Corruption and local democratization in Indonesia: The role of Islamic parties

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ABSTRACT

Indonesia has a tradition of corruption among local officials who harass and collect bribes from firms. This paper examines whether corruption is affected by local democratization and by the party composition of local assemblies. Democratization occurred in 1999 and decentralization in 2001. We have firm-level data for 2001 and 2004. The 2001 data benchmark corruption at the time of decentralization. We find that corruption declines between 2001 and 2004 overall, but much less so in districts with more secular party as opposed to Islamic party representatives in district assemblies. For a larger sample of districts, correspondingly, we find that corruption in 2004 is more in districts which voted more in favor of secular party representatives in the first elections in 1999. We argue that the effects seem to be causal, over above any effects of changing religiosity and economic circumstances across districts.

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In 1999 Indonesia democratized, electing representatives to national and district assemblies for 5-year terms. In 2001 after fiscal decentralization, districts had full responsibility for almost all local public services. Democratization occurred in a country that in the late 1990s ranked as among the most corrupt in the world (Bardhan, 1997; Mocan, 2004). With democratization, corruption in Indonesia has become a commanding political issue (McLeod, 2005). A significant portion of corruption occurs at the local level, where local bureaucrats collect bribes to supplement their salaries. We look at the effect of local democratization on local corruption, measured by the extent of bribes paid by firms to local officials.

Besides regular elections to local assemblies, democratization allowed political competition, or the free operation and formation of political parties. While this change in institutions itself seems to have reduced corruption nationally, our key result is that the party composition of local assemblies had a large effect on changes in local corruption. Party composition involves the role of the two longstanding major secular parties in Indonesia, relative to the remaining major parties in the first two election cycles, which have Islamic roots. For a sample of 1862 firms in 30 districts, we look at how local corruption changed between 2001 and 2004. We show that, as a time effect, local corruption declined overall, potentially the impact of democratization itself. Second, we show that districts which elected greater proportions of secular party

representatives to district assemblies in 1999 had significant relative increases in local corruption compared to districts that favored Islamic parties. Correspondingly, for a sample of 2632 firms in 87 districts, we find that at the end of the first election cycle in 2004, those districts that had elected greater proportions of secular party representatives in 1999 had significantly higher levels of local corruption. These correlations are of interest themselves. But we also argue that voting in greater proportions of secular party representatives in districts in 1999 led to more local corruption in 2004. This result was not anticipated and part of the task is to explain why such a relationship exists.

We believe the results are compelling for several reasons, in addition to the fact that they involve the fourth largest country in the world. They provide evidence that the composition of local assemblies can influence policy and behavior of bureaucrats and firms, with regard to a very sensitive issue in developing countries, a finding which relates to the literature on assembly composition effects (e.g., Pettersson-Lidbom, 2003). More fundamentally, they show that, even in very corrupt environments, local assemblies can act to reduce corruption. Also relevant is the literature debating the role of institutions in promoting growth (e.g., Persson, 2005; Glaeser et al., 2004). We will argue that local corruption in Indonesia is socially costly. Thus the results are consistent with the idea that the introduction of local democratic institutions and specific political competition in Indonesia may enhance economic performance prospects, by reducing corruption.

Another issue is that the cross-country literature argues that Islamic countries are more corrupt (e.g., Mocan, 2004); but these countries are also generally less democratic with more poorly

educated populations. This paper bases its results upon within-country political differences across districts in a newly democratized country. The point is that countries with Islamic populations need not be inherently corrupt; rather, certain institutional and economic settings may be breeding grounds for corruption. Changing institutions can have a significant impact.

Finally, the results are suggestive as to the role that new (and perhaps religiously-affiliated) parties may play in certain democratic situations. As such the results may contribute to our understanding of the motivations of voters in other contexts where parties that are labeled as Islamic (e.g., Hamas) may benefit from a perception of being anti-corruption. We think voters in 1999 associated Islamic parties with future corruption reduction; and accounting for that will be essential in identification of causal effects. Why might voters have perceived such a link? The two secular parties in Indonesian politics are tainted by the history of corruption under Suharto and Sukarno. Islamic parties in their role as "outsiders" were perceived as more willing to break corruption practices. Before democratization in Indonesia, the party environment was strictly regulated; and, under Suharto, only one party with Islamic roots was permitted to exist. With democratization, that party was allowed, for the first time, to participate fully in the election process (Kingsbury, 2001), and new Islamic parties arose.[1] At the local level, the anti-corruption stance of Islamic parties surely had a strong strategic element: to wrest power away from the two longstanding, secular parties that were perceived as corrupt. Even if there were no Islamic parties, other new parties might have arisen with similar stances.

Yet it seems that, apart from being outsiders, Islamic parties in Indonesia had credibility with voters in promising to reduce corruption. They were perceived generally as being "cleaner" and tougher on corruption, in part because they were more likely to select candidates for election who are believed to "lead an Islamic way of life" (Kuran, 2004) and who had a personal distaste for corruption. Of course, a new, non-religious party, perhaps with a compelling leader, might also have had credibility in promising corruption reduction. While the model we discuss below associates anti-corruption policy with personal tastes of legislators, such policy could also be part of a reputational equilibrium (Persson and Tabellini, 2000).

We note that Indonesia is a secular state; and, obviously, this paper is not advocating for religious parties. It is simply documenting the initial impact of Islamic parties in Indonesia on local corruption. The relative declines in corruption in districts that elected Islamic parties may not be sustained in the future, as these parties gain experience and themselves face ongoing corruption opportunities. Even if corruption reductions were to be long-lasting, districts dominated by Islamic representatives may impose other socially conservative policies not well-appreciated by the voters and may lose power. Certainly at the national level the third election cycle in 2009 points to this consideration. And as governmental and democratic institutions mature in Indonesia, any role of Islamic parties in corruption reduction may disappear, rather like formal institutions crowding out informal ones (Di Tella and MacCulloch, 2002).

In the rest of the introduction, we discuss the nature of corruption examined in the paper and the Indonesian political situation. Then we present the basic, stark relationships in the raw data between corruption in 2001 and 2004 versus assembly composition. In Section 1 we discuss the behavior of district assemblies and mechanisms for corruption reduction. Section 2 discusses the survey data we collected and the empirical specification and approach. In the first part of Section 3, we present our primary results from the overtime analysis, before turning to the cross-sectional work. Section 3 also discusses many robustness checks. Section 4 examines discontinuities in political effects and the role of political competitiveness and spillovers. Section 5 examines how the corruption environment affects firms, beyond bribe activity.

1. The nature of corruption

We examine bribes paid to officials who work in ministries that were under local governance before and after decentralization. Firms are required to obtain locally-set licenses. In 2004 in our sample, firms had a mean of 6.4 licenses, including those to operate, export, use particular kinds of machinery, make noise, create congestion, pollute in different dimensions, and so on. In addition, similar to licenses, firms face "levies", which are fees paid to operate an escalator, water pump, generator, and the like. In 2004 in our sample firms faced a mean of 2.6 items subject to levies. Both before and after decentralization, officials from the local Ministry of Industry monitor firms to make sure they have the full array of required licenses and paid-up levies. Officials from the local Ministry of Labor inspect licenses and equipment in connection with safety regulations. Visits to plants that are purportedly to inspect and monitor safety are the basic form of harassment used by officials from these two ministries to elicit bribe payments. In 2004, firms averaged about 7 visits a year from just these officials.

Firms pay bribes for several reasons. When a license is up for renewal, bribes reduce waiting time to renewal and harassment when a license has expired. Bribes are paid to expedite oversight and the time bureaucrats spend at the plant. Similarly, bribes are paid to placate officials who may claim a plant needs a license or levy that is not required. After 2001, empowered by a national "pro-labor" ministerial directive that greatly strengthened the application of pro-labor laws, other bribes (which we record separately) are paid by firms to resolve disputes over severance and overtime pay in their favor, as well as to have strikes declared illegal. While this is a separate source of bribe activity, it feeds into the first, since inspection of licenses and equipment safety allows local labor officials to sniff around plants for hints of labor troubles.

Thus, we are looking at the effect of local politics on corrupt practices of longstanding local officials who interact with firms. There are other forms of corruption for which we have no data. For example, firms pay bribes to reduce corporate income tax liabilities, issuance of FDI or export/import licenses, and police extortion. All these involve national officials outside of local control, and the first two only affect very largest firms. There are also forms of local corruption that don't involve our firms and the local ministries they interact with: corruption in procurement and public infrastructure projects, and possible corruption in education and health care, the responsibilities for which devolved to districts in 2001.

The corruption we look at is costly. For reasons discussed below, our basic survey asks about bribes as a percent of costs. At the time of decentralization in 2001, the bribes we examine averaged over 7% of costs for manufacturing firms on Java, with similar magnitudes paid by firms in other sectors. These bribes to reduce harassment from regulation fall under the "efficient grease" hypothesis (Lui, 1985; Beck and Maher, 1986; Bardhan 1997; Cai et al., 2005), recognizing that localities initially may have imposed regulations in part so local officials could demand bribes (e.g., Banerjee, 1994; Kaufman and Wei, 1999). Such corruption is not just income redistribution. It takes up the significant portions of time of entrepreneurs and firm employees (Kaufman and Wei, 1999; Svensson, 2003; Henderson and Kuncoro, 2006). Section 8 shows that harassment of firms is affected by assembly composition.

2. The Indonesian political situation and the timing of our surveys

In 1999, in the first elections, there were 5 major political parties, 2 of which are the longstanding secular ones—GOLKAR, the former

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[1] Suharto forced an amalgamation of traditional Islamic parties into one. Some of the new parties are entirely new while others represent the reemergence of traditional parties.
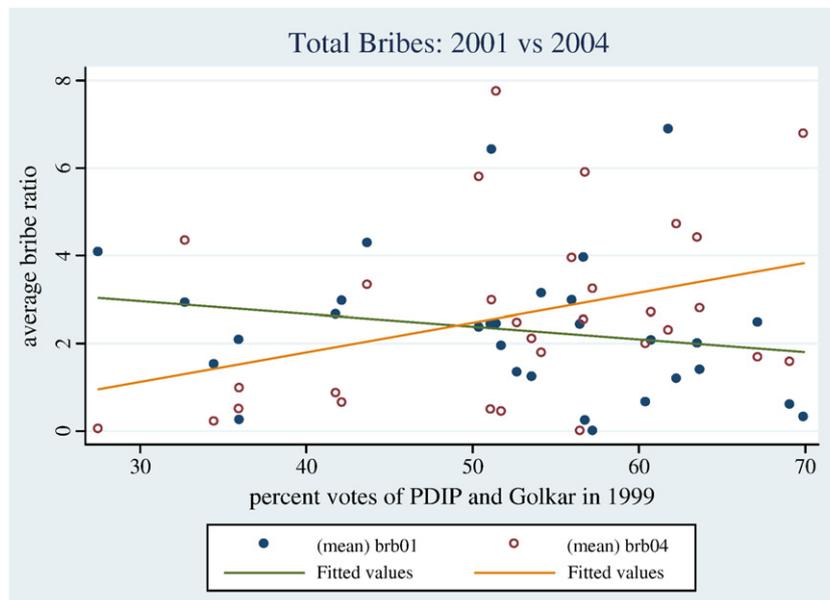
Fig. 1. Bribe patterns in 2001 versus 2004: PDIP-GOLKAR vote share in 1999[a] (unweighted) avg. of bribes as % of costs by district versus vote share.
a. District averages include firms who report paying no bribes.

ruling party under Suharto, and Megawati's PDIP party. All local parties must also be national ones.[2] Other significant parties in 1999 and 2004 had Islamic roots and are viewed as less accepting of corruption. While the dominant Islamic party (PKB) did not make corruption its national platform issue, our fieldwork suggests that it was viewed as substantially less corrupt at the local level than secular parties. Another Islamic party (PKS) emerged as a major party in the first years of democratization on an anti-corruption platform that focused on corruption associated with secular parties. At the national level, the 1999 national elections led initially to a coalition government between Megawati's secular party, PDIP, and the main Islamic party with the first President, Abdurachman Wahid, drawn from that party.

At the local level, democratization was followed by massive decentralization of governmental functions to district governments (by-passing provincial governments). Responsibilities such as education and health care devolved in January 2001, the beginning of a transition year in which the details of inter-governmental transfer formulas were adjusted and responsibilities clarified. In a second event of 2001, the national coalition government collapsed in July, with Megawati taking over as President. After that, at the local level, her secular PDIP party often aligned with the other major secular party, GOLKAR. Megawati held office until the end of the first election cycle in late October 2004.

