Anti-corruption in aid-funded procurement: Is corruption reduced or merely displaced?[1]

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ABSTRACT

Given a widespread sense among donors that mainstream anti-corruption reforms over the past 25 years have failed to yield results, there is a move towards more targeted interventions. Such interventions should, in principle, overcome implementation gaps and make it easier to evaluate impact, supporting learning. However, when interventions are narrowly targeted, there is a risk that corrupt actors simply adapt, shifting their focus to areas with weaker controls, so that overall corruption is not reduced but merely displaced. We analyse data points from World Bank-funded development aid tenders over 12 years in >100 developing countries, and observe the heterogeneous effects of a 2003 anti-corruption reform aimed at increasing oversight and opening up competition. Our tight matching estimations suggest that the reform is effective in the targeted area: it decreases corruption risks due to low competition (the share of single bidding falls from 22% to18%). But we also find that evasive tactics largely cancel out these positive direct effects: buyers switch to non-treated less competitive procedure types (whose share increases from 7% to 10%) and exploit them more intensively (single bidding goes from 61% to 81%). Our results demonstrate how data analytics can be used to observe public procurement at the system level to inform more adaptive and effective anti-corruption programming. More broadly, we underline that technical interventions might not represent the best way to tackle systemic corruption, instead strategies should target the root causes of corruption and contribute to building a culture of integrity.

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INTRODUCTION

Despite making considerable investments in fighting corruption over several decades, international development agencies are often criticised for having failed to achieve a significant impact on levels of graft. This is typically attributed to an implementation gap, with anti-corruption reforms often having introduced sweeping and one-size-fits-all changes in legal and institutional frameworks, but overlooked the challenges to implementation in a given development context. Such challenges include a lack of state capacity and weak resources (Pritchett, Woolcock and Andrews, 2010), the need to 'think and work politically' by paying attention to how affected interests might react (Rocha Menocal, 2014), as well as a collective action problem which means that individuals in contexts where corruption is systemic have little incentive to change their behaviour (Persson, Rothstein and Teorell, 2013). The tendency for anti-corruption reforms to be broad and ambitious, coupled with a convention of measuring corruption levels in aggregate and at the country level, also makes it difficult to ascertain the contribution of a programme to any perceived change in the prevalence of corruption.

These reflections have led the development community to move towards more targeted anti-corruption interventions. However, there is mounting evidence of a problem with this approach: a targeted intervention might achieve its specific goal and yet also trigger

strategic responses on the part of corrupt individuals, who simply adapt their behaviour and find new loopholes to exploit. Olken & Pande, for example, in a review of corruption interventions in developing countries, report that corrupt officials confronted with tougher regulations often find alternate strategies to pursue rents (Olken and Pande, 2012). A recent study of healthcare reform in Uganda found that, although a drastic increase in oversight and penalties for bribe-taking achieved a short-term reduction in bribe requests, the positive effects proved short-lived because staff began instead to solicit 'gifts' or other 'signs of appreciation' to supplement their incomes (Peiffer, Armytage and Marquette, 2018). Fisman & Golden (2017) report on an experiment in Romania where increased oversight of high-school exams and tougher penalties reduced collective cheating, but more affluent students switched to paying bribes to improve results (while less affluent students saw their grades drop) (Borcan, Lindahl and Mitrut, 2017; Fisman and Golden, 2017).

Such 'displacement effects' are widely observed in criminology too, with law enforcement actions against organised crime often found to have unintended consequences or to prompt strategic responses (Welsh and Farrington, 2002; Smith, Wolanin and Worthington, 2003; Levi and Maguire, 2004; Guerette and Bowers, 2009). Criminals under pressure move to different geographical locations to take advantage of weaker law enforcement or greater market opportunities (Varese, 2012), or to get around regulations (Vidal and Décary-Hétu, 2018). They also utilise new technologies to avoid detection, as with the growing use of drones to smuggle drugs into prisons (O'Hagan and Hardwick, 2017).

This body of literature and development experience supports the case for flexible and adaptive programming, in line with Andrews et al's Problem-Driven Iterative Approach (PDIA) (Andrews, Pritchett and Woolcock, 2013). However, to make informed decisions about how to adapt programming, it is necessary to observe a whole system and detailed data points within it, in order to detect displacement effects and unintended consequences. We demonstrate how this can be achieved in the context of corruption in public procurement, an area that is critical to good governance – government procurement typically accounts for 30-50% of public spending – but also highly prone to corruption (Ware *et al.*, 2007; Rose-Ackerman and Palifka, 2016). It is also a classic example of isomorphic mimicry, in that standardised and 'best-practice' public procurement laws have been introduced around the world, but have often made little difference to procurement practices (Jones, 2007; Williams-Elegbe, 2014; Williams-Elegbe, 2015; Atiga & Azanlerigu, 2017).

However, this standardisation has had one positive consequence: that the procurement process tends to be highly structured and subject to considerable regulation (partly to serve the goal of minimising corruption risks). This makes it a good locus of research as it is possible to observe the whole system and identify whether and how an intervention leads to changes in behaviour at different stages of the process. Moreover, learning how to reduce vulnerability to corruption in public procurement promises to bring important benefits, in the form of making public service delivery more efficient, helping to foster competitive markets and boost economic development.

To test hypotheses about how corrupt actors respond to anti-corruption regulation in public procurement, we collect and analyse a unique large-scale dataset of World Bank-

funded development aid tenders and contracts over 12 years in >100 developing countries. With data points from multiple stages of the procurement process as well as a range of outcomes, we observe the heterogeneous effects of a change in World Bank procurement rules. Underpinned by economic theories of corruption control, the new rules introduced limits on the discretion that public officials exercised over the process while also increasing opportunities for oversight and scrutiny, particularly by bidders. They also sought to open up competition by requiring that tenders were advertised more widely and by mandating greater use of e-procurement methods – changes which, by facilitating market entry, are expected to indirectly increase accountability. Our matching estimation strategy exploits the distinct break in the application of the new rules to World Bank-financed projects depending on a project start date before or after 1 November 2003. Crucially for the identification strategy, projects following old and new rules typically run in parallel resulting in tenders and contract awards being captured by our data. Hence, we can compare control and treatment contracts awarded after November 2003 in the same or very similar countries, buyers, markets, and financial years.

Through detailed observation of the impact of the intervention on behaviour at different stages of the procurement process, we seek to answer the research question*:* 'do the new rules *reduce* corruption risks or merely *displace* them?'

We find that the World Bank procurement reform is effective in a direct sense: it decreases corruption risks associated with low competition: the share of tenders with a single bidder decreases from 22.4% to 18.7%, the average number of bidders increases from 4.5 to 5.0; and the share of repeat winners falls from 71.8% to 65.4%. However, we also find evidence of evasive tactics which largely cancel out these positive direct effects.

First, buyers switch to non-treated non-competitive procedure types, whose prevalence increases from 7.3% to 9.6%. Second, they exploit these procedures more intensively: the outcomes of non-competitive procedure types deteriorate, e.g., the share of tenders attracting only one bidder increases from 67% to 81%. Overall, foreign companies lose out as their market share drops by 2 percentage points. Taken together, the net effect of the reform is likely to be ambiguous.

These findings highlight the importance of tracking likely displacement effects in order to better target anti-corruption interventions, and demonstrate how an evidence base to underpin a Problem-Driven Iterative Approach might be constructed in a major area of public spending. Substantively, the results also suggest that utilising distinct combinations of reform efforts – or sequencing reforms in ways that pre-empt evasive responses - may increase the impact of interventions over time. For example, any procurement reform aiming to expand the advertisement and publicity of tenders should be coupled with stronger regulation and monitoring of non-competitive, non-advertised tenders, to prevent this type of displacement. More broadly, our results suggest the need to complement incentives-based regulatory reforms with wider efforts to understand how regulations interact with social and economic drivers of corruption.

REDUCING CORRUPTION IN PROCUREMENT

Laws and regulations concerning public procurement are typically based on the assumption that greater competition for tenders increases welfare (Celentani and Ganuza, 2002; Ware *et al.*, 2007) and that the best way to achieve greater competition is to reduce transaction costs for suppliers (Williamson, 1981; Estache and Iimi, 2008).

Thus, procurement rules aim to make processes as open as possible by reducing the costs associated with learning about tenders and preparing and submitting bids. Empirical evidence supports this overall relationship. Kenny & Crisman (2016) show that better advertising of contract tenders increases the number of bidders, while Coviello & Mariniello (2014), in a study of national tenders in Italy, find that the number of bidders increased by 9.3% as a result of advertising in official bulletins rather than advertising only on the buyers' own local notice boards. Increased openness is also effective in enhancing value for money in public procurement (Estache and Iimi, 2008; Ohashi, 2009; Kenny and Crisman, 2016).

