

# Promises, promises: Vote-buying, institutionalized political parties and political budget cycles

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**Abstract:** This paper advances and tests a novel explanation for both vote-buying and political budget cycles. The former occurs because politicians cannot make credible commitments to voters regarding future policies and, instead, use pre-electoral transfers to mobilize electoral support. Such transfers trigger political budget cycles: they are often large in the aggregate, underwritten by government resources, and are rationally concentrated in the period just before elections are held. We use three proxies for the ability of politicians to make credible commitments to voters: the average age of all parties and the age of the government party at the time the current leader took office; and average country responses to a World Values Survey question asking about respondents' confidence in political parties. Using any of these variables, political budget cycles are significantly larger in countries where politicians are less able to make credible commitments.

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## **Promises, promises: Vote-buying, institutionalized political parties and political budget cycles**

Political budget cycles have long preoccupied scholars. Recent research has documented that the cycles are more pronounced in poor (Shi and Svensson 2006) and new (Brender and Drazen 2005) democracies. These researchers trace this variation across democracies in the amplitude of political business cycles to limited voter information, either about incumbent competence and public debt accumulation (Shi and Svensson) or about whether democracy serves citizen interests (Brender and Drazen). The analysis here draws on a different source of variation, the ability of even fully-informed citizens to act collectively to hold governments accountable for policy failure. Consistent with this argument, we show that political budget cycles are most pronounced in countries with less institutionalized political parties, the main organizational vehicle for mobilizing citizens for collective political action.

Our argument bridges a gap between research on political budget cycles and on the use of pre-electoral payments – vote-buying – to mobilize electoral support. From Ghana (Lindberg 2003) to Argentina (Stokes 2005), scholars have emphasized the importance of vote-buying as an important element of political competition. The argument here proposes a new explanation for greater reliance on vote-buying in some countries than in others, rooted in the ability of political competitors to make credible pre-electoral promises. As Keefer and Vlaicu (2006) demonstrate, in settings where politicians cannot make broadly credible commitments to citizens, they have strong incentives to provide targeted goods to narrow groups of citizens and to seek rents, and weak incentives to provide public goods. Keefer (2007) shows significant policy differences between countries with fewer and more years of continuous competitive elections that are exactly consistent with the inability of political competitors in younger democracies to make pre-electoral promises that are credible to more than narrow groups of voters.

The corollary of these arguments, explored here, is that where political credibility is limited, politicians are also more likely to rely on spot payments to mobilize electoral support. Vote-buying and related electioneering expenditures in less mature democracies – exactly those where the literature has identified the most striking evidence of political budget cycles – appear to be many times those of campaign costs in developed democracies. One key reason why political promises may not be credible is the inability of citizens to act collectively to enforce them. In young and poor democracies, political parties, the primary vehicle through which politicians can make their commitments credible to voters, are not institutionalized to perform this task: they are not organized to choose candidates who subscribe to a particular party program, nor to allow members to replace leaders who diverge from a program.

By linking the phenomenon of vote-buying to political budget cycles, the analysis sheds new light on both. With respect to vote-buying, the existing literature abstracts from why politicians would rely on pre-electoral payments rather than promises of (possibly larger) post-election transfers to mobilize support. The difference is fundamental, however. The clientelist networks that are believed to underlie vote-buying should be characterized precisely by the ability of patrons at the head of those networks to make credible commitments to the clients who are members of the networks. Patrons should therefore prefer to make promises of future policy benefits to clients rather than purchase their votes prior to the election. The analysis here predicts that politicians should target vote-buying efforts precisely to non-clients, those to whom they cannot make credible commitments.

The analysis also offers an alternative, complementary explanation of political budget cycles. Previous analyses have emphasized information asymmetries: voters' inexperience with democracy, (Brender and Drazen) or lack of knowledge of politician competence and the extent of government borrowing (Shi and Svensson 2006, Alt and Lassen 2006a, b). The analysis here demonstrates that

political budget cycles can emerge even when voters are fully informed about politician characteristics and the budget constraint as long as voters cannot act collectively to sanction politicians who renege on their pre-electoral promises.

The next section reviews the contribution of the paper to the literature on political budget cycles. A theoretical section then demonstrates rigorously how limited political credibility can give rise to an increase in spending in pre-electoral periods. Because the crux of credible commitment is the ability of citizens to act collectively, the section following presents a heuristic account, rooted in previous literature, of how political parties with particular organizational features can facilitate citizen collective action and cement political credibility. Party characteristics are key to the empirical tests reported below. To lend credence to the prediction that pre-electoral expenditures are sufficient to drive political budget cycles, the paper turns to the literature on vote-buying. This section surveys case studies that confirm that vote-buying is quantitatively important and financed by government spending. It also reviews the vote-buying literature to pin down the contribution made by the paper to the analysis of vote-buying. The paper concludes with extensive tests of the hypothesis that, in the presence of weakly institutionalized political parties, which do not facilitate citizen collective action, political budget cycles are more pronounced.

### **Previous research on political budget cycles**

Incomplete information is a central element of previous explanations of political budget cycles. In Rogoff and Sibert's (1988) and Rogoff's (1990) original analyses, a moral hazard problem arises because voters can only observe all components of the budget with a time lag, hindering full and contemporaneous accountability of government. Political candidates differ in their competence. An adverse selection problem arises because, while politicians know their own competence, voters do not. Given imperfect information on the national budget, incumbents have an incentive to manipulate fiscal policy to signal their competence: since voters can neither observe competence nor

all components of the budget, incumbents can use less visible instruments like seignorage or, indeed, public debt, to finance pre-electoral expansions, thus suggesting to voters that those expansions result from an increase in administrative efficiency. In this class of models, only competent incumbents will manipulate before elections as their administrative skills enable them to reduce the associated costs of such manipulations.

Later models of political budget cycles eschewed the adverse selection element and focused exclusively on moral hazard (see especially Shi and Svensson, 2006 and Alt and Lassen, 2006a, drawing on Persson and Tabellini, 1990 and Lohmann, 1998). In these models, political candidates do not know their own competence. 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. As voters are rational, they understand this incentive and political budget cycles are fully expected.

A number of recent studies have used this model class to explore how variation in the extent of information asymmetries conditions the magnitude of political budget cycles. Shi and Svensson (2006) vary the proportion of voters who are informed through the media; 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. Brender and Drazen (2005) do not develop a formal model in support of their argument, but they too focus on incomplete information. They 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" (Brender and Drazen 2005, p. 1273).

The microfoundations of competence models of political budget cycles have not been tested. However, Brender and Drazen (2008) examine the effect of fiscal expansions on incumbents' likelihood of reelection. They find that in many countries, pre-electoral expansions are punished at

the polls. This contradicts the original Rogoff and Sibert (1988) model type. According to the intuition of the model voters should *reward* governments for pre-electoral expansions since expansions serve as signals for incumbent competence (see especially Rogoff, 1990). The findings are also at odds with the widely-used moral hazard model of political budget cycles. Since pre-electoral expansions are fully expected, they should not have an impact on the vote choice. Although this theme deserves further study, these findings raise doubts about the validity of existing models of political budget cycles.

We therefore move away from incomplete information and propose an alternative rationale for political budget cycles: the ability of politicians to make credible pre-electoral promises only to narrow groups in society. There is no voter uncertainty about incumbent competence, how democracy functions, or how government spending is financed. Empirical tests of the predictions of the model, presented below, indicate that political credibility, proxied by various measures of political party institutionalization, is significantly and inversely related to political budget cycles, and that these results are robust to controls for other measures that have been used to test alternative theories of budget cycles, country income and country years of continuous competitive elections.

Other research looks at cycles in the composition rather than magnitude of government spending. Khemani (2004) focuses on state-level budget cycles in India and finds no evidence that total spending increases in election years, but strong evidence that road construction spending rises. She attributes this to political volatility within electoral periods: chief ministers of Indian states often did not finish their terms during the period she examines, reducing their incentives to expend resources for electoral purposes until just before the election. This affects the composition of spending, rather than total spending, because some types of spending are more informative about politician ability than others. The relatively unusual level of intra-period political instability in India may explain why her results, in contrast to the cross-country research, reveal no evidence of

aggregate expenditure cycles. In Drazen and Eslava (2008), voters are uncertain about the number of swing voters and about whether politicians share their preferences regarding the composition of public spending; political budget cycles emerge because knowledge of swing voters is most precise before elections. As with research on expenditure cycles, voter information is key to research on composition cycles.

