing and developed states has been all but coherent (for an overview see de Haan and Klomp, 2013). Brender and Drazen (2005) argue that the PBC effect in larger samples of developed and developing countries is mainly driven by new democracies and transition economies. This is supported by evidence of the EU's new Central and Eastern member states, all of which are relatively new democracies and transitioned from planned to market economies. Stanova (2012) finds PBCs in CEECs and shows them to recede over time with increasing rounds of parliamentary elections conducted. Hallerberg et al. (2002) also detect business cycles in CEECs. Given that monitoring and sanctioning tools are weaker after accession and that several CEECs have since also joined the Economic and Monetary Union, with levels of central bank independence also rising, monetary cycles are likely to be less frequent, while fiscal cycles may be encouraged (Hallerberg et al., 2002).

Additional evidence on the occurrence of PBCs in new and especially in old member states, however, casts doubts on the existence of an East-West divide in the enlarged EU. First, Eastern European states are arguably more heterogeneous with regard to PBCs than usually assumed. Redžepagić and Llorca (2007) suggest that cycles only occur in Poland, but not in other CEECs.

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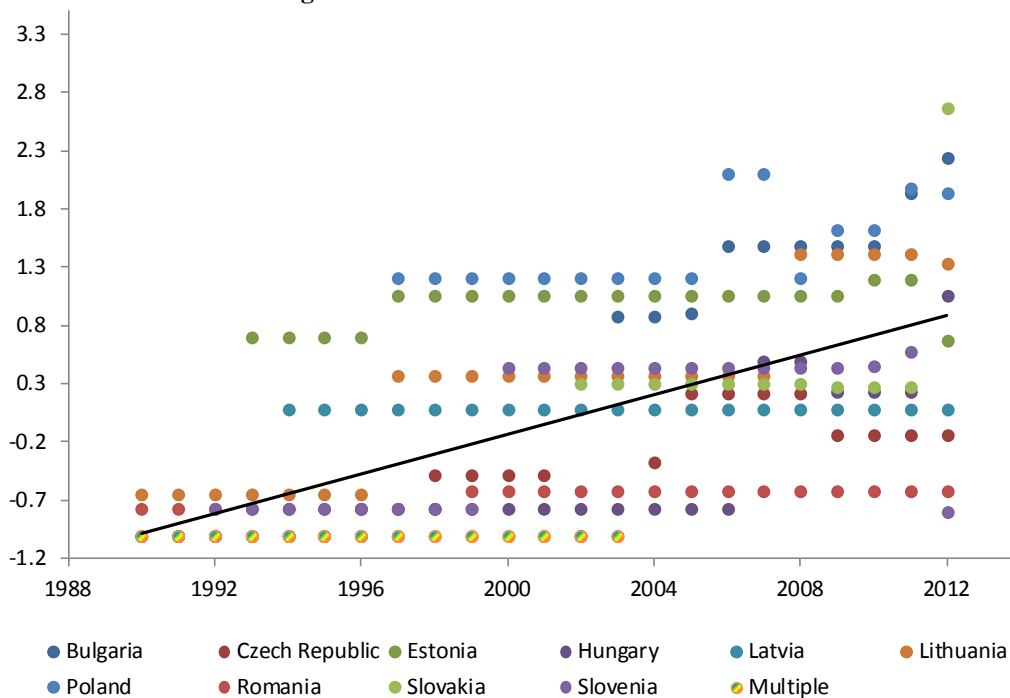
<sup>1</sup> These incentives are nicely formalized by the time inconsistency model originally proposed by Kydland und Prescott (1977) where the optimal policy differs before and after elections.

Berger et al (2007) find that accession conditionality linked to both EU and EMU membership only reduced electoral cycles in the Baltic states, but not in other CEECs. The divide is further questioned when considering more recent evidence on PBCs in the Western European member states. While Andrikopoulos et al. (2004) do not find budget cycles, other studies present evidence in favour of their occurrence in the enlarged EU, including in old member states (Buti and Noord, 2004; von Hagen, 2003; Efthyvoulou, 2012). Mink and de Haan (2006) find strong evidence for cycles in the Eurozone. They suggest that the Stability and Growth Pact has not removed Political Budget Cycles in the Euro Area, but that the process of creating EMU rather encouraged delaying PBCs until after its completion. While it seems as if the East-West divide frequently fails to accurately predict the occurrence of PBCs in the enlarged EU, to the best of our knowledge there is little systematic evidence on the CEECs' performance in comparison to old member states as well as over time; a research gap that this paper seeks to address.

