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## Abstract

We study the short-run effect of elections on monetary aggregates in a sample of 85 low and middle income democracies (1975-2009). We find an increase in the growth rate of M1 during election months of about one tenth of a standard deviation. A similar effect can neither be detected in established OECD democracies nor in other months. We argue that the cycle is related to systemic vote buying which requires significant amounts of cash at election times. The finely timed increase in M1 is consistent with this and it cannot be, fully, accounted for by alternative explanations.

JEL-Code: D720, E510, O100.

Keywords: political business cycles, vote buying, monetary economies.

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# 1 Introduction

The theory of political business cycles in monetary aggregates, pioneered by Nordhaus (1975) and MacRae (1977) and given its modern, rational choice interpretation by Persson and Tabellini (1990), predicts monetary expansions in the quarters leading up to an election and an election-time economic boom. The ultimate goal is to help the government win votes. Empirical tests of this theory have, however, fared badly and the evidence on monetary political cycles of the classical Nordhaus-MacRae type is, as pointed out in the surveys by Paldam (1997) and Drazen (2001), weak.

In this paper, we provide new evidence on the monetary effects of elections and strive to offer an alternative perspective on the money-election nexus. In contrast to past work on monetary political cycles, which emphasizes deliberate manipulations of monetary policy instruments in the quarters prior to the election, we emphasize that short-run monetary cycles may occur as an unintended by-product of the way that electoral politics work in many countries. This effect is concurrent with the election and works through cash demand.

We investigate if the growth rate of the monetary aggregate M1 – defined as cash and overnight bank deposits – increases in election months in a panel of around 85 low and middle income democracies for the years 1975-2009. We estimate a dynamic, multi-level panel model with year, month and country fixed effects, and we control for many country and time varying factors. We find evidence of an increase in the growth rate of M1 in election months in these countries. The effect is sizable: the growth rate of M1, on average, increases by 0.6-0.7 percentage points or by one tenth of a standard deviation in election months. We are unable to find similar effects in any other month or amongst established OECD democracies. The average effect masks a considerable amount of heterogeneity. The effect is strongest in low income countries, with a large fraction of the population below the poverty line and with low levels of education attainment. The effect is particularly strong in Sub-Saharan Africa, in East-Asia and the Pacific. Unlike

the evidence on classical political business cycles in M1 and other monetary aggregates, these results are remarkably robust. They suggest that the election calender induces concurrent fluctuations in M1 that can only be detected by studying monthly (or weekly) data. These results are new to the literature.

Our preferred explanation of the election date effect is that it is a manifestation of systemic vote buying. Vote buying – understood as payments or gifts in exchange for voting in a particular way or for showing up to vote – requires significant amounts of cash to be disbursed right before the election is held. This increases the demand for liquidity and affects (recorded) M1 through, at least, two channels. Firstly, the resources needed to buy votes may be obtained by converting illiquid assets into cash. This substitution from broad money into cash or deposits directly increases M1. Secondly, vote buying is an illegal activity and the required funds may come from the shadow economy. Once such shadow economy cash hoardings are used to buy votes, a fraction of them turns into deposits in banks. This will, in turn, increase the money multiplier and offer leeway for an increase in M1. Either way, the result is a spike in M1 just before elections. The finely timed effect on M1, that we find empirically, is consistent with this. Moreover, vote buying, as a viable electoral strategy, requires weak democratic institutions, poorly monitored elections, and an electorate willing to "sell" their votes. The vote buying interpretation is, therefore, reinforced by the patterns of heterogeneity we observe in the data, by the fact that the election date effect cannot be detected amongst established OECD democracies, and by the fact that it is strongest in regions where survey evidence points to widespread vote buying and where democratic institutions are comparably weak.

Vote buying is not the only possible interpretation, however, and there are several alternatives that must be considered. First, the election date effect could be caused by central banks expanding liquidity just before elections. We are, however, unable to detect any election date effect in central bank interest rates and we cannot find any abnormal increases in M1 in the months prior to the election month, as one would expect if the purpose of the central bank's actions is to increase real economic activity. These findings

speak against the alternative interpretation, but they do not rule out that governments with full control over the printing press might use that power to fund vote buying or other electoral expenses. Second, political parties demand cash to fund legal election campaigns. While this could induce fluctuations in M1, it takes time to prepare election campaigns. Accordingly, one would not expect the liquidity effect to be present only in the month of the actual election and not in the preceding ones. An equally important consideration that speaks against this alternative is the fact that we cannot find any election date effect amongst established OECD democracies where vast sums of private and public money are expended on election campaigns. Third, elections, in general, increase economic activity and could cause cash demand irregularities. We can, however, not find systematic increases in the growth rate of M1 around other events (such as national celebrations of independence days, etc.) which should be associated with similar irregularities. This casts doubt on this explanation. Finally, the government may pay wage arrays and clear debt to private sector creditors just before elections (Akhmedov and Zhuravskaya 2004). While this does not have a direct effect on M1, except if the funds are drawn directly from the central bank, it could affect the money multiplier. But insofar as the desired deposit-cash ratio of government agencies is higher than that of private agents, then the effect on M1 would be negative, not positive. While we are unable to rule these alternatives out for sure, the interpretation that provides the most coherent account of the collage of evidence that we present is the vote buying explanation.

The rest of the paper is organized as follows. Section 2 provides an overview of the relevant literatures, places our study within those and discusses the underlying monetary mechanisms that relate vote buying to fluctuations in M1. Section 3 presents some case-study and survey evidence on the extend of voting buying and the potential link between vote buying and election time spikes in the growth rate of M1. Section 4 introduces our data and identification strategy. Section 5 is devoted to our main results. Section 6 evaluates alternative explanations.

# 2 Background: Political cycles, money and vote buying

Political business cycle models have so far guided the search for a possible impact of election dates on monetary policy. The assumption of these approaches is that politicians who seek reelection will, besides fiscal tools, employ monetary instruments to generate a favorable economic environment prior to an election.[1] The original Nordhaus (1975) and MacRae (1977) model focuses on a Phillips curve trade-off between inflation and unemployment and predicts an expansion of monetary aggregates or a reduction in central bank rates prior to the election.[2]

Both conceptually and empirically, the relevance of monetary policy cycles remains contested (Drazen 2001). Conceptually, the independence of central banks from elected governments makes the theory questionable in many countries and uncertainty about the monetary transmission mechanism makes it unpractical. Empirically, the evidence is mixed. For the US in a time series context, empirical tests point to an impact of presidential elections on the growth of M1 until 1980 but not in later years (Drazen 2001; Alpanda and Honig 2009) or during the earlier period before World War II (Heckelman and Wood 2005). The international evidence based on cross-country panel analysis is not less ambiguous: Paldam (1979) fails to find evidence of election year monetary expansions; Alesina et al. (1992, 1993), at best, find weak evidence of a cycle in M1, but not in the money base; Leertouwer and Maier (2001) fail to find a central bank interest rate cycle in the OECD countries, while Klose (2012) reports that interest rate policy is less reactive to inflation and more reactive to output fluctuations prior to an election; Dreher and Vaubel (2009) find an electoral cycle in foreign exchange interventions.

None of these contributions pay attention to the immediate role of the election date for monetary aggregates. This is understandable since monetary policy affects growth and

[1] For evidence on political business cycles in public finance variables, see, e.g., Brender and Drazen (2005); Veiga and Veiga (2007); De Haan and Klomp (2013) and Aidt and Mooney (2014).

[2] Alesina et al. (1997) offers an excellent overview of these models.

employment with considerable lags (4 to 6 quarters) and monetary expansion prior to an election, therefore, must start well before an election date. Since monetary policy cycle considerations do not point to an immediate impact of an election, there has been a lack of interest in higher frequency (monthly) data. In fact, most of the literature makes use of quarterly or annual data which preclude the identification of concurrent election date effects.[3]

In contrast to this existing literature, we highlight a new reason why the election calendar might induce fluctuations in monetary aggregates which does not relay on direct central bank intervention. We explore how short-run monetary cycles occur as a by-product of electoral politics as conducted in many low and middle income countries with weak electoral institutions. In particular, we focus on the concurrent effect of elections on cash demand and explore the possibility that abnormally high monetary growth in the election month may be indicative of systemic vote buying. Detection of such an effect requires higher frequency data (monthly or even weekly and daily data) than those which traditionally have been considered in the political business cycle literature.

We use the term vote buying to refer to two related strategies for winning elections.[4] One strategy is to offer a monetary payment as a direct exchange of cash for votes (e.g., Stokes et al. 2013; Hicken 2011; Shefter 1977). Another strategy is to buy turnout, that is, to offer cash payments to induce core supporters to cast their vote (see, e.g., Nichter 2014) or to induce opposition voters to stay home (see, e.g., Cox and Kousser 1981). In addition to cash, parties often distribute a wide range of other material goods such as food, clothing, a bag of rice, as well as services such as medical care, transportation to the polling station etc. on the day of the election (Helmke and Levitsky 2006). The political science and economics literature is abundant with survey, case-study, and field experiment

[3]Klose (2012) is a recent exception employing monthly data but his focus is on variations in the parameters of the Taylor rule across the election cycle.

[4]Political parties use many strategies to win votes. We focus on pre-election attempts at delivering non-programmatic benefits to voters in return for their political support or promise of support in the election. An important alternative explored, for example, by Dixit and Londregan (1996) and Keefer and Vlaicu (2008) is to promise post-election programmatic benefits. Such promises will not have direct, pre-election effects on monetary aggregates or other macroeconomic variables.

evidence of systemic vote buying.[5] Historically, vote buying was facilitated by the absence of the secret ballot and by suffrage restrictions that created small electorates.All present-day democratic societies embrace secret ballot and universal suffrage. Secret ballot makes it hard to verify whether a voter whose vote is bought actually votes as agreed. Mass electorates make it expensive to buy enough votes to affect election outcomes. Yet, multiple forms of systematic vote buying persist in many modern democracies.

Effective systems of vote buying are often organized hierarchically in what is sometimes referred to as "political machines". In such systems, the resources to buy votes are allocated to middlemen or vote brokers who know the particular voters within their sphere of influence and who through repeated interaction can, at least to some extent, guarantee that deals are kept (Sobel 2005). Stokes (2005) documents how party machines in Buenos Aires embed agents deep inside the social networks of voters and how this enables parties to at least partly infer how individual voters vote despite the secret ballot. Krishna (2003) describes how a new class of village level intermediaries (Naya Neta) serves as an effective link between political parties and voters in rural India. By virtue of better education, these intermediates can offer help to villagers in their interaction with the government bureaucracy (e.g., filling in forms or getting permits). This allows them to build up trust relationships and knowledge of the voters in their village. Over time, they gain substantial influence on villagers, including on how they vote. Political parties can, then, exploit this by "buying" blocks of votes through the intermediates. Since aggregate vote totals are public information ex post, parties can verify, in a statistical sense, if the expected votes were delivered or not. Field experiments from Benin, Sao Tome and Philippines provide further insights into the strategies used by parties to buy votes and on how anti-vote buying campaigns can effectively reduce its extent (Wantchekon 2003; Vicente 2014; Hicken et al. 2015).

