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Vote Buying or (Political) Business (Cycles) as Usual?

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Abstract

We provide new evidence on the short-run effect of elections on monetary aggregates. We study month-to-month fluctuations in the growth rate of M1 in a sample of 85 low and middle income democracies from 1975 to 2009. The evidence shows 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 the months leading up to the election. The effect is larger in democracies with many poor and uneducated voters, and in Sub-Saharan Africa and in East-Asia and the Pacific. We show that the election month monetary expansion is demand driven and can be best explained by systemic vote buying. Systemic vote buying requires significant amounts of cash to be disbursed right before elections. The finely timed increase in M1 that we observe in the data is consistent with this. The timing is inconsistent with a monetary cycle aimed at creating an election time boom and it cannot be, fully, accounted for by other possible explanations.

Keywords: Political business cycles, vote buying, monetary economics
JEL codes: D72, E51, O10

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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 and that the 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 month 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 month 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 month effect could be caused by central banks expanding liquidity just before elections. We are, however, unable to detect any election month 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 month 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. Section 7 studies the correlation between the election

month effect and various electoral outcomes. Section 8 presents some cross validation tests. Section 9 provides concluding remarks.

2 Background: Political cycles, money and vote buying

Political business cycle models have so far largely 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. While the original Nordhaus (1975) and MacRae (1977) models focused on a Phillips curve trade-off between inflation and unemployment, a fiscal-financing channel, where the central bank finances the government's pre-election benefits to important constituencies, can be effective as well (Alpanda and Honig 2009).[1] No matter which of these channels is more important, their relevance should be detectable from anomalies in the behavior of monetary aggregates or central bank rates prior to the election.

Both conceptually and empirically, the relevance of monetary policy cycles remains contested (Drazen 2001). Conceptually, the independence of central banks from elected governments makes this theory questionable in many countries. Furthermore, the idea of repeating monetary policy patterns over the election cycles requires the assumption that voters are unable to learn from expectation errors. 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 (see the summary in Drazen 2001; Alpanda and Honig 2009). Heckelman and Wood (2005) study an earlier US era (1879 until 1932) and do not find support that monetary policy had been closely tied to the election cycle in this period. The international evidence based on country

[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).

panel analysis is not less ambiguous: Paldam (1979) fails to find evidence of election year 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 OECD countries, and this finding is robust even when they take account of heterogeneity in central bank independence or exchange rate regimes. Recently, Klose (2012) studies interest rate setting in OECD countries and takes a Taylor rule as its point of reference. On that basis the paper finds some evidence for interest rate policy being less reactive to inflation and more reactive to output fluctuations prior to an election. Alpanda and Honig (2009, 2010) extend the predominant OECD empirics to developing countries and their results stress the role of institutional constraints: based on monetary aggregates, monetary policy cycles are detectable only in developing countries that lack central bank independence. According to their results, fiscal conditions hardly matter so that the fiscal-financing channel appears to be of less relevance compared to the traditional Phillips curve-channel. A number of other studies assume that monetary political business cycles exist and then study their implications for other policy areas. Dreher and Vaubel (2009), for example, argue that foreign exchange interventions are likely to occur with these cycles.

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 employment with considerable lags and monetary expansion prior to an election, therefore, must start well before an election date. Typical leads between a detectable monetary expansion and the election date identified empirically amount to 4 to 6 quarters (Alesina et al. 1997). Since monetary policy cycle considerations do not point to an immediate impact of an election, there is a consequential lack of interest in higher frequency (monthly) data which would allow for an identification of concurrent effects. In fact, most of the literature makes use of quarterly or annual data which preclude the identification of immediate election date effects. Klose (2012) is a recent exception employing monthly data.

In contrast to this existing literature, we highlight an alternative and a new reason why the election calendar might induce fluctuations in monetary aggregates. We explore how short-run monetary cycles occur as a by-product of electoral politics in many low and middle income countries. 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.[2] 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 evidence of systemic vote buying.[3] Historically, vote buying was facilitated by the absence of the secret ballot and by suffrage restrictions that created small electorates.[4] 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

[2]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.

[3]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 (2007, 2008).

[4]For example, in nineteenth century Britain and the USA, it was a common practice for politicians to distribute food, drinks and money in pursuit of electoral support (Hasen 2000; Gardner 1990). Or in England during parliamentary elections, it was not uncommon to post the price for a vote openly outside of the polling station and it was being updated several times during a day, just like a stock price (Seymour 1970).

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. Two important field experiments from Benin and Sao Tome and Principe in Africa 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).

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 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 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. Moreover, since vote buying transaction takes place close to an election, the positive effect on M1 is timed around the day of the election. To summarize, insofar as systemic vote buying takes place, it will be detectable as a finely timed spike in M1 just round the election and can, thus, account for possible election time irregularities in the supply of money.

3 Survey and case study evidence

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.[5] 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.[6] 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 votes.

[Figure 1 About Here]

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.

[Figure 2 About Here]

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

[6]Social desirability bias suggests that voters tend to under-report having received money or gifts in exchange for their vote (see, e.g., Corstange 2010). 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.

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,[7] 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.[9] 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 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

[7] http://www.state.gov/documents/organization/204468.pdf

[8] Aravot Daily 10.05.2012 http://www.aravot.am/2012/05/10/297037/ (several criminal cases opened by the Prosecutor General's office); News.am 06.05.2012 http://news.am/arm/news/104315.html (includes video footage of presumed vote buying); A1plus.am 06.05.2012 http://www.a1plus.am/59112.html; etc.

[9] Institute for War and Peace Reporting 17.04.2012 https://iwpr.net/global-voices/sweeteners-votes-armenia

naira or about 4 USD.[10] 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.[11]. 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 staples like rice or oil, as part of campaigning".[12] 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

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

[11] (http://in.reuters.com/article/2009/10/29/bolivia-election-idINN2931879820091029)

[12] The Jakarta Post, 18 March 2014: http://www.thejakartapost.com/news/2014/03/17/banned-vote-buying-still-plagues-indonesias-election-process.html

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".[13] 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.[14] 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[15], 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 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

[13]New York Times, 22 April, 2009:
http://www.nytimes.com/2009/04/23/world/middleeast/23lebanon.html?pagewanted=all

[14] http://www.globalpost.com/dispatch/lebanon/090602/going-rate-vote-lebanon-700

[15]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).

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.[16] We also collected data for the 13 "old" OECD countries.[17] For each country, we record the month and year of each general election held during this period. 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 A1 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.

[16] 2009 is the most recent data point reported in the source.

[17] 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 from the main sample (see Table A2).

