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Incumbent Advantage, Voter Information and Vote Buying *

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Abstract

Results from a new experiment shed light on the effects of voter information on vote buying and incumbent advantage. The treatment provided voters with information about a major spending program and the proposed allocations and promises of mayoral candidates just prior to municipal elections. It left voters more knowledgeable about candidates' proposed policies and increased the salience of spending, but did not affect vote shares and turnout. Treated voters were *more* likely to be targeted for vote buying. We develop a model of vote buying that accounts for these results. The information raised voter expectations regarding incumbent performance, especially in incumbent strongholds. Incumbents increased vote buying in response. Consistent with this explanation, both knowledge and vote buying impacts were higher in municipalities with dominant incumbents. Our findings show that, in a political environment where vote buying is the currency of electoral mobilization, incumbent efforts to increase voter welfare may take the form of *greater* vote buying.

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1 Introduction

Results from a novel field experiment in the Philippines yield surprising insights into the effects of voter information on vote buying and incumbent advantage. Shortly before municipal elections, voters in 142 randomly selected villages in 12 municipalities received information about the existence and scope of a large fund provided by the central government to finance municipal development projects. This new information led to disappointment among voters, since mayors had weak incentives to use an unknown fund to provide public goods. Mayors responded to disappointment with greater vote buying. Moreover, voter ignorance of the local development fund was greater in municipalities with strong incumbents, consistent with the idea that information asymmetries between incumbents and voters are a source of incumbent advantage. Mayoral incentives to use the fund were correspondingly less, and vote buying more, in those municipalities.

In addition to increasing vote buying, the information treatment also raised voter knowledge about the funding program. Among treated respondents, municipal spending was also more salient: they were more likely to report that an important determinant of their vote choice was whether candidates proposed to spend the municipal budget on things that were important to their household. However, since incumbents were able to offset the information shock by increasing vote buying, turnout and candidate vote shares were unaffected by the information shock.

We asked all mayoral candidates how they would allocate the Local Development Fund. Households then received a flyer that contained information both about the fund itself and about candidate intentions with regard to fund allocation. We present a wide range of evidence that the information about the fund itself drives the results that we observe. In contrast, neither differences in candidate intentions, nor differences between candidate intentions and respondents' own preferences, can account for the treatment effects we observe.

First, the information shock has the largest effects on vote buying where voters report no recent municipality-provided infrastructure. It is precisely these voters who should have been most disappointed when they received information about the existence of a large funding source that incumbents could have used to provide public goods. Second, in addition to the field experiment, we separately conducted a survey experiment in which treated respondents were told *only* about the

municipal spending program. All respondents were then asked in a secret ballot to express the likelihood that they would vote for the incumbent in the next mayoral election. Treated respondents in the survey experiment were less likely to express support for the incumbent, consistent with the results in the field experiment. Third, consistent with the model and information-based theories of incumbent advantage more generally, the effects of the intervention on knowledge and vote buying are larger in incumbent strongholds, municipalities where incumbent margins of victory were largest in the previous election.

Our results have two important implications for efforts to improve the accountability of elected politicians. First, vote buying can be a manifestation of political accountability to voters: voter expectations for incumbent performance increased and vote buying rose accordingly. Second, information interventions that attenuate the advantages of dominant incumbents increase political incentives to deliver benefits to voters. However, as Khemani (2015) notes, the equilibrium outcome of increased political competition in clientelist settings tends to take the form of greater vote buying and worse public service delivery. As a result, despite increasing the leverage of voters vis-à-vis incumbents, interventions to make public spending decisions more salient in the week before an election are still not enough to shift the political equilibrium towards less vote buying. However, the results of the intervention offer a strong motivation to examine the effects of a similar intervention earlier in the electoral cycle, when incumbents have greater opportunity to react by increasing the provision of public goods.

In the remainder of the paper, Section 2 nests our analysis in a wide range of earlier contributions to the literature. Section 3 describes municipal elections in the Philippines, the experiment and data. Section 4 details the direct effects of the information intervention. In Section 5, we develop a retrospective voting model of political competition where candidates cannot make credible pre-electoral promises. The model accounts for the main findings of the paper, but also yields a number of ancillary predictions that we are able to test. Evidence for these predictions and some of the model's assumptions is presented in Section 6. In particular, we present results of a survey experiment that isolates one particular element of our information treatment, information about the program. Respondents who receive *just* this information are significantly less likely to favor the incumbent. Section 7 concludes.

2 Literature Review

Vote buying, voter information and incumbency advantage have each received substantial scholarly attention. We examine these phenomena jointly and show that the provision of information about what governments can do for citizens has significant effects on vote-buying and incumbency advantage. The sections below outline these contributions in more detail.

2.1 Vote Buying

Vote-buying is the pre-electoral provision of gifts of goods or money, aimed at persuading recipients to vote in a particular way, to turn out to vote, or not to vote at all (Hicken, 2011; Schaffer and Schedler, 2007; Nichter, 2008). The practice is pervasive and can entail large transfers to voters.¹ In our control group, 14 percent of households report vote buying.

A substantial literature emphasizes the potential tradeoffs between vote buying and the provision of broad public services. Models developed in Keefer and Vlaicu (2008) and Hanusch et al. (2016) link preferences for targeted transfers and vote buying, respectively, to the inability of politicians to make credible commitments to citizens. Kitschelt (2000) concludes that vote-buying is more common in countries with non-programmatic political parties, such as the Philippines.

The analysis here does not speak to the large literature focused on the enforcement of vote buying transactions, such as party machines and social networks (Brusco et al., 2009; Cruz, 2013) or norms of reciprocity (Finan and Schechter, 2012), and on the extent of leakage (voters who do not necessarily vote for the politicians who pay them, as in Schaffer and Schedler (2007)).

2.2 Information Provision

A large literature on information and electoral behavior examines the effects on voters of information related to the desirability of vote buying (Hicken et al., 2014;

¹For example, across the 17 countries surveyed in the 2005-06 wave of the Afrobarometer survey, 19 percent of more than 20,000 respondents reported that they had been offered a gift in the last election. Brusco et al. (2009) surveyed nearly 2,000 respondents in three Argentine provinces three months after the October 2001 elections. Forty-four percent of respondents said that parties had distributed food, clothing and other items to homes in their neighborhoods.

Vicente, 2014), voting procedures and voting irregularities (Vicente, 2014; Aker et al., 2011); valence issues such as candidate corruption, criminal records, education, attendance at parliamentary sessions (Banerjee et al., 2011; Chong et al., 2015; Humphreys and Weinstein, 2013) and radio broadcasts that increase the demand for public services (Keefer and Khemani, 2014). In their experiment in an Italian mayoral election, Kendall et al. (2015) look at the voting effects of information regarding candidate competence and effort relative to information regarding candidate ideology; information about competence increased incumbent vote shares.

Fujiwara and Wantchekon (2013) focus on deliberative town hall meetings in Benin. Their intervention did not have a significant effect on survey responses regarding vote buying, though it did affect an index of voter attitudes towards broadly clientelist forms of electoral mobilization. The information content of the town hall meetings is ambiguous, as it is in Casey (2015), who looks at voter information and pre-electoral spending in Sierra Leone. In her study, voters are assumed to be more informed in elections for local candidates, and when they are more exposed to community radio. She finds that parties undertake the most pre-electoral spending in districts with more informed voters and no dominant ethnic group.

The information intervention by Chong et al. (2015) also consists of the distribution of flyers, though they do not investigate vote buying. They distributed three different flyers in 12 Mexican municipalities. Like our flyers, theirs implicitly informed voters of the existence of a public infrastructure program of which they had been largely unaware. However, all contained additional performance information. One reported the percentage of program funds spent; one reported the percentage spent allocated to the poor; and the third reported the percentage of program spending that suffered from accounting irregularities. The first two treatments, most similar to ours, also had no effects on turnout nor incumbent or challenger vote share. The third, delivering the most negative information shock, significantly reduced turnout and incumbent and challenger vote shares.

Compared to prior research, the information received by voters in our experiment has the unique characteristic that it is both precise and entirely neutral regarding candidate characteristics. There is no performance information at all: it focuses only on knowledge of a particular government program and candidate

positions regarding spending under that program. Moreover, we are able to pin down the effects of neutral information (what incumbents could have done for voters) on both voter knowledge and candidate vote buying. At the same time, we show that the effects can be accounted for analytically using assumptions about electoral competition - especially, the inability of candidates to credibly commit - that closely correspond to conditions in the Philippines and other young democracies.

2.3 Incomplete Information and Dominant Incumbents

One strand of research on incumbent advantage focuses on whether the degree to which incumbents benefit from information asymmetries affects incumbent re-election.

Boas and Hidalgo (2011) find that incumbent control of community radio stations in Brazil increases incumbent vote share. MacDonald (2014) presents evidence of a smaller incumbent advantage in Zambia in districts where citizens have greater access to radio. Ansolabehere et al. (2006) attribute the electoral advantages of US congressional incumbents to the greater coverage they receive in print media. Using data from the United States, Klasnja (2011) concludes that an increase in political awareness—knowledge of basic political facts—systematically reduces support for incumbents accused of corruption.

Our paper highlights the effects of information about the capacity of the incumbent to deliver public goods. It is the first to offer a direct test of the effects of specific types of information on the behavior of dominant incumbents—incumbents elected by a large margin and who are more likely to possess an incumbency advantage.

This literature predicts that incumbents should exert greater effort on behalf of voters when their information advantage declines. The information intervention we examine has this effect: it tells voters that incumbents have greater capacity to deliver public goods than voters thought, leading incumbents to increase vote buying in response.

3 The Experiment and Data

We focus on the electoral incentives of mayoral candidates in the Philippines. Four characteristics of mayoral elections make them an ideal empirical context for the questions we examine. First, mayors control important public spending programs. Since the passage of the 1991 Local Government Code (LGC), municipalities have had an important role in the delivery of basic services. The code devolved a number of responsibilities, such as responsibility for nutrition programs (Khemani, 2015), and transferred a large number of civil servants to municipalities (Llanto, 2012). Despite the presence of one vice-mayor and eight municipal councilors, mayors exert significant control over how municipal resources are spent, but not over the size of the municipal budget (Hutchcroft, 2012).

Second, consistent with our focus on an exogenous source of municipal funding, mayors have little influence over municipal revenues. For the average municipality, fixed transfers from the central government pay for 85 percent of municipal spending (Troland, 2014). Laws governing transfers to municipalities encourage municipalities to allocate 20 percent of transfers to development projects.

Third, mayoral candidates are unable to make credible commitments about their future policies. Institutionally, Filipino mayors are often viewed as local bosses (Capuno, 2012; Sidel, 1999) subject to few checks and balances on their decisions regarding municipal budgets and spending decisions. Nor does party membership constrain them: policies and party platforms play little role in elections (Hutchcroft and Rocamora, 2003; Kerkvliet, 2002). Vote buying and retrospective voting play a significant role in electoral competition under these circumstances. Consistent with this, prior research documents not only that vote buying is pervasive in the Philippines, but also that Filipino voters use retrospective voting rules when deciding whether to re-elect the incumbent (Cruz and Schneider, 2013; Labonne, 2013).

Finally, fourth, vote buying takes place a few days before the mayoral elections (Cruz, 2013). The information intervention was consistent with this timing: voters received the flyers in the week before the election. Moreover, ample evidence demonstrates that incumbents routinely adjust the targeting of vote buying to shocks that occur in the days leading up to the election. One campaign staffer for an incumbent mayor described in detail how local brokers immediately inform

their candidates about village events that might affect the election.² Candidates then, with equal rapidity, adjust their vote buying strategies accordingly.

3.1 The Experiment

We collected data from candidates on their funding intentions and then produced flyers that described the Local Development Fund (LDF) and candidate plans for LDF spending. A non-governmental organization, the Parish Pastoral Council for Responsible Voting (PPCRV), distributed the flyers to all households in randomly selected villages in the weeks leading up to the May 13, 2013 mayoral elections.³

In April 2013, we interviewed every candidate for mayor in twelve municipalities in the provinces of Ilocos Norte and Ilocos Sur, in the northern reaches of the Philippines. Candidates were told that the information they provided would be given to randomly-selected barangays in their municipality prior to the election. Candidates took the allocation exercise seriously. During the interview, they typically spent several minutes to arrange and rearrange the tokens after considering their allocation.⁴

There are two quantitative indications of the seriousness of candidate allocations. First, the spending intentions of incumbents were correlated with how they had actually allocated their budgets prior to the interviews.⁵ Second, in response to one of the survey questions, candidates listed three specific projects and programs that they would implement if elected. Candidates consistently allocated a

²Example events include not only campaign activities of rival candidates, but also non-partisan activities, such as pre-election surveys, flyer distributions, and voter education campaigns. Consistent with this, just one day after our teams began to distribute flyers, the PPCRV received their first phone call from a candidate asking for clarification about PPCRV activities in his municipality.

³A copy of a flyer is included as Figures A.1 and A.2.

⁴Candidate names were taken from the official list of the Commission on Elections (COMELEC). Most candidates were actually eager to participate (only one refused), even contacting PPCRV to ask if they would be included. At the same time, incumbent willingness to participate may appear to be puzzling, given that one effect of the information treatment was to increase incumbent vote buying. In fact, since incumbents knew that the flyer would be distributed regardless of their participation, their best response to potential voter disappointment and exposure to challenger spending intentions was to be sure that at least their own spending intentions were shared with voters.

⁵We use budgetary data for the last full fiscal year before the election (2012) and compute the correlation between the share of the budget spent on each sector with the share of the budget that the incumbent proposes to spend on the sector. Despite changes in priorities and errors in budget data, the correlation is large, at 0.55.

greater share of their proposed budget to the sectors to which these projects and programs belonged (see Figure A.5).

Within each target municipality, villages were allocated to treatment and control using a pairwise matching algorithm.⁶ The final sample includes 142 treatment and 142 control villages in twelve municipalities (cf. Table A.2). The treated and control villages contained 89 percent of all voters in the municipalities, evenly split between treatment and control villages. The remaining 11 percent of voters resided in 30 villages that were not part of the experiment because we could not run the pairwise matching algorithm for them because of missing data.

PPCRV prepared flyers comparing the proposed allocations of all candidates in each municipality. Then, in the week leading up to the election, PPCRV volunteers distributed the flyers to all households in target villages through door-to-door visits. The teams were instructed to visit all households in the village and give the flyer to the head of household or spouse, and in his or her absence, a voting-age household member.⁷ Although candidates were not told which barangays would be treated, they quickly found out: the flyers were distributed by teams of 10-15 PPCRV volunteers who arrived in each village riding in minivans (*jeepneys*), an event that, within hours, candidates' brokers and representatives relayed to the candidates.

For each household visit, volunteers used a detailed script to introduce themselves and explain the information contained in the flyers. Visits lasted between 5 and 10 minutes and volunteers left a copy of the flyer. No households refused the flyers. Neither the flyer nor the script mentioned vote buying, reducing concerns related to social desirability bias. A detailed timeline of the experiment is available in Table A.1. The pre-analysis plan (PAP) was registered with J-PAL's hypotheses registry on May 12, 2013.⁸

⁶First, for all potential pairs, the Mahalanobis distance was computed using village-level data on population, number of registered voters, the number of precincts, a rural dummy, turnout in the 2010 municipal election and incumbent vote share in the 2010 elections. Second, among 5,000 randomly selected partitions, the partition that minimized the total sum of Mahalanobis distance between villages in the same pairs was selected. Third, within each pair, a village was randomly selected to be allocated to treatment; the other one serving as control.

⁷Due to time constraints, there were no additional visits on different days if no voting-age household member was present on the day of the visit. Our enumerators did not report problems with contacting households with the flyers.

⁸The submitted documents are available at: <http://www.povertyactionlab.org/Hypothesis-Registry>

The results in Table A.3 indicate that the village-level variables used to carry out the pairwise matching exercises are well-balanced across the treatment and control groups. We also use data from the survey to test if the treatment and control are balanced with respect to household composition, households assets, etc.⁹ Out of the 32 village- and household-level variables for which we test balance, only 2 exhibit differences that are significant at the 10 percent level. Controlling for these variables does not affect results reported below.

