

Incumbent Advantage, Voter Information and Vote Buying *

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Abstract

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

Keywords: Political Economy, Vote Buying, Information, Elections

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1 Introduction

The results of a novel experiment in the Philippines extend research on voter information, vote buying, and incumbent advantage. First, we find that voters' information about what politicians can do for them increases politician incentives to engage in vote buying. Second, it reduces incumbent advantage, the degree to which some incumbents can secure a higher vote share than others without exerting greater effort on behalf of constituents.

These conclusions emerge from an experiment in which, just before the municipal elections in the Philippines, we gave voters information about a large fund, provided by the central government to every municipality and intended to finance municipal development projects. The intervention was implemented in 142 randomly selected villages in 12 municipalities and significantly increased voter knowledge about the funding program and the concrete policies 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. Finally, voters who received the information were *more* likely to report vote buying.

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. This reaction depends on the size of the pre-shock information asymmetry regarding this potential. Consistent with the model, and information-based theories of incumbent advantage more generally, the effects of the intervention on knowledge and vote buying are larger in incumbent strongholds (municipalities where incumbent margins of victory were largest in the previous election). Since incumbents are able to respond to the information shock by increasing vote buying, we do not expect a change in turnout or candidate vote shares; the treatment has no effect on either.

Voter knowledge of the services that incumbents *could* provide them matters most in political settings where candidates cannot make credible pre-electoral commitments regarding the policies they plan to implement after the elections. In these settings, voters rely more on retrospective voting rules based on the observed performance of the incumbent, ignoring challenger characteristics. Incumbents with

an information advantage are able to persuade voters that there was little they could do for voters, leading voters to hold them to a lower performance challenge than they hold incumbents who do not enjoy this informational advantage.

The Philippines has several characteristics that make it ideal for this study. Political parties are weak and evanescent, such that politicians cannot make credible pre-electoral commitments; and vote buying is widespread. We do not seek to explain systemic incumbent advantage (e.g., why U.S. Congressmen are difficult to dislodge) or disadvantage (why Indian MPs experience notably high turnover). However, the Philippines allows us to explore how information affects dominant relative to other incumbents: dominant incumbents are common in the Philippines and in our study area.

Our results have several important implications. We provide the first direct evidence that vote-buying can be a response to higher voter expectations for incumbent performance. Information interventions that attenuate the advantages of dominant incumbents, making elections more competitive, do increase political incentives to deliver benefits to voters. However, 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. However, the results of the intervention offer a strong motivation to examine the effects of a similar intervention earlier in the electoral cycle, when incumbents have greater opportunity to react by increasing the provision of public goods.

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 ancillary predictions that we are able to test; these predictions are examined in Section 6. Section 7 concludes.

2 Literature Review

Although there is a considerable literature on vote buying, information and incumbency advantage, ours is the first paper to examine these phenomena jointly, and to show that the provision of information about what governments can do for citizens can have significant effects on vote-buying and incumbency advantage. The sections below explain in more detail our contributions to research on vote buying; information; and on the relationship between voter information and incumbent advantage.

2.1 Vote Buying

In many countries, vote-buying (gifts of goods or money before the election, aimed at persuading recipients to vote in a particular way, or to vote at all) is pervasive and pre-electoral transfers to voters are substantial. Across the 17 countries surveyed in the 2005-06 wave of the Afrobarometer survey, 19 percent of more than 20,000 respondents reported that they had been offered a gift in the last election. Brusco et al. (2009) surveyed nearly 2,000 respondents in three Argentine provinces three months after the October 2001 elections. Forty-four percent of respondents said that parties had distributed food, clothing and other items to homes in their neighborhoods. In our data, 14 percent of households (in the control group) report vote buying.¹

Ours is the first analysis that links vote buying to voter information and incumbent dominance. In contrast, a large literature focuses on enforcement, such as party machines and social networks (Brusco et al., 2009; Cruz, 2013) or norms of reciprocity (Finan and Schechter, 2012), and on the extent of leakage (voters who do not necessarily vote for the politicians who pay them, as in Schaffer (2007)). Hicken et al. (2014) show that exhortations not to buy votes interact with voter incentives to pre-commit to resist the temptation to sell their votes: voters who promise not to sell their vote are less likely to sell their vote than are voters who promise to vote their conscience even if they do sell their vote. Vicente (2014) looks at the effects of normative information on the acceptability of vote buying. Leaflets distributed to voters ahead of the 2006 elections in Sao Tome and Principe, discour-

¹Nichter (2008) observes that transactions that are commonly labelled vote buying may, instead, be turnout buying. Our analysis is consistent with either.

aging them from allowing vote buying to influence their vote, significant reduced turnout and challenger and incumbent vote shares.

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. Kitschelt (2000) concludes that vote-buying is more common in countries with non-programmatic political parties, such as the Philippines. These analyses do not address, as we do here, the effects on vote buying of voter uncertainty about the benefits that incumbents provide them, nor the heterogeneous incentives of dominant and non-dominant incumbents to engage in vote buying.

2.2 Information Provision

We examine a unique treatment, informing voters about what government could do for them. A large literature on information and electoral behavior examines other types of information, particularly on voting procedures and voting irregularities (Vicente, 2014; Aker et al., 2011); valence issues such as candidate corruption, criminal records, education and attendance at parliamentary sessions (Banerjee et al., 2011; Chong et al., 2011; Humphreys and Weinstein, 2013) and radio broadcasts that increase the demand for public services (Keefer and Khemani, 2014).

