# Buying Informed Voters:
# New Effects of Information on Voters and Candidates*

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**Abstract**

A theoretical model and two experiments in the Philippines show that information about the mere existence of government programs influences both voter *and* candidate behavior. Theory predicts that incumbents shirk when voters are unaware of programs. Consistent with this, in the survey experiment, information indicating the availability of municipal development funds significantly reduces support for incumbent mayors. The field experiment distributed similar information to voters prior to municipal elections, with the full knowledge of candidates. Incumbent mayors increased vote buying in treatment areas to counteract the decrease in voter support. Effects were strongest in villages with fewer incumbent-provided public goods.

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In many developing countries, politicians engage in clientelistic practices rather than providing public goods.[1] One explanation for this equilibrium is information asymmetry between voters and politicians: politicians have no incentive to provide public goods in political environments where voters are unable to assess or reward their performance (see, e.g., Besley and Burgess 2002). Consequently, politicians have incentives to pursue clientelism instead of campaigning on policies and promises (see, e.g., Keefer and Vlaicu 2017; Kitschelt and Wilkinson 2007). We make two contributions to the large literature on this issue. First, we show that voters' ignorance goes beyond their ability to observe politician effort on their behalf (an information asymmetry explored by Gottlieb 2016; Banerjee et al. 2018; Cruz and Schneider 2017; Labonne 2013): it extends to a lack of knowledge about the very policy instruments on which politicians could exert effort. Second, we take advantage of our ability, rare in the literature, to monitor how politicians react to information shocks. This allows us to propose an alternative reason for the seemingly modest or mixed effects of interventions that inform voters about politician characteristics and effort: politicians can counteract the electoral effects of information shocks in ways that researchers may be unable to observe.

We begin with a simple model of retrospective voting in which voters have incomplete information about what politicians can do for them. Incumbents have limited incentives to provide public goods because voters do not know that incumbents have resources to provide them. Voters who are informed that these resources exist are consequently disappointed by incumbents' performance and reduce their electoral support for them. Incumbents, however, react to voter disappointment by buying additional votes, thereby offsetting the electoral consequences of the information shock.

We test these predictions by combining both survey and field experiments, allowing us to assess both the direct voter effects and subsequent politician response to an information shock. We use data from the Philippines, where municipalities are responsible for implementing public goods using a large fund provided by the central government, the Local Development Fund (LDF).

The survey experiment allows us to establish our key first result, that information about the existence of the LDF negatively affects support for the incumbent. Treated respondents received a flyer information about the LDF. At the end of the survey, we asked all respondents whether they would support the incumbent in the next election. Respondents who received the flyer were significantly less likely to report that they would support the incumbent in the next election.

[1] For a comprehensive overview of clientelism, please see Hicken (2011).

Our second set of results, from the field experiment, concern incumbent response to this information. We implemented the experiment in 284 villages in 12 municipalities ahead of the May 2013 municipal elections. Voters in randomly selected villages received a flyer with information about the existence and scope of the LDF. To increase the salience of the flyer for both candidates and voters, the flyer also included candidates' intended allocations out of the LDF. Incumbents bought more votes among treated voters prior to the election. Consistent with the theory, the intervention had no effect on either turnout or incumbent vote share.

Last, just as the theory suggests, the effects of the information – both in the survey and field experiments - are greatest in villages where respondents report fewer municipality-provided infrastructure projects. Those are precisely the villages where voters have greatest reason to be disappointed in the incumbent's performance.

This study, and research with a different focus by Grossman and Michelitch (2018), Bidwell, Casey and Glennerster (2015) and Banerjee et al. (2018), are the first to examine the reaction of politicians to information programs directed at voters. Understanding these reactions is key, since the electoral effects of voter information are mediated by politician responses to it. In addition, the information we provide is both new to empirical research and central to the analysis of elections. Typically, analyses of electoral competition assume that voters know the policy instruments of government and politician intentions regarding those policies. We show that this assumption does not necessarily hold and that the voter's challenge is not only to assess whether governments have implemented the policies they prefer, but even to know what those policies are in the first place.

This paper extends the literature on information and electoral accountability by focusing precisely on a previously unexplored information asymmetry- knowledge of the existence of government programs-and by examining politician reactions to information shocks. An extensive literature has investigated the effects of informing voters about past performance or attributes of politicians. Among more recent contributions, Kendall, Nannicini and Trebbi (2015), Ferraz and Finan (2008), Gottlieb (2016) and Banerjee et al. (2011) find significant electoral effects while Humphreys and Weinstein (2013); Chong et al. (2015) and Larreguy, Marshall and Snyder Jr (2015) do not.[2] Dunning et al. (forthcoming) report on the results of a large research

[2]The treatment closest to ours is Gottlieb's (2016) experiment providing voters with information about local government capacity and responsibilities, although hers differs in also providing information about the relative performance of candidates. Chong et al. (2015) do not look at vote buying, but like us distributed flyers that implicitly informed voters of the existence of a public infrastructure program. However, the flyers also contained additional performance information that is absent from our treatment.

effort to conduct similar information interventions in different countries, with mixed results. One potential explanation for these mixed results is that researchers could not easily observe effects on candidate behavior, which Pande (2011) targeted as a priority area of future research.

Grossman and Michelitch (2018), Bidwell, Casey and Glennerster (2015) and Banerjee et al. (2018) are the only other research efforts with which we are familiar that have answered the call in Pande (2011) for more research on politician reactions to electoral interventions. Grossman and Michelitch (2018) provide information about legislator performance early in the term and show that politicians in competitive constituencies subsequently improve their performance. Bidwell, Casey and Glennerster (2015) examines the effects of voter exposure to candidate debates in Sierra Leone. Treated voters expressed policy preferences more aligned with those of their favorite candidate and were more likely to vote for the candidate who performed best during the debates. Debates significantly increased reported vote buying by the third party candidates, from less than one percent of voters to 1.3 percent, an effect driven by the most closely contested debates. Banerjee et al. (2018) organizes a voter awareness campaign that informed voters of their village leaders' role in implementing a large scale public works program in India. As a result of the intervention, worse-performing incumbents decided not to run, effects that persisted until the next village election.

The paper also contributes to the literature on vote buying, the pre-electoral provision of goods or money in exchange for electoral support (Hicken et al., 2018; Schaffer and Schedler, 2007; Schaffer and Baker, 2015). The practice is pervasive and can entail large transfers to voters. Numerous papers examine the effects on vote buying of diverse information treatments (Hicken et al., 2018; Vicente, 2014; Aker, Collier and Vicente, 2017; Fujiwara and Wantchekon, 2013), but none examine the impact on vote buying of voters' information about what politicians can do for them.

In the remainder of the paper, Section 2 presents a retrospective voting model of political competition where candidates cannot make credible pre-electoral promises. Section 3 describes municipal elections in the Philippines. Section 4 details the results of a survey experiment that isolates the effect of our information treatment on voter support for politicians. Section 5 presents the field experiment. Sections 6 and 7 report the treatment effects on salience and vote buying, respectively.

# 2 Information, Government Resources, and Vote Buying

The literature focuses on the need for information in order to assess politician performance: what politicians have previously done; and what politicians propose to do in the future. However, before voters can make use of information to assess politician performance, they need to know what politicians could potentially do for them in the first place. This is especially problematic in decentralized settings, where voters face multiple government actors at different levels and may be ignorant of the responsibilities and resources of each (see, e.g., Cruz and Schneider 2017; Labonne 2013). Nevertheless, the incomplete information of voters regarding what governments can do for them has received little attention.

At the same time, the effects of interventions that reduce this information asymmetry also depend on politician reactions to it. In the long run, reducing the asymmetry can reverse politician underperformance with respect to public good provision. However, in the short run, when politicians have little ability to change public good provision, they may respond with increased vote buying. Short run reactions matter, since many interventions take the form of one-shot treatments prior to elections. Politicians therefore react with strategies that are more readily implemented in a short period of time and may be difficult to observe, such as clientelism. Empirical studies of the effects of information on electoral outcomes can lead to null results or understated effects when they are unable to account for politician responses.

We develop a formal model to examine the effects of a shock to voter information about what government can do for citizens. Our specific intervention consists of a flyer that informed voters of the existence of a key government program to fund local development projects. Hence, we formalize the argument that, taking advantage of voter ignorance of the program, incumbents shirked in implementing it, leading to voter disappointment when voter information increased. To offset this disappointment, incumbents engaged in greater vote buying prior to the election.

## 2.1 Program information and retrospective voting

Reflecting the inability of mayoral candidates to make credible commitments regarding post-electoral policies, we adopt a retrospective voting framework: voters establish a performance threshold for incumbents that determines whether they will vote for her. If the threshold is too high, incumbents make no effort to deliver benefits to voters and, instead, maximize private rent-seeking. If the threshold is too low, voters extract fewer benefits from the incumbent than they could have. Assuming that voters can spontaneously coordinate on this threshold, as in Ferejohn (1986) and Persson and Tabellini (2000), their challenge in setting the threshold is uncertainty about the welfare that the incumbent could have potentially delivered. Voters' incomplete information makes it difficult for them to distinguish incumbent shirking from an unfavorable state of the world that would prevent any incumbent from improving welfare.[3]

This analytical approach is consistent with two key features of politics in many developing countries, and mayoral elections in the Philippines in particular. First, political competition does not center on policy promises, which are not credible. Hence challengers do not matter, and voters base their decisions only on whether incumbent performance meets the threshold voters have set. Second, mayors are the dominant decision makers in municipal government and voters should hold mayors accountable for their spending.

## 2.2 Basic Set-Up

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

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

[3]Challengers are absent from retrospective voting models, since the key parameter affecting voter choice is incumbent performance, which challengers cannot affect. In principle, challengers could exploit negative information shocks about incumbent performance by increasing vote buying in areas affected by the shock, but these reactions are driven by the same mechanism and operate in the same direction.

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

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

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

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

We add two features to this standard set-up. First, we introduce an information shock that affects voter knowledge of what government can do for them. In the field experiment, voters receive information about the existence of a government program about which they were previously ignorant. Such an information shock can be modeled in two equivalent ways. First, voters could be uninformed about the government budget constraint. Second, voters could be ignorant about how much it costs government to procure projects. An information shock that reveals that government can do more for voters than they thought would, in the first case, simply tell them that the government has more money than voters thought. In the second case, it would tell that that the government can implement projects more cheaply than voters thought. We adopt the second approach, which yields a continuous relationship between the amount that actual costs exceed expected costs and the probability of supporting the incumbent.[4]

Specifically, 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 $\theta_c$. The ability to produce is never less than one - it can never cost less than $g$ to produce $g$.

An intervention that changes voters' beliefs about what government can do more for them is equivalent to an unexpected shock that shifts this distribution for a randomly-selected fraction

[4]The first approach generates the same general conclusions, but less elegantly, since we observe changes in behavior only for corner solutions, when the difference between the actual and expected budget exceeds a certain threshold.

$\delta$ of all voters, $\delta \leq 1$.[5] 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 $\theta_c' = \theta_c + k\left(\bar{\theta} - \theta_c\right)$, and the shock parameter $k \sim [-1, 1]$. Recalling that citizens do not know $\bar{\theta}$, the true cost of producing public goods, the effect of the information shock reflects the assumption that the more accurate are the beliefs $\theta_c$ of citizens regarding the costs of public good provision, the less they change in the event of a shock. This is plausible in general, and specifically consistent with our experimental intervention, since we provided voters with the "true" ability of politicians to provide public goods; those voters who knew this already were therefore unaffected by the intervention.

