

# Political Corruption Cycles in Democracies and Autocracies: Evidence from micro-data on extortion in West Africa\*

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## Abstract

Using two large cross-national micro datasets on extortion and commodity flows, I provide evidence of corruption cycles around elections in five West African states. In democracies but not in autocracies, police and other officials extort bribes that are 27% higher in the buildup to elections. These cycles occur on the intensive margin—the price at which bribes are set—rather than on the extensive margin—the total number of agents extorting. When incumbents lose, prices remain abnormally high. They only return to normal levels if incumbents win. I find no evidence of political cycles in the composition, quantity, or direction of commodity flows. This pattern of results lends little support to the claim that new democracies have higher corruption than autocracies because politicians use extortion for illicit campaign fundraising. Traditional political business cycles also do not appear to explain corruption cycles in this context. Rather, I argue that corruption cycles may result from independent decision-making by bureaucrats who need to insure against the uncertainty of future leadership.

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Several theories of government predict that elections reduce corruption because political competition enhances the accountability of politicians (Ferejohn, 1986; Myerson, 1993; Rose-Ackerman, 1999; Przeworski et al., 2000; Adsera, Boix, and Payne, 2003; Persson, Tabellini, and Trebbi, 2003). However, the statistical relationship between political competition and cross-national measures of corruption suggests a more complicated story. While broadly negative (Treisman, 2000, 2007), the relationship exhibits a non-linear, inverted U, shape: corruption is frequently higher in new democracies where elections put leaders' incumbency at stake than in autocracies where elections pose almost no threat to incumbents (Montinola and Jackman, 2002; Sung, 2004; Méndez and Sepulveda, 2006; Rock, 2009; Charron and Lapuente, 2010; Saha et al., 2014; McMann et al., 2017). McMann et al. (2017, 5) conclude their analysis with the claim that the mere introduction of competitive elections<sup>1</sup> “unambiguously increases corruption.”

The question is whether the apparent relationship between corruption and competitive elections is causal. The previously cited studies measure this relationship through broad cross-national comparisons at the country-year level, based on indices built from expert-coded indicators. Unobserved confounders might cause a country to have elections that threaten incumbency and to have high perceived corruption, without any direct causal relationship between the two. Moreover, it is unclear what mechanisms produce such non-linear relationships.

In this paper, I make use of a direct measure of corruption disaggregated to the country-day level in order to investigate the relationship between corruption and six elections taking place in the democracies of Ghana, Senegal, and Mali and two elections taking place in the autocracies of Burkina Faso and Togo. The data on corruption comes from surveys filled out by truck drivers on some 31,000 trips from 2006 to 2013, containing information on over 270,000 bribes extorted at checkpoints manned by police and other agents (Bromley and Foltz, 2011). To understand the data-generating process behind this dataset, I conducted participant observation with truck drivers. During a three-week period in 2014, I traveled over 800 miles of highway through Burkina Faso,

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<sup>1</sup>In this paper, I distinguish between competitive and non-competitive elections. The distinction does not rest on a claim about the closeness of the actual election race, but about whether the institutions make it *possible* for a leader to lose. In competitive elections in democratic states, incumbency of the leader is at stake, even if the incumbent ends up winning by a substantial margin. In autocratic states, winning margins may be lower but the election is not more competitive in the sense that there is no question of the incumbent actually losing.

Ghana, Togo, and Benin, observing police extortion from the cab of a truck. I also interviewed members of the survey team in Burkina Faso and Ghana—two hub points for the survey—to better understand risks of systematic measurement error.

With bribe-level data disaggregated by country and day, I am able to estimate political corruption cycles independent of a host of potential temporal confounders, such as seasonal and secular trends that predict both elections and corruption. The pre-determined election calendar mitigates concerns that elections are selected into low or high corruption periods.

As part of a growing literature on political cycles,<sup>2</sup> this paper demonstrates the existence of a *political corruption cycle*. Like Block, Ferree, and Singh (2003), who study political business cycles in Africa, I only find evidence that cycles occur in democratic states (see also: Eibl and Lynge-Mangueira, 2017). I estimate the average bribe extorted by police and other officials increases by seventy cents (USD) in the three months preceding elections in Ghana, Senegal, and Mali. Compared to non-electoral periods, this translates to a 27% proportional increase. In addition to two-way clustered standard errors that characterize uncertainty as deriving from hypothetical random samples of countries and periods, I employ a quasi-experimental approach to hypothesis-testing that simulates placebo elections in the respective countries thousands of times. I show that, under both of these approaches to uncertainty, the estimated effect size is highly unlikely to occur under the null hypothesis of no effect (on average or for all units).

To put the average effect into context, consider that truck drivers in Ghana were stopped at checkpoints an average of seventeen times per trip in the three months preceding the elections in 2008 and 2012. The estimate implies that drivers were paying twelve U.S. dollars more per trip during these periods—more than twice the daily wages of most truck drivers<sup>3</sup>—due to the election’s effect on extortionary prices. Using an additional micro-dataset on commodity flows in and out of

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<sup>2</sup>See, for example: Mandon and Cazals (2019) and Philips (2016) for reviews of the large literature on political business cycles; Toral (2019) on political bureaucratic cycles; and Harish and Little (2017) on political violence cycles.

<sup>3</sup>West African shipper’s unions estimate that drivers made from 50 to 150 USD monthly (1.5-5 USD daily) in 2010 (West African Trade Hub, 2010), which is in line with estimates provided by the drivers I spoke with in truck yards and border crossings in Ghana and Burkina Faso. Whether these costs are actually borne by drivers depends on a host of factors, including whether they own the truck and contract it, as opposed to working as a driver for a shipping firm. In the latter case, drivers are typically given *frais de route* (road funds) whose purpose is, among other things, to pay bribes. However, drivers complain those funds are often not sufficient, and that money paid in bribes diminishes what remains for their and their apprentices’ meals and other costs on the road.

Burkina Faso to calculate a minimum bound on the number of trucks traveling during this period, the estimates suggest pre-election cycles led police and other officials to place an additional 40,000 USD burden in bribe payments on shippers.<sup>4</sup> In a context where more than 8% of the transport and logistics costs of imports are thought to derive from bribes (West African Trade Hub, 2010), the estimates suggest a reverse political business cycle for consumers.

After illustrating the robustness of this democracy-specific political corruption cycle to a range of alternative approaches, I develop and test theories of why it exists.

Work cited in the first paragraph has advanced illicit campaign financing to explain why corruption is higher in new democracies than in autocracies. I draw on theories of bureaucratic and redistributive politics to derive two predictions consistent with this mechanism: first, a principal-agent dilemma should lead political principals to raise funds by increasing the *number* of checkpoints, rather than the bribe price; second, rational political fundraisers should leverage economic geography to extort foreign rather than domestic trade. Neither prediction is born out by the evidence.

Instead, the evidence suggests political corruption cycles are not *political*, in the sense that they result from individual decisions made by bureaucrats—police officers, customs officials, forestry agents, and gendarmerie—made independently of political influence. The increase in the total cost of shipping a container before an election does not result from an increase on the extensive margin—the number of agents extorting bribes. Rather, it results entirely from increases on the intensive margin—the average bribe price. This is telling because prices are much more easily controlled by street-level bureaucrats than by their higher-ups, whereas the number of checkpoints is more easily controlled by high-level principals than by street-level bureaucrats.

Moreover, bribe prices remain unusually high when incumbents lose and new leaders enter into power. They only fall back to usual levels when incumbents win reelection. This pattern of evidence is consistent with the idea that bureaucrats raise prices as part of an insurance strategy in the face

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<sup>4</sup>The Burkinabé Shipper’s Council micro-data indicates that 3,534 trucks carried goods to or from Burkina Faso through Ghana during this period. The data on bribes indicates that trucks were stopped an average of seventeen times per day in Ghana during the three-month periods before the 2008 and 2012 elections. Given the main effect size of 340 West African Francs and an exchange rate of 500:1, these figures suggest that elections caused officials to extort an additional  $\frac{340}{500} \times 17 \times 3534 = 40,853$  USD. In all likelihood, this estimate is a lower bound, as the Burkinabé Shipper’s Council data does not count imports and exports to Ghana or through Ghana to other neighboring countries, who would also have been using those roads.

of electoral uncertainty. Qualitative accounts suggest the obtention and retention of coveted public sector jobs is determined by who holds power in many West African states. New leadership often brings sharp changes in budgetary priorities that can reduce bureaucrats' salaries or result in arrears. As uncertainty about public sector income increases, so too, presumably, does the opportunity cost of not extorting. During periods of heightened political uncertainty, therefore, we would expect to see officials extorting higher bribes on average than during periods of comparative certainty. Of course, such patterns might also reflect pure opportunism on the part of bureaucrats, who take advantage of the relative inability of new leaders to monitor and control the bureaucracy. But I find no evidence of an opportunistic reaction to the sudden death of a president in Ghana.

Finally, I consider the idea that political *business* cycles (PBCs) could induce supply-side dynamics that produce political corruption cycles. For example, the change in extortionary prices might reflect a shift toward higher-value imports or perishables, as politicians seek to “make the year before election a happy one” (Paldam, 1979, 324),<sup>5</sup> especially in African democracies and not in autocracies (Block, Ferree, and Singh, 2003). I test for knock-on effects of PBCs by analyzing a micro dataset detailing all truck-based commodity flows into and out of Burkina Faso from 2010 to 2014 ( $N > 340,000$ ). I find no evidence that political corruption cycles are caused by changes in the direction, composition, or quantity of commodity flows.

With this paper, I demonstrate a connection between elections and petty corruption at the micro-level, and conduct a more detailed investigation of mechanisms than has been possible in previous cross-national studies of elections and corruption. The paper does not conclusively establish any one mechanism as an explanation for this connection, nor does it explain broad cross-sectional correlations in levels of corruption and political competition—doing so is beyond its scope. Nevertheless, the paper makes at least three important contributions. First, employing more minimal identification assumptions, it confirms the basic pattern from previous cross-national work on elections and corruption, which has found that corruption is higher when incumbents can lose elections than when they cannot. However, it casts doubt on one of the main mechanisms put forth but not tested in this literature: illicit campaign fundraising. Second, it shows that political corruption

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<sup>5</sup>Citation from Mandon and Cazals (2019)

cycles not only exist, they follow a similar pattern to traditional PBCs in that they only occur in democratic states. This does not, however, imply that corruption cycles are produced by PBCs. The paper instead highlights the many additional ways in which an election might affect an economy, independent of any deliberate political influence. Finally, in proposing bureaucrats' uncertainty about future leadership as a mechanism, the paper contributes to a growing literature on the adverse effects of leader turnover.<sup>6</sup> Taken at face value, the findings suggest bureaucratic insulation may be a potentially understudied determinant of corruption. I highlight avenues for future research into the idea that civil service reforms can complement democratization efforts by staving off the adverse effects of increasing leader turnover.

## **Extortion in West Africa**

As truck drivers carry goods between ports on the coast and hinterland cities throughout West Africa, they are typically stopped dozens of times per trip by police, customs, gendarmerie, and other agents of the state such as forestry and road safety officials. On most stops, the driver must pay a bribe ranging anywhere from fifty cents to twenty dollars (USD), under threat of various sanctions. Those include long delays, physical violence, and even unlawful detention.

The data on corruption used in this study was collected by an organization funded by USAID and the Economic Community of West African States (ECOWAS) called the West African Trade Hub (WATH), under the auspices of the Improved Road Transport Governance (IRTG) initiative. In order to better understand the data-generating process and address any potential concerns with the data, I conducted in-depth interviews with the survey enumerators in Ghana and Burkina Faso, the two countries that served as "hub points" for distributing and collecting surveys. I also carried out two months of fieldwork with truck drivers in Burkina Faso, Ghana, Togo, and Benin, participating in three long-distance hauls along the main trade corridors with drivers of various nationalities. I supplemented these journeys with over sixty in-depth interviews with stakeholders in the trucking

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<sup>6</sup>However, turnover in leadership can have deleterious effects on policy outcomes due to the uncertainty it creates about future decision-making. Inspired by the early insights of Olson and his coauthors (Olson, 1993; Clague et al., 1996), a more recent wave of studies has analyzed the effects of leader turnover on a range of policy areas. Several authors have found that long-standing leaders do a better job of attracting investment, even in autocracies, because they can more credibly commit to protecting private interests when they stand to gain from long-run economic growth (Wright, 2008; Kendall-Taylor, 2011; Fails, 2014; Moon, 2015).

industry, including union representatives, drivers, and public agencies working on trade facilitation.

