Incumbent Advantage, Voter Information and Vote Buying *

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Abstract

Results from a new experiment in the Philippines shed light on the effects of voter information on vote buying and incumbent advantage. The treatment provided voters with information about the existence of a major spending program and the proposed allocations and promises of mayoral candidates just prior municipal elections. It left voters more knowledgeable about candidates' proposed policies and increased the salience of spending. 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 we provided attenuated incumbent advantage, prompting incumbents to increase their vote buying in response. Consistent with this explanation, both knowledge and vote buying impacts were higher in incumbent-dominated municipalities. 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

We use the results of a novel experiment in the Philippines to inform two significant issues in the political economy literature: the effects of voter information on incumbent advantage and on politician incentives to engage in vote buying. We find that an unanticipated pre-electoral information shock can reduce incumbent advantage. At the same time, the effect on the electoral equilibrium depends on how incumbents react: we argue that in a clientelistic political context, reducing the informational advantage of incumbents increases political incentives to engage in vote buying.

Incumbency advantage emerges if incumbents are able to influence the information that voters receive about their character and performance, reducing their electoral incentives to exert effort to improve voter welfare. A rich literature has focused on estimating incumbency advantage and the role of information in determining the electoral behavior of both candidates and voters. However, research has not yet examined the effects on incumbency advantage of one key dimension of voter information, voter knowledge of the services that candidates *could* provide them. This matters most in political settings where candidates cannot make credible pre-electoral commitments regarding the policies they plan to implement after the elections. In settings where they can, competition drives candidates to promise to do more for voters, even when voters are uncertain about what candidates could do. In settings where candidates cannot make credible commitments, voters rely more on retrospective voting rules based on the observed performance of the incumbent. Incumbents with an information advantage are able to persuade voters that there was little they could do and are held to a lower standard by voters.

We argue, and present evidence, that when an information shock a few days before the elections causes this information asymmetry to disappear, voters increase the performance threshold for re-election. In response, incumbents must do more for for voters in order to be re-elected. Since credible commitments are not possible and incumbents are unable to increase public goods provision a few days before the elections, the incumbent response takes the form of pre-electoral transfers to voters, vote buying. Hence, we show that in environments where credible commitments are difficult and vote buying is common, weakening incumbent advantage can trigger an increase in vote buying.

To investigate these issues, we conducted an experiment in the Philippines that sheds light on the dynamics of vote buying and the sources of incumbent advantage. The Philippines has several characteristics that make it ideal for this study: dominant incumbents are commonly observed; political parties are weak and evanescent, such that politicians cannot easily make credible pre-electoral commitments; and, consistent with this, vote buying is widespread.

Just before the municipal elections, we provided voters with information on a large fund, provided by the central government to every municipality, intended to finance municipal development projects. In the month before the mayoral elections, all mayoral candidates in twelve municipalities were asked how they would allocate resources from this fund across different types of projects. They were told that their commitments would be transmitted to randomly-selected *barangays* (villages) in their municipalities. Immediately thereafter, the Parish Pastoral Council for Responsible Voting (PPCRV), an election monitoring NGO

well-known throughout the Philippines, prepared and distributed flyers to 142 randomly selected neighborhoods comparing candidate promises. These flyers informed voters of the existence of this key program, detailed information on the types of projects the fund could finance, and the promises of the mayoral candidates regarding which projects they would finance through the program.

Three features of the treatment reinforced the salience and importance of the program: brochures were printed and distributed in person by trained volunteers; the candidates expressed views on how to spend resources; and a well-known and reputable NGO exerted the effort necessary to disseminate information about the program and candidate stances on program spending.

The nature and effects of the intervention suggest that the information we provided to voters raised their expectations about what incumbents could do for them. The intervention left voters more knowledgeable about the funding program and the concrete policies and programs that candidates promised to fund under the program. It also increased the salience of spending: voters 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.

We develop a framework to understand how voters and incumbents should react to an information shock that makes voters aware of the potential resources incumbents have to provide public goods, and how this reaction depends on the size of the pre-shock information asymmetry - and therefore, incumbent advantage - regarding this potential. Consistent with the model, and information-based theories of incumbency advantage more generally, the effects of the intervention on knowledge and on vote buying are stronger in incumbent-dominated municipalities. Since incumbents are able to respond to the information shock, by increasing vote buying, we do not expect a change in candidate vote shares. They remain, in fact, unchanged.

These results have several important implications. We provide the first direct evidence of the informational sources of incumbency advantage. Our results indicate that information interventions that attenuate the advantages of dominant incumbents, making elections more competitive, do increase political efforts to deliver benefits to voters. At the same time, as Khemani (2013) 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.

The remainder of the paper is organized as follows. Section 2 nests our analysis in a wide range of earlier contributions to the literature. Section 3 describes municipal elections in the Philippines and the experiment and data. Section 4 details the direct effects of the information intervention. In Section 5, to interpret our results, we develop a retrospective voting model of political competition where candidates cannot make credible pre-electoral promises. The model yields a number of ancilliary predictions that we are able to test; these predictions are examined in Section 5. Section 6 discusses the results and Section 7 concludes.

2 Literature Review

The paper contributes to several areas of the literature: research on the impact of information provision on voting behavior; on the links between vote buying and the provision of broader public goods; and on the relationship between voter information and incumbent advantage. The sections below address each of these in turn.

2.1 Information Provision

The contributions to the large literature on the effects of information on electoral behavior differ substantially in the actual information transmitted to voters and in how it is transmitted. They have variously emphasized the dissemination of voting procedures and voting irregularities (Vicente 2014 Aker, Collier, and Vicente 2011); valence issues such as candidate corruption, criminal records, education and attendance at parliamentary sessions (Banerjee, Kumar, Pande, and Su 2011 Chong, De La O, Karlan, and Wantchekon 2011 Humphreys and Weinstein 2013) and radio broadcasts that increase the demand for public services (Keefer and Khemani 2014).

Aker, Collier, and Vicente (2011) conducted three information interventions before the 2009 elections in Mozambique: one was a door-to-door information campaign to distribute and explain brochures about exactly how to vote; another disseminated SMS messages about electoral problems observed in senders' local areas, which had also been explained in treated areas through the door-to-door delivery of brochures; the third was the delivery of a free copy of the newspaper with the largest circulation in Mozambique (but largely absent in the study area), which both explained how to vote and how to send messages regarding election problems. All of the treatments in Aker, Collier, and Vicente (2011) increased voter turnout. Information about how to vote (though not about electoral irregularities) favored the incumbent party in Mozambique. These results suggest that incumbent advantage extends across voters who are more and less likely to vote, but do not illuminate the source of the advantage.

Numerous researchers have evaluated information interventions that inform households about corrupt or criminal behavior by candidates. Chong, De La O, Karlan, and Wantchekon (2011) examine local elections in Mexico, as we do in the Philippines, and provide voters with information on incumbent corruption. Treated voters were significantly less likely to vote for the incumbent, but the intervention also reduced turnout and votes for the challenger. Our analysis, nested in a retrospective voting model, is consistent with these results, to the extent that widespread criminal behavior by politicians indicates to voters that the incumbent failed to meet the ex post performance threshold that they had established (reducing support for the incumbent), and that politicians valued the non-pecuniary rewards of office so little that electoral accountability would have a negligible effect on voter welfare (leading voters to stay home rather than vote).

A campaign in India also informed voters about politicians' criminal behavior, but with effects. Banerjee, Kumar, Pande, and Su (2011) distributed information about the effort exerted by the incumbent legislator (the legislator's committee attendance, legislative activity, and spending of discretionary jurisdiction development funds), as well as about the wealth, education and criminal record of the incumbent and two main challengers. The informa-

tion was included in newspapers that were distributed to randomly-selected slums. These effects are more difficult to interpret since the treatment incorporated several pieces of information that could be expected to have heterogeneous effects on voter behavior, ranging from information that increases the salience to voters and their knowledge of development funds to factors that influence their expectations about the ability of incumbents to deliver (criminal records, wealth and education). We are able to focus on one particular information dimension, about a municipal development fund.

Researchers have also examined information campaigns and vote buying. Banerjee, Kumar, Pande, and Su (2011) examine the effects of their treatment on vote buying and gift-giving, finding negative effects on the first and no effects on the second. Our measure of vote buying is similar to their measure of gift-giving. Although we anticipated, in our pre-analysis plan, that the increased salience of public goods in our treatment area would suppress vote buying, in fact we also find no change. These results are difficult to compare, however, given differences both in treatment and context. With respect to treatment, their information treatment includes references to criminality, increasing the salience of criminal activity, while our treatment includes no references to crime or vote buying. In terms of context, vote buying is a criminal activity in India against which substantial law enforcement effort is dedicated. While vote buying is illegal in the Philippines, enforcement of anti-vote buying legislation is weak.

Vicente (2014) examined the effect of an information intervention on vote buying in Sao Tome and Principe. Ahead of the 2006 elections, they distributed leaflets discouraging voters from allowing vote buying to influence their vote. Voters in treated areas were significantly more likely to say that vote buying did not influence their vote. The campaign also reduced turnout and increased incumbent and reduced challenger vote shares. Vicente (2014) argues that the turnout effect is consistent with vote buying as a device to mobilize political participation (turnout): voters condition how and whether they vote on vote buying; observing the information campaign, candidates believe that voters are less likely to respond to vote buying and hence put less effort into buying votes; voters therefore do not go to the polls. To the extent that challengers are more reliant on vote buying than incumbents because, for example, voters are less familiar with them, this disproportionately affects them. A key aspect of our information intervention is that it makes no references to vote buying. However, we also interpret our findings as resulting from effects of the intervention on candidate incentives to buy votes.