Aker et al. (2011) conducted three information interventions before the 2009 elections in Mozambique, providing treatment areas with brochures about how to vote, SMS messages about electoral problems observed in senders' local areas, or free copies of the largest newspaper in Mozambique, which included information about how to vote and how to send messages about electoral problems. All of the treatments increased voter turnout. Information about how to vote (though not about electoral irregularities) favored the incumbent party in Mozambique.

Chong et al. (2011) provided voters with information on incumbent corruption prior to local elections in Mexico. The treatment reduced turnout and the vote shares of both incumbents and challengers. Banerjee et al. (2011) distributed information to voters in India about the effort exerted by the incumbent legislator (the legislator's committee attendance, legislative activity, and spending of discre-

tionary jurisdiction development funds), as well as about the wealth, education and criminal record of the incumbent and two main challengers. They find no effects on gift giving, which is similar to vote buying in the Philippines.

As in our intervention, Fujiwara and Wantchekon (2013) also provide voters with more neutral 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.

Our information intervention emphasizes broadly-targeted spending programs in a context where targeted pre-electoral transfers (vote buying) are common. In empirical research on Benin, Keefer and Khemani (2014) find that radio broadcasts of information touting 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 Dominant Incumbents

A growing literature focuses on incumbent advantage in developing countries or new democracies. Most of the research documents a tendency for incumbency to *reduce* prospects of re-election (Uppal, 2009; Klasnja, 2014). We are not concerned with the extent of systematic incumbent (dis)advantage in the Philippines. Instead, our analysis offers the first direct test of the effects of information on the behavior of dominant incumbents – incumbents elected by a large margin and who are more likely to possess an incumbency advantage.

Information plays a prominent role in research on the sources of incumbent advantage in developing countries.² Boas and Hidalgo (2011) find that control of

²A large literature considers the sources of incumbent advantage in the United States, focusing

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. One contribution of this paper is to highlight the effects of one particular type of information, about the capacity of the incumbent to deliver public goods, on incumbent advantage.³

The results of the Banerjee et al. (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 et al. (2006) trace the electoral advantages of US congressional incumbents to the greater coverage they receive in print media. Using data from the United States, Klasnja (2011) concludes that an increase in political awareness—knowledge of basic political facts—systematically reduces support for incumbents accused of corruption.

An implication of these analyses is that incumbents should increase their per-

on issues such as name recognition, resource and staffing advantages, and better connections with campaign financiers. These are all present in the context of developing country democracies, as well. Our analysis and review of the literature are concerned with other features of developing country democracies that contribute to incumbent advantage and are more pronounced in these countries, such as imperfect information about the existence of government programs and incumbent control of the media.

³Klasnja (forthcoming) argues that incumbent advantage arises when corruption increases with incumbent tenure (because of learning or fixed costs in the establishment of corruption networks). Voters then have an incentive to replace incumbents sooner. Klasnja and Titiunik (2013) attribute incumbent disadvantage in Brazilian municipalities to term limits and the weak attachment of incumbent mayors to their parties.

formance 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 et al. (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.⁴ Hodler et al. (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. Their actions produce outcomes that are just adverse enough to encourage voters to re-elect them, since they are the candidates best placed to address the policy problem.

3 The Experiment and Data

We focus on the electoral incentives of mayoral candidates 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, such as responsibility for nutrition programs (Khemani, 2013), and transferred a large number of civil servants to municipalities (Llanto, 2012). Despite the presence of one vice-mayor and eight municipal councilors, mayors exert significant control over how municipal resources are spent (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, with the average municipality taking 85 percent of its revenues from those fixed transfers (Troland, 2014). Laws governing transfers to municipalities encour-

⁴An 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 incumbent advantage.

age municipalities to allocate 20 percent of transfers to development projects.⁵

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. 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 and tends to take place a few days before the elections (Cruz, 2013). In addition, prior research has documented that Filipino voters use retrospective voting rules when deciding whether to re-elect the incumbent (Cruz and Schneider, 2013; Labonne, 2013).

3.1 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.⁶ 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, 2013.⁷

⁵Section 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.

⁶A copy of a flyer is included as Figure A.1.

⁷The submitted documents are available at: <http://www.povertyactionlab.org/Hypothesis-Registry>

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).⁸ In the course of the interview, we gave each candidate a worksheet with a list of sectors.⁹ 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. 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 it, they called PPCRV to make sure that they would be included.¹⁰ 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.

Further evidence of candidate engagement comes from comparisons of what incumbents said they would do with the money and what they actually did with it. For incumbents we can compare how they allocated the budget while in office with what they proposed. We use budgetary data for the last full fiscal year before the election (2012) and compute the correlation between the share of the budget

⁸Ilocos 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.

⁹The 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.

¹⁰Only one candidate refused to participate. One might think that more incumbents would have refused to participate, since the brochures would increase voter knowledge of incumbent capacity to provide them with public goods. In fact, because the brochures were going to be disseminated even if incumbents chose not to have their promises included in them, it was in the incumbents' interest to participate.

spent on each sector with the share of the budget that the incumbent proposes to spend on the sector. We find that the correlation is 0.55 which we interpret as large particularly since changes in priorities and errors in budget data make a perfect correlation unlikely. In addition, this is indicative that candidates conscientiously allocated their tokens.

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).¹¹

PPCRV put together flyers comparing the proposed allocations of all candidates in each municipality.¹² 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. On average, 44.5 percent of voters resided in treated villages and 55.5 in control villages. Importantly, the flyer and the script did not mention vote buying, reducing any concerns related to social desirability bias.

In our interpretation of the findings, we emphasize that the flyer informed vot-

¹¹The 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.

