Political budget cycles and the organization of political parties

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Abstract: We demonstrate that politicians have stronger incentives to increase spending around elections in the presence of younger political parties. Previous research has shown that political budget cycles are larger when voters are uninformed about politician characteristics and when politicians are less credible. We attribute the effects of party age to organizational differences between younger and older parties that precisely affect voter information and politician credibility. Parties organized around particular individuals, rather than around policy labels or a party machine, are less likely to survive the departure of party leaders, to adopt organizational attributes that promote voter information and political credibility, and to limit political budget cycles. Previous research has also shown larger political budget cycles in younger democracies. We present evidence indicating that party age accounts for this effect.

Disclaimer: The opinions and findings here are those of the authors and do not represent the views of the World Bank or its directors.

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Political budget cycles have long preoccupied scholars. Why do national governments increase spending around elections and why is this phenomenon more pronounced in some countries than in others? We provide evidence in support of a previously unexplored mechanism driving political budget cycles, rooted in political parties and, in particular, the age of parties. By looking at the characteristics of parties we go to an essential building block of democracy to identify the political incentive structures that give rise to pre-electoral strategic spending considerations. In particular, important recent research has shown that political budget cycles are largest in young democracies. We find that this effect can be explained by the age of political parties. Theory and evidence point to a plausible mechanism: younger parties are less likely to be organized to curb political incentives to engage in spending sprees around election time.

Previous research has argued that voters' incomplete information about politicians' competence in office increase incumbents' incentives to inflate spending around election years (building theoretically on Rogoff and Sibert 1988 and Lohmann 1998). Older political parties are more likely to have organizational characteristics that attenuate this problem: they are more likely than younger parties to have the capacity to credibly convey messages to voters. Consistent with this logic, we identify a significant effect of political party age on political budget cycles using Generalized Method of Moments (GMM) estimation: political budget cycles are larger when political parties are younger.

Although Shi and Svensson (2006) and Brender and Drazen (2005) demonstrate that political budget cycles are more pronounced in poorer and younger democracies, respectively, our results are not driven by a spurious correlation between party age and country income or democratic age. Even controlling for the effects of either income or democratic age on political budget cycles, political budget cycles are significantly larger in countries with younger parties. We conclude, instead, that the

disproportionate presence of younger political parties in younger democracies is the reason why politicians in these countries are more likely to trigger political budget cycles.

The next section of the paper briefly reviews the literature on political budget cycles and the relevant literature on political parties and develops the rationale for investigating the link between the age of parties and government spending around elections. We then turn to the data, particularly on government spending and political parties. The remainder of the paper presents the empirical estimates and discusses their implications for understanding the role of parties in government spending decisions.

Political budget cycles, political parties and party age: literature and theory

Political budget cycles have long been linked to imperfect voter information. The first theories advanced by Rogoff and Sibert (1988) and Rogoff (1990) explain cycles as a consequence of imperfect voter information about candidate competence. Incomplete information generates a moral hazard problem, because voters observe actual government spending with a time lag, hindering their ability to judge the competence of politicians. It also creates an adverse selection problem arises, since politicians know their own competence and voters do not. Voters can neither observe competence nor all components of the budget, allowing incumbents to use fiscal expansions to suggest to voters an increase in administrative efficiency. Later models of political budget cycles eschew the adverse selection element and focus exclusively on moral hazard (see Shi and Svensson 2006, Alt and Lassen, 2006a, Saporiti and Streb 2008, and Hanusch 2012 a, b, all drawing on Lohmann, 1998). Political candidates do not know their own competence, but all actors know that competence changes over time, so more recent signals of competence are more accurate than earlier signals. Accordingly, all incumbents have an incentive to exploit imperfect information about the budget and to abuse fiscal policy before elections in order to appear competent.

Other theories maintain the focus on information, but move away from competence. Brender and Drazen (2005, p. 1273) argue that in 'new' democracies "fiscal manipulation may work because

voters are inexperienced with electoral politics or may simply lack the information needed to evaluate fiscal manipulation that is produced in more established democracies." Alternatively, as in Brender and Drazen (2007), new democracies are more vulnerable to coups, particularly at election time, so incumbents use election-year spending to convince voters about the ability of elected officials to deliver benefits to them. Khemani (2004) and Drazen and Eslava (2008) emphasize instead that politicians are uncertain about the identity of pivotal voters. As elections draw closer, incumbents grow more certain and are more willing to direct resources to them.[1]

Research on political parties has long argued that one function of parties is exactly to provide voters with information about candidates that would otherwise be costly for candidates to provide (e.g., Cox and McCubbins 1993; Aldrich 1995; Snyder and Ting 2002; Bueno de Mesquita and Ashworth 2008). Parties with greater electoral experience, for example, are more likely to accurately identify pivotal voters. Snyder and Ting (2002) review substantial evidence that voters infer the policy positions of candidates from party labels. They model this process, observing that affiliating with a party known to have a particular policy position is a cheaper way for candidates to inform voters of their own positions. The problem for candidates is that party positions do not exactly match their own and may not be the optimal positions to adopt in the constituencies from which the candidates seek votes. The problem for parties is that candidates with extreme positions or low competence may seek to mask these attributes behind the party label. However, the recruitment of candidates whose policy preferences or competence diverge too far from the party norm reduces the information value of the party label.

The analysis in Snyder and Ting does not explicitly touch on candidate qualities preferred by all voters ("valence" issues such as competence). However, the model and evidence in Galasso and

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[1] Some results in the literature are consistent with the argument that voter information matters for political budget cycles. Shi and Svensson (2006) find that political budget cycles are larger when their composite variable, capturing both radio penetration and freedom of broadcasting, is higher. Alt and Lassen (2006 a, b) argue that the degree of fiscal transparency is a key source for the moral hazard problem in the national budget.

Nannicini (2011) make clear that parties care about candidate competence and take care to allocate more competent candidates to the most competitive districts. Parties require competent officials in order to implement the policies with which they are identified; policies that seek to establish a policy label, as in Snyder and Ting, are therefore also more likely to value candidate competence. In turn, where voters can rely on parties to monitor candidate competence, politicians have weaker incentives to signal competence with election year spending.

Parties may choose not to screen or may screen on issues unrelated to policy and competence. Charismatic individuals may establish a party to further their own private political careers; in this case, the information conveyed by the party label is support for that specific individual. These parties are more likely to screen candidates on the basis of personal loyalty to the leader than on competence or policy preferences. Such parties would therefore do little to limit political incentives to pursue political budget cycles. Parties may also be *ad hoc* associations of politicians, each of whom relies on their clientelist base rather than on a party label to mobilize electoral support. These parties are more likely to screen candidates on the size of their personal constituencies, again doing little to limit politician incentives to increase spending around elections. Were charismatic or clientelist parties to screen for candidate policy preferences or competence, they would limit their ability to recruit candidates who are loyal to the charismatic founder of the party; or who have substantial personal constituencies because of their extensive clientelist networks.

Parties with limited incentives to screen for candidate competence are also likely to be shorter-lived, giving rise to the prediction that political budget cycles should be larger in the presence of younger parties. Parties organized around charismatic individuals, for example, are not only less likely to screen for competence; they are also unlikely to survive the departure of their founding leaders. Parties organized around individual politicians with clientelist networks are likely to be ephemeral: it costs individual candidates little to leave one clientelist party and join or start another, since their individual

characteristics constitute the primary basis of their appeal voters. In contrast, the electoral appeal of parties that screen for competence and policy preferences depends less strongly on the particular candidates and party leaders in place. Their departure is consequently less likely to trigger party collapse.