The WATH dataset details some 299,674 self-reported incidents at which truck drivers were stopped at a checkpoint by officials during the seven-and-a-half-year period from early 2006 to mid-2013.<sup>7</sup> Not all of these stops record bribes: drivers were also asked to note when they encountered a checkpoint but were not asked to pay anything—in this case the data contains a bribe of 0 (N = 29,541). Bribes are expressed in three currencies in the data: West African Francs (XOF), pre-reform Ghanaian Cedis (GHC) and post-reform Ghanaian Cedis (GHS). As most bribes are reported in XOF, I use monthly average exchange rates to convert GHC and GHS into XOF. In 2010, 500 XOF was roughly equivalent to 1 USD. Some recorded bribe amounts are implausibly large, likely due to coding errors. To avoid outlier effects, I construct weekly averages from driver averages, and winsorize the panel data from 0 to 200 USD. The results are robust to not winsorizing (see Section B.2 of the SI).

During the data collection period, teams of two to three enumerators would sample truck drivers using a random-walk methodology at ports and truckyards in Ghana, Mali, Burkina Faso, Senegal, and Togo.<sup>8</sup> It is generally easy to predict which route the driver will take from a given departure point, as there are typically only one or two main trade corridors suitable for freight trucks (see Figure 1). The surveys are thus corridor-specific, listing locations at which the drivers may encounter checkpoints on the route, and leaving space for the drivers to enter their own checkpoints in case they are stopped at points not pre-listed. Enumerators used cellphones to coordinate with each other at opposite ends of the trade corridor to collect the surveys filled out by the drivers.

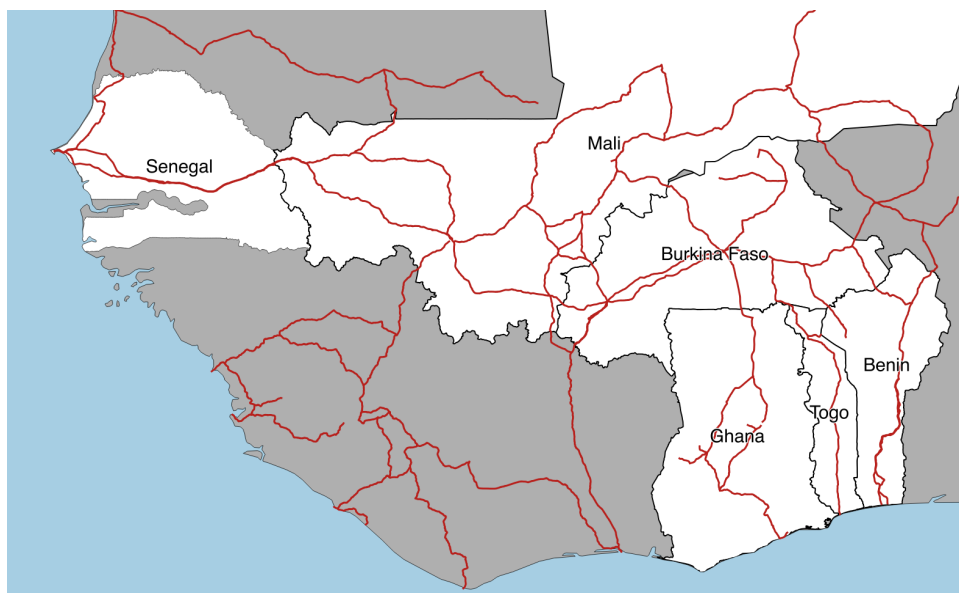
In terms of the sample frame, literate and illiterate drivers alike were included in the study. Illiterate drivers would either have their apprentice help them to fill out the survey or would do so with enumerators at the end of the trip. Because the original study targeted *illegal* forms of payment, it was restricted to drivers who had their official papers in order. This likely leads to an under-estimate of the average bribe paid by *all* drivers, as drivers might pay more in bribes to travel

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<sup>7</sup>The data from 2006 to 2012 was made publicly available on the WATH website, and has been used in studies of transport costs (Bromley and Foltz, 2011). Here, I supplement the dataset with additional data provided by Borderless Alliance (part of WATH), which covers the periods from 2012-2013.

<sup>8</sup>There are a small number of observations collected in Côte d'Ivoire, Benin, and Niger. However, the temporal coverage is too low to contribute usefully to the analysis, so I exclude these observations.

without their paperwork in order. Subsetting the sample to drivers with papers is advantageous, however, because it minimizes the chance of fines being miscoded as bribes.



**Figure 1:** Trade Highways in West Africa.

Countries in white included in the sample, red lines indicate major highways. Shapefiles for administrative boundaries from <http://www.gadm.org/> and for supply routes from <https://geonode.wfp.org/>

Importantly, drivers were not remunerated for their work and so were not financially incentivized to provide socially desirable responses. The drivers I spoke to seemed to have participated in the study because they saw it as an important way to shed light on the issues they face.<sup>9</sup>

One potential source of concern is that drivers have an incentive to exaggerate the amount they pay in order to bring greater attention to the problem of extortion. To address this concern, I traveled with drivers observing the bribes extorted from a hidden vantage point in the cab. On average, bribes taken were often higher than those reported in the data, suggesting that—if anything—surveys understate how much drivers pay. Assuming that any systematic error in reporting is not correlated with electoral periods, inferences about marginal effects remain unbiased. Nevertheless,

<sup>9</sup>Drivers either work for a transport company or are self-employed. Those who work for a company receive a commission for each haul, but they also keep any money left over from their travel allowance (*frais de route* in Francophone countries). The travel allowance is intended to cover the payment of bribes and other variable costs (the driver's meals, his apprentice's daily salary, etc.). Any surplus left from the travel allowance is income for the driver. Those who are self-employed make money from haulage contracts and keep as income whatever is not spent on operating costs, such as bribes, repairs, and fuel. Thus, regardless of whether a driver is self-employed or works for a transport company, the driver's personal income is reduced every time the driver pays a bribe.



drivers could exaggerate bribes around election times as a way of expressing discontent with their government. If anything, however, the effects of elections are larger (though not significantly so) for drivers who are foreigners in the country they are paying bribes, which mitigates the concern that partisan reporting bias drives the results.

At the country-driver-day-level, the panel is unbalanced in the sense that some country-days contain several observations and others contain none. Therefore, for the main analyses I construct a country-week panel of average bribes over the period from November 2006 to June 2013. I collapse firstly to the country-week-driver-level means (all drivers in the analysis record checkpoints in more than one country), weighting all driver-level averages equally, and then average to the country-week level, again weighting all country-weeks equally.

The panel is still missing data for some country-weeks. According to interviews with enumerators and survey coordinators, these periodic breaks in data collection were due to causes unrelated to election cycles, such as staffing issues and unforeseen delays in funding approval. Nevertheless, as missing data can constitute a source of bias if related to the outcome of interest (King et al., 2001), I use linear interpolation as implemented in the `imputeTS` package for R in order to impute missing country-weeks (Moritz and Bartz-Beielstein, 2017). In the main analyses, I report the results with and without interpolations, to illustrate that the results do not depend on interpolation. Finally, because Senegal and Mali present specific data challenges, I show in Section B.4 and B.3 of the appendix that the results do not depend on the inclusion of either country. In fact, they are stronger when excluding Senegal because most of the data around the 2007 election is interpolated, and so adds an estimate of zero to the estimated average effect.<sup>10</sup>

## Elections in West Africa

Over the period covered by the WATH bribe data, a total of eight presidential elections took place in Burkina Faso, Ghana, Mali, Senegal, and Togo. They are listed on Table 1.

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<sup>10</sup>For Senegal, there is very little data surrounding the 2007 election. In the main analysis, the estimate for electoral effects is biased downward by the interpolations. For Mali, the data collection ended before post-electoral data could be collected for the election in 2013, so I am only able to estimate the pre-electoral period for that election.

Country	Year	Vote Margin	Incumbent reelected
Burkina Faso	2010	0.72	Yes
Ghana	2008	0.01	No
Ghana	2012	0.02	Yes
Mali	2007	0.52	Yes
Mali	2013	0.20	No
Senegal	2007	0.41	Yes
Senegal	2012	0.08	No
Togo	2010	0.27	Yes

**Table 1:** Presidential elections analyzed.

Despite their geographic proximity (see Figure 1) and the fact that these five countries all hold elections, the threat that those elections posed to incumbency varied greatly. In their report on regimes around the world in 2010, the group behind the Polity IV regime measure classified Ghana, Mali, and Senegal as democracies because, despite imperfect electoral competition, there was some real chance of incumbents losing (Marshall and Cole, 2011). For example, the 2008 presidential election in Ghana was the closest ever as of 2019, with the incumbent NPP party losing its hold on the presidency to the NDC candidate John Atta Mills by less than 1% in runoff elections. While elections in 2007 in Senegal and Mali featured large margins for the winners and the results were contested by some of the losing parties, in general international observers agreed the elections were conducted in a free and fair manner, attributing the winning candidates’ wide margins to their popularity and to low turnout. Despite the wide margins that make the outcomes appear obvious *ex post*, both elections were very hard to predict. Almost no polling data was available in the buildup to either election.

By contrast, those democratic states’ direct neighbors, Togo and Burkina Faso, were classified as autocracies during this period. The incumbent President Blaise Compaoré predictably won the 2010 election in Burkina Faso with over 80% of the vote. The elections were widely criticized as unfair and mired by fraud allegations from international observers, with one reporter dismissing them as “little more than a formality.”<sup>11</sup> Elections in Togo were similarly criticized for their predictability, lack of competition, and strong signs of manipulation.

<sup>11</sup>Cristophe Châtelot, “Burkina Faso’s president is in a league of his own” in *The Guardian*, 12/30/2010. <https://www.theguardian.com/world/2010/nov/30/burkina-faso>

The differences in competition in these countries translate into strong differences in leader tenure. When Blaise Campaoré stood for reelection in 2010, he had been in power for twenty-three years. Similarly, when Faure Gnassingbé inherited the presidency following the death of his father Eyadéma Gnassingbé in 2005, he prolonged his family’s forty-three year reign over Togolese politics. By contrast, no leader in the three democratic countries served more than two constitutionally mandated terms over the period under analysis.

## Estimating Political Corruption Cycles

In keeping with the literature on political business cycles (see footnote 2), I conceptualize political cycles as temporal windows before and after elections during which election-related activities exert contemporaneous influence over individuals’ behavior, e.g. causing police to temporarily extort more. The question of interest is thus whether being inside the temporal window around an election has a causal effect on extortion of bribes by the police and other forces.

In the main set of results, I focus on the average amount extorted per bribe in the three-month periods before and after an election. An alternative approach is to consider, for example, the number of days to the closest election. However, not only does this require imposing strong functional form assumptions (such as linearity in the sum of polynomials), but the definition of the treatment variable and its support will vary between countries depending on the number of elections they hold and the latency between elections. I therefore use a three-month bandwidth in the main analyses. This allows for an intuitive 6-month window around elections, in line with other political cycle literature (e.g., Akhmedov and Zhuravskaya, 2004; Toral, 2019; Treisman and Gimpelson, 2001). I show that results are robust to the specification of a wide range of alternative bandwidths.