Our contribution to the literature on vote buying is to suggest that systemic, large-scale vote buying has short-run aggregate monetary effects. The logic is that vote buying

[5]For theoretical models of vote buying, see Dekel et al. (2008); Snyder (1991); Dal Bo (2007); Heckelman and Yates (2002); Aidt and Jensen (2012); Baland and Robinson (2008).

requires liquid resources (cash) to be distributed right before the election. This creates a spike in the demand for money with a very specific timing pattern. The cash demand induced by systemic vote buying can, in turn, create irregularities in the supply of money – cash and overnight bank deposits (M1) – through two main channels: first, the substitution from broad to narrow money and, second, the return of previous cash hoardings (e.g., from the shadow economy) into banking deposit. Transfers between cash and banking deposits as such are neutral in their effect on M1 since this monetary aggregate is defined as the sum of cash and deposits. However, if private sector agents liquidate broad money assets to finance vote buying transactions, then this substitution towards liquidity is recorded as an increase in M1. The second channel originates from the mechanics of money multipliers. When cash, which was previously hoarded outside of the banking system, is used for vote buying transactions, it (partially) returns to the banking sector as the money is spent or deposited in banks by the voters who receive it. Hence, the commercial banks experience an increase in their central bank reserves which increases their potential to lend. In the monetary terminology, this reduction in cash hoardings increases, possibly with some lag, the money multiplier and, hence, M1. On top of this, governments with full control over the central bank may fund vote buying or targeted social spending directly through the printing press. The basic insight from these monetary mechanics considerations is that there can be a positive effect of vote buying on M1 independent of whether vote buying is financed from cash hoardings or from broader financial assets or directly from the printing press. Moreover, since vote buying transaction takes place close to an election, the positive effect on M1 is timed around the day of the election and does not necessarily require direct intervention from the central bank. Accordingly, systemic vote buying will be detectable as a finely timed spike in M1 within a short window around the election.

# 3 Survey and case study evidence

In this section, we present some survey evidence related to the extent of vote buying in Latin America and Africa and some case studies that are suggestive of a relationship between vote buying and election-time monetary expansions.

## 3.1 Survey evidence on the extent of vote buying

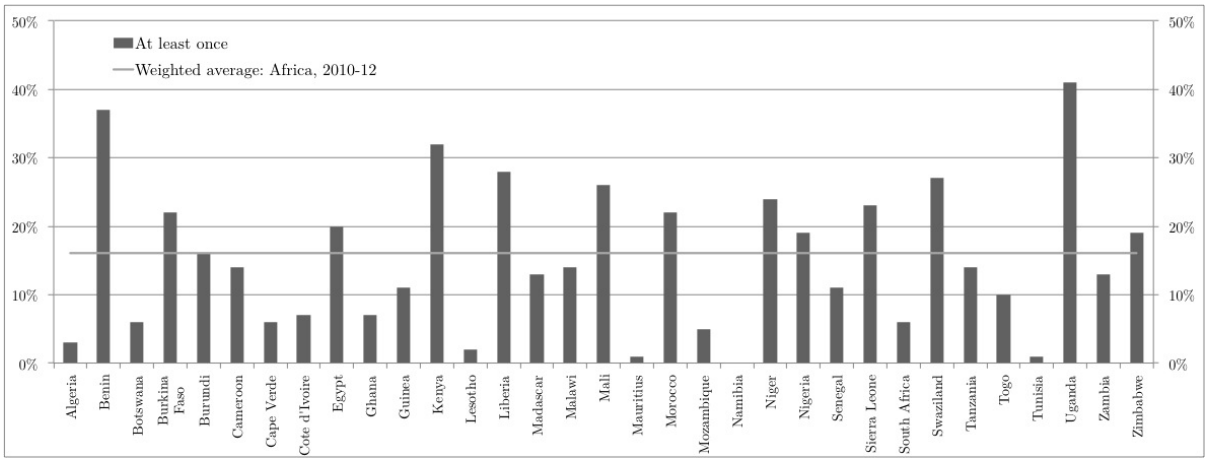
One might ask if vote buying really occurs on a scale that could, in principle, induce effects on a macroeconomic scale. Survey evidence suggest that it does. Figure 1 reports data from the Afro- and Latino-barometers on the fraction of survey respondents who report that they had "been offered a material benefit in exchange for a vote" or that they know of other people who had.[6] We note that the average share of people reporting vote buying is about 17% in Africa (weighted by population) and about 25% in Latin America (unweighted). In some countries, e.g., Benin, Uganda and the Dominican Republic, close to half the population reports knowledge of vote buying.[7] It is clear from these data that vote buying is widespread in many parts of the world, in particular in African democracies (Collier and Vicente 2012; Jensen and Justesen 2014). The amounts of money spent on vote buying is also substantial. For example, Phongpaichit et al. (2000) estimate that, during Thailand's 1996 election, 30% of the electorate was offered cash in exchange for votes, with an average offer of 27 USD. In Taiwan, many voters were offered about 10 USD for a vote during the 1993 election (Wang and Kurzman 2007). Based on a household survey, Finan and Schechter (2012, p. 869) estimate that in Paraguay during the 2006 municipal election voters were offered, on average, 48 USD in exchange for their vote.

[6] The Asian, Arab, or European barometers do not have questions on vote buying.

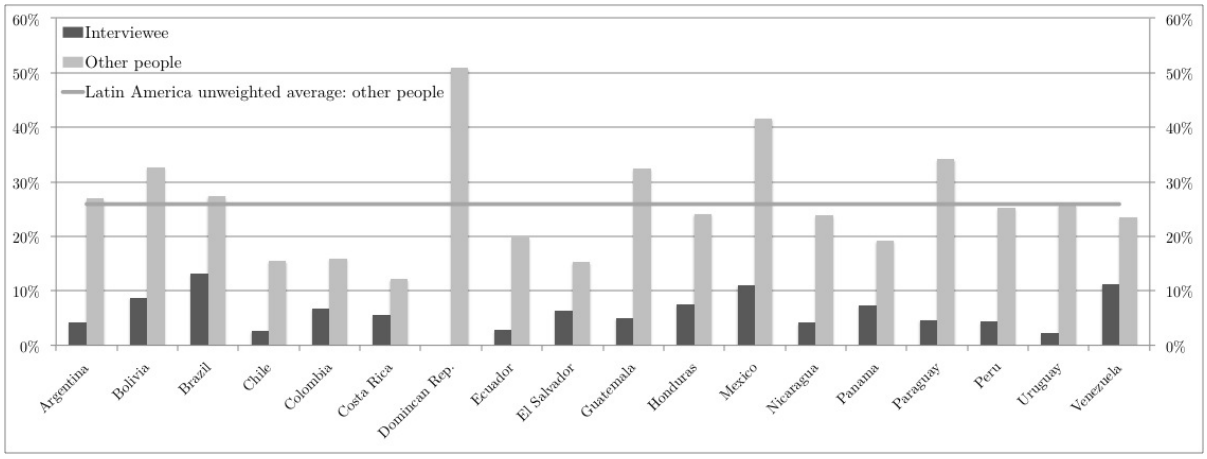
[7] Social desirability bias suggests that voters tend to under-report having received money or gifts in exchange for their vote (see, e.g., Corstange 2012). The work by Gonzalez-Ocantos et al. (2012) on Nicaraguan municipal elections shows that the bias can be very big: in a survey-based list experiment, 24% of the voters were offered a "gift" in exchange for their vote, but only 2% reported this fact when asked afterwards in a survey.

**Figure 1:** The extent of vote buying in Africa (2010-2012) and Latin America (2002)

**(a)** *Afro-barometer:* And during the last national election in [20xx], how often, if ever did a candidate or someone from a political party offer you something, like food or a gift or money, in return for your vote?




**(b)** *Latino-barometer:* Have you known of someone in the last elections who was pressured or received something to change his vote in a certain way? Has this happened to you?




Notes: Data for Dominican Republic is from the survey wave of 2005. Sources: Afro−barometer; Latino−barometer.

## 3.2 Case-study evidence on the monetary effects of elections

Several central banks publish weekly or daily data on M1, others report by month and most by year. The weekly and daily series allow us to track monetary movements around elections with varying degrees of accuracy and it is illuminating to consider some case studies before turning to the econometric analysis of monthly data. Figure 2 reports data on M1 around elections in six selected countries, Armenia, Indonesia, Lebanon, Bolivia, Nigeria and the USA. The dark bar in each panel indicates the election day, week or month.

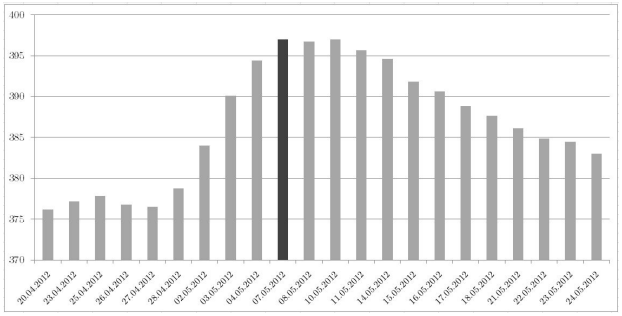
**ARMENIA 2012.** Armenia - a small landlocked country in the South-Caucasus - has held its last two national elections in a relatively peaceful and non-violent environment. The 2012 Human Rights report of the United States Department of State (US Department of State 2012), citing the election observation report of the Organization for Security and Cooperation in Europe, describes the 2012 parliamentary election held on the 6th of May as "competitive", but with significant violations, such as "credible allegations of vote buying" among others. International media does not report much on the small country, but local media are full of allegations of vote buying, typically amounting to 5,000 or 10,000 AMD per vote (about 10-20 USD)[8] and said to reach several hundred thousand voters in a country with a population of less than 3 million (Institute for War and Peace Reporting 2012). Unlike most other central banks, the Central Bank of Armenia reports monetary statistics on a daily basis and the pattern observed in Figure 2(a) is striking. The cash in the Armenian economy increased by 20 billion AMD (or by over 5%) in less than 10 days preceding the elections. This spike is concentrated very close to the election day, reaching its peak on the first working day after the election weekend and gradually declining during the following weeks.

**NIGERIA 2007.** The 2007 presidential election marked the first transition from one elected leader to another in the largest country in Africa. Many observers have noted that

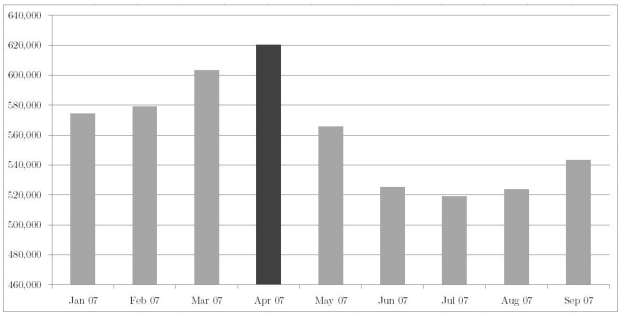
[8] See, e.g., Aravot Daily (2012).