The inability of politicians to make credible pre-electoral commitments to post-election public policies also creates incentives to trigger political budget cycles, in the form of vote-buying. In some countries, vote-buying is a large expenditure that occurs, by definition, in election years. The Afrobarometer surveys provide evidence of this. The 2005-06 wave of the surveys collected more than 20,000 responses. Nineteen percent of respondents reported that they had been offered a gift in the last election. This type of spending is particularly relevant for political budget cycles since poor and younger democracies, which exhibit the greatest political budget cycles, also appear to exhibit greater vote-buying.

Hanusch and Keefer (2013) formally model the phenomenon of vote-buying and argue that it increases systematically as political credibility falls. Politicians who are unable to mobilize political support with promises of post-electoral policy benefits, because such promises are not credible, are more likely to resort to vote-buying prior to the election. Consistent with this, Brusco, et al. (2004) present evidence that parties target vote-buying to voters who are likely to be most skeptical of party promises.[2]

Just as they can solve voter information problems, parties can also increase the credibility of candidates' pre-electoral policy commitments, reducing incentives to trigger political budget cycles. One way parties increase credibility is to adopt costly measures to establish a party label, as in Snyder and Ting (2002). In this sense, the information and credibility challenges overlap. In addition, though, Caillaud and Tirole (2002) argue that parties control intra-party disagreements and competition to

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[2] Nichter (2008) interprets evidence on vote-buying differently, arguing that politicians use gifts to induce turnout by voters whom they expect to support them, rather than to persuade the indifferent to vote for them.

enhance voters' trust in the politicians who stand for the party's positions. Grossman and Helpman (2005) show that politicians from more disciplined parties – those able to induce legislators to adhere to party positions – are better able to make credible commitments to voters regarding their future actions. Gehlbach and Keefer (2012) present a formal model showing that one aspect of party organization, information-sharing inside the party, plays a significant role in the credibility of party promises to members. Aldrich (1995) argues that one obstacle to citizen action to discipline politicians is the inability of politicians to credibly agree to act cohesively. Such politicians can therefore not credibly commit to voters that they will pursue particular policies that require their collective agreement. Under these conditions, since no individual politician is responsible for failing to pursue desirable policies, and voters cannot hold politicians collectively accountable, political incentives to pursue these policies are weak.

In all of these cases, the actions that parties take to bolster political credibility require substantial organizational decisions (Keefer 2011 makes a similar point). For example, the decisions require parties to delegate to leaders the authority to monitor and sanction free-riding by members and to allocate internal rewards to candidates who advance party policies and apply sanctions on candidates who do not. They also entail arrangements that allow members to discipline leaders who shirk. These are the same decisions as those required of any group that is capable of mobilizing members for collective action: they allow organization leaders to monitor and punish free-riding, and they allow members to replace leaders who fail to reward candidates who pursue the collective interests of party members.

Again, however, not all parties value arrangements to bolster candidates' credible commitment. Loose associations of clientelist politicians are unlikely to empower leaders with the authority to eject candidates with large personal constituencies because the candidates do not conform to party policies. Parties organized around a single charismatic leader are unlikely to empower party members to replace the leader when her decisions undermine the party's electoral appeal.

Organizational arrangements to bolster credibility, like those that screen candidates, are more likely to be present in older than in younger parties. Gehlbach and Keefer (2012) show that the survival of a party is more likely if party members can act collectively to discipline leader shirking; this capacity increases the ability of party members to commit credibly to policies, establishing one link between party age and the party's ability to make credible commitments. More generally, parties that are organized around individuals – charismatic leaders or candidates with large personal constituencies – are not only likely to be shorter-lived and more reluctant to adopt screening arrangements to ensure candidate competence. They are also less likely to establish organizational arrangements that reinforce credibility. Charismatic leaders are unlikely to allow members to act collectively to replace them should they shirk; the clientelist politicians who are loosely affiliated into an *ad hoc* political party are unlikely to give authority to the party leader to discipline them if they diverge from their party promises.

Evidence from the Democratic Accountability and Linkages Project (Kitschelt, 2011) supports the link between party age and the degree to which parties pursue clientelist policies, or rely on charismatic leaders, or mobilize support using claims of party competence. In 2009, the DALP recruited experts – at least six in each country – to assess numerous aspects of all political parties in countries, including these three dimensions related to clientelism, charisma and competence. Figure 1 relates these to the average age of the largest four political parties in a country in 2009. This is the key variable from the Database of Political Institutions that is used in the empirical analysis below. Since parties are likely to substitute among clientelism, charisma, and competence in mobilizing voters, Figure 1 reports the partial effects of each of these from a regression of average party age on the three DALP variables. As the previous discussion foreshadows, controlling for other mobilization strategies that they might undertake, younger parties are more likely to be clientelist; they are somewhat more reliant

on charismatic leaders; and they are significantly less likely to mobilize support on the basis of

competence. [3]

Figure title: Partial regresions of DALP variables on DPI Party Age, 2009
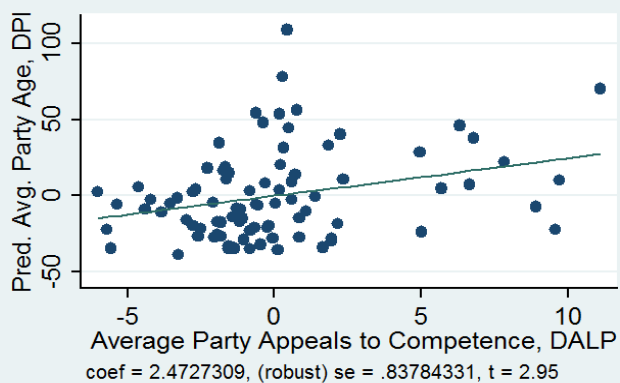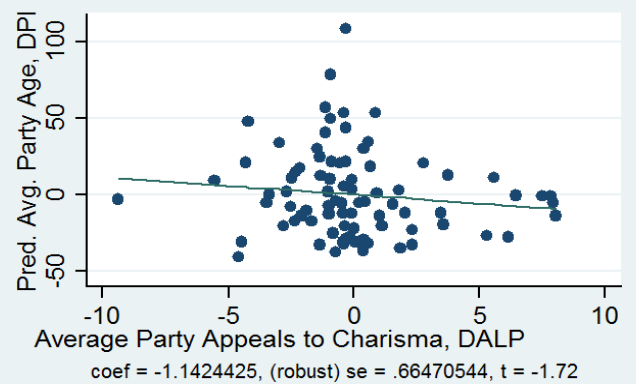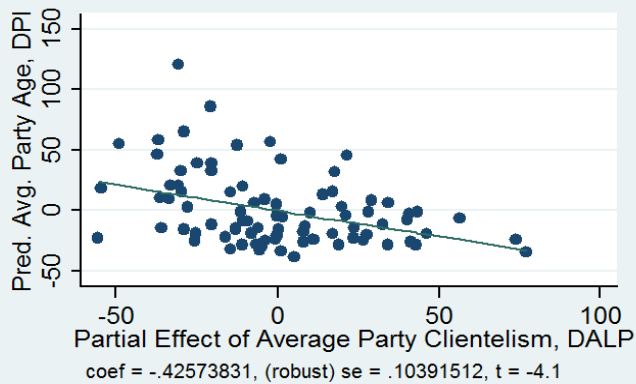
Party age is, of course, not an infallible measure of whether parties screen for candidate

competence or of political credibility. On the one hand, technocratic parties may emerge that attempt to

use the competence label to mobilize electoral support but fail to adopt popular policy positions. They

are unlikely to survive, despite the competence of their candidates. Such parties are rare, both in general

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[3] Brader, et al. (forthcoming) also report results of a survey experiment in Britain, Hungary and Poland and find further evidence that party age affects the credibility of party claims. They divide the supporters of parties into a treatment and control group, where the treatment is to indicate the party's support for a particular policies. Supporters of older parties were more likely to respond to this treatment by indicating that they, also, supported the policy.

and in our data. To the extent that they are present, they attenuate the association of younger parties with larger political budget cycles.

