Local Elections and Corruption during Democratization: Evidence from Indonesia

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Abstract

In this paper we ask whether the direct election of the local government increases accountability and decreases corruption. In order to identify the causal effect of direct elections, we exploit the gradual introduction of local elections in Indonesia and a novel dataset of corruption events that covers all districts during the period 1998-2008. We find that direct elections increase the number of corruption crimes by about half the pre-election average. We also find that embezzlement practices dominate all other types of corruption activities.

Key words: Political Decentralization; Political Institutions; Elections; Corruption.

JEL Classification codes: D02, D72, D73, H83, K4, 017, P16.

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1 Introduction

In this paper we ask whether a specific political institution, namely the appointment of the executive through direct elections, causes less or more corruption than the appointment through indirect elections.

In order to identify the causal effect of direct elections, we exploit the gradual introduction of a legislative reform across Indonesian districts. The legislative change provides the local electorate with the power to elect the district head directly rather than through representation by the local parliament. This institutional change is salient because the district head is responsible for the provision of local public goods. Elections are widely regarded as a powerful disciplining and selection device and therefore constitute a corruption-reducing mechanism which is well worth evaluating. Indeed, the reform was introduced primarily because many observers had reported widespread vote-buying practices between district heads and district parliaments.

Our measure of corruption is based on novel data on corruption prosecutions in Indonesia. Our dataset provides several advantages relative to the typical corruption measures used in the literature. First, corruption prosecutions constitute "hard" evidence of corruption, which makes them more reliable than, e.g., experts' surveys or perceived corruption measures. Second, they cover the entire universe of Indonesian districts, and therefore provide better coverage than typical measures generated by sectorial studies or randomized interventions. This is especially attractive because Indonesia is the fourth most populous country in the world. Third, they provide an encompassing view of corruption activities: there is no possibility of mis-measurement due to, say, officials switching from one type of corruption to another.[1] Fourth, they provide a long time span: more than ten years of data in a newly

[1] One advantage of this measure of corruption is that it is likely to go beyond, e.g., petty corruption. Over-focus on petty corruption may be instead the problem with other corruption measures like cross-checking (Banerjee, Mullainathan, and Hanna (2012):47). It is also less likely to be biased by media reports, as households' perception measures can be expected to be to a large degree. In addition, it is likely to be more responsive to changes in actual corruption than, again, households' perceptions: Olken (2009) finds that, although the correlation between actual and perceived corruption is positive, "increasing the missing expenditure measure by 10 percent is associated with just a 0.8 percent increase in the probability a villager believes that there is any corruption in the project" (p.951). Finally, differently from perception-based measures, our corruption measure also includes details about the type of corruption observed and so provides a chance to evaluate its

democratized developing country implies room for the study of several institutional features.

Using the typology in Persson and Tabellini (2004), the introduction of the direct election of the district government constitutes a change in the form of government, from a parlamentarian to a presidential system. A commonly held assumption (Persson and Tabellini (2004) and references therein) is that local deputies have better information on the government than citizens do. If local deputies were perfectly accountable to the citizens or had similar preferences, then a shift to a presidential form of government would unambiguously decrease its accountability to the citizens. However, local deputies are unlikely to be held perfectly accountable to the citizens. In addition, they may be reasonably thought to value eventual private gains from public office as much as government members may do. Thus, collusion between deputies and government members under the parlamentarian system may imply that a shift to a presidential form of government increases its accountability to the citizens (Persson, Roland, and Tabellini (1997)).

Our paper contributes to the empirical literature on political institutions and economic outcomes. Cross-country evidence on the relationship between form of government and corruption has been inconclusive. Persson, Tabellini, and Trebbi (2003) find some evidence that presidential systems are associated with less corruption, but the relationship holds only for "good democracies". In contrast, Kunicova and Rose-Ackerman (2001) find that presidential systems are associated with more corruption. During recent years researchers have tried to complement cross-country evidence with within-country studies whenever specific contexts provided convincing identification strategies. So far, attention has been mainly paid to term limits (Besley and Case (1995), Dal Bo' and Rossi (2011), and Ferraz and Finan (2011)) and information (Besley and Burgess (2002), Ferraz and Finan (2008)). The only papers some close to ours concern the introduction of village elections in China (Zhang, Fan, Zhang, and Huang (2004), Gan, Xu, and Yao (2007), Luo, Zhang, Huang, and Rozelle (2007), Shen and Yao (2008), Martinez-Bravo, Miquel, Qian, and Yao (2012)). The main differences between the two contexts concern: the country-level political system (non-democracy in China, young democracy in Indonesia); the level of election (village in China, district in Indonesia, where

efficiency implications and discuss policies aimed at reducing it.

districts typically include about 100 villages); the pre-election selection mechanism (appointment from upper-government tiers in China, elections from local parliament in Indonesia). While the difference in the nature of the country-level political system matters primarily for the external validity of the results, the differences in terms of administrative level of the elections and in terms of origin of the shift to direct elections matters directly for the interpretation of the results and the relationship with the rest of the literature. First of all, since villages are very small units, one can interpret the effects of Chinese local elections as the outcome of an increase in village-level monitoring and therefore relate to other studies like, for example, Reinikka and Svensson (2004) and Björkman and Svensson (2009). On the contrary, the relationship linking elections to citizens' monitoring is not as straightfoward in case of elections covering 100 or more villages. Second of all, a shift from appointment from upper government tiers may be very different from a shift from appointment from local parliament, particularly in terms of leaders' selection.

Our paper also contributes to the recent micro-literature on corruption (see Svensson (2005), Banerjee, Mullainathan, and Hanna (2012), Olken and Pande (2012) and Zitzewitz (Forthcoming) for some excellent surveys) by providing evidence on one of its institutional determinants and by documenting the prevalence of embezzlement over other types of corruption, such as, among others, bribes. Surprisingly, we find that the introduction of direct elections increases corruption, rather than decreasing it.

2 Context and theoretical framework

The administrative structure in Indonesia is composed of three layers: the central government, the provinces, and the districts. The district administration is responsible for the provision of all local public goods. It is divided into a district parliament (DPRD) and a district government (composed of a district head and a vice). The district parliament is elected (since 1999). Until 2004 the district parliament appointed the district head. Media and many policymakers observed that the power to appoint and dismiss the district head provided to the local parliaments had favored collusion between the two and had led to widespread vote-buying

and corruption. Therefore, in 2004 the central government passed a law shifting the power to elect the district head from the local parliaments to the local electorates. In the rest of the paper we will refer to the appointment of district heads by citizens through the election of local parliaments as *indirect elections*, whereas the appointment of district heads by citizens without intermediation will be referred to as *direct elections*.

There are two features of the Indonesian context that are relevant for our purposes. First, under indirect elections the local government should be accountable to the citizens through political representation in the local parliament, i.e., the local government is accountable to the local parliament and the local parliament is accountable to the citizens. However, the elections for the local parliament are over-shadowed by the national elections since the two take place at the same time. Thus, the local government may effectively be accountable only to the local parliament under indirect elections, and to the citizens under direct elections. Second, one of the main differences between the central and the local governments is that the latter have almost no authority to set tax rates. The average share of district revenues arising from own sources, like taxes on economic activities, is only about 15 percent.[2] The rest of the revenue comes from transfers from the central government. The local parliament can perfectly observe these transfers since it must approve the annual budget. In this context it seems reasonable to assume that the local parliament has as informational advantage over the citizens.[3]

If local deputies had the same preferences as the citizens, then under direct elections we would expect the district head to exploit the asymmetric information vis-a-vis the citizens and divert more resources for private purposes than she would have under indirect elections (Besley (2005), Besley and Smart (2007), Gadenne (2010)). However, once we allow local deputies' preferences to differ from the citizens' and, in addition, we allow them to collude with the district head, things become much more complicated. Apart from the theoretical political economy literature mentioned in the previous section, there are some relevant contributions stemming from the mechanism design literature. Baliga and Sjöström (1998) suggest a moral

[2] Author's tabulations based on the 1995-2006 budget data from the Ministry of Finance.