Formally, consider trips, indexed  $i$ , that can be divided into  $C$  countries and  $T$  periods. For every  $(c, t) \in \{1, \dots, C\} \times \{1, \dots, T\}$  country-period cell, let  $N_{c,t}$  denote the number of observations in country  $c$  at period  $t$ . In general, for a given variable  $X_{i,c,t}$ , let  $\bar{X}_{c,t} = \frac{1}{N_{c,t}} \sum_{i=1}^{N_{c,t}} X_{i,c,t}$ . Denote the total number of country-period observations  $N = C \times T$ . Let  $Y_{i,c,t}$  denote the outcome (e.g., average bribe paid) for the  $i$ ’th trip in country  $c$  at time  $t$ , and  $\bar{Y}_{c,t}$  the average of that outcome across trips taking place in country  $c$  at time  $t$ .

Treatment is defined by a temporal bandwidth around elections. Let  $(c^*, t^*)$  denote country-

period cells for which an election takes place in country  $c^*$  at time  $t^*$ . For all  $(c, t)$  and  $(c^*, t^*)$ , let

$$\underline{D}_{c,t} = \begin{cases} 1 & \text{if } 0 < t^* - t \leq b \\ 0 & \text{otherwise,} \end{cases} \quad \bar{D}_{c,t} = \begin{cases} 1 & \text{if } 0 \leq t - t^* < b \\ 0 & \text{otherwise.} \end{cases} \quad (1)$$

$\underline{D}_{c,t}$  and  $\bar{D}_{c,t}$  are thus binary indicators for whether  $(c, t)$  falls into a pre-election or post-election period, respectively, as defined by the bandwidth,  $b$ . Using the potential outcomes notation,  $Y_{c,t}(\cdot)$ , in which the outcome in country  $c$  at time  $t$  is a function of some causal parent or treatment, we can define two estimands:

$$\tau = \frac{1}{N} \sum_{c,t} Y_{c,t}(\underline{D}_{c,t} = 1, \bar{D}_{c,t} = 0) - Y_{c,t}(\underline{D}_{c,t} = 0, \bar{D}_{c,t} = 0), \quad (2)$$

$$\bar{\tau} = \frac{1}{N} \sum_{c,t} Y_{c,t}(\underline{D}_{c,t} = 0, \bar{D}_{c,t} = 1) - Y_{c,t}(\underline{D}_{c,t} = 0, \bar{D}_{c,t} = 0), \quad (3)$$

That is, the average effect of being in a pre-electoral period versus a non-electoral period ( $\tau$ ) and the average effect of being in a post-electoral period versus a non-electoral period ( $\bar{\tau}$ ), taken over every country and period in the sample.

To identify these estimands, we want to find the set of variables,  $Z$ , that blocks all backdoor paths from  $D$  to  $Y$  (Pearl, 2000). Equivalently, we wish to find the set of covariates,  $Z$ , such that potential outcomes are ignorable with respect to election periods:  $[Y_{c,t}(0, 1), Y_{c,t}(1, 0), Y_{c,t}(0, 0)] \perp\!\!\!\perp [\underline{D}_{c,t}, \bar{D}_{c,t}] \mid Z$ .

I assume that elections are caused to occur in  $(c^*, t^*)$  by three principal variables. First, the country: elections may occur at a higher rate in some countries than in others. Second, the season: elections may occur more often in December, for example, than in May. Third, trends: elections may tend to happen later than earlier in the series. I assume that this set of variables, denoted  $Z$ , also exert a causal influence on bribes and thus pose a risk of confounding. In the econometric specification detailed below, I condition analyses on country and period fixed effects, which flexibly account for seasons, trends, and country-level differences.

Of course, many other variables are correlated with election timing and corruption. For example, the time of year may cause economic trends that in turn cause more or less corruption. I denote this set of variables  $U$ , for unobserved. The key assumption I make with respect to this class of unobserved variables is that they do not exert any causal influence on the timing of elections. This assumption could be violated if, for example, elections were endogenous to economic conditions, as appears to be the case in some polities (Heckelman and Berument, 1998). This does not appear to be the case for any of the elections in this sample.

Many policy choices that affect corruption may be made conditional on *election* outcomes. For example, the election may usher in new leadership that seeks to reduce corruption. Variables falling into this set, denoted  $M$ , however, are mechanisms to be investigated, not biasing confounders.

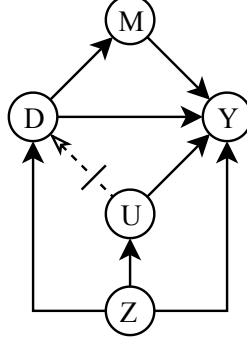
These identification assumptions are represented in the Directed Acyclic Graph (DAG) on Figure 2. For simplicity, the nodes represent sets of variables and I omit exogenous disturbances. The dashed arrow represents an edge assumed not to exist: this is the assumption that election timing is caused by trends, seasons, and countries and is not endogenous to other variables that also affect corruption. Under the assumptions encoded in the DAG, the total effect of  $D$  on  $Y$  can be estimated by controlling for the set of variables in  $Z$ .

Under these causal identification assumptions, I seek to estimate the estimands in equations 2 and 3 using linear least squares to solve the following equation:

$$Y_{c,t} = \lambda_c + \gamma_t + \underline{D}_{c,t}\hat{\underline{I}} + \bar{D}_{c,t}\hat{\bar{r}} + \epsilon_{c,t}, \quad (4)$$

where  $\lambda_c$  is an unknown country fixed effect,  $\gamma_t$  is an unknown period fixed effect, and  $\epsilon_{c,t}$  is an error term. I also estimate a model in which pre- and post-election effects are allowed to vary according to whether elections in the country are completely non-competitive (Burkina Faso, Togo) or are at least somewhat competitive (Ghana, Mali, Senegal). Superscripting variables and parameters with  $\mathcal{D}$  for democracy, and  $\mathcal{A}$  for autocracy:

$$Y_{c,t} = \lambda_c + \gamma_t + \underline{D}_{c,t}^{\mathcal{D}}\hat{\underline{I}}^{\mathcal{D}} + \bar{D}_{c,t}^{\mathcal{D}}\hat{\bar{r}}^{\mathcal{D}} + \underline{D}_{c,t}^{\mathcal{A}}\hat{\underline{I}}^{\mathcal{A}} + \bar{D}_{c,t}^{\mathcal{A}}\hat{\bar{r}}^{\mathcal{A}} + \epsilon_{c,t}, \quad (5)$$



**Figure 2:** Causal assumptions behind identification strategy.

Circles represent sets of random variables, solid arrows represent unidirectional causal paths, and crossed arrow represents specific causal path assumed to not exist (all other non-represented arrows, for example  $Z \rightarrow M$ , are also assumed not to exist).  $Z$  represents the country or period of an observation,  $D$  represents a variable indicating that an election occurred at an observation,  $U$  is a set of unobserved variables that are caused by the country or period, such as weather or economic conditions,  $M$  represents a set of mediators through which the timing of elections affect extortion, which is denoted  $Y$ . The subgraph  $D \leftarrow Z \rightarrow Y$  implies that country and period ( $Z$ ) exert a causal effect on both election timing ( $D$ ) and extortion ( $Y$ ). Country and time also exert a causal influence over extortion that is mediated by unobserved variables,  $U$ :  $Z \rightarrow U \rightarrow Y$ . For example, some countries may experience pressure from international partners to reduce petty corruption—the assumption embedded in the blocked arrow is that such variables do not also determine the timing of elections in this sample (though this assumption will not hold elsewhere). Election timing is thought to have both direct effects on extortion ( $D \rightarrow Y$ ) and mediated effects on extortion ( $D \rightarrow M \rightarrow Y$ ). Under these assumptions, conditioning on  $Z$  is sufficient to block all backdoor paths biasing estimates of the total effect of election timing on extortion.

This specification and the one above are two-way fixed effects (TWFE) estimators. The TWFE estimator is also known as the generalized difference-in-differences model, because  $\hat{\tau}$  estimates the causal effect of  $D$  on  $Y$  as the weighted average of every two-period difference-in-difference in the sample (Angrist and Pischke, 2008). A recent body of literature has pointed to possible estimation bias that arises with TWFE estimators when units enter into treatment at different times and treatment effects vary by unit or time, as they are assumed to here (e.g., de Chaisemartin and D’Haultfoeuille, 2019; Humphreys, 2009; Goodman-Bacon, 2018; Imai and Kim, 2018). In Section B.1 of the appendix, I investigate the potential for bias arising from the estimation strategy and use a simulation study to estimate the bias that would arise under very pessimistic assumptions. Briefly stated, because the units enter into and out of treatment relatively quickly and because unit-periods have relatively equal probabilities of assignment to treatment, the particular conditions that typically give rise to bias in other applications do not hold here. I also show that the weights

described in de Chaisemartin and D’Haultfoeuille (2019) are positive in all specifications.

One concern is that the bribe paid at time  $t - 1$  may be correlated with the average bribe paid at time  $t$ . In the main results, I supplement the two estimators to account for serial correlation through the inclusion of a one-period lag of the dependent variable. Section C.2 illustrates that the partial autocorrelation of the series is statistically insignificant beyond the second lag in most countries. In Section B.5 of the appendix, I show that the main results are robust to the inclusion of up to three lags.

### Estimation of Variance

Under the assumptions of the identification strategy, the timing of elections is conditionally independent of potential outcomes:  $[Y_{c,t}(0,1), Y_{c,t}(1,0), Y_{c,t}(0,0)] \perp\!\!\!\perp [\underline{D}_{c,t}, \bar{D}_{c,t}] \mid Z$ . In other words, elections are as-if randomly assigned to country-weeks.

The experimental analog for the study is one in which countries are blocks and groups of country-weeks are cluster-assigned to be in electoral periods through the election calendar. I report two-way clustered standard errors in all tables, which allow for error dependence in both periods and countries. These are conservative with respect to the quantity of interest (the standard deviation of the estimates given a fixed population and random elections) because they model random sampling of countries and periods (see section B.1 of the appendix for evidence of this).

As a secondary way of modeling variance in the estimates, I simulate the variation that would arise from randomization of country-periods to elections, holding fixed the sampling of countries or dates. Specifically, I generate the distribution of possible election effects under the sharp null of no electoral effects for any country-week by permuting 2,000 placebo elections between December 2006 and May 2013. On each permutation, I preserve the actual number of elections that took place in each country (two in Ghana, Mali, and Senegal, one in the other countries). The distribution of placebo estimates is presented graphically.

### Main Results: Evidence for Political Corruption Cycles

Table 2 reports the main results: strong evidence that the average bribe extorted increases in the three months preceding elections, especially in countries where elections are competitive. Figure 3

illustrates that these results do not depend on the choice of bandwidth and are highly unlikely to arise by chance under the assumptions of the design.

Column 1 of Table 2 reports the main specification from Equation 4, with the full balanced panel. Recall that 500 West African Francs (XOF) is equal to one USD using contemporaneous exchange rates. Averaging across all countries in the sample, the average bribe paid by drivers is estimated to be  $260 \text{ XOF} / 500 \text{ USD} \times 100 = 52$  cents higher in the three months preceding any election. The average bribe paid in non-electoral periods was  $1,266 \text{ XOF} / 500 \text{ USD} = 2.5$  dollars, indicating that police and other officials increased prices by  $.52 / 2.5 \times 100 = 21\%$  in the buildup to the seven elections across the countries in the sample. The effect size is substantial compared to the estimated variation in effect size estimates across hypothetical random samples of countries and weeks, suggesting we can reject the null hypothesis of zero average effect at the  $\alpha = .05$  level.

By contrast, bribes in the post-election period are estimated to be  $51 / 500 \times 100 = 10$  cents higher than the average for non-electoral periods, which I estimate to be no greater than one would expect to observe due to random sampling variability. In other words, I find no evidence of an effect of the post-election period. One obvious interpretation is that corruption increases only in the buildup to the election, and falls back to average levels in its aftermath.

Columns 2 and 3 present two kinds of robustness checks on the results presented in Column 1. In Column 2, I report the coefficients from Equation 4 using only the observed data, with no interpolations. The size of the effect is about 8% smaller and the estimated sampling variability is greater, but the result is consistent with the main finding of a strong and statistically significant increase in the average bribe extorted in the buildup to elections. The results do not depend on interpolation. In Sections B.2 and B.3 of the appendix, I show that they do not depend on winsorizing or on the large average bribes observed in Mali, either.