One survey of firm bribe activity covers all districts on the island of Java and was carried out in early 2005, just after Megawati left office. Information on corruption is for the calendar year 2004. Given the close timing, we associate the answers on bribes paid for 2004 with corruption at the end of the first election cycle, reflecting the influence of the composition of the assemblies elected in 1999. We also have access to an initial survey, which covers about 1/3 of the districts on Java and occurred in the fall 2001. This survey asks about corruption in 2001, the transition year to decentralization and the Megawati Presidency. We can't document what happened to local corruption

from the start of democracy in 1999 to this survey in 2001; no surveys exist. A prevailing view is that 1999–2001 was a period of "business as usual" at the local level, because of the political paralysis under the Wahid–Megawati coalition and the wait for fiscal decentralization (Kuncoro, 2003; World Bank, 2003). We believe 2001 is a reasonable benchmark; if anything the 2001–2004 reduction in bribes may understate the 1999–2004 drop.

In representing politics, we examine how greater local assembly shares of representatives from the two secular parties, PDIP and GOLKAR, per se, affect bribing. We chose the PDIP-GOLKAR share in part because we associate less corruption with smaller shares of the legislature of the known corrupt secular parties, as much as with the role of Islamic parties. Second, in IV estimation we have direct instruments for votes for the two particular secular parties, as discussed later. When we replace PDIP-GOLKAR by the share of votes held by the two key "anti-corruption" Islamic parties, the results mirror those we report, given the two measures are strongly negatively correlated.

3. The effect of local politics on corruption

Figs. 1–3 tell the basic story, with data details in the next section. First in Fig. 1, we compare bribe activity in 2001 and 2004 for the pooled sample of firms in the 30 districts on Java, where we surveyed in both years and have 1999 vote shares for local assemblies. The figure plots the average bribe ratio across our firms in each district for each of 2001 and 2004 against the combined 1999 vote shares for the two longstanding secular parties, PDIP-GOLKAR. In Indonesia, vote shares in a district translate directly into party assembly shares, as discussed later. In 2004 the district average bribe ratio *rises* with the PDIP-GOLKAR vote share (correlation coefficient of .37) suggesting that, ex post, corruption is greater in districts with more secular party representatives. However Fig. 1 also shows that in 2001 the average bribe ratio *declines* as the same PDIP-GOLKAR vote share increases (with a simple correlation coefficient of − .20). First, and crucially, this means that the higher corruption observed in districts in 2004 with more secular party representatives is not due to persistence of prior corruption patterns because of underlying cultural conditions; corruption changes. In fact, Fig. 1 suggests that districts with lower initial corruption were more inclined to vote for PDIP-GOLKAR in the first election cycle and then paid for their votes with relative increases

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[2] This constitutional constraint applies to elections in 1999 and 2004 throughout Indonesia. There is a current exception—the situation in Aceh, where the recent settlement of the insurgency movement allowed for local political parties, even though that violates the constitution.
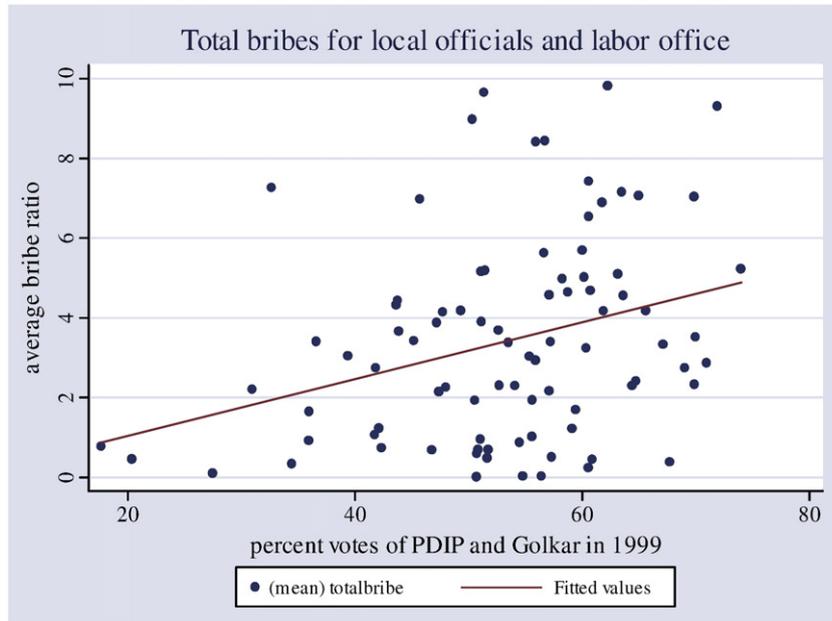
Fig. 2. 2004 Avg. bribe ratio by district versus PDIP-GOLKAR vote share (1999)[a].
a. District averages include firms who report paying no bribes.

in corruption by 2004, related to how heavily they voted for PDIP-GOLKAR.[3] Fig. 1 also raises the possibility that voting is affected by current corruption levels in a district with more evidence presented below.

For 2346 firms spread across 87 districts of Java, Fig. 2 plots the average ratio of total bribes paid to costs for all firms by district in 2004 against the 1999 PDIP-GOLKAR vote share. Fig. 2 shows that the 2004 pattern in Fig. 1 extends to the larger sample of districts. There is a sharp rise in the average 2004 bribe ratio as the 1999 PDIP-GOLKAR vote share rises. We will argue that these overtime and cross-section relationships between assembly composition and subsequent corruption are not simply correlations, but illustrate an underlying causal relationship.

We have direct evidence that voters are influenced by current corruption levels, based on the *second* election in late 2004. In Fig. 3a for the overtime changes for the 30 districts, we plot the 2001–2004 average bribe ratio change against the 1999–2004 vote change. In the second wave of elections, districts that experienced high relative increases in corruption then voted big reductions in PDIP-GOLKAR vote shares. A footnote to the table indicates this correlation is not due to mean reversion in voting patterns. Again, this is consistent with a scenario where initially less corrupt districts that voted for PDIP-GOLKAR in 1999 and experienced increases in corruption then voted to "throw the bums out of office" in late 2004, arguably either to punish these parties or because they believed that voting for Islamic parties might help. Fig. 3b shows that, for the cross-section of 87 districts, districts with low 2004 bribe activity saw little or no change in PDIP-GOLKAR vote shares between 1999 and 2004; but those with high bribe activity in 2004 saw big secular party vote share reductions. That is, districts with high levels of corruption in 2004 reduced their support for PDIP-GOLKAR in 2004 relative to 1999.

These figures suggest two things: (1) corruption reductions and ex post lower levels of corruption are associated with lower assembly shares of secular parties in Indonesia and (2) current voting seems to be influenced by current corruption levels. The latter presents what we believe to be the main issue in identification of assembly composition effects. If voters in less corrupt districts in 1999 voted in relatively more secular party representatives, the simple 2004 relationship in Fig. 2 will understate the effects of voting in more secular party representatives in 1999 on subsequent corruption in 2004.

That still leaves the question of whether the relative corruption increases and ex post higher levels in districts with greater dominance by secular parties is caused by local politics or is due to other correlated factors. One answer to that will be the extensive set of controls for local economic factors, cultural conditions, and changing social conditions we will employ. A major concern must be the increased religiosity in Indonesia in the last 10 years. For example, districts with increased religiosity could have experienced corruption reductions because increased religiosity gave firms the moral authority to say no to bribe collectors. We believe this can't explain the association between corruption reduction and Islamic parties. First, suppose we think districts with increased religiosity are more likely to vote for Islamic parties. In Fig. 3a, if voters in contemporaneously more corrupt districts voted more heavily for Islamic parties in 2004 because of increased religiosity in those districts, increased religiosity between 2001 and 2004 couldn't be associated with corruption reduction, since corruption increased in those districts in the same time interval. Second, more generally in the data and results to follow, we see no evidence that reductions in relative corruption are associated with increases in religiosity. Religion and religiosity may affect voting patterns and voting for assembly shares may affect subsequent corruption; but, in the data, increases in district religiosity won't be directly related to corruption reductions.

There are many other objections to establishment of a causal link between assembly composition and resulting corruption which we examine in the paper, three of which we note here. First, maybe there are not real differences in corruption, but instead differences in how firms respond to bribe questions over time and across districts. We address that issue in Section 5 on data and directly in estimation throughout the paper. Second, Islamic parties may impose other policies that affect the return to capital from doing business in a district, which may affect bribe activity, an issue addressed in Section 6. Third, maybe Islamic parties are not less corrupt per se, but simply less bureaucratic with, coincidentally, fewer rent − seeking

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[3] If, for $N = 30$, we regress the change in average bribe ratio on PDIP-GOLKAR vote share in 1999, the coefficient (standard error) is .0966 (.0438).

a)

b)
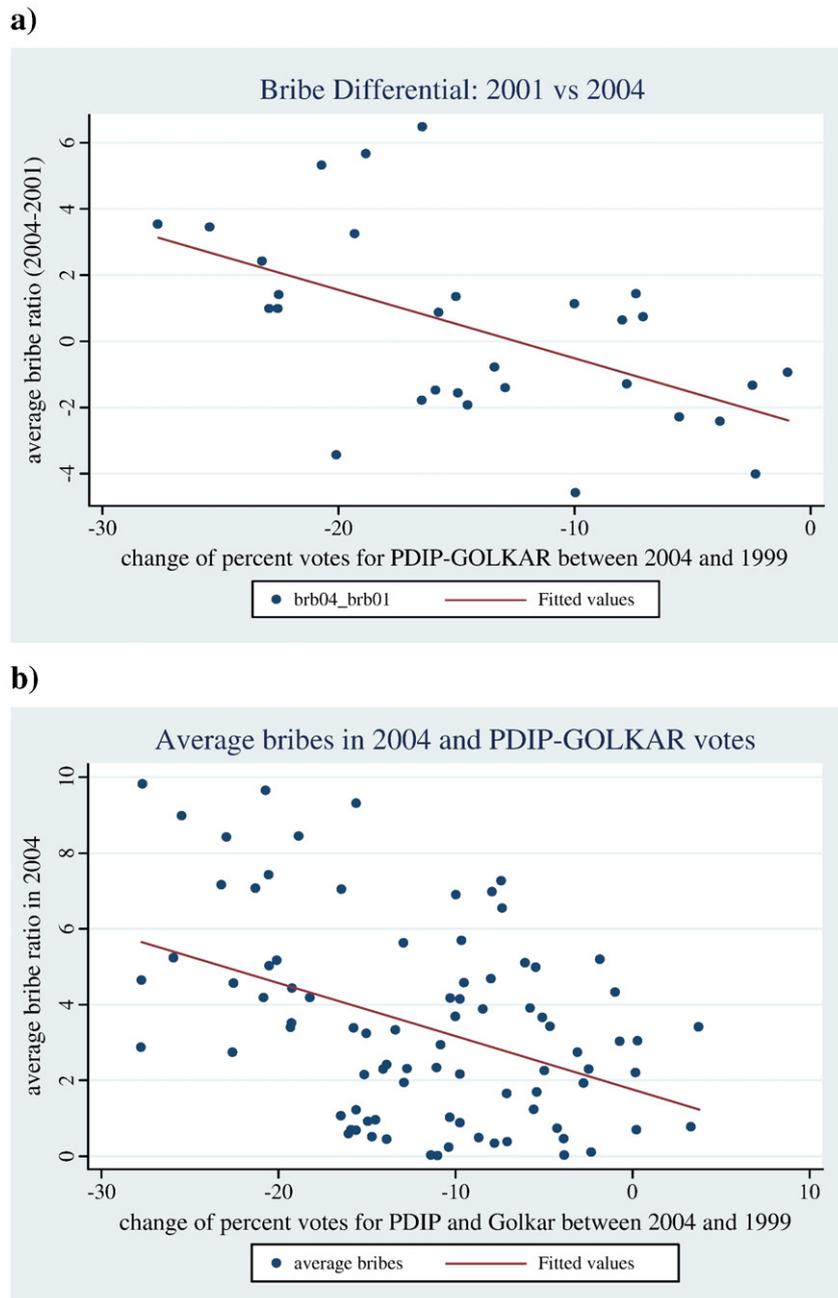
Fig. 3. Change in PDIP-GOLKAR support and bribe activities. a) Change in bribe activity: 2001–2004; 30 districts[a].
a. To check that the relationship is not simply reflecting mean reversion (in voting) or changing economic conditions, we report coefficients (s.e.'s) on the following regression (with an $R^2 = .36$). Δ share PDIP-GOLKAR 04–99 = − 1.87 − 1.08 (.425** Δ (bribe ratio 04–99) − .265 (.112) ** share PDIP-GOLKAR 99 + 7.26 (.6.40) % change GDPpc. b) Change in voting and 2004 bribe activity, all Java districts.

opportunities available. We will examine the effect of assembly composition on regulation in Section 8.