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:

$$\widehat{M1}_{cym} = \alpha_0 + \beta_1 \cdot Election_{cym} + Control_{cy} \cdot \beta_{\mathbf{2}} + \sum_{i=1}^{k} \alpha_i \cdot \widehat{M1}_{cym-i} + \mu_c + \eta_y + \nu_m + \epsilon_{cym}, \quad (1)$$

where the dependent variable - $\widehat{M1}$ - is the growth rate of M1 in country c in year y and month m; *Election* is a dummy variable indicating whether an election took place in country c in year y and month m; and *Control* is a vector of control variables (GDP per capita, GDP per capita growth, the annual inflation rate, the exchange rate against the US dollar, and resource rents as a share of GDP) measured at the level of countries and years. In dynamic specifications, we add the lagged value of $\widehat{M1}$ with up to k lags. The model includes country (μ), year (η) and month (ν) fixed effects. ϵ is the error term. Table 1 reports summary statistics for all the variables and lists the data sources.

[Table 1 About Here]

The parameter of interest is β_1. It measures 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 ϵ. 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 month effect within that sample. Third, we offer our interpretation of the results.

5.1 The average election month effect

Table 2 reports the main estimates of the average election month effect. Column (1) shows a static specification of equation 1, while columns (2) and (3) add dynamics with up to six lags of the monthly growth rate of M1.[18] In all three specifications, we find a significant increase in the growth rate of M1 in election months. The average size is about 0.6 percentage points. This corresponds to one tenth of a standard deviation.[19]

[Table 2 About Here]

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 strong growth in M1, our baseline results could be driven by reverse causality. We include the month fixed effects to control for this possibility.[20] 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 (4) to (6) report estimates based on the seasonally adjusted data. We observe that the election month effect continues to be significant at the 5% level but that the point estimate is a little smaller than previously (around 0.5 percentage points). This is expected because the seasonal adjustment is likely to smooth the variation in M1 in election months in countries where elections always take place in

[18] Table A2 in the appendix reports more details on the dynamic specifications.

[19] Under the assumption that, on average, elections take place in the middle of a month and bearing in mind that the monetary effect of vote buying gradually decreases after the election date, the increase of 0.6 percentage points in only two weeks is substantial.

[20] Alternatively, we could include *month*x*country* fixed effects. However, this would make it impossible to estimate the effect of elections in countries where elections take place regularly in the same month.

the same month.[21] This does not suggest that the results are due to election date timing effects.

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 2, columns (7) to (9) report specifications of equation 1 in which the dependent variable is the change in the M1-to-M2 ratio from month t to $t-2$.[22] 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.

5.2 Heterogeneity in the election month effect

Exploring heterogeneity in the election month effect is important because it offers potential insights into the underlying mechanism behind the average results reported in Table 2.

In Table 3, we split the main sample by income (top panel) and by geographic region (bottom panel).[23] The top panel shows that the election month effect is concentrated in countries with below median income (column 3). We can neither detect the effect in the richer countries in the sample (columns 4 to 6) nor amongst the poorest countries in the bottom quartile (columns 1 to 2). The bottom panel shows the results by geographical region. We observe that the election month effect is statistically significant in East-Asia and the Pacific (column 1) and in Sub-Saharan Africa (column 6).

[Table 3 About Here]

[21] Table A3 in the appendix reports the full set of estimates based on the seasonally adjusted data.

[22] 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. We report the full results in Table A4 in the appendix.

[23] The income groups are defined by the position of GDP per capita for a given country-year pair in the percentile rank of GDP per capita in the overall sample.

We also engage with the possibility of heterogeneity in the election month effect by studying its interaction with economic characteristics of the countries.[24] We focus on two characteristics: education attainment (net enrollment in secondary and university education) and poverty (defined as the share of the population earning less that 1.25 USD per day).

Table 4, columns (1) to (2) show that the election month effect is less pronounced in countries with a larger share of the relevant age group enrolled in either secondary or university education. Figure 3(a) shows that the election month effect is statistically significant up to a net enrollment rate in university education of about 30%. For very high enrollment rates, the point estimate is negative but not statistically different from zero. Column (3) shows that the election month effect is larger in countries where a big fraction of the population is below the poverty line.[25] Figure 3(b) shows that the election month effect is positive everywhere but not significant in countries where less than 15% of the population lives below the poverty line.

[Table 4 About Here]

[Figure 3 About Here]

5.3 Interpretation

Our baseline result is a robust, statistically significant, and economically meaningful monthly electoral cycle in M1. We interpret this as evidence of vote buying. This interpretation is bolstered by the pattern that we observe when we interact the election month dummy with 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 is correlated with electoral corruption and self-reported vote buying (e.g., Jensen and Justesen 2014). The fact that we find evidence that

[24] We do not observe these characteristics by month. Accordingly, we interact the election month dummy variable with the year-country average of the relevant characteristics.

[25] Table A3 of the appendix shows that these results can be replicated with seasonally adjusted data.

the election month effect is larger in countries with low enrollment in secondary and university education or with a high fraction of the population living below the poverty line is consistent with the vote buying mechanism. The regional pattern of the largest effect being observed in Africa points to 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 month 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 monthly electoral 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 and 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 involves 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 5 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. 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.[26] 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 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.

[Table 5 About Here]

To further probe this issue, we aggregate the monthly M1 data to the quarterly or annual frequency, and we redefine the election dummy to be equal to one in election quarters or years, respectively, and zero otherwise. With this data, we can then estimate two-ways fixed effects panel models similar to those typically used in the empirical literature on monetary political business cycles. Table 6 shows the results for the yearly sample. We observe that we cannot find any evidence of a classical monetary political business cycle at that frequency. This is true both for the main sample of low and middle income countries (columns 1 to 5) and for the sample of the 13 "old" OECD countries (columns 6 and 7). Table 7 shows the results for the quarterly sample. Here, 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. There is no effect for the sample of the 13 "old" OECD countries (columns seven to twelve).

[26] This can also be demonstrated with seasonally adjusted data, see Table A3.

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, 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 month effect.

[Table 6 About Here]

[Table 7 About Here]

6.2 Legal campaign spending and the quality of institutions

The election month effect that we have identified could 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 cycle that we have identified is concentrated precisely in the election month and not in the months leading up to the election, therefore, contradicts the story of legal campaign spending being the main cause of the election month effect.

Elections and election campaign spending go hand in hand everywhere. Accordingly, if the election month 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 cycle in these countries which would then have to be attributed to election campaigning (or some other cause).

Table 8 reports estimates of equation 1 with data from the 13 OECD countries only. We find no evidence of any monetary election cycle. This suggests that the monetary election 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 this, but 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 month effect observed in our main sample is associated with vote buying and that relatively weak electoral institutions play an important intervening role.

[Table 8 About Here]

6.3 National events

Elections are large national events that generate increased economic activity. This is partly due to the cost of organizing elections and partly due to private sector spending on election celebrations. The consequence of this could be extra demand for liquidity which would 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).[27]

[27]We require that the event is national in scope, and we focus on "independence days" to insure comparability across countries. The notes to Table 9 list for each country the month in which the "independence day" is celebrated.