¹²There were only two or three candidates in each municipality. The figures are available in Table A.2.

ers of the existence of a key government program to provide infrastructure, along with evidence of the significance of the program (the flyer, the participation of a prominent NGO, and the participation of the candidates). All of the effects that we identify are consistent with this interpretation. However, the flyer also provided voters with information about the candidates, especially who the candidates were and the allocation preferences of candidates. Neither of these can account for the results we present below. Knowledge of candidates was unaffected by the information treatment. Furthermore, all of our results are robust to controlling for differences between the allocation preferences of candidates and those of households. Indeed, consistent with our assumption, and the consensus of observers, that politicians cannot easily make credible pre-electoral promises in the Philippines, we do not observe that the treatment leads voters to vote for candidates closer to their own preferences.

Evidence that the treatment and control groups were similar prior to the intervention indicates that the randomization strategy was successful. The results in Table 1 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.¹³ 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.

3.2 Data

We use two main data sources in the paper. First, we gathered precinct-level election results from the COMELEC.¹⁴ 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. These interviews yielded

¹³This set of results is available in Table A.3-A.5.

¹⁴The data were available at: http://2013electionresults.comelec.gov.ph/res_reg0.html

the key variables that we use in the analysis. Descriptive statistics are reported in Table 2.

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 et al. (2007), we create an index aggregating the various indicators of knowledge of the campaign promises by taking the simple average of the demeaned indicators (divided by the control group standard deviation). So if K_{is} is individual i 's knowledge about sector s promises (*i.e.*, whether they correctly identified the candidate who proposed to spend the largest share of the LDF on sector s), then the knowledge index is:¹⁵

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

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.¹⁶ 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.¹⁷ They rated how important each of these was on a 0 - 4 scale, from "not important" to "very important". Respondents took flashcards,

¹⁵The formula included in the PAP contained a typo.

¹⁶Although the ordering of the six alternatives may have affected the response, the ordering was the same across treatment and control groups.

¹⁷To 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.

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.¹⁸

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 agreement index, 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.6 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 in-

¹⁸Respondents were given ballots with only ID codes corresponding to their survey instrument. The ballots contained the names and parties of the mayoral candidates in the municipality, in the same order and spelling as they appeared on the actual ballot. The respondents were instructed to select the candidate that they voted for, place the ballot in the envelope, and seal the envelope. Enumerators could not see the contents of these envelopes at any point and respondents were told that the envelopes remained sealed until they were brought to the survey firm to be encoded with the rest of the survey.

formation 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.7).

Occurrence of vote buying Observers assured us that vote buying occurs in the days before the election and that candidates and their brokers can re-target vote buying quickly.¹⁹ Hence, we expected that our information intervention, only a week before the election, might still affect vote buying. We measure vote buying through a series of questions asking whether respondents were aware of any case of vote buying in their village and if, during the recent election, someone offered them money for their vote. In the Philippines, social desirability bias associated with vote buying is low and responses to direct questions provide credible estimates of vote buying incidence. In Isabela, a province near our study area, Khemani (2013) uses vote buying estimates from direct questions. Further, Cruz (2013) 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.²⁰

¹⁹Stokes et al. (2013) argue that brokers, who are local, are given resources by the candidate to ensure a certain level of support for the candidate. They seek to retain some of these resources as rents for themselves, but rapidly disburse when they observe an information shock that reduces support for their candidate.

²⁰In the pre-analysis plan, we indicated that we intended to also use an unmatched count technique to assess 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 Knowledge and Vote Buying

Two direct effects of the experiment are of particular interest: did the information treatment in fact increase relevant political knowledge? And did it influence vote buying? The results in this Section first verify the essential assumption that treated voters are indeed more informed 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 Increase Knowledge?

The descriptive statistics reported in Table 2 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 knowledge.

To test the validity of the assumption that the treatment affected relevant political knowledge, we estimate the intervention’s impacts on knowledge of candidates’ promises using regressions of the form:

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

where Y_{ijk} is the knowledge index for individual i in village j in pair k , T_j is a dummy equal to one if the campaign was implemented in village j , v_k is a pair-specific unobservable and, u_{ijk} is the usual idiosyncratic error term. To account for the way the randomization was carried out, standard errors are clustered at the village level. 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 3.²¹ Supporting the strength of our randomization strategy and the balance between treated and control groups, the point estimates are essentially constant across the four different specifications though the standard errors get smaller as we include more fixed-effects and additional control variables. As is the case with a number of other outcome variables, the fixed effects explain a large share of the variation in voter knowledge.

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. Among the exceptions, such as Banerjee et al. (2011), researchers have tested 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 elect. In particular, we are interested in the treatment’s effect on the salience of local development spending on vote decisions. We estimate:

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

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

As expected 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 Information 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:

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

²¹The treatment had no effect on dimensions of political knowledge not included in the flyers (Table A.8).

where Y_{jk} is the prevalence of vote buying in village j in pair k during the May 2013 elections.²² The set-up is equivalent to the one used for equation (1).

Importantly, as described above, vote buying tends to take place a few days before the elections and so, even though our intervention was rolled out shortly before the elections, candidates had sufficient time to adjust their campaigning strategies. Results presented in Panels A and B of Table 5 indicate that vote buying intensified in treated villages.²³

In Panel A, the outcome variable is the share of respondents who were aware of instances of vote buying in their village. This is an imprecise measure of vote buying, since voters have incomplete information about whether their 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 used 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).²⁴

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

²²Recall that, as indicated in the PAP, we run those regressions at the village-level. We obtain similar results if we run those regressions at the individual-level instead (Table A.9).

²³The 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 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 increased incumbent incentives to improve voter welfare.

²⁴A 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.10).