### **Modeling credible commitment and political expenditure cycles**

One option that politicians have when they are confronted with the inability to make credible pre-electoral promises to voters is to invest resources to increase their credibility, including building political parties, making reputational (advertising) investments, etc. The conditions under which they make these expenditures are analyzed in Keefer and Vlaicu (2007). Another, the focus here, is to participate in the spot market for votes, exchanging gifts and money for votes or engaging in other election-day strategies that mobilize the support of the voters who cannot be reached with promises of future action. The model in this section identifies conditions under which these spot markets exist and shows that the existence of such markets is sufficient to generate political budget cycles whose amplitudes are greatest in countries where politicians are least credible.

The logic of the model is easily summarized. When politicians cannot make credible commitments to voters, they resort to spot transactions: money for votes. Vote-buying generates political costs. Though it helps mobilize voters who do not believe political promises, it is financed by voters who do believe those promises, constraining politicians in their ability to appeal to those voters. Vote-buying emerges, therefore, when politicians lose fewer votes by reducing promised transfers to voters who believe their political promises than they gain by increasing transfers (vote-buying) to voters who do not believe their promises.

This logic is easily conveyed more formally. Assume a probabilistic voting framework with heterogeneous groups, as in Dixit and Londregan (1996). As in Keefer and Vlaicu (2007), the

electorate consists of a continuum (rather than discrete number) of groups of measure  $N$ , each group of measure one. All citizens have the same income, normalized to one. Each group is indexed by the variable  $m \in [0, N]$ . Two political parties,  $A$  and  $B$ , compete for power. Voter  $i$  in group  $m$  has a partisan bias given by  $\sigma_i(m)$ . Positive values of  $\sigma_i$  signal that voter  $i$  prefers party  $B$ ; negative values, party  $A$ . As is usual, to deliver a closed form solution the bias in group  $m$  is assumed to have the density function  $\phi(m)$ , distributed uniformly over the interval  $\left[-\frac{1}{2\phi(m)}, \frac{1}{2\phi(m)}\right]$ . In groups with greater dispersion, the distance of the average group member from the unbiased median is greater, making it harder for politicians to mobilize such groups with transfers and public goods.

Every member of group  $m$  has preferences over government policy represented by the familiar quasi-linear utility function  $W(m) = 1 - \tau + I[f(m)] + J[k(m)] + H(g)$ :  $\tau$  is the tax rate;  $f(m)$  is the per capita transfer promised to members of group  $m$  and  $I[f(m)]$  is the utility of those transfers;  $k(m)$  is the per capita transfer made to members of group  $m$  just before the election and not previously promised to them and  $J[k(m)]$  is the utility of those transfers ( $J$  may equal  $I$ );  $g$  is public good provision and  $H(g)$  is the utility of public goods to all members of all groups. The functions  $I, J$ , and  $H$  are non-decreasing, concave and differentiable and equal to zero when transfers or public good provision are zero. As in Keefer and Vlaicu (2008), to capture the idea that transfers incur at least some small deadweight loss, the marginal effect of transfers and vote-buying on utility is less than one for all positive transfers:  $J'[k(m)] < 1, I'[f(m)] < 1$ , and equal to one only when transfers or vote-buying are equal to zero. Governments may also extract rents,  $r$ . Transfers  $f$ , along with public goods  $g$  and taxes  $\tau$ , are pre-electoral promises that are implemented post-election.

Crucially, these pre-electoral promises are credible only to a subset of groups,  $m_c \in [0, n]$ . The remaining groups  $m_{nc} \in (n, N]$  do not believe pre-electoral political promises.

Without loss of generality, assume that in each set,  $m_c$  and  $m_{nc}$ , groups with higher index numbers exhibit greater dispersion in partisan bias (lower densities  $\phi(m)$ ).

Two additional assumptions are common in probabilistic models and used here. First, political parties seek rents  $(R + \gamma r)$ , where rents  $R$  are non-pecuniary “ego” rents, and pecuniary rents  $r$  are discounted by  $\gamma \leq 1$ , the costs to politicians of turning public into private resources. Second, while politicians know the distribution of the partisan bias of the electorate, the distribution is subject to a shock just before the election. The shock,  $\delta$ , is also distributed uniformly, over the interval  $\left[-\frac{1}{2\psi}, \frac{1}{2\psi}\right]$ .

The order of play is the following. In the period before the election, politicians make vote-buying expenditures and pre-electoral promises regarding taxes, transfers and public goods. Their promises cover two periods: the immediate post-election period, and the period following, which immediately precedes the next election. A shock to partisan bias then occurs and the electorate then votes. After the election, the winner carries out the promised policies in both periods.

There are several things to note about this set-up. First, pre-electoral promises must take into account expected vote-buying two periods hence, just prior to the next election. Current vote-buying is irrelevant to those promises. However, the only parameters that affect policy are  $n$ , the groups to which politicians can make credible agreements, and  $\phi(m)$ , the distribution of partisan bias in each group. The first is fixed over time. The second is subject to a shock, but the shock before the current election and the shock before the next election are uncorrelated. As a consequence, the expected distribution of partisan bias in period zero, before the election in period zero, is the same as the expected distribution in the second post-election period, before the election at the end of that period:  $E_0[\phi_0(m)] = E_2[\phi_0(m)] = \phi(m)$ . Consequently, voters and politicians rationally treat vote-buying in the current elections as indicative of vote-buying two periods hence,

prior the next election. This allows the costs of current vote-buying to enter into the budget constraint governing pre-electoral promises made regarding the two post-electoral periods.

Second, politicians have no incentive to make promises to groups  $(n, N]$  that do not believe the promises. Since these groups do not believe their promises, their votes are not affected by them, and politicians do not make them. Instead, politicians target promises only to those voters who believe them, those in groups  $[0, n)$ . This implies, among other things, that for all  $m \in (n, N]$ , promised transfers  $f(m)$  are zero.

Third, politicians engage in spot market transactions for votes only with groups  $(n, N]$  that do not believe their promises. Groups that believe politician promises cannot extract higher welfare from the political process than the policies that politicians promise prior to the election. There are two ways to see this. First, vote-buying can be viewed as transfers above and beyond those that were already promised. However, since nothing prevented politicians from promising these taxes and transfers in the first place, the fact that they did not implies that voters preferred that they did not. Second, vote-buying in the second period can be seen as a preference by these voters for higher consumption in the second period than in the first. Even in the absence of discounting, from which the analysis abstracts, they cannot have such a preference. From the concavity assumptions on  $I$  and  $H$ , voters experience diminishing marginal utility to government spending. By itself, this leads citizens who believe political promises to prefer smooth spending over time rather than lower benefits in the first period and higher in the second. Hence, for all voters in groups who believe political promises,  $m \in [0, n)$ , vote-buying transfers  $k(m)$  are equal to zero.

Fourth, only in the immediate pre-electoral period can politicians buy the votes of voters who do *not* believe their promises. They cannot credibly commit to provide benefits to these voters after the election (in the immediate post-election period). At the same time, these voters cannot credibly commit to vote for them in the next election if they receive the benefits in the period

immediately following the last election. That is, vote-buying must be a spot transaction in the immediate pre-electoral period and therefore shows up only in the second period budget constraint. Other mobilization expenditures – bringing voters to the polls, for example – must by definition be made just prior to or during the election.

Voter  $i$  in group  $m$  votes for party  $A$  if party  $A$ 's policy vector  $\mathbf{q}_A = [\tau_A, f_A(m), k(m), g_A]$  offers her greater welfare than  $B$ 's, after taking partisan bias into account. Party  $A$ 's share of the vote is therefore given by

$$\pi_A(\mathbf{q}_A, \mathbf{q}_B) = \frac{1}{2} + \frac{1}{n} \int_0^n \phi(m) \left[ \left( \sum_{t=1}^2 [1 - \tau_{t,A} + I[f_{t,A}(m)] + H(g_{t,A})] \right) - W_i(m)(\mathbf{q}_B) - \delta \right] dm + 1N - nnN\phi m / km - Wim\mathbf{q}_B - \delta dm.$$

The vote share depends on the average, across groups, of the partisan preferences of the swing voters, taking into account both the competing policy offers of the two parties and the swing voters' partisan bias. The swing voter in each group  $m$  is that voter whose partisan bias towards party  $A$  is just equal to the difference between party  $A$ 's policy offer and the sum of party  $B$ 's policy offer and the shock to partisan bias:  $\hat{\sigma}(m) = W(m)(\mathbf{q}_A) - W(m)(\mathbf{q}_B) - \delta$ .

