How Political Competition Can Increase Corruption: Electoral cycles in police extortion in West Africa*

Jasper Cooper†

September 10, 2018

Abstract

Several studies have found a positive association between democratization and corruption. Few, however, posit a convincing mechanism. I explain why the relationship between political competition and corruption may be concave. In countries where bureaucracies are poorly insulated from political influence, the introduction of elections increases uncertainty about future leadership. By threatening future income, competitive elections raise the opportunity cost of remaining honest, incentivizing corruption. These dynamics do not exist in autocratic countries, where elections provoke no uncertainty about future leadership, or in consolidated democracies, where civil service legislation protects bureaucrats from political vicissitudes. Examining electoral cycles in over 300,000 bribes paid over a seven-year period by truck-drivers in five West African countries, I show that the average bribe extorted by bureaucrats increases by twenty-three percent in the buildup to competitive elections. Consistent with the idea that political competition only increases extortion when it increases uncertainty, bribes in the post-election period return to the non-electoral average when incumbents win reelection, but remain high when challengers win. I find no evidence that such dynamics exist around elections in autocracies. The findings suggest that democratization can have adverse effects on corruption in the short-term, and highlight the importance of civil service insulation as an anti-corruption policy.

*With sincere thanks to the West African Tradehub and the Borderless Alliance for providing the data and assistance with fieldwork, and to the Columbia University Center for the Study of Development Strategies for funding. Special thanks go to Macartan Humphreys, Miriam Golden, Donald Green and Kimuli Kasara for their in-depth feedback on the manuscript. The author thanks participants for their helpful comments at the annual meeting of the American Political Science Association (APSA), San Francisco, September 3, 2015 and at the Columbia University Mini-APSA workshop on December 9, 2016.

†Ph.D. candidate in Political Science, Columbia University. jjc2247@columbia.edu. `http://jasper-cooper.com`

> Democracy is a system in which parties lose elections. [...] Actors know what winning or losing can mean to them, and they know how likely they are to win or lose, but they do not know if they will lose or win. Hence, democracy is a system of ruled open-endedness, or organized uncertainty.
>
> Adam Przeworski (1991, 10-13)

Introduction

Several prominent theories of government predict that democracy reduces corruption because political competition enhances the accountability of leaders (Ferejohn, 1986; Myerson, 1993; Rose-Ackerman, 1999; Przeworski et al., 2000; Adsera, Boix, and Payne, 2003; Persson, Tabellini, and Trebbi, 2003). Yet, while the cross-national relationship between measures of political competition and corruption is negative overall (Treisman, 2000, 2007), it appears non-linear. Different cross-national measures of corruption and democratic competition find a concave statistical relationship—the "inverted U" (Montinola and Jackman, 2002; Sung, 2004; Méndez and Sepulveda, 2006; Rock, 2009; Charron and Lapuente, 2010; Saha et al., 2014; McMann et al., 2017). Corruption actually *increases* with political competition among the least democratic states, before decreasing among the most institutionalized democracies. As McMann et al. (2017, 5) put it, "the mere introduction of elections (regardless of nature and quality) unambiguously increases corruption."

Much recent literature assumes that corruption persists in the face of elections due to a lack of true competition, and so focuses anti-corruption efforts on impediments such as information sparsity (Ferraz and Finan, 2008, 2011; Chong et al., 2014; Cruz, Keefer, and Labonne, 2016; Weitz-Shapiro and Winters, 2017; Dunning et al., forthcoming). Yet the fact that new democracies often exhibit more corruption than autocracies poses an important question to which accountability-centric theories do not provide a clear answer: why is corruption higher in countries with relatively competitive elections than in autocracies where leaders are electorally unaccountable?

In answer to this question, I emphasize that electoral competition does not only increase leader accountability. It also increases the likelihood of *leader turnover* and thereby heightens uncertainty about future leadership. Even when increased accountability reduces incentives for leaders to engage in high-level corruption, this does not imply reductions in petty corruption by bureaucrats—the form of corruption on which this and many other cross-national studies focus.

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). 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). These dynamics should be weaker in autocratic countries, where elections provoke no uncertainty about future leadership, and in consolidated democracies, where civil service legislation protects bureaucrats from political vicissitudes (Horn, 1995; Ting, 2012).

I test this theory using data that provides an unprecedented level of detail in the comparison of bureaucratic corruption around elections in autocracies and democracies. Specifically, I analyze electoral cycles in over 300,000 bribes that police and other armed forces extorted from truck drivers during some 31,000 trips they made between Burkina Faso, Ghana, Mali, Senegal and Togo from 2006 to 2013. To understand the data-generating process behind this dataset, I conducted participant observation with truck drivers. Over a three-week period in 2014, I traveled over 800 miles of highway through Burkina Faso, 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.

The analysis represents a substantial improvement on previous research insofar as I am able to estimate the effect of elections *within* and *across* countries. Estimation of electoral cycles in a time-series cross-sectional framework reduces concerns that the observed election effects arise due to cross-national institutional confounders or temporal confounders in the seasonal or secular timing of elections. In addition to parametric approaches to statistical inference, I use a quasi-experimental approach that simulates placebo elections in the respective countries 2,000 times in order to estimate the probability of the observed effect sizes given the sharp null hypothesis of no electoral cycles.

The key finding is that police increase the average bribe they extort in the buildup to elections—by up to 22% relative to non-electoral periods ($p < 0.01$)—but only when incumbents stand some chance of losing those elections. There is no evidence that corruption follows electoral cycles in autocracies, where elections pose no threat to leadership. When elections usher in new leadership, police continue to extort at levels 15% higher than average ($p < 0.10$) in the post-election period. However, the average bribe drops to normal levels if instead incumbents are reelected. This pattern of evidence is consistent with the idea that political competition increases extortion because it increases uncertainty about leadership.

Supplementary pieces of evidence support this interpretation of the findings and help to rule out alternative mechanisms linking electoral cycles to extortion. First, one may stipulate that electoral cycles in extortion arise because parties use the bureaucracy to collect campaign funds. However, I find no evidence for electoral cycles in the *number* of checkpoints, which undermines this interpretation. Rather, the fact that the intensive margin increases while the extensive margin does not suggests that electoral cycles are due to the decisions of individual bureaucrats and not their higher-ups. Second, one might also believe that officials are simply opportunistic: they take advantage of the sparse information and political connections available to new leadership in order to extort more while monitoring capacity is low. Yet I find that, whereas an unexpected coup in Mali increased extortion, the death of a President and his replacement by the vice president did not increase extortion in Ghana. In other words, non-electoral leader turnover increases extortion when it changes the leadership direction but does not increase extortion when the party and policies of the leader remain constant. This pattern lends further support to the interpretation that uncertainty about future leadership is the key mechanism linking elections to petty corruption, rather than simple opportunism.

Taken together, the empirical findings demonstrate the plausibility of the key theoretical claim, namely: political competition can increase corruption by making bureaucrats less certain about their wages, and thus more willing to extort. This is a novel explanation for the concave relationship between political competition and corruption. As I explain in greater detail in the discussion, other authors have mostly analyzed the "inverted U" under the assumption that corruption persists due to a *lack* of political competition, and not due to political competition itself. While the findings pertain to short- to intermediate-term differences and thus do not conclusively explain slow-moving cross-national differences in corruption, they point to bureaucratic insulation as a potentially understudied determinant of corruption. In particular, the results suggest the need for more research on the possibility that civil service reforms can complement democratization efforts by staving off the adverse effects of increasing leader turnover.

This paper proceeds in six sections. The first presents a micro-level theory of the conditions under which political competition induces bureaucrats to extort bribes and derives from this theory empirical hypotheses. The second section describes the data on police extortion and elections that is used to test these hypotheses and explains the identification strategy. The third section presents the main results, while the fourth section appraises evidence for alternative mechanisms that might also link elections and bureaucratic corruption. The final section discusses the findings in light of existing theories and concludes.

1 Theory

Theories of the relationship between political competition and corruption that focus on accountability mechanisms potentially miss some adverse effects of heightened competition because they do not consider direct effects on the decision-making processes of bureaucrats. In many ways, this is surprising given that the data used in many cross-national studies pertains to bribes paid to bureaucrats by individuals or firms. In what follows I give a simplified theoretical account of how political competition might directly affect the decision-making processes of civil servants.

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). Olken and Pande encapsulate this understanding of petty corruption with the claim that a bureaucrat will extort a bribe in exchange for some government good or service iff

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

where the w wage that the bureaucrat receives net of her v outside option must be lower than the benefit of the b bribe net of the associated d moral cost, conditional on the p probability of being caught (Olken and Pande, 2011). Bureaucrats extort when they expect to do better off by taking bribes, in light of the associated risks and the relative worth of their government wage.[1]

Note in this formulation that the bureaucrat is sure of receiving her wage in full if she chooses not to extort. However, this common assumption may not be accurate in practice. A more realistic formulation might multiply the w in equation 1 by $q \in [0, 1]$, where q is the bureaucrat's subjective appraisal of the probability with which they receive their wage. As I describe below, many factors might influence q—beliefs about future economic conditions, about job security, about future budgetary decisions. Clearly, however, as q decreases the opportunity cost of not engaging in extortion increases, incentivizing corruption.

In the theoretical argument below, I argue that two conditions are sufficient to reduce bureaucratic uncertainty about receiving w: low leader turnover or high bureaucratic insulation. While bureaucracies in non-democratic states are not insulated from political influence, the low risk of leader turnover contributes to relatively certain expectations that the status quo will be maintained through upcoming elections. In consolidated democracies, while leadership may change frequently, bureaucracies are insulated from political influence by civil service legislation. It is in those young democracies where, as noted, corruption is the highest, that high leader turnover and low bureaucratic insulation translate into high levels of uncertainty around elections.

1.1 Leader Turnover, Patronage and Bureaucratic Insulation

A minimal requirement of democratic accountability is that the potential exists for the ruling party to lose elections: when elections work, they make changes in leadership more likely (Przeworski et al., 2000).

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 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

[1] It is typically assumed that $w > v$, otherwise the bureaucrat will simply leave the bureau.

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). Along similar lines, Gamboa-Cavazos, Garza-Cantú, and Salinas (2007) and Campante, Chor, and Do (2009) find evidence in Mexico that leaders with longer tenure are less corrupt than those with short horizons.

I argue that leadership turnover may produce pessimism among bureaucrats about their future income, because new leaders may change bureaucratic appointments and budgetary priorities.

In young democracies, office-seekers often use their ability to appoint positions in the public sector toward electoral ends, promising jobs to important political allies or doling out public sector work as a form of patronage on a broader scale. 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%. When political leaders can easily reappoint bureaucrats to suit their aims, and political leaders are frequently replaced, bureaucratic turnover is likely to be particularly high.

In West African democracies, elections frequently result in transformations of the bureaucracy. In Ghana, for example, after Kwame Nkrumah was elected as the country's first Prime Minister in 1951, the senior civil service grew from 1,970 to 3,515 positions (Lentz, 2014, 179).

Ghanaians appear to rely heavily on political patronage to obtain and retain coveted positions in the civil service. Based on expert surveys, Kopeckỳ (2011) estimates that 67% of jobs in the police sector, for example, are based on appointment by the executive branch. Lentz (2014) describes the case of a civil servant in Ghana who attested to having lost his position as Director General of the Prison Service due to a lack of connections to the right patrons in the ruling party.

Analyzing reappointments of forestry agents in Senegal, 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.

Anecdotal accounts thus suggest the obtention and retention of coveted public sector jobs is often determined by who holds executive and parliamentary power in West African states. But this is not the only way in which public sector incomes may be affected by changes in leadership. In developing countries especially, budgetary priorities can shift radically in response to popular pressure. New leaders bring with them new priorities and new electoral coalitions. They may seek to radically alter the budgetary priorities in order to suit their aims. As Terry Moe put it,

> while the right to exercise public authority happens to be [with the incumbent party] today, other political actors with different and perhaps opposing interests may gain that right tomorrow, along with legitimate control over the politics and structures that their predecessors put in place. Whatever today's authorities create, therefore, stands to be subverted or perhaps completely destroyed—quite legally and without any compensation whatever—by tomorrow's authorities (Moe, 1990, 227).