4. Corruption and politics

So far the stated result is simply that corruption is relatively higher in districts dominated by secular parties. There are two issues. First there is a pattern in the results which might be considered unusual, so we outline a model that generates outcomes consistent with that pattern. In particular, our results indicate that corruption *rises monotonically* as the fraction of secular party legislators in the local assembly rises, *with no sharp discontinuities*. Second, we need to explain why legislature intentions and policy actually affect behavior of local officials.

4.1. Politics

Suppose assemblies adopt explicit or implicit enforceable policies (e.g., replacement of corrupt bureaucrats) on how much corruption to permit.[4] Each of the n legislators has preferences $I + f(|\theta - P|)$, where I is income. The concave function $f(\cdot)$ peaks when each legislator's idiosyncratic value of θ equals P, where P is the implicit policy on the

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[4] Similar behavior can emerge even if assemblies have no policies per se, but individual legislators have preferences about corruption. If an assembly has more anti-corruption legislators that raises the probability that any firm has contacts with such a legislator in seeking redress from bad behavior by bureaucrats. There are also fewer legislators who are likely to take bribes to ignore illicit activity by bureaucrats.

amount of corruption allowed by the assembly. We consider two existing paradigms for how assemblies set P; both assume policies are based upon the individual preferences of legislators, not party positions per se. First is Oberholzer-Gee and Strumpf's (2002) Nash bargaining outcome, where legislators make side-payments, so that the assembly adopts the P where $\sum_1^n f'(|\theta-P|) = 0$. Here, for those preferring less corruption (lower θ's) than the equilibrium policy, $f' < 0$; while for those preferring more, $f' > 0$. A shift in assembly composition substituting a representative wanting relatively more corruption for one wanting less results in a higher value of P (under concavity), which satisfies the first-order condition required by the Nash bargaining outcome, and hence less corruption. The second paradigm is the familiar Condorcet winner within the assembly; the position of the assembly concerning corruption is that of the median member. Indonesians characterize their political and social decision making as requiring a "consensus," and both paradigms suggest a way to view this notion of consensus.

In linking preferences for Islamic versus secular party representatives, assume Islamic representatives' θ consists of a common mean, θ_I, plus a individual draw, ε, while secular representatives' θ consists of a higher common mean, θ_S, plus an ε from the same distribution. Then, an assembly dominated by Islamic representatives generally will prefer distinctly less corruption than one dominated by secular candidates. If the distributions of θ's for assembly members for each party generally overlap or almost overlap in the tails (right tail for Islamic and left for secular), then substituting an Islamic party legislator for a secular one will lead to a reduced P, with no sharp discontinuity.[5]

Besides local assemblies, there are local premiers, or *bupatis*. Starting in 1999, *bupatis* were elected by local assemblies in time-staggered elections (over a 5-year horizon across districts), although post-2004 they face direct election. We know the sponsoring party of each assembly-elected *bupati*. Like city managers in the USA appointed by a city council, some *bupatis* are professionals (bureaucrats) and some are political figures. In Section 7, we find that the party sponsorship of the *bupati* does not influence corruption outcomes. We presume that *bupatis* act on behalf of the assembly, meaning that if firms appeal directly to the *bupati* for relief from bureaucratic harassment, the *bupati's* response reflects the consensus position of the assembly as just outlined. When we look for effects of *bupati* selection in Section 7, we find none, consistent with this presumption.

4.2. Mechanisms

Why would assembly composition affect corruption of local bureaucrats? There must be two parts. First, some local political parties must make post-election efforts to reduce corruption. Apart from ideological considerations, political parties seeking later re-election would do this because either a) voters as, say, employees and as readers of the local newspaper know and care about the high degree of local corruption facing firms, or b) that there are other forms of corruption such as bribes associated with public procurement and infrastructure projects that concern voters, and the two forms of corruption are strongly, locally correlated.

Second, policies to reduce corruption must actually affect the behavior of entrenched bureaucrats. While we do not have quantitative evidence, the scenario we present is based on discussions over the years with firms, local officials, and representatives of private associations of firms (the local chambers of commerce), as well as a reading of articles on corruption in local newspapers. What are the ways by which districts reduce corruption?

With corruption as a commanding political issue, newspapers write exposés (Brunetti and Weder, 2003). Local newspapers present themselves as a form of "radar" that monitors local government practices. Young and ambitious local prosecutors make reputations through investigations and indictments. Under democratization, while there haven't been significant new national legislative measures (World Bank, 2003, Chapter 3), there is greater enforcement of existing laws and introduction of local initiatives. A key initiative in some districts is to replace existing bureaucrats with those "known to be clean." Districts have set up hotlines and direct mail for complaints about individual bureaucrats. Firms and local offices of the national chamber of commerce lobby legislators to protect firms from harassment and remove corrupt officials.

Punishment costs for corrupt bureaucrats include dealing with complaints, indictments and convictions, hindering of career advancement, and, as noted above, loss of employment. Following Mookherjee and Png (1995),[6] significant increases in punishment deter bribe solicitation and amounts, especially in a context like Indonesia where the firms may now more readily seek redress. With expanded opportunities for redress, bureaucrats may reduce bribe demands, so firms continue to find it cheaper to pay bribes than to make the effort to seek redress. In terms of the anti-corruption stance of Islamic parties, in those districts with more Islamic representatives, the government may not only be more inclined to discipline bureaucrats, but also prosecutors may feel freer to pursue corruption cases, and NGO's investigating corruption may get more local support with the political backing offered by Islamic representatives. Firms may perceive a greater proportion of legislators they can encourage to crack down on bureaucrats who harass them.

5. Data, specifications, and econometric issues

5.1. Data

We have the data from corruption surveys in late 2001 (for 2001) and in early 2005 (for 2004). Both involved extensive fieldwork and pre-testing of questionnaires, to determine how best to elicit bribe responses. Both surveys were carried out by the same supervising field team, based upon the same general format for the survey instrument. The survey environment was carefully constructed and used qualified locals as interviewers (skilled in local dialect and social issues) who tried to simulate an Indonesian "conversation among friends," with many examples of the types of "gifts" that firms might

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[5] This specification ignores one detail of electoral institutions in Indonesia. If there are n seats in the local assembly, before a local election, political parties each announce a slate of up to n candidates, in rank order of who has first priority for selection to the local assembly. Party assembly seats are distributed proportional to vote shares. So if a party wins m fraction of the votes, it gets mn seats, which go to its mn top-ranked candidates. If ε's are not observable by party officials and the electorate, so candidate rankings are random with respect to candidates' specific tastes (θ) for corruption, given $\theta_I < \theta_S$, prior results go through in terms of how the median assembly person changes as assembly composition changes. Even if ε's are observable and party rankings not random, under plausible scenarios about how parties rank candidates consistent with impressions from fieldwork and reading of local newspaper accounts, our results go through. In particular, suppose Islamic parties generally rank the most worldly candidates highest (highest θ's among Islamic candidates) so as to appeal to the typical non-orthodox voter; and secular parties either rank candidates independent of their θ's or rank less corrupt candidates higher (again to have the slate appeal to voters). Then, when we substitute an Islamic for secular party representative, we substitute in a person with a very low value of preferred corruption for one with a higher value. In general, this will shift the median assembly member to one with the next lowest θ.

[6] The economics literature discusses multiple equilibria under corruption (Cadot 1987; Andvig and Moene, 1990; Tirole 1996; Bardhan, 1997), based on information asymmetries, intergenerational reputation modeling, or punishments versus rewards when corrupt bureaucrats are few versus many. In the decentralization literature, officials may deter corruption to attract local investment, in the context of inter-jurisdictional competition for firms (Brueckner and Saavendra, 2001; Henderson and Kuncoro, 2006; Fisman and Roberta, 2002; Mocan, 2004). But it isn't clear what the impact of local democratization is on this process.

be induced to pay in various contexts. Fieldwork indicates that firms are quite willing to reveal bribe information as a fraction of costs (as discussed below), but less willing to reveal exact bribe amounts.

In both surveys, a key question asked about the fraction of costs devoted to monies paid to local officials to "smooth business operations". We call these "red tape" bribes, paid to overcome harassment associated with licenses and retributions. They also include "voluntary" contributions to local political parties or for local political events (usually parades). Our fieldwork clearly indicates that political contributions are tiny; payments are to bureaucrats. For 2004 survey, there is a second question about distinctly different bribes — those paid to local labor officials in dealing with strikes, severance terms, minimum wages, and overtime pay, which we call "labor bribes." Labor bribes are supposedly new since 2001, based upon a national pro-labor directive in that year. For 2004 we sum the two types of bribes to obtain total bribes paid to local officials, although we have looked at the two types of bribes separately.[7]

The survey for 2004 covers 2707 firms, all in manufacturing and only on Java. The survey has information on employment, sales, and capital stock, as well as questions about bribes, attitudes, harassment, red tape and the like; relevant details are given as the analysis proceeds.

We covered all of Java, in order to be able to define each district's neighbors to look for spillover effects (Section 7). We distinguish 97 districts on Java[8]; but have recorded vote shares for only 87. Due to its designation as a national capital region, Jakarta has provincial status and its 5 districts have no local assemblies. Second, in 5 of the 97 districts, votes were not published, generally due to controversies about the voting in those districts, and we have been unable to uncover the numbers. Thus identification of legislature composition effects in cross-section work for 2004 is based on 87 districts. Fig. 2 shows red tape plus labor bribes as a percent of costs averaged across firms within districts against vote shares for the 87 districts. The 2004 survey is not entirely random. We over-sampled in a few districts and the effects of that are analyzed in Section 7.[9]

The 2001 survey was a random sample of 1808 enterprises spread over all economic sectors in 64 districts of Indonesia. It contains somewhat less detailed economic information than the 2004 survey. In terms of overlap with the survey for 2004, there are 37 districts surveyed on Java in 2001; and vote share information applies to 30 of these. The overtime results presented later in the text are based on the pooled sample of 733 and 1129 manufacturing firms from these districts in 2001 and 2004 respectively. The firm sample overlap in the end was small, just 178 firms.[10] In Appendix B, we report some results for the 178 firms.

A survey issue is whether, for a given extent of actual bribing, a firm's response to the bribe question varies with local political–social conditions, or there is a response effect. We believe response effects are not a problem. First, we control for conditions which might influence bribe responses, such as changing religiosity of districts and the attitude of other firms towards the district government. Second, the fieldwork and surveying, as well as two other post-2001 corruption surveys (confined to the Greater Jakarta area) carried out by the team lead us to believe the data. Almost all firms seemed very willing to talk about corruption,[11] and any hesitation seemed idiosyncratic and unrelated to local socio-political conditions. Third, firms' answers to questions were consistent. In particular, we worried in the 2004 survey that some firms may have double-counted bribes, mistakenly including labor bribes as part of red tape bribes to smooth business operations (asked near the middle of the questionnaire), as well as answering a question on labor bribes separately later in the questionnaire. We carried out detailed interviews and resurveyed 50 firms spread over a number of districts, some months after the initial survey. The responses made it clear firms did not add labor bribes in the red tape category. Second, firm responses on all questions were remarkably consistent over time, which reinforced our confidence in the data.

5.2. Specification

Experimentation suggested a simple form for bribes paid by firm i in district j in time t:

$$bribe/costs_{it} = C_t(X_{it}) + S_t\left(Y_{jt}, P_j\right) + \eta_{lijt}. \qquad (1)$$

The $C_t(X_{it})$ function captures cost effects, any firm-specific bribe-related characteristics, such as whether the owner is a Chinese–Indonesian (traditionally subject to more harassment), and firm characteristics that influence the number of licenses required and visits received by local officials. The $S_t(Y_{jt}, P_j)$ function relates to district social–economic conditions (Y_{jt}) and assembly composition (P_j) in district j, which may affect the willingness to offer or press for bribes. Assembly composition is based on 1999 party vote shares which determine composition for the period 1999–2004. Note Eq. (1) is specified so functional forms may vary over time, which we will allow in the pooled sample. η_{lijt} represents unmeasured components of the locality, that might affect corruption at the time of voting. In estimation as detailed below, we will experiment with different error structures. Eq. (1) is a reduced form specification where politics may affect bribes not just directly, but also indirectly through affecting required licenses and visits by local officials (see Section 8). More structural approaches are analyzed in Henderson and Kuncoro (2006).