Using the distribution of the partisan shock, the probability that party  $A$ 's vote share will exceed one-half is then given by

$$\begin{aligned} & \mathbf{P} \left\{ \pi_A(\mathbf{q}_A, \mathbf{q}_B) > \frac{1}{2} \right\} \\ &= \frac{1}{2} \\ &+ \frac{\psi}{\frac{1}{n} \int_0^n \phi(m) dm + \frac{1}{N-n} \int_n^N \phi(m) dm} \left\{ \frac{1}{n} \int_0^n \phi(m) \sum_{t=1}^2 [1 - \tau_{t,A} + I(f_{t,A}(m)) + H(g_{t,A}) \right. \\ &\left. - W_i(m)(\mathbf{q}_B)] dm + \frac{1}{N-n} \int_n^N [\phi(m)J(k(m)) - W_i(m)(\mathbf{q}_B)] dm \right\} \end{aligned}$$

Competitors choose policies that maximize their expected rents – their probability of election times the rents at stake – given the policies of the other party. Party  $A$  therefore maximizes

$$\begin{aligned} \max_{\tau, f, k, g, r} \mathbf{P} \left\{ \pi_A(\mathbf{q}_A, \mathbf{q}_B) > \frac{1}{2} \right\} \cdot \sum_{t=1}^2 (R + \gamma r_{t,A}) \\ \text{s. t. } N\tau_1 = \int_0^n f_1(m) dm + g_1 + r_1 \\ N\tau_2 = \int_0^n f(m) dm + \int_n^N k(m) dm + g_2 + r_2 \end{aligned}$$

Party  $B$ 's problem is symmetrical.

The maximization problem addresses the time-inconsistency problem in vote-buying. Vote-buying contributes nothing to the welfare of voters who believe politician promises so, in principle, politicians who can credibly commit to less vote-buying can make correspondingly more attractive promises to those voter groups that believe them. This promise is only credible, however, if these voters can credibly commit to switch their support away from the incumbent who violates the no-vote-buying pledge by unexpectedly raising taxes to finance vote-buying before the next election. The maximization problem identifies exactly that level of vote-buying below which no such pledge is credible.

Consider an incumbent who, in the second period after his election, and confronting the need to buy votes for the next election, reneges on his commitment to forego vote-buying by unexpectedly raising taxes to finance vote-buying. In response, voters in groups that believe political promises can punish the incumbent by switching their support to the other politician. Only some can credibly commit to switching, however. Each group  $m$  contains a distribution of voters, ranging from those who exhibit a strong partisan bias in favor of the incumbent party to those who have a strong bias in favor of the challenger. Pre-electoral policy promises were set to attract the swing voter in each of these groups. As a consequence, those with greater partisan bias in favor of the

incumbent receive higher welfare than those in the group who are indifferent between the two parties: they receive the same policy benefits (public goods and transfers), but in addition receive the additional welfare that comes from being governed by a political party that they intrinsically prefer. These voters would have supported the party even if its policy promises had been less generous. If the incumbent party then reneges on the policy package and imposes higher taxes, those with sufficiently high partisan bias still enjoy higher welfare under the incumbent party than under the opposition party; they can therefore not credibly commit to switch their support to the opposition party.

As long as those with a small partisan bias are few relative to the votes that incumbents can mobilize by engaging in vote-buying, incumbents will therefore renege on their pre-electoral promises. The maximization problem captures exactly this tradeoff. Faced with vote-buying by the incumbent, however, the challenger will do the same. Anticipating this, voters who believe the promises of politicians will incorporate prospective vote-buying into their comparison of the two political parties.

The first order conditions of the maximization problem immediately yield key results concerning the relationship between credibility, vote-buying and political budget cycles, as well as other effects of limited credibility and vote-buying on public policy. For convenience, set  $\Psi =$

$\frac{\psi}{\frac{1}{n} \int_0^n \phi(m) dm + \frac{1}{N-n} \int_n^N \phi(m) dm}$ , the effects of partisan shocks on voters who believe and voters who do

not believe political promises. The first order conditions are:

$$f_t(m): \Psi \frac{1}{n} [\phi(m) \cdot I'(f_t(m))] (R + \gamma r_t) - \lambda_t = 0 \text{ for } f(m) > 0$$

$$k(m): \Psi \frac{1}{N-n} [\phi(m) \cdot J'(k(m))] (R + \gamma r_2) - \lambda_2 = 0 \text{ for } k(m) > 0$$

$$g_t: \Psi \frac{1}{n} \left[ H'(g) \int_0^n \phi(m) dm \right] (R + \gamma r_t) - \lambda_t = 0$$

$$\tau_t: -\Psi \frac{1}{n} \left[ \int_0^n \phi(m) dm \right] (R + \gamma r_t) + N \lambda_t = 0$$

$$r_t: \frac{\gamma}{2} = \lambda_t, \text{ since in equilibrium } W_i(m)(\mathbf{q}_A) = W_i(m)(\mathbf{q}_B).$$

Several conclusion immediately follow from these conditions. First, vote-buying is entirely incremental to spending. If there is vote-buying, therefore, it must give rise to political budget cycles: higher spending in the immediate pre-electoral period compared to the immediate post-electoral period.

**Proposition 1:** If  $k > 0$ , then  $N\tau_2 > N\tau_1$ .

**Proof:** The first order conditions for  $r_t$  indicate that  $\lambda_t$  – the marginal utility of additional rents to the party – is the same in both periods, regardless of vote-buying, and equal to  $\frac{\gamma}{2}$ . Substituting this into the first order conditions for  $g_t$  and dividing the first order condition for  $g_1$  by the condition for  $g_2$  yields  $\frac{(R+\gamma r_1)}{(R+\gamma r_2)} = 1$ . Rents, then, are also the same in both periods, regardless of vote-buying.

This implies, however, that public good spending  $g_t$  must also be the same in both periods. By identical logic, transfers to groups that believe political promises are the same in both periods.

Therefore, if there is any vote-buying at all in the second period, spending in the second period is also greater.

The intuition for this is straightforward: if it was worthwhile for politicians to set taxes high enough to promise a certain level of transfers and public good spending in the first period to voters who believe their promises, it continues to be worthwhile for them to use tax revenues to maintain those promises in the second period, regardless of vote-buying.

The conditions under which vote-buying occurs can be seen from the first order conditions for transfers in period two and vote-buying. These conditions indicate that vote-buying occurs as long as the vote-buying technology,  $J$ , is sufficiently efficient in using resources to improve voter

welfare; and as long as the density of partisan bias in the group targeted for vote-buying is sufficiently high relative to the average density of partisan bias among groups that believe political promises.

**Proposition 2.** Index the groups that do not believe political promises by  $n$ .

a) Vote-buying is positive if  $\frac{\int_0^n \phi(m)dm}{nN} < \frac{\phi(m_{nc})}{N-n}$ , the density of partisan bias of the targeted group is relatively high and the share of voters who believe political promises is relatively low.

b) Vote-buying is inversely related to the share of voters who believe political promises if

$$(N - n) \cdot \phi(n) > \frac{1}{n} \cdot \int_0^n \phi(m)dm.$$

**Proof:** The first part of the proposition follows immediately if the first order condition for  $k$  is

divided by the first order condition for  $\tau_2$ , yielding  $J'(0) = 1 > \frac{N-n}{nN} \frac{\int_0^n \phi(m)dm}{\phi(m_{nc})}$ . To prove the second

part of the proposition, differentiate the condition for optimal vote-buying by  $n$ :

$$\frac{\partial J'(k(m))}{\partial n} = -\frac{1}{n^2} \cdot \frac{\int_0^n \phi(m)dm}{\phi(m_{nc})} + \frac{N-n}{nN} \cdot \frac{\phi(n)}{\phi(m_{nc})}$$

Rearranging,

$$\frac{\partial J'(k(m))}{\partial n} = \frac{1}{n\phi(m_{nc})} \cdot \left[ -\frac{1}{n} \cdot \int_0^n \phi(m)dm + (N-n) \cdot \phi(n) \right]$$

By the concavity of  $J$ , when the right hand side is positive, vote-buying must fall with  $n$ .

Proposition 2 describes when vote-buying emerges and when vote-buying, and therefore political budget cycles, rise as the share of voters who believe political promises declines. The easier are the targets of vote-buying to persuade, and the more of them there are, the more likely is vote-buying to occur and the greater is vote-buying as the share of voters who do not believe political promises increases. Ease of persuasion is captured by the density of partisan bias. When it is more

dense, as is well-known, a larger share of voters in a group are clustered around the swing voter and are more susceptible to small changes in policy or vote-buying. The denser is the group targeted with vote-buying, and the lower the average density of groups who believe political promises, therefore, the more likely is vote-buying; under these conditions, vote-buying also increases as the share of voters who believe political promises declines.

