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

Toke Aidt

University of Cambridge *and* CESifo, Munich

Zareh Asatryan*

ZEW Mannheim

Lusine Badalyan

University of Giessen

Friedrich Heinemann

ZEW Mannheim *and* University of Heidelberg

December 18, 2018

**Abstract**

We report robust evidence of a new short-run monetary election cycle: the monthly growth rate of the money supply (M1) around elections is higher than in other months in a sample of low and middle income countries. We hypothesize this is related to systemic vote buying. Consistent with this, we find no cycle in authoritarian countries and countries with strong political institutions and a pronounced cycle in elections where international election monitors reported vote buying or in close elections. Using survey data on daily consumer expenditure, we show that within household consumption of food increases in the days before elections.

**Keywords**: Political business cycles, vote buying, monetary economics

**JEL codes**: D72, E51, O10

---

*Correspondence: Zareh Asatryan, Centre for European Economic Research (ZEW), L7 1, 68161 Mannheim, Germany; email: asatryan@zew.de; phone: +49 6241235392.

We would like to thank Bagrat Asatryan, Vardan Baghdasaryan, Niclas Berggren, Frank Bohn, Oana Borcan, Adi Brender, Vasco Calvalho, Axel Dreher, Jac Heckelman, Philip Keefer, Ruben Ruiz Rufino, Arne Steinkraus, Thomas Stratmann, Kaj Thomsson, Francisco Veiga, Ekaterina Zhuravskaya, various seminar participants, as well as Rohini Pande (the editor) and two anonymous referees for valuable comments. We thank Annika Havlik and Colja Maser for excellent research assistance.

# 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 incumbent 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 weak, as pointed out in the surveys by Paldam (1997) and Drazen (2001). 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 by the central bank in the quarters prior to elections, we argue that the effect is concurrent with elections and works through money demand rather than through supply side interventions by the central bank.

We investigate if the growth rate of the monetary aggregate M1 – defined as the total amount of cash in circulation plus transferable deposits held by all money holding sectors – increases in election months in a panel of up to 104 low and middle income countries for the years 1975-2015. We estimate a dynamic short-run money demand function. Our baseline specification is a dynamic panel model with year, month and country fixed effects. In a more demanding specification, we include interactions between these fixed effects and thus identify the effect of elections on the growth rate of M1 by testing if, within an election year, the growth rate of M1 is higher during the month of an election, after removing common shocks that happen within a month in a given year and country-specific seasonality. We find evidence of an increase in the growth rate of M1 in election and post-election months in these countries. The effect is sizable: the growth rate of M1, on average, increases by between 0.41 and 0.61 percentage points or by about 1/13th of a standard deviation in election months. We are unable to find similar effects among

established OECD democracies. These results are remarkably robust and suggest that the election calendar induces concurrent fluctuations in M1 that can only be detected by studying high-frequency (monthly) data. The evidence on this monetary expansion in the election month in non-OECD countries with elections – which we shall refer to as the *election date effect* – is new to the literature and robustly establishing this new stylized fact is a main contribution of the paper.

To explain the election date effect, we propose the *vote buying hypothesis* according to which the effect is a manifestation of systemic vote buying. Vote buying – understood as payments 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 money and affects (recorded) M1 around elections. The resources needed to buy votes may be obtained by converting illiquid into liquid assets. This substitution from broad money into cash or deposits directly increases M1. On the other hand, vote buying is an illegal activity and the 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 banks' ability to lend and offer leeway for an increase in M1. Finally, if overseas funds are converted to local currency to fund vote buying, then this will, in fixed or managed exchange rate systems, increase M1. In all cases, the result is an increase in M1 centered around elections.

We offer four pieces of evidence that vote buying is a possible explanation for the election date effect but a conclusive case cannot be made. First, vote buying, as a viable electoral strategy, requires weak democratic institutions, poorly conducted elections, and an electorate willing to "sell" their votes. By drawing on data from hundreds of reports from international election monitors (Kelley 2012), we test this implication. Consistent with the vote buying hypothesis, we find that the increase in the growth rate of M1 is systematically larger in elections which according to international election monitors were far from "free and fair" or were riddled with "electoral fraud" and "vote buying". In particular, we cannot find evidence of an election date effect in elections that were

reported by the monitors to be free and fair. Second, large-scale vote buying does not usually occur in countries with consolidated "authoritarian" political institutions where the elections are heavily controlled by the incumbent government eliminating the need to buy votes or conversely in countries with strong democratic institutions where checks and balances make large-scale vote buying impossible. Consistent with this line of reasoning, we find that the election date effect exhibits an inverse U-shaped relation with indicators on the quality of a country's political institutions: the election date effect is statistically significant only for countries in between the two extremes. Third, we also find evidence that the election date effect is largest in close elections during which competition among candidates is intense and vote buying of greatest value. Fourth, vote buying can affect M1 by funding extra consumption. This happens, for example, if the cash used to buy votes was hoarded in the black economy and returns to the banking system when voters spend it. To provide evidence on this mechanism, we undertake a micro-econometric study of anomalies in household consumption around elections in Armenia. Armenia exhibits a marked increase of currency in circulation in the days around the elections it has held since 2003 and reports from international election monitors and in the local press are full of anecdotal evidence of vote buying on a massive scale. Using daily household level consumption diaries from a large consumption survey, we adopt the approach first developed by Mitra et al. (2017) in a study of vote buying in India. We find that consumption expenditures on many food items spike in the days around elections. A plausible funding source for this extra consumption is income earned by selling votes.

Our paper contributes directly to two strands of literature. First, we contribute to the research on monetary political business cycles with a new stylized fact: the growth rate of M1 is systematically higher around elections and this cannot be explained by country-specific macro-economic shocks in election years, by common shocks that affect all countries in a given month, or by country-specific seasonality. In doing so, we shift the attention away from central bank engineered cycles of the type proposed by Nordhaus (1975) and MacRae (1977) to cycles created at the demand side of the money market

around elections. Second, we contribute to the literature on vote buying by suggesting that systemic, large-scale vote buying has aggregate monetary effects. Specifically, we add to an emerging literature that relates illegal but unobserved electoral activities to observable anomalies timed around elections. Kapur and Vaishnav (2013) shows that Indian construction firms divert short-term funds to political campaigns (in anticipation of post-electoral preferential treatment) and that this induces a short-term election cycle in cement consumption in the election and post-election month. The macroeconomic effect on M1 that we find is also concentrated in the election and post-election month. Sukhtankar (2012) finds evidence of an election cycle in the prices paid by sugar mills in India. This is consistent with illegal campaign funding activities and such activities could, in principle, fund vote buying (as well as other election expenses). Finally, Mitra et al. (2017) study consumption data from across Indian states and identify anomalies in consumption patterns which are consistent with vote buying. Our work relates directly to this by showing similar evidence from Armenia.[1]

The rest of the paper is organized as follows. Section 2 introduces our data, identification strategy, and the main results. Section 3 discuses evidence on the monetary mechanisms behind the election date effect. Section 4 introduces the vote buying hypothesis and presents evidence consistent with it. Section 5 concludes. The supplementary material contains extra estimation results, case-study evidence and a simple model of the money market that illustrates the possible links between vote buying and M1.

# 2 A new monetary election cycle

Existing models of political business cycles in monetary aggregates emphasize that politicians who seek reelection will employ monetary instruments to generate a favorable eco-

[1] Zitzewitz (2012) reviews the literature that uses the tool-kit of applied economics to detect illegal behavior, including corruption. For reviews of the literature on corruption, see Aidt (2003), Pande (2008) or Olken and Pande (2012).

nomic environment prior to an election.[2] Accordingly, any monetary expansion must start well before the election date to have the desired effect on the real economy. Both conceptually and empirically, the relevance of such monetary policy cycles remains contested (Drazen 2001). 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.[3] In contrast to the existing empirical literature, which studies quarterly or annual data, we study monthly data and find robust evidence of a monetary cycle in the growth rate of M1 centered around elections. This stylized fact is new to the literature.

## 2.1 Data

To establish the new stylized fact, we study two panels of countries for which we can observe M1 at the monthly frequency for the years between 1975 and 2015. The primary sample consists of up to 104 non-OECD countries while the secondary sample consists of 17 OECD countries.[4] The unit of analysis is a country, year and month triple. To be included in the sample, a country must hold elections and its central bank must report monthly data on M1 to the International Monetary Fund (IMF). As a consequence, the panels are unbalanced. Data on M1 are published by International Monetary Fund (2018) and are recorded at the end of each month. We obtain data on election months from the

[2] The original Nordhaus (1975) and MacRae (1977) models focus on a Phillips curve trade-off between inflation and unemployment and predict an expansion of monetary aggregates or a reduction in central bank rates prior to the election. Alesina et al. (1997, Chapter 1) offers an overview of these models.

[3] See, for example, Alesina et al. (1992, 1993); Drazen (2001); Heckelman and Wood (2005); Alpanda and Honig (2009); Dreher and Vaubel (2009); Klose (2012).

[4] OECD membership is defined as of 2009. Our estimates remain robust if we define OECD membership as of 1975 or 2017, see Table A5, columns (7), (8), (12), (13) and (14) in the supplementary material.

Database of Political Institutions (DPI) constructed by Beck et al. (2001) and data on election dates from the International Foundation for Electoral Systems (2015). Table A1 in the supplementary material lists the countries in the two samples.

As the baseline, we estimate a short-run money demand function of the following kind:

$$\begin{aligned}\Delta \ln M1_{cym} &= \sum_{i=1}^{k} \alpha_i \Delta \ln M1_{cym-i} + \beta_0 E_{cym} + \beta_1 \Delta \ln Y_{cy} + \beta_2 \Delta \ln R_{cym} \\ &\quad + \beta_3 \Delta \ln P_{cy} + X_{cy}\beta_4 + \mu_c + \eta_y + \upsilon_m + \varepsilon_{cym}.\end{aligned} \tag{1}$$

The dependent variable - $\Delta lnM1$ - is the growth rate of M1, where M1 is defined as the total amount of cash in circulation plus transferable deposits held by all money holding sectors, in country $c$ and month $m$ in year $y$.[5] Short-run money demand is a function of past growth in M1 (between one and six lags), the annual growth rate of the price level ($P$), the annual growth rate of real GDP per capita ($Y$) and, in some specifications, the monthly change in the nominal interest rate ($R$).[6] The vector $X$ includes control variables in levels measured for countries and years: GDP per capita, a proxy for wealth (resource rents as a share of GDP), the exchange rate against the US dollar,[7] the quality

[5] One reason we take the growth rates of M1 as our dependent variable rather than its levels is that M1 is measured in national currency. The data quality also varies from country to country. Consequently we trim the data on $\Delta lnM1$ at its bottom and top one percentiles. Our estimates remain robust to alternative strategies, see Table A5, columns (5) and (6) in the supplementary material.

[6] We proxy the short-run nominal interest rate by the monthly interest rate on treasury bonds. These data come from International Monetary Fund (2018) and are only available for around half of the countries in our sample. Consequently, we do not include the interest rate variable in most specifications.

[7] The source of these data is World Development Indicators (2014). Resource rents are the sum of oil rents, natural gas rents, coal rents (hard and soft), mineral rents, and forest rents.

of institutions proxied by the Polity IV index normalized between zero and one (Center for Systematic Peace 2015), and for whether a country in a given year is a new democracy in the sense of Brender and Drazen (2005).[8] Table A2 in the supplementary material reports summary statistics and data sources. All specifications include country ($\mu$), year ($\eta$) and month ($\nu$) fixed effects. In more demanding specifications, we seasonally adjust the data on M1, as well as replacing country, year and month fixed effects with *country*×*year*, *year*×*month* and *country*×*month* fixed effects. $\epsilon$ is the error term.[9]

The main variable of interest is $E$. It captures the timing of elections and is coded in two alternative ways. The main coding records the month in which an election takes place. This can be coded for all elections in our samples. Specifically, the dummy variable *Election month* is defined as being equal to one if at least one election takes place in country $c$ in month $m$ and year $y$ and zero otherwise. The second coding – *Election day* – in the spirit of Franzese (2000), takes into account the precise timing of an election within a month. It is equal to the election date divided by 31 for the election month and zero otherwise.[10]

---

[8] Brender and Drazen (2005) define a country as being a new democracy during its first four elections following a transition from autocracy (negative score on the Polity IV index) to democracy (non-negative score on the Polity IV index) after which it becomes an old democracy.

[9] We cluster the errors at the level of countries. A small fraction of our monthly observations corresponds to elections and, for this reason, it may be better to bootstrap the errors than to cluster. We find that the bootstrapped and cluster standard errors are very similar. Table A5, column (4) in the supplementary material reports a representative specification with bootstrapped standard errors (based on 1000 replications).

[10] International Foundation for Electoral Systems (2015) provides information on the exact election days from 1998 onward. *Election day* is not coded for the elections before then. The rationale for the particular coding of *Election day* is that M1 is recorded at

The parameter of interest is $\beta_0$. 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 (or interactions thereof), is unrelated to $\epsilon$. [11]

## 2.2 The average election date effect

Table 1, columns (1) to (4) report the baseline estimates of the average election date effect as captured by the *Election month* and *Election day* variables. Columns (1) and (2) show specifications with the seven main time-varying controls. Columns (3) and (4) add controls for the level, lag and change in the treasury bill rate for the smaller sub-sample of countries for which this information is available. All these specifications of equation (1) include year, month and country fixed effects and three lags of the monthly growth rate of M1.[12] The specifications utilize the maximum number of country-year pair observations available (the total sample) in each case, and the number of observations, therefore, varies from column to column. In all cases, we find a significant (at the five percent level or better) increase in the growth rate of M1 in election months. In the

---

the end of the month. An election that takes place at the end of the month gets weight 1, while an election that takes place at the beginning gets weight 1/31.

[11] We estimate equation (1) with a fixed effects estimator. In the dynamic specifications, this causes Nickell's bias. However, since our data are monthly, we have over 500 time periods, so the size of the bias is likely to be very small. We have nonetheless estimated equation (1) with a difference-GMM estimator which instruments the lagged dependent variables with their lags. The results, which are reported Table A5, columns (10) and (11) in the supplementary material, are very similar to those obtained with the fixed effects estimator.

[12] Table A5 in the supplementary material reports additional dynamic specifications with up to six lags and shows that the results are very similar.

baseline specification in column (1), the average election month increase in the growth rate of M1 is 0.54 percentage points. This corresponds to about 1/13th of a standard deviation. Column (2) reports the corresponding result for *Election day* which takes into account the precise timing of the election within a month. The average election day effect is positive and significant. The effects are a little larger when we control for the interest rate (columns (3) and (4)).[13]

The baseline specification in equation (1) includes country, year and month fixed effects and estimates the election date effect using within country variation across years and months. We can restrict the variation further to engage with three potentially confounding factors. First, we can control for *country*×*year* fixed effects. This enables us to identify the election date effect from high-frequency changes in the growth rate of M1 happening before an election while controlling for all other country-specific macro-economic changes that may occur during election years.[14] Second, we can control for *month*×*year* fixed effects. Unlike the month fixed effects in the baseline specification, this controls for common macro shocks, such as a financial crisis, international financial flows etc., that affect all countries in a given month within a year. Finally, the monthly data exhibit a high degree of seasonality. We can control for this by seasonally adjusting the monthly M1 series for each country with the X12-ARIMA procedure used by the US Census Bureau and via *country*×*month* fixed effects.[15]

---

[13] The range of the observed monthly growth rate in M1 is from -11 to 19%. Table A5, columns (5) and (6) in the supplementary material exclude the 1% and 5% most extreme values of M1, respectively, and show that the estimate of the election date effect is not sensitive to outliers.

[14] In the baseline, such country-specific macro-economic effects are picked up by the control variables.

[15] The combination of these three interactions subsumes the country, year and month fixed effects and the control variables included in the baseline.

**Table 1:** Main results: The election date effect

| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | $\Delta lnM1$ | | | | | | | $\Delta lnM1_{SA}$ | | $\Delta lnM1$ | | | $\Delta lnM1_{SA}$ | |
| | *Total sample* | | | | *Fixed sample* | | | | | *Fixed sample* | | | | |
| Election month | 0.0054*** | | 0.0068** | | 0.0061*** | 0.0041** | | 0.0037* | | 0.0076** | 0.0097*** | | 0.0087*** | |
| | (0.0019) | | (0.0028) | | (0.0021) | (0.0020) | | (0.0021) | | (0.0032) | (0.0032) | | (0.0029) | |
| Election day | | 0.0123*** | | 0.0159*** | | | 0.0084** | | 0.0078** | | | 0.0148** | | 0.0138** |
| | | (0.0036) | | (0.0055) | | | (0.0038) | | (0.0036) | | | (0.0059) | | (0.0053) |
| GDP pc (log) | -0.0121*** | -0.0175*** | -0.0033 | -0.0181** | -0.0162*** | | | | | -0.0120 | | | | |
| | (0.0031) | (0.0038) | (0.0052) | (0.0083) | (0.0039) | | | | | (0.0078) | | | | |
| GDP pc growth | 0.0004*** | 0.0005*** | 0.0003 | 0.0003* | 0.0005*** | | | | | 0.0003* | | | | |
| | (0.0001) | (0.0001) | (0.0002) | (0.0002) | (0.0001) | | | | | (0.0002) | | | | |
| Inflation | 0.0001*** | 0.0002*** | 0.0003*** | 0.0002* | 0.0002*** | | | | | 0.0001 | | | | |
| | (0.0000) | (0.0001) | (0.0000) | (0.0001) | (0.0001) | | | | | (0.0001) | | | | |
| Exchange rate (log) | -0.0028*** | -0.0006 | -0.0015** | -0.0023 | -0.0000 | | | | | -0.0023 | | | | |
| | (0.0006) | (0.0027) | (0.0007) | (0.0022) | (0.0029) | | | | | (0.0019) | | | | |
| Resources rents / GDP | 0.0004*** | 0.0004*** | 0.0003** | 0.0003*** | 0.0004*** | | | | | 0.0003** | | | | |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | | | | | (0.0001) | | | | |
| Polity IV | -0.0004 | 0.0002 | 0.0035 | 0.0088** | 0.0000 | | | | | 0.0095** | | | | |
| | (0.0034) | (0.0038) | (0.0040) | (0.0038) | (0.0038) | | | | | (0.0037) | | | | |
| New democracy | -0.0004 | 0.0005 | 0.0013 | 0.0010 | 0.0009 | | | | | 0.0018 | | | | |
| | (0.0011) | (0.0014) | (0.0017) | (0.0021) | (0.0014) | | | | | (0.0021) | | | | |
| Treasury bill rate | | | -0.0005*** | -0.0004** | | | | | | -0.0004** | | | | |
| | | | (0.0001) | (0.0002) | | | | | | (0.0002) | | | | |
| Treasury bill rate (t-1) | | | 0.0004*** | 0.0004* | | | | | | 0.0003** | | | | |
| | | | (0.0001) | (0.0002) | | | | | | (0.0001) | | | | |
| $\Delta$(Treasury-bill rate) | | | -0.0003 | -0.0002 | | | | | | 0.0004 | | | | |
| | | | (0.0008) | (0.0007) | | | | | | (0.0007) | | | | |
| C, Y, M FE | x | x | x | x | x | | | | | x | | | | |
| C×Y, C×M & Y×M FE | | | | | | x | x | x | x | | x | x | x | x |
| Observations | 20,455 | 17,247 | 8,875 | 7,422 | 16,034 | 16,034 | 16,034 | 16,034 | 16,034 | 6,988 | 6,988 | 6,988 | 6,988 | 6,988 |
| R-squared | 0.1436 | 0.1421 | 0.1291 | 0.1306 | 0.1353 | 0.4644 | 0.4645 | 0.2300 | 0.2302 | 0.1184 | 0.4654 | 0.4652 | 0.2551 | 0.2551 |
| Countries | 104 | 104 | 55 | 55 | 98 | 98 | 98 | 98 | 98 | 52 | 52 | 52 | 52 | 52 |

*** p<0.01, ** p<0.05, * p<0.1. Notes: All regressions control for three lags of the dependent variable. CY, CM & YM FE mean, respectively, *country×year*, *country×month* and *year×month* fixed effects. They subsume the country, year and month fixed effects and the control variables. Standard errors are robust to heteroscedasticity and are clustered at the level of countries. $M1_{SA}$ is $M1$ seasonally adjusted. *Resource rents* are the sum of oil rents, natural gas rents, coal rents (hard and soft), mineral rents, and forest rents. Columns (1)-(4) use the total available sample. Columns (5)-(9) and (10)-(14) fix the sample where the samples of all of these five specifications overlap.

