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VOTE BUYING OR (POLITICAL) BUSINESS (CYCLES) AS USUAL?

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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 expenditures, we show that within-household consumption of food increases in the days before elections.

I. 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. But empirical tests of this theory have fared badly, and the evidence on monetary political cycles of the classical Nordhaus-MacRae type is weak, as pointed out in 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 that 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 to 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, its growth rate 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 postelection months in these countries. The effect is sizable: the growth rate of M1 increases on average by between 0.41 and 0.61 percentage points, or 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 be detected only by studying high-frequency (monthly) data. The evidence on this monetary expansion in the election month in non-OECD countries with elections, which we 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. 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, which increases 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

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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 theory of vote-buying, we find that the increase in the growth rate of M1 is systematically larger in elections that 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 microeconometric 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, Mitra, and Mukherji (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 macroeconomic shocks in election years, common shocks that affect all countries in a given month, or 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 economic activities to observable anomalies timed around elections. Kapur and Vaishnav (2013) show that Indian construction firms divert short-term funds to political campaigns (in anticipation of postelectoral preferential treatment) and that this induces a short-term election cycle in cement consumption in the election and postelection month. The macroeconomic effect on M1 that we find is also concentrated in the election and postelection 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 that 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 II introduces our data, identification strategy, and the main results. Section III discusses evidence on the monetary mechanisms behind the election date effect. Section IV introduces the vote-buying hypothesis and presents evidence consistent with it. Section V 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.

II. 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 economic 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 impractical. 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.

¹Zitzewitz (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 Alesina, Cohen, and Roubini (2003), Pande (2008), and Olken and Pande (2012).

²The 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, Roubini, and Cohen (1997) offer an overview of these models.

³See, for example, Alesina, Cohen, and Roubini (1992, 1993); Drazen (2001); Heckelman and Wood (2005); Alpanda and Honig (2009); Dreher and Vaubel (2009); and Klose (2012).
A. 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, and the secondary sample consists of 17 OECD countries. 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 (IMF) and are recorded at the end of each month. We obtain data on election months from the 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:

\[
\Delta \ln M_{1cym} = \sum_{i=1}^{k} \alpha_i \Delta \ln M_{1cym-i} + \beta_0 \Delta \ln Y_c + \beta_1 \Delta \ln Y_y + \beta_2 \Delta \ln R_{cy} + \beta_3 \Delta \ln P_{cy} + X_i \beta_4 + \mu_c + \eta_v + \nu_m + \epsilon_{cym},
\]

The dependent variable, \(\Delta \ln M1\), 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 U.S. dollar,\(^7\) the quality of institutions proxied by the Polity IV index normalized between 0 and 1 (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 \((\mu_c)\), year \((\eta_v)\), and month \((\nu_m)\) fixed effects. In more demanding specifications, we seasonally adjust the data on M1, as well as replacing country, year, and month fixed effects with \(\text{country} \times \text{year} \times \text{month}\), and \(\text{country} \times \text{month}\) 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 \(\text{Election month}\) is defined as being equal to 1 if at least one election takes place in country \(c\) in month \(m\) and year \(y\) and 0 otherwise. The second coding, \(\text{Election day}\), 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 0 otherwise.\(^10\)

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 nonelection 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 \(\epsilon\).\(^11\)

B. The Average Election Date Effect

Table 1, columns 1 to 4, reports the baseline estimates of the average election date effect as captured by the \(\text{Election month}\) and \(\text{Election day}\) 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 subsample of countries for which this information is available. All of these specifications

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\(^{5}\)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.

\(^{6}\)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 \(\Delta \ln M1\) at its bottom and top 1 percentiles. Our estimates remain robust to alternative strategies, see table A5, columns 5 and 6, in the supplementary material.

\(^{7}\)We proxy the short-run nominal interest rate by the monthly interest rate on Treasury bonds. These data come from International Monetary Fund (IMF) and are available for only around half of the countries in our sample. Consequently, we do not include the interest rate variable in most specifications.

\(^{8}\)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.

\(^{9}\)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 (nonnegative score on the Polity IV index), after which it becomes an old democracy.

\(^{10}\)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 them. 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 1,000 replications).