However, theories of corruption control in political science emphasise a slightly different logic. They tend to focus on the 'buyer' side and suggest that corruption can be deterred by reducing the discretionary power of the officials who administer the process and/or increasing oversight (Becker, 1968; Klitgaard, 1991; Rose-Ackerman and Palifka, 2016). The threat of external audit is widely found to be effective in increasing competition and reducing corruption (Olken 2007; Knack et al. 2017; Zamboni & Litschig 2013; Avis et al. 2016). Sometimes even simple changes to the rules – for example, requiring approval from seniors - can significantly reduce the use of non-competitive procedure types (David-Barrett et al 2016). Increased oversight is also linked to concrete welfare benefits: the performance of intensive audits reduces the prices paid for homogeneous goods (Di Tella and Schargrodsky, 2003). Indeed, the two mechanisms are related: if greater openness increases competition by bringing more bidders into the process, more actors will have a stake in holding public officials to account, thus more competition should *ceteris paribus* indirectly improve oversight.

Knack et al (2017) analyse firm surveys in 88 developing countries, and find evidence that both openness and oversight help to increase competition: firms are more likely to submit bids if they perceive procurement systems to be transparent, particularly in the case of smaller firms, and firms report paying fewer and smaller bribes in countries with more transparent procurement systems, more effective complaint mechanisms and better external auditing arrangements (Knack, Biletska and Kacker, 2017). Many common reforms to procurement are in line with both logics: e-procurement, for example, both reduces transactions costs and constrains the discretionary power of officials overseeing the process. The introduction of e-procurement has been found to reduce prices (Auriol, 2006; Singer *et al.*, 2009) and, in both India and Indonesia, to increase the probability that the winning bidder comes from outside the region where the contract takes place – an indicator of widening access (Lewis-Faupel *et al.*, 2016). In these cases, while the intervention did not lead to reduced prices, it changed the nature of supply, bringing in higher-quality suppliers, such that there were reduced rents and increased efficiency of public spending. In Slovakia, the introduction of e-procurement together with requirements to publish tenders on a central procurement repository website achieved an increase in the average number of bids per contract, from 2.3 bids per tender in 2009 to 3.6 bids per tender in 2011 (Šípoš, Samuek and Martin, 2015).

However, relatively few studies investigate whether these benefits last or are counteracted by other unintended consequences. The research which does address this question suggests that corrupt actors administering public procurement processes respond strategically. Olken's case study in Indonesia, for example, finds that an increase in auditing of road expenditures leads to a reduction in missing expenditures, but also to an increase in the distribution of contracts to family members of project officials (Olken,

2007). Gerardino et al (2017) use a regression discontinuity design to test the impact of audits on choice of procurement procedure, and find that this classic anti-corruption intervention perversely leads to a decrease in the use of auctions and an increase in the use of direct (non-competitive) contracts (Gerardino, Litschig and Pomeranz, 2017). There is also some evidence that procuring entities seek to evade regulations by bringing procedures outside the applicability of the Public Procurement Law or into less open and competitive procedure types (Kenny and Musatova, 2010; Heggstad and Froystad, 2011; Podumljak and David-Barrett, 2015). This is done by splitting lots so that they fall below thresholds at which certain controls or transparency are required (Papanek, 2009 ch. 6; Piga, 2011); or invoking exceptions to the rules on grounds of national security or extreme urgency (Soreide, 2002; OECD, 2007; Schultz and Soreide, 2008).

Corruption in public procurement typically occurs when insiders manipulate different parts of the process - for example, by writing the specification of the tender very narrowly such that only one company would meet the conditions (Grodeland, 2005; Báger, 2011; Heggstad and Froystad, 2011; Goldman, Rocholl and So, 2013), or advertising the tender for a very short period so that only companies with advance knowledge have time to write a bid (Tanzi and Davoodi, 1997; Kenny and Musatova, 2010). The fact that the process is complex yet structured means that, when confronted with changes in procurement rules which increase oversight or constrain their discretionary power, corrupt officeholders often have considerable scope to respond strategically by shifting their manipulations to another phase of the process. Shifting is facilitated where corrupt networks control several stages of the process. Particularly in clientelist systems, for example, politicians may be able to influence several of the stages through political influence over bureaucrats that they appoint or as part of socially complex patterns of

loyalty and reciprocal obligations (Charron et al, 2017; Mavrogordatos 1997; Dávid-Barrett & Fazekas, 2019; Goldman et al, 2013). Substitute evasive strategies abound. For example, tighter control of the advertisement of tenders (to eliminate use of short advertisement periods to provide an advantage to cronies) could displace corrupt behaviour to the stage where bids are evaluated (where cronies may be favoured instead through improper influence over the evaluation committee).

We expect that, if corruption is more tightly controlled in some parts of the procurement process, corrupt actors seeking to respond strategically will wish to minimise transaction costs related to switching. This might mean that, rather than entering a new sector or geographical location, which will incur information costs and potentially resistance from other corrupt groups who control that 'turf', their first response might be to hold most aspects of their business model constant, but try to corrupt a different part of the same process. In order to track as wide as possible a range of evasive strategies within the procurement process, we develop corruption risk indicators for three stages of the procurement process: pre-bidding, bidding, and post-bidding.[i]

Hypotheses

We use procurement under investment lending by the World Bank as a case study, largely because of the superior quality of this data relative to public procurement data collected by governments, particularly in developing countries. The Bank is also a very significant spender; its procurement system affects a portfolio of around USD 42 billion in over 1,800 projects in 172 countries (World Bank, 2015). Moreover, this dataset is ideal for measuring the impact of a regulatory intervention, since the World Bank imposes its own

procurement rules on client governments under investment lending. The Bank also provides pre- and post-review of these contracts, which are often carried out by technical assistants hired by the Bank for the project. This allows us to focus on the impact of a change in World Bank rules on a very large dataset of tenders.

We take the November 2003 update to World Bank procurement rules as the main intervention of interest. The reform sought to increase competition through an intervention targeted at opening up access to procurement tenders, specifically by requiring that tenders were electronically advertised and extending the use of e-procurement methods. It also sought to limit officials' discretion and increase oversight, through requiring procurement plans (to which buyers can be better held to account), introducing obligatory prior review mechanisms for cases where all bids are rejected (to check that reasons for rejecting bids were legitimate), and extending oversight to bidders (through audit requirements).

Our theory suggests that the heterogeneous impacts of this intervention can be decomposed into intended (H1) and unintended (H2 and H3) effects which together add up to the total net effect (H4). We discuss each of these hypotheses applied to the specific institutional and data framework of this study.

The main thrust of the 2003 reform sought to increase competition as a way of mitigating corruption by making the bidding stage of the procurement process more open (e.g. electronic advertisement). The intervention was thus based on economic theories of corruption control for which there is a body of empirical evidence, as outlined above, where increasing competition is established as an effective way to combat corruption.

Building on this evidence base and prior research analysing the intervention's impact on competitiveness (Dávid-Barrett et al, 2017), we hypothesise the intervention's main intended impact as

H1: The reform to open up access and increase oversight decreases corruption risks associated with lack of competition during the bidding stage.

As outlined above, there are two main ways corrupt actors can react to increased corruption controls: i) move on to other, less controlled phases of the procurement process; or ii) exploit existing loopholes more intensively. H2 and H3 elaborate on each of these in turn.

Corrupt actors can move on to less controlled areas in a plethora of ways; in the specific context of World Bank-funded procurement tenders and the corresponding dataset we examine, two particular evasive responses are pre-eminent. First, corrupt actors may switch to procedure types which are less competitive by nature such as sole-sourcing or negotiated procedures; here, the new requirements of online advertising and use of e-procurement have little impact. Second, if changing procedure type is costly – e.g., if procedure type choice is tightly regulated – corrupt actors might alternatively seek to corrupt the competitive procedures after the bidding stage, for example by pushing companies into corruption during the contract signature negotiations (where officials may use delays or threats to extract kickbacks). Hence, we hypothesise the intervention's first unintended impact as:

H2: The reform to open up access and increase oversight displaces corruption risks to untreated phases of the procurement process such as (a) pre-bidding procedure choice and (b) post-bidding contract signature negotiations.

Corrupt actors might also change how they use existing corruption techniques, e.g., by exploiting existing loopholes more intensively. In our context, the most straightforward corruption technique remaining consistently available throughout the intervention is non-competitive procedure types. These procedure types carry a high corruption risk, i.e., it is relatively easy to exploit them to channel public funds to cronies and they can be misused in varying ways, producing different outcomes such as single bidding or repeated awards to the same company. Hence, we hypothesize the intervention's second unintended impact as:

H3: The reform to open up access and increase oversight intensifies corruption exploiting existing loopholes such as risky non-competitive procedure types.