Table 1, columns (6) to (9) report specifications that include these three combinations of fixed effects. The dependent variable in columns (8) and (9) is the growth rate of seasonally adjusted M1. Columns (10) to (14) show the corresponding results for the smaller sample where we can control for the interest rate. As discussed above, the data on election months, election days and the control variables are available for different samples and the seasonal adjustment implies a small loss of observations. To ensure comparability across these regressions, we fix the sample such that the country-year pairs are the same across columns (5) to (9) and (10) to (14), respectively. Columns (5) and (10) replicate the baseline regressions without interacted fixed effects on the two "fixed samples" and confirm the baseline results.

These specifications identify the election date effect by testing if, within an election year, the growth rate of M1 is higher during the month of an election, after removing common shocks and country-specific seasonality. The coefficient on *election month* is 0.0041 without seasonally adjusting the data (column (6)) and 0.0037 with seasonal adjustment (column (8)). In both cases, the estimates are smaller than the corresponding estimate in column (5) but significant at the 5 and 10 percent level, respectively. Columns (7) and (9) show the corresponding results for *election day* and columns (10) to (14) show the results for the smaller sample for which interest rate data is available. In all cases, the election date effect is statistically significant. These results show that the baseline estimates are remarkably robust and that the election date effect that we find is not an artifact of common shocks (*month*×*year* fixed effect), country-specific seasonality (*country*×*month* fixed effects) or country-specific macro-economic events within election years (*country*×*year* fixed effects).

The elections in our sample are not spread uniformly across the year. October to November typically host more elections while January hosts about half of the number of elections happening during an average month of a year. 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 results could be driven by reverse causality. We include the month fixed effects to control for this possibility in the baseline and we show that the results are robust to country-specific month fixed effects and to seasonal adjustment of the data (columns (6) to (9)). We can address the issue of strategic timing of elections more directly by restricting attention to the 28 countries (n=4353) in our main sample that have fixed election days (for their legislature) and where reverse causality by definition cannot be an issue. The estimate of *election month* for this sub-sample, based on a specification similar to that reported in Table 1, column (1) is equal to 0.012 with a standard deviation of 0.0040 and, thus, significant at the 1% level. All in all, this suggests that the results are not due to timing effects and reverse causality.

## 2.3 The timing of the election date effect

The evidence presented in Table 1 does not show whether the election date effect begins in the months before the election and/or lingers into the months afterwards. To investigate the timing of the effect, we estimate equation (1) with lags or leads of *election month* included. Table A3 in the supplementary material reports the results. Column (1) shows a specification with two leads and two lags while columns (3) to (6) show specifications with each lag or lead on its own and column (2) shows, for comparison, a specification with *Election month*. There is no evidence of any monetary expansion in the two months before the election but there is evidence of an effect in the post-election month. The point estimate on the post-election month dummy is larger than for the election month, but not statistically different. This suggests that the monetary effect of elections persists into the month after the election consistent with some lag in the monetary transmission.[16]

[16] We have estimated specification with three lags and leads and the third lag/lead is insignificant.

## 2.4 The OECD sample

The sample of 17 "old" OECD countries is of particular interest because the "old" OECD countries have long-established democratic institutions and, generally, score highly on indexes of the quality of institutions (e.g., Freedom House 2012) and because they have sophisticated monetary systems. We have estimated equation (1) on this sample and Table A3, columns (7) to (12) in the supplementary material report the results. We find no evidence of any monetary election cycle in the election month, nor in the months before or after the election. The election date effect that we find is, therefore, present only in the sample of non-OECD countries.

## 2.5 Legislative and executive elections

Our samples include a mixture of legislative and executive elections. For some countries, executive and legislative elections take place on the same day. To investigate heterogeneity in the election date effect across election types, we have split *Election month* into three sub-indicators: one for legislative elections only; one for executive elections only; and one for simultaneous legislative and executive elections.[17] We, then, re-estimated equation (1) with these refined *Election month* variables. Table A4, columns (1) to (4) in the supplementary material report the results for the non-OECD sample; columns (5) to (8) report the corresponding results for the OECD sample. We observe that the point estimates are positive for the non-OECD sample, but that it is executive elections of the head of state that drive the significance of the overall election date effect (column (3)). Joint elections almost triples the size of the point estimate (column (4)). The results for the OECD sample are not statistically significant.

In conclusion, our baseline result is a robust, statistically significant, and economically meaningful monthly election cycle in the growth rate of M1 in non-OECD countries. The

[17] We have 649 elections, of which 155 are only executive, 395 are only legislative, and 99 are simultaneous elections.

effect is centred on the month of the election and lingers into the post-election month. The effect is strongest in executive elections. On the other hand, we cannot detect the effect in the sample of OECD countries. These empirical facts are new to the literature.

# 3 The monetary mechanism behind the election date effect

Since M1 is endogenous to the monetary system, short-term fluctuations in M1 near elections can reflect either shocks directly to the supply of the base money or shocks to the demand for money that get accommodated through the banking system.[18]

First, the central bank can via open market operations, funding of government spending, or a change in its refinancing rate affect the *supply* of base money which, in turn, affects M1. Second, short-term fluctuations in the *demand* for cash money can create irregularities in the supply of money through three main channels. The first channel is substitution from broad (M2 or M3) to narrow (M1) money. Transfers between cash and bank deposits as such are neutral in their effect on M1, since this monetary aggregate is defined as the sum of the two. However, if agents liquidate broader financial assets around elections, 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 (e.g., in the shadow economy), is used for transactions, it (partially) returns to the banking sector. This can happen in two ways. First, the hoarded cash returns directly to banks if the recipients deposit it or substitutes it for deposits that they would otherwise have withdrawn. This effect is clearly strongest in societies with easy access to banking services. Secondly, the hoarded cash also returns to the banks if the recipients spend the cash on goods and the retailers

[18] In supplementary material appendix C, we sketch a simple model of the banking sector that illustrates the economics of these various effects.

subsequently deposit their revenue from these transactions in a bank.To the extent that the hoarded cash returns to the banking system, the commercial banks experience an increase in their 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. Third, in a fixed or managed exchange rate system with a convertible currency, an increase in the demand for local currency from abroad around the time of elections will trigger appreciation pressure which must be accommodated by liquidity from the central bank.

To discern the degree of active central bank involvement in generating the election date effect, we ideally would study the growth rate in the supply of primary money (the money base) around elections as this, rather than M1, is what the central bank controls. Since the relevant data on a monthly frequency for a sufficient number of countries is unavailable, we employ an alternative approach. We investigate the extent to which the central banks in our sample of non-OECD countries use the discount window (rather than open market operations) to induce an election-motivated expansion of the money base. If so, the interest rate that the central bank charges its borrowers (formally the "refinancing rate" as reported by the respective central banks) should fall in the months leading up to an election. Monthly data on the refinancing rate is available from 21 central banks in the non-OECD sample (Delta Stock 2018). Table 2, columns (1) to (6) show that the central bank's lending rate neither changes in the election month nor in the months prior to or after that.

Another way the central bank could affect the growth rate of M1 in election months is to fund short-term government spending directly.[19] If so, the funds constitute an injection of primary money into the economy and affect M1 directly. If this is the monetary

[19] While most of literature on fiscal political cycles focus on deviations during the election year (e.g. De Haan and Klomp 2013; Aidt and Mooney 2014), there is evidence on short-run spending cycles of this sort. Akhmedov and Zhuravskaya (2004) document a sizable increase in direct monetary transfers to voters from the regional

**Table 2:** Refinancing rates and higher powered money

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| VARIABLES | *Refinancing rate* | | | | | | *M1/(M2-M1)* | $\Delta lnM2$ | $\Delta lnM3$ |
| Election month | -0.0065 | -0.0064 | | | | | 0.1269** | 0.0023 | 0.0013 |
| | (0.0069) | (0.0070) | | | | | (0.0616) | (0.0014) | (0.0019) |
| Election (t-2) | 0.0082 | | 0.0086 | | | | | | |
| | (0.0073) | | (0.0074) | | | | | | |
| Election (t-1) | -0.0111 | | | -0.0113 | | | | | |
| | (0.0069) | | | (0.0070) | | | | | |
| Election (t+1) | -0.0016 | | | | -0.0012 | | | | |
| | (0.0036) | | | | (0.0035) | | | | |
| Election (t+2) | -0.0059 | | | | | -0.0064 | | | |
| | (0.0052) | | | | | (0.0054) | | | |
| Observations | 3,326 | 3,326 | 3,326 | 3,326 | 3,326 | 3,326 | 20,170 | 20,170 | 10,106 |
| R-squared | 0.9485 | 0.9485 | 0.9485 | 0.9485 | 0.9485 | 0.9485 | 0.0221 | 0.0879 | 0.1205 |
| Countries | 21 | 21 | 21 | 21 | 21 | 21 | 101 | 101 | 49 |

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

Notes: The variables *Election (t+i)* for $i = -2, -1, 1, 2$ are coded one in month $i$ around the election. All regressions control for GDP growth, GDP p.c., inflation, the exchange rate, resource rents, polity index of democracy, new democracy dummy; include three lags of the dependent variable; and country, year and month fixed effects. In the sample with the 21 countries for which data on the refinancing rate can be obtained in columns (1) to (6), the point estimate on *election month* is positive but not statistically significant. Standard errors are robust to heteroscedasticity and are clustered at the level of countries.

mechanism behind the election date effect, then the effect should be stronger in countries in which the central bank is under government influence (e.g., Berger et al. 2001). We investigate this with the data on de jure central bank independence collected by Bodea and Hicks (2015). Their *CBI index* ranges from 0 to 1, with 1 reflecting maximum independence, and is coded for 72 of the countries in the non-OECD sample covering the years from 1975 to 2015.[20] To this end, we augment equation (1) with *CBI index* and its interaction with *Election month*. Figure 1(a) graphs the interaction effect showing that the election date effect does not vary with the degree of central bank independence.

This militates against the hypothesis that the election date effect is caused by public spending funded directly by the central bank.[21] Taken together, these results strongly speak against active central bank intervention as the main explanation for the election date effect.

To discern if the election date effect is caused by extra demand for cash, we can study the degree of substitution from broad (M2 or M3) to narrow (M1) money in election months.[22] To do this, we study the $\frac{M1}{M2-M1}$ ratio around the election. We net out M1 in the denominator because M1 is contained in M2 and we want to know if assets which are part of M2 and *not* part of M1 are converted into M1 around the election. We also study the growth rates of M2 and M3 directly. These should *not* increase in election months if the increase in cash demand is funded entirely by liquidizing assets unique to M2 or M3. Table 2, column (7) reports the results with $\frac{M1}{M2-M1}$ as the outcome variable. We observe that the ratio is higher in election months than in other months. This is consistent with the hypothesis that the election date effect is, in part, caused by substitution effects from broad to narrow money. This conclusion is supported by the fact that the effect

governments of Russia in the days leading up to the election. Labonne (2016) documents a short-term employment cycle in municipalities in the Philippines.

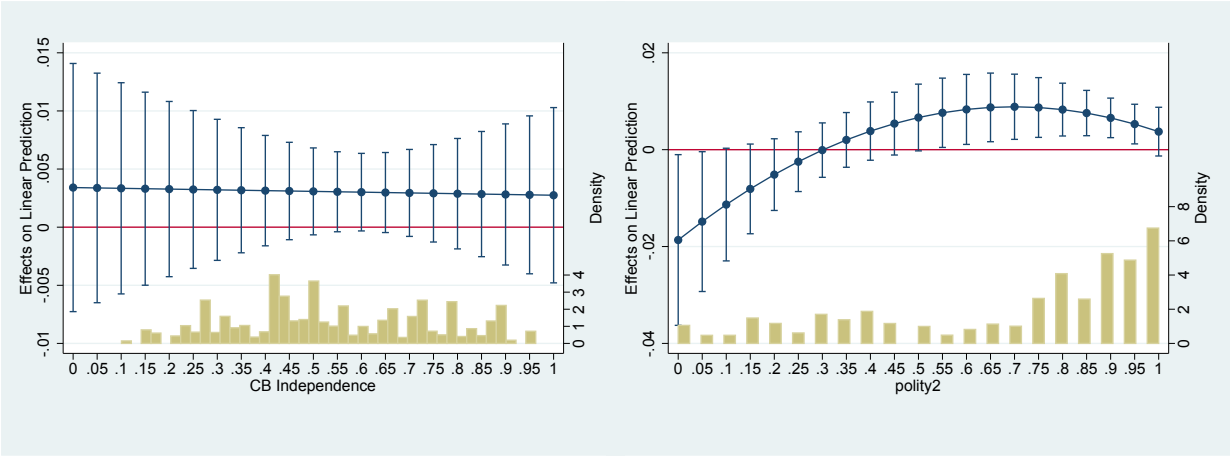
[20] A central bank is coded as being more independent if its governors serve longer terms; if the appointment and dismissal procedures for governors are insulated from the government; if the bank's mandate is focused on price stability; if the formulation of monetary policy is in the hands of the central bank; and if the terms on central bank lending to the government are restrictive.

[21] The evidence presented in Alpanda and Honig (2009, 2010) supports this conclusion.

[22] M2 comprises M1 plus deposits with agreed maturity up to two years and deposits redeemable at notice up to three months. M3 comprises M2 plus repurchase agreements, money market fund shares and money market papers, and debt securities up to two years. The source of these data is International Monetary Fund (2018).

**Figure 1:** The election date effect at different values of the central bank independence (CBI) index and the Polity IV index

**(a)** CBI **(b)** Polity IV




Notes: The point estimates with 95% confidence intervals show the predicted marginal effects of elections on the growth rate of M1 for different values of: (a) the central bank independence index ranging from 0 (lowest) to 1 (highest), and (b) the Polity IV index of democracy ranging from zero (lowest) to one (highest). The bars in sub-figures (a) and (b) indicate the densities of, respectively, the central bank independence index and the Polity IV index, and are measured on the right-hand y-axis. The sample includes both non-OECD and OECD countries. The underlying regressions are reported in Table A7 in the supplementary material.

of *Election month* on the growth rates of M2 (column (8)) and M3 (column (9)) are not statistically significant.

# 4 The vote buying hypothesis

The election date effect is present only in the sample of non-OECD countries, many of which have comparably weak electoral institutions, and not in established OECD democracies.[23] That fact that the cycle is timed around the election – in the election month and

[23] Brender and Drazen (2005), Shi and Svensson (2006) and Hanusch and Keefer (2014), amongst others, have previously shown that electoral politics in societies with inexperienced or uninformed voters or with "young" political parties facilitates political business cycles in government spending and other fiscal outcomes. Potrafke (2018)

the month thereafter – and not in the months leading up to the election suggests that this phenomenon cannot be explained by traditional political business cycle models that focus on attempts by the incumbent government to manipulate monetary policy in the run-up to an election with the aim of engineering favorable economic conditions. An alternative explanation is required. We propose that the abnormally high monetary growth in the election month may be indicative of systemic vote buying triggered by the effect it has on cash demand.[24] The political science and economics literature is abundant with surveys, case studies, and field experimental evidence of systemic vote buying.[25] The logic is that vote buying requires liquid resources (cash) to be distributed to voters. This creates a spike in the demand for money causing irregularities in the supply of money. Since vote buying takes place close to an election, the effect on M1 is timed around the day of the election. Accordingly, systemic vote buying will be detectable as a spike in the growth rate of M1 within a short window around the election. The link between the extra cash demand triggered by vote buying and M1 can, as discussed in section 3, operate through substitution from broader assets to cash or through a short-run multiplier effect induced by the return to the banking sector of hoarded cash that is spent on consumption goods. We refer to this as the vote buying hypothesis.

---

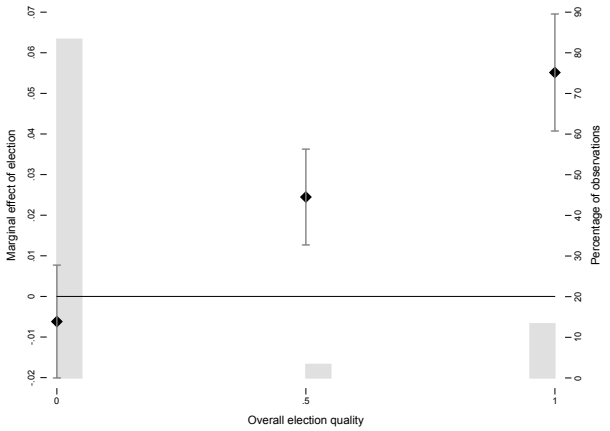
reports evidence of an election cycle in perceived corruption. Keefer and Vlaicu (2008), Hanusch and Keefer (2014) and Hidalgo and Nichter (2015) consider the link between vote buying and political budget cycles in fiscal variables.

[24] We use the term vote buying to refer to two related strategies. One strategy is to offer a monetary payment as a direct exchange of cash for votes (see, e.g., Shefter 1977; Heckelman and Yates 2002; Hicken 2011; Stokes et al. 2013; Aidt and Jensen 2017). Another strategy is to buy turnout, i.e., 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).

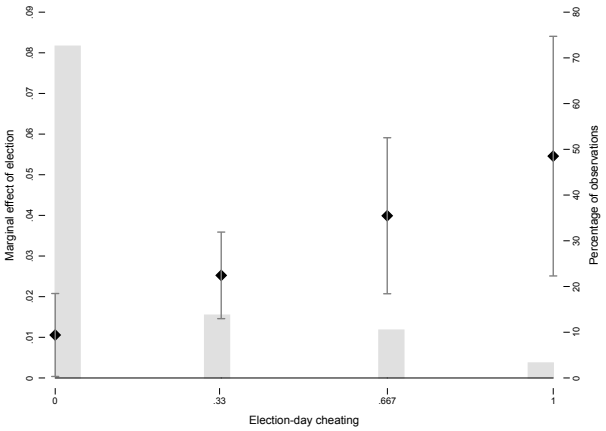
[25] See Stokes (2005),Finan and Schechter (2012),Wantchekon (2003), Vicente (2014) or Hicken et al. (2015).