[3] Gadenne (2010) provides evidence from Brazil that whenever the revenues of local governments are primarily given by transfers rather than taxes, the local government performs strictly worse.

hazard mechanism in the Industrial Organization literature that, applied in this context, would suggest that the district head would divert more resources under direct elections than under indirect elections. In contrast, however, Mookherjee and Tsumagari (2004) focus on an adverse selection mechanism and conclude that direct elections would unambiguously lead to less divertion than indirect elections.[4]

3 Construction of the corruption database

Our measure of corruption is based on documents on corruption cases prosecuted or coordinated by the General Attorney Office (AGO).

The AGO is "responsible for investigating certain types of cases, bringing prosecutions, playing an intermediary position between the investigation process and the trial process, and ensuring the enforcement of judicial orders and decisions of final and conclusive effect. (..) it is the institution that determines whether a case should proceed to Court based on admissible evidence" (General Attorney Office (2011):7).

Following a recent improvement in transparency, the AGO has made available a description of virtually all corruption cases in Indonesia. The documentation includes a description of the case, a description of the accusation, the district attorney office prosecuting the practice, the stage of the prosecution, and several demographic characteristics of the person accused. In order to operationalize the information included in this documentation we extract location of the corruption event, date (or time frame) of the corruption event, whether the case concerns primarily the public or private sphere, what sector the case concerns about, and what type of criminal case.

Table 1 includes some descriptive statistics on the corruption cases that we coded. Out of an initial sample of 1,365 corruption cases, we drop 33 cases due to missing or unclear location, 114 cases due to being located in provinces with special status,[5] 247 cases due to

[4] Again, Mookherjee and Tsumagari (2004) discuss their theoretical framework relative to the IO literature. Hence, this is our interpretation of their result in this context. See Mookherjee (2006) for an excellent survey on decentralization and collusion within the mechanism design literature.

[5] They are Jakarta, Aceh, Papua, and Papua Barat (previously called Irian Jaya Barat).

missing time references, and 2 cases due to them dating back to years preceding 1998, which is when the first significant anti-corruption legislation became law.

The final sample consists of 985 cases for which we have (at least) location and time references. Among them, 212 (or 21 percent) span more than one year. In order to keep the descriptive statistics consistent with the econometric analysis, we duplicate these cases for each year in which they occured. The final dataset includes 1,396 corruption events: 133 are classified as concerning the private sphere, and 1,289 are classified as concerning (at least partially) the public sphere.[6]

We further decompose cases by type of corruption. Cases of embezzlement refer to instances where the suspect misuses or appropriates part or all the funds that the local government has placed in their care. The typical conflict of interest case refers to instances where the suspect allegedly sets up an auction that benefits some specific parties. Among the cases that recur frequently within the private sphere, we have hazard, which typically refers to fishermen using illegal devices (explosive) to fish, and illegal practice, which typically refers to lack of documents to carry out a private business or smuggling goods across the border.

The most common type of criminal case is embezzlement (75.6%), followed by conflict of interest (12.0%), fraud, distribution, extortion, bribery, illegal practice and hazard. Embezzlement is possibly even more dominant among cases concerning the public sphere (80.8%). Among cases concerning the private sphere it is the second most common type (26.3%) together with hazard, after illegal practice (38.3%).

The dominant role of embezzlement relative to other corruption activities is very interesting considering that the literature on corruption has largely focused on bribes notwithstanding the broad definition of corruption as "abuse of public office for private gain" (Olken and Pande (2012), Banerjee, Mullainathan, and Hanna (2012)). Our data suggest that the focus on bribes has come at the expense of embezzlement.[7] Overlooking embezzlement practices may be dangerous, because the relative economic theory (and therefore the policy implication) is likely to be different. For example, since bribes typically involve a private counterpart to the public of-

[6] With respect to the original 1,006 cases, 115 are classified as concerning the private sphere, and 891 are classified as concerning (at least partially) the public sphere

[7] See Reinikka and Svensson (2004), Reinikka and Svensson (2011) and Olken (2009) for two exceptions.

ficial, bribe-reducing schemes may provide incentives to private individuals to report requests for bribes. In contrast, embezzlement may not involve any private counterpart and therefore may require other strategies to tackle the issue.

In the rest of the analysis we focus on public cases for three reasons. First, corruption practices may be structurally different across the private and public sphere, which implies that an aggregate analysis may be misleading. Second, cases concerning the public sphere seem more appropriate for the study of the impact of direct elections on corruption. Third, cases concerning the public sphere constitute a large majority of the recorded cases.

Panel A disaggregates the public sphere cases by sector: the most common sector is administration (54.4%), followed by education (11.7%), food distribution and health care (5.5%), infrastructures (5.5%), and agriculture (4.0%).[8] The corruption-based ranking of the sectors is similar when we restrict our attention to embezzlement.

Panel B, Columns 1-6, shows the distribution of the corruption cases over time. We record very few cases during the years following the inauguration of the anti-corruption strategy (Law 16/1999, then modified in 2002). The proportion of (public sphere) cases per year increases progressively reaching 8.8% in 2005, 11.4% in 2006, 17.8% in 2007, and 20.4% in 2008, and decreasing to 13.4% in 2009, 4.4% in 2010, and 0.2% in 2011. The late decrease in corruption cases is most likely driven by the data collection process: since it typically takes typically between a few months and 2-3 years years to detect a corruption event,[9] it is not strange to observe relatively few cases for recent periods since we coded our sample in the autumn 2011. In the rest of the paper we will restrict our analysis to corruption events that took place during the period 1998-2008.

The next step is to generate a measure of corruption at the district-year level. Given the abundance of districts in Indonesia and the relatively long time span under investigation, we collapse the data at the district-year level and consider a simple binary variable indicating whether a district experienced one or more corruption events in a specific year. The second

[8] For 10.6% of the cases we lack enough information to pin down the exact sector. We feel that this lack of information is not serious enough to drop the observations though.

[9] The mean number of years to detect a crime is 2, while the median is 2.6 and the standard deviation is 2.3.

set of descriptive statistics in Panel B shows how the average number of corruption crimes per district evolves over time. Corruption cases increased steadily over the decade, yet declined in 2008, 2010 and 2011, again, presumably due to the data collection process.

The last two columns show the pattern of prosecutions, i.e., the number of prosecutions per district in a given year and the number of districts with at least one prosecution. The pattern suggest that the first prosecutions started much later than the first recorded corruption crimes. In particular, the timing of the first prosecutions is contemporaneous to the constitution of the Anti-Corruption Commission in 2004, which national and international observers praised for having boosted anti-corruption activities in Indonesia.

4 Identification strategy

Law 32/2004 modifies the selection mechanism of district heads (and vices) by requiring that citizens vote for them. The legislative change was implemented gradually across districts. The reason for this was that elections were held only once the mandate of a ruling district head expired, and the expiration date varied across districts due to, e.g., year of formation of the districts (districts formed quite continuously since 1956), natural deaths of the district heads, and district heads running for governor or for the national parliament. In our sample, 178 districts held elections in 2005, 59 in 2006, 32 in 2007, 113 in 2008, and 32 in 2009 or later. The timing of direct elections in Indonesia has already been used as a source of exogenous variation by Skoufias, Narayan, Dasgupta, and Kaiser (2011)[10] and Burgess, Hansen, Olken, Potapov, and Sieber (2012).[11] Nonetheless, we run an informal test of the quasi-random assignment of the timing of gradual elections across districts by trying to predict a wide range of village characteristics *before* the direct elections were introduced. The econometric specification corresponding to this test is the following:

$$c_{ki} = \mu + \theta D_{ki} + v_{ki},$$

[10] Skoufias et al. (2011) show that the only determinant of the timing of direct elections that is statistically significant is the expiration of the mandate of the previous district head.