Fujiwara and Wantchekon (2013) also provide voters with policy information, but in the context of deliberative town hall meetings in Benin, where candidate positions on policies may or may not have been revealed. Their intervention did not have a significant effect on survey responses regarding vote buying, though it did significantly affect a broader index of survey responses that they jointly characterized as reflecting voter attitudes towards clientelist forms of electoral mobilization. Their experiment did not permit them to examine whether effects differed in incumbent strongholds.

2.2 Credible Promises and Vote Buying

Our information intervention emphasizes broadly-targeted spending programs in a context where targeted pre-electoral transfers (vote buying) are common. A substantial literature

emphasizes the potential tradeoffs between vote buying and other narrowly targeted transfers, on the one hand, and the provision of broad public services, on the other. For example, models developed in Keefer and Vlaicu (2008) and Hanusch and Keefer (2012) link preferences for targeted transfers and vote buying, respectively, to the inability of politicians to make credible commitments to citizens. These analyses do not address the effects on vote buying of voter uncertainty about the benefits that incumbents provide them. In empirical research on Benin, Keefer and Khemani (2014) find that radio broadcasts concerning the benefits of health and education services shift household preferences towards candidates who promise jobs for a few rather than more health and education benefits for all. However, consistent with the results reported below, they also find that exposure to this programming has no effect on preferences for candidates who give electoral gifts, when it is unclear whether those gifts come at the expense of health and education services. In addition, this information does not seem to affect actual government behavior: villages with better access to radio do not have better schools nor access to more free anti-malaria bed nets.

2.3 Incomplete Information and Incumbency Advantage

Though incumbents do not always enjoy an electoral advantage, when they do, numerous scholarly contributions link it to incomplete voter information. Boas and Hidalgo (2011) find that control of community radio stations in Brazil increases incumbent vote share. Though their evidence does not allow them to unpack the radio programming that gives rise to this advantage, it is most plausible that it simply restricts the airtime of challengers and exaggerates the contributions of incumbents to listener welfare. MacDonald (2014), investigating a pattern of incumbent *disadvantage* in Zambia, argues that incumbents enjoy a smaller incumbency advantage in Zambia in districts where citizens have greater access to radio. These contributions highlight the importance of control over or access to sources of information, but do not investigate the particular types of information that might matter and the effects of access on vote buying.

The results of the Banerjee, Kumar, Pande, and Su (2011) experiment also support the notion of an information-linked incumbent advantage. They inform voters about incumbent legislative effort and the criminal records of incumbents and challengers. The strongest effects on turnout and vote-shares emerge in jurisdictions where the incumbent's performance rating (e.g., attendance at committee meetings) was worse and where the challengers were better qualified–that is, where incumbent performance was worse than uninformed voters might have anticipated. Our experiment instead examines the effects of information that signals what benefits incumbents could have provided voters.Ansolabehere, Snowberg, and Snyder (2006) trace the electoral advantages of US congressional incumbents to the greater coverage they receive in print media. Using data from the United States, Kasnja (2011) concludes that an increase in political awareness–knowledge of basic political facts–systematically reduces support for incumbents accused of corruption.

An implication of all of these analyses is that incumbents should increase their performance on behalf of voters when their information advantage shrinks. The information intervention we examine has this effect: it tells voters that incumbents have greater capacity to deliver public goods than voters thought.

The theoretical literature also describes circumstances under which citizen information

undermines incumbents' advantages, though because of strategic reactions by incumbents to voter information, the effects are subtle and often indirect. Gordon, Huber, and Landa (2007) endogenize both the quality of challengers who enter a race and voters' decision to acquire information about challengers. Incumbency advantage arises from the presence of incomplete voter information about incumbent quality.[1] Hodler, Loertscher, and Rohner (2010) argue that incumbents' advantage emerges from their better knowledge of the state of the world. Incumbents use this knowledge to strategically choose inefficient policies in areas where they are more competent than the challenger in order to create a state of the world that is just bad enough to encourage voters to re-elect them, since they are the candidates best placed to address the policy problem.

3 The Setting

3.1 Local Politics in the Philippines

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 and transferred a large number of civil servants to municipalities (Llanto 2012). For example, municipalities are responsible for the implementation of all nutrition programs (Khemani 2013). Despite the presence of one vice-mayor and eight municipal councilors, mayors exert significant control over how municipal resources are spent (Hutchcroft 2012). However, local officials exert little control over the size of the municipal budget. The typical municipality relies on fixed fiscal transfers from the central government, which constitute the large majority of resources available in most municipalities. Laws governing transfers to municipalities encourage mayors to allocate 20 percent of transfers to development projects.[2]

Second, municipal electoral campaigns tend to be centered around personalities and family alliances rather than around policies and party platforms (Hutchcroft and Rocamora 2003 Kerkvliet 2002). Filipino mayors are often viewed as *local bosses* (Capuno 2012 Sidel 1999) with substantial control over municipal budgets and spending decisions. Parties are often associated with specific politicians rather than with specific policies (Hutchcroft and Rocamora 2003). As a result, party affiliations do not provide information about proposed policies and programs but rather about alliances between local and national politicians. Consistent with the lack of programmatic parties, vote buying is prevalent (Cruz 2012). In addition, prior research has documented that Filipino voters use retrospective voting rules when

[1] An improvement in voter estimates of incumbent quality, which they infer from incumbent performance, either increases the probability that the uninformed voter retains the incumbent, or the probability that the uninformed voter exerts effort to become informed; either outcome boosts the unconditional probability of incumbent victory. An information shock that reduces voter perceptions of incumbent quality relative to the challenger correspondingly reduces the incumbent advantage.

[2] Section 287 of the Local Government Code states that *Each local government unit shall appropriate in its annual budget no less than twenty percent (20%) of its annual internal revenue allotment for development projects. Copies of the development plans of local government units shall be furnished the Department of Interior and Local Government.* In its Memorandum Circular 2010-138, the Department of Interior and Local Government further clarified that *development means the realization of desirable, social, economic and environmental outcomes essential in the attainment of the constitutional objective of a desired quality of life for all.* Those guidelines were further refined in the Joint Memorandum Circular 2011-1, issued on April 13, 2011 by the Department of Interior and Local Government and the Department of Budget and Management.

deciding whether to re-elect the incumbent (Cruz and Schneider 2013 Labonne 2013).

3.2 The Experiment

Our intervention is unique in eliciting and disseminating the public spending promises of candidates. It was designed with, and implemented by, the Parish Pastoral Council for Responsible Voting (PPCRV). We collected data from candidates on their proposed policies and platforms and distributed that information in brochures to voters in randomly selected villages ahead of the May 13, 2013 mayoral elections.[3] These brochures and the household visits by PPCRV staff to distribute and explain the brochures increased voter information about the types of public good provision that mayors could provide and to inform them of the availability of a large fund to finance provision of municipal public goods. 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.[4]

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. Candidate names were taken from the official list of the Commission on Elections (COMELEC). [5] In the course of the interview, we gave each candidate a worksheet with a list of sectors.[6] Candidates were told the average amount that they would have to spend from their local development fund (LDF) and asked to allocate money across sectors.

To facilitate this decision, candidates received 20 tokens to place on the worksheet and were told that each token represented five percent of the total LDF. They were instructed that they could put the tokens on any combination of sectors that they wished, and once they indicated they were satisfied with the sector allocation, the enumerators would record it. This information would then be given to randomly-selected barangays in their municipality before the elections.

If candidates did not give careful consideration to the exercise, the sectoral allocations described in the brochures could represent only a weak signal of what they would do if elected. Fortunately, the opposite was the case. Candidates were eager to participate in the program. Once they learned of the program, they called PPCRV to make sure that they would be included.[7] They also took the process of allocating tokens across sectors seriously, typically spending several minutes to arrange and rearrange the tokens after reconsidering their allocation.

In addition, for incumbents we can compare how they allocated the budget while in office with what they propose. We use budgetary data for the last full fiscal year before the election (2012) and find that the correlation between the share of the budget spent on each

[3] A copy of a flyer is included as Figure A.1.

[4] The submitted documents are available at: http://www.povertyactionlab.org/Hypothesis-Registry

[5] Ilocos Norte is the home province of the Marcos family and, both Imelda Marcos and her daughter Imee Marcos are still politically active there. Imee has been governor of Ilocos Norte since 2010 and Imelda has been Representative for Ilocos Norte's Second District since 2010.

[6] The sectors: public health services; public education services; cash or in-kind transfers (such as loans or job assistance); water and sanitation services; road construction and rehabilitation; construction of community facilities (such as multipurpose halls or basketball courts); business loans and other private economic development programs; agricultural assistance and irrigation systems; peace and security; community events and festivals.

[7] Only one candidate refused to participate.

sector with the share of the budget that the incumbent proposes to share on the sector is 0.55. This is indicative that candidate conscientiously allocated their tokens, particularly since changes in priorities and errors in budget data make a perfect correlation unlikely. Finally, we asked candidates to list projects and programs that they would implement if elected. Most of the proposals were quite specific, reducing concerns that candidates allocated tokens in an arbitrary and ad hoc manner. In addition, the sectoral allocations of candidates were in line with their listed projects and programs. Candidates consistently allocated a greater share of their proposed budget to those sectors that matched their promises (see Figure A.2).

Within each target municipality, villages were allocated to the treatment and control groups using a pairwise matching algorithm. First, 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. The final sample includes 142 treatment and 142 control villages in twelve municipalities (cf. Table A.2).[8]

PPCRV put together flyers comparing the proposed allocations of all candidates in each municipality.[9] Then, in the week leading up to the election, PPCRV volunteers, trained to use a detailed script, distributed the flyers through door-to-door visits in the target villages. Importantly, the flyer and the script did not mention vote buying, reducing any concerns related to social desirability bias.

Evidence that the treatment and control groups were similar prior to the intervention indicates that the randomization strategy was successful. The results in Table 2 indicate that the village-level variables used to carry out the pairwise matching exercises are well-balanced. We also use data from the survey to test if the treatment and control are balanced along sectoral preferences, alignment with the candidates (incumbent and challengers), household composition, households assets, etc.[10] Out of the 42 village- and household-level variables for which we test balance, only 4 exhibit differences that are significant at the 10 percent level. Controlling for those variables does not affect results reported below.