Section 6 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. Retrospective voting models emphasize the key insight that vote buying is linked to the absence of credible commitment, but assume that politicians cannot make credible commitments to any voters. Unlike probabilistic voting models, though, retrospective models are a natural vehicle for understanding voter uncertainty about the benefits that incumbents provide them. Challenger characteristics are irrelevant (since challengers cannot credibly commit to pursue different policies than the incumbent), but voter beliefs about incumbent performance are central.

Retrospective voting models provide a natural vehicle for illustrating the effects of our intervention, given that the treatment was intended to affect voter beliefs about what the incumbent could do for them. The 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 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 . 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 . 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 the public goods that incumbents could provide and about which they were previously unaware. Analytically, this information could be modeled as shifting voter beliefs either about the ability of government to finance services of any kind - the government budget constraint - or about the relative costs to government of turning budgetary resources into public goods versus transfers. Since the intervention informed voters only about the existence of an important program to finance infrastructure, the anal-

ysis below adopts the second approach. However, a model that adopts the first approach yields the same conclusions that we report below.²⁵

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

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

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

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

The welfare of voters in (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.

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

²⁵For beliefs about the size of the government budget to matter, voters must believe that the participation constraint of the incumbent binds when they choose the performance threshold. Conditional on this, the incumbent provides the public goods consistent with the performance threshold; when voters are unexpectedly informed that the budget is larger than they thought, support for the incumbent drops unless the incumbent makes targeted transfers to some voters. The identity of those voters is established in Proposition 2, below—the conclusions of the proposition are independent of whether the information shock affects voter beliefs about the budget or about the relative costs of providing public goods versus transfers.

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

Our intervention is equivalent to an unexpected shock that shifts this distribution for a randomly-selected fraction δ of all voters, $\delta \leq 1$ (approximately 44.5 percent in the case of our treatment). Incumbents know which voters are subject to the shock, but beyond that only know that the distribution of beliefs about the costs of producing public goods follows $\theta'_i \sim [1, 2\theta'_c - 1]$, where $\theta'_c = \theta_c + k(\bar{\theta} - \theta_c)$, $k \sim [-1, 1]$. 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.²⁶

²⁶We abstract from anticipated information shocks. Their inclusion would complicate the analysis, but not change the key results. An anticipated shock would take the form of some random variable z that would change the cost parameter in the distribution of beliefs according to $(\theta_c + z(\bar{\theta} - \theta_c))$. As in standard retrospective voting models, voters and incumbents would be aware of the distribution of z and anticipate the possibility of the shock in the construction of their performance thresholds and decisions regarding public goods. Again, however, once equilibrium public goods are provided and the unanticipated shock occurs, the dynamics of vote buying remain the same: in the event of negative shocks (that reduce voter beliefs about the costs of providing pub-

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 $\bar{\omega}_i \geq H(g_{\theta'})$, where $g_{\theta'}$ is given by $H_g(g_{\theta'}) = \frac{2\theta'_i}{N}$.²⁷

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 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 and voters observe the distribution of beliefs about the costs of public good provision, $\theta_i \sim [1, 2\theta_c - 1]$, that voters will have before the election.
2. Voters coordinate on a voting rule $\hat{\omega} = \omega(g_i)$, where g_i is given by $H_g(g_i) = \frac{2\theta_i}{N}$.
3. Incumbents choose the level of public good provision g .²⁸

lic goods), as long as they are not too large, the incumbent buys votes from the voters who were subjected to the shocks.

²⁷In 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.

²⁸First, 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.

4. A randomly-selected subset of all voters $\delta \leq 1$ are subject to an unanticipated shock k to the distribution of their beliefs about the costs of producing public goods, such that for these voters $\theta'_i \sim [1, 2\theta'_c - 1]$, where $\theta'_c = \theta_c + k(\bar{\theta} - \theta_c)$, $k \sim [-1, 1]$.
5. Incumbents choose the level of spending on transfers to voters.
6. Voters' individual beliefs about the costs of public good provision are revealed to them.
7. The election takes place.

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

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

Proof: See technical appendix.

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

Lemma 1 *After a positive unanticipated information shock, $k(\bar{\theta} - \theta_c) > 0$, increasing the beliefs among a fraction of voters δ about the costs of providing public goods, the public goods provided by the incumbent meet the performance threshold of more than half of the voters. After a negative unanticipated information shock affecting a fraction of voters δ , $k(\bar{\theta} - \theta_c) < 0$, public good provision meets the threshold of less than half of the voters.*

Proof: See technical appendix.

Proposition 2 describes the incumbent response to an information shock. In particular, if the shock is adverse (it tells voters that it is less expensive to provide

public goods than the incumbent anticipated), incumbents have an incentive to target those affected by the shock rather than others, targeting a larger fraction of those voters when their information advantage is larger.

Proposition 2 *After a positive unanticipated information shock, $k(\bar{\theta} - \theta_c) > 0$, there is no change in public policy. In the event of a negative unanticipated information shock, incumbents target transfers $f_k = H(g_{\theta_c+k}) - H(g_{\theta_c})$ to a fraction α of voters in δ who received the information shock, where α is given by $\alpha^* = \frac{M - \bar{\theta}g_{\theta_c} + R + l\delta f_k}{\delta f_k}$, $l = \frac{1}{2} \left(\frac{k(\bar{\theta} - \theta_c)}{\theta_c + k(\bar{\theta} - \theta_c) - 1} \right)$*

Proof: See technical appendix.

The model accounts for the puzzling result from Section 4, that more information can trigger greater vote buying: voters who receive more accurate information about the public goods the incumbent *could* have provided raise their performance threshold, in accordance with the voting rule. Since incumbents cannot adjust the provision of public goods in time for the election, they respond to the higher threshold with vote buying. This result is only possible if public good spending begins substantially before the election, while transfers can be made right before the elections. Evidence from the Philippines, discussed earlier, supports these assumptions.