On the other hand, some clientelist parties, which do not appear to convey significant information to voters about candidate policy positions or competence, survive for generations. The presence of these parties in the data correspondingly attenuates the predicted association between older parties and smaller political budget cycles. However, the incentives of these parties are also likely to conform to the logic articulated here. Long-lived clientelist parties are generally machine parties – clientelist parties where transactions with individual voters are mediated by the party more than by individual candidates. The management of the party machine, however, is a demanding task that requires both competence and loyalty to the party. Compared to leaders of short-lived parties that are simply *ad hoc* coalitions of politicians with their own personal, clientelist constituencies, leaders of long-lived machine parties have a greater incentive to screen for competence and, therefore, to limit political budget cycles.

Party age itself could be evidence of competence and credibility. Incompetent parties are less likely to perform well, making them more vulnerable to electoral defeat and, ultimately, to extinction. Politicians in older parties therefore benefit less from spending around elections than those from younger parties, since voters are less certain about the competence of candidates from younger parties.[4] This argument is entirely consistent with the idea that party organization is likely to differ between older and younger parties, however, since candidate competence is not an exogenous attribute of political

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[4] Political budget cycles themselves could be regarded as indicators of poor performance, creating a tautology: older parties are those that have not engaged in political budget cycles. However, the literature on political budget cycles, especially Rogoff and Sibert (1988) and Rogoff (1990), makes precisely the opposite assumption: spending around elections is an indicator of competence – of good performance – not of incompetence.

competitors. It requires a conscious decision by party leaders to promote competence within the organization and to select candidates on the basis of competence.[5]

It is possible that parties have a history of good performance for reasons unrelated to politician competence. However, this is unlikely to create a spurious negative association between party age and political budget cycles. For example, parties could have experienced positive shocks during their time in office. In that case, older parties are simply luckier, but otherwise no different than younger parties. To the extent that this is true, it creates a bias against finding that party age has a negative effect on political budget cycles. On the contrary, the opposite is more plausible: older, luckier parties, benefiting from positive economic shocks, are able to spend more around elections relative to younger parties. The results below, however, are exactly contrary to this.

The next section of the paper discusses the data used in the analysis, particularly the party measures and the public spending data. We then present extensive evidence that political budget cycles are more pronounced in the presence of younger political parties. These results are robust to controlling for the effects of democratic age on cycles. It is not the case that a spurious correlation of some unobserved characteristic of older democracies with our party variables can account for the results that we report. Rather, the results point to a possible mechanism that explains the association of democratic age and political budget cycles.

Data

Our main party variable, from the Database of Political Institutions (DPI) (Beck, et al. 2001) is the average age of the largest four political parties in a country (the largest three government parties and the largest opposition party, according to the number of seats they have in the legislature). This is *average party age*. The previous section makes a series of arguments pointing to a theoretical effect of party age

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[5] Keefer and Vlaicu (2008), for example, explicitly model the tradeoffs that politicians make when they invest in party credibility.

on political budget cycles, principally that younger parties are less likely to have developed the organizational characteristics that allow them to convey information about candidate quality and to bind politicians to their promises. Consistent with the view that party age captures fundamental aspects of party organization, Enikolopov and Zhuravaskaya (2007) also use party age as a measure of whether national parties can discipline local politicians. This is a key organizational prerequisite for parties seeking to boost the credibility of its candidates' commitments.

We examine the robustness of our results to the use of two additional proxies for political party organization. One benchmark for whether party age is likely to be associated with a more organized party is the age of a party relative to the tenure of the party's leader. If the party was created during the leader's tenure, it is more likely that it is a personalized vehicle of the party leader, one that has few of the organizational characteristics that might attenuate political budget cycles. In contrast, the more years that the party has existed prior to the leader taking office, the more likely it is that the party has experienced leadership transitions and exhibits the organizational characteristics that promote party survival during transitions – the same characteristics that limit fiscal cycles around elections.

Such a variable, employed by Gehlbach and Keefer (2012), is available for only the largest government party. Using the Database of Political Institutions, they calculate the age of the largest government party at the time that the leader of the country took office, *ruling party age* (calculated as the age of the largest ruling party minus the years in office of the incumbent, set equal to zero when this difference is negative).

We also examine the robustness of our results to an entirely different party variable, measuring confidence in parties, from the World Value Surveys (WVS) (Inglehart 2004). One of the questions in the surveys is:

"I am going to name a number of organizations. For each one, could you tell me how much

confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very

much confidence or none at all?"

One of the organizations that respondents evaluate is political parties. To construct the variable *party*

confidence we calculate the country-means for valid answers and construct indices where higher values

imply higher confidence. We expect that in either of their roles, as solving information problems or in

solving collective action and credible commitment problems, more organized parties should increase

respondents' reported confidence in political parties.

Survey responses regarding confidence in parties are endogenous to party performance. Still,

there are a number of reasons to regard estimates with this variable as useful tests of robustness. First,

our estimation methodology (GMM) addresses endogeneity. Second, the endogeneity should operate in

the opposite direction of our hypothesis. If government spending, as is commonly argued, increases

voters' appreciation of parties, then we would expect high spending parties – those that spend more

around elections, for example – to be viewed with greater confidence. We find the opposite: political

budget cycles are muted in countries where respondents report greater confidence in parties. Finally,

third, in placebo regressions, where we substitute confidence in parties with confidence in other

government institutions, we find no evidence of an effect on political budget cycles.

The spending and elections data are based on the original data set constructed by Brender and

Drazen (2005) and expanded by us. We focus on the total expenditures of central government as our

dependent variable, in part because theories of political budget cycles (such as the theory of vote-buying

in Hanusch and Keefer 2013) emphasize pre-electoral spending, and in part because Brender and

Drazen (2005) find that political budget cycles are in particular driven by expenditure. They do not

detect cycles in revenue and accordingly, the cycle in the budget balance they document is likely to be

driven by higher spending.

Because the party age variables are available over the period 1975 - 2008, while Brender and Drazen's data are at most available from 1960 to 2001and for many countries only the 1990s, our main specifications rely on an expanded version of their spending data. This uses information from the *International Finance Statistics* (IFS), compiled by the International Monetary Fund (IMF), which has become available since their research. For overlapping years, the correlation between our extended expenditure measure and Brender and Drazen's is 0.99, with very minor differences probably due to subsequent revisions by the IMF of data they used. We also extend sample coverage to the following countries: Albania, The Bahamas, Botswana, Croatia, Ghana, Kenya, Latvia, Malta, Nigeria, and Thailand.[6] Overall we manage to gain up to 307 observations compared to the Brender and Drazen dataset. We also, however, report results using their original data.

The WVS confidence measures pose particular data challenges. The WVS data are only available from the late 1990s, with gaps. Unfortunately, neither the original nor the extended IFS data have sufficient coverage for estimation in this case because the IMF statistics division changed its fiscal accounting methodology during the 1990s. Expenditure data using the old methodology continued to be compiled, but only until the late 1990s and early 2000s for most countries. All of our estimates, like those of Brender and Drazen, rely on these data. Expenditure data using the new methodology begin in 1990 for some countries, but for most countries, data coverage under the new methodology begins only in the late 1990s. Since neither series overlaps sufficiently with the WVS data, we turn instead to the only expenditure variable that is available throughout the 1990s and 2000s and that has broad country coverage, general government final consumption expenditure from the WDI.

This variable differs from the IFS series in two ways. First, it includes general rather than central government spending, and so includes government spending at subnational levels. However, coverage

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[6] We only include democracies, i.e. countries that score 7 on both the DPI's legislative and executive indices of electoral competitiveness.

of subnational spending is sporadic and unlikely to be a significant issue. Second, and more importantly, final government consumption expenditure excludes the capital budget. Since previous research has identified capital spending as one source of funds for pre-electoral mobilization, this is problematic. However, Block (2002) finds for a sample of African countries that fiscal expansions around elections are particularly pronounced in government consumption expenditure, and the correlation between this variable and the IFS data is 0.7. Assuming that cycles in government consumption and capital expenditure are related, this measure should be a good approximation of total government expenditure.