### 3. Constraining Political Budget Cycles: Fiscal Institutions and Press Freedom

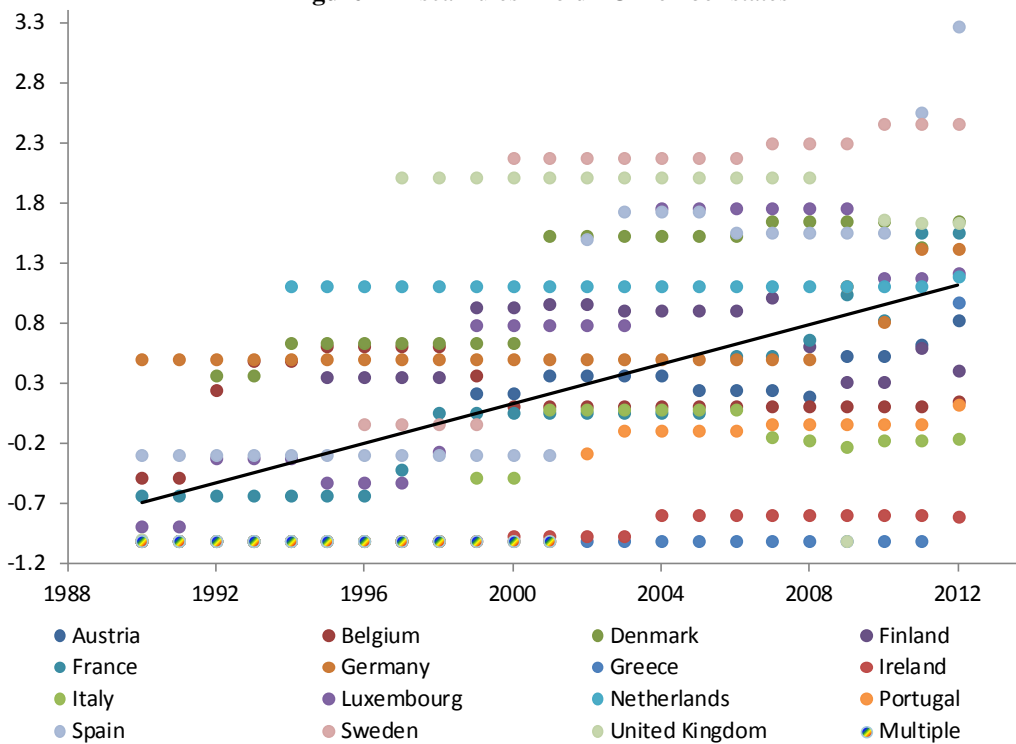
In order to better understand the conditions under which PBCs occur in heterogeneous country samples, institutional constraints to PBCs have increasingly attracted academic attention. In light of the sovereign debt crisis, this is especially the case for fiscal institutions. Driven in part by financial market worries during the crisis about the solvency of several countries, a stronger EU-wide effort to improve fiscal sustainability stimulated the development of fiscal institutions to restrict governments' ability to shape fiscal policy. Yet, the reform of the Stability and Growth Pact allows for a lot of freedom in achieving supranational goals for fiscal policy via national policies. Therefore national rules differ in type, stringency, enforceability and punishments between countries (Marneffe et al., 2010), but a general trend towards improved fiscal institutions is noticeable, both in the new member states in the East and the old ones in the West, as demonstrated by the Fiscal Rule Index compiled by the European Commission (see Figures 1 and 2). The index captures the characteristics of fiscal rules in the EU 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. In our sample, it ranges from -1 indicative of rather weak fiscal rules to roughly +3 denoting stronger and more enforceable fiscal rules.

**Figure 1** Fiscal rules in new EU member states



Source: European Commission, 2014

**Figure 2** Fiscal rules in old EU member states



Source: European Commission, 2014



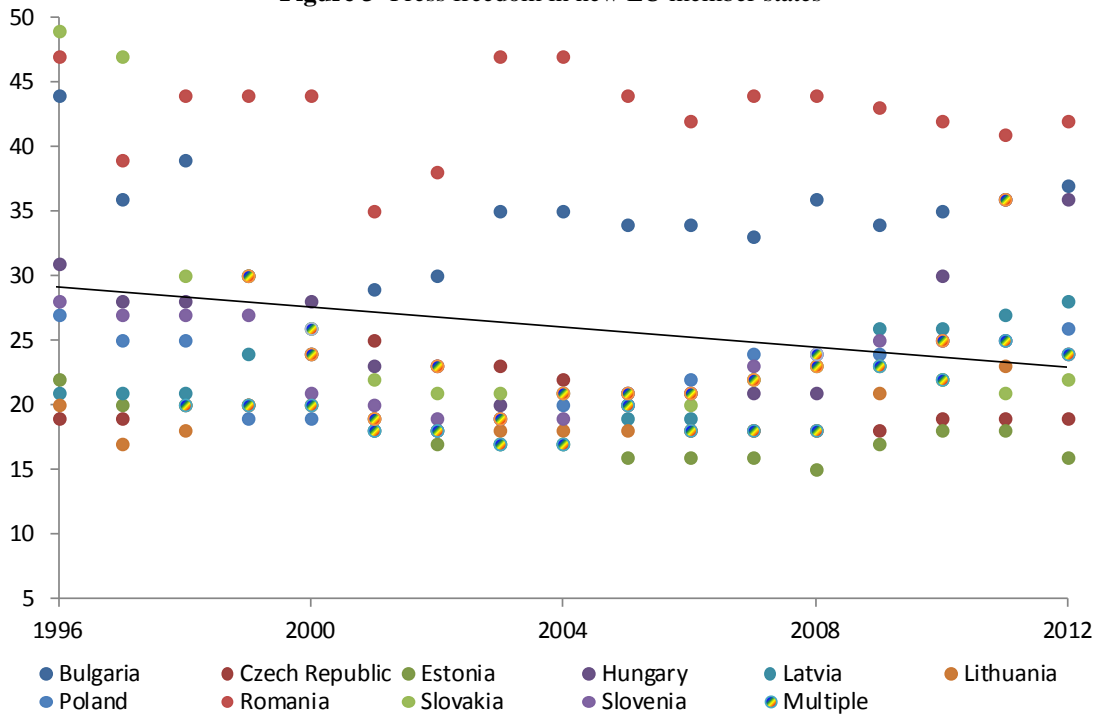
Evidence for the effect of fiscal institutions on PBCs is similarly patchy as is the existence of patterned PBCs among certain country groups: work by Berger et al (2007) suggests that fiscal institutions did not limit PBCs prior to 2002,<sup>2</sup> while Ebeke and Ölçer (2013) provide evidence for fiscal rules reducing Political Budget Cycles in low income countries from 1990 to 2010 (see also Stanova, 2012 for a similar effect in CEECs). Given the relatively recent phenomenon of strengthening fiscal institutions in the enlarged EU (see Figures 1 and 2), and the theoretical considerations outlined above, we expect to find a positive effect of fiscal institutions on the budget surplus of EU member states. We expect this effect to manifest itself especially in election years, when the government is tempted to create cycles, but fiscal institutions restrict at least the degree to which they can do so.