While the characteristics of the tendering process and associated corruption risks are expected to shift around as a result of the intervention, in line with the three above hypotheses, they are also likely to impact on which companies can benefit from corruption. Corruption in public procurement is predominantly about erecting barriers between 'insiders' or connected firms on the one hand and 'outsiders' or non-connected firms on the other, in order to confer a competitive advantage on the former. Where political connections are important to procurement success, this tends to benefit domestic firms which, *ceteris paribus*, are more likely to build long-term relationships with local public authorities than foreign firms, especially those without a domestic

subsidiary (Coviello and Gagliarducci, 2017).[ii] If the intended and unintended impacts of the intervention shift the boundary between these two groups, it may change the advantage conferred by connections. Where the intended positive effects of a reform dominate the compound effect, we would expect to see outsiders gaining relative market share, while if the unintended negative effects are stronger, outsiders are likely to lose ground. Thus, we hypothesise the intervention's net effect as:

H4: As net effect, the reform to open up access and increase oversight leads to an increase in participation by foreign bidders at the expense of domestic bidders.

METHODS, DATA AND INDICATORS

Methods

Following David-Barrett et al (2017), we employ a quantitative research design which exploits the distinct break in the application of the new rules to World Bank-financed projects and the time lag in issuing tenders and awarding contracts in control and treatment projects (projects governed by the old and new rules, respectively). In other words, in the years following the 2003 regulatory change, we exploit the fact that the same or very similar countries, buyers, markets, and financial years see similar contracts awarded from projects which are either treated or not depending on the project approval date (Table 1).

We employ propensity score matching[iii] on the level of contracts using covariates:

- country (average corruption risks prior to the intervention),
- buyer organisation (average corruption risks prior to the intervention),
- year (World Bank financial year running from July to June),
- market (10 main sectors such as energy or health), and
- contract value (natural log of inflation adjusted USD).

As the matched pairs' main remaining difference is the regulatory regime governing their projects, we suggest that this approach provides a reliable second-best estimate of the true causal impact of the intervention, in the absence of fully random assignment to treatment status. Our control variables are superior to traditional confounding factors controlled for in the literature such as ethnic fractionalisation or democracy because the level of measurement is closer to the hypothesized impact mechanisms and uses variables more directly relevant for causal identification on the contract level. Detailed goodness of fit statistics for our matching estimation can be found in Appendix D.

However, our approach is not without limitations. First, we allow for matching across organisations with similar pre-intervention corruption risk scores as enforcing strict within-organisation matching would have reduced the sample to a few hundred contracts and a couple of organisations. Second, the national and World Bank oversight mechanisms are expected to cluster on the project level, for example the procurement officials tend to remain the same throughout a project's lifetime but may differ across projects and projects may be of different length or size. As the treatment is at the project level, we cannot carry out matching within projects to counter this bias.

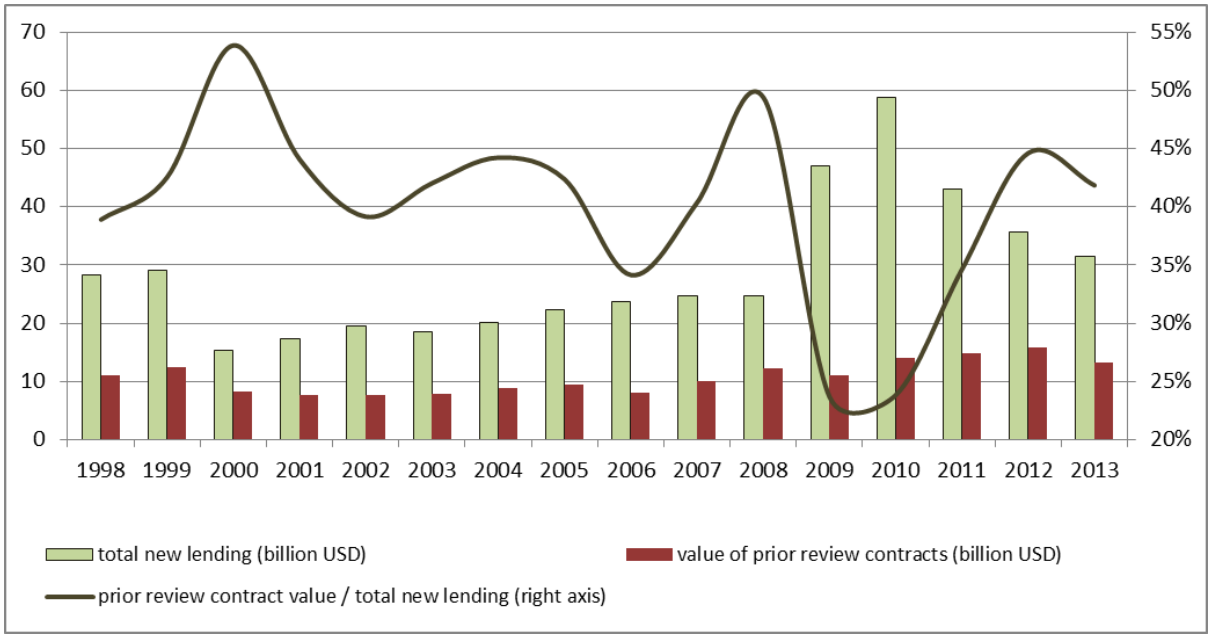
Because the date from which the new rules apply is globally imposed by the World Bank, and because designing, negotiating, and approving projects is a lengthy exercise, we expect no gaming around the temporal cut-point. This is also supported by statistical tests of observed project distributions showing that there is no evidence that project approval dates are brought forward artificially to avoid using the new regulatory regime (see Appendix B).

Data

Our source database contains all major contract awards of World Bank-financed projects for calendar years 1997-2014.[iv] The replication dataset is available at Harvard Dataverse while the full dataset with a richer data content (i.e. more variables and other donor contracts data) is described in a linked Data in Brief article. 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 prior review.[v] The other tenders, so-called post-review tenders, are managed wholly by the recipients of World Bank loans, with World Bank staff reviewing and auditing only after the end of the project.[vi] 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. For other World Bank-financed procurement tenders, we assume that donor corruption controls are of lesser importance as oversight is much more light touch and risks are lower (at least in principle).[vii]

Prior-review contracts represent a significant, albeit fluctuating, share of total lending (see Figure 1). This fluctuation is due to the constantly changing country, sector, and organisational composition of spending and project start and completion dates. While we cannot fully rule out a range of sample biases such as gaming of prior-review thresholds for bureaucratic cost avoidance reasons, our interviews and review of procedures (e.g., number and range of people required to approve changes in thresholds) suggest that any gaming is likely to be of minor importance.

Figure 1. Share of prior review contracts compared to total new lending by the World Bank (FY1998-2013)



Source: Own calculation based on World Bank data

We compiled a dataset from data scraped or downloaded directly from the World Bank's public website to have the most up-to-date data (a full description of data sources is provided in Appendix A). In addition, we also used an internal database of the World Bank

which includes a slightly richer set of variables for the major contract awards dataset, allowing us to construct indices of competition such as whether a contract was awarded in a tender which received only one bidder.[viii]

We focus on changes introduced by the 2003 update to the rules for tenders of goods, works and services. The new rules apply to projects where the project concept note is approved after the new rules became effective; the regulations to follow are specified in the financial agreement in each project. For projects approved prior to the introduction of the new rules, contracts continue to be awarded according to the old regulatory regime.[ix] This means that tendering processes that occur at the same time may operate under different regulations, depending on whether their project's approval date is before or after the effective date of the new regulation. This is critical to our identification strategy, and hence we have fully investigated possible exceptions.[x]

In Table 1, the number of contracts in the control and treatment groups is summarized on a yearly basis, where the control group consists of projects approved before 1 November 2003 and the treatment group consists of projects approved afterwards. There are no contracts beyond 2014 which derive from contracts approved before 1 November 2003; hence 2014 is the end-point of our analysis. We only consider contracts larger than USD25,000 to exclude small contracts where competition is less likely. In addition, we exclude consultancy contracts, keeping only goods and works, in order to remove those contracts where specific skills and requirements are likely to limit competition even in the absence of corruption.

