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# **Elections and corruption: incentives to steal or incentives to invest?**

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## **Abstract**

Despite the fact that most political systems around the world now hold regular multi-party elections, we know little about the effect of elections on political corruption. To address this gap in the literature, we employ a multi-method research design—combining unmatched and matched quantitative comparisons with a qualitative small-N study of Indonesia and the Philippines—to analyse a novel government contracting dataset that provides objective measurements of corruption. We find that, all things being equal, corruption risks increases in the immediate pre-election period by 1.3-6.1% points (measured as single bidding in competitive tenders). Moreover, we are able to demonstrate that the corruption-enhancing effect of elections among low and middle income countries is stronger under conditions of (i) high electoral competitiveness, (ii) mid-level party institutionalisation, and (iii) “localised club goods” clientelism.

## Introduction

The academic literature on corruption has, since its take-off in the mid-1990s, been strongly guided by analytical frameworks that emphasise the role of formal political institutions in explaining the prevalence of corrupt practices (see Golden and Mahdavi 2015; Kunicová 2006). Yet, despite this heavy focus on institutional factors, we know little about 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 democratisation 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 regular multiparty elections. In fact, nowadays, so-called “electoral authoritarian regimes probably (depending on the definition) comprise the modal type of political regime in the developing world” (Schedler 2009: 382; emphasis added). Taken together, it has been estimated that, at the beginning of the 21st century, around 75 per cent of countries worldwide regularly hold multiparty elections (Magaloni and Kricheli 2010: 125)—albeit to varying degrees of competitiveness.

Certainly, existing work on the relationship between democracy and corruption has gone some way in shedding light on the question of whether elections incentivise or discourage corrupt behaviour (e.g. Pellegata 2013; Lederman et al. 2005; Sung 2004; Adsera et al. 2003). Yet, the problem with these studies is that they generally bundle elections together with other political regime properties (such as horizontal accountability and civil rights), which makes it impossible to determine the specific effect of electoral competition on the extent of corruption. To our knowledge, the only exception is a recent working paper by McMann et al. (2017) who disaggregate the concept of democracy into its different sub-components.

Moreover, existing studies on the democracy-corruption link are limited in that they typically rely on expert surveys to measure corruption (such as the World Bank’s Worldwide Governance Indicators or Transparency International’s Corruption Perceptions Index). While expert-based measurements of corruption are generally considered to be highly problematic (e.g. Heywood and Rose 2014; Ko and Samajdar 2010; Razafindrakoto and Roubaud 2010), methodological issues are further aggravated in large-N analyses of the democracy-corruption relationship (Stephenson 2015: 108). First, experts’ subjective evaluations may be “contaminated” by unconscious assumptions about the causal link between regime types and corruption—specifically, the notion that democracies are less corrupt than dictatorships. Second, by their very nature, autocratic regimes are more secretive than democratic polities which can lead experts to overstate the extent of corruption in the latter.

Finally, existing work on the democracy-corruption link assumes that elections create incentive structures that exhibit persistence in their effect over time. While a number of studies argue that newly democratizing polities need to go through a process of consolidation before participatory institutions develop a strong corruption-reducing effect (e.g. McMann et al. 2017; Pellegata 2013), there is generally no consideration of the possibility that the effect of elections on political elite behaviour may vary across the short time period in between successive elections. Partly, this has to do with the corruption measurements adopted: because expert surveys provide general assessments of corruption at the country level—fed by perceptions of corruption in various areas of political and economic activity—they do not allow researchers to investigate whether elections incentivise or disincentivise political actors to steal from government funds that are directly under their control.

In this paper, we aim to further our understanding of the elections-corruption relationship by addressing the weaknesses just outlined. Specifically, we analyse the short-term effects of elections on corrupt behaviour: do elections, as a single event, reduce or increase the extent of political corruption? We do this by focusing on electoral institutions as our independent variable and, to measure corruption, by drawing on a new, large-scale contracting dataset that enables an objective assessment of corruption risks in the spending of developmental aid. We believe that corruption in aid spent by recipient countries is a powerful measure to examine the elections-corruption link for two reasons. First, decisions about how and where to invest developmental aid are generally made by national-level politicians. Second, when it comes to the spending of aid, political elites are, in principle, not only accountable to citizens but also to donor organisations. The risk as well as cost of getting caught stealing is therefore—compared to corruption in the spending of national budget funds—considerably higher. 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 (see Levy 2008: 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.

To test our hypotheses, we perform both an unmatched comparison as well as a comparison after propensity score matching 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 increases in the immediate pre-election period. Moreover, we are able to demonstrate that the corruption-enhancing effect of elections in low and middle income countries is stronger under conditions of (i) high electoral competitiveness, (ii) mid-level party institutionalisation, and (iii) party-voters linkages based on the clientelistic distribution of localised club goods. We illustrate the causal mechanisms that underpin our observed findings through a most-similar systems design (MSSD) comparison of the Philippines and Indonesia.

## Theoretical background and Hypotheses

Despite the global proliferation of elections in recent years, we lack systematic studies that specifically examine the elections-corruption link. Nevertheless, it is still possible to derive causal mechanisms from the institutional literature on corruption—in particular, the literature on the relationship between democracy and corruption—as a way to formulate testable hypotheses.

Current analyses of the effects of democracy on corruption are divided in their findings. While a number of scholars claim that democracy helps to control corruption (e.g. Kolstad and Wiig 2016; Lederman et al. 2005; Adsera et al. 2003; Sandholtz and Koetzle 2000), other studies find no significant correlation between the level of democracy and the extent of corruption (e.g. Blake and Martin 2006; Chowdhury 2004; Treisman 2000). Elections, even though they are bundled together with other institutional features of democracy, play an important role in explaining these findings. 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. On the other hand, scholars who are pessimistic about democracy's corruption-curbing effect stress that the pressure of electoral competition may increase incentives to engage in various forms of political corruption, such as illicit activities to raise campaign funding or the misuse of public resources for partisan purposes (e.g. Sun and Johnston 2009; Mungiu-Pippidi 2006).

While we do not deny that elections may allow voters to retrospectively punish politicians for corrupt behaviour, we believe that—all things being equal—the pressure to secure an unfair advantage through corrupt means will dramatically intensify in the immediate run-up to the election and outweigh politicians' concerns about being voted out of office by angry citizens. We therefore hypothesise:

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

However, as already hinted at in the introduction, 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: 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: 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 maximise their winning margins to project strength and deter elite defections (Simpser 2013: 86; Magaloni 2006: 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 is only one strategy among many to influence the outcome of elections. On the other hand, politicians in genuinely democratic systems face a much higher probability of losing office. Given that—again, by definition—the “menu of manipulation” is considerably more restricted, incumbent politicians in highly competitive settings may find that corrupting public investment projects provides a uniquely effective means of getting ahead of the competition (Broms, Dahlström, & Fazekas, 2019; Coviello & Gagliarducci, 2017; Klasnja, 2016). 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.

In addition, we believe that is not only the competitiveness of the electoral process that shapes incentives for corrupt behaviour, but the organisational vehicles with which politicians compete in elections—that is, political parties—also matter. In particular, a number of scholars have established a link between party system institutionalisation—defined as the degree of “stability in who the main parties are and in how they behave” (Mainwaring 1998)—and corruption.

Much of the existing literature would expect that high levels of party systems institutionalization have corruption-reducing effect—for a number of reasons. To begin with, strongly institutionalised 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 institutionalised party generally succeed in sustaining themselves in power for longer than dictators who only have a weakly institutionalised party at their disposal or lean on other organisations 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

incentivise dictators to restrain corrupt activities and invest public resources toward 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 institutionalised political parties increase elites' certainty over future political interactions—in particular, electoral contestation. That is to say, under conditions of low party system institutionalisation, 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 institutionalisation 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: 26; Keefer 2007). Moreover, it has been argued that strongly institutionalised 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 2016; Tavits 2007).

However, it also needs to be remembered that strongly institutionalised political parties provide formidable organisations to coordinate the large-scale theft of public resources, such as funds designated for public works projects. As Gingerich explains, parties characterized by high degrees of institutionalisation typically exert a lot of control over politicians' and bureaucrats' career paths. “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” (2013: 242). What is more, uneven party system institutionalisation—that is, situations where one party is more strongly institutionalised than other parties—may undermine electoral accountability mechanisms and thus encourage greater corruption. This applies in particular to one-party dominant and hegemonic party systems (e.g. Doorenspleet and Nijzink 2013; Kuenzi and Lambright 2001).

In short, scholars cannot agree on whether it is high or low party system institutionalisation that drives corruption. Bringing the different arguments together, we expect that there is a “sweet spot” where incentives to steal and capabilities to steal intersect. Specifically, we expect that:

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

Finally, elections not only differ in the degree to which political parties, as the main vehicles for competition, are institutionalised, but they can also be distinguished in terms of how political parties link themselves to the electorate. Broadly speaking, parties can choose one of two linkage 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: 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). Typically, scholars distinguish two types of clientelism, depending on whether politicians exchange votes for either private goods (e.g. money, food, clothing, building materials) or localised club goods (e.g. roads, public utilities, sporting facilities).