One example of the potential for such destruction was given in Mali in the run-up to the 2007 elections. In 2002, Alpha Oumar Konaré had endorsed a Poverty Reduction Service Paper (PRSP), a strategic document required by the World Bank and International Monetary Fund in order to qualify for the sort of debt relief and loans that are often used 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. He instead campaigned on his own *Programme de développement économique et social*, provoking fears that Mali might lose foreign aid necessary to fund the public sector. Thus, the sensitivity of the public sector to snap decision-making around foreign aid and budgetary priorities in West Africa can lead to a highly uncertain environment for public servants.

According to Erdmann and Engel (2007, 107-8), corrupt behavior by bureaucrats under such "neopatrimonial" environments is in part "a means to gain protection [...] in a situation of societal uncertainty created by public institutions which may behave in ways that are not calculable." In other words, supplementing one's income through extortion becomes more attractive when one's expectation of enjoying current wages into the future appears less likely.

Compared to simply doing one's job and receiving one's salary as a public servant, extortion can be a very costly strategy. In May 2017, the Ghanaian Ministry of Finance suspended the salaries of over 26,000 public sector workers suspected of corruption. But even when monitoring capacity is low, extortion is a tiresome way to earn a living. 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. Police would sometimes wait hours for the more resistant drivers to produce the demanded bribe, arguing all the while. Moreover, petty corruption is maligned throughout West African media as a scourge on economic development. I often observed what can only be described as embarrassment on the faces of police officers and customs officials when, as they extorted us, they noticed me watching (although, as I show in Cooper, 2018, this did very little to prevent them from extorting the driver).

Since officials cannot increase the amount of traffic they can stop, increasing prices is the only means at their disposal for raising their income from extortion. Yet it is a difficult and costly process. While many drivers were willing to pay a small "going rate," the occasional official who departed from this, and demanded more than the going rate, met with outraged protests and threats from the driver.

As argued above, as the threat of losing one's job or salary increases, so too 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. When one is certain of keeping one's income, the benefits of engaging in extortion may not outweigh the costs.

There are of course countries in which elections do not translate into job or income insecurity for bureaucrats. A civil servant in an autocracy that is about to have facade elections can reasonably expect continuity in the current policy arrangements, confident in the tenure of the present leaders. In most consolidated democracies, competitive elections are coupled with strong civil service legislation insulating the bureau from political influence. The depoliticization of the bureaucracy is a key process in the transition from clientelist to programmatic politics (Ting et al., 2013; Cruz and Keefer, 2015). As democracies mature and elected officials pursue more ambitious policy programs, they may seek to perennialize the bureaucratic arrangements necessary to implement large programs by insulating the bureau from future political influence (Horn, 1995). Ting (2012) argues, for example, that incumbent parties undertook insulating civil service reforms in the U.S. when they anticipated upcoming electoral losses, effectively seeking to make it harder for incoming parties to undo prevailing bureaucratic arrangements.

1.2 Hypotheses

Low leader turnover and high bureaucratic insulation thus constitute sufficient conditions for bureaucratic certainty about future income streams. When leader turnover is low, as in autocracies, bureaucrats can be relatively confident in the continuity of prevailing arrangements, even if the bureau is not insulated from political influence. Similarly, even if leader turnover is very high, in consolidated democracies where civil service legislation insulates the bureau from political influence, bureaucrats can be relatively sure that their jobs and incomes will survive elections unaltered.

By contrast, in many young democracies—such as India, Senegal, Ghana, Thailand, Papua New Guinea, Uganda, Brazil, Mexico and so on—high rates of leader turnover are accompanied by low levels of bureaucratic insulation. Such states typically find themselves at the 'apex' of the inverted U relationship between bureaucratic corruption and political competition. In effect, the temptation to engage in risky or costly extraction of bribes may be stronger in such states because the opportunity cost associated with not taking bribes is higher when there is uncertainty about future wages. In such contexts, the extortion of bribes might constitute an insurance strategy for bureaucrats facing conditions of uncertainty.

If elections affect bureaucratic corruption primarily by creating uncertainty about future income streams among bureaucrats, we should expect to see that corruption increases in the buildup to elections, but only if the election is actually competitive to some extent:

H1. The average bribe extorted by bureaucrats should increase in the buildup to elections with uncertain outcomes (competitive elections).

H2. The average bribe extorted by bureaucrats should be unaffected by the buildup to elections with certain outcomes (uncompetitive elections).

After the election we should expect different dynamics as a function of the outcome. Specifically, if the incumbent wins the election this should restore bureaucrats' certainty about future income streams. If a challenger wins, however, this might further increase extortion by increasing uncertainty about future public employment policies.

H3. The average bribe extorted by bureaucrats should decrease in the post-election period when the incumbent wins (challenger loses).

H4. The average bribe extorted by bureaucrats should increase in the post-election period when the incumbent loses (challenger wins).

2 Research Design

To test for the effect of elections on bureaucratic corruption, I build a weekly panel on extortion in five West African countries from 2006 to 2013. The data comes from two principal sources: micro-data on over 300,000 bribes paid by a representative sample of some 31,000 truck drivers in West Africa from 2006 to 2013; and data on seven elections in the same five countries.

2.1 Data on bribes

The data on extortion 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). 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 ethnographic fieldwork with truck drivers in Burkina Faso, Ghana, Togo and Benin, participating in 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 industry, including union representatives, drivers, and public agencies working on trade facilitation.

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 0.50 USD to 20 USD, under threat of various sanctions, including long delays, physical violence and even unlawful detention.

The WATH dataset details over 300,000 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. Not all of these stops record bribes: drivers were also asked to note when they were stopped but not asked to pay anything—in this case the data contains a bribe of 0. 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.

During the data collection period teams of two to three enumerators would randomly sample truck drivers using a random-walk methodology at ports and truckyards in Ghana, Benin, Mali, Burkina Faso, Senegal and Togo. 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. Since 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 underestimate of the average bribe paid by *all* drivers, as drivers might pay more in bribes to travel 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.

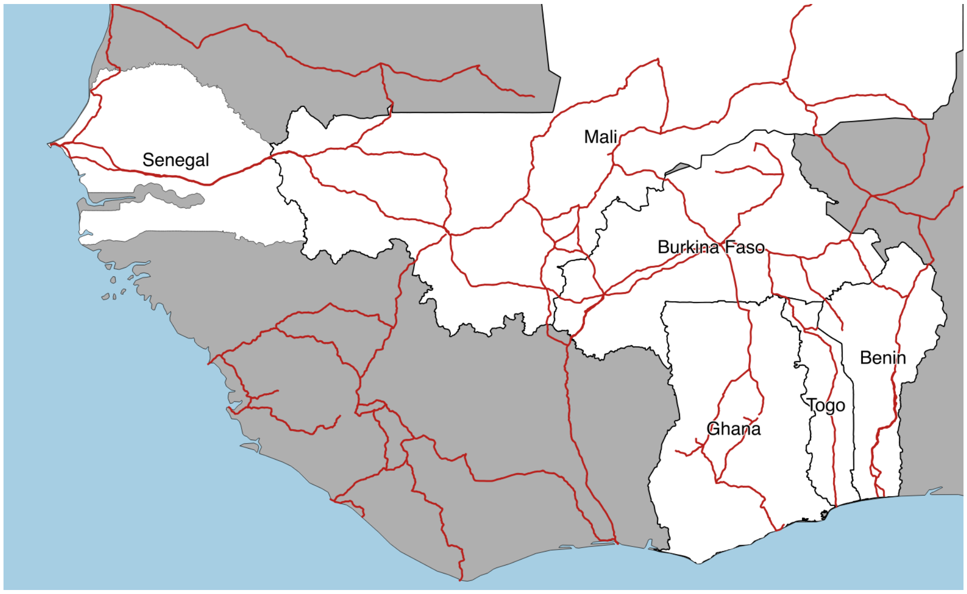

Figure 1: Trade Highways in West Africa.
Countries in white included in the sample, red lines indicate major highways. Shapefiles for administrative boundaries from http://www.gadm.org/ and for supply routes from https://geonode.wfp.org/

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.[2]

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. However, to mitigate concerns about reporting bias, I do not focus on the absolute level of bribes reported, but rather on marginal effects. Assuming that any systematic error in reporting is not correlated with electoral periods, inferences about marginal effects will remain unbiased. Nevertheless, there is some concern that drivers could

[2]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' 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, his personal income is reduced every time he pays a bribe.

exaggerate bribes around election times as a way of expressing discontent with their government. For this reason I condition analyses on whether the driver was a foreigner in the country in which he paid the bribe, as it is less likely that foreigners will be prone to partisan cycles in countries where they cannot vote.

2.2 Elections in West Africa

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

Country	Year	Vote Margin	Incumbent reelected
Ghana	2008	0.01	No
Ghana	2012	0.02	Yes
Mali	2007	0.52	Yes
Burkina Faso	2010	0.72	Yes
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, they exhibit considerable variation in their degree of electoral competition. In their report on regimes around the world in 2010, for example, the group behind the Polity IV regime measure classified Ghana, Mali and Senegal as democracies and their neighbors, Togo and Burkina Faso, as autocracies (Marshall and Cole, 2011).

As Table 1 illustrates, the elections also vary within and across countries in terms of their vote margins. At the date of writing, the 2008 presidential election in Ghana was the closest ever, 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. On the other end of the spectrum, the incumbent President Blaise Campaoré 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."[3] Elections in the same year

[3]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`

in Togo were similarly criticized for their predictability, lack of competition, and strong signs of manipulation. By contrast, 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 competitive in the sense that vote shares were very hard to predict. There was almost no polling data in the buildup to either election.

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, Faure Gnassingbé prolonged his family's forty-three year reign over Togolese politics, inherited following the death of his father Eyadéma Gnassingbé in 2005. By contrast, no leader in the three democratic countries served more than two constitutionally mandated terms over the period under analysis.

In sum, while all countries had elections during the period covered by the bribes data, they produced very different expectations about future leadership. While elections in Burkina Faso and Togo were seen as facade institutions that posed no serious threat to the longevity of the rulers there, those in other countries represented very real threats of leadership change.

2.3 Identification Strategy

Because the theoretical predictions pertain to the calculations and decisions of individual bureaucratic agents, in the main analysis I focus on differences in prices set by individual bureaucrats, and not the total amount that a driver is extorted in a given country or over the course of a given trip. In supplementary analyses I also examine effects on the total number of checkpoints encountered over a given trip, however, as a way of testing an alternative theory in which the decision-making of principals causes electoral cycles in extortion.

The principal outcome in which I am interested is the average bribe that a bureaucrat extorts from a truck driver at a given point in time in a given country. The main estimand is the true average difference in the average bribe extorted in a given country-week during electoral versus non-electoral periods. An electoral period is defined as the three months preceding or following an election. I am also interested in the heterogeneity of electoral period effects by whether the period is post- or pre- election, and by whether the election was competitive (had an uncertain outcome).

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. This represents a relatively conservative approach to clustering.

The panel is 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 occasional unforeseen delays in funding approval. Nevertheless, as missing data can constitute a source of bias even if only incidentally 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). As Figures 2 and 3 in section A.1 of the appendix show, many fewer periods require imputation when collapsing to the month level, although this reduces efficiency. I demonstrate robustness of the main results to imputation by estimating the main models at the month level in section B.3 of the appendix. Finally, since Senegal accounts for many of the missing values, I also demonstrate the results are robust to the exclusion of Senegal in section B.4 of the appendix.

The principal identification concern is that the relationship between the average bribe extorted in a given country-week and the timing of an electoral period is confounded by some other set of variables. To address this concern I take two main approaches.