These conditions hold under a wide range of circumstances under which general. Consider countries in which the distribution of partisan bias is the same across groups. Then the condition in (a) is fulfilled if  $n > 1$  and condition (b) is fulfilled for all  $n$  less than  $N-1$ . These limits on  $n$  become even weaker in a setting in which partisan bias is more dispersed – density is lower – among groups that believe political promises. That is, more group members have extreme partisan feelings among groups that believe political promises than among other groups. This might be the case, for example, in a setting in which voters prefer co-ethnic parties and the two parties that have established themselves aim their promises at two particular ethnic groups. If the average density of partisan bias is higher among groups who believe political promises than among groups, the range of values for  $n$  under which the proposition holds begins to narrow; such differences have to be improbably large, however, for the predictions not to hold at all.

One other prediction of the model is also useful. Vote-buying is often associated with corruption and, in most places, vote-buying is illegal. However, the association between vote-buying and rent-seeking behavior turns out to be more subtle than this and, in fact, there is no necessary association between vote-buying and rent-seeking, defined (as here) as politicians' accumulation of public resources for their own private uses. Consider an increase in the ability of politicians to make credible commitments, which in Proposition 2 is shown to reduce vote-buying under a broad range of circumstances. Proposition 3 shows that a similar increase in credibility reduces rent-seeking only

if the additional group that believes political promises has a distribution of partisan preferences that is more tightly clustered around the swing voter (is more dense).

**Proposition 3:** Rent-seeking falls as the share of voters who believe political promises increases if  $\phi(n) > \frac{1}{n} \int_0^n \phi(m) dm$  and rises if  $\phi(n) < \frac{1}{n} \int_0^n \phi(m) dm$ .

**Proof:** Optimal rent-seeking can be found by using the first order condition for  $r$  to substitute for  $\lambda$  in the first order condition for taxes  $\tau$  and rearranging:

$$r_t = \frac{N}{2} \left( \frac{\Psi}{n} \int_0^n \phi(m) dm \right)^{-1} - \frac{R}{\gamma}$$

The condition emerges immediately after differentiating with respect to  $n$ :

$$\frac{\partial r_t}{\partial n} = -\frac{N\Psi}{2n} \left( -\frac{1}{n} \int_0^n \phi(m) dm + \phi(n) \right) \left( \frac{\Psi}{n} \int_0^n \phi(m) dm \right)^{-2}$$

The term  $\phi(n)$  is the density of the marginal group that now believes political promises. If it is large relative to the average density of partisan bias among those who already believe political promises, then their inclusion among voters who believe political promises puts greater pressure on politicians to reduce rent-seeking and provide transfers and public goods. However, the effect is reversed if the condition goes the other way.

This ambiguous prediction contrasts with the findings of Shi and Svensson (2006), who conclude that political budget cycles are more pronounced in countries with greater corruption, since politicians able to extract higher rents have greater incentives to persuade voters of their competence. The argument here suggests a different interpretation of their results. In formal models of rent-seeking, such as the one here, rents accrue to the politician. Measures of corruption, however, refer to all forms of extraction that are illegal, including rents that politicians (illegally) transfer back to voters in the form of campaign expenditures. Consistent with this, the model

developed here shows that vote-buying is not systematically related to rent-seeking by politicians. In particular, in political settings where politicians feel pressed to engage in greater vote-buying, they are also compelled to restrict their efforts to seek pecuniary rents. Empirical measures of rent-seeking, however, such as the *International Country Risk Guide* measure of corruption used by Shi and Svensson, categorize as “corruption” all efforts to use public office to extract rents, whether the rents are used for vote-buying or for rent-seeking in the theoretical sense of politician self-enrichment.

A key prediction of this analysis, emerging from Proposition 2, is that vote-buying is greater the lower is the share of voters to whom politicians can make credible commitments. The remainder of the paper takes this prediction to the data. In particular, it looks at one key determinant of political credibility, the level of institutionalization of political parties, as proxied by their age, to show that in countries where political parties are older, political budget cycles are significantly less pronounced.

### **Political parties and political credibility**

The ability of political actors to make credible commitments is a function of citizens’ ability to sanction them if they renege. However, individually, citizens can do little to punish defaulting politicians; their ability to do so is a function of the ability of citizens to act collectively. For example, Ferejohn (1984) identifies the substantial scope for political shirking that arises when citizens can rely on nothing more than spontaneous coordination on an *ex post* voting rule to discipline politicians who under-perform. Citizens can mitigate the coordination problem if they can take advantage of organizations that facilitate collective action among them. Such organizations have two characteristics. First, members delegate to leaders the ability to discipline group members who free-ride. However, leaders can shirk on their responsibilities. To prevent this, second, organizational arrangements make it easy for members to observe leader actions and to replace them

if they fail to pursue member interests (e.g., by failing to sanction free-riding or by allowing members into the group who do not share group goals).

Political parties, in particular, can overcome citizens' collective action problems if they permit citizens to discipline party candidates who renege on their commitments and, consequently, allow those politicians to make credible commitments in the first place. Aldrich (1995) identified one obstacle to citizen action to discipline politicians, 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.

A second obstacle to accountability is informational: voters are not sure about the policy preferences of politicians. Snyder and Ting (2002) demonstrate that parties can reduce information costs to voters of identifying the policy preferences of politicians, but only to the extent that they adopt organizational rules that limit the preference heterogeneity of their candidates.

The third obstacle is collective action by voters themselves. However, parties with collectively organized politicians committed to a particular policy program are more likely to invest unilaterally in solving voters coordination problems. Moreover, though this is a subject for future research, it is plausible to conjecture that the ability of parties to make credible commitments attenuates voter coordination problems. In a world with no credible commitment, as in Ferejohn (1984), incumbents and challengers cannot differentiate themselves with respect to their future conduct and voters are correspondingly indifferent between them. Voters must coordinate on a performance threshold for the incumbent, but this is less likely to succeed if they have different beliefs about the value to the incumbent of holding office and about the effect of incumbent performance on their individual welfare. In contrast, confronted with candidates able to make

credible commitments, coordination succeeds if voters agree on the group of candidates who are most likely to win and if they believe that other voters will support the candidate from that group whose promises they most prefer.

Political parties often do not have the two characteristics needed to facilitate collective action by politicians and voters: group delegation to leaders to discipline free-riding, and easy oversight by group members of leaders. The first characteristic is lacking in parties comprised of politicians with strong clientelist networks. They know that their individual support base is sufficient to get them elected in a plurality system, no matter which party they join; and in a proportional system is sufficient to make them attractive to any party's list. They have less interest, then, in exposing themselves to the discipline of party leaders. At the same time, party leaders are also often reluctant to embrace the second characteristics. They prefer not to make it easy for members to replace them in the event of malfeasance.

If institutionalized parties are key to credible commitment, they should also influence policy and economic outcomes. Consistent with this, Keefer (2011) identifies a significant association between the degree to which parties are organized to solve citizen collective action problems and public policy outcomes. In their analysis of ruling-party institutionalization in non-democracies, Gehlbach and Keefer (2009) argue that simply allowing higher information flows about leader behavior among ruling party members than among non-members is sufficient to increase the credibility of leader commitments to party members. They find extensive evidence that non-democracies that exhibit ruling-party institutionalization therefore attract more private investment than those that do not.

### **Political budget cycles and vote-buying**

Three key claims undergird the analysis here: the magnitude of vote-buying expenditures can be high and more than sufficient to explain political budget cycles; political competitors use

government expenditures either directly or indirectly to finance vote-buying; and, in those countries where there is evidence of significant vote-buying, political parties are fragmented and the policy promises of political competitors appear to lack credibility. This section asks whether the qualitative and quantitative assessments of vote-buying in the literature are consistent with these claims. In addition, it surveys the vote-buying literature, which advances several explanations for why politicians undertake vote-buying, and identifies how the arguments advanced here differ from previous explanations.

### ***Evidence on the scope and financing of vote-buying***

No study captures all of the mechanisms that politicians use to buy votes. These range from pre-electoral handouts to individuals of money and food, to infrastructure projects targeted to specific communities, to “get out the vote” efforts meant to bring likely supporters to the polling stations (including paying voters for voting). Robinson and Torvik (2005), for example, explain the proliferation of inefficient and incomplete white elephants as a response to politicians’ lack of political credibility. The analysis here indicates that such projects can give rise to political budget cycles. Government spending can be used directly to finance vote-buying (through the expansion of pre-existing transfer programs or the acceleration of infrastructure projects) or indirectly (e.g., using government-funded infrastructure projects to raise money from contractors to finance vote-buying, as in Samuels 2002).