3.3 Data

We use two main data sources in the paper. First, we gathered precinct-level election results from the COMELEC. [11] The data included information on the number of votes obtained by all candidates in the mayoral elections. We then used data from the Project of Precincts to match precincts to villages. Every village contains at least one precinct. Second, we implemented a household survey in 284 villages in twelve municipalities in June 2013. Twelve households were interviewed in each village for a total sample size of 3,408 households.

[8]The list differs slightly from the one included in the Pre-Analysis Plan as volunteers could not distribute the flyers in Banayoyo, Pagudpud and Tagudin. In addition, we had to drop one pair in Pasuquin as we found out during the endline survey that the control village in that pair was a military camp. This is consistent with the protocols listed in Section 5.4 "Potential Adjustments for non compliance" of the Pre-Analysis Plan.

[9]There were only two or three candidates in each municipality. The figures are available in Table A.2.

[10]This set of results is available in Table A.3-A.5

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

These interviews yielded the key variables that we use in the analysis. Descriptive statistics are reported in Table 1.

Political Knowledge In order to examine whether the intervention was effective, we compare the knowledge of budget allocations between treated and 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. Following Kling, Liebman, and Katz (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 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:[12]

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

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. 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 both the raw responses and responses adjusted for the average answers in the other five categories.

Preferences over candidates and spending allocations We analyze the treatment's effects on voter preferences for candidates as a function of the proximity of candidates' allocations and voters' preferred allocations. We therefore collected data on respondents' candidate preferences and vote choice and asked them to express their preferences over the ten different spending categories that were given to the mayoral candidates. Each respondent rated all mayoral candidates on a 0 - 4 scale (strongly disagree to strongly agree). In addition, in order to reduce over-reporting votes in support of the winner, we used a secret ballot.[14]

[12]The formula included in the PAP contained a typo.

[13]To 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.

[14]Respondents 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

With respect to preferences over spending allocations, a similar procedure was used as with 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. We then construct an index of agreement, defined as $A_{vc} = \sum_s min\,(S_{vs}, S_{cs})$: the total spending over which the candidate and voter agree.

A potential concern with this variable is that, since it represents a choice that respondents aren't used to making, the quality of the data collected might suffer. 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.11 suggest that stated preferences over spending priorities match up relatively well with 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 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.10).

Occurrence of vote buying We measure vote buying through a series of questions asking about whether they are 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. In Isabela, a province near our study area, Khemani (2013) uses vote buying estimates from direct questions. Further, Cruz (2012) reports that, in the same province, the vote buying estimates obtained through direct questions are similar to the ones obtained with the unmatched count technique.[15]

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.

[15]In the pre-analysis plan, we indicated that we intended to also use an unmatched count technique to asses the extent of social desirability bias. The question was included in the household questionnaire but it referred to vote buying on 'election day'. Specifically, the question was *Here are some things that can happen to people during election day. How many of these things happened to you? You don't have to tell us which things happened, just how many*. Given that in Ilocos, vote buying tends to take place over a longer period of time, those estimates are unreliable and we do not use them in the analysis. Consistent with this, the unmatched estimates are much lower than the direct questions. Note that this is the opposite of what we would expect if the direct questions elicited significant social desirability bias.

4 Results: Effects on Beliefs and Vote Buying

Two direct effects of the experiment are of particular interest: did the information treatment in fact change beliefs? And did it influence vote buying? The results in this section first verify the essential assumption that more informed voters differ in their beliefs about candidates and in the issues that they regard as electorally salient. We then report results showing that the intervention increased vote buying. To explain the increase in vote buying, in Section 5 we develop a model that yields a number of additional predictions related to heterogeneity in treatment effects and voter choice. We test these predictions in Section 6.

4.1 Did the Treatment Affect Beliefs?

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. In this section, we report results concerning the information's treatment effect on voter beliefs.

To test the validity of the assumption that the treatment affected voter beliefs, we estimate the intervention's impacts on knowledge of candidates' promises using regressions of the form:

$$Y_{ijk} = \alpha T_j + v_k + u_{ijk} \tag{1}$$

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. As indicated in the Pre-Analysis Plan, we estimate equation (1) without fixed-effects, with municipal fixed effects and with pair fixed effects (Brunh and McKenzie 2009). Our preferred specification is the one with pair fixed effects and without additional controls. We also test if results are robust to the inclusion of the four variables that are not balanced between treatment and control.

As expected and outlined in the PAP, voters in treatment villages are more likely to know which candidates is promising to spend the largest share of the LDF on any given sector. Results are available in Table 3.[16] 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, of course, 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.

The fact that we asked voters precisely about the information provided during the intervention, and that we can confirm that the intervention indeed increased voter knowledge, is an improvement over the existing literature, where these tests have not been possible. For example, Aker, Collier, and Vicente (2011) do not test for knowledge effects (whether individuals actually knew more about election procedures or about electoral irregularities). In other experiments, such as Banerjee, Kumar, Pande, and Su (2011), researchers have tested

[16] The treatment had no effect on dimensions of political knowledge not included in the flyers (Table A.7).

treatment effects on voter knowledge, but the knowledge tested differed from the information provided to voters through the intervention.

We further explore whether the treatment affected how respondents decide which candidate to vote. In particular, we are interested in the treatment's effect on the salience of local development spending on vote decisions. We estimate:

$$Y_{ijk} = \alpha T_j + v_k + u_{ijk} \tag{2}$$

where Y_{ijk} captures how salient sectoral allocations are when individual i in village j in pair k decides which candidate to vote for, 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. As indicated in the Pre-Analysis Plan, we estimate equation (2) without fixed-effects, with municipal fixed effects and with pair fixed effects (Brunh and McKenzie 2009). Our preferred specification is the one with pair fixed effects and without additional controls. We also test if results are robust to the inclusion of the four variables that are not balanced between treatment and control.

As expected and outlined in the PAP, voters are more likely to report that candidates' proposals for local development spending are important when they decide which candidate to vote for. Results are available in Table 4. This holds whether raw or adjusted ratings are used.

4.2 Effects on Vote Buying

Survey results indicated high levels of vote buying - 13 percent of voters in the control group indicated being offered money for their votes. We can show that vote buying increased in the treatment villages, estimating equations of the form:

$$Y_{jk} = \alpha T_j + v_k + u_{jk} \tag{3}$$

where Y_{jk} is the prevalence of vote buying in village j in pair k during the May 2013 elections, 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. As indicated in the Pre-Analysis Plan, we estimate equation (3) without fixed-effects, with municipal fixed effects and with pair fixed effects (Brunh and McKenzie 2009). Our preferred specification is the one with pair fixed effects and without additional controls. We also test if results are robust to the inclusion of the four variables that are not balanced between treatment and control.

Results presented in Panels A and B of Table 5 indicate that vote buying intensified in treated villages.[17] In Panel A, the outcome variable is the share of respondents who were

[17]The specifications we examined in Table 5 were anticipated in our PAP. However, in the PAP we predicted that information would reduce vote buying . The theory underlying the PAP anticipated that the flyers would increase the salience and credibility of candidate promises regarding public good provision, leading them to substitute away from vote buying in treated areas. However, although an important part of our treatment was to argue that the PPCRV would monitor whether new mayors would adhere to their spending promises, nothing about our intervention increased the potential sanctions that voters could impose on candidates who reneged. For example, the intervention did not affect households' capacity to engage in collective action. On the other hand, the PAP did not anticipate that the flyers would give households new information that would lead them

aware of instances of vote buying in their village. his is an imprecise measure of vote buying, since voters have incomplete information about whether their neighbours have been targeted for vote buying.

In Panel B, the 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 variable used in Panel A is a noisier measure of vote buying than the one use in Panel B, we can only reject the null of no effect in Panel B. The intervention led to a 3.4 percentage points increase in vote buying (31 percent of the control group mean).[18]

5 Accounting for the Results

The fact that an apparently desirable information intervention should have increased vote buying is surprising. This section offers an explanation for it, and the following section provides a wealth of evidence supporting the explanation. The model developed here embeds two key characteristics that are central to mayoral elections in the Philippines: political competition does not center on policy promises, which are not credible; the mayoral office is a strong one and mayors are often dominant; and the provision of public goods out of funds provided by the central government is a key policy for mayors.

Previous research on targeted transfers and vote buying assumes that politicians can make credible commitments to some voters, but not all, and use a probabilistic voting model in which challenger promises influence voter decisions. In these models, voters are not uncertain about the policy benefits provided by politicians. To introduce this uncertainty and provide scope for an information shock, we use a retrospective voting model to describe voter decision making. Here, although challenger characteristics are irrelevant (since challengers cannot credibly commit to pursue different policies than the incumbent), voter beliefs about incumbent performance are central. Retrospective voting models emphasize the key insight of the earlier research, that vote buying is linked to the absence of credible commitment, but in contrast to earlier research, assume that politicians cannot make credible commitments to any voters.

Retrospective voting models provide a natural vehicle for illustrating the effects of our intervention, given that the treatment was intended to affect beliefs about what the incumbent could do for them. As indicated above, our treatment informs voters about a public spending program with concrete outputs and benefits, about which voters may be ignorant. Incumbents know about our intervention and know that voters can observe whether or not they have received benefits from the spending program to which their attention has been drawn. If the voters do not observe any benefits, and if incumbents believe that the brochure has convinced voters that they *should* have observed them, the incumbent will respond by buying votes. The gap between voter expectations about incumbent performance

to update incumbent performance thresholds. It is this effect that we model and test. It is also important to reiterate that, while the intervention increased vote buying, we argue that this was a result of an intervention that incumbent incentives to improve voter welfare.