	<i>Dependent variable:</i>					
	Average Bribe Paid					
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Election	260.360** (105.849)	239.862* (127.684)	126.332** (52.736)			
Post-Election	50.647 (95.290)	36.219 (95.674)	19.273 (52.038)			
Pre-Election (autocracies)				60.994 (73.666)	79.431 (71.039)	38.256 (41.585)
Post-Election (autocracies)				−53.818 (117.829)	−41.348 (124.303)	−32.541 (65.851)
Pre-Election (democracies)				339.675*** (106.714)	360.316*** (126.099)	169.173*** (65.474)
Post-Election (democracies)				101.493 (111.675)	114.151 (109.580)	49.694 (65.341)
Country FE	Y	Y	Y	Y	Y	Y
Period FE	Y	Y	Y	Y	Y	Y
Interpolation used	Y	N	Y	Y	N	Y
Interpolation FE	Y	NA	Y	Y	NA	Y
Lagged DV	N	N	Y	N	N	Y
Observations	1,770	1,284	1,765	1,770	1,284	1,765
Adjusted R <sup>2</sup>	0.678	0.673	0.749	0.680	0.676	0.750

*Note:*

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 2:** Extortion of truck drivers exhibits political corruption cycles in democracies.

All data aggregated from the checkpoint-driver-day level to the driver-day-level and then to the country-week level through arithmetic averaging. Missing country-weeks imputed through linear interpolation. Standard errors and  $p$ -values are calculated using two-way clustering at the country-week level. In columns labeled ‘Y’ for ‘Country FE’, ‘Period FE’, and ‘Interpolation FE’, fixed effects for countries, periods, and interpolation are included (‘N’ otherwise). In columns labeled ‘N’ for ‘Interpolation used’, interpolated observations are not used, ‘Y’ when they are used. ‘Pre-Election Period’ and ‘Post-Election Period’ are 1 if bribe paid in three months preceding or following an election in that country, respectively, 0 otherwise. ‘Democracies’ are Ghana, Senegal, and Mali; ‘Autocracies’ are Togo and Burkina Faso (Marshall and Cole, 2011).

Column 3 presents the results including one lag of the dependent variable. The inclusion of a lag reduces the size of the pre- and post-election estimates below 50% of their original value in the main regression. It is well-known that this diminution can result from bias.<sup>12</sup> Thus, caution should be taken in interpreting these coefficients; I treat the results of Column 1 as the best guess of the true effects. The effect estimated conditional on the previous week’s average bribe suggests a more modest increase in average bribes prior to elections, equal to  $126 / 500 \times 100 = 25$  cents, a ten percent increase. Nevertheless, the effect is statistically at the  $\alpha = .05$  level. I infer that pre-election periods increase the average amount extorted, even when accounting for the previous week’s bribes.

Column 4 presents the results using the specification outlined Equation 5: pre-electoral increases appear driven by the countries whose elections pose a threat to incumbency. Relative to a model in which the effect of pre-election periods is assumed constant across polities, as in Column 1, the effect of the pre-election period is almost one-third larger. Given the average bribe of 1,269 in non-electoral periods in the democratic countries, the  $340 \text{ XOF} / 500 \text{ USD} \times 100 = 68$  cent increase translates to a 27% increase in extortion relative to prices outside of the election period. The effect is statistically significant at the  $\alpha = .01$  level.

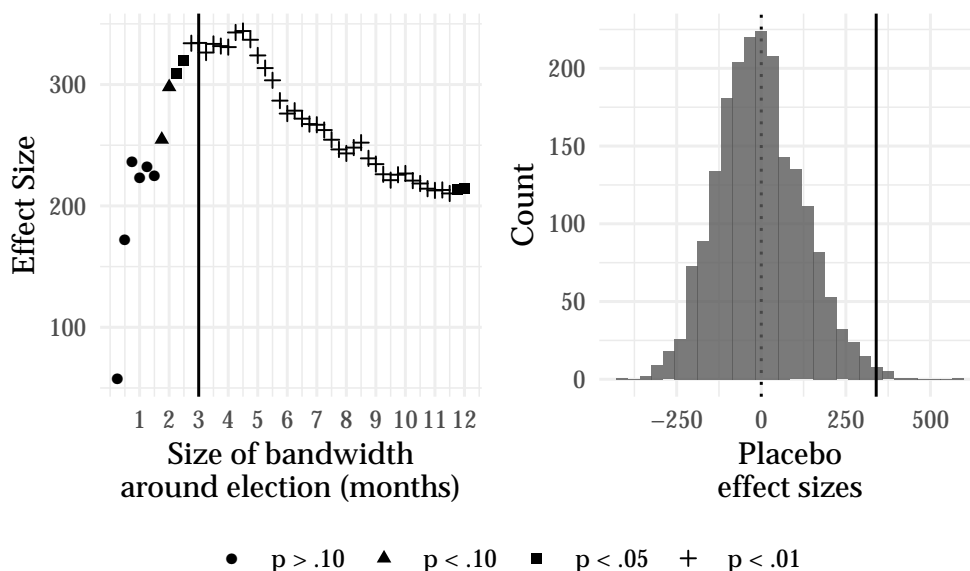
This effect is five times larger than the increase estimated in autocratic states, which is not statistically distinguishable from zero. In other words, evidence of political corruption cycles is limited to democracies. Columns 5 and 6 show that these results do not depend on interpolation or the exclusion of autoregressive components.

On the first panel of Figure 3, I show that the results also do not depend on the choice of bandwidth around elections. Each point represents an estimated democratic pre-election effect from the specification in Column 4 of Table 2 (‘Pre-Election (democracies)’). Circles indicate that we cannot reject the null of no effect and other shapes indicate differing levels of statistical significance. The vertical bar represents the bandwidth chosen for this study (three months). For any bandwidth greater than two months and less than eleven months, the effects are large and statistically significant. The largest effect is observed when using a five-month bandwidth, and

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<sup>12</sup>If there is no autoregression in the outcome, but serial correlation in the errors, for example, then the inclusion of a lagged dependent variable proxies for serial correlation in unmeasured variables and for any trending in the independent variables (Achen, 2000; Keele and Kelly, 2006).

diminishes as an increasingly larger set of months is included in the period defined as pre-election.



**Figure 3:** Robustness of estimate of pre-election effect in democracies to different specifications of bandwidth around election (left) and distribution of effects given simulated randomly occurring elections (right).

The second panel illustrates that, although the choice of bandwidth does not appear to matter greatly, it is not as though any set of periods chosen at random could have easily produced such effects. The plot represents the distribution of 2,000 estimates obtained by permuting random elections in each country and re-estimating the effects in Column 4 on each draw. Each bar represents the frequency of effects estimated within a certain bin, for example, effects between 100-150 XOF. The distribution is centered at zero because the permutation breaks any true relationship between the observed bribes and the timing of elections, providing the distribution of the estimates under the null of no effect for any unit in any period. Here, rather than hypothetical random samples, the variance in estimates arises from hypothetical random assignments of country-weeks to elections across a finite set of countries and periods. Given this hypothetical variation, small effects are somewhat likely under this null (e.g., 0-150), but the observed effect size of 340 XOF was highly unlikely. The fraction of placebo effects as large in absolute value is below the  $\alpha = .05$  level. In sum, I infer that there is evidence of political corruption cycles within those countries that have elections incumbents can lose.

## Mechanisms

The previous sections have presented a dataset and empirical strategy for investigating the existence of political corruption cycles. Employing less stringent identification assumptions than those used in previous work assessing the relationship between corruption and elections, I find police and other officials extort bribes that are 27% higher in the buildup to elections, but only in countries where incumbents can lose. I consider several candidate explanations for such cyclicity.

### **Do politicians use extortion for illicit campaign finance?**

In explaining why elections might increase (perceived) corruption, some authors cited in the introduction point to the possibility that petty corruption is used as a form of illicit campaign finance. Sung (2004, 181), for example, points to “the enormous costs of mounting electoral campaigns” in explaining why elections might increase corruption, while McMann et al. (2017, 4) argue that “introduction of elections, regardless of how free and fair they are, motivates government officials to engage in illicit activities to raise funds for garnering political support.”

Indeed, though it was never mentioned to me as a possibility during fieldwork in West Africa, a small but growing literature suggests that illicit campaign finance through embezzlement and extortion may be an important feature of young democracies (Gingerich, 2010; Kapur and Vaishnav, 2011). Sukhtankar (2012), for example, shows that political candidates in India embezzle funds from sugar mills around elections. While these incentives might be stronger in countries where elections represent a true leadership contest, this does not rule out the possibility that petty corruption funds elections in autocratic states, where elections can play an important set of institutional functions (Magaloni, 2008; Blaydes, 2010), and where firms have been shown to benefit from expensive electoral funding cycles (Mironov and Zhuravskaya, 2016). In principle, therefore, incumbent parties or leaders in both autocracies and new democracies may use the bureaucracy to gather additional revenues for use in campaigns (Doig, 1999; McMann et al., 2017), although these incentives may be stronger where competition creates the need for greater spending.

Raising money in this way poses at least two major challenges that I leverage to develop observable implications: the first is a principal-agent problem; the second is a redistributive problem.

Turning first to the principal-agent problem, it can be very hard for principals—politicians who need campaign funding—to monitor the behavior of their agents—bureaucrats who extort bribes. Agents and principals likely have antagonistic preferences, insofar as the agent bears a cost for effort exerted on behalf of the principal. This is particularly true of road extortion, which can be a physically exhausting and dangerous task. The victims of extortion have at their disposal counter-strategies ranging from negotiation to violent resistance. During my participatory observation with truck drivers, I witnessed numerous holdout situations.<sup>13</sup> And, of course, agents have an incentive to pilfer some proportion of extorted funds, rather than pass the entirety up the chain.

Given this incentive structure, unobservability of bureaucratic effort likely makes the task of raising additional funds through extortion as difficult as any other unobservable delegated task (Gailmard and Patty, 2012). One response to such dilemmas is *redundancy* (Ting, 2003): by increasing the number of agents devoted to achieving a given task, one can increase the likelihood of its being achieved successfully. In the context of highway extortion, a rational politician may find it easier to raise funds by increasing along the extensive margin—the number of checkpoints on a given stretch of road—rather than along the intensive margin—the price. While extortionary prices are impossible to observe from afar, it is simple to count the number of checkpoints that are set up by driving along the road.

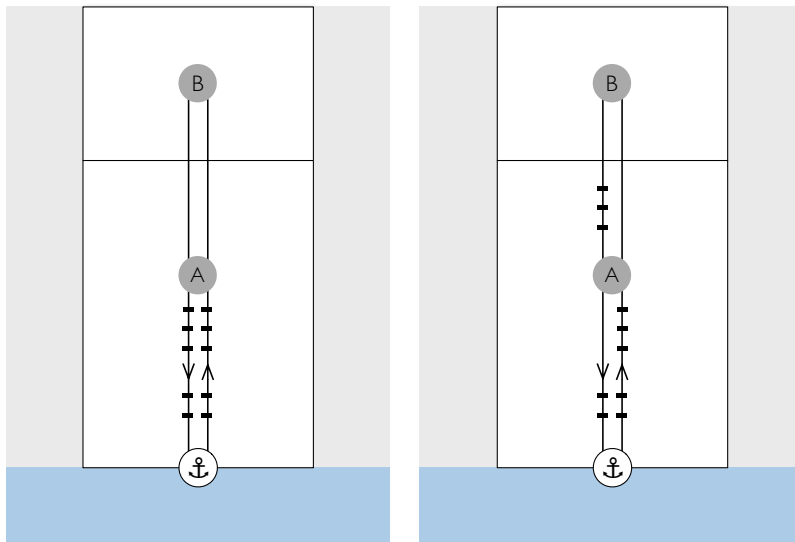
The second challenge to raising illicit campaign funding arises from classic redistributive theory: rational politicians raise campaign funds unevenly across the pool of taxable voters so as to maximize their chances of winning (Dixit and Londregan, 1996; Snyder, 1989). Particularly in democracies, the challenge is to extort those outside the politician’s coalition and protect those within it. Again, there is a problem of observability with respect to fundraising effort—rather than monitoring the amount of effort, rational politicians want to monitor and reward the differential concentration of extortionary effort among non-coalition members. On international trade corridors, an obvious way to do this is to target foreign imports and exports.

