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Elections and Corruption: Incentives to Steal or Incentives to Invest?

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Abstract

By now, most political systems around the world hold regular multiparty elections of different quality and type. However, we know relatively little about the effect of elections on corruption, especially in high-discretion, public procurement contracts implementing development aid. To address this gap in the literature, we employ unmatched comparisons and matching estimators to analyze a global government contracting dataset that provides an objective proxy for corruption: the incidence of single bidding in competitive markets. We find that, all things being equal, corruption risks increase in the immediate pre-election period: single bidding is higher by 1.3–6.1% points. We demonstrate that the corruption-enhancing effect of elections is stronger under conditions of (i) high electoral competitiveness, (ii) medium-level party institutionalization, and (iii) "localized collective goods" clientelism.

Keywords Elections · Corruption · Public procurement · World Bank

Introduction

The academic literature on corruption has, since its take-off in the mid-1990s, been strongly guided by analytical frameworks that emphasize the role of formal political institutions in explaining the prevalence of corrupt practices (see Golden and Mahdavi 2015; Kunicová 2006). Yet, despite this long-standing focus on institutional factors, research remains unclear on how corruption is affected by those institutions that, in many political systems, are key to regulating access to power: *elections*.

This is the more surprising since, in recent years, we have witnessed the global diffusion of elections. Not only did the "third wave" of democratization significantly increase the number of electoral democracies around the world (Markoff 2009) but, what is more, a growing number of autocratic regimes have also implemented

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regular multiparty elections. In fact, nowadays, the so-called "*electoral authoritarian* regimes probably (depending on the definition) comprise the modal type of political regime in the developing world" (Schedler 2009, p. 382; emphasis added). Taken together, it has been estimated that, at the beginning of the twenty-first century, around 75% of countries worldwide regularly hold multiparty elections (Magaloni and Kricheli 2010, p. 125)—albeit to varying degrees of competitiveness.

Despite these global trends, academic scholarship continues to be sharply divided over whether elections feed or constrain corruption. On the one side, scholars claim that electoral competition has a reducing effect on corrupt behavior. The main argument here is that elections provide citizens with a mechanism to remove corrupt politicians from office and thus function as a deterrent against misuse of public resources for particularistic gains (e.g., Ferraz and Finan 2008; Krause and Méndez 2009; Winters and Weitz-Shapiro 2013). Scholars also maintain that, in the immediate run-up to elections, scrutiny of government performance—for example, through audits and the media—is more intense and out in the open, which incentivizes politicians to "lay low" as polling day draws closer (e.g., Lehne et al. 2018; Olken 2007). On the other side, research has revealed a political business cycle whereby corruption increases significantly in the period preceding elections (e.g., Figueroa 2021; Mironov and Zhuravskaya 2016; Potrafke 2019). A commonly posited causal mechanism is that elections are costly affairs for aspiring and incumbent politicians, and thus generate significant pressure to engage in corrupt activities.

We believe that this profound disagreement among scholars stems from three shortcomings of current scholarship. First, many studies in the "elections as a deterrent" camp do not designate corruption as the dependent variable, but instead focus largely on the question of whether voters punish corrupt politicians at the polls. This is problematic because, in political systems where corruption has become informally institutionalized as "the way of doing things," penalizing individual politicians for their corrupt practices will do very little to lower the overall level of corruption (e.g., Hellmann 2017). Second, save for a small number of exceptions (e.g., Figueroa 2021; Lehne et al. 2018), existing studies on the election-corruption link are limited in that they typically rely on expert- and survey-based assessments of corruption at the country level as the dependent variable—for example, using Transparency International's Corruption Perceptions Index (e.g., Potrafke 2019). Such measures rely on subjective perceptions of corruption and focus heavily on the effect of corruption on business (Heywood and Rose 2014; Ko and Samajdar 2010; Razafindrakoto and Roubaud 2010; Lancaster and Montinola 2001). More specifically, these assessments are problematic, as they may spike before elections due to greater media attention on corruption scandals and mudslinging between rival politicians (cf. Figueroa 2021, p. 485). Third, scholarship has, so far, paid little attention to third factors that may affect the relationship between elections on corruption. While studies on retrospective corruption voting have considered a number of intervening variables, such as partisan loyalties (e.g., Anduiza et al. 2013) and economic performance (e.g., Zechmeister and Zizumbo-Colunga 2013), analyses that employ corruption as the dependent variable often investigate elections in isolation from contextual factors such as electoral system or the nature of party competition.

Our paper addresses these limitations in two ways. First, we tackle the measurement challenges in examining the elections-corruption link by analyzing a novel

government contracting dataset and developing an objective proxy for corruption: the extent of single bidding in competitive development aid-funded government tenders. We believe that corruption in aid spent by recipient countries is a powerful measure to examine the elections-corruption link. Decisions about how and where to invest developmental aid are generally made by national-level politicians and top bureaucrats appointed by them. Moreover, when it comes to the spending of aid, political elites are, in principle, not only accountable to citizens but also to donor organizations. Both the risks and costs of getting caught stealing are therefore—compared to corruption in the spending of national budget funds—considerably higher (even though taxpayers may consider aid as external money, less needed to be controlled). Based on this, it is reasonable to argue that our inquiry is based on the "Sinatra inference"—if our theoretical assumptions can make it here, they can make it anywhere (Levy 2008, p. 12). Or, put differently, any evidence we find of elections increasing the extent of corruption can probably also be extended to the spending of national budget funds.

Second, to evaluate the effects of elections on corruption in the spending of aid, we perform matched comparisons between the year immediately preceding the election on the one side and the election year and subsequent year on the other side. These statistical analyses reveal that, all things being equal, corruption risks increase in the immediate pre-election period. Moreover, we are able to demonstrate that the corruption risk-enhancing effect of elections in low- and middle-income countries is stronger under conditions of (i) high electoral competitiveness, (ii) mid-level party institutionalization, and (iii) party-voter linkages based on the clientelistic distribution of localized collective goods.

Before presenting our analysis, we do need to highlight two caveats. First, our sample is somewhat biased in that we analyze corruption and elections only in countries that receive developmental aid. Second, our study is limited to *one* specific form of corruption: manipulating public procurement so that the tendering process favors a single bidder. In the conclusion, we make suggestions for how future research can address these limitations.

Theoretical Background and Hypotheses

Notwithstanding the global proliferation of electoral institutions in recent years, academic research continues to be divided over whether elections have a positive or negative effect on corruption. While a number of scholars claim that elections help to control corrupt behavior by politicians (e.g., Kolstad and Wiig 2016; Lederman et al. 2005), other studies present evidence that elections are associated with in an increase in corruption (e.g., Figueroa 2021; Potrafke 2019). We anticipate that these contradicting findings are due the fact that there are a number of mechanisms at play simultaneously. Put differently, the average effect of elections on corruption can go in either direction, depending on the strength of the countervailing mechanisms.