[18] A common issue with is how to deal with the limited number of respondents who refuse 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.8).

before and after seeing the handout should be largest where the incumbent was most successful in suppressing expectations–where the incumbent was dominant. The effects of the intervention, therefore, should be greatest in incumbent strongholds. This section presents a more formal analysis of this logic.

5.1 Basic Set-Up

We model incumbent advantage as emerging from imperfect voter information about the public goods that incumbents can provide. In 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.

Under retrospective voting, 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 main 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 keep any incumbent from improving welfare.

We begin with a standard setup (see, e.g., Persson and Tabellini 2000, pp 236 - 238). 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. 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 given by $\sum f_i$. Our field experiment gave voters information about what public goods incumbents could provide for them, which can be modeled in terms of the ability of government to turn budgetary resources g into public goods that citizens value. This ability is given by the cost, $\bar{\theta}$, of producing public goods g , or $\bar{\theta}g$. It might be more difficult for politicians to turn budgetary resources into public goods valued by citizens - that is, $\bar{\theta}$ is higher - when there are restrictions on the type of public goods that can be purchased, or when the costs of inputs and construction are high, or when an immovable bureaucracy is incompetent. Spending on local public infrastructure delivers greater welfare to voters per peso of spending compared to direct transfers as long as the costs $\bar{\theta}$ are sufficiently low.

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. Mayors therefore choose direct transfers and public good spending to maximize their pecuniary rents, r, 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 (arbitrarily small) 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. However, incumbents are re-elected if they receive a majority of votes and therefore can oblige individual voters to compete with each other for the right to receive transfers. Equilibrium public goods and transfers are therefore determined by the following process, exactly as in Persson and Tabellini (2000).

Incumbents need the support of $\frac{N}{2}$ voters. They can reduce the public goods they supply as long as they can offer transfers to $\frac{N}{2}$ voters to make up for the welfare loss from foregone public goods. This is possible as long as the marginal utility of the transfers exceeds the marginal utility foregone from the reduction in public goods. At $H_g(g) = \frac{\bar{\theta}}{N}$, for example, a marginal reduction in public goods reduces the utility of each voter by $\frac{\bar{\theta}}{N}$. The incumbent can offset this welfare loss for $\frac{N}{2}$ voters by making transfers to them that total $\frac{N}{2}\frac{\bar{\theta}}{N} = \frac{\bar{\theta}}{2}$. Since each unit of public good provision costs $\bar{\theta}$, the total cost of these transfers is less than it would have cost the incumbent to provide the marginal unit of public goods. This tradeoff continues to be feasible for the incumbent until public good provision falls to $H_g(g) = \frac{2\bar{\theta}}{N}$ 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, $\frac{N}{2}\frac{2\bar{\theta}}{N} = \bar{\theta}$.

This pins down the public goods that voters can demand from incumbents, given the costs of producing public goods, $\bar{\theta} : H_g(g_{\bar{\theta}}) = \frac{2\bar{\theta}}{N}$. As in Persson and Tabellini (2000), though, actual transfers are driven to zero. Incumbents use the lure of transfers to reduce the public good spending that they offer, but competition between voters to be part of the majority that receives these transfers drives actual redistributive transfers to zero. The performance threshold set by voters is therefore $\bar{\omega} = H(g_{\bar{\theta}})$.

We add three features to this standard set-up. First, for most public goods, such as infrastructure, 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 in order to ensure that it has an electoral impact (Robinson and Torvik 2005) Transfers, however, can be implemented quickly, even at the end of the mayor's term, 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 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 that they value. Just before the election, each voter's beliefs about about the costs of producing public goods are drawn from a uniform distribution given by $\theta_i' \sim [1, 2\theta_c - 1]$. 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, $\delta \leq 1$. Incumbents know which voters are subject to the shock, but beyond that know only that the distribution of beliefs among them about the costs of producing public goods follows $\theta_i' \sim [1, 2\theta_c' - 1]$, where $\theta_c' = \theta_c + k\left(\bar{\theta} - \theta_c\right)$, $k \sim [-1, 1]$. 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 the beliefs θ_c of citizens regarding the costs of public good provision, the less they change in the event of a shock. Where this gap between the true cost of providing public goods and the cost perceived by the median voter is large, the information shock has a potentially larger effect on voter beliefs; where it is small, it does not. This is 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.[19]

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. Here, the voting rule that voters establish at the beginning of the mayor's term is that, given their individual draw from the distribution of potential pre-electoral beliefs, θ_i', about the costs of public good production, they will support the incumbent who meets the performance threshold $\bar{\omega}_i \geq H\left(g_{\theta'}\right)$, where $g_{\theta'}$ is given by $H_g\left(g_{\theta'}\right) = \frac{2\theta_i'}{N}$.[20]

The third feature of the set-up is that it provides an immediate link to the literature on incumbent advantage. Incumbents with a significant advantage are those for whom voters' beliefs voters about the costs they 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 observe the distribution of voter beliefs about the costs of public good provision, $\theta_i' \sim [1, 2\theta_c - 1]$.

2. Voters coordinate on a voting rule $\hat{\omega} = \omega\left(g_i\right)$, where g_i is given by $H_g\left(g_i\right) = \frac{2\theta_i'}{N}$ and θ_i' is observed after spending decisions on public goods are made, but before the next election.

3. Incumbents choose the level of public good provision, g^*.

[19] The results we derive here emerge, as well, in a more general formulation in which the distribution of voter beliefs is subject to both an anticipated and an unanticipated shock. To clarify exposition, we focus here on the case where there is only an unanticipated shock.

[20] In 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 shock occurs, observed by the incumbent, but not the voter, and then the incumbent makes policy. In the analysis here, neither politicians nor voters observe the information that voters will have about politician ability before politicians must make decisions about public good provision. Politicians can therefore not exploit an information asymmetry between themselves and voters, as in the canonical model of retrospective voting: there is no asymmetry at the time that they decide on public good spending. Voters, therefore, 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.

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 $\theta_c' = \theta_c + k\left(\bar{\theta} - \theta_c\right)$, $k \sim [-1, 1]$.

5. Incumbents choose the level of spending on transfers to voters.

6. The election takes place.

To see the equilibrium level of public goods and transfers that emerge from this environment, consider first the case in which there is no unanticipated shock. The voters' problem is to choose the highest performance threshold such that, given each voter i's beliefs about the costs of public good production, the incumbent would prefer to provide public goods that meet the performance threshold and be re-elected rather than retain the entire budget in the form of rents and forego re-election ($M - \bar{\theta} g_{\theta_i} + R > M$, recalling that incumbents know the cost of production of public goods, $\bar{\theta}$, though voters do not). The performance threshold must also be robust to inter-voter competition for transfer payments. The performance threshold selected by each voter is therefore $\bar{\omega}_i = H\left(g_{\theta_i}\right)$, where g_{θ_i} solves $H_g\left(g_{\theta_i}\right) = \frac{2\theta_i}{N}$.

The voting rule must satisfy the incumbent's participation constraint, $M - \bar{\theta} g_{max} + R \geq M$. A performance threshold based on beliefs θ_{max} yields the maximum public goods that the incumbent is willing to finance, where $H_g\left(g_{max}\right) = \frac{2\theta_{max}}{N}$ and $M - \bar{\theta} g_{max} + R = M$. Knowing the distribution of voter beliefs, the incumbent provides just enough public goods to ensure the support of $N/2$ voters, an amount determined by the median voter's beliefs, $H_g\left(g_{\theta_c}\right) = \frac{2\theta_c}{N}$. Since there is no unanticipated shock, and assuming that $\theta_c \geq \theta_{max}$so that the participation constraint on the incumbent does not bind, the equilibrium entails no transfers, public goods are given by $H_g\left(g_{\theta_c}\right) = \frac{2\theta_c}{N}$, and the re-election of the incumbent.

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 $\theta_c' = \theta_c + k\left(\bar{\theta} - \theta_c\right)$. Voters can still do no better than to set the same performance threshold as before and the incumbent can still do no better than to provide public goods given by the median voter's beliefs, $H_g\left(g_{\theta_c}\right) = \frac{2\theta_c}{N}$.

Case 1: The unanticipated shock is positive $\left(k\left(\bar{\theta} - \theta_c\right) > 0\right)$**.** The shock shifts up the distribution of beliefs about the costs of providing public goods among a fraction δ of voters. It continues to be the case that one-half of the voters who were not subjected to the information shock, given by $\frac{1}{2}(1 - \delta)$, believe the incumbent met their performance threshold. However, the beliefs of the δ fraction of voters exposed to the shock are distributed according to $\theta_i' \sim \left[1, 2\left(\theta_c + k\left(\bar{\theta} - \theta_c\right)\right) - 1\right]$. This means the incumbent's performance meets the threshold for some voters for whom it previously did not. The expectations of these voters regarding incumbent performance fell when, as a consequence of the information shock, their beliefs about the incumbent's costs increased. The fraction of voters in δ for whom the incumbent's performance is now sufficient, but previously was not, is given by $\left(\frac{\theta_c' - \theta_c}{2\left(\theta_c + k\left(\bar{\theta} - \theta_c\right)\right) - 2}\right) = \frac{1}{2}\left(\frac{k\left(\bar{\theta} - \theta_c\right)}{\theta_c + k\left(\bar{\theta} - \theta_c\right) - 1}\right) > 0$. The total fraction of voters in δ for whom the incumbent's performance is sufficient is therefore $\frac{1}{2}\left(1 + \frac{k\left(\bar{\theta} - \theta_c\right)}{\theta_c + k\left(\bar{\theta} - \theta_c\right) - 1}\right) > \frac{1}{2}$. 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 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 $\left(k\left(\bar{\theta}-\theta_c\right)<0\right)$**.** 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, $\bar{\omega}_i \leq H\left(g_{\theta_c}\right)$, and now do not believe this, $\bar{\omega}_i > H\left(g_{\theta_c+k}\right)$. Now, the fraction of the voters exposed to the information shock who are satisfied by the incumbent's performance is given by $\frac{1}{2}\left(1+\frac{k\left(\bar{\theta}-\theta_c\right)}{\theta_c+k\left(\bar{\theta}-\theta_c\right)-1}\right)<\frac{1}{2}$. 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. If they could, they would target the most persuadable voters, those for whom transfers $f_k = H\left(g_{g_{\theta_c+k}}\right) - H\left(g_{\theta_c}\right)$ are just sufficient to shift their support to the incumbent. However, they 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.