The first is premised on a parallel trends assumption, and involves regressing the average bribe in a given country-week on indicators for country, week and electoral period,

$$y_{ct} = \gamma_c + \lambda_t + \tau Z_{ct} + \epsilon_{ct}, \tag{2}$$

where y_{ct} is the average bribe in country c in week t, γ_c is a country fixed effect, λ_t is a week fixed effect, and τZ_{ct} is an indicator for whether week t less than three months before or after an election in country c. The parameter τ thus identifies the effect of an electoral period on the average bribe, using the generalized difference-in-differences estimator (Angrist and Pischke, 2009).

One issue with this approach is that it does not leverage information at a lower level of aggregation. The second approach to identification involves residualizing the dependent variable at the country-week-driver-level using a linear model that conditions on confounders, before aggregating bribes to the country-week panel. I focus on three sources of confounding.

First, elections may take place at certain times in the year or even on certain days of the week for which bribes are seasonally higher or lower. I therefore difference out weekday (e.g., Tuesday), calendar month (e.g., February) and year (e.g., 2010) effects to account for potential confounding in the timing of elections and bribe seasonality.

Second, elections of a certain kind may be more or less frequent at times when bribes in a country were generally trending upwards or downwards, say due to anti-corruption policies or changing economic conditions. To account for such confounding, I difference out country-specific linear trends.

Finally, there is a concern that certain kinds of drivers may self-select into or out of electoral periods. Qualitatively, there is very little evidence of such trends: logistics tends to be a margin-driven industry, and drivers respond to demand as soon as it appears. Nevertheless, I also difference out a range of driver-level effects, including whether the driver is foreign when he pays the bribe, and the specific route and direction of travel—which proxy for whether the good was destined for import or export. Both foreignness and whether the truck is headed in an import or export direction have been identified as significant predictors of the bribe paid in previous work on this data (Bromley and Foltz, 2011).

The remaining variation in the dependent variable is thus independent of many season-, country- and driver-specific effects. I estimate electoral cycles using the following linear equation,

$$\tilde{y}_{ct} = \gamma_c + \tau Z_{ct} + \epsilon_{ct}, \tag{3}$$

where $\tilde{y}_{ct}$ is the average of the residualized bribe in country c in week t, and other parameters are as above. In section B.5 of the appendix, I show that the main results are robust to alternative residualization methods.

2.4 Statistical Inference

I take two approaches to inferring the probability of observing point estimates given the estimated sampling variability.

The first takes a parametric approach that accounts for both spatial and temporal autocorrelation in order to construct p-values from standard errors (Shin, 2017). As noted, within-driver and within-week correlation in errors is accounted for by simply collapsing to the week-country level. There is some concern that the bribe paid at time $t-1$ is correlated with the average bribe paid at time t. Serial correlation in the data is accounted for through the use of an AR(1) model in the main results. In all main specifications I thus add an one-period lag of the dependent variable to equations 2 and 3. In Tables 7 and 8 of the appendix, I show that the substantive size of the coefficients and their statistical significance are unchanged when a lag of the dependent variable is not included, and the regressions are estimated exactly as above.

As section A.2 of the appendix illustrates, the partial autocorrelation of the time-series appears to fall short of significance after one or two lags in most countries. However, in section B.2 of the appendix, I show that main results are robust to the inclusion of up to four lags. To account for the possibility that errors are non-spherical, I use a panel-corrected standard error (PCSE) sandwich type estimator to estimate the covariance matrix (see Beck and Katz, 1995), implemented in the `pcse` package for `R` (Bailey and Katz, 2011). All main regression specifications include a country-level fixed effect.

The second approach treats the assumption that the residualized potential outcomes are independent of election timing as a quasi-experimental assumption. The experimental analogue for the study is one in which countries are blocks and groups of country-weeks are cluster-assigned to be in electoral periods. This analogue gives rise to a basis for inference using Fisher-style permutation tests.

I generate the distribution of possible election effects under the sharp null of no electoral effects for any week-month by permuting 2,000 placebo elections between December 2006 and May 2013. On each permutation I respect the actual number of elections that took place in each country (two in Ghana and Senegal, one in the other countries). I also permute incumbent wins and losses in countries with competitive elections (with probability .5), while setting incumbent win probabilities to 1 in Togo and Burkina Faso. I compute randomization inference (RI) p-values by taking the proportion of estimated election effects at least as large in absolute value to the observed election effects.

3 Main Results

Table 2 illustrates the average bribe that bureaucrats extort from drivers systematically increases around elections, particularly in the buildup to the election. The first two columns estimate the effect of the so-called “Election Period” by regressing the bribe variable on an indicator that is 1 if the country-week falls within the three months preceding or following an election in that country and 0 otherwise. Bribes are roughly 0.36 USD higher during this period ($p < 0.05$). The average bribe extorted in all countries in all country-weeks in the panel was 1,304 XOF, implying average bribes are approximately 14% higher during election periods. As with all others, this estimate is conditional on time-invariant differences between countries, country-specific linear trends, the year, month of year and day of week in which the bribe was paid, as well as the direction of travel and whether the driver was a co-national of the bureaucrats when paying the bribe.

	Average Bribe Paid			
	(1)	(2)	(3)	(4)
Lagged Avg. Bribe	0.399***	0.391***	0.392***	0.387***
	(0.028)	(0.027)	(0.028)	(0.027)
Election Period	165.889***	179.940***		
	(30.285)	(28.827)		
Pre-Election Period			235.757***	230.408***
			(41.255)	(38.038)
Post-Election Period			40.698	68.887*
			(40.860)	(37.251)
Residuals	No	Yes	No	Yes
Period FE	Yes	No	Yes	No
Country FE	Yes	Yes	Yes	Yes
RI p-value: Elec. Per.	0.0185	0.0095		
RI p-value: Pre-Elec.			0.005	0.007
RI p-value: Post-Elec.			0.5485	0.244
Observations	1,770	1,770	1,770	1,770
Adjusted R^2	0.657	0.202	0.658	0.203
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 2: Police extortion of truck drivers exhibits electoral cycles. Extortion is especially high in the buildup to elections.

All data aggregated from the checkpoint-driver-day level to the driver-level and then to the country-week level through arithmetic averaging. Missing country-weeks imputed through linear interpolation. The p-values and variance estimates reported in the main table arise from a panel-corrected standard error estimator. RI p-values are calculated by comparing observed effect sizes to the distribution of effects under the sharp null of no election effects, calculated by simulating placebo elections and re-estimating effects 2,000 times. In columns labeled 'Yes' for residuals, dependent variable is the residuals from a regression of the bribe on year, weekday, calendar month, country-specific linear trend, direction of travel, trade corridor, and whether driver is national of country in which bribe is paid. 'Election Period' is 1 if bribe paid in three months preceding or following election in that country, 0 otherwise. '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.

Columns 2 and 3 indicate that this relationship is driven primarily by an increase in average bribes extorted in the three-month period building up to elections. Post-election, bribes appear to fall to levels much closer to the average in non-electoral periods. The variables "Pre-Election Period" and "Post-Election Period" are indicators that take the value 1 if the country-week falls within the three months preceding or following an election in that country, respectively, and 0 otherwise. Police extorted bribes that were almost .50 USD higher on average in the buildup to elections, representing an 18% increase in relative terms. Simulating all elections at random dates and re-estimating the effects 2000 times, we obtain an estimate as large in absolute value as the observed one less than 1% of the time, suggesting we can reject the null of no pre-election effect with 99% confidence ($p < 0.01$).

In Table 17 of the appendix, I report heterogeneity in pre- and post- election effects by country. The observed χ^2 statistic of the difference in the sum of squared residuals between a model in which slopes vary by country versus the models in Table 2 is highly unlikely to arise by chance ($p < 0.01$). In other words, there exists statistically significant heterogeneity in electoral cycles by country. The results suggest electoral periods produce much smaller (even negative) effects in the two autocratic states of Burkina Faso and Togo, and much larger positive effects in the three democratic states.

The fact that bureaucrats change their extortionary strategies around elections most obviously in democratic states and least obviously in autocratic states is suggestive of a political logic that we can address directly. In the final set of main results, which provide the most direct test of hypotheses 1-4, I model pre- and post-electoral periods as a function of whether the country holds competitive elections and of the outcome of the election. Thus, pre- and post-election trends for non-competitive elections are estimated by constraining the effects to be the same for Togo and Burkina Faso. For competitive states, the pre-election variable is constrained to be common across all elections in Ghana, Mali and Senegal, while the post-election variables are split into situations in which a challenger won (as in Ghana in 2008 and Senegal in 2012), and in which the incumbent was reelected (all other elections in those three countries).

Column 2 of Table 3 presents the main results. The effect of pre- and post-election periods of average bribes in non-competitive states is substantively small (6-10 cents in USD) and statistically insignificant. By contrast, average bribes are estimated to increase by 297 XOF (0.60 USD) in the buildup to competitive elections, which translates to a 22% increase relative to the non-electoral average of 1,305 XOF in those same countries. These findings are consistent with hypotheses 1 and 2, according to which corruption will increase in the buildup to elections, but only if they are competitive and thus provoke uncertainty about future leadership.

Turning to post-election outcomes in competitive states, we see that when new leaders are elected bribes remain higher than average, to the order of 15% relative to non-electoral periods in those countries. Parametric variance estimates suggest a highly significant effect, whereas the randomization inference approach yields a p-value of .173 for the difference-in-differences model and of .081 for the residualized model. Thus, while the estimates certainly do not point to a large post-election increase, they suggest bureaucrats continue to extort at relatively high levels when challengers win.

However, when incumbents win elections effects bribes fall again to levels that are not statistically distinguishable from non-electoral periods. This pattern of evidence is consistent with hypotheses 3 and 4, according to which the uncertainty provoked by new leadership causes bureaucrats to extort more in the aftermath of elections. Under this model of political competition's effect on corruption, we do not expect to see strong evidence of increased corruption when incumbents are reelected, and indeed we do not.

	Average Bribe Paid	
	(1)	(2)
Lagged Avg. Bribe	0.382***	0.376***
	(0.029)	(0.028)
Pre-Election (Non-Competitive)	51.438	31.668
	(82.679)	(71.486)
Post-Election (Non-Competitive)	25.474	−24.703
	(82.696)	(71.481)
Pre-Election (Competitive)	295.316***	296.925***
	(48.567)	(44.882)
Post-Election (Competitive - Challenger Won)	177.133***	193.187***
	(59.742)	(48.717)
Post-Election (Competitive - Incumbent Won)	−59.038	35.302
	(70.111)	(65.479)
Residuals	No	Yes
Period FE	Yes	No
Country FE	Yes	Yes
RI *p*-value: Pre-Elec. (NC)	0.587	0.618
RI *p*-value: Post-Elec. (NC)	0.765	0.72
RI *p*-value: Pre-Elec. (C)	0.01	0.01
RI *p*-value: Post-Elec. (C-CW)	0.173	0.081
RI *p*-value: Post-Elec. (C-IW)	0.627	0.764
Observations	1,770	1,770
Adjusted R^2	0.660	0.208
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 3: Elections only increase police extortion when they pose a real threat of replacing the incumbent leader.

All data aggregated from the checkpoint-driver-day level to the driver-level and then to the country-week level through arithmetic averaging. Missing country-weeks imputed through linear interpolation. The *p*-values and variance estimates reported in the main table arise from a panel-corrected standard error estimator. RI *p*-values are calculated by comparing observed effect sizes to the distribution of effects under the sharp null of no election effects, calculated by simulating placebo elections and re-estimating effects 2,000 times. In columns labeled 'Yes' for residuals dependent variable is the residuals from a regression of the bribe on year, weekday, calendar month, country-specific linear trend, direction of travel, trade corridor, and whether driver is national of country in which bribe is paid. 'Non-competitive' indicates the variable is common to Togo and Burkina Faso, while 'Competitive' indicates the variable is common to Ghana, Mali and Senegal. 'Pre-Election' and 'Post-Election' are 1 if bribe paid in the three months preceding or following an election in that country, respectively, 0 otherwise. 'Challenger Won' and 'Incumbent Won' are 1 if the challenger or incumbent won the preceding election, respectively, 0 otherwise.