Three main mechanisms stand out in the literature, which we will explore in the subsequent analysis. First, those scholars who demonstrate that democracy has a corruption-reducing effect generally highlight the punitive functions of

elections. Based on theories of public choice and retrospective voting, they argue that incumbent politicians anticipate being punished by voters in the next election and therefore refrain from corruption, including corrupt contracting. Second, scholars who are pessimistic about democracy's corruption-curbing effect stress that the pressure of electoral competition increases incentives to raise funds for political campaigns. Such pressures can also induce fundraising through corrupt deals, such as offering government contracts in return for donations (Fazekas et al. 2022a). Third, in a similar vein, elections also increase pressures for rewarding voters and those who can directly mobilize voters. This, for example, can involve directing government contracts to companies, which—in exchange—make their employees vote for the party. These three mechanisms play out differently, depending on a range of factors. In particular, our hypotheses investigate the following factors: strength of electoral competition (H2), political parties' capacity to orchestrate collective action (H3), and parties' use of local public goods to link themselves to the electorate (H4). While these three mediating factors do not map onto the mechanisms directly, they still allow us to investigate the workings of the three mechanisms.

However, before delving into the interactions between elections and mediating factors, we begin our analysis by probing the grand average effect of elections on corruption. While we acknowledge that elections—in principle—provide a powerful vertical accountability mechanism, we side with scholars such as Bauhr and Charron (2018), Solaz et al. (2019), and Figueroa (2021) who argue that voters' ability and willingness to punish politicians for corrupt behavior is often lacking (also see Incerti 2020). Hence, focusing on the question of how recipient governments spend developmental aid, we propose that the pressure to secure an unfair advantage through corrupt means will increase in the immediate run-up to the election and outweigh politicians' concerns about being voted out of office by angry citizens. These arguments apply well to the specific context of development aid (World Bank funding) and national legislative or presidential elections. National elections represent the main contestation for controlling national governments which, in turn, control development projects and the ensuing procurement contracts funded by the World Bank. We therefore hypothesize the following:

H1: Corruption risks increase in the immediate period leading up to elections.

We anticipate that the pressure to secure an unfair advantage vis-à-vis challengers is greater when elections are more competitive, thereby amplifying the effects of the "corrupt party finances" and "rewarding supporters" mechanisms. As already hinted at in the "Introduction" section, elections vary considerably in their degree of competitiveness. Scholars who classify different types of regimes along a competitiveness continuum usually situate "politically closed" regimes, which "do not have any of the architecture of political competition and pluralism" (Diamond 2002, p. 26) at one end of the spectrum and electoral democracies at the other end. In between these two extremes, we find "electoral authoritarian" regimes, in which (i) "a ruling party allows (generally via the constitution) opposition groups to form parties and participate in elections and the legislature," (ii) "[p]olitics are highly biased in favor

of the ruling party, but competition is real," and (iii) "parties other than the ruling one have representation in the parliament" (Magaloni et al. 2013, p. 8).

In regimes that approximate the "politically closed" type, electoral competitiveness is—by definition—very low. Consequently, incentives to misdirect public money in the immediate run-up to elections should also be very low. Electoral competition is stronger in electoral authoritarian regimes, not least because leaders in this particular regime type typically seek to maximize their winning margins to project strength and deter elite defections (Simpser 2013, p. 86; Magaloni 2006, p. 46). However, to boost their winning margins, regime leaders can choose from a whole "menu of manipulation" (Schedler 2002), which includes unfair tactics such as hindering opposition parties in contesting effectively, restricting media freedom, and drawing electoral boundaries so that regime supporters are given greater weight. That is to say, for ruling elites in electoral authoritarian regimes, corruptly misusing funds earmarked for public development projects, especially development aid projects, is only *one* strategy among many to influence the outcome of elections, thus making it a much less widely used strategy. Politicians in genuinely democratic systems, on the other hand, face a much higher probability of losing office. At the same time, they have—by definition—a considerably more restricted "menu of manipulative tactics." Hence, incumbent politicians in highly competitive settings may find that corrupting public investment projects in return for campaign funds or voter mobilization provides an effective means of getting ahead of other parties (Klasnja 2016). Stronger electoral competition should therefore increase corrupt contracting either because winning tight elections requires comparatively more corrupt money or mobilizing voters through corrupt means is more important for winning—or both. We thus put forward a second hypothesis:

> H2: The increase in corruption risks in the immediate period leading up to elections is larger when the electoral process is *highly competitive*.

Yet, while electoral competition creates incentives for corrupt behavior, politicians also need the *capacity* to divert public funds, such as public contracts, and convert these funds into electoral assets. Regarding the capacity to misappropriate funds for particularistic purposes, we anticipate that the degree of party system *institutionalization*—defined as the degree of "stability in who the main parties are and in how they behave" (Mainwaring 1998)—has a central part to play. Strongly institutionalized political parties provide formidable organizations to coordinate the large-scale theft of public resources, such as funds designated for public works projects. As Gingerich explains, parties characterized by a high degree of institutionalization typically exert a lot of control over politicians' and bureaucrats' career paths. Simultaneous control over political and bureaucratic actors is crucial for steering development aid-financed procurement contracts towards corrupt purposes, the reason being that bureaucrats designing and administering procurement tenders are indispensable for favoritism and corruption in public procurement (Dahlström et al. 2021). "Such influence easily translates into party-directed corruption: because politically ambitious bureaucrats know that party leaders have the institutional wherewithal to reward them for risky and illegal actions undertaken at the behest and for the benefit

of their party, those risks often will be judged as well worth running. Stealing for the team, as it were, flourishes" (Gingerich 2013, p. 242; also see Yadav 2012). Party organization is an especially critical asset when elites have to monitor and enforce complex, large-value corrupt deals, such as illicit agreements, whereby private companies are awarded aid-funded contracts in exchange for mobilizing their employees as voters on election day. In such a case, the party has to be able to simultaneously coordinate highly regulated bureaucratic action—that is, the contract award—*and* monitor the delivery of votes by the company at the local level.