[11] See also Fukumoto and Horiuchi (2011) for a similar research design using Japanese municipalities.

where c_{ki} is the characteristic of village k in district i, D_{ki} is the year of the direct elections (2005,..,2009), and v_{ki} indicates the error term clustered at the district level. If the timing of direct elections was uncorrelated with district characteristics, then θ would be small and statistically insignificant. Data on village characteristics come from a village census collected just before the first direct elections.[12] Table 2 shows the coefficient estimates. The timing of direct elections cannot predict any of the district characteristics except, marginally, the average village area and the per capita oil and gas transfer. Otherwise the coefficient estimate is always small and statistically insignificant.

In this paper we take advantage of this heterogeneity in timing to identify the impact of direct elections in two distinct ways. The first identification strategy considers all districts and makes use of a rather standard Difference-in-Difference (DD) strategy with many periods. Our outcome of interest is local corruption. Throughout the paper we use corruption prosecutions as a measure of corruption. Other works using the same idea are Fisman and Gatti (2002b) and Glaeser and Saks (2006), who study the determinants of corruption across US states, and Fisman and Gatti (2002a), who investigate the determinants of corruption across countries. Differently from them, we have information not only on the date of prosecution of each corruption event, but also on the date of the crime itself. Our dependent variable is the number of corruption crimes *committed* in a given year, rather than the number of corruption crimes *prosecuted* in a given year. This makes our measure not only more accurate, but, as we will see shortly, also opens up ways to improve our identification strategy.

The baseline econometric specification is the following:

$$y_{ijt} = \alpha_{1i} + \beta_1 D_{ijt} + \gamma_1 E_{it-1} + \Phi_{jt} + \varepsilon_{1ijt}, \tag{1}$$

where y_{ijt} is the number of corruption events committed in district i at time t, D_{ijt} is a binary variable indicating whether the district elections have taken place, E_{it-1} is a binary variable indicating the year before the elections, α_{1i} indicates the district fixed effects, Φ_{jt}

[12] The village census is the PODES 2006, which was collected by the Indonesian National Institute of Statistics in May-June 2005.

is a vector of region-year fixed effects, and ε_{1ijt} is the error term clustered at the district level[13]. The coefficient associated with the pre-election year (γ_1) captures the possible impact of pre-election campaigning on the number of corruption events. The coefficient associated with direct elections (β_1) captures the impact of direct elections on the number of corruption events as long as there are no omitted variables that vary over time (within regions) and are correlated both with the timing of direct elections and with local corruption.

The second identification strategy exploits some additional features of the legislative change. Following the approval of Law 32/2004 (December 2004), the central government postponed all elections in late 2004 and early 2005 to June 2005 to allow time to prepare the elections.

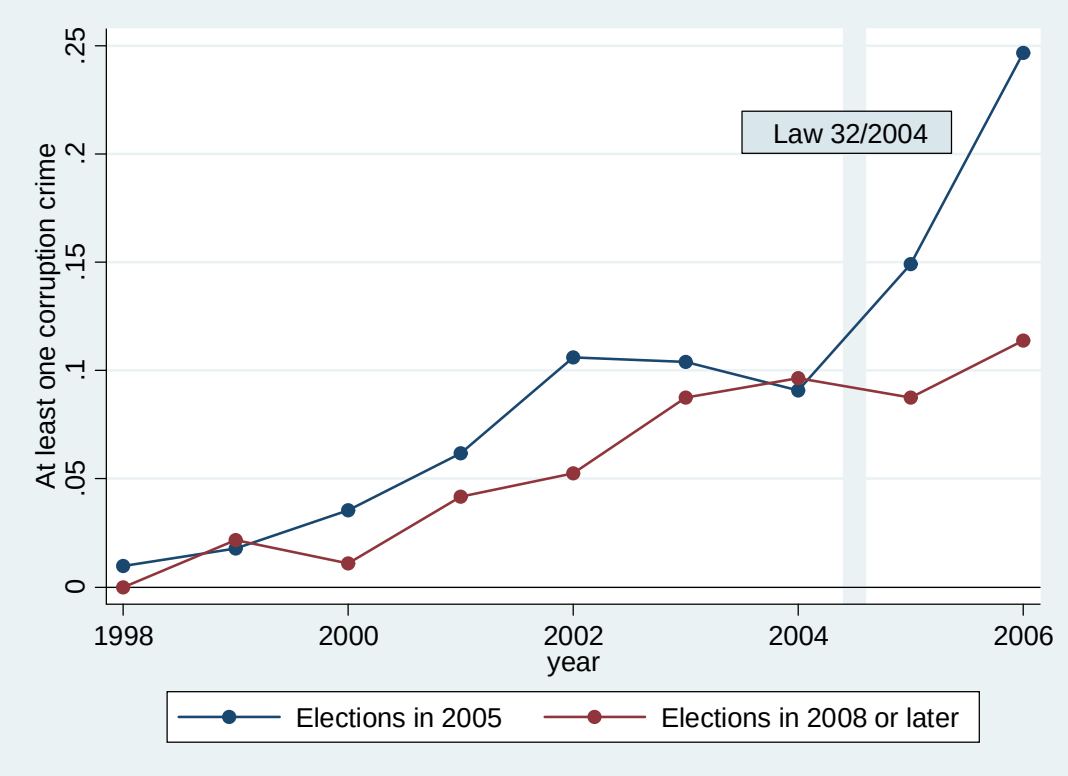

Figure 1: Corruption over time in districts with early and late elections

There are three interesting features of this legislative change: it was discussed and approved in the national parliament in a relatively short time; it was approved in the last

[13] Since many districts split during the period under investigation we cluster the error term according to the district borders as they were in 1999, well before the direct elections were introduced.

quarter of 2004, i.e., after the approval of the 2005 district budget and expenditure plans; and it required incumbents aiming to run for re-election to hand over their seats at least six months before elections to a caretaker appointed by the Ministry of Home Affairs. These three features imply that district governments facing elections in 2005 had very limited opportunities to "anticipate" them (i.e., to modify the district expenditure in order to get re-elected) compared to those facing elections in 2006 or later.

We take advantage of this aspect by comparing districts facing elections in 2005 (treatment group) to those facing elections in 2008 or later (control group). This restriction is also convenient as it allows us to visualize the evolution of corruption in treatment and control districts. Figure 1 suggests that the corruption levels of the two groups are very similar up until the introduction of the election, which is associated with a stark increase in corruption levels in the treatment group. The econometric specification associated with this identification strategy is the same as specification (1) except for the exclusion of the pre-election dummy.

5 Results

5.1 Baseline results

Table 3 shows the coefficient estimates associated with the "restricted sample."[14] Direct elections are associated with a significant increase of about 9 percentage points in the likelihood of having at least one corruption event, i.e., about 90 percent of the pre-election average (Panel A, Columns 1-5). The magnitude of the increase is consistent with the finding that direct elections increase the number of corruption events by about 0.200, i.e., about 100 percent of the pre-election average. Decomposing corruption cases by type yields the following results: direct elections are associated with more embezzlement cases (Panel B), more cases of conflict of interest (Panel C), and more cases of conflict of interest, bribery, and extortion (Panel D). Next, we estimate the impact of direct elections on our treatment group before (1999-2004),

[14] Throughout this section we will discuss primarily the coefficient estimates associated with the specification with region-year fixed effects (Columns 4 and 9 in most of the tables). The results are typically very similar to those with year fixed effects and to those with province-year fixed effects, although the latter are typically less precisely estimated due to the relatively small amount of observations identifying the coefficient of interest.

during (2005) and after elections (2006). The coefficient estimates (Figure 2) substantially mirror the descriptive statistics (Figure 1) and suggest that the bulk of the impact took place the year after elections.