Table 1. Number of contracts awarded in the treated and control groups, contracts above 25,000 USD, goods and works, 2003-2014

	Contract award year												
	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	Total
control	735	4,077	3,494	2,219	1,747	1,216	676	466	277	155	21	3	15,086
treated	0	321	1,157	1,641	2,164	2,266	2,488	2,164	1,456	1,036	469	42	15,204
Total	735	4,398	4,651	3,860	3,911	3,482	3,164	2,630	1,733	1,191	490	45	30,290

Indicators

All hypotheses take different types of corruption risk as the dependent variable. One of the innovations of this article is the identification of objective proxy indicators of corruption in aid-funded public procurement building on a methodology widely applied to national public procurement datasets (Klasnja, 2016; Charron *et al.*, 2017). Our indicators are based on the assumption that public procurement is least prone to corruption where the process is open and competitive and utilises procurement regulations which set a number of maxims intended to ensure openness. Where the procurement process deviates from these maxims, we suggest that this constitutes a 'red flag', as it may indicate a deliberate manipulation of the process by a corrupt public official (or network of public and private actors) to favour a particular company and gain a private advantage (Fazekas & Kocsis, 2017). Note that it does not necessarily indicate that corruption has occurred; rather, we characterise it as a corruption *risk* indicator. Our work, therefore, also contributes to a growing literature which seeks to develop objective corruption indicators from administrative data around the world (Escresa and Picci, 2015, 2016; Cordis and Milyo, 2016), addressing the widely accepted shortcomings of

perceptions-based corruption indices (Andersson and Heywood, 2009; Foster, Horowitz and Méndez, 2012). The 'red flags' approach also has its critics (e.g. Kenny & Musatova, 2010) and our methodology explicitly addresses these concerns. First, we don't aggregate red flags into a single score or count the number of red flags which is often misleading indicating low governance capacity than corruption. Second, we only employ a small set of tested and validated red flags rather than the wide and often misleading array the literature criticises (Fazekas & Kocsis, 2017). Finally, we predominantly rely on indicators linked to competition and competitive outcomes which have been found to be superior to only process or inputs-based red flags.

In order to track displacement effects, our set of corruption risk indicators must cover a wide range of possible corruption techniques at different stages of the public procurement process as well as its outcomes. Given data constraints, we develop indicators characterising three stages of the tendering cycle: 1) the pre-bidding phase, when the tender is prepared and design choices are made; 2) the bidding phase, when the bids are submitted, evaluated, the award decision made and the winning bidder announced; and 3) the post-bidding stage, when the awarded contract is negotiated and the final contract signed by both parties. The indicators used and the typical corruption schemes proxied are highlighted in Table 2.

Table 2. Summary of corruption risk indicators used as dependent variable

Tendering phase	Indicator name	Indicator definition	Typical corruption scheme
Pre-bidding	Non-competitive procedure type	1=non-open procedure types** 0=open procedure types***	Awarding contract to connected firm without competition
Bidding	Single bid	1=1 bidder per contract 0=2 or more bidders per contract	Setting tendering terms which only one firm can satisfy
	Bidder number (trimmed)*	Bidder number (50+ bidders set at 50)	Organising a collusive ring of a few firms where the winner is pre-determined in advance.
	Repeat winner	1=supplier won at least 2 contracts in 1998-2014 0=supplier won only 1 contract in 1998-2014	Although there is a façade of competition, the same few well-connected firms keep winning contracts.
	Foreign supplier*	1=supplier is registered in a foreign country 0=supplier is registered in the country of buyer	Domestic firms with good local connections enjoy unfair treatment, e.g., receiving information through informal channels.

Post-bidding	Risky signature period	1=Time between award date and contract signature date is shorter than 14 days 0=Time between award date and contract signature date is longer than 14 days	Contract is signed very quickly, without substantive work on the exact contractual terms, laying the ground for incomplete or inadequate delivery without penalty.

*Note: *for these outcomes higher values indicate lower risk of corruption; ** non-open procedure types are the following: Direct Contracting, National/International Shopping, and Limited International Bidding; *** open procedure types are the following: International Competitive Bidding and National Competitive Bidding.*

The November 2003 regulatory change is the main independent variable, defined as a 0-1 binary variable taking the value of 0 if the project concept note approval date was before this date (control group) and 1 if it was after (treatment group). As there were other regulatory changes both before and after the 2003 change, we restricted the treatment and control groups to projects approved between January 1999 and September 2006, inclusive.

Descriptive statistics for all variables used in the analysis are in Appendix C.

RESULTS

First, we investigate hypothesis 1 (H1) regarding the intended effect of the intervention on corruption risks associated with a lack of competition in the bidding phase. The empirical evidence provides support for H1, using both the naïve comparison of group averages (Table 6) and matching (Table 3). As a result of the intervention, the share of tenders attracting only a single bidder decreases from 22.4% to 18.7% in the matched samples, while the average bidder number goes up from 4.5 to 5.0 (Table 3). Not only does the intensity of competition improve but the pool of bidders also widens: the share of repeat winners - i.e., companies who win a contract more than once, proxying incumbency – falls from 71.8% to 65.4%. Surprisingly, foreign winners' market share slightly decreases too, from 15.8% to 13.7%, which is contrary to H1 and alludes to H4. As corrupt deals are more difficult to conduct when there are many other companies watching and market entrants are challenging connected incumbents (Coviello and Gagliarducci, 2017; Fazekas and Kocsis, 2017), we consider competition-related corruption risk to decrease.

Second, we test H2 by looking for signs of corruption displacement which use alternative corruption techniques to restrict competition during the bidding phase (recall wider advertisement and easier bid submission represent the main treatment in the 2003 reform). Both simple comparisons and matching estimations lend support to our expectations that evasive responses are systemic: the use of non-competitive or closed procedure types goes up from 7.3% to 9.6% in the matched samples, consistent with H2a, while the frequency of high-risk signature periods also increases from 25.0% to 29.4%[xi], consistent with H2b (Table 3). As this phase of the process is not directly affected by the

2003 intervention, the increased use of these techniques suggests that corrupt actors respond to a direct curtailment of their corruption opportunities by moving on to other tactics that are not affected: they either limit competition prior to advertisement (pre-bidding) or engage in corruption during the contract signature period (post-bidding).

Table 3. Matched comparisons of treatment and control groups, contracts above 25,000 USD, goods and works, 2003-14

	closed procedure type	single bid	bidder number (trimmed)	repeat winner	foreign supplier	risky signature period
control	7.3%	22.4%	4.50	71.8%	15.8%	25.0%
treatment	9.6%	18.7%	5.04	65.4%	13.7%	29.4%
diff(treatment - control)	**2.3%***	**-3.8%***	**0.54***	**-6.4%***	**-2.0%***	**4.5%***
95% c.interval-lower bound	1.4%	-6.8%	0.12	-7.9%	-3.1%	3.0%
95% c.interval-upper bound	3.2%	-0.8%	0.95	-5.0%	-0.9%	5.9%
N control	7,515	1,404	1,404	7,515	7,515	7,515
N treatment	7,515	1,404	1,404	7,515	7,515	7,515
matching variables						
log contract value	Y	Y	Y	Y	Y	Y
main sector	Y	Y	Y	Y	Y	Y
year dummies	Y	Y	Y	Y	Y	Y
country prior DV avg.	Y	Y	Y	Y	Y	Y
buyer prior DV avg.	Y	Y	Y	Y	Y	Y

** 5% significance level*

We investigate H3 which posits that untreated corruption tactics would be more intensively exploited following the targeted anti-corruption intervention. To test this hypothesis, we split the sample according to competitive and non-competitive procedures and perform matching separately on the sub-samples (Table 4 and Table 5). Comparing competitive and non-competitive sub-samples informs our hypothesis because the competitive procedure types - where wide advertisement and many bidders are expected - are treated by the 2003 intervention, while non-competitive procedure types are by and large unaffected. The comparison of the two matched sub-samples reveals support for our hypothesis. In competitive procedures, the share of tenders with a single bidder goes down (from 18.5% to 10%) while the bidder number goes up (from 4.6 to 5.5). In non-competitive procedures, single bidding drastically increases (from 67.3% to 81%) and number of bidders drops (from 1.7 to 1.4). In addition, the repeat winners' share of contracts decreases from 72.5% to 64.5% in competitive procedures; the change is insignificant and only half as big for non-competitive procedures (even though it must be noted that the small sample size for the non-competitive subsample makes confidence intervals large). For foreign winners, there is no significant change in competitive procedures but a marked and significant drop in non-competitive procedures from 29.9% to 20.9%. Taken together, we observe further support for H1 as the most directly treated procedure types perform a lot better on competition-related risks as a result of the treatment. However, non-competitive procedures appear to be more intensively exploited in line with H3, resulting in further clustering of risks.