Section 6 offers evidence for other assumptions and ancillary predictions of the model. First, from our assumption that politicians cannot make credible commitments, following the logic of retrospective voting models, voters care most about incumbent characteristics and performance. We should therefore observe stronger information effects with respect to incumbents than to challengers on voter knowledge of candidates and on candidate vote buying. These stronger effects are found in the data.

Second, we assume that the information advantage of incumbents emerges from the degree to which voter beliefs about the costs of providing public goods are higher than the actual costs. Especially in the Philippines, it is reasonable that dominant incumbents might control the information reaching voters in their jurisdictions. For example, journalists in the Philippines are subject to significant pressure relative to other countries. Freedom House (2014) report on press freedom rates the Philippines as only partly free, with journalists the target of libel

suits by local politicians, harassment and assassination attempts (many successful). The term $k(\bar{\theta} - \theta_c)$ embeds our assumption that when the difference between voter beliefs and the actual costs of public good provision is greater, as with dominant incumbents, information shocks should have a larger impact on voter beliefs. This is consistent with the evidence below.

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

6 Results: Mechanisms and Alternative Explanations

This Section systematically explores the heterogeneous effects of information on voter knowledge and vote buying, comparing incumbents and challengers and dominant and non-dominant incumbents.

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

Our interpretation of the effects of the information shock, formalized in the model, implies that incumbents should be more affected than challengers. First, incumbents, but not challengers, should exhibit significantly fewer information advantages among informed than among uninformed voters. Second, incumbents, but again, not challengers, should react to the information shock with greater vote buying. The data are consistent with both of these predictions.

To see the contrasting effects on voter knowledge, 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. The results displayed 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).²⁹

By assumption, the model predicts that challengers do not react to the information shock. The information effects on respondent knowledge 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 did not ask respondents which candidates bought their votes because this would have been too sensitive.

However, we implemented a secret ballot to find out from respondents whom they voted for. If the information treatment primarily affected incumbents, then we would expect treated areas to express less support for the incumbent, but those whose votes were bought should express more support for the incumbent. These effects would not emerge if the treatment affected both incumbents and challengers, or only challengers. We therefore estimate equation (1), where Y_{ijk} is a dummy equal to one if the respondent declared voting for the incumbent. We con-

²⁹Importantly, as reported in Table A.11, incumbent and challenger promises do not seem to differ systematically, which could explain those results.

trol for treatment status, whether the individual was targeted for vote buying, and their interaction. The results, in Table 7, are consistent with our argument that the information provided to voters unexpectedly raised their performance threshold for incumbents and that incumbents reacted by buying 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.

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

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 2010.³⁰ 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.³¹ 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 in municipalities with dominant incumbents (Table 8). 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.

The model also predicts that the intervention should have larger effects on vote

³⁰Since the theory predicts that dominant incumbents provide fewer public goods than non-dominant incumbents, a natural empirical exercise would be to compare effort across these two incumbent classes with respect to spending out of the Local Development Fund. This would require spending data across the different municipalities, but also data on the quality of the spending (the actual projects that were implemented). We hope to undertake this significant data-gathering exercise in the future.

³¹Results are similar when using vote share defined as a percentage of the voting population (Table A.12).

buying in incumbent-dominated villages and municipalities.³² 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 incumbents' 2010 vote share increases the impact of the intervention on vote buying by more than 10 percent.

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 et al. (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).³³

6.3 Robustness Checks and Alternative Explanations

We contend that our main results and heterogeneous 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 political knowledge (the intervention has an unfavorable effect on knowledge 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 interven-

³²This 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 et al., 2014).

³³These two hypotheses were also included in the PAP and are reported in Table A.13. 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.

tion 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 8: 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 better quality and therefore better able to react to the intervention. Again, this explanation does not account for the disproportionate effects of the intervention on political knowledge 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 knowledge about in-

cumbents 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 knowledge 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: Balance Tests

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

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

Table 2: Descriptive Statistics

	Treatment (1)	Control (2)
Know promises	0.051 (0.501)	0.000 (0.470)
Know politicians	0.034 (0.544)	0.009 (0.573)
Salience sectors	2.458 (1.470)	2.300 (1.513)
Salience sectors (adjusted)	0.884 (1.188)	0.776 (1.207)
Error incumbent	4.763 (3.080)	4.974 (3.110)
Error challenger	2.059 (3.017)	2.084 (3.111)
Relative preference	12.597 (12.101)	12.529 (11.663)
Turnout (self-reported)	0.965 (0.183)	0.965 (0.184)
Vote Buying	0.165 (0.371)	0.138 (0.345)

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

Table 3: Effects of Treatment on: Knowledge of Promises

	(1)	(2)	(3)	(4)
Treat	0.051 (0.036)	0.051** (0.022)	0.051*** (0.015)	0.052*** (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 (0.029)	0.033 (0.023)	0.033 (0.023)	0.030 (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 (0.021)	0.034** (0.016)	0.034** (0.016)	0.043*** (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-Favoring Errors				
Treat	-0.211 (0.281)	-0.211* (0.120)	-0.211** (0.089)	-0.218** (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-Favoring Errors				
Treat	-0.0250 (0.216)	-0.0250 (0.177)	-0.0250 (0.118)	-0.0260 (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: Effects of Treatment on : Self-reported vote for the incumbent