Every empirical study that attempts to assess vote-buying necessarily focuses on only a few modalities. These studies nevertheless provide evidence that the magnitude of vote-buying can be large and more than enough to account for political budget cycles. Brusco, et al. (2004) surveyed nearly 2,000 respondents in three Argentine provinces three months after the October 2001 elections. Forty-four percent of respondents said that parties had distributed food, clothing and other items to homes in their neighborhoods; seven percent of respondents acknowledging receiving

something themselves. Their survey abstracts from government transfers that could also have been used to mobilize electoral support.

Wang and Kurzman (2007) estimate that the costs of vote-buying and all other campaign expenditures associated with the elections of a single county executive in Taiwan amounted to at least eight million US dollars. Assuming the costs of this single election were one percent of total campaign costs incurred by the Kuomintang across all county and national legislative elections, total campaign costs would have amounted to 3.5 percent of government spending in 1993.<sup>1</sup>

More generally, studies have shown enormous campaign costs in democracies in which political promises lack credibility. Wurfel (1963) estimated campaign costs in the Philippines elections prior in the 1950s and 1961 (Ferdinand Marcos came to power in 1965) at approximately 13 percent of the national budget. A large share of the expenditures went to vote-buying. He cites other estimates putting total campaign costs in the United States in 1952 at less than .20 percent of the national budget. Even if actual campaign costs in the Philippines were half as high and those in the US were ten times as high, the difference is large. OpenSecrets.blog, an activist organization that tracks campaign costs in the United States, estimates that the total costs of the 2008 elections were \$5.8 billion, half of which they attribute to the presidential race. This was a tiny fraction of government spending (general government final consumption expenditure was 17 percent of total national income of approximately \$14.2 trillion). Keefer (2002) reports estimates by high-placed insiders who claimed that presidential campaigns in the Dominican Republic cost at least \$20 million, or \$2.50 per Dominican, compared to approximately \$1.00 per American represented by the \$193 million campaign of George W. Bush in 2000. Adjusted for differences in purchasing power

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<sup>1</sup> Total costs were likely more, taking into account campaigns for the national legislature that occurred in December 1992. The costs of campaigning in the single county were 248 million Taiwanese dollars. Final government consumption expenditures in 1993 were 971,912 million Taiwanese dollars, according to the National Statistics website, <http://ebas1.ebas.gov.tw/pxweb/Dialog/statfile1L.asp>.

parity-adjusted per capita income, which was more than seven times greater in the United States, the differences are on the order of 18 to 1 and large enough to give rise to observable political budget cycles.

Pre-electoral expenditures need not be confined to mobilizing voters. Weak parties also attempt to buy popular candidates. Callahan and McCargo (1996) said that before the 1995 elections in Thailand, parties offered members of Parliament representing northern Thailand 10 – 20 million Thai baht, or 400,000 to 800,000 US dollars to affiliate with their list.

Observers also argue that government financing plays a large role in vote-buying. Ockey (1994) reports that Thai parties typically use control of ministries to finance vote-buying. Wurfel (1964) claims that the incumbent Nacionalistas relied on government financing and the opposition Liberals on private wealth in the 1957 elections. Keefer (2002) does not identify government-financed pre-electoral expenditures in the Dominican Republic directly, but does report large government expenditures that were intended to prevent demonstrations against the incumbent president. Finally, across the 17 countries surveyed in the 2005-06 wave of the Afrobarometer survey, 19 percent of more than 20,000 respondents reported that they had been offered a gift in the last election.

### ***Credible commitment and other theories of pre-electoral transfers***

The bulk of the literature on pre-electoral transfers to voters is concerned with two issues: who do politicians target, swing or core supporters? how do they enforce the vote-buying transaction and, if they cannot, why do they do it? The analysis here is concerned with a third question, why these expenditures are so much larger in some democracies than in others.

Kitschelt (2000) comes closest to the argument here, when he concludes that vote-buying is more common in countries with non-programmatic political parties. However, his analysis, following the literature in this area, emphasizes the clientelist nature of vote-buying – the targeting

of transfers to particular narrow groups of voters – rather than its timing. That is, on the one hand, the literature defines clientelist politicians as those who are embedded in clientelist networks, distinguished by the ability of network members to make credible, inter-personal commitments to each other, if not to the broader community. On the other hand, though, the vote-buying literature does not explain why these politicians rely on pre-electoral payoffs to clients when they can make credible promises of post-electoral payoffs to them. Our argument is that political competitors make pre-electoral payments precisely to voters to whom they cannot make credible commitments.

Estimates of the determinants of vote-buying in Brusco, et al. (2004) are consistent with this explanation. On the one hand, as Dixit and Londregan (1996) predict, they find that parties in Argentina (largely the Peronists) target vote-buying to the poorest voters, for whom the marginal utility of transfers is highest. On the other, though, their evidence confirms the importance of targeting voters who are likely to be most skeptical of party promises, as in the analysis here.

First, they speculate that reliance on vote-buying was greater in 2001 because, in the years prior to the election, the Peronist party – most closely identified with vote-buying – had adopted strongly market-oriented policies, entirely at odds with the policies historically favored by the party's leaders. This would have clouded the party's programmatic appeal. Second, they find evidence that younger voters, who became politically active during this period when the party's programmatic stance was in flux, were most likely to report having received a handout from a party.

A related concern in the vote-buying literature is the enforceability of the vote-buying contract. From this literature, it could be possible to argue that variations in vote-buying depend on the costs of enforcing contracts in the market for votes. Especially with a secret ballot, political competitors might appear to be unable to observe whether targeted voters cast their ballots as agreed. Two key points emerge from the literature, however.

First, candidates actually have a substantial capacity to monitor the vote-buying contract, and considerable social sanctions at their disposal to deal with recipients who renege. From their extensive interviews in Argentina, Brusco, et al. (2004) conclude that party activists feel comfortable confirming voting behavior by observing the demeanor and actions of recipients of payments outside of the ballot box, together with the polling station results themselves. Moreover, their evidence suggests that vote-buying is most common when vote-buyers and vote-sellers are closely bound up in social networks, allowing vote-buyers to apply social and other sanctions to vote-sellers who renege. A multitude of other, discrete contracting devices also exist. Philippine politicians in some constituencies have distributed carbon paper ballots, requiring voters to return the carbon copies of the marked ballots to receive their payment. Afghan politicians have signed contracts with local patrons, paying them half of the money to buy votes before the elections and placing the other half in escrow with trusted local merchants upon delivery of the votes.

Second, though, parties have a high tolerance for non-compliance; they are willing to engage in vote-buying even when default rates are high. Wang and Kurzman (2007) look at the 1993 election of a county executive in Taiwan and conclude that at least 45 percent of voters who had sold their vote to the Kuomintang did not, in the end, vote for the party's candidate. The party anticipated this rate of defection, since it asked its vote-buying "brokers" to buy the votes of 67 percent of the constituency's voters.

Another debate in the literature concerns whether payoffs to voters are aimed at buying votes or simply turnout. Stokes (2005) concludes that the former is key. Nichter (2008) argues that that latter is more accurate, supporting his claim with evidence that the core supporters of the Peronist party in Argentina were most likely to receive handouts. The conclusions of the analysis here hold regardless of whether pre-electoral transfers are meant to persuade individuals to vote for one party rather than the other, or to persuade individuals to vote rather than to abstain. In both

cases, previous research assumes that voters differ only in the degree to which their ideological stance differs from that of the competing parties and abstract from the question of why pre-electoral payments are high in some electoral settings and not in others. The current analysis addresses this gap, maintaining the assumption that voters have an ideological bias towards the parties, but allowing parties to differ in their ability to attract voters with promises about their post-electoral policies.

The view of vote-buying outlined here is relatively benign: the vote-buying transaction between politicians and voters differs only in its timing from other transfers that are at the center of traditional political economy models. Moreover, it has no necessary connection with rent-seeking, in the sense of politician self-enrichment. This framing of the vote-buying transaction therefore is at odds with most discussions (e.g., Brusco, et al. 2004), which see it as distinctly corrosive. Here, the corrosive factor is the inability of citizens to act collectively to hold politicians accountable for their promises; pre-electoral payments are simply symptomatic of this. However, the interpretation is consistent with the conclusions of Kitschelt (2000), who argues that in weakly developed democracies, clientelist transactions – by which he means narrowly targeted transfers either before elections or after – are the only vehicle for distributing public sector benefits to citizens.