Table 9 reports estimates of equation 1 with the election month dummy replaced by the dummy variable *national independence day* which is equal to one in the month in which the "independence day" of a country is celebrated and zero otherwise. 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 10% 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 month 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 month effect can attributed to the fact that elections are big national events associated with unusual economic activity.

[Table 9 About Here]

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. Akhmedov and Zhuravskaya (2004) document one important example of this from Russia. They find a sizable increase in direct monetary transfers to voters from the regional governments in the election month. 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 month effect.[28] 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

[28]If the funds come from the central bank, they would 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 July 7, 2012).

would, therefore, not involve a direct substitution of illiquid for liquid assets.[29] 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 month 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.

7 The election month effect and electoral outcomes

This section investigates if the election month increase in the growth rate of M1, that we documented above, is correlated with electoral outcomes and if so, whether the nature of the correlation is consistent with the cycle being caused by vote buying. To this end, we create a new data set where the unit of analysis is an election month in a country in a given year. The outcome variable is the growth rate of M1 in election months. We correlate this with three indicators of electoral outcomes: turnout, the share of votes won by the incumbent, and the share of invalid votes. Table 10 reports the results.

In the political science literature on vote buying, there is a long-standing debate about the feasibility of vote buying under the secret ballot. Since it is easier to observe whether voters show up to vote than to verify which candidate they vote for, it has been suggested that vote buying is, in fact, turnout buying (e.g., Nichter 2008). That is, politicians may pay voters whom they expect will support them to turn out to vote (positive turnout buying) or they may, as documented in the classical study by Cox and Kousser (1981), pay

[29]For example, the EU manual 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."

voters whom they expect to vote against them to stay home (negative turnout buying). Table 10, columns (1) and (2) show that the correlation between the turnout rate in parliamentary and presidential elections and the growth rate of M1 in the election month is negative and significantly so for turnout in parliamentary elections. This is consistent with the vote buying explanation of the election month monetary expansion and suggests that at least part of the cycle is associated with negative turnout buying.

The fundamental assumption behind any theory of opportunistic political business cycles is that opportunism, in its many different forms, pays off and helps politicians to win elections.[30] Table 10, column (3) reports a strong positive correlation between the growth rate of M1 in election months and the share of votes obtained by the the winning government party (or parties if there is a coalition government) in parliamentary elections.[31] This pattern is consistent with vote buying helping parties to win elections.

The share of invalid ballots is a rough indicator of electoral fraud, in particular in situations where the share is abnormally high. Table 10, columns (4) and (5) report a positive correlation between the growth rate of M1 in the election month and the share of invalid ballots. The correlation is significant for presidential elections. This, again, is consistent with our interpretation that it is vote buying that generates the monetary expansion in the election month that we observe.

[Table 10 About Here]

8 Vote buying and the election cycle: cross validation

The evidence presented in the previous sections bolsters the vote buying explanation for the concurrent election month expansion of M1 and contradicts a number of other

[30] For evidence that it does, see, for example, Aidt et al. (2011) and for evidence pointing in the other direction, see Brender and Drazen (2008).

[31] A similar analysis was not possible for presidential elections because of the small number of observations.

potential explanations. This section investigates whether the size of the expansion correlates with measures of perceived electoral clientelism[32] and with survey-based data on the extend of vote buying.[33] If the monetary expansion is caused, at least in part, by vote buying, then these correlations should be positive.

For the purpose of this investigation, we create a cross country data set and two measures of the size of the election month effect: i) a simple country average of the growth rate of M1 in election months; and ii) the country-specific beta-coefficients obtained by estimating equation 1 country-by-country.

[Figure 4 About Here]

Figure 4 shows correlation plots between these measures of the size of the election month effect and the data on perceived clientelism (sub-figures a, b) and on self-reported vote buying from the Latinobarometer (sub-figures c, d). The correlations are positive in all cases and statistically significant in sub-figures 4(a) and 4(d). These correlations strengthen our interpretation of the monetary election cycle as a manifestation of vote buying and suggest that the size of the election month increase in the growth rate of M1 can provide useful information about the extent of election corruption.

9 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

[32]The source of our measure of perceived electoral clientelism is the Democratic Accountability and Linkages Project (DALP). The measure 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?"

[33]We use information from the 2002 wave of the Latinobarometer to measure the extent of vote buying (see Figure 1).

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. This finding complies with the timely pattern to be expected since systemic vote buying requires significant amounts of money to be disbursed right before elections. 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 the sole explanation. Third, we systematically evaluate and rule out other alternative explanations, such as particular private consumption hikes around important national holidays which might also characterize election days.

Our findings help to paint a more comprehensive picture of the potential link between elections and monetary aggregates than the picture painted by the literature on monetary political business cycles. In particular, they point 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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Tables and Figures

Table 1: Summary statistics

Variable	Obs	Frequency	Mean	Std. Dev.	Min	Max	Source
M1 growth	17,355	Monthly	0.016	0.065	-0.876	2.204	EconStats
M1 seasonally adjusted growth	17,069	Monthly	0.015	0.054	-0.874	2.327	EconStats
Election dummy	41,062	Monthly	0.019	0.138	0	1	DPI
Parliamentary election dummy	41,062	Monthly	0.016	0.125	0	1	DPI
Presidential election dummy	41,062	Monthly	0.007	0.081	0	1	DPI
Turnout parliamentary election	699	Monthly	0.696	0.178	0	0.989	DPI, IDEA
Turnout presidential election	355	Monthly	0.688	0.167	0	0.947	DPI, IDEA
Vote share government party(s)	632	Monthly	0.512	0.239	0	1	DPI, IDEA
Vote share president	96	Monthly	0.572	0.076	0.500	0.822	DPI, IDEA
Invalid ballots parliamentary election	437	Monthly	0.032	0.036	0.001	0.401	DPI, IDEA
Invalid ballots presidential election	159	Monthly	0.034	0.032	0.001	0.188	DPI, IDEA
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)	36,642	Annual	197	789	0	10390	WDI
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
University education (net enrollment rate)	27,795	Annual	0.228	0.205	0	1.018	WDI
Poverty, share of population earning less than 1.25$/day	6,328	Annual	0.158	0.209	0	0.846	WDI
Polity IV score of democracy	33,546	Annual	1.754	7.311	-10	10	Polity IV
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	DeltaStock, Gecodia
							National Central Banks