**Figure 2:** Evidence on the daily, weekly, or monthly stock of money supply during elections in selected countries

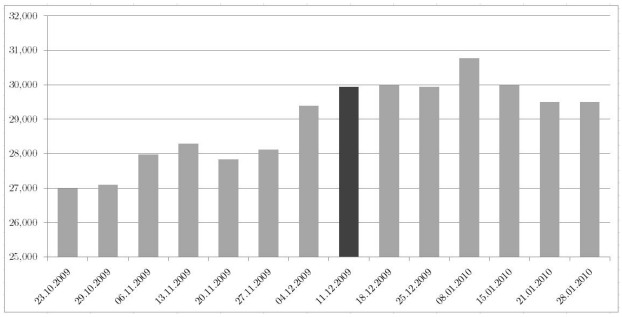
**(a)** Armenia 2012 (billion AMD)



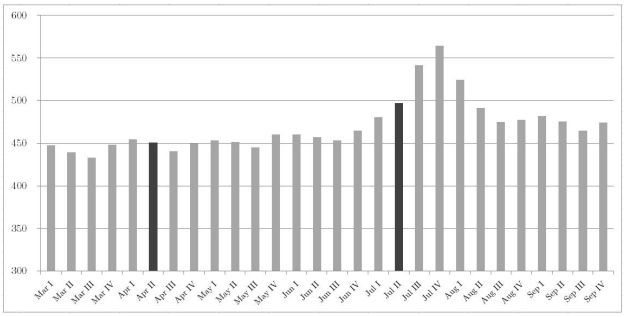

**(b)** Nigeria 2007 (million NGN)



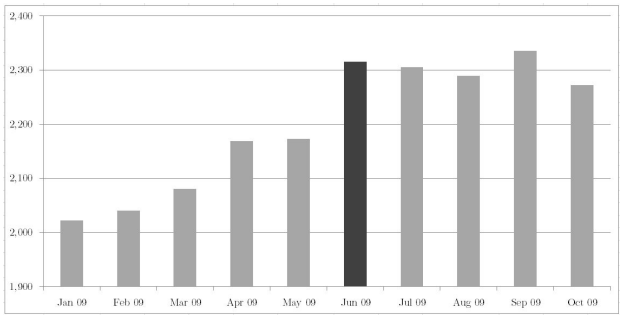

**(c)** Bolivia 2009 (million BOB)



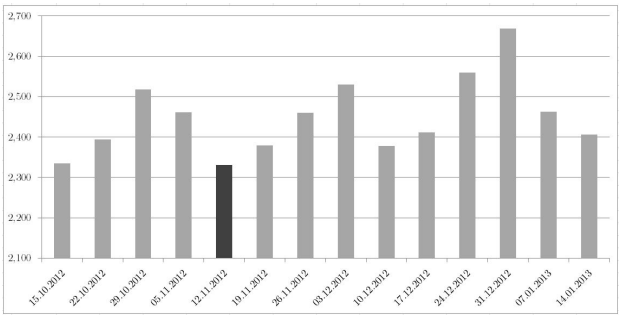

**(d)** Indonesia 2014 (billion IDR)




**(e)** Lebanon 2009 (billion LBP)




**(f)** USA 2012 (billion USD)




Notes: Sources of the monetary data are the respective national monetary authorities. The exact definitions vary: currency in circulation outside of the Central Bank in sub-figure (a), currency in circulation outside of banks in sub-figure (b), M1 in sub-figures (c) and (f), currency in circulation in sub-figures (d) and (e).

vote buying, along with electoral violence and fixes to falsify vote tallies, were common currency in this and other Nigerian elections (Lucky 2013; Collier and Vicente 2014). In an Afro-barometer survey undertaken half-way through the election campaign, 12% of the interviewed acknowledged that they had been offered something in return for their vote (Bratton 2008, p. 623). As one might expect in an economy awash with oil money, voters are usually offered money in exchange for their vote but gifts such as food or clothing are also common. The going price for a vote in 2003 and 2007 was around 500 naira or about 4 USD.[9] Figure 2(b) shows how M1 evolved before and after the elections held in April 2007. We observe a clear increase in March with a peak in April. After the election, M1 falls back to its normal level.

**BOLIVIA 2009.** The 2009 elections took place in a violence-free environment and saw the highest turnout rate in Bolivia's history. The elections ended with a clear victory for Morales, who obtained 64.1% of votes. Monitors from the European Union characterized the elections as generally free and fair, but they also confirmed the misuse of state resources and the international press reported that "cash handouts for poor families, passionate speeches against foreign companies and heavy social spending" were all helping Morales get re-elected (Reuters 2009). Survey data suggest that just under 10% of the voting population in Bolivia find vote buying acceptable (Gonzalez Ocantos et al. 2014). The monetary consequences of this are visible in Figure 2(c) which shows the movement of M1 by week around the 2009 election. We observe a significant increase in the outstanding stock of money between the fourth week of November and the election held in the second week of December.

**INDONESIA 2014.** Indonesia is a large electoral democracy where almost half of the population earns less than 2 USD per day. Indonesians voted in two elections in 2014, first, on the 9th of April for the parliament, and second, on the 8th of July for the president. A recent survey by Jakarta based pollster Indikator reports that 41,5% of 15,600 people interviewed "find it acceptable for politicians to hand out money or

[9]See (Bratton 2008, p. 624).

staples like rice or oil, as part of campaigning".(Jakarta Post 2014) In the region of East Java, for example, a candidate admitted to paying 117 million Indonesian rupiah (over 10,000 USD) to 13 sub-district committee members in exchange for 5,000 votes. Media reports indicate that these intermediaries (called "korlap" by the locals) typically hand out 50,000 to 100,000 Indonesian rupiah (about 4-8 USD) per vote. Figure 2(d) shows the movement of M1 in the weeks around the two elections. We observe a jump of around 100 billion Indonesian rupiah (or by 20%) in the month of the presidential election but no jump around the parliamentary election.

**LEBANON 2009.** Lebanon held a relatively peaceful parliamentary election in June 2009 in which allegations of vote buying were abundant. The New York Times reported in April of that year that the election could "shape up to be amongst the most expensive ever held anywhere, with hundreds of millions of dollars streaming into this small country [of only four million people] from around the globe" (New York Times 2009). The headline of the June 2 issue of the Globalpost, "Going rate for a vote in Lebanon? $700", gives an indication of the inflated prices at which votes apparently were sold (Globalpost 2009). Corstange (2012) uses survey data and a list experiment to show that over half of the Lebanese voters sold their votes in 2009. Figure 2(e) shows the movement in M1 around the election month and we observe a big, positive spike in June.

The examples from Armenia, Nigeria, Indonesia, Bolivia and Lebanon are suggestive that there is an association between vote buying and the supply of money centered on the election day. It is clear, however, that these countries are not a random sample of electoral democracies: They are relatively poor, "young" democracies and their political institutions are comparably weak. According to the Freedom House index of political rights[10], Armenia, Lebanon, Bolivia and Nigeria are classified as partially free and only Indonesia is considered to have better institutions than that. Additionally, they are all perceived

[10]The Freedom House index of political rights is coded on a scale from 1 to 7 with seven being the worst and one being the best. Armenia and Lebanon score 5, Bolivia 4, Indonesia 2 and Nigeria 4 (Freedom House 2012).

to have high levels of corruption (e.g., Transparency International 2014). Cross-country studies reveal a strong association between weak political institutions and vote buying, in particularly amongst "young" democracies (Keefer 2005; Keefer and Vlaicu 2008). Evidence from Africa and Latin America, moreover, highlights a strong relationship between poverty and vote buying within a given country (Weitz-Shapiro 2012; Jensen and Justesen 2014). This, arguably, suggests that systemic vote buying flourishes mostly in societies with weak electoral institutions characterized by ineffective monitoring, lack of credible alternative strategies that would allow parties to reach mass electorates, weak electoral accountability, and a significant fraction of the voting population willing to exchange their vote for pre-election material benefits. Insofar as the correlation between the supply of money and the timing of elections, shown in the diagrams above, is related to vote buying, we would not expect to find similar effects in countries with comparably strong political institutions. Figure 2(f) shows the movement of M1 in the USA around the election in 2012. If anything, it looks like M1 is lower in the election week than in the preceding ones. Similar patterns emerge from other established OECD democracies and other elections.

# 4 Data and identification strategy

We collect monthly data on the amount of narrow money (M1) defined as the total amount of cash in circulation plus transferable deposits held by all money holding sectors. Our main sample consists of 85 low and middle income countries for the years between 1975 and 2009.[11] We also collected data for the 13 "old" OECD countries.[12] For each country, we record the date, month and year of each general election held during this period.[13]

[11] Year 2009 is the most recent data point reported in the IMF's EconStats database (World Economic Outlook 2015) which is the source of the monthly data on M1.

[12] OECD membership is defined as of 1975, i.e., at the beginning of the sample period. Our estimates remain robust if we exclude countries that obtained OECD membership in 2009 or 2014 (see Table A3).

[13] We obtained the data on election months from the Database of Political Institutions (DPI) constructed by Beck et al. (2001). Exact elections days are available from 1998 onwards and were retrieved from International Foundation for Electoral Systems (2015).

To be included in the sample, a country must, therefore, as a minimum hold regular elections and its central bank must report monthly data on M1. As a consequence of these restrictions, the panel is unbalanced. Table A2 in the appendix lists the sample of countries and the number of months out of the maximum of 420 for which each of them qualifies to be in the sample.

To estimate the effect of elections on the monthly growth rate of M1, we consider the following three-ways fixed effects multi-level panel model:

$$\Delta M1_{cym} = \alpha_0 + \beta_1 \cdot Election_{cym} + Control_{cy} \cdot \beta_{\mathbf{2}} + \sum_{i=1}^{k} \alpha_i \cdot \Delta M1_{cym-i} + \mu_c + \eta_y + \nu_m + \epsilon_{cym}, \quad (1)$$

where the dependent variable - $\Delta M1$ - is the growth rate of M1 in country $c$ in year $y$ and month $m$; *Election* is a dummy capturing the timing of elections, and *Controls* is a vector of control variables. The vector *Controls* includes GDP per capita, GDP per capita growth, the inflation rate, and resource rents as a share of GDP, measured at the level of countries and years, and the exchange rate against the US dollar, measured at the monthly frequency.[14] In some specifications, we also control for the quality of institutions (using the Polity IV index of democracy normalized to be between zero and one[15] and for whether a country in a given year is a new democracy in the sense of Brender and Drazen (2005).[16] In dynamic specifications, we add the lagged value of $\Delta M1$ with up to $k$ lags. All models include country ($\mu$), year ($\eta$) and month ($\nu$) fixed effects, and some also include country-specific month fixed effects. $\epsilon$ is the error term.

The main variable of interest is *Election*. It captures the timing of elections and is measured in two alternative ways. The first measure simply records the month in which

[14] We obtain these data from World Development Indicators (2014) and World Economic Outlook (2015).

[15] Center for Systematic Peace (2015)

[16] Brender and Drazen (2005) defines a country as being a new democracy during its first four elections following a transition from autocracy to democracy after which it becomes an old democracy. We do not control for the quality of institutions in all specifications because the Polity IV index is not defined for some of the small countries in the sample.

an election takes place. Specifically, the dummy variable *Election month* is defined as being equal to one if an election takes place in country $c$ in year $y$ and month $m$ and zero otherwise. Data on M1 is measured at the end of each month and *Election month* may, therefore, in cases where the election takes place early in a month, mostly capture (monetary) events happening after the election. The second measure takes into account the precise timing of an election within a month. In the spirit of Franzese (2000), we define *Election day* as being equal to $1/(62-x)$ for the pre-election, $1/x$ for the election, and $1/(32-x)$ for the post-election month and zero otherwise, where $x$ is the date of the election within the election month. Table A1 reports summary statistics for all the variables and lists the data sources.