Moreover, any bias generated by using the general government measure is likely to be against the hypothesized relationship between budget cycles and party characteristics. Subnational spending introduces noise into central government spending, making it more difficult to detect cycles in central government spending. Similarly, to the extent that capital spending is more important for pre-electoral expenditures, it should once again be more difficult to detect cycles in spending using the general government consumption data.

Previous research has not addressed the noise that hyperinflation can introduce into analyses of political budget cycles. Hyperinflation generates very large within-fiscal year differences in the real value of nominal government spending. As a consequence, small errors in assumptions regarding the timing of election spending can have large effects on the estimation. For example, if the election falls in the beginning of the fiscal year, even a large increase in government spending might go unnoticed, since nominal spending could be one half of the nominal spending recorded in the last part of the fiscal year. Bulgaria and Nicaragua experienced hyperinflation in 1997 and 1989, respectively, and illustrate this problem. In both countries, reported nominal expenditures as a fraction of GDP during these years experienced a massive reduction of 26 percentage points (Bulgaria) and 14 percentage points (Nicaragua). These drops exceed any plausible fiscal adjustment that these governments might have undertaken to stamp out hyperinflation, and they are orders of magnitude greater than the mean annual

change in expenditures, of 0.2 percentage points. To eliminate this potential for noise, our base specification excludes expenditure observations where GDP inflation is two standard deviations above the sample mean.[7] Our results are, however, robust to restoring these observations.

We use the election dummy variable (*election*) from Brender and Drazen to capture election timing. Since we rely on party variables from the DPI, however, we also check the consistency of their dummy with election dates reported in the DPI. In those few cases where there were inconsistencies, we did further research and made changes where appropriate.[8] Then we extend the measure in accordance with Brender and Drazen's coding rule in order to match the extended coverage of spending.

Our main controls include two economic variables and two political ones. The economic variables are standard in the literature (e.g. Brender and Drazen 2005, Shi and Svensson 2006, Alt and Lasen 2006b, Hanusch 2012 b). To capture business cycle fluctuations we use Brender and Drazen's variable of the output gap (computed using the Hodrick-Prescott filter) when replicating their findings. For our extended expenditure measure and the general government final consumption measure we simply use real economic growth, as in Shi and Svensson, Alt and Lassen, and Hanusch, taken from the World Bank's *World Development Indicators* (WDI). The second standard measure is the log of real GDP per capita to capture a country's degree of economic development, also taken from the WDI.

Because we are concerned with the effects of parties on budget cycles, we control for political factors capturing institutional arrangements that might influence both political incentives to engage in pre-electoral transfers to voters and incentives to organize political parties. Saporiti and Streb (2008) argue that political budget cycles are less likely to occur if government is divided and a second chamber

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[7] Another, minor, adjustment is the correction of Spain's expenditure in the last year of the IFS series where the currency amount is recorded rather than a percentage of GDP.

[8] These adjustments are documented in appendix 1.

can veto the budget, especially before elections.[9] A control, *unified government* (named 'allhouse' in the

DPI), takes this effect into account. Prior research also argues that the size of the ruling coalition

influences political incentives to increase spending around elections. Hanusch (2012a) demonstrates

formally that parties have different incentives to engage in electioneering, depending on their size and

ideology, while Chang (2008) shows empirically that more parties in a coalition dampen the magnitude

of political budget cycles since each additional coalition member adds another veto player. Our

regressions therefore include a second political control, *Ruling coalition size*. In addition, we show that our

results are robust to controlling for two additional political variables. Because many new democracies

choose presidential systems, and are also known to exhibit larger political budget cycles, we show that

parties continue to have a robust effect after controlling for *system*, from the DPI, capturing whether a

country is presidential, semi-parliamentary, or parliamentary. In addition, previous research has

associated political budget cycles with political polarization (Alt and Lassen 2006a,b; Hanusch and

Magleby, forthcoming). Controlling for a measure of polarization from the DPI has no effect on our

results.

Finally, we replicate our results using all of Brender and Drazen's (2005) original control

variables, including the share of international trade as a percentage of GDP, and the fractions of the

population aged 15-64 and above 65. Where our sample does not overlap with theirs we obtain these

variables from the WDI.

Expenditure timing

Two important timing issues are relevant for empirical tests of political budget cycles. The first is

matching the timing of expenditures, which are reported by fiscal year, with the timing of non-fiscal

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[9] Whether election rules are proportional or plurality-based can also influence the effects of parties on candidate behavior, but no theory links these rules to political budget cycles. In fact, controlling for whether legislative seats are elected using rules of proportional representation has no effect on our coefficient of interest, the interaction of the party variables with the election year. Electoral rules are themselves rarely significant and never in the preferred System-GMM specification.

variables, which are reported by calendar year. The second is how to take into account the timing of

elections within a year: when elections are held late in the calendar year, electoral expenditures occur

mostly in the same calendar year; when elections are held early in the calendar year, the electoral

expenditures may occur mostly in the previous calendar year.

We follow Brender and Drazen in addressing these issues. They assign fiscal measures to the

calendar year that overlaps the most with the fiscal year. For example, in the US the fiscal year 2011 runs

from 1 October 2010 to 31 September 2011. Nine months of the fiscal year thus fall in the calendar

year 2011. Accordingly, the fiscal data reported for fiscal year 2011 are matched with calendar year data

from 2011. In the UK, the fiscal year 2010/2011 lasts from 1st April 2010 to 31st March 2011. Eight

months of the fiscal year thus fall into the year 2010. Accordingly, the fiscal data reported for fiscal year

2010/2011 are matched with calendar year data from 2010.

With respect to election timing, ideally the election would be recorded as occurring in the fiscal

year in which most pre-election expenditures occurred. For example, if the election takes place two

months into the fiscal year, and it is in those two months prior to the election that election expenditures

are concentrated, then the election year should be the same as the fiscal year. In fact, Mironov and

Zhuravskaya (2011) find that shadow transfers from firms to politicians in Russia are concentrated in

the four weeks prior to the election. If the transfers support campaign expenditures, including vote-

buying, this suggests that even elections occurring in the first month of the fiscal year are appropriately

coded as occurring in that same fiscal year. However, we do not know whether the particular timing of

campaign expenditures found in Russia is pervasive across countries.

The prior literature simply matches the election year to the fiscal year in which it occurs. In our

main specifications, we follow this methodology, replicating Brender and Drazen's (2005) election

dummy (with a small number of adjustments reported in table A2). However, it is possible that election

expenditures are spread out over several months prior to the election. In this case, it would be more

appropriate to code an election occurring early in the fiscal year as having occurred in the prior fiscal year.

We therefore go beyond the prior literature and show that our results are robust to the use of an alternative election dummy, *election (M1)*. This reports an election as occurring in the previous calendar year if it fell in the first month of the calendar year. The purpose of this re-coding is to ensure that we capture a fiscal expansion *before* the election. If an election is held in the first month of the fiscal year, most of the electorally-motivated pre-electoral expenditures may have taken place in the prior fiscal year.

Estimation

The empirical model for the analysis is

$$F_{i,t} = \alpha + \beta_1 F_{i,t-1} + \beta_2 PARTY_{i,t} + \beta_3 ELECT_{i,t} + \beta_4 PARTY_{i,t} * ELECT_{i,t} + \sum \gamma' x_{i,t} + \mu_i + \theta_t + \varepsilon_{i,t}$$

$F_{i,t}$ is government expenditure for country i at time t, and $F_{i,t-1}$ is the lagged dependent variable. The β's are the coefficients for our key variables and γ is a vector of coefficients for our control variables x. The variables $PARTY$ and $ELECT$ are the political party measures and the election dummies, respectively. Our main prediction is that the interaction term $PARTY * ELECT$ is negative: the older are parties, the lower is election year spending. Finally, μ and θ denote country and time effects; the overall error term is given by ε.

