Does Corruption Information Inspire the Fight or Quash the Hope? A Field Experiment in Mexico on Voter Turnout, Choice and Party Identification *

Alberto Chong
Ottawa University
120 University Ave
Ottawa, Ontario K1N-6N5
(613) 562-5800
alberto.chong@uottawa.ca

Ana L. De La O
Yale University
77 Prospect St. C122
New Haven, CT. 06510
(203) 432-5234
ana.delao@yale.edu.

Dean Karlan
Yale University
P.O. Box 208269
New Haven, CT 06520-8269
(203) 432-4479
dean.karlan@yale.edu

Leonard Wantchekon
Princeton University
230 Corwin Hall
Princeton, NJ 08544
(609) 258-6723
lwantche@princeton.edu

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Abstract

Retrospective voting models assume that offering more information to voters about their incumbents' performance streghtens electoral accountability. However, it is unclear whether incumbent corruption information translates into higher political participation and increased support for challengers. We provide experimental evidence that such information not only decreases incumbent party support in local elections in Mexico, but also decreases voter turnout and support for the challenger party, as well as erodes partisan attachments. While information clearly is necessary to improve accountability, corruption information is not sufficient because voters may respond to it by withdrawing from the political process. We conclude with a discussion of the implications of our findings for studies of voting behavior.

Keywords: Corruption; Voting behavior; Party Identification; Turnout; Accountability

Most models of retrospective voting posit that informing voters on the quality of politicians enhances the likelihood that well-performing incumbents retain their position, and that poorly performing incumbents are ousted (Manin et al. 1999). Then, information on incumbent corruption, purportedly a measure of quality, should help challengers, by either shifting votes to the challenger or engaging individuals who would otherwise abstain.

However, information about corruption could lead to less voting. Previous work suggests that perceptions and experiences of corruption undermine voters' confidence in public institutions (Bowler and Karp 2004; Clausen et al. 2011; della Porta 2000), erode the legitimacy of the political system, reduce trust in politicians and civil servants (Anderson and Tverdova 2003; Eek and Rothstein 2006; Morris and Klesner 2010; Pharr 2000; Richey 2010; Seligson 2002), and lower voters' confidence in their government's ability (Caillier 2010). Similarly, if voters perceive corruption information as negative advertising, then such information could weaken citizens' "confidence in the responsiveness of electoral institutions and public officials" (Ansolabehere et al. 1994, 835). If the exposure of corruption leads voters to believe that voting will not benefit them–either because they lose trust in governments or those governments' ability to respond to constituents' needs–then they are likely to abstain.

Information about corruption could also decrease the support of challenger candidates. In a corrupt environment, only a political challenger who is not already deeply embroiled in and compromised by ongoing corrupt transactions offers a credible prospect for better governance. The revelation of the incumbent's corruption may lead voters to reevaluate the likelihood that challengers have the capacity to reduce corruption. Once corruption reaches a certain level, voters may interpret it as an equilibrium from which individual politicians, especially "low quality" ones, cannot credibly withdraw. Hence, support for the challenger may also drop. In other words, if corruption information leads voters to reevaluate the utility differences between candidates and deem them negligible, then, in accordance with a decision theoretic analysis, voters will not bother to vote because the costs of casting a ballot would be bigger than the benefits (Downs 1957) or perhaps, the regret of abstaining when their preferred candidate looses is minimal (Ferejohn and Fiorina 1974).

The empirical record shows that exposing corruption leads to incumbent vote loss although not necessarily ouster (Chang et al. 2010; Ferraz and Finan 2008; Peters and Welch

1980; Winters and Weitz-Shapiro 2012).[1] The scant evidence from observational studies about the effect of exposing corruption on electoral turnout is inconclusive.[2] Recently, a few field experiments have shown that offering people information on incumbents' performance has either no effect (Banerjee et al. 2010; Humphreys and Weinstein 2012; Malesky et al. 2012), or has a negative effect on electoral turnout (de Figueiredo et al. 2011).

Most studies to date focus on how voters sanction or reward individual incumbents upon learning about their behavior while in office. Yet a large number of political offices across countries are subject to term limits (Johnson and Crain 2004). We know that term-limited incumbents exert less effort (Alt et al. 2011; Besley and Case 1995 & 2003), and are more corrupt (Ferraz and Finan 2011) than incumbents without term limits. But we do not know how does information about incumbents who have exhausted their time in office affect the support of the incumbent party and challenger parties. We conducted a field experiment in Mexico, where elected officials face one-term limits, to answer this question.

We randomly assigned voting precincts to a campaign spreading information on corruption and public expenditure conducted one week before the 2009 municipal elections in Mexico. At the time when the information dissemination occurred, political parties were by law no longer allowed to campaign and therefore had no opportunity to respond to the information in their own canvassing. Thus, our intervention primarily offered information to voters about the performance of their mayors. Our campaign consisted of distributing flyers door-to-door in selected voting precincts. The information provided was taken from publicly available audit reports produced by the Mexican Federal Auditor's Office (ASF). All flyers stated that it was the mayor's responsibility to provide public lighting, safe water, sewage, and local roads. Also, all flyers included information on the total amount of resources available to the mayor in that particular municipality to invest in public services, and the amount the mayor actually spent. In the first of three treatment groups, the "corruption information" group, the flyer included information about the percentage of resources the mayor spent in a corrupt manner. Our definition of corruption is public spending with some form of irregularity such as over-invoicing, fake receipts, diverting resources, fraud, etc. The two other treatment groups were placebos. In one, the flyer included only information about the percent of resources spent by the end of the fiscal year: this was the "budget expenditure"

group. In the other, the flyer included information about the percent of resources mayors directed toward improving services for the poor: this was the "poverty expenditure" group. Because these two treatments are not related to corruption, we pool them together as a general placebo category.[3] We also had a control group, which received no information.

Using electoral data at the voting precinct level, we find that our corruption information treatment led to a 2.5% decrease in turnout, and a 2.5% decrease in votes for the incumbent party and votes for the challengers parties (as a share of registered voters). To get a sense of the substantive importance of these effects, consider that the average margin of victory was 8% (with a standard deviation of 4%) among municipalities in our study. We also find that exposing high levels of corruption led to larger behavioral effects than exposing low levels of corruption. We supplement the administrative government voter records with survey results, which show that, for the most part, our corruption information treatment did not change people's widespread belief that the municipal government is dishonest. However, when the corruption exposed is high, it did lead to an increase (from an already high level) in the belief that the municipal government is dishonest. Hence the more modest overall effect on voting behavior and the larger effect when corruption is high. Also, incumbent corruption information decreased substantively the probability that a person identifies with the corrupt incumbent's party. We collected data on candidates' job prior to the election to explore why our treatment affected challengers' support. We find that disseminating corruption information led to larger effects when the challenger parties' candidates were local congressman, one the most discredited jobs in Mexican politics.[4] This finding is compatible with information about corruption leading voters to think that no politician, especially low quality ones, can credibly withdraw from corruption.