The parameter of interest is $\beta_1$. It measures the election date effect: the increase (or decrease) in the growth rate of M1 in election months relative to non-election months within a given country and year. It can be given a causal interpretation if the timing of elections, conditional on the controls and the three-ways fixed effects, is unrelated to $\epsilon$. This assumption is satisfied in countries where the election date is pre-determined, but could be violated in countries where the incumbent government can decide on the timing of elections. We return to this issue below. We estimate equation (1) with a fixed effects estimator. In the dynamic specifications, this causes Nickell bias. However, since our data are monthly, we have up to 420 time periods, so the size of the bias is likely to be very small.

# 5 Main results

We present the main results in three subsections. First, we report the estimates of equation (1) for the main sample of low and middle income countries. Second, we explore potential heterogeneity in the estimate of the election date effect within that sample. Third, we offer our interpretation of the results.

## 5.1 The average election date effect

Table 1 reports the main estimates of the average election date effect for *Election month* in columns (1) to (6) and for *Election day* in columns (7) to (10). Column (1) shows a static specification of equation (1) while column (2) adds dynamics with three lags of the monthly growth rate of M1.[17] In both specifications, we find a significant increase in the growth rate of M1 in election months. Columns (3) and (4) add controls for the quality of democratic institutions and for whether a country at a given point in time is a "new" or an "old" democracy. We see that this makes little difference to the point estimate and the statistical significance of *Election month*. In our preferred specification in column (2), the average size of the election month effect is about 0.67 percentage points. This corresponds to one tenth of a standard deviation. Column (7) reports the corresponding results for *Election day* which takes the precise timing of the election within a month into account. The average election day effect is significant at the five percent level and corresponds to an increase in the growth rate of M1 of about 2.3 percentage points.

As noted in section 2, the increase in cash demand induced by wide-spread vote buying might be accommodated by conversion of illiquid assets such as long-term deposits (which are part of M2) into more liquid deposits and cash (which are part of M1). Empirically, we should then observe an increase in the M1-to-M2 ratio around the election. Table 1, columns (5) to (6) report specifications of equation (1) in which the dependent variable is the change in the M1-to-M2 ratio from month $t$ to $t-2$.[18] We observe that the M1-to-M2 ratio increases in election months. This suggests that part of the election month increase in $\widehat{M1}$ is due to conversion of illiquid assets into liquid ones.[19]

[17] Table A3 in the appendix reports more details on the dynamic specifications and shows that the results are very similar with up to six lags.

[18] For the purpose of calculating changes in the ratio, we take $t-2$ as the base month. We do this because it is reasonable to assume that the conversion of M2 assets into M1 assets may take some time.

[19] To gain deeper insights into the monetary mechanism underlying the election date effect, it would be useful to investigate data on total cash circulation or on the denomination of notes in circulation. However, this is ruled out by the absence of such data on a monthly frequency for a sufficient number of countries.

**Table 1:** Main results: Growth of M1 and M1/M2 during elections

| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | $\Delta M1$ | | | | $\Delta_{(t-2)} M1/M2$ | | $\Delta M1$ | | $\Delta M1_{Seasonally\ Adjusted}$ | |
| Election month | 0.0050* | 0.0067** | 0.0050* | 0.0064** | 0.0023* | 0.0016* | | | | |
| | (0.0026) | (0.0028) | (0.0028) | (0.0030) | (0.0012) | (0.0009) | | | | |
| Election day | | | | | | | 0.0228*** | 0.0163** | 0.0139** | 0.0156** |
| | | | | | | | (0.0078) | (0.0074) | (0.0062) | (0.0072) |
| GDP pc (log) | -0.0118*** | -0.0155*** | -0.0119** | -0.0160** | -0.0026 | -0.0023 | -0.0069 | 0.0542*** | -0.1824*** | 0.0554*** |
| | (0.0041) | (0.0054) | (0.0049) | (0.0063) | (0.0029) | (0.0021) | (0.0059) | (0.0166) | (0.0530) | (0.0164) |
| GDP pc growth | 0.0462*** | 0.0461*** | 0.0395*** | 0.0493*** | 0.0039 | 0.0004 | 0.0599*** | -0.0060 | -0.0097 | -0.0069 |
| | (0.0125) | (0.0126) | (0.0122) | (0.0153) | (0.0092) | (0.0054) | (0.0170) | (0.0059) | (0.0203) | (0.0061) |
| Inflation | 0.0211*** | 0.0255*** | 0.0206*** | 0.0255*** | 0.0028*** | 0.0015*** | 0.0370*** | 0.0336*** | 0.0322* | 0.0306*** |
| | (0.0026) | (0.0040) | (0.0025) | (0.0040) | (0.0005) | (0.0004) | (0.0037) | (0.0040) | (0.0191) | (0.0034) |
| Exchange rate (log) | -0.0025*** | -0.0038*** | -0.0033*** | -0.0047*** | -0.0003 | -0.0003 | 0.0047 | 0.0038 | 0.0562*** | 0.0032 |
| | (0.0009) | (0.0013) | (0.0011) | (0.0016) | (0.0005) | (0.0004) | (0.0048) | (0.0049) | (0.0158) | (0.0049) |
| Resource rents / GDP | 0.0456*** | 0.0577*** | 0.0462*** | 0.0579*** | 0.0270* | 0.0207 | 0.0207 | 0.0230 | -0.0070 | 0.0236* |
| | (0.0164) | (0.0177) | (0.0169) | (0.0187) | (0.0154) | (0.0135) | (0.0134) | (0.0138) | (0.0058) | (0.0133) |
| New democracy | | | -0.0031 | -0.0035 | | | | | | |
| | | | (0.0020) | (0.0024) | | | | | | |
| Polity IV | | | 0.0065 | 0.0091 | | | | | | |
| | | | (0.0045) | (0.0056) | | | | | | |
| Country x Month effect | No | No | No | No | No | No | No | Yes | No | Yes |
| Up to 3-lag dep. var. | No | Yes | No | Yes | No | Yes | Yes | Yes | Yes | Yes |
| Observations | 13,017 | 12,777 | 11,449 | 11,242 | 12,530 | 12,283 | 9,275 | 9,275 | 9,250 | 9,250 |
| R-squared | 0.1207 | 0.1398 | 0.1323 | 0.1505 | 0.0316 | 0.3049 | 0.1191 | 0.3328 | 0.0455 | 0.1065 |
| Countries | 84 | 84 | 73 | 73 | 83 | 83 | 84 | 84 | 83 | 83 |
| F | 2093 | 188021 | 1683 | 2666 | 451.8 | 828.5 | 25.26 | . | 35.89 | . |

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

Notes: All regressions include country, year and month fixed effects. Standard errors are robust to heteroscedasticity and are clustered at the level of countries.

With data recorded by month, the dependent variable ($\widehat{M1}$) exhibits strong seasonal patterns. Insofar as politicians can time election dates within a certain time window (e.g., a calendar year) and they perceive it to be beneficial to hold elections in months which are known, for seasonal reasons, to be associated with high economic activity and strong growth in M1, our baseline results could be driven by reverse causality. Inspection of the data on the timing of elections shows that the frequency of elections by month is not uniform across the year. For example, October to December, typically periods of high economic activity, on average, host over five times more elections than January to August. We include the month fixed effects in all models to control for this possibility. Column (8) reports a more demanding specification which includes country-specific mouth fixed effects.[20] We observe that it makes little difference to the size of the point estimate and the election day effect remains statistically significant. We can, however, go one step further and seasonally adjust the monthly M1 series for each country by the X12-ARIMA procedure used by the US Census Bureau. Columns (9) to (10) report estimates based on the seasonally adjusted data. We observe that the election day effect continues to be significant at the 5% level but that the point estimate is a little smaller than previously (around 1.4 percentage points). This is expected because the seasonal adjustment smooths the variation in M1 around elections in countries where elections always take place in the same month. A final check is to look at the sub-sample of 18 countries in our main sample which have fixed election days. The baseline results hold for this sub-sample [not reported]. All in all, this does not suggest that the results are due to election date timing effects and reverse causality.

[20]It is impossible to estimate the effect of *Election month* with country-specific mouth fixed effects in countries where elections take place regularly in the same month. For this reason, we report the results for *Election day*. We note that this requires estimating more than thousand dummies variables and makes it impossible to calculate the F-statistic for overall significance.

## 5.2 Heterogeneity in the election date effect

Exploring heterogeneity in the election date effect is important because it offers potential insights into the underlying mechanism behind the average results reported in Table 1. In Table 2, we split the sample by geographic region. The top panel shows the results for *Election month* while the lower panel shows the results for *Election day*. We observe that the election month effect is consistently statistically significant in East-Asia and the Pacific (column 1) and in Sub-Saharan Africa (column 6). In the specifications with *Election day*, the positive point estimates in Latin America and in South-Asia become statistically significant.

**Table 2:** Growth of M1 during elections by geographic region

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| VARIABLES | *Monthly growth of M1* | | | | | |
| REGION | East-Asia & Pacific | East-Europe & Central-Asia | Latin America | Middle-East & North-Africa | South-Asia | Sub-Saharan Africa |
| Election month | 0.0097* | 0.0022 | 0.0042 | 0.0037 | 0.0060 | 0.0192*** |
| | (0.0050) | (0.0029) | (0.0052) | (0.0028) | (0.0098) | (0.0074) |
| Observations | 1,840 | 2,121 | 2,663 | 1,424 | 464 | 3,348 |
| R-squared | 0.13 | 0.35 | 0.45 | 0.07 | 0.10 | 0.11 |
| Countries | 9 | 18 | 14 | 10 | 6 | 20 |
| F | 11.29 | 13.66 | 113.5 | 14.24 | 134.8 | 51.12 |
| Election day | 0.0281*** | 0.0032 | 0.0178* | 0.0053 | 0.0525*** | 0.0301** |
| | (0.0075) | (0.0059) | (0.0101) | (0.0100) | (0.0188) | (0.0118) |
| Observations | 1,144 | 1,956 | 1,717 | 1,008 | 463 | 2,364 |
| R-squared | 0.11 | 0.36 | 0.50 | 0.07 | 0.10 | 0.10 |
| Countries | 9 | 18 | 14 | 10 | 6 | 20 |
| F | 3.839 | 11.17 | 7.269 | 0.332 | 133.4 | 29.02 |

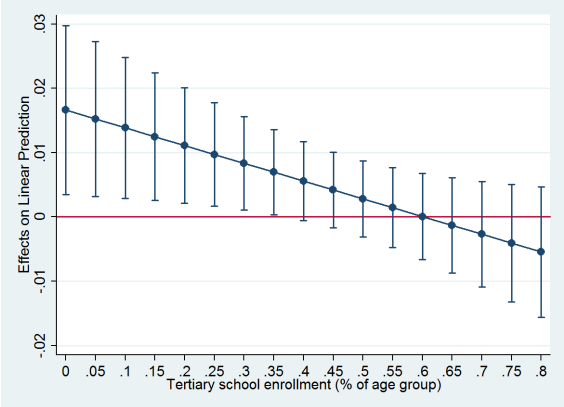
*** p<0.01, ** p<0.05, * p<0.1

Notes: All regressions control for GDP growth, GDP p.c., inflation, the exchange rate, and resource rents; include three lags of the dependent variable; and country, year and month fixed effects. Standard errors are robust to heteroscedasticity and are clustered at the level of countries.