Overall, about 35% of firms report zero bribes. In estimation of Eq. (1) we utilize a Tobit specification, treating zero bribe responses as a censoring problem. This is one simple and commonly accepted approach. Regular and IV MLE estimates of Tobits cluster error terms by district. One drawback is that if either there is heteroscedasticity that is not independent of the covariates, or if the normality assumption is violated, Tobit estimates are inconsistent. In Section 7, we give results for other estimators, not dependent on these assumptions for consistency. We also report 2SLS results on key specifications, where standard errors are robust-clustered. Count formulations (e.g., Poisson and IV versions thereof) are not really appropriate. The bribe/cost ratio has some modest bunching around integer percentages; but generally the numbers starting from zero are pretty continuous with over 155 different ratio values in the pooled sample and over 200 in the 2004 sample.

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[7] In cross-section work for 2004 both types of bribes are higher in districts with greater secular party vote shares. In overtime work, red tape bribes declined significantly between 2001 and 2004, with labor bribes making up some of the difference. The interconnection between the two types of bribes in a Shleifer and Vishny (1993) framework could involve competition between bureaucrats from the labor and industry ministries leading to a division of bribes associated with industrial activities. But the presumption is that more bribes will be generated in this circumstance. There are more officials to harass firms and complementary dimensions on which to harass: labor officials sniffing around for labor troubles may also incidentally harass firms over machinery safety.

[8] There are 105 districts in Java, but 2 are essentially national parks and 6 have almost no manufacturing. These second 6 are integrated into surrounding areas to define the 97 districts which we examine.

[9] We over-sampled in districts with low populations of firms with a target of a minimum of 20 responses per district (ex post the lowest number is 16); and we over-sampled in 4 districts with large numbers of original firms, to try to increase the number of firms surveyed in both years.

[10] We expected to find about 300 or so manufacturing firms from the first survey in the 37 districts of Java. The lower resurvey rate was due to difficulty in finding small firms that had closed or moved, as well as the fact that 1/3 of the original questionnaires (with the recorded addresses) were destroyed by flooding of the storage facility in Jakarta. We note that flooding in Jakarta is a major problem and the University of Indonesia might have taken greater precautions. But we also note that at the time of the 2001 survey there was no intention to ever resurvey the selected firms.

[11] In 2004 we asked surveyors to distinguish firms paying absolutely zero bribes from those paying very minimal bribes and those unwilling to answer. While about 35% report zero bribes, only 2.5% would not provide an answer.

A final estimation issue noted here is that we do not account for selectivity in location decisions; that is, the effect of corruption on where firms locate. For example, firms adept at dealing with local officials may be more willing to choose corrupt areas. We do not have the data to model selection but we believe it is not an issue. For the 2004 sample, only 5% of our firms were created after 2001, meaning the firms in our sample did not move across districts in response to changing political and harassment conditions after decentralization.[12]

6. Econometric evidence: the effect of democratization on corruption

In this section we start with the overtime analysis of the effect of assembly composition on how corruption changed between 2001 and 2004. Then we turn to a cross-section analysis of the effect of 1999 assembly composition on subsequent corruption in 2004, as well as a variety of robustness checks. These are our main results. In Section 7, we examine discontinuities in political effects, political competitiveness and cross-district spillovers.

6.1. Overtime evidence on the effects of politics on changes in local corruption

We start with raw data on overall corruption changes. Fig. 1 already indicated the impact of assembly composition on bribe activity in 2001 versus 2004. Apart from assembly composition effects, what is the overall effect of the regime switch to local democracy? In examining local bribe activity as discussed earlier, in 2001 we have red tape bribes; while in 2004, we add in bribes for labor troubles. To the extent the presumption that labor bribes are zero in 2001 is incorrect, the decline in total bribes which we find between 2001 and 2004 understates the true decline. In Table 1, in 2001, 71% of firms report paying bribes in the 37 districts, while in 2004 that percent fell to 67%, a change that is not quite significant at the 5% level. The average bribe ratio fell from 9.84 to 6.54 for those reporting bribes and from 7.01 to 4.38 overall for all firms. Both declines are significant. Tests on the median and the ranking also indicate significant declines in bribe activity. These results suggest regime switch effects from local democratization.

Turning to econometric specifications, we pool firm-level data for 2001 and 2004 on bribe behavior and estimate

$$bribe/costs_{it} = X_{it}(a + D\alpha) + Y_{jt}(b + D\beta) + P_j(d + D\delta) + Dc + \psi_j + \eta_{lijt}. \tag{1a}$$

In the pooled sample we control for vectors of basic firm (X_{it}) and district (Y_{jt}) characteristics, as well as assembly composition (P_j). We allow their effects to vary over time, where $D = 1$ if the year is 2004 and 0 otherwise (we report the effect of constraining α and β to be zero). In the error structure, we have a time effect and in some specifications we add district fixed effects (ψ_j). With district fixed effects, the coefficient d in Eq. (1a) is not identified, since assembly composition is the same throughout this time period. The issue of endogeneity of P_j is important in the paper, and we discuss IV strategies below.

In terms of details on covariates, for firm characteristics, we have firm size in four categories of sales and a dummy for whether the firm exports or not. At the district level, P_j measures the 1999 PDIP-GOLKAR vote share, which is proportional to assembly share. In the pooled sample we measure this for 30 districts, with a control for 7 no-vote districts. For district socio-economic conditions, we control for just two time varying conditions, given the limited number of districts, although we experiment with other controls and footnote some specifics. First, GDP per capita in 1999 (applied to the 2001 observations) and in 2004 (applied to the 2004 observations) control

Table 1
The 2001 and 2004 comparison (all firms in 37 districts).

	2001	2004	Ho: difference is 0 p-value
Total firms	733	1129	
No. [proportion] of firms paying bribes	522 [.72]	758 [.67]	[.064]
If pay, bribe as percent of costs (mean)	9.84	6.52	.000
Bribe overall as percent of costs: mean (t-test)	7.01	4.38	.000
Median (Chisq-test)	2.0	.50	.000
Wilcoxon–Mann–Whitney test			.000

for conditions affecting the willingness to either demand or pay bribes. Second, we control for a measure of changes in religiosity. Changes in religiosity could drive both changes in bribes and district assembly composition, although as noted earlier this possibility seems inconsistent with the raw data. The basic religiosity measure is the ratio of Islamic to state elementary schools, which is taken from the PODES[13] conducted every three years. For 2001 firm data, we use the 2000 measure and for the (late) 2004 firm data we use the 2006 PODES measure. This ratio doubles in the time period. With increased presence of Islam in everyday life in districts, more people send their children to Islamic schools or supplement secular school education with Islamic school education. Of course there is a danger in including this variable, since it is potentially endogenous. Results suggest the two are not correlated; but we report results without and without this control. Means, standard deviations, and simple correlation coefficients for relevant variables used in the paper are given in Tables A1–A3 in Appendix A.

6.1.1. Empirical results

The basic results for the pooled sample are in Table 2. Column 1 shows the ordinary Tobit results; and column 2 adds in district fixed effects. Columns 3 and 4 contain IV results with and without districts fixed effects. Once we have presented the basic results along with discussing the IV approach, we then turn to robustness checks, on both use of covariates and instruments.

For firm variables, as all columns in Table 2 reveal, firm size effects change dramatically over time. In 2001, bribes as a fraction of costs decline with firm size, while in 2004 no such pattern exists, suggesting officials start to harass bigger firms relatively more after decentralization. In 2001 exporters pay more bribes; by 2004 that effect seems to disappear. The Asian financial crisis may have eventually helped ease harassment of exporters and smaller firms. In terms of district controls, GDP per capita has no significant effect. The religiosity variable without fixed effects has no impact either. With district fixed effects, religiosity is initially *positively* but somewhat modestly associated with corruption, but the effect is smaller in 2004. The positive association might hint at reverse causality: high corruption induces religiosity. As we will see, assembly composition results are the same with or without the religiosity measure.

6.1.1.1. Assembly composition effects.
In column 1, for an ordinary Tobit, the base slope (insignificant) coefficient on PDIP-GOLKAR of −.049 suggests (under a non-"marginal" interpretation of Tobit coefficients) that, in 2001, a 10% increase in PDIP-GOLKAR vote share is associated with a .5 *reduction* in the percent bribe ratio. However, consistent with Fig. 1, the .260 coefficient on vote share interacted with time suggests that the net effect in 2004 is reversed, and that a 10% vote share increase (where the standard deviation is 20) then is associated with a net 2.1 bribe ratio *increase* in 2004 (where the mean is 4.4). Assembly composition effects are very large. For time changes

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[12] Dropping that 5% of firms leaves results unchanged.

[13] The PODES is a tri-annual national inventory of facilities and village population and economic characteristics.

Table 2
All bribes: pooled sample, 2001 and 2004.

	(1) Tobit	(2) Tobit	(3) IV Tobit (2-step)	(4) IV Tobit (2-step)
Small–medium size	1.39	1.57	1.39	1.56
(500 m–1b rupiah sales)	(1.79)	(1.73)	(1.42)	(1.40)
*D2004	1.20	.419	1.21	.439
	(2.03)	(1.93)	(1.90)	(1.88)
Medium size	−4.41*	−3.03	−4.40**	−3.06**
(1b–5b rupiah sales)	(2.31)	(1.94)	(1.45)	(1.46)
*D2004	8.70**	7.08**	8.66**	7.08**
	(2.80)	(2.71)	(1.88)	(1.88)
Large size	−6.17**	−5.03**	−6.12**	−5.10**
(>5b rupiah sales)	(2.18)	(1.80)	(1.55)	(1.60)
*D2004	10.3**	9.06**	10.3**	9.13**
	(2.80)	(2.77)	(1.96)	(1.99)
Export or not	4.59**	4.40**	4.49**	4.42**
	(1.48)	(1.44)	(1.12)	(1.13)
*D2004	−2.41	−2.73	−2.38	−2.80*
	(1.56)	(1.51)	(1.49)	(1.47)
Time dummy, 2004	−23.6**	−20.4**	−23.7**	−18.9**
	(5.86)	(7.25)	(5.91)	(5.26)
% share PDIP-GLKR vote '99	−.049	n.a.	−.071	n.a.
	(.073)		(.073)	
*D2004	.260**	.202**	.261**	.169*
	(.081)	(.083)	(.116)	(.091)
Ratio: Islamic to secular schools	6.41	30.5**	6.21	27.5**
(t)	(9.63)	(13.8)	(5.49)	(8.20)
*D2004	−3.02	−14.3	−2.93	−16.8**
	(9.34)	(11.0)	(5.96)	(6.19)
Control: no-vote areas, lnGDPpc (t); _*D2004	Yes	Yes	Yes	Yes
District fixed effects	No	Yes	No	Yes
Variance σ²	13.0**	12.9**		
	(1.01)	(1.17)		
N [districts = 37] {zeros = 582}	1862	1862	1862	1862

Standard errors for Tobits are based on clustering (except for 2-step IV). One asterisk indicates a 10% significance level and two a 5% level.

in bribes between 2001 and 2004, given the time dummy of −23.6, in column 1, the bribe ratio is lower in 2004 than 2001 for all realized values of PDIP-GOLKAR (for the point estimates, the turning point is over 100% PDIP-GOLKAR vote share).

In column 2 of Table 1, we add in district fixed effects to the column 1 specification. Now only the net PDIP-GOLKAR coefficient is identified (from the coefficient for PDIP-GOLKAR*time dummy 2004).[14] The PDIP-GOLKAR coefficient with district fixed-effects is .202, the same net effect as in column 1. That is, controlling for time invariant district characteristics has no effect on the assembly composition results. This should not be surprising. Given corruption patterns in Fig. 1 change dramatically across districts, they do not seem to be anchored by time invariant unobservables representing a persistent culture of corruption.

A key issue is whether there are unobserved time varying characteristics which affect bribe differences between 2001 and 2004 and are correlated with the assembly vote share *level* in 1999. To study this issue we turn to IV estimation, starting with a discussion of instruments. Given the small sample of districts in the pooled sample, we treat these IV estimates with caution. We rely more heavily on IV work in the larger sample, cross-section analysis to follow, but we discuss instruments in detail now.