Information about corruption and voters' behavior

Voters need information to discern representative from unrepresentative governments (Manin et al. 1999). In line with this argument, many studies show that improving information availability (via increased media access) causes electoral gains for better performing politicians (Banerjee et al. 2010), promotes government responsiveness (Besley and Burgess 2002), contains opportunistic behavior (Besley et al. 2005), prevents widespread theft of public

resources (Adsera et al. 2003, Reinikka and Svensson 2005) and improves the performance of representatives in parliaments (Snyder and Stromberg 2010). However, not all evidence is so optimistic. Humphreys and Weinstein (2012), for example, show that offering people information on the performance of Members of Parliament (MPs) in randomly selected sites in Uganda, and letting MPs know, had no effect on MPs' performance or their re-election rates. Similarly, Malesky et al. (2012) find no evidence that randomly increased transparency improves delegate performance in Vietnam.

Our understanding of the electoral effects of corruption information is limited because much of the literature focuses on governments' response to increased transparency (Ferraz and Finan 2008). Moreover, among studies on voters, few examine voter turnout.

Observational studies on the effect of exposure of corruption on turnout produce mixed results. Peters and Welch (1980) find no effect of corruption scandals on turnout in the U.S. Kostadinova (2009) finds that among post-communist countries perceptions of corruption have a small mobilizing effect. Caillier (2010) finds that people who perceived that corruption was on the rise were less likely to vote in Louisina. McCann and Domínguez (1998) show that perceptions of electoral corruption decrease turnout in Mexico. This pattern seems to hold more generally (Davis et al. 2004, Simpser 2004). Finally, Bauhr and Grimes (2011) show that in countries with high corruption, increases in governmental transparency demobilize voters. On the other hand, a few studies report that voters' abstentions associated with corruption hurt opposition parties more than the incumbent (Davis et al. 2004, McCann and Dominguez 1998).

These mixed results could reflect that studies address heterogeneous cases of corruption, and there is no reason to assume that various forms of corruption produce the same effects. Moreover, three methodological issues are prevalent in this literature. First, most of these studies use self-reported perceptions and exposure to corruption, which is prone to measurement error because people are unwilling to admit their involvement in corruption (Rothstein 2009), or they inflate their perceptions of corruption in response to their partisanship. Furthermore, perceptions and experiences of corruption capture indirectly the information available to voters about the misuse of public resources by incumbents. Second, many studies use self-reported voting behavior, which is prone to social desirability bias.

Third, recall of corruption can be caused by, and be itself a cause of, self-reported turnout. For example, people who abstain may justify not turning out to vote by expressing the view that corruption is widespread. Furthermore, acquisition of information is endogenous to participation and vote choice. Informed citizens are different from uninformed citizens. Thus, comparing the electoral behavior of the informed and uninformed may conflate preexisting differences between these two groups with the effect of information on corruption.

To circumvent these methodological challenges, recent work examines the effects of random variation in corruption information. Ferraz and Finan (2008), for example, compare the electoral returns of incumbent mayors randomly selected to be audited before the 2004 election in Brazil to the returns of incumbent mayors audited afterward. They find that exposing corruption hurts the incumbent's electoral performance. Ferraz and Finan (2008) do not explore how audits shape electoral turnout, which is intuitive since voting is mandatory in Brazil. However, de Figueiredo et al. (2011) find that exposing the criminal record of the left-party candidate in a 2008 Brazilian election had a negative effect on voter turnout despite the mandatory voting. They also find that exposing the criminal record of the center-right candidate had no effect on election outcomes. Similarly, Banerjee et al. (2010) find that information about candidates' criminal records in India had no significant effect on electoral outcomes. We take a complementary approach and study the effects of corruption information drawing from a field experiment, which allows us to study how exposing an incumbent as corrupt affects citizens' beliefs and opinions about their government and public services, their political behavior, and their partisan attachments.

Mayors, local expenditures, and federal audits in Mexico

Municipal authorities are elected to serve three-year terms, and, like all other elected officials in Mexico, have single-term limits. Scholars have typically assumed that voters punish or reward the incumbent party for individual incumbents' performance. However, there is little evidence that electoral competition influences municipal government performance (Cleary 2007), and incumbent parties are strongly entrenched (Diaz-Cayeros 2005).

Mayors are in charge of providing basic public services to the municipality, including garbage collection, sewage treatment, electricity, local roads construction and maintenance,

and public safety. Despite optimistic views about fiscal decentralization, local governments' performance has remained poor (Pardinas 2008), and voters have insufficient information about service delivery (Keefer 2007). The legacy of six decades of fiscal centralization left behind a variety of misconceptions among voters. For example, less than half of our survey respondents identified correctly that mayors are responsible for the sewage systems, the provision of clean water and public lighting. Also, respondents think that mayors have insufficient resources to provide basic social services. Even if decentralization has changed the amount and allocation criteria of federal transfers, local governments commonly blame a higher level of government for the lack of service delivery. As a result, political responsibility is diluted [5] and perceptions of corruption are very high.

As an institutional response to the misuse of federal resources, a constitutional reform in 1999 established the creation of the ASF, which is an auxiliary entity to the Lower House of Congress, but has constitutionally granted management autonomy.[6] On a yearly basis, the ASF selects municipalities in each state to be audited according to fixed criteria, which prioritize municipalities with higher allocations of federal transfers, with higher variation in federal transfer amounts across years, and without audits in prior years.[7] Thus, ASF's algorithm –not publicly available– selects a mix of municipalities, which often includes urban and rural, as well as big and medium size places.

In the selected municipalities, the ASF examines public accounts in search of accounting irregularities, deviations from guidelines outlined by the budget and program objectives, and misuse of resources. The auditors inspect public works and physical investment to verify that expenditures are in accordance with the budget, specifications and costs stipulated in contracts. Then, ASF issues monetary sanctions and initiates proceedings against appropriate public servants. All audit reports are presented simultaneously to the Lower House of Congress and then are made publicly available on the ASF's website.

Experimental design and implementation

In the selection of our experimental sites, we took into account a few factors. First, our budget allowed us to work in three of the six states that held only municipal elections in 2009: Jalisco, Morelos and Tabasco. These three states are located in different geographical regions

in the country. Figure A1 in Supplemental Information (SI) file contains a map indicating the location of our experimental sites. In each state, we selected all municipalities audited by ASF the previous year. This criterion, which was necessary because our information campaign relied on the availability of audit reports, left us with 12 municipalities, including 3 state capitals and 9 municipalities of varying levels of development.[8] Table 1 lists the 12 municipalities in our study. In each municipality, we assigned all voting precincts to one of our four treatment conditions. The total number of voting precincts in our study is 2,360.

For our flyers, we collected data on mayors' use of the Fund for Social Infrastructure (FISM) from the audit reports corresponding to the 2007 auditing process, which was the most recent year available and corresponded to the term of the municipal government in office up to the 2009 elections. FISM is an earmarked federal transfer scheme to municipalities intended to improve the provision of public services. In average, municipalities in our study received 60 million pesos in 2007 (about 4.5 million US dollars), or about 244 pesos per capita. As a benchmark, consider that revenue from municipal taxes is 386 pesos per capita, in average. Thus, FISM is equal to 63% of the resources municipalities collect from local taxes. To be sure, FISM is one of the many federal transfer schemes allocated to municipalities. Still, it constitutes 35% of the total federal earmarked funds allocated to municipalities in our study, in average. Also, according to municipal treasurers, who were surveyed by Mexico's Statistics Office in 2008, a substantial percentage of the total infrastructure expenditures in the municipality is paid with FISM. Table 1 lists this by municipality. Few survey respondents were familiar with FISM. Indeed, only about 10% of respondents had heard of it.