**Figure 2:** The marginal effect of *election month* interacted with indicators of the quality of elections.

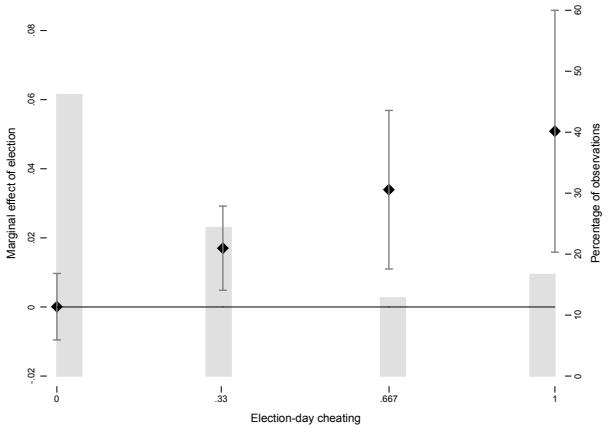
**(a)** Overall election quality



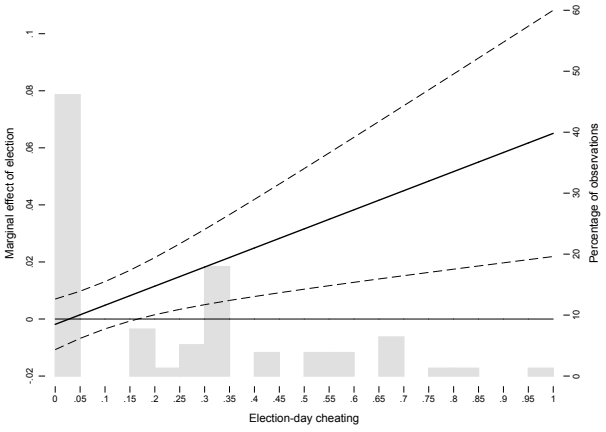

**(b)** US State Dep.: Election day cheating




**(c)** Max of election day cheating




**(d)** Mean of election day cheating




Notes: Each panel shows the point estimate of the interaction effect (the dot or solid line) and the 95% confidence interval (bars or dotted lines), both measured on the left-hand vertical axis; the distribution of observations is indicated with the gray blocks and is measured on the right-hand vertical axis. Panel (a) reports the interaction effect evaluated at different values of the overall electoral quality index constructed from the reports of the Organization of Security and Co-operation in Europe; panels (b) to (d) report the interaction effect evaluated for different values of the election day cheating index. In panel (b) the source of the election day cheating index is the US State Department; in panel (c) the election day cheating index is the maximum (worst) score reported by any monitor for each election; in panel (d) the election day cheating index is the average score across all available monitoring reports. The underlying regressions are reported in Table A6 in the supplementary material.

We are not able to provide causal evidence in support of this hypothesis since we do not observe any plausibly exogenous variation across elections in vote buying. However,

we present four pieces of evidence that are consistent with the vote buying hypothesis, but we cannot establish its validity conclusively.

## 4.1 Electoral irregularities

An implication of the vote buying hypothesis is that, within a country, the magnitude of the election date effect should be larger in elections with a lot of vote buying than in elections with less. Many of the elections included in the non-OECD sample were subject to external monitoring by the European Union, the United Nations, the US State Department or numerous non-governmental organizations. The purpose of such monitoring is to verify if elections are fairly conducted, and to record and report any irregularities. Drawing on Kelley (2012), who has systematized the information contained in hundreds of monitoring reports for the period between 1978 and 2004,[26] we can classify elections according to how much vote buying the monitors observed.[27]

[26] The data are coded in two separate datasets. The Dataset on International Election Monitoring (DIEM) codifies the reports of external monitors and covers 108 countries between 1980 and 2004. The Quality of Elections Dataset (QED) codifies the information about the conduct of elections contained in the US State Department's annual country reports on human rights practices and covers 172 countries between 1978 and 2004. The advantage of these data is that they derive from a single source. Kelley (2012, Appendix A) documents how the data were collected and coded. The sources for the DIEM dataset are 673 mission reports from 21 international organizations (including the Commonwealth Secretariat, the European Union, the Council of Europe, the Organization of American States, the Organization for Security and Co-operation in Europe, African Union, the United Nations, or the Economic Community of West African States).

[27] We note that a potential problem with using the information from the monitoring reports for this test is that not all elections are monitored and the selection into monitoring is not random. In particular, governments intending to cheat might do what

Our test adds the measures of election irregularities and their interaction with *Election month* to equation (1) and re-estimate it on the sample for which monitoring data is available. If the vote buying hypothesis is true, then the interaction effect should be positive. Specifically, we use two quantitative measures of election irregularities. First, the variable *Overall election quality* codes the summary assessment of the monitoring organization regarding irregularities before, during and after an election. An election is scored as a zero if the election is considered to be "free and fair" (acceptable) and is scored as one if it does "not represent the will of the people" or if it is judged to be "fraudulent and to fall short of international standards" (unacceptable). Elections in between these extremes are given a score of 0.5. Second, the variable *Election day cheating* records evidence of vote padding, inflated vote counts, ballot stuffing, double voting and vote buying, etc. This is a direct measure of any irregularities that took place close to the election. The original data are coded on a "no problems" to "major problems" scale in four steps which we normalize to a point distribution between zero and one. To construct these variables, we use the evaluations of the US State Department and the Organization of Security and Co-operation in Europe, both of which must be considered relatively objective and consistent observers, as well as the average or the maximum score from all the monitoring reports available from Kelley (2012).

Figure 2 shows the point estimate of the interaction effect evaluated at different values of *Overall election quality* (panel (a)) or *Election day cheating* (panels (b) to (d)). From

---

they can to discourage external monitoring. If so, this would bias our test against finding evidence of the vote buying hypothesis. For our test, it would also be problematic if the monitors overstated cheating in monitored elections during which M1 grew fast. These and other potential biases of these reports are discussed in Kelley (2012, Chapter 4). We mitigate against potential biases by either drawing on the reports of one organization over time (the bias would then approximately be the same from year to year) or by averaging the reports associated with a particular election across organizations.

panel (a), we see that the election month increase in the growth rate of M1 is larger in elections that are reported to be fraudulent and to fall short of international standards. In fact, the election month effect is not statistically significant in elections which are free and fair. Panels (b) to (d) show a similar picture for *Election day cheating*: the election date effect is increasing in the extent of recorded vote buying and other election day irregularities. These results provide support for the vote buying hypothesis.

## 4.2 The quality of institutions

The vote buying hypothesis implies that the size of the election date effect depends on the quality of a country's political institutions. In particular, large-scale vote buying does not usually occur in countries with "authoritarian" political institutions where the elections that do take place are tightly controlled by the ruling government party, making vote buying redundant. Similarly, in countries with strong democratic institutions where a vibrant press and other well-working checks and balances make large-scale vote buying impossible. Vote buying, therefore, on a scale that can effect on M1 is most likely to take place in countries with institutions in between these extremes, i.e., in countries with contested elections that, because of prevailing institutional weaknesses, are susceptible to vote buying (and other types of fraud). Given this, the vote buying hypothesis predicts an inverse U-shaped relationship between the quality of a country's political institutions and the size of the election date effect.

To test this implication, we use the Polity IV index (Center for Systematic Peace 2015), normalized to be between zero (weak institutions) and one (strong institutions), to quantify the "quality of political institutions". We include the index and its square along with interactions of the two with *Election month* in equation 1. To maximize the range of the Polity IV index, we combine the non-OECD and OECD samples. Figure 1 plots the estimate of the election date effect at different values of the Polity IV index along with 95% confidence intervals. We observe an inverse U-shaped relationship between the size of the election date effect and the Polity IV index, with the pattern being clearer

for the seasonally adjusted data (in panel (b)). The election date effect is statistically significant only for countries located in the middle range of the index. Put the other way around, the election date effect is neither significant in countries with a low Polity IV scores and week institutions nor in those countries (belonging mostly in the OECD) with a "perfect" Polity IV score of one. van Ham and Lindberg (2015) finds a similar effect in their study of self-reported vote buying in Africa. Overall, this is consistent with the vote buying hypothesis.

### 4.3 The closeness of elections

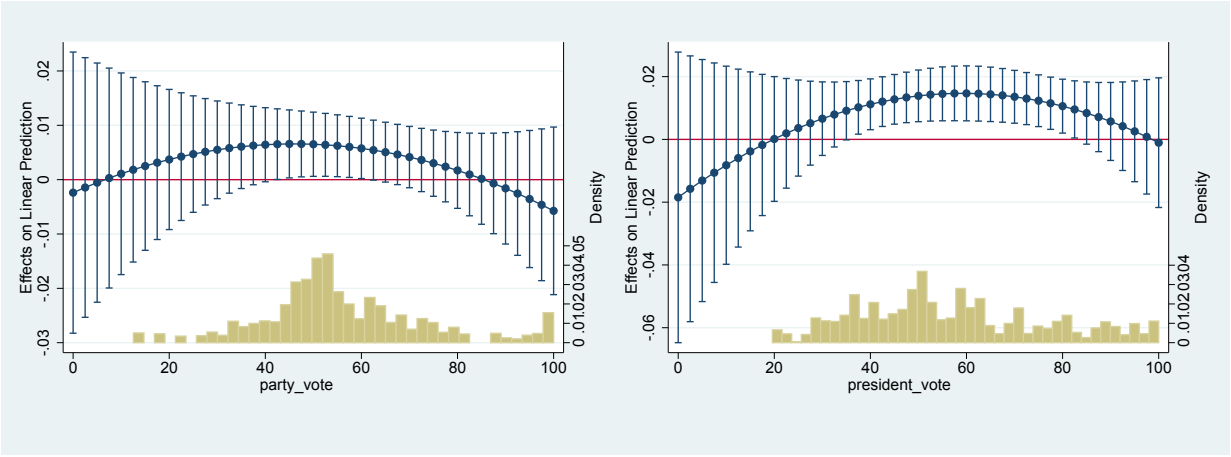
Another implication of the vote buying hypothesis is that vote buying is most likely to be used as an electoral strategy in elections that are expected ex ante to be close. Candidates and political parties have less incentive to buy vote if they are almost sure to lose or to win.[28] To test this implication, we approximate the "closeness" of an election by the ex post vote share of the incumbent government party or coalition in parliamentary elections and of the president in executive elections.[29] We augment equation 1 with the variable *vote share* and its square along with interactions of the two with *Legislative election* or *Executive election*, respectively. Figure 3 shows that the election date effect has an inverse U-shaped relation with the vote share of the incumbent in the non-OECD countries. The effect is statistically significant in elections where the vote share of the incumbent is between 40 and 60% in legislative elections (Figure 3(a)), and between 40 and 80% in executive elections (Figure 3(b)). This evidence is consistent with the vote buying hypothesis. In interpreting this, we should bear in mind that the ex post outcome

[28] The logic is similar to the result from the literature on probabilistic voting that parties will promise post-election programmatic benefits to districts (or groups of voters) who are willing to swing their vote (e.g., Dixit and Londregan 1996; Keefer and Vlaicu 2008).

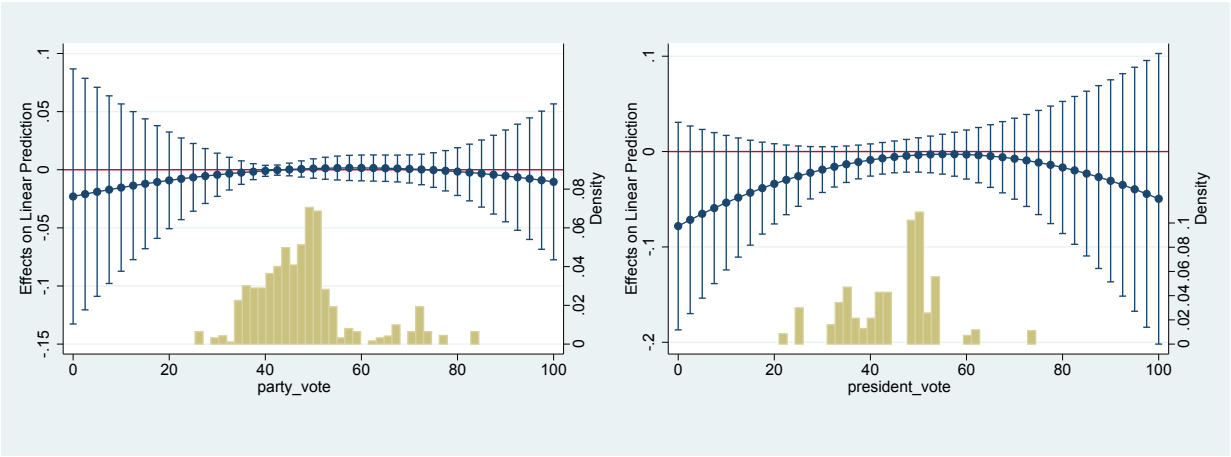
[29] The source of the data on vote shares is Beck et al. (2001). In non-election years, *vote share* is coded as the vote share gained by the incumbent in the most recent election.

**Figure 3:** The election date effect for different vote shares gained by the incumbent

**(a)** Non-OECD: Legislative election **(b)** Non-OECD: Executive election




**(c)** OECD: Legislative election **(d)** OECD: Executive election




Notes: The point estimates with 95% confidence intervals show the predicted marginal effects of *Legislative election* or *Executive election* on the growth rate of M1 for different values of the level and square of the share of votes gained by the incumbent government party (coalition) or the president, respectively. The sample is restricted to non-OECD and OECD countries in upper and lower sub-figures, respectively. The bars indicate the density of votes gained and are measured on the right-hand y-axis. The underlying regressions are reported in Table A7 in the supplementary material.

of an election is, partly, a function of the amount of ex ante vote buying. Figure 3 (c) and (d) show that the election date effect is insignificant irrespective of the "closeness" of the election in the sample of OECD countries.

## 4.4 Consumption expenditures and elections: evidence from Armenia

One of the mechanisms through which vote buying can affect M1 is that cash hoarded in the black economy gets dispersed to a large number of voters who spend the cash which subsequently, when deposited by retailers and shopkeepers, finds its way back to the banking system. A necessary condition for this effect to operate is that household consumption expenditures increase around elections indicating that extra income is being spent.

In this section, we present micro-econometric evidence to substantiate this aspect of the vote buying hypothesis. The idea, which was first developed by Mitra et al. (2017) in a study of vote buying in India, is to use household survey data to look for an increase in consumption around elections.[30] It is not possible to implement this approach in a cross-national panel setting as the required survey data are not available for enough countries. We, therefore, focus on a particular country and have, for two reasons, chosen the Republic of Armenia. Firstly, the structure of the Armenian household survey with its daily recording is particularly well-suited for this investigation. Secondly, anecdotal evidence on vote buying in Armenia is abundant. The 2012 Human Rights Report issued by the US Department of State (US Department of State 2012), for example, describes that year's parliamentary election as "competitive", but with significant violations, including "credible allegations of vote buying". The local media were also full of allegations of vote buying, reporting typical "prices" in the range from 5,000 to 10,000 Armenian Dram (AMD) per vote (10 to 20 USD)[31] and which was said to have reached up to five hundred thousand voters in a country with a population of less than 3 million and around 1.5 million registered voters (Institute for War and Peace Reporting 2012).

[30] See also Gillitzer and Prasad (2018).

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

### 4.4.1 The survey data

We draw on the annual waves of the Integrated Survey of Living Standards (ISLS) for the period 2001 to 2016 (National Statistical Service of the Republic of Armenia 2018). The ISLS collects data on consumption, income and various socio-demographic variables from a representative sample of Armenian households. Between 4,000 and 8,000 households are surveyed every year. The sample is redrawn yearly and so the same households are not followed from one year to the next. An important feature of the survey is that the sampled households fill a diary out *daily*. This diary collects information on purchases of consumption items dis-aggregated into detailed product categories. Each surveyed household fills in the diary for one month and trained interviewers visit the households multiple times during that month to ensure timely and correct entries. The households, on average, submit 28.6 days of information. The overall data set is a sequence of repeated yearly across sections with a total of 95,779 households and 2,734,856 observations, but *within* a month in a given year, we can track the consumption of the *same* household on multiple days.

Between 2001 and 2016, Armenia held three parliamentary (2003, 2007, and 2012) and four presidential elections (2003 with two rounds, 2008, and 2013).[32] The Armenian household survey is conducted throughout a calendar year. This means that within a year households start their one month of daily records of consumption at different times. In contrast to many other consumption surveys, this allows us to study the consumption patterns of those households that were surveyed in the days and weeks around each election. An additional advantage is that, unlike the data used, for example, by Mitra et al. (2017), the survey records consumption expenditures daily for the *same* household.

[32] Our empirical strategy cannot accommodate situations where more than one election is held within an interval of 80 days (as we detail below). For this reason, the second round of the 2003 presidential election and the 2003 parliamentary election, both of which followed the first round of the presidential election in 2003, are excluded.

We can, therefore, not only study finely timed (daily) consumption patterns but we can also control for unobserved household fixed effects.

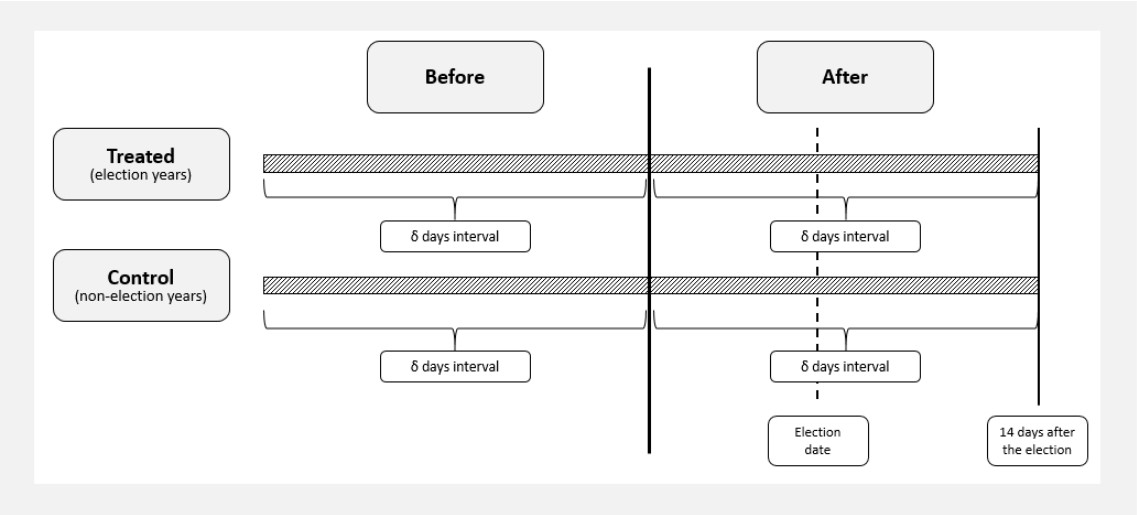
### 4.4.2 Identification strategy

As in Mitra et al. (2017), we hypothesize that if vote buying occurs, then it should be reflected in household consumption patterns. According to the permanent income hypothesis, rational households with access to a perfect capital market do not react to anticipated income shocks and respond to unanticipated ones by smoothing consumption over their life-cycle. In such a world, extra income from vote buying, whether anticipated or not, will have no or little immediate effect on consumption. However, many of the households that "sell" their vote are poor and face liquidity constraints. Such households will respond to both anticipated and unanticipated income shocks by increasing consumption.[33]

We do not know the exact timing of vote buying activity, except that they will have to take place before the election, nor do we know precisely when any extra income might be converted into extra consumption. We, therefore, specify an empirical model that allows us to study the timing of potential consumption responses in a flexible manner with different time windows. In particular, we follow Mitra et al. (2017) and specify a differences-in-differences model but with daily intervals around the election date. To create the treatment group (which is treated to income shocks from vote buying), we start

[33] This is demonstrated by a large literature that studies the effects of various anticipated and unanticipated income shocks on consumption behavior (Parker 1999; Stephens Jr 2003; Johnson et al. 2006; Stephens and Unayama 2011; Mian and Sufi 2012; Aaronson et al. 2012; Parker et al. 2013; Agarwal et al. 2017). For reviews, see Jappelli and Pistaferri (2010) and Fuchs-Schuendeln and Hassan (2016).