Incumbents can increase their probability of making useful transfers by targeting only voters exposed to the information shock. Recalling that $k\left(\bar{\theta}-\theta_c\right)$ is less than zero, $-\frac{1}{2}\left(\frac{k\left(\bar{\theta}-\theta_c\right)}{\theta_c+k\left(\bar{\theta}-\theta_c\right)-1}\right)$ is the fraction of voters in the group δ that received the information shock who would be "persuaded" by a transfer f_k. The fraction of voters in the group not exposed to the shock and that is equally persuadable is given by -$\frac{1}{2}\left(\frac{k\left(\bar{\theta}-\theta_c\right)}{\theta_c-1}\right)$. Since $\left(\theta_c-1\right) > \left(\theta_c+k\left(\bar{\theta}-\theta_c\right)-1\right)$ for $k\left(\bar{\theta}-\theta_c\right)<0$, the probability that a transfer will reach a persuadable voter is greater if it is targeted to voters in the group δ.[21]

Incumbents' probability of re-election ρ is now determined by the fraction α of the voters in δ to whom they provide the transfer $f_k = H\left(g_{g_{\theta_c+k}}\right) - H\left(g_{\theta_c}\right)$. The probability equals zero for $\alpha < \frac{1}{2}\left(\frac{k\left(\bar{\theta}-\theta_c\right)}{\theta_c+k\left(\bar{\theta}-\theta_c\right)-1}\right)$ - 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 goes to one as all members of δ receive the transfer, or $\alpha = 1$. Incumbents will therefore chooseα from $\left[\frac{1}{2}\left(\frac{k\left(\bar{\theta}-\theta_c\right)}{\theta_c+k\left(\bar{\theta}-\theta_c\right)-1}\right), 1\right]$ to maximize rents, $\rho\left[M-\bar{\theta}g_{\theta_c}-\alpha\frac{N}{2}f_k+R\right]$, subject to non-pecuniary rents from seeking office continuing to be sufficiently large that the incumbent still prefers to seek re-election, $M-\bar{\theta}g_{\theta_i}-\alpha\frac{N}{2}f_k+R \geq M-\bar{\theta}g_{\theta_i}$. That is, the incumbent chooses transfers such that the

[21]The intuition is straightforward: the information shock affects the upper limit of the distribution of voter beliefs of the costs of producing public goods, but not the lower limit. A shock that reduces voter beliefs about costs makes the uniform distribution denser at every point and, in particular, at the median. This effect is not unique to a uniform distribution. It would, for example, emerge for any distribution (e.g., a normal distribution) for which the density is highest at the median.

increase in the expected value of pecuniary and non-pecuniary rents just equals the cost of increased transfers.[22]

Three technical issues merit discussion. First, when 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.

Second, we assume that voters are unaware of the distribution of their beliefs - voters do not know where their beliefs stand compared to others'. This assumption has no influence on public good provision, since voter knowledge of the distribution does not change the incentives of the incumbent to provide public goods that just satisfy the performance threshold of the voter with the median beliefs about the costs of provision. It also has no influence on the effects of the unanticipated shock since it is, again, in the interests of the incumbent to target the most persuadable voters with transfers and in the interests of the voters targeted to support the incumbent if he meets their performance threshold.

Third, we 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 $\left(\theta_c + z\left(\bar{\theta} - \theta_c\right)\right)$. 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 established, and the unanticipated shock occurs, the dynamics of vote buying remain the same: in the event of negative shock(s), as long as they are not too large, the incumbent buys votes from the voters who were subjected to the shocks.

The model accounts for the puzzling result that more information can trigger greater vote buying: voters who receive more accurate information about the public goods the incumbent could have provided them raise their performance threshold. When the incumbent cannot adjust the provision of public goods in time for the election, incumbents respond to the higher threshold with vote buying. This result follows from the particular, though realistic, assumption that public good spending must begin substantially before the election and transfers can be made right before the elections. However, results presented in the next section support both the information assumptions underlying the model and ancilliary predictions of the model that are less directly tied to assumptions about the timing of public good spending.

First, the model assumes that incumbent advantage is a function of information: the more that θ_c exceeds $\bar{\theta}_c$, the greater is the advantage. This implies that an information shock should have a larger impact on voter beliefs in municipalities with a large incumbent ad-

[22] In the canonical model of retrospective voting, voters compete away transfers, driving them to zero. That is not true in this case, because the voting rule is credible: voters cannot make themselves better off by reneging on it and cannot make themselves worse off by adhering to it. Hence, even if the incumbent were to force competition among voters so that they could offer less than $H\left(g_{\theta'}\right) - H\left(g_i\right)$ to $N/2$ voters, falling short of the performance threshold, the equilibrium behavior of these voters would still be to vote for the challenger; as a consequence, the incumbent would not gain by making these offers.

vantage. The evidence below is consistent with this. First, in incumbent strongholds, voter beliefs are more likely to be mistaken in favor of the incumbent. Second, the information intervention changes the beliefs of voters in incumbent strongholds more than the beliefs of voters elsewhere.

Second, where the incumbent advantage is strong, the model predicts greater voter disatisfaction with the incumbent after an information shock that reveals a greater ability of incumbents to provide public goods and, therefore, a greater incentive for the incumbent to engage in vote buying. To see this, note first that the greater is the incumbent advantage, the larger the fraction of voters whose beliefs are shifted by the information shock. That fraction, $-\frac{1}{2}\left(\frac{k\left(\bar{\theta}-\theta_c\right)}{\theta_c+k\left(\bar{\theta}-\theta_c\right)-1}\right)$ for $k\left(\bar{\theta}-\theta_c\right)<0$, increases in the distance between $\bar{\theta}$ and θ_c. This implies that the minimum number of voters who must receive transfers for the incumbent to increase his probability of re-election above zero, in the event of a negative information shock, is larger the greater is the incumbent's information advantage. Second, the transfers each voter receives must increase, since $f_k = H\left(g_{g_{\theta_c+k}}\right) - H\left(g_{\theta_c}\right)$ and the larger is the effect of the shock, the larger is the welfare gap that the incumbent must close. Consistent with this, our evidence reveals significantly greater vote buying in treated villages located in incumbent strongholds than elsewhere.

6 Results: Mechanisms and Alternative Explanations

The model points to numerous heterogeneous effects of the information intervention in our field experiment. This section systematically explores these, ranging from the heterogeneous effects of information on beliefs about incumbents and challengers to heterogeneous effects on vote buying across municipalities according to whether they are incumbent strongholds or not.

6.1 The Heterogeneous Effects on Knowledge about Incumbents and Challengers

If our explanation of the positive effect of the information intervention on vote buying is accurate, we should, first, observe that incumbents enjoy significantly fewer information advantages among informed than among uninformed voters. Second, since the model, like all retrospective voting models, assumes that challenger characteristics are largely irrelevant to voter decision making, voters should pay more attention to incumbents and treatment effects should have larger effects on voter beliefs about incumbents than about challengers. The data are consistent with both of these predictions.

To see this, we 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 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.

We estimate equation (1) where Y_{ijk} is either the number of errors that favor the incumbent or the number of errors made that favor the challenger. Results available in Table 6

indicate that we can reject the null hypothesis of no effect for the incumbent-favoring errors but not for the challenger-favoring errors. 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). [23]

The model further predicts that information shocks should matter most where incumbents are dominant and where, as a consequence, their pre-intervention information advantage was greatest. To test this prediction, we examine the role of incumbent dominance, proxied by incumbent vote share in the previous municipal elections that took place in May 2010. We look at dominance measured in terms of both barangay (village) and municipal electoral results. Vote share, in turn, is measured as a percentage of the registered population.[24] To facilitate interpretation, the interacted variables are demeaned so the coefficient on the treatment dummy still captures the average treatment effect.

The estimates again support the hypothesis that the information treatment has the strongest effects on beliefs in municipalities with dominant incumbents (Table 7). The positive impact of the treatment on knowledge of campaign promises is significantly higher in incumbent-dominated municipalities and villages. Moreover, the effect is driven, as we would expect, by a reduction in the number of incumbent-favoring errors. There is no associated reduction in the number of challenger-favoring errors.

6.2 The Heterogeneous Effects on Vote Buying

The model also predicts that the intervention should also have larger effects on vote buying in incumbent-dominated villages and municipalities.[25] The data provide substantial evidence of the predicted effect. Proxying incumbent strongholds according to incumbent vote share in the 2010 elections, a one standard deviation in 2010 vote share more than doubles the impact of the intervention on vote buying.

By assumption, the model predicts that challengers do not react to the information shock. The information effects on respondent beliefs about incumbents and challengers support this assumption. Nevertheless, it is possible that it is not incumbents, but challengers, who increase their vote buying in incumbent strongholds. We take advantage of data collected through the secret ballot to estimate equation (1) where Y_{ijk} is a dummy equal to one if the respondent declared voting for the incumbent. We control for treatment status, whether the individual was targeted for vote buying and their interaction. The results, available in Table 8 are consistent with our argument that the information provided to voters unexpectedly raised their performance threshold for incumbents and that incumbents reacted by buying

[23] Importantly, as reported in Table A.6, incumbent and challenger promises do not seem to differ systematically, which could explain those results.

[24] Results are similar when using vote share defined as a percentage of the voting population (Table A.9).