Table 4. Matched comparisons of treatment and control groups, competitive procedures only, contracts above 25,000 USD, goods and works, 2003-14

	single bid	bidder number (trimmed)	repeat winner	foreign supplier	risky signature period
control	18.5%	4.60	72.5%	14.6%	24.1%
treatment	10.0%	5.52	64.5%	13.6%	28.5%
diff(treatment - control)	**-8.5%***	**0.92***	**-8.0%***	**-1.1%**	**4.4%***
95% c.interval-lower bound	-11.2%	0.46	-9.5%	-2.2%	2.9%
95% c.interval-upper bound	-5.8%	1.39	-6.5%	0.1%	5.9%
N control	1,235	1,237	6,966	6,966	6,966
N treatment	1,235	1,237	6,966	6,966	6,966
matching variables					
log contract value	Y	Y	Y	Y	Y
main sector	Y	Y	Y	Y	Y
year dummies	Y	Y	Y	Y	Y
country prior DV avg.	Y	Y	Y	Y	Y
buyer prior DV avg.	Y	Y	Y	Y	Y

** 5% significance level*

Table 5. Matched comparisons of treatment and control groups, non-competitive procedures only, contracts above 25,000 USD, goods and works, 2003-14

	single bid	bidder number (trimmed)	repeat winner	foreign supplier	risky signature period
control	67.3%	1.70	63.2%	29.9%	36.8%
treatment	81.0%	1.43	58.5%	20.9%	41.3%
diff(treatment - control)	**13.7%***	**-0.27***	**-4.7%**	**-8.9%***	**4.6%**
95% c.interval-lower bound	4.4%	-0.51	-10.5%	-14.1%	-1.2%
95% c.interval-upper bound	23.0%	-0.03	1.0%	-3.8%	10.3%
N control	168	167	549	549	549
N treatment	168	167	549	549	549
matching variables					
log contract value	Y	Y	Y	Y	Y
main sector	Y	Y	Y	Y	Y
year dummies	Y	Y	Y	Y	Y
country prior DV avg.	Y	Y	Y	Y	Y
buyer prior DV avg.	Y	Y	Y	Y	Y

** 5% significance level*

We consider H4 by exploring who benefits and who loses as a net result of the intended and unintended impacts. The validity of this hypothesis will point at the likely total net effect of the intervention, albeit there are different ways of trading off positive and negative effects, so we cannot reach a definitive conclusion. All matching estimations

suggest that the dominance of incumbents falls (from 71.8% to 65.4%, see Table 3), increasing the total pool of successful bidders. However, foreign firms - which are less connected on average – do not benefit from this broadening of access: their total market share slightly decreases (from 15.8% to 13.7%, see Table 3). In competitive procedure types, some of which are explicitly designed for facilitating international competition (i.e., International Competitive Procedure), no change is observed. These findings contradict H4, suggesting that broader access favours those who were more readily able to exploit connections (i.e., domestic firms) at the expense of those who were more likely to lack connections (i.e., foreign firms).

Thus, taken together, the net benefit of the reform may be nil: the decrease in competition-related corruption risks in the bidding phase is offset by increasing risks in the pre- and post-bidding phases. These effects in opposite directions appear to be of comparable size, although measurement error prevents us from reaching precise conclusions. Nevertheless, the overall decreasing share of foreign firms further supports a conclusion that the net effect may be minimal.

DISCUSSION AND CONCLUSIONS

Analysing a unique large-scale contracts database covering virtually all developing and transition economies, we investigated the direct and displacement effects of a donor anti-corruption reform. Using a tightly coupled matching estimator, we find that the reform – which seeks to broaden access through targeting tender advertisement and submission - is effective in a direct sense: it decreases corruption risks due to low competition, with

the share of single bidding decreasing from 22.4% to 18.7% and the average bidder number increasing from 4.5 to 5.0. It also broadens the pool of bidders that win contracts, with the share of repeat winners falling from 71.8% to 65.4%.

However, we also observe strong displacement effects which may cancel out the direct positive impacts. These evasive strategies follow two main logics: i) substituting corruption techniques in more tightly-controlled areas with corruption techniques in less tightly-controlled areas; and ii) exploiting remaining weaknesses in the control framework more intensively. Corrupt buyers switch to non-treated non-competitive procedure types, whose prevalence increases from 7% to 10% while risky signature periods also become more common, increasing from 25.0% to 29.4%. Moreover, the already high-risk (but non-treated) non-competitive procedure types appear to be more intensely exploited, with the share of single bidding increasing from 67.3% to 81.0% and average number of bidders dropping from 1.7 to 1.4. While the net welfare effect remains unclear, we see foreign companies lose out (their market share drops by 2 percentage points), while domestic companies – which tend to be better connected – fare better in the market (repeat winning falls). This suggests that the displacement effects may cancel out the observed direct benefits.

Our analysis suggests that even a well-designed, thoroughly implemented and seemingly successful anti-corruption intervention may face difficulty in achieving overall improvements in corruption control, if corrupt actors are able to engage in evasive strategies. The reform is not ineffective. It closes some loopholes and indeed some corrupt actors comply with the new rules in the targeted domain - in line with theories of corruption control which suggest that increasing the expected risk of detection can deter

corruption. However, other actors simply adapt their corrupt behaviour to the new regulatory conditions, finding new ways to control administrative procedures and maintain their access to illicit private gains – or, in other words, finding and exploiting remaining loopholes. The procurement process offers many ways of manipulating the process to steer a contract to a favoured bidder or solicit kickbacks. The study of displacement effects helps us understand how it is that, despite so many countries adopting and implementing good policies and IT tools to support open and fair competition for government contracts, this domain continues to be plagued by corruption.

One crucial limitation of our analysis remains, which future research may address: we lack sufficiently detailed data to assess the impact of the intervention on the contract implementation phase, to which at least some of the corrupt activity is likely to shift. In this sense, our study only provides a lower bound estimate of the total direct and indirect effects. A fuller set of potential strategic responses could be observed with more detailed data on contract implementation. Moreover, such data would allow us to analyse key assumptions of the corruption control literature, including whether reducing discretion improves social welfare outcomes. An alternative view is that bureaucratic discretion may be a condition for adaptive and iterative change in otherwise sclerotic systems. (Rasul & Rogger, 2018).

Our findings lend themselves to policy advice. From the outset, reformers should plan for likely evasive strategies, prepare to monitor behaviour beyond the target area, and be ready to adapt programming as necessary. Given that rich and real-time public procurement datasets are increasingly available, a staged or iterative approach to reform

may allow for displacement effects to be observed and addressed one by one, until most major loopholes are closed. Closing most major loopholes simultaneously necessitates that the minimum effective reform package is rather comprehensive: targeting only one phase of the procurement cycle is unlikely to produce sustainable impact. In essence, we argue that increasing the cost of doing corruption is only likely to impact the level of corruption if the cost goes up systematically across the whole policy area; otherwise, the availability of surrogate corruption techniques with similar costs enable corruption displacement rather than genuine corruption reduction.

More broadly, our results invite scepticism about the framing of anti-corruption as a game of 'whack-a-mole' or even chess (Sparrow, 2019), with 'corruption hunters' valiantly closing loopholes while corrupt actors demonstrate agility and persistence in creating new ones. One important strand of the public policy literature suggests that the problem may be deeper. Policy interventions may lead to paradoxes and unintended effects because their design unwittingly *exacerbates* the kinds of motivations and behaviour that they are intended to curb (Margetts et al. 2010). For example, critics of new public management theory argue that efforts to measure performance often undermine the results they intend to achieve by prizing targets and metrics that do not reflect the broader objectives of reform (see, for example, Hood 2002). Reforms can also disadvantage particular groups: in public procurement, for example, e-procurement might lower entry costs for the average bidder but increase them for small firms that lack internet access and skills (Lewis-Faupel *et al.*, 2016), undermining key aims of economic development related to promoting enterprise and diversification.

The argument that economic theories of regulation are fundamentally flawed because of such unintended consequences is also found in the corruption literature. Philp suggests that efforts to specify accountability in democratic systems, for example, often undermine wider concepts of integrity (Philp, 2001, 2009). Heywood argues that the introduction of accountability mechanisms in the UK public service has undermined core values intrinsic to the public service ethos (Heywood, 2010), while Osrecki postulates that initiatives that demand transparency, accountability, and compliance run the risk of creating an inflexible and ineffective work-to-rule regime that may stifle adaptability (Osrecki, 2015). In the Uganda healthcare experiment mentioned in our introduction, even the initial benefits of the anti-corruption reform – which were not sustained - came at the cost of undermining the morale of healthcare workers, with potentially negative consequences in unforeseen areas.