6.1.1.2. Instruments. We want to instrument for 1999 assembly composition with district characteristics which, for historical–cultural reasons, we believe reflect voters' tastes for Islamic versus secular

parties, but meet two criterion First, ceteris paribus, they should not directly affect changes in bribe activity. Second, they should not be correlated with unobserved changes in district conditions which might affect bribe activity, given the existing controls on district characteristics (including district fixed effects in one of the two cases). We consider 4 possible instruments. The first three concern specifics of religious affiliations of voters. Identification then requires that while district corruption may be affected by current religiosity of Islamic voters, it is not affected by historical religious composition of the district in the specific dimensions that we define, that helped determine 1999 party allegiances. For historical and cultural reasons (Liddle, 1999; Vatikiotis, 1998), people with certain specific religious affiliations support particular parties.

On Java, there are *abangan* and *santri* Muslims, and both have experienced strong increases in religiosity. But *abangan* have historically incorporated cultural practices adapted from Buddhism and Hinduism, two religions that at different times dominated parts of Java.[15] For us there are two key distinctions: (i) *abangan* Muslims are more inclined to "separation of church and state", or more averse to the existence of Islamic parties, to incorporating Islam into politics, and, thus, to voting for Islamic parties; and (ii) *abangan* Muslims tend to live in non-coastal areas, more in the hinterland of Central and East Java where Buddhism and Hinduism once flourished. There is no Census information on religious divisions among Muslims. So for the first instrument, we have the fraction of a district's population in 2000 that was living in coastal villages (noting Java is a long, narrow island), thus indicating populations that are more likely to be *santri* and more willing to vote for Islamic parties.

One may worry that districts which have a greater fraction of the population living in coastal villages may differ systematically from other districts. In the larger cross-section sample in the next section, we control for relevant characteristics including the percent of manufacturing firms engaged in export activity. We also experiment with looking at results for a sub-sample of districts on the coast and for a sub-sample that excludes all ports. Finally we experiment with including the coastal variable as a covariate and making the instrument just for villages on the north coast of Java where the *santri* dominance may be stronger.

A different way to represent the fraction of Muslims likely to be *abangan* in a district, is the, on average, small fraction (under 1%) of the population in districts on Java that still report they are Hindu. For the third, we note that PDIP is partially an outgrowth of an amalgam of parties forced in the Suharto era, which included the traditional Christian parties. The fraction of the population (average 4.3%) that was Christian in 1995 is a strong instrument for PDIP vote share in 1999. For the last instrument, one of the secular parties, GOLKAR, draws strength from former government employees, who worked for the Suharto regime in 1990 and out of loyalty tend still to vote GOLKAR. The fraction of the population in 1990 who were government employees (average 1.9%[16]) is a strong instrument for GOLKAR. While one might worry that former Suharto employees were more corrupt than the general population, most former government employees were not in our relevant local ministries; and the vast stock was hired before corruption accelerated after 1990.

In this section we use all four instruments to help strengthen first stage regressions, given the limited sample of districts, although we report on some results where we use just the two strongest: % of population in a district living in coastal villages and % former government employees. In Section A2 of the Appendix we show some first stage regressions relevant to Tables 2 and 3, and discuss

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[14] The same issue of identification applies to the dummy variable for no-vote districts.

[15] The distinction between *santri* versus *abangan* has diminished with time, and decades ago *santri* Muslims broke into two groups: traditional (more rural) and reform.

[16] The fraction of households with a government employee would obviously be much higher.

formal and informal tests of strength and validity of instruments, including "placebo" experiments to show that instruments are not driving general district conditions, just voting. For validity, conceptually we are asking that these historical–cultural instruments predict 1999 voting patterns, but be unrelated to district conditions influencing changes in bribe activity from 2001 to 2004. With fixed effects all we require is that these historical affiliations are not correlated with unobserved *changes* in district conditions which affect changes in corruption. Recall that the religiosity measure in Table 2 suggests no association between increasing religiosity and reductions in corruption.

6.1.1.3. IV results. We estimate the base formulation from columns 1 and 2 in Table 2 by IV Tobit, instrumenting for the vote share variables, with results reported in columns 3 and 4. MLE estimates with clustered errors wouldn't converge, so we report 2-step IV Tobit results. The IV results in column 3 are very similar to the ordinary Tobit results in column 1, with net assembly effects in 2004 being .19 in column 3 versus .21 in column 1. Inclusion of fixed effects is very demanding of the data with such a small sample. Nevertheless, while the net effect in column 4 of .17 under fixed effects is less than the .20 effect in column 2, it is still very large. Given we are effectively looking at changes in bribes as dependent on assembly composition, ordinary Tobit estimation seems appropriate for the pooled sample.

In summary, we get a point estimate of the Tobit net effect in the range .17 to .21, depending on specification. Robustness checks below also leave the net effect in this range. Under a non-marginal interpretation to Tobit coefficients, a 10% increase in PDIP-GOLKAR vote share in 1999 leads to an increase in the ratio of bribes to costs of about 2 in 2004, from a mean of 4.4. The marginal Tobit effect is about 1.3, accounting for the probability of paying a bribe (about .65). This is consistent with the 2SLS effect of 1.5, reported below. These are very large effects, and ones that will be duplicated in the cross-section work to follow.

6.1.1.4. Robustness checks. In this sub-section, we list some of the robustness checks we performed. For ordinary Tobits, we look at the impact of changing district controls, of allowing firm coefficients to vary over time, and for heterogeneity of assembly effects. Results in columns 1 and 2 are virtually the same if we drop the religiosity and GDP district controls or if we add in a perceived efficiency index.[17] The perceived efficiency index helps control for attitude and differential response effects across districts to bribe questions. We asked firms early in each interview, on a scale of 1 (best) to 6 (worst) how they rate the efficiency of the local government's provision of basic services in 2001 and in 2004. The control is the average response of *other* firms in the district in the relevant year to the question. The PDIP-GOLKAR net effect is also the same when we do not allow firm characteristic coefficients to change over time.[18] For heterogeneity an issue is whether assembly composition has bigger impacts on bigger firms. In this sample to look at this, we drop the small–medium size category which is not significant and combine the medium–large and large categories which have similar coefficients into one larger firm category. Interactions between assembly composition and the larger firm size variable and its interaction with time are both insignificant.[19]

For IV estimation, given the small sample of districts we limit the robustness checks but have many more for the cross-section estimation. First for functional form and the use of a Tobit, in column 3, estimation by 2SLS yields a significant coefficient for D2004*PDIP-GOLKAR and a net PDIP-GOLKAR effect on bribes of 0.15,[20] which is the expected reduction from 0.19 in column 3 given the proportion zeros in the sample. The net assembly effect in 2004 on corruption is robust to a shorter instrument list and removal of district time varying controls because one might be worried about their endogeneity.[21] In terms of the use of an instrument for proportion of the population living in coastal villages, we conducted two experiments. We drop firms in the 5 districts which are ports in this sample (the DKI Jakarta districts). For this sample of 1459 firms the net PDIP-GOLKAR effect is .18.[22] Second to column 3, we add % population living in coastal villages as a covariate and instrument with this variable interacted with whether a district is on the north coast of Java. The coastal covariate is tiny (.1) and insignificant; the net effect of PDIP-GOLKAR is 0.29, although instruments are weakened as noted in Section A2.[23]

In summary, all robustness checks strongly supported the results. Finally, there are 178 firms which overlap in 24 districts (20 with voting) for 2001 and 2004. Firm fixed-effect results are in Appendix B and are consistent with results here.

6.2. Cross-section specifications

In the cross-section for 2004 the basic specification is

$$bribe\,/\,costs_i = X_i a + Y_j b + P_j d + \psi_s v + \eta_{ij}. \qquad (1b)$$

In Eq. (1b) covariates are time-invariant but we control for a vector of industry fixed effects, ψ_s, at the 2-digit manufacturing level. In 2004, we have a rich set of both firm (X_i) and district socio-economic (Y_j) characteristics. For firms, we have employment and capital intensity measures, whether firms export or not, whether they receive FDI, and whether owners are Chinese–Indonesians. In all specifications, we have a dummy variable to control for the small fraction of firms who rent some of their capital stock and a measure of transport costs of visits by local officials, which may also affect the degree of harassment a firm faces. This is the population-weighted average of distance from villages in the firm's sub-district to the district capital.

For district socio-economic characteristics, we experimented with many combinations of controls, none affecting the PDIP-GOLKAR vote share coefficient. The controls we report on include ln (GDP per capita 1999), ln (number of manufacturing firms in 2001),[24] percent of manufacturing firms in the district exporting in 2001, and the average profitability rate of manufacturing firms in the district in 2001 (operating profits over book value of capital stock). Profitability, income levels, and % exporting may all affect the bribes that local officials believe they can collect and may be correlated with voting, as well. Scale of manufacturing may affect costs of collecting bribes, as may the cost-of-visit variable discussed above. Additional controls are income growth (percent increase in GDP per capita from 1999 to 2004), increase in religiosity, and perceived average efficiency of government by other firms in the district. The first two variables

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[17] For example in column 1, for the former the coefficients (s.e.'s) on PDIP-GOLKAR and PDIP-GOLKAR*time2004 are respectively −.062 (.068) and .285 (.071), while for the latter they are −.056 (.066) and .254 (.081).

[18] In column 1, the coefficients (standard errors) are for the time dummy .18.4 (4.53), for the base PDIP-GOLKAR covariate −.070 (.070) and for PDIP-GOLKAR interacted with the time dummy .299 (.085).

[19] Without fixed effect the coefficients (s.e.'s) of Dlargerfirm*PDIP-GOLKAR99 and Dlargerfirm*PDIP-GOLKAR99*D2004 are respectively .117 (.080) and −.128 (.091). With fixed effects they are .039 (.062) and .032 (.076). In both cases, Dlargerfirm and Dlargerfirm*D2004 have large, significant and respectively negative and positive coefficients.

[20] Coefficients (s.e.'s) for PDIP-GOLKAR and PDIP-GOLKAR*time2004 respectively in column 3 are [−.060 (.080), .211 (.067)] under 2SLS.

[21] Coefficients (s.e.'s) for PDIP-GOLKAR and PDIP-GOLKAR*time2004 respectively in column 3 are [.034 (.077) and .171 (.125)] if we use only the coastal and government employee variables as instruments; and [−.114 (.069), .312 (.101)] if we drop district time varying controls. Thus net effects are .21 and .20 respectively.

[22] Coefficients (s.e.'s) for PDIP-GOLKAR and PDIP-GOLKAR*time2004 respectively in column 3 are [−.022 (.069) and .206 (.080).

[23] Coefficients (s.e.'s) for PDIP-GOLKAR and PDIP-GOLKAR*time2004 respectively in column 3 are [−.033 (.078) and .325 (.108)].

[24] The manufacturing variables are from the Annual Survey of Medium and Large Size Enterprises, effectively a census of all enterprises over about 12 employees.

reflect changes in district conditions from the time of voting (1999), which might affect corruption levels in 2004. The last is a control on how perceptions might influence bribe question answers. On the political side apart from the assembly composition variable of central interest, we control for districts with no recorded vote shares.

6.2.1. Results

The main results are in Table 3. In column 1 we present ordinary Tobit results where we do not control for district socio-economic variables. In column 2, we add to column 1 the 7 district socio-economic control variables discussed above. Columns 3–5 contain IV results, with varying numbers of district controls as discussed below.

We start by looking at the overall results on firm characteristics, which hold in all columns. The bribe ratio increases over all ranges of firm employment, and it increases initially with capital stock, but peaks before the biggest firms. Being an exporter or having FDI has a weak positive effect on bribes. Chinese entrepreneurs who face discrimination and have fewer opportunities for redress in general pay significantly more bribes, with the bribe ratio rising by a 2–3 points. As we will see in Section 8, the bribe effects of capital stock and being Chinese may work indirectly through impacts on numbers of licenses "required". Results on other firm variables are reported for columns 2 and 5 in Table A5 in Appendix A2.1. In general, the cost-of-visit variable has a weak effect, as does the dummy for renting capital or not. Industry dummies don't affect other coefficients, and generally are insignificant. As reported in

Table A5 in Appendix A2.1, district socio-economic controls are insignificant. Change in religiosity for example has insignificant and tiny coefficients (given its mean and standard deviation).