Based on existing academic work on the nature of electoral competition, we formulate the following hypothesis:

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 localised club goods.

We ground this expectation on a number of arguments. Most generally, it has been claimed that 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, because they need to ensure that their promised policy packages are effectively and efficiently implemented, face positive incentives to curb corruption. For example, they need to protect public funds earmarked for programmatic policies from theft and make certain that public organisations (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 believe that clientelism will have a particularly strong effect on corruption in the run-up to elections when politicians distribute localised club goods, as opposed to private goods. This prediction is based on the fact that the distribution of clientelistic club goods, such as road construction, will almost certainly have to be funded by public money; in contrast, the clientelistic distribution of private goods, such as food or clothing, can be fuelled by parties’ and politicians’ own resources.





Moreover, the delivery of club goods tends to require the involvement of a third party—in particular, private companies contracted to supply the targeted investment, such as public infrastructure or facilities. Hence, the clientelistic delivery of localised club goods—because it involves administrative decisions over the allocation of public resources—not only entails the systematic infiltration of the state bureaucracy by parties and politicians (see Hopkin 2012: 200), but it also impels political actors to use their particularistic control over the public administration to manipulate procurement processes in such a way as to ensure that a pre-selected company wins the contract—a company that will comply with the clientelistic strategy. Existing research has described a number of methods by which private companies are used to facilitate the clientelistic delivery of local club goods. For example, 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: 268). Similarly, 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: 72) and the US (Dahlström et al. 2019).

## Research design, data and indicators

To test our hypotheses and map causal mechanisms, we employ a mixed-methods research design. The first stage consists in a quantitative analysis of our novel contract-level dataset, which provides objective indicators of corruption in the spending of developmental aid. The data is examined through unmatched and matched comparisons. At the second stage, we complement this statistical analysis with a brief small-N study of the Philippines and Indonesia. Based on our quantitative findings, these two countries are a “typical” and an “extreme” case, respectively.

### Quantitative data

We combine two major global datasets for our analysis: 1) A large-scale contracts dataset scraped from the World Bank’s official website and subsequently cleaned by the Government Transparency Institute as part of the UK government-funded Anticorruption Evidence Programme<sup>1</sup> and 2) a large-scale country-year expert assessment of key electoral and political variables of the Varieties of Democracy (V-Dem) project<sup>2</sup>.

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<sup>1</sup> <http://www.govtransparency.eu/index.php/2018/02/13/aiddata/>

<sup>2</sup> <https://www.v-dem.net/en/data/data-version-9/>

First, the public procurement or contracts database contains all major contract awards of World Bank-financed projects for the fiscal years 1997-2007, over 110,000 contracts with value above 25,000 USD.<sup>3</sup> Major contract awards refer to all ‘prior-reviewed’ contracts, i.e. contracts awarded in tenders that were reviewed by the World Bank at key stages throughout the procurement cycle such as the call for tenders or award decision. Only contracts with an estimated value above a certain, context-specific, threshold undergo the prior-review process. The other tenders, the so-called post-reviewed tenders, are managed completely by the recipients of World Bank loans with World Bank staff reviewing and auditing only after the end of the project.<sup>4</sup> As our dataset only contains such high-risk tenders with greater World Bank controls, our findings are not representative of all aid spending financed by the World Bank, but only the part where risks are higher, and hence this greater degree of control is deemed necessary. Because greater supranational control of the World Bank is expected to be more disconnected from domestic electoral cycles this is exactly the part of public spending which is the least susceptible to elections-induced manipulation. Hence our data allow for examining the least likely scenario.

We compiled the contracts dataset from data scraped or downloaded directly from the World Bank’s public website to have the most complete dataset possible (a full description of data sources is provided in Appendix A). Data coming from different sources has been combined and a common set of cleaning procedures applied such as correcting for purchasing power and inflation differences across the countries and years we investigate (for full technical details see also the dataset download page<sup>5</sup>). This dataset allows for carrying out a high granularity contract-level analysis with most of the main dependent and independent variables defined at this micro-level, such as single bidding in competitive tenders, our main corruption proxy indicator.

Second, the country-year political variables derive from the V-Dem project<sup>6</sup> at the Department of Political Science at the University of Gothenburg. It contains data on 201 countries of annual records for the period 1789-2017. It, therefore, provides a full overlap with our public procurement data country-year sample. V-Dem data is, in essence, a well-executed expert assessment containing a host of

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<sup>3</sup> A fiscal year begins in July and ends with June the next year, so in fact we observe each major contract award between July 1997 – June 2008.

<sup>4</sup> 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 based on the risks associated with the economic sector, the implementing agency, and the procurement method. The World Bank provides an indicative list of thresholds for each country, but the risk assessment is outlined and the exact thresholds are determined in the procurement plans which are subject to the World Bank’s ‘no objection’ scrutiny at key stages throughout.

<sup>5</sup> <http://www.govtransparency.eu/index.php/2018/02/13/aiddata/>

<sup>6</sup> <https://www.v-dem.net/en/data/data-version-9/>



precise questions about, among others, key concepts of our theoretical frame such as free and fair elections, political party institutionalisation, and party linkages (Coppedge et al, 2019).

## Quantitative indicators

To operationalise the dependent variable we build on a growing literature using proxy indicators of corruption in administrative datasets such as infrastructure spending (Golden & Picci, 2005; Lewis-Faupel, Neggers, Olken, & Pande, 2016) and public procurement data (Fazekas, Cingolani, & Tóth, 2018). Our approach is based on a methodology widely applied to national public procurement datasets (Charron, Dahlström, Fazekas, & Lapuente, 2017; Klasnja, 2016) as well as to aid-financed contracts (Dávid-Barrett, Fazekas, Hellmann, Márk, & McCorley, 2017). Such work addresses the widely accepted shortcomings of country-level perception-based corruption indices while also offering far greater granularity (Foster, Horowitz, & Méndez, 2012).

A single bid submitted in a competitive tender serves as our dependent variable and corruption proxy indicator. Public procurement is assumed to be least prone to corruption where the process is open and competitive, and procurement regulation sets a number of maxims intended to ensure openness. Where the process deviates from these maxims, this may indicate a deliberate manipulation by a corrupt public official (or network of public and private actors) to favour a particular company and gain a private advantage. The outcomes of the public procurement process serve as the best indicators of corruption risk (Kenny & Musatova, 2010). In particular, 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 analysed across large datasets – can point to overall patterns that warrant investigation or a policy response (Fazekas, Ugale, & Zhao, 2019). 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 can be found in Appendix B.<sup>7</sup>

To operationalise the main independent variable of interest, national/federal elections, we use two variables from the V-Dem dataset: legislative or constituent assembly election (`v2xel_elecparl`) and

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<sup>7</sup> Single bidding in competitive tenders, nevertheless, only captures one particular form of high-level corruption closely aligned with closed access and institutionalised corrupt relationships between public and private elites. There are other types of corruption where competition occurs among oligarchic groups, with multiple firms competing on official tendering criteria as well as bribes.

presidential election (`v2xel_elecpres`). If either of these elections has taken place we identify the country-year as elections. This approach, neglecting local elections, matches our focus on World Bank financed contracts which are typically most strongly influenced by national governments. The time-scale for elections effect 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/federal elections serving as treatment group, that is where we expect electoral considerations influencing government contracting the most. We denote the election year and one year after it as the control group where pre-elections clientelism and favouritism is likely to be the lowest. Such a contrasting treatment-control group split allows for comparing as close as possible years with each other which minimises bias from temporal shocks; while it also allows for contrasting politically and electorally very different periods (for a simple visual representation these periods in terms of single bidding see Appendix C, Figure 8).

To operationalise the independent variables interacting with the elections treatment in hypotheses 2-4: free and fair elections, political party institutionalisation, and party linkages, we make use of variables aiming to directly measure these concepts in the V-Dem dataset. First, we use the clean elections index (`v2xel_frefair`) which captures the degree to which elections are free and fair, that is 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 make use of the party system institutionalisation index (`v2xps_party`) which expresses the degree to which political parties are institutionalized in a country. It aggregates a number of party attributes such as level and depth of organization, links to civil society, cadres of party activists, party supporters within the electorate, coherence of party platforms and ideologies, party-line voting among representatives within the legislature. A high score on these attributes generally indicates a more institutionalized party system. Third, we employ the party linkages index (`v2psprlnks`) which captures major parties' most common form of linkage to their constituents. Experts score countries on an ordinal scale ranging from clientelistic linkage when constituents are rewarded with goods, cash, and/or jobs; through local collective linkage when constituents are rewarded with local collective goods, e.g., wells, roads, or local development; to programmatic linkage when constituents are offered the party's positions on national policies, general party programs, and visions for society. The ordinal scale is transformed into an interval scale with lower values clientelistic linkages while higher values representing programmatic linkages.

In addition to dependent and independent variables of interest we also include in the quantitative analysis a battery of control variables which are expected to account for most variation potentially confounding our causal identification. These variables derive from the micro-level public procurement

dataset. They are i) year (World Bank financial year running from July to June), ii) sector (10 main sectors such as energy or health), iii) contract value (natural log of inflation adjusted USD), iv) country (either as fixed effects or as average single bidding rate throughout the whole period); and v) public organisation average corruption risk (average single bidding rate for the whole period).