The information shock in our field experiment, and in the model here, is unanticipated. Hence, incumbents do not take it into account when deciding on public goods.[6]

The second feature of the model that we add to the standard set-up is to recognize that for most public goods, spending takes time to implement before voters perceive a change in their welfare. Mayors must therefore decide to spend money on public goods early in their terms (Robinson and Torvik, 2005). Transfers, however, can be implemented quickly, even at the end of the mayor's term and right before the next election. This accurately reflects the limitations on incumbents' ability to react to information shocks in the weeks before an election.

As usual in retrospective voting models, citizens coordinate on a voting rule that is conditional on their beliefs about the costs of public good production just before the election, after the mayor has provided public goods. At the beginning of the mayor's term, voters establish the rule that, given their individual draw from the distribution of potential pre-electoral beliefs about the costs of public good production, $\theta_i'$ , they will support the incumbent who meets the performance threshold $\bar{\omega}_i \geq H\left(g_{\theta'}\right)$, where $g_{\theta'}$ is determined implicitly by $H_g\left(g_{\theta'}\right) = \frac{2\theta_i'}{N}$.[7]

The stages of the game are the following:

[5]As discussed in more detail below, $\delta$ is approximately 44.5 percent in the case of our field experiment.

[6]We abstract from anticipated information shocks. Their inclusion would complicate the analysis, but not change the key results.

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

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 $g$.[8]

4. A randomly-selected subset of all voters $\delta \leq 1$ are subject to an unanticipated shock $k$ to the distribution of their beliefs about the costs of producing public goods, such that for these voters $\theta_i' \sim [1, 2\theta_c' - 1]$, where $\theta_c' = \theta_c + k\left(\bar{\theta} - \theta_c\right)$, $k \sim [-1, 1]$.

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

6. 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.

**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 online appendix.

Lemma 1 confirms that unanticipated information shocks that change voter expectations about the costs of providing public goods affect voter support for the incumbent.

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

[8] 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 $\theta$ by necessity satisfies those who believe the costs were higher, and who set a lower performance threshold.

**Proof**: See online appendix.

Proposition 2 describes the incumbent response to an information shock. If the shock is adverse (it tells voters that it is less expensive to provide public goods than they anticipated), incumbents increase pre-electoral transfers and they target those transfers to those affected by the shock. This case is particularly relevant to our analysis, since voters in the Philippines are more likely to underestimate what local politicians can do for them, and to be disappointed when provided accurate information about government programs.[9]

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

**Proof**: See online appendix.

Proposition 2 shows that voters who receive more accurate information about the public goods the incumbent *could* have provided raise their performance threshold, in accordance with the voting rule. Since incumbents cannot adjust the provision of public goods in time for the election, they increase vote buying instead. Moreover, because the density of voters around the median is greater in the informed group (see the Theoretical Appendix), they target the vote buying to the informed group. This result emerges because public good spending begins substantially before the election, while transfers can be made right before the elections. Evidence from the Philippines, discussed below, supports these assumptions.

The experiments we report below offer evidence in support of the main predictions of the model. We also present evidence for ancillary predictions and assumptions of the model.

[9]This prediction depends on whether incumbents have an information advantage vis-à-vis voters, which is plausible in many low information political environments, and especially at the local level in the Philippines (Campos and Hellman, 2005).

## 3 Context

Our experiments examine the electoral incentives of voters and candidates in mayoral elections in the Philippines. This context is ideal for three main reasons. First, mayors control important public spending programs. The 1991 Local Government Code devolved a number of responsibilities to municipalities (Khemani, 2015; Llanto, 2012). Mayors exert significant control over how municipal resources are spent (Hutchcroft, 2012) and are often viewed as local bosses (Capuno, 2012; Sidel, 1999) subject to few checks and balances on their decisions. Nor does party membership constrain them: policies and party platforms play little role in elections (Montinola, 1999; Hutchcroft and Rocamora, 2003; Kerkvliet, 2002). Hence, voters can reasonably attribute the outcomes of municipal programs to the mayor.

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

Third, vote buying plays a significant role in elections (Cruz, 2018; Canare, Mendoza and Lopez, 2018). Moreover, ample evidence demonstrates that incumbents routinely adjust the targeting of vote buying to shocks that occur in the days leading up to the election. One campaign staffer for an incumbent mayor described in detail how local brokers immediately inform their candidates about village events that might affect the election.[10] Candidates then, with equal rapidity, adjust their vote buying strategies accordingly.

## 4 Isolating Information Effects on Politician Support: Survey Experiment

We seek to test a key prediction of the model: new information about the existence of government programs should reduce voter support for incumbents. A key constraint on our test is

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

that we cannot use actual voting behavior as a measure of support. We expect incumbents to take measures to offset the drop in support, so actual behavior could be unchanged. Instead, we conducted a survey experiment in three Philippine municipalities. During the survey, treated respondents were given the opportunity to study a flyer that described the Local Development Fund. By construction, the survey experiment design precluded a strategic reaction by incumbents. At the end of the survey, all respondents received a secret ballot, asking how likely they were to support the incumbent in the next election. Respondents who received the flyer with information about the Local Development Fund were significantly less likely to report that they would support the incumbent. This is the first demonstration in the literature that the mere revelation of the existence of a government program can reduce support for incumbents.

Within each of the three municipalities, 100 randomly-assigned respondents received information on the LDF and 100 randomly-assigned control respondents did not, for a total of 600 respondents. We selected three municipalities where the incumbent was in his/her first or second term, to avoid incumbents who are ineligible for reelection, and randomly selected 10 villages per municipality. Within each village, the survey team used the village list to randomly select 20 respondents for the survey experiment. Ten of them received the flyer and ten did not.

Treated and control respondents are balanced across 15 variables for which we have information: there are no significant differences among them with respect to their length of residence in their village; their gender, age, education levels; their household size; whether they receive remittances from abroad; whether they benefit from the Philippines conditional cash transfer program; whether they have asked the mayor or village captain for assistance; and whether they voted in the 2013 municipal elections (Table A.9).

Towards the end of the interview, treated respondents were then presented with a flyer with information about the LDF, including the ten categories of spending that could be undertaken under the program.[11] The survey ended with a secret ballot in which respondents indicated how likely they would be to support the incumbent mayor in the next election.

Our argument predicts that the information intervention should have led voters who were previously ignorant of the Local Development Fund to believe that the incumbent had greater capacity to provide services than they had previously thought. These respondents would have therefore raised their performance threshold—their expectations of incumbent performance. To

[11] A copy of the flyer is available in Figures A.1 - A.2. The translation is available in Table A.1.

the extent that incumbents had taken advantage of voter ignorance by shirking on their obligations to deliver LDF-funded projects, some respondents who would have expressed support for the incumbent prior to the information intervention should have been disappointed and instead indicated that they were neutral, unlikely or very unlikely to support the incumbent. Among respondents who already did not support the incumbent, the higher performance threshold simply meant that they continued not to support the incumbent. Among respondents who already knew of the existence of the Fund, support for the incumbent should have been unchanged.

In fact, we find that respondents who received information about the Local Development Fund were significantly less likely to express support for the incumbent. Table 1 indicates the percentage of respondents in the treatment and control groups who chose each of the response categories. A notably smaller fraction of respondents (six percentage points fewer) said that they were "likely to support the mayor". Correspondingly, a notably larger fraction of respondents in the treatment group (6.6 percentage points more) were neutral.

Table 1: How likely would you be to support the mayor in the next elections?

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

Notes: Data from the survey experiment.

We then classify each respondent as supporting the incumbent (very likely or likely categories), being neutral or not supporting the incumbent (unlikely or very unlikely categories). Controlling for village fixed effects, treated voters are between seven and eight percentage points less likely to express support for the incumbent and approximately 6.5 percentage points more likely to be neutral (Table 2). The magnitude of these effects is large, reducing support for the incumbent by 12 percent. Although the results are noisy, all are significant at least at the 10 percent level. In addition, the negative treatment effect on support for the incumbent is significant at the five percent level when we include individual controls to improve power.

The survey experiment also yields evidence in support of the mechanism. We asked participants in the survey experiment to report public investments in their villages that had been financed by the incumbent mayor. Their responses were averaged over each village to create a variable for the number of mayor-funded projects reported by respondents. The original regression specification examines the effects of the information treatment on support for the incumbent. This specification is supplemented with the interaction of the number of projects in each village with treatment status (the base effect is captured by the village fixed-effects). In an alternative specification, we control instead for a dummy variable, whether the village is above or below the median with respect to number of projects and its interaction with the treatment dummy.

Table 3 displays the results: the negative treatment effect on support for the incumbent is concentrated in villages where the incumbent provided fewer public goods.

These effects emerge despite the fact that the survey experiment occurred in the middle of the electoral cycle, rather than shortly before the election, like the field experiment. This timing gives rise to a possible spurious downward bias in the estimation of the survey experiment

Table 2: Survey Experiment: Exposure Only to Program Information Reduces Support for Incumbent

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

Notes: Results from individual-level regressions with village fixed-effects. In Column 1, the dependent variable is a dummy equal to one if the respondent indicated being either very likely or likely to support the incumbent during the upcoming elections. In Column 2, the dependent variable is a dummy equal to one if the respondent indicated being neutral in her support for the incumbent. In Column 3, the dependent variable is a dummy equal to one if the respondent indicated being either unlikely or very unlikely to support the incumbent during the upcoming elections. In Panel B, the regressions control for how long the respondent has lived in her current village of residence, family size, respondent's age, respondent's gender, respondent's education, whether the respondent received remittances from abroad, whether the respondent benefited from a large-scale CCT program, whether the respondent asked for assistance from the mayor, whether the respondent asked for assistance from the village captain and whether the respondent voted in the 2013 municipal elections. Robust standard errors are (in parentheses). * $p < .10$, ** $p < 0.05$, *** $p < .01$.

treatment effect. First, elections were less salient, and therefore the treatment less powerful. Second, it would be reasonable for respondents to evaluate incumbents more leniently in the middle of their term, when they still have time to implement projects, than at the end. Despite this downward bias, the information treatment had significant effects.

These results are novel in and of themselves. It is well-known that events out of the control of incumbents, from natural disasters to changes in commodity prices, can significantly affect their support (see Healy and Malhotra 2013 for a review of the literature). This is the first instance in which research has documented that information about the simple existence of a public program—without information about politician performance—can depress support for incumbents. These findings extend work by, for example, Gottlieb's (2016), who provides

Table 3: Survey Experiment: Heterogeneity by Public Investment

| | Support Incumbent: | | | | | |
|---|---|---|---|---|---|---|
| | Yes | Neutral | No | Yes | Neutral | No |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treat | -0.070* | 0.065* | 0.005 | | | |
| | (0.038) | (0.035) | (0.030) | | | |
| Treat*# mayor projects | 0.168 | -0.282** | 0.114 | | | |
| | (0.129) | (0.120) | (0.097) | | | |
| Treat*# mayor projects above median | | | | -0.027 | 0.012 | 0.015 |
| | | | | (0.049) | (0.046) | (0.041) |
| Treat*# mayor projects below median | | | | -0.127** | 0.134** | -0.007 |
| | | | | (0.058) | (0.055) | (0.044) |
| Observations | 597 | 597 | 597 | 597 | 597 | 597 |
| R-squared | 0.186 | 0.106 | 0.088 | 0.187 | 0.107 | 0.087 |

Notes: Results from individual-level regressions with village fixed-effects. In Columns 1 and 4, the dependent variable is a dummy equal to one if the respondent indicated being either very likely or likely to support the incumbent during the upcoming elections. In Columns 2 and 5, the dependent variable is a dummy equal to one if the respondent indicated being neutral in her support to the incumbent. In Columns 3 and 6, the dependent variable is a dummy equal to one if the respondent indicated being either unlikely or very unlikely to support the incumbent during the upcoming elections. The variable Treat* Above median is (treatment* dummy if number of projects financed by the mayor in the village between 2013 and 2015 is above the median) and the variable Treat* Below median is (treatment* dummy if number of projects financed by the mayor in the village between 2013 and 2015 is below the median). Robust standard errors are (in parentheses). * $p < .10$, ** $p < 0.05$, *** $p < .01$.

information about relative incumbent performance, and Banerjee et al. (2018), whose treatment includes information about a program to finance basic village infrastructure and exhortations about the importance of electing candidates who will do a good job in implementing the program. Our results underscore that citizen evaluations of incumbent performance hinge crucially on basic information about public policy.