Both controls for fixed country effects and lagged spending are essential, given unobserved country characteristics that might influence both country political characteristics and political tendencies to engage in greater spending during election years, and significant dependence of current on past spending. However, this introduces dynamic panel bias (Nickell 1981). The political budget cycle literature addresses this bias by relying on Generalized Method of Moments, originally proposed by Holtz-Eakin et al. (1988) and further developed and popularized by Arellano and Bond (1991), Arellano and Bover (1995), and Blundell and Bond (1998). Shi and Svensson (2003) argue that moving towards

GMM estimation has been one of the major advances in the empirical study of political budget cycles. We follow this approach here.

GMM estimation involves a number of choices. The first is between Difference and System GMM. Brender and Drazen use the 'original' Arellano-Bond estimator, the so-called Difference-GMM, where unit effects are purged by first differencing the estimation equation and lagged differences of endogenous repressors are instrumented with internally available lags. However, Blundell and Bond (1998: 134) find that System-GMM, which makes additional instruments in differences available by including level equations in the analysis, outperforms Difference-GMM when the dependent variable is highly persistent. This is the case with government spending: our estimates for the lagged dependent variable range from roughly 0.7 to 0.8, so System-GMM is more appropriate. In addition, some of our independent variables vary little over time. As System-GMM draws on equations in differences as well as in levels, it preserves some of the variation in rarely changing variables, making this an attractive estimator for our purposes. We demonstrate below that System-GMM indeed performs better than Difference-GMM.

Researchers also have a choice between using a one- or two-step method to calculate the GMM estimator. We use a one-step method, using the version derived by Arellano and Bond (1991) that is robust to substantial heteroskedasticity.[10]

In GMM estimation, the number of instruments is the square of the number of periods. For relatively long panels, this can cause over-fitting of the endogenous variables (see, e.g., Roodman 2006). We therefore restrict the number of instruments we use to a maximum lag number of three; where our sample contains a significant number of missing observations, we collapse our instruments.

Even when the number of instruments is restricted, GMM estimates use a large number of

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[10] Standard errors for the two-step version are severely downward biased. Windmeijer (2000) provides a bias correction, which can result in two-step estimation which is superior to one-step estimation. In our estimation, however, we do not detect any efficiency gains from bias-corrected two-step estimation. We therefore focus on the robust one-step estimator.

instruments. This weakens the power of standard tests of instrument endogeneity, such as those proposed by Sargan (1958) and Hansen (1982). For example, the power of the Hansen test, which performs better than the Sargan test when errors are heteroskedastic, falls rapidly with the number of instruments (Bowsher 2002). Although we limit our instrument count, the Hansen test statistic is consequently improbably large, with a p-value of one. This is not uncommon, given the data structure. For example, Shi and Svensson (2006) report Hansen scores of .99 in their GMM specifications. In view of the weakness of the Hansen statistic, two alternative diagnostics (Bond 2002 and Roodman 2007) are useful to establish the validity of the GMM results.[11]

First, we examine the robustness of our results to fixed effects OLS estimation and compare the coefficient on the lagged dependent variable under fixed effects with that under GMM. Since our emphasis on GMM is motivated by the downward bias in models that include a lagged dependent variable (LDV) and exhibit unit effects (Nickell 1981), the LDV coefficient in a correctly specified GMM model should not lie below the LDV coefficient in the FE model (Bond 2002). As a second diagnostic test, we report autocorrelation of order 1 and 2 in the first-differenced residuals. Given the lag structure of our instruments, this supports the validity of our instruments if there is first-order but not second-order autocorrelation.

Results

Table 1 reports our main results, using the extended database in different specifications and estimations. Across all models, the interaction term *average party age x election* is significant and negative, indicating that political budget cycles are significantly smaller in countries with older political parties. Column one contains the results of a 'naïve' Ordinary Least Squares (OLS) regression; the specification in column two accounts for fixed effects, while period dummies are controlled for in column three. In

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11 The GMM estimators are derived under the assumption that there is no contemporaneous correlation. We therefore also include time dummies in all our analyses.

all of them, central government spending rises significantly more in election years in those countries

with younger political parties.

Table 1: Party age and Political Budget Cycles (extended sample):

Dependent Variable: Total central government expenditure/GDP	(1) Pooled OLS	(2) Fixed Effects OLS	(3) Fixed Effects OLS	(4) Extended Sys-GMM	(5) Extended Diff-GMM
Avg. party age	-0.002	0.011	0.012*	-0.002	0.011
	(0.002)	(0.008)	(0.006)	(0.005)	(0.008)
Election	0.517**	0.482**	0.508**	0.674***	0.614***
	(0.258)	(0.237)	(0.236)	(0.246)	(0.208)
Avg. party age X Election	**-0.006***	**-0.005***	**-0.005***	**-0.009****	**-0.008****
	(0.004)	**(0.003)**	**(0.003)**	**(0.004)**	**(0.003)**
Lagged Dependent Variable	0.975***	0.820***	0.839***	0.929***	0.594***
	(0.008)	(0.023)	(0.025)	(0.023)	(0.091)
Log GDP per capita	0.126**	-0.849	0.940	0.578***	1.523
	(0.058)	(0.522)	(0.953)	(0.195)	(0.091)
Growth	-0.123***	-0.163***	-0.145***	-0.120***	-0.119***
	(0.031)	(0.039)	(0.037)	(0.031)	(0.025)
Ruling coalition size	-0.379	-0.619	-1.028	0.118	-0.388
	(0.453)	(0.717)	(0.638)	(0.859)	(0.835)
Unified government	-0.010	-0.211	-0.179	1.024***	0.847**
	(0.142)	(0.234)	(0.221)	(0.318)	(0.432)
Constant	1.086***	6.858***	1.622	-0.821	6.434
	(0.381)	(1.349)	(2.570)	(0.885)	(9.226)
Time dummies	No	No	Yes	Yes	Yes
Hansen statistic (p-value)				1.00	1.00
AR (1)				-4.28***	-3.96***
AR (2)				0.38	0.34
Number of observations	1,128	1,128	1,128	1,128	1,128
Number of countries	65	65	65	65	65

Standard errors in parentheses (robust standard errors for OLS). *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Instruments are restricted to 3 lags to minimize the instrument count. Endogenous variables are the lagged dependent variable and growth. First order serial correlation in first-differenced residuals (AR 1 significant) with no second order serial correlation (AR 2 insignificant) supports the claim that instruments for both GMM analyses are valid. Lagged dependent variable coefficient for Difference-GMM below fixed effects (FE) coefficient suggests a weak model fit; LDV coefficient of System-GMM between OLS and FE coefficients suggest a good model fit.

Column four reports results of a System-GMM estimation. The coefficient on the party age-

election variable is larger in magnitude and is much more precisely estimated than in the first three

models. In addition, as expected, the coefficient estimate on the lagged dependent variable indicates that this estimation controls for bias induced by persistence in the dependent variable. If it failed to correct for the bias, we would observe a significantly lower coefficient in the lagged dependent variable in the System-GMM estimation in column four than in the fixed effects estimation in column three. Instead, the coefficient for the lagged dependent variable under System-GMM lies between the pooled OLS and FE estimates, supporting the conclusion that System-GMM is the most appropriate specification.

Further supporting the use of the System-GMM estimation is that the first and second order serial correlation is consistent with the validity of the GMM instruments. First order serial correlation in the first-differenced residuals (AR 1) is significant and negative. There is no second order serial correlation, since AR(2) is insignificant. We therefore rely on System-GMM for most of the remaining specifications.