## **Data**

The foregoing analysis makes two predictions. First, the lack of political parties that are organized to facilitate collective action by citizens makes it difficult for politicians to make credible pre-electoral commitments and, instead, encourages them to engage in pre-electoral spending. We have no cross-country information on pre-electoral spending by individual politicians, however, we can test this prediction by noting that such pre-electoral spending, if large enough, should manifest itself in the form of political budget cycles. The second prediction, which is testable, is that in countries that lack such parties, political budget cycles should be larger.

Two kinds of data are key to this test. The first concerns the measurement of political party organization and the degree to which parties facilitate collective action to hold governments accountable. Direct measures of the internal characteristics of political parties that promote credible pre-electoral promises are not available, but three plausible proxies are. The first two are from the Database of Political Institutions (Beck, et al. 2001). One 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), or *partyage*. Younger parties are less likely to have developed the organizational characteristics that allow them to make credible commitments. First, they are more likely to be personalized vehicles for the party leader; such parties disappear when the leader departs, and are therefore disproportionately represented among younger parties. Gehlbach and Keefer (2010) argue that the ability of the ruling party to survive leadership transitions indicates that party members can undertake collective action independent of the party leader. Second, in societies where potential political candidates are better endowed with “clients” (e.g., by the cultural traditions and economic characteristics of the country), they are less likely to cohere into stable parties. Again, such parties will be more common among younger parties. By the same token, parties organized around the pursuit of particular programmatic policies are more likely to survive leadership transitions and the defection of clientelist politicians; they will be disproportionately common among older parties. Third, organizational arrangements to bind politicians together often take time to develop.

The first variable has the advantage that it takes into account information on up to four parties in a country. However, party age, *per se*, is a noisy indicator of the degree of independence of the party from the leader. To take the leader’s control of the party into account more directly, one ideal solution would be to use the age of the party at the time the party leader took over. This information is not available, however. Instead, following Gehlbach and Keefer (2010), we can use

information from the DPI on the age of the largest government party at the time that the leader of the country took office to construct the variable *ruling party age – years in office* .

The logic underlying the party age variables is distinct from the idea of a new democratic system as in Brender and Drazen (2005). For them, it is the experience with democratic institutions that determines how well citizens can hold government accountable. In our case, parties can exist even during non-democratic periods and develop organizational characteristics that make them appear credible to voters. To verify this distinction econometrically, we purge the effect of the age of democracy from *partyage* by first calculating the years elapsed since a country first held fully competitive elections.<sup>2</sup> The correlation between *partyage* and the age of democracy variables is 0.56. We regress the *partyage* variable on the age of democracy and show that our results are robust to using the residual from this regression (the component of party age not explained by the age of democracy).

The third variable we explore to capture whether political parties solve the collective action problems of citizens comes two relevant questions in the *World Value Surveys* (Inglehart 2004):

“I am going to name a number of organisations. 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?”

The questions are respectively asked for political parties and government. Conceptually, credibility and confidence should be closely related: how can voters have confidence in parties/government if they cannot credibly commit to carry out the policies they promise? Of the two questions, we

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<sup>2</sup>The first year that multiple parties could and did run for election, and no party received more than 75 percent of the vote, in both legislative and executive elections. That is, the countries receive the highest score of 7 on the Executive and Legislative Indices of Electoral Competitiveness in the DPI.

believe that the one relating to political parties is most closely related to the credibility of parties. Moreover, the effects of confidence in parties is not likely a reflection of general confidence in politicians and government: the party confidence results are robust to controlling for confidence in government.

To construct the variable *party confidence* we calculate the country-means for valid answers and construct indices where higher values imply higher confidence. The World Value Surveys are administered in waves, and not all countries are included in every wave. Our confidence series thus have considerable missing data. We take this into account in our estimation.

To mimic our analyses for the age of parties, we regress our confidence in parties variable on the age of democracies. This will allow us to test whether the effects in the analysis are driven by the age of the democratic system rather than party credibility. We name this measure *party-confidence(resid)*.

The budget data and other controls are taken from the original data set constructed by Brender and Drazen (2005). This data set provides the dependent variable, total expenditure of central government, the election dummy (*election*) that indicates whether a year was an election year or not, and their control variables: the output gap (computed using the Hodrick-Prescott filter), the log of real GDP per capita, the share of international trade as a percentage of GDP, and the fractions of the population aged 15-64 and above 65.

We focus on government expenditure as our dependent variable because our theory predicts vote-buying before elections, which should manifest itself in the form of pre-electoral expenditure hikes. This choice is consistent with Brender and Drazen's (2005) findings 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 expansions in expenditure.

Sample coverage poses a challenge for our analysis. Our party age variable is available from 1975 up to the year 2008 while Brender and Drazen's data are only available from 1960 to 2001, at

the most, and for many countries only the 1990s. We therefore extend their data with available historical data from the International Finance Statistics (IFS), provided by the International Monetary Fund (IMF). 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 later data. We also extend sample coverage to the following countries: Albania, The Bahamas, Botswana, Croatia, Ghana, Kenya, Latvia, Malta, Nigeria, and Thailand.<sup>3</sup> Overall we manage to gain another 307 observations compared to the Brender and Drazen dataset.

Sampling issues are more acute for the World Values Survey confidence measures, which are only available from the 1990s, with gaps. Neither the original nor the extended IFS data have sufficient coverage for estimation in this case. Since the IMF statistics division changed its fiscal accounting methodology during the 1990s, historical data phase out in the 1990s and early 2000s for some countries; the current data begin in 1990 but for most countries, data coverage begins only in the late 1990s. The only expenditure variable that is available throughout the 1990s and 2000s and has broad country coverage is general government final consumption expenditure from the World Bank's *World Development Indicators*.

This variable differs from the IFS series in that it includes general rather than central government spending. General government includes the central government and government at subnational levels. However, coverage of subnational spending is sporadic and unlikely to be a significant issue. 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. Nevertheless, the correlation between this variable and

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<sup>3</sup> We only include democracies, i.e. countries that score 7 on both the DPI's legislative and executive indices of electoral competitiveness.

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 and, if anything, should generate a bias against finding significant cycling results, to the extent that capital spending is more important for pre-electoral expenditures. Block's (2002) study of political budget cycles in Africa offers a second reason for confidence in this variable. He finds that fiscal expansions are particularly pronounced in government consumption expenditure.

Since these expenditure measures include more recent observations, we extend the Brender and Drazen control variables or replace them with reasonable alternatives. The key variable we take from their data set is the election dummy. We review it and make a few minor adjustments based on the DPI and, if in doubt, external sources.<sup>4</sup> Then we extend the measure in accordance with Brender and Drazen's coding rule.

Two important timing issues are at the center of 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 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 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 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

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<sup>4</sup> These adjustments are documented in appendix 1.

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. What, however, if election expenditures occur over many months prior to the election? Then, in this case, it would be more appropriate to code the election as having occurred in the prior fiscal year.

Since the actual timing of these expenditures is unknown, we, like Brender and Drazen, simply match the election year to the fiscal year in which it occurs. Using this methodology we replicate Brender and Drazen's election dummy (with a small number of adjustments reported in table A2). However, we also show that our results are largely robust to the use of two alternative election dummies. One, *election (M1)*, codes an election as occurring in the previous calendar year if it fell in the first month of the calendar year. The other, *election (M6)*, codes the election as occurring in the previous calendar year if it fell in the first six months of the current calendar year. The confidence in party results are robust to both changes. The party age results are robust to using the first of these two alternative variables.

With respect to control variables, we retrieve real GDP per capita and real economic growth data from the World Development Indicators. We include Brender and Drazen's international trade share and dependency ratios when we mimic their analyses. To test the robustness of our findings we replace these three variables, which are usually insignificant, with alternative controls. We thus add two political variables from the DPI to take into account alternative institutional rules that might influence political incentives to engage in pre-electoral transfers to voters. One is *system*, whether a country is presidential, semi-parliamentary, or parliamentary. With this variable we control

for the fact that many new democracies happened to choose presidential systems. A second control, *unified government* (names ‘allhouse’ in the DPI), captures the intuition provided by Saporiti and Streb (2008), that political budget cycles are less likely to occur if government is divided and a second chamber can veto the budget, especially before elections.

### Estimation

The empirical model for the analysis is

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

$F_{i,t}$  is government expenditure for country  $i$  at time  $t$ , and  $F_{i,t-1}$  is the lagged dependent variable. The  $\beta$ 's are the coefficients for our key variables and  $\gamma$  is a vector of control variables  $x$ . The variables *CREDIBILITY* and *ELECT* are the political party measures and the election dummies respectively. Our main prediction is that the interaction term *CREDIBILITY* \* *ELECT* is negative: the more that parties facilitate citizen collective action, the lower is election year spending. Finally,  $\mu$  and  $\theta$  denote country and time effects; the overall error term is given by  $\varepsilon$ .