**Figure 4:** Example of how the before/after and control/treatment groups are constructed.




Notes: The treatment to the election ends 14 days after the election day (the cut-off). The treatment group consists of households that filled in daily diaries $2*\delta$ days before the cut-off. The households are in the "after" group during the $[0, \delta)$ days before the cut-off (which is set at 14 days after the election) and in the "before" group during the $[\delta, 2*\delta]$ day before the cut-off. The control group consists of households that filled in daily diaries on the same days as the treatment group but in the year before and after the election year.

by defining a cut-off of 14 days after an election[34] and count $2*\delta$ days backwards in time from that cut-off. All the households that fill in a daily diary within this time window of $2*\delta$ days are in the treatment group. We assume that a treated household is in the "after" group during the $[0, \delta)$ days and in the "before" group during the $[\delta, 2*\delta]$ before the cut-off. Figure 4 visualizes this approach. For example, if $\delta$ is equal to 20 days, then the households in the treatment group are treated (the "after" group) during the 6 days before and 14 days after the election and untreated (the "before" group) during the 20 days prior to that (i.e., the "before" period starts 26 and ends 6 days before the election). By varying $\delta$ between 4 and 40, we can split the treatment group into different "before" and "after" group and in that way create different treatment windows around the election. For example, for $\delta = 40$ the treatment starts 66 days before the election while for $\delta = 4$ the treatment window is much shorter and starts 10 days after the election. To construct

the control group, Mitra et al. (2017) take advantage of the staggered nature of elections across Indian states and use neighboring states without elections as the control group. Since we study national elections that are held on the same day everywhere, we cannot follow that approach. Instead, our control group consists of the households surveyed on the same dates ($2*\delta$ days) as the treatment group but in the year *before* and the year *after* the election year. Table A8 reports sample sizes for the control and treatment groups for the maximum time window of 80 days ($\delta = 40$). In this case, the combined sample consists of about 570 thousand daily observations from about 25.8 thousand households.

The differences-in-differences specification that we estimate for different values of $\delta$ is:

$$\begin{aligned} Y_{it} =& \alpha_0 + \alpha_1 * Treated(\delta)_{it} + \alpha_2 * After(\delta)_{it} + \alpha_3 * Treated(\delta)_{it} * After(\delta)_{it} + \\ & \sum_{k=2}^{7} \gamma_k * DOW_k + \sum_{l=2}^{365} \beta_l * DOY_l + \theta_i + \epsilon_{it}, \end{aligned} \tag{2}$$

where $i$ is the index for a household and $t$ is the index for calendar days and $Y_{it}$ is a consumption outcome of interest (see below). The dummy variable $Treated(\delta)_{it}$ is coded 1 for the households that for a given interval of $2 * \delta$ days belong to the treatment group and zero otherwise and the dummy variable $After(\delta)_{it}$ is coded 1 for the $\delta$ days round the election where the treated households are subject to the "election treatment". The coefficient of interest is $\alpha_3$ which captures the average treatment effect (ATE) on consumption. As in Stephens Jr (2003), we include day of the week, $DOW$, and day of the year, $DOY$, fixed effects to ensure that our results are not biased by particular dates (such as Christmas) or certain days of the week (as elections are not always held on a specific weekday). Importantly, the specification includes household-specific fixed effects. The former control for household specific attributes like income, family size, number of adults, etc. $\epsilon$ is the error term clustered at the level of households. Under the

[34] We have chosen 14 days after the election as the cut-off as this will give the households a couple of weeks to spend the extra income they may get prior to the election.

parallel trends assumption that the expenditures during the days where we suspect the income from vote buying is spent would have been the same for the treated households as the consumption expenditures of the control households over the corresponding days in non-election years, we can interpret $\alpha_3$ as an unbiased average treatment effect (ATE). The outcome variable, $Y$, is daily expenditures on various consumption goods, measured in Armenian Dram (AMD). We focus on total food consumption and the eight food categories listed in Table 3.[35] We observe that at the median food is about 13% of household income but that this share varies substantially across the income distribution.

### 4.4.3 Results

We present the results in a sequence of diagrams. Figure 5(a) plots the average responses of total food purchases to the election treatment and Figure 5(b) plots the responses separately for the four income quartiles. In each diagram, the horizontal axis records different values of $\delta$ between 4 and 40 in bins of two days. For each value of $\delta$, the point estimate of $\alpha_3$ from equation (2), which captures the average treatment effect of elections on consumption, is indicated with a circle and the bars indicate 95% confidence intervals. The election day is denoted with a vertical line (at $\delta = 14$). To understand how to read the diagram, take, for example, a value of $\delta = 20$. The corresponding point estimate represents a scenario where the before treatment period starts 26 days and ends 6 days before the election, and the treatment period starts 6 days before and ends 14 days after the election.

Figure 5(a) shows that total food consumption increases significantly around elections. The largest effect is for a treatment period of 6 days before and 14 days after the election ($\delta = 20$) where daily consumption of food increases by 100 AMD, which corresponds to a 7% increase, or a total of 2000 AMD (for the 20 days). The smallest (significant) treatment effect (for $\delta = 14$) is 50 AMD per day or 700 AMD over the 14 days in this

[35] We study food consumption because these items are purchased at a high frequency and, therefore, recorded in the daily diaries for most days.

**Table 3:** Monthly budget and expenditure shares, food consumption

| | Full sample | | | Control group | | | Treatment group | | |
|---|---|---|---|---|---|---|---|---|---|
| | Median | 10%-ile | 90%-ile | Median | 10%-ile | 90%-ile | Median | 10%-ile | 90%-ile |
| Budget share of food | 12.7% | 4.5% | 56% | 13.3% | 4.5% | 58.3% | 13.7% | 4.7% | 72.6% |
| Expenditure shares: | | | | | | | | | |
| Starch products | 28,4% | 8,8% | 52,9% | 28,9% | 8,1% | 54,7% | 27,0% | 7,2% | 53,2% |
| Meat and fish products | 19,6% | 4,0% | 38,0% | 21,5% | 4,3% | 40,9% | 22,8% | 5,6% | 41,7% |
| Dairy products | 8,1% | 0,0% | 17,8% | 7,5% | 0,0% | 17,5% | 7,3% | 0,0% | 17,7% |
| Fruits and vegetables | 12,9% | 0,8% | 27,9% | 9,4% | 0,0% | 22,2% | 9,1% | 0,0% | 22,9% |
| Sugar and confectionery | 6,1% | 1,1% | 16,0% | 6,4% | 1,2% | 16,0% | 6,5% | 1,3% | 15,9% |
| Other food products | 14,8% | 6,7% | 27,5% | 15,4% | 6,7% | 29,3% | 15,3% | 6,4% | 29,4% |
| Non-alcoholic drinks | 0,0% | 0,0% | 3,2% | 0,0% | 0,0% | 3,6% | 0,0% | 0,0% | 4,3% |
| Alcoholic drinks | 0,0% | 0,0% | 4,5% | 0,0% | 0,0% | 6,5% | 0,0% | 0,0% | 7,2% |

Note: The control and treatment groups are defined for a time window of $\delta = 40$ days. The summary statistics are different but not substantially so for other values of $\delta$. We report the budget share of food (total food expenditure in percentage of income) and expenditure shares (expenditure on an item in percentage of total consumption expenditure) at the median, tenth and ninetieth percentiles of the household income distribution.

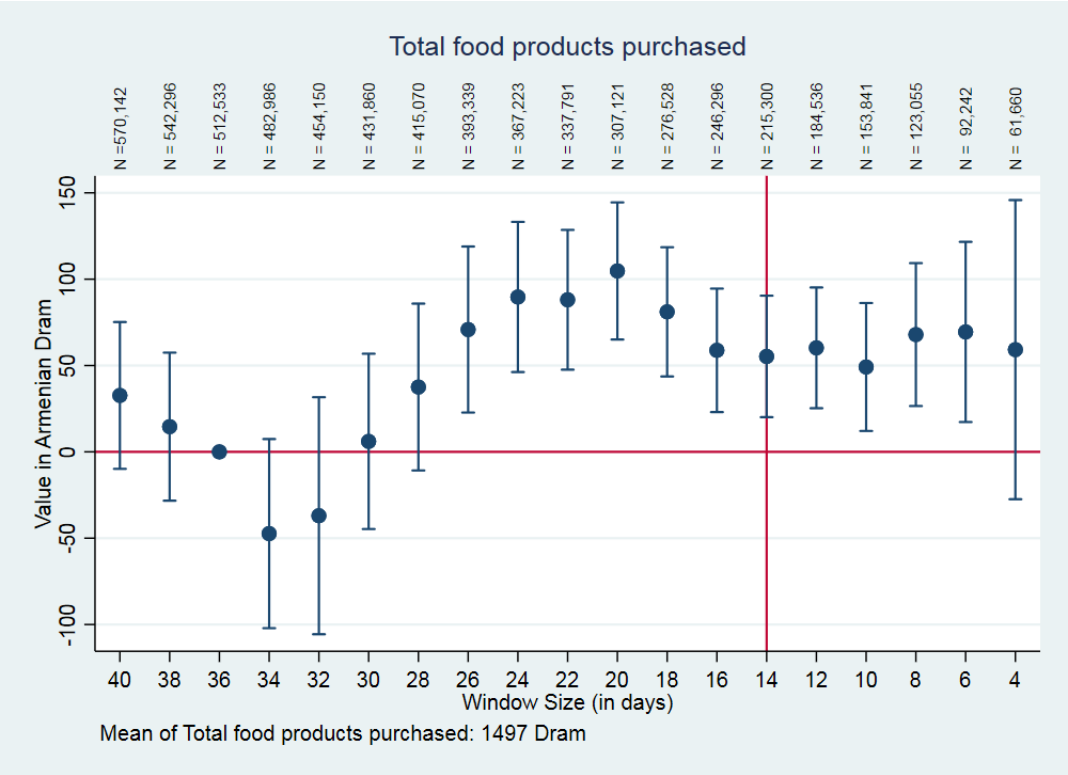
window. These effects are substantial compared to average food purchases of 1,497 AMD per day and the size of the effect is not very different to that reported by Mitra et al. (2017) for Indian elections.[36]

Figure 5(b), which consists of four sub-diagrams, one for each income quartile, shows that the consumption response to the election treatment is concentrated among households in the second quartile of the income distribution, i.e., towards the bottom of the income distribution but not among the very poor. Responses for households in the other quartiles are positive prior to elections but generally not statistically different from zero

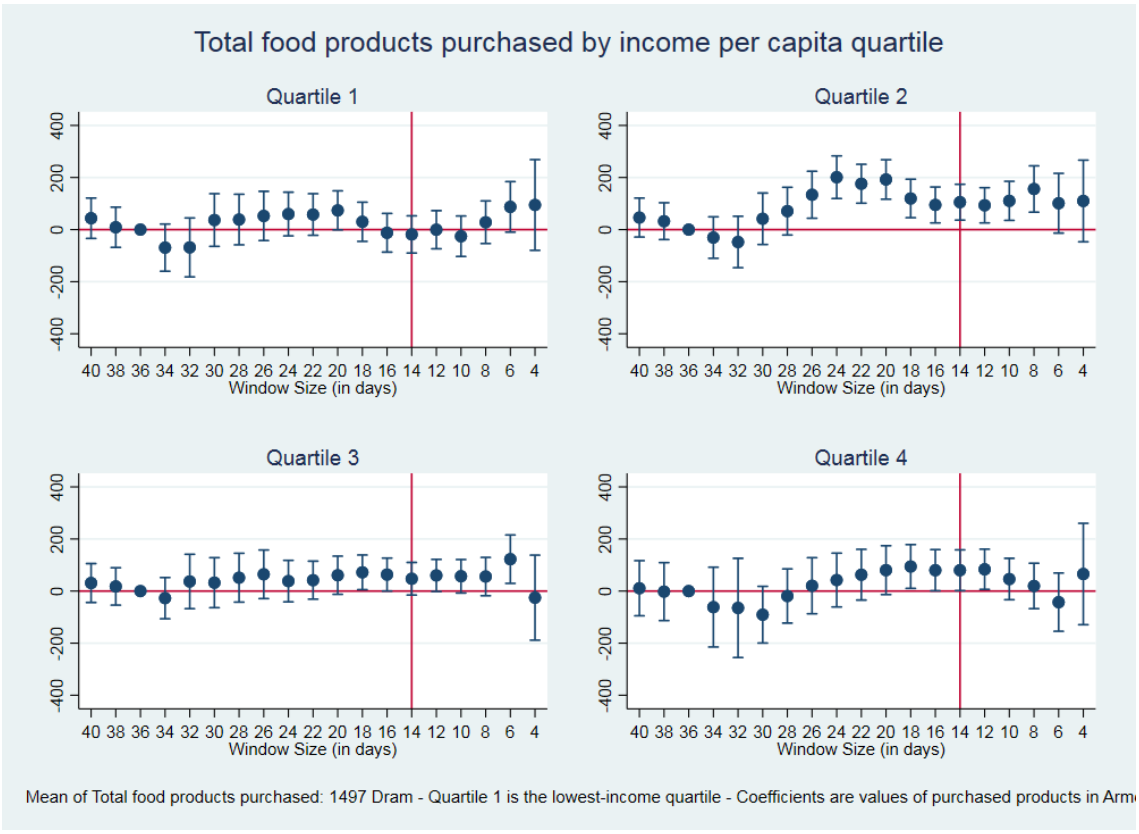
[36] They report a 10% increase in spending on pulses prior to Indian elections.

**Figure 5:** Estimates of the ATE for total food expenditures, aggregated and by income quartile

**(a)** Average responses



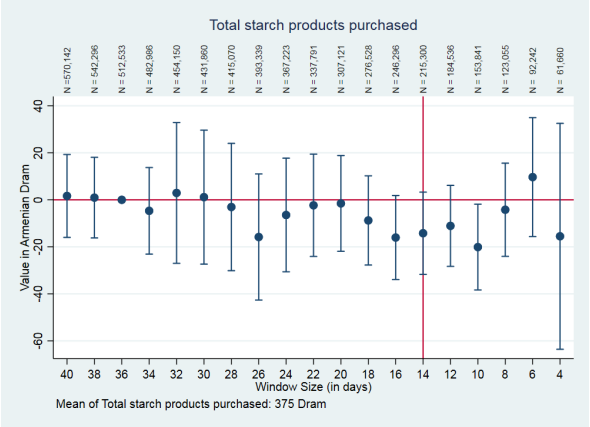

**(b)** Responses by income quartile



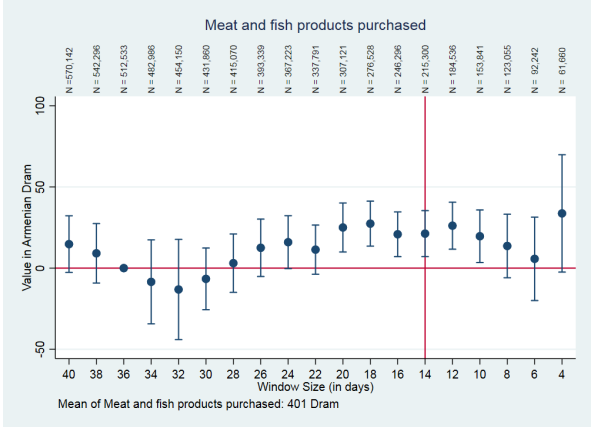

Note: The diagrams plot the point estimates (the dots) of the average treatment effect (ATE) ($\alpha_3$) from equation 2 for different values of $\delta$ (treatment windows) as recorded on the x-axis. The bars are 95% confidence intervals.

**Figure 6:** Estimates of the ATE for eight food-related consumption items

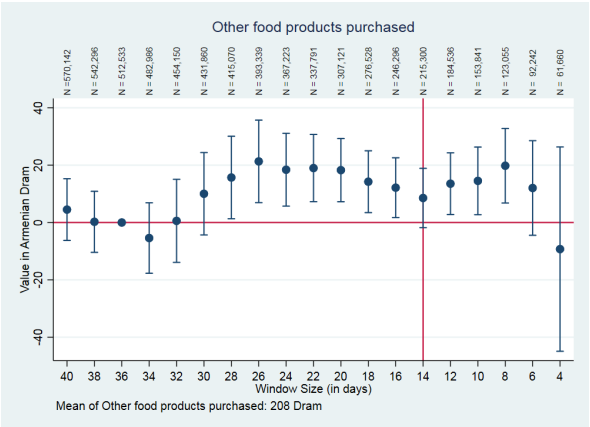
**(a)** Starch products



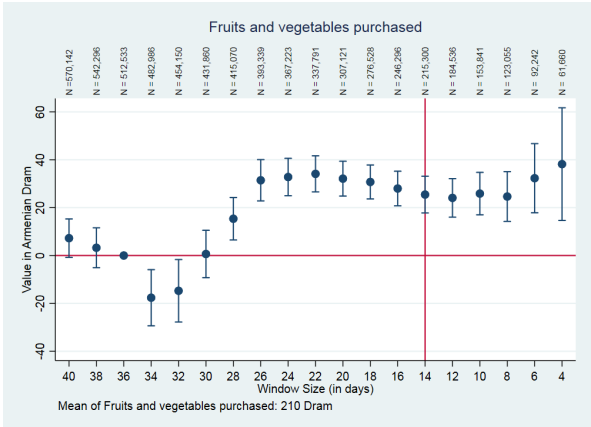

**(b)** Meat and fish



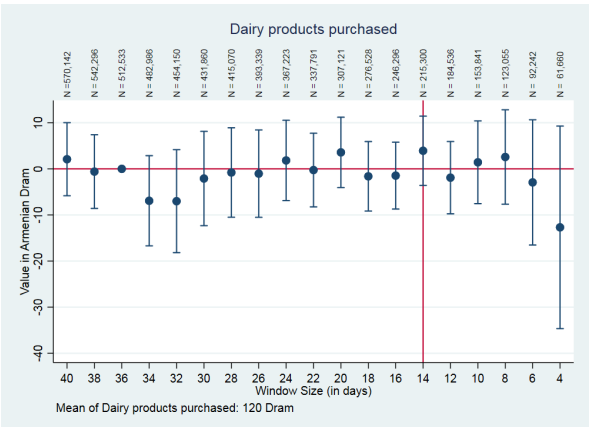

**(c)** Other food



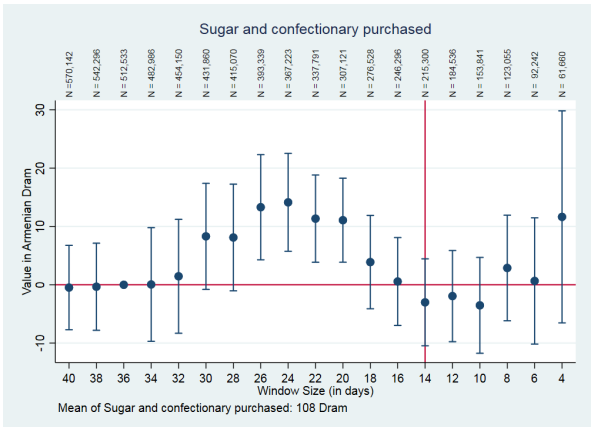

**(d)** Fruits and vegetables



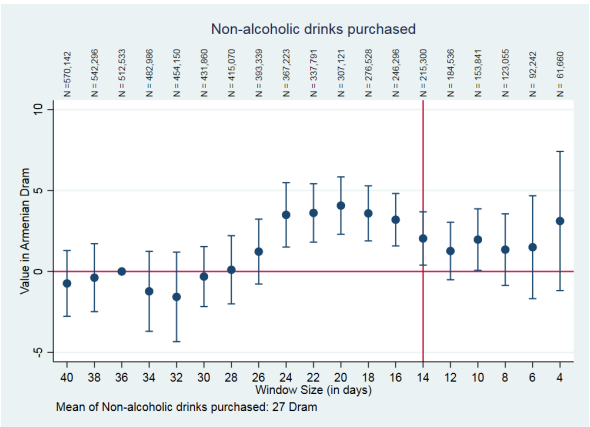

**(e)** Dairy products



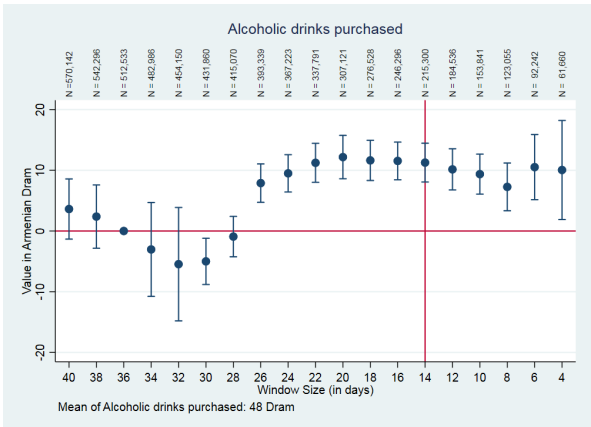

**(f)** Sugar and confectionery




**(g)** Non-alcoholic drinks




**(h)** Alcoholic drink




Note: See the notes to Figure 5

**Table 4:** Summary of ATE estimates by item and income quartile

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Is there a significant effect? Which direction? | | | | |
| | Average | Quartile I | Quartile II | Quartile III | Quartile IV |
| Total food purchases | + | | + | | |
| Starch products | | - | + | | |
| Meat and fish | + | | + | + | - |
| Other food | + | | | | + |
| Fruits and vegetables | + | + | + | + | + |
| Dairy products | | | | - | |
| Sugar and confectionery | + | + | + | | |
| Non-alcoholic drinks | + | + | + | | |
| Alcoholic drink | + | + | + | + | - |