Figures 4 and 5 represent a simplification of the trade network presented on Figure 1. The diagrams illustrate ways in which a politician could use police and other officials to extort addi-

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<sup>13</sup>Police would sometimes wait hours for the more resistant drivers to produce the demanded bribe, arguing all the while.

tional campaign funds given the principal-agent and redistributive problems. Grey circles  $A$  and  $B$  represent the main trading centers in countries  $A$  and  $B$ , white circles with anchors are seaports, thin vertical lines are trade corridors, and small horizontal bars are checkpoints. Suppose, for example, that the diagram represents Ghana as country  $A$  and Burkina Faso as country  $B$  (i.e., the grey circle in  $A$  is akin to Kumasi and in  $B$  is akin to Ouagadougou—the other countries in the sample have roughly equivalent structures, even with no seaport). In Ghana, there are few major import and export hubs located between cities  $A$  and  $B$ —most of the trucks driving this road are for imports and exports to Burkina Faso. If each checkpoint charges one dollar, the marginal cost from extortion is equal for foreign and domestic exports on Figure 4, but six dollars higher for foreign exports on Figure 5. Thus, a principal who can observe and control how many checkpoints there are *and* where they are located can independently manipulate both the amount raised and the burden placed on voters versus foreigners. My qualitative fieldwork indicates foreign-targeting also works through simple identification of foreign trucks using license plate registrations.



**Figure 4:** Equal trade extortion.

**Figure 5:** Higher foreign export extortion.

Two observable implications of this theory are that, if political principals want to use the bureaucracy to raise illicit campaign funds, good ways to do so are by manipulating the number of checkpoints and the degree to which officials at checkpoints target foreigners.

On Table 3, I present the results of tests for these observable implications. Beginning with column 1, I report results on the average number of checkpoints a driver encounters on any given trip. In autocratic states, drivers encountered nine checkpoints on average during non-electoral periods. In relative terms, the pre-electoral average was only 2% higher—a result that is highly likely under the null of no electoral cycles ( $p = 0.947$ ) given variability in the data.

	Total Number of Checkpoints Encountered		
	All Trucks	Domestic Trucks	Foreign Trucks
	(1)	(2)	(3)
Pre-Election (autocracies)	0.151 (1.893)	0.193 (1.451)	0.067 (1.769)
Post-Election (autocracies)	1.872 (1.545)	1.432 (1.627)	2.190* (1.212)
Pre-Election (democracies)	−1.430 (1.177)	−0.709 (0.981)	−1.266 (1.025)
Post-Election (democracies)	−1.739 (3.254)	−1.006 (2.773)	−1.423 (2.692)
Country FE	Y	Y	Y
Period FE	Y	Y	Y
Interpolation used	Y	Y	Y
Interpolation FE	Y	Y	Y
Observations	1,770	1,770	1,770
Adjusted R <sup>2</sup>	0.454	0.408	0.528

*Note:*

\* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

**Table 3:** No evidence that political principals systematically increase the number of checkpoints around elections.

All data aggregated from the the driver-day-level to the country-week level through arithmetic averaging. Missing country-weeks imputed through linear interpolation. The  $p$ -values are calculated by comparing observed estimates to the distribution of estimates under the sharp null of no election effects, calculated by simulating placebo elections and re-estimating effects 2,000 times. Standard errors are calculated using two-way clustering at the country-week level. In columns labeled ‘Y’ for ‘Country FE’, ‘Period FE’, and ‘Interpolation FE’, fixed effects for countries, periods, and interpolation are included (‘N’ otherwise). In columns labeled ‘N’ for ‘Interpolation used’, interpolated observations are not used, ‘Y’ when they are used. ‘Pre-Election Period’ and ‘Post-Election Period’ are 1 if bribe paid in three months preceding or following an election in that country, respectively, 0 otherwise. ‘Democracies’ are Ghana, Senegal, and Mali; ‘Autocracies’ are Togo and Burkina Faso (Marshall and Cole, 2011).

The theory elaborated above predicted that positive electoral cycles in the number of checkpoints would be even likelier in democratic states due to the comparatively higher costs of raising campaign

funds there. Yet, if anything, we see the opposite. Drivers encountered roughly one and a half fewer checkpoints on average in the three months before and after elections in democratic states. Rather than the illicit campaign fundraising hypothesis, the effects are more consistent with a classic accountability prediction: to the extent that voters dislike visible signs of petty corruption (such as checkpoints), there should be less of it around elections (Rose-Ackerman, 1999). Neither effect is statistically significant, however. And relative to the non-electoral average of seventeen checkpoints in democratic states, the 1.5 checkpoint effect size is not very large (9%).

Putting aside the statistical insignificance of the findings, one interpretation is that the increase in average bribes reported on Table 2 is simply a response to the decrease in checkpoints. However, note that the 27% proportional increase in bribe prices in Table 2 is almost three times the size of the 9% decrease in checkpoints. Indeed, as Table 6 in the appendix shows, the increase in the average bribe extorted means that drivers are paying considerably more per trip in total. The reduction in checkpoints is more than offset by the increase in prices, so that the net effect is an increase in the total amount paid by drivers.

To test the idea that political principals may concentrate fundraising on those outside their coalition during elections while holding the extensive margin constant, columns 2 and 3 of Table 3 analyze electoral effects on the amount of checkpoints encountered by trucks that are native versus foreign to the country. We see very little evidence of heterogeneity—if anything, checkpoints are less of a hindrance to drivers of foreign trucks during election periods than they are to drivers of domestic trucks. The results look very similar if, instead of the truck, I analyze heterogeneity in effects by co-nationality of the *driver*. I do find a substantial increase in the number of checkpoints that foreign trucks encounter during the post-election period in the autocratic states: there are almost two more checkpoints on average. This does not explain why prices increase before elections in democratic states, however.

In sum, the findings suggest that political influence and fundraising are an unlikely explanation for the presence of political corruption cycles established in Table 2. While I find no evidence of cycles on the extensive margin, it is worth noting that these findings do not enable us to rule out the presence of a political logic to the organization and exercise of corruption. In absolute terms,



democratic states extorted foreign trucks at 19 checkpoints on average, compared to 14 checkpoints for domestic trucks. No such differences exist for the autocratic states, who extort foreign and domestic drivers at about nine checkpoints per trip alike, on average. Although consistent with a redistributive story, however, the extra five checkpoints may result from the additional length of route that foreign versus domestic trucks typically travel, and so forth.

### **Do bureaucrats create corruption cycles independent of political influence?**

In the previous section, I theorized that political principals might be able to control the number of checkpoints but not the price charged at those checkpoints. Bureaucrats, however, have a much easier time manipulating extortionary prices but typically do not control the overall number of checkpoints. One interpretation of the results, therefore, is that political corruption cycles may be principally driven by bureaucratic decision-making. So how might we explain such decisions?

An influential body of literature on bureaucratic corruption explains the bureaucrat's decision to extort as a tradeoff between the anticipated gains from extortion and the risk of punishment (Becker and Stigler, 1974; Rose-Ackerman, 1978; Klitgaard, 1988; Shleifer and Vishny, 1993; Olken, 2007; Treisman, 2007). According to Olken and Pande (2011), a bureaucrat extorts a bribe in exchange for some government good or service iff

$$w - v < \frac{1 - p}{p}(b - d), \quad (6)$$

where the wage,  $w$ , that the bureaucrat receives net of an outside option,  $v$ , must be lower than the benefit of the bribe,  $b$ , net of the associated moral or punitive cost,  $d$ , conditional on the probability,  $p$ , of being caught (Olken and Pande, 2011). Bureaucrats extort when they expect to be better off by taking bribes, in light of the associated risks and the relative worth of their government wage.<sup>14</sup>

Corruption cycles do not appear to be driven by political manipulation of, say, the probability ( $p$ ) or severity ( $d$ ) of punishment. I focus instead on how decreasing expected wages ( $w$ ) incentivizes extortion by raising the opportunity costs of not extorting.

When incumbents lose elections in West Africa, this typically affects wages and job appointments

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<sup>14</sup>It is typically assumed that  $w > v$ , otherwise the bureaucrat simply leaves the bureau. It is also assumed that the probability of being caught is non-zero even if small.

within the bureaucracy. Kopecký (2011) estimates that 67% of jobs in the Ghanaian police sector are attained through appointment by incumbents in the executive. Similarly, Lentz (2014) shows how officials in the Ghanaian security sector rely on personal connections to patrons in the ruling party, and tells the story of a civil servant in Ghana who lost his position as Director General of the Prison Service due to a change in leadership. Analyzing reappointments of the forestry agents in Senegal who account for many of the checkpoints in rural areas, Blundo (2014, 75) describes how ministerial reshuffles resulted in no less than sixty-eight personnel transfers per year in the decade from 1995-2005. He estimates that up to a fifth of the entire forestry workforce may have been affected each time. Even when changes in leadership do not result in reappointment, they can result in changes to funding priorities that adversely affect public wages. In Mali in 2002, for example, Alpha Oumar Konaré had endorsed the World Bank and International Monetary Fund Poverty Reduction Service Paper (PRSP), as required to qualify for loans that were to sustain public sector employment. When Touré ran for reelection in 2007, he refused to endorse the existing PRSP because it had been drawn up by his predecessor, provoking fears that Mali might lose foreign aid necessary to fund the public sector. Frantz (2018) demonstrates that uncertainty provoked by elections provokes political cycles of capital flight in election years in African countries. The vulnerability of public sector wages to leadership turnover in West Africa can produce a highly uncertain environment for public servants.

Elections that incumbents can lose might therefore influence beliefs about  $w$  wages—by increasing bureaucrats’ uncertainty about future economic conditions, about job security, about future budgetary decisions. As  $w$  decreases, the opportunity cost of not engaging in extortion increases, incentivizing corruption.

The first column of Table 4 presents evidence consistent with this notion. I leverage within-country variation in election outcomes in Ghana and Senegal, where one election is won by the incumbent and one election lost by the incumbent.<sup>15</sup> The pattern of results suggests that bureaucrats set much higher prices in the buildup to elections and reduce those prices back down to the non-electoral price when incumbents win. When incumbents lose and leadership changes, however, bureaucrats keep extortionary prices high.

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<sup>15</sup>The WATH data only covers the pre-electoral period for Mali’s 2013 election, which the incumbent lost.

	<i>Dependent variable:</i>	
	Average Bribe Paid	
	(1)	(2)
Pre-election (autocracies)	60.315 (74.489)	
Post-election (autocracies)	−54.123 (118.313)	
Pre-election (democracies)	335.409*** (106.896)	
Post-election (democracies—incumbent reelected)	−37.599 (106.426)	
Post-election (democracies—incumbent defeated)	302.643** (151.717)	
Period Following Atta-Mills' Death		−40.349 (101.988)
Country FE	Y	Y
Period FE	Y	Y
Interpolation used	Y	Y
Interpolation FE	Y	Y
Lagged DV	N	N
Observations	1,770	1,770
Adjusted R <sup>2</sup>	0.683	0.666
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

**Table 4:** Bureaucrats keep extortionary prices high when incumbents lose, reduce them to normal levels when incumbents win. Non-electoral turnover does not affect extortion.