3.2 Data

The analysis relies on two main data sources. First, precinct-level election results from the COMELEC include information on the number of votes obtained by all candidates in the mayoral elections.¹⁰ Data from the Project of Precincts allowed these data to be matched to villages. Every village contains at least one precinct. Second, we implemented a household survey in 284 villages in twelve municipalities in June 2013. In each village, the team obtained the list of registered voters for the May 2013 elections and randomly selected twelve individuals to be interviewed for a total sample size of 3,408 households. These interviews yielded the key variables that we use in the analysis. Descriptive statistics are reported in Table 1.

Political Knowledge One test of whether the intervention was effective is whether treated households were more knowledgeable about candidate budget allocations than untreated households. For each of the ten sectors about which respondents received information, respondents were asked to name the candidate with the highest proposed allocation.¹¹ Following Kling et al. (2007), we create an index aggregating the various indicators of knowledge of the campaign promises by taking the simple average of the demeaned indicators (divided by the control group standard deviation). So if K_{is} is individual i 's knowledge about sector s promises (*i.e.*, whether they correctly identified the candidate who proposed to spend the largest share of the LDF on sector s), then the knowledge index is:

⁹This set of results is available in Table A.4-A.6.

¹⁰The data were available at: http://2013electionresults.comelec.gov.ph/res_reg0.html

¹¹Respondents were not constrained in their responses and were free to – incorrectly – indicate that one candidate was going to spending a higher share of the budget across all ten sectors.

$$K_i = \frac{1}{10} \sum_s \frac{K_{is} - \bar{K}_s}{\sigma_s}$$

where $\bar{K}_s$ and σ_s are respectively the control group mean and control group standard deviation.

Salience Another test of effectiveness of the intervention is whether treated households cared more about local development spending than untreated households. To establish the salience of local development spending in household voting decisions, we asked respondents about six possible influences on their decision to vote.¹² One of these was whether candidates spend the municipal budget on things that are important to the household. The other five were the preferences of friends and family; gift or money from the candidates before the elections; the candidates' ability to use political connections to get money and projects for the municipality; fear of reprisal from candidates; and the approachability or helpfulness of candidates.¹³ They rated how important each of these was on a 0 - 4 scale, from "not important" to "very important". Respondents took flashcards, each with a reason for voting, and laid it on a worksheet with the numbers 0 - 4, to indicate the importance of that factor.

We use two salience variables. One is simply the raw response: do treated households assign a higher score to the municipal budget criterion than untreated households? However, the treatment could have increased scores on all voting influences. To adjust for this, we constructed a second measure of salience that divides the response for the municipal budget criterion by the average answers in the other five categories. This measure allows us to demonstrate that, relative to the importance they attach to other influences, treated households place more importance on the municipal budget criterion than untreated households.

Preferences over candidates and spending allocations A natural issue to investigate is whether treatment households express different candidate preferences

¹²Although the ordering of the six alternatives may have affected the response, the ordering was the same across treatment and control groups.

¹³To ensure that the six possible influences were all salient to respondents, the lists were extensively field-tested by one of the authors ahead of a similar survey carried out in the nearby province of Isabela.

and whether those are related to the proposed budgetary allocations of candidates. To address these issues, we collected data on respondents' candidate preferences and vote choice. Respondents rated all mayoral candidates on a 0 - 4 scale (strongly disagree to strongly agree) and were also asked directly whom they voted for. In order to reduce the tendency of respondents to claim they voted for the winner when they did not, we used a secret ballot.¹⁴

We also asked respondents to express their preferences over the same ten spending categories that were given to the mayoral candidates. Like the candidates, respondents were given 20 tokens and asked to allocate the tokens in any manner they wished across the ten categories. We then calculated how close the preferences of the candidates were to those of the household by comparing the share S that voter v allocated to sector s with the share that candidate c allocated to the sector. The total spending over which the candidate and voter agree is given by an agreement index, defined as $A_{vc} = \sum_s \min(S_{vs}, S_{cs})$.

A potential concern with this variable is that it represents a choice that respondents are not used to making, undermining the quality of the data collected. To check this, we regress preferences on a number of household characteristics that we expect to be correlated with preferences for a given sector. For example, we expect families with children to favor spending on education and farmers to favor spending in agriculture. Results presented in Table A.8 suggest that stated preferences over spending priorities match observable household characteristics.

It is also possible that, since household preferences were collected after the information about candidates' promises had been distributed to voters in the treatment group, voters might have adjusted their preferences to match their preferred candidate's promises. Two pieces of evidence suggest that this is not the case. First, we are unable to reject the null hypothesis that the alignment between respondents and their preferred candidate is the same between the treatment and control group. This holds whether we define the preferred candidate as the top-ranked candidate on the 0-4 scale or as the candidate whom respondents indicated voting for in the

¹⁴Respondents were given ballots with only ID codes corresponding to their survey instrument. The ballots contained the names and parties of the mayoral candidates in the municipality, in the same order and spelling as they appeared on the actual ballot. The respondents were instructed to select the candidate that they voted for, place the ballot in the envelope, and seal the envelope. Enumerators could not see the contents of these envelopes at any point and respondents were told that the envelopes remained sealed until they were brought to the survey firm to be encoded with the rest of the survey.

secret ballot exercise. Second, the correlation between alignment and support for given candidates is essentially the same across the treatment and control groups (Results in Table A.12).

Occurrence of vote buying Candidates have a wide network of brokers (or *liders*) across barangays, often building on existing social ties and obligations, such as their family members, employees, and tenants (Lande, 1996; Fegan, 2009; Cruz et al., 2014). These brokers are involved in many different aspects of the campaign, including distributing flyers and posters, coordinating rallies, and assisting with vote buying and other illegal strategies. They even serve as poll watchers on election day itself. Because vote buying is a logistically demanding electoral strategy, candidates do their hiring and recruiting months before the election to ensure that they have sufficient staff to be able to buy votes during the campaign period.¹⁵ Hence, we expected that our information intervention, a week before the election, would affect vote buying.

We measure vote buying through a series of questions asking whether respondents were aware of any case of vote buying in their village and if, during the recent election, someone offered them money for their vote. In the Philippines, social desirability bias associated with vote buying is low and responses to direct questions provide credible estimates of vote buying incidence. Khemani (2015) uses direct questions to estimate vote buying in research in Isabela, a province near our study area. Using a survey in the same province, Cruz (2013) finds that the estimated rate of vote buying using an unmatched count technique is 21.4 percent, statistically indistinguishable from the estimate calculated using the direct question, 23.9 percent.

These results indicate that it is unlikely that respondents who received the flyer—which neither mentioned vote buying at all nor made normative claims of any kind—became *more* willing to report vote buying than respondents who did not receive the flyer, since there is no evidence of social desirability bias in the first place. Furthermore, the treatment did not affect response rates to the “*did someone try to buy your vote?*” question: the non-response rate was 6.0 percent in the control

¹⁵This is consistent with Stokes et al. (2013), who argue that candidates give local brokers resources to ensure a certain level of support for the candidate. Brokers retain some of these resources as rents for themselves, but rapidly disburse when they observe an information shock that reduces support for their candidate.

group and 7.3 percent in the treatment group. The difference is not significant (p-value = .128).

4 Results: Effects on Knowledge and Vote Buying

Two direct effects of the experiment are of particular interest: did the information treatment in fact increase relevant political knowledge? And did it influence vote buying? The results in this section first verify that treated voters are indeed more informed about candidates and more likely to regard municipal spending as electorally salient. We then report results that the intervention increased vote buying.

4.1 Did the Treatment Increase Knowledge?

The descriptive statistics reported in Table 1 suggest that voters tend to be poorly informed about candidates' promises: voters in the control group make an average of seven mistakes over the ten sectors. The information treatment, however, had the effect of increasing voter knowledge of those promises.

To show this, we estimate regressions of the form:

$$Y_{ijk} = \alpha T_j + v_k + u_{ijk} \quad (1)$$

where Y_{ijk} is the knowledge index for individual i in village j in pair k , T_j is a dummy equal to one if the campaign was implemented in village j , v_k is a pair-specific unobservable and u_{ijk} is the usual idiosyncratic error term. To account for the way the randomization was carried out, standard errors are clustered at the village level. We also test if results are robust to the inclusion of the two variables that are not balanced between treatment and control.

As expected, voters in treatment villages are more likely to know which candidate is promising to spend the largest share of the LDF on any given sector. Results are available in Table 2.¹⁶ The treatment had no effect on dimensions of political

¹⁶Further results, with different control variables and fixed effects, are available in Panel A of Table A.9. Supporting the strength of our randomization strategy and the balance between treated and control groups, the point estimates are essentially constant across the four different specifications, though the standard errors get smaller as we include more fixed-effects and additional control variables. As is the case with a number of other outcome variables, the fixed effects explain a large share of the variation in voter knowledge.

knowledge not included in the flyers (Table A.13).

We further explore whether the treatment affected the salience of local development spending on vote decisions. We estimate:

$$Y_{ijk} = \alpha T_j + v_k + u_{ijk} \quad (2)$$

where Y_{ijk} captures how salient sectoral allocations are when individual i in village j in pair k decides which candidate to vote for. The set-up is equivalent to the one used for equation (1).

As expected, treated respondents are more likely to report that candidates' proposals for local development spending are important when they decide which candidate to vote for. This is true for both salience measures, absolute and relative to other voting influences. Of particular note, the information treatment had no effect on the salience of vote buying. The point estimate is small (0.018, p-value equal to 0.65) and about one-tenth of the point estimates on the salience of budgetary allocations. Results are available in Table 2. These results are robust to specifications with alternative controls (see Panels B and C of Table A.9).

4.2 Information Effects on Vote Buying

Survey results indicated high levels of vote buying - 14 percent of voters in the control group indicated being offered money for their votes. Vote buying tends to take place a few days before the elections and, as the earlier discussion indicates, even though our intervention was rolled out shortly before the elections, candidates had sufficient time to adjust their campaigning strategies, should they have chosen to do so. In fact, we can show that vote buying increased in the treatment villages, estimating equations of the form:

$$Y_{jk} = \alpha T_j + v_k + u_{jk} \quad (3)$$

where Y_{jk} is the prevalence of vote buying in village j in pair k during the May 2013 elections.¹⁷ The set-up is equivalent to the one used for equation (1).

The results in Table 2 indicate that vote buying intensified in treated villages (effects that are robust across specifications with alternative controls, shown in Ta-

¹⁷Recall that, as indicated in the PAP, we run those regressions at the village-level. We obtain similar results if we run those regressions at the individual-level instead (Table A.14).

ble A.10).¹⁸ The first outcome variable is the share of respondents who were aware of instances of vote buying in their village. This is an imprecise measure of vote buying, since voters have incomplete information about whether their neighbors have been targeted for vote buying. The second outcome variable is the share of respondents who were directly offered money for their votes. The point estimates are very close in both specifications. However, consistent with the fact that the first variable used is a noisier measure of vote buying, we can only reject the null of no effect with the second one. The intervention led to a 3.4 percentage points increase in vote buying (24 percent of the control group mean).¹⁹

These results are consistent with qualitative evidence about vote buying activities in the study area. Observers assured us that vote buying occurs in the days before the election and that candidates and their brokers can re-target vote buying quickly. In many cases, the candidates contacted PPCRV with specific questions about the intervention activities. Importantly, these sources also reported that candidates redoubled efforts to buy votes in the treatment barangays and that most of the additional vote buying occurred on election day or the day before.²⁰

The treatment effects on vote buying could have emerged if candidates injected additional resources into the campaign, raising the welfare of vote buying recipients without reducing the welfare of other voters; or if candidates had instead transferred resources from barangays that had not received the flyer to those that did. The theoretical discussion below indicates that transferring resources away from control and towards treated barangays would have sub-optimally reduced candidate support in the control group. That is, theory suggests that the vote buy-

¹⁸The specifications we examine in Table A.10 were anticipated in the PAP. However, we also anticipated that information would reduce vote buying, based on the intuition that the flyers would increase the salience of candidate promises regarding public good provision, leading them to substitute away from vote buying in treated areas. However, this intuition was incomplete, since nothing about our intervention introduced sanctions that voters could impose on candidates who reneged on their promises. In addition, the PAP did not anticipate that the flyers would give households new information about the spending program itself, which would lead them to update incumbent performance thresholds. It is this effect that we model and test. Note that, while the intervention increased vote buying, we argue that this was a result of an intervention that actually increased incumbent incentives to improve voter welfare.

¹⁹A limited number of respondents refused to answer the vote buying questions. In the main regression we code 'refuse to answer' as missing. We obtain similar results if we code 'refuse to answer' as yes (Table A.15).

²⁰Interviews conducted during the debriefing with PPCRV staff after the May 2013 elections, with follow-up interviews conducted April 2016.

ing response to the information intervention should have been welfare-improving for voters. However, we do not have sufficient information to allow us to discriminate between these two cases.

5 Information shocks and support for the incumbent

The fact that an apparently desirable information intervention increased vote buying is surprising. This section develops a model that accounts for it, and section 6 presents additional evidence supporting the explanation. The core of the argument is that the flyer informed voters of the existence of a key government program to provide infrastructure, along with evidence of the significance of the program (the flyer, the participation of a prominent NGO, and the participation of the candidates). Taking advantage of voter ignorance of the program, incumbents shirked in implementing it, leading to voter disappointment when voter information increased. To offset this disappointment, incumbents engaged in greater vote buying prior to the election. All of the effects that we identify in the previous section and in section 6 are consistent with this interpretation.

Reflecting the inability of mayoral candidates to make credible commitments regarding post-electoral policies, we adopt a retrospective voting framework: voters establish a performance threshold for incumbents and vote for or against the incumbent depending on whether the incumbent has met the threshold. If the threshold is too high, incumbents make no effort to deliver benefits to voters and, instead, maximize private rent-seeking. If the threshold is too low, voters extract fewer benefits from the incumbent than they could have. Assuming that voters can spontaneously coordinate on this threshold, as in Ferejohn (1986) and Persson and Tabellini (2000), their challenge in setting the threshold is uncertainty about the welfare that the incumbent could have potentially delivered. Voters' incomplete information makes it difficult for them to distinguish incumbent shirking from an unfavorable state of the world that would prevent any incumbent from improving welfare.

This analytical approach is consistent with two key features of mayoral elections in the Philippines. First, political competition does not center on policy promises, which are not credible. Hence challenger promises do not matter, and voters base their decisions only on whether incumbent performance meets the

threshold voters have set. Second, mayors are the dominant decision makers in municipal government and voters should hold mayors accountable for their spending out of the Local Development Fund.

Incomplete voter information shapes incumbent advantage in the literature and this analysis. As voters are less informed about what incumbents can do for them, they set a lower performance threshold, allowing incumbents to do less and still be re-elected. Advantaged incumbents are those whose voters exhibit a larger knowledge gap. The gap between voter expectations about incumbent performance before and after seeing the flyer should be largest where the incumbent was most successful in suppressing expectations—where incumbent advantage was present.²¹

This section presents a more formal analysis of this logic, extending the standard retrospective model to address a pervasive and previously unanalyzed phenomenon, voter uncertainty about the public goods that politicians can supply. It also links voter uncertainty to another common phenomenon, vote buying, offering one explanation for the pervasiveness of vote buying in some electoral systems and its rarity in others.

5.1 Basic Set-Up

There are N arbitrarily small groups of voters indexed by i . Incumbent mayors can spend money either on public goods such as infrastructure, g , or on direct transfers to voters, f_i . Since subnational governments in many countries, including the Philippines, rely on transfers from the central government, the government budget is exogenous and given by M . As in the canonical retrospective voting model (e.g., Persson and Tabellini 2000, pp 236 - 238), public goods deliver welfare $H(g)$ to each voter, while transfers deliver welfare equal to the amount of transfers that the voter receives. The cost of all transfers received by voters is $\sum f_i$.