For descriptive statistics of all of these variables see Appendix C, while we summarize all variables in the dataset here (Table 1).

**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 institutionalisation 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	World Bank financial year running from July to June	WB
Control	sector	10 main sectors such as energy or health	WB
Control	contract value	natural log of contract award value (inflation adjusted USD)	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

Note: WB=World Bank administrative data; V-DEM=Varieties of Democracy expert assessment data

## Mixed methods and Causal identification

We employ a tightly coupled mixed methods research design in order to both provide quantitative estimates of effect sizes across the full universe of developing economies; while also exploring the impact mechanisms in great depth, using a most-similar systems design (MSSD) comparison of the Philippines and Indonesia.

The causal identification in the quantitative analysis exploits both the width of the data and independence of World Bank project design from national election timing. First, the exceptional global reach of our dataset covering over a decade while also capturing the minute detail of the contracting process such as contract value or procedure type used (e.g. international competitive tendering) allows for i) observing a great number of elections, in a great number of contexts typically multiple times in the same country; ii) while also registering tens of thousands of potentially impacted contracts. This powerful combination of macro and micro level datasets circumvents the typical small-N problem of cross-country research looking at the effect of elections.

Second, we argue that election timings (i.e. year of election) around the world are quasi-independent of World Bank project design and procurement planning. This is because i) election years are typically set by national laws based on strict numerical rules (e.g. every four years) with some exceptions such as early elections when governments fail; and ii) World Bank financed procurement tenders follow procurement plans (i.e. detailed plans of the timing, value and object of each tender of the project) set in the loan agreement years before contracts are awarded and such plans are hard to modify once the project runs; and iii) in our sample of prior-reviewed contracts, tender timing and specifications are signed off by World Bank staff most likely independent of domestic political considerations (i.e. World Bank staff is keen to avoid any visible, electorally-motivated manipulation of procurement tenders).

By implication, we conduct both an unmatched comparison of control and treatment groups as well as a comparison after propensity score matching<sup>8</sup> balancing covariates listed in Table 1. First, the simple comparison of group average single bidding rates takes our independence assumption, argued for above, at face value. However because we cannot be 100% sure about the validity of this assumption, we only consider this comparison as an upper bound estimate of the true causal effect of elections on corruption risks in World Bank financed procurement tenders. Nevertheless, the logistic regressions, explaining treatment assignment and hence delivering the propensity scores, are of generally very poor fit with pseudo-R<sup>2</sup> only about 0.03 across all our specifications. This suggests that the major observable characteristics of World Bank financed tenders are almost perfectly random on the two sides of national elections.

Second, we also conduct a treatment-control group comparison, following propensity score matching<sup>9</sup> balancing covariates influencing our outcome variable, single bidding, such as year,

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<sup>8</sup> We use Stata 14.2, `psmatch2` command enforcing common support, logit regression fit and no replacement (i.e. equally sized control and treatment groups)

<sup>9</sup> 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.



economic sector, contract value, buyer average single bidding score, and country. As shown in Appendix E, there is no significant imbalance remaining following matching. Given the more restrictive nature and smaller samples of matching, we consider these effect sizes as a lower bound estimate for the true causal impact of elections. Furthermore, when testing H2-4, we also include country-year electoral and party system characteristics among the covariates to match on, again, matching results in practically no discernible difference on observables across treatment and control groups (Appendix E).

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

- Contracts above 25,000 USD: 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 only one company bidding.
- Only regimes that hold regular, multiparty elections (i.e. the V-DEM v2xel\_frefair variable being higher than 0): we exclude “politically closed” regimes from our analysis, such as China or North Korea, as our theory centred on elections has no bearing on such regimes..
- Countries whose number of contracts in the treatment and control groups is larger than 25: In order to restrict our analysis, by and large, to within country, before-after elections comparisons, we exclude those countries with too few contracts in either the control or treatment groups.
- Treatment-control periods: our treatment-control group definitions cover a 3-year period around elections (1 year before elections vs election year plus the year after election), all other years are excluded from the analysis.

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

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

Given the apparent independence of the election intervention from World Bank financed procurement contracts composition, it is important to spell out what mechanisms remain for national governments to corruptly exploit such contracts. Crucially for our impact mechanisms, 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. What remains in the hands of national governments to determine as projects progress and tenders are launched is the minute detail of the tendering terms and product specification. Hence, corrupt governments seeking to extract electoral gains from World Bank projects, which are often highly visible electorally and of high value, can manipulate the implementation of public procurement by tailoring tendering terms and product specifications to fit an electorally relevant company while excluding others (Dahlström, Fazekas, & Lewis, 2019; David-Barrett & Fazekas, 2016; Fazekas, Ferrali, & Wachs, 2018). Such subtle manipulation is hard to notice for World Bank staff who are typically at an informational disadvantage compared to local government officials, knowing their supplier markets and favoured companies intimately.

To explore causal mechanisms in greater depth we also employ case study methods—specifically, we conduct a small-N comparison based on a most-similar systems design (MSSD). We compare two countries that have been major recipients of World Bank financing: the Philippines and Indonesia. Based on our findings regarding H1, the Philippines and Indonesia constitute a “typical” and an “extreme” case, respectively. At the same time, they share a range of similarities in terms of political institutions and democratisation pathways.

## Results

### Quantitative Results

First, we test H1 suggesting that corruption risks increase in the immediate period leading up to elections. The empirical evidence provides support for H1, using both the naïve comparison of group averages and matching (





Table 2). The share of single bidder contracts increases by 1.3-6.1% points from the control (election year and the year after election year) to the treatment period (one year before election year). In our preferred specification, Matching (2) in



Table 2, the increase goes from 32% to 36%, a substantial 4% points or more than 12% increase compared to the baseline.



**TABLE 2. SIMPLE AND MATCHED COMPARISONS OF TREATMENT AND CONTROL GROUPS (H1), SINGLE BIDDER %, CONTRACTS ABOVE 25,000 USD, 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%
<i>diff(treatment - control)</i>	<b>6.1%*</b>	<b>1.7%*</b>	<b>4.0%*</b>	<b>1.3%*</b>
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
<i>matching variables</i>				
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

Note: \* significant at the 5% level

Second, we test H2 which proposes that the increase in corruption risks prior to elections is highest in highly competitive elections. We do so by incorporating the clean elections variable into the analysis and decomposing the total effect in



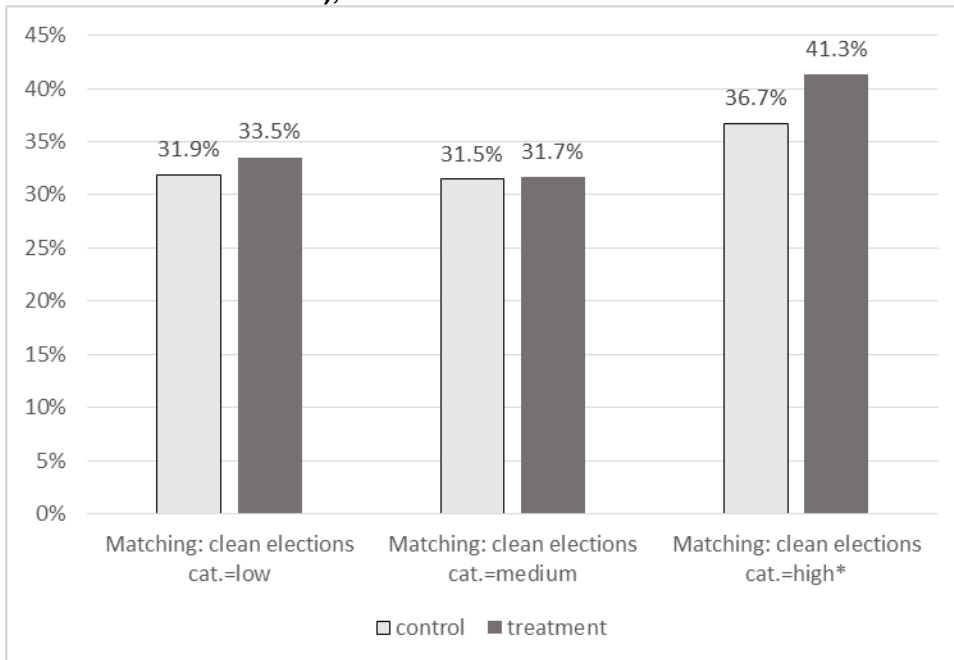
Table 2 by low/medium/high categories of clean elections (we conduct separate propensity score matching exercises for each group, for full results see Appendix D). We find that the total effect is driven by countries falling in the cleanest elections category with the low and middle groups displaying positive but insignificant small effects (



Figure 1). The share of single bidder contracts increases by 4.6% points from the control (election year and the year after election year) to the treatment period (one year before election year) in the high clean elections category—that is, the countries with the most competitive elections (an increase from 37% to 41%).