However, there are reasons to believe that high levels of party system institutionalization have a corruption-reducing effect—for a number of reasons. To begin with, strongly institutionalized party systems elongate political elites' time horizons—both in dictatorships and democracies. Regarding autocratic regimes, it has been found that leaders who can rely on a highly institutionalized party generally succeed in sustaining themselves in power for longer than dictators who only have a weakly institutionalized party at their disposal or lean on other organizations to secure their power, such as the military or a close circle of cronies (e.g., Geddes 1999; Smith 2005). In turn, longer time horizons incentivize dictators to restrain corrupt activities and invest public resources towards economic growth, knowing that this will allow them to loot more in the long run (e.g., Kelsall 2013; Wright 2008). Similarly, in democratic regimes, strongly institutionalized political parties increase elites' certainty over future political interactions—in particular, electoral contestation. At the other end of the spectrum, under conditions of low party system institutionalization, politicians find it difficult—if not impossible—to estimate whether they will gain (or retain) access to office in future rounds of voting. As a result, low party institutionalization may lead elites to emphasize short-term extractive gains from holding office rather than long-term reputations as capable managers of the state (e.g., Kitschelt et al. 2010, p. 26; Keefer 2007). Moreover, it has been argued that strongly institutionalized party systems reduce the level of corruption, as they make it easier for citizens to pin responsibility for government mismanagement and corruption on parties and politicians. In contrast, inchoate party systems—where parties tend to be short-lived and politicians regularly switch between parties—undermine citizens' ability to establish responsibility, thereby lowering the risk that comes with engaging in political corruption (Schleiter and Voznaya 2018; Tavits 2007).

While there is considerable disagreement in the literature, we can clearly see 2 countervailing factors at play regarding the effect of party system institutionalization on corruption risks prior to elections. At high levels of party system institutionalization, organizational capacity to divert public funds for political gain is substantial, but—because of long time horizons and clear accountability relationships—incentives to engage in corruption are weak. Meanwhile, at low levels of party system institutionalization, politicians are—due to short time horizons and murky accountability—motivated to indulge in corrupt practices, but political parties lack the infrastructure to misdirect public resources from their intended purpose towards electoral objectives. That is to say, as party system institutionalization increases, incentives to steal weaken and capabilities to steal strengthen; as party system institutionalization decreases, incentives to steal strengthen and capabilities to steal weaken. At which exact value, party system institutionalization leads to most corruption is

theoretically unclear, nevertheless put forward an empirically testable claim that countervailing forces balance out around the middle of the scale. We therefore expect to see the strongest effect of elections on corruption when incentives to steal and organizational capabilities to steal are balanced at medium levels of party system institutionalization.

H3: The increase in corruption risks in the immediate period leading up to elections is larger when the party system is characterized by *medium* levels of institutionalization.

So far, we have not been able to differentiate our two main mechanisms linking elections to corruption: campaign finance and targeted spending on supporters. While a full delineation is beyond the scope of our empirical material, we are able to offer some insights into the strength of the "rewarding supporters" mechanism by measuring different types of party-voter linkages. Specifically, we argue that when party-voter linkages rely on localized spending rewarding supporters, it is more likely that procurement corruption involves targeted spending on companies that have promised to deliver local support in elections.

At the most fundamental level, party systems can be structured around either *distributive* or a *non-distributive* linkage (Stokes et al. 2013, p. 7; Kitschelt et al. 2009, p. 745). While non-distributive party-voter linkages typically take the form of *affective* ties, including ethnic and religious identities, distributive linkages are commonly subdivided into *programmatic* and *clientelistic* strategies. In the case of a *programmatic* strategy, politicians develop "packages of policies that they commit to enact if elected to political office with sufficient support," and these policy packages "award benefits to citizens regardless of whom they voted for in the election" (Kitschelt et al. 2010, p. 16). *Clientelistic* strategies, in contrast, are not guided by transparent principles of distribution. Instead, the delivery of material benefits comes with "electoral strings" attached—that is, benefits are only distributed to individuals or small groups who have already delivered or who promise to deliver their votes (Hicken 2011). Furthermore, it is possible to distinguish two sub-types of clientelism, depending on whether politicians exchange votes for either *private* goods (e.g., money, food, clothing, building materials) or *localized collective* goods (e.g., roads, public utilities, sporting facilities).

At a general level, clientelism undermines the accountability mechanism that is built into elections: when parties and voters are connected through patron-client linkages, accountability becomes perverted. Instead of voters holding politicians accountable, it is politicians who—by rewarding electoral support and punishing defection through preferential access to material benefits—hold voters to account (Stokes 2005). Conversely, politicians campaigning on programmatic policies face positive incentives to curb corruption because they need to ensure that their promised policy packages are effectively and efficiently implemented. For example, they need to protect public funds earmarked for programmatic policies from theft and make certain that public organizations (such as the civil service and judicial authorities) implement policies in accordance with what is stated in the law, rather than being guided by particularistic interests (cf. Fukuyama 2013; Holmberg et al. 2009).

Specifically, we argue that clientelism will have a particularly strong effect on corruption in the run-up to elections when politicians distribute localized collective goods, as opposed to private goods. This expectation is based on two considerations. First, while private goods clientelism is often fueled by corrupt practices in public procurement, these corrupt exchanges can happen at any time between elections. Notwithstanding the fact that private goods clientelism usually ramps up in the immediate period before elections (e.g., Aspinall et al. 2022, ch. 4), the "war chest" to finance the clientelistic distribution of private goods—such as money, food, construction materials, and household goods—can be built up throughout the whole inter-election period. In other words, when politician-voter linkages are mainly maintained through private goods clientelism, we should not expect public procurement corruption to spike before elections. Second, when incumbent politicians seek to mobilize voters through the clientelistic delivery of localized collective goods, they have a greater chance of achieving their objectives when public procurement favors companies which are part of the clientelistic network and procurement manipulations are implemented close to the election. To name two well-documented examples, politicians—in collusion with private companies—may start public construction projects in targeted constituencies shortly before an election but only finish these projects *after* the election if, and only if, the constituency in question provided sufficient electoral support (e.g., Duncan and Hassall 2011, p. 268); parties and politicians may enter into an agreement with local companies whereby, in exchange for public contracts, the latter deliver their employees as block of votes or at least encourage voting for the governing party—a phenomenon that has even been observed in industrialized economies such as Japan (Scheiner 2006, p. 72). Hence, when party systems are structured around localized collective goods clientelism, it is reasonable to expect an increase in public procurement corruption as the elections draw closer.

H4: The increase in corruption risks in the immediate period leading up to elections is larger when party-voter linkages are founded on a clientelistic distribution of localized collective goods.

Research Design

To test our hypotheses and investigate causal mechanisms, we analyze a novel contract-level dataset, which provides objective indicators of corruption in the spending of developmental aid, using unmatched and matched comparisons.

Data

We combine two major global datasets for our analysis: (i) a large-scale contracts dataset scraped from the World Bank's official website and (ii) Varieties of Democracy (V-Dem) data on key political variables based on expert assessments.