# 5 Voter Effects and Incumbent Response: Randomized Information Campaign

To explore both voter effects and politician responses, we conducted a field experiment distributing flyers to all households in randomly selected villages in the week leading up to the May 13, 2013 mayoral elections. After the elections, we conducted a household survey in treated and control villages.

The flyer for the field experiment had the same format as the flyer presented to the respondents in the survey experiment. It contained the same information about the Local Development Fund. However, two circumstances compelled us to also include statements from all mayoral candidates in the municipality regarding their LDF allocation preferences.

First, our collaborator, a well-regarded non-governmental organization, the Parish Pastoral Council for Responsible Voting (PPCRV), wanted to increase the electoral salience of policy relative to vote buying. The PPCRV preferred an intervention that encouraged candidates to express policy preferences and subsequently disseminated those preferences to voters. Second, by introducing the LDF into the election, we invited a range of possible responses from candidates. In particular, although there is no tradition of campaigning based on policy statements in the Philippines, we could not preclude that candidates would respond to the flyer with last-minute position-taking. We therefore wanted to control for potential variation across treated households in exposure to candidate statements regarding their preferences over LDF allocations. To do this, we solicited those statements directly from candidates and included them in the flyer. All treated voters therefore received the same information about candidates' allocation preferences.

This additional information introduces potential ambiguities regarding the interpretation of any treatment effect. To offset this ambiguity, we collected information on voter preferences regarding LDF and are able to show that the distance between candidate and voter allocation preferences has no influence on either vote buying or vote choice.

In April 2013, we interviewed every candidate for mayor in twelve municipalities in the provinces of Ilocos Norte and Ilocos Sur, in the northern Philippines.[12] Candidates were told that the information they provided would be given to randomly-selected villages in their municipality

[12]Note that the survey and field experiments were conducted in different regions of the Philippines, since the results of one would have been contaminated by exposure to the other.

prior to the election. In the course of the interview, we gave each candidate a worksheet with a list of sectors. 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.[13]

Within each target municipality, villages were allocated to treatment and control using a pairwise matching algorithm.[14] The final sample includes 142 treatment and 142 control villages in twelve municipalities (cf. Table A.2).

PPCRV prepared flyers showing the proposed allocations of all candidates in each municipality, together with the basic information about the Local Development Fund (LDF), in the same format as the flyer for the survey experiment. Then, in the week leading up to the election, PPCRV volunteers distributed the flyers to all households in target villages through door-to-door visits.[15] The teams were instructed to visit all households in the village and give the flyer to the head of household or spouse, and in his or her absence, a voting-age household member.[16]

Although candidates were not told which villages would be treated, they had ample capacity to modify their vote buying in response. The flyers were distributed by teams of 10-15 PPCRV volunteers who arrived in each village riding in minivans (*jeepneys*), an event that, within hours, candidates' brokers and representatives relayed to the candidates. In the Philippines, candidates have a wide network of brokers (or *liders*) across villages, often building on existing social ties and obligations to family members, employees, tenants and others (Lande, 1996; Fegan, 2009; Cruz, Labonne and Querubin, 2017). These brokers are involved in distributing flyers and posters, coordinating rallies, and assisting with vote buying and other illegal strategies. Because vote buying is a logistically demanding electoral strategy, candidates do their hiring and recruiting months before the election to ensure that they have sufficient staff to be able to buy votes during the campaign period. This infrastructure permits them to react quickly when

[13]For more on the allocation exercise and data quality checks, please refer to the appendix.

[14]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.

[15]A copy of a flyer is included as Figures A.3 and A.4. The translation is available in Table A.4.

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

circumstances change, as turned out to be the case with the distribution of the flyer.[17] Hence, we expected that our information intervention could potentially affect vote buying.

For each household visit, volunteers used a detailed script to introduce themselves and explain the information contained in the flyers. Visits lasted between 5 and 10 minutes and volunteers left a copy of the flyer. No households refused the flyers. Neither the flyer nor the script mentioned vote buying, nor contained any other normative information concerning the electoral process. A detailed timeline of the experiment is available in Table A.3. The pre-analysis plan (PAP) was registered with J-PAL's hypotheses registry on May 12, 2013.[18]

The results in Table A.10 indicate that the village-level variables used to carry out the pairwise matching exercises are well-balanced across the treatment and control groups. We also use data from the survey to test if the treatment and control are balanced with respect to household composition, households assets, etc.[19] Out of the 32 village- and household-level variables for which we test balance, only 2 exhibit differences that are significant at the 10 percent level. Controlling for these variables does not affect results reported below.

## 5.1 Data

The analysis relies on two main data sources. First, precinct-level election results from the COMELEC include information on the number of votes obtained by all candidates in the mayoral elections.[20] Second, we implemented a household survey in 284 villages in twelve municipalities in June 2013. In each village, the team obtained the list of registered voters for the May 2013 elections and randomly selected twelve individuals to be interviewed for a total sample size of 3,408 households. Descriptive statistics are reported in Table 4.

**Occurrence of vote buying** The main concern with survey-based measures of vote buying is social desirability bias - the reluctance of people to answer questions about a potentially sensitive subject. In our case, we are also concerned with whether our treatment could have differentially

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

[18] The submitted documents are available at: http://www.povertyactionlab.org/Hypothesis-Registry and https://www.socialscienceregistry.org/trials/688

[19] This set of results is available in Table A.11-A.13.

[20] http://2013electionresults.comelec.gov.ph/res_rego.html

Table 4: Descriptive Statistics (Main experiment)

| | Treatment (1) | Control (2) |
|---|---|---|
| Salience sectors | 2.46 | 2.30 |
| | (1.47) | (1.51) |
| Salience sectors (adjusted) | 0.88 | 0.78 |
| | (1.19) | (1.21) |
| Vote buying | 0.16 | 0.14 |
| | (0.37) | (0.34) |
| Turnout (self-reported) | 0.97 | 0.96 |
| | (0.18) | (0.18) |
| Incumbent vote share (self-reported) | 0.63 | 0.65 |
| | (0.48) | (0.47) |
| Turnout (official) | 0.82 | .82 |
| | (.07) | (.06) |
| Incumbent vote share (official) | 0.64 | 0.63 |
| | (0.19) | (0.20) |

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

affected the willingness to respond. We argue that this is unlikely for three reasons. First, using a survey in Isabela, a province near our study area, Cruz (2018) finds that the estimated rate of vote buying using an unmatched count technique are statistically indistinguishable from the estimate calculated using the direct question.[21] Second, the flyer is entirely silent on normative issues in general, and specifically on issues related to electioneering, campaigning and vote buying itself. We demonstrate below that vote buying was no more electorally salient for the treatment than for the control group. Third, even in cases where normative information is present, it need not affect social desirability bias. Vicente (2014), for example, analyzes the effects of an information intervention explicitly directed at reducing voter acceptance of vote buying in Sao Tome and Principe and finds that even such an overtly anti-vote buying intervention has no effect on social desirability.

This suggests that responses to direct questions provide credible estimates of vote buying incidence, allowing us to measure vote buying according to whether respondents reported being offered money for their vote during the recent election. Note that in some electoral contexts, it may be possible to ask voters not only about whether their votes were bought, but also

[21]Similarly, Khemani (2015) also uses direct questions to estimate vote buying in research in the same province.

who bought them. In the area of the Philippines where we conducted the field experiment, however, PPCRV advised us that the second question is highly sensitive, even though the first is not. While it would be convenient to have been able to show *direct* evidence that incumbent vote buying was higher in treatment villages, we marshall numerous pieces of indirect evidence that yield only one plausible interpretation: the information shock increased vote buying by incumbents.

**Salience** One test of the intervention's effectiveness is whether treated households cared more about local development spending than untreated households. We therefore asked respondents about six possible influences on their decision to vote.[22] 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. They rated how important each of these was on a 0 - 4 scale, from "not important" to "very important".

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

# 6 Results: Effects on Salience

To show that the treatment affected the salience of local development spending for vote decisions, we estimate regressions of the form:

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

where $Y_{ijk}$ captures the salience of sectoral allocations for the vote choice of 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. Standard errors

[22] To reduce the possibility that the ordering of the alternatives would affect the responses, the flashcards were shuffled for each respondent and the same interview protocol was used across treatment and control groups.

are clustered at the village level.

Treated respondents were more likely to report that candidate spending of the municipal budget is important when they decide which candidate to vote for. Note that the information treatment does not mention vote buying and therefore should have no effect on the electoral salience of vote buying. Indeed, the point estimate is small (0.018, p-value equal to 0.65) and about one-tenth of the point estimates on the salience of budgetary allocations (Table 5). The salience results are robust to specifications with alternative controls (see Panels B and C of Table A.15).[23]

[23]The PAP also indicated that we would test if the treatment would increase voter knowledge of candidate promises. The treatment did increase knowledge: voters in treatment villages were more likely to know which candidate promised to spend the largest share of the LDF on any given sector. Those results are available in Panel A of Table A.15.

Table 5: Main treatment effects

| Dependent Variable: | Control Group Mean (1) | Average Treatment Effect (2) | Obs. (3) |
|---|---|---|---|
| 1. Salience sectors | 2.30 | 0.161** | 3,346 |
| | (1.51) | (0.070) | |
| 2. Salience sectors (adjusted) | 0.78 | 0.109** | 3,346 |
| | (1.21) | (0.044) | |
| 3. Are you aware of vote buying in your village? | 0.28 | 0.033 | 284 |
| | (0.45) | (0.023) | |
| 4. Did someone offer you money for your vote? | 0.14 | 0.034** | 284 |
| | (0.35) | (0.016) | |
| 5. Vote for the incumbent (self-reported) | 0.72 | -0.026 | 3,077 |
| | (0.447) | (0.017) | |
| 6. Incumbent vote share (precinct-level) | 0.63 | 0.004 | 314 |
| | (0.207) | (0.013) | |

Notes: Each cell in Column 1 contains the control group mean and standard deviation in (parentheses). Each cell in Column 2 contains the coefficient on the treatment dummy variable (indicating whether the campaign was implemented in the village) from the corresponding OLS regression, and standard error in (parentheses). Each regression includes pair fixed-effects. Dependent variables from the different regressions: row 1, the rating given to "Whether candidates will spend the municipal budget on things that are important to me and my family" when the respondent was asked about 'voting influences'; row 2, same as row 1, but adjusted to account for the average rating given to the other categories; row 3, the share of respondents who indicated being aware of instances of vote buying in their village; row 4, the share of respondents who indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'missing']; row 5, a dummy equal to one if the respondent declared voting for the incumbent; row 6: official incumbent vote share at the precinct-level. Standard errors (in parentheses) are clustered by village. * $p < .10$, ** $p < 0.05$, *** $p < .01$.