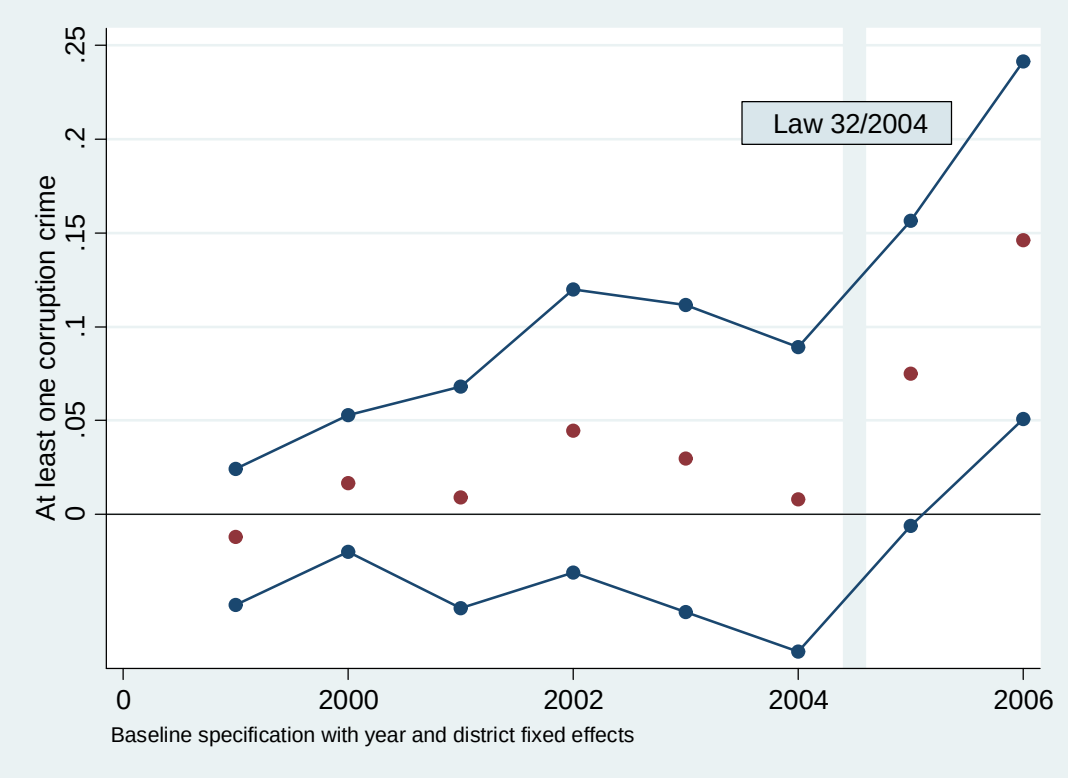

Figure 2: Corruption over time (coefficient estimates)

Table 4 shows the coefficient estimates associated with specification (1). In contrast to the previous estimates we include all districts and control for anticipatory behavior (i.e., campaigning or last-term budget appropriation) by including a binary indicator for the year preceding the elections. The impact of direct elections is again positive, although the magnitude is smaller (about 5 percent, i.e., half the pre-election average) and the coefficient is not always precisely estimated. This holds true even when we consider the number of corruption crimes (Columns 6-10), embezzlement cases (Panel B), or other cases (Panel C and D). The coefficient estimate associated with the pre-election dummy (not reported) is generally positive, insignificant and half the coefficient of interest.

Table 5 shows the coefficient estimates associated with the decomposition of the effect into election year and following years. The coefficient estimates associated with all corruption crimes (Panel A) and embezzlement cases (Panel B) suggest that the magnitude of the impact is rather consistent over time, although the coefficients tend to be precisely estimated only for election years. On the other hand, the coefficient estimates associated with less frequent corruption types (Panel C and D) are positive for elections years and close to zero afterwards. This may explain the lack of significance of the second set of coefficient estimates in Panel A.

In order to test whether the impact of direct elections is driven by some particular area of Indonesia, we re-estimate specification (1) dropping one region/island at a time (Jawa, Kalimantan, Nusa Tenggara and Maluku, Sulawesi, and Sumatera). Table 6 shows the coefficient estimates associated with this robustness check. The impact of direct elections is positive and relatively large across all sample restrictions (it ranges from 0.036 excluding Nusa Tenggara and Maluku to 0.079 excluding Sulawesi).

5.2 Increase in corruption or increase in law enforcement?

The main challenge with the use of corruption prosecutions data is that they (may) capture not only differences in corruption but also differences in law enforcement across districts. In our context this constitutes a measurement error in the dependent variable. This is, however, innocuous for our identification strategy as long as direct elections have no impact on law enforcement at the district level. This could be the case if, for example, direct elections provided a voice to the district electorate, which in turn managed to influence district prosecutors through local newspapers. In a recent work on the US judicial system, Lim, Snyder and Strömberg (2012) find evidence supporting the view that local media can influence local judges. However, they also find that the relationship is driven by elected judges and does not hold for appointed judges, which is exactly how district prosecutors are selected in Indonesia.[15] Corruption prosecutions are typically initiated by district prosecutors, who depend on the provincial prosecutors rather than on the district governments or the district electorate; and provincial prosecutors depend on the General Attorney rather than on the

[15] See also Gordon (2009) for a recent study of prosecutions in the US.

province government or the province electorate.[16]

However, one may still suspect that informal or illegal connections between the district prosecutors and the district government changed with the introduction of direct elections and therefore led to a (positive or negative) change in enforcement, or that direct elections increased the pressure of media and civil society, which in turn may have affected the activity of the prosecutors. We will henceforth refer to this possibility as the "enforcement" channel. In order to rule out the enforcement channel we take the following steps.

The first step is to estimate the impact of direct elections together with a factor that unambiguously increases corruption but not law enforcement.[17] One obvious candidate is a revenue windfall: an increase in government revenue should increase the opportunities for (and the return from) imbezzlement,[18] while having no obvious impact on corruption enforcement.[19] In a companion paper Olsson and Valsecchi (2012), we study the impact of the redistribution of oil and gas revenues across Indonesian districts on a wide range of public goods. In the present paper we exploit the oil and gas revenue transfers to test whether the impact of direct elections on corruption prosecutions captures an increase in corruption or an increase in enforcement. In particular, we estimate the following econometric specification:

$$y_{ijt} = \alpha_{2i} + \beta_{21} D_{ijt} + \beta_{22}(D_{ijt} * windfall_{ij}) + \gamma_2 E_{it-1} + \Phi_{jt} + \varepsilon_{2ijt}, \tag{2}$$

where $windfall_{ij}$ is the amount of per capita transfers from oil and gas revenues for district i in region j. The coefficient associated with the interaction term (β_{22}) captures the impact of the resource windfall together with the introduction of direct elections. A positive coefficient

[16] Corruption prosecution in Indonesia works as follows: the General Attorney and his staff coordinates the provincial offices, which in turn coordinate the district offices; prosecutions start from investigations by the police or direct complaints from the citizens; on the basis of this preliminary evidence prosecutors decide whether a case is worth further investigation; once they gather enough evidence they send a letter of indictment to the district court office; cases are decided at the district offices; once the verdict has been reached, either the prosecutor or the defendant can bring the case to a higher level (appeal, cassation) in provincial or central courts.

[17] We wish to thank Rohini Pande for this suggestion.

[18] In principle, a greater district government revenue could decrease corruption by increasing officers' salaries. However, in Indonesia officers' salaries are determined and paid by the central government.

[19] In principle, a greater district government revenue could affect enforcement through greater resources allocated to the district attorneys. However, in Indonesia district attorneys are paid by the higher tiers of the AGO structure.

estimate associated with the interaction term should reassure us that the enforcement channel is not driving our results. Table 7 shows the results associated with this robustness test. Since the data on resource-related transfers to district governments date back to 2001, i.e., before some districts formed or split, the sample associated with this analysis is smaller than the one used previously. Panel A shows that this sample restriction reduces the precision (but not the magnitude) of our baseline estimates. Panel B shows the coefficient estimates associated with direct elections and resource abundance: the coefficient of interest is positive and highly significant in all specifications. The magnitude ranges from 0.248 to 0.288 (Columns 1-5) and from 3.515 to 3.832 (Columns 6-10). This implies that an increase in resource transfers of one standard deviation, which equals 0.31 or 310,000 IDR (Table 2), together with direct elections, increases the likelihood of having at least one corruption crime by 76-89 percent (i.e., about 76-89 percent of the pre-election mean) and the number of corruption crimes by 109-119 percent (i.e., about 50-60 percent of the pre-election mean).