Note: The Table summarizes the results of estimating equation (2) for eight consumption categories on average (column 1) and by household income quartile (columns 2-5). The results are based on graphs reported in Figures 5, 6 and A1 (in the supplementary material). A + indicates a significant increase and a − indicates a significant decrease in at least one day within the treatment window around the election.

(at the 5% level). For households in the second quartile, the treatment effect is 200 AMD, i.e., twice as large as the average effect.

Figure 6 dis-aggregates food consumption into eight categories and shows their responses to the election treatment. The diagrams show that household consumption increases around the election in all but two categories (starch and dairy products). The strongest responses are found in the consumption of fruits and vegetables and alcohol which increase by, respectively, around 15% and 20%. In the supplementary material (Figure A1), we report the corresponding results dis-aggregated by income quartile and Table 4 provides a summary. Again, we observe that the ATE is positive and significant among households at the bottom of the income distribution, with the exception of fruits and vegetables and other food where the top quartiles are also affected.

In summary, the average Armenian household experiences an abnormal increase in consumption around elections. The effect is between 700 and 2,000 AMD and, for most consumption items, the effect is concentrated among households towards the bottom of the income distribution. Vote buying is a potential source of the income needed to fund this extra consumption. In supplementary appendix D, we show how the micro evidence from Armenia can be reconciled with the macro evidence on the election date effect.

# 5 Conclusions

This paper offers a new perspective on the monetary effects of elections by studying monthly data on M1. We report robust evidence of a systematic monetary expansion during the election and post-election months in a sample of up to 104 non-OECD countries between 1975 and 2015. The expansion amounts to about $1/13th$ of a standard deviation in the month-to-month growth rate of M1. We cannot find a similar effect in mature OECD democracies. This stylized fact is new to the literature on monetary political business cycles.

We propose the vote buying hypothesis to explain this short-run monetary cycle. Large-scale, systematic vote buying creates a spike in the demand for money timed around the election, which through conversion of broader monetary instruments (from M2 or M3 to M1) or through black economy returns to the banking system leading to an endogenous expansion of M1 around elections. Although we cannot provide conclusive proof that this is the mechanism behind the observed cycle in M1, we present comprehensive evidence that bolster the credibility of the hypothesis. We find that the cycle is most pronounced in elections which are reported by independent election monitors to be affected by vote buying and other irregularities and absent in elections that are assessed to be free and fair. Moreover, the election-date effect in monetary expansion is stronger in close elections where political competition is intense and, hence, vote buying particularly rewarding for candidates. We also present micro-econometric evidence from Armenia of an increase in

consumption around elections. The magnitude of this increase could have been funded by income from selling votes and is also similar to the size of the monetary expansion.

Our findings complement 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.

# References


Aaronson, D., S. Agarwal, and E. French (2012). The spending and debt response to minimum wage hikes. *American Economic Review 102*(7), 3111–3139.

Agarwal, S., N. Marwell, and L. McGranahan (2017). Consumption responses to temporary tax incentives: Evidence from state sales tax holidays. *American Economic Journal: Economic Policy 9*(4), 1–27.

Aidt, T. S. (2003). Economic analysis of corruption: a survey. *The Economic Journal 113*(491), 632–652.

Aidt, T. S. and P. S. Jensen (2017). From open to secret ballot: Vote buying and modernization. *Comparative Political Studies 50*, 555–93.

Aidt, T. S. and G. Mooney (2014). Voting suffrage and the political budget cycle: Evidence from the London Metropolitan Boroughs 1902-1937. *Journal of Public Economics 112*, 53–71.

Akhmedov, A. and E. Zhuravskaya (2004). Opportunistic political cycles: Test in a young democracy setting. *The Quarterly Journal of Economics 119*(4), 1301–1338.

Alesina, A., G. D. Cohen, and N. Roubini (1992). Macroeconomic policy and elections in OECD democracies. *Economics and Politics 9*(4), 1–31.

Alesina, A., G. D. Cohen, and N. Roubini (1993). Electoral business cycle in industrial democracies. *European Journal of Political Economy 9*(1), 1–23.

Alesina, A., N. Roubini, and G. D. Cohen (1997). *Political Cycles and the Macroeconomy*. Cambridge, MA: MIT Press.

Alpanda, S. and A. Honig (2009). The impact of central bank independence on political monetary cycles in advanced and developing nations. *Journal of Money, Credit and Banking 41*(7), 1365–1389.

Alpanda, S. and A. Honig (2010). Political monetary cycles and a de facto ranking of central bank independence. *Journal of International Money and Finance 29*(6), 1003–1023.

Aravot Daily (2012). Several criminal cases opened by the Prosecutor General's office. Retrieved 28 august 2015 from http://www.aravot.am/2012/05/10/297037.

Beck, T., G. Clarke, A. Groff, P. Keefer, and P. Walsh (2001). New tools in comparative political economy: The database of political institutions. *World Bank Economic Review 15*(1), 165–176.

Berger, H., J. De Haan, and S. Eijffinger (2001). Central bank independence: An update of theory and evidence. *Journal of Economic Surveys 15*(1), 3–40.

Bodea, C. and R. Hicks (2015). Price stability and central bank independence: Discipline, credibility, and democratic institutions. *International Organization 69*(01), 35–61.

Brender, A. and A. Drazen (2005). Political budget cycles in new versus established democracies. *Journal of Monetary Economics 52*(7), 1271–1295.

Center for Systematic Peace (2015). Polity IV dataset. Retrieved from http://www.systemicpeace.org/.

Cox, G. W. and M. J. Kousser (1981). Turnout and rural corruption: New York as a test. *American political science review 25*(4), 646–663.

De Haan, J. and J. Klomp (2013). Conditional political budget cycles: A review of recent evidence. *Public Choice 157*(3-4), 387–410.

Delta Stock (2018). Central Bank Interest Rates. Retrieved 01 May 2018 from https://www.deltastock.com/, .

Dixit, A. and J. Londregan (1996). The determinants of success of special interests in redistributive politics. *Journal of Politics 58*(4), 1132–1155.

Drazen, A. (2001). The political business cycle. *NBER Macroeconomics Annual 2000 15*, 75–138.

Dreher, A. and R. Vaubel (2009). Foreign exchange intervention and the political business cycle: A panel data analysis. *Journal of International Money and Finance 28*(5), 755–775.

Finan, F. and L. Schechter (2012). Vote-buying and reciprocity. *Econometrica 80*(1), 863–881.

Franzese, R. J. (2000). Electoral and partisan manipulation of public debt in developed democracies 1956−1990. In R. Strauch and J. von Hagen (Eds.), *Institutions, Politics and Fiscal Policy*, pp. 61–83. Dordrecht, Netherlands: Kluwer Academic Press.

Freedom House (2012). Freedom in the world. Report, Freedom House.

Fuchs-Schuendeln, N. and T. A. Hassan (2016). Natural experiments in macroeconomics. *Handbook of Macroeconomics II*, 923–1012.

Gillitzer, C. and N. Prasad (2018). The effect of consumer sentiment on consumption: Cross-sectional evidence from elections. *American Economic Journal: Macroeconomics 10*(4), 234–269.

Hanusch, M. and P. Keefer (2014). Younger parties, bigger spenders? Party age and political budget cycles. *European Economic Review 72*, 1–18. cited By 0.

Heckelman, J. C. and J. H. Wood (2005). Political monetary cycles under alternative institutions: the independent Treasury and the Federal Reserve. *Economics and Politics 17*(3), 331–350.

Heckelman, J. C. and A. J. Yates (2002). Incumbency preservation through electoral legislation: The case of the secret ballot. *Economics of Governance 3*(1), 47–57.

Hicken, A. (2011). Clientelism. *Annual Review of Political Science 14*, 289–310.

Hicken, A., S. Leider, N. Ravanilla, and D. Yang (2015). Measuring vote-selling: Field evidence from the Philippines. *American Economic Review: Papers and Proceedings 105*(5), 352–356.

Hidalgo, F. and S. Nichter (2015). Voter buying: Shaping the electorate through clientelism. *American Journal of Political Science*. cited By 0; Article in Press.

Institute for War and Peace Reporting (2012). Sweeteners for votes in Armenia. Retrieved 28 august 2015 from https://iwpr.net/global-voices/sweeteners-votes-armenia.

International Foundation for Electoral Systems (2015). Electionguide. Retrieved 28 august 2015 from http://www.ifes.org/.

International Monetary Fund (2018). Monetary and Financial Statistics. Retrieved 01 May 2018 from https://fred.stlouisfed.org/, .

Jappelli, T. and L. Pistaferri (2010). The consumption response to income changes. *Annual Review of Economics 2*(1), 479–506.

Johnson, D. S., J. A. Parker, and N. S. Souleles (2006). Household expenditure and the income tax rebates of 2001. *American Economic Review 96*(5), 1589–1610.

Kapur, D. and M. Vaishnav (2013). Quid pro quo: Builders, politicians, and election finance in India. CGD Working Paper 276 276, Washington, D.C.: Center for Global Development.

Keefer, P. and R. Vlaicu (2008). Democracy, credibility, and clientelism. *Journal of Law, Economics, and Organization 24*(2), 371–406.

Kelley, J. G. (2012). *Monitoring democracy: When international election observation works, and why it often fails*. Princeton University Press.

Klose, J. (2012). Political business cycles and monetary policy revisited-an application of a two-dimensional asymmetric Taylor reaction function. *International Economics and Economic Policy 9*(3), 265–295.

Labonne, J. (2016). Local political business cycles: Evidence from Philippine municipalities. *Journal of Development Economics 104*, 73–88.

MacRae, D. C. (1977). A political model of the business cycle. *Journal of Political Economy 82*(2), 239–263.

Mian, A. and A. Sufi (2012). The effects of fiscal stimulus: Evidence from the 2009 cash for clunkers program. *The Quarterly journal of economics 127*(3), 1107–1142.

Mitra, A., S. Mitra, and A. Mukherji (2017). Cash for votes: Evidence from India. Technical report, School of Economics Discussion Papers.

National Statistical Service of the Republic of Armenia (2018). Integrated Survey of Living Standards Annual survey waves from 2001 to 2016. Retrieved 01 May 2018 from http://microdata.worldbank.org/index.php/catalog.

Nichter, S. (2014). Conceptualizing vote buying. *Electoral Studies 35*, 315–327.

Nordhaus, W. D. (1975). The political business cycle. *The Review of Economic Studies 42*(2), 169–190.

Olken, B. A. and R. Pande (2012). Corruption in developing countries. *Annual Review of Economics 4*(1), 479–509.

Paldam, M. (1997). Political business cycles. In D. C. Mueller (Ed.), *Perspectives on Public Choice. A Handbook*, pp. 342–372. Cambridge, United Kingdom: Cambridge University Press.

Pande, R. (2008). Understanding political corruption in low income countries. In T. Schultz and J. Strauss (Eds.), *Handbook of development economics, volume 4*, pp. 3155–3184. North Holland.

Parker, J. A. (1999). The reaction of household consumption to predictable changes in social security taxes. *American Economic Review 89*(4), 959–973.

Parker, J. A., N. S. Souleles, D. S. Johnson, and R. McClelland (2013). Consumer spending and the economic stimulus payments of 2008. *American Economic Review 103*(6), 2530–2553.

Persson, T. and G. E. Tabellini (1990). *Macroeconomic poklicy, credibility and politics*. Chur, Switzerland: Harwood Academic Publishers.

Potrafke, N. (2018). Perceived corruption, elections, political manipulation, panel data, democracies, political business cycles. *Journal of Comparative Economics, In Press*.

Shefter, M. (1977). *Patronage and its opponents: A theory and some European cases*. Cornell University Press.

Shi, M. and J. Svensson (2006). Political budget cycles: Do they differ across countries and why. *Journal of Public Economics 90*(8), 1367–1389.

Stephens, M. and T. Unayama (2011). The consumption response to seasonal income: Evidence from Japanese public pension benefits. *American Economic Journal: Applied Economics 3*(4), 86–118.

Stephens Jr, M. (2003). " 3rd of tha month": Do social security recipients smooth consumption between checks? *American Economic Review 93*(1), 406–422.

Stokes, C. S. (2005). Perverse accountability: A formal model of machine politics with evidence from Argentina. *American Political Science Review 99*(3), 325–325.

Stokes, C. S., T. Dunning, M. Nazareno, and V. Brusco (2013). *Brokers, Voters, and Clientelism: The puzzle of distributive politics*. Cambridge University Press.

Sukhtankar, S. (2012, July). Sweetening the deal? Political connections and sugar mills in India. *American Economic Journal: Applied Economics 4*(3), 43–63.

US Department of State (2012). Armenia 2012 human rights report. Retrieved 28 august 2015 from http://www.state.gov/documents/organization/204468.pdf.

van Ham, C. and S. I. Lindberg (2015). Vote buying is a good sign: Alternate tactics of fraud in Africa 1986-2012. Technical report, The varieties of democracy Institute, Working Paper 2015-3.

Vicente, C. P. (2014). Is vote buying effective? Evidence from a field experiment in West Africa. *Economic Journal 124*(574), 356–387.

Wantchekon, L. (2003). Clientelism and voting behavior: Evidence from a field experiment in Benin. *World Politics 55*, 399–422.

World Development Indicators (2014). The World Bank: Data on world development indicators. Retrieved from http://data.worldbank.org/data-catalog/world-development-indicators.

Zitzewitz, E. (2012). Forensic economics. *Journal of Economic Literature 50*(3), 731–769.

# Online Appendix for:

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

**by:**

Toke Aidt  Zareh Asatryan  Lusine Badalyan  Friedrich Heinemann

**A: Additional Tables and Figures**

**B: Survey and Case Study Evidence**

**C: The Monetary Mechanisms**

**D: Reconciling the micro and macro evidence**

**Table A1:** Sample of countries

| OECD (as of 2009) | | Non-OECD | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| *No* | Country | *No* | Country | *No* | Country | *No* | Country | *No* | Country |
| No | Country | No | Country | No | Country | No | Country | No | Country |
| 1 | Australia | 1 | Afghanistan, IR | 27 | Congo, Rep | 53 | Kyrgyz Republic | 79 | Romania |
| 2 | Austria | 2 | Albania | 28 | Croatia | 54 | Latvia | 80 | Russian Federation |
| 3 | Canada | 3 | Algeria | 29 | Cyprus | 55 | Lebanon | 81 | Rwanda |
| 4 | Czech Republic | 4 | Angola | 30 | Cote d'Ivoire | 56 | Lesotho | 82 | Saudi Arabia |
| 5 | Denmark | 5 | Argentina | 31 | Dominican Rep | 57 | Liberia | 83 | Senegal |
| 6 | France | 6 | Armenia, Rep | 32 | Egypt | 58 | Libya | 84 | Serbia, Republic of |
| 7 | Hungary | 7 | Bahrain, Kingdom | 33 | Equatorial Guinea | 59 | Lithuania | 85 | Sierra Leone |
| 8 | Japan | 8 | Bangladesh | 34 | Estonia | 60 | Macedonia, FYR | 86 | Singapore |
| 9 | Korea, Republic of | 9 | Belarus | 35 | Ethiopia | 61 | Malawi | 87 | Slovenia |
| 10 | Mexico | 10 | Benin | 36 | Gabon | 62 | Malaysia | 88 | Solomon Islands |
| 11 | Poland | 11 | Bhutan | 37 | Gambia, The | 63 | Mali | 89 | South Africa |
| 12 | Portugal | 12 | Bolivia | 38 | Ghana | 64 | Mauritius | 90 | Sri Lanka |
| 13 | Slovak Republic | 13 | Botswana | 39 | Guatemala | 65 | Moldova | 91 | Sudan |
| 14 | Spain | 14 | Bulgaria | 40 | Guinea-Bissau | 66 | Mongolia | 92 | Suriname |
| 15 | Sweden | 15 | Burkina Faso | 41 | Guyana | 67 | Morocco | 93 | Swaziland |
| 16 | Switzerland | 16 | Burundi | 42 | Haiti | 68 | Mozambique | 94 | Tajikistan |
| 17 | United States | 17 | Cabo Verde | 43 | Honduras | 69 | Namibia | 95 | Tanzania |
| | | 18 | Cambodia | 44 | India | 70 | Nepal | 96 | Thailand |
| | | 19 | Cameroon | 45 | Indonesia | 71 | New Zealand | 97 | Togo |
| | | 20 | Central African Republic | 46 | Iraq | 72 | Nicaragua | 98 | Turkey |
| | | 21 | Chad | 47 | Israel | 73 | Niger | 99 | Uganda |
| | | 22 | Chile | 48 | Jamaica | 74 | Nigeria | 100 | Ukraine |
| | | 23 | China, P.R.: Mainland | 49 | Jordan | 75 | Oman | 101 | United Arab Emirates |
| | | 24 | Colombia | 50 | Kazakhstan | 76 | Pakistan | 102 | Uruguay |
| | | 25 | Comoros | 51 | Kenya | 77 | Paraguay | 103 | Venezuela, Rep |
| | | 26 | Congo, DR | 52 | Kuwait | 78 | Qatar | 104 | Zambia |