Despite efforts to reform its procurement procedures, the World Bank only achieved a small change in its most direct goal (i.e. increase competitive bidding) and had no (or even negative) effect on broader objectives such as lowering the share of contracts going to 'connected' or 'insider' firms. These findings are consistent with the hypotheses that officials evaded the constraints of reformed procedures by more extensively using procedures which they could still manipulate and more intensively exploit them. This is in line with a broader literature which indicates that anti-corruption reforms fail because they do not address the underlying political and social conditions that foster corruption, or the difficulty of motivating individuals to overcome an entrenched collective action problem (Persson et al 2013; Rocha Menocal 2014; Marquette & Peiffer 2015). Technical interventions might not represent the best way to tackle systemic corruption, instead

strategies should target the root causes of corruption and contribute to building a culture of integrity.

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APPENDICES

Appendix A. Description of datasets

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.

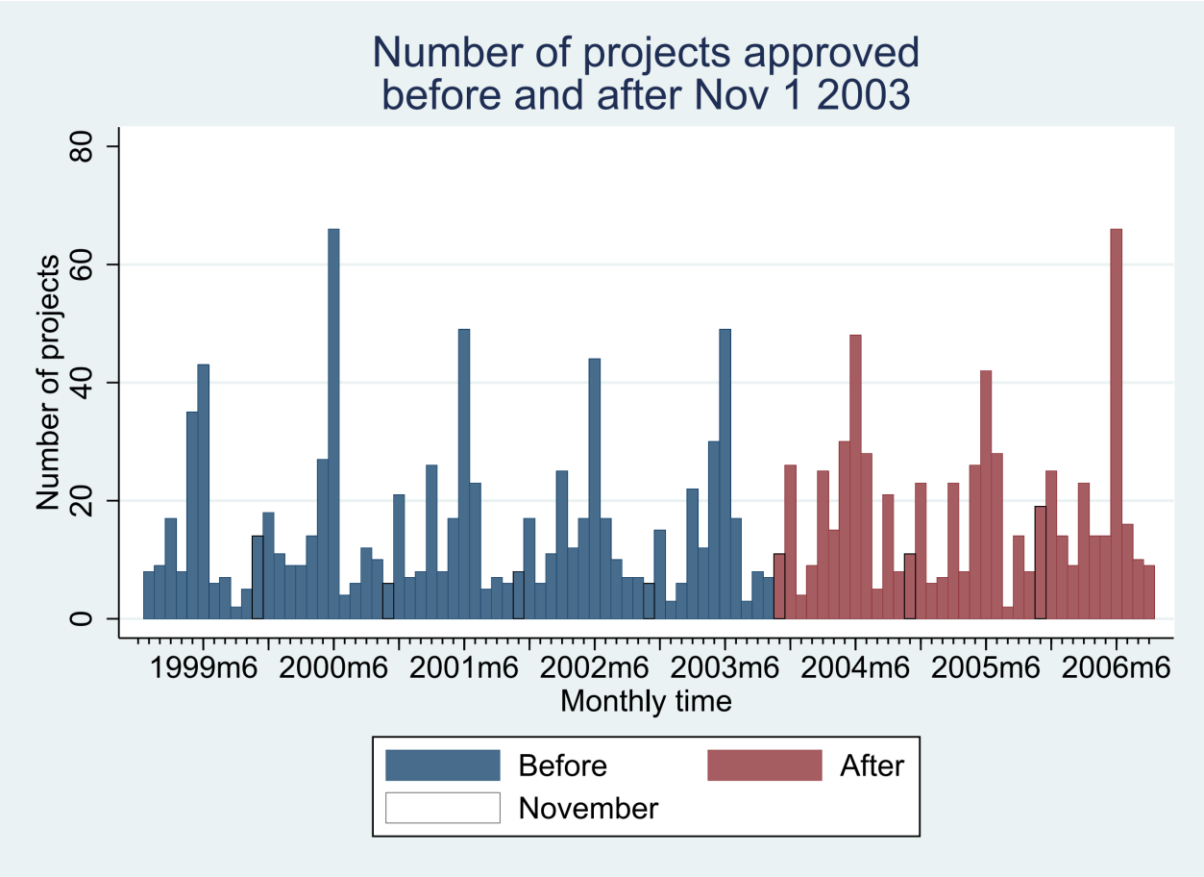
The combined complete datasets can be downloaded at http://www.govtransparency.eu/index.php/2018/02/13/data-publication-foreign-aid-of-world-bank-europeaid-and-iadb/

Appendix B. Evidence for the absence of manipulation around the threshold

The main question in assessing potential manipulation around the threshold is whether there was gaming in project approvals, i.e. artificially postponing or bringing forward the approval in order to fall under the desired regulations. If actors follow such practices, our identification strategy would not be credible as we could not assume a quasi-random timing of project approvals around the intervention.

To test whether there was gaming we first plotted the number of projects launched monthly in the years before and after the November 2003 intervention (Figure 2) beginning with the latest and ending with the next intervention in WB regulations. We can see a strong seasonality in this graph with peaks in June each year that is the last month of a fiscal year at World Bank. According to this graph there was no extraordinary pattern around November 2003.

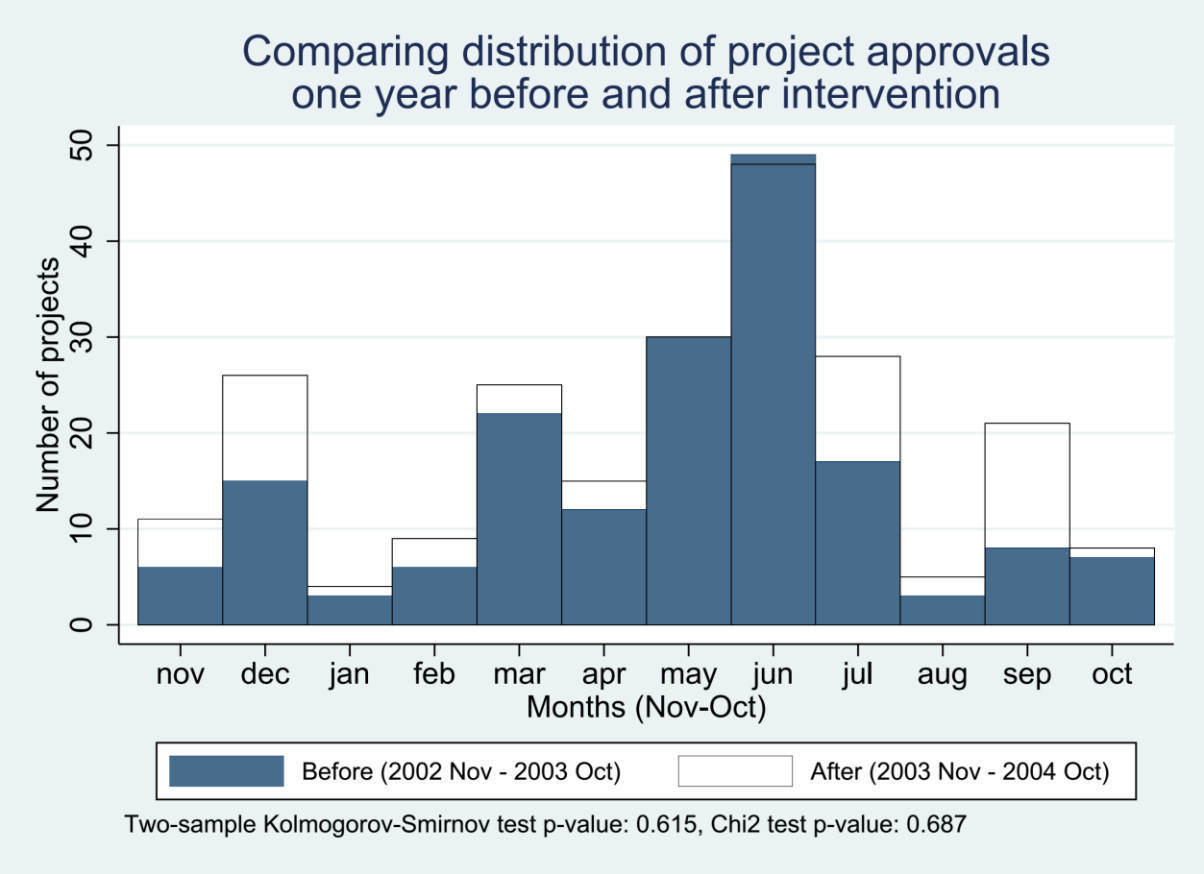
Figure 2. Seasonal distribution of project approvals by months (Jan 1999 - Sep 2006)



Source: (David-Barrett et al., 2017)

We also made some formal tests to make sure there is no irregular pattern in the timely distribution of project approvals around the intervention. On Figure 3, we show the overlapping histograms of project approval dates monthly for the years preceding and following Nov 2003. The two distributions look very much alike and we did not find any significant differences between them with the two-sample Kolmogorov-Smirnov test and simple chi2 tests, either. We also tested the differences in distributions for broader time periods and for periods with November in the middle and we also did not find any significant differences in these versions.