# 7 Results: Effects on Vote Buying

We also show that vote buying increased in the treatment villages, estimating equations of the form:

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

where $Y_{jk}$ is the prevalence of vote buying in village $j$ in pair $k$ during the May 2013 elections.[24] The set-up is equivalent to the one used for equation (1).

The results in Table 5 indicate that vote buying intensified in treated villages. These effects are robust across specifications with alternative controls, shown in Table A.17.[25] The information treatment had a large and significant effect on our main outcome measure, the percentage of village respondents who said that they were offered money for their vote. It led to a 3.4 percentage points increase in vote buying (24 percent of the control group mean).

A potential concern is that slightly more treated respondents refused to answer the vote buying questions, 7.3 percent versus 6 percent in the control group. While this difference is insignificant, we conduct several checks to demonstrate that differential rates of non-response cannot account for our results. In the results reported in Table 5 non-responses are coded as missing. To check robustness, we first recoded all non-responses as "yes", someone in fact offered the respondent money for her vote, to reflect the possibility that non-response reflects reluctance to report vote buying that actually occurred. The top panel in Table A.18 shows that treatment effects remain large and significant. In the bottom panel of Table A.18, we instead code 'refuse to answer' as "no", to verify robustness to the less plausible assumption that more reticent respondents were actually not offered money for their votes. Although the treatment effect drops from 3.4 percentage points to 2.3 percentage points, it remains significant controlling for pair fixed-effects, as in Table 5. We obtain similar results - with higher levels of statistical significance -

[24] Recall 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.16).

[25] The specifications we examine in Table A.17 were anticipated in the PAP. However, the PAP also registered the prediction that the information treatment would reduce vote buying, based on the argument that subsidizing promises would lead candidates to substitute away from vote buying in treated areas as in Keefer and Vlaicu (2017). We did not anticipate widespread voter ignorance of the Local Development Fund and the fact that, in the face of this ignorance, our treatment would lead households to substantially revise their evaluations of incumbent performance. Note that, while the intervention increased vote buying, we argue that this was a result of an intervention that actually increased incumbent incentives to improve voter welfare.

when we run those regressions at the individual-level (Table A.19). Finally, we derive the Lee bounds, .025 and .039, which allow us to reject the null hypothesis that the treatment increased vote buying by less than 2.5 percentage points, consistent with the more heuristic approach to recoding non-responses.

For completeness, we also report the treatment effect on the share of respondents who were aware of instances of vote buying in their village. By construction, this variable is less sensitive to the treatment, since it should remain stable, no matter how many households are offered money for their votes, as long as the presence of any vote buying at all in the village is common knowledge. Nevertheless, the treatment effect is nearly the same, 3.3 percentage points. It is not significant in the village level regressions (Table 5). However, in individual level regressions, reported in Panel A of Table A.16, the treatment effects are highly significant and of almost identical magnitudes to the ones obtained with our preferred measure of vote-buying incidence.

Qualitative evidence from local observers in the study area confirms these results: vote buying occurs in the days before the election and candidates and their brokers can re-target vote buying quickly. In many cases, the candidates contacted PPCRV with specific questions about the intervention activities. These sources also reported that candidates redoubled efforts to buy votes in the treatment villages and that most of the additional vote buying occurred on election day or the day before.[26]

## 7.1 Treatment Effects are Largest When Respondents Report Fewer Public Works

The field experiment also supports the mechanism through which the information shock should have increased vote buying: the vote buying effects of the flyer were strongest among those respondents who reported fewer recent projects in their villages. Respondents who received the flyers *and* who reported fewer projects were more likely to report vote buying in the field experiment, just as in the survey experiment they were less likely to express support for the incumbent.

The endline survey of the field experiment asked voters to report public investments in their

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

village that had been financed by the incumbent mayor since the previous election.[27] We therefore add an interaction term, public investment financed by the mayor and treatment status, to the earlier vote buying regression. We expect that the positive treatment effect on vote buying should be lower in villages that reported more public investment. Consistent with this, the interaction of treatment and reported public investment in Column 1 of Table 6 is significant and negative.[28]

One concern is whether self-reported public investment is influenced by the treatment. In fact, respondents in treatment and control villages report similar rates of municipal projects: .29 on average in the control group and .28 in the treatment group. We are unable to reject the null that the means are equal (p-value = .66). Those results are available in Table A.10.

## 7.2 Vote Buying Offset Treated Voters' Disappointment with Incumbents

A second piece of indirect evidence for increased incumbent vote buying emerges from treatment effects on support for the incumbent. The survey experiment indicates significantly lower support for the incumbent among respondents who are informed about the Local Development Fund. Treated voters in the field experiment, however, were no less likely to support the incumbent than untreated voters, consistent with incumbents having responded to the treatment with greater vote buying. Rows 5 and 6 of Table 5 report precisely this result. Consistent with incumbents using vote buying to offset the disappointment of treated voters, support for the incumbent among treated voters should be no different than among control voters.[29] Incumbent vote share, whether official or self-reported, was no different in treated or control villages.

Although vote buying is significantly greater in the treated villages, both self-reported and

[27] It is not plausible that respondents who report more public investment had received that investment because they had more demanding performance thresholds. On the one hand, incumbents should prefer to satisfy lower performance thresholds before they satisfy higher thresholds. Knowing this, voters should not set higher thresholds. On the other hand, if voters who reported lower public investment had lower thresholds, incumbents would have had no reason to increase vote buying in their villages, contrary to what we observe.

[28] We also confirm that results are similar when using a measure of whether public investment is greater or less than the median, in case there are concerns that the relationship is not linear. In villages reporting below-median public investment, the treatment significantly increased vote buying by 6.4 percentage points (Column 2 of Table 6). Furthermore, to address concerns that the results are capturing an interaction with other village level characteristics, we control for a wide range of village-level variables and their interaction with the treatment dummy in (Table A.20).

[29] Full results are reported in Table A.22.

Table 6: The treatment effect on vote buying is lower when reported public investment is higher

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

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

official incumbent vote shares are indistinguishable in treated and control villages. There are only three possible explanations for this. One is that the treatment had no effect on support for either incumbent or challenger, but instead simply caused both to campaign more intensively, leading to greater vote buying by both sides in treatment villages. The second is that the treatment had a *positive* effect on support for the incumbent, offset by challenger vote buying. The third is that the treatment had a *negative* effect on support for the incumbent, offset by incumbent vote buying. Results from the survey experiment are only consistent with the third interpretation. Three further arguments support this conclusion.

First, treatment effects are strongest where the incumbent provided the fewest public goods, not where the incumbent provided the most. Consistent with the survey experiment results showing a reduction in incumbent support, this rejects the competing hypotheses that the treatment had either no effect on voter preferences, or had a positive effect on preferences for the incumbent.

Second, vote buying is a demanding strategy and incumbents confront different costs of engaging in it. Those with lower vote buying costs should increase vote buying as a result of the experiment, offsetting the loss of support induced by the treatment. High cost incumbents, in contrast, should react less and experience lower levels of support as a consequence.

The data are consistent with this predicted pattern. For each municipality, we compute the levels of vote buying in the control group and distinguish municipalities where control group vote buying is lower and higher than the mean. Levels of control group vote buying vary greatly between the two municipalities: 3.4 percent in the former and 22.4 in the latter. We argue that incumbents in the former were more constrained in their ability to respond to the treatment.

In Column 1 of Table 7 we show that the treatment increased vote buying by 5.9 percentage-points in municipalities with high levels of vote buying in the control group. The point estimate in the other municipalities is minuscule (0.1 percentage-points). Conversely, in Column 3 of Table 7 we show that the treatment decreased support for the incumbent by 5.6 percentage-points in municipalities with low levels of control group vote-buying.[30] Strikingly, this effect is of similar magnitude as the one obtained in the survey experiment. The point estimate in the other municipalities is tiny (0.2 percentage-points). Those results are robust to controlling for the number of projects financed by the incumbent between 2010 and 2013 (Columns 2 and 4 of Table 7).

This pattern is consistent with incumbent vote buying to offset disappointment. It is inconsistent with the possibility that the treatment increased support for the incumbent. If the treatment had increased incumbent support, then in municipalities with low levels of vote buying (in the control group), treated respondents should have expressed greater support for the incumbent than control respondents. Instead, they express less.

The pattern is also inconsistent with the possibility that the treatment increased campaign intensity and had no partisan effects. In this case, voters in municipalities where incumbents

[30]Related results indicate that among respondents whose votes were *not* bought, treated respondents were significantly (4.1 percentage points) *less* likely to support the incumbent than control respondents (Table A.23). This difference is robust to adding a number of controls in addition to the pair fixed effects. The model we present earlier predicts that incumbents react to the information shock only by changing vote buying among the treated group. The information shock does not change equilibrium incumbent vote buying among unaffected voters. The difference in support among respondents whose votes were not bought indicates that they confronted logistical and financial constraints that prevented them from buying as many additional votes in the treated group as they might have wanted.

Table 7: Heterogeneity by extent of vote buying at the municipal-level (control group)

| | Vote Buying | | Support Incumbent | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treat * High VB | 0.059*** | 0.055*** | 0.002 | 0.009 |
| | (0.018) | (0.017) | (0.025) | (0.025) |
| Treat * Low VB | 0.001 | 0.006 | -0.056** | -0.061*** |
| | (0.011) | (0.012) | (0.022) | (0.022) |
| Additional Controls | No | Yes | No | Yes |
| Observations | 3,181 | 3,181 | 3,077 | 3,077 |
| R-squared | 0.178 | 0.179 | 0.307 | 0.309 |

Notes: Results from individual-level regressions with pair fixed-effects. In Columns 1-2, the dependent variable is a dummy equal to one of the respondent indicated that someone attempted to buy his/her vote [with 'refused to answer' coded as 'missing']; Columns 3-4, a dummy equal to one if the respondent declared voting for the incumbent. In Columns 2 and 4, regressions control for the number of project provided by the incumbent mayor between 2010 and 2013 and its interaction with the treatment dummy. Standard errors (in parentheses) are clustered by village. * $p < .10$, ** $p < 0.05$, *** $p < .01$.

did not respond to the treatment by buying more votes should have been no more likely to support a candidate in the treated than the control group. Instead, treated voters were far less likely to express support for the incumbent.

Third, qualitative evidence also supports our claim that incumbents were responsible for treatment-induced vote buying. Two local PPCRV affiliates in Ilocos Sur confirmed that incumbents conducted additional vote buying in the treatment areas after our intervention was completed. They specified that most of the additional vote buying occurred on election day or the day before.[31]

## 7.3 Additional Robustness Checks

A potential concern is that the results we find are driven by voter preferences over candidates, either because of candidate policy intentions or perceptions of candidate quality. First, we also demonstrate empirically that candidates' allocation intentions had no effect: treated voters were no more likely to prefer candidates with preferences closer to theirs than control voters, nor did

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

policy alignment between voters and candidates affect vote choice more generally (Table A.6).

Second, we establish that voter perceptions of the candidates are not driving the results. We show in Table A.8 that voter opinions of candidate quality are not affected by the treatment. Neither is our effect likely to be driven by raising the profile of the challenger relative to the incumbent: recall that even our survey experiment showed a measurable reduction in incumbent support without reference to either candidate, suggesting that voter disappointment with the incumbent does not depend on comparison with a challenger.

Full tables and discussion are available in the appendix.