Closely linked to fiscal institutions is fiscal transparency, which represents when and how the public can observe budgets that may be manipulated. Shi and Svensson's (2006; see also Alt and Lassen, 2006) political agency model suggests that incumbents may try to appear competent by providing more public goods to voters (thereby increasing debt). Alt and Lassen (2006) show that under these conditions transparency reduces the incentive to create electoral cycles and thus deficits and debt levels (Alt and Lassen, 2006). In this vein, Shi and Svensson (2006) also emphasize the importance of access to media, as well as the overall freedom of the press to provide adequate information to citizens (see also Akhmedov and Zhuravskaya, 2004). While their study suggests that the absence of press freedom and hence, less informed voters may explain the occurrence of PBCs in developing countries, differences in strength of a free media may also matter in developed countries. Following the assumption of the moral hazard model, PBCs are by definition a "hidden effort [by the incumbent government to ...] use a policy instrument unobservable to the public that is a substitute for competence" (Mink and de Haan, 2006: 194). It is thus fair to assume that it is not only access to information or the general freedom of the media that matters to effectively deter PBCs, but especially the power of media outlets to investigate potential undue pre-election manipulations by the incumbent government. While all current EU members can be considered to have a free press in general, more detailed data by Freedom House on laws and regulations, as well as political and economic influence on the freedom of print and broadcast media (Figures 3 and 4) shows substantial variation among the generally free EU member states. Freedom House's indicator runs from 0 denoting a maximally free press to 100 indicative of a maximally unfree press. Countries scoring below the value of 30 are considered to have a generally free media environment.

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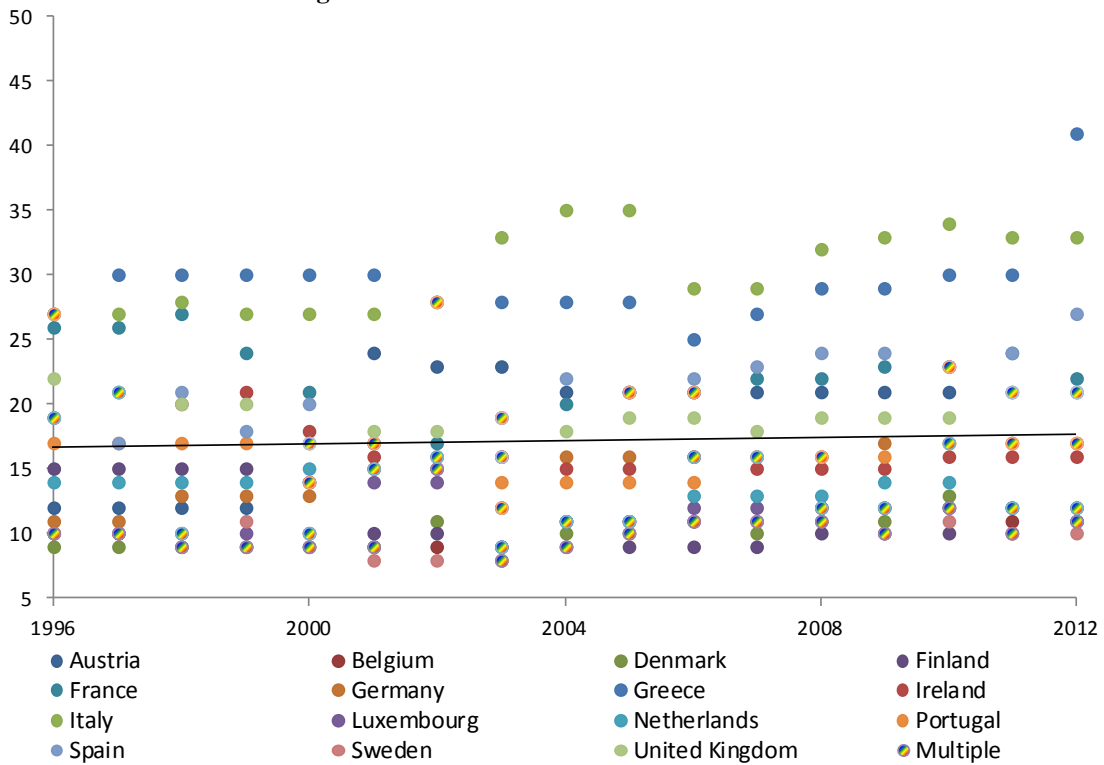
<sup>2</sup> Albuquerque (2011) shows that fiscal institutions reduce the discretion of EU governments in fiscal policies in general, irrespective of election times, for the period from 1980 to 2007.