Figure 3. Overlapping histograms of project approvals (monthly) for the years preceding and the following year of Nov 1 2003



Source: (David-Barrett et al., 2017)

Appendix C. Descriptive statistics

Table 6. Simple, un-matched comparisons of treatment and control groups, contracts above 25,000 USD, goods and works, 2003-14

	closed procedure type	single bid	bidder number (trimmed)	repeat winner	foreign supplier	risky signature period
control	8.9%	21.7%	4.09	70.9%	18.0%	25.2%
treatment	13.3%	18.2%	4.84	65.9%	18.0%	28.6%
diff(treatment - control)	***4.5%****	***-3.6%****	***0.74****	***-5.0%****	***0.0%***	***3.3%****
95% c.interval-lower bound	3.8%	-4.8%	0.60	-6.0%	-0.9%	2.3%
95% c.interval-upper bound	5.2%	-2.3%	0.88	-4.0%	0.9%	4.3%
N control	15,086	12,610	12,610	15,086	15,086	15,086
N treatment	15,204	5,778	5,778	15,204	15,204	15,204

** 5% significance level*

Table 7. Descriptive statistics of the dependent variables used in the estimations, contracts above 25,000 USD, goods and works, 2003-14

Variable	Obs.	Mean	Std. Dev.	Min	Max
closed procedure type	30,290	0.11	0.31	0	1
single bid	18,388	0.21	0.40	0	1
bidder number (trimmed)	18,388	4.33	4.52	1	50
repeat winner	30,290	0.68	0.46	0	1
foreign supplier	30,290	0.18	0.38	0	1
risky signature period	30,290	0.27	0.44	0	1

Table 8. Descriptive statistics of the independent variables used in the estimations, contracts above 25,000 USD, goods and works, 2003-2014

Variable	Obs.	Mean	Std. Dev.	Min	Max
Log of contract value	30,290	13.81	1.52	10.47	21.48
ANB-level closed procedure prevalence before intervention	17,797	0.04	0.16	0	1
Country-level closed procedure prevalence before intervention	29,462	0.12	0.18	0	1
ANB-level single bidding before intervention	17,797	0.09	0.24	0	1
Country-level single bidding before intervention	29,462	0.24	0.22	0	1
ANB-level bidder number (trimmed) before intervention	17,797	3.78	1.68	1	26
Country-level bidder number (trimmed) before intervention	29,462	4.60	2.13	1	17
ANB-level repeat winner rate before intervention	17,797	0.72	0.18	0	1
Country-level repeat winner rate before intervention	29,462	0.80	0.16	0	1
ANB-level foreign supplier rate before intervention	17,797	0.16	0.19	0	1
Country-level foreign supplier rate before intervention	29,462	0.24	0.20	0	1
ANB-level risky signature period prevalence before intervention	17,797	0.25	0.21	0	1

Country-level risky signature period prevalence before intervention	29,462	0.25	0.21	0	1

Table 9. List of sectors and the number of contracts per sector in the sample, contracts above 25,000 USD, goods and works, 2003-14

Sector name	Freq.	Percent	Cum. Percent
Agriculture	2,935	9.69	9.69
Education	3,209	10.59	20.28
Finance	3,809	12.58	32.86
Energy & mining	437	1.44	34.3
Finance	5,304	17.51	51.81
Industry and trade	763	2.52	54.33
Info & communication	281	0.93	55.26
Public admin, Law	3,463	11.43	66.69
Transportation	5,319	17.56	84.26
Water, sanitation, flood protection	4,769	15.74	100
Total	30,289	100	

Table 10. List of countries and the number of contracts per country in the sample, contracts above 25,000 USD, goods and works, 2003-14

Country name	Freq.	Percent	Cum. Percent
Afghanistan	427	1.41	1.41
Africa	298	0.98	2.39
Albania	329	1.09	3.48
Algeria	18	0.06	3.54
Angola	56	0.18	3.72
Argentina	373	1.23	4.96
Armenia	314	1.04	5.99
Azerbaijan	272	0.9	6.89
Bangladesh	1,138	3.76	10.65
Barbados	2	0.01	10.65
Belarus	195	0.64	11.3
Belize	6	0.02	11.32
Benin	128	0.42	11.74
Bhutan	48	0.16	11.9
Bolivia	119	0.39	12.29
Bosnia&Herzegovina	723	2.39	14.68
Brazil	373	1.23	15.91
Bulgaria	459	1.52	17.42
Burkina Faso	190	0.63	18.05
Burundi	199	0.66	18.71
Cambodia	205	0.68	19.39
Cameroon	40	0.13	19.52
Cape Verde	34	0.11	19.63
Caribbean	11	0.04	19.67

Central African Republic	14	0.05	19.71
Central America	10	0.03	19.75
Central Asia	10	0.03	19.78
Chad	90	0.3	20.08
Chile	13	0.04	20.12
China	1,611	5.32	25.44
Colombia	127	0.42	25.86
Comoros	24	0.08	25.94
Congo	80	0.26	26.2
Costa Rica	28	0.09	26.29
Cote d'Ivoire	1	0	26.3
Croatia	355	1.17	27.47
Dem. Rep. of the Congo	369	1.22	28.69
Djibouti	97	0.32	29.01
Dominica	7	0.02	29.03
Dominican Republic	117	0.39	29.42
East Asia and P	45	0.15	29.56
Ecuador	36	0.12	29.68
Egypt	197	0.65	30.33
El Salvador	74	0.24	30.58
Eritrea	91	0.3	30.88
Ethiopia	299	0.99	31.87
Europe and Cent	3	0.01	31.88
Gabon	8	0.03	31.9
Gambia	61	0.2	32.1
Georgia	527	1.74	33.84
Ghana	468	1.55	35.39

Grenada	82	0.27	35.66
Guatemala	178	0.59	36.25
Guinea	176	0.58	36.83
Guinea-Bissau	28	0.09	36.92
Guyana	45	0.15	37.07
Haiti	78	0.26	37.33
Honduras	212	0.7	38.03
Hungary	12	0.04	38.07
India	1,690	5.58	43.64
Indonesia	451	1.49	45.13
Iran, Islamic Republic of	460	1.52	46.65
Iraq	327	1.08	47.73
Jamaica	20	0.07	47.8
Jordan	71	0.23	48.03
Kazakhstan	92	0.3	48.34
Kenya	154	0.51	48.84
Kiribati	3	0.01	48.85
Kosovo	37	0.12	48.98
Kyrgyzstan	225	0.74	49.72
Lao People's Dem. Republic	208	0.69	50.41
Latin America	2	0.01	50.41
Latvia	9	0.03	50.44
Lebanon	224	0.74	51.18
Lesotho	105	0.35	51.53
Liberia	23	0.08	51.6
Lithuania	51	0.17	51.77
Macedonia, Form. Y. Rep.	216	0.71	52.49

Madagascar	341	1.13	53.61
Malawi	166	0.55	54.16
Maldives	12	0.04	54.2
Mali	134	0.44	54.64
Mauritania	211	0.7	55.34
Mexico	361	1.19	56.53
Moldova, Republic of	273	0.9	57.43
Mongolia	156	0.52	57.95
Montenegro	60	0.2	58.14
Morocco	68	0.22	58.37
Mozambique	291	0.96	59.33
Nepal	480	1.58	60.91
Nicaragua	625	2.06	62.98
Niger	136	0.45	63.43
Nigeria	777	2.57	65.99
OECS Countries	8	0.03	66.02
Pakistan	445	1.47	67.49
Panama	35	0.12	67.6
Papua New Guinea	135	0.45	68.05
Paraguay	47	0.16	68.2
Peru	232	0.77	68.97
Philippines	330	1.09	70.06
Poland	52	0.17	70.23
Red Sea and Gul	2	0.01	70.24
Romania	475	1.57	71.81
Russian Federation	658	2.17	73.98
Rwanda	136	0.45	74.43