4 Alternative Mechanisms

The main results illustrate that there are electoral cycles to corruption, even when accounting for the timing of elections, country-specific trends, and a host of contextual factors. Overall, the heterogeneity in these cycles among different countries produces a pattern of evidence that is consistent with the proposition that political competition increases corruption by provoking uncertainty about public sector incomes. The estimation strategy helps to rule out a number of possible confounders that would lead to spurious relationships between electoral cycles and bribes, and in the appendix I show the results are robust to a range of alternative approaches. To delve more deeply into the proposed mechanism I address here two alternative substantive mechanisms that might link elections and corruption.

The first mechanism that might produce a systematic link between extortionary dynamics and elections is the instrumentalization of the bureaucracy by the ruling party in order to raise campaign funds through extortion. I refer to this as the *Party Capture* mechanism.

Bribes might constitute an important source of campaign finance during elections. Especially where bureaucratic insulation is low, incumbent parties may be able to use the bureaucracy to gather additional revenues for use in campaigns (Doig, 1999; McMann et al., 2017). This would create a systematic relationship between bureaucratic corruption and elections, even in the absence of leadership uncertainty. As many scholars of autocratic regimes have shown, non-competitive elections can play an important institutional function in autocratic and semi-autocratic regimes (Magaloni, 2008; Blaydes, 2010). In both autocracies and new democracies, ruling parties may instrumentalize the bureau to fund their election campaigns.

The second mechanism that might link leadership change to bureaucratic extortion is *Opportunism* by bureaucrats.

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 building personal relationships with the heads of those bureaus, which takes time and political capital. Civil servants 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.

As a first appraisal of the evidence in support of these three alternative accounts, we can contrast their predictions with the evidence presented in Table 3 above. The rows of Table 4 represent four different changes in the electoral dynamics in a given week-month and predictions about how extortionary dynamics will be affected under the mechanisms posited in the columns.

Predictions in Table 4 are underlined when they find support in the main specification reported in column 2 of Table 3. As the final row of the table indicates, the predictions of the leadership uncertainty mechanism are best supported by the data. If party capture of the bureaucracy explains the relationship between corruption and elections in West Africa, extortion should not remain high after elections and should always increase before elections. Similarly, if bureaucrats act in a purely opportunistic manner, extortion should not increase before elections. Both stipulations are contradicted by the data.

	Proposed mechanism		
	Leadership Uncertainty	Party Capture	Raw Opportunism
Moving from non-electoral period to period before **uncompetitive** election, average bribe will...	<u>remain constant</u>	increase	<u>remain constant</u>
Moving from non-electoral period to period before **competitive** election, average bribe will...	<u>increase</u>	<u>increase</u>	remain constant
Moving from pre-electoral period to period following **competitive** election won by **incumbent**, average bribe will...	<u>decrease</u>	<u>decrease</u>	remain constant
Moving from pre-electoral period to period following **competitive** election won by **challenger**, average bribe will...	<u>remain constant</u>	decrease	increase
Predictions consistent with Table 3	4/4	2/4	1/4

Table 4: The evidence from the main results most strongly supports the leadership uncertainty mechanism. Predictions are underlined when they are consistent with the estimates in column 2 of Table 3.

The party capture and opportunism mechanisms imply other observable hypotheses that I test here. First, if extortion increases because the incumbent party uses the bureaucracy to generate funds around elections, we would expect to see the number of checkpoints increase. While individual bribes are very hard for principals to monitor and thus to manipulate, the existence of a checkpoint is easily observed. Thus, a principal looking to extract a larger amount cumulatively would likely do better by increasing the extensive margin (increasing the number of agents extracting bribes) than by increasing the intensive margin (increasing the average bribe extorted).

However, as Table 5 illustrates, there is no evidence to support this mechanism. We see no signs that the average number of checkpoints encountered by truck drivers over the period under consideration varies systematically with electoral cycles. All of the estimated coefficients are substantively small and statistically insignificant.

	N Checkpoints Per Trip		
	(1)	(2)	(3)
Lagged N Checkpoints	0.562***	0.562***	0.562***
	(0.025)	(0.025)	(0.025)
Election Period	−0.010		
	(0.330)		
Pre-Election Period		−0.037	
		(0.439)	
Post-Election Period		0.090	
		(0.436)	
Pre-Election (Non-Competitive)			0.006
			(0.555)
Post-Election (Non-Competitive)			0.260
			(0.555)
Pre-Election (Competitive)			−0.047
			(0.562)
Post-Election (Competitive - Challenger Won)			−0.009
			(0.739)
Post-Election (Competitive - Incumbent Won)			0.050
			(0.813)
Country FE	Yes	Yes	Yes
Observations	1,770	1,770	1,770
Adjusted R^2	0.454	0.454	0.453
Note:	*p<0.1; **p<0.05; ***p<0.01		

Table 5: There is no evidence of electoral cycles in the number of checkpoints.
All data aggregated from the checkpoint-driver-day level to the driver-level and then to the country-week level through arithmetic averaging. Missing country-weeks imputed through linear interpolation. All p-values and variance estimates from panel-corrected standard error estimator. 'Election Period' is 1 if bribe paid in three months preceding or following election in that country, 0 otherwise. 'Non-competitive' indicates the variable is common to Togo and Burkina Faso, while 'Competitive' indicates the variable is common to Ghana, Mali and Senegal. 'Pre-Election' and 'Post-Election' are 1 if bribe paid in the three months preceding or following an election in that country, respectively, 0 otherwise. 'Challenger Won' and 'Incumbent Won' are 1 if the challenger or incumbent won the preceding election, respectively, 0 otherwise.

Finally, to discriminate better between the opportunism and leader uncertainty accounts I analyze two non-electoral leadership changes that took place in Ghana and Mali over the period under consideration. The first is the sudden death of John Atta Mills in Ghana in 2012. Following his death, the Vice President John Mahama took over in a smooth transition. While the 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. The second non-electoral leadership change is the 2012 coup in Mali. The coup brought in a new government whose priorities were very unclear, introducing a significant amount of uncertainty about their future policies. While it was known that there were tensions between the government and the armed forces tasked with controlling the Tuareg rebellion in the north, there were very few indications that this would result in a total change in leadership via military means. The militia that took over had very different aims and goals to the political leaders they replaced.

The two instances of non-electoral turnover in 2012 represent very different outcomes in terms of leadership uncertainty: while Mahama's takeover in Ghana augured continuity with the status quo, the coup in Mali overturned pre-existing arrangements in a dramatic fashion. Under the leadership uncertainty mechanism, we would therefore only expect an increase in bribes resulting from the Malian coup, while the opportunism mechanism would lead to increases in both cases.

Table 6 provides support for the leadership uncertainty mechanism and against the opportunism mechanism. If anything, bribes *decreased* in the aftermath of Atta Mills' replacement by Mahama, although the coefficient is statistically insignificant. Consistent with both mechanisms, the coup is estimated to increase the average bribe extorted, although the estimates are very noisy. Again, we see a strong electoral cycle in the average bribe paid, suggesting regular, routine elections are a more substantial determinant of extortion than political instability.

	Average Bribe Paid	
	(1)	(2)
Lagged Avg. Bribe	0.391***	0.390***
	(0.028)	(0.027)
Post-Presidential Death	−123.185	−91.954
	(104.180)	(92.356)
Post-Coup	293.900*	45.274
	(153.037)	(154.783)
Election Period	177.307***	183.353***
	(30.588)	(29.036)
Residuals	No	Yes
Period FE	Yes	No
Country FE	Yes	Yes
Observations	1,770	1,770
Adjusted R^2	0.658	0.202
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 6: Non-electoral leader turnover only increases extortion when it changes the direction of leadership. All data aggregated from the checkpoint-driver-day level to the driver-level and then to the country-week level through arithmetic averaging. Missing country-weeks imputed through linear interpolation. All p-values and variance estimates from panel-corrected standard error estimator. In columns labeled 'Yes' for residuals dependent variable is the residuals from a regression of the bribe on year, weekday, calendar month, country-specific linear trend, direction of travel, trade corridor, and whether driver is national of country in which bribe is paid. 'Post-Presidential Death' is 1 if bribe paid in Ghana in three months following the death of President John Atta Mills, 0 otherwise. 'Post-Coup' is 1 if bribe paid in Mali in three months following the coup in 2012, 0 otherwise. 'Election Period' is 1 if bribe paid in three months preceding or following election in that country, 0 otherwise.

5 Discussion

Puzzling over young democracies' tendency to exhibit higher levels of corruption than autocracies, Treisman (2000, 45) concludes that "The fact that a country is democratic today makes just about no difference to how corrupt it is perceived to be. What matters is whether or not it has been democratic for decades. The regression estimates suggest a painfully slow process by which democracy undermines the foundations of corruption."

In this paper, I have proposed an explanation for this slowness: against the predictions of accountability theory, political competition may actually increase corruption in the absence of structures that insulate the bureaucracy from political influence. By increasing the likelihood of future leader turnover, political competition decreases bureaucrats' expectation of enjoying present income streams into the future and raises the opportunity cost of remaining honest. Using fine-grained time-series cross-sectional data on bureaucratic extortion over a seven-year period in five countries that vary strongly in their degree of political competition, I provide evidence in support of this theory. In the buildup to elections that are competitive, the average bribe extorted by bureaucrats increases by 22% relative to non-electoral periods, and when new leaders win office it remains high at around 15% above the non-electoral period average. Consistent with the idea that political competition only increases extortion when it increases uncertainty, bribes in the post-election period return to the non-electoral average when incumbents win reelection. Moreover, we observe no such dynamics around elections in states with little to no political competition. The estimation strategy rules out a range of potential temporal and contextual confounders related to the timing of elections, and supplementary analyses cast doubt on alternative substantive mechanisms linking extortion and elections.

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). Surprisingly, however, this insight has not been applied to understanding the relationship between political competition 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. The results and theory presented in this paper shed new light on the literature on the non-linear relationship between democracy and corruption.

McMann et al. (2017, 4), for example, 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." Sung (2004, 181) similarly points to "the enormous costs of mounting electoral campaigns" in explaining why elections might increase corruption.

However, 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. Furthermore, I find that elections only increase the average bribe extorted when they are competitive. These facts are inconsistent with the notion that extortion is used to fund autocratic elections.

Other studies argue that political liberalization does not reduce corruption due to a lack of political competition. According to such accounts, 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." 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). By contrast, the theory presented in this paper provides good reasons to see 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.

By taking seriously the direct effects of political competition on bureaucratic decision-making and testing the resulting predictions empirically through unusually fine-grained data, this paper provides some nuance to the understanding of how political competition affects corruption. Increased political competition may not constitute a silver bullet for reducing bureaucratic corruption, even if it helps to dampen high-level corruption by improving the incentives of elected legislators and executives in the government (Ferraz and Finan, 2011). Without the necessary insulation of the bureau from their influence, corruption may even increase as a result of democratization, due to the (real or perceived) harm that increased leader turnover does to public sector job security.

More research into the direct effects of democratization on bureaucratic behavior is required in order to substantiate these findings. In particular, because the present study has focused on dynamic processes that vary over the short- to intermediate-term, we cannot conclude from its findings that long-term differences in corruption result from leadership uncertainty. Rather, the findings presented in this paper illustrate the plausibility of leader uncertainty as a causal mechanism mediating the concave relationship between political competition and bureaucratic corruption. If the results of this study are to be believed, then 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 cross-national levels of corruption among democracies.

6 Conclusion

I have proposed a novel explanation for the concave relationship between political competition and corruption: by increasing the risk of job or wage volatility through increased leader turnover, democratization increases bureaucrats' temptation to engage in risky extortionary strategies, thereby worsening corruption in the short-term. The empirical analysis leveraged a dataset on over 300,000 self-reported bribes paid in five West African countries to test this claim against competing theoretical accounts, looking specifically at the effect of moments of potential and actual leadership changes on the corrupt behavior of bureaucrats. 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, but only when challengers with new policy priorities win. 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.

References

Adsera, Alicia, Carles Boix, and Mark Payne. 2003. "Are You Being Served? Political Accountability and Quality of Government." *The Journal of Law, Economics and Organization* 19 (2): 445–490.