**FIGURE 1. MATCHED COMPARISONS OF TREATMENT AND CONTROL GROUPS BY CLEAN ELECTIONS CATEGORIES (H2), SINGLE BIDDER %, CONTRACTS ABOVE 25,000 USD, GOODS AND WORKS (NO CONSULTING SERVICES), 1997-2007**



*Note: \* significant at the 5% level*

Third, we test H3 which proposes that the increase in corruption risks prior to elections is highest in moderately institutionalised party systems. We do so by incorporating the party system institutionalisation variable into the analysis and decomposing the total effect in



Table 2 by low/medium/high categories (we conduct separate propensity score matching exercises for each group, for full results see Appendix D). We find a positive significant effect across all three country-year categories according to party system institutionalisation, albeit effect sizes differ (

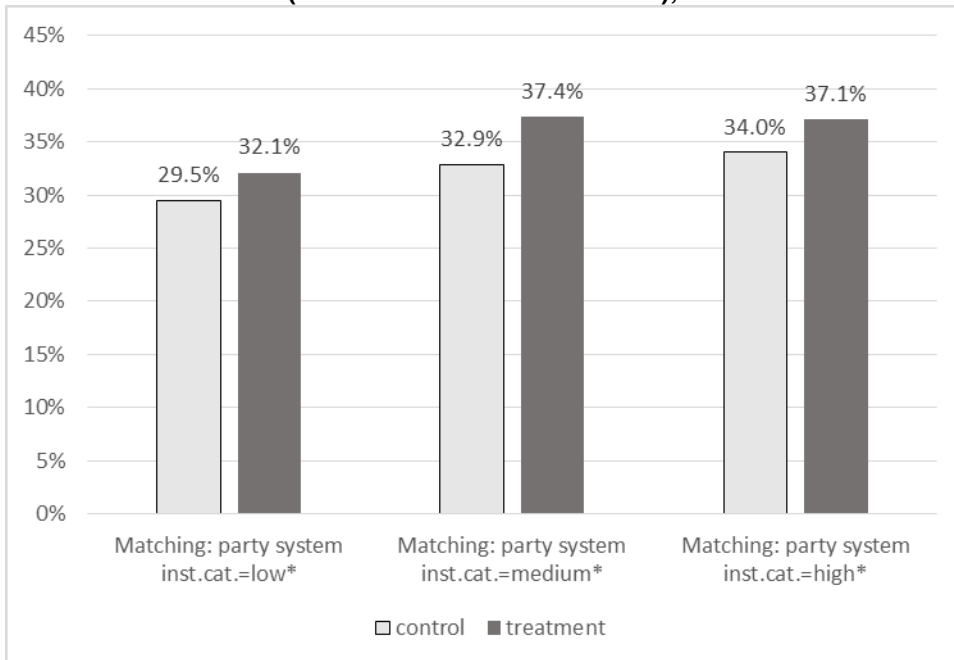


Figure 2). The share of single bidder contracts increases the most in the medium party system institutionalisation category, where the effect 4.5% points from the control (election year and the year after election year) to the treatment period (one year before election year), an increase from 33% to 37%. The positive significant effect across the whole sample, albeit with different magnitudes, confirms our theoretical expectation that two countervailing factors are at play here, creating a “sweet spot” where incentives to steal (low party institutionalisation) and capacities to steal (high party institutionalisation) intersect.





**FIGURE 2. MATCHED COMPARISONS OF TREATMENT AND CONTROL GROUPS BY PARTY SYSTEM INSTITUTIONALISATION CATEGORIES (H3), SINGLE BIDDER %, CONTRACTS ABOVE 25,000 USD, GOODS AND WORKS (NO CONSULTING SERVICES), 1997-2007**



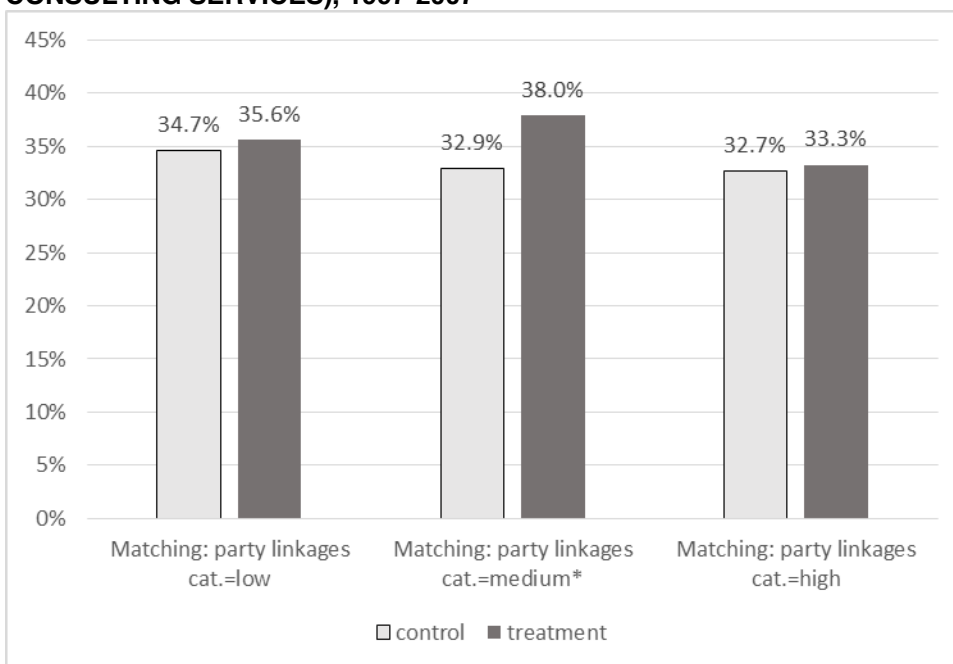
*Note: \* significant at the 5% level*

Fourth, we test H4 which proposes that the increase in corruption risks prior to elections is highest when party linkages are characterised by the clientelistic distribution of localised public goods. We do so by incorporating the party linkages variable into the analysis and decomposing the total effect in



Table 2 by low/medium/high categories of party linkages (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 in the medium clientelism category, that is country-years where main parties tend to offer localised club goods to their clientele, that is which most World Bank projects contribute to such as local roads or schools (Figure 3). The share of single bidder contracts increases by 5.1% points from the control (election year and the year after election year) to the treatment period (one year before election year) in this country-year category (an increase from 33% to 38%). The effects are very small and insignificant in both the most clientelistic category (low) and most programmatic category (high) which is in line with our expectations: with most clientelistic party linkages, votes are outright bought with little local public good provision, hence there is little need to direct World Bank projects to particular areas; and with most programmatic party linkages, voters are offered generic, national programmes and policies, hence again, little expectation of targeted local spending.

**FIGURE 3. MATCHED COMPARISONS OF TREATMENT AND CONTROL GROUPS BY PARTY LINKAGE CATEGORIES (H4), SINGLE BIDDER %, CONTRACTS ABOVE 25,000 USD, GOODS AND WORKS (NO CONSULTING SERVICES), 1997-2007**



Note: \* significant at the 5% level

### Small-N Comparative analysis

To explore causal mechanisms and to identify causal factors that are difficult to quantify for statistical analysis, this section will employ a most-similar systems design (MSSD) to compare two countries that



have been major recipients of World Bank financing: the Philippines and Indonesia. The case selection is guided by the methodological principles outlined in Seawright and Gerring (2008). Specifically, judged by the distribution of data regarding the main effect theorised in H1 (i.e. the average effect of elections on the extent of single bidding), the Philippines and Indonesia constitute a typical and an extreme case, respectively: while the Philippines is representative of our sample, Indonesia exhibits an unusually high value on the effect of elections on the dependent variable (see



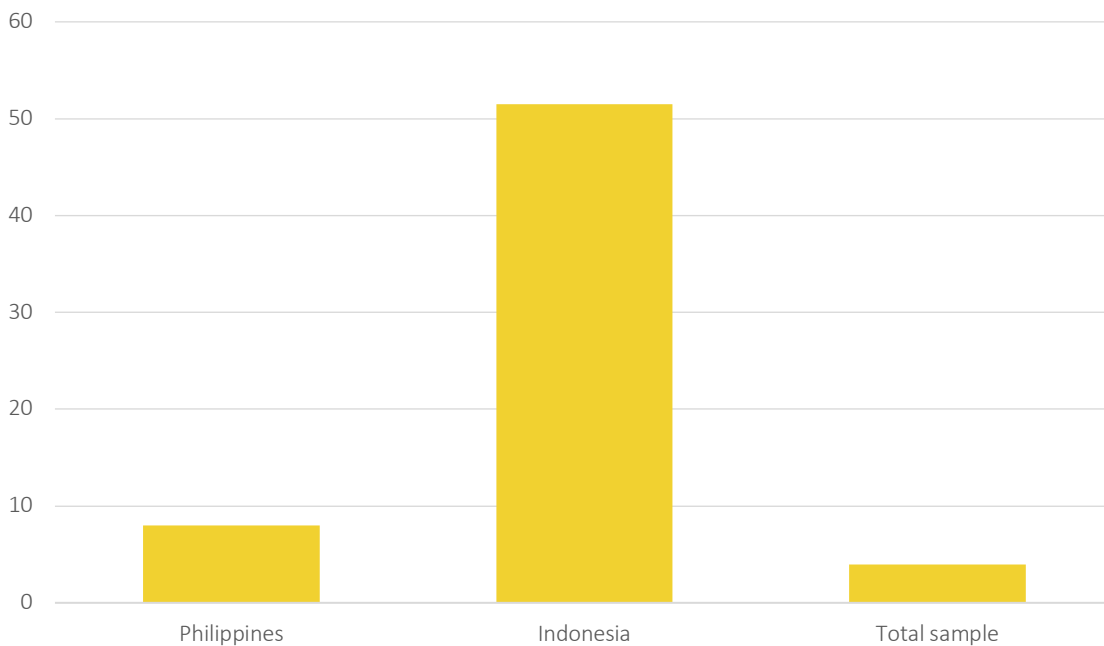
Figure 4).