The field experiment gave voters new information about the public goods that incumbents could provide. The intervention could be modeled as a shock to voter beliefs either about the ability of government to finance services of any kind - the

²¹In contrast to previous research, in our analysis the information available to voters is a parameter. Prior research is concerned with the emergence of incumbency advantage and therefore models voter information as a choice variable. Our question, however, is how an exogenous information shock affects public policy choices in the presence of an incumbent advantage. The effects of the intervention, therefore, should be greatest in incumbent strongholds.

government budget constraint - or about the relative costs to government of turning budgetary resources into public goods versus transfers. Since the intervention informed voters only about the existence of an important program to finance infrastructure, the analysis below adopts the second approach.²²

The cost parameter governing public good provision is $\bar{\theta}$ and total costs of providing public goods are therefore $\bar{\theta}g$. The cost is higher when there are restrictions on the type of public goods that can be purchased, when the costs of inputs and construction are high, or when the bureaucracy is incompetent. As long as the costs $\bar{\theta}$ are not too high, government decisions to spend more on local public infrastructure delivers greater welfare to voters per peso of spending than do direct transfers.

Mayors choose direct transfers and public good spending to maximize their pecuniary rents, $r = M - \sum_N f_i - \bar{\theta}g$, and the non-pecuniary rents from being re-elected, R :

$$M - \sum_N f_i - \bar{\theta}g + pR$$

where p is the probability of re-election. In the event that they do not expect to be re-elected, they set $g = f = 0$ and take as pecuniary rents the entire budget.

The welfare of voters in group i is given by $\omega = f_i + H(g)$. Voters prefer that the mayor dedicates the municipal budget to public goods until $H_g(g) = \frac{\bar{\theta}}{N}$, the Samuelsonian condition for public good provision, and then to distribute any remaining budget in the form of transfers.

We add three features to this standard set-up. First, for most public goods, spending takes time to implement before voters perceive a change in their welfare. Mayors must therefore decide to spend money on public goods early in their terms (Robinson and Torvik, 2005). Transfers, however, can be implemented quickly, even at the end of the mayor's term and right before the next election. Mayors have two opportunities, then, to make budget decisions. Earlier in their tenure, they can decide to supply public goods or transfers (though, for any expenditure

²²The first approach yields the same conclusions if voters believe that the participation constraint of the incumbent binds at the performance threshold. When voters are unexpectedly informed that the budget is larger than they thought, support for the incumbent drops unless the incumbent makes targeted transfers to some voters. The identity of those voters is established in Proposition 2, below.

amount, public goods deliver greater welfare to voters). Late in their tenure, they can only deliver transfers. This accurately reflects the limitations on incumbents' ability to react to information shocks in the weeks before an election.

Second, voters are uncertain about the costs to the incumbent of providing them with public goods. Just before the election, each voter's beliefs about the costs of producing public goods are drawn from a uniform distribution given by $\theta_i \sim [1, 2\theta_c - 1]$, $\theta_c > 1$. Incumbents know this distribution, but not the beliefs of individual voters. The median belief about the incumbent's costs of producing public goods is given by the cost parameter θ_c . The ability to produce is never less than one - it can never cost less than g to produce g .

Our intervention is equivalent to an unexpected shock that shifts this distribution for a randomly-selected fraction δ of all voters, $\delta \leq 1$ (approximately 44.5 percent in the case of our treatment). Incumbents know which voters are subject to the shock, but beyond that only know that the distribution of beliefs about the costs of producing public goods follows $\theta'_i \sim [1, 2\theta'_c - 1]$, where $\theta'_c = \theta_c + k(\bar{\theta} - \theta_c)$, and the shock parameter $k \sim [-1, 1]$. Recalling that citizens do not know $\bar{\theta}$, the true cost of producing public goods, the effect of the information shock reflects the assumption that the more accurate are the beliefs θ_c of citizens regarding the costs of public good provision, the less they change in the event of a shock. This is plausible in general, and specifically consistent with our experimental intervention, since we provided voters with the "true" ability of politicians to provide public goods; those voters who knew this already were therefore unaffected by the intervention.

The information shock in our field experiment, and in the model here, is unanticipated. Hence, incumbents do not take it into account when deciding on public goods.²³

As usual in retrospective voting models, citizens coordinate on a voting rule that is conditional on their beliefs about the costs of public good production just before the election, after the mayor has provided public goods. At the beginning of

²³We abstract from anticipated information shocks. Their inclusion would complicate the analysis, but not change the key results. An anticipated shock would take the form of some random variable z that would change the cost parameter in the distribution of beliefs according to $(\theta_c + z(\bar{\theta} - \theta_c))$. As in standard retrospective voting models, voters and incumbents would be aware of the distribution of z and anticipate the possibility of the shock in the construction of their performance thresholds and decisions regarding public goods. Again, however, once equilibrium public goods are provided and the unanticipated shock occurs, the dynamics of vote buying remain the same.

the mayor's term, voters establish the rule that, given their individual draw from the distribution of potential pre-electoral beliefs about the costs of public good production, θ'_i , they will support the incumbent who meets the performance threshold $\bar{\omega}_i \geq H(g_{\theta'})$, where $g_{\theta'}$ is determined implicitly by $H_g(g_{\theta'}) = \frac{2\theta'_i}{N}$.²⁴

The third feature of the set-up provides an immediate link to the literature on incumbent advantage. Incumbents have an advantage when voters' beliefs about the costs incumbents confront are above their true costs of providing public goods. Recall that voters draw their pre-electoral beliefs from the distribution $\theta_i \sim [1, 2\theta_c - 1]$. Where incumbents have an information advantage, $\theta_c > \bar{\theta}$.

The stages of the game are therefore the following:

1. Incumbents and voters observe the distribution of beliefs about the costs of public good provision, $\theta_i \sim [1, 2\theta_c - 1]$, that voters will have before the election.
2. Voters coordinate on a voting rule $\hat{\omega} = \omega(g_i)$, where g_i is given by $H_g(g_i) = \frac{2\theta_i}{N}$.
3. Incumbents choose the level of public good provision g .²⁵
4. A randomly-selected subset of all voters $\delta \leq 1$ are subject to an unanticipated shock k to the distribution of their beliefs about the costs of producing public goods, such that for these voters $\theta'_i \sim [1, 2\theta'_c - 1]$, where $\theta'_c = \theta_c + k(\bar{\theta} - \theta_c)$, $k \sim [-1, 1]$.
5. Incumbents choose the level of spending on transfers to voters.

²⁴In the usual retrospective voting model, both an economic shock and government policy affect voter welfare; voters do not observe either, but take the distribution of the shock into account when setting a performance threshold for the incumbent. The incumbent observes the shock and makes policy. Here, neither politicians nor voters anticipate the shock that will inform voters about politician ability; and politicians do not observe the shock before they set public goods provision. Since politicians cannot exploit an information asymmetry between themselves and voters, as in the canonical model of retrospective voting, voters can do no better than to require politicians to meet the performance threshold that is indicated by the revelation of θ' , voters' best information about the true efficiency of public good provision.

²⁵When voters observe public good spending g , from the participation constraint of the incumbent they can infer an upper limit on the cost of providing public goods, $\theta \leq \frac{R}{g}$. The voters who believed that the cost was higher than this immediately update their beliefs about costs. However, this updating does not change their voting behavior, since incumbent spending that satisfies the performance threshold of voters who believe the costs were θ by necessity satisfies those who believe the costs were higher, and who set a lower performance threshold.

6. Voters' individual beliefs about the costs of public good provision are revealed to them.
7. The election takes place.

Proposition 1 establishes the equilibrium level of public good provision. The remainder of the analysis then describes the conditions under which vote buying takes place, the amount of vote buying, and the voters targeted for it.

Proposition 1 *Incumbents set public good provision to meet the expected performance threshold given the voting rule, $\bar{\omega} = H(g_{\theta_c})$, where public good provision is given by $H_g(g_{\theta_c}) = \frac{2\theta_c}{N}$.*

Proof: See technical appendix.

Lemma 1 confirms the effects on voter support of unanticipated information shocks that increase or reduce voter expectations about the costs of providing public goods, given the public goods that the incumbent chose to provide.

Lemma 1 *After a positive unanticipated information shock, $k(\bar{\theta} - \theta_c) > 0$, a fraction of voters δ believe that the costs of providing public goods are higher than they previously believed and the public goods provided by the incumbent meet the performance threshold of more than half of the voters. After a negative unanticipated information shock, $k(\bar{\theta} - \theta_c) < 0$, a fraction of voters δ believe the costs are lower than previously believed and public good provision meets the threshold of less than half of the voters.*

Proof: See technical appendix.

Proposition 2 describes the incumbent response to an information shock. In particular, if the shock is adverse (it tells voters that it is less expensive to provide public goods than the incumbent anticipated), incumbents have an incentive to target transfers to those affected by the shock rather than others. Moreover, more advantaged incumbents (those with a larger information advantage) target a correspondingly larger fraction of those voters.

Proposition 2 *After a positive unanticipated information shock, $k(\bar{\theta} - \theta_c) > 0$, there is no change in public policy. In the event of a negative unanticipated information shock,*

incumbents target transfers $f_k = H(g_{\theta_c+k}) - H(g_{\theta_c})$ to a fraction α of voters in δ who received the information shock, where α is given by $\alpha^* = \frac{M - \bar{\theta}g_{\theta_c} + R + l\delta f_k}{\delta f_k}$, $l = \frac{1}{2} \left(\frac{k(\bar{\theta} - \theta_c)}{\theta_c + k(\bar{\theta} - \theta_c) - 1} \right)$.

Proof: See technical appendix.

Proposition 2 shows that voters who receive more accurate information about the public goods the incumbent *could* have provided raise their performance threshold, in accordance with the voting rule. Since incumbents cannot adjust the provision of public goods in time for the election, they respond to the higher threshold by targeting more informed voters with greater vote buying. This result is only possible if public good spending begins substantially before the election, while transfers can be made right before the elections. Evidence from the Philippines, discussed earlier, supports these assumptions.

Section 6 offers evidence for other assumptions and ancillary predictions of the model. First, results from a survey experiment demonstrate that merely presenting respondents with information about the existence of the Local Development Fund reduces support for the incumbent mayor. Second, respondents who received the flyer and who report that their village has received public goods in the previous three years are significantly less likely to report vote buying than respondents who received the flyer and did not report recent public good provision. That is, treated respondents who report the recent provision of public goods have less reason to be disappointed in incumbent performance and were correspondingly less likely to be targeted for vote buying by the incumbent.

Third, in the retrospective framework employed here, voters care most about incumbent characteristics and performance. We should therefore observe stronger information effects with respect to incumbents than to challengers on voter knowledge of candidates and on candidate vote buying. These stronger effects emerge in the data.

Fourth, the evidence supports the argument that incomplete voter information is a source of incumbent advantage. The term $k(\bar{\theta} - \theta_c)$ embeds our assumption that when the difference between voter beliefs and the actual costs of public good provision is greater, as with dominant incumbents, information shocks should have a larger impact on voter beliefs. The evidence in section 6 indicates

that this is in fact the case.

In addition, the evidence also supports a straightforward extension of the model, that the shocks should have a larger effect on vote buying in municipalities with advantaged (dominant) incumbents. Differentiating the optimal fraction of voters to be targeted with vote buying, α^* , with respect to incumbent advantage, $(\bar{\theta} - \theta_c)$, is equivalent to $\frac{\partial \alpha^*}{\partial l} \frac{\partial l}{\partial (\bar{\theta} - \theta_c)}$. The term $\frac{\partial l}{\partial (\bar{\theta} - \theta_c)}$ describes the effects of incumbent advantage on the fraction of voters whose beliefs are shifted by the information shock. Since for a negative information shock $k(\bar{\theta} - \theta_c) < 0$, $-\frac{1}{2} \left(\frac{k(\bar{\theta} - \theta_c)}{\theta_c + k(\bar{\theta} - \theta_c) - 1} \right)$ increases in the distance between $\bar{\theta}$ and θ_c , $\frac{\partial l}{\partial (\bar{\theta} - \theta_c)} > 0$. That is, the fraction of disappointed voters increases when incumbent advantage is greater. Then differentiating α^* with respect to l equals one: conditional on meeting the participation constraint, the share of voters α^* whose votes are bought rises one for one with the fraction of voters who are dissuaded from supporting the incumbent by the information shock. The fraction of voters whose vote is bought therefore rises with incumbent advantage. We find, indeed, that vote buying is higher in incumbent strongholds.

6 Results: Mechanisms and Alternative Explanations

To test the robustness of the explanation put forward in the model, we examine a number of additional predictions of the model: informing voters *only* about the existence of a spending program reduces preferences for the incumbent; vote buying effects are weak in treated areas that report recent public works projects and strong in areas that do not; voters are more likely to make mistakes that favor the incumbent and those mistakes are significantly reduced by the information treatment; knowledge effects are significant for the incumbent and not the challenger; and the correlation between respondents experience with vote buying and their candidate preferences is consistent with vote buying by the incumbent, not the challenger. These results support the interpretation developed above, that program information drives effects on vote buying. They cannot be accounted for by the information in the flyer regarding candidates and their spending allocations.

6.1 Results of a survey experiment that isolates the effects of program information

To further support this claim that information about the Local Development Program, and not information about candidates, accounts for the results we report, we conducted a follow-up survey experiment in September 2015 in three Philippine municipalities. Treated respondents received exactly the same information about the Local Development Program, presented in a flyer with the same format, as respondents received in the field experiment. The only change was the omission of candidate information and candidate proposed allocations. We then asked whether respondents who received the modified flyer were less likely to support the incumbent.

Within each of the three municipalities, 100 randomly-assigned respondents received information on the LDF and 100 randomly-assigned control respondents did not, for a total of 600 respondents.²⁶

The treated and control respondents are balanced across 15 variables for which we have information: there are no significant differences among them with respect to their length of residence in their barangay; their gender, age, education levels; their household size; whether they receive remittances from abroad; whether they benefit from the Philippines conditional cash transfer program; whether they have asked the mayor or barangay captain for assistance; and whether they voted in the 2013 municipal elections (Table A.7).

Towards the end of the interview, treated respondents were then presented with a flyer with information about the LDF including, as in the field experiment, the ten categories of spending that could be undertaken under the program.²⁷ Right after that, the survey ended with a secret ballot in which respondents indicated how likely they would be to support the incumbent mayor in the next election.

The secret ballot asked respondents whether they were very likely, likely, nei-

²⁶For cost reasons, we implemented the experiment in rural areas of Laguna, a province located south of Metro Manila. We selected three municipalities where the incumbent was in his/her first or second term (to avoid politicians that are ineligible for reelection) and randomly selected 10 villages per municipality. Within each village, the survey team used the village list to randomly select 20 respondents for the survey experiment, 10 of them received the flyer and 10 of them did not.

²⁷A copy of the flyer is available in Figures A.3 - A.4.

ther likely nor unlikely, unlikely or very unlikely to support the incumbent in the next election. Our argument predicts that the information intervention should have raised the performance threshold of those who were previously ignorant of the Local Development Program. As a result, some respondents who would have expressed support for the incumbent prior to the information intervention should have been disappointed and instead indicated that they were neutral, unlikely or very unlikely to support the incumbent. However, among respondents who already did not support the incumbent, the higher performance threshold simply meant that they continued not to support the incumbent.

The survey experiment took place well outside the electoral cycle (nine months before the next municipal elections). Nevertheless, respondents who received information about the Local Development Fund were significantly less likely to express support for the incumbent. Table 3 indicates the percentage of respondents in the treatment and control groups who chose each of the response categories. A notably smaller fraction (six percentage points fewer respondents) said that they were "likely to support the mayor". Correspondingly, a notably larger fraction of respondents in the treatment group (6.6 percentage points) were neutral.

We then classify each respondent as supporting the incumbent (very likely or likely categories), being neutral or not supporting the incumbent (unlikely or very unlikely categories). Controlling for village fixed effects, treated voters are between seven and eight percentage points less likely to express support for the incumbent and approximately 6.5 percentage points more likely to be neutral (Table 4). The magnitude of these effects is large, reducing support for the incumbent by 12 percent. Although the results are noisy, all are significant at least at the 10 percent level. In addition, the negative treatment effect on support for the incumbent is significant at the five percent level when we include individual controls.

Respondents who already believed that the incumbent did not meet their performance threshold (voters who were neutral or unsupportive of the incumbent prior to the survey) did not change their stance when they were exposed to the information treatment. The increase in neutral voters comes exclusively from the group of respondents who would have been likely to support the incumbent in the absence of the information treatment, but who instead became neutral as a result of hearing about the Local Development Program. These results offer direct evidence of the mechanism that we claim accounts for the results of the field experiment.