Akhtari, Mitra, Diana Moreira, and Laura Carolina Trucco. 2017. "Political Turnover, Bureaucratic Turnover, and the Quality of Public Services." Working Paper.
URL: *https://scholar.harvard.edu/files/makhtari/files/akhtari_moreira_trucco_feb_15.pdf*

Angrist, Joshua David, and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics.* Princeton, Princeton University Press.

Bailey, Delia, and Jonathan N. Katz. 2011. "Implementing Panel Corrected Standard Errors in R: The pcse Package." *Journal of Statistical Software* 42 (1): 1–11.

Beck, Nathaniel, and Jonathan N. Katz. 1995. "What to do (and not to do) with time-series cross-section data." *American Political Science Review* 89 (3): 634–647.

Becker, Gary S., and George J. Stigler. 1974. "Law Enforcement, Malfeasance and the Compensation of Enforcers." *Journal of Legal Studies* 3: 1 – 18.

Blaydes, Lisa. 2010. *Elections and distributive politics in Mubarak's Egypt.* Cambridge, Cambridge University Press.

Blundo, Giorgio. 2014. "Seeing like a state agent: The ethnography of reform in Senegal's forestry services." *States at work: Dynamics of African bureaucracies* pp. 69–90.

Bromley, Daniel, and Jeremy Foltz. 2011. "Sustainability under siege: Transport costs and corruption on West Africa's trade corridors." *Natural Resources Forum* 35.

Campante, Filipe R., Davin Chor, and Quoc-Anh Do. 2009. "Instability and the Incentives for Corruption." *Economics and Politics* 21 (1): 42 – 92.

Charron, Nicholas, and Victor Lapuente. 2010. "Does democracy produce quality of government?" *European Journal of Political Research* 49 (4): 443–470.
URL: *http://dx.doi.org/10.1111/j.1475-6765.2009.01906.x*

Chong, Alberto, Ana L. De La O, Dean Karlan, and Leonard Wantchekon. 2014. "Does corruption information inspire the fight or quash the hope? A field experiment in Mexico on voter turnout, choice, and party identification." *The Journal of Politics* 77 (1): 55–71.

Clague, Christopher, Philip Keefer, Stephen Knack, and Mancur Olson. 1996. "Property and Contract Rights in Autocracies and Democracies." *Journal of Economic Growth* 1 (2): 243 – 276.

Cooper, Jasper. 2018. "How Robust is Institutionalized Extortion? A Field Experiment with Truck Drivers in West Africa.".
URL: *http://jasper-cooper.com/papers/Cooper_trucks.pdf*

Cruz, Cesi, and Philip Keefer. 2015. "Political Parties, Clientelism, and Bureaucratic Reform." *Comparative Political Studies* 48 (14): 1942–1973.

Cruz, Cesi, Philip Keefer, and Julien Labonne. 2016. "Incumbent Advantage, Voter Information and Vote Buying." IDB Working Paper.
URL: *http://pubdocs.worldbank.org/en/107701477620421939/Incumbent-Advantage-Voter-Information-and-Vote-Buying.pdf*

Doig, Alan. 1999. "In the state we trust? Democratisation, corruption and development." *Journal of Commonwealth & Comparative Politics* 37 (3): 13–36.

Dunning et al. forthcoming. *Information, Accountability, and Cumulative Learning.* Cambridge, Cambridge University Press.

Erdmann, Gero, and Ulf Engel. 2007. "Neopatrimonialism reconsidered: Critical review and elaboration of an elusive concept." *Commonwealth & Comparative Politics* 45 (1): 95–119.

Fails, Matthew D. 2014. "Leader Turnover, Volatility, and Political Risk." *Politics and Policy* 42 (3): 369 – 399.

Ferejohn, John. 1986. "Incumbent Performance and Electoral Control." *Public Choice* 50: 5–25.

Ferraz, Claudio, and Frederico Finan. 2008. "Exposing Corrupt Politicians: The Effects of Brazil's Publicly Released Audits on Electoral Outcomes." *The Quarterly Journal of Economics* 123 (2): 703–745.

Ferraz, Claudio, and Frederico Finan. 2011. "Electoral accountability and corruption: Evidence from the audits of local governments." *The American Economic Review* 101 (4): 1274–1311.

Gamboa-Cavazos, Mario, Vidal Garza-Cantú, and Emiliano Salinas. 2007. "The Organization of Corruption: Political Horizons and Special Interests." *Working Paper Harvard University* .

Gorodnichenko, Yuriy, and Klara S. Peter. 2007. "Public sector pay and corruption: Measuring bribery from micro data." *Journal of Public Economics* 91 (5): 963–991.

Horn, Murray J. 1995. *The Political Economy of Public Administration: Institutional Choice in the Public Sector.* Cambridge, Cambridge University Press.

Iyer, Lakshmi, and Anandi Mani. 2012. "Traveling agents: political change and bureaucratic turnover in India." *Review of Economics and Statistics* 94 (3): 723–739.

Kendall-Taylor, Andrea. 2011. "Instability and Oil: How Political Time Horizons Affect Oil Revenue Management." *Studies in Comparative International Development* 46: 321 – 348.

King, Gary, James Honaker, Anne Joseph, and Kenneth Scheve. 2001. "Analyzing incomplete political science data: An alternative algorithm for multiple imputation." *American Political Science Review* 95 (1): 49–69.

Klitgaard, Robert. 1988. *Controlling corruption.* California: University of California Press.

Kopeckỳ, Petr. 2011. "Political competition and party patronage: public appointments in Ghana and South Africa." *Political Studies* 59 (3): 713–732.

Lentz, Carola. 2014. ""I take an oath to the state, not the government": career trajectories and professional ethics of Ghanaian public servants." In *States at Work: dynamics of African bureaucracies. Leiden: Brill*, ed. Thomas Bierschenk. Leiden, Brill.

Magaloni, Beatriz. 2008. "Credible power-sharing and the longevity of authoritarian rule." *Comparative Political Studies* 41: 715–741.

Marshall, Monty G., and Benjamin R. Cole. 2011. "Global Report 2011: Conflict, Governance and State Fragility." Report for the Center for Systemic Peace.
URL: *http://www.systemicpeace.org/vlibrary/GlobalReport2011.pdf*

McMann, Kelly M., Brigitte Seim, Jan Teorell, and Staffan I. Lindberg. 2017. "Democracy and Corruption: A Global Time-Series Analysis with V-Dem Data." Working Paper.
URL: *https://dx.doi.org/10.2139/ssrn.2941979*

Méndez, Fabio, and Facundo Sepulveda. 2006. "Corruption, growth and political regimes: Cross country evidence." *European Journal of Political Economy* 22 (1): 82 – 98.
URL: *http://www.sciencedirect.com/science/article/pii/S0176268005000443*

Moe, Terry. 1990. "The Politics of Structural Choice: Toward a Theory of Public Bureaucracy." In *Organization Theory: From Chester Barnard to the Present and Beyond*, ed. Oliver E. Williamson. Oxford University Press.

Mohtadi, Hamid, and Terry L. Roe. 2003. "Democracy, rent seeking, public spending and growth." *Journal of Public Economics* 87 (3): 445–466.

Montinola, G. R., and R. W. Jackman. 2002. "Sources of Corruption: A Cross-National Study." *British Journal of Political Science* 32: 147–170.

Moon, Chungshik. 2015. "Foreign Direct Investment, Commitment Institutions, and Time Horizon: How Some Autocrats Do Better than Others." *International Studies Quarterly* pp. 1 – 13.

Moritz, Steffen, and Thomas Bartz-Beielstein. 2017. "imputeTS: Time Series Missing Value Imputation in R." *The R Journal* 9 (1): 207–218.

Myerson, Roger B. 1993. "Effectiveness of electoral systems for reducing government corruption: a game-theoretic analysis." *Games and Economic Behavior* 5 (1): 118–132.

Olken, Benjamin. 2007. "Monitoring Corruption: Evidence from a Field Experiment in Indonesia." *Journal of Political Economy* 115 (2): 200–249.

Olken, Benjamin, and Rohini Pande. 2011. "Corruption in Developing Countries." *NBER Working Paper No. 17398* .

Olson, Mancur. 1993. "Dictatorship, Democracy, and Development." *The American Political Science Review* 87 (3): 567–576.

Persson, Torsten, Guido Tabellini, and Francesco Trebbi. 2003. "Electoral rules and corruption." *Journal of the European Economic Association* 1 (4): 958–989.

Przeworski, Adam. 1991. *Democracy and the market: Political and economic reforms in Eastern Europe and Latin America*. Cambridge, Cambridge University Press.

Przeworski, Adam, Michael Alvarez, Jose Cheibub, and Fernando Limongi. 2000. "Democracy and Development: Political Institutions and Material Well Being in the World.".

Rijckeghem, Caroline Van, and Beatrice Weder. 2001. "Bureaucratic corruption and the rate of temptation: do wages in the civil service affect corruption, and by how much?" *Journal of Development Economics* 65: 307 – 331.

Rock, Michael T. 2009. "Corruption and democracy." *The Journal of Development Studies* 45 (1): 55–75.

Rose-Ackerman, Susan. 1978. *Corruption: A study in political economy*. Academic Press New York.

Rose-Ackerman, Susan. 1999. *Corruption and government: Causes, consequences, and reform*. Cambridge, Cambridge University Press.

Saha, Shrabani, Rukmani Gounder, Neil Campbell, and J. J. Su. 2014. "Democracy and corruption: a complex relationship." *Crime, Law and Social Change* 61 (3): 287–308.

Shin, Youseop. 2017. *Time Series Analysis in the Social Sciences: The Fundamentals*. California: University of California Press.

Shleifer, Andrei, and Robert W. Vishny. 1993. "Corruption." *The Quarterly Journal of Economics* 108 (3): 599–617.

Sung, Hung-En. 2004. "Democracy and Political Corrupton: A cross-national comparison." *Crime, Law and Social Change* 41: 179–194.

Ting, Michael. 2012. "Legislatures, Bureaucracies, and Distributive Spending." *American Political Science Review* 106 (2): 367 – 385.

Ting, Michael M., James M. Snyder, Shigeo Hirano, and Olle Folke. 2013. "Elections and reform: The adoption of civil service systems in the US states." *Journal of Theoretical Politics* 25 (3): 363–387.

Treisman, Daniel. 2000. "The causes of corruption: a cross-national study." *Journal of Public Economics* 76 (3): 399–457.

Treisman, Daniel. 2007. "What Have We Learned About the Causes of Corruption from Ten Years of Cross-National Empirical Research?" *Annual Review of Political Science* 10: 211–244.

Weitz-Shapiro, Rebecca, and Matthew S. Winters. 2017. "Can citizens discern? Information credibility, political sophistication, and the punishment of corruption in Brazil." *The Journal of Politics* 79 (1): 60–74.

Wright, Joseph. 2008. "To Invest or Insure? How Authoritarian Time Horizons Impact Foreign Aid Effectiveness." *Comparative Political Studies* 41 (7): 971 – 1000.

How Political Competition Can Increase Corruption: Electoral cycles in police extortion in West Africa

Online Appendix Jasper Cooper

A Supplementary Information

- Figures 2 and 3 in subsection A.1 plot the bribe data aggregated to the week and month levels, respectively. They show the raw trends in average bribes paid in West African Francs (XOF) and where panel data was interpolated.
- Figures 4 to 8 in subsection A.2 illustrate the autocorrelation and partial autocorrelation in the weekly panel data of the raw bribes for each country. Most of the partial autocorrelations drop below the 95% confidence interval at the second or third lag, which is why the main regression specification is fit as an AR(1) process. The next section of the appendix shows that results are robust to modeling series as higher order autoregressive processes.