# 8 Conclusion

We show that even in clientelistic settings, voters use information rationally, making information campaigns a potentially powerful and cost-effective way to decrease information asymmetries between voters and politicians. Even providing ostensibly neutral information about government capabilities allows voters to make their own assessment about candidate performance.

We combine a theoretical model with two experiments to test the effects of information about the existence of a spending program in an environment where candidates cannot make credible commitments. In the model, information shocks that raise voters' thresholds for incumbent performance shortly before an election oblige incumbents to do more to increase voter welfare than they anticipated. With little time before the election to improve the provision of public goods, incumbents turned to vote buying.

The survey experiment provides direct evidence that merely informing individuals of the existence of the spending program reduces support for incumbents; especially those who have under-provided public goods during their term in office. We further explore these effects in the context of real world elections using a unique field experiment providing voters with the same information just prior to the May 2013 municipal elections in the Philippines. Consistent with the survey experiment results, the intervention led to a decrease in voter support for incumbents, prompting the subsequent incumbent response, which in this case took the form of increased vote buying. The intervention led to significant changes in voter knowledge about incumbents and vote buying.

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

The results raise questions for future research. First, 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.

Second, 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 our argument: that if the intervention had occurred earlier in the electoral cycle (or at least if incumbents had known earlier that the intervention would take place), it might have prompted incumbents to provide more public goods, with no change, or even a reduction, in vote buying.[32] In fact, a survey conducted in the same municipalities after the 2016 elections broadly suggests that increased voter knowledge of the program increased respondent incentives to provide public goods: respondents to the 2016 survey reported 58% more incumbent-provided infrastructure in the 3 years before the survey than respondents from those municipalities after the 2013 elections.

[32]Grossman and Michelitch's (2018) results offer important initial insights along these lines. They disseminate a scorecard of legislator performance early in the electoral term and find evidence that some aspects of legislator performance improved subsequently, in competitive constituencies.

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# Appendix for Online Publication

## A.1 Technical Appendix

In this technical appendix we report the proofs of the Lemma and Propositions discussed in Section 4.

### 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 $\theta_c$. 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 $H_g\left(g_{\theta_c}\right) = \frac{\theta_c}{N}$. The performance threshold of the median voter would then be $\bar{\omega} = H\left(g_{\theta_c}\right)$, where $H_g\left(g_{\theta_c}\right) = \frac{\theta_c}{N}$. However, voters anticipate that incumbents can marginally reduce public goods, saving incumbents $\theta_c$ 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 $H_g\left(g^*\right) = \frac{2\theta_c}{N}$ 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 $g_{\theta_c}$. In the event of an unanticipated shock, a fraction $\delta$ of voters have beliefs distributed according to $\theta_i' \sim [1, 2\theta_c' - 1]$, where $\theta_c' = \theta_c + k\left(\bar{\theta} - \theta_c\right)$, 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 $(k\left(\bar{\theta} - \theta_c\right) > 0)$.** The shock shifts up the median of the distribution of beliefs about the costs of providing public goods among a fraction $\delta$ of voters. Consequently, among the $\delta$ 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 $\theta_i' \sim \left[1, 2\left(\theta_c + k\left(\bar{\theta} - \theta_c\right)\right) - 1\right]$, the fraction of voters in $\delta$ for whom the incumbent's performance is expected to be sufficient, but previously was not, is given by $\left(\frac{\theta_c' - \theta_c}{2\left(\theta_c + k\left(\bar{\theta} - \theta_c\right)\right) - 2}\right) = \frac{1}{2}\left(\frac{k\left(\bar{\theta} - \theta_c\right)}{\theta_c + k\left(\bar{\theta} - \theta_c\right) - 1}\right) > 0$. The total fraction of voters in $\delta$ for whom the incumbent's performance is expected to be sufficient is therefore $\frac{1}{2}\left(1 + \frac{k\left(\bar{\theta} - \theta_c\right)}{\theta_c + k\left(\bar{\theta} - \theta_c\right) - 1}\right) > \frac{1}{2}$. Incumbents have the support of one-half of the voters who were not exposed to the shock and more than one-half of the voters who were, and are 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 $(k\left(\bar{\theta} - \theta_c\right) < 0)$.** When the unanticipated shock reduces the beliefs of a fraction $\delta$ 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 $\frac{1}{2}\left(1 + \frac{k\left(\bar{\theta} - \theta_c\right)}{\theta_c + k\left(\bar{\theta} - \theta_c\right) - 1}\right) < \frac{1}{2}$. Fewer than one-half of the voters subjected to the information shock, and therefore fewer than one-half of all voters, are satisfied by incumbent performance. However, these incumbents can still be re-elected if they use transfers to increase voter welfare. ■

## 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 $\delta$. 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 $f_k = H\left(g_{g_{\theta_c+k}}\right) - H\left(g_{\theta_c}\right)$: 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 $f_k = H\left(g_{g_{\theta_c+k}}\right) - H\left(g_{\theta_c}\right)$ 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 $\delta$ with transfers rather than other voters. We then establish the fraction of voters in $\delta$ whom they target.

Recalling that $k\left(\bar{\theta} - \theta_c\right)$ is less than zero, $-\frac{1}{2}\left(\frac{k\left(\bar{\theta}-\theta_c\right)}{\theta_c + k\left(\bar{\theta}-\theta_c\right) - 1}\right)$ is the fraction of voters in the group $\delta$ that received the information shock and would be "persuaded" by a transfer $f_k$. Other voters in $\delta$ 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 -$\frac{1}{2}\left(\frac{k\left(\bar{\theta}-\theta_c\right)}{\theta_c-1}\right)$. Since $\left(\theta_c - 1\right) > \left(\theta_c + k\left(\bar{\theta}-\theta_c\right) - 1\right)$ for $k\left(\bar{\theta}-\theta_c\right) < 0$, the probability that a transfer will reach a persuadable voter is greater if it is targeted to voters in the group $\delta$.[33]

[33] The 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

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

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

## A.2 Background on the Experiment

### A.2.1 Data Quality

Candidates took the allocation exercise seriously. During the interview, they typically spent several minutes to arrange the tokens after considering their allocation.[34]

There are also two quantitative indications of data quality. First, the spending intentions of incumbents were correlated with how they had actually allocated their budgets prior to the interviews.[35] Second, in response to one of the survey questions, candidates listed three specific projects and programs that they would implement if elected. Candidates consistently allocated a greater share of their proposed budget to the sectors to which these projects and programs belonged (see Figure A.5).

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

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

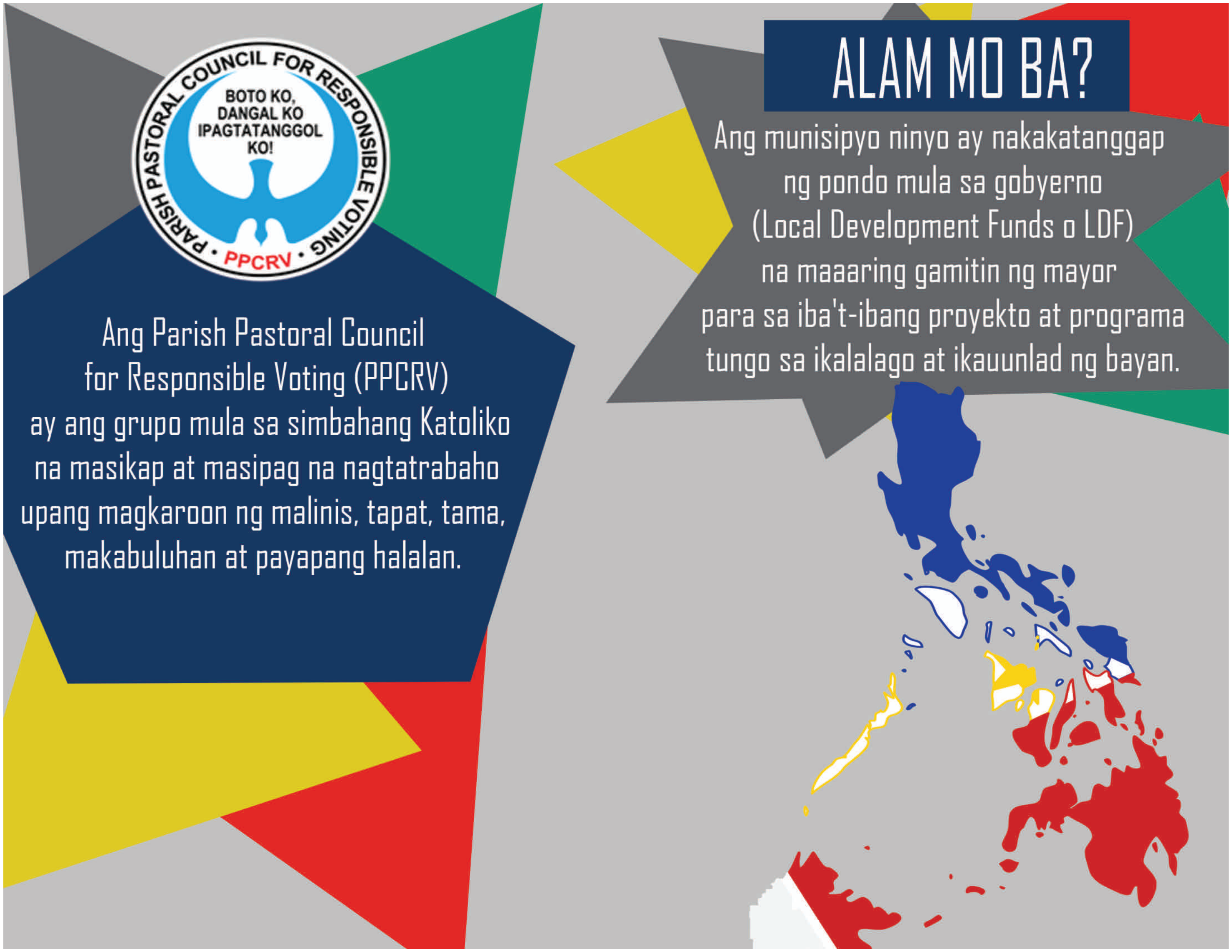


Figure A.1: Cover for the Flyer (Survey Experiment)


Figure A.2: Inside of the flyer (Survey Experiment)