6.2.1.1. Assembly composition effects.
We now turn to the key results for political effects, starting with ordinary Tobits. In column 1 of Table 3 without district socio-economic controls, the PDIP-GOLKAR coefficient is .101. In column 2 with the full set of district control variables, the PDIP-GOLKAR coefficient is minimally affected: .101 in column 1 versus .108 in column 2. These effects are half those from the pooled sample. However, as discussed earlier, in this cross-section work, we expect a downward bias in estimation of assembly effects, because voting is influenced by current corruption. In 1999, districts with lower levels of corruption tended to vote PDIP-GOLKAR. As a result the positive effect of PDIP-GOLKAR on corruption levels in 2004 will be understated, since these districts started with lower corruption levels. To correct for this bias, as well as potential problems with omitted district controls, we do MLE IV estimation,[25] as well as reporting results on other IV specifications such as 2SLS.

As noted above, we have different possible instruments. The requirement is that these instruments describing historical religious composition of districts predict voting patterns in the first round of elections in 1999, but that they are not correlated with district unobservables driving corruption in 2004. The dramatic cross-district changes in corruption observed in the prior section suggest corruption within districts can change quickly and thus is not anchored in historical conditions. But we still require that unobserved changes within districts that drove corruption levels in 2004 not be correlated with these historical variables. With this larger sample size, we prefer to use just the two strongest instruments: the share of population in a district living in coastal villages in 2000 and the percent of the population who were government employees historically in 1990, which also yields the highest partial-F's as reported in Section A2. We conduct many robustness check reported below, using different instruments, controls, and samples. Several experiments deal just with objections to the use of percent population living in coastal villages as an instrument.

6.2.1.2. IV results.
Given that non-political district controls in cross-section estimation are insignificant in estimation, the preferred IV specification is the one in column 3 of Table 3, where we omit these potentially endogenous controls. There, the PDIP-GOLKAR coefficient is .20, which is double the ordinary Tobit coefficient, consistent with the expected direction of bias. The coefficient is now the same as the net overtime PDIP-GOLKAR result in the overtime analysis in Table 2. Under a non-marginal interpretation to Tobit coefficients, a 10% increase in secular party assembly composition raises the total bribe ratio by 2.0 (with a mean overall bribe ratio in the cross-section of 3.4). As we add back in district covariates, alter instruments, or alter econometric specifications, the assembly effect in column 3 tends to be at the lower end of estimates we get.

In column 4 of Table 3, we add in base period income and manufacturing level, profitability and export behavior district variables as pre-determined economic variables, but we exclude the potentially endogenous controls (growth in income and in religiosity and perceived inefficiency of local governments). In column 5, we add in the controls on perceived government efficiency, growth in GDP per capita and growth in religiosity. In both columns, the coefficient on PDIP-GOLKAR is .24 and the added controls are insignificant as seen in Table A5 in Appendix A2.1.

Table 3
All bribes, cross-section sample, 2004.

	(1) Tobit	(2) Tobit	(3) IV Tobit [MLE]	(4) IV Tobit [MLE]	(5) IV Tobit [MLE]
'99 vote: PDIP-GOLKAR	**.101****	**.108****	**.199****	**.243****	**.240****
	(.0346)	(.0408)	(.0897)	(.0995)	(.110)
ln (employ)	2.66**	2.30*	2.68**	2.10	2.20
	(1.31)	(1.30)	(1.33)	(1.30)	(1.30)
ln (employ) sq.	−.166	−.137	−.175	−.126	−.136
	(.142)	(.140)	(.145)	(.142)	(.142)
Capital size: 500 m–1b	2.49**	2.32**	2.46**	2.42**	2.33**
	(.825)	(.849)	(.872)	(.892)	(.907)
Capital size: 1b–5b	4.15**	3.81**	4.21**	4.00**	3.89**
	(1.20)	(1.19)	(1.22)	(1.21)	(1.22)
Capital size: 5b–20b	3.15**	2.60**	3.06**	2.79**	2.61**
	(.956)	(1.00)	(.924)	(.964)	(.976)
Capital size: over 20b	1.06	.399	1.02	.831	.845
	(1.42)	(1.50)	(1.36)	(1.42)	(1.42)
Dummy FDI	.936	.560	.831	.406	.449
	(1.23)	(1.22)	(1.18)	(1.19)	(1.48)
Dummy export	.278	−.0089	.596	.0426	.089
	(.897)	(.884)	(.971)	(.881)	(.891)
Dummy Chinese	2.81**	2.25**	2.61**	2.32**	2.13**
	(1.12)	(.973)	(1.26)	(1.14)	(1.08)
Controls: no vote, ind. dummy, capital rent dummy, visit cost	Yes	Yes	Yes	Yes	Yes
Controls: lnGDPpc99, ln (no. firms01), % export01, avg. profit01	No	Yes	No	Yes	Yes
%ΔGDPpc9904, Δratio Islamic sch0006, avg "efficiency" dist. govt.	No	Yes	No	No	Yes
N [zeros]	2474 [932]	2474 [932]	2474 [932]	2474 [932]	2474 [932]
Variance σ^2 [ρ][a]	11.0**	11.0**	11.0 [−.12]	10.9 [−.16]	10.9 [−.15]
	(.813)	(.804)	(.751) [(.11)]	(.739) [.12]	(.736) [.13]

Standard errors for Tobits are based on clustering. One asterisk indicates a 10% significance level and two a 5% level.

[a] ρ's are the correlation of the error terms between the main bribe equation and the equation for vote share.

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[25] In terms of specification tests, while Wald-tests can't reject exogeneity of covariates overall in IV estimation below, the p-value is not large. If we add to an OLS specification of column 1 in Table 3, the predicted value of PDIP-GOLKAR, the t-statistic is 1.29. These weak non-rejections of exogeneity, combined with our beliefs, suggest that doing IV estimation is appropriate.

6.2.1.3. Robustness checks. We examine robustness of the results in Table 3 to use of different instrumental variable strategies, to imposition of a Tobit in estimation, and to over-sampling of firms in certain districts in 2004. As a summary before we start, robustness exercises indicate the IV coefficient of .20 is a conservative estimate.

6.2.1.3.1. Instrumental variables analysis. First we experiment with different instruments. In column 4, with the 1990 government employee variable and percent population in coastal villages in a district as instruments, we got a PDIP-GOLKAR coefficient of .24. If we use only percent Hindu and percent Christian in 1995 as instruments, the coefficient (s.e.) is .25 (.13). If we use all 4 instruments, the coefficient (s.e) is .23 (.085). Focusing on the use of percent population in coastal districts as an instrument, we report on three experiments. First we add the percent population in coastal villages as a covariate (which has an insignificant coefficient) and use as an instrument that variable interacted with north coast of Java where *santri* Muslims dominate. With this, the PDIP-GOLKAR coefficient (s.e.) becomes 0.293 (.139), although instruments are weaker.[26] Then we restrict the sample to firms in coastal districts, so the instrument on percent of population living in coastal sub-districts just defines variation within coastal districts. For this sub-sample of 1300 firms for the column 4 specification, the PDIP-GOLKAR coefficient (s.e.) is .256 (.120). Finally we drop firms that are located in major ports covering 10 districts of DKI Jakarta, Cirebon, Semarang, and Surabaya. For this sub-sample of 2202 firms the PDIP-GOLKAR coefficient (s.e.) becomes 0.181 (.097).

6.2.1.3.2. Imposition of a Tobit specification. To check that the Tobit is not a basic misspecification, we re-estimated columns 3–5 by 2SLS. The 2SLS estimates of the PDIP-GOLKAR effect in columns 3–5 all lie in the range .13–.15 (consistent with the 65% non-zero dependent variable observations, for a Tobit estimate of .20–.24). The use of a Tobit also raises issues of heteroscedasticity and non-normality, and in any estimation there is also an issue of heterogeneity of responses. One approach to addressing these issues is to use quantile (or LAD) estimators. In the literature, there is now implementation of censored quantile models and of (uncensored) IV quantile models. However, while there are a few recent theoretical papers (e.g., Blundell and Powell, 2004; Honore and Hu, 2004) that explore censored quantile models with endogenous covariates, there has been no implementation nor general approach developed, let alone one that accounts for clustering. As an experiment, we estimate the model with a sample of the firms at the 75th percentile of bribe activity in each district. That almost eliminates censoring (7 censored observations), has only one observation per district, allows for heteroscedasticity, and looks at the response to assembly composition for firms at the higher end of bribe activity. The 2SLS estimate of the PDIP-GOLKAR coefficient (s.e.) is large at .331 (.133) (Sargan *p*-value of .88). The result hints that assembly composition effects could be stronger at the higher end of bribe activity.

6.2.1.3.3. Other checks. Our 2004 sample of firms is non-random, with over-sampling of firms in a few districts in 2001. We have not weighted in estimation, because we don't know the relevant population of firms in 2004 to create exact weights for our full sample. However, to see if lack of weighting is an issue, we constructed one experiment using weights derived from the economic census, getting results that are consistent with Table 3.[27] We worried about firm

characteristics being endogenous, but experiments to try to correct for that left the political result unchanged.[28] We also omitted firms in the 10 no-vote districts, in case their inclusion affects results, given non-reporting may not be random. For this sub-sample of 2184 firms, the IV PDIP-GOLKAR coefficient (s.e.) is .190 (.090).

7. Modeling other local political considerations

We modeled and estimated assembly composition as having a continuous effect on bribe activity. That decision in part was based on results we explore in this section, showing a lack of discontinuities. We also note the evidence indicates that assuming linearity to effects as we did is reasonable.[29] A second set of issues has to do with whether bribe activity is affected by the degree of political competition within districts and whether there are spillovers across districts.

7.1. Discontinuities in political effects

We start by looking for discontinuities in political effects, following regression discontinuity approaches (Van der Klaauw, 2002). For a sharp discontinuity, in an ordinary Tobit, we controlled for a cubic in vote shares and tested for a discontinuity with a dummy variable for when the PDIP-GOLKAR share tops 50%. We also did this for PDIP vote shares alone. PDIP-GOLKAR has over 50% of the vote in 71% of the districts, although a single party only holds the majority in 11% of districts. In 29% of districts, PDIP-GOLKAR has over 60% of the vote, and in 13% of the districts under 40%. For either PDIP-GOLKAR or PDIP alone, the coefficient on the dummy variable for having a majority of assembly seats is insignificant with a negative sign, when a positive one is expected. We also looked at the 26 districts where PDIP-GOLKAR vote shares lie between 45 and 55%; a dummy variable for being over 50% is insignificant and negative.

There remains the issue of whether the sponsoring party of the assembly-elected *bupati*, or premier of the district, has any impact. Traditionally *bupatis* have strong powers. However the period we look at is a transition one where *bupatis* move from being appointed to being elected by assemblies. An evaluation is complicated the fact that, post-2004, *bupatis* start to face direct election. However in 2004, the *bupatis* in office are beholden to assemblies. As such, we expect the policies of the *bupati* to reflect those of the median assembly member, and our results are consistent with that notion. First, we checked with simple probit analysis that the chances a selected *bupati* was sponsored by either PDIP or GOLKAR is increasing in the PDIP-GOLKAR vote share in 1999, which it is. We note there is no discontinuity in that selection process—as, for example, when one or both parties top 50% of the vote or attain a plurality.

Then we turned to bribe ratio specifications. Controlling for PDIP-GOLKAR vote share, the party sponsorship of the *bupati* has no affect

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[26] The partial-*F* falls to 4.8 in this formulation, compared to an *F* that is typically over 7.5 as reported in Section A2.

[27] In drawing our overall 2004 sample, we used the census bureau's [BPS] list of medium- and large-sized firms (most firms over about 12 employees) in 2003. In over-sampling in districts with few firms, we sometimes extended the sample into smaller-sized firms. In the experiment, we restrict the estimating sample to all firms over 12 employees (2272 of 2474), and use our sample count relative to the BPS count in 2003 as weights for each district in estimating a weighted IV Tobit. With weighting, the PDIP-GOLKAR coefficient (s.e.) is .25 (.11), in column 3 of Table 3.

[28] We estimated the model, allowing the different firm size variables and FDI and export status to be endogenous by removing these variables and substituting in controls for local economic conditions that should determine firm characteristics—a measure of market potential, average employee compensation from the annual survey of manufacturers, indirect taxes (which are mostly local property taxes) over capital stock as a proxy for the local cost of capital, and the number of own-industry enterprises as a source of local scale externalities. These variables are from the mid-1990s. In the revised formulation, the IV vote share coefficient is .21, suggesting the indirect effects of corruption through firm size are not an issue.