With respect to the information revealed by the audit reports and included in the flyers, we were surprised to learn that mayors spend in average only 56% of the money they receive from FISM. By regulation, mayors should use FISM resources to improve service delivery in poor areas. Indeed, audit reports show that in average 83% of FISM expenditures go to poor areas. Finally, audit reports detect that mayors spent 30% of FISM in corrupt manners, in average. This information is summarized in Figure A2 in the SI file, and table 1 includes the percent of corruption by municipality.

We conducted our information dissemination campaign in collaboration with Innovations

for Poverty Action, a research organization. Two local firms with experience in leafleting distributed our flyers. As mentioned in the introduction, flyers were customized for each municipality to include information on the total amount of FISM resources available to the mayor, and the amount of resources the mayor actually spent. The flyer in the "corruption information" group included a pie chart with the percentage of FISM resources the mayor spent in a corrupt manner. The flyer in the placebo groups included either a pie chart with the percentage of FISM resources actually spent or the percentage of FISM resources that the mayor spent in poor areas. The control group received no information.

We designed the flyers in consultation with a locally based graphic designer and conducted focus groups to pilot their content (see Figures 1 and 2). To establish credibility and political independence, flyers included a reference to the source of the information and a legend explaining that the informational campaign was nonpartisan. Our campaign took place approximately one week before municipal elections. All households within the boundaries of an experimental voting precinct were assigned to receive their corresponding flyer.[9]

We used block randomization, stratified on municipality, to assign our treatments. Outside of Mexico, nonpartisan leafleting experiments typically produce effects that are overall statistically indistinguishable from zero (Gerber and Green 2000, Azari and Washington 2006). Partisan leaflets, however, have been found to produce small effects (Nickerson et al. 2006). We set our sample to have sufficient power to detect a minimum effect of 2.5 percentage points.[10]

In total, 150 voting precincts were randomly assigned to each of the three interventions, for a total of 450 treatment and 1910 control precincts. We distributed 44,000 flyers per treatment. Minor problems in the field kept a small fraction of precincts from receiving full treatment. We discuss this and other non-compliance issues in detail below.

We collected four types of data. First, electoral results at the precinct level come from state electoral institutes, which also provided maps and geo-referenced voting precincts that we used for the distribution of the flyers. Second, demographic baseline characteristics come from census data originally reported at the village and block levels. Villages and blocks are units of analysis that are smaller than municipalities. In rural areas, villages are smaller than precincts. In urban areas, blocks are smaller than precincts. To aggregate

census data to the voting precinct level, we first matched voting precincts to their villages (or blocks in urban areas) using GIS. Then we calculated averages from the villages (or blocks) inside the voting precinct. Third, we conducted a survey approximately two weeks after the interventions (ten days after the elections). Since our intent was to measure the effects of the information campaign, at the time of the follow-up survey we did not expose respondents again to the information in the flyers.[11] We did not conduct a baseline survey because of budgetary constraints. Hence we do not have information on opinions and beliefs prior to our intervention. There are no precinct opinion polls that we are aware of that we could use to gather information about beliefs at such disaggregated level. Finally, we collected data on candidates' jobs prior to the 2009 election.

Table 2 shows balance in the baseline characteristics of our experimental voting precincts. The last row of the table presents the p-values of two F-tests of joint significance of all independent variables, from a regression of each treatment variable on baseline characteristics and municipality fixed effects. The last column includes the p-values of F-tests on the joint significance of all treatment variables, from a regression of each baseline characteristic on treatment dummies and municipality fixed effects.

Results

Because voting precincts randomly assigned to different treatments have comparable potential outcomes, we analyze our data based on the assignment to treatment (intent-to-treat estimates), and later on we discuss the robustness of these estimates.

We first estimate separate models following the general specification:

$$Y = \beta_0 + \beta_1 CorruptionInformation + \beta_2 NoInformation + M_j + \epsilon \quad (1)$$

The dependent variable is Y. M_j are municipality fixed effects (because the randomization was stratified by municipality). $CorruptionInformation$ is a dummy variable that refers to the group that received information about the percent of FISM spent in a corrupt manner, $NoInformation$ refers to the experimental group that received no information (i.e. the control group). The omitted group is the set of placebo groups. Choosing the placebo groups to be the reference allows us to disentangle the effect of corruption infor-

mation from the effects of other aspects of flyers. A comparison between the corruption information treatment group and the control group confounds the effect of corruption information with that of leafleting in proximity to the election and that of the information about the amount of money available to mayors. Thus, with the placebo group as reference, β_1 estimates the overall effect of corruption information. That said, we also report whether $CorruptionInformation$ is statistically different to $NoInformation$.

Previous studies have consistently found that the effect of exposing corruption depends on the severity of the malfeasance (Chang et al. 2010, Ferraz and Finan 2008). To take the level of corruption into account, we estimate two specifications for each outcome variable. In one (equation 2), we include interaction terms between $CorruptionInformation$ and the level of corruption, which is bounded between 0 and 1, linearly and quadratic.

$$Y = \beta_0 + \beta_1 CorruptionInfo + \beta_2 CorruptionInfo * CorruptionLevel + \beta_3 CorruptionInfo * CorruptionLevel^2 + \beta_4 NoInformation + M_j + \epsilon \quad (2)$$

In the other (equation 3), we include interaction terms between $CorruptionInformation$ and three dummy variables that indicate whether the level of corruption revealed in the audit was between: 0 and 33%, 33% and 66%, and more than 66%.[12] Since equations 2 and 3 include municipality fixed effects (because randomization was blocked by municipality), the main effect of levels of corruption is excluded due to collinearity.

$$Y = \beta_0 + \beta_1 CorruptionInfo * C_{0-33} + \beta_2 CorruptionInfo * C_{33-66} + \beta_3 CorruptionInfo * C_{66-100} + \beta_4 NoInformation + M_j + \epsilon \quad (3)$$

These flexible specifications allow us to explore whether the effect of corruption information is increasing with the level of corruption under different functional form assumptions.

In our block randomized experiment treatment probabilities vary by block. Therefore, following Gerber and Green (2012), we weighted each treatment observation by the inverse of the probability of treatment (which is constant within municipality, but not across).[13]

Effects of corruption information on people's beliefs and opinions

Table 3 presents the effects of our informational campaign on citizens' beliefs and opinions. The dependent variable in columns 1 through 3 takes the value of 1 when a survey respondent

agrees that the local government is dishonest, and 0 otherwise; in columns 4 through 5 it takes the value of 1 when a respondent agrees that the local government is honest, 0 otherwise; in columns 7 though 9, it takes the value of 1 when respondents approve of the mayor, 0 otherwise; and, in columns 10 through 12, it takes the value of 1 when respondents report they are unsatisfied with their public services, 0 otherwise. Since these outcomes are binary, we estimated linear probability models with robust standard errors clustered at the voting precinct level. In the SI file, we present probit models, with similar results.

Overall, corruption information does not change people's beliefs. Only when the corruption exposed is high do we observe an increase in the belief that the municipal government is dishonest, a decrease in the belief that the municipal government is honest, and an increase in dissatisfaction with public services. The set of placebo groups has no statistically significant effect on any of the beliefs or opinions.