**Figure 3** Press freedom in new EU member states



Source: Freedom House, 2014

**Figure 4** Press freedom in old EU member states



Source: Freedom House, 2014

Figures 3 and 4 show that press freedom improved over time in Eastern Europe and remained on average at the same level in Western Europe with a slight worsening after the financial crisis. However, there are crucial differences in both old and new member states, both over time and between countries. Romania and Bulgaria, and increasingly Hungary record a more constrained press. Italy, and at times Greece and France, however, do so, too, in some years of our sample period. As opposed to this, Estonia and the Czech Republic fare as least as well, if not better, than many old member states. While there is no general hypothesis as to whether a free media prefers more or less spending, it is likely that a strong media environment constrains electoral fiscal cycles, as better informed voters are likely to punish politicians for engaging in strategic economic manipulations prior to elections, rendering PBC behaviour a highly risky exercise that may eventually even backfire on candidates (Peltzman, 1992; see Alt and Lassen, 2006 for a similar argument related to fiscal transparency).<sup>3</sup>

While we assume a positive individual effect of press freedom and strong fiscal institutions on budgets in election years, they may jointly work in a more differential way. Schick (2003) highlights that a side effect of increased fiscal transparency is that politicians – regardless of their partisan orientation – are increasingly exposed to pressure from outside. They may also be affected by public opinion – prominently shaped and communicated by media outlets. Building on Schick (2003), we suggest that a massive exposure to media pressure may at times as well weaken the commitment of politicians to fiscal constraints; a commitment that is pivotal in rendering fiscal institutions effective in the first place. Hence, in contexts where pressure from a strong and free press is high, fiscal rules may be largely irrelevant, especially when considering a larger time span in which governments and public opinion may change. Yet, fiscal institutions may also not be necessary for constraining PBCs, as a strong media has sufficient power to control and deter undue voter manipulations prior to elections. In contexts where the media is weak, however, but fiscal institutions are strong, they may indeed help reduce PBCs, at least partially. As fiscal institutions are the result of political decisions, strong institutions hint at a general commitment towards fiscal discipline of decision-makers. Yet, as governments may change, as politicians still favour re-election over adherence to fiscal rules, and as they remain relatively unconstrained by a strong media environment, we still assume to see opportunistic behaviour in election times, with fiscal institutions reducing the degree of PBCs, rather than preventing their overall occurrence.

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<sup>3</sup> As research by Drazen and Eslava (2010) suggests, 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.

#### 4. Data and Methods

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

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

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. We use surplus data by the World Development Indicators of the World Bank as it has the greatest coverage in particular for the early years of the sample. The term  $election_{i,t} * fiscalinst_{i,t}$  is the interaction between elections and fiscal institutions. 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. The fiscal institutions variable  $fiscalinst_{i,t}$  is based on data provided by the European Commission's (2014) Fiscal Rule Index. Due to high degrees of multicollinearity, the inclusion of an additional interaction term of press freedom ( $press_{i,t}$ ) with  $election_{i,t}$  strongly blurs the results. 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, as hypothesized above. We measure media freedom 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 in developing and developed countries previously (Shi and Svensson, 2006). However, since its 1994 edition (1993 coverage), the Index also awards countries with more detailed scores to assess a country's media environment in terms of laws and regulations, political pressure, economic influences, and repressive actions against media representatives and outlets (Freedom House, 2014). Scores for countries that are generally considered to host free media still range from 0 indicating a country to be maximally free to 30, at the point of which countries are on the verge of falling into the partly free category.

We also include a number of controls that have been identified in the literature to impact on the budget surplus irrespective of elections. In line with Hallerberg et al. (2002) our controls include the 1 year lag of the budget surplus ( $surplus_{i,t-1}$ ) to measure path dependence. We also include the GDP

growth rate ( $growth_{i,t}$ ) to control for business cycle dynamics in the respective economy that are not related to Political Budget Cycles.<sup>4</sup> In order to check our results against potential biases stemming from the choice of control variables, we also run a robustness check that uses data on the output gap instead of growth. The variable  $debt_{i,t-1}$  captures general government consolidated gross debt. We assume that voters in high debt countries may eye excessive spending more critically (see Redžepagić and Llorca, 2007; Staehr, 2008 for related arguments and empirical evidence). We additionally implement robustness checks that do justice to alternative explanations that have been discussed in the literature. Based on the Mundell-Fleming Model, Hallerberg et al (2002) argue that it is the exchange rate regime that determines whether electoral cycles appear in monetary or fiscal policy.<sup>5</sup> We checked for the robustness of our model once we included the exchange rate regime classification as brought forward by Reinhart and Rogoff (2004). This classification also controls 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 included government ideology<sup>6</sup> and government type as suggested e.g. by Hallerberg and Yläoutinen (2010) or Efthyvoulou (2011) using the Party Government Data Set (Woldendorp et al., 2011).<sup>7</sup> All three variables remained mostly insignificant in our regressions, and left the significances of our central results as well as the size of coefficients largely unaltered. Our sample covers all current EU countries except for the Mediterranean accession countries Malta and Cyprus and the latest newcomer Croatia. The time frame of our sample ranges from 1996, which is the year in which all CEECs had formally applied for EU membership (Cameron, 2009: 5) to 2012. Table 1 gives an overview of variables, descriptive statistics and data sources for our sample.