A.1 Time-Series Data

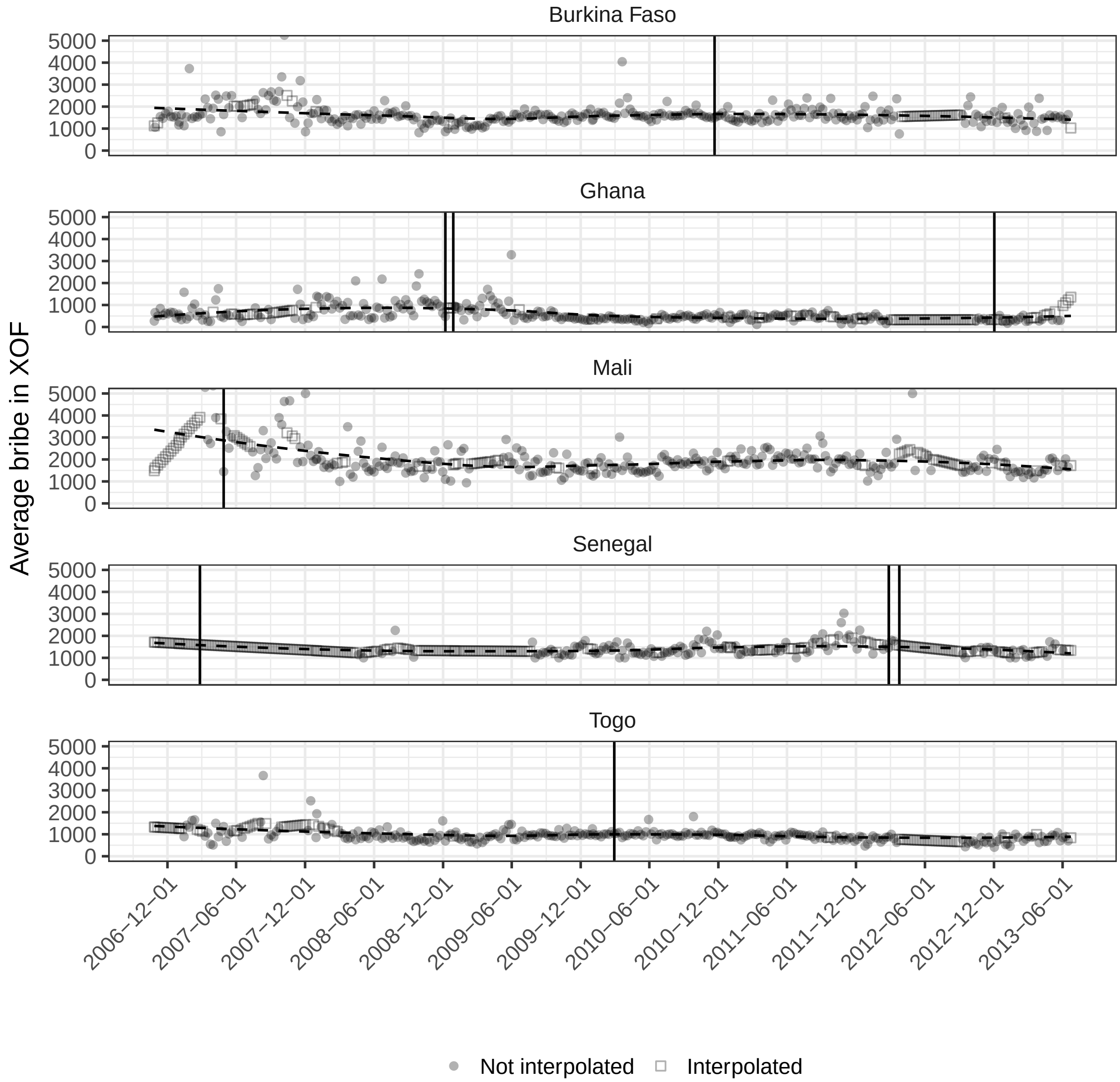

Figure 2: Time-series aggregated to week-level.
Each point represents an average of driver-level arithmetic averages of bribes paid in that week of the year in that country. Each square represents an imputed average from linear interpolation. Vertical bars represent elections. Dotted horizontal line represents LOESS-smoothed trend.

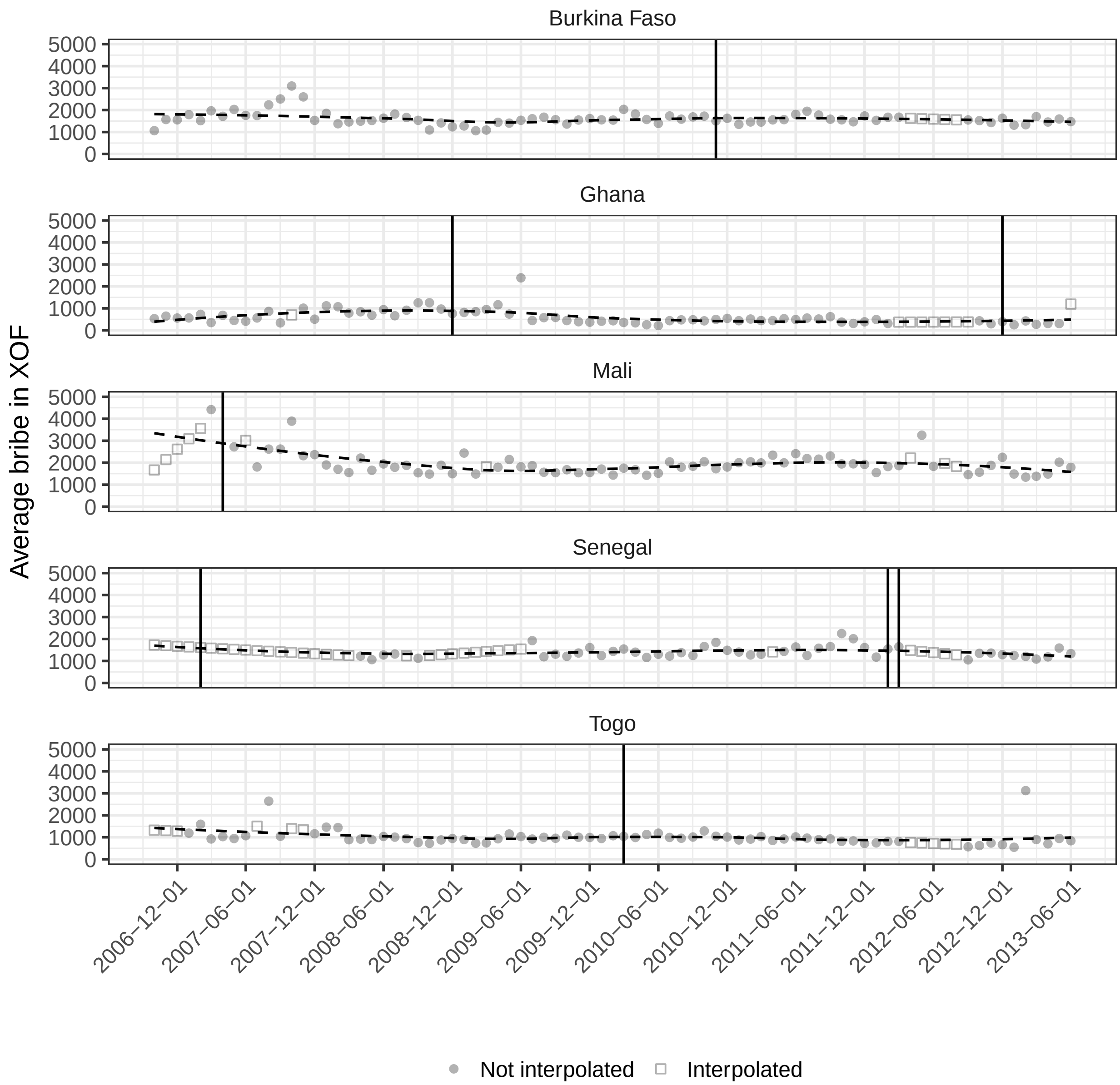

Figure 3: Time-series aggregated to month-level.
Each point represents an average of driver-level arithmetic averages of bribes paid in that month of the year in that country. Each square represents an imputed average from linear interpolation. Vertical bars represent elections. Dotted horizontal line represents LOESS-smoothed trend.

A.2 Autocorrelation and Partial Autocorrelation

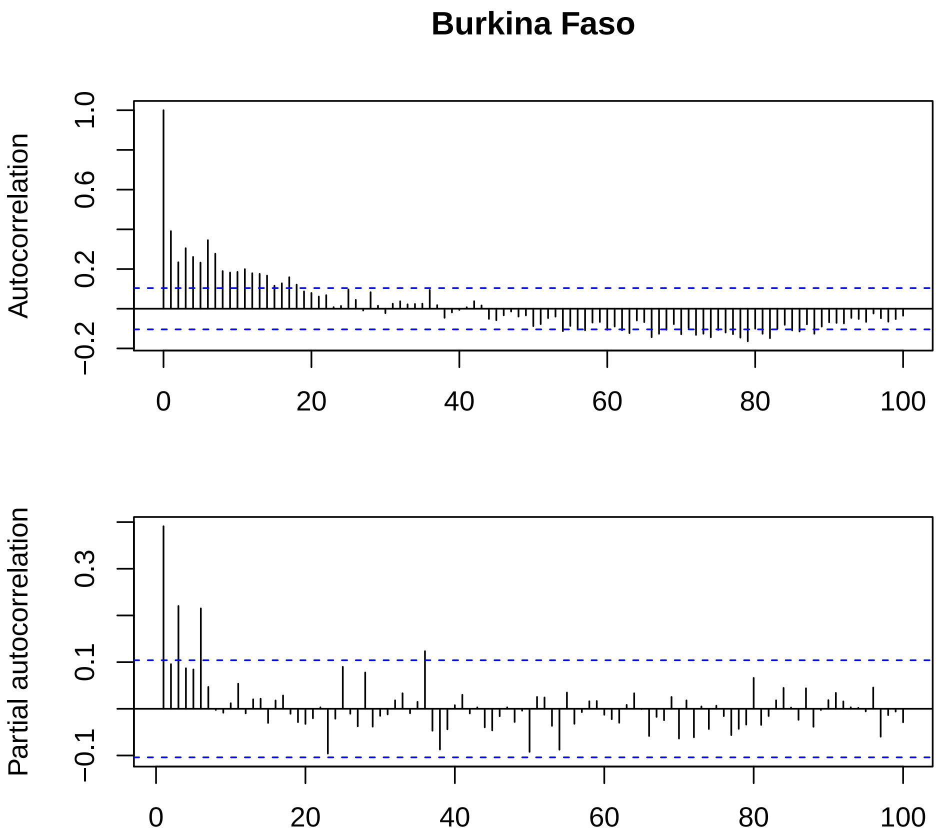

Figure 4: Autocorrelation and partial autocorrelation in Burkina Faso time-series of average bribes extorted by bureaucrats.
Dotted horizontal lines represent 95% confidence interval.