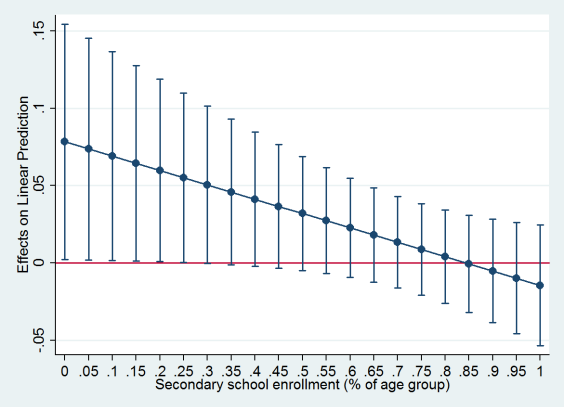
We can also engage with the possibility of heterogeneity in the election date effect by studying its interaction with underlying economic characteristics.[21] We focus on four characteristics: education attainment (net enrollment in secondary and tertiary education); poverty (defined as the share of the population earning less than 2.00 US dollars per day); and unemployment (defined as the fraction of the workforce out of employment). We summarize the results in Figures 3(a) to (d) which show point estimates of the interaction effects along with 95 percent confidence intervals.[22]

**Figure 3:** The marginal effects of the election timing variable for different values of the interacting variables

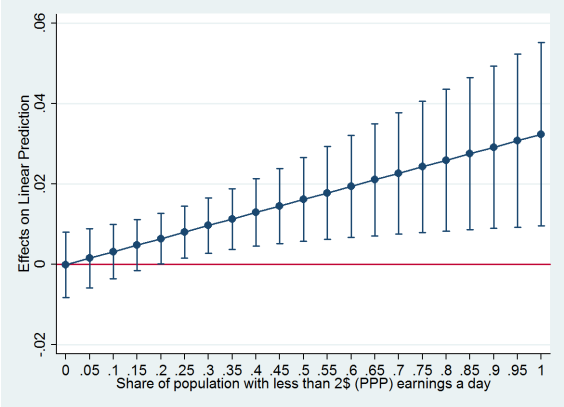
**(a)** Tertiary Education (Table A4, column 2)



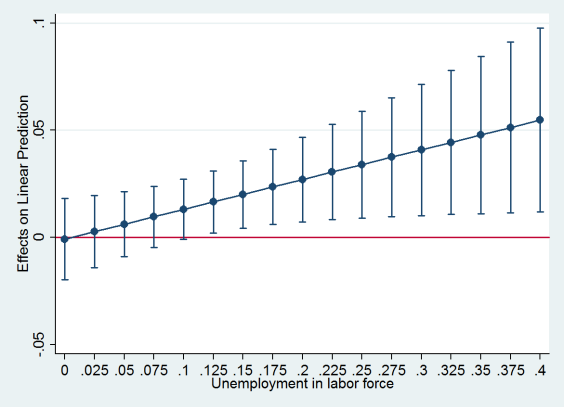

**(b)** Secondary Education (Table A4, column 5)




**(c)** Poverty (Table A4, column 3)




**(d)** Unemployment (Table A4, column 12)




[21] We do not observe these characteristics by month. Accordingly, we interact the two election timing variables with the year-country average of the relevant characteristics.

[22] The regression results are reported in Table A4 in the appendix.

Figure 3(a) shows that the election date effect is statistically significant up to a net enrollment rate in tertiary education of about 30%. For very high enrollment rates, the point estimate is negative but not statistically different from zero. Figure 3(b) shows a similar pattern for secondary education. Figure 3(c) and 3(d) show the results for the interactions with poverty and unemployment, respectively. The election date effect is stronger in societies with a larger faction of the population below the poverty line and with higher unemployment rates, and it is not statistically significant for relatively prosperous places with low unemployment.

## 5.3 Interpretation

Our baseline result is a robust, statistically significant, and economically meaningful monthly electoral cycle in the growth rate of M1. We interpret this as evidence of vote buying. This interpretation is bolstered by the pattern that we observe when we interact the election timing variables with underlying economic characteristics. We know from previous studies of vote buying that uneducated populations are more prone to electoral corruption (e.g., Krishna 2003). We also know that poverty and unemployment is correlated with electoral corruption and self-reported vote buying (e.g., Jensen and Justesen 2014). The fact that we find evidence that the election date effect is larger in countries with low enrollment in secondary and tertiary education, or with a high fraction of the population living below the poverty line, or with high unemployment is consistent with the vote buying mechanism. The regional pattern, with the largest effect being observed in Sub-Saharan Africa, points in the same direction.

It is clear, however, that other mechanisms could be at play, either as a complement to or as a substitute for the vote buying mechanism. It is, therefore, essential to evaluate alternative explanations carefully. The rest of the paper is devoted to that task. At this point, we simply note that the heterogeneity we observe in the estimate of the election date effect is hard to square with most of these alternatives, while vote buying provides a straightforward explanation for the observed pattern.

# 6 Alternative explanations

In this section, we evaluate alternative explanations for the election date cycle in M1. We consider the following alternatives to the vote buying mechanism: i) the central bank actively expands liquidity just before elections; ii) political parties demand cash to fund legal election campaigns; iii) elections, in general, increase economic activity and the demand for liquidity; and iv) the government pays wage arrays and clears debt to private sector creditors just before elections.

## 6.1 Monetary political business cycles

In line with traditional theories on monetary policy cycles, governments may use their influence on the central bank to engineer a monetary expansion prior to elections. The purpose is to reduce unemployment or generate additional economic activity in the hope that this will improve the government's re-election prospect. In contrast to vote buying which affects M1 through an increase in the demand for liquidity, the monetary political business cycle requires an active expansion of the primary supply of money and, therefore, must involve the central bank. Moreover, since monetary expansions affect the real economy with significant lags, the central bank intervention would have to take place well in advance of the election.

Table 3 reports two sets of results that evaluate this possibility. First, we investigate if the increase in the growth rate of M1 occurs in the months prior to the election month. Columns (1) to (4) show that this is not the case.[23] The increase in the growth rate of M1 happens exactly in the month of the election and not in any of the three months before that. Second, the central bank could use the discount window rather than open market operations to induce an election-motivated expansion of the monetary base. If so, the interest rate that the central bank charges its borrowers should fall in the months leading up to an election. Columns (5) to (8) show that the central bank's lending rate

[23]We focus on *Election month* because it captures the timing effect more clearly. Similar patterns, however, emerge with *Election day*.

neither changes in the election month nor in the months prior to that. These two sets of results strongly speak against the classical monetary political business cycle explanation of our results. They, however, do not rule out that governments with full control over the central bank could drawn on primary liquidity just prior to elections in order to buy votes directly or indirectly through targeted spending. We note that such effects would be consistent with the voting buying interpretation of the election date effect.

**Table 3:** Growth of M1 and central bank interest rates during and before election months

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| VARIABLES | *Monthly growth of M1* | | | | *Central Bank interest rate* | | | |
| Election month | 0.0061** | | | | 0.0004 | | | |
| | (0.0028) | | | | (0.0006) | | | |
| Election month (t+1) | | 0.0058 | | | | 0.0003 | | |
| | | (0.0042) | | | | (0.0004) | | |
| Election month (t+2) | | | -0.0020 | | | | -0.0006 | |
| | | | (0.0025) | | | | (0.0006) | |
| Election month (t+3) | | | | 0.0052 | | | | -0.0000 |
| | | | | (0.0037) | | | | (0.0005) |
| Observations | 12,536 | 12,534 | 12,486 | 12,429 | 2,434 | 2,418 | 2,402 | 2,386 |
| R-squared | 0.14 | 0.14 | 0.14 | 0.14 | 0.98 | 0.98 | 0.98 | 0.98 |
| Countries | 84 | 84 | 84 | 84 | 16 | 16 | 16 | 16 |
| F | 175361 | 31893 | 160525 | 192666 | 360937 | 375889 | 319195 | 52366 |

*** p<0.01, ** p<0.05, * p<0.1
Notes: The variables *Election (t+i)* for $i = 1, .., 3$ are coded one in month $i$ before the election. All regressions control for GDP growth, GDP p.c., inflation, the exchange rate, resource rents; include six lags of the dependent variable; and country, year and month fixed effects. Standard errors are robust to heteroscedasticity and are clustered at the level of countries.

To further probe the issue of traditional monetary political business cycles, we aggregate the monthly M1 data to the quarterly frequency and redefine the election dummy to be equal to one in election quarters and zero otherwise (*Election quarter*). With this data, we can estimate three-ways fixed effects panel models similar to those typically used in the empirical literature on monetary political business cycles (e.g., Alesina et al. 1993).

Table 4 shows the results for the quarterly sample. We observe a significant increase in the growth of M1 four quarters before the election quarter (column 5) for the sample of low and middle income countries.[24]

These results, on the one hand, provide further evidence against the classical monetary political business cycle as an explanation for the cycle we find in the monthly data. On the other hand, by studying the monetary data at different frequencies, we are able to disentangle the concurrent monetary effect of elections, which we attribute to vote buying, from monetary expansions aimed at generating a Nordhaus-MacRae type political business cycle. Both appear to be present in our sample of low and middle income countries, but the later materializes as a monetary expansion four quarters in advance of the election, as one would expect given the speed of the monetary transmission mechanism, and, therefore, cannot offer an explanation for the election date effect.

[24]We have also aggregated the data to the yearly frequency. Table A5 in the appendix shows the results for the yearly sample. We observe that we cannot find any evidence of a traditional monetary political business cycle at that frequency. We have estimated similar models for the sample of the 13 "old" OECD countries. We find no evidence of cycles in neither the quarterly nor the annual data.

**Table 4:** Political business cycles in M1 around election quarters

| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | *Quarterly growth of M1* | | | | | | | | | | | |
| Sample | Low and middle income countries | | | | | | Old OECD countries | | | | | |
| Election quarter | 0.0024 | | | | | | -0.0030 | | | | | |
| | (0.0063) | | | | | | (0.0073) | | | | | |
| Election quarter (t+1) | | 0.0024 | | | | | | 0.0003 | | | | |
| | | (0.0046) | | | | | | (0.0035) | | | | |
| Election quarter (t+2) | | | 0.0055 | | | | | | 0.0015 | | | |
| | | | (0.0058) | | | | | | (0.0050) | | | |
| Election quarter (t+3) | | | | 0.0033 | | | | | | -0.0022 | | |
| | | | | (0.0085) | | | | | | (0.0044) | | |
| Election quarter (t+4) | | | | | 0.0261** | | | | | | -0.0027 | |
| | | | | | (0.0129) | | | | | | (0.0046) | |
| Election quarter (t+5) | | | | | | -0.0057 | | | | | | 0.0031 |
| | | | | | | (0.0064) | | | | | | (0.0024) |
| Observations | 3,994 | 3,992 | 3,928 | 3,856 | 3,782 | 3,705 | 1,089 | 1,088 | 1,081 | 1,073 | 1,065 | 1,057 |
| R-squared | 0.45 | 0.45 | 0.47 | 0.47 | 0.47 | 0.47 | 0.36 | 0.36 | 0.36 | 0.36 | 0.36 | 0.37 |
| Countries | 85 | 85 | 85 | 85 | 85 | 84 | 13 | 13 | 13 | 13 | 13 | 13 |
| F | 4657 | 4219 | 4014 | 4155 | 3077 | 3169 | 40.18 | 32.76 | 24.35 | 50.69 | 38.26 | 46.95 |

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

Notes: The data is quarterly. The variables *Election quarter (t+i)* for $i = 1, .., 5$ are coded one in quarter $i$ before the election. All regressions control for GDP growth, GDP p.c., inflation, the exchange rate, resource rents; include six lags of the dependent variable; and country, year and quarter fixed effects. Standard errors are robust to heteroscedasticity and are clustered at the level of countries.