To account for country fixed effects, we estimate this equation using the within-country transformation, i.e. fixed effects (FE). As is well known, this results in bias of order  $1/T$  in a dynamic model (Nickell 1981). The average sample length is 24 years for Brender and Drazen – our data series are significantly shorter as our key independent variables are only available from 1975 or later, whereas Brender and Drazen’s data series start in 1960. We are thus concerned about this dynamic panel bias. We therefore follow Brender in Drazen and use a general methods of moments (GMM) procedure which was 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.

Brender and Drazen use the ‘original’ Arellano-Bond estimator, 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. Our estimates for the lagged dependent variable range from roughly 0.7 to 0.8, so System-GMM seems 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 will demonstrate below that System-GMM indeed performs better than Difference-GMM.

The GMM estimator can be calculated in two steps. One-step GMM is calculated on the initial assumption of homoscedasticity. However, Arellano and Bond (1991) derive a robust version which does not perform significantly worse, even under considerable heteroskedasticity, than the two-step estimator (see Arellano and Bond (1991), Blundell and Bond (1998) and Blundell, Bond and, Windmeijer (2000)). Standard errors for the two-step version are moreover 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, we however do not detect any efficiency gains from bias-corrected two-step estimation. We therefore focus on the robust one-step estimator.

We restrict the number of instruments we use to a maximum lag number of three to prevent over-fitting. Where our sample contains a significant amount of missing observations, we collapse our instruments. The two standard tests of instrument exogeneity in GMM are based on Sargan

(1958) and Hansen (1982). Where the errors are heteroskedastic, Hansen is preferred. However, the power of the Hansen test falls rapidly with the number of instruments. Shi and Svensson (2006), for example, report Hansen scores of .99 in their GMM specifications.<sup>5</sup> Our set-up is similar to theirs and even though we restrict the instrument count the Hansen scores are similarly improbably high

Instead, to gauge our model specification we focus on two other diagnostics. Firstly, although we focus on GMM estimation, we report the coefficients for FE results. Since our emphasis on GMM is motivated by the downward bias in models including a lagged dependent variable (LDV) and unit effects (Nickell 1981), the LDV coefficient in a correctly specified GMM model should lie above the LDV coefficient in the FE model (Bond 2002). If this is not the case, the GMM model does not adequately address the endogeneity problem. This may be due to an inadequate lag structure of the instruments or because some of the other regressors are not strictly exogenous. Indeed, we identify endogenous regressors in our models and instrument them with past lags as well (for details see notes in tables 1-3). As a second diagnostic test, we report autocorrelation of order 1 and 2 in the first-differenced residuals where, given the lag structure of our instruments, our instruments are valid if there is first-order but no second-order autocorrelation.

In summary, like the literature on political budget cycles in general, we cannot directly test for the exogeneity of the GMM instruments; however, we can infer that the GMM models are appropriately specified if the coefficient estimate for the lagged dependent variable is high compared

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<sup>5</sup> Brender and Drazen (2005) frequently report significant Sargan test statistics that in fact reject the hypothesis that their instruments are valid. However, the Sargan test is inconsistent since their analyses encounter heteroskedasticity. They do not report the more appropriate but weak Hansen test.

to the FE coefficient, and if there is no second-order autocorrelation in the differenced residuals. Finally, the GMM estimators are derived under the assumption that there is no contemporaneous correlation. We thus include time dummies in all our analyses.

## Results

We begin our analysis by re-estimating Brender and Drazen's base specification in Table 1, substituting our first credibility measure, *partyage*. Across all models, political budget cycles are significantly smaller in countries with older political parties. We first present the results of a 'naïve' Ordinary Least Squares (OLS) regression in column 1 and account for fixed effects in column 2, including period dummies in column 3. Dynamic panel bias is evident in the divergent coefficient estimates for the lagged dependent variable in the OLS and FE models, reinforcing the appropriateness of GMM estimation.

Column (4) reports results from the Difference-GMM estimator, used by Brender and Drazen. The coefficient of the lagged dependent variable (0.30) in column 4 is far below the estimate with fixed effects (0.73) in column 3. This indicates that the Difference-GMM specification in fact exhibits even *higher* bias than the FE model. System-GMM in column 5, on the other hand, yields more credible results: the coefficient for the lagged dependent variable under System-GMM lies in between the FE and OLS estimates, suggesting that it is a more appropriate specification.

**Table 1: Credibility and Political Budget Cycles**

Dependent Variable	(1)	(2)	(3)	(4)	(5)
Method	OLS	FE	FE	D-GMM	S-GMM
Partyage	-0.002 (0.004)	0.000 (0.013)	-0.000 (0.014)	0.014 (0.010)	-0.000 (0.0130)
Election	0.561* (0.335)	0.492 (0.318)	0.368 (0.354)	0.509** (0.249)	0.836** (0.357)
Partyage x Election	-0.009** (0.004)	-0.008** (0.004)	-0.007* (0.004)	-0.006** (0.003)	-0.010** (0.004)
LDV	0.934*** (0.024)	0.721*** (0.047)	0.727*** (0.048)	0.309** (0.131)	0.770*** (0.100)
Output gap	0.022 (0.099)	-0.351*** (0.127)	-0.418*** (0.129)	-0.786** (0.319)	-0.198 (0.238)
Log(GDP per capita)	-0.297 (0.199)	-2.609** (1.277)	-0.548 (0.684)	-5.449 (3.501)	1.684 (1.359)
Intl. trade share	0.001 (0.002)	0.010 (0.017)	0.010 (0.018)	0.016 (0.030)	0.000 (0.011)
Populationaged 15-64	-0.035 (0.041)	0.128* (0.075)	0.252** (0.125)	0.448 (0.417)	-0.315 (0.218)
Population aged≥65	0.183* (0.095)	1.201 (0.944)	1.315 (1.025)	2.978* (1.800)	0.428 (0.335)
Constant	5.130*** (1.868)	12.390*** (3.560)	-11.670 (17.450)	13.120 (28.160)	10.410 (11.920)
Period Dummies	No	No	Yes	Yes	Yes
R-squared	0.89	0.39	0.42		
AR(1)				-2.89***	-3.21***
AR(2)				1.28	1.36
Observations	1,051	1,051	1,051	971	1,051
# countries	67	67	67	67	67

Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Instruments for first differences are lags 2-4 of expenditure and lags 2-3 of endogenous regressors (intl. trade share and output gap). Differences of remaining regressors as standard instruments. In addition to these instruments for Difference-GMM (column 4), lagged differences are instruments for the level equation for System-GMM (column 5). First order serial correlation in first-differenced residuals with no second order serial correlation indicates that instruments for both GMM analyses are valid. LDV (lagged dependent variable) coefficient for Difference-GMM below fixed effects (FE) coefficient suggesting weak model fit; LDV coefficient of System-GMM between OLS and FE coefficients suggesting good model fit.

Substantively, our main focus lies on the election dummy, *election*, and its interaction with our credibility measure, *partyage*. The interaction is statistically significant across all specifications. In terms of econometric soundness, System-GMM is our preferred specification. The election dummy is positively signed and significant at the 5% level. This means that generally, government

expenditure increases in election years, as predicted by the theory of political budget cycles. The interaction effect is negative and also significant at the 5% level. This means that the magnitude of political budget cycles increases with the average age of parties, in other words credibility. This lends strong support to our theory.

We test the robustness of this result in table 2. First, we include controls that we believe to be more appropriate in this setting: we exclude the international trade share and the two age dependency ratios and instead include controls for the political system and unified government. Column 1 of table 2 shows that our results from table 1 are robust to this alternative specification. In fact, the significance of the interaction effect increases slightly to 1 percent. In column 2, the dependent variable is our extended data set. The dependent variable is the same as in Brender and Drazen but now ranges up to 2008. The election dummy retains significance at the 5% level and the interaction effect is correctly signed and significant at the 5 percent level.

Does party age just capture the effect of the age of the democratic system, as argued by Brender and Drazen? To guard against this possibility, we replace our *partyage* variable with *partyage (resid)*, which is based on the residuals from a regression of *partyage* on the age of the democratic system. The absolute size of the coefficients for both the election dummy and the interaction term decrease slightly, both in the sample using Brender and Drazen's original data in column 3 and our data in column 4. However, the interaction effect retains significance at the 10% level, corroborating our theory that it is credibility rather than whether a democracy is 'new' that determines the magnitude of political budget cycles.