That voters respond differently to information about high rates of irregularities in the use of municipal infrastructure funds suggests that the prior expectation was that mayors were somewhat corrupt. Voters in municipalities whose audit reports show low to moderate corruption rates (0 to 66%) are unaffected in their views about the honesty of local government when they receive this information, implying that for these voters, the information confirmed what they already believed. The negative effect on beliefs in the honesty of the local government among voters in municipalities with high levels of corruption suggests that these voters have been informed that corruption is even worse than they had believed.

Effects on electoral outcomes

Table 4 displays the effects of information on electoral outcomes. We compute turnout as the total number of votes cast in the polling precinct divided by the number of people registered to vote. Similarly, we define incumbent and challenger votes as the votes cast for the incumbent and challenger party, respectively, divided by the number of people registered to vote (and then multiplied by 100). We compute all outcomes with respect to number of registered voters because voter registration happened before the experiment, i.e., is unaffected by treatment. Therefore, we focus on experimentally induced changes in the numerators.

We find that the corruption information treatment leads to a 1.3 percentage points

(s.e.=0.32 pp) decrease in turnout. Given that turnout in the placebo group is 52%, the overall decrease in turnout amounts to 2.5%. Moreover, information about corruption leads to a 0.43 percentage point decrease (s.e.=0.2 pp) in the incumbent parties' votes, and a 0.86 percentage point (s.e.=0.26 pp) decrease in challengers' votes.

The corruption information group is statistically different to the control group. On the other hand, the set of placebos are statistically indistinguishable from the control group. Thus, whereas information about overall spending and distributive allocations have no discernible effect, information about corruption depresses turnout and lowers incumbents' and challengers' support.

Once we let the effect of corruption information to vary with the level of corruption in a linear and quadratic interaction, we find that exposing information about corruption has a diminishing effect on turnout, incumbent and challenger votes. To interpret the magnitude of the effects, consider the effect of exposing the median level of corruption in our sample (54%): Turnout decreases by 12 percentage points, incumbent parties' votes decrease by 5.3 percentage points, and challengers' votes decrease by 6.5 percentage points.[14]

When we include the interaction of the corruption information treatment dummy with low, middle and high levels of corruption, we find that disseminating information about low levels of corruption (0 to 33 %) leads to a 1.8 percentage points (s.e.=0.47 pp) decrease in turnout. At middle levels of corruption (33-66%), the corruption information treatment had a negative effect on turnout of 0.3 percentage points (s.e.=0.44). At high levels of corruption (more than 66%), disseminating information about corruption leads to a 7 percentage points decrease (s.e.=1.37 pp) in turnout, or 13% decrease off a base of 52.

Disseminating information about low and high levels corruption also has a negative effect on votes for the incumbent parties and the challengers. The treatment group that received information about low corruption (0-33%) casted 0.67 percentage points (s.e.=0.29 pp) less votes for the incumbent parties, and 1.10 percentage points (s.e.=0.37 pp) less for the challengers. The treatment group that received information about middle levels of corruption (33-66%) is not different to the placebo groups. Finally, the treatment group that received information about high levels of corruption (more than 66%) casted 2.6 percentage points (s.e.=0.87 pp) less votes for the incumbent parties and 4.5 percentage points (s.e.=1.09 pp)

less votes for the challenger parties. Jointly, the interactions are statistically different to zero in all specifications.[15]

Despite the apparently different effect of corruption information on the incumbent parties' and challengers' votes, a Chi-squared test of the equality of the effect of $CorruptionInformation$ across equations suggests that the effect is not statistically different. Hence, corruption information is demobilizing and affects the incumbent and challenger parties' votes negatively, especially when the corruption exposed is high. In addition, the effect by corruption levels seems to be non-linear: exposing high levels of corruption leads to substantively larger behavioral effects than exposing low levels of corruption.

As a first robustness test, we included in our models a baseline poverty index computed at the voting precinct level; as well as turnout, incumbent and challenger parties' votes (computed as a share of registered voters) in the previous election. Baseline controls do not change substantively the magnitude of our estimates, and they slightly improve the precision of our estimates.[16]

Like in most field experiments, the possibility that actual treatment does not coincide with assigned treatment is a concern. We consider two forms of non-compliance. First, our corruption information treatment could have spilled to the placebo and control groups. For example, people who received information about incumbent corruption could have talked to people in other treatment groups. Spillovers could dilute the magnitude of our effects. In a block randomized experiment, randomization takes place within blocks. Thus, problematic blocks can be dropped without compromising the internal validity of the rest (Hayes and Moulton 2009). Therefore, we estimated equations 1, 2 and 3 without the three state capitals because they are more prone to spillovers, since their population density and media coverage are higher compared to outlying municipalities. We find that the effects of disseminating incumbent corruption information are slightly larger when we exclude state capitals. This suggests that our intent-to-treat estimates are conservative.

A second form of non-compliance takes the form of failure-to-treat, which occurs when subjects do not receive the treatment to which they are assigned. We confronted minor logistical issues such as one attempt of assault of a flyer distributor, a few gated neighborhoods, and hard-to-reach voting precincts. Still, compliance with treatment assignment was

overall high. Among voting precincts in the state of Jalisco, 97% received full treatment; among voting precincts in Morelos, 89% received full treatment; and among voting precincts in Tabasco, where we faced more logistical challenges, 60% of precincts were fully treated, 20% were partially treated, and 20% failed to receive any treatment. Voting precincts with high rates of failure-to-treat (equal or more than 25%) are clustered in 3 municipalities out of the 12 in our study. Whereas in those 3 municipalities average coverage of treatment was 59%, in the rest of our municipalities average coverage was 93%. We estimated equations 1, 2 and 3 excluding the three municipalities were failure-to-treat was problematic and find that the effect of exposing corruption information is robust. For ease of presentation, Figure 3 includes the estimation of equation 1 with the various robustness tests and the SI file includes all tables with estimations of equations 1, 2 and 3.

Why incumbent corruption information affects incumbent's and challengers' candidates?

Although we cannot fully explain why providing information about incumbent's corruption affects all parties, we discuss and test one possible mechanism. In a corrupt environment, incumbents are commonly believed to be involved in corruption. Our follow-up survey confirms this: 50% of the control group reports to disagree with the statement that their municipal government used public resources with honesty. Incumbent party's candidates are linked to the incumbent (because they belong to the same party, and in some cases worked in the incumbent government). Up until a certain point of corruption, voters already suspected that corruption was taking place, thus the effect of our corruption information treatment on incumbent parties' support is modest. However, beyond a certain level of corruption, informed voters do update their belief about the dishonesty of their local government, and incumbent party support falls at a higher rate.

Only a political challenger who is not already deeply embroiled in and compromised by ongoing corrupt transactions offers a credible prospect for better governance in a context such as the Mexican. The revelation of corruption may lead voters to reevaluate the likelihood that challengers' candidates have the capacity to reduce corruption. Once corruption reaches a certain level, voters may interpret it as an equilibrium from which individual

politicians, especially low quality ones, cannot credibly withdraw. At that point, even the challenger is believed potentially tainted by an environment that is seen as thoroughly corrupt. When voters believe that corruption is an equilibrium, challengers no longer represents a political alternative and the rationale for supporting them falls accordingly. This kind of pubic reaction to revelations about corruption could be prevalent in high-corruption equilibrium environments, and would explain why voters disengage from politics when confronted with political corruption.