6.2 The effects on vote buying of public investment in barangays

Further direct evidence of our mechanism comes from another module of the end-line survey of the field experiment. There, respondents indicated the public investments in their barangay that had been financed by the incumbent mayor since the previous election. Voters who observe public investment in their barangay should have been less likely to be disappointed by information about the Local Development Program and therefore less likely to be targeted by the incumbent for vote buying.²⁸

To see if this was the case, we supplement the earlier vote buying regression by controlling for public investment financed by the mayor and its interaction with treatment status. We expect that the positive treatment effect on vote buying should be lower in villages that reported public investment. Consistent with this, the interaction terms in Columns 1 and 2 of Table 5 are significant and negative. We also can ask whether the effects of the information intervention on vote buying differ depending on whether villages report greater than or less than median public investment by the incumbent. In villages reporting below-median public investment, the treatment significantly increased vote buying by 6.4 percentage points (Columns 3 and 4 of Table 5). The treatment effect in villages with above median public investment was tiny (0.2 percentage points) and insignificant. These results offer an information rationale for the negative correlation that Khemani (2015) documents between public good provision and vote buying in the Philippines.

6.3 Heterogeneous Effects on Knowledge and Vote Buying across Incumbents and Challengers

Our explanation for the results of the field experiment implies that the information shock should have affected incumbents more than challengers. First, the flyer should have significantly reduced the information advantages of incumbents, but not challengers. Second, incumbents, but not challengers, should react to the information shock with greater vote buying. The data are consistent with both of these

²⁸It is not plausible that respondents who report more public investment had received that investment because they had higher thresholds. On the one hand, incumbents should prefer to satisfy lower performance thresholds before they satisfy higher thresholds. Knowing this, voters should not set higher thresholds. On the other hand, if this were the case, we would not observe the results we report here, that vote buying falls in treated villages that report more public investment.

predictions.

One manifestation of a candidate information advantage is that voters incorrectly attribute favorable characteristics to candidates. A natural way to characterize these mistakes systematically is to first create a variable that captures whether respondents make a mistake in identifying which candidate promised to spend the greatest share of the development budget on some sector s . We then classify any error made by the respondent as favoring the incumbent when the respondent claims that the incumbent promised to spend the greatest share, but actually the challenger did; and favoring the challenger in the reverse case. Consistent with an information-based theory of incumbency advantage, on average, five of the seven errors made by the average respondent favor the incumbent. That is, respondents were systematically more likely to say, mistakenly, that the incumbent would spend the most in some particular sector.

This variable can then be used to estimate whether the information treatment had a significant effect on incumbent-favoring, but not challenger-favoring, errors. We estimate equation (1) where Y_{ijk} is either the number of errors that favor the incumbent or the number of errors that favor the challenger. The results displayed in Table 2 indicate that the information treatment significantly reduced incumbent-favoring errors but had no effect on challenger-favoring errors.²⁹ The point estimate of the effect of the information shock on incumbent-favoring errors is more than eight times larger than point estimate for challenger-favoring errors. Treated households—more informed households—made significantly fewer errors favoring the incumbent, consistent with our earlier arguments that incumbent advantage is related to voter over-estimates of incumbent contributions to voter welfare (or, in terms of model parameters, that voters in incumbent strongholds are particularly likely to over-estimate the costs of providing public goods).³⁰

By construction, the model predicts that incumbents are responsible for the increase in vote buying in response to the information treatment. We cannot directly document this, because of the sensitivity of questions that probe which candidates bought respondent votes. However, indirect evidence indicates that incumbents are responsible for higher vote buying in treated areas.

²⁹Further results are available in Table A.11

³⁰These results cannot be explained by differences in incumbent and challenger promises: as reported in Table A.17, incumbent and challenger promises do not differ systematically.

Our argument is that informed voters were disappointed in the incumbent and therefore less likely to support the incumbent. However, incumbent vote buying would have mitigated the disappointment of informed voters. In contrast, challenger vote buying would have exacerbated the negative effect of the treatment on incumbent support. In fact, we find a strong negative correlation between treatment status and support for the incumbent, but this correlation disappears among those treated households that reported vote buying. This is consistent with treatment effects on vote buying by the incumbent, not by the challenger.

To show this, we used the secret ballot from the endline survey (where respondents indicated whom they supported in the election) to estimate equation (1), where Y_{ijk} is a dummy equal to one if the respondent declared voting for the incumbent. Table 6 divides our sample into those who reported that their votes were bought and those who did not. Among treated voters, those who report that someone offered to buy their vote were no less likely to support the incumbent than untreated voters who reported that someone offered to buy their votes. However, treated voters who did not report vote buying were significantly less likely to report that they voted for the incumbent than untreated voters who did not report vote buying. Among those whose votes were not bought, the fraction of treated voters who indicated they voted for the incumbent was four percentage points less than the corresponding fraction of untreated voters.³¹

This pattern is only consistent with incumbent vote buying, which offset the negative effect of the treatment on preferences for the incumbent. It is inconsistent with challenger vote buying, which should have increased the tendency of treated voters to vote against the incumbent.³²

If incumbents are responsible for the increased vote buying in treated barangays, and the reason they undertake this vote buying is to offset voter disappointment induced by the information treatment, then we should see no treatment effect on incumbent vote shares. Table 8 reports precisely these results (Columns 8 and 9). Incumbent vote share, whether official or self-reported, was no different in treated

³¹Non-response rates for the secret ballot were 9.7 percent. There was no difference in non-response rates between the treatment and control groups.

³²A different approach to the data yields the same conclusion. Assume that voters who report both voting for the incumbent (challenger) and having had their vote bought in fact had their vote bought by the incumbent (challenger). Table A.16 shows that this proxy for vote-buying by the incumbent is significantly higher in the treatment group (columns 1 and 2), but that the proxy for vote buying by the challenger is unaffected by the information treatment (columns 3 and 4).

or control villages.³³ Vote buying should equally offset disappointment in municipalities with dominant incumbents (where we argue voter disappointment was greater). Consistent with this, the interaction of the treatment with incumbent vote shares in 2010 (our proxy for incumbent dominance) is always insignificant.

Qualitative evidence also supports our claim that incumbents were responsible for treatment-induced vote buying. Two local PPCRV affiliates in Ilocos Sur confirmed that incumbents conducted additional vote buying in the treatment areas after our intervention was completed. They specified that most of the additional vote buying occurred on election day or the day before.³⁴

6.4 Heterogeneous Effects on Knowledge and Vote Buying across Dominant and Non-dominant Incumbents

If our explanation for the results of the field experiment is correct, then information shocks should have larger effects where incumbents are dominant and where, as a consequence, incumbents' pre-intervention information advantage was greatest. The connection between limited voter information and incumbent advantage is especially plausible in the Philippines, where journalists are subject to significant pressure relative to other countries. The Freedom House (2014) report on press freedom rates the Philippines as only partly free, with journalists the target of libel suits by local politicians, harassment and assassination attempts (many successful). According to Campos and Hellman (2005), the lack of information especially affects local politics, due to poor media penetration and reduced capacity at the local level.

We examine the role of incumbent dominance, proxied by incumbent vote share in the previous municipal elections that took place in May 2010. Dominance is measured in terms of both barangay (village) and municipal electoral results. Vote share, in turn, is calculated as a percentage of the registered population.³⁵

If dominant incumbents have an information advantage, then voters in their

³³To facilitate interpretation, the interacted variables are demeaned so the coefficient on the treatment dummy still captures the average treatment effect.

³⁴Interviews conducted during the debriefing with PPCRV staff after the May 2013 elections, with follow-up interviews conducted April 2016.

³⁵Results are similar when using vote share defined as a percentage of the voting population (Table A.18).

municipalities should be less knowledgeable, allowing dominant incumbents to provide, and these voters to report, fewer projects. Consistent with this, Table 7 shows, first, that the larger is the 2010 incumbent vote share, the lower is an index of respondent knowledge of candidate promises, an index that captures respondent knowledge of politicians in their village, municipality and province; and an index that reflects the respondent's knowledge of mayoral candidates' political experience and education levels. Second, the greater the 2010 incumbent vote share, the higher the number of incumbent-favoring errors and the fewer are the projects that respondents report were implemented in their village between 2010 and 2013.³⁶

If information is central to incumbent dominance, as both theory and the evidence in Table 7 suggest, then the information treatment should have had correspondingly larger effects in municipalities with dominant incumbents. Table 8 reports coefficients on the interaction of the treatment effect and incumbent's 2010 vote share.

Incumbent dominance has a significant effect on all key treatment effects. The positive impact of the treatment on knowledge of campaign promises is significantly higher in municipalities and villages with dominant incumbents. The information treatment reduces the number of incumbent-favoring errors by more in municipalities with dominant incumbents. There is no associated reduction in the number of challenger-favoring errors. Finally, a one standard deviation in incumbents' 2010 vote share increases the impact of the intervention on vote buying by more than 10 percent.

6.5 Alternative Explanations

A number of alternative explanations do not account for the foregoing findings. For example, the vote buying results might simply reflect the fact that candidates increased the intensity of their campaign in treated areas, and vote buying reflected just one element of this increased intensity rather than an effort to ease voter dis-

³⁶Since the theory predicts that dominant incumbents provide fewer public goods than non-dominant incumbents, a natural empirical exercise would be to compare effort across these two incumbent classes with respect to spending out of the Local Development Fund. This would require spending data across the different municipalities, but also data on the quality of the spending (the actual projects that were implemented). We hope to collect these data in the future.

appointment. However, our field workers reported no large scale changes in campaigning intensity – for example, rallies in treatment villages. In addition, we can show directly that there were no treatment effects on one particular type of campaigning, candidate distribution of flyers. During the endline survey, we asked respondents whether they received flyers from one of the candidates in the week before the elections. Voters in the treatment group were no more likely to receive flyers from the candidates. Results are available in Table A.21.

Two alternative explanations might be offered for the heightened treatment effects in municipalities with dominant incumbents. We test and reject each of them: that challenger promises are, in fact, credible and preferred by voters in municipalities with dominant incumbents, prompting greater vote buying by the incumbent; and that dominant incumbents are better able to respond to the information shock.

One reason to question these alternative explanations is that neither can account for the stronger effects of the treatment on knowledge and incumbent-favoring errors in municipalities with dominant incumbents. In addition, however, the empirical evidence speaks against them.

First, if challenger promises were actually credible and more appealing to voters in municipalities with dominant incumbents, challenger and incumbent promises would have differed. This implies that the treatment effects would have disappeared after controlling for the degree of overlap between candidate promises. Panel A of Table 9 reports results rejecting this possibility. The models include two additional terms compared to those of Panel A in Table 7: the measure of the overlap between candidate promises (the share of the budget on which the candidates agree), and the interaction of this variable with the treatment dummy. The new interaction term is insignificant, however, and the inclusion of these new variables has no effect on the significance of the main interaction of interest, between the treatment and the measure of candidate dominance.

A second alternative explanation is that dominant incumbents are of better quality and therefore able to react more adroitly to the intervention. If this mechanism accounted for our results, the interaction of the treatment dummy and incumbent stronghold would disappear after allowing for an interaction between the treatment dummy and measures of incumbent quality. To test this, we further interact the treatment dummy with measures of incumbent's education levels and affiliations with national and provincial politicians. The results reported in Panel B

of Table 9 indicate that the point estimates on the interactions of treatment dummy and incumbent stronghold remain stable and highly significant.

We cannot explicitly test two additional alternative arguments. One is that dominant incumbents can simply afford to do more vote buying and, hence, we observe more vote buying in response to the flyer treatment. Second, dominant incumbents may have had better vote buying infrastructure and, therefore, were able to react more quickly to the flyer. Neither of these claims is supported by qualitative evidence. For example, the first requires that non-dominant incumbents be budget-constrained, but we encountered no indication of this, either in the field or the literature. Moreover, the qualitative evidence reviewed above suggests that all incumbents had extensive networks of political brokers. Furthermore, neither of these explanations can explain the stronger effects of the treatment on knowledge and incumbent-favoring errors.

7 Conclusion

A unique intervention provided voters with information about the existence and importance of a large public spending program, the types of services the program could finance, and candidate priorities and promises regarding the program just prior to the May 2013 municipal elections in the Philippines. The intervention led to significant changes in voter knowledge about incumbents and also led candidates to expend more resources on vote buying. We account for these results with a new model of vote buying and incumbent information advantage in an environment where candidates cannot make credible commitments. Information shocks that raise voters' thresholds for incumbent performance shortly before an election oblige incumbents to do more to increase voter welfare than they anticipated. With little time before the election to improve the provision of public goods, incumbents turned to vote buying.

Further tests support this explanation: a survey experiment in the Philippines demonstrates that merely informing individuals of the existence of the spending program reduces support for incumbents; vote buying is less likely to rise in response to the treatment in those barangays that report more recent public good provision; knowledge and vote buying effects are strongest in municipalities with dominant incumbents; and the correlation of vote buying with voter preference is

consistent with incumbent, and not challenger vote buying.

The results raise questions for future research. Our intervention took place shortly before the election, which we argue is the reason that it increased vote buying. Additional research is needed to assess an important corollary of this argument, that if the intervention had occurred earlier in the electoral cycle (or at least if incumbents had known earlier that the intervention would take place), it might have prompted incumbents to provide more public goods, with no change, or even a reduction, in vote buying. In addition, the information in the intervention related primarily to local infrastructure. A further open question is whether information about service delivery, such as the quality of health facilities or the effectiveness of schools, would have elicited similar responses with respect to voter knowledge and politician vote buying.

The findings also have implications for improving the accountability effects of elections in developing countries. They demonstrate that voters are poorly informed about what politicians can do for them and that relatively simple information interventions have a significant effect on this information asymmetry. Moreover, since the asymmetry reduces the incentives of incumbents to improve citizen welfare, such an intervention has potential welfare effects. Consistent with this, incumbents in our treatment area made significant attempts to increase voter welfare. Moreover, the theoretical framework suggests that increased transfers to voters should have come at candidate expense, not at the expense of lower vote buying in untreated areas. In our setting, where their time for reaction was short and only vote buying was feasible, they significantly increased vote buying in areas where voters were better informed.

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Table 1: Descriptive Statistics

	Treatment	Control
	(1)	(2)
Know promises	0.05 (0.50)	0.00 (0.47)
Know politicians	0.03 (0.54)	0.01 (0.57)
Salience sectors	2.46 (1.47)	2.30 (1.51)
Salience sectors (adjusted)	0.88 (1.19)	0.78 (1.21)
Error incumbent	4.76 (3.08)	4.97 (3.11)
Error challenger	2.06 (3.02)	2.08 (3.11)
Relative preference	12.60 (12.10)	12.53 (11.66)
Vote buying	0.16 (0.37)	0.14 (0.34)
Turnout (self-reported)	0.97 (0.18)	0.96 (0.18)
Incumbent vote share (self-reported)	0.63 (0.48)	0.65 (0.47)
Turnout (official)	0.82 (.07)	.82 (.06)
Incumbent vote share (official)	0.64 (0.19)	0.63 (0.20)

Notes: n= 3,408 (except for the official turnout and incumbent vote share data n=284). The standard deviations are in (parentheses) (Columns 1-2)

Table 2: Main treatment effects

Dependent Variable:	Control Group Mean (1)	Average Treatment Effect (2)	Obs. (3)
1. Knowledge of Promises	0.00 (0.47)	0.051*** (0.015)	3,408
2. Salience	2.30 (1.51)	0.161** (0.070)	3,346
3. Salience (adjusted)	0.78 (1.21)	0.109** (0.044)	3,346
4. Are you aware of vote buying in your village?	0.28 (0.45)	0.033 (0.023)	284
5. Did someone offer you money for your vote?	0.14 (0.35)	0.034** (0.016)	284
6. Incumbent-favoring errors	4.97 (3.11)	-0.211** (0.089)	3,408
7. Challenger-favoring errors	2.08 (3.11)	-0.025 (0.118)	3,408

Notes: Each cell in Column 1 contains the control group mean and standard deviation in (parenthesis). Each cell in Column 2 contains the coefficient on the treatment dummy variable (indicating whether the campaign was implemented in the village) from the corresponding OLS regression, and standard error in (parenthesis). Each regression includes pair fixed-effects. Dependent variables from the different regressions: row 1, an index capturing the respondent's knowledge of candidate promises; row 2, the rating given to "Whether candidates will spend the municipal budget on things that are important to me and my family" when the respondent was asked about 'voting influences'; row 3, same as row 2, but adjusted to account for the average rating given to the other categories; row 4, the share of respondent who indicated being aware of instances of vote buying in their village; row 5, the share of respondent who indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'missing']; row 6, the number of errors made by the respondent about candidate promises that were favoring the incumbent; row 7, the number of errors made by the respondent about candidate promises that were favoring the challenger. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 3: How likely would you be to support the mayor in the next elections?