The second step we take to rule out the enforcement channel is to estimate the impact of direct elections on the number of corruption crimes *prosecuted* (rather than committed) at time t. In Section 2 we observed that it takes some time to detect a corruption event (the median number of years is 2, while the mean is 2.6), i.e., the crimes prosecuted at time t typically concern events that happened at time t-2. Hence, if direct elections increased mainly law enforcement, the impact on corruption crimes *prosecuted* should be strictly greater than the impact on corruption cases *committed* at a given point in time. If, on the other hand, direct elections truly increased corruption, then the impact on corruption cases prosecuted at a given point in time should be strictly lower than the impact on cases committed. Table 8 shows the coefficient estimates associated with this falsification experiment. The coefficient estimate associated with direct elections is very small and is never significant across all specifications. We interpret such evidence as highly supportive of the main message of the paper.

As an alternative robustness check to the previous falsification experiment we also re-estimated specification (1) controlling for the number of corruption cases committed at time t and prosecuted at time t ($y_{it,t}$) or at time $t+1$ ($y_{it,t+1}$). By controlling for crimes prosecuted

within less than two years, we control for any possible (short-term) effect of direct elections on enforcement. Table A1 shows the results: the coefficient estimates are consistent with previous findings and even more precisely estimated.

6 Conclusions

In this paper we asked whether direct elections of the local government have affected local corruption. In order to answer this question we exploited the gradual introduction of local elections in Indonesian districts and a novel database on corruption prosecutions. We used the number of corruption crimes *committed* at a given time in a district as a measure of local corruption. Coefficient estimates are robust across various specifications and suggest that direct elections increased local corruption by about as much as the pre-election average. In order to verify whether the baseline results are driven by a possible increase in law enforcement, we estimated the impact of direct elections joint with a factor that is unambiguously associated with greater corruption but not with greater law enforcement. In addition, we estimated the impact of direct elections on corruption crimes prosecuted (rather than committed) at a given time. Both robustness checks strongly support the view that law enforcement is not driving our results.

The paper contributes to the literature on corruption by shedding new light on its institutional determinants and informing central governments about the potential costs of a form of political decentralization with direct election of the local government relative to political decentralization with indirect election of the local government.

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Table 1

DESCRIPTIVE STATISTICS

PANEL A: TYPE OF CORRUPTION AND SECTOR DISTRIBUTION

	Total	Total	Private	Private	Public	Public	Public sphere, sector shares										
	No	Col %	No	Col %	No	Col %	admin	agr	dev	edu	food/ health	infrastru cture	private	regional company	state company	missing	Total
All	1396	100.0	133	100.0	1263	100.0	54.4	4.0	2.7	11.7	5.5	5.5	2.9	1.4	1.5	10.6	100
Embezzlement	1056	75.6	35	26.3	1021	80.8	55.8	3.3	2.8	10.7	5.2	5.3	1.3	1.6	1.8	12.2	100
Conflict of interest	168	12.0	2	1.5	166	13.1	29.5	11.4	2.4	19.9	9	15.7	9.6	0.6	0.6	1.2	100
Fraud	50	3.6	7	5.3	43	3.4	67.4	4.7	0	11.6	2.3	0	9.3	0	0	4.7	100
Distribution	34	2.4	4	3.0	30	2.4	36.7	10	0	3.3	46.7	0	0	0	3.3	0	100
Extortion	20	1.4	1	0.8	19	1.5	84.2	0	0	5.3	0	0	0	0	0	10.5	100
Bribery	15	1.1	2	1.5	13	1.0	53.8	0	0	30.8	0	7.7	7.7	0	0	0	100
Illegal practice	63	4.5	51	38.3	12	1.0	75	0	8.3	0	0	0	8.3	0	0	8.3	100
Hazard	36	2.6	35	26.3	1	0.1	0	0	0	0	0	0	100	0	0	0	100

PANEL B: DISTRIBUTION OVER TIME OF CORRUPTION CASES

Distribution of corruption events							Distribution of corruption crimes concerning the public sphere across districts				
Year	Total		Private		Public		Year	Commited		Prosecuted	
	No	Col %	No	Col %	No	Col %		Number of events	At least one event	Number of events	At least one event
	1	2	3	4	5	6		mean	mean	mean	mean
1998	2	0.1	0	0.0	2	0.2	1998	0.01	0.01	0.00	0.00
1999	8	0.6	0	0.0	8	0.6	1999	0.03	0.02	0.00	0.00
2000	12	0.9	0	0.0	12	1.0	2000	0.04	0.03	0.00	0.00
2001	30	2.1	0	0.0	30	2.4	2001	0.10	0.05	0.00	0.00
2002	63	4.5	0	0.0	63	5.0	2002	0.16	0.07	0.00	0.00
2003	94	6.7	2	1.5	92	7.3	2003	0.22	0.09	0.00	0.00
2004	93	6.7	3	2.3	90	7.1	2004	0.23	0.09	0.01	0.01
2005	111	8.0	0	0.0	111	8.8	2005	0.29	0.12	0.03	0.01
2006	147	10.5	3	2.3	144	11.4	2006	0.34	0.19	0.05	0.04
2007	232	16.6	7	5.3	225	17.8	2007	0.55	0.26	0.07	0.04
2008	274	19.6	16	12.0	258	20.4	2008	0.66	0.30	0.33	0.17
2009	211	15.1	42	31.6	169	13.4	2009	0.44	0.24	0.66	0.31
2010	86	6.2	30	22.6	56	4.4	2010	0.15	0.09	0.77	0.31
2011	33	2.4	30	22.6	3	0.2	2011	0.01	0.01	0.25	0.12
Total	1396	100.0	133	100.0	1263	100.0					

Table 2

EXOGENEITY TEST

PANEL A

	Urban village	Agricultural village	%HHs in agriculture	Village area	% rice area	Population	Population per hectare	Electricity in village	Households with electricity	Slum	Households in slums	TV reception
	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se
Direct Elections	0.010	-0.009	-0.998	-259.281*	-0.552	8 354.488	0.974	-0.002	0.001	0.006	0.000	0.051
	(1.193)	(-1.230)	(-1.360)	(-1.740)	(-0.044)	(0.255)	(1.028)	(-0.640)	(0.169)	(1.344)	(0.621)	(0.291)
N	51 009	51 119	51 119	51 119	51 119	46 738	51 119	51 119	51 119	51 119	51 119	51 119
Adjusted R2	0.001	0.001	0.003	0.001	-0.000	0.000	0.001	0.000	0.000	0.001	0.000	0.000
outcome: mean	0.21	0.87	71.34	2419.30	224.31	817097.90	14.40	0.97	0.61	0.09	0.01	5.54
outcome: s.d.	0.40	0.34	28.36	10663.91	1566.11	597099.10	38.97	0.18	0.31	0.28	0.06	4.52

PANEL B

	Primary school	Junior-high school	Senior-high school	Hospital	Maternity house	Health center	Mosques[1]	Prayer houses[1]	Churches[1]	Buddhist temples[1]	Local newspaper	Oil and Gas transfers per capita[2]
	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se
Direct Elections	0.001	-0.000	0.001	-0.000	-0.000	0.002	-0.380	0.306	0.304	0.001	-0.008	-0.020*
	(0.494)	(-0.077)	(0.178)	(-0.027)	(-0.115)	(0.644)	(-1.368)	(0.341)	(0.869)	(0.068)	(-0.781)	(-1.892)
N	51 119	51 119	51 119	51 119	51 119	51 119	51 119	51 119	51 119	51 119	46 738	44 726
Adjusted R2	0.000	-0.000	-0.000	-0.000	-0.000	0.000	0.002	0.000	0.001	-0.000	0.002	0.010
outcome: mean	0.96	0.37	0.18	0.02	0.06	0.13	12.54	23.81	4.87	0.12	0.46	0.06
outcome: s.d.	0.19	0.48	0.38	0.15	0.24	0.33	11.62	26.89	14.90	1.59	0.23	0.30