**Table A2:** Summary statistics

| Variable | Obs | Mean | Std. Dev. | Min | Max | Source |
|---|---|---|---|---|---|---|
| M1 growth | 35.310 | 0,0124 | 0,0405 | -0,1104 | 0,1901 | IMF |
| M1 seasonally adjusted growth | 33.202 | 0,0123 | 0,1922 | -29,6378 | 12,4582 | IMF |
| M2 growth | 33.442 | 0,0123 | 0,0291 | -0,2228 | 1,2474 | IMF |
| M3 growth | 17.532 | 0,0144 | 0,1556 | -0,9009 | 9,1149 | IMF |
| M1/(M2-M1) | 33.443 | 1,7347 | 20,5265 | -3643,0040 | 173,7598 | IMF |
| Election month dummy | 29.877 | 0,0240 | 0,1530 | 0,0000 | 1,0000 | DPI |
| Legislative election dummy | 29.877 | 0,0185 | 0,1348 | 0,0000 | 1,0000 | DPI |
| Executive election dummy | 29.877 | 0,0091 | 0,0948 | 0,0000 | 1,0000 | DPI |
| Election day | 22.608 | 0,0138 | 0,0954 | 0,0000 | 1,0000 | IFES |
| GDP per capita growth (annual %) | 33.746 | 2,5501 | 5,2310 | -62,2251 | 122,9683 | WDI |
| Ln of GDP per capita (constant 2010 US$) | 33.787 | 8,4530 | 1,4298 | 5,1818 | 11,2474 | WDI |
| Inflation, annual % | 32.952 | 8,6896 | 24,6436 | -35,8367 | 1058,3740 | WDI |
| Ln of exchange rate to USD | 35.224 | 2,9074 | 2,5455 | -7,4320 | 9,5747 | WDI |
| Resource rents in % of GDP | 33.500 | 6,9733 | 11,0953 | 0,0000 | 64,1110 | WDI |
| Polity IV score of democracy | 29.100 | 0,7031 | 0,3081 | 0,0000 | 1,0000 | DPI |
| New democracy dummy | 29.100 | 0,1175 | 0,3220 | 0,0000 | 1,0000 | DPI |
| Vote share president, % | 13.757 | 56,4308 | 19,6777 | 19,6000 | 100,0000 | DPI |
| Vote share government party, % | 18.049 | 53,9021 | 16,0350 | 9,2000 | 100,0000 | DPI |
| Treasury bill rate | 16.066 | 8,6905 | 10,0938 | -1,3000 | 193,7100 | IMF |
| Treasury bill rate growth | 15.750 | 0,0197 | 0,5554 | -28,8118 | 30,3405 | IMF |
| CB refinancing rate | 4.228 | 1,0909 | 2,5627 | -0,0050 | 17,5000 | DS |
| Central bank independence | 20.877 | 0,5348 | 0,2033 | 0,1006 | 0,9628 | BH |
| Overall election quality (OSCE) | 5.509 | 0,0016 | 0,0381 | 0,0000 | 1,0000 | DIEM |
| Election day cheating (mean reports) | 6.938 | 0,0036 | 0,0438 | 0,0000 | 1,0000 | DIEM |
| Election day cheating (max reports) | 6.938 | 0,0053 | 0,0625 | 0,0000 | 1,0000 | DIEM |
| Electionday cheating (US State Dep) | 12.528 | 0,0029 | 0,0426 | 0,0000 | 1,0000 | QED |

Notes on data sources: IMF is International Monetary Fund (2018), DPI is Database of Political Institutions (Beck et al. 2001), WDI is World Development Indicators (2014), IFES is International Foundation for Electoral Systems (2015), DS is Delta Stock (2018), BH is Bodea and Hicks (2015), DIEM and QED are Kelley (2012).

**Table A3:** Growth of M1 around elections in OECD and Non-OECD countries

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| VARIABLES | | | | | | $\Delta lnM1$ | | | | | | |
| SAMPLE | *Non-OECD* | | | | | | *OECD* | | | | | |
| Election month | 0.0062*** | 0.0062*** | | | | | -0.0017 | -0.0017 | | | | |
| | (0.0021) | (0.0021) | | | | | (0.0020) | (0.0020) | | | | |
| Election (t-2) | 0.0007 | | 0.0007 | | | | -0.0021 | | -0.0021 | | | |
| | (0.0025) | | (0.0025) | | | | (0.0035) | | (0.0035) | | | |
| Election (t-1) | -0.0035 | | | -0.0034 | | | 0.0002 | | | 0.0002 | | |
| | (0.0025) | | | (0.0025) | | | (0.0021) | | | (0.0021) | | |
| Election (t+1) | 0.0079*** | | | | 0.0080*** | | 0.0019 | | | | 0.0019 | |
| | (0.0020) | | | | (0.0020) | | (0.0022) | | | | (0.0023) | |
| Election (t+2) | -0.0001 | | | | | -0.0001 | 0.0001 | | | | | 0.0001 |
| | (0.0015) | | | | | (0.0015) | (0.0022) | | | | | (0.0022) |
| Observations | 15,864 | 15,864 | 15,864 | 15,864 | 15,864 | 15,864 | 5,205 | 5,205 | 5,205 | 5,205 | 5,205 | 5,205 |
| R-squared | 0.1357 | 0.1346 | 0.1341 | 0.1342 | 0.1350 | 0.1341 | 0.2739 | 0.2737 | 0.2737 | 0.2736 | 0.2737 | 0.2736 |
| Countries | 98 | 98 | 98 | 98 | 98 | 98 | 17 | 17 | 17 | 17 | 17 | 17 |

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

Notes: The variables *Election (t+i)* for $i = -2, -1, 1, 2$ are coded one in month $i$ around the election. All regressions control for GDP growth, GDP p.c., inflation, the exchange rate, resource rents, polity index of democracy, new democracy dummy; include three lags of the dependent variable; and country, year and month fixed effects. The sample in columns (1) to (6) is the same as in Table 1, column (5) adjusted for the loss of observations induced by the introduction of the two lags and leads.

**Table A4:** Heterogenity in the election date effect: Legislative and executive elections

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| VARIABLES | $\Delta lnM1$ | | | | | | | |
| SAMPLE | *Non-OECD* | | | | *OECD* | | | |
| Election month | 0.0061*** | | | | -0.0016 | | | |
| | (0.0021) | | | | (0.0020) | | | |
| Legislative election | | 0.0018 | | | | 0.0003 | | |
| | | (0.0023) | | | | (0.0022) | | |
| Executive election | | | 0.0074* | | | | -0.0065 | |
| | | | (0.0039) | | | | (0.0049) | |
| Legislative & executive elections | | | | 0.0160*** | | | | -0.0094 |
| | | | | (0.0047) | | | | (0.0064) |
| Observations | 16,034 | 16,034 | 16,034 | 16,034 | 5,227 | 5,227 | 5,227 | 5,227 |
| R-squared | 0.1353 | 0.1348 | 0.1350 | 0.1354 | 0.2723 | 0.2722 | 0.2724 | 0.2725 |
| Countries | 98 | 98 | 98 | 98 | 17 | 17 | 17 | 17 |

*** 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, polity index of democracy, new democracy dummy; 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. The sample in columns (1) to (4) is the same as in Table 1, column (5). *Legislative election* is coded one in months with a legislative election only and zero otherwise; *Executive election* is coded one in months with an excecutive election only; and *Legislative & executive elections* is coded one in months with both an excecutive and a legislative election.

**Table A5:** Growth of M1 during elections: dynamics, outliers and variations in the definition of OECD countries

| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | $\Delta lnM1$ | | | | | | | | | | | | | |
| Sample | Non-OECD as of | | | | | | | | | | | OECD as of | | |
| | 2009 | | | | Outliers trimmed at: | | 1975 | 2017 | 2009 | | | 1975 | 2009 | 2017 |
| | | | | Bootstrap | 1% | 5% | | | | GMM | | | | |
| Election month | 0.0045** | 0.0052*** | 0.0054*** | 0.0054*** | 0.0052*** | 0.0041*** | 0.0042** | 0.0048** | 0.0056*** | 0.0053*** | 0.0054*** | 0.0014 | -0.0016 | 0.0015 |
| | (0.0018) | (0.0019) | (0.0019) | (0.0019) | (0.0019) | (0.0014) | (0.0018) | (0.0019) | (0.0018) | (0.0018) | (0.0018) | (0.0021) | (0.0020) | (0.0024) |
| M1 growth (t-1) | | -0.0961*** | -0.1045*** | -0.1045*** | -0.0879*** | -0.0453*** | -0.1034*** | -0.1118*** | -0.1086*** | -0.1005*** | -0.1060*** | -0.0005 | -0.0006 | -0.0005 |
| | | (0.0149) | (0.0163) | (0.0161) | (0.0144) | (0.0098) | (0.0159) | (0.0164) | (0.0154) | (0.0058) | (0.0059) | (0.0009) | (0.0010) | (0.0010) |
| M1 growth (t-2) | | | -0.0202*** | -0.0202** | -0.0168*** | -0.0067** | -0.0201*** | -0.0215*** | -0.0094 | | -0.0188*** | 0.0005 | 0.0007 | 0.0007 |
| | | | (0.0063) | (0.0087) | (0.0057) | (0.0033) | (0.0062) | (0.0067) | (0.0104) | | (0.0041) | (0.0006) | (0.0006) | (0.0006) |
| M1 growth (t-3) | | | -0.0060 | -0.0060 | -0.0012 | 0.0037 | -0.0039 | -0.0083 | -0.0076 | | -0.0053 | 0.0031 | 0.0038* | 0.0040* |
| | | | (0.0076) | (0.0088) | (0.0065) | (0.0048) | (0.0075) | (0.0078) | (0.0084) | | (0.0047) | (0.0019) | (0.0022) | (0.0024) |
| M1 growth (t-4) | | | | | | | | | -0.0224** | | | | | |
| | | | | | | | | | (0.0104) | | | | | |
| M1 growth (t-5) | | | | | | | | | -0.0288*** | | | | | |
| | | | | | | | | | (0.0097) | | | | | |
| M1 growth (t-6) | | | | | | | | | -0.0066 | | | | | |
| | | | | | | | | | (0.0103) | | | | | |
| GDP pc (log) | -0.0101*** | -0.0115*** | -0.0121*** | -0.0121*** | -0.0096*** | -0.0071*** | -0.0132*** | -0.0114*** | -0.0127*** | -0.0122*** | -0.0124*** | -0.0130 | -0.0069*** | -0.0061** |
| | (0.0027) | (0.0030) | (0.0031) | (0.0036) | (0.0026) | (0.0025) | (0.0028) | (0.0031) | (0.0032) | (0.0025) | (0.0025) | (0.0083) | (0.0020) | (0.0026) |
| GDP pc growth | 0.0004*** | 0.0004*** | 0.0004*** | 0.0004*** | 0.0004*** | 0.0003*** | 0.0005*** | 0.0004*** | 0.0005*** | 0.0004*** | 0.0005*** | 0.0001 | 0.0003* | 0.0006*** |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0002) | (0.0002) | (0.0002) |
| Inflation | 0.0001*** | 0.0001*** | 0.0001*** | 0.0001 | 0.0001*** | 0.0001*** | 0.0001*** | 0.0001*** | 0.0001*** | 0.0001*** | 0.0001*** | 0.0001 | 0.0003*** | 0.0003*** |
| | (0.0000) | (0.0000) | (0.0000) | (0.0001) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0001) | (0.0000) | (0.0000) |
| Exchange rate (log) | -0.0024*** | -0.0028*** | -0.0028*** | -0.0028*** | -0.0027*** | -0.0015*** | -0.0026*** | -0.0029*** | -0.0029*** | -0.0028*** | -0.0028*** | -0.0013 | 0.0003 | 0.0004 |
| | (0.0006) | (0.0006) | (0.0006) | (0.0011) | (0.0006) | (0.0005) | (0.0007) | (0.0006) | (0.0006) | (0.0005) | (0.0005) | (0.0013) | (0.0017) | (0.0018) |
| Resources rents / GDP | 0.0003*** | 0.0003*** | 0.0004*** | 0.0004*** | 0.0003*** | 0.0002*** | 0.0004*** | 0.0004*** | 0.0004*** | 0.0003*** | 0.0004*** | -0.0002 | 0.0003 | 0.0005* |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0004) | (0.0005) | (0.0003) |
| Polity IV | -0.0006 | 0.0000 | -0.0004 | -0.0004 | -0.0008 | -0.0011 | -0.0026 | 0.0007 | -0.0012 | -0.0017 | -0.0015 | -0.0156*** | -0.0090* | -0.0114* |
| | (0.0030) | (0.0033) | (0.0034) | (0.0034) | (0.0033) | (0.0023) | (0.0033) | (0.0035) | (0.0036) | (0.0027) | (0.0027) | (0.0053) | (0.0049) | (0.0069) |
| New democracy | -0.0003 | -0.0005 | -0.0004 | -0.0004 | -0.0004 | -0.0009 | -0.0003 | -0.0001 | -0.0005 | -0.0004 | -0.0005 | 0 | 0.0031* | 0.0001 |
| | (0.0011) | (0.0011) | (0.0011) | (0.0011) | (0.0011) | (0.0011) | (0.0011) | (0.0012) | (0.0012) | (0.0011) | (0.0011) | 0 | (0.0016) | (0.0025) |
| Observations | 20,725 | 20,639 | 20,455 | 20,455 | 20,026 | 18,319 | 21,963 | 19,575 | 20,182 | 20,182 | 20,182 | 3,719 | 5,227 | 6,107 |
| R-squared | 0.1289 | 0.1415 | 0.1436 | 0.1436 | 0.1206 | 0.0766 | 0.1517 | 0.1444 | 0.1473 | 0.1438 | 0.1447 | 0.2477 | 0.2723 | 0.2500 |
| Countries | 104 | 104 | 104 | 104 | 104 | 104 | 110 | 99 | 104 | 104 | 104 | 11 | 17 | 22 |

*** 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, other than in column (4) where they are bootstraped with 1000 replications. OECD membership is defined as of at 1975, 2009, or 2017. The samples "outliers at 1% & 5%" trims the data at respective top and bottom percentiles of money growth. All regression are estimated using an OLS model as specified in Equation 1 other than those in columns 7 & 8, which are estimated with a difference-GMM model by instrumenting the specified lags of the dependent variable on their up to six lags.

**Table A6:** Interactions between *Election month* and measures of the quality of elections

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| VARIABLES | $\Delta lnM1$ | | | | | | | |
| Monitoring Agency | Organization for Security and Co-operation in Europe | | All election-monitoring reports | | | | "Country reports on human rights practices" by US State Dep | |
| | | | Mean | | Maximum | | | |
| (source) | (DIEM) | | (DIEM) | | | | (QED) | |
| Election month | 0.0071* | 0.0069* | 0.0034 | 0.0073** | 0.0044 | 0.0073** | 0.0077** | 0.0069** |
| | (0.0041) | (0.0040) | (0.0036) | (0.0034) | (0.0037) | (0.0034) | (0.0038) | (0.0032) |
| Overall election quality | -0.0612*** | | | | | | | |
| | (0.0106) | | | | | | | |
| x Election month | 0.0556*** | | | | | | | |
| | (0.0131) | | | | | | | |
| Election day cheating | | | -0.0376** | | -0.0261 | | -0.0402** | |
| | | | (0.0144) | | (0.0165) | | (0.0157) | |
| x Election month | | | 0.0598*** | | 0.0371* | | 0.0344* | |
| | | | (0.0221) | | (0.0205) | | (0.0178) | |
| Observations | 4,032 | 4,032 | 5,248 | 5,248 | 5,248 | 5,248 | 8,032 | 8,032 |
| R-squared | 0.1266 | 0.1250 | 0.1354 | 0.1346 | 0.1351 | 0.1346 | 0.1588 | 0.1583 |
| Countries | 93 | 93 | 95 | 95 | 95 | 95 | 96 | 96 |

*** 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, polity index of democracy, new democracy dummy; include three lags of the dependent variable; and country, year and month fixed effects. *Overall election quality* is the overall assessment of the election provided by the monitoring agency, and is coded on an acceptable-to-unacceptable scale (normalized between 0 & 1). *Election day cheating* is a measure combining various cheating practices on the election day (such as vote processing, voter fraud including vote buying, and voter intimidation), and is coded on a scale ranging from no problem to major problem (normalized between 0& 1). Odd numbered columns run the baseline model on the sample where election monitoring data is available. Standard errors are robust to heteroscedasticity and are clustered at the level of countries.

**Table A7:** The election date effect interacted with central bank independence (CBI) index, Polity IV index of democracy, and vote shares gained by the incumbent party or the president

| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | $\Delta lnM1$ | | | | | |
| Sample | OECD & non-OECD | | Non-OECD | OECD | Non-OECD | OECD |
| Election type | Legislative &/or Executive | | Legislative | | Executive | |
| Election month | 0.0034 | -0.0186** | -0.0024 | -0.0230 | -0.0185 | -0.0782 |
| | (0.0054) | (0.0090) | (0.0132) | (0.0560) | (0.0236) | (0.0555) |
| CB independence | -0.0092*** | | | | | |
| | (0.0026) | | | | | |
| x Election | -0.0007 | | | | | |
| | (0.0086) | | | | | |
| Polity IV | -0.0029 | 0.0342*** | -0.0066 | -0.0076 | -0.0073* | -0.0476* |
| | (0.0035) | (0.0125) | (0.0040) | (0.0081) | (0.0043) | (0.0207) |
| x Election | | 0.0788** | | | | |
| | | (0.0367) | | | | |
| Polity IV square | | -0.0304** | | | | |
| | | (0.0116) | | | | |
| x Election | | -0.0564* | | | | |
| | | (0.0296) | | | | |
| Vote share | | | -0.0003* | -0.0001 | -0.0002 | 0.0005 |
| | | | (0.0001) | (0.0002) | (0.0002) | (0.0009) |
| x Election | | | 0.0004 | 0.0008 | 0.0011 | 0.0027 |
| | | | (0.0004) | (0.0021) | (0.0009) | (0.0022) |
| Vote share square | | | 0.0000* | 0.0000 | 0.0000 | -0.0000 |
| | | | (0.0000) | (0.0000) | (0.0000) | (0.0000) |
| x Election | | | -0.0000 | -0.0000 | -0.0000 | -0.0000 |
| | | | (0.0000) | (0.0000) | (0.0000) | (0.0000) |
| GDP pc (log) | 0.0004*** | -0.0156*** | -0.0145*** | -0.0099*** | -0.0178*** | -0.0087 |
| | (0.0001) | (0.0036) | (0.0047) | (0.0031) | (0.0048) | (0.0075) |
| GDP pc growth | -0.0114*** | 0.0005*** | 0.0005*** | 0.0002 | 0.0005*** | 0.0004 |
| | (0.0026) | (0.0001) | (0.0001) | (0.0002) | (0.0001) | (0.0002) |
| Inflation | 0.0001*** | 0.0002*** | 0.0001*** | 0.0003*** | 0.0001*** | 0.0004*** |
| | (0.0000) | (0.0001) | (0.0000) | (0.0000) | (0.0000) | (0.0001) |
| Exchange rate (log) | -0.0021*** | 0.0001 | -0.0029*** | -0.0002 | -0.0033** | 0.0062 |
| | (0.0004) | (0.0026) | (0.0010) | (0.0023) | (0.0016) | (0.0056) |
| Resources rents / GDP | 0.0002** | 0.0004*** | 0.0005*** | 0.0011* | 0.0005*** | 0.0011 |
| | (0.0001) | (0.0001) | (0.0001) | (0.0005) | (0.0001) | (0.0007) |
| New democracy | -0.0007 | 0.0004 | -0.0021 | 0.0022 | -0.0001 | 0.0019 |
| | (0.0011) | (0.0013) | (0.0015) | (0.0014) | (0.0014) | (0.0012) |
| M1 growth (t-1) | -0.0027 | -0.1166*** | -0.0755*** | -0.0010 | -0.0723*** | -0.0988 |
| | (0.0034) | (0.0160) | (0.0222) | (0.0012) | (0.0185) | (0.0708) |
| M1 growth (t-2) | 0.0001 | -0.0291*** | -0.0144** | 0.0005 | -0.0195** | -0.0620 |
| | (0.0013) | (0.0104) | (0.0062) | (0.0007) | (0.0097) | (0.0376) |
| M1 growth (t-3) | 0.0037* | -0.0076 | -0.0086 | 0.0035 | -0.0236** | 0.0660 |
| | (0.0022) | (0.0119) | (0.0070) | (0.0021) | (0.0113) | (0.0679) |
| Observations | 18,763 | 18,348 | 10,949 | 4,557 | 11,249 | 1,682 |
| R-squared | 0.1650 | 0.1366 | 0.1830 | 0.2576 | 0.1983 | 0.4423 |
| Countries | 87 | 114 | 76 | 17 | 65 | 6 |

*** 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. For graphical presentation of the marginal effects of columns (1) to (2) and (3) to (6) see Figures 1 and 3, respectively.