	Control (1)	Treatment (2)
Very Unlikely	6.0	6.4
Unlikely	10.3	10.4
Neutral	24.0	30.6
Likely	36.0	30.0
Very Likely	23.7	22.6
Total	100	100
Observations	300	297

Notes: Data from the survey experiment.

Table 4: Survey Experiment: Exposure Only to Program Information Reduces Support for Incumbent

	Support Incumbent:		
	Yes	Neutral	No
	(1)	(2)	(3)
Panel A: Village fixed effects only			
Treatment	-0.070*	0.065*	0.005
	(0.038)	(0.036)	(0.030)
Observations	597	597	597
R-squared	0.185	0.102	0.087
Panel B: Village fixed effects and individual controls			
Treatment	-0.078**	0.067*	0.012
	(0.038)	(0.036)	(0.030)
Observations	597	597	597
R-squared	0.216	0.125	0.154

Notes: Results from individual-level regressions with village fixed-effects. In Column 1, the dependent variable is a dummy equal to one if the respondent indicated being either very likely or likely to support the incumbent during the upcoming elections. In Column 2, the dependent variable is a dummy equal to one if the respondent indicated being neutral in her support to the incumbent. In Column 3, the dependent variable is a dummy equal to one if the respondent indicated being either unlikely or very unlikely to support the incumbent during the upcoming elections. In Panel B, the regressions control for how long the respondent has lived in her current village of residence, family size, respondent's age, respondent's gender, respondent's education, whether the respondent receive remittances from abroad, whether the respondent benefit from a large-scale CCT program, whether the respondent asked for assistance from the mayor, whether the respondent asked for assistance from the barangay captain and whether the respondent voted in the 2013 municipal elections. Robust standard errors are (in parentheses). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 5: The mediating effects of public investment in barangays

Dep. Var.: Did Someone Offered you Money for your Vote?				
	(1)	(2)	(3)	(4)
Treatment	0.033** (0.015)	0.035** (0.015)		
Treatment * Nb projects (mayor)	-0.138* (0.075)	-0.135* (0.076)		
Treatment * Nb projects (mayor) above median			-0.000 (0.030)	0.002 (0.029)
Treatment * Nb projects (mayor) below median			0.064*** (0.023)	0.064*** (0.023)
Pair Fixed-Effects	Yes	Yes	Yes	Yes
Additional Controls	No	Yes	No	Yes
Observations	284	284	284	284
R-squared	0.738	0.740	0.735	0.736

Notes: Results from village-level regressions. The dependent variable is the share of respondent who indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'missing']. In Columns 1 and 2, regressions control for the number of projects financed by the incumbent mayor. In Columns 3 and 4, regressions control for a dummy of whether or not the number of projects financed by the mayor was above the median. In Columns 2 and 4, the regression includes the share of respondents with an household member who belongs to a group and the share of respondent who participated in any collective action activity in the village in the past six months. The standard errors are (in parentheses). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 6: Effects of Treatment on Self-reported Vote for the Incumbent

Sample:	Someone tried to buy your vote:			
	Yes		No	
	(1)	(2)	(3)	(4)
Treat	0.010 (0.051)	0.000 (0.054)	-0.041** (0.019)	-0.039** (0.019)
Pair Fixed-Effects	Yes	Yes	Yes	Yes
Additional Controls	No	Yes	No	Yes
Observations	426	426	2,456	2,455
R-squared	0.309	0.321	0.344	0.349

Notes: Results from individual-level regressions. The dependent variable is a dummy equal to one if the respondent indicated voting for the incumbent. In Columns 1 and 2 the sample includes all respondents who indicated that someone tried to buy their votes. In Columns 3 and 4 the sample includes all respondent who did not indicate that some tried to buy their votes. In Columns 2 and 4, the regression includes a dummy equal to one if someone in the household is a member of any group, a dummy equal to one if someone in the household participated in any collective action activity in the village in the past six months, alignment between the respondent and the incumbent, how long the respondent has lived in her current village of residence, family size, respondent's age, whether the respondent receive remittances from abroad and whether the respondent benefit from a large-scale CCT program. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 7: Dominant incumbents face more poorly informed voters and provide fewer public goods

	Promises (1)	Knowledge Politicians (2)	Candidates (3)	Error Incumbents (4)	Number Projects
Panel A: 2010 Incumbent Vote Share (Municipal)					
2010 Incumbent Vote Share	-0.004 (0.003)	-0.008** (0.003)	-0.009*** (0.003)	0.035 (0.027)	-0.002** (0.001)
Observations	3,408	3,187	3,408	3,408	284
R-squared	0.026	0.075	0.111	0.049	0.036
Panel B: 2010 Incumbent Vote Share (Village)					
2010 Incumbent Vote Share	-0.003*** (0.001)	-0.007*** (0.001)	-0.007*** (0.001)	0.026*** (0.006)	-0.002*** (0.001)
Observations	3,408	3,187	3,408	3,408	284
R-squared	0.022	0.069	0.096	0.036	0.027

Notes: Results from individual-level regressions (Columns 1-3) and village-level regressions (Column 4). In Column 1, the dependent variable is an index capturing the respondent's knowledge of candidate promises. In Column 2, the dependent variable is an index capturing the respondent's knowledge of politicians in their village, municipality and province. In Column 3, the dependent variable is an index capturing the respondent's knowledge of mayoral candidates' political experience and education levels. In Column 4, the dependent variable is the number of projects implemented by the incumbent in the village between 2010 and 2013. All regressions control for the treatment dummy. The standard errors (in parentheses) account for potential correlation within municipality (Columns 1-3). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 8: Treatment Effects Vary with Incumbent Dominance

	Know Promises (1)	Incumbent (2)	Errors Challenger (3)	Salience (4)	Vote Buying (5)	Turnout Self-Reported (6)	Official (7)	Inc. Vote Share Self-Reported (8)	Official (9)
Panel A: 2010 Incumbent Vote Share (Municipal)									
Treat	0.051*** (0.015)	-0.211** (0.088)	-0.025 (0.118)	0.109** (0.044)	0.034** (0.015)	0.001 (0.004)	-0.000 (0.006)	-0.022 (0.018)	0.007 (0.014)
Interaction	0.007*** (0.001)	-0.024*** (0.008)	0.002 (0.012)	0.006 (0.004)	0.004*** (0.001)	-0.001 (0.000)	-0.000 (0.001)	-0.001 (0.002)	-0.001 (0.001)
Observations	3,408	3,408	3,408	3,346	284	3,408	284	3,408	284
R-squared	0.330	0.528	0.253	0.090	0.742	0.058	0.738	0.283	0.838
Panel B: 2010 Incumbent Vote Share (Barangay)									
Treat	0.051*** (0.015)	-0.212** (0.090)	-0.039 (0.117)	0.104** (0.043)	0.035** (0.015)	0.001 (0.004)	-0.001 (0.005)	-0.022 (0.018)	0.005 (0.013)
Interaction	0.006*** (0.001)	-0.019*** (0.006)	0.000 (0.009)	0.009** (0.004)	0.003** (0.001)	-0.001** (0.000)	0.000 (0.000)	0.000 (0.001)	0.001 (0.001)
Observations	3,408	3,408	3,408	3,346	284	3,408	284	3,408	284
R-squared	0.330	0.528	0.255	0.093	0.741	0.060	0.756	0.282	0.843

Notes: Results from individual-level (Columns 1-4, 6 and 8) and village-level (Columns 5, 7 and 9) regressions. All regression include pair dummies. §Interaction¶ is (treatment x incumbent vote share in 2010 elections). Dependent variables are: Column 1, an index capturing the respondent's knowledge of candidate promises; Column 2, number of errors made by the respondent about candidate promises that were favoring the incumbent; Column 3, the number of errors made by the respondent about candidate promises that were favoring the challenger; Column 4, rating given to "Whether candidates will spend the municipal budget on things that are important to me and my family" when the respondent was asked about 'voting influences', adjusted to account for the average rating given to the other categories; Column 5, the share of respondents who indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'missing']; Column 6, a dummy equal to one of the respondent indicated voting in the 2013 elections; Column 7, official village-level turnout in the 2013 elections; Column 8, a dummy equal to one if the respondent indicated voting for the incumbent in the 2013 elections; Column 9, official incumbent vote share at the village-level in the 2013 elections. The standard errors (in parentheses) account for potential correlation within village (Columns 1-4). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 9: Alternative Channels

	Know Promises (1)	Errors Incumbent (2)	Challenger (3)	Salience (4)	Vote Buying (5)
Panel A: Controlling for Overlap between Candidate Promises					
Treat	0.050*** (0.015)	-0.209** (0.088)	-0.025 (0.118)	0.109** (0.044)	0.034** (0.015)
Treat*Strongholds	0.007*** (0.001)	-0.024*** (0.007)	0.003 (0.012)	0.006 (0.004)	0.004*** (0.001)
Treat*Overlap Promises	0.000 (0.002)	-0.001 (0.013)	-0.012 (0.014)	-0.004 (0.005)	-0.001 (0.002)
Observations	3,408	3,408	3,408	3,346	284
R-squared	0.330	0.528	0.253	0.090	0.743
Panel B: Controlling for Incumbent Quality					
Treat	0.050*** (0.015)	-0.210** (0.088)	-0.023 (0.117)	0.109** (0.044)	0.034** (0.015)
Treat*Strongholds	0.007*** (0.002)	-0.028*** (0.008)	0.002 (0.013)	0.006 (0.005)	0.003** (0.002)
Treat* College	-0.003 (0.040)	-0.196 (0.193)	-0.171 (0.296)	-0.002 (0.112)	-0.013 (0.044)
Treat* Affiliated	0.021* (0.013)	-0.138* (0.082)	0.217** (0.096)	0.031 (0.040)	-0.010 (0.014)
Observations	3,408	3,408	3,408	3,346	284
R-squared	0.331	0.529	0.255	0.090	0.743

Notes: Results from individual-level regressions. All regression include pair dummies. In Panel A, regressions also control for the interaction between the treatment dummy and overlap between candidate promises. In Panel B, regressions also control for the interactions between the treatment dummy and incumbent education and affiliations with national and provincial politicians. College is a dummy equal to one if the incumbent graduated from College and Affiliated is the sum of three dummies each capturing whether the incumbent is from the same party as the congressman (or the governor or the president). Those variables are included in the regressions but not reported. Dependent variables: Column 1, an index capturing the respondent's knowledge of candidate promises; Column 2, the number of errors made by the respondent about candidate promises that were favoring the incumbent; Column 3, the number of errors made by the respondent about candidate promises that were favoring the challenger; Column 4, the rating given to "Whether candidates will spend the municipal budget on things that are important to me and my family" when the respondent was asked about 'voting influences', adjusted to account for the average rating given to the other categories; Column 5, the share of respondents who indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'missing']. The standard errors (in parentheses) account for potential correlation within village (Columns 1-4). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Appendix for Online Publication

A.1 Technical Appendix

In this technical appendix we report the proofs of the Lemma and Propositions discussed in Section 5.

Proof of Proposition 1

The proof follows from the canonical model in Persson and Tabellini (2000). Recall that the information shock is unanticipated. Hence, voters coordinate on the pre-electoral performance threshold according to their expected individual beliefs about the costs of providing public goods, drawn from $\theta_i \sim [1, 2\theta_c - 1]$: for all voters, the expected cost of providing public goods is given by θ_c . Voters would most prefer to set the performance threshold to require that public goods be provided at the Samuelsonian optimum or, given their expected beliefs, at $H_g(g_{\theta_c}) = \frac{\theta_c}{N}$. The performance threshold of the median voter would then be $\bar{\omega} = H(g_{\theta_c})$, where $H_g(g_{\theta_c}) = \frac{\theta_c}{N}$. However, voters anticipate that incumbents can marginally reduce public goods, saving incumbents θ_c in expectation, thereby reducing the utility of each voter by $\frac{\theta_c}{N}$. Incumbents can then offset this welfare loss for $\frac{N}{2}$ voters by offering transfers to them that total $\frac{N}{2} \frac{\theta_c}{N} = \frac{\theta_c}{2} < \theta_c$. This tradeoff continues to be feasible for the incumbent, in expectation, until public good provision falls to $H_g(g^*) = \frac{2\theta_c}{N}$ and the cost of using transfers to offset the welfare losses from additional marginal reductions in public good provision exactly equals the reduced cost of providing public goods, $\frac{N}{2} \frac{2\theta_c}{N} = \theta_c$. The provision of g^* is feasible as long as it is less than g_{max} , defined by the incumbent's participation constraint, including the actual costs of providing public goods, $M - \bar{\theta}g_{max} + R \geq M$. The performance threshold sets transfers to zero since, as in Persson and Tabellini (2000), voters anticipate that competition between voters to be part of the majority that receives these transfers drives actual redistributive transfers to zero. ■

Proof of Lemma 1

Based on the performance threshold $\bar{\omega} = H(g_{\theta_c})$ incumbents provided g_{θ_c} . In the event of an unanticipated shock, a fraction δ of voters have beliefs distributed

according to $\theta'_i \sim [1, 2\theta'_c - 1]$, where $\theta'_c = \theta_c + k(\bar{\theta} - \theta_c)$, and the remaining $(1 - \delta)$ voters have beliefs distributed as before, $\theta_i \sim [1, 2\theta_c - 1]$. Therefore, one-half of the voters who were not subjected to the information shock, given by $\frac{1}{2}(1 - \delta) < \frac{1}{2}$, are expected to conclude that the incumbent met their performance threshold, as before.

Case 1: The unanticipated shock is positive ($k(\bar{\theta} - \theta_c) > 0$). The shock shifts up the median of the distribution of beliefs about the costs of providing public goods among a fraction δ of voters. Consequently, among the δ fraction of voters exposed to the shock, the incumbent's performance will, in expectation, meet the threshold for some voters for whom it previously did not. Recalling that their beliefs are now distributed according to $\theta'_i \sim [1, 2(\theta_c + k(\bar{\theta} - \theta_c)) - 1]$, the fraction of voters in δ for whom the incumbent's performance is expected to be sufficient, but previously was not, is given by $\left(\frac{\theta'_c - \theta_c}{2(\theta_c + k(\bar{\theta} - \theta_c)) - 2}\right) = \frac{1}{2} \left(\frac{k(\bar{\theta} - \theta_c)}{\theta_c + k(\bar{\theta} - \theta_c) - 1}\right) > 0$. The total fraction of voters in δ for whom the incumbent's performance is expected to be sufficient is therefore $\frac{1}{2} \left(1 + \frac{k(\bar{\theta} - \theta_c)}{\theta_c + k(\bar{\theta} - \theta_c) - 1}\right) > \frac{1}{2}$. Incumbents have the support of one-half of the voters who were not exposed to the shock and more than one-half of the voters who were, and are therefore re-elected with no additional effort. However, they provided more public goods than they needed to in order to secure the support of $N/2$ voters.