Column five reports results from the Difference-GMM estimator, used by Brender and Drazen. The estimated effects of party age are slightly larger and slightly less precisely estimated. However, the persistence in the dependent variable has a significant effect on the estimation. The coefficient on the lagged dependent variable in the Difference-GMM estimation in column five (0.59) is substantially smaller than the coefficient in the fixed effects estimation in column three (0.84) in column 3. This indicates that the Difference-GMM specification does not correct for the downward bias introduced by persistence in the lagged dependent variable, according to Bond (2002).

In all specifications, the election dummy is positive and significant, implying an (out-of-sample) estimate of the effects of elections on spending when parties are zero years old of 0.5 to 0.6 percent of GDP. In the System-GMM estimation, the effect is 0.61 percent of GDP. However, if a country succeeded in establishing political parties right after independence in the 1960s, such that average party

age reached 50 years, political budget cycles drop to 0.21 percent of GDP and are not statistically significant.

We examine the implications of endogenous election timing directly in column two of Table 2. Brender and Drazen identify all election dates that are "exogenous" and not called by the incumbent government. We use their definition and apply it to all elections in our extended database that are not in theirs.[12] We then remove all endogenous elections – those called by the government – from the sample. Once again, the results are robust to this change. Again, the magnitude of the party effect remains large and statistically significant. Precision drops compared to Table 1, but the argument above suggests that this may be due to cases that are consistent with our theory being removed from the data.

Table 2 examines the robustness of the party finding results under different specifications. The results in column one show that our results are not the result of differences between our sample and control variables and those of Brender and Drazen (2005).[13] We use their sample and three control variables in their specification, but not in ours: total trade as a fraction of GDP, the fraction of the population greater than 65, and the dependency ratio. We omit the two political control variables used in our specification, but not in theirs (ruling coalition size and unified government). The magnitude of the party effect is nearly the same, and significant, if less precisely estimated than in the System-GMM specification in Table 1.

The estimation results in Table 1 abstract from the possible endogeneity of party effects to the timing of elections. While in most countries in our sample, elections are fixed, in some the timing is determined by the incumbent government. It is not obvious that the endogeneity of election timing

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[12] An election is "exogenous" if it occurs in a country with constitutionally determined election intervals and where the election occurs at the fixed interval or within the expected year of the constitutionally fixed term.

[13] Although our analysis suggests that Difference-GMM is not appropriate given the data structure, we re-estimate Table 2 with this method to compare our results with Brender and Drazen's. The results are highly robust to using Difference-GMM instead of System-GMM.

introduces a spurious bias in favor of a significant effect of party age. The theory predicts that younger parties should be more likely to call early elections in the event of a budget windfall, since they benefit more from election spending. Since this effect is consistent with the theory, it raises no concerns of endogeneity bias.

Columns three and four show that results are robust to two key political features of countries that might influence both party age and incentives to spend around elections. Introducing a control for political polarization in column three has no effect on the results, although prior research has linked polarization to political budget cycles (Alt and Lassen 2006a, b, Hanusch and Magleby forthcoming).[14] The same is true when we control for whether countries have presidential, semi-presidential or parliamentary forms of government, in column four. This last is important since young democracies are more likely to exhibit presidential systems – and larger political budget cycles – than older democracies.

In column 5 we remove the hyperinflation adjustment which we have applied to all expenditure data to avoid spurious effects induced by the timing of elections under hyperinflation. Consistent with our earlier discussion, Column 5 shows, reversing this adjustment, and putting these observations back in the data, actually leads to a substantial increase in the magnitude of the party effect. Budget cycles driven by spending prior to elections early in the fiscal year appear to be masked in hyperinflationary countries.

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[14] The party age – election interaction also remains significant when we control for a polarization – election interaction, as in Alt and Lassen.

Table 2: Robustness checks: Additional controls + different modifications to expenditure measure (all estimated with System-GMM)

Dependent variable: Total central government expenditure/GDP	(1) Original B&D sample and control variables	(2) Base sample, "exogenous" elections	(3) Base sample, polarization	(4) Base sample, political system	(5) Base sample, (w/o inflation adjustment)
Avg. party age	0.002 (0.007)	0.001 (0.007)	-0.005 (0.006)	-0.002 (0.006)	-0.007 (0.010)
Election	0.570** (0.262)		0.659** (0.293)	0.661*** (0.244)	1.042*** (0.339)
Election (exogenous)		0.436 (0.317)			
Avg. party age X Election	**-0.00715*** **(0.004)**		**-0.008*** **(0.004)**	**-0.009**** **(0.004)**	**-0.012**** **(0.004)**
Avg. party age X Election (exog)		**-0.007*** **(0.004)**			
Lagged dependent variable	0.901*** (0.046)	0.897*** (0.023)	0.935*** (0.027)	0.922*** (0.026)	0.835*** (0.053)
Log GDP/capita	0.830 (0.796)	0.852 (0.519)	0.509* (0.263)	0.533*** (0.185)	1.299*** (0.497)
Output gap/growth	-0.262** (0.124)	-0.167 (0.120)	-0.134*** (0.032)	-0.120*** (0.030)	-0.076** (0.031)
Trade/GDP	0.001 (0.007)				
Population >65	0.067 (0.172)				
Dependency ratio	-0.087 (0.092)				
Ruling coalition size		-0.711 (0.896)	-0.0923 (0.779)	0.0350 (0.838)	0.565 (1.055)
Unified government		0.750* (0.446)	0.683* (0.365)	1.077*** (0.332)	0.323 (0.469)
Polarization			-0.304* (0.161)		
System				0.243 (0.240)	
Constant	3.792 (7.450)	-6.197 (4.398)	1.438* (0.749)	0.713 (0.815)	-0.237 (1.025)
Time dummies	Yes	Yes	Yes	Yes	Yes

Hansen statistic (p-value)	1.00	1.00	1.00	1.00	1.00
AR (1)	-4.13***	-4.08***	-3.93***	-4.29***	-2.99***
AR (2)	0.73	0.93	1.09	0.37	1.40
Number of observations	1,035	1,010	1,012	1,128	1,144
Number of countries	67	65	63	65	65

Base sample is the extended sample from Table 1. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. System-GMM specification analogous to explanation in table 1. The sample in column 5 includes all available observations, independent of whether hyperinflation was present or not.

In Table 3 we report results using the two alternative measures of party institutionalization, the age of the largest government party less the leader's years in office, *ruling party age,* and the survey measure of citizens' confidence in parties, *party confidence.* As usual, the table reports only results using the most appropriate specification, System-GMM, as in column four in Table 1. Again, the key variable is the interaction effect of the party variable with the election dummy. Columns one and two examine (age of largest government party – ruler years in office) in our base sample and in the original Brender-Drazen sample. Using either sample, political budget cycles are significantly smaller when ruling party age is larger.

The third column examines the party confidence variable, but using general government consumption expenditures as the dependent variable. Again, the interaction of the party variable with the election year is negative: parties in which citizens express greater confidence are associated with smaller political budget cycles. This column also substitutes growth for the output gap control, because of the latter's limited coverage.