The contracts database contains all major contract awards of World Bank-financed projects for the fiscal years 1997–2007, a period during which the dataset has remained largely comparable. It contains over 110,000 contracts with a value above US$25,000 (Fazekas et al. 2022b).[1] Major contract awards refer to all "prior-reviewed" contracts. These are contracts awarded in tenders run by recipient government agencies, while also subject to review by the World Bank at key stages throughout the procurement cycle, such as the call for tenders or award decision. These contracts have to follow the World Bank's procurement rules[2] and be published on the central advertisement portal.[3] Each contract is part of a project approved by the World Bank as well as the recipient government with dedicated staff on both sides for oversight and project management. Only contracts with an estimated value above a pre-set threshold undergo the prior-review process (thresholds are defined according to a range of contractual and organizational criteria such as product type or risk level). Other tenders—the so-called post-reviewed tenders—are managed by the recipient countries themselves, with World Bank staff reviewing and auditing only *after* the end of the project.[4] Since our dataset only contains high-risk tenders with greater World Bank control, our findings are not representative of all aid spending financed by the World Bank. The dataset only captures contracts where risks are higher and where a greater degree of control is deemed necessary (David-Barrett et al. 2020).

We compiled the contracts dataset from data scraped or downloaded directly from the World Bank's public website (a full description of data sources is provided in Appendix A). After combining data coming from different sources, we applied a common set of cleaning procedures: (i) we harmonized nominal dollar prices by applying purchasing power parity and inflation adjustment to make contract values comparable across countries and years; (ii) we standardized sector categories (e.g., the health sector was marked as "Health" for some years in the data while simply "H" in other years); (iii) we also corrected for any variations and abbreviations in country names (e.g., from East Timor to Timor-Leste); and (iv) we assigned calendar years to each contract based on contract award date. .

Second, we derive key political variables from the V-Dem project. This dataset contains annual data on 201 countries for the period 1789–2017 and thus overlaps with our public procurement sample. V-Dem data is a rigorously executed expert

[1] A World Bank fiscal year begins in July and ends in June the next year, meaning that we observe major contract awards between July 1997 and June 2008.

[2] For the most recent and historic rules, see https://projects.worldbank.org/en/projects-operations/products-and-services/brief/procurement-policies-and-guidance#Guidelines (accessed on the 1st of June, 2023).

[3] https://projects.worldbank.org/en/projects-operations/procurement?srce=both

[4] Thresholds for "prior review" are set in a complex process and are reviewed regularly (details available here: http://bit.ly/2wa6Qc1). The World Bank first decides to what degree a recipient country can be trusted to manage aid-funded procurement on its own through the Country Procurement Assessment Review (CPAR). Based on this assessment, a project risk level—or review threshold—is established. The World Bank provides an indicative list of thresholds for each country. Exact thresholds are determined in individual procurement plans, which are subject to the World Bank's "no objection" scrutiny at key project stages.

survey that contains a host of precise questions about theoretical concepts that underpin our hypotheses, including the quality of elections, political party institutionalization, and party-voter linkages (Coppedge et al. 2019).

Indicators

To operationalize our dependent variable, we build on a growing literature that uses proxy indicators of corruption in administrative datasets, such as infrastructure spending (Golden and Picci 2005; Lewis-Faupel et al. 2016) and public procurement data (Bosio et al. 2022). Our approach is based on a methodology widely applied to national public procurement datasets (2017; Klasnja 2016) as well as to aid-financed contracts (David-Barrett et al. 2020). Such work addresses the widely accepted shortcomings of country-level perception-based corruption indices, while at the same time offering far greater granularity (Foster et al. 2012).

Following recent research (e.g., David-Barrett et al. 2020), *single bidding* in competitive tenders serves as our dependent variable and corruption proxy indicator. Public procurement is assumed to be the least prone to corruption when the process is open and competitive, and procurement regulations set a number of requirements intended to ensure openness. Where the process deviates from these requirements, this may indicate deliberate manipulation by a corrupt public official (or network of public and private actors) to favor a particular company and gain a private advantage. Public procurement outcomes thus serve as the best indicators of corruption risk (Kenny and Musatova 2010). Especially, where only *one* company submitted a bid—even though the process should have been open to competition, international or domestic—the risk of corruption is particularly high.

Single bidding does not prove that corruption occurred, but it is an indicator of corruption risk, which—when analyzed across large datasets—can point to overall patterns that warrant investigation or a policy response (David-Barrett et al. 2020). As long as market conditions predict healthy competition and World Bank public procurement regulations assume that development aid-funded tenders are competitive in principle, single bidding can be regarded as indicative of corruption (rather than immature markets or low administrative capacity). Statistical evidence of the validity of single bidding as a corruption proxy, both on the country and contract levels, can be found in Appendix B.[5] Among other things, the single-bidding rate on the country level in the World Bank dataset correlates with widely used country-level corruption perception indicators such as Transparency International's Corruption Perceptions Index (linear correlation coefficient = −0.2 for the period 1998–2009) (Table B1 in Appendix B). In a similar vein, single bidding using a more homogeneous and broader sample of European public procurement datasets

[5] We have to stress, again, that single bidding in competitive tenders only captures *one* particular form of high-level corruption. What distinguishes single bidding is that it is typically organized through institutionalized and lasting corrupt relationships between public and private elites. Other types of corruption are more competitive—for example, when multiple firms seek to outbid each other with bribes to procurement officials.

has an even higher correlation with TI's CPI (linear correlation coefficient = −0.7 for the period 2009–2013) (Fazekas and Kocsis 2020). Furthermore, single bidding on the contract level correlates with procedural risk factors in the World Bank dataset, such as non-open procedure types (Table B2 in Appendix B), which lends further support to the claim that corruption risks arise from single bid contracts (for more on the indicator validity testing approach, see Fazekas and Kocsis (2020)).

To operationalize our main independent variable—national *elections*—we employ two variables from the Varieties of Democracy (V-Dem) dataset: legislative or constituent assembly election (v2xel_elecparl) and presidential election (v2xel_elecpres). When either of these elections took place, we identify the country-year as an election. This approach, which ignores local elections, aligns with our focus on World Bank-financed contracts, which are typically under the control of national governments.[6]

The timescale of election effects is theoretically ambiguous and probably differs somewhat across countries. In order to keep the analysis tractable, we imposed a common timeline on all countries, with the year before the national election serving as the treatment group—which is when we expect electoral considerations to have the strongest influence on government contracting—and the election year and the year after the election serving as the control group. Such a contrasting treatment-control group split allows us to compare adjacent years—thus minimizing bias from temporal effects—and different time periods in relation to the timing of elections (for a simple visual representation of these periods in terms of single bidding, see Appendix C, Figure C1). We opt for using full financial years to define control and treatment groups, as public procurement spending funded by the World Bank follows annual plans with strong seasonality. Hence, comparing full financial years affords us the most directly comparable sets of contracts.