[25] This effect would be muted if incumbent politicians are not able to adjust vote buying easily. In fact, vote buying in the Philippines is conducted by local brokers with close connections to politicians, giving politicians significant logistical capacity to adapt vote buying to changing circumstances in their strongholds (Cruz, Labonne, and Querubin 2014).

more votes. First, the treatment is associated with lower support for the incumbent. Second, the interaction term is positive and we are unable to reject the null that the sum of the treatment dummy and the interaction term is zero. If challengers were buying the votes we would expect to observe the opposite pattern

These results differ from those in prior research in ways that are consistent with our explanation of the effects we identify. The information interventions of Chong, De La O, Karlan, and Wantchekon (2011) in Mexico and of Vicente (2014) in Sao Tome and Principe both reduced turnout. In contrast, in our experiment, the information intervention led dominant incumbents to increase their use of the main mobilization tactic at their disposal–vote buying. Vote buying by dominant incumbents offset the effects of the information shock on the performance threshold that voters establish for incumbent re-election, leading to no net change in voter behavior (either with respect to turnout or to vote shares).[26]

6.3 Robustness Checks and Alternative Explanations

We contend that our main results and heterogenous treatment effects are explained by a reduction in the informational advantage that gives incumbents an electoral advantage. The evidence is robust both for effects on voter beliefs (the intervention has an unfavorable effect on voter beliefs about incumbents, and the effect is strongest in municipalities with dominant incumbents) and on politicians' reactions (vote buying rises in treated areas, with the largest increases in municipalities with dominant incumbents). It is nevertheless possible that the intervention affected voters and candidates through other channels and that dominant incumbents were better able to react to the intervention.

One alternative explanation for our findings is that challenger promises regarding infrastructure spending are actually credible and are more appealing to voters than incumbent promises in incumbent strongholds. For example, dominant incumbents may believe that they are more insulated from competition and better able to spend resources according to their personal preferences and not those of their constituents. When a challenger emerges who promises to spend in accordance with constituent preferences, incumbents are forced to react by buying more votes. If this were the case, however, our results would disappear after controlling for the overlap between candidate promises. In Panel A of Table 9 we report results of a specification that includes 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 with a measure of overlap between the candidate promises. The inclusion of these additional terms does not affect our results.

A second alternative explanation for our findings is that dominant incumbents are of bet-

[26]These two hypotheses were also included in the PAP and are reported in Table 10. The differences are likely related to the nature of the information provided. Voters in Mexico were provided with salient information about corruption cases, reducing voter expectations about the effectiveness of electoral accountability in improving voter welfare and, therefore, their incentive to vote. The information provided in Sao Tome and Principe explicitly discouraged vote buying, but not other features of the electoral system, and therefore may have reduced the benefits that voters expected to receive from electoral participation, again suppressing turnout. Our intervention was neutral with respect to the desirability or feasibility of vote buying, but not neutral with respect to the effects on voter expectations regarding incumbent performance. Its effects on vote buying and voter turnout were correspondingly different.

ter quality and therefore better able to react to the intervention. Again, this explanation does not account for the disproportionate effects of the intervention on voter beliefs in incumbent strongholds. In addition, if this were the mechanism accounting for our results, the interaction of the treatment dummy and incumbent stronghold would disappear after accounting for 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. In the results reported in Panel B of Table 9, however, the interaction of treatment dummy and incumbent stronghold remain significant.

7 Conclusion

We present results from a unique intervention that 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 beliefs 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 turn to vote buying.

The theory predicts that the effects of the intervention on voter beliefs and vote buying should be strongest in incumbent strongholds. In fact, voter beliefs about incumbents change more than their beliefs about challengers and the effect is strongest for dominant incumbents. In addition, vote buying significantly increased in areas exposed to this unanticipated information shock, and that effect was, again , greatest in incumbent strongholds.

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 knew 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 beliefs and politician vote buying,

Our findings also have implications for improving the accountability effects of elections in developing countries. It demonstrates 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 tends to reduce 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. 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	0.051	0.000
	(0.501)	(0.470)
Know politicians	0.034	0.009
	(0.544)	(0.573)
Salience sectors	2.458	2.300
	(1.470)	(1.513)
Salience sectors (adjusted)	0.884	0.776
	(1.188)	(1.207)
Error incumbent	4.763	4.974
	(3.080)	(3.110)
Error challenger	2.059	2.084
	(3.017)	(3.111)
Relative preference	12.597	12.529
	(12.101)	(11.663)
Turnout (self-reported)	0.965	0.965
	(0.183)	(0.184)
Vote Buying	0.165	0.138
	(0.371)	(0.345)

Notes: n= 3,408. The standard deviations are in (parentheses) (Columns 1-2)

Table 2: Balance Tests

	Treatment	Control	T-test	K-Smirnov test	OLS
	(1)	(2)	(3)	(4)	(5)
Precincts	1.092	1.099	0.181	0.021	-0.007
	(0.289)	(0.363)	[0.857]	[1.000]	[0.828]
Registered Voters	526.261	544.937	0.484	0.070	-18.676
	(306.785)	(342.678)	[0.629]	[0.842]	[0.526]
Population	842.197	895.923	0.826	0.056	-53.725
	(492.927)	(598.277)	[0.410]	[0.969]	[0.305]
Turnout	0.785	0.785	0.010	0.056	0.000
	(0.082)	(0.081)	[0.992]	[0.969]	[0.982]
Incumbent vote share 2010	0.728	0.720	-0.314	0.063	0.008
	(0.212)	(0.230)	[0.754]	[0.919]	[0.520]
Incumbent vote share [corrected]	0.559	0.552	-0.452	0.056	0.007
	(0.130)	(0.145)	[0.652]	[0.969]	[0.526]
Rural	0.880	0.873	-0.180	0.007	0.007
	(0.326)	(0.334)	[0.857]	[1.000]	[0.836]

Notes: n=352 (Panel A). 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 3: Effects of Treatment on : Knowledge of Promises

	(1)	(2)	(3)	(4)
Treat	0.051	0.051**	0.051***	0.052***
	(0.036)	(0.022)	(0.015)	(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

Notes: Results from individual-level regressions. The dependent variable is an index capturing the respondent's knowledge of candidate promises. 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 4: Effects of Treatment on : Salience of Budgetary Allocations

	(1)	(2)	(3)	(4)
Panel A: Salience				
Treat	0.159*	0.158*	0.161**	0.168**
	(0.096)	(0.094)	(0.070)	(0.068)
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	0.150
Panel B: Salience (adjusted)				
Treat	0.107*	0.107*	0.109**	0.113**
	(0.060)	(0.058)	(0.044)	(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 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 B, 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, 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 5: 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	0.033	0.033	0.030
	(0.029)	(0.023)	(0.023)	(0.025)
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	0.695
Panel B: Did Someone Offered you Money for your Vote?				
Treat	0.034	0.034**	0.034**	0.043***
	(0.021)	(0.016)	(0.016)	(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	0.740

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 6: Effects of Treatment on : Errors

	(1)	(2)	(3)	(4)
Panel A: Incumbent-Favouring Errors				
Treat	-0.211	-0.211*	-0.211**	-0.218**
	(0.281)	(0.120)	(0.089)	(0.090)
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	0.528
Panel B: Challenger-Favouring Errors				
Treat	-0.0250	-0.0250	-0.0250	-0.0260
	(0.216)	(0.177)	(0.118)	(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	0.001	0.117	0.253	0.254

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 7: Heterogeneity

	Know Promises	Errors		Salience	Vote Buying
		Incumbent	Challenger		
	(1)	(2)	(3)	(4)	(5)
Panel A: 2010 Incumbent Vote Share (Municipal)					
Treat	0.051***	-0.211**	-0.025	0.109**	0.034**
	(0.015)	(0.088)	(0.118)	(0.044)	(0.015)
Interaction	0.007***	-0.024***	0.002	0.006	0.004***
	(0.001)	(0.008)	(0.012)	(0.004)	(0.001)
Observations	3,408	3,408	3,408	3,346	284
R-squared	0.330	0.528	0.253	0.090	0.742
Panel B: 2010 Incumbent Vote Share (Barangay)					
Treat	0.051***	-0.212**	-0.039	0.104**	0.035**
	(0.015)	(0.090)	(0.117)	(0.043)	(0.015)
Interaction	0.006***	-0.019***	0.000	0.009**	0.003**
	(0.001)	(0.006)	(0.009)	(0.004)	(0.001)
Observations	3,408	3,408	3,408	3,346	284
R-squared	0.330	0.528	0.255	0.093	0.741

Notes: Results from individual-level (Columns 1-4))and village-level (Column 5) regressions. All regression include pair dummies. In Column 1, the dependent variable is an index capturing the respondent's knowledge of candidate promises. In Column 2, the dependent variable is the number of errors made by the respondent about candidate promises that were favoring the incumbent. In Column 3, the dependent variable is the number of errors made by the respondent about candidate promises that were favoring the challenger. In Column 4, 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'. The variable is adjusted to account for the average rating given to the other categories. In Column 5, the dependent variable is the share of respondent 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 8: Effects of Treatment on : Self-reported vote for the incumbent

	(1)	(2)	(3)	(4)
Treat	-0.030	-0.041**	-0.041**	-0.041**
	(0.036)	(0.019)	(0.019)	(0.019)
Vote-buying	-0.156***	-0.001	-0.001	0.004
	(0.037)	(0.034)	(0.034)	(0.034)
Interaction	0.043	0.039	0.039	0.032
	(0.056)	(0.049)	(0.049)	(0.048)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	2,882	2,882	2,882	2,881
R-squared	0.012	0.314	0.314	0.319

Notes: Results from individual-level regressions. The dependent variable is a dummy equal to one if the respondent indicated voting for the incumbent. 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, the share of the local budget the respondent would like to spend on roads, 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 9: Alternative Channels