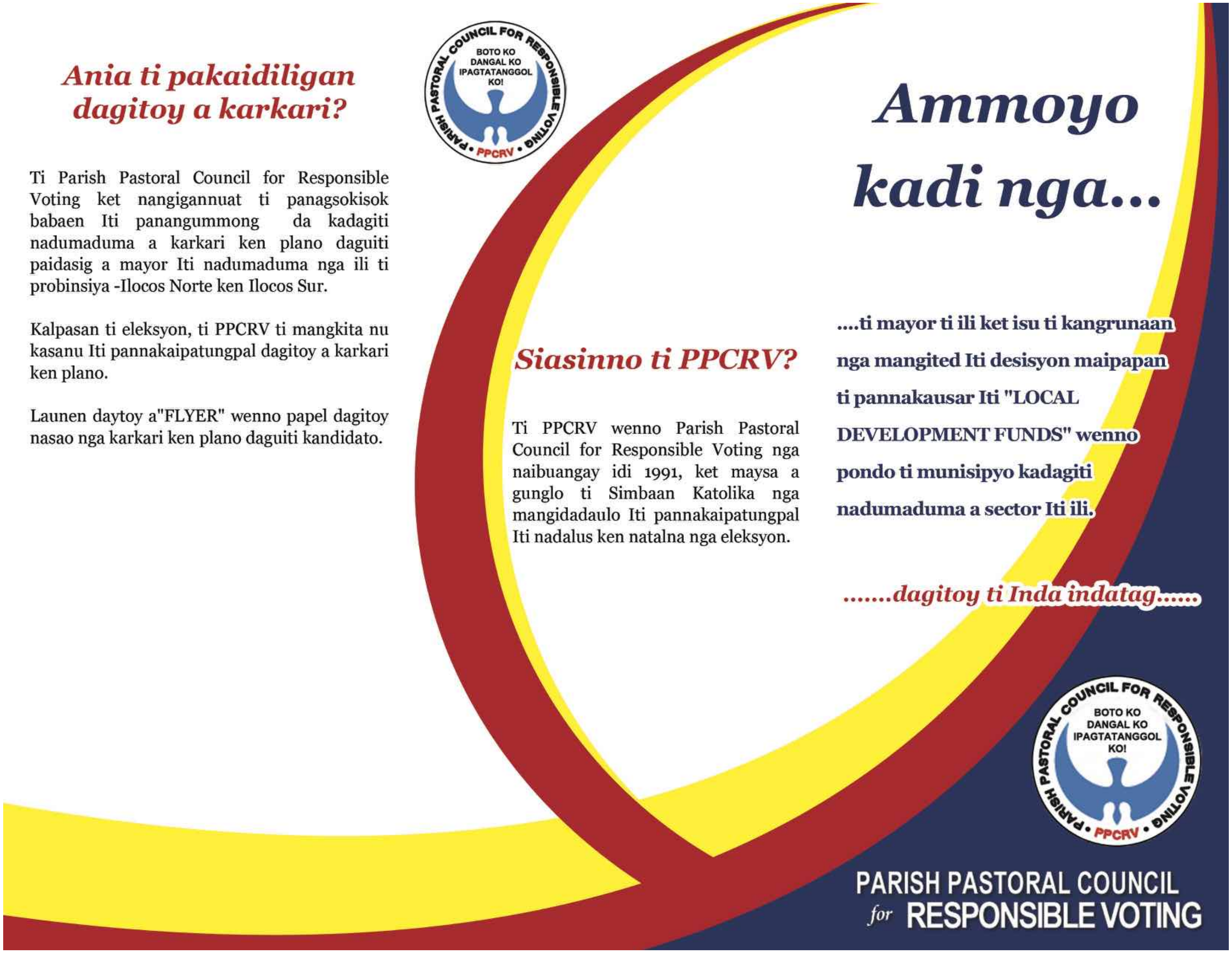


Figure A.3: Cover for the Flyer

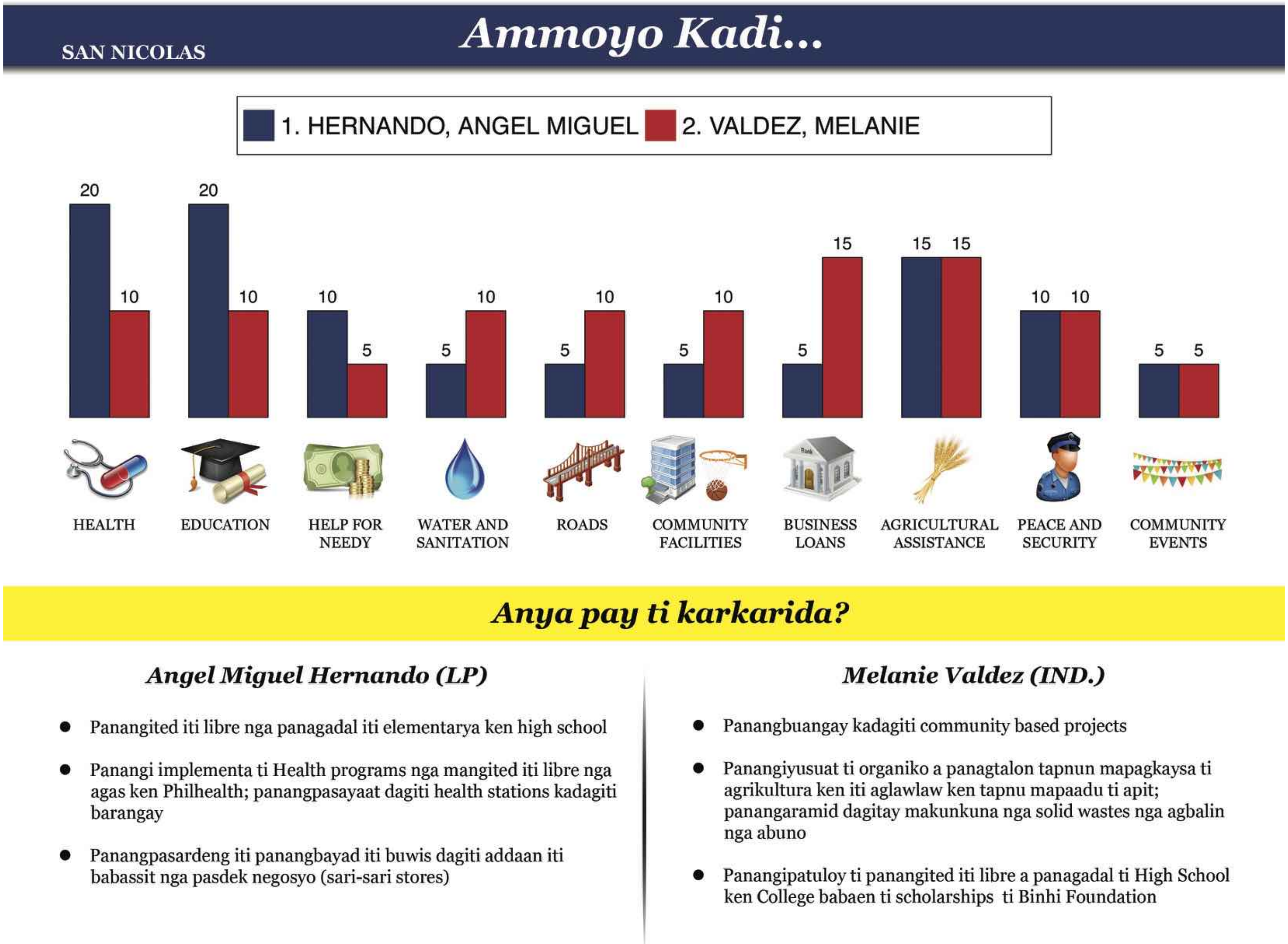


Figure A.4: Flyer for the Municipality of San Nicolas, Ilocos Norte

Table A.1: Translation of Flyer for the Survey Experiment (Fig. A.1)

| Front Flap | Back |
|---|---|
| Did you know... | About the PPCRV |
| Your municipality receives funds from the government (Local Development Funds or LDF) that the mayor can use for various projects and programs to improve your community. | The Parish Pastoral Council for Responsible Voting (PPCRV) is the non-partisan voter education and elections monitoring arm of the Catholic Church advocating for free and fair elections in the Philippines. |

Note: The inside of the flyer presents the sectoral allocations with identical visuals and labels available in English or Tagalog (Tagalog version shown).

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.

Table A.3: 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.4: Translation of Flyer for the Intervention (Fig. A.3)

| Front Page | Inner Flap | Back |
| --- | --- | --- |
| Did you know... | What makes these promises different? | About the PPCRV |
| The mayor makes important decisions about how money is spent in your municipality. The PPCRV asked all the candidates for mayor how they would allocate Local Development Funds across sectors. This is what they said: | The PPCRV collected these promises and the PPCRV will monitor implementation after the election. The PPCRV asked all the mayoral candidates about the policies and programs that they will implement if elected. This flyer presents those proposals. | Established in 1991, PPCRV is the non-partisan voter education and elections monitoring arm of the Catholic Church. The PPCRV is the leading civil society organization advocating for free and fair elections in the Philippines. |

Note: The inside of the flyer presents the sectoral allocations (with visuals and text in English) as well as additional promises that candidates have opted to convey to voters at the bottom.

## A.3 Alternative Explanations

### A.3.1 Candidate Policy Intentions and Respondent Preferences over Candidates

Observers of Philippine elections generally agree that programmatic policies are not salient (see, e.g., Montinola 1999). This is especially the case for municipal elections. Indeed, if policy promises were important, candidates should have already disseminated their intentions regarding the LDF prior to our intervention, but they did not. Furthermore, if candidate LDF allocation intentions had affected voter attitudes, vote buying should have dropped in treated areas; instead it rose. For example, Keefer and Vlaicu (2017) show that a decline in the probability that a party will renege on its pre-electoral promises reduces politicians' payoffs to vote buying. Cox (1987) examines the expansion of the franchise in Great Britain and shows that it increased the salience of (credible) party programs and reduced vote buying.[36]

For those intentions to have mattered, it must have been the case that treated respondents with preferences closest to one candidate's intended allocations should have been more likely to have preferred that candidate compared to control respondents. In addition, the closer are a voter's preferences to the policy announcements of one candidate relative to the other, the more difficult it is to sway that voter with vote buying. Hence, reported vote buying should also fall the greater is respondent alignment with one candidate compared to the other.

To investigate these issues, we collected data on respondents' candidate preferences and vote choice. Respondents rated all mayoral candidates on a 0 - 4 scale (strongly disagree to strongly agree) and were also asked directly whom they voted for. In order to reduce the tendency of respondents to claim they voted for the winner when they did not, we used a secret ballot.[37]

We also asked respondents to express their preferences over the same ten spending categories that were given to the mayoral candidates. Like the candidates, respondents were given 20 tokens and asked to allocate the tokens in any manner they wished across the ten categories.

[36]See also Cox and Kousser (1981).

[37]Respondents were given ballots with 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.

We then calculated how close the preferences of the candidates were to those of the household by comparing the share $S$ that voter $v$ allocated to sector $s$ with the share that candidate $c$ allocated to the sector. The total spending over which the candidate and voter agree is given by an agreement index, defined as $A_{vc} = \sum_s min\,(S_{vs}, S_{cs})$.

Table A.5 reports results of a vote choice regression where we control for both the treatment, the alignment between the voter's preferences and the candidate promises and their interactions. If the results were driven by promises, treatment group voters should expressed support for the candidate whose promises were more closely aligned with their own preferences. This alignment should have no effect on control voters, who are unaware of the promises. We run those regressions with both candidate preferences and vote choice. In both cases the point estimates on the interaction term is very close to zero and not significant.

Table A.5: Effects of Treatment on Self-reported Support for Candidate

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

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

Furthermore, if policy stances matter, then the relative distance between candidate's policy stances and the policy preferences of respondents should affect key outcomes, such as knowledge and vote buying, and those effects should be greater among treated households. Table A.6 shows, in contrast, that the relative policy stances almost never matter. Those for whom one of the candidate's promises are relatively closer to the respondent's preferences do not report differences in vote buying nor in the salience of spending. In all these cases, the treatment effect (higher vote buying and greater salience of municipal spending) is unaffected by relative policy stances.