\(^{11}\)International Foundation for Electoral Systems (2015) provides information on the exact election days from 1998 onward. \(\text{Election day}\) is not coded for the elections before then. The rationale for the particular coding of \(\text{Election day}\) is that M1 is recorded at 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.
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<td>52</td>
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</table>

*** p < 0.01, ** p < 0.05, * p < 0.1. All regressions control for three lags of the dependent variable. Full FE stands for country × year, country × month, and year × month fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the level of countries. M1SA 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 as share of GDP. Columns 1 to 4 use the total available sample. Columns 5 to 9 and 10 to 14 fix the sample where the samples of all of these five specifications overlap. C = country; Y = year; M = month.
of equation (1) include year, month, and country fixed effects and three lags of the monthly growth rate of M1. The specifications use 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 5% level or better) increase in the growth rate of M1 in election months. In the 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 that 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).

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 \( \times \) 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 macroeconomic changes that may occur during election years. Second, we can control for month \( \times \) year fixed effects. Unlike the month fixed effects in the baseline specification, this controls for common macroshocks, such as a financial crisis or international financial flows, 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 U.S. Census Bureau and via country \( \times \) month fixed effects.

Table 1, columns 6 to 9, reports 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% levels, 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 are 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 \( \times \) year fixed effect), country-specific seasonality (country \( \times \) month fixed effects), or country-specific macroeconomic events within election years (country \( \times \) 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 perceive it to be beneficial to hold elections in months that 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 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 (\( n = 4,353 \)) 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 subsample, 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 is significant at the 1% level. All in all, this suggests that the results are not due to timing effects and reverse causality.

C. 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 or lingers into the months afterward. 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 postelection month. The point estimate on the postelection 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.\footnote{We have estimated specification with three lags and leads, and the third lag/lead is insignificant.}

D. The OECD Sample

The sample of seventeen “old” OECD countries is of particular interest because these countries have long-established democratic institutions and generally score high 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 reports the results. We find no evidence of any monetary election cycle in the election month or 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.

E. 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 subindicators: one for legislative elections only, one for executive elections only, and one for simultaneous legislative and executive elections.\footnote{We have 649 elections, of which 155 are only executive, 395 are only legislative, and 99 are simultaneous elections.} We then reestimated equation (1) with these refined Election month variables. Table A4, columns 1 to 4 in the supplementary material reports 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 triple 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 effect is centered on the month of the election and lingers into the postelection month. The effect is strongest in executive elections. We cannot detect the effect in the sample of OECD countries. These empirical facts are new to the literature.

\section{III. 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 are accommodated through the banking system.\footnote{In supplementary material appendix C, we sketch a simple model of the banking sector that illustrates the economics of these various effects.}

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 toward 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 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 substitute it for deposits that they would otherwise have withdrawn. This effect is clearly strongest in societies with easy access to banking services. Second, the hoarded cash also returns to the banks if the recipients spend the cash on goods and the retailers 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 are 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, 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 toward 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 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 substitute it for deposits that they would otherwise have withdrawn. This effect is clearly strongest in societies with easy access to banking services. Second, the hoarded cash also returns to the banks if the recipients spend the cash on goods and the retailers 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.

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as reported by the respective central banks) should fall in the months leading up to an election. Monthly data on the refinancing rate are available from 21 central banks in the non-OECD sample (Delta Stock, 2018). Table 2, columns 1 to 6, shows that the central bank’s lending rate changes in neither the election month nor 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. If so, the funds constitute an injection of primary money into the economy and affect M1 directly. If this is the monetary mechanism behind the election date effect, then the effect should be stronger in countries in which the central bank is under government influence (Berger, De Haan, & Eijffinger, 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 1975 to 2015. To this end, we augment equation (1) with CBI index and its interaction with Election month. Figure 1a 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. 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. To do this, we study the ratio of M1 to M2 or 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 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 of Election month on the growth rates of M2 (column 8) and M3 (column 9) is not statistically significant.

### Table 2.—Refinancing Rates and Higher-Powered Money

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### IV. 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. That fact that the cycle is timed around the election—in the election month and the month thereafter—and not in

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19While most of literature on fiscal political cycles focuses on deviations during the election year (De Haan & Klomp, 2013; Aidt & Mooney, 2014), there is evidence on short-run cycles of this sort. Akhmedov and Zhuravskaya (2004) document a sizable increase in direct monetary transfers to voters from the regional governments of Russia in the days leading up to the election. Labonne (2016) documents a short-term employment cycle in municipalities in the Philippines.