Table 2: Main results: Growth of M1 and M1/M2 during elections

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Monthly growth of M1			*Monthly growth of seasonally adjusted M1*			$\Delta_{(t-2)}M1/M2$		
Election dummy	0.0058**	0.0067**	0.0060**	0.0047**	0.0049**	0.0050**	0.0022*	0.0015*	0.0007
	(0.0027)	(0.0028)	(0.0027)	(0.0023)	(0.0021)	(0.0022)	(0.0012)	(0.0009)	(0.0007)
Dependent variable (t-1)		-0.1055***	-0.1130***		-0.0914***	-0.0971***		0.6316***	0.7379***
		(0.0398)	(0.0383)		(0.0339)	(0.0316)		(0.0368)	(0.0416)
Dependent variable (t-2)		-0.0332**	-0.0359***		-0.0145	-0.0033		-0.4444***	-0.6435***
		(0.0130)	(0.0133)		(0.0125)	(0.0111)		(0.0256)	(0.0341)
Dependent variable (t-3)		0.0125	0.0093		0.0670**	0.0879***		0.2214***	0.5048***
		(0.0284)	(0.0280)		(0.0298)	(0.0292)		(0.0180)	(0.0349)
Dependent variable (t-4)			-0.0199			0.0768***			-0.4093***
			(0.0153)			(0.0167)			(0.0342)
Dependent variable (t-5)			-0.0864***			-0.0585			0.2482***
			(0.0327)			(0.0620)			(0.0340)
Dependent variable (t-6)			-0.0476			-0.0849			-0.1376***
			(0.0290)			(0.0597)			(0.0145)
GDP pc (log)	-0.0172***	-0.0206***	-0.0231***	-0.0182**	-0.0207**	-0.0210**	-0.0028	-0.0023	-0.0023
	(0.0048)	(0.0059)	(0.0066)	(0.0091)	(0.0101)	(0.0099)	(0.0027)	(0.0020)	(0.0022)
GDP pc growth	0.0453***	0.0432***	0.0561***	0.0675***	0.0541***	0.0649***	0.0025	-0.0002	0.0007
	(0.0125)	(0.0118)	(0.0147)	(0.0204)	(0.0189)	(0.0209)	(0.0092)	(0.0053)	(0.0057)
Inflation	0.0136***	0.0152***	0.0176***	0.0132***	0.0134***	0.0140***	0.0004**	0.0002***	0.0004**
	(0.0006)	(0.0014)	(0.0010)	(0.0009)	(0.0014)	(0.0010)	(0.0002)	(0.0001)	(0.0002)
Exchange rate (log)	-0.0049***	-0.0062***	-0.0073***	-0.0041***	-0.0051***	-0.0055***	-0.0000	-0.0001	-0.0000
	(0.0006)	(0.0007)	(0.0010)	(0.0007)	(0.0008)	(0.0008)	(0.0002)	(0.0002)	(0.0002)
Resource rents / GDP	0.0542***	0.0657***	0.0621***	0.0384***	0.0450***	0.0418***	0.0281*	0.0209	0.0235
	(0.0169)	(0.0177)	(0.0157)	(0.0137)	(0.0147)	(0.0150)	(0.0155)	(0.0134)	(0.0149)
Observations	13,134	12,891	12,647	9,402	9,241	9,077	12,646	12,396	12,154
R-squared	0.1631	0.1774	0.1860	0.0725	0.0845	0.0976	0.0307	0.3047	0.3616
Countries	84	84	84	79	79	79	83	83	83
F	1484	5344	3233	5640	1765	8361	3835	566.6	1190

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

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

Table 3: Growth of M1 by income group and geographic region

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Monthly growth of M1					
INCOME	Percentile of per capita Gross Domestic Product (in constant 2005 USD)					
	0% - 10%	10% - 25%	10% - 50%	50% - 90%	75% - 90%	90% -100%
Election dummy	0.0033	0.0107	0.0078*	0.0024	0.0021	0.0056
	(0.0092)	(0.0076)	(0.0043)	(0.0032)	(0.0021)	(0.0037)
Observations	1,215	1,254	4,714	7,842	2,846	2,327
R-squared	0.1017	0.0674	0.1152	0.2744	0.2063	0.2140
Countries	12	15	45	50	24	13
F	387.2	190.0	12184	288.5	83.14	11.18

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Monthly growth of M1					
REGION	East-Asia & Pacific	East-Europe & Central-Asia	Latin-America & Caribbean	Middle-East & North-Africa	South-Asia	Sub-Saharan Africa
Election dummy	0.0087*	0.0017	0.0046	0.0040	-0.0027	0.0177**
	(0.0051)	(0.0030)	(0.0037)	(0.0029)	(0.0090)	(0.0074)
Observations	1,813	2,070	3,374	1,400	443	3,287
R-squared	0.1480	0.3532	0.3871	0.1095	0.1126	0.1151
Countries	9	18	19	10	6	20
F	3.702	98.10	242.5	3.234	30.80	35.78

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

Notes: All regressions include full set of controls, 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.

Table 4: Interaction effects: education and poverty

VARIABLES	(1)	(2)	(3)
	Monthly growth of M1		
Election dummy	0.0233*	0.0186**	0.0046
	(0.0119)	(0.0081)	(0.0086)
Secondary education	0.0403*		
	(0.0219)		
x Election dummy	-0.0270*		
	(0.0151)		
University education		-0.0150	
		(0.0127)	
x Election dummy		-0.0346**	
		(0.0153)	
Poverty (1.25$/day)			0.0678
			(0.0506)
x Election dummy			0.0434*
			(0.0261)
Controls			
M1 growth lags (1-6)	Yes	Yes	Yes
Country, Year, Month FE	Yes	Yes	Yes
Observations	5,612	9,632	3,851
R-squared	0.0974	0.1225	0.2468
Countries	67	80	63
F	15.89	277.2	690.8

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

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

Table 5: Growth of M1 and central bank interest rates during and before election months

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Monthly growth of M1				*Central Bank interest rate*			
Election dummy	0.0060**				0.0004			
	(0.0027)				(0.0006)			
Election dummy (t+1)		0.0039				0.0003		
		(0.0044)				(0.0004)		
Election dummy (t+2)			-0.0031				-0.0006	
			(0.0025)				(0.0006)	
Election dummy (t+3)				0.0035				0.0000
				(0.0038)				(0.0005)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent variable lags (1-6)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country, Year, Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,647	12,645	12,597	12,540	2,434	2,418	2,402	2,386
R-squared	0.1860	0.1858	0.1859	0.1864	0.9885	0.9885	0.9884	0.9883
Countries	84	84	84	84	16	16	16	16
F	3233	2687	3784	11242	52102	89929	267644	35938

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

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

Table 6: Political monetary cycles 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 dummy	1.2288	0.4656	0.4065	0.1065		-0.5862	
	(1.7256)	(1.4148)	(1.2627)	(1.2683)		(1.1511)	
Election dummy (t+1)					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	0.0303	0.0452	0.0583	0.0724	0.0726	0.2701	0.2737
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.