	(1)	(2)	(3)	(4)
Treat	-0.030 (0.036)	-0.041** (0.019)	-0.041** (0.019)	-0.041** (0.019)
Vote-buying	-0.156*** (0.037)	-0.001 (0.034)	-0.001 (0.034)	0.004 (0.034)
Interaction	0.043 (0.056)	0.039 (0.049)	0.039 (0.049)	0.032 (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 8: Heterogeneity

	Know Promises (1)	Errors Incumbent (2)	Challenger (3)	Salience (4)	Vote Buying (5)
Panel A: 2010 Incumbent Vote Share (Municipal)					
Treat	0.051*** (0.015)	-0.211** (0.088)	-0.025 (0.118)	0.109** (0.044)	0.034** (0.015)
Interaction	0.007*** (0.001)	-0.024*** (0.008)	0.002 (0.012)	0.006 (0.004)	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*** (0.015)	-0.212** (0.090)	-0.039 (0.117)	0.104** (0.043)	0.035** (0.015)
Interaction	0.006*** (0.001)	-0.019*** (0.006)	0.000 (0.009)	0.009** (0.004)	0.003** (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 9: Alternative Channels

	Know Promises (1)	Errors Incumbent (2)	Challenger (3)	Salience (4)	Vote Buying (5)
Panel A: Controlling for Overlap between Candidate Promises					
Treat	0.050*** (0.015)	-0.209** (0.088)	-0.025 (0.118)	0.109** (0.044)	0.034** (0.015)
Treat*Strongholds	0.007*** (0.001)	-0.024*** (0.007)	0.003 (0.012)	0.006 (0.004)	0.004*** (0.001)
Observations	3,408	3,408	3,408	3,346	284
R-squared	0.330	0.528	0.253	0.090	0.743
Panel B: Controlling for Incumbent Quality					
Treat	0.050*** (0.015)	-0.210** (0.088)	-0.023 (0.117)	0.109** (0.044)	0.034** (0.015)
Treat*Strongholds	0.007*** (0.002)	-0.028*** (0.008)	0.002 (0.013)	0.006 (0.005)	0.003** (0.002)
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.

Technical Appendix

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

Proof of Proposition 1

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

Proof of Lemma 1

Based on the performance threshold $\bar{\omega} = H(g_{\theta_c})$ incumbents provided g_{θ_c} . In the event of an unanticipated shock, a fraction δ of voters have beliefs distributed according to $\theta'_i \sim [1, 2\theta'_c - 1]$, where $\theta'_c = \theta_c + k(\bar{\theta} - \theta_c)$, and the remaining $(1 - \delta)$ voters have beliefs distributed as before, $\theta_i \sim [1, 2\theta_c - 1]$. Therefore, one-half of the

voters who were not subjected to the information shock, given by $\frac{1}{2}(1 - \delta) < \frac{1}{2}$, are expected to conclude that the incumbent met their performance threshold, as before.

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

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

Proof of Proposition 2

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

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

Recalling that $k(\bar{\theta} - \theta_c)$ is less than zero, $-\frac{1}{2} \left(\frac{k(\bar{\theta} - \theta_c)}{\theta_c + k(\bar{\theta} - \theta_c) - 1} \right)$ is the fraction of voters in the group δ that received the information shock and would be "persuaded" by a transfer f_k . Other voters in δ either already support the incumbent or are sufficiently hostile to the incumbent that they would not be persuaded by the transfer. The fraction of voters in the group not exposed to the shock and that would be equally persuadable by the transfer f_k is given by $-\frac{1}{2} \left(\frac{k(\bar{\theta} - \theta_c)}{\theta_c - 1} \right)$. Since $(\theta_c - 1) > (\theta_c + k(\bar{\theta} - \theta_c) - 1)$ for $k(\bar{\theta} - \theta_c) < 0$, the probability that a transfer will reach a persuadable voter is greater if it is targeted to voters in the group δ .³⁴

³⁴The intuition is straightforward. Incumbents would like to target transfers to voters for whom the distribution of voter beliefs is most dense around the median: these are the most persuadable voters. An information shock that tells voters that the maximum costs of providing public goods

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

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

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

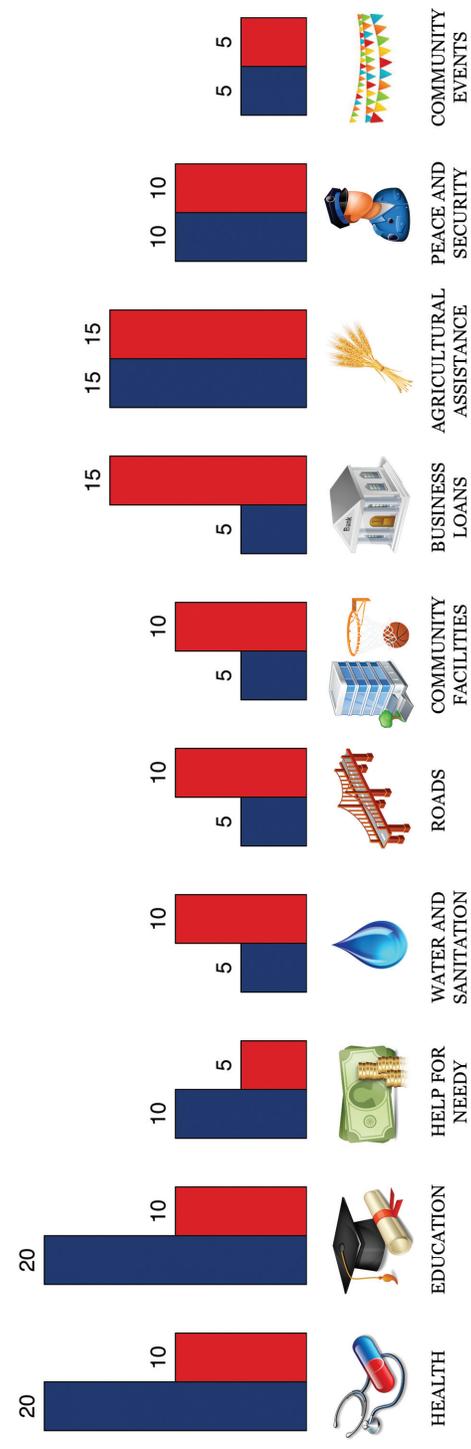
Table A.2: List of Intervention Municipalities