All data aggregated from the the driver-day-level to the country-week level through arithmetic averaging. Missing country-weeks imputed through linear interpolation. The  $p$ -values are calculated by comparing observed estimates to the distribution of estimates under the sharp null of no election effects, calculated by simulating placebo elections and re-estimating effects 2,000 times. Standard errors are calculated using two-way clustering at the country-week level. In columns labeled ‘Y’ for ‘Country FE’, ‘Period FE’, and ‘Interpolation FE’, fixed effects for countries, periods, and interpolation are included (‘N’ otherwise). In columns labeled ‘N’ for ‘Interpolation used’, interpolated observations are not used, ‘Y’ when they are used. ‘Pre-Election Period’ and ‘Post-Election Period’ are 1 if bribe paid in three months preceding or following an election in that country, respectively, 0 otherwise. ‘Democracies’ are Ghana, Senegal, and Mali; ‘Autocracies’ are Togo and Burkina Faso (Marshall and Cole, 2011).

The pattern of results presented in the first column is precisely the pattern we would expect to see if elections caused corruption cycles because the possibility of new leadership provokes uncertainty about future bureaucratic wages: fearing a change in leadership, officials extort more in the buildup to the election in order to insure against a change in their circumstances. When their fears are

confirmed, they maintain those high prices. When the incumbent is reelected and their wage is assured, they reduce prices back to levels determined by the associated marginal costs of extortion. Notice that the illicit campaign funding and accountability theories of political influence do not predict that extortion should remain high *after* elections.

One rival explanation for the effect of new leadership is that it changes agents' beliefs about the probability of punishment ( $p$ ). New leaders may arrive into power with less information about the bureaucracy and less capacity to implement strong anti-corruption crackdowns than their predecessors, at least in the early phases of their tenure (Shleifer and Vishny, 1993; Saha et al., 2014). Political intervention into the complex organizational structures that characterize modern bureaucracies requires personal relationships with the heads of those bureaus; it takes time and political capital. Bureaucrats may take advantage of the window of opportunity afforded by the arrival of a new leader, benefiting from a relatively lax environment to extract more from the citizenry and further supplement their income. In this case, even if new leadership does not provoke uncertainty, we would expect to see an increase in extortionary trends following the election of new leaders.

To discriminate better between the wage uncertainty and opportunism explanations, I analyze a non-electoral change in leadership that took place in Ghana following the sudden death of John Atta Mills in 2012. Following his death, the Vice President John Mahama took over in a smooth transition. While the exact weekly timing of his death came as a shock, news of Atta Mills' poor health had circulated in Ghana for some months and it was well-known that Mahama would take over from Mills. Mahama had often expressed that he would maintain Mills' platform. While Mahama's takeover in Ghana augured continuity with the status quo, however, he entered into leadership with less control over the inner workings of the bureaucracy than his predecessor. In other words, his takeover likely provoked little uncertainty about bureaucratic wages, but provided bureaucrats an opportunity to take advantage of a slightly more lax monitoring environment.

As seen on Column 2 of Table 4, however, the analysis provides evidence against the opportunism mechanism. If anything, bribes *decreased* in the aftermath of Atta Mills' replacement by Mahama, although the coefficient is small and statistically insignificant.

## Do electoral business cycles increase extortion?

I have established that the average bribe paid by truck drivers increases substantially in the buildup to elections that incumbents can lose. I have argued this results from price-setting decisions of bureaucrats made independently of influence by political principals. In support of this interpretation, I highlight that electoral cycles occur only on the intensive margin—easily manipulated by bureaucrats and difficult for politicians to monitor—and not on the extensive margin. Furthermore, when incumbents lose elections bureaucrats retain high prices into the post-election period.

A possibility not explored so far, however, is that extortionary prices change due to supply side factors, such as the number of trucks, the type of goods they are carrying, or the budget constraint of the drivers. A large literature on political business cycles (PBCs) suggests that incumbents “try to make the year before an election a ‘happy one’ in order to be reelected” (Paldam, 1979, p. 324, see footnotes 2 and 5).

In line with the results presented above, Block, Ferree, and Singh (2003) find evidence of “electorally timed interventions in fiscal, monetary and exchange rate policy *exclusively in the cases of competitive elections*” in African countries (465, emphasis added). If politicians intervene in the economy in the buildup to competitive elections in order to maximize their chances of electoral success, then the subsequent stimulation of economic activity might have several kinds of knock-on effect on extortion. For example, voters may increase consumption, causing an increase in the trade of perishable goods. My fieldwork suggests such goods are easier to extort due to the time-sensitivity of the delivery.

To test for electoral business cycles in commodity flows, I analyze a comprehensive dataset that tracks every truck that entered or left Burkina Faso carrying imports or exports from January 2010 to May 2014. The dataset was produced by the Burkinabé Shipper’s Council. It details the type of goods the truck carried, the direction in which the goods were being carried, and the date and time of travel. Because Burkina Faso is a hub for the countries in the sample (see Figure 1), this dataset provides a partial but very broad cross-section of all of the trucks on the road during the period analyzed.

Column 1 of Table 5 presents the results on the total number of trucks. In column 2, I present

the results on imports, which are likely to be affected by changes in consumption. Column 3 presents results on foodstuffs: because foodstuffs are perishable, shipping decisions are made in closer proximity to the moment of consumption, so they are more likely to reflect changes in consumption.

In non-electoral periods, an average of 320 trucks traveled into or out of Burkina Faso. Elections in autocratic states appear to have little to no impact on this average. In democratic states, there is no evidence that a classic pre-election business cycle drives a spike in consumption prior to the election.

	<i>Dependent variable:</i>		
	Total Cargo	Imports Only	Foodstuffs Only
	(1)	(2)	(3)
Pre-Election (autocracies)	−17.694 (95.329)	10.241 (86.800)	−11.093 (12.351)
Post-Election (autocracies)	−0.421 (82.286)	3.595 (78.940)	−11.784 (14.348)
Pre-Election (democracies)	3.647 (53.555)	−14.616 (56.108)	−11.536 (14.607)
Post-Election (democracies)	65.868 (81.983)	63.834 (85.590)	1.766 (11.379)
Country FE	Y	Y	Y
Period FE	Y	Y	Y
Interpolation used	Y	Y	Y
Interpolation FE	Y	Y	Y
Lagged DV	N	N	N
Observations	909	909	909
Adjusted R <sup>2</sup>	0.738	0.710	0.641

*Note:*

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 5:** No evidence of an effect of elections on commodity flows in West Africa.

All data aggregated from the the truck-day-level to the country-week level through summation. The  $p$ -values are calculated by comparing observed estimates to the distribution of estimates under the sharp null of no election effects, calculated by simulating placebo elections and re-estimating effects 2,000 times. Standard errors are calculated using two-way clustering at the country-week level. In columns labeled ‘Y’ for ‘Country FE’, ‘Period FE’, and ‘Interpolation FE’, fixed effects for countries, periods, and interpolation are included (‘N’ otherwise). In columns labeled ‘N’ for ‘Interpolation used’, interpolated observations are not used, ‘Y’ when they are used. ‘Pre-Election Period’ and ‘Post-Election Period’ are 1 if bribe paid in three months preceding or following an election in that country, respectively, 0 otherwise. ‘Democracies’ are Ghana, Senegal, and Mali; ‘Autocracies’ are Togo and Burkina Faso (Marshall and Cole, 2011).

## Discussion

Using time-series cross-sectional microdata on extortion over a seven-year period in three democracies and two autocracies, I provide evidence of political corruption cycles. In the buildup to elections that incumbents can lose, bureaucrats increase the average bribe they extort by 27% relative to non-electoral periods. When new leaders win office, they keep prices high. In West African democracies from 2006 to 2013, elections caused a kind of reverse political business cycle, whereby an additional drain of twelve U.S. dollars per trip was placed on the transport costs of container trucks carrying goods to and from markets.

The absence of strong electoral effects in some domains is just as informative about what processes might explain these corruption cycles as the presence of large effects in others.

I find no support for the idea that elections increase the aggregate amount of corruption because money from bribes is used to fund campaigns: the extensive margin of extortion is unaffected by elections, and extortion increases in the post-election period when challengers win. Of course, it is possible to imagine a story whereby the observed pattern of results reflects illicit fundraising by incumbents and an attempt by challengers to fill the coffers when they enter office. But this story assumes an implausibly high degree of monitoring capacity and leaves unexplained why politicians do not opt for the much simpler and politically feasible option of manipulating the quantity and geographic distribution of checkpoints to target foreign trade. Although I cannot isolate any single mechanism, I posit corruption cycles likely result from independent decision-making by bureaucrats.

One possibility is that this decision-making responds primarily to supply-side factors caused by political business cycles. For example, politicians seeking to keep people happy in election year might stimulate greater consumption, leading to an increase in high-value or perishable import goods that fetch a higher bribe. Yet I find no evidence of political cycles in the composition, quantity, or direction of commodity flows. Although previous work has illustrated the presence of democracy-specific business cycles in Africa (Block, Ferree, and Singh, 2003), these appear to exist alongside rather than as an explanation for corruption cycles.

Taken as a whole, the specific pattern of findings supports the notion that bureaucratic uncertainty can cause increases in corruption: extortion does increase in the buildup to elections, but

*only* when there is a serious possibility of the incumbents losing those elections; elections *do* increase corruption into the post-election period, but only when challengers with new policy priorities win.

The possibility that these are driven by bureaucrats' uncertainty about future leadership raises some interesting implications for the literature on democratization.

Other studies argue that corruption is higher in democracies because democratization reduces control over the bureaucracy in the short term without increasing political competition sufficiently to reduce corruption. For example, Montinola and Jackman (2002, 163) argue that "the pronounced corruption-inhibiting political competitiveness and transparency generated by democracy comes into play [when] democracies become fully competitive."<sup>16</sup> By contrast, the theory presented in this paper portrays political competition *itself* as a cause of corruption, because at the core of competition for leadership is the assurance that incumbents can lose. Empirically, it is those elections that are the most competitive that most strongly exacerbate extortionary dynamics.

The findings highlight that introducing competitive elections does not only increase leader accountability, but also has the important consequence of increasing the likelihood of *leader turnover*. By design, truly competitive elections increase uncertainty about future leadership (Przeworski et al., 2000). In countries where bureaucracies are poorly insulated from political influence, new leadership often brings personnel transfers, new appointments, new approaches to foreign aid, and new budgetary priorities (Iyer and Mani, 2012; Cruz and Keefer, 2015).<sup>17</sup> By raising the prospect of new leadership, competitive elections may increase public servants' uncertainty about future income streams. Uncertainty about income raises the opportunity cost of remaining honest, incentivizing corruption (Rijckeghem and Weder, 2001; Gorodnichenko and Peter, 2007). Autocratic countries do not face such issues because their elections provoke no uncertainty about future leadership. And in consolidated democracies, civil service legislation protects bureaucratic income streams from po-

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<sup>16</sup>In a similar vein, Mohtadi and Roe (2003) present a theoretical argument tested empirically by Rock (2009), according to which democracy increases both the opportunities and competition for people outside government to seek rents by bribing officials. However, as democratic consolidation proceeds "eventually increased competition among rent-seekers and increased sanctions against rent-seeking and corruption drive the returns to rent-seeking so low that aggregate rents (and corruption) fall when the state of democracy is sufficiently well developed" (Rock, 2009, 58).

<sup>17</sup>Employing an electoral regression discontinuity design, Akhtari, Moreira, and Trucco (2017) find that up to a quarter of publicly employed headmasters are replaced when new mayoral leaders win elections in Brazil, for example, while Iyer and Mani (2012) find that the election of a new Chief Manager in India increases the probability of new bureaucratic appointments by 10%.



litical vicissitudes (Horn, 1995; Ting, 2012). Thus, while other authors have mostly analyzed the inverted U between corruption and electoral competition under the assumption that corruption persists due to a *lack* of political competition, I suggest another reason for this concavity may be the heterogeneous effects of political competition across varying degrees of bureaucratic insulation.