**FIGURE 4: MEAN EFFECT OF ELECTIONS ON SINGLE BIDDING (**



**TABLE 2, MODEL: MATCHING(2)**

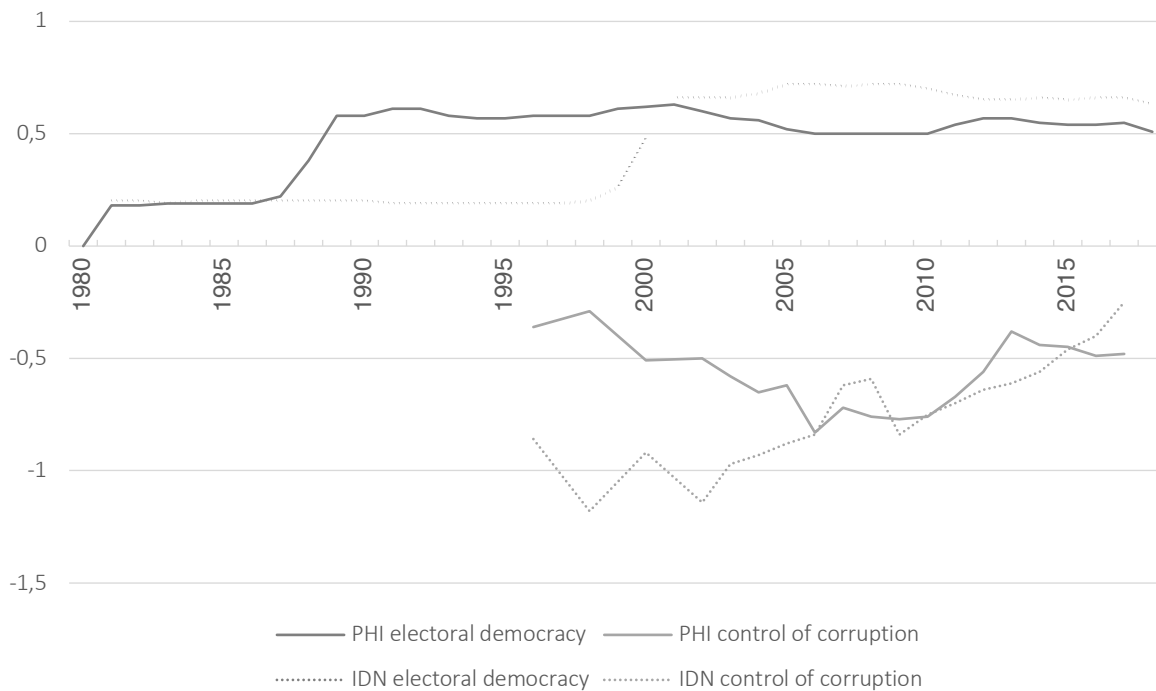


Moreover, following the logic of MSSD—according to which “common systemic characteristics are conceived of as ‘controlled for’, whereas inter-systemic differences are viewed as explanatory variables” (Teune and Przeworski 1970: 33)—the two cases share a number of similarities on theoretically relevant causal factors concerning corruption in the spending of developmental aid (see Figure 5). Perhaps most importantly, both the Philippines and Indonesia witnessed the breakdown of autocratic rule and the implementation of competitive multiparty elections during the so-called “third wave” of democratization. Despite the fact that elections in both countries suffer from a number of shortcomings, both the Philippines and Indonesia are commonly classified as minimal “electoral” democracies.<sup>10</sup> In addition, the two countries have similar profiles in terms of how corruption pervades the political system: in both the Philippines and Indonesia, corruption is structured around the competition between particularistic networks that employ a range of strategies to illegally extract public resources out of the state (see Hellmann 2017); in other words, the corruption “market” is highly fragmented, rather than centralized and concentrated. What is more, both countries have, in recent years, had their fair share of corruption scandals involving World Bank funded projects. For example, in 2009, World Bank investigators uncovered a major business cartel that had colluded in the bidding for contracts under the Philippines’ National Roads Improvement and Management Program (Philstar 2009). Meanwhile, in Indonesia, the Bank discovered that 30 percent of its funds in a US\$76-million

<sup>10</sup> For an overview of some of these issues, see Norris (2018).

urban development program in Sulawesi had been stolen (Perlez 2003). Three years later, the Bank cancelled three infrastructure projects and demanded the return of US\$4.7 million already spent over alleged bribery in the hiring of consultants (Devex 2006).

**FIGURE 5. CONTROLLING FOR ELECTORAL COMPETITION AND GENERAL CORRUPTION**



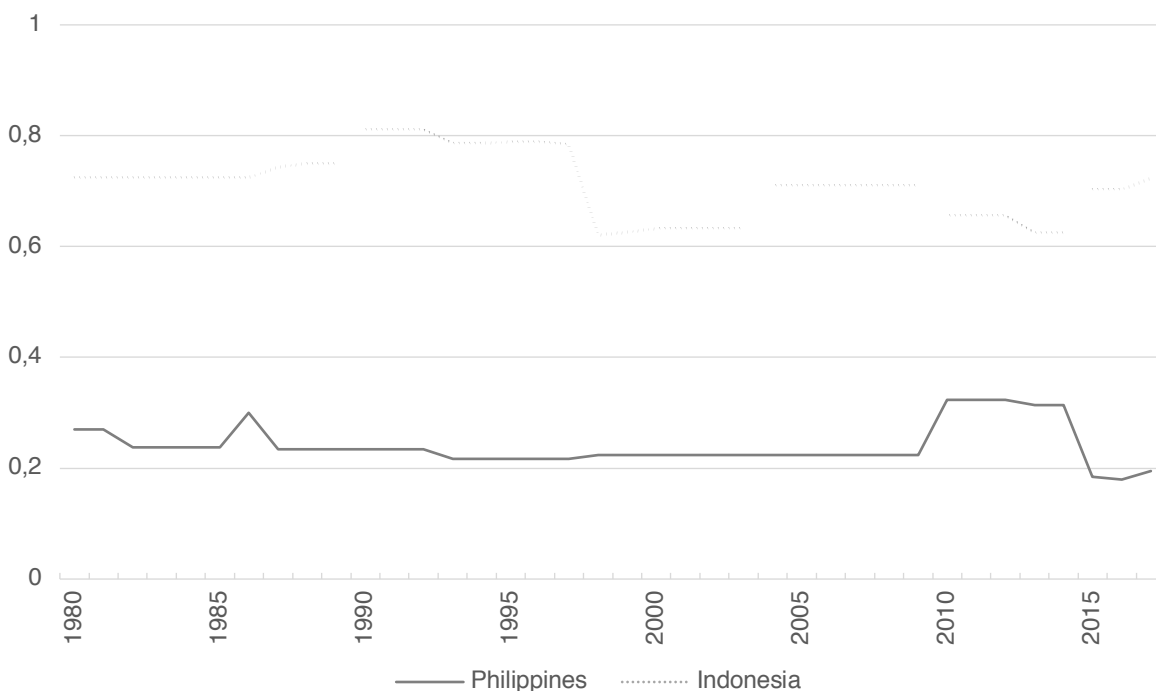
Source: V-Dem electoral democracy index; World Bank control of corruption indicator

Yet, the question that remains is why, in Indonesia, elections significantly increase incentives for political elites to misappropriate World Bank funds, while this effect is not observed in the Philippines. One possible response would be to argue that Filipino politicians do not lack the incentives to steal aid money in the run-up to elections but they simply lack the capacity to do so. The problem with this argument, however, is that it ignores the extraordinary extent to which Filipino politicians control bureaucrats through patronage networks. In fact, with de-colonization in the mid-20th century, domestic elites designed the bureaucratic state in such a way that it subordinated itself to the particularistic interests of the political elite. As Hutchcroft summarizes, “one can say that the colonial administrative apparatus was itself largely colonized by emergent Filipino politicians from the provinces. This clearly stunted the coherence of the Philippine state, undermined central efforts to supervise local affairs, and encouraged the patterns of rampant patronage under which it still suffers today” (2000: 302). More specifically, numerous studies discuss how political corruption in the Philippines is greatly facilitated by the fact that a large share of bureaucrats owe their positions to patronage appointments (e.g. Teehankee 2012; Quimpo 2009). Put differently, there is no reason to believe that the low corruption-

increasing effect of elections in the Philippines is due to politicians' inability to misuse bureaucratic organisations towards particularistic ends.