Case 2: The unanticipated shock is negative ($k(\bar{\theta} - \theta_c) < 0$). When the unanticipated shock reduces the beliefs of a fraction δ of voters regarding incumbent costs, these voters expect higher performance, on average, than the incumbent anticipated they would. Some of these voters would have believed that the incumbent met the performance threshold in the absence of the shock, $\bar{\omega}_i \leq H(g_{\theta_c})$, and now do not believe this, $\bar{\omega}_i > H(g_{\theta_c + k})$. Now, the fraction of the voters exposed to the information shock who are satisfied by the incumbent's performance is given by $\frac{1}{2} \left(1 + \frac{k(\bar{\theta} - \theta_c)}{\theta_c + k(\bar{\theta} - \theta_c) - 1}\right) < \frac{1}{2}$. Fewer than one-half of the voters subjected to the information shock, and therefore fewer than one-half of all voters, are satisfied by incumbent performance. However, these incumbents can still be re-elected if they use transfers to increase voter welfare. ■

Proof of Proposition 2

Recall from Lemma 1, Case 2, that public good provision meets the performance threshold of fewer than half of the voters in δ . Incumbents cannot increase public good provision to recapture the support of $\frac{N}{2}$ voters, but they can use transfers. It follows immediately that the transfers must be sufficient to meet the condition that $f_k = H(g_{\theta_c+k}) - H(g_{\theta_c})$: they must be enough to bring voters' evaluation of incumbent performance up to the performance threshold for enough voters such that the incumbent has the support of $\frac{N}{2}$ voters. Note that inter-voter competition for transfers does not drive transfers to zero because incumbents have no incentive to initiate it. Voters have already coordinated on a voting rule. Consequently, individual voters cannot credibly commit their vote to the incumbent if they receive transfers that are lower than needed to bring the incumbent's performance up to the threshold that is consistent with the voting rule.

If incumbents could, they would target these transfers to the most persuadable voters, those for whom transfers $f_k = H(g_{\theta_c+k}) - H(g_{\theta_c})$ are just sufficient to shift their support to the incumbent. However, incumbents know only the distribution of voter beliefs and not the beliefs of each voter. Hence, they have to make transfers to voters without knowing whether those voters already support them, even without transfers, or whether those voters will not support them, even with transfers. We first show, therefore, that incumbents prefer to target voters in δ with transfers rather than other voters. We then establish the fraction of voters in δ whom they target.

Recalling that $k(\bar{\theta} - \theta_c)$ is less than zero, $-\frac{1}{2} \left(\frac{k(\bar{\theta} - \theta_c)}{\theta_c + k(\bar{\theta} - \theta_c) - 1} \right)$ is the fraction of voters in the group δ that received the information shock and would be "persuaded" by a transfer f_k . Other voters in δ either already support the incumbent or are sufficiently hostile to the incumbent that they would not be persuaded by the transfer. The fraction of voters in the group not exposed to the shock and that would be equally persuadable by the transfer f_k is given by $-\frac{1}{2} \left(\frac{k(\bar{\theta} - \theta_c)}{\theta_c - 1} \right)$. Since $(\theta_c - 1) > (\theta_c + k(\bar{\theta} - \theta_c) - 1)$ for $k(\bar{\theta} - \theta_c) < 0$, the probability that a transfer will reach a persuadable voter is greater if it is targeted to voters in the group δ .³⁷

³⁷The intuition is straightforward. Incumbents would like to target transfers to voters for whom the distribution of voter beliefs is most dense around the median: these are the most persuadable voters. An information shock that tells voters that the maximum costs of providing public goods

Incumbents cannot identify these voters, however, since they know only the distribution of preferences. Incumbents' probability of re-election ρ is therefore determined by the fraction α of the voters in δ to whom they provide the transfer $f_k = H(g_{\theta_c+k}) - H(g_{\theta_c})$. The probability equals zero for $\alpha < \frac{1}{2} \left(\frac{k(\bar{\theta}-\theta_c)}{\theta_c+k(\bar{\theta}-\theta_c)-1} \right)$ - if they provide transfers to fewer voters than those whose support they lost because of the information shock, they cannot be re-elected, so they would prefer to provide zero and forego re-election. The probability of re-election goes to one as all members of δ receive the transfer, or as α goes to one. Hence, incumbents if they choose to seek re-election, incumbents will choose α from $\left[\frac{1}{2} \left(\frac{k(\bar{\theta}-\theta_c)}{\theta_c+k(\bar{\theta}-\theta_c)-1} \right), 1 \right]$. Set $l = \frac{1}{2} \left(\frac{k(\bar{\theta}-\theta_c)}{\theta_c+k(\bar{\theta}-\theta_c)-1} \right)$. Since the distribution of voter beliefs about costs is uniform, the incumbent's probability of re-election is therefore $\frac{\alpha-l}{1-l}$, for $\alpha \in \left[\frac{1}{2} \left(\frac{k(\bar{\theta}-\theta_c)}{\theta_c+k(\bar{\theta}-\theta_c)-1} \right), 1 \right]$. The incumbent chooses α to maximize expected rents, $\frac{\alpha-l}{1-l} [M - \bar{\theta}g_{\theta_c} - \alpha\delta f_k + R]$, subject to non-pecuniary rents from seeking office continuing to be sufficiently large that the incumbent still prefers to seek re-election, $M - \bar{\theta}g_{\theta_i} - \alpha\delta f_k + R \geq M - \bar{\theta}g_{\theta_i}$. Assuming the participation constraint does not bind, the incumbent maximizes rents choosing $\alpha^* = \frac{M - \bar{\theta}g_{\theta_c} + R + l\delta f_k}{\delta f_k}$. ■

are lower than they thought reduces the upper limit, but has no effect on the lower limit, of the distribution of voter beliefs regarding the costs of producing public goods. Hence, the shock increases the density of the uniform distribution at every point, including the median, making treated voters more attractive targets for vote buying than untreated voters. This effect is not unique to a uniform distribution, but occurs for any distribution for which the information shock increases the density of voters at the median.

A.2 Background on the Experiment

Table A.1: Timeline

Date	Activity
April 17-29	Candidates Interview
April	Randomization
May 5	Flyer printing
May 7-10	Flyer distribution
May 13	Elections
June	Household survey

Table A.2: List of Intervention Municipalities

Province	Municipality	# Pairs	# Candidates
ILOCOS NORTE	BANGUI	7	2
	BANNA (ESPIRITU)	10	3
	DINGRAS	15	3
	PAOAY	15	2
	PASUQUIN	15	3
	PINILI	10	2
	SAN NICOLAS	11	2
ILOCOS SUR	BURGOS	11	2
	LIDLIDDA	5	3
	MAGSINGAL	13	2
	SAN JUAN (LAPOG)	13	2
	SANTA LUCIA	17	2

Notes: The list differs slightly from the one included in the Pre-Analysis Plan as volunteers could not distribute the flyers in Banayoyo (Ilocos Sur), Pagudpud (Ilocos Norte) and Tagudin (Ilocos Sur). In addition, we had to drop one pair in Pasuquin (Ilocos Norte) as we found out during the endline survey that the control village in that pair was a military camp.

Ania ti pakaidiligan dagitoy a karkari?

Ti Parish Pastoral Council for Responsible Voting ket nangigannuat ti panagsokisok babaen Iti panangummong da kadagiti nadumaduma a karkari ken plano daguti paidasig a mayor Iti nadumaduma nga ili ti probinsiya -Ilocos Norte ken Ilocos Sur.

Kalpasan ti eleksyon, ti PPCRV ti mangkita nu kasanu Iti pannakaipatungpal dagitoy a karkari ken plano.

Launen daytoy a "FLYER" wenno papel dagitoy nasao nga karkari ken plano daguti kandidato.



Ammoyo kadi nga...

....ti mayor ti ili ket isu ti kangrunaan nga mangited Iti desisyon maipapan ti pannakausar Iti "LOCAL DEVELOPMENT FUNDS" wenno pondo ti munisipyo kadagiti nadumaduma a sector Iti ili.

.....dagitoy ti Inda indatag.....

Siasinno ti PPCRV?

Ti PPCRV wenno Parish Pastoral Council for Responsible Voting nga naibuangay idi 1991, ket maysa a gunglo ti Simbaan Katolika nga mangidadaulo Iti pannakaipatungpal Iti nadalus ken natalna nga eleksyon.



**PARISH PASTORAL COUNCIL
for RESPONSIBLE VOTING**

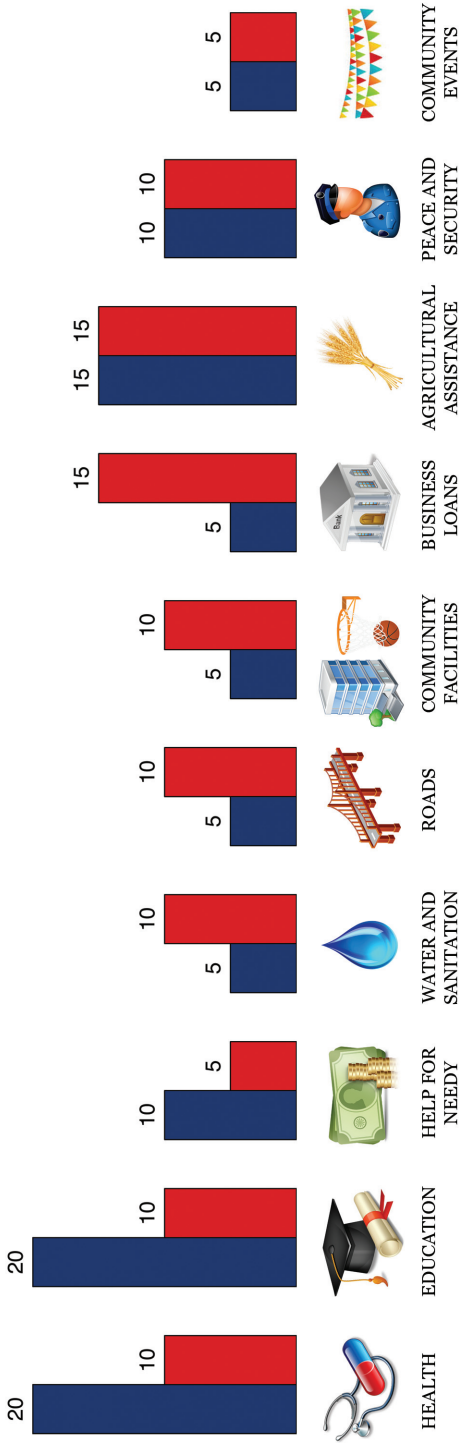
Figure A.1: Cover for the Flyer

SAN NICOLAS

Ammoyo Kadi...

1. HERNANDO, ANGEL MIGUEL

2. VALDEZ, MELANIE



Anyaya pay ti karkarida?

Angel Miguel Hernandez (LP)

- Panangited iti libre nga panagadal iti elementarya ken high school
- Panangi implementa ti Health programs nga mangited iti libre nga agas ken Philhealth; panangpasayaat dagiti health stations kadagiti barangay
- Panangpasardeng iti panangbayad iti buwis dagiti addaan iti babassit nga pasdek negosyo (sari-sari stores)

Melanie Valdez (IND.)

- Panangbuangay kadagiti community based projects
- Panangiyusuat ti organiko a panagtalon tapnun mapagkaysa ti agrikultura ken iti aglawlaw ken tapnu mapaadu ti apit; panangaramid dagitay makunkuna nga solid wastes nga agbalin nga abuno
- Panangipatuloy ti panangited iti libre a panagadal ti High School ken College babaen ti scholarships ti Binhi Foundation

Figure A.2: Flyer for the Municipality of San Nicolas, Ilocos Norte



Figure A.3: Cover for the Flyer (Survey Experiment)

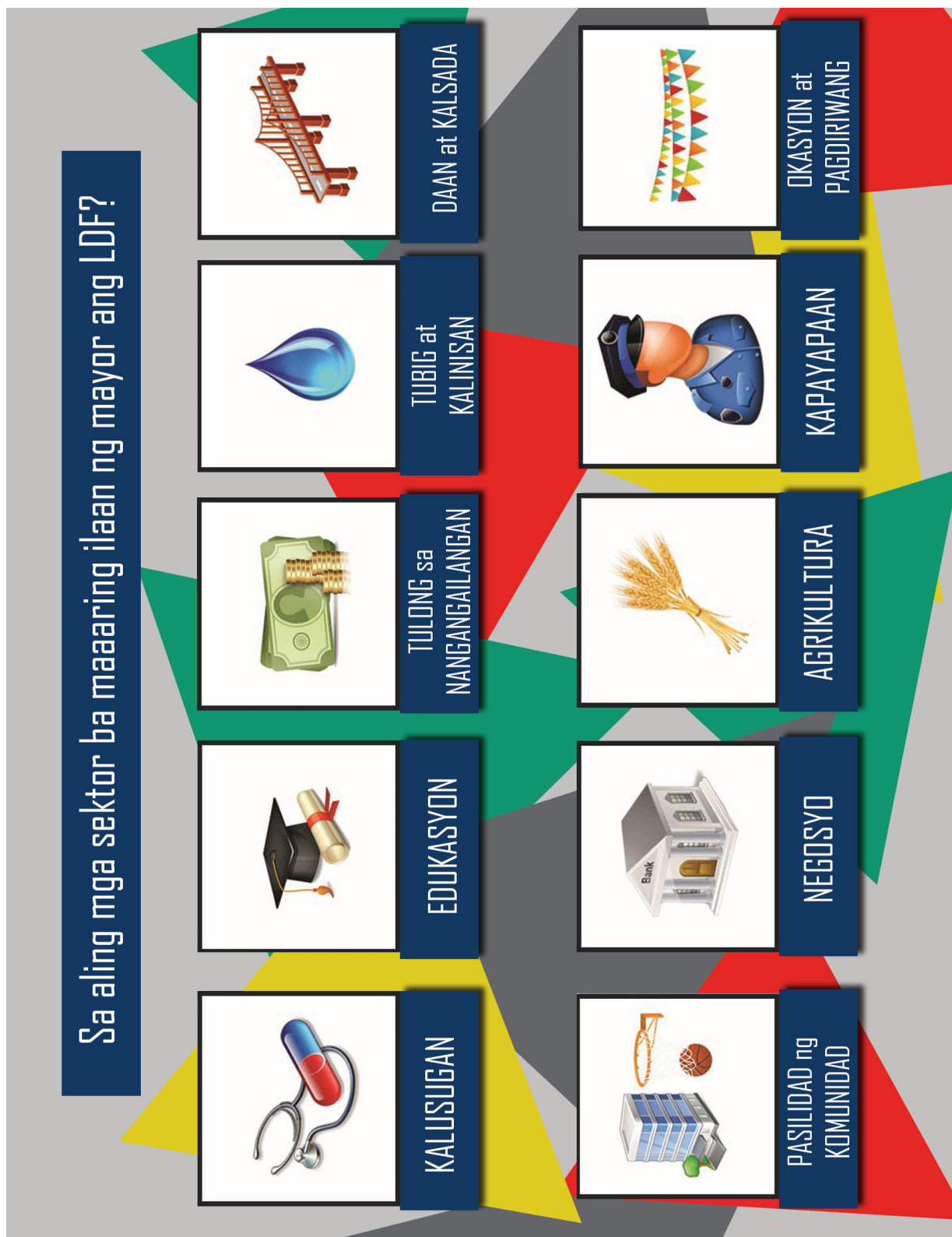


Figure A.4: Inside of the flyer (Survey Experiment)

A.3 Additional Results

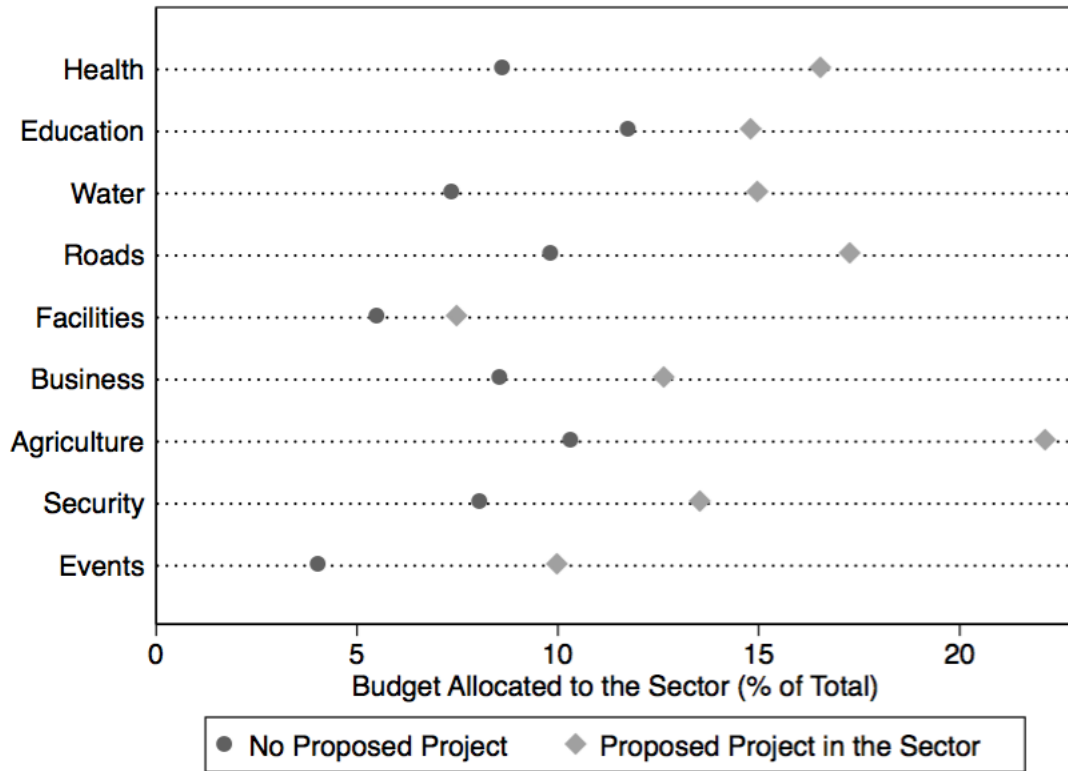


Figure A.5: Candidate Proposed Projects and Budget Allocations

Note: Candidates were asked to propose budgetary allocations for each sector. Separately, they were given the opportunity to indicate three specific projects that they would implement if elected. For each of the sectors listed, the figure compares the budgetary allocations of candidates who indicated specific projects in that sector with the budgetary allocations of incumbents who did not.