	Know Promises	Errors		Salience	Vote Buying
		Incumbent	Challenger		
	(1)	(2)	(3)	(4)	(5)
Panel A: Controlling for Overlap between Candidate Promises					
Treat	0.050***	-0.209**	-0.025	0.109**	0.034**
	(0.015)	(0.088)	(0.118)	(0.044)	(0.015)
Treat*Strongholds	0.007***	-0.024***	0.003	0.006	0.004***
	(0.001)	(0.007)	(0.012)	(0.004)	(0.001)
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***	-0.210**	-0.023	0.109**	0.034**
	(0.015)	(0.088)	(0.117)	(0.044)	(0.015)
Treat*Strongholds	0.007***	-0.028***	0.002	0.006	0.003**
	(0.002)	(0.008)	(0.013)	(0.005)	(0.002)
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 interaction between the treatment dummy and incumbent education and affiliations with national and provincial politicians. In Column 1, the dependent variable is an index capturing the respondent's knowledge of candidate promises. In Column 2, the dependent variable is the number of errors made by the respondent about candidate promises that were favoring the incumbent. In Column 3, the dependent variable is the number of errors made by the respondent about candidate promises that were favoring the challenger. In Column 4, 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'. The variable is adjusted to account for the average rating given to the other categories. In Column 5, the dependent variable is the share of respondent 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 10: Further Results

	(1)	(2)	(3)	(4)
Panel A: Turnout				
Treat	0.034	-0.109	-0.125	0.330
	(0.729)	(0.506)	(0.510)	(0.558)
Relative Preference	0.075	0.045	0.151	0.237*
	(0.047)	(0.075)	(0.121)	(0.126)
Interaction	-0.053	-0.053	-0.072	-0.076
	(0.071)	(0.051)	(0.053)	(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	0.00500	0.513	0.722	0.732
Panel B: Candidate Vote Share				
Treat	0.169	0.169	0.229	
	(0.312)	(0.312)	(0.304)	
Alignment	0.003	0.003	-0.015	-0.087
	(0.041)	(0.041)	(0.066)	(0.288)
Interaction	-0.056	-0.056	-0.080	-0.276
	(0.066)	(0.066)	(0.098)	(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. he standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Appendix for Online Publication

A.1 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

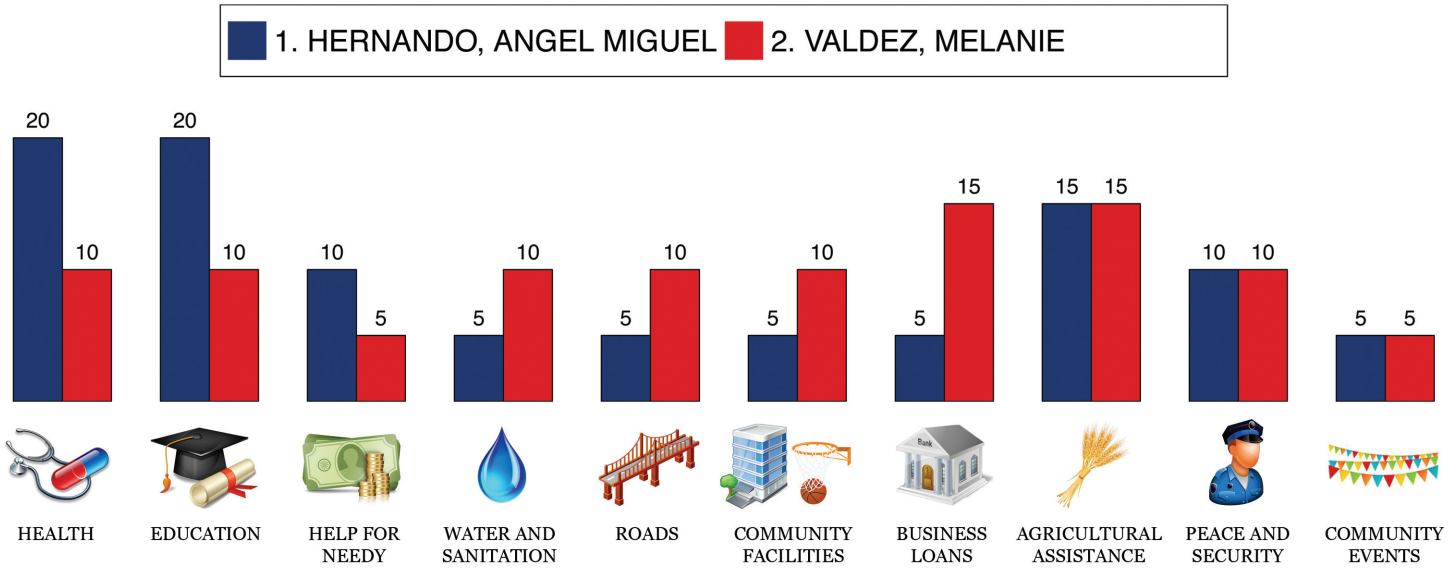
Table A.2: List of Intervention Municipalities

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.

SAN NICOLAS

Ammoyo Kadi...



Anya pay ti karkarida?

Angel Miguel Hernando (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.1: Flyer for the Municipality of San Nicolas, Ilocos Norte

A.2 Additional Results

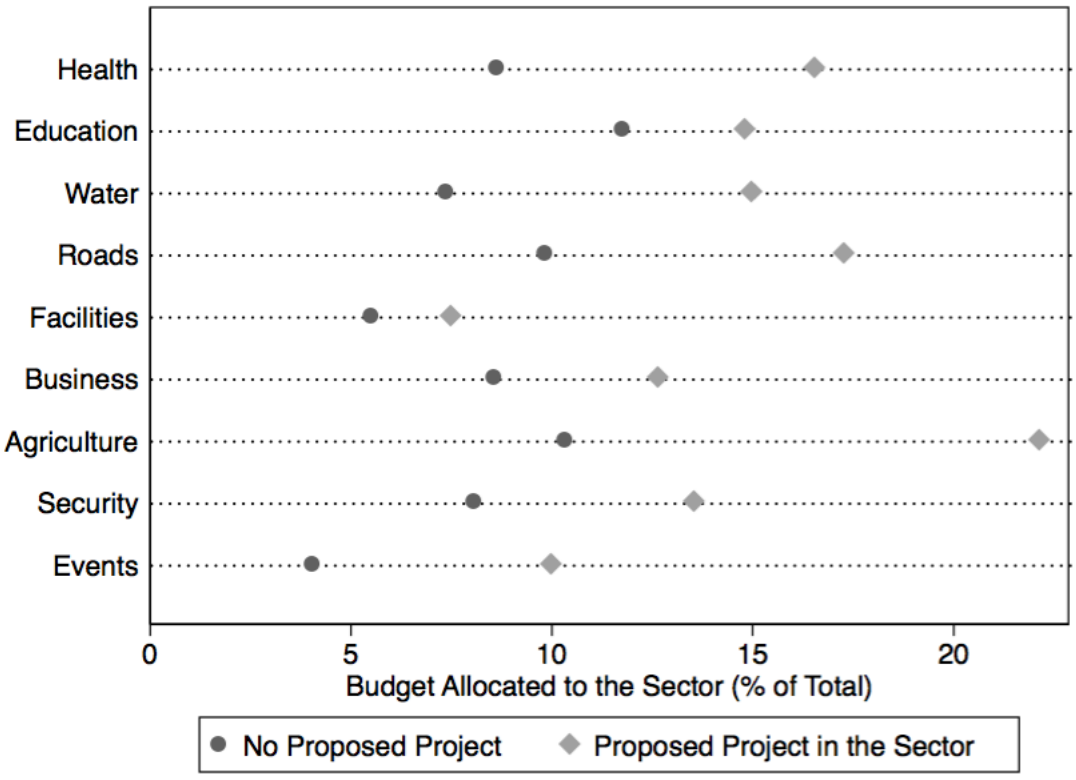


Figure A.2: Candidate Proposed Projects and Budget Allocations

Table A.3: Balance Tests : Preferences Over Sectoral Allocations

	Treatment	Control	T-test	K-Smirnov test	OLS
	(1)	(2)	(3)	(4)	(5)
Panel A: Sectoral Preferences					
Health	18.915	18.714	-0.453	0.012	0.204
	(12.834)	(13.094)	[0.651]	[1.000]	[0.641]
Education	17.378	18.048	1.530	0.021	-0.665
	(12.527)	(12.993)	[0.126]	[0.828]	[0.124]
Help for Needy	8.086	8.162	0.228	0.012	-0.077
	(9.610)	(9.742)	[0.820]	[1.000]	[0.814]
Water and Sanitation	7.822	8.500	2.363	0.036	-0.675
	(7.989)	(8.731)	[0.018]	[0.213]	[0.017]
Roads	7.340	6.541	-2.792	0.050	0.791
	(9.035)	(7.587)	[0.005]	[0.028]	[0.004]
Community Facilities	4.877	4.689	-0.891	0.021	0.186
	(6.413)	(5.872)	[0.373]	[0.851]	[0.366]
Business Loans	5.802	5.637	-0.496	0.016	0.161
	(9.917)	(9.538)	[0.620]	[0.982]	[0.624]
Agricultural Assistance	21.517	21.947	0.690	0.025	-0.422
	(18.078)	(18.249)	[0.490]	[0.644]	[0.482]
Peace and Security	5.473	5.197	-1.238	0.018	0.272
	(6.662)	(6.363)	[0.216]	[0.934]	[0.213]
Community Events	2.790	2.566	-1.354	0.023	0.223
	(4.771)	(4.859)	[0.176]	[0.759]	[0.172]
Panel B: Alignment					
Alignment	58.396	58.267	-0.285	0.011	0.128
	(19.418)	(19.674)	[0.776]	[0.984]	[0.764]
Alignment (challenger)	57.168	57.063	-0.182	0.015	0.105
	(18.562)	(18.557)	[0.856]	[0.977]	[0.849]
Alignment (incumbent)	59.883	59.727	-0.220	0.031	0.156
	(20.314)	(20.861)	[0.826]	[0.365]	[0.811]