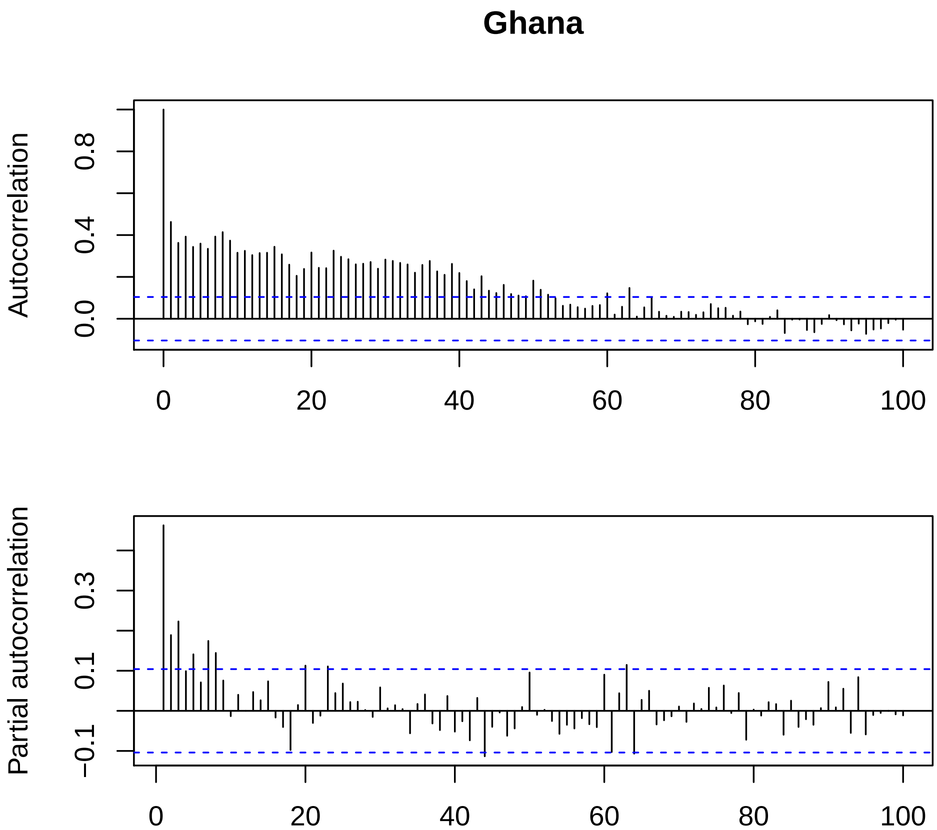

Figure 5: Autocorrelation and partial autocorrelation in Ghana time-series of average bribes extorted by bureaucrats. Dotted horizontal lines represent 95% confidence interval.

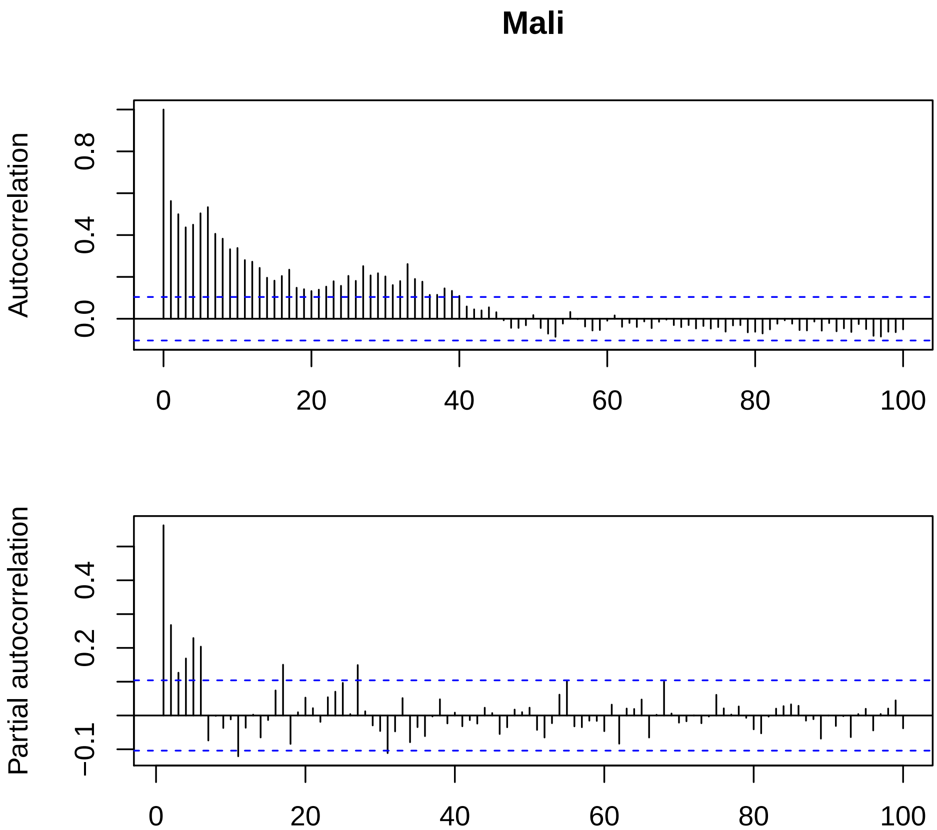

Figure 6: Autocorrelation and partial autocorrelation in Mali time-series of average bribes extorted by bureaucrats.

Dotted horizontal lines represent 95% confidence interval.

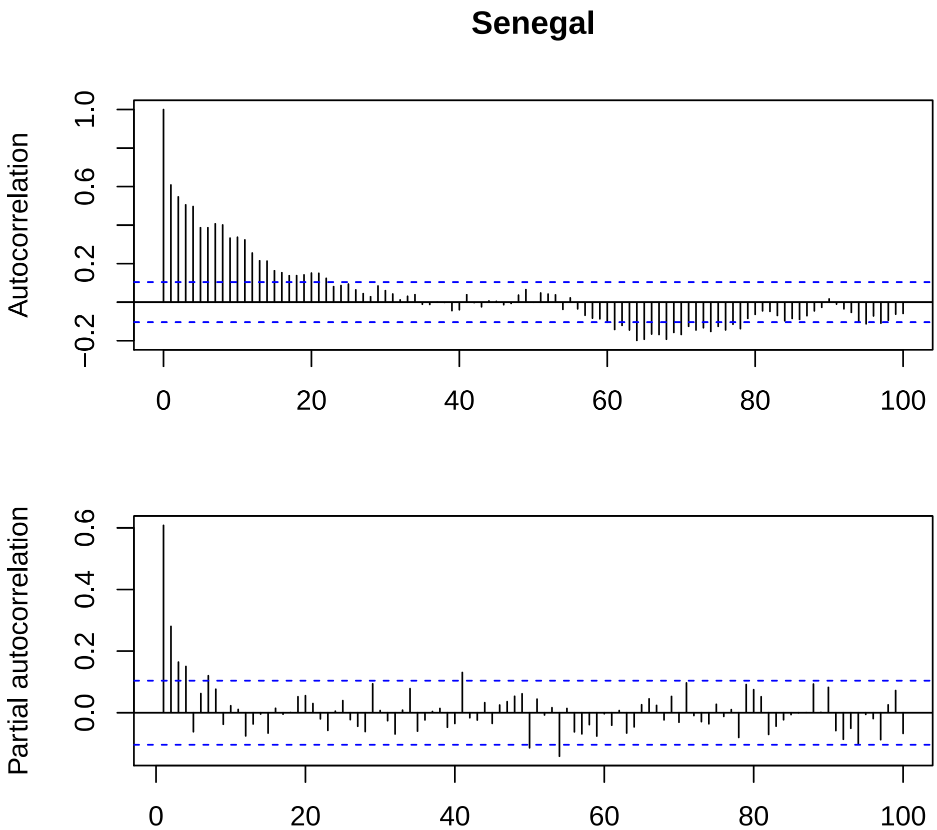

Figure 7: Autocorrelation and partial autocorrelation in Senegal time-series of average bribes extorted by bureaucrats.
Dotted horizontal lines represent 95% confidence interval.

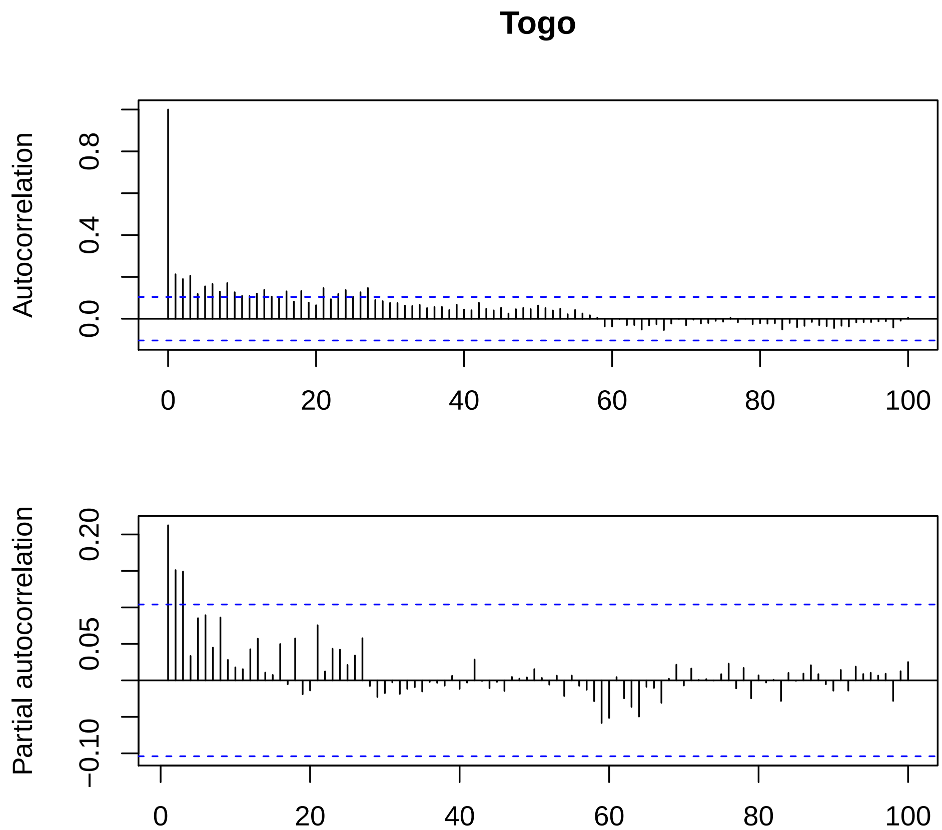

Figure 8: Autocorrelation and partial autocorrelation in Togo time-series of average bribes extorted by bureaucrats.

Dotted horizontal lines represent 95% confidence interval.

B Identification and Robustness

- Tables 7 and 8 in subsection B.1 illustrate that the simple version of the main specifications, without a lag, does not change substantive or statistical interpretation of the main results.

- To show robustness of the main results to the specification of higher-order autoregressive processes, Tables 9 and 10 in subsection B.2 report the main results with up to four lags of the dependent variable included. Estimates are attenuated somewhat but the substantive and statistical significance of the main results is unchanged.

- Less of the panel data is missing when aggregating to the country-month level, so Tables 11 and 12 in subsection B.3 report main results when aggregating data to the country-month level. Overall, this does not change the substantive interpretation of the results, and the statistical significance remains unchanged.

- As Figures 2 and 3 show, many observations are imputed for Senegal. Thus, to assess robustness to imputation Tables 13 and 14 in subsection B.4 report the main results when excluding Senegal from the analysis. The substantive and statistical significance of all main analyses remains unchanged.

- Main results residualize the dependent variable by differencing out effects for year, weekday, calendar month, country-specific linear trend, direction of travel, trade corridor, and whether driver is national of country in which bribe is paid. To assess robustness to the specific differencing strategy employed, Tables 15 and 16 in subsection B.5 report the main results under two alternative residualizing strategies. When country-specific trend is labeled ‘No’ the linear trend specific to countries is not included in residual estimation. When year, month and weekday FE is labeled ‘No’ those fixed effects are not included in residual estimation. Main results are robust to these two methods.

B.1 Simple Specification of Main Results

	Average Bribe Paid			
	(1)	(2)	(3)	(4)
Election Period	282.942***	301.554***		
	(30.285)	(28.827)		
Pre-Election Period			403.473***	390.671***
			(41.255)	(38.038)
Post-Election Period			74.238	120.956*
			(40.860)	(37.251)
Residuals	No	Yes	No	Yes
Period FE	Yes	No	Yes	No
Country FE	Yes	Yes	Yes	Yes
RI p-value: Elec. Per.	0.0185	0.0095		
RI p-value: Pre-Elec.			0.006	0.004
RI p-value: Post-Elec.			0.5425	0.2405
Observations	1,775	1,775	1,775	1,775
Adjusted R^2	0.592	0.059	0.596	0.064
Note:		*p<0.1; **p<0.05; ***p<0.01		

Table 7: Electoral Cycles in Extortion of Truck-Drivers by Police in West Africa, **using simplest specification.**
See caption of Table 2 for more details.