Saint Kitts and Nevis	23	0.08	74.5
Saint Lucia	54	0.18	74.68
Saint Vincent&Grenadines	39	0.13	74.81
Samoa	52	0.17	74.98
Sao Tome and Principe	8	0.03	75.01
Senegal	269	0.89	75.9
Serbia	296	0.98	76.87
Seychelles	1	0	76.88
Sierra Leone	176	0.58	77.46
Slovakia	3	0.01	77.47
Solomon Islands	3	0.01	77.48
South Africa	17	0.06	77.53
South Sudan	61	0.2	77.74
Sri Lanka	84	0.28	78.01
Sudan	23	0.08	78.09
Syrian Arab Republic	2	0.01	78.1
Tajikistan	393	1.3	79.39
Thailand	26	0.09	79.48
Timor-Leste	139	0.46	79.94
Tonga	13	0.04	79.98
Trinidad and Tobago	20	0.07	80.05
Tunisia	203	0.67	80.72
Turkey	176	0.58	81.3
Uganda	271	0.89	82.19
Ukraine	189	0.62	82.82
United Rep. of Tanzania	284	0.94	83.75
Uruguay	47	0.16	83.91

Uzbekistan	196	0.65	84.56
Venezuela	4	0.01	84.57
Viet Nam	3,828	12.64	97.21
West Bank and Gaza	171	0.56	97.77
Yemen	456	1.51	99.28
Zambia	2019	0.72	100.00
Total	30,290	100	

Appendix D. Goodness of fit for propensity score matching

Figure 4. Comparison of propensity scores in the control and treatment groups

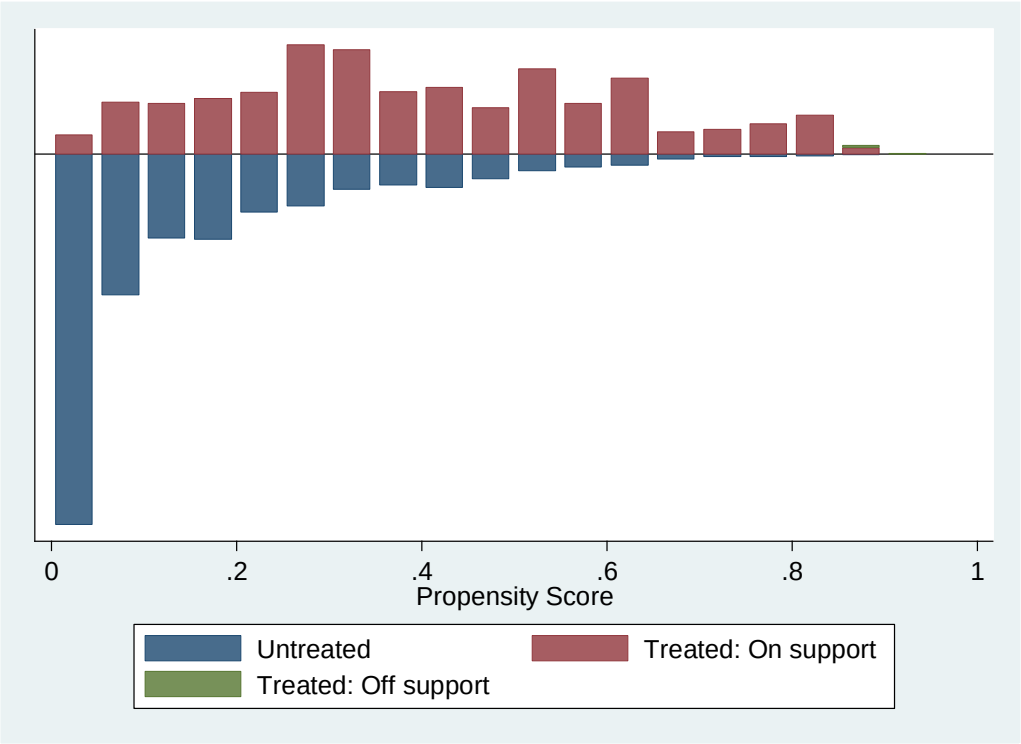


Figure 5. Variable level balance in the matched and unmatched comparisons

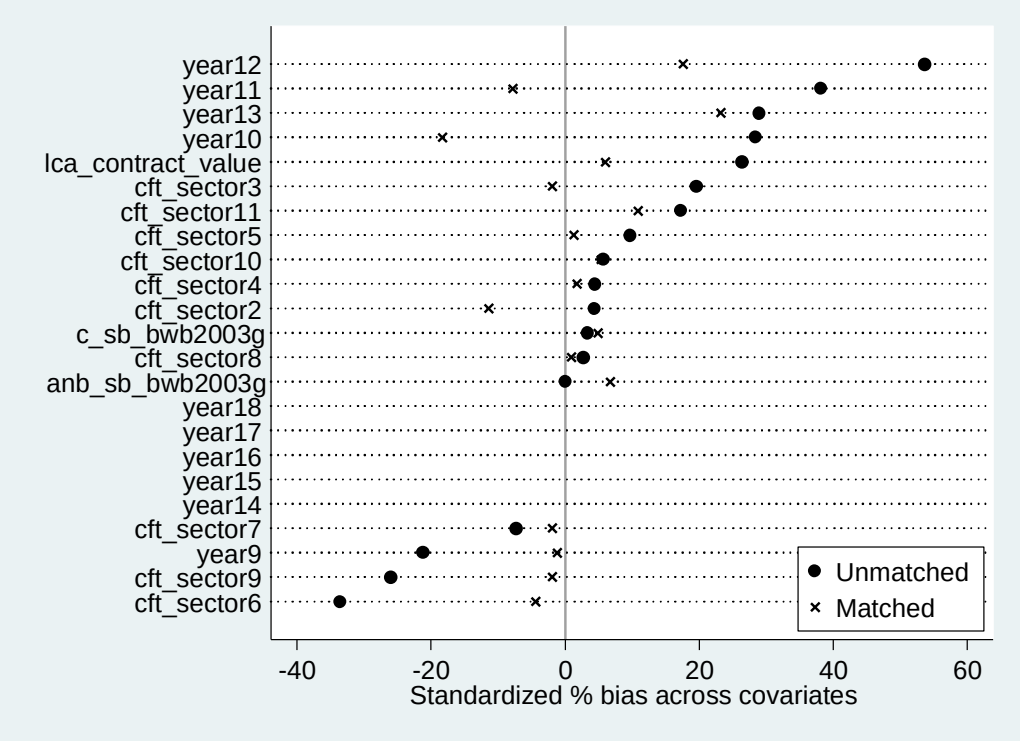


Table 11. Summary of balances before and after matching

Sample	*Ps R2*	*LR chi2*	*p>chi2*	*Mean Bias*	*Med. Bias*	*B*	*R*	*% Var*
Unmatched	0.25	1729.64	0	18.4	18.4	140.9*	0.63	67
Matched	0.021	81.15	0	7.1	5.1	33.8*	2.33*	100

[i] The final contract implementation phase is not covered by our indicators as data is only available on project level rather than contract level which makes our identification strategy ineffectual.

[ii] This assumption is supported, among others, by research by Coviello & Gagliarducci (2017) which suggests that local firms have an advantage and benefit from political connections. While some foreign firms also have good political connections, it seems plausible to assume that, on average, foreign firms may be at a disadvantage in terms of local links, especially those without a local subsidiary to support personal ties.

[iii] The alternative matching algorithm we considered is Coarsened Exact Matching which delivers somewhat tighter matches but shrinks samples by about 1/3. We opted for propensity score matching as it allows for greater flexibility and larger samples.

[iv] While World Bank used fiscal years for accounting purposes beginning in July and ending with June the next year, we rather use calendar years for comparability with other scholarship.

[v] See Appendix 1 of World Bank Procurement Guidelines: http://bit.ly/2wuj2a9.

[vi] 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).[vi] 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.

[vii] Not observing post-review contracts and assuming that at least some strategic gaming of the prior review vs post-review categories is possible imply that a potentially important displacement effect is not accounted for by our dataset and analysis.

[viii] The full dataset is downloadable at http://www.govtransparency.eu/index.php/2017/05/22/data-publication-world-bank-public-procurement-data-for-fiscal-years-1998-2013/.

[ix] Although in theory the borrower may request a switch to the new rules in an already ongoing project and the Bank may agree, the World Bank procurement expert we interviewed told us that, "Most Borrowers and Bank staff would rather not go through a formal restructuring if the only modification is the change of procurement rules" (email correspondence with World Bank procurement specialist, 18 May 2017).

[x] A key concern is whether the new or old regulations are applied when additional financing takes place (i.e. project extension), which occurs in about 25% of projects. Although the new regulations apply by default, most Borrowers request to remain with the old rules and the Bank has approved these requests in all cases (email correspondence with World Bank procurement specialist, 18 May 2017).

[xi] Recall, too-short signature periods suggest that the contract was not properly negotiated paving the way for corrupt contract enforcement and monitoring. The treatment did not make contract signature electronic and the matching balances the two samples by contract value and product type, hence our preferred corruption risk-related explanation is the most plausible.