## 6.2 Legal campaign spending

The election date effect that we have identified could, in principle, be driven by legal spending on election campaigns. Like vote buying, election campaigns may generate unusually high demand for money which may manifest itself as unusually high election month growth in M1. Election campaigns, however, take time to plan and spending on campaign staff, advertising etc. is, unlike spending on vote buying, spread out over a longer time period before elections. The fact that the monetary election date cycle that we have identified is concentrated precisely in the election month and not in the months leading up to the election, therefore, speaks against legal campaign spending being the main cause of the election date effect.

Elections and election campaign spending go hand in hand everywhere. Accordingly, if the election date effect in the sample of low and middle income countries were caused by election campaigns, a similar cycle should be present in the sample of the 13 "old" OECD countries. After all, vast sums of private and public money are spent on campaigns in all these countries. Equally importantly, the "old" OECD countries have long-established democratic institutions, strong accountability, vigilant media, and an independent judiciary and they generally score highly on indexes of the quality of institutions (e.g., Freedom House 2012). While isolated instances of electoral corruption are observed also in those countries and reported by the media, the institutions are such that systemic vote buying is not likely to prevail. We can, therefore, plausibly rule out systemic vote buying as the cause of any monetary election date cycle in these countries which would then have to be attributed to election campaigning (or some other cause).

Table 5 reports estimates of equation (1) with data from the 13 OECD countries only. We find no evidence of any monetary election cycle, neither in the election month nor around the election day, or during the months leading up to the election. This suggests that the monetary election date cycle that we find in the sample of low and middle income countries does not just reflect legal election campaign spending. We can, of course, not rule out that part of the cycle is generated by such spending, or that there are sys-

tematic differences in the way campaigning is conducted and funded in the two samples. However, if campaign spending were the only or even the main cause, the cycle should also be observable in the OECD sample. This strengthens our interpretation that the election date effect observed in our main sample is associated with vote buying and that relatively weak electoral institutions play an important intervening role.

**Table 5:** Growth of M1 around elections in the sample of "old" OECD countries

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | *Monthly growth of M1* | | | | |
| Election day | 0.0153 | | | | |
| | (0.0307) | | | | |
| Election month | | 0.0011 | | | |
| | | (0.0037) | | | |
| Election month (t+1) | | | 0.0045 | | |
| | | | (0.0034) | | |
| Election month (t+2) | | | | -0.0008 | |
| | | | | (0.0028) | |
| Election month (t+3) | | | | | 0.0015 |
| | | | | | (0.0035) |
| Observations | 1,257 | 3,451 | 3,450 | 3,444 | 3,437 |
| R-squared | 0.17 | 0.30 | 0.30 | 0.30 | 0.30 |
| Countries | 13 | 13 | 13 | 13 | 13 |
| F | 47072 | 13.72 | 21.28 | 13.74 | 16.40 |

*** p<0.01, ** p<0.05, * p<0.1
Notes: The variables *Election (t+i)* for $i = 1, .., 3$ are coded one in month $i$ before the election. All regressions control for GDP growth, GDP p.c., inflation, exchange rate, resource rents; include three lags of the dependent variable; and country, year and month fixed effects. Standard errors are robust to heteroscedasticity and are clustered at the level of countries.

## 6.3 National events

Elections are large national events that generate increased economic activity. This is partly due to the cost of organizing and running elections and partly due to private sector spending on election celebrations. The consequence of this could be extra demand

for liquidity which would then show up as an increase in the growth rate of M1 in the election month. To investigate the power of this explanation, we explore the idea that similar monetary effects should, if this is an important factor, be present during other big national events. We collect information on the dates of "independence days" (or if no such day is celebrated in a country, the most celebrated national holiday) and define the dummy variable *national independence day* as being equal to one in the month in which the "independence day" of a country is celebrated and zero otherwise.[25]

Table 6 reports estimates of equation (1) with the election timing variable replaced by *national independence day*. Columns (1) to (6) show the results for the seasonally unadjusted data while columns (7) to (11) show the results for the seasonally adjusted data. The point estimate is negative and significant at the ten percent level in some specifications (columns 1 and 5). These significant results, however, disappear when the seasonality of the growth rate of M1 is taken into account. Thus, unlike the election date effect, which is positive, significant and robust to the seasonal adjustment, there is no evidence for an "independence day" effect. This makes it unlikely that the election date effect can attributed to the fact that elections are big national events associated with unusual economic activity.

[25]We require that the event is national in scope, and we focus on "independence days" to insure comparability across countries. The notes to Table 6 list for each country the month in which the "independence day" that we code is celebrated.

**Table 6:** Growth of M1 during national holidays

| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | *Monthly growth of M1* | | | | | | *Monthly growth of seasonally adjusted M1* | | | | |
| Sample | Low and middle income countries | | | | | OECD | Low and middle income countries | | | | OECD |
| National Independence Day | -0.0045* | -0.0043 | -0.0043 | -0.0043 | -0.0049* | -0.0002 | -0.0005 | -0.0005 | -0.0005 | -0.0004 | 0.0004 |
| | (0.0027) | (0.0027) | (0.0026) | (0.0026) | (0.0026) | (0.0058) | (0.0012) | (0.0012) | (0.0013) | (0.0011) | (0.0010) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| M1 growth lagged at: | | 1 | 1-2 | 1-3 | 1-6 | 1-6 | | 1 | 1-3 | 1-6 | 1-6 |
| Country, Year, Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 13,134 | 13,053 | 12,972 | 12,891 | 12,647 | 3,451 | 9,402 | 9,348 | 9,241 | 9,077 | 3,305 |
| R-squared | 0.16 | 0.17 | 0.18 | 0.18 | 0.19 | 0.29 | 0.07 | 0.08 | 0.08 | 0.10 | 0.08 |
| Countries | 84 | 84 | 84 | 84 | 84 | 13 | 79 | 79 | 79 | 79 | 13 |
| F | 1505 | 2093 | 2569 | 8648 | 3290 | 19.66 | 4179 | 3862 | 1384 | 6790 | 56.07 |

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

Notes: Standard errors are robust to heteroscedasticity and are clustered at the level of countries. Wikipedia is the main source of the *independence day* dummy, which is coded as follows:

January: Australia , Czech Republic, Haiti, Samoa, Slovak Republic; February: Chile, Kuwait, New Zealand, Sri Lanka, St Lucia; March: Bangladesh, Bosnia-Herzogovina, Ghana, Lithuania, Mauritius, Namibia; April: Japan, Sierra Leone, South Africa; May: Eritrea, Ethiopia, Guyana, Jordan, Nepal, Paraguay, Romania; June: Croatia, Denmark, Mozambique, Slovenia, Sweden; July: Algeria, Argentina, Bahamas, Belarus, Cape Verde, Colombia, Comoros, France, Liberia, Malawi, USA, Vanuatu; August: Afghanistan, Bahrain, Dominican Republic, Estonia, Indonesia, Jamaica, Korea, Kyrgyz Republic, Moldova, Pakistan, Singapore, Switzerland, Ukraine, Uruguay; September: Armenia, Belize, Brazil, Bulgaria, El Salvador, Guatemala, Honduras, Macedonia, Malaysia, Malta, Mexico, Nicaragua, Swaziland; ; October: Austria, China, Cyprus, Egypt, Hungary, Iraq, Lesotho, Nigeria, Spain, Turkey, Uganda, Zambia; November: Albania, Angola, Barbados, Cambodia, Lebanon, Mongolia, Oman, Poland, Suriname; December: Bhutan, Iceland, Kazakhstan, Kenya, Portugal, Qatar, Tanzania.

## 6.4 Election day as pay day

Opportunistic politicians may pay civil servants a "bonus" or "clear arrays" just before elections in the hope that it will win them votes or, if they control the central bank, they may let the central bank finances pre-election benefits to important constituencies. Akhmedov and Zhuravskaya (2004) document one important example of the former effect from Russia. They find a sizable increase in direct monetary transfers to voters from the regional governments in the days leading up to the election. This could be viewed as vote buying with public funds. Insofar as the resources needed to finance such public spending come from either a Treasury account or a local government account, and not directly from the central bank, this is unlikely to be the main explanation for the election date effect.[26] The reasons is that such funds will be counted as part of M1 before and after they are transferred to the bank accounts of the government employees or creditors or are handed out in cash. The underlying financial transactions would, therefore, not involve a direct substitution of illiquid for liquid assets.[27] Any effect on the growth rate of M1 would have to come from an increase in the money multiplier. Under the plausible assumption that government funds are held mostly in deposit accounts while the beneficiaries of the transfers hold some of their liquid funds in cash, the transactions would *reduce* rather than increase the average size of the money multiplier. Although we cannot entirely rule out that part of the election date effect could be a by-product of a high frequency political spending cycle, direct vote buying appears to offer a more plausible and consistent explanation.

[26]If the funds come from the central bank, they would, as noted above, constitute an injection of money to the economy and affect M1 directly. An example of this is Venezuela under Hugo Chavez. His government controlled the central bank and apparently funded large off-budget transfers to key supporters in the run-up to the election in 2009 from this source (Economist 2012). The empirical support for such effects from broader samples of countries is, however, not strong (Alpanda and Honig 2009, 2010).

[27]For example, the EU manual (European Central Bank 2015) says "A harmonized definition of the money-holding sector, which comprises all non-MFIs resident in the Euro-area (except central government). In addition to households, non-financial corporations and financial institutions which are not MFIs are included, as well as state and local governments and social security funds. Central governments are considered to constitute a "money-neutral" sector, with one exception: central government liabilities with a monetary character (Post Office accounts, national savings accounts and Treasury accounts) are included as a special item in the definition of monetary aggregates."

# 7 Cross validation and "back of the envelope calculations"

To further bolsters the vote buying explanation, we present some cross validation checks and "back of the envelope calculations".

## 7.1 Cross validation

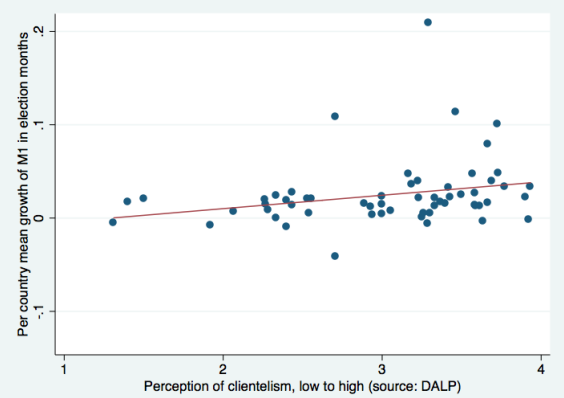
Insofar as the estimated election date effect is caused by vote buying, the effect should be bigger in countries which accordingly to available survey data are more prone to electoral corruption. For the purpose of investigating if this is the case, we create a cross-country data set with two measures of the size of the election date effect. The first measure is a simple country average of the growth rate of M1 in election months. The second measure is the estimate of the beta-coefficient (on *Election month*) obtained by estimating equation (1) country-by-country. We correlate these country-specific estimates of election date effect with an index of perceived electoral clientelism[28] and with survey-based data on the extent of vote buying. If the monetary expansion is caused, at least in part, by vote buying, then these correlations should be positive. Figure 4 shows correlation plots between the size of the election date effect and perceived clientelism (sub-figures a, b) and self-reported vote buying from the Latino-Barometer (sub-figures c, d). The correlations are positive in all cases and statistically significant in sub-figures 4(a) and 4(d).[29] These correlations strengthen our interpretation of the monetary election date cycle as a manifestation of vote buying.