To operationalize the independent variables interacting with the election treatment in hypotheses 2–4, we make use of variables in the V-Dem dataset. First, as a measure of electoral competitiveness, we use the *clean elections* index (v2xel_frefair). This index captures the degree to which elections are free and fair—that is, the extent to which they are free of registration fraud, systematic irregularities, government intimidation of the opposition, vote buying, and election violence. A higher score means cleaner elections.

Second, we employ the *party system institutionalization* index (v2xps_party), which expresses the degree to which political parties are institutionalized in a country. The index aggregates a number of party attributes, including the level and depth of organization, links to civil society, cadres of party activists, party supporters within the electorate, coherence of party platforms and ideologies, and party-line voting among representatives within the legislature. A high score on these attributes generally indicates a more institutionalized party system.

[6] Over 4/5th of World Bank loans have been received by the national government directly with the rest typically going to large state-owned enterprises (e.g., State Railway of Thailand). For a full dataset of borrowers, see https://www.govtransparency.eu/data-update-of-world-bank-iadb-and-europeaid-datasets-on-development-aid-funded-contracts-and-projects-4/.

Third, we rely on the *party linkages* index (v2psprlnks), which captures major parties' preferred strategy of voter mobilization. The index is based on expert assessments, scoring countries on an ordinal scale that distinguishes private clientelistic rewards (e.g., money, jobs) from local collective rewards (e.g., wells, roads) and programmatic policies. The ordinal scale is transformed into an interval scale, with lower values indicating clientelistic linkages and higher values indicating programmatic linkages.

In addition, our quantitative analysis also includes a battery of control variables. These variables derive from the micro-level public procurement dataset, capturing the following properties of each World Bank-funded contract: *year* (World Bank financial year running from July to June), *sector* (10 main sectors such as energy or health), *monetary value* (natural log of inflation adjusted US$), *recipient country* (either as fixed effects or as average single-bidding rate throughout the whole period), and the public organization's *average corruption risk* (average single-bidding rate for the whole period).

All variables in our dataset are summarized in Table 1. For descriptive statistics of all these variables, see Appendix C.

Methods for Causal Inference

The analysis of causal effects benefits from the temporal and spatial range of our contract-level dataset as well as the quasi-independence of World Bank project design from electoral cycles in recipient countries.

First, our dataset—which provides granular detail of the World Bank-funded contracting processes, such as contract value or procedure type (e.g., international competitive tendering)—covers over a decade of contracting across an exceptionally large number of countries. This allows us to observe (i) a large number of elections on a wide spectrum of political contexts, (ii) multiple elections within individual countries, and (iii) tens of thousands of potentially corrupted contracts. Hence, the powerful combination of macro- and micro-level datasets circumvents the small-N problem typically associated with cross-country research on drivers of corruption. By comparing potentially corrupted contracts within the same countries over elections, a range of unobserved contextual factors are controlled for, such as media freedoms and pluralism—that is, factors that (at least in theory) help control corruption. Moreover, using data from the World Bank implies that all our tenders and contracts were administered following the same procedural rules and transparency requirements set by the World Bank.

Second, we argue that election timings in recipient countries—that is, the decision of when to table an election—are quasi-independent of World Bank project design and procurement planning. First, election years are—save for a few exceptions (e.g., a vote of no confidence triggering early elections)—typically set by national laws based on strict numerical rules (e.g., every four years). Second, World Bank-financed procurement tenders follow procurement plans—that is, detailed plans of the timing, value, and object of each tender—that are written into loan agreements long before contracts are awarded. Importantly, these plans are very

Table 1 Summary of variables used in the analysis

Role	Name	Definition	Source
DV	Single bidding	• 1 if only one bidders submitted a bid for a tender • 0 if more than one bids were submitted	WB
IV	Treatment	• 1 if one year before election year • 0 if election year or one year after election year	V-DEM
IV	Clean elections index	Degree to which elections are free and fair	V-DEM
IV	Party system institutionalization index	Degree to which political parties are institutionalized (e.g., party platform coherence)	V-DEM
IV	Party linkages index	Major parties' most common form of linkage to their constituents (e.g., clientelistic)	V-DEM
Control	Year	Calendar year running from January to December	WB
Control	Sector	10 main sectors such as energy or health	WB
Control	Contract value	Natural log of contract award value (inflation adjusted US$)	WB
Control	Country FE	Dummy variable for each country	WB
Control	Country avg. single-bidding rate	Average single-bidding rate throughout 1997–2007	
Control	Buyer avg. single-bidding rate	Average single-bidding rate throughout 1997–2007	WB

DV dependent variable, *IV* independent variable, *WB* World Bank, *V-DEM* Varieties of Democracy

difficult to modify once a project has been initiated. Third, in our sample of prior-reviewed contracts, tender timings and specifications have to be signed off by World Bank staff; it can generally be taken for granted that these individuals are shielded from particularistic practices in recipient countries.

To counter any remaining bias in the comparison of control and treatment contracts, we used propensity score matching to balance the covariates listed Table 1 (in Appendix G, we employed coarsened exact matching).[7] As a simple benchmark, we also show the unmatched comparison of group-average single-bidding rates, which may well be biased as we cannot be absolutely certain about the validity of our assumption regarding the quasi-independence of election timings and contract awards. As a more advanced benchmark, we show binary logit regressions controlling for the variables we match on in our main specification, with essentially the same conclusions as the main estimation (Appendix J). For our main estimates, we conducted a treatment vs. control group comparison—using propensity score matching—that balances covariates influencing our outcome variable (single bidding), including year, economic sector, contract value, buyer average single-bidding score, and country.[8] As shown in Appendix E, no significant imbalance remains after matching. Furthermore, for the tests of H1, we run robustness tests running out that our results are driven by electoral system (proportionate versus majoritarian) (Appendix I). Moreover, when testing H2–H4, we also added country- and year-specific institutional characteristics to the covariates. Again, matching leaves practically no discernible difference on observables across treatment and control groups (Appendix E). Finally, we also tested whether there is a political business cycle influencing World Bank-financed contract awards, as an uptick in the volume of procurement spending prior to elections could cause an increase in single bidding even in the absence of corruption.