Table A.3: Balance Tests (Main Experiment)

	Treatment (1)	Control (2)	T-test (3)	K-Smirnov test (4)	OLS (5)
# Precincts	1.09 (0.29)	1.10 (0.36)	0.18 [0.86]	0.02 [1.00]	-0.01 [0.83]
Registered Voters	526.26 (306.78)	544.94 (342.68)	0.48 [0.63]	0.07 [0.84]	-18.68 [0.53]
Population	842.20 (492.93)	895.92 (598.28)	0.83 [0.41]	0.06 [0.97]	-53.73 [0.31]
Turnout 2010	0.78 (0.08)	0.78 (0.08)	0.01 [0.99]	0.06 [0.97]	0.00 [0.98]
2010 Incumbent vote share	0.73 (0.21)	0.72 (0.23)	-0.31 [0.75]	0.06 [0.92]	0.01 [0.52]
2010 Incumbent vote share [adj.]	0.56 (0.13)	0.55 (0.15)	-0.45 [0.65]	0.06 [0.97]	0.01 [0.53]
Rural	0.88 (0.33)	0.87 (0.33)	-0.18 [0.86]	0.01 [1.00]	0.01 [0.84]

Notes: n=284. The standard deviations are in (parentheses) (Columns 1-2). In Columns 3-4, the test statistics are reported along with the p-values [bracket]. Each cell in Column 5 is either the coefficient on the dummy variable indicating whether the campaign was implemented in the village from a different OLS regression with pair fixed-effects or the associated p-value in [bracket].

Table A.4: Balance Tests : Alignment of Preferences and Promises (Main Experiment)

	Treatment (1)	Control (2)	T-test (3)	K-Smirnov test (4)	OLS (5)
Alignment	58.40 (19.42)	58.27 (19.67)	-0.28 [0.78]	0.01 [0.98]	0.13 [0.76]
Alignment (challenger)	57.17 (18.56)	57.06 (18.56)	-0.18 [0.86]	0.01 [0.98]	0.10 [0.85]
Alignment (incumbent)	59.88 (20.31)	59.73 (20.86)	-0.22 [0.83]	0.03 [0.37]	0.16 [0.81]

Notes: 7,896. The standard deviations are in (parentheses) (Columns 1-2). In Columns 3-4, the test statistics are reported along with the p-values [bracket]. Each cell in Column 5 is either the coefficient on the dummy variable indicating whether the campaign was implemented in the village from a different OLS regression with pair fixed-effects or the associated p-value in [bracket].

Table A.5: Balance Tests : Respondent Characteristics (Main Experiment)

	Treatment	Control	T-test	K-Smirnov test	OLS
	(1)	(2)	(3)	(4)	(5)
Length of residence	40.69 (19.07)	40.39 (19.03)	-0.46 [0.64]	0.02 [0.94]	0.30 [0.64]
Family size	5.02 (2.22)	5.10 (2.05)	1.01 [0.31]	0.03 [0.32]	-0.07 [0.30]
Female	0.49 (0.50)	0.49 (0.50)	0.14 [0.89]	0.00 [1.00]	0.00 [0.89]
Age	49.36 (13.54)	48.85 (13.37)	-1.10 [0.27]	0.03 [0.26]	0.50 [0.27]
Education (years)	9.46 (3.47)	9.35 (3.42)	-0.93 [0.35]	0.04 [0.13]	0.11 [0.34]
Remittances abroad	0.22 (0.42)	0.24 (0.43)	1.33 [0.18]	0.02 [0.90]	-0.02 [0.17]
CCT Beneficiary	0.15 (0.36)	0.16 (0.37)	0.38 [0.71]	0.00 [1.00]	0.00 [0.70]
Group Member	0.67 (0.47)	0.64 (0.48)	-1.70 [0.09]	0.03 [0.52]	0.03 [0.08]
Barangay assembly	0.93 (0.26)	0.94 (0.24)	1.21 [0.22]	0.01 [1.00]	-0.01 [0.21]
Collective Action	0.74 (0.44)	0.77 (0.42)	2.10 [0.04]	0.03 [0.37]	-0.03 [0.03]
Religion: never	0.09 (0.28)	0.09 (0.28)	0.18 [0.85]	0.00 [1.00]	0.00 [0.85]
Religion: weekly	0.37 (0.48)	0.36 (0.48)	-0.57 [0.57]	0.01 [1.00]	0.01 [0.56]

Notes: n=3,408. The standard deviations are in (parentheses) (Columns 1-2). In Columns 3-4, the test statistics are reported along with the p-values [bracket]. Each cell in Column 5 is either the coefficient on the dummy variable indicating whether the campaign was implemented in the village from a different OLS regression with pair fixed-effects or the associated p-value in [bracket].

Table A.6: Balance Tests : Respondent Characteristics (Main Experiment)

	Treatment (1)	Control (2)	T-test (3)	K-Smirnov test (4)	OLS (5)
Electricity	0.97 (0.18)	0.97 (0.16)	0.91 [0.36]	0.01 [1.00]	-0.01 [0.36]
Radio	0.73 (0.44)	0.74 (0.44)	0.51 [0.61]	0.01 [1.00]	-0.01 [0.61]
Television	0.88 (0.33)	0.88 (0.33)	-0.10 [0.92]	0.00 [1.00]	0.00 [0.92]
Phone	0.89 (0.31)	0.90 (0.29)	1.41 [0.16]	0.01 [0.99]	-0.01 [0.15]
Washing Machine	0.33 (0.47)	0.35 (0.48)	1.27 [0.20]	0.02 [0.86]	-0.02 [0.20]
Fridge	0.53 (0.50)	0.54 (0.50)	0.76 [0.45]	0.01 [1.00]	-0.01 [0.44]
Gas stove	0.60 (0.49)	0.61 (0.49)	0.63 [0.53]	0.01 [1.00]	-0.01 [0.51]
bicycle	0.41 (0.49)	0.39 (0.49)	-0.94 [0.34]	0.02 [0.98]	0.02 [0.34]
Boat	0.02 (0.15)	0.03 (0.16)	0.55 [0.58]	0.00 [1.00]	0.00 [0.56]
Motorcycle	0.54 (0.50)	0.55 (0.50)	0.83 [0.41]	0.01 [1.00]	-0.01 [0.40]

Joint test of significance of the 22 variables reported in Tables A.5 and A.6:

$$\chi^2 = 12.99$$

$$p\text{-value} = 0.93$$

Notes: n=3,408. The standard deviations are in (parentheses) (Columns 1-2). In Columns 3-4, the test statistics are reported along with the p-values [bracket]. Each cell in Column 5 is either the coefficient on the dummy variable indicating whether the campaign was implemented in the village from a different OLS regression with pair fixed-effects or the associated p-value in [bracket].

Table A.7: Balance Tests : Respondent Characteristics (Survey Experiment)

	Treatment (1)	Control (2)	T-test (3)	K-Smirnov test (4)	OLS (5)
Length of residence	31.72 (18.66)	31.91 (17.23)	0.13 [0.90]	0.07 [0.48]	-0.25 [0.86]
Female	0.60 (0.49)	0.65 (0.48)	1.35 [0.18]	0.05 [0.76]	-0.05 [0.18]
Age	43.73 (14.67)	43.71 (14.93)	-0.02 [0.99]	0.03 [0.99]	-0.04 [0.98]
Education levels:					
Some primary	0.12 (0.33)	0.11 (0.31)	-0.51 [0.61]	0.01 [1.00]	0.01 [0.64]
Primary graduate	0.13 (0.33)	0.17 (0.38)	1.60 [0.11]	0.05 [0.88]	-0.05 [0.12]
Some high school	0.20 (0.40)	0.15 (0.35)	-1.62 [0.10]	0.05 [0.82]	0.05 [0.10]
High school graduate	0.32 (0.47)	0.29 (0.45)	-0.80 [0.43]	0.03 [1.00]	0.03 [0.40]
Vocational training	0.05 (0.23)	0.07 (0.26)	1.00 [0.32]	0.02 [1.00]	-0.02 [0.32]
College +	0.14 (0.35)	0.18 (0.38)	1.11 [0.27]	0.03 [0.99]	-0.03 [0.24]
Household size	5.33 (2.39)	5.33 (2.21)	0.00 [1.00]	0.02 [1.00]	0.00 [0.99]
Remittances abroad	0.19 (0.39)	0.20 (0.40)	0.31 [0.76]	0.01 [1.00]	-0.01 [0.76]
CCT Beneficiary	0.16 (0.37)	0.19 (0.39)	0.75 [0.45]	0.02 [1.00]	-0.02 [0.43]
Ask assistance from:					
Mayor	0.24 (0.43)	0.25 (0.45)	0.28 [0.78]	0.01 [1.00]	-0.01 [0.76]
Barangay captain	0.26 (0.44)	0.22 (0.41)	-1.24 [0.22]	0.04 [0.93]	0.04 [0.21]
Turnout (2013)	0.98 (0.15)	0.95 (0.21)	-1.56 [0.12]	0.02 [1.00]	0.02 [0.12]

Joint test of significance of the variables reported this Table:

$$\chi^2 = 16.00$$

$$p\text{-value} = 0.31$$

Notes: n=600. The standard deviations are in (parentheses) (Columns 1-2). In Columns 3-4, the test statistics are reported along with the p-values [bracket]. Each cell in Column 5 is either the coefficient on the dummy variable indicating whether the respondent received the flyer during the interview or the associated p-value in [bracket].

Table A.8: Correlates of Sectoral Preferences

	(1) Health	(2) Education	(3) Emergencies	(4) Water	(5) Road	(6) ComFaci	(7) EconProg	(8) Agriculture	(9) Peace	(10) Festivals
Educ. (years)	-0.098 (0.072)	0.358*** (0.069)	-0.182*** (0.050)	0.061 (0.039)	0.129*** (0.046)	0.059* (0.032)	-0.113** (0.045)	-0.298*** (0.088)	0.130*** (0.035)	-0.045* (0.024)
Children < 6	0.011 (0.348)	0.856*** (0.298)	-0.080 (0.207)	-0.024 (0.169)	-0.098 (0.223)	-0.049 (0.130)	-0.491*** (0.159)	0.203 (0.326)	-0.119 (0.129)	-0.209** (0.100)
Children 7-14	-0.544** (0.241)	1.126*** (0.230)	-0.269* (0.160)	0.008 (0.149)	-0.336** (0.153)	-0.101 (0.112)	0.133 (0.238)	0.235 (0.338)	-0.267** (0.109)	0.016 (0.110)
Farmer	-2.132*** (0.633)	-1.593*** (0.540)	-1.515*** (0.405)	-0.140 (0.364)	0.947*** (0.349)	-1.109*** (0.284)	-1.783*** (0.413)	8.514*** (0.667)	-0.589** (0.288)	-0.601*** (0.214)
Business	-0.502 (0.588)	0.729 (0.576)	-0.741* (0.390)	0.162 (0.344)	0.598* (0.352)	0.308 (0.283)	0.795* (0.458)	-2.209*** (0.692)	0.332 (0.265)	0.527*** (0.187)
Female	0.845* (0.468)	0.510 (0.439)	2.214*** (0.367)	-0.021 (0.327)	-1.415*** (0.310)	-0.529** (0.223)	0.785** (0.359)	-2.306*** (0.618)	-0.060 (0.244)	-0.022 (0.179)
Treatment	0.069 (0.537)	-0.654 (0.513)	-0.145 (0.417)	-0.694** (0.352)	0.786* (0.424)	0.112 (0.274)	0.077 (0.369)	0.063 (0.786)	0.199 (0.272)	0.187 (0.198)
Observations	3,404	3,404	3,404	3,404	3,404	3,404	3,404	3,404	3,404	3,404
R-squared	0.018	0.035	0.035	0.013	0.032	0.019	0.025	0.100	0.038	0.012

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Notes: Results from individual-level regressions. The dependent variables are the share of the LDF that the respondent would like to allocate to Health (Column 1), Education (Column 2), Emergencies (Column 3), Water (Column 4), Roads (Column 5), Community Facilities (Column 6), Economic Programs (Column 7), Agriculture (Column 8), Peace and Order (Column 9) and Festivals (Column 10). The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.9: Effects of Treatment on Knowledge and Salience

	(1)	(2)	(3)	(4)
Panel A: Knowledge of Promises				
Treat	0.051 (0.036)	0.051** (0.022)	0.051*** (0.015)	0.051*** (0.015)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	3,408	3,408	3,408	3,408
R-squared	0.003	0.245	0.326	0.326
Panel B: Salience				
Treat	0.159* (0.096)	0.158* (0.094)	0.161** (0.070)	0.170** (0.069)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	3,346	3,346	3,346	3,346
R-squared	0.003	0.014	0.146	0.149
Panel C: Salience (adjusted)				
Treat	0.107* (0.060)	0.107* (0.058)	0.109** (0.044)	0.113** (0.044)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	3,346	3,346	3,346	3,346
R-squared	0.002	0.013	0.089	0.091

Notes: Results from individual-level regressions. In Panel A, the dependent variable is an index capturing the respondent's knowledge of candidate promises. In Panel B, the dependent variable is rating given to "Whether candidates will spend the municipal budget on things that are important to me and my family" when the respondent was asked about 'voting influences'. In Panel C, the variable is adjusted to account for the average rating given to the other categories. In Column 4, the regression includes a dummy equal to one if someone in the household is a member of any group and a dummy equal to one if someone in the household participated in any collective action activity in the village in the past six months. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.10: Effects of Treatment on Vote Buying

	(1)	(2)	(3)	(4)
Panel A: Are you Aware of Instances of Vote Buying in your Village?				
Treat	0.033 (0.029)	0.033 (0.023)	0.033 (0.023)	0.032 (0.023)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	284	284	284	284
R-squared	0.005	0.370	0.688	0.690
Panel B: Did Someone Offered you Money for your Vote?				
Treat	0.034 (0.021)	0.034** (0.016)	0.034** (0.016)	0.035** (0.016)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	284	284	284	284
R-squared	0.009	0.478	0.730	0.731

Notes: Results from village-level regressions. In Panel A, the dependent variable is the share of respondent who indicated being aware of instances of vote buying in their village. In Panel B, the dependent variable is the share of respondent who indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'missing']. In Column 4, the regression includes the share of respondents with an household member who belongs to a group, the share of respondent who participated in any collective action activity in the village in the past six months, the village-average share of the local budget that respondents would like to spend on water and the village-average share of the local budget the respondent would like to spend on roads. The standard errors are (in parentheses). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.11: Effects of Treatment on Errors