Table 7: Political monetary cycles 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 dummy	0.0024						-0.0030					
	(0.0063)						(0.0073)					
Election dummy (t+1)		0.0024						0.0003				
		(0.0046)						(0.0035)				
Election dummy (t+2)			0.0055						0.0015			
			(0.0058)						(0.0050)			
Election dummy (t+3)				0.0033						-0.0022		
				(0.0085)						(0.0044)		
Election dummy (t+4)					0.0261**						-0.0027	
					(0.0129)						(0.0046)	
Election dummy (t+5)						-0.0057						0.0031
						(0.0064)						(0.0024)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent variable lags (1-6)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country, Year, Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
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	0.4526	0.4526	0.4651	0.4662	0.4671	0.4716	0.3570	0.3569	0.3571	0.3587	0.3613	0.3653
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: Standard errors are robust to heteroscedasticity and are clustered at the level of countries.

Table 8: Main results for the sample of old OECD countries

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Monthly growth of M1					*Monthly growth of M1 (seasonally adjusted)*	$\Delta_{(t-2)}$*M1/M2*			
Election dummy	0.0011	0.0011				0.0001	0.0006			
	(0.0036)	(0.0037)				(0.0031)	(0.0011)			
Election dummy (t+1)			0.0045					0.0001		
			(0.0034)					(0.0007)		
Election dummy (t+2)				-0.0008					-0.0001	
				(0.0028)					(0.0008)	
Election dummy (t+3)					0.0015					-0.0012
					(0.0035)					(0.0015)
Controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent variable lags (1-6)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country, Year, Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,451	3,451	3,450	3,444	3,437	3,305	2,756	2,755	2,750	2,744
R-squared	0.2860	0.2945	0.2948	0.2946	0.2943	0.0747	0.4081	0.4081	0.4083	0.4085
Countries	13	13	13	13	13	13	11	11	11	11
F	5.746	13.07	6.473	13.11	15.67	55.50	246.4	264.8	227.6	226.5

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

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

Table 9: 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	0.1632	0.1743	0.1762	0.1775	0.1862	0.2945	0.0724	0.0811	0.0843	0.0974	0.0747
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 national independence day dummy, which is coded as follows:

January: Australia , CzechRep, Haiti, Samoa, SlovakRepublic; February: Chile, Kuwait, NewZealand, SriLanka, StLucia; March: Bangladesh, BosniaHerzogovina, Ghana, Lithuania, Mauritius, Namibia; April: Japan, SierraLeone, SouthAfrica; May: Eritrea, Ethiopia, Guyana, Jordan, Nepal, Paraguay, Romania; June: Croatia, Denmark, Mozambique, Slovenia, Sweden; July: Algeria, Argentina, Bahamas, Belarus, CapeVerde, Colombia, Comoros, France, Liberia, Malawi, USA, Vanuatu; August: Afghanistan, Bahrain, DominicanRepublic, Estonia, Indonesia, Jamaica, Korea, KyrgyzRepublic, Moldova, Pakistan, Singapore, Switzerland, Ukraine, Uruguay; September: Armenia, Belize, Brazil, Bulgaria, ElSalvador, 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.

Table 10: Growth of M1 and electoral outcomes

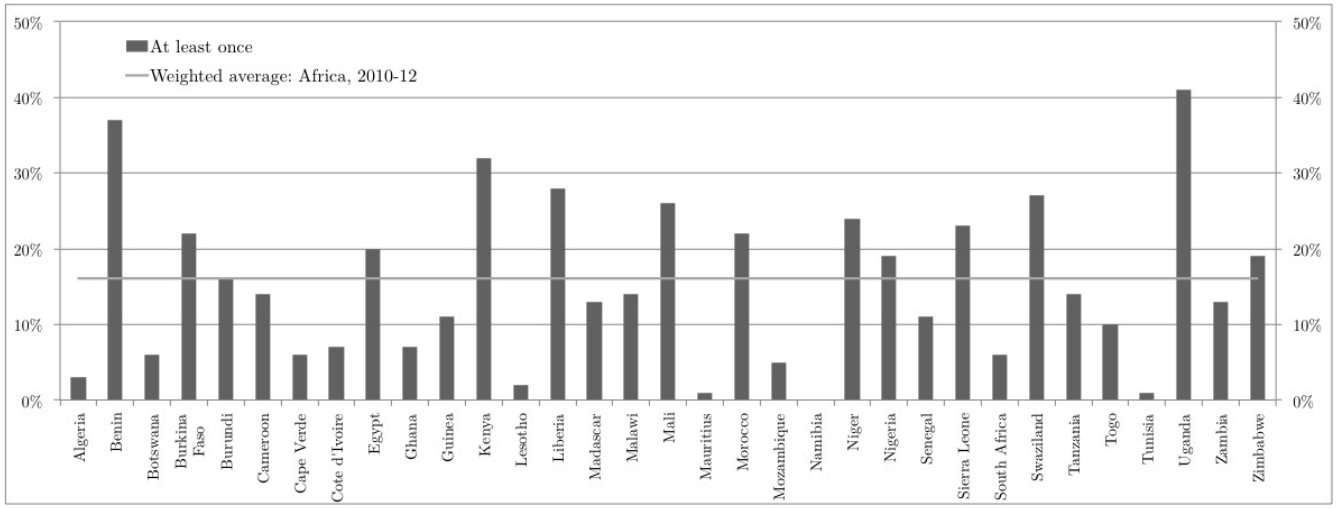
VARIABLES	(1)	(2)	(3)	(4)	(5)
	Monthly growth of M1				
Turnout parliamentary election	-0.1492***				
	(0.0356)				
Turnout presidential election		-0.0940			
		(0.0864)			
Vote share government party(s)			0.0772**		
			(0.0383)		
Invalid ballots parliamentary election				0.2168	
				(0.1462)	
Invalid ballots presidential election					0.2895*
					(0.1699)
Controls	Yes	Yes	Yes	Yes	Yes
M1 growth lags (1-6)	Yes	Yes	Yes	Yes	Yes
Country, Year FE	Yes	Yes	Yes	Yes	Yes
Observations	164	91	116	126	68
R-squared	0.8970	0.8651	0.9352	0.5342	0.9012
Countries	48	34	35	42	27
F	12018	484805	404593	137.0	5888