Notes: n=3,408 (Panel A) and 7,896 (Panel B). 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 : Respondent Characteristics

	Treatment	Control	T-test	K-Smirnov test	OLS
	(1)	(2)	(3)	(4)	(5)
stay	40.693	40.390	-0.464	0.018	0.303
	(19.069)	(19.028)	[0.643]	[0.941]	[0.639]
Family size	5.022	5.096	1.012	0.032	-0.074
	(2.217)	(2.045)	[0.312]	[0.321]	[0.297]
Female	0.490	0.492	0.137	0.002	-0.002
	(0.500)	(0.500)	[0.891]	[1.000]	[0.889]
Age	49.361	48.853	-1.103	0.034	0.505
	(13.537)	(13.374)	[0.270]	[0.262]	[0.269]
Education (years)	9.462	9.353	-0.930	0.039	0.110
	(3.468)	(3.422)	[0.353]	[0.134]	[0.345]
Remittances abroad	0.225	0.244	1.334	0.019	-0.019
	(0.418)	(0.430)	[0.182]	[0.899]	[0.175]
CCT Beneficiary	0.154	0.159	0.377	0.005	-0.005
	(0.361)	(0.366)	[0.706]	[1.000]	[0.700]
Group Member	0.671	0.644	-1.697	0.028	0.028
	(0.470)	(0.479)	[0.090]	[0.518]	[0.077]
Barangay assembly	0.925	0.936	1.214	0.011	-0.011
	(0.263)	(0.245)	[0.225]	[1.000]	[0.213]
Collective Action	0.736	0.767	2.102	0.031	-0.031
	(0.441)	(0.423)	[0.036]	[0.365]	[0.026]
Religion: never	0.085	0.087	0.183	0.002	-0.002
	(0.279)	(0.282)	[0.855]	[1.000]	[0.853]
Religion: weekly	0.373	0.363	-0.568	0.009	0.009
	(0.484)	(0.481)	[0.570]	[1.000]	[0.561]

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.5: Balance Tests : Respondent Characteristics

	Treatment	Control	T-test	K-Smirnov test	OLS
	(1)	(2)	(3)	(4)	(5)
	(0.177)	(0.162)	[0.363]	[1.000]	[0.358]
Radio	0.735	0.742	0.507	0.008	-0.008
	(0.442)	(0.437)	[0.612]	[1.000]	[0.605]
Television	0.877	0.876	-0.104	0.001	0.001
	(0.328)	(0.329)	[0.917]	[1.000]	[0.915]
Phone	0.890	0.905	1.412	0.015	-0.015
	(0.313)	(0.293)	[0.158]	[0.992]	[0.154]
wash mach	0.326	0.346	1.269	0.021	-0.021
	(0.469)	(0.476)	[0.204]	[0.855]	[0.197]
Fridge	0.529	0.542	0.755	0.013	-0.013
	(0.499)	(0.498)	[0.450]	[0.999]	[0.443]
Gas stove	0.603	0.614	0.632	0.011	-0.011
	(0.489)	(0.487)	[0.528]	[1.000]	[0.515]
Bicycle	0.406	0.390	-0.945	0.016	0.016
	(0.491)	(0.488)	[0.345]	[0.981]	[0.336]
Boat	0.023	0.026	0.549	0.003	-0.003
	(0.151)	(0.160)	[0.583]	[1.000]	[0.558]
Motorcycle	0.535	0.549	0.825	0.014	-0.014
	(0.499)	(0.498)	[0.409]	[0.995]	[0.402]

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: Comparing Incumbent and Challenger Promises

	Incumbent	Challenger	T-test	K-Smirnov test	OLS
	(1)	(2)	(3)	(4)	(5)
Health	15.417	15.333	-0.033	0.117	0.256
	(5.418)	(7.188)	[0.974]	[1.000]	[0.919]
Education	15.417	12.333	-1.393	0.283	3.333
	(4.502)	(6.510)	[0.176]	[0.544]	[0.192]
Emergencies	6.250	4.333	-1.566	0.200	1.667
	(3.108)	(3.200)	[0.130]	[0.915]	[0.195]
Water and Sanitation	8.750	5.333	-2.128	0.333	2.949
	(4.330)	(3.994)	[0.043]	[0.336]	[0.122]
Road	10.417	11.667	0.587	0.300	-1.026
	(5.418)	(5.563)	[0.563]	[0.468]	[0.610]
Community Facilities	6.667	5.333	-0.934	0.183	0.897
	(3.257)	(3.994)	[0.359]	[0.958]	[0.465]
Business Loans	8.750	10.667	0.849	0.317	-2.308
	(3.769)	(7.037)	[0.404]	[0.398]	[0.362]
Agricultural Assistance	14.167	24.667	1.809	0.267	-9.744
	(7.017)	(19.036)	[0.082]	[0.624]	[0.077]
Peace and Security	10.000	7.000	-1.531	0.283	3.077
	(5.222)	(4.928)	[0.138]	[0.544]	[0.100]
Community Events	4.167	3.333	-0.597	0.133	0.897
	(3.589)	(3.619)	[0.556]	[1.000]	[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.7: Effects of Treatment on : Knowledge of Politicians and Candidates

	(1)	(2)	(3)	(4)
Panel A: Knowledge of Local Politicians				
Treat	0.025	0.027	0.029	0.029
	(0.040)	(0.030)	(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	3,187	3,187	3,187	3,187
R-squared	0.001	0.160	0.277	0.278
Panel B: Knowledge of Candidates				
Treat	-0.022	-0.022	-0.022	-0.024
	(0.040)	(0.027)	(0.020)	(0.019)
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	0.314

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, 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.8: 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*	0.038**	0.038**	0.047***
	(0.021)	(0.016)	(0.016)	(0.017)
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	0.728

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.9: Heterogeneity

	Know Promises	Errors		Salience	Vote Buying
		Incumbent	Challenger		
	(1)	(2)	(3)	(4)	(5)
Panel A: 2010 Incumbent Vote Share (Municipal, non-corrected)					
Treat	0.051***	-0.211**	-0.025	0.110**	0.034**
	(0.015)	(0.088)	(0.118)	(0.044)	(0.015)
Interaction	0.003***	-0.014***	0.001	0.003	0.002***
	(0.001)	(0.004)	(0.006)	(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***	-0.210**	-0.046	0.105**	0.035**
	(0.014)	(0.090)	(0.115)	(0.043)	(0.015)
Interaction	0.003***	-0.012***	0.001	0.005**	0.002**
	(0.001)	(0.004)	(0.006)	(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. In Column 1, the dependent variable is an index capturing the respondent's knowledge of candidate promises. In Column 2, the dependent variable is the number of errors made by the respondent about candidate promises that were favoring the incumbent. In Column 3, the dependent variable is the number of errors made by the respondent about candidate promises that were favoring the challenger. In Column 4, 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'. The variable is adjusted to account for the average rating given to the other categories. In Column 5, the dependent variable is the share of respondent 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.10: Effects of Treatment on : self-reported support for candidate

	(1)	(2)	(3)	(4)
Panel A: Candidate Ratings				
Treat	0.001	0.001	0.001	
	(0.009)	(0.009)	(0.006)	
Alignment	0.009	0.009	-0.008	-0.018
	(0.061)	(0.061)	(0.065)	(0.454)
Interaction	0.013	0.013	0.020	-0.007
	(0.091)	(0.091)	(0.097)	(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	-0.001	
	(0.007)	(0.007)	(0.005)	
Alignment	0.052*	0.052*	0.050*	0.048
	(0.029)	(0.029)	(0.029)	(0.174)
Interaction	-0.006	-0.006	-0.031	-0.096
	(0.048)	(0.048)	(0.048)	(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.11: Correlates of Sectoral Preferences

	(1) Health	(2) Education	(3) Emergencies	(4) Water	(5) Road	(6) ComFaci	(7) EconProg	(8) Agriculture	(9) Peace	(10) Festivals
Education (years)	-0.106	0.347***	-0.195***	0.064	0.128***	0.052	-0.110**	-0.262***	0.125***	-0.045*
	(0.075)	(0.069)	(0.051)	(0.039)	(0.048)	(0.033)	(0.045)	(0.087)	(0.036)	(0.025)
Children below 6	0.022	0.843***	-0.034	-0.019	-0.116	-0.057	-0.469***	0.124	-0.090	-0.204**
	(0.349)	(0.296)	(0.205)	(0.170)	(0.226)	(0.132)	(0.159)	(0.331)	(0.132)	(0.102)
Children 7-14	-0.605**	1.152***	-0.317**	0.005	-0.324**	-0.106	0.128	0.366	-0.305***	0.006
	(0.242)	(0.230)	(0.161)	(0.146)	(0.154)	(0.113)	(0.235)	(0.345)	(0.109)	(0.111)
Farmer	-1.582**	-1.375***	-1.433***	-0.245	0.106	-1.285***	-1.337***	8.810***	-0.993***	-0.666***
	(0.666)	(0.500)	(0.445)	(0.352)	(0.381)	(0.301)	(0.429)	(0.733)	(0.281)	(0.219)
Business Owner	-0.484	0.758	-0.413	0.033	-0.017	-0.187	0.943	-1.194	0.204	0.355
	(0.808)	(0.712)	(0.582)	(0.435)	(0.478)	(0.387)	(0.643)	(0.822)	(0.377)	(0.294)
Female	0.931**	0.491	2.197***	-0.052	-1.488***	-0.575**	0.789**	-2.199***	-0.070	-0.025
	(0.469)	(0.444)	(0.368)	(0.331)	(0.326)	(0.225)	(0.362)	(0.647)	(0.246)	(0.178)
Observations	3,404	3,404	3,404	3,404	3,404	3,404	3,404	3,404	3,404	3,404
R-squared	0.007	0.028	0.022	0.001	0.012	0.013	0.012	0.077	0.015	0.008

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.