Throughout the whole analysis, we restrict our sample to maximize the fit between our theoretical predictions and our data:

- Contracts above US$25,000. Small contracts tend to be less competitive, especially in less developed economies with weak supplier markets.
- Only non-consultancy contracts. Consultancy contracts tend to be less standardized and there is a host of non-corrupt reasons for single bidding.
- Only regimes that hold regular, multiparty elections (value on the V-DEM v2xel_frefair variable larger than 0). We exclude "politically closed" regimes from our analysis, such as China or North Korea, as our theoretical expectations regarding the elections-corruption relationship do not apply to these types of political systems.

[7] We use Stata 14.2, psmatch2 command enforcing common support, logit regression fit, and no replacement (i.e., equally sized control and treatment groups).

[8] We use propensity score matching rather than coarsened exact matching, because the weights produced by the former are more balanced. Coarsened exact matching produces some very high weights, potentially exacerbating measurement error or random features of some tenders.

- Countries in which the number of contracts in both the treatment and control groups is larger than 25. In order to facilitate within-country comparisons (before election vs. after election), we exclude countries with too few contracts in either the control or treatment groups.
- Treatment-control period of three years. One year prior to the election vs. election year plus one year after the election; all other years are excluded from the analysis to minimize potential bias from unobserved temporal shocks. As a robustness test, we also rerun the analysis comparing one year before election year with one year after the election, hence leaving out the election year form the comparison, as this can be considered a transitory year (see Appendix H).

As a result of these restrictions, our initial sample of about 110,000 contracts decreases to about 52,000 contracts for the 1997–2007 period.

Given that our measurement of corruption risk using single bidding rests heavily on the assumption that, in the absence of corrupt intent, there would be more than one company bidding, we also conduct robustness tests on a sample restricted only to international and national competitive tenders—that is, processes where the expectations of vigorous competition are the strongest (Appendix F). These alternative specifications lead to essentially the same conclusions, with even larger effect sizes.

As we assume that election timings are quasi-independent from World Bank-financed project design, it is important to spell out the strategies that political elites in recipient countries have available to exploit World Bank-funded contracts for their own gain. What is important to highlight in this regard is that procurement plans set out in World Bank project descriptions only determine the high-level, key characteristics of contracts for the lifetime of a project, such as timing, value, and object of contracts. As projects move forward and tenders are released, more detailed contract specifications are determined by national governments. In particular, corrupt governments seeking to extract electoral gains from World Bank projects—which are often of high value and thus highly visible to the public—can manipulate the procurement process by tailoring tendering terms and product specifications to fit a preferred company while excluding others (David-Barrett and Fazekas 2019; Fazekas et al. 2022a). Such subtle manipulative tactics are difficult to notice for World Bank staff who are typically at an informational disadvantage compared to local government officials—for example, in relation to supplier markets and company ownership.

Results

To summarize our hypothesized argument, we anticipate that, as elections draw closer, corruption risks will become greater. In particular, this should be the case when elections are highly competitive, as incumbents will face stronger incentives to seek an unfair advantage vis-à-vis challengers through corrupt means. Moreover, we argue that incentives are only part of the equation; politicians also need the capacity to divert public funds and convert their short-term gains into electoral assets. We expect that the capacity to steal is greater when the party system is characterized by

Table 2 Simple and matched comparisons of treatment and control groups (H1), single bidding %, contracts above US$25,000, goods and works (no consulting services), 1997–2007

Model	Naive comparison	Matching (1)	Matching (2)	Matching (3)
Control	29.7%	36.0%	31.9%	34.6%
Treatment	35.9%	37.8%	35.9%	35.9%
Diff(treatment − control)	6.1%*	1.7%*	4.0%*	1.3%*
95% c. interval-lower bound	5.2%	0.4%	2.8%	0.1%
95% c. interval-upper bound	7.1%	3.0%	5.1%	2.5%
N control	37,884	10,398	13,047	13,047
N treatment	13,052	10,398	13,047	13,047
Matching variables				
Log contract value	N	Y	Y	Y
Main sector	N	Y	Y	Y
Year dummies	N	Y	Y	Y
Country dummies	N	Y	N	N
Country prior single bidder %	N	N	Y	Y
Buyer prior single bidder %	N	N	N	Y

*Significant at the 5% level

medium levels of institutionalization. The capacity to turn corrupt windfall gains into an electoral advantage, on the other hand, is higher when the party system is structured around clientelistic linkages that distribute localized goods to voters.

We begin by investigating whether corruption risks increase in the immediate period leading up to elections. Our empirical evidence—both naïve comparisons of group averages and matching—provides support for H1 (Table 2). The share of single-bidding contracts for the treatment period, compared to the control period, increases by 1.3–6.1% points. In our preferred matching estimation (Matching [2] in Table 2), single bidding increases by 4% points from 32 to 36%—an increase of over 12% compared to the baseline. This specification controls for both contract-level characteristics—such as log contract value, year, and main sector—while also taking into account country-level variation in terms of baseline average single bidding. The specification thus strikes a balance between restrictiveness of matching and precision of estimation (for example, Matching [3] also incorporates the buyer baseline single-bidding rate, which is too restrictive if corrupt governments shift spending from less to more corrupt agencies). Please note that we also use this specification for testing H2–H4 below.

These findings are further confirmed by robustness tests. Running propensity score matching on a more restricted sample of competitive procedures leads to an even larger positive effect: a surge of 4.6% points (Table F1 in Appendix F). Using coarsened exact matching, we find a similarly positive, albeit smaller, increase in single bidding: plus 1.1% points (Table G1 in Appendix G). Comparing pre- and post-election years while excluding the election year itself, we get essentially the same results (the only notable difference is that the significance level of our most demanding specification, matching model [3], is 10% rather than the usual 5%)

(Table H1 in Appendix H). Running simple binary logistic regressions leads to the same conclusions as the main matching estimation (Appendix J). Finally, we find that considering electoral system type does not bias our results (proportional versus majoritarian) (Table I1 in Appendix I).

These results could, nevertheless, reflect the political business cycle. As elections approach, the World Bank—prompted by uncertainty in terms of parliamentary majorities and government composition—may want to get key strategic projects on the books in time, i.e., award contracts before elections. This could lead to competitive bidding requirements being relaxed, which would translate into higher values on our corruption risk indicator—that is, a higher share of single-bidding contracts. If this alternative explanation was true, we would need to see an uptick in either the total number and value of contracts awarded or a quicker execution of tenders in a country in the year preceding elections. Reassuringly for our hypothesis, there is no such pattern in the data. First, the average number of contracts in the pre-election year is 74 (std. error=7.2), while it is 75 for election and post-election years (std. error=6.2). The average total value of contracts awarded amounts to US$227 million in the pre-election year (std. error=US$34.8 million), while it is US$209 million for election and post-election years (std. error=US$24.3 million). Second, the average number of days needed for contract signature is nearly for days longer in the pre-election year (50.2 days) than in the election and post-election years (46.6 days). Similarly, procedure types which are quicker to execute as they require shorter time frames for competition or involve direct award without time for bidding are the same in the 2 periods (difference of 0.5% points, but insignificant at p=0.31). The absence of a political business cycle effect in World Bank-financed procurement strengthens the point we made earlier that World Bank officials are effectively insulated from local political considerations. Hence, now that the main potential counterhypotheses have been addressed, we are more confident in interpreting increases in the share of single-bidding contracts as election-driven increases to corruption risks.