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...*

1. HERNANDO, ANGEL MIGUEL
 2. VALDEZ, MELANIE



A.2

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

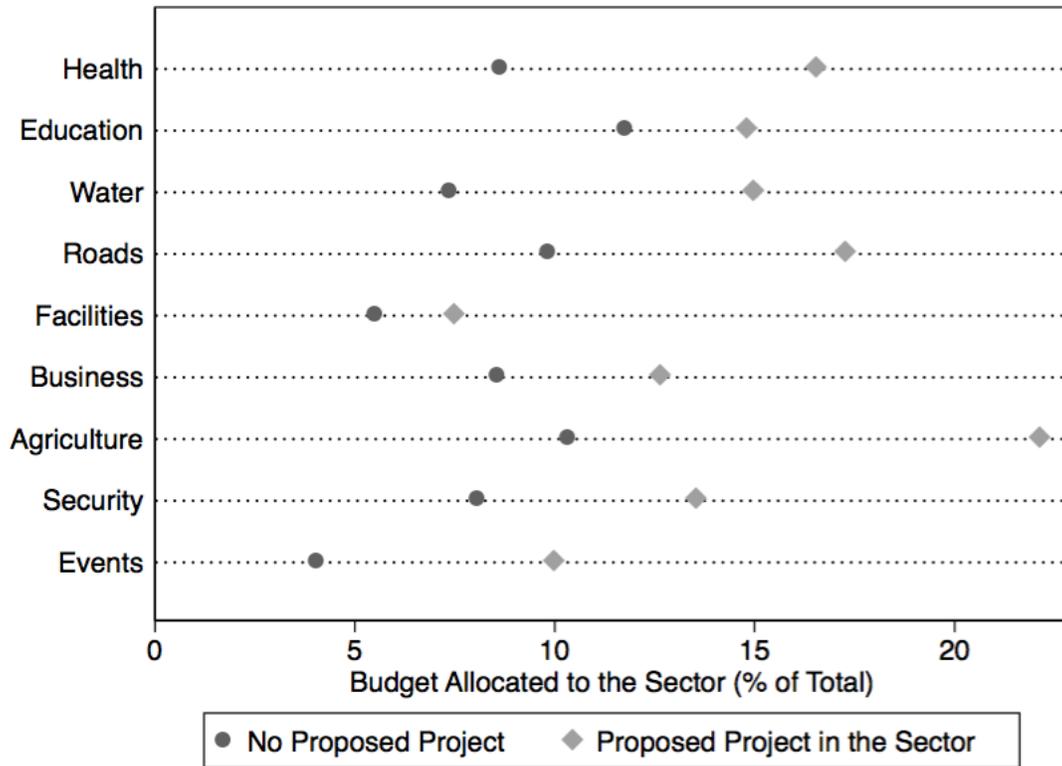


Figure A.2: Candidate Proposed Projects and Budget Allocations

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

	Treatment (1)	Control (2)	T-test (3)	K-Smirnov test (4)	OLS (5)
Panel A: Sectoral Preferences					
Health	18.915 (12.834)	18.714 (13.094)	-0.453 [0.651]	0.012 [1.000]	0.204 [0.641]
Education	17.378 (12.527)	18.048 (12.993)	1.530 [0.126]	0.021 [0.828]	-0.665 [0.124]
Help for Needy	8.086 (9.610)	8.162 (9.742)	0.228 [0.820]	0.012 [1.000]	-0.077 [0.814]
Water and Sanitation	7.822 (7.989)	8.500 (8.731)	2.363 [0.018]	0.036 [0.213]	-0.675 [0.017]
Roads	7.340 (9.035)	6.541 (7.587)	-2.792 [0.005]	0.050 [0.028]	0.791 [0.004]
Community Facilities	4.877 (6.413)	4.689 (5.872)	-0.891 [0.373]	0.021 [0.851]	0.186 [0.366]
Business Loans	5.802 (9.917)	5.637 (9.538)	-0.496 [0.620]	0.016 [0.982]	0.161 [0.624]
Agricultural Assistance	21.517 (18.078)	21.947 (18.249)	0.690 [0.490]	0.025 [0.644]	-0.422 [0.482]
Peace and Security	5.473 (6.662)	5.197 (6.363)	-1.238 [0.216]	0.018 [0.934]	0.272 [0.213]
Community Events	2.790 (4.771)	2.566 (4.859)	-1.354 [0.176]	0.023 [0.759]	0.223 [0.172]
Panel B: Alignment					
Alignment	58.396 (19.418)	58.267 (19.674)	-0.285 [0.776]	0.011 [0.984]	0.128 [0.764]
Alignment (challenger)	57.168 (18.562)	57.063 (18.557)	-0.182 [0.856]	0.015 [0.977]	0.105 [0.849]
Alignment (incumbent)	59.883 (20.314)	59.727 (20.861)	-0.220 [0.826]	0.031 [0.365]	0.156 [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 (1)	Control (2)	T-test (3)	K-Smirnov test (4)	OLS (5)
Length of residence	40.693 (19.069)	40.390 (19.028)	-0.464 [0.643]	0.018 [0.941]	0.303 [0.639]
Family size	5.022 (2.217)	5.096 (2.045)	1.012 [0.312]	0.032 [0.321]	-0.074 [0.297]
Female	0.490 (0.500)	0.492 (0.500)	0.137 [0.891]	0.002 [1.000]	-0.002 [0.889]
Age	49.361 (13.537)	48.853 (13.374)	-1.103 [0.270]	0.034 [0.262]	0.505 [0.269]
Education (years)	9.462 (3.468)	9.353 (3.422)	-0.930 [0.353]	0.039 [0.134]	0.110 [0.345]
Remittances abroad	0.225 (0.418)	0.244 (0.430)	1.334 [0.182]	0.019 [0.899]	-0.019 [0.175]
CCT Beneficiary	0.154 (0.361)	0.159 (0.366)	0.377 [0.706]	0.005 [1.000]	-0.005 [0.700]
Group Member	0.671 (0.470)	0.644 (0.479)	-1.697 [0.090]	0.028 [0.518]	0.028 [0.077]
Barangay assembly	0.925 (0.263)	0.936 (0.245)	1.214 [0.225]	0.011 [1.000]	-0.011 [0.213]
Collective Action	0.736 (0.441)	0.767 (0.423)	2.102 [0.036]	0.031 [0.365]	-0.031 [0.026]
Religion: never	0.085 (0.279)	0.087 (0.282)	0.183 [0.855]	0.002 [1.000]	-0.002 [0.853]
Religion: weekly	0.373 (0.484)	0.363 (0.481)	-0.568 [0.570]	0.009 [1.000]	0.009 [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 (1)	Control (2)	T-test (3)	K-Smirnov test (4)	OLS (5)
Electricity	0.968 (0.177)	0.973 (0.162)	0.909 [0.363]	0.005 [1.000]	-0.005 [0.358]
Radio	0.735 (0.442)	0.742 (0.437)	0.507 [0.612]	0.008 [1.000]	-0.008 [0.605]
Television	0.877 (0.328)	0.876 (0.329)	-0.104 [0.917]	0.001 [1.000]	0.001 [0.915]
Phone	0.890 (0.313)	0.905 (0.293)	1.412 [0.158]	0.015 [0.992]	-0.015 [0.154]
wash mach	0.326 (0.469)	0.346 (0.476)	1.269 [0.204]	0.021 [0.855]	-0.021 [0.197]