Table A.6: The mediating effects on the treatment of relative preference over the candidate

| | Know Promises | Salience | Vote Buying |
|---|---|---|---|
| | (1) | (2) | (3) |
| Treat | 0.051*** | 0.109** | 0.033*** |
| | (0.015) | (0.044) | (0.012) |
| Relative Preference | 0.002** 4 | 0.003 | -0.001 |
| | (0.001) | (0.003) | (0.001) |
| Interaction | -0.003* | -0.004 | -0.001 |
| | (0.001) | (0.003) | (0.001) |
| | | | |
| Observations | 3,408 | 3,346 | 3,181 |
| R-squared | 0.327 | 0.090 | 0.178 |

Notes: Results from individual-level regressions with pair fixed-effects. Dependent variables are: Column 1, an index capturing the respondent's knowledge of candidate promises; Column 2, the rating given to "Whether candidates will spend the municipal budget on things that are important to me and my family" when the respondent was asked about 'voting influences', adjusted to account for the average rating given to the other categories; Column 3, the share of respondents who indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'missing']; The standard errors are (in parentheses). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

A potential concern with the policy preferences variable is that it represents a choice that respondents are not used to making. However, respondent preferences seemed to correspond to their family circumstances. We regress preferences on a number of household characteristics that should be correlated with preferences for a given sector. For example, families with children should favor spending on education and farmers should favor spending in agriculture. Results presented in Table A.7 below suggest that stated preferences over spending priorities match observable household characteristics.

Table A.7: Correlates of Sectoral Preferences

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

hline

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.

Allocation preferences were collected after the information about candidates' promises had been distributed to voters in the treatment group. It is therefore possible that respondents 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.5).

### A.3.2 Comparisons of Candidates and Assessments of Candidate Quality

We also show that potential alternative explanations regarding voter perceptions of candidate quality are unlikely to be driving the results. One possibility is that the treatment raised the profile of the challenger relative to that of the incumbent, making the mayoral race more competitive and driving both candidates to increase vote buying. Another possibility is that the treatment affected voter beliefs about candidate quality, prompting candidates to respond with vote buying.

These potential explanations are inconsistent with the results in three ways. First, the survey experiment demonstrated a measurable reduction in incumbent support even without reference to either candidate, suggesting that voter disappointment with the incumbent does not depend on comparison with the challenger. Second, the flyer had no information that voters could use to assess candidate quality, other than the fact that both candidates were capable of formulating a policy regarding the allocation of the Local Development Fund. Third, the survey asked respondents for their opinions on four candidate qualities, honesty, approachability, experience and political connectedness. We observe no treatment effects on any of them. Results are summarized in Table A.8.[38]

[38]The exact question in the survey is: "Now we are going to show you a set of worksheets, one for each candidate, as well as some flashcards containing some traits [Approachable/Friendly; Experienced in politics; Honest; and Politically well-connected] that candidates might have. For each of these traits, please place them on the worksheet of the candidate that you most associate with that trait. You may place the same trait on both worksheets or you may choose not to place a trait at all if you feel that it does not apply to any

Table A.8: Effects of Treatment on Perceived Candidate Characteristics

| | (1) Honest | (2) Approachable | (3) Experienced | (4) Connected |
|---|---|---|---|---|
| Panel A: All candidates | | | | |
| Treat | 0.004 | -0.003 | 0.012 | 0.016 |
| | (0.013) | (0.012) | (0.010) | (0.012) |
| Observations | 7,886 | 7,887 | 7,887 | 7,886 |
| R-squared | 0.054 | 0.055 | 0.036 | 0.040 |
| Panel B: Incumbent Only | | | | |
| Treat | -0.012 | -0.020 | 0.023* | 0.017 |
| | (0.014) | (0.014) | (0.013) | (0.014) |
| Observations | 3,406 | 3,406 | 3,406 | 3,406 |
| R-squared | 0.163 | 0.158 | 0.292 | 0.153 |

Notes: Results from individual*candidate-level regressions. The dependent variable is the honesty rating given to the candidate (Column 1), the approachability rating given to the candidate (Column 2), the experience rating given to the candidate (Column 3) and the political connections rating given to the candidate (Column 4). Standard errors (in parentheses) are clustered by village. * $p < .10$, ** $p < 0.05$, *** $p < .01$.

# A.4 Additional Results

of the candidates."

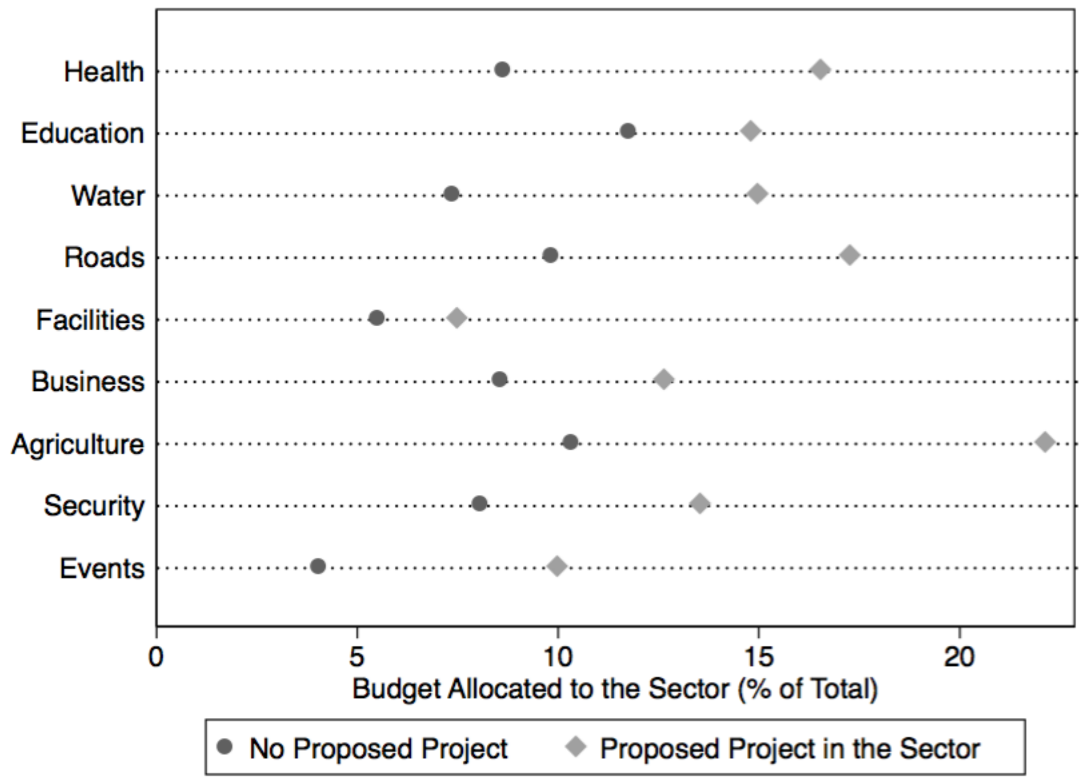


Figure A.5: Candidate Proposed Projects and Budget Allocations
Note: Candidates were asked to propose budgetary allocations for each sector. Separately, they were given the opportunity to indicate three specific projects that they would implement if elected. For each of the sectors listed, the figure compares the budgetary allocations of candidates who indicated specific projects in that sector with the budgetary allocations of incumbents who did not.

Table A.9: Balance Tests : Respondent Characteristics (Survey Experiment)

| | Treatment | Control | T-test | K-Smirnov test | OLS |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Length of residence | 31.72 | 31.91 | 0.13 | 0.07 | -0.25 |
| | (18.66) | (17.23) | [0.90] | [0.48] | [0.86] |
| Female | 0.60 | 0.65 | 1.35 | 0.05 | -0.05 |
| | (0.49) | (0.48) | [0.18] | [0.76] | [0.18] |
| Age | 43.73 | 43.71 | -0.02 | 0.03 | -0.04 |
| | (14.67) | (14.93) | [0.99] | [0.99] | [0.98] |
| Education levels: | | | | | |
| Some primary | 0.12 | 0.11 | -0.51 | 0.01 | 0.01 |
| | (0.33) | (0.31) | [0.61] | [1.00] | [0.64] |
| Primary graduate | 0.13 | 0.17 | 1.60 | 0.05 | -0.05 |
| | (0.33) | (0.38) | [0.11] | [0.88] | [0.12] |
| Some high school | 0.20 | 0.15 | -1.62 | 0.05 | 0.05 |
| | (0.40) | (0.35) | [0.10] | [0.82] | [0.10] |
| High school graduate | 0.32 | 0.29 | -0.80 | 0.03 | 0.03 |
| | (0.47) | (0.45) | [0.43] | [1.00] | [0.40] |
| Vocational training | 0.05 | 0.07 | 1.00 | 0.02 | -0.02 |
| | (0.23) | (0.26) | [0.32] | [1.00] | [0.32] |
| College + | 0.14 | 0.18 | 1.11 | 0.03 | -0.03 |
| | (0.35) | (0.38) | [0.27] | [0.99] | [0.24] |
| Household size | 5.33 | 5.33 | 0.00 | 0.02 | 0.00 |
| | (2.39) | (2.21) | [1.00] | [1.00] | [0.99] |
| Remittances abroad | 0.19 | 0.20 | 0.31 | 0.01 | -0.01 |
| | (0.39) | (0.40) | [0.76] | [1.00] | [0.76] |
| CCT Beneficiary | 0.16 | 0.19 | 0.75 | 0.02 | -0.02 |
| | (0.37) | (0.39) | [0.45] | [1.00] | [0.43] |
| Ask assistance from: | | | | | |
| Mayor | 0.24 | 0.25 | 0.28 | 0.01 | -0.01 |
| | (0.43) | (0.45) | [0.78] | [1.00] | [0.76] |
| Village captain | 0.26 | 0.22 | -1.24 | 0.04 | 0.04 |
| | (0.44) | (0.41) | [0.22] | [0.93] | [0.21] |
| Turnout (2013) | 0.98 | 0.95 | -1.56 | 0.02 | 0.02 |
| | (0.15) | (0.21) | [0.12] | [1.00] | [0.12] |
| Joint test of significance of the variables reported this Table: | | | | | |
| | | | $\chi^2 = 16.00$ | | |
| | | | p-value = 0.31 | | |

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

Table A.10: Balance Tests (Main Experiment)

| | Treatment (1) | Control (2) | T-test (3) | K-Smirnov test (4) | OLS (5) |
|---|---|---|---|---|---|
| # Precincts | 1.09 | 1.10 | 0.18 | 0.02 | -0.01 |
| | (0.29) | (0.36) | [0.86] | [1.00] | [0.83] |
| Registered Voters | 526.26 | 544.94 | 0.48 | 0.07 | -18.68 |
| | (306.78) | (342.68) | [0.63] | [0.84] | [0.53] |
| Population | 842.20 | 895.92 | 0.83 | 0.06 | -53.73 |
| | (492.93) | (598.28) | [0.41] | [0.97] | [0.31] |
| Turnout 2010 | 0.78 | 0.78 | 0.01 | 0.06 | 0.00 |
| | (0.08) | (0.08) | [0.99] | [0.97] | [0.98] |
| 2010 Incumbent vote share | 0.73 | 0.72 | -0.31 | 0.06 | 0.01 |
| | (0.21) | (0.23) | [0.75] | [0.92] | [0.52] |
| 2010 Incumbent vote share [adj.] | 0.56 | 0.55 | -0.45 | 0.06 | 0.01 |
| | (0.13) | (0.15) | [0.65] | [0.97] | [0.53] |
| Rural | 0.88 | 0.87 | -0.18 | 0.01 | 0.01 |
| | (0.33) | (0.33) | [0.86] | [1.00] | [0.84] |
| Nb projects (HH survey) | 0.28 | 0.29 | -0.43 | 0.06 | -0.01 |
| | (0.02) | (0.02) | [0.66] | [0.98] | [0.64] |

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

Table A.11: Balance Tests : Alignment of Preferences and Promises (Main Experiment)

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

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

Table A.12: Balance Tests : Respondent Characteristics (Main Experiment)