H2 proposes that the increase in corruption risks prior to elections is greatest when elections are highly competitive. We test this hypothesis by incorporating the *clean elections* variable into the analysis and decomposing the total effect into (i) low, (ii) medium, and (iii) high categories of electoral competitiveness. We conduct separate propensity score matching exercises for each group (for full results see Appendix D). We find that the total effect is driven by countries falling into the "high competitiveness" category, with the low and middle groups displaying positive but insignificantly small effects (Fig. 1). In the group of countries with the cleanest and most competitive elections, the share of single bidding contracts increases by 4.6% points between the control and the treatment periods—a rise from 37 to 41%. This finding is further supported by robustness tests using a more restricted sample (Table F2), coarsened exact matching (Table G1), narrower set of periods (Table H2), and regression analysis (Table J2).

Next, we move on to H3, which proposes that the increase in corruption risks prior to elections is highest in moderately institutionalized party systems. We test H3 by incorporating the *party system institutionalization* variable into the analysis and, once again, decomposing the total effect into (i) low, (ii) medium, and (iii) high categories (we conduct separate propensity score matching exercises for each group;

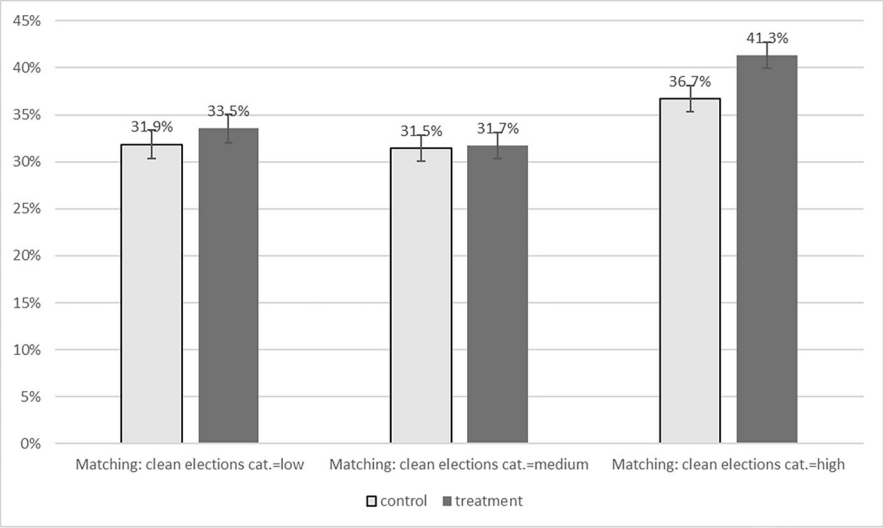

Fig. 1 Matched comparisons of treatment and control groups by clean elections categories (H2), single bidder %, contracts above US$25,000, goods and works (no consulting services), 1997–2007

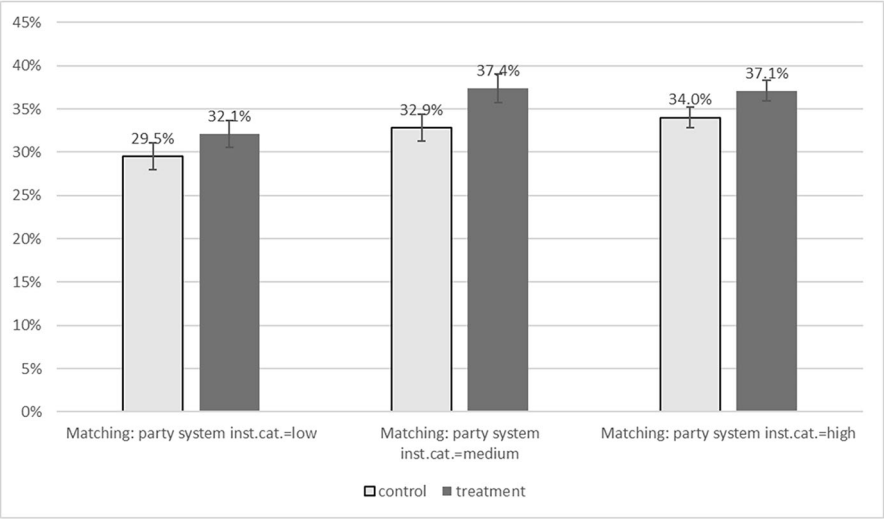

Fig. 2 Matched comparisons of treatment and control groups by party system institutionalization categories (H3), single bidder %, contracts above US$25,000, goods and works (no consulting services), 1997–2007

for full results, see Appendix D). We find a positive significant effect across all three categories; however, effect sizes differ (Fig. 2). The share of single-bidding contracts increases the most in the "medium party system institutionalization" category where the election effect leads to a 4.5% point increase in single-bidding contracts—from

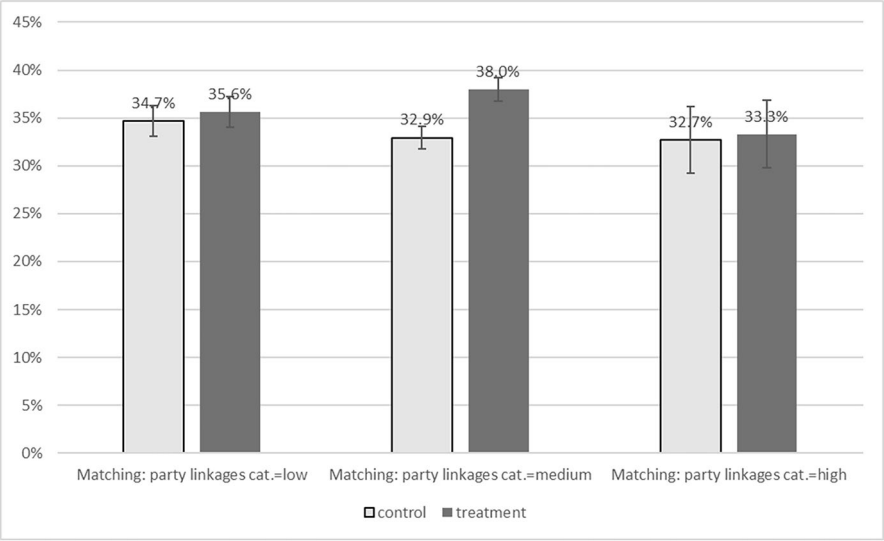

Fig. 3 Matched comparisons of treatment and control groups by party linkage categories (H4), single bidder %, contracts above US$25,000, goods and works (no consulting services), 1997–2007