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<sup>4</sup> We also tested for specific crisis effects by including a crisis dummy constructed on the basis of Laeven and Valencia's (2012) 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.

<sup>5</sup> This line of reasoning goes back to the fact that the exchange rate regime directly affects monetary and fiscal policy. Under a fixed exchange rate, monetary policy is bound by the duty of carrying out interventions to keep the parity fixed. For this reason, fixed exchange rates are among the proposed solutions to address the time inconsistency problem. The effect of fiscal policy also varies between fixed and flexible exchange rates since interest changes are not permitted in the former case owing to the assumption that uncovered interest rate parity holds.

<sup>6</sup> The concept of partisan cycles goes back to Hibbs (1977) who argues that left-wing governments are expected to have higher deficits due to a preference for low unemployment in the trade-off with inflation.

<sup>7</sup> 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).

**Table 1** Descriptive Statistics

Variable	Definition	Source	Obs.	Mean	Std. Dev.	Min	Max
<i>surplus</i>	Cash surplus/ deficit (as % of GDP)	World Bank, World Development Indicators	393	-2.11	3.58	-30.69	6.79
<i>election</i>	Share of 12 months prior to election	Inter-Parliamentary Union, <i>own calc.</i>	393	0.26	0.31	0.00	1.00
<i>fiscalinst</i>	Fiscal Rule Index	European Commission	393	0.43	0.96	-1.01	3.26
<i>press</i>	Freedom of the Press Index (0 to 100=least free)	Freedom House (2014)	393	19.76	7.86	8.00	47.00
<i>growth</i>	Annual growth (as % of GDP)	World Bank, World Development Indicators	393	2.63	3.72	-17.95	12.23
<i>ldebt</i>	General Government Consolidated Gross Debt (as % of GDP, lag)	European Commission, AMECO database	393	52.68	30.61	3.69	170.31
<i>govsize</i>	No. of parties in government and supporting parties	Woldendorp et al. 2011/ Seki and Williams 2014, Party Government Data Set	348	2.70	1.42	1.00	9.00
<i>govideo</i>	'Ideological complexion of government and parliament' (1=right to 5=left)	Woldendorp et al. 2011/ Seki and Williams 2014, Party Government Data Set	344	3.05	0.79	1.71	5.00
<i>exrate</i>	Exchange rate regime (1=no separate legal tender to 14=freely falling)	Reinhart and Rogoff (2004)	393	4.46	4.21	1.00	13.00
<i>outputgap</i>	Output gap	European Commission, AMECO database	384	0.27	3.18	-12.25	12.39

Following previous studies on Political Budget Cycles (see e.g. Efthyvoulou, 2012), we initially conduct fixed effects regression to control for further country level heterogeneity beyond the explanatory variables and controls. As the growth rate of a given year also directly affects the budget surplus due to e.g. tax increases, we deal with this potential endogeneity of growth by running two stage least squares regressions (2SLS) in which we use two lags of growth as instruments in the first stage regression. As we seek to investigate both whether and where PBCs prevail in the enlarged EU and whether they are constrained by fiscal institutions or a strong press, we work with sample splits to derive conditional coefficients (cf. Hallerberg et al., 2002). In order to cope with the inconsistency of least-squares dummy variable (LSDV) 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 correction LSDVC regressions<sup>8</sup> (Bruno, 2005; Bun and Kiviet, 2003). In addition, we report the results of a Difference

<sup>8</sup> The bias correction is initialised by the Arellano-Bond estimator. This method requires the assumption of strict exogeneity to hold for all variables besides the dynamic variable (surplus).

Generalised Method of Moments (GMM) estimation (Arellano and Bond, 1991)<sup>9</sup> 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.

## 5. Empirical Results

The empirical results are shown in Tables 2 to 4, starting with the full sample, for which we apply 2SLS fixed effects, bias corrected and GMM estimations. We then proceed with sample splits along the East-West divide in Table 3 and degrees of press freedom in Table 4, respectively.

### 5.1 Political Budget Cycles in the Enlarged EU

Table 2 shows 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 significant coefficient of *election* suggests that the budget will typically worsen considerably in the run-up to 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.

Table 3 reveals strong and significant cycles in the CEECs after their accession to the EU (Columns 2, 5, 8). Yet, the CEECs seem to be in good company: old EU member states also show substantial and significant PBCs (Columns 3, 6, 9, Table 3). In terms of size, there seems to be a larger ‘Eastern Problem’ if we consider the result of the 2SLS and bias corrected regressions (Columns 2, 3, 5, 6, Table 3), but the DGMM estimates (Columns 8, 9, Table 3) record the opposite, showing that PBCs in the West exceed the Eastern ones. This evidence supports the claim that the East-West divide fails to accurately predict the occurrence of PBCs in the enlarged EU. These results are largely robust to alternative controls and estimation techniques, especially for CEE member states. PBCs in Western member states slightly differ in size and significance, if alternative controls are added into the DGMM model.