Fridge	0.529 (0.499)	0.542 (0.498)	0.755 [0.450]	0.013 [0.999]	-0.013 [0.443]
Gas stove	0.603 (0.489)	0.614 (0.487)	0.632 [0.528]	0.011 [1.000]	-0.011 [0.515]
Bicycle	0.406 (0.491)	0.390 (0.488)	-0.945 [0.345]	0.016 [0.981]	0.016 [0.336]
Boat	0.023 (0.151)	0.026 (0.160)	0.549 [0.583]	0.003 [1.000]	-0.003 [0.558]
Motorcycle	0.535 (0.499)	0.549 (0.498)	0.825 [0.409]	0.014 [0.995]	-0.014 [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: Correlates of Sectoral Preferences

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

Table A.7: Effects of Treatment on : self-reported support for candidate

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

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

Table A.8: Effects of Treatment on : Knowledge of Politicians and Candidates

	(1)	(2)	(3)	(4)
Panel A: Knowledge of Local Politicians				
Treat	0.025 (0.040)	0.027 (0.030)	0.029 (0.020)	0.029 (0.020)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	Yes
Additional Controls	No	No	No	Yes
Observations	3,187	3,187	3,187	3,187
R-squared	0.001	0.160	0.277	0.278
Panel B: Knowledge of Candidates				
Treat	-0.022 (0.040)	-0.022 (0.027)	-0.022 (0.020)	-0.024 (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.9: Effects of Treatment on : vote buying (individual-level)

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

Notes: Results from individual-level regressions. In Panel A, the dependent variable is a dummy equal to one if the respondent indicated being aware of instances of vote buying in her village. In Panel B, the dependent variable is a dummy equal to one if the respondent indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'missing']. In Column 4, the regression includes a dummy equal to one if someone in the household is a member of any group, 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.10: Effects of Treatment on : vote buying

	(1)	(2)	(3)	(4)
Panel A: Did Someone Offered you Money for your Vote ? [Alternative Coding]				
Treat	0.038* (0.021)	0.038** (0.016)	0.038** (0.016)	0.047*** (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.11: Comparing Incumbent and Challenger Promises

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

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

Table A.12: Heterogeneity

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

Notes: Results from individual-level (Columns 1-4) and village-level (Column 5) regressions. All regressions 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.13: Further Results

	(1)	(2)	(3)	(4)
Panel A: Turnout				
Treat	0.034 (0.729)	-0.109 (0.506)	-0.125 (0.510)	0.330 (0.558)
Relative Preference	0.075 (0.047)	0.045 (0.075)	0.151 (0.121)	0.237* (0.126)
Interaction	-0.053 (0.071)	-0.053 (0.051)	-0.072 (0.053)	-0.076 (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 (0.312)	0.169 (0.312)	0.229 (0.304)	
Alignment	0.003 (0.041)	0.003 (0.041)	-0.015 (0.066)	-0.087 (0.288)
Interaction	-0.056 (0.066)	-0.056 (0.066)	-0.080 (0.098)	-0.276 (0.264)
Municipal Fixed-Effects	No	Yes	No	No
Pair Fixed-Effects	No	No	Yes	No
Barangay Fixed-Effects	No	No	No	Yes
Observations	689	689	689	689
R-squared	0.860	0.860	0.864	0.873

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