	Average Bribe Paid	
	(1)	(2)
Pre-Election (Non-Competitive)	83.595	47.283
	(82.679)	(71.486)
Post-Election (Non-Competitive)	38.670	−28.521
	(82.696)	(71.481)
Pre-Election (Competitive)	501.084***	497.815***
	(48.567)	(44.882)
Post-Election (Competitive - Challenger Won)	263.678***	291.486***
	(59.742)	(48.717)
Post-Election (Competitive - Incumbent Won)	−56.636	83.178
	(70.111)	(65.479)
Residuals	No	Yes
Period FE	Yes	No
Country FE	Yes	Yes
RI *p*-value: Pre-Elec. (NC)	0.589	0.664
RI *p*-value: Post-Elec. (NC)	0.804	0.8
RI *p*-value: Pre-Elec. (C)	0.006	0.006
RI *p*-value: Post-Elec. (C-CW)	0.231	0.112
RI *p*-value: Post-Elec. (C-IW)	0.79	0.673
Observations	1,775	1,775
Adjusted R^2	0.602	0.079
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 8: The Effect of Political Competition and Leader Turnover on Extortion of Truck-Drivers by Police in West Africa, **using simplest specification.**
See caption of Table 2 for more details.

B.2 Inclusion of More Lags

	Average Bribe Paid		
	(1)	(2)	(3)
Lagged (1) Avg. Bribe	0.322***	0.298***	0.285***
	(0.029)	(0.029)	(0.030)
Lagged (2) Avg. Bribe	0.176***	0.133***	0.120***
	(0.029)	(0.030)	(0.030)
Lagged (3) Avg. Bribe		0.136***	0.109***
		(0.029)	(0.030)
Lagged (4) Avg. Bribe			0.092***
			(0.030)
Election Period	145.129***	121.852***	107.726***
	(28.551)	(28.462)	(28.509)
Residuals	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Observations	1,765	1,760	1,755
Adjusted R^2	0.226	0.239	0.244
Note:	*p<0.1; **p<0.05; ***p<0.01		

Table 9: Electoral Cycles in Extortion of Truck-Drivers by Police in West Africa, **including up to four lags.**
See appendix page 9 and captions of Tables 2 - 3 in main text for explanatory notes.

	Average Bribe Paid		
	(1)	(2)	(3)
Lagged (1) Avg. Bribe	0.313***	0.289***	0.277***
	(0.029)	(0.029)	(0.029)
Lagged (2) Avg. Bribe	0.169***	0.127***	0.115***
	(0.029)	(0.030)	(0.030)
Lagged (3) Avg. Bribe		0.135***	0.109***
		(0.029)	(0.030)
Lagged (4) Avg. Bribe			0.092***
			(0.030)
Pre-Election (Non-Competitive)	26.661	22.840	18.778
	(71.652)	(71.125)	(71.411)
Post-Election (Non-Competitive)	−20.365	−19.031	−19.145
	(71.648)	(71.110)	(71.383)
Pre-Election (Competitive)	241.662***	213.182***	195.871***
	(43.983)	(43.836)	(43.701)
Post-Election (Competitive - Challenger Won)	161.917***	140.682***	127.169***
	(47.964)	(47.406)	(47.321)
Post-Election (Competitive - Incumbent Won)	−7.686	−37.947	−60.754
	(63.994)	(63.381)	(62.714)
Residuals	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Observations	1,765	1,760	1,755
Adjusted R^2	0.229	0.242	0.247
Note:	*p<0.1; **p<0.05; ***p<0.01		

Table 10: The Effect of Political Competition and Leader Turnover on Extortion of Truck-Drivers by Police in West Africa, **including up to four lags.**
See appendix page 9 and captions of Tables 2 - 3 in main text for explanatory notes.

B.3 Month-Level Analysis

	Average Bribe Paid			
	(1)	(2)	(3)	(4)
Lagged Avg. Bribe	0.440***	0.353***	0.429***	0.345***
	(0.062)	(0.061)	(0.061)	(0.060)
Election Period	154.099***	193.270***		
	(53.346)	(51.258)		
Pre-Election Period			256.184***	285.280***
			(72.406)	(66.181)
Post-Election Period			43.482	102.852
			(68.421)	(63.040)
Residuals	No	Yes	No	Yes
Country FE	Yes	No	Yes	No
Country FE	Yes	Yes	Yes	Yes
Observations	400	400	400	400
Adjusted R^2	0.723	0.192	0.728	0.209
Note:		*p<0.1; **p<0.05; ***p<0.01		

Table 11: Electoral Cycles in Extortion of Truck-Drivers by Police in West Africa, **with data aggregated to month-level.**
See appendix page 9 and caption of Table 2 in main text for explanatory notes.

	Average Bribe Paid	
	(1)	(2)
Lagged Avg. Bribe	0.417***	0.321***
	(0.061)	(0.059)
Pre-Election (Non-Competitive)	−12.629	11.437
	(122.651)	(109.360)
Post-Election (Non-Competitive)	32.207	−40.560
	(122.599)	(109.340)
Pre-Election (Competitive)	356.489***	390.506***
	(88.956)	(80.192)
Post-Election (Competitive - Challenger Won)	132.330	189.398**
	(106.759)	(90.663)
Post-Election (Competitive - Incumbent Won)	0.641	154.845
	(118.765)	(107.957)
Residuals	No	Yes
Country FE	Yes	No
Country FE	Yes	Yes
Observations	400	400
Adjusted R^2	0.730	0.223
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 12: The Effect of Political Competition and Leader Turnover on Extortion of Truck-Drivers by Police in West Africa, **with data aggregated to month-level.**
See appendix page 9 and caption of Table 3 in main text for explanatory notes.

B.4 Excluding Senegal

	Average Bribe Paid			
	(1)	(2)	(3)	(4)
Lagged Avg. Bribe	0.367***	0.381***	0.361***	0.376***
	(0.031)	(0.029)	(0.031)	(0.029)
Election Period	239.657***	211.339***		
	(41.451)	(38.219)		
Pre-Election Period			316.145***	282.110***
			(56.623)	(51.259)
Post-Election Period			93.082*	64.303
			(56.245)	(49.988)
Residuals	No	Yes	No	Yes
Period FE	Yes	No	Yes	No
Country FE	Yes	Yes	Yes	Yes
Observations	1,416	1,416	1,416	1,416
Adjusted R^2	0.669	0.193	0.669	0.194
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 13: Electoral Cycles in Extortion of Truck-Drivers by Police in West Africa, **with Senegal excluded**.
See appendix page 9 and caption of Table 2 in main text for explanatory notes.

	Average Bribe Paid	
	(1)	(2)
Lagged Avg. Bribe	0.343***	0.358***
	(0.031)	(0.029)
Pre-Election (Non-Competitive)	82.170	32.412
	(87.979)	(71.282)
Post-Election (Non-Competitive)	30.001	−24.908
	(87.953)	(71.277)
Pre-Election (Competitive)	450.950***	427.589***
	(74.709)	(69.683)
Post-Election (Competitive - Challenger Won)	343.972***	217.676***
	(103.912)	(83.917)
Post-Election (Competitive - Incumbent Won)	22.049	70.129
	(98.646)	(93.386)
Residuals	No	Yes
Period FE	Yes	No
Country FE	Yes	Yes
Observations	1,416	1,416
Adjusted R^2	0.673	0.202
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 14: The Effect of Political Competition and Leader Turnover on Extortion of Truck-Drivers by Police in West Africa, **with Senegal excluded**.
See appendix page 9 and caption of Table 3 in main text for explanatory notes.

B.5 Alternative Residualizing Approaches

	Average Bribe Paid			
	(1)	(2)	(3)	(4)
Lagged Avg. Bribe	0.374***	0.471***	0.371***	0.469***
	(0.028)	(0.028)	(0.028)	(0.028)
Election Period	191.854***	140.548***		
	(29.012)	(29.020)		
Pre-Election Period			241.045***	175.236***
			(38.175)	(38.789)
Post-Election Period			79.490**	51.307
			(37.292)	(37.773)
Country-specific trend	No	No	No	No
Year, month, weekday FE	Yes	No	Yes	No
Residuals	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Observations	1,770	1,770	1,770	1,770
Adjusted R^2	0.339	0.398	0.340	0.398
Note:			*p<0.1; **p<0.05; ***p<0.01	

Table 15: Electoral Cycles in Extortion of Truck-Drivers by Police in West Africa, **with alternative approaches to residualizing before collapsing to week-level**.
See appendix page 9 and caption of Table 2 in main text for explanatory notes.

	Average Bribe Paid	
	(1)	(2)
Lagged Avg. Bribe	0.361***	0.464***
	(0.028)	(0.028)
Pre-Election (Non-Competitive)	49.787	34.000
	(71.173)	(73.722)
Post-Election (Non-Competitive)	6.865	21.568
	(71.160)	(73.731)
Pre-Election (Competitive)	305.079***	221.827***
	(45.206)	(45.812)
Post-Election (Competitive - Challenger Won)	190.104***	115.102**
	(48.577)	(46.896)
Post-Election (Competitive - Incumbent Won)	42.032	19.845
	(65.884)	(67.567)
Country-specific trend	No	No
Year, month, weekday FE	Yes	No
Residuals	Yes	Yes
Country FE	Yes	Yes
Observations	1,770	1,770
Adjusted R^2	0.343	0.399
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 16: The Effect of Political Competition and Leader Turnover on Extortion of Truck-Drivers by Police in West Africa, **with alternative approaches to residualizing before collapsing to week-level**.

See appendix page 9 and caption of Table 3 in main text for explanatory notes.

C Supplementary Analyses

- Table 17 in subsection A.1 illustrates that electoral periods produce much smaller (even negative) effects in the two autocratic states of Burkina Faso and Togo, and much larger positive effects in the three democratic states.

C.1 Heterogeneous Effects by Country

The effect of the electoral period is statistically significant in the three democratic countries of Ghana, Mali and Senegal. In those countries the election period increases the average bribe paid by truck drivers to bureaucrats by about a quarter to one USD ($p < 0.10$).

While it is tempting to interpret the differences in country-level intercepts as differences in the degree of corruption in each country, this is a somewhat misleading inference to draw, as the average bribe says little about the overall amount of corruption in the country. For one thing, the length of road and the number of checkpoints vary from one country to another. Thus, drivers will mechanically pay more in bribes in large countries, simply because more road leads to more checkpoints. Suppose, for example, that there are two countries of equal size, A and B, but there are only two checkpoints in country A and ten in country B. Then even if the officials in country A extort twice as much on average as those in country B (producing a large difference in intercept), country A will still have less corruption in relative terms.

	Average Bribe Paid	
	(1)	(2)
Intercept (Ref = Burkina Faso)	952.213***	53.396***
	(49.497)	(19.322)
Lagged Avg. Bribe	0.414***	0.360***
	(0.027)	(0.028)
Ghana	−621.778***	−86.451***
	(41.456)	(27.592)
Mali	179.915***	−19.681
	(40.078)	(39.481)
Senegal	−146.127***	−83.089***
	(25.179)	(22.519)
Togo	−377.940***	−53.388*
	(35.873)	(29.286)
Election Period (Burkina Faso)	−26.124	−12.033
	(78.057)	(67.050)
Ghana x Election Period	93.824	237.886***
	(90.162)	(82.721)
Mali x Election Period	625.451***	555.427***
	(137.324)	(130.535)
Senegal x Election Period	147.996*	121.929*
	(82.633)	(72.618)
Togo x Election Period	56.617	33.780
	(113.354)	(105.139)
Residuals	No	Yes
Observations	1,770	1,770
Adjusted R^2	0.641	0.216
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 17: Country-Specific Electoral Cycles in Extortion of Truck-Drivers by Police in West Africa. All data aggregated from the checkpoint-driver-day level to the driver-level and then to the country-week level through arithmetic averaging. Missing country-weeks imputed through linear interpolation. All p-values and variance estimates from panel-corrected standard error estimator. In columns labeled 'Yes' for residuals dependent variable is the residuals from a regression of the bribe on year, weekday, calendar month, country-specific linear trend, direction of travel, trade corridor, and whether driver is national of country in which bribe is paid. 'Election Period' is 1 if bribe paid in the three months preceding or following election in that country, 0 otherwise.