The idea that leader turnover is an important determinant of political behavior was developed in work by Olson and his coauthors and has been used in the explanation of a number of diverse outcomes (Olson, 1993; Clague et al., 1996; Wright, 2008; Gamboa-Cavazos, Garza-Cantú, and Salinas, 2007; Campante, Chor, and Do, 2009; Kendall-Taylor, 2011; Moon, 2015; Fails, 2014). In closely related work, Frantz (2018) shows that African elections provoke capital flight and posits that this happens due to increased uncertainty about future leadership, for example. However, these ideas have not been applied to understanding the relationship between elections and corruption—even though leader turnover is at the heart of political liberalization and many cross-national measures of corruption focus specifically on bureaucratic behavior.<sup>18</sup>

The results suggest an understudied connection between the fight against bureaucratic corruption and civil service legislation aimed at insulating public sector jobs and salaries from political influence. One policy implication is that bureaucratic insulation is an important anti-corruption measure, not just for limiting patronage, but also for ensuring efficient and honest bureaucratic performance. Thus, future work might look at the long-run impact of civil service reform on levels of corruption among democracies.

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<sup>18</sup>However, see Gamboa-Cavazos, Garza-Cantú, and Salinas (2007) and Campante, Chor, and Do (2009), who find evidence in Mexico that leaders with longer tenure are less corrupt than those with short horizons.

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Online Appendix to  
Political Corruption Cycles in Democracies and Autocracies:  
Evidence from micro-data on extortion in West Africa  
Jasper Cooper

A: Additional Results

- Table 6 illustrates the pre-election period causes a large increase in the total bribe extorted per trip.

B: Robustness

- In Section B.1, I discuss the problems highlighted with two-way fixed effects estimators in recent econometrics literature and why these issues do not pose a serious threat to inference in this study. I describe a simulation study in which I show that, even under extremely pessimistic assumptions, any estimation bias is inconsequential to the main results.
- Tables 8, 9, and 10 illustrate that the results are robust and sometimes even stronger when I do not winsorizing, exclude Mali, or exclude Senegal, respectively.
- Table B.5 shows the results are robust to the inclusion of up to three lags.

C: Supplementary Information

- Figure 6 presents a plot of the weekly panel data.
- Figures 7 and 7 present the partial autocorrelation in the series of the democratic and autocratic countries, respectively.

## A Additional Results

	Total Bribe Extorted per Trip		
	All Trucks	Domestic Trucks	Foreign Trucks
	(1)	(2)	(3)
Pre-Election (autocracies)	793.605 (3,397.450)	−843.004 (3,135.701)	275.926 (3,009.795)
Post-Election (autocracies)	2,647.938 (2,401.204)	286.858 (2,534.459)	3,355.786* (1,880.898)
Pre-Election (democracies)	4,331.197 (3,309.305)	5,075.670* (2,797.215)	3,624.701** (1,751.330)
Post-Election (democracies)	381.083 (5,216.588)	1,708.062 (4,675.124)	1,077.815 (4,517.379)
Country FE	Y	Y	Y
Period FE	Y	Y	Y
Interpolation used	Y	Y	Y
Interpolation FE	Y	3	
Observations	1,770	1,770	1,770
Adjusted R <sup>2</sup>	0.596	0.501	0.692

*Note:*

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 6:** Effect of elections on total bribe extorted per trip.

All data aggregated from the the driver-day-level to the country-week level through arithmetic averaging. Missing country-weeks imputed through linear interpolation. The  $p$ -values are calculated by comparing observed estimates to the distribution of estimates under the sharp null of no election effects, calculated by simulating placebo elections and re-estimating effects 2,000 times. Standard errors are calculated using two-way clustering at the country-week level. In columns labeled ‘Y’ for ‘Country FE’, ‘Period FE’, and ‘Interpolation FE’, fixed effects for countries, periods, and interpolation are included (‘N’ otherwise). In columns labeled ‘N’ for ‘Interpolation used’, interpolated observations are not used, ‘Y’ when they are used. ‘Pre-Election Period’ and ‘Post-Election Period’ are 1 if bribe paid in three months preceding or following an election in that country, respectively, 0 otherwise. ‘Democracies’ are Ghana, Senegal, and Mali; ‘Autocracies’ are Togo and Burkina Faso (Marshall and Cole, 2011).



## B Robustness

### B.1 Diagnosis of Two-Way Fixed Effects Estimator

The two specifications in equations 4 and 5 are equivalent to the two-way fixed effects (TWFE) estimator. This estimator is also known as the generalized difference-in-differences model because it estimates the causal effect of  $D$  on  $Y$  as the weighted average of every two-period difference-in-difference in the sample (Angrist and Pischke, 2008).

A recent body of literature has pointed to two main estimation issues that arise with the TWFE estimator when three conditions hold, as they are assumed to here: a) countries enter into and out of treatment at different times; b) treatment effects vary by country, and; c) treatment effects vary by time. I show here descriptively and through a simulation study, however, that any bias arising from these two issues in my particular application is likely very small due to the relative homogeneity in weights.

The estimation issues arise because the TWFE estimator’s estimate of the average treatment effect is equivalent to the weighted average of estimators that use time, unit, and no fixed effects (Humphreys, 2009; Goodman-Bacon, 2018; Imai and Kim, 2018). To get an efficient estimate of the average effect, those composite estimators weight time-specific, unit-specific, and overall estimates by the time-specific, unit-specific, and overall variance of the treatment variable, and not by the respective sample sizes.

The first potential issue therefore arises from a divergence between the average treatment effect and the estimate of the average treatment effect calculated from the average of country-level heterogeneous treatment effects weighted by the country-level treatment variance (Humphreys, 2009; Lin, 2013; Solon, Haider, and Wooldridge, 2015; Gibbons, Serrato, and Urbancic, 2018). If the rate at which countries have elections is correlated with country-level heterogeneity in electoral effects, for example, this can bias the average of the effects towards the effect size of those countries that have the most variance in elections. In my data, however, the proportion of treated periods is very even across countries, especially when splitting them into autocracies and democracies as the main results do. For Mali, Ghana, and Senegal, the average of the pre-election treatment variable is 7%, 8%, and 9%, respectively, and for Togo and Burkina Faso these are both 4%.

The second issue arises from the combination of period-level effect heterogeneity and treatment timing variation. de Chaisemartin and D’Haultfoeuille (2019) show that certain setups can lead to negative weighting of the estimate across the two-period difference-in-differences in the sample. This phenomenon arises from the way in which differenced treatment periods function as controls for other treatment periods in two-way FE models. In my case, units’ rapid entry into and out of treatment prevents such scenarios. The de Chaisemartin and D’Haultfoeuille weights on an indicator for the pre- or post-election period in my study are all positive, with a mean and median of 1 and a minimum and maximum of 0.74 and 1.12, respectively. Thus, the weights cannot cause sign-flipping, and are sufficiently homogeneous as to pose minimal threat of bias.

To assess sensitivity to these issues in my study, I conducted a simulation study in `DeclareDesign` assuming the worst case scenario, in which treatment effects and assignment probabilities are highly heterogeneous and correlated.

The untreated and treated potential outcomes  $Y_{c,t}(0)$  and  $Y_{c,t}(1)$  are generated from the observed data according to the following model:

$$Y_{c,t}(0) = \tilde{Y}_{c,t}, \quad Y_{c,t}(1) = \tilde{Y}_{c,t} + t \times 10 + \lambda_c \times 100,$$

where  $\tilde{Y}_{c,t}$  is the observed average bribe in country  $c$  at time  $t$  (in XOF) from the actual data,  $t$  is an integer increasing with each period, and  $\lambda$  is a country-specific shock. The election treatment variable,  $D_{c,t}$ , is more likely to equal 1 in some countries than in others, and is five times likelier to occur in September, October, November, and December of each year than in other months, though this information is assumed hidden to the researcher. Using 4,000 simulations, the average distance between the TWFE estimate and the true underlying ATT and ATE is less than 1% of the estimated effect size. Moreover, this estimate of the bias is small enough as to be indistinguishable from simulation error. Even under extremely pessimistic assumptions, the design appears to feature minimal if any estimation bias.

On Table 7, I present the results of the simulation study. I include a version of the TWFE estimator that weights observations by the inverse of their treatment propensity, which is assumed unknown. In both estimators the estimate is very close to the estimand, and the difference in the ATT and ATE is not large. The estimated bias is lower for the IPW estimator, but indistinguishable from zero in both cases. Note that the standard deviation across repeated simulations of the study is lower than the estimated standard error in the main results, suggesting the approach taken may be moderately conservative under the assumptions of this design.

Estimand Label	Estimator Label	N Sims	Mean Estimate	Mean Estimand	Bias	SD Estimate
ATE	IPW 2wayFE	4000	446.67 (1.63)	446.62 (0.00)	0.05 (1.63)	93.78 (1.08)
ATE	Unweighted 2wayFE	4000	447.18 (1.61)	446.62 (0.00)	0.56 (1.61)	95.48 (1.03)
ATT	IPW 2wayFE	4000	446.67 (1.63)	446.71 (0.11)	-0.04 (1.61)	93.78 (1.08)
ATT	Unweighted 2wayFE	4000	447.18 (1.61)	446.71 (0.11)	0.47 (1.60)	95.48 (1.03)

**Table 7:** Simulation study of the design under pessimistic assumptions about heterogeneous effects and assignment probabilities.

Simulation study conducted in `DeclareDesign` for R (Blair et al., 2019). Simulations conducted using real data. Parentheses give simulation standard error, calculated through 100 bootstraps of the diagnosand.

## B.2 Results Without Winsorizing

	<i>Dependent variable:</i>	
	Average Bribe Paid	
	(1)	(2)
Pre-Election	354.471*	
	(190.007)	
Post-Election	38.482	
	(101.509)	
Pre-Election (autocracies)		57.885
		(78.268)
Post-Election (autocracies)		-89.053
		(125.543)
Pre-Election (democracies)		471.642**
		(230.844)
Post-Election (democracies)		102.178
		(123.978)
Country FE	Y	Y
Period FE	Y	Y
Interpolation used	Y	Y
Interpolation FE	Y	Y
Lagged DV	N	N
Observations	1,770	1,770
Adjusted R <sup>2</sup>	0.525	0.528
<i>Note:</i>		
*p<0.1; **p<0.05; ***p<0.01		

**Table 8:** Evidence of political corruption cycles, **without winsorizing outliers**.  
See appendix page 2 and caption of Table 2 in main text for explanatory notes.

### B.3 Excluding Mali

	<i>Dependent variable:</i>	
	Average Bribe Paid	
	(1)	(2)
Pre-Election	168.734** (83.452)	
Post-Election	49.844 (74.166)	
Pre-Election (autocracies)		36.717 (75.063)
Post-Election (autocracies)		-68.012 (112.119)
Pre-Election (democracies)		241.359*** (71.662)
Post-Election (democracies)		114.443* (66.836)
Country FE	Y	Y
Period FE	Y	Y
Interpolation used	Y	Y
Interpolation FE	Y	Y
Observations	1,416	1,416
Adjusted R <sup>2</sup>	0.614	0.617
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

**Table 9:** Evidence of political corruption cycles, **with Mali excluded**.

See appendix page 2 and caption of Table 2 in main text for explanatory notes.

## B.4 Excluding Senegal

	<i>Dependent variable:</i>	
	Average Bribe Paid	
	(1)	(2)
Pre-Election	318.119*** (118.239)	
Post-Election	90.605 (116.098)	
Pre-Election (autocracies)		100.333 (68.097)
Post-Election (autocracies)		−70.520 (145.645)
Pre-Election (democracies)		448.983*** (104.863)
Post-Election (democracies)		205.031** (97.087)
Country FE	Y	Y
Period FE	Y	Y
Interpolation used	Y	Y
Interpolation FE	Y	Y
Observations	1,416	1,416
Adjusted R <sup>2</sup>	0.695	0.699
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

**Table 10:** Evidence of political corruption cycles, **with Senegal excluded**.

See appendix page 2 and caption of Table 2 in main text for explanatory notes.