[28]The source of our index is Democratic Accountability and Linkages Project (2014). It is based on a survey in which the respondents were asked to score the following question on a 1 (low or negligible effort) to 4 (high effort) scale: "In general, how much effort do politicians and parties in [this country] make to induce voters with preferential benefits to cast their votes for them?"

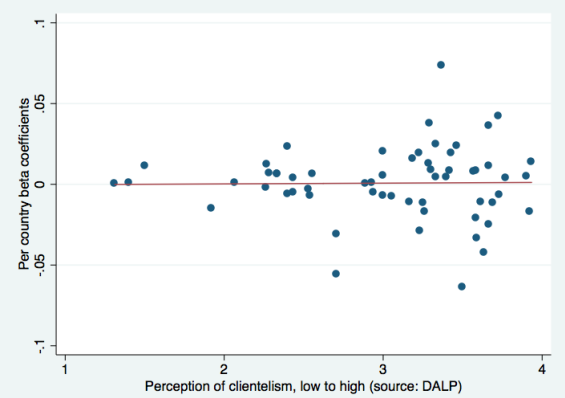
[29]We use information from the 2002 wave of the Latino-barometer. The corresponding correlation between average growth in M1 in election months and the data on vote buying from the Afro-barometer is positive but not significant.

**Figure 4:** Cross country correlations of the election month increase in M1 and perception of clientelism (a, b) or survey-based measures of vote buying (c, d)

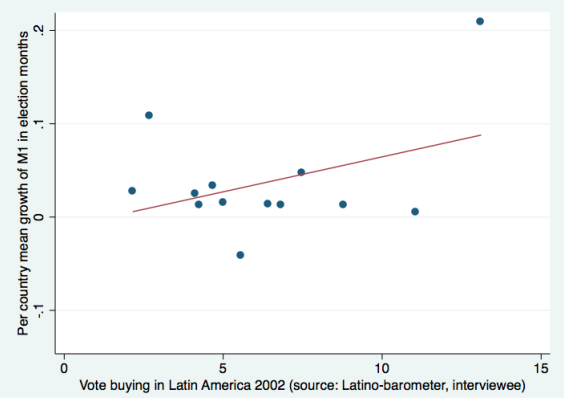
**(a)** Election-M1 - Clientelism (*corr.=.26***)



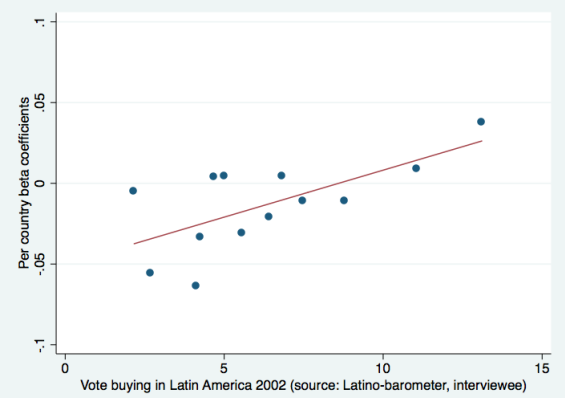

**(b)** Election-M1 - Clientelism (*corr.=.01*)




**(c)** Election-M1 - Vote buying (*corr.=.39*)




**(d)** Election-M1 - Vote buying (*corr.=.67****)



### 7.2 "Back of the envelope calculations"

By combining the information on the average size of the election month increase in M1 in each country with survey estimates of the extent of vote buying, we can calculate what the implied "price" per vote must have been in order to fully account for the observed increase in M1.[30] For our five case-study countries from Section 3, the implied vote prices are (prices reported in the literature shown in bracket): 50 (20) US dollars in Armenia, 80 (700) US dollars in Lebanon, 90 (n.a.) US dollars in Bolivia, 10 (4) US dollars in Nigeria, and 85 (8) US dollars in Indonesia. These implied prices are higher than the prices reported in the literature for the various countries, but not by an order of magnitude. Moreover, the prices reported in the literature ignore the fact that the dealers and brokers make lots of money. Since this adds to M1, the gap between implied and reported prices is overestimated.

## 8 Conclusions

This paper offers a new perspective on the monetary effects of elections. We report robust evidence of a systematic monetary expansion in the month of elections in a sample of 85 low and middle income democracies. The expansion amounts to about one tenth of a standard deviation in the month-to-month growth rate of M1. Our preferred interpretation is that the expansion is demand driven and that it is induced by systemic vote buying broadly understood. Our empirical findings are consistent with this explanation in several dimensions and speak against alternative explanations. First, we detect a finely timed increase in the demand for liquidity centered just around elections, as one would expect if vote buying is the underlying cause. Second, the strong asymmetry between established OECD democracies and low and middle income democracies also reinforces the vote buying explanation since systemic vote buying is not (as far as we can tell) present in mature democracies. Moreover, it helps rule out that legal campaign spending can be

[30]Specifically, we divided the average election month increase in M1 for a country measured in US dollars with an guesstimate of the size of the voting population which might have received a bribe.

the sole explanation. Third, we systematically evaluate and rule out other alternative explanations. Our findings complements the literature on monetary political business cycles by pointing to the role of passive monetary developments that do not require any monetary policy decisions. This obviously allows for new avenues for monetary political cycles even in democracies where central banks are independent from political influence. Our approach also opens up potentially useful ways to quantify vote buying and electoral corruption more generally.

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**Table A1:** Summary statistics

| Variable | Obs | Frequency | Mean | Std. Dev. | Min | Max | Source |
|---|---|---|---|---|---|---|---|
| M1 growth | 17,355 | Monthly | 0.016 | 0.065 | -0.876 | 2.204 | WEO/EconStats |
| M1 seasonally adjusted growth | 17,069 | Monthly | 0.015 | 0.054 | -0.874 | 2.327 | WEO/EconStats |
| Election month dummy | 41,062 | Monthly | 0.019 | 0.138 | 0 | 1 | DPI |
| Election day (normalized distance to election date) | 14,774 | Monthly | 0.0054 | 0.0419 | 0 | 1 | IFES |
| GDP per capita (constant 2005 USD) | 34,801 | Annual | 7975 | 11476 | 50 | 58066 | WDI |
| GDP per capita growth (PPP) | 34,801 | Annual | 0.020 | 0.062 | -0.502 | 0.917 | WDI |
| Inflation | 34,742 | Annual | 0.466 | 3.681 | -0.257 | 136 | WDI |
| Exchange rate (per USD, log) | 39,229 | Monthly | 1.725 | 3.660 | -21.73 | 9.54 | WEO/EconStats |
| Resource rents in GDP | 35,855 | Annual | 0.082 | 0.127 | 0.000 | 0.894 | WDI |
| Secondary education (net enrollment rate) | 11,433 | Annual | 0.656 | 0.263 | 0.027 | 1 | WDI |
| Tertiary education (net enrollment rate) | 27,795 | Annual | 0.228 | 0.205 | 0 | 1.018 | WDI |
| Poverty, share of population earning <2.00$/day | 6,328 | Annual | 0.270 | 0.281 | 0 | 0.978 | WDI |
| Unemployment in labor force | 22,743 | Annual | 0.094 | 0.068 | 0.003 | 0.393 | WDI |
| Polity IV score of democracy (normalized) | 35,628 | Annual | 0.595 | 0.364 | 0 | 1 | CSP |
| Dummy for new democracies | 35,401 | Annual | 0.235 | 0.424 | 0 | 1 | CSP |
| Independence day dummy | 41,062 | Monthly | 0.078 | 0.268 | 0 | 1 | Wikipedia |
| Central bank interest rate | 3,800 | Monthly | 0.085 | 0.068 | 0.000 | 0.540 | DS, Gecodia |
| | | | | | | | National Central Banks |

Notes: The annual variables are repeated monthly. WEO/EconStats is World Economic Outlook (2015), DPI is Database of Political Institutions (Beck et al. 2001), WDI is World Development Indicators (2014), IFES is International Foundation for Electoral Systems (2015), CSP is Center for Systematic Peace (2015), DS is Deltastock (2015), and Gecodia is Gecodia (2015).

**Table A2:** Sample of countries

| OECD countries | | | Low and middle income countries | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| *No* | Country | Months | *No* | Country | Months | *No* | Country | Months | *No* | Country | Months |
| *Member as of 1975* | | | 23 | Afghanistan | 35 | 49 | Eritrea | 161 | 75 | Mozambique | 154 |
| 1 | Australia | 416 | 24 | Albania | 82 | 50 | Ethiopia | 172 | 76 | Namibia | 88 |
| 2 | Austria | 277 | 25 | Algeria | 104 | 51 | Ghana | 87 | 77 | Nepal | 104 |
| 3 | Denmark | 225 | 26 | Angola | 116 | 52 | Guatemala | 142 | 78 | Nicaragua | 72 |
| 4 | France | 252 | 27 | Argentina | 141 | 53 | Guyana | 137 | 79 | Nigeria | 117 |
| 5 | Iceland | 97 | 28 | Armenia | 106 | 54 | Haiti | 142 | 80 | Oman | 82 |
| 6 | Japan | 415 | 29 | Bahamas | 46 | 55 | Honduras | 178 | 81 | Pakistan | 112 |
| 7 | New Zealand | 189 | 30 | Bahrain | 67 | 56 | Indonesia | 178 | 82 | Paraguay | 250 |
| 8 | Portugal | 227 | 31 | Bangladesh | 130 | 57 | Iraq | 40 | 83 | Qatar | 334 |
| 9 | Spain | 287 | 32 | Barbados | 89 | 58 | Jamaica | 130 | 84 | Romania | 95 |
| 10 | Sweden | 142 | 33 | Belarus | 120 | 59 | Jordan | 238 | 85 | Samoa | 92 |
| 11 | Switzerland | 299 | 34 | Belize | 65 | 60 | Kazakhstan | 71 | 86 | Sierra Leone | 162 |
| 12 | Turkey | 284 | 35 | Bhutan | 87 | 61 | Kenya | 118 | 87 | Singapore | 226 |
| 13 | USA | 419 | 36 | Bosnia & Herzogovina | 146 | 62 | Kuwait | 93 | 88 | South Africa | 418 |
| *Member as of 2009* | | | 37 | Brazil | 283 | 63 | Kyrgyz Republic | 56 | 89 | SriLanka | 17 |
| 14 | Czech Republic | 94 | 38 | Bulgaria | 166 | 64 | Lebanon | 349 | 90 | St Lucia | 307 |
| 15 | Hungary | 142 | 39 | Cambodia | 95 | 65 | Lesotho | 94 | 91 | Suriname | 119 |
| 16 | Korea | 408 | 40 | Cape Verde | 167 | 66 | Liberia | 99 | 92 | Swaziland | 417 |
| 17 | Mexico | 287 | 41 | China | 131 | 67 | Lithuania | 155 | 93 | Tanzania | 375 |
| 18 | Poland | 155 | 42 | Colombia | 358 | 68 | Macedonia | 82 | 94 | Uganda | 94 |
| 19 | Slovak Republic | 71 | 43 | Comoros | 95 | 69 | Malawi | 118 | 95 | Ukraine | 167 |
| *Member as of 2014* | | | 44 | Croatia | 95 | 70 | Malaysia | 418 | 96 | Uruguay | 418 |
| 20 | Chile | 298 | 45 | Cyprus | 178 | 71 | Malta | 335 | 97 | Vanuatu | 163 |
| 21 | Estonia | 200 | 46 | Dominican Republic | 94 | 72 | Mauritius | 379 | 98 | Zambia | 151 |
| 22 | Slovenia | 155 | 47 | Egypt | 133 | 73 | Moldova | 119 | | | |
| | | | 48 | El Salvador | 106 | 74 | Mongolia | 156 | | | |