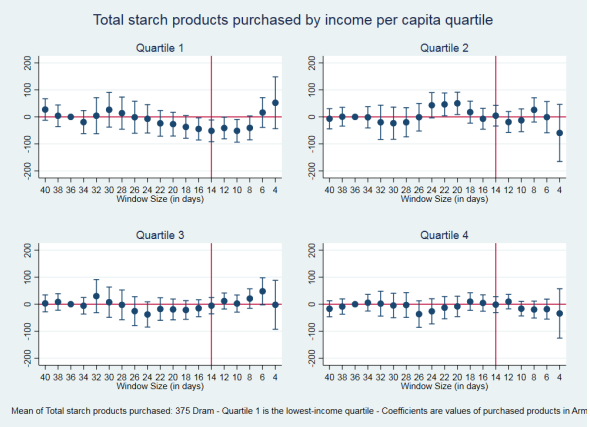
**Table A8:** Sample statistics for Armenian survey study

| | Full sample | | Control group | | Treatment group | |
|---|---|---|---|---|---|---|
| | Number | Share | Number | Share | Number | Share |
| No. of observations | 2,734,856 | 100% | 383.987 | 14.0% | 186.155 | 6.8% |
| No. of households | 95.779 | 100% | 17.727 | 18,6% | 8.074 | 8,4% |
| Obs. in after group | - | - | 206.213 | 53.7% of control | 99.621 | 53.5% of treatment |

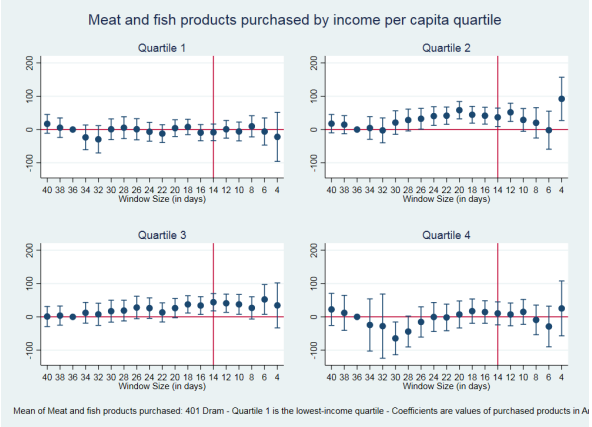
Note: The full sample pools together all observations from the 2001 to 2016 waves of the Integrated Survey of Living Standards. The table assumes that $\delta = 40$. The treatment group limits the sample to households surveyed 66 days before and 14 days after an election, and the control group consists of households surveyed within the same 80 days window as the treatment group but in the year before and the year after the election year. The sizes of the two groups will be different for other values of $\delta$.

**Figure A1:** Consumption responses by income quartile

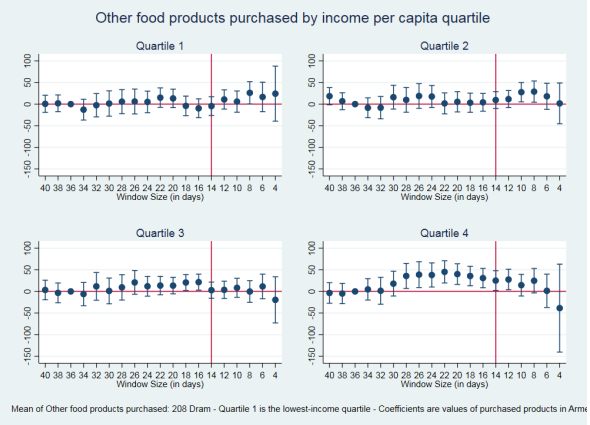
**(a)** Starch products



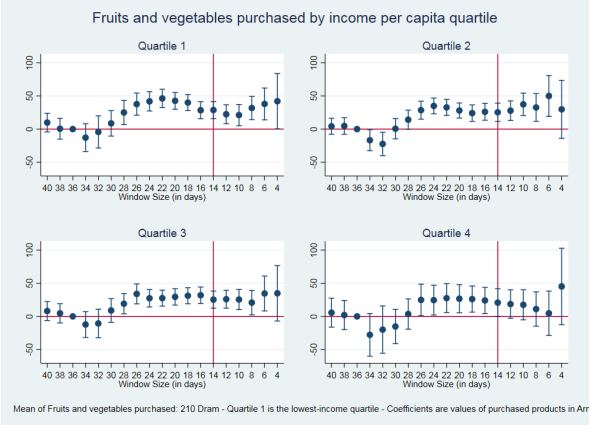

**(b)** Meat and fish



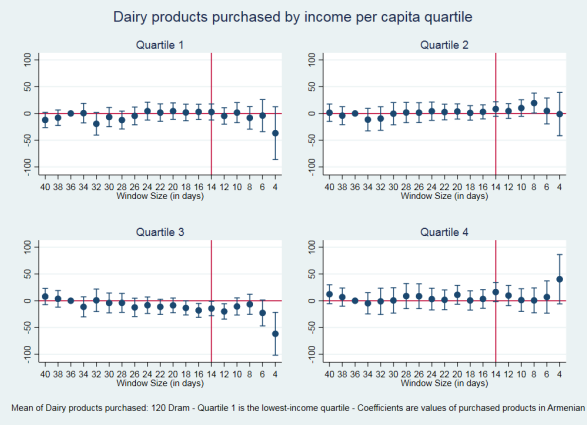

**(c)** Other food



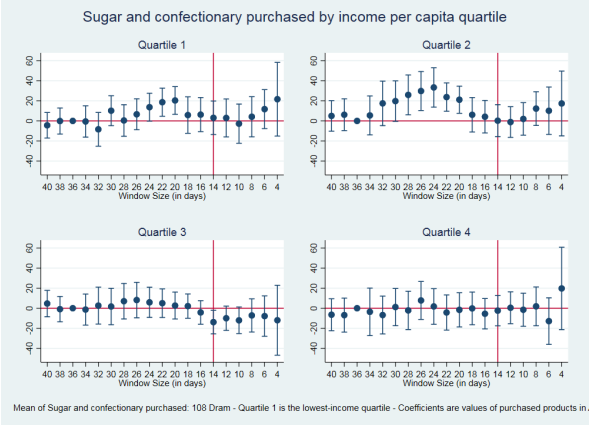

**(d)** Fruits and vegetables



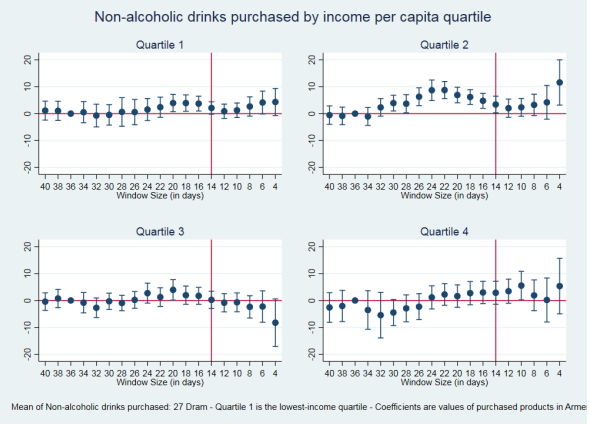

**(e)** Dairy products



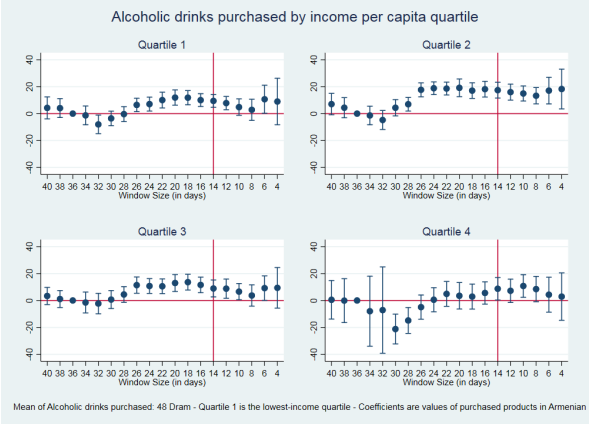

**(f)** Sugar and confectionery




**(g)** Non-alcoholic drinks




**(h)** Alcoholic drink



# Online Appendix B: Survey and case study evidence

In this section we present survey evidence on vote buying from Latin American and African Barometers and from the World Values Survey, as well as case studies illuminating the relationship between vote buying and election day monetary expansions.

### Survey evidence on the extent of vote buying

One might ask if vote buying really occurs on a scale that could, in principle, induce macroeconomic effects. Survey evidence suggests that it does. Figure B1 reports data from the Afro- and Latino-barometers and from the World Values Survey (WVS) on the fraction of survey respondents who report that they themselves or other people they know were offered to sell their vote during elections. The exact questions vary from survey to survey making it hard to compare them in a consistent way. Thus, the statistics should be treated with care.

The average share of people reporting having been offered something in return for their vote is about 17% in Africa and about 8% in Latin America, with the percentage of voters knowing of someone else being bribed being considerably higher (25% in Latino-barometer and over 50% in the WVS). In some countries, e.g., Benin, Uganda and the Dominican Republic, close to half the population reports knowledge of vote buying.[1] It is clear from these data that vote buying is widespread in many parts of the world, in particular in African democracies (see also, Collier and Vicente 2012). 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. In Taiwan, many voters were offered about $10 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

[1]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.

the 2006 municipal election voters were offered, on average, $48 in exchange for their vote.

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

Most central banks report monthly or annual data on M1 and few publish weekly or daily data. These data series allow us to track monetary movements around elections with varying degrees of accuracy. Figure B2 reports data on M1 or related monetary aggregates around elections in six selected countries: Nigeria, Bolivia, Venezuela, Indonesia, Lebanon and the USA.[2] The dark bar in each panel indicates the election day, week or month.

**NIGERIA 2007.** The 2007 presidential election marked the first transition from one elected leader to another in the largest country in Africa. Many observers 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. The going price for a vote was around 500 Naira or about $4.[3] Figure B2(a) 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 fell 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 the incumbent president Evo 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. The international press reported that "cash

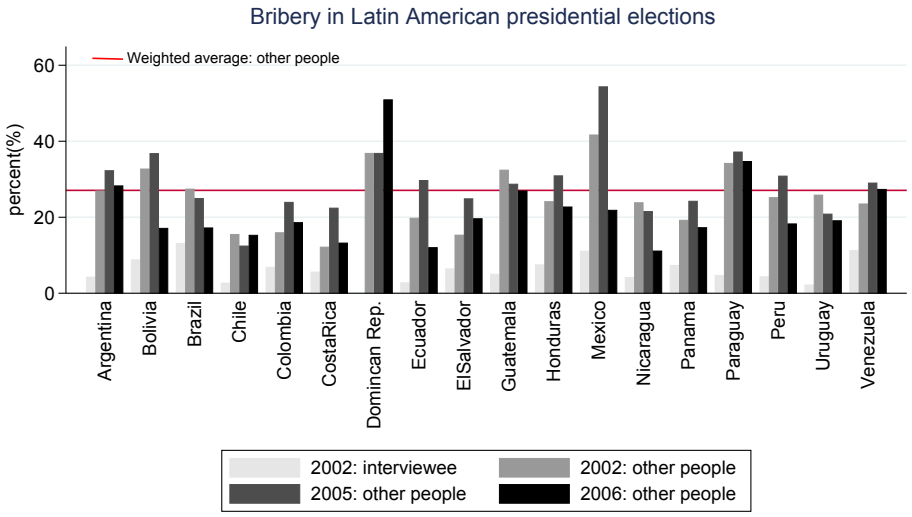
[2]The elections in the five low and middle income countries were chosen because of abundance of circumstantial evidence of vote buying. The 2012 election in the USA was chosen to illustrate the stark difference between established OECD and low and middle income non-OECD countries. The selection is obviously *not* random and we do not pretend that it is representative.

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

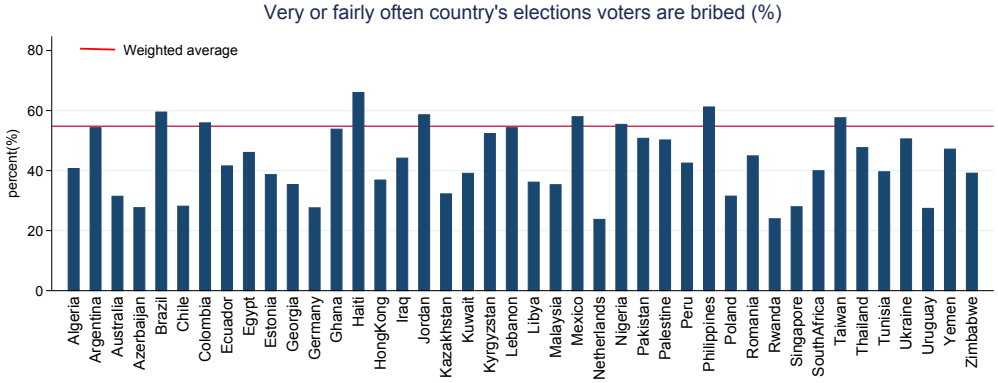
**Figure B1:** The extent of vote buying in Africa (2010-2012), Latin America (2002) and WVS

**(a)** *Afro-barometer:* And during the last national election in [year], 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?



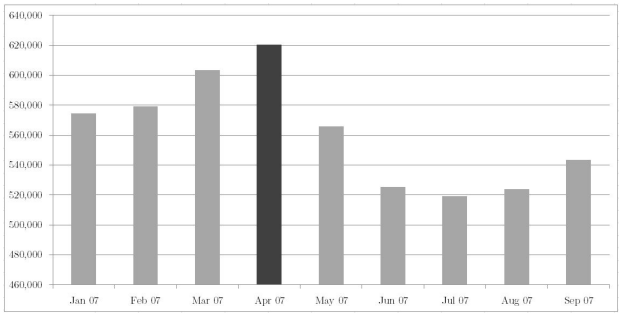

**(c)** *World Values Survey:* How often are in [country]'s elections voters being bribed?



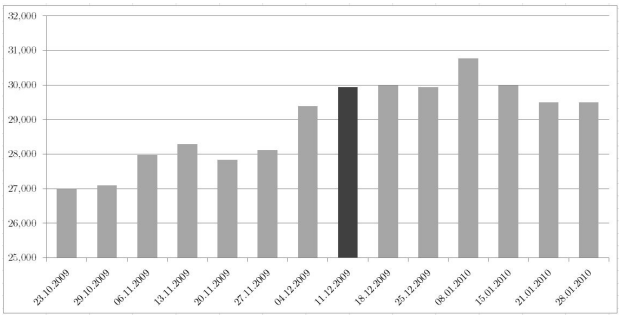

Notes: Data for Dominican Republic is from the survey wave of 2005. Sources: Afro−barometer (2015); Latino−barometer (2015).

**Figure B2:** Movements in monetary aggregates by day, week or month during selected elections in six countries.

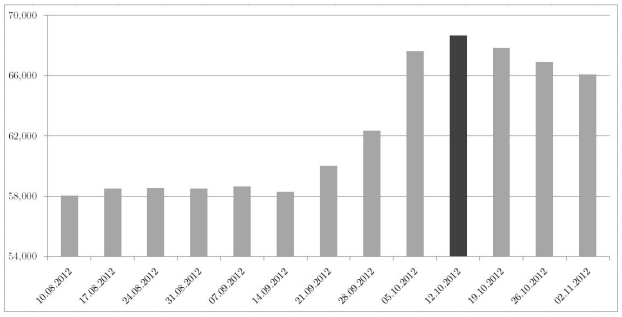
**(a)** Nigeria 2007 (million NGN)



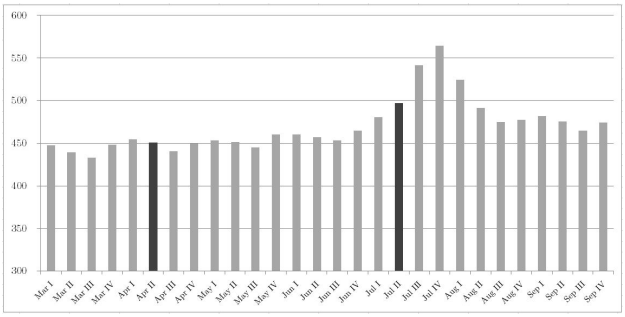

**(b)** Bolivia 2009 (million BOB)



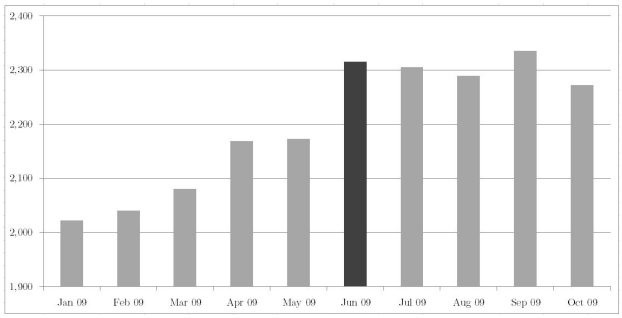

**(c)** Venezuela 2012 (billion VEF)



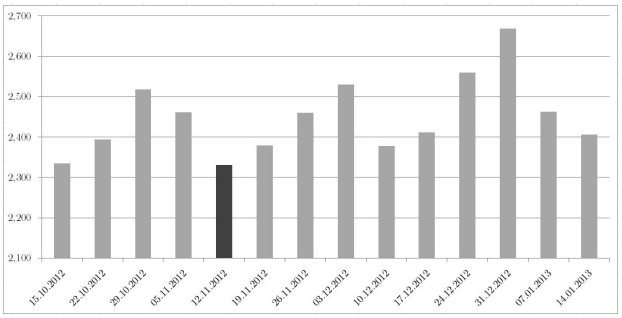

**(d)** Indonesia 2014 (billion IDR)




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




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




Notes: The data on the monetary aggregates come from the respective national central banks and are in units of the local currency. Panel (a) reports currency in circulation outside of banks; panels (b) and (f) report M1; and panels (d), (e) and (f) report currency in circulation.

handouts for poor families ..... and heavy social spending" were all helping Morales get re-elected (Reuters 2009). Survey data suggest that about 10% of the voting population in Bolivia find vote buying acceptable (Gonzalez Ocantos et al. 2014). Figure B2(b) shows the movement of M1 by week around the 2009 election. We observe a significant increase between the fourth week of November and the election held in the second week of December.