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<sup>9</sup> Note that the Difference GMM regression only uses data from 1997-2012

**Table 2** Results for the full sample and the sample including EU member states only

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS Full	2SLS EU	LSDVC Full	LSDVC EU	DGMM Full	DGMM EU
election <sub>t</sub>	-1.117*** (0.356)	-1.323*** (0.412)	-1.120*** (0.258)	-1.305*** (0.482)	-1.435** (0.559)	-1.506** (0.677)
fiscalinst <sub>t</sub>	0.151 (0.216)	-0.005 (0.255)	-0.051 (0.211)	-0.214 (0.264)	0.756 (0.531)	1.546 (1.153)
election <sub>t</sub> *fiscalinst <sub>t</sub>	0.967*** (0.338)	1.141*** (0.379)	1.059*** (0.237)	1.230*** (0.384)	1.139** (0.487)	1.204** (0.547)
surplus <sub>t-1</sub>	0.582*** (0.041)	0.585*** (0.047)	0.691*** (0.040)	0.702*** (0.049)	0.623*** (0.059)	0.619*** (0.053)
growth <sub>t</sub>	0.515*** (0.072)	0.540*** (0.078)	0.331*** (0.025)	0.345*** (0.029)	0.508*** (0.113)	0.510*** (0.124)
debt <sub>t-1</sub>	0.018* (0.009)	0.020 (0.012)	0.022** (0.009)	0.027*** (0.009)	0.042 (0.028)	0.028 (0.041)
press <sub>t</sub>	0.058 (0.041)	0.077 (0.051)	0.042 (0.042)	0.050 (0.038)	-0.046 (0.077)	-0.098 (0.112)
constant	-4.196*** (0.924)	-4.446*** (1.068)				
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)					0.816	0.759
Sargan(p)					0.978	0.908
Hansen(p)					0.901	0.923

*Note:* 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<sub>t-1</sub> and growth<sub>t-2</sub> as instruments. LSDVC results apply a bias correction to the LSDV 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 and including lag 4. In the choice of instruments, press<sub>t</sub>, debt<sub>t-1</sub>, and election<sub>t</sub> are treated as exogenous, while fiscalinst<sub>t</sub> and election<sub>t</sub>\*fiscalinst<sub>t</sub> are treated as predetermined, but potentially endogenous. Growth<sub>t</sub> 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 of 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).



**Table 3** Results for individual samples of old and new EU member states

Variables	(1) 2SLS CEEC	(2) 2SLS CEEC-EU	(3) 2SLS West	(4) LSDVC CEEC	(5) LSDVC CEEC-EU	(6) LSDVC West	(7) DGMM CEEC	(8) DGMM CEEC-EU	(9) DGMM West
election <sub>t</sub>	-0.774* (0.433)	-1.577** (0.618)	-1.096** (0.530)	-0.723* (0.378)	-1.438*** (0.537)	-1.284*** (0.340)	-1.134*** (0.245)	-1.445** (0.697)	-1.800** (0.870)
fiscalinst <sub>t</sub>	0.401 (0.290)	-0.273 (0.465)	0.401 (0.317)	0.304 (0.212)	-0.325 (0.522)	-0.065 (0.270)	0.704 (0.745)	1.982 (1.791)	-0.597 (0.777)
election <sub>t</sub> *fiscalinst <sub>t</sub>	0.697 (0.482)	1.631** (0.638)	0.985** (0.466)	0.792* (0.411)	1.700*** (0.510)	1.118*** (0.378)	1.042*** (0.230)	1.189 (0.740)	1.534** (0.672)
surplus <sub>t-1</sub>	0.500*** (0.067)	0.552*** (0.091)	0.542*** (0.057)	0.590*** (0.072)	0.691*** (0.088)	0.680*** (0.047)	0.634*** (0.203)	0.587** (0.233)	0.811*** (0.105)
growth <sub>t</sub>	0.316*** (0.071)	0.275*** (0.078)	0.953*** (0.122)	0.230*** (0.027)	0.220*** (0.032)	0.508*** (0.047)	0.299*** (0.093)	0.341*** (0.095)	0.910*** (0.144)
debt <sub>t-1</sub>	0.031*** (0.012)	0.093*** (0.033)	0.016 (0.014)	0.033*** (0.011)	0.113*** (0.032)	0.021*** (0.007)	0.075** (0.035)	0.036 (0.105)	0.055 (0.041)
press <sub>t</sub>	0.078* (0.045)	0.018 (0.092)	0.019 (0.066)	0.065* (0.036)	-0.024 (0.086)	0.003 (0.078)	0.100*** (0.032)	0.169 (0.109)	-0.144 (0.135)
constant	-5.218*** (1.242)	-5.137*** (1.926)	-4.161*** (1.338)						
Observations	142	84	251	137	76	237	132	83	236
Countries	10	10	15	10	10	15	10	10	15
Time Periods	17	9	17				16	9	16
Instruments							11	11	11
AB stat.(p)							0.695	0.440	0.573
Sargan(p)							0.890	0.796	0.182
Hansen(p)							0.897	0.312	0.653

Note: Standard errors in parentheses \*\*\* p<0.01, \*\* p<0.05, \* p<0.1; Difference GMM estimations uses up to and including lag 2, as further lags violate the over-identifying restrictions tests.