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

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

Table A.13: Balance Tests : Respondent Characteristics (Main Experiment)

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

Joint test of significance of the 22 variables reported in Tables A.12 and A.13:

$\chi^2 = 12.99$

p-value = 0.93

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

Table A.14: Effects of Treatment on Knowledge of Politicians and Candidates

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

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

Table A.15: Effects of Treatment on Knowledge and Salience

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Panel A: Knowledge of Promises | | | | |
| Treat | 0.051 | 0.051** | 0.051*** | 0.051*** |
| | (0.036) | (0.022) | (0.015) | (0.015) |
| Municipal Fixed-Effects | No | Yes | No | No |
| Pair Fixed-Effects | No | No | Yes | Yes |
| Additional Controls | No | No | No | Yes |
| Observations | 3,408 | 3,408 | 3,408 | 3,408 |
| R-squared | 0.003 | 0.245 | 0.326 | 0.326 |
| Panel B: Salience | | | | |
| Treat | 0.159* | 0.158* | 0.161** | 0.170** |
| | (0.096) | (0.094) | (0.070) | (0.069) |
| Municipal Fixed-Effects | No | Yes | No | No |
| Pair Fixed-Effects | No | No | Yes | Yes |
| Additional Controls | No | No | No | Yes |
| Observations | 3,346 | 3,346 | 3,346 | 3,346 |
| R-squared | 0.003 | 0.014 | 0.146 | 0.149 |
| Panel C: Salience (adjusted) | | | | |
| Treat | 0.107* | 0.107* | 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 an index capturing the respondent's knowledge of candidate promises.In Panel B, the dependent variable is rating given to "Whether candidates will spend the municipal budget on things that are important to me and my family" when the respondent was asked about 'voting influences'. In Panel C, the variable is adjusted to account for the average rating given to the other categories. In Column 4, the regression includes a dummy equal to one if someone in the household is a member of any group and a dummy equal to one if someone in the household participated in any collective action activity in the village in the past six months. The standard errors (in parentheses) account for potential correlation within village. * p < .10, ** p < 0.05, *** p < .01.

Table A.16: 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 | 0.033 | 0.035** | 0.034** |
| | (0.029) | (0.023) | (0.016) | (0.016) |
| Municipal Fixed-Effects | No | Yes | No | No |
| Pair Fixed-Effects | No | No | Yes | Yes |
| Additional Controls | No | No | No | Yes |
| Observations | 3,212 | 3,212 | 3,212 | 3,212 |
| R-squared | 0.001 | 0.105 | 0.193 | 0.193 |
| Panel B: Did Someone Offer you Money for your Vote? | | | | |
| Treat | 0.027 | 0.031** | 0.032*** | 0.033*** |
| | (0.021) | (0.015) | (0.011) | (0.011) |
| Municipal Fixed-Effects | No | Yes | No | No |
| Pair Fixed-Effects | No | No | Yes | Yes |
| Additional Controls | No | No | No | Yes |
| Observations | 3,181 | 3,181 | 3,181 | 3,181 |
| R-squared | 0.001 | 0.117 | 0.176 | 0.176 |

Notes: Results from individual-level regressions. In Panel A, the dependent variable is a dummy equal to one if the respondent indicated being aware of instances of vote buying in her village. In Panel B, the dependent variable is a dummy equal to one if the respondent indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'missing']. In Column 4, the regression includes a dummy equal to one if someone in the household is a member of any group and a dummy equal to one if someone in the household participated in any collective action activity in the village in the past six months. The standard errors (in parentheses) account for potential correlation within village. * $p < .10$, ** $p < 0.05$, *** $p < .01$.

Table A.17: Effects of Treatment on Vote Buying

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

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

Table A.18: Effects of Treatment on Vote Buying [Village-level / Alternative Coding]

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Panel A: Did Someone Offer you Money for your Vote? (missing coded as yes) [Alternative Coding] | | | | |
| Treat | 0.036 | 0.036** | 0.036** | 0.036** |
| | (0.023) | (0.017) | (0.017) | (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 | 0.008 | 0.501 | 0.735 | 0.735 |
| Panel B: Did Someone Offer you Money for your Vote? (missing coded as no) [Alternative Coding] | | | | |
| Treat | 0.023 | 0.023 | 0.023 | 0.024 |
| | (0.019) | (0.014) | (0.014) | (0.015) |
| 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 | 0.456 | 0.723 | 0.724 |

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']. The dependent variable is the share of respondent who indicated that someone attempted to buy their votes [with 'refused to answer' coded as 'no']. 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.19: Effects of Treatment on Vote Buying [Individual-level / Alternative Coding]

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Panel A: Did Someone Offer you Money for your Vote? (missing coded as yes) | | | | |
| Treat | 0.036 | 0.036** | 0.036*** | 0.036*** |
| | (0.023) | (0.017) | (0.012) | (0.012) |
| 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 | 0.002 | 0.119 | 0.174 | 0.174 |
| Panel B: Did Someone Offer you Money for your Vote? (missing coded as no) | | | | |
| Treat | 0.023 | 0.023 | 0.023** | 0.023** |
| | (0.019) | (0.014) | (0.010) | (0.010) |
| Municipal Fixed-Effects | No | Yes | No | No |
| Pair Fixed-Effects | No | No | Yes | Yes |
| Additional Controls | No | No | No | Yes |
| Observations | 3,408 | 3,408 | 3,408 | 3,408 |
| R-squared | 0.001 | 0.098 | 0.156 | 0.156 |

Notes: Results from individual-level regressions. In Panel A, the dependent variable is a dummy equal to one if the respondent indicated that someone attempted to buy their votes [with all missing values coded as yes]. In Panel B, the dependent variable is a dummy equal to one if the respondent indicated that someone attempted to buy their votes [with all missing values coded as no]. In Column 4, the regression includes a dummy equal to one if someone in the household is a member of any group and a dummy equal to one if someone in the household participated in any collective action activity in the village in the past six months. The standard errors (in parentheses) account for potential correlation within village. * $p < .10$, ** $p < 0.05$, *** $p < .01$.

Table A.20: The mediating effects on the treatment of public investment in villages, education, number of voters, and poverty

| Dep. Var.: Did Someone Offer you Money for your Vote? | | |
|---|---|---|
| | (1) | (2) |
| Treat | 0.028* | |
| | (0.016) | |
| Treat*Nb projects | -0.149** | |
| | (0.073) | |
| Treat*Nb projects above median | | -0.007 |
| | | (0.030) |
| Treat*Nb projects below median | | 0.058*** |
| | | (0.021) |
| Observations | 284 | 284 |
| R-squared | 0.770 | 0.767 |

Notes: Results from village-level regressions with pair fixed-effects. 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 1, regressions control for the number of projects financed by the incumbent mayor. In Column 2, regressions control for a dummy of whether or not the number of projects financed by the mayor was above the median. Regressions also control for the number of registered voters in the village, average years of educations of household heads in the village, the share of households who benefit from the government's large-scale CCT programme, the share of households engaging in farming, the share of households with at least one group member and the share of households who participate in bayanihan activities. All variables are interacted with the treatment dummy. The standard errors are (in parentheses). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.21: The mediating effects on the treatment of public investment in villages

| | Know Promises | Salience |
|---|---|---|
| | (1) | (2) |
| Panel A: | | |
| Treat | 0.051*** | 0.115*** |
| | (0.015) | (0.042) |
| Treat*Nb projects | -0.051 | 0.040 |
| | (0.102) | (0.264) |
| Observations | 3,408 | 3,346 |
| R-squared | 0.326 | 0.094 |
| Panel B: | | |
| Treat* | 0.037 | 0.045 |
| Nb projects above median | (0.026) | (0.073) |
| Treat* | 0.064** | 0.179*** |
| Nb projects below median | (0.028) | (0.069) |
| Observations | 3,408 | 3,346 |
| R-squared | 0.326 | 0.093 |

Notes: Results from individual-level regressions with pair fixed-effects. Dependent variables are: Column 1, an index capturing the respondent's knowledge of candidate promises; Column 2, the rating given to "Whether candidates will spend the municipal budget on things that are important to me and my family" when the respondent was asked about 'voting influences', adjusted to account for the average rating given to the other categories. In Panel A, regressions control for the number of projects financed by the incumbent mayor. In Panel B, regressions control for a dummy of whether or not the number of projects financed by the mayor was above the median. Regressions also control for the the number of registered voters in the village, average years of educations of household heads in the village, the share of households who benefit from the government's large-scale CCT programme, All variables are interacted with the treatment dummy. The estimated treatment effects are the same if they are excluded. The standard errors are (in parentheses). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.22: Effects of Treatment on Vote for the Incumbent

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Panel A: Did you vote for the incumbent? | | | | |
| Treat | -0.027 | -0.023 | -0.026 | -0.025 |
| | (0.034) | (0.023) | (0.017) | (0.017) |
| Municipal Fixed-Effects | No | Yes | No | No |
| Pair Fixed-Effects | No | No | Yes | Yes |
| Additional Controls | No | No | No | Yes |
| Observations | 3,077 | 3,077 | 3,077 | 3,077 |
| R-squared | 0.001 | 0.222 | 0.306 | 0.308 |
| Panel B: Incumbent vote share | | | | |
| Treat | -0.001 | 0.002 | 0.004 | 0.010 |
| | (0.024) | (0.014) | (0.013) | (0.013) |
| 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 | 0.000 | 0.670 | 0.831 | 0.836 |

Notes: Results from individual-level (Panel A) and village-level (Panel B) regressions. In Panel A, the dependent variable is a dummy equal to one if the respondent declared voting for the incumbent. In Panel B, the dependent variable is the incumbent vote share in the 2013 elections. In Column 4 of Panel A, the regression includes a dummy equal to one if someone in the household is a member of any group and a dummy equal to one if someone in the household participated in any collective action activity in the village in the past six months. In Column 4 of Panel B, the regression includes the share of respondents with an household member who belongs to a group and the share of respondent who participated in any collective action activity in the village in the past six months. The standard errors are (in parentheses). * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.23: The information treatment reduces support for incumbent among households that report no vote buying

| | (1) | (2) |
|---|---|---|
| Treat | -0.041** | -0.039** |
| | (0.019) | (0.019) |
| Pair Fixed-Effects | Yes | Yes |
| Additional Controls | No | Yes |
| Observations | 2,456 | 2,455 |
| R-squared | 0.344 | 0.349 |

Notes: Results from individual-level regressions. The dependent variable is a dummy equal to one if the respondent indicated voting for the incumbent. The sample includes all respondents who did not indicate that someone tried to buy their votes. In Column 2, the regression includes a dummy equal to one if someone in the household is a member of any group, a dummy equal to one if someone in the household participated in any collective action activity in the village in the past six months, alignment between the respondent and the incumbent, how long the respondent has lived in her current village of residence, family size, respondent's age, whether the respondent receive remittances from abroad and whether the respondent benefit from a large-scale CCT program. Standard errors (in parentheses) are clustered by village. * $p < .10$, ** $p < 0.05$, *** $p < .01$.

Table A.24: Voter Budget Preferences and Treatment Effects on Turnout and Vote Share

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

Notes: Results from precinct-level regressions (Panel A) and candidate*precinct-level regressions (Panel B). In Panel A, the dependent variable is turnout in the 2013 mayoral elections. In Panel B, the dependent variable is the candidate vote share in the 2013 elections. All regressions include a full set of candidate dummies. he standard errors (in parentheses) account for potential correlation within village. * $p < .10$, ** $p < 0.05$, *** $p < .01$.