PANEL C

	Traffic through land	Paved road	Distance to sub-district capital	Distance to district capital	Conflict episode (last year)	Safety post	Police post	Village head: age	Village head: male	Village head: high school	Village head: diploma	Village head: bachelor
	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se	coef/se
Direct Elections	0.000	0.004	-0.162	-0.621	0.001	0.005	-0.002	-0.003***	0.064	0.001	-0.003	-0.003
	(0.002)	(0.397)	(-0.570)	(-0.795)	(0.683)	(0.677)	(-0.673)	(-2.623)	(0.691)	(0.184)	(-0.498)	(-0.522)
N	51 119	49 305	51 119	51 119	51 119	51 119	51 119	50 444	50 444	50 445	50 445	50 445
Adjusted R2	-0.000	0.000	0.000	0.001	0.000	0.000	0.000	0.000	0.000	-0.000	0.000	0.000
outcome: mean	0.96	0.62	8.99	34.82	0.02	0.88	0.12	0.97	44.70	0.69	0.20	0.16
outcome: s.d.	0.19	0.48	13.69	30.00	0.15	0.33	0.32	0.17	8.49	0.46	0.40	0.36

Note: *** p<0.01, ** p<0.05, * p<0.1. The unit of analysis is the village. Standard errors are clustered at the district-level in brackets. Sample restricted to district which did not form or split during the period 2003-2009. Data source: PODES 2006 for all outcomes but local newspapers (PODES 2003) and resource windfall (Ministry of Finance).

[1] Number of temples for every 10,000 people.

[2] The measurement unit is millions IDR, where 1 million IDR is approximately 100 USD.

TABLE 3

BASELINE RESULTS, ONLY DISTRICTS WITH ELECTIONS IN 2005 OR AFTER 2008

DEP VARIABLE	At least one corruption event					Number of corruption events				
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PANEL A: ALL CORRUPTION CRIMES										
DIRECT ELECTIONS	0.133***	0.081**	0.096***	0.092***	0.069*	0.274***	0.152	0.215*	0.205*	0.151
	(0.026)	(0.033)	(0.034)	(0.034)	(0.039)	(0.090)	(0.107)	(0.114)	(0.115)	(0.112)
R-squared	0.030	0.049	0.076	0.086	0.179	0.015	0.027	0.042	0.058	0.223
PANEL B: EMBEZZLEMENT										
DIRECT ELECTIONS	0.112***	0.064**	0.081**	0.076**	0.057	0.230***	0.123	0.184*	0.173	0.120
	(0.025)	(0.031)	(0.032)	(0.032)	(0.038)	(0.083)	(0.100)	(0.107)	(0.109)	(0.103)
R-squared	0.024	0.042	0.069	0.079	0.164	0.012	0.023	0.036	0.053	0.218
PANEL C: CONFLICT OF INTEREST										
DIRECT ELECTIONS	0.035***	0.030**	0.028*	0.029**	0.021	0.051**	0.047**	0.049**	0.052**	0.038*
	(0.012)	(0.015)	(0.015)	(0.015)	(0.014)	(0.020)	(0.022)	(0.023)	(0.025)	(0.022)
R-squared	0.012	0.018	0.020	0.034	0.143	0.010	0.016	0.019	0.030	0.131
PANEL D: CONFLICT OF INTEREST, BRIBERY AND EXTORTION										
DIRECT ELECTIONS	0.037***	0.032**	0.031**	0.032**	0.025*	0.037***	0.032**	0.031**	0.032**	0.025*
	(0.013)	(0.015)	(0.015)	(0.015)	(0.015)	(0.013)	(0.015)	(0.015)	(0.015)	(0.015)
R-squared	0.013	0.019	0.021	0.034	0.161	0.013	0.019	0.021	0.034	0.161
Time Effects		year	year	region year	province year		year	year	region year	province year
Fixed Effects			district	district	district			district	district	district
Observations	2,112	2,112	2,112	2,112	2,112	2,112	2,112	2,112	2,112	2,112
Number of clusters	214	214	214	214	214	214	214	214	214	214
Number of districts	268	268	268	268	268	268	268	268	268	268

*** p<0.01, ** p<0.05, * p<0.1 Standard errors (in brackets) clustered at the district level, using district borders as in 1999.

TABLE 4

BASELINE RESULTS, ALL DISTRICTS

DEP VARIABLE	At least one corruption event					Number of corruption events				
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PANEL A: ALL CORRUPTION CRIMES										
DIRECT ELECTIONS	0.187***	0.062*	0.056*	0.052	0.039	0.406***	0.145	0.161*	0.153	0.125
	(0.021)	(0.035)	(0.032)	(0.033)	(0.034)	(0.067)	(0.106)	(0.093)	(0.094)	(0.086)
R-squared	0.063	0.084	0.124	0.134	0.215	0.037	0.049	0.075	0.090	0.181
PANEL B: EMBEZZLEMENT										
DIRECT ELECTIONS	0.161***	0.065*	0.067**	0.061*	0.053	0.308***	0.118	0.141*	0.137	0.104
	(0.020)	(0.034)	(0.031)	(0.032)	(0.034)	(0.059)	(0.095)	(0.083)	(0.085)	(0.080)
R-squared	0.052	0.068	0.106	0.120	0.193	0.027	0.037	0.059	0.078	0.161
PANEL C: CONFLICT OF INTEREST										
DIRECT ELECTIONS	0.050***	0.020	0.019	0.015	0.008	0.082***	0.046*	0.047*	0.044	0.035
	(0.009)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.026)	(0.027)	(0.028)	(0.030)
R-squared	0.021	0.026	0.030	0.043	0.128	0.020	0.024	0.028	0.036	0.110
PANEL D: CONFLICT OF INTEREST, BRIBERY AND EXTORTION										
DIRECT ELECTIONS	0.056***	0.023	0.018	0.014	0.011	0.057***	0.023	0.019	0.014	0.011
	(0.010)	(0.017)	(0.016)	(0.016)	(0.017)	(0.010)	(0.017)	(0.016)	(0.017)	(0.017)
R-squared	0.024	0.030	0.034	0.046	0.143	0.023	0.029	0.034	0.045	0.135
Pre-election dummy	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time Effects		year	year	region year	province year		year	year	region year	province year
Fixed Effects			district	district	district			district	district	district
Observations	3,470	3,470	3,470	3,470	3,470	3,470	3,470	3,470	3,470	3,470
Number of clusters	255	255	255	255	255	255	255	255	255	255
Number of districts	349	349	349	349	349	349	349	349	349	349

*** p<0.01, ** p<0.05, * p<0.1 Standard errors (in brackets) clustered at the district level, using district borders as in 1999.