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

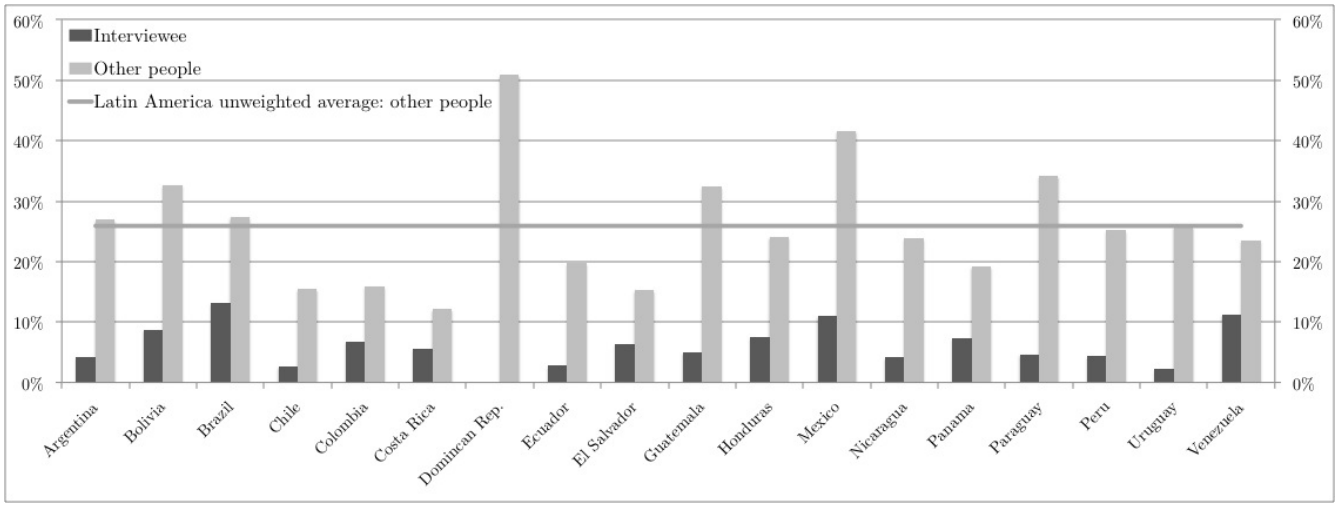
Notes: Standard errors are robust to heteroscedasticity and are clustered at the level of countries.

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

(a) *Afrobarometer:* 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?



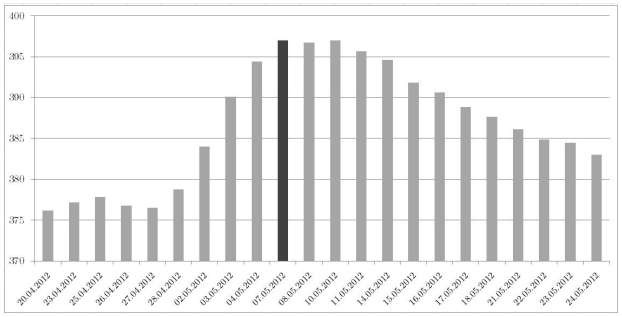
(b) *Latinobarometer:* Have you known of someone in the last elections who was pressured or received something to change his vote in a certain way? Has this happened to you?



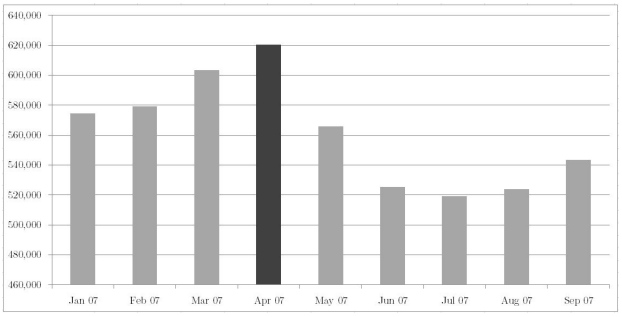
Notes: Data for Dominican Republic is from the survey wave of 2005.

Figure 2: Case-study evidence on the daily, weekly, or monthly stock of money supply during elections in selected countries

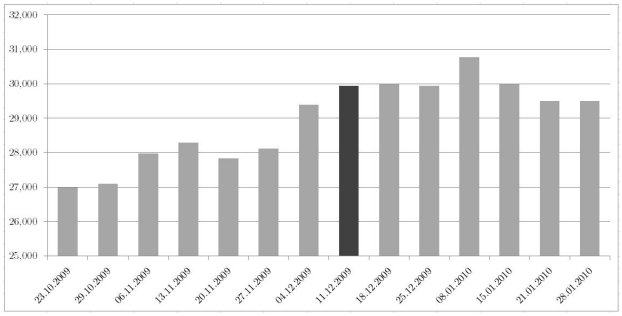
(a) Armenia 2012 (billion AMD)



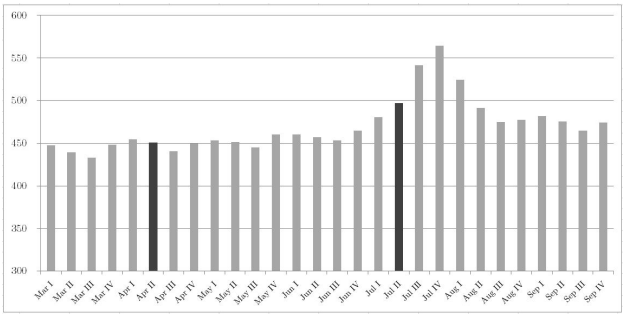
(b) Nigeria 2007 (million NGN)



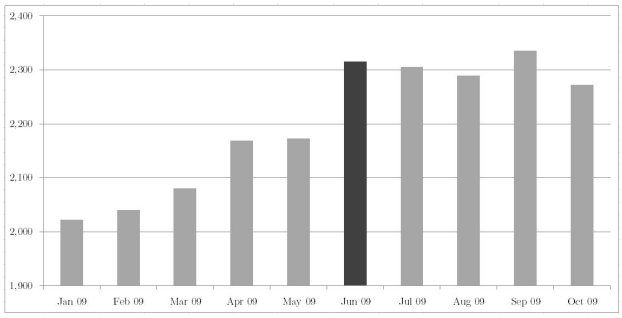
(c) Bolivia 2009 (million BOB)



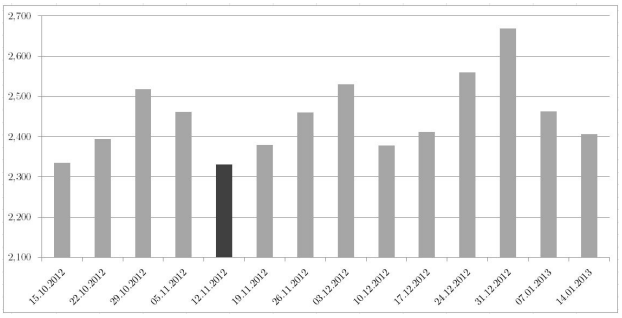
(d) Indonesia 2014 (billion IDR)



(e) Lebanon 2009 (billion LBP)