Table 3: Robustness checks: alternative party institutionalization measures and alternative timing assumptions (System-GMM)

Dependent variable: Central government expenditure (except column 3)	(1) Base sample	(2) Original B&D sample	(3) Dep var: Gen. gov. final consumption expenditure	(4) Base Sample, Election dummy adjustment for elections in first month of fiscal year
Election	0.496**	0.488*	0.978**	0.640**
	(0.232)	(0.264)	(0.460)	(0.263)
Avg. party age				-0.002
				(0.005)
Ruling party age	0.001	0.005		
	(0.006)	(0.009)		
Party confidence			0.063	
			(0.464)	
Respective party variable X Election	**-0.005***	**-0.007****	**-0.802****	**-0.009****
	(0.003)	**(0.003)**	**(0.394)**	**(0.004)**
Lagged dependent variable			0.788***	0.929***
			(0.110)	(0.023)
Log GDP per capita	0.514**	0.705	0.328	0.577***
	(0.204)	(0.492)	(0.220)	(0.194)
Output gap/ Growth	-0.121***	-0.261**	-0.001	-0.119***
	(0.031)	(0.118)	(0.026)	(0.031)
Ruling coalition size	0.073	-0.597	-0.112	0.119
	(0.852)	(0.913)	(0.774)	(0.855)
Unified government	1.016***	0.703	0.148	1.033***
	(0.317)	(0.494)	(0.189)	(0.318)
Constant	-0.887	-5.063	2.580**	0.666
	(0.897)	(3.891)	(1.284)	(0.852)
Time dummies	Yes	Yes	Yes	Yes
Hansen statistic (p-value)	1.00	1.00	1.00	1.00
AR (1)	-4.20***	-4.06***	-2.29**	-4.282***
AR (2)	0.46	1.24	-0.47	0.387
Number of observations	1,128	998	419	1,128
Number of countries	65	65	57	65

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ System-GMM specification analogous to explanation in table 1.

The specifications in Table 3 investigate the sensitivity of the results to an alternative rule for election timing. The specifications use all three dependent variables (Brender and Drazen's expenditure measure, our extended expenditure measure, and general government final consumption expenditure), and all three measures of party institutionalization (average party age, ruling party age, and confidence in parties). However, as a check on whether the earlier results captured pre-electoral fiscal expansions, elections are coded as occurring in the previous year if the election falls in the first month of the fiscal year. As Table 3 indicates, the coefficient on our key variable of interest, the interaction of the party variables with the election variable, is everywhere robust to this alternative coding.

Distinguishing the effects of age of democracy and party organization

The results in Tables 1, 2 and 3 provide strong evidence that political party characteristics have a significant effect on political budget cycles. Although we do not have direct measures of party organization, we are able to use diverse proxies that each have a plausibly strong correlation with features of political parties that attenuate political incentives to spend more around elections. In addition, the GMM estimates address the possibility that unobserved country characteristics might drive both political budget cycles and the emergence of well-organized political parties (or longer-lived parties and parties in which citizens express greater confidence). Nevertheless, in view of important prior research demonstrating that political budget cycles are significantly larger in young democracies, the results here raise the question of whether the party effects we observe are simply the result of spurious correlation between party and democratic age. We address this question here.

In fact, the evidence below indicates that party characteristics themselves explain the differences in political budget cycles between younger and older democracies. We extend Brender and Drazen's dummy for new democracies across time. To see if the party effects observed earlier are the spurious result of correlation with the age of democracy, we construct a second interaction term consisting of this

New Democracy dummy variable and elections. We then re-estimate our main specification, column four in Table 1, additionally controlling for this new interaction term and the new democracy dummy.

If the party age results in Table 1 are the spurious result of the correlation of party age with democratic experience, then the party age-election result should be insignificant in this new regression. If, on the other hand, differences in political party characteristics account for the larger political budget cycles in younger relative to older democracies, then the estimated magnitude of the age of democracy interaction should decline. The results reported in Table 4 support the latter interpretation.

The first two columns use the original database from Brender and Drazen (2005) and their New Democracy dummy variable. Column one excludes the party variables and shows, as in their results, political budget cycles are larger in new democracies. The second column adds the party age variables. The party age variable remains significant and of a similar magnitude as in Table 1. In contrast, the new democracy interaction falls in magnitude and loses significance. Columns 3 and 4 repeat the exercise, using our extended sample; we supplement Brender and Drazen's measure of new democracy following their coding rules. The results are exactly the same: in the absence of party variables, new democracies exhibit significantly larger budget cycles. The estimated effect of new democracy falls and becomes insignificant after controlling for the party variables. However, controlling for the new democracy variables, political budget cycles are still more pronounced in countries with younger political parties.

These results support the argument that effects of young democracies on political incentives to pursue political budget cycles emerge because of the nature of political parties in young democracies. The results are entirely inconsistent with the possibility that the results on party age reported here arise because of the spurious correlation between party age and the unobserved features of new democracies.[15]

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[15] The party age – election interaction term is robust to other, similar controls. The earlier discussion on polarization observes that the introduction of a polarization – election term does not affect the party age results, although Alt and Lassen

Table 4: Robustness checks: Party age or democratic age?

Dependent variable: Central government total expenditures	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Original B&D sample		Base sample			
Election	-0.113 (0.173)	0.404 (0.255)	0.032 (0.161)	0.408* (0.224)	0.613** (.251)	0.787 (0.297)
New democracy	-0.249 (0.654)	0.190 (0.876)	-0.137 (0.645)	0.080 (0.655)		
New democracy X Election	**0.744** (0.368)**	**0.474 (0.360)**	**0.579* (0.342)**	**0.438 (0.352)**		
Years of continuous competitive elections					-0.019 (0.015)	-0.019 (0.014)
Continuous election years X Election					**-0.013* (0.007)**	**-0.008 (0.007)**
Avg. party age		0.005 (0.360)		-0.003 (0.006)		-0.002 (0.006)
Avg. party age X Election		**-0.008** (0.003)**		**-0.006** (0.003)**		**-0.006** (0.003)**
Lagged dependent variable	0.913*** (0.022)	0.904*** (0.025)	0.936*** (0.021)	0.929*** (0.022)	0.936*** (0.022)	0.930*** (0.024)
Log GDP per capita	0.737 (0.473)	0.811 (0.516)	0.441** (0.021)	0.618*** (0.172)	0.736*** (0.230)	0.842*** (0.214)
Output gap (Growth)	-0.228* (0.116)	-0.256** (0.117)	-0.109*** (0.028)	-0.121*** (0.030)	-0.112*** (0.029)	-0.121*** (0.031)
Ruling coalition size	-0.470 (0.912)	-0.524 (0.949)	0.104 (0.852)	0.224 (0.871)	0.151 (0.849)	0.274 (0.858)
Unified government	0.635 (0.387)	0.702 (0.486)	0.890*** (0.296)	1.012*** (0.325)	0.837*** (0.285)	0.976*** (0.315)
Constant	-5.082 (4.156)	-1.008 (4.480)	0.679 (0.962)	0.459 (0.003)	-0.794 (0.934)	0.792 (0.859)
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
Hansen statistic (p- value)	1.00	1.00	1.00	1.00		
AR (1)	-4.15***	-4.13***	-4.32***	-4.28***	-4.36***	-4.29***
AR (2)	0.95	1.19	0.43	0.37	0.29	0.31

find that polarization significantly conditions the incentives of politicians to spend money around elections. Shi and Svensson observe that political budget cycles are more common in poorer countries. We find, however, that adding an interaction between elections and the log of income per capita also has no effect on the party age results. All of these results are available upon request.

| Number of					1,184	1128
observations	1,053	998	1,184	1,128		
Number of countries	65	65	65	65	65	65

*** p<0.01, ** p<0.05, * p<0.1

One could argue that in such a horse race, continuous variables like party age will explain more variation than dichotomous variables, like New Democracy. However, when we replace the dichotomous "New democracy" measure in Table 4 with a continuous measure of democratic age (the number of years of continuous competitive elections, from the Database of Political Institutions), it is still the case that the party age-election interaction term is large and significant. Columns 5 and 6 of Table 4 display these results. Column 5 shows that in the base sample, the interaction between years of competitive elections and the election variable is negative: in older democracies, political budget cycles are smaller. The column 6 specification adds the party age variables. The party age interaction is significant and large – as before, political budget cycles are dampened in countries with older political parties. The years of election interaction is no longer significant, providing additional evidence that the democratic age effect operates through political parties.[16]

Figure 1a graphically displays the results from Column 4 in Table 4. The magnitude of political budget cycles declines significantly with the age of political parties. No significant evidence for the cycles can be found when parties are older than 50 years: the 95% confidence interval around 50 years does not exclude zero. Figure 1b indicates that the sample includes many observations on both sides of the 50 year threshold: 57.4 percent of the observations are below this threshold. The diminishing effect of political budget cycles in countries with older political parties is not an artifact of the rarity of such countries.