Instead, we argue that the differences in the effect of elections on the extent of corruption in aid-funded procurement between Indonesia and the Philippines can be traced back to party funding structures, which are themselves deeply embedded in the party system and the nature of electoral competition. Crucially, the two countries differ significantly in terms of the degree to which their party systems are structured along social cleavages. Parties in Indonesia are deeply divided over religious lines: not only does a “church-state” cleavage pit religious parties against secular parties, but religious parties are, among themselves, also polarized on the confrontation between modern and traditional understandings of Islam (Mietzner 2013: 145-146; Ufen 2012). In contrast, the Philippines has failed to develop a nationalized party system in which electoral competition is rooted in social cleavages. Instead, politicians mobilize voters primarily through localized patron-client networks (Hicken 2014; Teehankee 2012).<sup>11</sup>

**FIGURE 6. PARTY SYSTEM INSTITUTIONALIZATION SCORES, PHILIPPINES AND INDONESIA, 1980-2017**



Source: V-Dem party institutionalization index

<sup>11</sup> For completeness' sake, it should be pointed out that, more recently, scholars have also observed a growing importance of clientelism and other particularistic forms of voter mobilization in Indonesia (e.g. Choi 2014; Aspinall 2013). However, it seems that this trend—at least in national elections—can largely be traced back to electoral reform in 2009, which replaced *closed-list* proportional representation with *open-list* proportional representation. In other words, this trend set in *after* the end of our observation period and does therefore not play a role in our analysis.





The differences in the means of voter mobilization reflect in the extent to which political parties are institutionalized as adaptable and resilient organizations (see Figure 6). In Indonesia, political parties—based on their strong roots in social milieus—are effective gatekeepers for individuals accessing positions of political power.<sup>12</sup> In particular under the closed-list PR electoral system—which was used for legislative elections until 2009, when it was replaced by an open-list variant—parties and their leaders wielded tremendous power over political career paths (Hellmann 2014: 286). On the other hand, in the Philippines—where the means of voter mobilization are concentrated in the hands of individual politicians, rather than parties as abstract organizations—political parties are merely loose, short-lived alliances between political elites. These alliances form and disband with staggering frequency, driven by elites’ strategic calculations: before elections, politicians group themselves around presidential candidates whom they consider to have good chances of winning; after elections, politicians from losing parties will flock to the newly-elected president’s party in large numbers (Hellmann 2011: 106-108).

Taken together, the nature of electoral competition and party organizational structures can explain why, in the run-up to elections, Indonesian politicians face greater incentives to steal funds through aid-funded procurement, whereas fewer incentives exist in the Philippines. Crucially, in Indonesia, political parties have to shoulder substantial electoral campaign costs—including, for example, costs for supporter rallies and TV ads (Lindsay 2007). Since the implementation of free and fair elections, these costs have soared to dizzying heights. The expenses for commercial media advertising in particular have risen significantly, with some sources estimating an increase of more than tenfold between 1999 and 2009 (Ufen 2010: 29-30; also see Mietzner 2007: 253-255; Prasetyawan 2012: 314). Given that classic sources of political party finance—such as party membership fees and public subsidies—are negligible (Hellmann 2011: 137-140), parties rely heavily on corrupt activities to generate monetary funds as a means to cover their extraordinarily high campaign expenditures. Specifically, parties exploit their near monopolistic control over political career paths to force members in public office to steal “for the team”. Mietzner (2015: 603) describes two forms of corruption that are of particular relevance to our analysis here: party leaders may force parliamentarians to (i) engage in “budget scalping”, whereby the latter approve public projects—including projects funded by developmental aid—in exchange for kickbacks from businesses who stand to benefit from these projects, or (ii) misuse their influence over government ministries to “sell” public

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<sup>12</sup> It should be noted that a number of personalistic parties have successfully challenged the established parties—most notably, Susilo Bambang Yudhoyono, a retired military general, and his Democratic Party (*Partai Demokrat*) won the 2004 and 2009 presidential elections. Still, most major parties remain socially rooted in the electorate.



contracts—possibly financed by aid donors—to those private companies that promise to pay the largest bribes. In both of these cases, manipulating the procurement process in such a way so that the preferred company emerges as the only bidder—i.e. our dependent variable—is an effective technique to facilitate corrupt exchanges.

In contrast, in the Philippines—given that local clientelistic machines play the key role in mobilizing voters—political finance operates on a different logic. The first step to understanding this logic is knowing that patron-clientelism describes ongoing exchange relationships: “Whether it is a bag of rice in exchange for a promised vote, or a vote in exchange for a promised job, clientelist exchange usually requires one of the parties to trust that the other will deliver on their promises. Such promises are more credible when there is an expectation that the relationship will be an ongoing one. Repeated interaction reinforces social norms of reciprocity” (Hicken 2011: 293). One-off sources of income—such as kickbacks and bribes—are therefore not a reliable way to fuel patron-client networks with material resources. Instead, Filipino politicians mainly rely on pork barrel funds—in particular, the Priority Development Assistance Fund (PDAF)—to grease their clientelistic machines (Holmes 2018; Teehankee 2012). An illustration of how this works in practice is given in a report by the Philippine Center for Investigative Journalism (Chua and Cruz 2004). Politicians may, for example, use PDAF money to finance public health facilities in their constituencies. However, rather than making health care provision accessible to all constituents, politicians instruct hospitals to dispense medical assistance only to constituents bearing a “political ID” issued by the politician’s office—in other words, only to constituents who are loyal political supporters. If hospitals do not comply with the politicians’ instructions, PDAF funding will be cut off.

This is not to say that Filipino politicians do not face incentives to manipulate public procurement processes as a way to generate corruption profits—in fact, there is a rich literature that shows that such practices are widespread (e.g. Jones 2013; Reyes 2007). Rather, what the above example shows is that public procurement corruption, because—unlike domestic discretionary spending (such as the PDAF)—it does not provide a steady income stream, is an unsuitable method for nurturing patron-client linkages with the electorate. This can explain why, in the Philippines, elections do not exert a strong effect on the extent of corruption. The findings of our statistical analysis add further support to such an interpretation. As outlined above, we do not find a meaningful correlation between clientelistic party-voter linkages and the effect of elections on corruption in the spending of aid.

In short, this section has explored some of the causal mechanisms that explain how an increase in electoral competition can strengthen politicians’ incentives to steal funds earmarked for development. However, the MSSD comparison of Indonesia and the Philippines has also shown that the corruption-enhancing effect of electoral competition depends on political finance structures—a



factor that is not easily quantifiable for statistical analysis. In Indonesia, where political parties have to shoulder considerable campaign expenses, the effect of elections on the extent of corruption in aid-funded procurement is much stronger than in the Philippines, where personalistic clientelistic machines play the key part in mobilizing votes.

## Conclusions

Since the end of the Cold War, most political systems across the world have adopted regular multi-party elections. This paper took a first step towards analysing how differences in the competitiveness of elections and the institutional embedding affect the extent of political corruption. Based on a multi-method research design that combines unmatched and matched quantitative comparisons with a small-N study of Indonesia and the Philippines, we arrived at four key findings. First, all things being equal, elections increase the extent of corruption in the immediate year before the ballot. Second, this effect is stronger when elections are highly competitive. In contrast, in authoritarian regimes where ruling elites systematically manipulate the electoral process, the effect is weaker. Third, the increase in corruption in immediate pre-election years is greater when the party system is characterized by medium levels of institutionalisation. This indicates that a “sweet spot” exists where incentives to steal and organisational capabilities to steal are balanced. The comparative analysis of Indonesia and the Philippines further supports our argument: in Indonesia, where parties command relatively strong organisations and have to shoulder the burden of campaign costs, the corruption-enhancing effect of elections is much stronger than in the Philippines, where electoral competition is mainly between family-owned clientelistic machines. Fourth, our findings suggest that incentives to steal public resources in the run-up to elections are particularly strong in contexts where politicians mobilise voters through the clientelistic distribution of localised club goods. We argue that this is because targeting voters with clientelistic club goods (such as roads) will almost always have to be funded with public money; in contrast, private goods (such as clothes or food) can often be funded out of politicians’ own pockets. Moreover, the clientelistic delivery of localised club goods usually requires politicians to bring a third party on board, such as a private company contracted to supply the good in question. Taken together, these mechanisms create strong incentives for politicians to corrupt public procurement processes in the immediate run-up to elections.

However, before concluding, we should highlight two caveats. First, we only analysed corruption in the spending of development aid. Even though we believe that corruption in the spending of aid—because political elites are accountable to both voters and donor organisations—constitutes a



particularly challenging test for our theoretical hypotheses, we cannot be sure that our findings also apply to corruption in the spending of national budgets. Second, 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 statistical results can also be observed in industrialised, established democracies.

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## Appendices

### Appendix A. Description of World Bank contracts data Sources

**Major contract awards** <https://finances.worldbank.org/Procurement/Major-Contract-Awards/kdui-wcs3>

Contains "prior-reviewed" contracts by World Bank, i.e. the contract award commitments that were reviewed by the World Bank before they were awarded. Each contract is being prior-reviewed in case their value is above a certain threshold. Thresholds vary by country and the type of contract (goods, works, services) and are defined in the procurement plans.