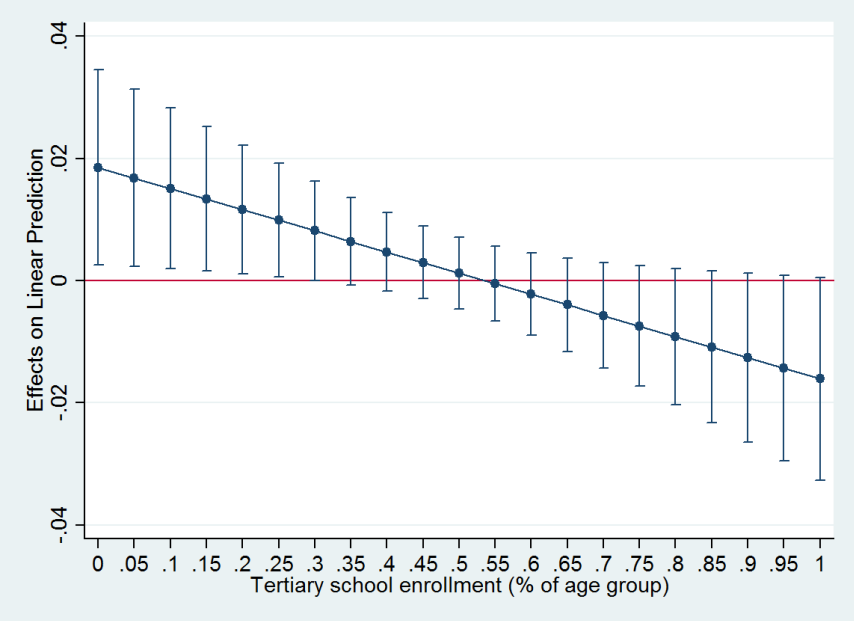
(f) USA 2012 (billion USD)



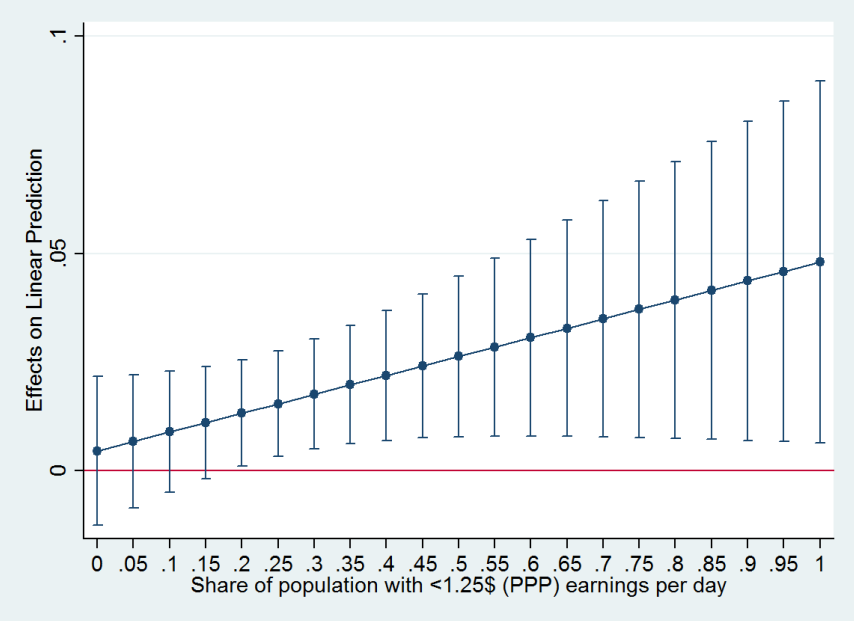
Notes: Sources of the monetary data are the respective national monetary authorities. In national sources the exact definitions of monetary data vary, and are defined as follows: currency in circulation outside of the Central Bank in sub-figure (a), currency in circulation outside of banks in sub-figure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

Figure 3: Marginal effects of interaction terms

(a) Education: table 4, column 2



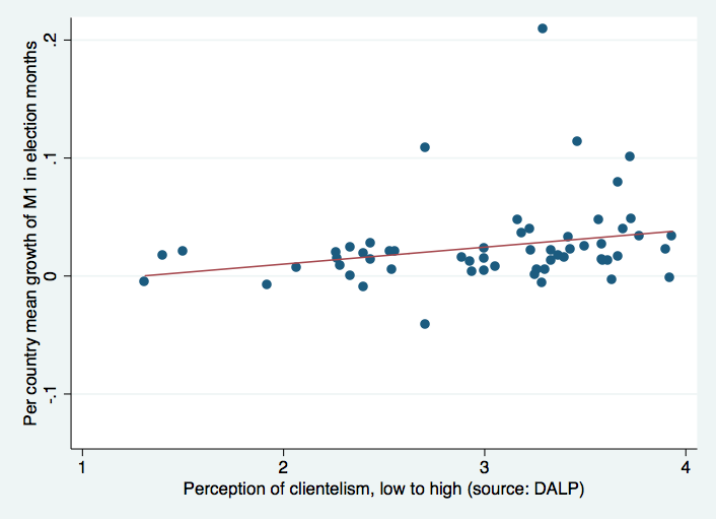
(b) Poverty: table 4, column 3



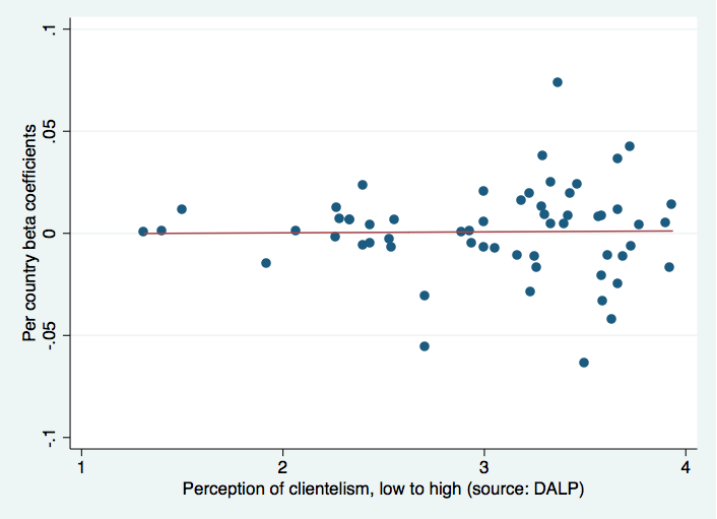
Notes:

Figure 4: Correlations of M1-induced measures of vote buying with perception of clientelism (a, b) and survey-based measures of vote buying (c, d)