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[16] We also perform the reverse exercise and ask whether results are robust to substituting a continuous version of party age with a dichotomous version, a dummy variable equal to one if average party age is older than the median (43 years) and zero otherwise. Our results in columns 3 and 4 are similar or stronger after making this substitution. The party age interactions, using the dichotomous party variable, are highly significant and the New Democracy interaction loses significance.

Figure 1a: The effect of elections on expenditure, conditional on party age
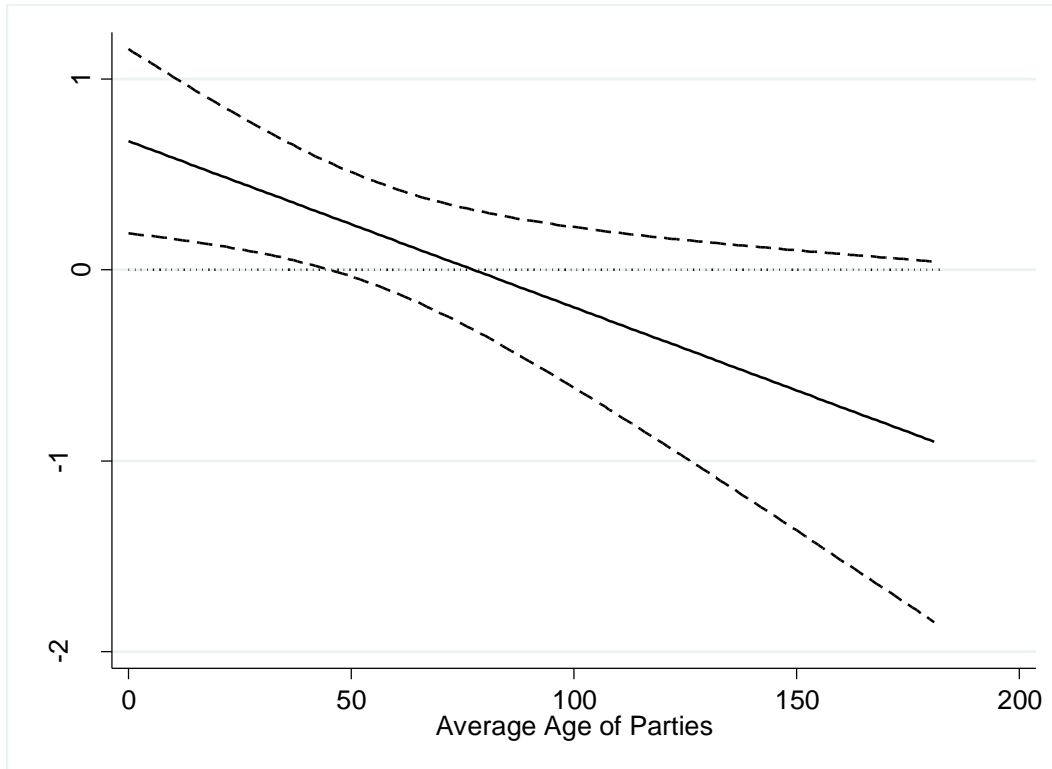
Notes: The figure displays the marginal effect of elections on expenditure, conditional on party age. The dotted lines represent 95% confidence intervals. Where confidence intervals include the value of zero on the y-axis, a political budget cycle cannot be detected statistically. The figure is based on the main regression in Table 1, column 4.

Figure 1b: Histogram of party age
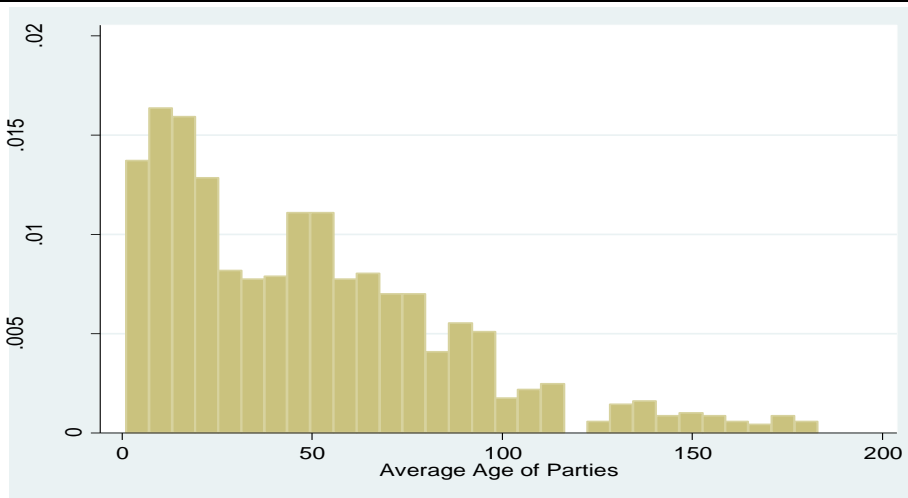
Notes: The histogram displays the distribution of observations across the average age of parties, corresponding to the sample used in Table 1, column 4. 57.4 percent of observations score 50 years or less on the party age variable.

Conclusion

We identify a new determinant of political budget cycles, with robust evidence that countries with younger political parties experience larger cycles. A growing literature on political parties points to two functions that organized parties can fulfill that potentially explain these findings. On the one hand, organized political parties can provide voters with information that allows them to distinguish candidate characteristics. This links parties directly to one prominent explanation for political budget cycles: where citizens are uncertain of candidate characteristics, particularly their competence, politicians have greater incentives to engage in pre-electoral spending .

The literature also points to an important role for political parties in reinforcing the credibility of candidates' pre-electoral commitments to voters, a function again of a party's organization and ability to solve collective action problems among party members. The inability of political actors to make credible commitments has been linked to increased shirking and rent-seeking by politicians (Ferejohn 1984), clientelism (Keefer and Vlaicu 2008), and significant differences in the policy choices of younger and older democracies (Keefer 2007). Hanusch and Keefer (2013) identify a link between credible commitments and cyclical spending, in the form of vote-buying: non-credible politicians are more likely to buy votes.

The analysis here raises questions for future research. One is to examine election year spending in greater detail to more directly establish the underlying mechanisms that drive political budget cycles. For example, are they, in fact, intended to signal candidate competence (the information channel), or to purchase votes (the credibility channel)? Another is to open up the black box of party organization with more informative variables than party age. What are the precise organizational characteristics of parties that allow them to solve voter information and politician credibility problems, and under what conditions to parties adopt these characteristics? These questions are of general interest, since they also

inform the broader incentives of political actors to pursue development-oriented public policies in

younger and poorer democracies.

Appendix 1

Table A1: Re-coding of the election dummy

Country	Year	B&D	Adjusted
Austria	1996	election	no election
Ecuador	2000	election	no election
Fiji	1994	no election	election
India	1983	election	no election
India	1984	no election	election
Lithuania	1997	election	no election
Lithuania	1998	no election	election
Mauritius	1987	election	no election
Mauritius	1988	no election	election
Nepal	1997	election	no election
Pakistan	1989	no election	election
Papua New Guinea	1999	election	no election
Peru	2000	no election	election
Philippines	1995	election	no election
South Africa	1994	no election	election
South Africa	1995	election	no election
South Africa	1999	no election	election
South Africa	2000	election	no election
Trinidad and Tobago	2001	no election	election
Venezuela	1998	no election	election

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