**Table A3:** Growth of M1 during elections: dynamics and variations in the definition of OECD countries

| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | *Monthly growth of M1* | | | | | | | | | | |
| Sample | Low and middle income countries | | | | | | | Non-OECD 2009 | | Non-OECD 2014 | |
| Election month | 0.0050* | 0.0064** | 0.0065** | 0.0067** | 0.0065** | 0.0061** | 0.0061** | 0.0073*** | 0.0086*** | 0.0061** | 0.0072** |
| | (0.0026) | (0.0028) | (0.0028) | (0.0028) | (0.0028) | (0.0028) | (0.0028) | (0.0027) | (0.0029) | (0.0027) | (0.0028) |
| M1 growth (t-1) | | -0.1239*** | -0.1323*** | -0.1335*** | -0.1343*** | -0.1367*** | -0.1386*** | | -0.1425*** | | -0.1435*** |
| | | (0.0356) | (0.0377) | (0.0381) | (0.0384) | (0.0385) | (0.0384) | | (0.0393) | | (0.0426) |
| M1 growth (t-2) | | | -0.0254* | -0.0300** | -0.0303** | -0.0312** | -0.0332** | | -0.0354** | | -0.0357** |
| | | | (0.0131) | (0.0142) | (0.0147) | (0.0152) | (0.0154) | | (0.0160) | | (0.0173) |
| M1 growth (t-3) | | | | -0.0199 | -0.0222 | -0.0244 | -0.0239 | | -0.0260 | | -0.0172 |
| | | | | (0.0169) | (0.0179) | (0.0182) | (0.0183) | | (0.0190) | | (0.0181) |
| M1 growth (t-4) | | | | | -0.0330*** | -0.0408*** | -0.0403*** | | -0.0393*** | | -0.0377** |
| | | | | | (0.0127) | (0.0133) | (0.0132) | | (0.0138) | | (0.0148) |
| M1 growth (t-5) | | | | | | -0.0500*** | -0.0524*** | | -0.0536*** | | -0.0549*** |
| | | | | | | (0.0129) | (0.0124) | | (0.0127) | | (0.0138) |
| M1 growth (t-6) | | | | | | | -0.0221 | | -0.0261 | | -0.0155 |
| | | | | | | | (0.0230) | | (0.0236) | | (0.0235) |
| GDP pc (log) | -0.0118*** | -0.0138*** | -0.0143*** | -0.0155*** | -0.0161*** | -0.0161*** | -0.0161*** | -0.0120** | -0.0160** | -0.0111** | -0.0141** |
| | (0.0041) | (0.0048) | (0.0050) | (0.0054) | (0.0056) | (0.0055) | (0.0055) | (0.0052) | (0.0068) | (0.0056) | (0.0071) |
| GDP pc growth | 0.0462*** | 0.0436*** | 0.0459*** | 0.0461*** | 0.0495*** | 0.0531*** | 0.0554*** | 0.0448*** | 0.0537*** | 0.0434*** | 0.0499*** |
| | (0.0125) | (0.0116) | (0.0120) | (0.0126) | (0.0131) | (0.0138) | (0.0143) | (0.0127) | (0.0142) | (0.0133) | (0.0140) |
| Inflation | 0.0211*** | 0.0239*** | 0.0248*** | 0.0255*** | 0.0264*** | 0.0276*** | 0.0279*** | 0.0208*** | 0.0278*** | 0.0208*** | 0.0274*** |
| | (0.0026) | (0.0032) | (0.0037) | (0.0040) | (0.0040) | (0.0038) | (0.0036) | (0.0024) | (0.0034) | (0.0024) | (0.0035) |
| Exchange rate (log) | -0.0025*** | -0.0032*** | -0.0035*** | -0.0038*** | -0.0038*** | -0.0038*** | -0.0038*** | -0.0024*** | -0.0038*** | -0.0025*** | -0.0038*** |
| | (0.0009) | (0.0010) | (0.0012) | (0.0013) | (0.0013) | (0.0011) | (0.0010) | (0.0009) | (0.0009) | (0.0009) | (0.0009) |
| Resource rents / GDP | 0.0456*** | 0.0527*** | 0.0554*** | 0.0577*** | 0.0561*** | 0.0553*** | 0.0518*** | 0.0435*** | 0.0490*** | 0.0462*** | 0.0524*** |
| | (0.0164) | (0.0167) | (0.0173) | (0.0177) | (0.0173) | (0.0164) | (0.0158) | (0.0164) | (0.0158) | (0.0173) | (0.0166) |
| Observations | 13,017 | 12,937 | 12,857 | 12,777 | 12,697 | 12,617 | 12,536 | 11,860 | 11,415 | 11,218 | 10,785 |
| R-squared | 0.12 | 0.14 | 0.14 | 0.14 | 0.14 | 0.14 | 0.14 | 0.11 | 0.14 | 0.11 | 0.14 |
| Countries | 84 | 84 | 84 | 84 | 84 | 84 | 84 | 78 | 78 | 75 | 75 |
| F | 2093 | 161.0 | 69458 | 188021 | 49505 | 2.290e+06 | 175361 | 472.1 | 495.2 | 176.7 | 3915 |

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

All regressions include country, year and month fixed effects. Standard errors are robust to heteroscedasticity and are clustered at the level of countries. The samples "non-OECD 2009" and "non-OECD 2014" exclude the countries which joined the OECD in 2009 and 2014 from the sample of low and middle income countries which excludes the OECD countries as of 1975.

**Table A4:** Interactions between *Election month* or *Election day* and measures of education, poverty and unemployment

| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | *Monthly growth of M1* | | | | | | | | *Monthly growth of seasonally adjusted M1* | | | |
| Election month | 0.0139 | 0.0119** | -0.0015 | 0.0029 | | | | | | | | |
| | (0.0116) | (0.0060) | (0.0043) | (0.0040) | | | | | | | | |
| Election day | | | | | 0.0795** | 0.0324 | -0.0100 | 0.0209 | 0.0478* | 0.0236 | -0.0049 | -0.0029 |
| | | | | | (0.0390) | (0.0229) | (0.0123) | (0.0141) | (0.0245) | (0.0151) | (0.0082) | (0.0090) |
| Secondary education | 0.0190 | | | | 0.0338 | | | | 0.0416 | | | |
| | (0.0239) | | | | (0.0337) | | | | (0.0363) | | | |
| x Election | -0.0148 | | | | -0.0916* | | | | -0.0617** | | | |
| | (0.0135) | | | | (0.0468) | | | | (0.0299) | | | |
| Tertiary education | | 0.0044 | | | | -0.0045 | | | | 0.0028 | | |
| | | (0.0066) | | | | (0.0120) | | | | (0.0109) | | |
| x Election | | -0.0190* | | | | -0.0343 | | | | -0.0360 | | |
| | | (0.0108) | | | | (0.0447) | | | | (0.0292) | | |
| Poverty (2.00$/day) | | | -0.0118 | | | | 0.0062 | | | | 0.0066 | |
| | | | (0.0144) | | | | (0.0151) | | | | (0.0157) | |
| x Election | | | 0.0349** | | | | 0.2644*** | | | | 0.1511** | |
| | | | (0.0138) | | | | (0.0950) | | | | (0.0631) | |
| Unemployment | | | | 0.0120 | | | | 0.0245 | | | | 0.0193 |
| | | | | (0.0236) | | | | (0.0225) | | | | (0.0201) |
| x Election | | | | 0.0188 | | | | 0.0181 | | | | 0.1481** |
| | | | | (0.0275) | | | | (0.0824) | | | | (0.0688) |
| Observations | 6,590 | 11,502 | 3,739 | 11,883 | 5,218 | 7,192 | 3,158 | 9,422 | 5,207 | 7,181 | 3,158 | 9,397 |
| R-squared | 0.13 | 0.19 | 0.29 | 0.15 | 0.11 | 0.15 | 0.34 | 0.13 | 0.05 | 0.04 | 0.08 | 0.05 |
| Countries | 68 | 82 | 59 | 85 | 64 | 80 | 59 | 85 | 64 | 80 | 59 | 84 |
| F | 5607 | 3727 | 288.3 | 30.35 | 32.15 | 13.71 | 141.5 | 29.51 | 69.00 | 18.47 | 100.0 | 45.43 |

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

Notes: All regressions control for GDP growth, GDP p.c., inflation, the exchange rate, resource rents, new democracy, polity IV; include three lags of the dependent variable; and country, year and month fixed effects. Standard errors are robust to heteroscedasticity and are clustered at the level of countries.

**Table A5:** Political business cycles in M1 around election years

| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | *Annual growth of M1* | | | | | | |
| Sample | Low and middle income countries | | | | | Old OECD countries | |
| Election year | 1.2288 | 0.4656 | 0.4065 | 0.1065 | | -0.5862 | |
| | (1.7256) | (1.4148) | (1.2627) | (1.2683) | | (1.1511) | |
| Pre-election year | | | | | 1.4930 | | 4.9308 |
| | | | | | (1.3842) | | (4.3328) |
| M1 growth (t-1) | | -0.0527*** | -0.0720*** | -0.0857** | -0.0856** | -0.4738*** | -0.4722*** |
| | | (0.0202) | (0.0252) | (0.0334) | (0.0335) | (0.0614) | (0.0598) |
| M1 growth (t-2) | | | -0.0534** | -0.0749** | -0.0751** | -0.0743*** | -0.0749*** |
| | | | (0.0259) | (0.0312) | (0.0312) | (0.0216) | (0.0211) |
| M1 growth (t-3) | | | | -0.0589* | -0.0587* | -0.1160*** | -0.1095*** |
| | | | | (0.0306) | (0.0311) | (0.0087) | (0.0048) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Country, Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1,060 | 982 | 902 | 823 | 823 | 240 | 240 |
| R-squared | 0.03 | 0.05 | 0.06 | 0.07 | 0.07 | 0.27 | 0.27 |
| Countries | 83 | 82 | 81 | 80 | 80 | 13 | 13 |
| F | 105.4 | 225.9 | 958.3 | 23352 | 13697 | 2872 | 10.72 |

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

Notes: Standard errors are robust to heteroscedasticity and are clustered at the level of countries.