There are also substantial differences when comparing the CEEC sample that includes only the post-accession phase (Columns 2, 5, 8, Table 3) with the entire CEEC sample (Columns 1, 4, 7, Table 3).<sup>10</sup> In line with the post-accession hypothesis, the CEEC post-accession sample displays a larger negative *election* coefficient than the sample that also includes pre-membership periods. While the different samples cannot strictly be compared in sheer quantitative terms given the differences in observations, the bias of a low number of observations rather favours the post-accession backlash interpretation. This effect is also robust to most alternative estimation techniques and controls.<sup>11</sup> However, this evidence still does not support the finding of an Eastern problem, as the CEECs' seemingly deteriorated performance after accession does not go beyond the 'normal' level of pre-election manipulations in old member states. Taken together, this suggests that while PBCs prevail in the enlarged EU, they do not run along the East-West divide.

### 5.2 Fiscal Institutions as Constraints to PBCs

One explanation to account for the lack of a clearly identifiable 'Eastern PBC problem' after accession may be the strengthening of fiscal institutions. The highly positive  $election_{i,t} * fiscalinst_{i,t}$  coefficient in all regression results shown in Tables 2 and 3 suggests that fiscal institutions have a substantial positive effect on budget balances especially in election times. This seems to be especially true for CEECs after accession. While the  $election_{i,t} * fiscalinst_{i,t}$  coefficient is larger in new member states, it is additionally consistently significant below or around the 10 percent level throughout all models. In the sample of old member states, however, the effect is usually smaller. Its significance additionally vanishes if we control for government ideology, government size and the exchange rate regime in 2SLS and DGMM regressions. Note that we do not find fiscal institutions to significantly constrain PBCs in the CEECs if we include both the CEECs' pre-accession and post-accession period in a 2SLS regression (Column 1, Table 3). This inconsistency is not surprising given the relatively late evolution of fiscal institutions and fiscal rules across the EU (see Figures 1 and 2). The estimation results of the 2SLS, bias corrected and DGMM models yet 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; especially in the context of CEECs. 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'

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<sup>10</sup> A full temporal sample split or the inclusion of a CEEC\*election interaction term is not possible, as the pre-accession sample size either becomes too small or multicollinearity between CEEC and CEEC\*election is too high to separate individual effects.

<sup>11</sup> The effect is slightly reversed in a 2SLS regression that uses output gap data instead of growth as a control.

times. This result 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, especially in CEECs, they have trouble explaining why cycles still occur in non-CEECs, where fiscal institutions are on average slightly stronger than in new member states. One potential explanation may be, as hypothesized above, that the effect of fiscal institutions on PBCs may depend on other external influences on decision makers, such as a strong and free press.

### 5.3 Press Freedom

We included  $press_{i,t}$  as a control variable into our model for reasons outlined in the methodological section. The overall results for this control are unsurprisingly relatively inconsistent (see Tables 2 and 3): while it is positive and at times significant in the 2SLS and bias corrected estimations, it is mostly insignificant and negative in the GMM regression and sensitive to alternative model specifications. 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. 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. We subsequently assess if countries with smaller values and a stronger press are less likely to display PBCs.

As Table 4 shows, we observe that splitting our sample according to the freedom of press 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 freedom, 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  $election_{i,t} * fiscalinst_{i,t}$  is negative and insignificant in the sample which hosts a stronger press. These results suggest that while increasing the capacity of a free press to actually holding governments accountable seems to be the most promising way to contain PBCs also in developed countries, fiscal institutions seemingly help to reduce the extent to which PBCs occur in settings where the press is relatively weak.

**Table 4** Results for press freedom sample splits

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS High Press	2SLS Low Press	LSDVC High Press	LSDVC Low Press	DGMM High Press	DGMM Low Press
election <sub>t</sub>	-0.796 (1.052)	-1.023** (0.412)	-0.414 (0.802)	-1.039** (0.415)	0.061 (0.946)	-1.435** (0.591)
fiscalinst <sub>t</sub>	0.004 (0.473)	0.042 (0.265)	0.123 (0.356)	-0.156 (0.228)	-3.727 (3.293)	0.237 (0.658)
election <sub>t</sub> *fiscalinst <sub>t</sub>	-0.011 (0.849)	1.387*** (0.420)	-0.264 (0.650)	1.527*** (0.453)	-0.426 (0.493)	1.706*** (0.455)
surplus <sub>t-1</sub>	0.660*** (0.092)	0.587*** (0.051)	0.717*** (0.091)	0.709*** (0.061)	0.739*** (0.148)	0.614*** (0.094)
growth <sub>t</sub>	1.031*** (0.304)	0.459*** (0.080)	0.470*** (0.063)	0.308*** (0.026)	0.668*** (0.203)	0.464*** (0.118)
debt <sub>t-1</sub>	-0.027 (0.029)	0.025** (0.011)	0.009 (0.018)	0.027** (0.011)	-0.053 (0.070)	0.054* (0.030)
constant	-0.300 (1.586)	-3.565*** (0.641)				
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	0.930
Sargan(p)					0.286	0.832
Hansen(p)					0.250	0.809