TABLE 5

BASELINE RESULTS, ALL DISTRICTS

DEP VARIABLE	At least one corruption event					Number of corruption events				
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PANEL A: ALL CORRUPTION CRIMES										
ELECTION YEAR	0.148***	0.060*	0.057*	0.051	0.041	0.366***	0.166	0.173*	0.168*	0.135
	(0.022)	(0.032)	(0.031)	(0.032)	(0.034)	(0.075)	(0.102)	(0.094)	(0.099)	(0.098)
FOLLOWING YEARS	0.208***	0.065	0.055	0.052	0.034	0.428***	0.124	0.130	0.112	0.100
	(0.025)	(0.045)	(0.043)	(0.043)	(0.047)	(0.078)	(0.128)	(0.120)	(0.115)	(0.103)
R-squared	0.065	0.084	0.124	0.134	0.215	0.037	0.049	0.075	0.090	0.181
PANEL B: EMBEZZLEMENT										
ELECTION YEAR	0.132***	0.065**	0.067**	0.061*	0.054	0.275***	0.123	0.141*	0.142	0.106
	(0.022)	(0.031)	(0.031)	(0.031)	(0.034)	(0.068)	(0.094)	(0.085)	(0.090)	(0.091)
FOLLOWING YEARS	0.177***	0.065	0.065	0.061	0.051	0.326***	0.114	0.140	0.125	0.101
	(0.024)	(0.043)	(0.041)	(0.041)	(0.044)	(0.067)	(0.110)	(0.104)	(0.100)	(0.088)
R-squared	0.053	0.068	0.106	0.120	0.193	0.028	0.037	0.059	0.078	0.161
PANEL C: CONFLICT OF INTEREST										
ELECTION YEAR	0.051***	0.030*	0.026	0.021	0.013	0.088***	0.067**	0.060*	0.058*	0.048
	(0.013)	(0.017)	(0.017)	(0.018)	(0.018)	(0.025)	(0.031)	(0.032)	(0.033)	(0.035)
FOLLOWING YEARS	0.049***	0.010	0.002	-0.002	-0.004	0.079***	0.026	0.012	0.010	0.002
	(0.010)	(0.020)	(0.019)	(0.019)	(0.020)	(0.018)	(0.033)	(0.032)	(0.033)	(0.032)
R-squared	0.021	0.027	0.031	0.044	0.129	0.020	0.025	0.030	0.038	0.111
PANEL D: CONFLICT OF INTEREST, BRIBERY AND EXTORTION										
ELECTION YEAR	0.050***	0.028	0.023	0.018	0.013	0.053***	0.029*	0.024	0.018	0.013
	(0.013)	(0.017)	(0.018)	(0.018)	(0.018)	(0.014)	(0.017)	(0.018)	(0.019)	(0.019)
FOLLOWING YEARS	0.059***	0.017	0.007	0.004	0.005	0.060***	0.016	0.006	0.002	0.005
	(0.012)	(0.021)	(0.020)	(0.020)	(0.021)	(0.013)	(0.022)	(0.020)	(0.020)	(0.021)
R-squared	0.024	0.030	0.035	0.046	0.143	0.023	0.030	0.034	0.046	0.135
Pre-election dummy	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time Effects		year	year	region year	province year		year	year	region year	province year
Fixed Effects			district	district	district			district	district	district
Observations	3,470	3,470	3,470	3,470	3,470	3,470	3,470	3,470	3,470	3,470
Number of clusters	255	255	255	255	255	255	255	255	255	255
Number of districts	349	349	349	349	349	349	349	349	349	349

*** p<0.01, ** p<0.05, * p<0.1 Standard errors (in brackets) clustered at the district level, using district borders as in 1999.

TABLE 6

ROBUSTNESS CHECK: EXCLUDE ONE REGION AT A TIME

DEP VARIABLE	At least one corruption event					Number of corruption events				
Excluded region	Jawa	Kalimantan	Nusa Tenggara & Maluku	Sulawesi	Sumatera	Jawa	Kalimantan	Nusa Tenggara & Maluku	Sulawesi	Sumatera
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PANEL A: ALL CORRUPTION CRIMES										
DIRECT ELECTIONS	0.046	0.066*	0.036	0.079**	0.050	0.223*	0.152	0.122	0.207**	0.109
	(0.037)	(0.035)	(0.034)	(0.034)	(0.039)	(0.120)	(0.095)	(0.099)	(0.104)	(0.104)
R-squared	0.106	0.132	0.123	0.132	0.125	0.053	0.109	0.073	0.079	0.070
PANEL B: ALL CORRUPTION CRIMES										
ELECTION YEAR	0.057	0.060*	0.039	0.076**	0.047	0.255**	0.143	0.141	0.219**	0.116
	(0.036)	(0.034)	(0.033)	(0.033)	(0.037)	(0.125)	(0.089)	(0.101)	(0.106)	(0.111)
FOLLOWING YEARS	0.015	0.081*	0.028	0.085*	0.058	0.133	0.174	0.073	0.176	0.092
	(0.049)	(0.047)	(0.046)	(0.046)	(0.052)	(0.148)	(0.131)	(0.130)	(0.136)	(0.123)
R-squared	0.108	0.132	0.123	0.132	0.125	0.054	0.110	0.073	0.079	0.070
PANEL C: EMBEZZLEMENT										
DIRECT ELECTIONS	0.067*	0.079**	0.049	0.072**	0.064*	0.176*	0.143*	0.112	0.159*	0.120
	(0.035)	(0.035)	(0.033)	(0.034)	(0.037)	(0.106)	(0.083)	(0.090)	(0.095)	(0.097)
R-squared	0.085	0.116	0.104	0.117	0.106	0.037	0.098	0.057	0.064	0.053
PANEL D: EMBEZZLEMENT										
ELECTION YEAR	0.076**	0.077**	0.051	0.070**	0.060	0.187*	0.135*	0.116	0.157	0.118
	(0.035)	(0.034)	(0.033)	(0.034)	(0.037)	(0.110)	(0.079)	(0.092)	(0.097)	(0.105)
FOLLOWING YEARS	0.041	0.085*	0.045	0.078*	0.072	0.146	0.164	0.100	0.164	0.127
	(0.045)	(0.045)	(0.044)	(0.045)	(0.049)	(0.131)	(0.113)	(0.113)	(0.118)	(0.106)
R-squared	0.086	0.116	0.104	0.117	0.106	0.037	0.098	0.057	0.064	0.053
Pre-election dummy	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time Effects	year	year	year	year	year	year	year	year	year	year
Fixed Effects	district	district	district	district	district	district	district	district	district	district
Observations	2,220	2,998	3,178	2,956	2,528	2,220	2,998	3,178	2,956	2,528
Number of clusters	234	300	317	295	250	234	300	317	295	250
Number of districts	145	227	234	218	196	145	227	234	218	196

*** p<0.01, ** p<0.05, * p<0.1 Standard errors (in brackets) clustered at the district level, using district borders as in 1999.

TABLE 7

INTERACTION WITH PER CAPITA OIL AND GAS TRANSFERS

DEP VARIABLE	At least one corruption event					Number of corruption events				
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PANEL A: ALL CORRUPTION CRIMES										
DIRECT ELECTIONS	0.220***	0.048	0.049	0.050	0.029	0.507***	0.161	0.164	0.166	0.111
	(0.023)	(0.036)	(0.036)	(0.036)	(0.038)	(0.081)	(0.106)	(0.105)	(0.103)	(0.095)
R-squared	0.078	0.102	0.136	0.148	0.239	0.050	0.064	0.087	0.104	0.206
PANEL B: ALL CORRUPTION CRIMES										
DIRECT ELECTIONS	0.221***	0.037	0.040	0.040	0.018	0.474***	0.033	0.037	0.035	-0.030
	(0.024)	(0.037)	(0.037)	(0.037)	(0.039)	(0.074)	(0.098)	(0.100)	(0.097)	(0.090)
ELECTIONS × WINDFALL	0.217*	0.300***	0.248**	0.255***	0.288***	3.338*	3.533**	3.515**	3.564**	3.832**
	(0.126)	(0.109)	(0.096)	(0.094)	(0.101)	(1.702)	(1.649)	(1.717)	(1.700)	(1.628)
R-squared	0.081	0.106	0.141	0.154	0.242	0.108	0.127	0.159	0.176	0.276
PANEL C: EMBEZZLEMENT										
DIRECT ELECTIONS	0.199***	0.049	0.052	0.051	0.036	0.365***	0.014	0.018	0.019	-0.045
	(0.023)	(0.036)	(0.036)	(0.036)	(0.039)	(0.065)	(0.089)	(0.090)	(0.088)	(0.085)
ELECTIONS × WINDFALL	0.211*	0.279**	0.208**	0.214**	0.229*	3.331*	3.491**	3.438*	3.469**	3.715**
	(0.124)	(0.109)	(0.102)	(0.102)	(0.119)	(1.705)	(1.665)	(1.748)	(1.741)	(1.671)
R-squared	0.073	0.095	0.123	0.138	0.221	0.118	0.134	0.162	0.181	0.268
PANEL D: CONFLICT OF INTEREST										
DIRECT ELECTIONS	0.055***	0.015	0.015	0.012	0.004	0.091***	0.049	0.048	0.047	0.036
	(0.010)	(0.019)	(0.018)	(0.018)	(0.018)	(0.019)	(0.032)	(0.032)	(0.033)	(0.033)
ELECTIONS × WINDFALL	-0.011	0.007	0.032	0.042	0.064**	-0.025	-0.007	0.044	0.063	0.070
	(0.016)	(0.020)	(0.024)	(0.027)	(0.031)	(0.020)	(0.026)	(0.044)	(0.056)	(0.053)
R-squared	0.026	0.031	0.036	0.062	0.155	0.023	0.027	0.032	0.048	0.127
PANEL E: CONFLICT OF INTEREST, BRIBERY AND EXTORTION										
DIRECT ELECTIONS	0.061***	0.017	0.016	0.014	0.009	0.064***	0.017	0.017	0.014	0.009
	(0.010)	(0.019)	(0.018)	(0.018)	(0.019)	(0.011)	(0.019)	(0.019)	(0.019)	(0.019)
ELECTIONS × WINDFALL	-0.013	0.008	0.033	0.042	0.063**	-0.015	0.007	0.033	0.042	0.062**
	(0.016)	(0.020)	(0.024)	(0.026)	(0.031)	(0.016)	(0.020)	(0.024)	(0.026)	(0.031)
R-squared	0.028	0.034	0.040	0.061	0.162	0.026	0.033	0.039	0.059	0.151
Pre-election dummy	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Resource × year	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time Effects		year	year	region year	province year		year	year	region year	province year
Fixed Effects			district	district	district			district	district	district
Observations	2,988	2,988	2,988	2,988	2,988	2,988	2,988	2,988	2,988	2,988
Number of clusters	246	246	246	246	246	246	246	246	246	246
Number of districts	275	275	275	275	275	275	275	275	275	275