33 to 37%—between the control and the treatment period. The positive significant effect across the whole sample, albeit with different magnitudes, aligns with our theoretical expectation that two countervailing factors are at play here, creating a "sweet spot" where incentives to steal (low party institutionalization) and the capacity to steal (high party institutionalization) intersect. Such a bi-directionality is further confirmed by alternative specifications, which consistently deliver large, positive, and significant differences for the "high party system institutionalization" group, but offer mixed results or considerably smaller effect sizes for the other two categories (Table F3, Table G2, Table H3, Table J3).

Finally, we conduct a test of H4, which proposes that the increase in corruption risks prior to elections is highest when party-voter linkages are primarily organized around clientelistic distribution of localized public goods. We do so by incorporating the *party linkages* variable into the analysis and, as for H2 and H3, decomposing the total effect intro (i) low, (ii) medium, and (iii) high categories (we conduct separate propensity score matching exercises for each group; for full results, see Appendix D). We define the 3 categories from the interval scale party linkages variable using terciles which approximately map onto different linkage types: low, private clientelistic rewards (e.g., cash, food, jobs); medium, local collective goods (e.g., wells, public toilets, roads); and high, programmatic linkages (e.g., national policies, vision for society). We find that the total effect is driven by countries falling into the "medium" category—that is, election years where major parties predominantly sought to mobilize voters through clientelistic distribution of localized collective goods (Fig. 3). The share of single-bidding contracts increases by 5.1% points—from 33 to 38%—between the control and the treatment period. The effects are very small and insignificant in the "low" and "high" categories. This is in line with our

theoretical expectations. If politicians aim to buy votes with private rewards, the manipulation of World Bank-funded procurement—which is typically designed to provide local collective goods—is expected to happen throughout the years between elections, rather than spiking just before elections. Furthermore, if parties campaign on programmatic platforms, steering World Bank tendering processes towards a single-bidding company will bring little electoral gain. In fact, corruption is likely to undermine the delivery of programmatic policies and programs, thus harming the party's electoral chances. We find further support for these findings in robustness tests on a more restricted sample (Table F4), coarsened exact matching (Table G2), comparisons between more restricted time periods (Table H4;), and regression analysis (Table J4).[9]

Conclusion

Since the end of the Cold War, most political systems across the world have adopted regular multiparty elections. However, despite the global spread of elections, academic scholarship continues to be divided over the effects of elections on corruption: do elections provide incentives for politicians to engage in corrupt behavior or to invest public funds in ways that maximize the public wellbeing?

Using unmatched and matched quantitative comparisons to analyze a novel dataset of aid-funded procurement contracts, our paper makes an original and significant contribution towards answering this question. To begin with, we show that—all things being equal—national parliamentary or presidential elections increase the risk of corruption in the immediate year before the ballot. Moreover, our analysis reveals a number of mechanisms that help explain this finding. First, we demonstrate that the corruption-enhancing effect of elections is greater when electoral contestation is highly competitive; in contrast, in authoritarian regimes where ruling elites systematically manipulate the electoral process, the effect is weaker. We thus have evidence that politicians manipulate public procurement processes to edge ahead of their rivals. Second, we argue that incentives are only part of the story. Politicians also require the capacity to divert public funds and convert these funds into votes. While well-oiled political parties provide formidable vehicles to organize the theft of public resources, we also highlight that—at the same time—strongly institutionalized party systems create disincentives against corrupt practices. In line with these arguments, our analysis provides evidence that the increase of corruption risk in immediate pre-election years is greater when the party system is characterized by medium levels of institutionalization, suggesting that a "sweet spot" exists where incentives to steal and organizational capabilities to steal are balanced. Finally, we show that turning short-term corrupt profits from public procurement into an electoral advantage is easiest in contexts where politicians mobilize voters through the clientelistic distribution of localized collective goods, the reason being that these

[9] Note that we also find a positive significant effect for the high category in Tables H4 and J4, albeit with a smaller effect size than the medium category in J4 and higher effect size in H4.

localized goods, such as public infrastructure or facilities, are typically delivered—often in very visible ways—shortly before the election. In contrast, private goods clientelism can be fueled by corrupt practices at any time in between elections (i.e., collecting money for the "war chest" deployed around elections), while programmatic linkages incentives politicians to invest in the production of general public goods.

Phrased in more general terms, our analysis provides empirical evidence that contradicts and refines "elections as a corruption deterrence" arguments. Instead, our findings support the other side in the elections-corruption debate: because they are costly and thus put pressure on politicians to generate campaign funding, elections have a corruption-enhancing effect under certain contextual conditions. Moreover, while existing research has studied elections mainly in isolation from contextual factors, our paper shows that elections increase public procurement corruption especially strongly when elections are highly competitive, party system institutionalization is medium, and parties link to voters through the clientelistic distribution of localized public goods.

Before concluding, we must highlight some limitations of our study. First, by focusing on corruption in aid-funded procurement, our sample is somewhat biased, as it only includes countries that have received significant developmental aid in the past. Future research will have to evaluate whether our results can also be observed in industrialized, established democracies. In particular, we believe that, in the context of high-income economies, it would be fruitful to pay greater attention to party system properties (fragmentation, polarization) and political institutions (electoral systems, government types) when analyzing the corruption effect of elections. Second, World Bank-funded procurement is likely the tip of the iceberg with national public procurement spending having fewer checks and balances, hence potentially more corruptible for electoral purposes. Third, our study focused on a particular type of corruption: manipulating public procurement with the aim of receiving only a single bid in the tender process. Going forward, research on the election-corruption link should investigate other forms of corrupt behavior. In particular, we know relatively little about the drivers of corruption during the implementation phase of public contracts—for example, civil servants and private enterprises colluding to deliver work that fails to meet contract standards.

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Data Availability The global public procurement dataset used in this article is available at and thoroughly described in Fazekas, Mihály; Abdou, Aly; Kazmina, Yuliia & Regős, Nóra (2022) Development Aid Contracts Database: World Bank, Inter-American Development Bank, and EuropeAid. Data in Brief, 42. See: https://www.sciencedirect.com/science/article/pii/S2352340922003316.

Declarations

Competing Interests The authors declare that they have no competing interests.

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