Lastly, we use our alternative measure of party consolidation, *ruling party age – years in office*. We use this variable both in an analysis using the original Brender and Drazen data set in column 5 and our extended data in column 6. In both cases, the interaction effects with the election dummy are positively signed, as expected, and significant at the 5% level.

**Table 2: Robustness checks –alternative controls, extended sample, age of democracy, ruling party age**

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Central government total expenditure					
Expenditure data	Original	Extended	Original	Extended	Original	Extended
Method	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM	S-GMM
Partyage	0.001 (0.013)	0.002 (0.008)				
Partyage (residual)			-0.005 (0.017)	0.000 (0.008)		
Ruling party age – years in office					-0.004 (0.013)	0.001 (0.007)
Election	0.740** (0.315)	0.769** (0.308)	0.243 (0.204)	0.353* (0.181)	0.542* (0.306)	0.617** (0.291)
Partyage x Election	-0.011*** (0.004)	-0.009** (0.004)				
Partyage (residual) x Election			-0.007* (0.003)	-0.006* (0.004)		
Ruling party age ... X Election					-0.007** (0.003)	-0.006** (0.003)
LDV	0.791*** (0.067)	0.828*** (0.052)	0.782*** (0.071)	0.827*** (0.053)	0.779 (0.071)	0.826*** (0.052)
Output gap	-0.152 (0.211)		-0.175 (0.212)		-0.165 (0.201)	
Growth		-0.063** (0.028)		-0.064** (0.028)		-0.064** (0.028)
Log(GDP per capita)	2.070** (1.025)	0.958** (0.453)	2.070** (0.996)	0.964** (0.455)	2.190** (1.108)	0.966 (0.472)
System	0.883 (0.715)	0.937** (0.475)	0.939 (0.739)	0.933* (0.477)	0.962 (0.752)	0.934** (0.474)
Unified government	1.240 (0.995)	0.594 (0.405)	1.233 (1.000)	0.587 (0.407)	1.184 (0.973)	0.582 (0.408)
Constant	-10.420 (7.827)	1.407* (0.831)	-14.230* (7.825)	0.407 (0.963)	-14.992* (8.321)	0.396 (0.953)
Period dummies	Yes	Yes	Yes	Yes	Yes	Yes
AR(1)	-3.01***	-3.15***	-3.05***	-3.14***	-3.02***	-3.14***
AR(2)	1.49	1.11	1.54	1.19	1.45	1.14
Observations	1,034	1,289	1,034	1,289	1,033	1,288
# countries	65	75	65	75	65	75

Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . 'Partyage (residual)' calculated as the residuals from a regression of our partyage variable on the time a democratic system was in place. 'Original data' refers to Brender and Drazen's (2005) data set; 'extended data' refers to extended IFS variables up to 2008. Instruments for first differences are lags 2-4 of expenditure and lags 2-3 of endogenous regressors (output gap or growth). Differences of remaining regressors as standard instruments, as well as lagged differences as instruments for the level equation. First order serial correlation in first-differenced residuals with no second order serial correlation indicates that instruments for both GMM analyses are valid. LDV coefficient for FE regression with equivalent specification in 'extended data' (columns 2, 4, and 6) is 0.77 with standard error 0.06. LDV estimates for FE regression equivalent to table 1. The diagnostics suggest correct specification of the GMM models.

In Table 3 we examine whether confidence in political parties also suppresses political budget cycles. Since we are using a different dependent variable in this analysis, general government final consumption expenditure, we also report the FE results: all GMM coefficients of the lagged dependent variable lie above the FE coefficient, suggesting correct empirical specification.

Substantively, we find that, again, expenditure increases in election years, but less so the more confidence voters have in parties. We employ different operationalisations of the election dummy to check the robustness of this result. In columns 1 and 2, we employ the election dummy based on Brender and Drazen's coding rule of election years. The results are significant at the 5% level for both the election dummy and the interaction with party confidence. In column 3, we code elections as occurring in the previous year if held in the first month of the fiscal year. The absolute size of the coefficients increases slightly but significance is unaltered. In column 4, we re-code elections correspondingly if they occurred in the sixth month of the fiscal year – the results are weakened but still significant at the 10% level. These results give us confidence that our results are sensible, given different codings.

Finally, we repeat the test of table 2, and use our alternative party confidence measure, purged of the effect of the age of the democratic system (*party-confidence (resid)*). The interaction with the election dummy is positive, as predicted by our theory, and significant at the 5% level. This shows that the effect of party confidence on the expenditure in election years is not driven by the experience with democracy. This is reasonable as our main emphasis lies with parties, rather than government per se. Overall, there is thus considerable evidence in support of our theory.

**Table 3: Robustness checks – party confidence, election timing, age of democracy**

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	General government final consumption expenditure				
Method	FE	S-GMM	S-GMM	S-GMM	S-GMM
Party-Confidence	0.650 (0.458)	0.005 (0.192)	0.023 (0.193)	-0.025 (0.204)	
Election	0.739** (0.348)	1.011** (0.460)			0.117 (0.116)
Party-Confidence x Election	-0.671** (0.317)	-0.858** (0.389)			
Elect (M1)			1.057** (0.460)		
Party-Confidence x Election (M1)			-0.926** (0.389)		
Election (M6)				0.914* (0.508)	
Party-Confidence x Election (M6)				-0.739* (0.420)	
Party-Confidence (Resid)					0.009 (0.180)
Party-Confidence (Resid) x Election					-0.831** (0.387)
LDV	0.468*** (0.075)	0.925*** (0.035)	0.928*** (0.036)	0.930*** (0.037)	0.928*** (0.038)
Growth	-0.011 (0.021)	-0.001 (0.033)	-0.002 (0.033)	-0.002 (0.033)	-0.013 (0.034)
Log(GDP per capita)	-1.325 (1.121)	0.097 (0.077)	0.096 (0.078)	0.088 (0.078)	0.099 (0.084)
System	1.614* (0.926)	-0.047 (0.080)	-0.049 (0.079)	-0.057 (0.079)	-0.043 (0.081)
Unified government	0.314** (0.135)	0.140 (0.124)	0.140 (0.123)	0.149 (0.122)	0.152 (0.128)
Constant	6.940*** (2.198)	0.927* (0.556)	0.884 (0.568)	0.913 (0.580)	0.925** (0.433)
Period dummies	Yes	Yes	Yes	Yes	Yes
R-squared	0.34				
AR(1)		-2.81***	-2.79***	-2.84***	-2.77***
AR(2)		-0.70	-0.75	-0.88	-0.86
Observations	429	429	429	429	425
# countries	57	57	57	57	56

Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . 'Party-Confidence (residual)' calculated as the residuals from a regression of our party confidence variable on the time a democratic system was in place. Election (M1) corresponds to our main election dummy but elections were coded as occurring in the previous fiscal year if the election was held in the first month of the fiscal year; election (M6) equivalent for the sixth month of the fiscal year. Due to many missing observations in the party and government confidence variables, all available instruments for endogenous variables (LDV and growth) are collapsed and orthogonal deviations (see Arellano and Bover 1995) are used to maximise sample size. First order serial correlation in first-differenced residuals with no second order serial correlation indicates that instruments for both GMM analyses are valid. GMM LDV coefficient is larger than the FE coefficient in column 1, suggesting correct specification of the GMM model.

## Conclusion

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). The analysis here links the inability to make credible commitments to politician incentives to make targeted transfers to voters prior to elections in order to mobilize electoral support. This same theory offers a novel explanation of differences across countries in political budget cycles: less credible politicians have greater incentives to shift expenditures to the period before elections. We argue that institutionalized political parties, which facilitate collective action by citizens to punish politicians who renege on their promises, are key to credibility. Accordingly, we use several measures of party institutionalization to test the proposition that political budget cycles – concentrated government expenditures around elections due to vote-buying – are greatest in countries with weakly institutionalized parties.

The tests provide robust support for this proposition and for the underlying logic. First, institutionalized political parties are essential for political credibility. Second, the absence of political credibility encourages vote-buying. Third, differences in political incentives to undertake vote-buying expenditures explain variations in political budget cycles. Our tests of these detailed hypotheses are indirect and based on the reduced form examination of how expenditure cycles vary with particular measures of political party institutionalization. The results point to a future research agenda, which separately examines the structural links in the reduced form relationship and employs more direct measures of political party organization.

## Appendix 1

Table A1: Re-coding of the election dummy

<b>Country</b>	<b>Year</b>	<b>B&amp;D</b>	<b>Adjusted</b>
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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