20A central bank is coded as being more independent if its governors serve terms longer; 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.

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

22M2 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 paper, and debt securities up to two years. The source of these data is International Monetary Fund (2018).

23Brender and Drazen (2005), Shi and Svensson (2006), and Hanusch and Keefer (2014), among 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 (2019) reports evidence of an election cycle in perceived corruption. Keefer and Vlaicu (2008), Hanusch and Keefer (2014), and Hidalgo and Nichter (2016) consider the link between vote buying and political budget cycles in fiscal variables.
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 III, 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.

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.

A. 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 U.S. State Department, or numerous nongovernmental 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.\(^{27}\)

\(^{24}\)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 (Shefter, 1977; Heckelman & Yates, 2002; Hicken, 2011; Stokes et al., 2013; Aidt & Jensen, 2017). Another strategy is to buy turnout, that is, to offer cash payments to induce core supporters to cast their vote (Nichter, 2014) or to induce opposition voters to stay home (Cox & Kousser, 1981).


\(^{26}\)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 U.S. 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 data set are 673 mission reports from 21 international organizations (including the Commonwealth Secretariat, the EU, the Council of Europe, the Organization of American States, the Organization for Security and Co-operation in Europe, the 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 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).
Our test adds the measures of election irregularities and their interaction with Election month to equation (1) and reestimates it on the sample for which monitoring data are 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 0 if the election is considered to be free and fair (acceptable) and is scored as 1 if it does “represent the will of the people” or 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, for example. 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 0 and 1. To construct these variables, we use the evaluations of the U.S. 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 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 that are fair and free. 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.

B. 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 on a scale that can effect M1 therefore is most likely to take place in countries with institutions in between these extremes: 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 0 (weak institutions) and 1 (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 another way, the election date effect is not significant in countries with a low Polity IV scores and weak institutions or in countries (belonging mostly in the OECD) with a “perfect” Polity IV score of 1. Van Ham and Lindberg (2015) find a similar effect in their study of self-reported vote buying in Africa. Overall, this is consistent with the vote-buying hypothesis.

C. 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 votes 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 3a) and between 40% and 80% in executive elections (figure 3b). This evidence is consistent with the vote-buying hypothesis. In interpreting this, we should bear in mind that the ex post outcome of an election is partly a function of the amount of ex ante vote buying.

28The logic is similar to the result from the literature on probabilistic voting that parties will promise postelection programmatic benefits to districts (or groups of voters) willing to swing their vote (Dixit & Londregan, 1996; Keefer & Vlaicu, 2008).

29The source of the data on vote shares is Beck et al. (2001). In nonelection years, vote share is coded as the vote share gained by the incumbent in the most recent election.
D. 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 that 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 microeconometric evidence to substantiate this aspect of the vote-buying hypothesis. The idea, 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. 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. First, the structure of the Armenian household survey with its daily recording is particularly well suited for this investigation. Second, anecdotal evidence on vote buying in Armenia is abundant. The 2012 Human Rights Report issued by the U.S. 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

30See also Gillitzer and Prasad (2018).
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 the upper and lower subfigures, 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.

Armenian dram (AMD) per vote (10 to 20 USD) and which was said to have reached up to 500,000 voters in a country with a population of fewer than 3 million and around 1.5 million registered voters (Institute for War and Peace Reporting, 2012).

**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 sociodemographic 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 out a diary daily. This diary collects information on purchases of consumption items disaggregated 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). The Armenian household survey is conducted throughout a calendar year. This means that within a year, households start their one month

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**Figure 3.**—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

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of daily records of consumption at different times. In contrast to many other consumption surveys, this allows us to study the consumption patterns of 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. We can therefore not only study finely timed (daily) consumption patterns but can also control for unobserved household fixed effects.

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 by defining a cutoff of fourteen days after an election34 and count 2 × δ days back in time from that cutoff. All the households that fill in a daily diary within this time window of 2 × δ days are in the treatment group. We assume that a treated household is in the “after” group during the [0, δ) days and in the “before” group during the [δ, 2 × δ] before the cutoff. Figure 4 visualizes this approach. For example, if δ is equal to twenty days, then the households in the treatment group are treated (the “after” group) during the six days before and fourteen days after the election and untreated (the “before” group) during the twenty days prior to that (i.e., the “before” period starts 26 and ends 6 days before the election). By varying δ between 4 and 40, we can split the treatment group into different before and after groups and in that way create different treatment windows around the election. For example, for δ = 40, the treatment starts 66 days before the election, while for δ = 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 days (2 × δ days) as the treatment group but in the year before and the year after the election year. Table A9 reports sample sizes for the control and treatment groups for the maximum time window of 80 days (δ = 40). In this case, the combined sample consists of about 570,000 daily observations from about 25,800 households.