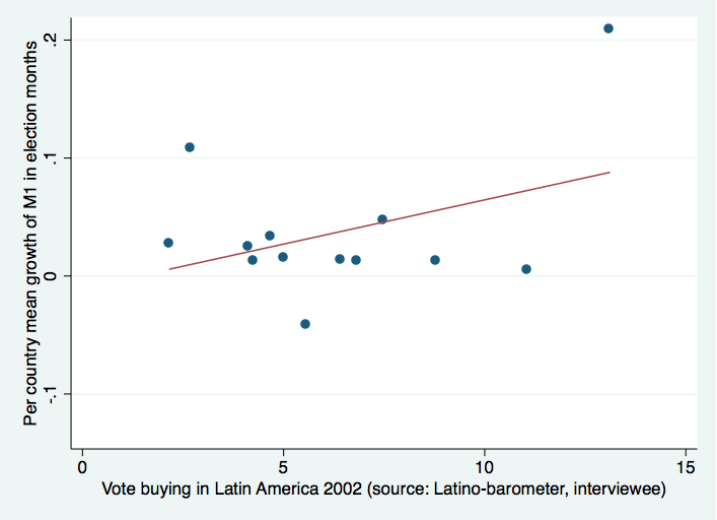
(a) Election-M1 - Clientelism (*corr.=.26***)



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



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



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

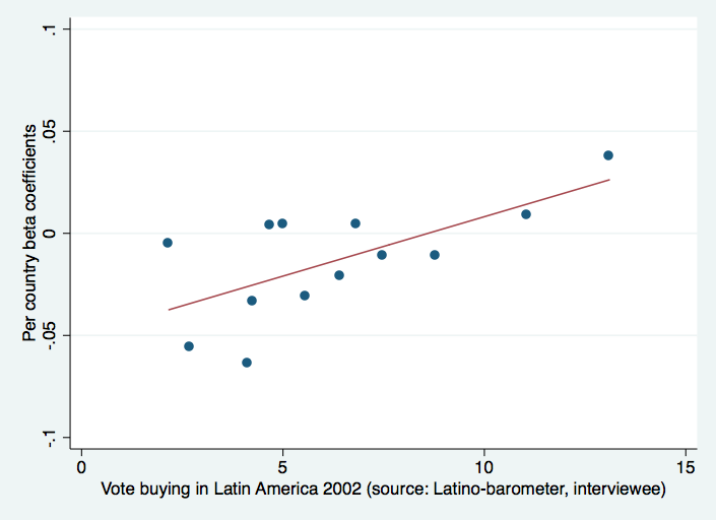


Table A1: Sample of countries

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 A2: Baseline estimations: Growth of M1 during elections in the sample excluding OECD countries

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Monthly growth of M1										
Sample	Non-OECD 1975							Non-OECD 2009		Non-OECD 2014	
Election dummy	0.0074**	0.0077**	0.0078**	0.0073**	0.0075**	0.0067**	0.0068**	0.0100**	0.0092**	0.0090**	0.0077**
	(0.0037)	(0.0035)	(0.0035)	(0.0034)	(0.0035)	(0.0034)	(0.0034)	(0.0039)	(0.0036)	(0.0041)	(0.0036)
M1 growth (t-1)		-0.0148	-0.0150	-0.0207	-0.0292	-0.0312	-0.0327		-0.0339		-0.0315
		(0.0752)	(0.0733)	(0.0665)	(0.0582)	(0.0577)	(0.0576)		(0.0585)		(0.0604)
M1 growth (t-2)			0.0572	0.0596*	0.0556**	0.0550**	0.0502**		0.0502**		0.0511**
			(0.0371)	(0.0320)	(0.0266)	(0.0256)	(0.0235)		(0.0236)		(0.0231)
M1 growth (t-3)				0.1058	0.1129*	0.1118*	0.1080*		0.1098*		0.1211**
				(0.0644)	(0.0612)	(0.0609)	(0.0572)		(0.0580)		(0.0566)
M1 growth (t-4)					0.0772	0.0795*	0.0782*		0.0818*		0.0864*
					(0.0470)	(0.0473)	(0.0455)		(0.0452)		(0.0445)
M1 growth (t-5)						0.0096	0.0144		0.0147		0.0137
						(0.0103)	(0.0099)		(0.0102)		(0.0113)
M1 growth (t-6)							0.0412*		0.0392*		0.0474**
							(0.0212)		(0.0215)		(0.0202)
Country, Year, Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,826	13,734	13,642	13,550	13,459	13,369	13,279	12,669	12,158	12,016	11,523
R-squared	0.0938	0.0960	0.0993	0.1092	0.1155	0.1160	0.1179	0.0877	0.1125	0.0857	0.1148
Countries	85	85	85	85	85	85	85	79	79	76	76
F	71049	8509	42661	1046	1077	165146	12033	393.3	566.6	422.1	232.1

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

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

Table A3: Robustness test: Seasonally adjusted M1

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Monthly growth of seasonally adjusted M1								
Election dummy	0.0047**	0.0050**	0.0049**	0.0050**				0.0119**	0.0005
	(0.0023)	(0.0022)	(0.0021)	(0.0022)				(0.0059)	(0.0070)
Election dummy (t+1)					0.0030				
					(0.0026)				
Election dummy (t+2)						-0.0010			
						(0.0018)			
Election dummy (t+3)							0.0001		
							(0.0025)		
University education								-0.0107	
								(0.0098)	
x University education								-0.0235*	
								(0.0120)	
Poverty (2,00$/day)									0.0522
									(0.0431)
x Poverty (2,00$/day)									0.0194*
									(0.0112)
M1 (SA) growth lagged at:		1	1-3	1-6	1-6	1-6	1-6	1-6	1-6
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Country, Year, Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,402	9,348	9,241	9,077	9,075	9,037	8,993	9,450	3,851
R-squared	0.0725	0.0813	0.0845	0.0976	0.0976	0.0979	0.0981	0.0570	0.1676
Countries	79	79	79	79	79	79	79	80	63
F	5640	3778	1765	8361	6563	5869	7470	2051	2735

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

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

Table A4: Change of the M1-to-M2 ratio around elections

VARIABLES	(1) $\Delta_{(t-1)}M1/M2$	(2) $\Delta_{(t-2)}M1/M2$	(3) $\Delta_{(t-3)}M1/M2$	(4)	(5) $\Delta_{(t-2)}M1/M2$	(6)
Election dummy	0.0001	0.0021*	0.0023	0.0015*		
	(0.0007)	(0.0012)	(0.0015)	(0.0009)		
Election dummy (t+1)					0.0003	
					(0.0026)	
Election dummy (t+2)						-0.0000
						(0.0013)
Dependent variable (t-1)				0.6306***	0.6309***	0.6358***
				(0.0361)	(0.0368)	(0.0396)
Dependent variable (t-2)				-0.4434***	-0.4434***	-0.4370***
				(0.0250)	(0.0255)	(0.0208)
Dependent variable (t-3)				0.2229***	0.2216***	0.2200***
				(0.0177)	(0.0178)	(0.0167)
Controls				Yes	Yes	Yes
Country, Year, Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,414	13,327	13,341	13,044	12,394	12,347
R-squared	0.0352	0.0293	0.0256	0.3037	0.3039	0.3056
Countries	84	84	84	84	83	83
F	6981	35702	893463	103858	2078	2370

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

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