An implication of this argument is that votes for challengers should decrease at higher rates when voters perceive them to be "low quality". As a proxy for candidates' quality, we use the jobs challengers held prior to running in the 2009 local election. Challengers come from three pools of politicians: (1) Political parties' local offices (non-elected positions); (2) Local congress (elected positions, congressman apply for a leave of absence to run for a different office); (3) and one candidate was a federal congressman.[17]

In our follow-up survey, we asked respondents whether they approve or disapprove the way in which the president, the mayor, the state governor, local congress, and federal congress do their job. Our control group reports low levels of approval across the board. Still, they approve at much lower rates, and disapprove at much higher rates, the work of legislators.[18] Other work has also documented legislators' bad reputation in Mexico (Morgenstern 2002). Thus, challengers who were congressman with a leave of absence had recently ran for office in the municipality, won, and belong to a discredited pool of politicians.

We estimated the interaction of our corruption information treatment with a dummy variable that indicates that the challenger was a local congressman. Table 5 reports this analysis. Disseminating information about incumbent corruption leads to a 3.6 and a .6 percentage points reduction in challengers votes when the challenger comes from local congress and when he does not, respectively. Turnout and incumbent party support also decrease at higher rates when the challenger comes from local congress, 5.8 and 2.2 percentage points, respectively. All interactions are statistically significant at the 1%. Thus, when voters are informed about incumbent corruption they disengage from politics, especially when "low quality" challengers ran for office.

Effects on partisan attachment

If people exposed to incumbent corruption disengage from politics, then incumbent corruption information could affect people's political attitudes–particularly their partisan attachment– in addition to affecting voting behavior. We use our follow-up survey to test this. As a reminder, we did not expose people to the information again at the time of the survey.

Based on linear probability models, we find that respondents who were assigned to the corruption information treatment group are 0.07 percentage points (s.e.=0.03 pp) less likely to identify with the incumbent party. This amounts to a 46% reduction in party identification relative to the placebo groups, where 15% of respondents identified with the incumbent party. In the SI file we show that probit model produce similar results. Incumbent corruption information has no influence on identification with a challenger party.[19]

The effect of disseminating corruption information is more pronounced when either medium or high levels of corruption are exposed. While exposing corruption between 0-33% has no effect, exposing 33-66% and more than 66% of corruption leads to a 0.14 (s.e.=0.05 pp) and a 0.08 (s.e.=0.03 pp) percentage point decrease, respectively. Although the joint effect of the interaction terms is statistically significant at the 1%, the effects of medium and high levels of corruption are not different from each other. This could explain why the model with linear and quadratic interactions produces no significant results.

Table 6 uses individual level data and includes robust standard errors clustered at the voting precinct level, which is the unit of randomization. A concern with analyzing clustered data such as ours at the individual level is that standard errors may be misleading if sampling variability is not taken into account. A simple and transparent alternative to analyze our data, according to Dunning (2012), is to aggregate individual level responses up to the voting precinct level because analysis at the cluster level follows the design of the randomization. In the lower panel of Figure 3, we show that estimates of equation 1 with aggregate data are equivalent to the estimates using individual level data. That the aggregate and individual level data produce the same estimates increases our confidence in the result that disseminating information about incumbent corruption erodes party identification with the incumbent party.

With the aggregated follow-up survey data, we also find that the estimates, although

slightly less precise, are robust to the inclusion of baseline precinct-level poverty, turnout, incumbent and challengers votes. As before, to deal with possible spillover effects, we estimated models without the three municipalities that are state capitals. We find that disseminating corruption information has a negative effect on identification with the incumbent party. Yet this result is not statistically significant. However, this test may be underpowered because to implement it we are left with only 54% of our voting precincts. Finally, to take into account the failure-to-treat, we estimated our models excluding the three municipalities were failure to treat was problematic (equal or higher than 25%) and find that the effect of exposing corruption information on identification with the incumbent party is slightly larger (10 percentage points decrease). The SI file includes all tables corresponding to these tests, and Figure 3 presents estimates of equation 1.

Conclusion

This article presents experimental evidence that information about incumbent political corruption leads to incumbent parties' and challenger parties' vote losses, to a decrease in electoral turnout, and to a decrease in people's identification with the incumbent party. Thus, information about incumbent corruption disengages voters from the political process. These results are relevant across contexts of high corruption, of which Mexico is an example, and also speak to a central debate about voting behavior.

One strand of literature explains voting behavior with a decision-theoretic model. According to Downs (1957), a citizen would vote rather than abstain if the utility difference between the candidates discounted by the citizen's probability of affecting the outcome outweighs the costs of voting. Alternatively, voters may choose to minimize their maximum regret instead of maximize their expected utility when making their vote decisions (Ferejohn and Fiorina 1974). Our results are consistent with both versions of the decision-theoretic model because people exposed to information about incumbent corruption behave as if they calculate that, given the extent of corruption, utility differences between candidates are negligible. Hence, the results of the election is inconsequential and abstention is a rational choice. Another strand of literature posits that voters are strategic. Feddersen and Pesendorfer (1999), for example, argue that uninformed people abstain to delegate public

decisions to the informed. Our results are incompatible with theories of strategic abstention since we find that, in the case of corruption information, it is the informed who abstain.

Our results also have implications for theories of retrospective voting, which posit that voters evaluate an incumbent's performance in office, and use elections to reelect high-performing incumbents and throw out poor performers. The theory predicts voters take their chances on an unknown challenger when they deem an incumbent as low quality (Persson and Tabellini 2002). Our results suggest that voters' evaluations of an incumbent permeate to their evaluations of other politicians. Party cues may explain why the incumbent party's candidate looses votes when the term-limited incumbent is exposed as corrupt. Yet that challenger parties' candidates also loose votes suggests that voters use an incumbent's performance to evaluate their political environment. In contexts of high corruption, informed voters may conclude that no candidate can credibly withdraw from corruption. Thus incumbent corruption information taints all candidates. Moreover, since the effect of corruption information is stronger when challengers are local congressman, one the most discredited jobs in Mexican politics, the assumption in retrospective models that challengers are unknown is unrealistic.

Clearly the answer to the basic question, how corruption information changes voter engagement, choice and party identification, depends on key factors, such as voters' prior knowledge and awareness, their choice set, and their ability to influence government actions. More generally, this topic is a perfect example of where theory-led experimentation can provide better guidance to policy. Anti-corruption efforts, in order to be effective, must learn how citizens' prior beliefs and institutional realities influence the effect that information has on voter decisions, and thus eventual reform.

Notes

[1]Only Vaishnav (2011) argues that voters reward corrupt politicians in India because they could benefit personally from the corruption.

[2]See: Bauhr and Grimes (2011); Caillier (2010); Davis et al. (2004); McCann and Domínguez (1998); Kostadinova (2009); Peters and Welch (1980).

[3]We compare the corruption information and the control groups to the placebos to

disentangle the effect of mere distribution of flyers from the corruption content of the flyers.

[4]See Morgenstern (2002) on the bad reputation of legislatures in Latin America.

[5]Sometimes municipalities form multi-municipality districts to provide services, but audit reports confirm that mayors do not pool FISM money.

[6]The Lower House of Congress appoints the Auditor for a term of eight years, renewable once. In 2009, a constitutional reform formalized the ASF's mandate.

[7]ASF's results report 2006, 2007.