	(1)	(2)	(3)	(4)
Panel A: Incumbent-Favoring Errors				
Treat	-0.211 (0.281)	-0.211* (0.120)	-0.211** (0.089)	-0.208** (0.089)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	3,408	3,408	3,408	3,408
R-squared	0.001	0.477	0.527	0.527
Panel B: Challenger-Favoring Errors				
Treat	-0.025 (0.216)	-0.025 (0.177)	-0.025 (0.118)	-0.025 (0.118)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	3,408	3,408	3,408	3,408
R-squared	0.000	0.117	0.253	0.253

Notes: Results from individual-level regressions. In Panel A, the dependent variable is the number of errors made by the respondent about candidate promises that were favoring the incumbent. In Panel B, the dependent variable is the number of errors made by the respondent about candidate promises that were favoring the challenger. In Column 4, the regression includes a dummy equal to one if someone in the household is a member of any group, a dummy equal to one if someone in the household participated in any collective action activity in the village in the past six months, the share of the local budget the respondent would like to spend on water and the share of the local budget the respondent would like to spend on roads. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.12: Effects of Treatment on Self-reported Support for Candidate

	(1)	(2)	(3)	(4)
Panel A: Candidate Ratings				
Treat	0.001 (0.009)	0.001 (0.009)	0.001 (0.006)	
Alignment	0.009 (0.061)	0.009 (0.061)	-0.008 (0.065)	-0.018 (0.454)
Interaction	0.013 (0.091)	0.013 (0.091)	0.020 (0.097)	-0.007 (0.578)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	No
Individual Fixed-Effects	No	No	No	Yes
Observations	6,825	6,825	6,825	6,825
R-squared	0.411	0.411	0.414	0.437
Panel B: Self-reported vote choice				
Treat	0 (0.007)	0 (0.007)	-0.001 (0.005)	
Alignment	0.052* (0.029)	0.052* (0.029)	0.050* (0.029)	0.048 (0.174)
Interaction	-0.006 (0.048)	-0.006 (0.048)	-0.031 (0.048)	-0.096 (0.249)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	No
Individual Fixed-Effects	No	No	No	Yes
Observations	6,793	6,793	6,793	6,793
R-squared	0.470	0.470	0.477	0.528

Notes: Results from candidate*individual-level regressions. All regressions include a full set of candidate dummies. In Panel A, the dependent variable is the rating given to the candidate relative to the average rating given to the other candidates. In Panel B, the dependent variable is a dummy equal if the respondent indicated voting for the candidate in our secret ballot exercise. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.13: Effects of Treatment on Knowledge of Politicians and Candidates

	(1)	(2)	(3)	(4)
Panel A: Knowledge of Local Politicians				
Treat	0.025 (0.040)	0.027 (0.030)	0.029 (0.020)	0.029 (0.020)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	3,187	3,187	3,187	3,187
R-squared	0.001	0.160	0.277	0.278
Panel B: Knowledge of Candidates				
Treat	-0.022 (0.040)	-0.022 (0.027)	-0.022 (0.020)	-0.020 (0.020)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	3,408	3,408	3,408	3,408
R-squared	0.001	0.218	0.309	0.312

Notes: Results from individual-level regressions. In Panel A, the dependent variable is an index capturing the respondent's knowledge of politicians in their village, municipality and province. In Panel B, the dependent variable is an index capturing the respondent's knowledge of mayoral candidates' political experience and education levels. In Column 4, the regression includes a dummy equal to one if someone in the household is a member of any group and a dummy equal to one if someone in the household participated in any collective action activity in the village in the past six months. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.14: Effects of Treatment on Vote Buying (individual-level)

	(1)	(2)	(3)	(4)
Panel A: Are you Aware of Instances of Vote Buying in your Village?				
Treat	0.029 (0.029)	0.033 (0.023)	0.035** (0.016)	0.034** (0.016)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	3,212	3,212	3,212	3,212
R-squared	0.001	0.105	0.193	0.193
Panel B: Did Someone Offered you Money for your Vote?				
Treat	0.027 (0.021)	0.031** (0.015)	0.032*** (0.011)	0.033*** (0.011)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	3,181	3,181	3,181	3,181
R-squared	0.001	0.117	0.176	0.176

Notes: Results from individual-level regressions. In Panel A, the dependent variable is a dummy equal to one if the respondent indicated being aware of instances of vote buying in her village. In Panel B, the dependent variable is a dummy equal to one if the respondent indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'missing']. In Column 4, the regression includes a dummy equal to one if someone in the household is a member of any group and a dummy equal to one if someone in the household participated in any collective action activity in the village in the past six months. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.15: Effects of Treatment on Vote Buying

	(1)	(2)	(3)	(4)
Panel A: Did Someone Offered you Money for your Vote ? [Alternative Coding]				
Treat	0.038* (0.021)	0.038** (0.016)	0.038** (0.016)	0.040*** (0.016)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	284	284	284	284
R-squared	0.012	0.462	0.718	0.719

Notes: Results from village-level regressions. The dependent variable is the share of respondent who indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'yes']. In Column 4, the regression includes the share of respondents with an household member who belongs to a group, the share of respondent who participated in any collective action activity in the village in the past six months, the village-average share of the local budget that respondents would like to spend on water and the village-average share of the local budget the respondent would like to spend on roads. The standard errors are (in parentheses). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.16: Effects of Treatment on Vote Buying by the Incumbent and the Challenger

	Voted for the Incumbent and Offered Money (1)	Accepted Money (2)	Voted for the Challenger and Offered Money (3)	Accepted Money (4)
Panel A: Pair fixed effects only				
Treat	0.017* (0.009)	0.018** (0.008)	-0.001 (0.006)	0.006 (0.007)
Control Group Mean	0.076	0.056	0.043	0.052
Pair Fixed-Effects	Yes	Yes	Yes	Yes
Additional Controls	No	No	No	No
Observations	3,077	3,077	3,077	3,077
R-squared	0.111	0.106	0.105	0.115
Panel B: Pair fixed effects and additional controls				
Treat	0.017* (0.009)	0.019** (0.008)	-0.001 (0.006)	0.006 (0.007)
Pair Fixed-Effects	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
Observations	3,077	3,077	3,077	3,077
R-squared	0.111	0.107	0.105	0.115

Notes: Results from individual-level regressions. The dependent variable is a dummy equal to one if the respondent indicated voting for the incumbent and having been offered money for her vote (Column 1). The dependent variable is a dummy equal to one if the respondent indicated voting for the incumbent and having been accepted money for her vote (Column 2). The dependent variable is a dummy equal to one if the respondent indicated voting for the challenger and having been offered money for her vote (Column 3). The dependent variable is a dummy equal to one if the respondent indicated voting for the challenger and having been accepted money for her vote (Column 4). In Panel B, regressions include a dummy equal to one if someone in the household is a member of any group and a dummy equal to one if someone in the household participated in any collective action activity in the village in the past six months. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.17: Comparing Incumbent and Challenger Promises

	Incumbent (1)	Challenger (2)	T-test (3)	K-Smirnov test (4)	OLS (5)
Health	15.417 (5.418)	15.333 (7.188)	-0.033 [0.974]	0.117 [1.000]	0.256 [0.919]
Education	15.417 (4.502)	12.333 (6.510)	-1.393 [0.176]	0.283 [0.544]	3.333 [0.192]
Emergencies	6.250 (3.108)	4.333 (3.200)	-1.566 [0.130]	0.200 [0.915]	1.667 [0.195]
Water and Sanitation	8.750 (4.330)	5.333 (3.994)	-2.128 [0.043]	0.333 [0.336]	2.949 [0.122]
Road	10.417 (5.418)	11.667 (5.563)	0.587 [0.563]	0.300 [0.468]	-1.026 [0.610]
Community Facilities	6.667 (3.257)	5.333 (3.994)	-0.934 [0.359]	0.183 [0.958]	0.897 [0.465]
Business Loans	8.750 (3.769)	10.667 (7.037)	0.849 [0.404]	0.317 [0.398]	-2.308 [0.362]
Agricultural Assistance	14.167 (7.017)	24.667 (19.036)	1.809 [0.082]	0.267 [0.624]	-9.744 [0.077]
Peace and Security	10.000 (5.222)	7.000 (4.928)	-1.531 [0.138]	0.283 [0.544]	3.077 [0.100]
Community Events	4.167 (3.589)	3.333 (3.619)	-0.597 [0.556]	0.133 [1.000]	0.897 [0.349]

Notes: n=27). The standard deviations are in (parentheses) (Columns 1-2). In Columns 3-4, the test statistics are reported along with the p-values [bracket]. Each cell in Column 5 is either the coefficient on the dummy variable indicating whether the campaign was implemented in the village from a different OLS regression with municipal fixed-effects or the associated p-value in [bracket].

Table A.18: Treatment Effects Vary with Incumbent Dominance (Vote Shares)

	Know Promises (1)	Errors Incumbent (2)	Challenger (3)	Salience (4)	Vote Buying (5)
Panel A: 2010 Incumbent Vote Share (Municipal, non-corrected)					
Treat	0.051*** (0.015)	-0.211** (0.088)	-0.025 (0.118)	0.110** (0.044)	0.034** (0.015)
Interaction	0.003*** (0.001)	-0.014*** (0.004)	0.001 (0.006)	0.003 (0.002)	0.002*** (0.001)
Observations	3,408	3,408	3,408	3,346	284
R-squared	0.330	0.528	0.253	0.090	0.745
Panel B: 2010 Incumbent Vote Share (Barangay, non-corrected)					
Treat	0.051*** (0.014)	-0.210** (0.090)	-0.046 (0.115)	0.105** (0.043)	0.035** (0.015)
Interaction	0.003*** (0.001)	-0.012*** (0.004)	0.001 (0.006)	0.005** (0.002)	0.002** (0.001)
Observations	3,408	3,408	3,408	3,346	284
R-squared	0.330	0.528	0.257	0.093	0.745

Notes: Results from individual-level (Columns 1-4) and village-level (Column 5) regressions. All regression include pair dummies. Interaction is (treatment $\times$ incumbent vote share in 2010 elections). Dependent variables are: Column 1, an index capturing the respondent's knowledge of candidate promises; Column 2, number of errors made by the respondent about candidate promises that were favoring the incumbent; Column 3, the number of errors made by the respondent about candidate promises that were favoring the challenger; Column 4, rating given to "Whether candidates will spend the municipal budget on things that are important to me and my family" when the respondent was asked about 'voting influences', adjusted to account for the average rating given to the other categories; Column 5, the share of respondents who indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'missing']. The standard errors (in parentheses) account for potential correlation within village (Columns 1-4). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.19: Treatment-Incumbent Dominance Interaction and Interactions with Barangay Education and Poverty

	Know Promises (1)	Incumbent (2)	Errors Challenger (3)	Salience (4)	Vote Buying (5)	Turnout Self-Reported (6)	Official (7)	Self-Reported (8)	Inc. Vote Share Official (9)
Panel A: 2010 Incumbent Vote Share (Municipal)									
Treat	0.051*** (0.014)	-0.218** (0.088)	-0.015 (0.117)	0.104** (0.043)	0.033** (0.015)	0.000 (0.004)	0.000 (0.006)	-0.023 (0.017)	0.007 (0.014)
Interaction	0.006*** (0.001)	-0.024*** (0.007)	0.002 (0.012)	0.006 (0.004)	0.004*** (0.001)	-0.001 (0.000)	-0.000 (0.001)	-0.001 (0.002)	-0.001 (0.001)
Observations	3,408	3,408	3,408	3,346	284	3,408	284	3,408	284
R-squared	0.331	0.529	0.254	0.092	0.754	0.062	0.749	0.284	0.844
Panel B: 2010 Incumbent Vote Share (Barangay)									
Treat	0.051*** (0.014)	-0.219** (0.089)	-0.031 (0.117)	0.096** (0.042)	0.034** (0.015)	0.001 (0.004)	-0.001 (0.005)	-0.023 (0.017)	0.005 (0.013)
Interaction	0.005*** (0.001)	-0.018*** (0.006)	-0.000 (0.009)	0.010*** (0.003)	0.003** (0.001)	-0.001** (0.000)	0.000 (0.000)	0.001 (0.001)	0.001 (0.001)
Observations	3,408	3,408	3,408	3,346	284	3,408	284	3,408	284
R-squared	0.332	0.529	0.256	0.096	0.753	0.063	0.764	0.284	0.850
Panel C: Number of projects implemented between 2010 and 2013									
Treat	0.052*** (0.015)	-0.222** (0.089)	-0.011 (0.117)	0.111*** (0.042)	0.032** (0.015)	0.001 (0.004)	-0.000 (0.006)	-0.021 (0.017)	0.008 (0.014)
Interaction	-0.033 (0.098)	0.637 (0.499)	-0.658 (0.759)	-0.005 (0.266)	-0.163** (0.077)	-0.048** (0.022)	-0.033 (0.037)	-0.069 (0.112)	-0.037 (0.084)
Observations	3,408	3,408	3,408	3,346	284	3,408	284	3,408	284
R-squared	0.327	0.528	0.255	0.096	0.753	0.063	0.755	0.285	0.845

Notes: δ Interaction $\tilde{\Gamma}$ is (treatment x incumbent vote share in 2010). All regressions include treatment interactions with barangay education and poverty. Columns 1-4, 6 and 8: individual-level data. Columns 5, 7 and 9: village-level. All regression include pair dummies. Dependent variables: Column 1, an index capturing the respondent's knowledge of candidate promises; Column 2, number of errors made by the respondent about candidate promises that were favoring the incumbent; Column 3, the number of errors about candidate promises that were favoring the challenger; Column 4, rating given to "Whether candidates will spend the municipal budget on things that are important to me and my family" when the respondent was asked about 'voting influences', adjusted to account for the average rating given to the other categories; Column 5, share of respondents who indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'missing']; Column 6, dummy equal to one if the respondent indicated voting in the 2013 elections; Column 7, official village-level turnout in the 2013 elections; Column 8, dummy equal to one if the respondent indicated voting for the incumbent in the 2013 elections; Column 9, official incumbent vote share at the village-level in the 2013 elections. The standard errors (in parentheses) account for potential correlation

Table A.20: Voter Budget Preferences and Treatment Effects on Turnout and Vote Share

	(1)	(2)	(3)	(4)
Panel A: Turnout				
Treat	0.085 (0.739)	-0.057 (0.508)	-0.055 (0.512)	0.091 (0.556)
Relative Preference	0.075 (0.047)	0.045 (0.075)	0.151 (0.121)	0.178 (0.121)
Interaction	-0.053 (0.071)	-0.053 (0.051)	-0.072 (0.053)	-0.077 (0.055)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	314	314	314	314
R-squared	0.005	0.513	0.722	0.726
Panel B: Candidate Vote Share				
Treat	0.169 (0.312)	0.169 (0.312)	0.229 (0.304)	
Alignment	0.003 (0.041)	0.003 (0.041)	-0.015 (0.066)	-0.087 (0.288)
Interaction	-0.056 (0.066)	-0.056 (0.066)	-0.080 (0.098)	-0.276 (0.264)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	No
Barangay Fixed-Effects	No	No	No	Yes
Observations	689	689	689	689
R-squared	0.860	0.860	0.864	0.873

Notes: Results from precinct-level regressions (Panel A) and candidate*precinct-level regressions (Panel B). In Panel A, the dependent variable is turnout in the 2013 mayoral elections. In Panel B, the dependent variable is the candidate vote share in the 2013 elections. All regressions include a full set of candidate dummies. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.21: Effects of Treatment on Campaign intensity (flyer distribution)

	(1)	(2)	(3)	(4)
Treat	-0.005 (0.016)	-0.005 (0.014)	-0.005 (0.010)	-0.003 (0.010)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	3,408	3,408	3,408	3,408
R-squared	0.000	0.025	0.116	0.121

Notes: Results from individual-level regressions. The dependent variable is a dummy equal to one if the respondent indicated having received a flyer from one of the candidates in the week before the elections. In Column 4, the regression includes a dummy equal to one if someone in the household is a member of any group and a dummy equal to one if someone in the household participated in any collective action activity in the village in the past six months. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.