The differences-in-differences specification that we estimate for different values of δ is

$$Y_{it} = \alpha_0 + \alpha_1 \times Treated(\delta)_it + \alpha_2 \times After(\delta)_it + \alpha_3 \times Treated(\delta)_it \times After(\delta)_it$$

$$= \sum_{k=2}^{365} \gamma_k \times DOW_k + \sum_{l=2}^{7} \beta_l \times DOY_l + \theta_i + \epsilon_{it}, \hspace{1cm} (2)$$

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(δ)_it is coded 1 for the households that for a given interval of 2 × δ days belong to the treatment group and 0 otherwise, and the dummy variable After(δ)_it is coded 1 for the δ days around the election where the treated households are subject to the “election treatment.” The coefficient of interest is α_3, which captures the average treatment effect (ATE) on consumption. As in Stephens (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 (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, and so on. ε is the error term clustered at the level of households. Under the 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 nonelection years, we can interpret α_3 as an unbiased average treatment effect. The outcome variable, Y, is daily expenditures on various consumption goods, measured in Armenian dram. We focus on total food consumption and eight food categories listed in table A8 of the supplementary material.35 We observe that at the median, food is about 13% of

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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, 2003; Johnson, Parker, & Souleles, 2006; Stephens & Unayama, 2011; Mian & Sufi, 2012; Aaronson, Agarwal, & French, 2012; Parker et al., 2013; Agarwal, Marwell, & McGranahan, 2017). For reviews, see Jappelli and Pistarini (2010) and Fuchs-Schuendeln and Hassan (2016).

34 We have chosen fourteen days after the election as the cutoff as this will give the households a couple of weeks to spend the extra income they may get prior to the election.

35 We study food consumption because these items are purchased at a high frequency and therefore recorded in the daily diaries for most days.
The treatment to the election ends fourteen days after the election day (the cutoff). The treatment group consists of households that filled in daily diaries $2 \times \delta$ days before the cutoff. The households are in the “after” group during the $[0, \delta)$ days before the cutoff (which is set at fourteen days after the election) and in the “before” group during the $[\delta, 2 \times \delta)$ day before the cutoff. 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.

household income, but this share varies substantially across the income distribution. Within food consumption, the largest five product categories in terms of the share of expenditures are starch (28%), meat and fish (20%), fruits and vegetables (13%), dairy (8%), and sugar and confectionery (6%) products.

**Results.** We present the results in a sequence of diagrams. Figure 5a plots the average responses of total food purchases to the election treatment, and figure 5b 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 5a shows that total food consumption increases significantly around elections. The largest effect is for a treatment period of six days before and fourteen 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 2,000 AMD (for the twenty days). The smallest (significant) treatment effect (for $\delta = 14$) is 50 AMD per day, or 700 AMD over the fourteen days in this 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 from that reported by Mitra et al. (2017) for Indian elections.\(^{36}\)

Figure 5b, which consists of four subdiagrams, 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, that is, toward 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 0 (at the 5% level). For households in the second quartile, the treatment effect is 200 AMD, twice as large as the average effect.

Figure 6 disaggregates 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 disaggregated by income quartile. 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. Table A10 of the supplementary material provides a summary of these results.

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

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\(^{36}\)They report a 10% increase in spending on pulses prior to Indian elections.
consumption items, the effect is concentrated among households toward 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 microevidence from Armenia can be reconciled with the macroevidence on the election date effect.

V. Conclusion

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 postelection months in a sample of up to 104
Figure 6.—Estimates of 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

See the notes to figure 5.
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 the conversion of broader monetary instruments (from M2 or M3 to M1) or through the return of black economy cash to the banking system, leads 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 bolsters the credibility of the hypothesis. We find that the cycle is most pronounced in elections 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 is particularly rewarding for candidates. We also present microeconometric 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 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


VOTE BUYING OR (POLITICAL) BUSINESS (CYCLES) AS USUAL?