[8]Our municipalities are not among the richest or the poorest in Mexico. Because of the three state capitals, in average our municipalities are more developed than the national average (our average municipality and the national average were: income per capita 8,399 and 4,724 pesos; literacy rate 90% and 82%; revenue from municipal taxes per capita was 386 and 126 pesos; and, revenue from earmarked funds per capita 636 pesos and 1,045 pesos, respectively). Still, our sample includes 9 municipalities that are closer to the national average.

[9]Teams of four or five distributors were assigned to a supervisor. And, one author and IPA staff supervised the distribution process in all states. In Morelos and Tabasco, we used GPS. Steps were taken to correct early implementation errors.

[10]Allocation concealment to participants was not an issue given our unit of analysis. Authors implemented the randomization. For logistical reasons, the two distribution firms were informed about group assignments one week before the intervention.

[11]For the survey's sample (750 voters in 75 precincts), we randomly selected 15 precincts in each of the treatment groups and 30 precincts in the control group (stratified by state). Then, we randomly selected two blocks within each precinct, and surveyed five households within each block.

[12]This specification is equivalent to including $CorruptionInformation$, its interaction with two levels of corruption, and leaving out one interaction term as reference group.

[13]This weighted regression "produces the same estimate as weighting the estimated Average Treatment Effect for each block" (Gerber and Green 2012, 130). The SI file includes the unweighted estimations.

[14]Turnout:$2.5 + (16.75 - 2(24.64)(.54))$; Incumbent party:$1 + (7.79 - 2(11.22)(.54))$; Chal-

lengers: $1 + (8.96 - 2(13.42)(.54))$

[15]We also find that corruption information decreases self-reported turnout (See Table A13). We do not elaborate on this because self-reported turnout is inflated (See Table A14), as in most surveys (Holbrook and Krosnick 2010).

[16]Table A2 shows that with the inclusion of baseline controls, turnout and incumbent votes are lower in the control group compared to the placebo groups. We do not discuss these results further becasue they are not robust.

[17]Challengers were local congressman in 3 out of the 12 municipalities in our study.

[18]33% of respondents approve the work of the mayor and governors, but only 17% of respondents approve the work of local congressman. On the other hand, 10%, 14% and 23% of respondents strongly dissaprove the work of the mayor, the governor, and local congressman, respectively.

[19]We also find that medium levels of corruption increase indentification with challenger parties.

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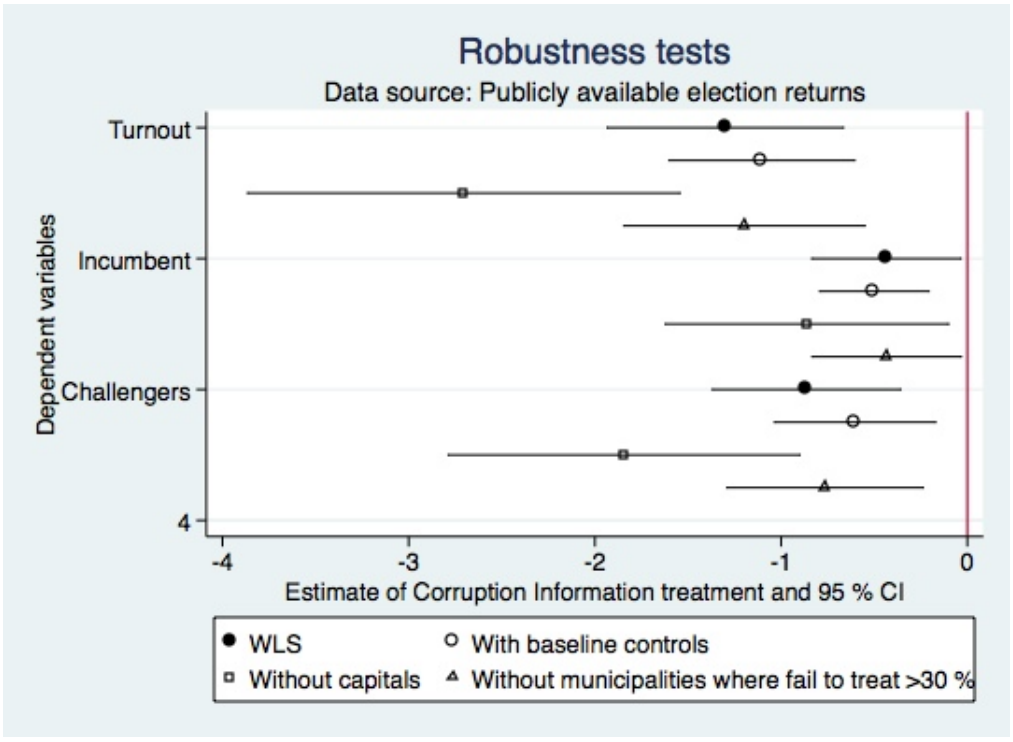
Alberto Chong is a Professor at Ottawa University, Ottawa, ON, CA K1N-6N5; Ana L. De La O is an Associate Professor at Yale University, New Haven, CT 06510; Dean S. Karlan is a Professor at Yale University, New Haven, CT 06520; Leonard Wantchekon is a Professor at Princeton University, Princeton, NJ 08544.

Figure 1: Example of "Corruption Information" flyer

Notes: The flyer was folded in half. The upper image is the front and back of the flyer, the lower image is the inside of the flyer. This figure shows the "Corruption information" flyer. The "Budget Information" and the "Poverty expenditure" flyers are identical to the flyer shown here except for the graph, which includes the relevant information for each treatment group. Figure 2 includes examples of the flyers in both placebo groups.

Figure 2: Example of flyers for the two placebo groups

Notes: The flyer was folded in half. The front of the flyer is the same as the upper image in Figure 1 for all flyers. In the "Budget Information" group the inside of the flyer was as shown in the upper image in this figure. In the "Poverty expenditure" group the inside of the flyer was as shown in the lower image.