[29] Ordinary Tobit results from dividing PDIP-GOLKAR vote shares into a series of dummy variable categories (<40%, 40% to <50%, 50% to <60%, 60% to <70% and ≥70%) suggest a sharper increase in bribing as we move into the last two categories, although when allowing for a simple differential in slope coefficient beyond 50%, such an effect is zero. We then experimented in ordinary and IV Tobit estimation with quadratic and cubic formulations. A cubic doesn't produce significant results. A quadratic specification has suggestive ordinary Tobit results [coefficients (standard errors) of −.241 (.161) for PDIPGLKR and .00350** (.00172) for PDIPGLKRsq.] but completely insignificant IV ones. In general there is not sufficient evidence of non-linearity to make that the main approach.

Table 4
Other political considerations (covariates added to Table 3, columns 1 and 3 formulation).

	Tobit	IV Tobit MLE	Tobit	IV Tobit MLE
	(1a)	(1b)	(2a)	(2b)
Share '99 vote PDIP-GOLKAR	**.105****	**.265****	**.101****	**.223****
	(.0331)	(.0949)	(.0344)	(.0925)
Political competition: HHI of vote shares	7.86	18.6		
	(5.12)	(25.3)		
Avg. bribe ratio 2004 in contiguous districts			.0502	−.195
			(.332)	(.503)

Instruments for column (1b) are % population living in coastal villages 2000, % pop Christian in 1995, and % population government employees 1990 (for districts with 1999 vote shares). For column (2b), the average in contiguous districts of % population living in coastal villages, of % pop Christian, and of % pop government employees are added as instruments.
Standard errors for Tobits are based on clustering. One asterisk indicates a 10% significance level and two a 5% level.

on bribe activity per se. For example, in a regression discontinuity approach with a cubic in the vote share measure, a dummy variable for PDIP-GOLKAR being the sponsoring party for the *bupati* has an insignificant and negative coefficient. What happens if we remove the PDIP-GOLKAR vote share control leaving just the covariate for whether the bupati was sponsored by PDIP-GOLKAR? Then one might expect a positive coefficient for PDIP-GOLKAR being the sponsoring party. For the columns 1 and 3 formulations in Table 3, in an ordinary Tobit the coefficient (s.e.) on that variable is .018 (.917), while for the IV MLE Tobit it is .35 (2.3).[30] We believe the lack of effect stems from two sources. First the assembly composition effect is continuous, while a *bupati* dummy variable is looking for a regime effect. Second, a number of *bupatis* who are sponsored by PDIP or GOLKAR are also sponsored by other parties and thus are a con-sensus candidate, in keeping with the reputed Indonesian proclivity toward decisions by consensus.

7.2. Political competition and spillovers

Given overall assembly competition, does the degree of "political competition" affect corruption? It could be that, in districts where votes are less concentrated, or more spread across parties, there is a greater degree of "competition." That could induce, say, less corruption, because the governing coalition is more responsive to voters in its attempt to later retain office. In Table 4, we examine overall competition by the degree of vote concentration as measured by a standard Hirschman–Herfindahl index: the sum of squared vote shares of each of the 40 parties in 1999. The higher the index, the more votes are concentrated. In column 1a and 1b in Table 4, this variable is positive suggesting that political competition matters, but it is insignificant in both the IV and regular Tobit specifications.

In columns 2a and 2b of Table 4, we look for spillover effects, whereby bribe patterns in neighboring districts affect bribes in the own district. Such an effect might arise from "yardstick competition", where politicians, journalists, or prosecutors in one district look to neighbors to see what level of corruption seems acceptable (Besley and Case, 1995). For each district, we add the average over contiguous neighbors of the 2004 bribe ratios in those districts. In both the ordinary and IV Tobits, the coefficient is small and insignificant. We also experimented with spillover effects of PDIP-GOLKAR vote shares

in contiguous districts. Again the effects are insignificant and the own PDIP-GOLKAR coefficient is the same or larger than in Table 3.

8. Other effects of assembly composition

The results so far for assembly composition effects include both direct and indirect effects on bribes. In Henderson and Kuncoro (2006) we look at a more structural model and empirical approach where assembly composition can affect bribes by affecting the level of harassment of firms and the amount of red tape, as well as directly. Red tape is measured by the number of required licenses and retribution, and harassment is measured by the number of visits to the plant made by local officials in the past year. Identification of direct and indirect effects on bribes is strained. Here we simply look at the effect of assembly composition on harassment and red tape. The results will shed light on the question of whether districts that elect more secular representatives not only have more bribes but also more red tape; and, correspondingly, whether harassment is also related to assembly composition.

Basic results are reported in Table 5. We start with the harassment, or visit equation, where visits are determined by firm characteristics and the cost of visits. For the cost variables, besides the population-weighted average of distance from villages in the firm's sub-district to the district capital used earlier, we add in the proportion of the population living in coastal areas in the sub-district (as raising transport costs) and a variable representing the ease of visiting multiple local plants, the ratio of total manufacturing establishments to land in the sub-district from the PODES. Columns 1 and 2 report

Table 5
Harassment and red tape.

	Visits		Licenses and retributions	
	Tobit	IV Tobit (MLE)	OLS	2SLS
'99 vote share: PDIP-GOLKAR	**.0518***	.0904	**.0309***	.0575
	(.0230)	(.0829)	(.0180)	(.0434)
ln (employ)	3.63**	3.68**	.777	.793
	(.884)	(.874)	(.502)	(.496)
ln (employ) sq.	−.272**	−.280**	.0669	.0637
	(.101)	(.0995)	(.0596)	(.0589)
Capital size: 500 m–1b	2.43**	2.41**	2.00**	1.98**
	(.620)	(.617)	(.281)	(.281)
Capital size: 1b–5b	3.19**	3.21**	3.35**	3.35**
	(.957)	(.993)	(.409)	(.405)
Capital size: 5b–20b	3.34**	3.34**	4.29**	4.26**
	(1.17)	(1.15)	(.694)	(.696)
Capital size: over 20b	1.69	1.74	5.84**	5.83**
	(1.43)	(1.33)	(.625)	(.616)
Dummy: Chinese	1.26**	1.18**	1.26**	1.20**
	(.975)	(.946)	(.489)	(.478)
Dummy: export	1.96**	2.07**	1.04**	1.12**
	(.756)	(.809)	(.295)	(.303)
Avg. distance from sub-dist. To capital	−.00873	−.0105	n.a.	n.a.
	(.0181)	(.0168)		
% pop in sub-district on coast	−1.62	−2.24	n.a.	n.a.
	(4.44)	(3.86)		
District manu. enterprises/ land	1.11	.942	n.a.	n.a.
	(.965)	(.903)		
No-vote controls	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Variance σ^2 [ρ]	10.2	10.2 [−.0447]	2591	2591
	(.850)	(.779) [(.0822)]		
N [zeros]	2631 [309]	2632 [309]	2591	2591
Rsq [Sargan p-value]			.500	[.94]

Standard errors for Tobits are based on clustering; and, for 2SLS, standard errors are robust ones accounting for clustering. One asterisk indicates a 10% significance level and two a 5% level.
Instruments for columns 2 and 4 are % population living in coastal villages 2000 and % population government employees 1990 (for districts with 1999 vote shares).

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[30] This uses all 4 instruments which gives a first stage partial-*F*-statistic of 7.2 (with 2 instruments it is 4.4).

ordinary and IV Tobit results; they are virtually identical. The cost-of-visit variables have the correct signs, but they are not significant. Increases in firm employment and a move from small capital stock to a larger one, as well as being an exporter or Chinese entrepreneur, lead to more visits. For the key variable, PDIP-GOLKAR vote share, the ordinary Tobit is positive and significant at a 10% level. The IV coefficient is larger, but the standard error even more so.

Next we turn to the question of whether districts with more secular representatives have less red tape. In columns 3 and 4 of Table 5, we estimate equations for the total number of licenses and retributions per firm by OLS and 2SLS. Results indicate that license requirements increase monotonically with firm employment and capital stock size. We find that being Chinese or an exporter, or having FDI increases the licenses and retributions firms face. The Chinese effect is surely a discrimination effect: Chinese are frequently "intimidated" into subscribing to licenses that other firms typically would not be required to hold. In terms of assembly composition, the positive PDIP-GOLKAR vote share coefficient is significant at the 10% level in OLS, and larger but insignificant in 2SLS.

Together, the equations on visits and red tape suggest assembly composition may affect harassment and the amount of red tape. But the main effect on corruption is directly through reduced bribe demands.

9. Conclusions

In Indonesia, the introduction of local democracy is associated with decreased local corruption. However, specific politics matter. As local vote shares and hence local legislature shares of secular parties rise, the relative degree of corruption rises. The baseline results suggest that a secular party vote share increase of 10% raises the bribe ratio by 1.3 (marginal Tobit effect), where the mean ratio is 3.6. The raw data and IV estimation indicate that voters do respond to current corruption levels: in higher corruption areas voters tended to favor the new Islamic parties, whose local platforms emphasized anti-corruption policies. And these parties seem to follow through by offering subsequent relative corruption reductions compared to districts which favored secular parties. This is not a statement about religion in politics per se. Rather, the results based on local politics may provide optimism for democratic processes to reduce corruption and they signal a potential for new parties to garner political support in corrupt environments based on anti-corruption stances. In Indonesia, Islamic parties at the local level seemed to have credibility in voters' minds, perhaps based on the perceived personal integrity of their candidates and certainly on their record in the first election cycle. Seeing voters turn to Islamic parties in the face of corrupt incumbent assemblies suggests that corruption is a powerful political issue.

Appendix A

A1. Statistics on covariates

Table A1
Mean and standards deviations of variables for pooled sample (Table 2).

	2001 sample		2004 sample	
	Mean	Stand. dev.	Mean	Stand dev.
Total bribe ratio	7.01	11.6	4.38	9.30
% PDIP-GOLKAR vote share	47.4	19.7	43.1	22.6
Dummy: small–med size firm	.224		.165	
Dummy: medium size firm	.261		.196	
Dummy: large size firm	.240		.297	
Dummy: export	.501		.371	
Dummy: no district recorded vote	.106		.177	
Ratio: Islamic to state schools	.133	.0999	.282	.210
ln (GDP per capita)	1.72	.694	2.27	.791
Perceived average inefficiency	3.47	.358	3.28	.284
N	733		1129	

Table A2
Means and standard deviations for 2004 cross-section (Tables 3–5).

	Mean	Stand. dev
Total bribe ratio	3.58	8.33
% PDIP-GOLKAR vote share 99[a]	52.9	11.6
ln (firm employment)	3.94	1.33
HHI of vote shares	.235	.0972
Avg. profit manu. firms in district, 2001	2.13	4.08
ln (no. of manu. firms in district 2001)	4.80	1.06
% manu. firms exporting in district	10.7	10.5
ln (district GDPpc 99)	1.62	.758
% change in ln (district GDPpc), 1999–2004	.640	.244
Perceived avg. efficiency of local govt. by firms in district	3.27	.327
Avg. bribe ratio: contiguous districts	2.73	1.79
Ratio: Islamic to state elementary schools 2000	.146	.151
Change in ratio: Islamic to state elementary schools, 2000–2006	.159	.128
Percent population Christian, 1995	4.34	4.50
Percent population Hindu, 1995	.935	1.57
Percent population in government 1990	1.86	1.07
Percent population in villages on the coast	4.73	6.93
Red tape bribe ratio	1.77	4.42
ln (no. of visits)	6.56	9.90
ln (no. of licenses and retributions)	2.01	.657
Avg. distance from sub-district to district capital	20.2	14.9
Fraction pop. in sub-district on coast	.0495	.112
No. of district manu. enterprises/land	.121	.252
Dummy: capital 500 m–1b	.116	
Dummy: capital 1b–5b	.115	
Dummy: capital 5b–20b	.0776	
Dummy: capital over 20b	.0424	
Dummy: rent capital	.0384	
Dummy: export	.245	
Dummy: FDI	.0804	
Dummy: Chinese owner	.120	
Dummy: no vote	.117	
Dummy: DKI Jakarta	.0562	

[a] Generally the sample size is 2474 for these calculations. However the PDOP-GOLKAR numbers are for the 2186 firms in districts with no voting.

A2. Instrumental variables analysis

This part of the Appendix reports on the strength and validity of instruments used in the paper. Given the small sample of districts in the pooled sample in Table 2, we report on fewer experiments with instruments and reserve the more detailed analysis for the larger cross-section sample, where results are under more suspicion. Table A4 contains a sample of first stage regressions for the IV work in the paper. We do not report standard errors to save space, but report whether a variable is significant at the 10% (*) or 5% or better (**) level.