## B.5 Inclusion of More Lags

	<i>Dependent variable:</i>			
	Average Bribe Paid			
	(1)	(2)	(3)	(4)
Pre-Election (autocracies)	38.256 (41.585)	34.747 (36.713)	29.967 (35.087)	26.144 (34.993)
Post-Election (autocracies)	−32.541 (65.851)	−25.888 (54.723)	−20.365 (47.232)	−17.069 (44.864)
Pre-Election (democracies)	169.173*** (65.474)	132.188** (58.011)	104.965* (57.846)	88.225 (57.570)
Post-Election (democracies)	49.694 (65.341)	32.702 (57.700)	16.817 (54.230)	6.738 (56.018)
Lag 1	0.463*** (0.074)	0.381*** (0.058)	0.355*** (0.056)	0.338*** (0.055)
Lag 2		0.177*** (0.036)	0.121*** (0.030)	0.107*** (0.029)
Lag 3			0.148*** (0.031)	0.110*** (0.035)
Lag 4				0.113*** (0.029)
Country FE	Y	Y	Y	Y
Period FE	Y	Y	Y	Y
Interpolation FE	Y	Y	Y	Y
Observations	1,765	1,760	1,755	1,750
Adjusted R <sup>2</sup>	0.750	0.757	0.762	0.765

*Note:*

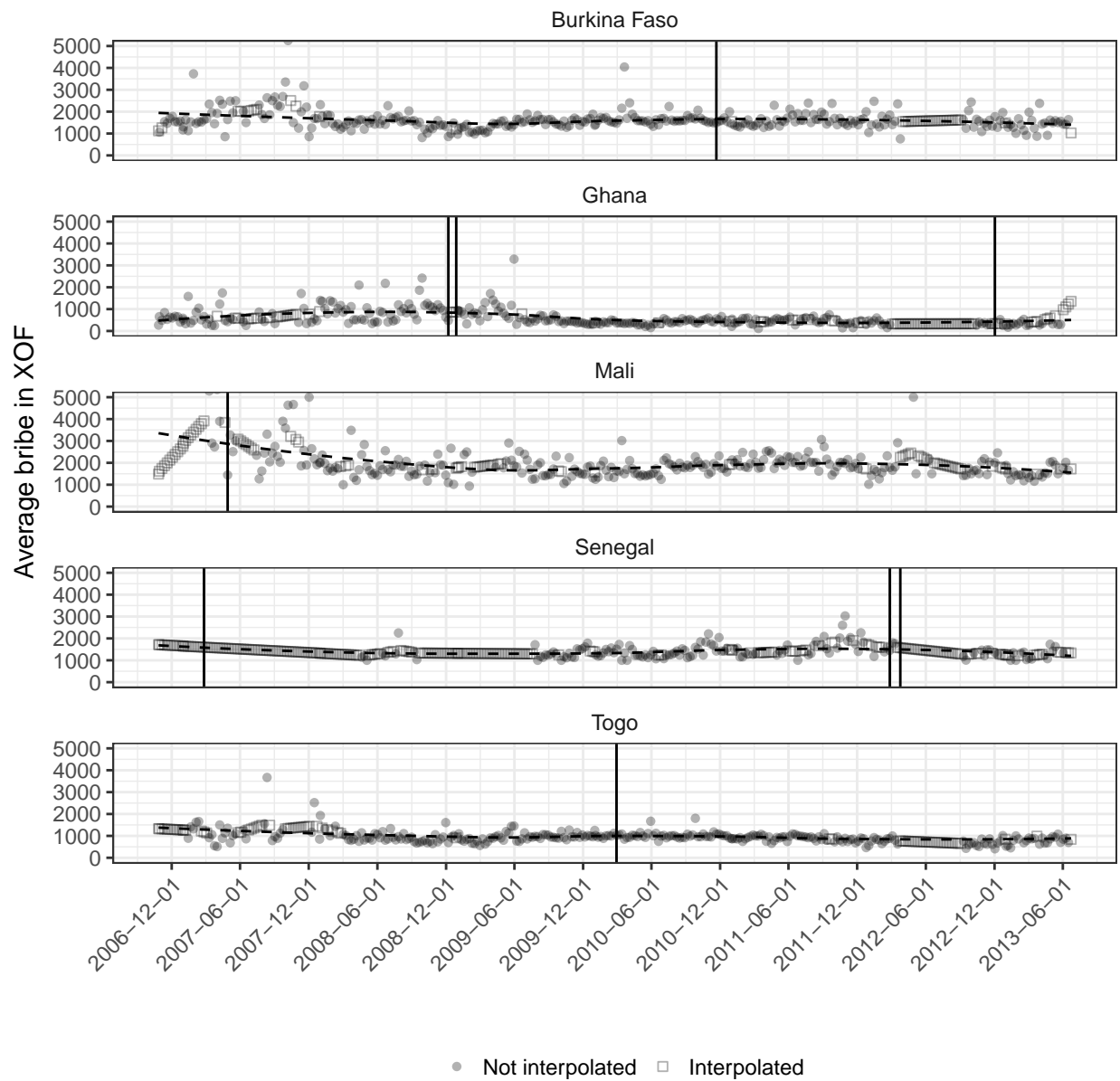
\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 11:** Evidence of political corruption cycles, **including up to four lags**.

See appendix page 2 and captions of Table 2 in main text for explanatory notes.

## C Supplementary Information

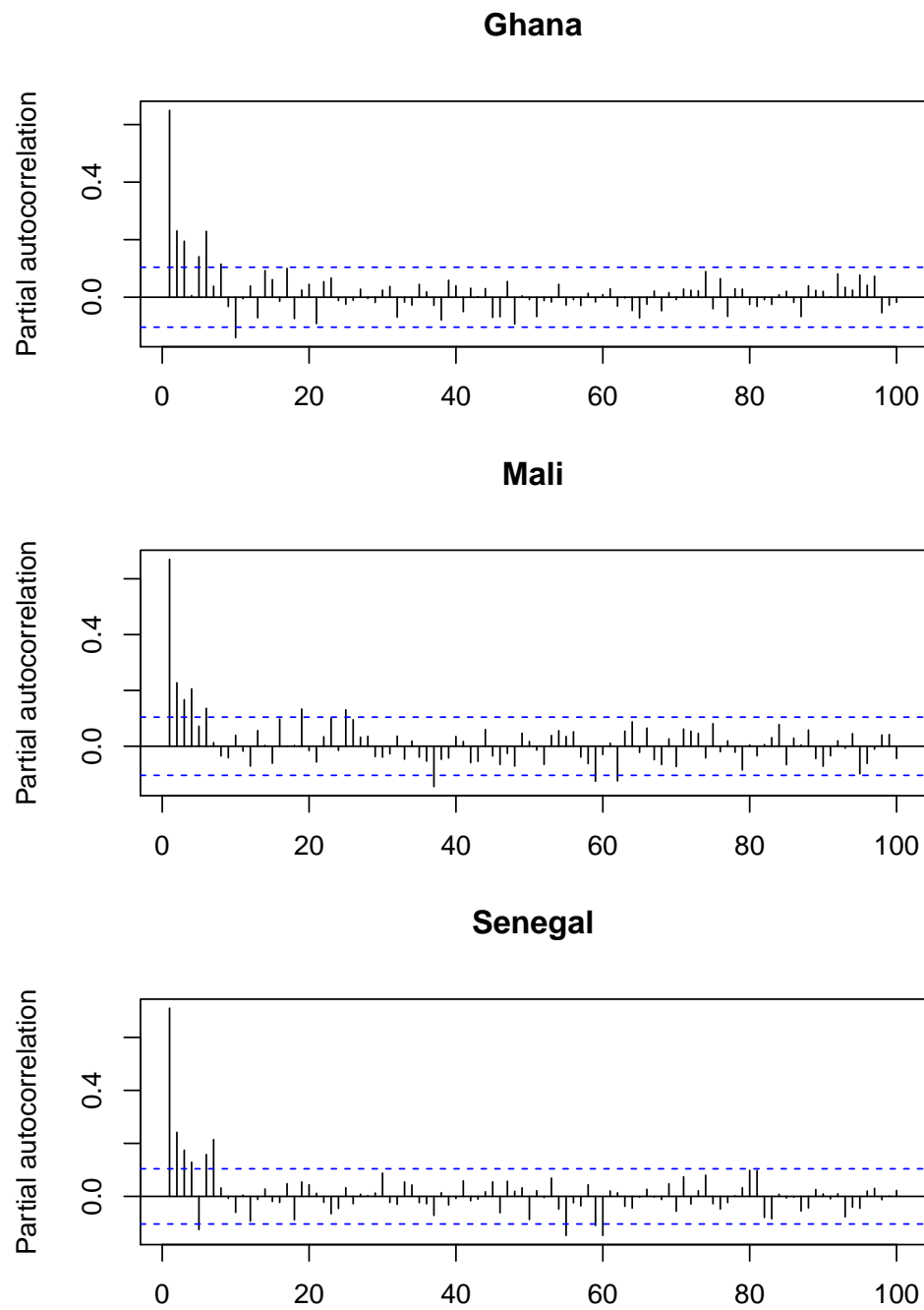
### C.1 Time-Series Data



**Figure 6:** Time-series aggregated to week-level.

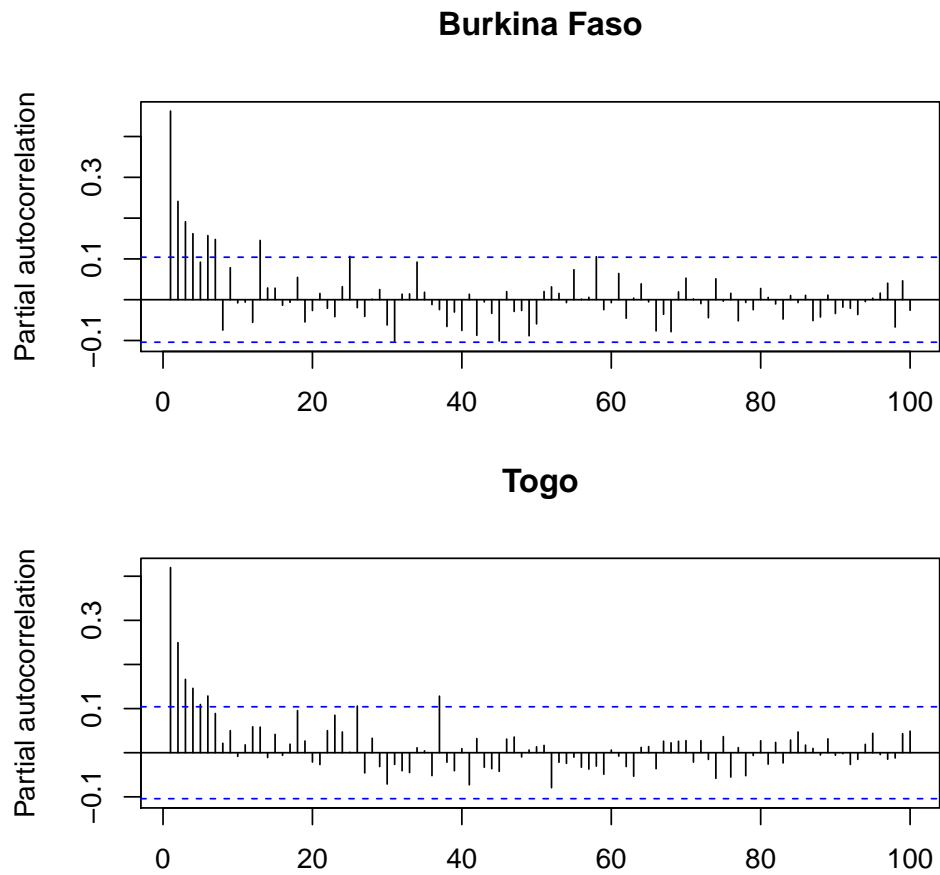
Each point represents an average of driver-level arithmetic averages of bribes paid in that week of the year in that country. Each square represents an imputed average from linear interpolation. Vertical bars represent elections. Dotted horizontal line represents LOESS-smoothed trend.

## C.2 Partial Autocorrelation



**Figure 7:** Partial autocorrelation in time-series of average bribes extorted by bureaucrats in democracies. Dotted horizontal lines represent 95% confidence interval.





**Figure 8:** Partial autocorrelation in time-series of average bribes extorted by bureaucrats in autocracies. Dotted horizontal lines represent 95% confidence interval.