*** p<0.01, ** p<0.05, * p<0.1 Standard errors (in brackets) clustered at the district level, using district borders as in 1999.

TABLE 8

FALSIFICATION EXPERIMENT

DEP VARIABLE	At least one corruption event					Number of corruption events				
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PANEL A: ALL CORRUPTION CRIMES										
DIRECT ELECTIONS	0.089***	0.005	0.005	0.001	-0.001	0.163***	0.002	0.004	-0.005	-0.002
	(0.011)	(0.013)	(0.014)	(0.014)	(0.016)	(0.025)	(0.032)	(0.030)	(0.028)	(0.036)
R-squared	0.049	0.095	0.109	0.128	0.228	0.033	0.068	0.078	0.096	0.205
PANEL B: ALL CORRUPTION CRIMES										
ELECTION YEAR	0.068***	0.005	0.005	-0.001	-0.003	0.135***	0.007	0.007	-0.006	-0.001
	(0.015)	(0.017)	(0.017)	(0.016)	(0.020)	(0.039)	(0.043)	(0.041)	(0.038)	(0.046)
FOLLOWING YEARS	0.103***	0.004	0.005	0.007	0.002	0.182***	-0.010	-0.004	-0.005	-0.007
	(0.013)	(0.017)	(0.018)	(0.018)	(0.019)	(0.027)	(0.038)	(0.038)	(0.037)	(0.034)
R-squared	0.052	0.095	0.109	0.128	0.228	0.034	0.068	0.078	0.096	0.205
PANEL C: EMBEZZLEMENT										
DIRECT ELECTIONS	0.079***	0.008	0.009	0.005	0.004	0.122***	0.003	0.007	0.001	0.005
	(0.010)	(0.013)	(0.013)	(0.013)	(0.015)	(0.020)	(0.030)	(0.028)	(0.025)	(0.033)
R-squared	0.043	0.078	0.090	0.110	0.211	0.027	0.052	0.059	0.076	0.175
PANEL D: EMBEZZLEMENT										
ELECTION YEAR	0.065***	0.010	0.011	0.005	0.005	0.108***	0.010	0.011	0.003	0.010
	(0.014)	(0.016)	(0.016)	(0.015)	(0.019)	(0.031)	(0.039)	(0.036)	(0.033)	(0.042)
FOLLOWING YEARS	0.088***	0.002	0.005	0.005	0.001	0.131***	-0.013	-0.005	-0.004	-0.009
	(0.012)	(0.016)	(0.017)	(0.017)	(0.018)	(0.022)	(0.032)	(0.031)	(0.030)	(0.027)
R-squared	0.044	0.078	0.090	0.110	0.211	0.027	0.052	0.059	0.076	0.175
Pre-election dummy	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time Effects		year	year	region year	province year		year	year	region year	province year
Fixed Effects			district	district	district			district	district	district
Observations	3,470	3,470	3,470	3,470	3,470	3,470	3,470	3,470	3,470	3,470
Number of clusters	255	255	255	255	255	255	255	255	255	255
Number of districts	349	349	349	349	349	349	349	349	349	349

*** p<0.01, ** p<0.05, * p<0.1 Standard errors (in brackets) clustered at the district level, using district borders as in 1999.

TABLE A1

CONTROL FOR CORRUPTION CRIMES COMMITED AT TIME T AND PROSECUTED AT TIME T AND T+1

DEP VARIABLE	At least one corruption event					Number of corruption events				
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PANEL A: ALL CORRUPTION CRIMES										
DIRECT ELECTIONS	0.115***	0.059*	0.049*	0.050*	0.038	0.228***	0.180**	0.170**	0.169**	0.144*
	(0.019)	(0.031)	(0.029)	(0.029)	(0.031)	(0.059)	(0.089)	(0.083)	(0.083)	(0.077)
R-squared	0.265	0.275	0.299	0.306	0.366	0.274	0.279	0.289	0.299	0.352
PANEL B: ALL CORRUPTION CRIMES										
ELECTION YEAR	0.087***	0.049*	0.046*	0.047	0.036	0.217***	0.181*	0.177**	0.181**	0.152*
	(0.020)	(0.029)	(0.028)	(0.029)	(0.030)	(0.074)	(0.095)	(0.087)	(0.091)	(0.089)
FOLLOWING YEARS	0.130***	0.069*	0.057	0.058	0.041	0.235***	0.178*	0.151	0.140	0.124
	(0.021)	(0.038)	(0.038)	(0.038)	(0.040)	(0.067)	(0.100)	(0.101)	(0.096)	(0.086)
R-squared	0.266	0.275	0.299	0.306	0.366	0.274	0.279	0.289	0.299	0.352
PANEL C: EMBEZZLEMENT										
DIRECT ELECTIONS	0.100***	0.058*	0.056**	0.056*	0.048	0.181***	0.149*	0.159**	0.161**	0.130*
	(0.018)	(0.031)	(0.028)	(0.029)	(0.030)	(0.050)	(0.080)	(0.074)	(0.075)	(0.069)
R-squared	0.253	0.261	0.285	0.293	0.347	0.251	0.255	0.269	0.281	0.329
PANEL D: EMBEZZLEMENT										
ELECTION YEAR	0.083***	0.056**	0.056**	0.057**	0.049	0.190***	0.162*	0.168**	0.175**	0.141*
	(0.019)	(0.028)	(0.028)	(0.028)	(0.030)	(0.066)	(0.086)	(0.078)	(0.082)	(0.079)
FOLLOWING YEARS	0.109***	0.059	0.055	0.055	0.045	0.176***	0.137	0.136	0.126	0.103
	(0.021)	(0.037)	(0.037)	(0.037)	(0.039)	(0.056)	(0.089)	(0.089)	(0.086)	(0.077)
R-squared	0.253	0.261	0.285	0.293	0.347	0.251	0.256	0.269	0.281	0.329
Pre-election dummy	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Cases prosecuted at t and t+1	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time Effects		year	year	region year	province year		year	year	region year	province year
Fixed Effects			district	district	district			district	district	district
Observations	3,470	3,470	3,470	3,470	3,470	3,470	3,470	3,470	3,470	3,470
Number of clusters	255	255	255	255	255	255	255	255	255	255
Number of districts	349	349	349	349	349	349	349	349	349	349

*** p<0.01, ** p<0.05, * p<0.1 Standard errors (in brackets) clustered at the district level, using district borders as in 1999.