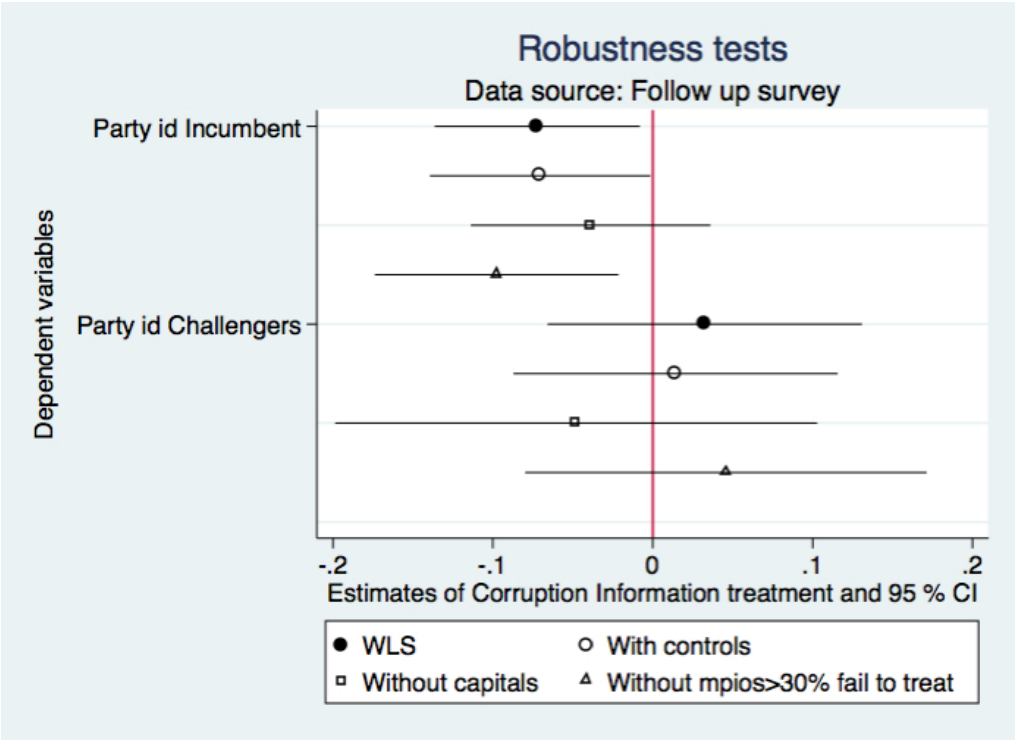


Figure 3: Robustness test

Notes: This figure presents various robustness tests of the estimates of equation 1. Please see the SI file for all relevant tables.

Table 1: List of municipalities in the experiment, their municipal infraestructure fund (FISM) and local taxation

State	Municipality	FISM total amount granted	FISM per capita	Local taxes per capita	FISM/total federal earmarked funds	% of total municipal infrastructure expenditures paid with FISM	Corruption Treatment information: % of FISM spent with corruption
Jalisco							
	Guadalajara	90,617,700	55.04	383.03	0.15	11-20	54
	Tlajomulco de Zúñiga	14,652,900	118.53	1993.47	0.17	0-10	100
	Tlaquepaque	47,313,100	99.77	353.52	0.21	41-50	61
	Tonalá	38,298,700	113.59	287.59	0.23	41-50	67
Morelos							
	Cuautla	25,915,100	169.01	218.81	0.26	>50	16
	Cuernavaca	43,426,300	128.21	625.58	0.24	21-30	8
	Jiutepec	22,160,000	129.90	359.39	0.21		11
Tabasco							
	Cárdenas	87,284,000	401.74	31.94	0.56	>50	5
	Centro	111,373,200	214.05	282.83	0.38	>50	4
	Comalcalco	65,089,100	395.34	36.15	0.54	41-50	1
	Huimanguillo	103,958,700	655.58	23.08	0.66	>50	3
	Macuspana	60,699,500	453.03	39.12	0.57	>50	27
Average		59,232,358	244.48	386.21	0.35		29.75

Notes: This table lists the municipalities in our study, and reports information related to the municipal infrastructure fund (FISM). Total FISM granted and percent of FISM spent in a corrupt manner come from the audit reports. Total federal earmarked funds, income from local taxation and population come from INEGI. Percent of total municipal infrastructure expenditures paid with FISM comes from INEGI's Encuesta Nacional de Gobierno, Seguridad Publica y Justicia Municipal 2009.

Table 2: Baseline Precinct-Level Statistics and Orthogonality Tests

	Means and Standard Deviations			P-value from orthogonality test
	Treatment: Corruption information	Placebos: Budget & Poverty expenditure information	Control: No information	
% of Polling Precinct's Households with:				
Illiteracy	0.04	0.04	0.04	0.42
	(0.04)	(0.04)	(0.04)	
No primary school	0.22	0.22	0.23	0.71
	(0.08)	(0.08)	(0.08)	
No sewage	0.07	0.07	0.07	0.54
	(0.04)	(0.05)	(0.05)	
No electricity	0.06	0.06	0.06	0.44
	(0.05)	(0.05)	(0.06)	
No potable water	0.15	0.16	0.15	0.22
	(0.20)	(0.22)	(0.22)	
No cement flooring	0.05	0.05	0.04	0.58
	(0.09)	(0.08)	(0.08)	
No refrigerator	0.16	0.16	0.15	0.53
	(0.12)	(0.13)	(0.13)	
Electoral outcomes in previous election (as a share of registered):				
Turnout 2006	57.65	57.64	59.67	0.25
	(7.60)	(9.47)	(8.00)	
Incumbent party votes 2006	25.41	25.14	27.19	0.35
	(8.14)	(6.85)	(7.59)	
All challengers votes 2006	32.23	32.5	32.50	0.77
	(6.90)	(6.69)	(6.40)	
P-value from orthogonality tests	0.87	0.13		

Notes: This table reports baseline summary statistics using publicly available precinct level data. Columns (1) and (2) present the means (and standard deviations in parentheses) for the treatment group and the placebos, respectively. Column (3) report summary statistics for the control group. The last column present the p-values of a F-test on joint significance of all treatment variables, from a regression of each baseline characteristic on treatment assignment dummies and municipality fixed effects. The last row shows the p-values of a F-test on joint significance of all independent variables, from regressions of each treatment dummy on all baseline covariates and municipality fixed effects.

Table 3: Did Information about corruption change beliefs and opinions?

Data source: Follow-up Survey (completed two weeks after 2009 elections)

	Municipal government is Dishonest			Municipal government is Honest			Approve mayor			Unsatisfied with public services		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Corruption Information	0.02	-0.02		0.01	0.05		-0.02	0.03		0.06	-0.03	
	(0.06)	(0.08)		(0.04)	(0.07)		(0.03)	(0.06)		(0.10)	(0.10)	
No Information	0.05	0.07	0.05	-0.03	-0.03	-0.02	-0.04	-0.04	-0.04	0.09	0.08	0.08
	(0.06)	(0.06)	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.07)	(0.07)	(0.07)
Corruption Info. X linear		-0.65			0.09			-0.35			0.45	
		(0.68)			(0.49)			(0.37)			(1.16)	
Corruption Info. X quad.		1.48			-0.40			0.36			-0.31	
		(1.02)			(0.65)			(0.43)			(1.36)	
Corruption Information X C_{0-33}			-0.07			0.06			0.01			0.03
			(0.05)			(0.05)			(0.03)			(0.06)
Corruption Information X C_{33-66}			0.01			0.00			-0.05			-0.03
			(0.08)			(0.07)			(0.05)			(0.30)
Corruption Information X C_{66-100}			0.36**			-0.12**			-0.04			0.33*
			(0.09)			(0.03)			(0.03)			(0.15)
Constant	0.42**	0.40**	0.41**	0.25**	0.26**	0.25**	0.07**	0.07**	0.07**	0.53**	0.53**	0.52**
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.05)	(0.05)	(0.05)
Observations	749	749	749	749	749	749	749	749	749	719	719	719
R-squared	0.14	0.15	0.16	0.08	0.09	0.09	0.23	0.23	0.23	0.09	0.09	0.10
P-value:												
Corr Info = No Info	0.6054			0.3562			0.5298		0.7709			
Joint Hypotheses test =0		0.1557	0.0019		0.3906	0.0005		0.5190	0.7051		0.9165	0.1793
Mean dep. variable in placebos	.45	.45	.45	.23	.23	.23	.04	.04	.04	.54	.54	.54

Notes: The dependent variable in columns (1) - (3) can take the value of 1 when a respondent agrees that the local government is dishonest, 0 otherwise; in columns (4)-(6) can take the value of 1 when a respondent agrees that the local government is honest, 0 otherwise; in columns (7)-(9) the dependent variable takes the value of 1 if respondent approves of the mayor, 0 otherwise; and in columns (10)-(12) can take the value of 1 if respondent is unsatisfied with public services in his or her place of residence, 0 otherwise. All specifications include municipality fixed effects. Corruption is measured at the municipality level, hence the municipality fixed effects capture any underlying effect of corruption on the outcome variable. Corruption level is bounded between 0 and 1. Robust standard errors clustered by voting precinct in parentheses. Probability of assignment to treatment varied by municipality, thus observations are weighted by the inverse probability of treatment. *** p<0.01, ** p<0.05 on two sided test.