A2.1. IV work for Table 2

Column 1 in Table A4 presents the key first stage regression for column 3 of Table 2. In column 1, the only significant variables explaining variation in DPIP-GOLKAR are % population living in coastal villages in a district, government employees per capita 1990, and a dummy variable for no-vote districts. The last is significant by construction since the dependent variable has zero values for this variable. A properly calculated partial-F for a district (not firm) level variable for this equation is a reasonable 10.9.[31] However if we drop the % Hindu and % Christian instruments the partial-F drops to 6.4. The other instrumental variable for column 3 in Table 2 is D2004*PDIP-GOLKAR but many of the first stage coefficients for this variable are not-identified (everything not interacted with D*2004). For column 4 in Table 2 (not reported in Table A4) with district fixed effects added,

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[31] An issue in calculating first stage F-statistics is that they are for a variable that is only at the district (not firm) level. Corresponding to clustering of error terms in calculation of standard errors, partial-F's based on changes in Rsq that ignore this issue are very high (over 100).

Table A3
Simple correlation matrix (2004 firms in districts with voting; N = 2184).

	1. Bribe ratio 04	2. Vote	3. Δ Vote	4. Income	5. Relig.	6. Δ Relig.	7. Firms	8. Profit	9. Export	10. Δ Income	11. Chris	12. Hindu	13. Coast	14. Gov.
2. PDIP-GOLK vote 99	.10													
3. Δ vote 1999–2004	−.13	−.56												
4. income ln (GDPpc99)	.10	.13	−.50											
5. relig (ratio Islamic/state sch.)	−.05	−.23	.37	.06										
6. Δ relig 2000–2006	.01	−.20	.28	.09	.32									
7. ln (no. manu firm 2001)	.10	−.07	−.27	−.13	.08	.12								
8. profit rate 01 (profit/cap)	−.01	.22	−.14	.05	.00	−.10	.26							
9. % manu firm export 01	−.00	−.31	.21	−.06	.06	.09	.06	−.21						
10. Δ income 2004–1999	.03	.26	−.14	−.04	.06	.09	−.13	.05	−.06					
11. % pop Christian 1995	.08	.31	−.57	.07	.29	−.29	.14	.12	.04	.07				
12. % pop Hindu 1995	.06	.09	−.31	.12	.07	−.00	.21	−.10	.00	.12	.23			
13. % pop in coastal villages 00	−.08	−.37	.25	−.09	.12	.10	−.05	−.06	.14	−.09	−.22	.05		
14. % pop govern. emp. 90	.06	.27	−.43	.07	−.20	−.24	−.01	.01	−.09	.07	.28	.28	−.18	
15. per. eff. of local govt. 04	.09	.05	−.14	−.05	.23	.11	.16	−.03	−.02	−.05	−.05	.19	−.13	.10

the partial-F on the political variable, D2004*PDIP-GOLKAR, is 10.5 with the coastal and Christian (each times D2004) instruments significant at the 5% level.

In terms of validity of instruments, as an informal test, when we add our instruments to the ordinary Tobits in columns 1 and 2 of Table 2, there is no effect on assembly composition coefficients. If our instruments were correlated with unobservables driving corruption changes and if assembly composition is endogenous in the model and correlated with our instruments, then inclusion of instruments should absorb some of the correlation between unobservables and assembly composition measures and alter assembly composition coefficients. That does not happen.[32] Also, in the 2SLS version of column 3 of Table 2 the Sargan p-value is .44.[33]

A2.2. IV work for Table 3

Column 2 of Table A4 gives the first stage regression for column 3 of Table 3. Apart from the dummy for a firm exporting, again the only significant variables are % population living in coastal villages in a district, government employees per capita 1990, and the dummy for no-vote districts. Here the partial-F for this district level variable is 7.5. Adding % Christian and % Hindu to that equation drops the partial-F to 5.2. Adding % population in coastal villages as a covariate and making the instrument that variable interacted with whether a district is on the north coast of Java (as well as government employees per population in 1990) reduces the partial-F to 4.8. For the two instruments we use in columns 4 and 5 of Table 3, the partial-F's are respectively 7.4 and 5.6. In these two other first stage regressions, the percent firms exporting has a significant negative sign and for column 5 the % growth in GDP per capita '99-'04 has a significant positive sign.

In column 3 of Table A4, we report a "placebo" first stage for % growth in GDP per capita '99-'04, relevant to Tables 2 and 3. No variable is significant at the 5% level in that regression. We also did the same regression for the increase in religiosity (growth in ratio of Islamic to secular schools). Again our instruments are completely insignificant in that regression (coefficients (s.e.'s) of −.010 (.012) and .0014 (.0018)).

Finally, as an informal test of validity, if instruments are correlated with error terms, they should absorb the correlation of PDIP-GOLKAR with bribe activity reducing the PDIP-GOLKAR coefficient. If we add these instruments to the column 1 [2] of Table 3 ordinary Tobit, the

coefficient on PDIP-GOLKAR does drop modestly 18% [14%]. However the Sargan p-values on 2SLS versions of IV estimation in columns 3–5 are all over 0.82 suggesting orthogonality.

Table A4
First stage regressions and a "placebo".

	(1) PDIP-GOLKAR		(2) PDIP-GOLKAR	(3) Δ lnGDPpc '99-'04
Small–medium size	.35	ln (employ)	−.18	−.02
*D2004	−.07	ln (employ) sq.	.07	−.000
Medium size	−.22	Cap 500 m–1b	−.14	.003
*D2004	.32	Cap 1b–5b	−1.3	−.04
Large size	−.40	Cap5b–20b	−.000	.06
*D2004	3.3*	Cap >20b	−.82	.09
Export	−2.6*	FDI	1.2	−.05
*D2004	−.51	Export	−2.7**	.001
D2004	−.45	Chinese	−.64	.002
Ratio: Islamic/sec.	10.5	Food products	1.4	.007
*D2004	−7.1	Textile, apparel	1.8	−.02
D no vote	−56**	Wood products	1.4	−.02
*D2004	.17	Chemicals	2.8	−.01
ln (GDPpc)	4.0	Non-m. min.	.58	−.02
*D2004	−.70	Metal products	2.6	−.01
% coastal	−.98**	Mach and el. Mach.	1.5	−.03
*D2004	.36**	Dummy: rent equipment	−.64	.003
% govt. employ90	1.5	Dummy: no vote	−51**	.11
*D2004	−2.1**	*DKI Jakarta	.39	.02
%Hindu95	.76	Rent equip	−1.8	.003
*D2004	−.34	Cost of visit	.04	.003*
%Christian	.42	%coastal	−.60**	.−003
*D2004	.23	% govt employ90	2.45**	.03
N	1862	N	2474	2474
Partial-F	10.9	Partial-F	7.5	

One asterisk indicates a 10% significance level and two a 5% level. Errors are clustered.

A3. Non-reported coefficients from Table 3

Table A5
Non-reported coefficients for covariates Table 3, columns 2 and 5.

	Column 2	Column 5
Dummy: food products	.814	.961
	(1.68)	(1.78)
Dummy: textile, apparel	−.462	−.565
	(1.71)	(1.77)
Dummy: wood products	3.75**	3.69**
	(1.79)	(1.80)
Dummy: chemicals	3.16	2.78
	(1.92)	(2.06)
Dummy: non-metallic minerals	2.02	1.98
	(1.87)	(1.97)

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[32] Adding the four instruments (and those interacted with time) to column 1 yields coefficients (s.e.'s) on PDIP-GOLKAR and PDIP-GOLKAR*time2004 of respectively −.026 (.114) and .258 (.093). For column 2 with the 4 instruments interacted with time added to the model, the coefficient (s.e.) on PDIP-GOLKAR*time2004 is .222 (.101).

[33] For the 2SLS version of column 4 in Table 3 some test statistics are not computed because the moment conditions were not of full rank, possibly because of error clustering.

Table A5 (*continued*)

	Column 2	Column 5
Dummy: metal products	1.34	1.16
	(2.23)	(2.27)
Dummy: machinery, electrical machinery	−.228	−.501
	(2.22)	(2.24)
Dummy: rent equipment	.697	.915
	(1.24)	(1.27)
Dummy: no vote	5.60**	12.17*
	(2.54)	(5.99)
*Dummy DKI Jakarta	− 2.22	− 1.21
	(2.47)	(2.71)
Cost of visit	.0329	.030
	(.0286)	(.031)
ln (GDPpc 1999)	.966	.783
	(.650)	(.750)
ln (no. of manu. firms 2001)	.614	.659
	(.447)	(.478)
Avg profitability rate 2001	.0580	.0051
	(.101)	(.115)
% manu firms exporting 2001	.0391	.074*
	(.0365)	(.042)
ln (GDPpc2004)–ln (GDPpc1999)	.427	−.954
	(1.24)	(1.90)
Perceived avg. inefficiency of local government	1.62	1.37
	(1.14)	(1.14).
Change: ratio of Islamic schools 2000–2006	1.02	3.32
	(3.23)	(3.99)

Appendix B. Firm fixed-effect results

We have 178 firms in 2001 that were resurveyed in 2004. We note these are larger than typical firms in either time period in the 37 districts (for example by 2004, 53% are in the largest size category of employment, compared to 30% for the overall sample). For the 178 firms, the number reporting bribes fell from 136 in 2001 to 131 in 2004, although there are substantial changes in who pays and who doesn't.[34] For those reporting bribes, bribes as a share of costs fell from a mean of 8.04 to 7.88, and the average bribe ratio over all firms of 6.1 in 2001 declines to 5.8 in 2004. These differences are insignificant, but a sign-rank one-tailed test on whether bribes in 2004 are lower than in 2001 is on the borderline of significance (*p*-value of .051).

A key reason to explore bribe behavior by the 178 firms who overlap time periods is to control for unobserved firm characteristics such as "slickness in avoiding bribes," through use of firm fixed effects. For the Tobit formulation in the paper, fixed-effect Tobit estimation with just two observations per firm is strongly biased. However, "conditional" or fixed effects logit estimation is not, where, with a fixed-effect logit, identification comes from firms which switch bribe–no bribe status.[35] Among our 178 firms, 40 that paid bribes in 2001 reported absolutely zero bribes in 2004, while 30 firms that paid no bribes in 2001 reported bribes in 2004.

The key logit results concern time effects and political parties and are reported in Table B1. There are four districts with no recorded political votes. In column 1 a pure time effect is negative but insignificant. In column (2), we interact the time effect with vote shares to see the impact of assembly share of PDIP-GOLKAR on 2004 bribes (where assembly share then alters the time effect, noting that, with fixed effects, a coefficient just on assembly share is not identified). The base time effect is negative and significant, but the

time effect is less in districts that voted more for PDIP-GOLKAR, implying legislature composition is correlated with changing bribe activity. Point estimates suggest the negative time effect reverses in districts with heavy PDIP-GOLKAR support (over 56%). These logit results support the basic results in Table 2. However, magnitudes from fixed-effect logit coefficients are difficult to interpret, since we can't anchor initial probabilities, and fixed-effect estimation assumes that firm covariate effects are constant over time, which from Table 2 seems not to be the case.

Table B1
Bribe or not, 2001 versus 2004. "Fixed effects" logit; 178 firms in 24 districts.

	Bribe or not	Bribe or not
Small–medium size (500 m–1b)	.00391	− .0728
	(.579)	(.611)
Medium size (1b–5b)	− .0705	.378
	(.679)	(.742)
Large size (>5b)	− .00590	− .358
	(.735)	(.817)
Dummy: export or not	.996	.968
	(.680)	(.747)
ln (no. of licenses)	n.a.	n.a.
Time dummy, 2004 [D2004]	**− .0775**	**− 3.33****
	(.281)	(1.27)
D2004* share '99 PDIP-GOLKAR vote		**.0598****
		(.0234)
Control: no vote, no vote*time dummy		Yes
N [no. firms]	140 [70]	160 [70]
{bribe '01 and no bribe '04}	{40}	{40}

One asterisk indicates a 10% significance level and two a 5% level.

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[34] In 2001, people reluctant to answer the bribe question were given a zero, while for 2004 they were a given a missing value. Recall in the overall 2004 sample, only 2.5 are non-respondents (which are dropped as missing values in the econometrics). These numbers treat firms in the two years the same. Explicit (2004) and implicit (2001) non-respondents are given zeros.

[35] For firms reporting bribes in both periods in this small sample, no significant OLS or fixed-effect results on bribe amounts emerge in statistical analysis, although the time effect is noticeably negative.

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