**VENEZUELA 2012.** After the 2009 constitutional amendment that abolished presidential term limits, Hugo Chavez could, after 14 years in power, run as the presidential candidate for the United Socialist Party of Venezuela (PSUV) for the third consecutive time in October 2012. He was reelected by 55% of the popular vote in an election with a turnout of over 80% and in which 20 to 40% of Venezuelans are reported to doubt that election anonymity is guaranteed.[4] As the incumbent, Chavez effectively exploited the apparatus of the Venezuelan state to help secure this result (Economist, July 7, 2012). One common strategy was to use patronage power to award jobs, contracts, and subsidies to supporters. The Financial Times[5] reports that central government spending grew by 41.1% in real terms in the led-up to the election, and that much of this increase went towards wage increases and construction. In a country where the central bank is also under government control much of this appears to have been funded by direct transfers from the central bank to the off-budget fund called the National Development Fund (known as Fonden) that Chavez used to finance his popular social programs. The monetary consequences of this are visible in Figure B2(c) which shows the movement of M1 by week around the October election. We observe more than a doubling of the outstanding stock of money between the third week of September and the election held in the second week of October.

**INDONESIA 2014.** 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 survey by Jakarta-based pollster Indikator reported 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" (Jakarta Post 2014). In the region of East Java, for example, a candidate admitted to paying 117 million Indonesian rupiah (over $10,000) 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

[4]SeeAngeby et al. (2012).

[5]http://blogs.ft.com/beyond-brics/2012/09/25/venezuelas-pre-election-spending-spree-saps-exchange-reserves/

rupiah (about $4-8) per vote. Figure B2(c) 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. Interestingly, the Central Bank of Indonesia reports data on the circulation of different denominations of its banknotes. From the bolded line in Figure B2(d), which graphs the circulation of banknotes with a nominal value of 50,000 Indonesian rupiahs, we see an increase of over 20% in July, while the circulation of coins with nominal values of 1,000 rupiahs is constant (the dotted line). Given that the typical price of a vote is reported to be in the range between 50,000 to 100,000 rupiahs, this pattern is consistent with widespread vote buying.

**LEBANON 2009.** Lebanon held a relatively peaceful parliamentary election in June 2009. Allegations of vote buying were abundant. The New York Times reported 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). Transparency International and Lebanese Transparency Association (2009) claimed that "the value of a vote varied from $60 to $100 in Saida, $800 in Zahle, up to $3,000 in Zghorta". Corstange (2012) uses survey data and a list experiment to show that over half of the Lebanese voters sold their votes in the 2009 election. Figure B2(e) shows the movement in M1 around the election month. We observe a big, positive spike in June.

The examples from Nigeria, Indonesia, Venezuela, Bolivia, Lebanon along with Armenia discussed in the main text 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 "young" democracies and their political institutions are comparably weak.[6] Cross-country stud-

[6]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. Lebanon scores 5, Bolivia 4, Indonesia 2 and Nigeria 4 (Freedom House 2012).

ies reveal a strong association between weak political institutions and vote buying, in particular amongst "young" democracies (Keefer 2007; Keefer and Vlaicu 2008).

Insofar as the correlation between the monetary aggregates 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 strong political institutions. Figure B2(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.

# References


Afro−barometer (2015). Survey rounds 1-5. Retrieved from http://www.afrobarometer.org/.

Angeby, M., F. Malm, and H. Kastenholm (2012). The 2012 presidential election in venezuela. Report, Swedish International Liberal Centre.

Bratton, M. (2008). Vote buying and violence in Nigerian election campaigns. *Electoral Studies 27*(4), 621 – 632.

Collier, P. and P. C. Vicente (2012). Violence, bribery, and fraud: The political economy of elections in Sub-Saharan Africa. *Public Choice 153*(1-2), 117–147.

Collier, P. and P. C. Vicente (2014). Votes and violence: Evidence from a field experiment in Nigeria. *The Economic Journal 124*(574), F327–F355.

Corstange, D. (2012). Vote trafficking in Lebanon. *International Journal of Middle East Studies 44*(3), 483–505.

Finan, F. and L. Schechter (2012). Vote-buying and reciprocity. *Econometrica 80*(1), 863–881.


Additionally, they are all perceived to have high levels of corruption (e.g., Transparency International 2014).

Freedom House (2012). Freedom in the world. Report, Freedom House.

Globalpost (2009). Going rate for a vote in Lebanon? USD700. Retrieved 28 august 2015 from http://www.globalpost.com/dispatch/lebanon/090602/going-rate-vote-lebanon-700/.

Gonzalez Ocantos, E., C. K. de Jonge, and D. W. Nickerson (2014). The conditionality of vote-buying norms: Experimental evidence from Latin America. *American Journal of Political Science 58*(1), 197–211.

Gonzalez-Ocantos, E., C. K. D. Jonge, C. Melendez, J. Osorio, and D. W. Nickerson (2012). Vote buying and social desirability bias: Experimental evidence from Nicaragua. *American Journal of Political Science 56*(1), 202–217.

Jakarta Post (2014). Banned, but vote−buying still plagues Indonesia's election process. Retrieved 28 august 2015 from http://www.thejakartapost.com/news/2014/03/17/banned-vote-buying-still-plagues-indonesias-election-process.html.

Keefer, P. (2007). Clientelism, credibility, and the policy choices of young democracies. *American Journal of Political Science 51*(4), 804–821. cited By 127.

Keefer, P. and R. Vlaicu (2008). Democracy, credibility, and clientelism. *Journal of Law, Economics, and Organization 24*(2), 371–406.

Latino−barometer (2015). Survey wave 2002. Retrieved from http://www.latinobarometro.org/lat.jsp.

Lucky, O. O. (2013). Money politics and vote buying in nigeria: The bane of good governance. *Afro Asian Journal of Social Sciences 4*(3), 1–19.

New York Times (2009). Foreign money seeks to buy Lebanese votes. Retrieved 28 august 2015 from http://www.nytimes.com/2009/04/23/world/middleeast/23lebanon.html?pagewanted=all.

Phongpaichit, P., N. Treerat, Y. Chaiyapong, and C. Baker (2000). Corruption in the public sector in Thailand: Perceptions and experience of households. Working paper, Chulalongkorn University.

Reuters (2009). Bolivia's Morales woos voters with cash, charisma. Retrieved 28 august 2015 from http://in.reuters.com/article/2009/10/29/bolivia-election-idinn2931879820091029.

Transparency International (2014). The global corruption report. Report, Transparency International.

Transparency International and Lebanese Transparency Association (2009). National integrity system study- Lebanon 2009. Technical report.

Wang, C. S. and C. Kurzman (2007). Dilemmas of electoral clientelism: Taiwan, 1993. *International Political Science Review 28*(2), 225–245.

# Online Appendix C: The monetary mechanisms

Our empirical results show that elections have a positive short-run effect on the supply of M1 in the month of the election and in the post-election month. To understand the source of this short-run effect on M1, we develop in this appendix a simple textbook model of a money market with a fractional banking sector.

## The model

The equilibrium in the money market describes the level of real money balances held by the private sector, either as cash or as deposits in the commercial banks. The economy has a central bank that issues reserves (the monetary base) either through open market operations as nonborrowed reserves ($R_{nb}$) or as borrowed reserves (or discount loans) ($R_b$) issued through its discount mechanism which enables the commercial banks to borrow reserves from the central bank at the refinancing (or discount) rate ($r_d$). The monetary base (the total reserves) of the economy is $R_b + R_{nb}$. The monetary base is used in three ways: it is held as required reserves by the commercial banks ($R_r$), as excess reserves ($R_e$) also held by the banks, or as cash held by the public $C$. The nonborrowed reserves supplied by the Central Bank to the economy through its open market operations can be expressed as

$$R_{nb} = R_r + R_f + C, \tag{1}$$

where $R_f = R_e - R_b$ are the free reserves held by the commercial banks, not owed to the central bank. The free reserves are an increasing function of the difference between the market interest rate $r$ and the refinancing rate, i.e., $R_f(r - r_d)$ with $\frac{\partial R_f}{\partial(r-r_d)} > 0$.

The money supply (M1) is the currency in the hands of the public outside the commercial banks plus their demand deposits in the banks. We assume that there are two types of agents in the economy, index by $i \in \{1, 2\}$, and which we call the hoarders ($i = 1$) and the traders ($i = 2$) for reasons that will become clear later. The part of M1 held by

type $i$ is $M1_i$ with $M1 = \sum_i M1_i$. The two types differ with respect to the fraction of its money it wants to hold in cash ($C_i$) and as deposits ($D_i$)

$$C_i = h_i M1_i \tag{2}$$

$$D_i = (1 - h_i)\, M1_i, \tag{3}$$

where $h_i$ is the fraction of money that type $i$ holds in cash. The hoarders hold all their cash outside the banks so $h_1 = 1$, while the traders hold deposit accounts and $h_2 < 1$. The hoarders hold their money in cash either because they are excluded from the banking system (say, because of poverty) or because their money is earned illegally and they want the holdings kept secret. The commercial banks are required by law to hold a faction of their total deposits as required reserves so

$$R_r = z \sum_i D_i = z \sum_i (1 - h_i) M1_i. \tag{4}$$

We can derive the relationship between the supply of M1 and the nonborrowed reserves supplied by the central bank through open market operations by combining equations (1), (2) and (4) to get

$$R_{nb} - R_f = z \sum_i (1 - h_i) M1_i + \sum_i h_i M1_i. \tag{5}$$

Using the assumption that $h_1 = 1$, we can calculate the money held by the traders (of type 2) as

$$M1_2 = \frac{R_{nb} - R_f - M1_1}{z + h_2(1 - z)}, \tag{6}$$

and the total supply of money (M1) as

$$M1 = M1_2 + M1_1 = \frac{R_{nb} - R_f - M1_1}{z + h_2(1 - z)} + M1_1. \tag{7}$$

We observe that the money supply (M1) depends positively on the reserves supplied by the central bank through open market operations ($R_{nb}$), negatively on the excess

reserves held by the commercial banks ($R_f$), and, as a new feature, on how much cash is hoarded outside the banking system:

$$\frac{\partial M1}{\partial M1_1} = 1 - \frac{1}{z + h_2(1-z)} < 0 \tag{8}$$

as the money multiplier ($\frac{1}{z+h_2(1-z)}$) of the fractional banking system is greater than one. Since the free reserves are increasing in the market interest rate and decreasing in the refinancing rate, the supply of money increases with $r$ and falls with $r_d$.

We model the demand for money ($M1$) with a Keynesian liquidity preference function $L(r, y, v)$, where the demand for real money balances is a negative function of the market interest rate, and a positive function of the level of activity in the economy $y$. The parameter $v$ represents a demand shifter. The equilibrium in the money market requires

$$\frac{M1(r - r_d)}{P} = L(r, y, v). \tag{9}$$

In the very short-run, prices ($P$) and the level of activity ($y$) can be taken as given.

## Sources of short-run fluctuations in M1

The model can help us understand various sources of short-run fluctuations in the money supply (M1) around elections. We can divide these into supply and demand side factors.

1. Supply side factors: The central bank may start an open market operation, fund government spending directly, or lower its refinancing rate around elections. This directly shifts the money supply curve out and increases the money held by the public via a fall in the market interest rate.

2. Demand side factors: We highlight two types of demand shocks that could be generated by elections

(a) Hoarded money returns to the banking sector: If those who hoard money outside the banking system spend some of their cash and this cash ends up in the commercial banks, it will increase the supply of money endogenous as the banks expand their lending. In the model, this corresponds to a reduction in the money holdings of the hoarders ($M1_1$ falls) and an increase in the money holdings of the traders. From equation (8), we observe that this "swap" increases M1 and shifts the money supply curve out.

(b) The demand for money increases through substitution from broad to narrow money: This shifts the demand function out, increases the market interest rate and encourages the banks to lower their free reserves. This expands M1 endogenously. This effect is reinforced if the central bank is concerned about higher market interest rates (e.g., because it got a target rate) and expands liquidity, either through open market operations or through the discount window.

These two demand effects can both be triggered by vote buying. First, if vote buying is funded with hoarded cash money from the black economy and some of it enters the banking system when it gets into the hands of the voters (corresponding to the "traders" in the model), then this will cause an increase in M1 around the election. If the votes are bought from voters without access to the banking system there will not be any immediate effect on M1, but when they spent the money in shops and the shopkeepers deposit the money in their bank accounts, the effect will show up with a delay. Second, if vote buying means that the demand for cash money from within the banking system goes up around elections, then the second effect will operate. This is what happens if broader assets not included in M1 are sold for cash in the open market.

# Online Appendix D: Reconciling the micro and macro evidence

We present a "back of the envelope calculation" aimed at linking the micro-econometric evidence on the consumption consequences of elections and the macro evidence on the increase in money supply to each other and to vote buying. Assuming that all these effects are, in fact, caused by vote buying, we proceed in three steps: (1) we calculate the price of a vote implied by the consumption evidence; (2) we calculate the price of a vote implied by the increase in money supply; and (3) we compare these to the vote prices reported in the press.

In the previous section, we reported a statistically significant increase in food expenditure around elections. The minimum daily increase over a 14 days period is 50 AMD and the maximum daily increase over a 20 days period is 100 AMD (see Figure 5a in the text). This bounds the total increase in consumption expenditure on food between 700 and 2,000 AMD. On the (conservative) assumption that households spend (rather than save) the windfall income they get from selling their votes[1] and that they allocate that extra income across the various consumption goods according to the observed budget shares, we can calculate the implied increase in income required to fund the observed increase in food consumption as the ratio of the budget share of food (13%) to the increase in food expenditure. This gives us estimates of the required increase in income between 5,385 and 15,385 AMD for the average household or between 1,795 and 5,051 AMD for the average voter.[2] To get the "implied vote price", we need to take into account that only a subset of voters were bribed. According to Institute for War and Peace Reporting (2012) a third of the 1.5 million Armenian voters were bribed in 2012. Based on the

[1] Jappelli and Pistaferri (2010) report that the short-run marginal propensity to consume from both anticipated and unanticipated income is high for poor and liquidity constrained households.

[2] The calculation is 700/0.13 and 2,000/0.13 for each household. The average number of voters in each household is about 3.

**Table D1:** The election date effect in Armenia

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| VARIABLES | *M1 (billion AMD)* | | | | | |
| Election month | 4.6748*** | | | | | |
| | (0.9424) | | | | | |
| Election month (t-1) | | -0.5540 | | | | |
| | | (0.9090) | | | | |
| Election month (t+1) | | | -0.6808 | | | |
| | | | (0.9058) | | | |
| 20 days around election | | | | 7.4266*** | | |
| | | | | (1.4943) | | |
| 20 days around election; 80 days control | | | | | 5.1408*** | |
| | | | | | (1.9006) | |
| 20 days around election; 40 days control | | | | | | 3.2973*** |
| | | | | | | (1.1440) |
| Observations | 5,935 | 5,887 | 5,912 | 4,171 | 735 | 367 |
| R-squared | 0.9942 | 0.9941 | 0.9941 | 0.9912 | 0.9931 | 0.9984 |

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

Notes: Robust standard errors reported in brackets. The dependent variable is the level of $M1$ measured daily in billion AMD. Daily data are not available on weekends and holidays. All regressions include year, month, week and day of the month fixed effects. "20 days around the election" corresponds to the 12 days before and the 6 days after each election; 80 days and 40 days control means that the comparison group is restricted to, respectively, 80 and 40 days around the election date in election and non-election years. All regressions control for year, month, week, and day of the month fixed effects.

consumption estimates we, therefore, arrive at an "implied vote price" in the range from 5,385 to 15,153 AMD.[3]

Next, we need to estimate the increase in money supply around elections in Armenia. Unlike most central banks, the Central Bank of Armenia reports monetary statistics on a daily basis. Using these data to back out the "implied vote prices", we first estimate the effect of elections on the level of $M1$. To account for seasonality, we include year,

[3]We calculate this as 1,795*1.5/0.5 and 5,051*1.5/0.5, respectively. If we assume that the marginal propensity to consume is lower than one, then these estimates would be larger.

month, week, and day of the month fixed effects. The results are reported in Table D1. We consider two "election treatments". The first "treatments" is tailored to the approach taken in Section 2 in the textto estimate the macroeconomic effect of elections. It uses the days of election month as the "treatment" window and all other (non-election) days in the sample as control. The results are reported in columns (1) to (3) for the election, the pre-, and post-election month, respectively. The second "treatment" is tailored to the approach taken in text to estimate the consumption effect of elections. It considers the "treatment window" to be the 20 days around the elections during which the estimated consumption effect is significant (see Figure 5 in the text). In column (4), we report results for this "treatment window" where we use all other days as the control, while in columns (5) and (6), we narrow the control to, respectively, 80 and 40 days around the election date in election and non-election years. The total sample is available for the period 1995:m12-2018:m8 in columns 1-3 (with ten national election), and is restricted to the period 2001:m1-2016:m12 in columns 4-6 (with six national elections) as in the text.

Consistent with the vote buying hypothesis, Table D1 shows that the circulation of money in the Armenian economy increases substantially during elections while there is no effect in the pre- and post-election months. The increase in the supply of money ranges from 3.30 to 7.43 billion AMD. Assuming that these increases were entirely due to vote buying and that 500,000 voters were bribed, the "implied vote price" is between 6,595 to 14,860 AMD. These estimates are in the same range as the "implied vote prices" calculated from the consumption responses (5,385 to 15,153 AMD).

As the final step, we can compare these "implied vote prices" to the vote prices reported in the local media which were in the range from 5,000 to 10,000 AMD per vote (e.g., Aravot Daily 2012). We observe that the vote prices reported in the local media, those implied by the observed increase in the money supply, and those implied by the consumption increase observed around the election are of the same order of magnitude. It is, therefore, possible that the observed increase in the circulation of cash around the election is related to vote buying and that black economy money re-entering the formal

banking system via the consumption channel plays an important role in linking the two. Obviously, we do not claim that the results from Armenia (or any other case study) will necessarily be representative for the countries in our macro sample. Nonetheless, the Armenian case study provides another piece of evidence suggesting that at least part of the increase in $M1$ that we observe around the world at election times is an indication of vote buying.

# References


Aravot Daily (2012). Several criminal cases opened by the Prosecutor General's office. Retrieved 28 august 2015 from http://www.aravot.am/2012/05/10/297037.

Institute for War and Peace Reporting (2012). Sweeteners for votes in Armenia. Retrieved 28 august 2015 from https://iwpr.net/global-voices/sweeteners-votes-armenia.

Jappelli, T. and L. Pistaferri (2010). The consumption response to income changes. *Annual Review of Economics 2*(1), 479–506.