**World Bank Projects and Operations** <http://data.worldbank.org/data-catalog/projects-portfolio>

Includes basic information of all World Bank projects, such as the project title, task manager, country, project id, sector, commitment amount and financing. It also provides links to publicly disclosed online documents.

**Notices and Contracts (WB website)**

<http://projects.worldbank.org/procurement/procurementsearch?lang=en&srce=both>

Contract notices and contract awards are continuously published here, so the website provides the potential for building a self-updating database.

**Internal World Bank Database**

Internal database of World Bank that contains a wider range of variables than the publicly available data. Our key variable, single bidding is from this database. This dataset is available at:

<http://www.govtransparency.eu/index.php/2017/05/22/data-publication-world-bank-public-procurement-data-for-fiscal-years-1998-2013/>

The combined complete dataset can be downloaded at:

<http://www.govtransparency.eu/index.php/2018/02/13/data-publication-foreign-aid-of-world-bank-europeaid-and-iadb/>



## Appendix B. Validity of single bidding as a corruption proxy

As for what we refer to as ‘macro validation’ we checked the correlations with some well-established perception-based corruption indicators on country-level (similarly to (Fazekas & Kocsis, 2017)): World Governance Indicators’ Control of Corruption, Transparency International’s Corruption Perception Index, and Global Competitiveness Index’s Favoritism in decisions of government officials (indicator 1.0713). All three perception indices indicate lower corruption with higher values, so we expect to see negative correlations (Kaufmann, Kraay, & Mastruzzi, 2009; Transparency International, 2012; World Economic Forum, 2010). This strategy has been originally used for national procurement data and for procurement notices published on Tender Electronic Daily (TED), the procurement page of the European Union; however, the corruption risks of procurement from development aid sources might not go hand in hand with the corruption patterns of national procurement. Furthermore, following from the regulations of the donor institutions (Fazekas & Tóth, 2014) contracts below country-specific thresholds are not published on donor websites, thus we cannot even track the full amount of development aid spent through procurement. It might be the case that suspicious transactions are managed below the threshold value and larger contracts are kept transparent. Consequently, we do not necessarily expect to see strong correlations with these indicators, but still, some level of correlation would strengthen the validity of our red flags.

The correlations with perception-based indicators for our most important red flag, single-bidding, are presented in Table 3. Single bidding is our most straight-forward red flag. In order to secure that resources are allocated to specific favoured individuals other competitors should be somehow ruled out from competition. Unless fake competitors are commissioned single bidding is necessary, but not sufficient sign of a tender.

**TABLE 3. CORRELATION OF SINGLE BIDDING AND PERCEPTION-BASED CORRUPTION INDICATORS**

	TI - CPI (2009)	WGI - CoC (2009)	GCI - Fav (2009)
<i>Weighted with number of contracts</i>	-0.20	-0.15	-0.20
<i>Weighted with sum of contract values</i>	-0.18	-0.11	-0.15

*Note: Only countries with more than 100 contracts are considered*

<sup>13</sup> The question was, ‘In your country, to what extent do government officials show favoritism to well-connected firms and individuals when deciding upon policies and contracts? [1 = always show favoritism; 7 = never show favoritism]’



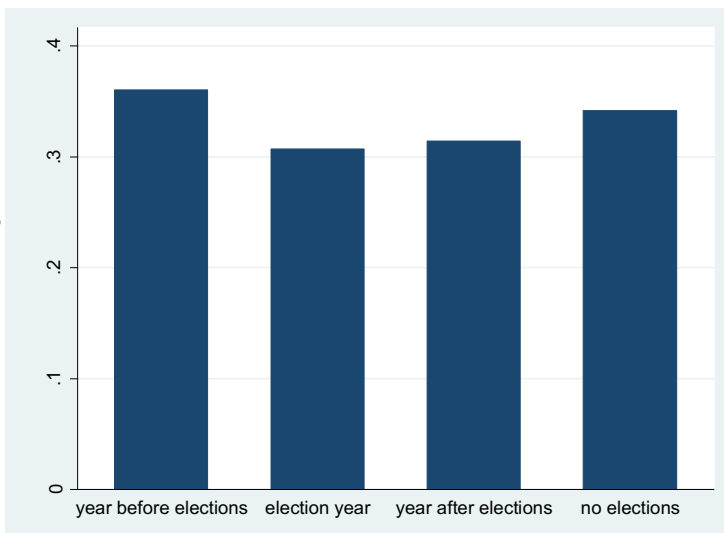


## Appendix C: Descriptive statistics

TABLE 4.

Variable	Obs	Mean	Std. Dev.	Min	Max
single bidding rate	69,215	0.32	0.47	0	1
treatment(=1)	52,297	0.26	0.44	0	1
log contract value	71,139	13.40	1.44	10.43	21.48

FIGURE 8. SINGLE BIDDING BY PERIOD





**TABLE 5.**

Country	ISO code	treatment status		Total
		0	1	
AFG		357	0	357
ALB		335	147	482
ARM		497	218	715
AZE		431	144	575
BDI		69	0	69
BEN		443	103	546
BFA		377	74	451
BGD		548	177	725
BGR		309	284	593
BIH		598	120	718
BOL		308	95	403
BRA		1,168	1,010	2,178
CIV		239	100	339
CMR		125	45	170
COL		194	121	315
COM		69	30	99
CPV		60	42	102
CRI		99	35	134
DJI		94	44	138
ECU		287	49	336
EGY		273	242	515
EST		60	27	87
ETH		371	215	586
GEO		515	165	680
GHA		769	293	1,062
GIN		254	90	344
GMB		85	54	139
GNB		106	30	136
GTM		202	143	345
GUY		78	77	155
HND		290	143	433
HRV		595	137	732
IDN		1,199	96	1,295
IND		6,512	856	7,368
IRN		448	192	640
IRQ		115	0	115
JAM		135	40	175
JOR		162	17	179
KAZ		160	59	219
KEN		336	87	423





KGZ	210	85	295
KHM	142	97	239
LAO	323	73	396
LBN	405	97	502
LKA	269	90	359
LSO	142	26	168
LVA	121	108	229
MAR	214	46	260
MDA	192	50	242
MDG	868	141	1,009
MEX	1,199	496	1,695
MKD	248	54	302
MLI	223	124	347
MNG	219	91	310
MOZ	499	156	655
MRT	306	92	398
MWI	226	111	337
NER	122	73	195
NGA	675	112	787
NIC	366	135	501
NPL	113	57	170
PAK	288	89	377
PAN	89	78	167
PER	265	127	392
PHL	589	345	934
PNG	80	69	149
POL	523	261	784
PRY	187	71	258
ROU	426	259	685
RUS	1,616	925	2,541
RWA	51	17	68
SEN	838	54	892
SLE	77	12	89
SLV	387	41	428
SRB	270	113	383
TCD	206	54	260
TGO	137	98	235
THA	60	39	99
TJK	388	117	505
TLS	242	25	267
TUN	435	262	697
TUR	593	276	869
TZA	375	239	614



UGA	305	129	434
UKR	377	53	430
URY	169	118	287
UZB	357	43	400
VEN	473	111	584
VNM	1,293	740	2,033
WBG	168	68	236
YEM	1,001	226	1,227
ZMB	254	133	387
ZWE	64	53	117
Total	38,937	13,360	52,297

**TABLE 6.**

Contract award year	treatment status		Total
	0	1	
1997	2,737	578	3,315
1998	5,513	1,329	6,842
1999	5,480	1,296	6,776
2000	4,428	963	5,391
2001	3,544	1,840	5,384
2002	3,091	1,159	4,250
2003	2,631	1,787	4,418
2004	3,213	1,229	4,442
2005	3,335	1,150	4,485
2006	2,284	1,422	3,706
2007	2,681	607	3,288
Total	38,937	13,360	52,297

**TABLE 7.**

sector	treatment status		Total
	0	1	
Agriculture	4,058	1,294	5,352
Education	4,610	1,784	6,394
Energy & mining	3,202	1,110	4,312
Finance	877	311	1,188
Health & social serv	6,522	2,191	8,694
Industry and trade	1,342	398	1,740
Info & communication	252	70	322
Public admin, Law	9,422	3,194	12,616
Transportation	4,669	1,624	6,293
Water/sanit/flid prot	3,976	1,379	5,355
Total	38,930	13,355	52,285



**TABLE 8.**

Country	ISO code	Average group membership category (1-3)		
		clean elections category	party system inst. category	party linkages category
AFG		1.00	1.00	2.00
ALB		1.64	1.52	2.00
ARM		1.00	1.00	1.41
AZE		1.00	1.00	3.00
BDI		2.00	1.00	1.00
BEN		2.00	1.00	1.00
BFA		2.00	1.00	1.00
BGD		2.00	2.00	3.00
BGR		3.00	3.00	2.30
BIH		2.93	2.00	1.00
BOL		3.00	2.44	1.59
BRA		3.00	3.00	2.30
CIV		1.00	1.18	1.00
CMR		1.00	1.00	3.00
COL		2.00	1.35	1.00
COM		2.06	2.94	2.00
CPV		3.00	2.00	3.00
CRI		3.00	2.90	2.61
DJI		1.00	1.00	1.00
ECU		3.00	2.00	1.00
EGY		1.00	2.00	2.00
EST		3.00	3.00	3.00
ETH		1.00	1.00	2.00
GEO		1.59	1.68	2.59
GHA		2.63	2.00	1.63
GIN		1.00	2.00	1.00
GMB		1.88	2.00	2.00
GNB		1.97	1.00	1.00
GTM		2.00	2.00	1.87
GUY		2.51	2.00	1.00
HND		2.69	3.00	1.00
HRV		2.70	2.70	2.31
IDN		2.26	2.66	2.74
IND		3.00	3.00	2.00
IRN		1.49	1.00	3.00
IRQ		2.00	2.00	2.00
JAM		2.26	3.00	1.00
JOR		1.57	1.00	1.00
KAZ		1.00	2.00	1.69