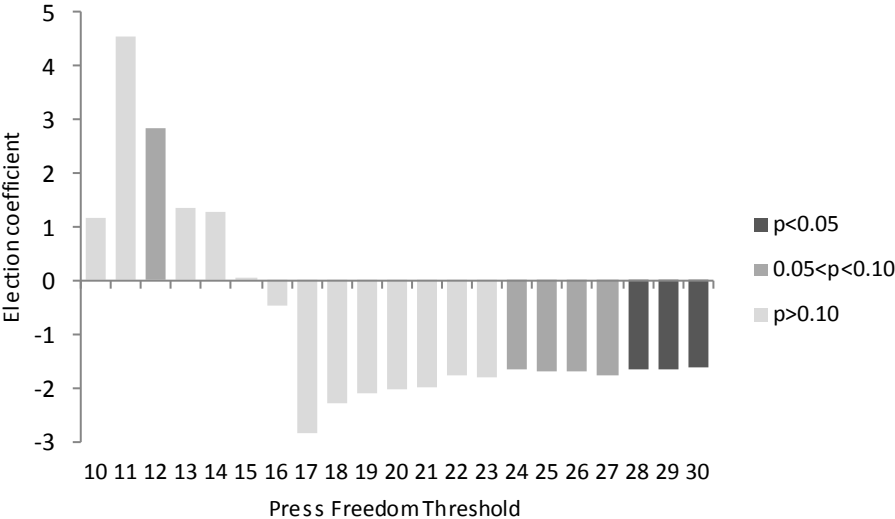
Standard errors in parentheses \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Difference GMM estimations uses up to and including lag 2, as further lags violate the over-identifying restrictions tests.

Having set the threshold at a value of 15, we simply split within our generally free press sample into a freer and less free half. Figure 5 displays the GMM regression results for the size of the election coefficient and its p-values if we relax or tighten the threshold value that makes countries qualify as having a high degree of press freedom. The starting sample applies a highly restrictive threshold in which only countries that display a press freedom score lower than 10 are included. We then continuously allow the sample to include less press free countries up to a score of 30. Figure 5 suggests that if we design the threshold to be more watery, the constraining effect of press freedom on the occurrence of PBCs indeed gets blurred. At a threshold of 17, there is no significant difference in PBCs to be detected from the lower press sample reported above: the election coefficient is large, positive, but (still) statistically insignificant. At a threshold of 24, PBCs in the sample become statistically significant again. More interestingly, Figure 5 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. This supports the finding that PBCs vanish from contexts with a strong and capable press – at least when considering overall budget surplus data.

The data even suggest 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.

**Figure 5** Press freedom thresholds and PBCs



*5.4 Controls and Model Fit*

With view to our control variables, the results of 2SLS, bias corrected and DGMM estimations for subsamples (Table 3) 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 as shown by a mostly positive and significant coefficient of the lagged debt variable. Our GMM regressions apply the Arellano-Bond test to check for (first order) autocorrelation of residuals in levels (i.e. second order autocorrelation in differences). The results indicate that this is not a problem and that we do not have to adjust the lags used in our regression. In order to limit the number of internal instruments, we specify that only one instrument is used per variable and lag distance, which is particularly important given our rather small sample size. If we had instead included one instrument per variable and time period, our instrument count would have risen dramatically and yielded Sargan and Hansen test statistics casting doubt on the suitability of the instrument set as a whole. The instruments we have chosen imply that these tests report encouraging results, indicating exogeneity of instruments, also when taking into account the decreasing power of the Hansen test as the number of instruments rises.

## 6. Conclusion

This paper investigated the occurrence of Political Budget Cycles in the enlarged EU. Starting from the assumption in the literature that PBCs should occur especially after accession of the younger democracies of Central and Eastern Europe to the EU, we investigated whether such a post-accession backlash with view to the CEECs is indeed noticeable. Estimations based on 2SLS, bias corrected and DGMM regressions of 25 EU new and old EU member states from 1996 to 2012 provide some evidence of a post-accession backlash for the new member states in Central and Eastern Europe, but fail to confirm the existence of an ‘Eastern problem’ within the EU in terms of PBCs. Rather, we substantiate earlier studies that find substantial PBCs in the entire EU (Efthyvoulou, 2012; Mink and de Haan, 2006).

In addition, we asked to what extent fiscal institutions and press freedom constrain governments in their attempts to fiscally stimulate the economy prior to elections. We find 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. This is especially the case in the new member states of Central and Eastern Europe (see also Stanova, 2012). Unlike previous studies, our regression results also hint at a peculiar interaction effect of fiscal institutions and media freedom in the enlarged EU. Splitting our sample into a group of countries and years with a strong versus a comparatively weaker press shows that the dividing line throughout Europe in terms of PBCs rather operates along the press freedom border than along an East-West divide. 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. In addition, fiscal institutions only help to limit the extent of PBCs in the sample with lower press freedom, whereas they fail to have a significant 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 a strong and powerful media capable of holding governments accountable and deterring PBCs is seemingly a more accurate recipe to fully eradicate their occurrence, also in developed countries in the EU.

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