Table 4: Estimates of the effects of information about corruption on electoral outcomes

Data Source: Publicly available election returns at the electoral precinct level

	Turnout			Incumbent party votes/reg. voters			Challenger parties votes/reg. voters		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Corruption Information	-1.30**	-2.52**		-0.43*	-1.05**		-0.86**	-1.47**	
	(0.32)	(0.61)		(0.20)	(0.38)		(0.26)	(0.48)	
No Information	-0.32	-0.32	-0.32	-0.04	-0.04	-0.04	-0.28	-0.28	-0.28
	(0.32)	(0.32)	(0.32)	(0.20)	(0.20)	(0.20)	(0.26)	(0.26)	(0.26)
Placebos (omitted category)									
Corruption Info. X linear		16.75**			7.79**			8.96**	
		(4.14)			(2.62)			(3.31)	
Corruption Info. X quad.		-24.64**			-11.22**			-13.42**	
		(5.78)			(3.66)			(4.62)	
Corruption Information X C_{0-33}			-1.78**			-0.67*			-1.10**
			(0.47)			(0.29)			(0.37)
Corruption Information X C_{33-66}			-0.30			-0.00			-0.29
			(0.44)			(0.28)			(0.35)
Corruption Information X C_{66-100}			-7.12**			-2.65**			-4.47**
			(1.37)			(0.87)			(1.09)
Constant	52.13**	52.13**	52.13**	17.89**	17.89**	17.89**	34.24**	34.24**	34.24**
	(0.19)	(0.19)	(0.19)	(0.12)	(0.12)	(0.12)	(0.15)	(0.15)	(0.15)
Observations	2,340	2,340	2,340	2,340	2,340	2,340	2,340	2,340	2,340
R-squared	0.47	0.47	0.47	0.56	0.56	0.56	0.44	0.44	0.44
P-values:									
Corr Info = No Info	0.0089			0.0976			0.0486		
Corr X C_{0-33} = Corr X C_{33-66}			0.0174			0.0882			0.1037
Corr X C_{0-33} = Corr X C_{66-100}			0.0002			0.0295			0.0034
Corr X C_{33-66} = Corr X C_{66-100}			0.0000			0.0034			0.0003
Joint Hypotheses test =0		0.0000	0.0000		0.0030	0.0024		0.0002	0.0000
Mean dep. variable in placebos	52	52	52	17.15	17.15	17.15	34.75	34.75	34.75

Notes: The dependent variables are: in columns (1) - (3) total number of votes; in columns (4) - (6) votes for the incumbent party; and in column (7) and (9) votes for any challenger party. All dependent variables are divided by number of registered voters and multiplied by 100. All specifications include municipality fixed effects. Corruption is measured at the municipality level, hence the municipality fixed effects capture any underlying effect of corruption on the outcome variable. Note that standard errors are not clustered as they are in other tables with individual-level observations, since the unit of observation in this table is the same as the unit of randomization. Probability of assignment to treatment varied by municipality, thus observations are weighted by the inverse probability of treatment. Corruption level is bounded between 0 and 1. ** p<0.01, * p<0.05 on two sided test.

Table 5: Heterogenous effects of information about corruption by challengers' previous job

	Turnout	Incumbent party votes/reg. voters	Challenger parties votes/reg. voters
	(1)	(2)	(3)
Corruption information	-0.86*	-0.26	-0.60*
	(0.34)	(0.21)	(0.27)
Corruption Information X challenger was a local congressman	-4.98**	-1.98**	-3.00**
	(1.07)	(0.68)	(0.85)
No information	-0.32	-0.04	-0.28
	(0.32)	(0.20)	(0.26)
Placebos (omitted category)			
Constant	52.13**	17.89**	34.24**
	(0.19)	(0.12)	(0.15)
Observations	2,340	2,340	2,340
R-squared	0.47	0.56	0.44
P-values:			
Corr.info + Corr.i nfo X challenger was a local congressman	0.0000	0.0006	0.0000

Notes: The dependent variables are: in column (1) total number of votes; in column (2), votes for the incumbent party; and in column (3), votes for any challenger party. All dependent variables are divided by registered voters and multiplied by 100. All specifications include municipality fixed effects. Corruption is measured at the municipality level, hence the municipality fixed effects capture any underlying effect of corruption on the outcome variable. Note that standard errors are not clustered as they are in other tables with individual-level observations, since the unit of observation in this table is the same as the unit of randomization. Probability of assignment to treatment varied by municipality, thus observations are weighted by the inverse probability of treatment. Corruption level is bounded between 0 and 1. ** p<0.01, * p<0.05 on two sided test.

Table 6: Estimates of the effects of information about corruption on party identification

Data source: Followup Survey (completed two weeks after 2009 elections)

	Incumbent Party (0,1)			Challenger Parties (0,1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Corruption Information	-0.07*	-0.05		0.04	-0.04	
	(0.03)	(0.04)		(0.06)	(0.05)	
No Information	-0.05	-0.05	-0.04	0.09	0.07	0.08
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Placebos (omitted category)						
Corruption Info. X linear		0.13			0.91	
		(0.35)			(0.58)	
Corruption Info. X quad.		-0.38			-1.26	
		(0.44)			(0.88)	
Corruption Information X C_{0-33}			-0.02			0.01
			(0.03)			(0.03)
Corruption Information X C_{33-66}			-0.14**			0.19**
			(0.05)			(0.07)
Corruption Information X C_{66-100}			-0.08**			-0.17
			(0.03)			(0.17)
Constant	0.17**	0.18**	0.17**	0.52**	0.53**	0.52**
	(0.02)	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)
Observations	720	720	720	720	720	720
R-squared	0.08	0.08	0.08	0.07	0.07	0.08
P-values:						
Corr Info = No Info	0.4805			0.3545		
Corr X C_{0-33} = Corr X C_{33-66}			0.0508			0.0163
Corr X C_{0-33} = Corr X C_{66-100}			0.1786			0.3223
Corr X C_{33-66} = Corr X C_{66-100}			0.2886			0.0572
Joint Hypotheses test =0		0.0058	0.0011		0.3821	0.0384
Mean dep. variable in placebos	.15	.15	.15	.54	.54	.54

Notes: The dependent variables are dummy variables indicating: identification with the incumbent party in columns (1)-(3), and identification with a challenger party in columns (4)-(6). The exact wording of the survey question is: "With which political party do you identify, if any?" All specifications include municipality fixed effects. Corruption is measured at the municipality level, hence the municipality fixed effects capture any underlying effect of corruption on the outcome variable. Robust standard errors clustered by voting precinct in parentheses. Probability of assignment to treatment varied by municipality, thus observations are weighted by the inverse probability of treatment. Corruption level is bounded between 0 and 1. *** p<0.01, ** p<0.05 on two sided test.