KEN	1.00	1.00	3.00
KGZ	1.00	1.00	1.00
KHM	1.03	1.00	1.00
LAO	1.00	2.00	3.00
LBN	1.09	3.00	1.00
LKA	2.00	3.00	3.00
LSO	3.00	2.00	2.00
LVA	3.00	2.76	2.00
MAR	1.32	2.00	1.89
MDA	2.52	2.00	1.00
MDG	1.80	1.00	1.00
MEX	3.00	3.00	2.75
MKD	2.17	3.00	1.00
MLI	2.00	1.00	2.00
MNG	3.00	3.00	2.19
MOZ	1.69	1.00	2.00
MRT	1.12	2.00	1.00
MWI	1.94	1.00	2.00
NER	2.70	3.00	3.00
NGA	1.00	2.00	1.00
NIC	2.00	3.00	2.00
NPL	1.00	2.16	2.00
PAK	1.00	2.76	2.00
PAN	3.00	2.00	3.00
PER	2.48	1.54	2.17
PHL	1.81	1.00	1.00
PNG	1.00	1.00	1.00
POL	3.00	3.00	3.00
PRY	2.00	2.00	1.00
ROU	2.00	3.00	1.39
RUS	1.71	2.62	2.00
RWA	1.00	1.00	3.00
SEN	3.00	2.00	2.00
SLE	2.00	1.00	1.00
SLV	2.00	3.00	3.00
SRB	2.95	3.00	2.00
TCD	1.00	1.00	1.00
TGO	1.00	1.00	1.00
THA	2.30	2.00	2.42
TJK	1.00	2.00	3.00
TLS	2.00	1.82	1.82
TUN	1.00	1.00	1.00
TUR	3.00	3.00	3.00



Elections and corruption

TZA	1.00	2.00	3.00
UGA	1.00	2.00	2.00
UKR	1.42	1.10	2.34
URY	3.00	3.00	3.00
UZB	1.00	3.00	3.00
VEN	2.78	2.00	1.00
VNM	2.00	3.00	3.00
WBG	3.00	3.00	2.00
YEM	1.00	1.00	1.72
ZMB	2.00	1.44	2.00
ZWE	1.00	1.00	1.00
Total	2.09	2.20	2.02



## Appendix D: Full Propensity Score Matching Results for H2, H3, and H4

**TABLE 9. MATCHED COMPARISONS OF TREATMENT AND CONTROL GROUPS BY CLEAN ELECTIONS CATEGORIES (H2), SINGLE BIDDER %, CONTRACTS ABOVE 25,000 USD, GOODS AND WORKS (NO CONSULTING SERVICES), 1997-2007**

Model	Matching: full sample	Matching: clean elections cat.=low	Matching: clean elections cat.=medium	Matching: clean elections cat.=high*
control	32.4%	31.9%	31.5%	36.7%
treatment	35.9%	33.5%	31.7%	41.3%
diff(treatment - control)	<b>3.5%</b>	<b>1.7%</b>	<b>0.2%</b>	<b>4.6%</b>
95% c.interval-lower bound	2.3%	-0.4%	-1.7%	2.7%
95% c.interval-upper bound	4.6%	3.8%	2.2%	6.6%
N control	13,047	3,747	4,393	4,907
N treatment	13,047	3,747	4,393	4,907
<i>matching variables</i>				
log contract value	Y	Y	Y	Y
main sector	Y	Y	Y	Y
year dummies	Y	Y	Y	Y
clean elections index	Y	N	N	N
clean elections category	N	Y	Y	Y
country prior single bidder %	Y	Y	Y	Y

**TABLE 10. MATCHED COMPARISONS OF TREATMENT AND CONTROL GROUPS BY PARTY SYSTEM INSTITUTIONALISATION CATEGORIES (H3), SINGLE BIDDER %, CONTRACTS ABOVE 25,000 USD, GOODS AND WORKS (NO CONSULTING SERVICES), 1997-2007**

model	Matching: full sample	Matching: party system inst.cat.=low*	Matching: party system inst.cat.=medium*	Matching: party system inst.cat.=high*
control	33.2%	29.5%	32.9%	34.0%
treatment	35.9%	32.1%	37.4%	37.1%
diff(treatment - control)	<b>2.7%</b>	<b>2.6%</b>	<b>4.5%</b>	<b>3.1%</b>
95% c.interval-lower bound	1.5%	0.4%	2.3%	1.4%
95% c.interval-upper bound	3.8%	4.8%	6.8%	4.8%
N control	13,046	3,352	3,400	6,267
N treatment	13,046	3,352	3,400	6,267
<i>matching variables</i>				
log contract value	Y	Y	Y	Y
main sector	Y	Y	Y	Y
year dummies	Y	Y	Y	Y
party system inst. index	Y	N	N	N
party system inst. category	N	Y	Y	Y
country prior single bidder %	Y	Y	Y	Y



**TABLE 11. MATCHED COMPARISONS OF TREATMENT AND CONTROL GROUPS BY PARTY LINKAGE CATEGORIES (H4), SINGLE BIDDER %, CONTRACTS ABOVE 25,000 USD, GOODS AND WORKS (NO CONSULTING SERVICES), 1997-2007**

model	Matching: full sample	Matching: party linkages cat.=low	Matching: party linkages cat.=medium*	Matching: party linkages cat.=high
control	32.4%	34.7%	32.9%	32.7%
treatment	35.9%	35.6%	38.0%	33.3%
diff(treatment - control)	<b>3.4%</b>	<b>0.9%</b>	<b>5.1%</b>	<b>0.6%</b>
95% c.interval-lower bound	2.3%	-1.3%	3.3%	-4.4%
95% c.interval-upper bound	4.6%	3.2%	6.8%	5.6%
N control	13,047	3,447	5,881	682
N treatment	13,047	3,447	5,881	682
<i>matching variables</i>				
log contract value	Y	Y	Y	Y
main sector	Y	Y	Y	Y
year dummies	Y	Y	Y	Y
party linkages index	Y	N	N	N
party linkages category	N	Y	Y	Y
country prior single bidder %	Y	Y	Y	Y



## Appendix E: Propensity Score matching diagnostics

**TABLE 12. PSTEST H1 (MODEL 2)**

Sample	Ps R2	LR chi2	p>chi2	MeanBias	MedBias	B	R	%Var
Unmatched	0.03	1749.49	0	6	2.4	42.7*	1.2	100
Matched	0.002	66.63	0	1.7	1.5	10.1	1.09	50

**TABLE 13. PSTEST H2 (MODEL 1)**

Sample	Ps R2	LR chi2	p>chi2	MeanBias	MedBias	B	R	%Var
Unmatched	0.03	1751.23	0	5.7	1.7	42.7*	1.22	67
Matched	0.003	114.85	0	2.1	1.8	13.3	0.89	33

**TABLE 14. PSTEST H3 (MODEL 1)**

Sample	Ps R2	LR chi2	p>chi2	MeanBias	MedBias	B	R	%Var
Unmatched	0.031	1791.83	0	6	2.7	43.6*	1.08	67
Matched	0.004	151.07	0	2.4	2	15.2	1.05	0

**TABLE 15. PSTEST H4 (MODEL 1)**

Sample	Ps R2	LR chi2	p>chi2	MeanBias	MedBias	B	R	%Var
Unmatched	0.031	1791.83	0	6	2.7	43.6*	1.08	67
Matched	0.004	151.07	0	2.4	2	15.2	1.05	0





## Appendix F: Robustness tests: competitive tendering procedures only

**TABLE 16. SIMPLE AND MATCHED COMPARISONS OF TREATMENT AND CONTROL GROUPS (H1), SINGLE BIDDER %, CONTRACTS ABOVE 25,000 USD, GOODS AND WORKS (NO CONSULTING SERVICES), COMPETITIVE TENDERING PROCEDURES ONLY, 1997-2007**

model	naive comparison	Matching (1)	Matching (2)	Matching (3)
control	16.2%	17.4%	16.6%	18.1%
treatment	21.2%	21.2%	21.2%	21.2%
<i>diff(treatment - control)</i>	<b>4.9%</b>	<b>3.7%</b>	<b>4.6%</b>	<b>3.0%</b>
95% c.interval-lower bound	4.0%	2.6%	3.5%	1.9%
95% c.interval-upper bound	5.8%	4.8%	5.7%	4.2%
N control	29,389	9,738	9,740	9,742
N treatment	9,742	9,738	9,740	9,742
<i>matching variables</i>				
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