Making policies matter: Voter responses to campaign promises *

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June 2021

Abstract

Do campaign promises matter? We combine a structural model and a large-scale field experiment disseminating candidate policy platforms in Philippine mayoral elections to show how voters respond to campaign promises. Voters who randomly received information about current campaign promises are more likely to vote for candidates closer to their own preferences; those also informed about past promises reward incumbents who fulfilled them. The structural model shows that information about campaign promises affects both the salience of policies and voter beliefs about candidate quality and policy platforms.

JEL Code: D72, P16
Keywords: Elections; Political Behavior, Bayesian Updating; Valence; Salience; Philippines.

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1 Introduction

Although campaign promises and their fulfillment are central to foundational models of electoral accountability, our understanding of how voters process this information and incorporate it into their vote choice is limited. This understanding is crucial to explaining voter responses to campaigns. In less consolidated democracies, candidates campaign on promises of direct transfers to voters rather than policies, significantly distorting public policies. If candidates began to make policy promises, would voters shift their support to candidates who make promises that align most closely with their preferences? Even in advanced democracies, the extent to which voters pay attention to policy promises is an open question, as is how they respond to them. In both settings, evidence is also sparse on whether voters change their evaluations of candidates depending on whether candidates keep their promises; it is not even clear whether voters track promise-keeping.

We address these fundamental gaps in the literature by combining a structural model of vote choice with a large-scale field experiment in the Philippines that provides voters with information on current and past policy promises of candidates. The analysis yields novel conclusions: even in electoral environments where politicians do not routinely make policy promises, vote choice and voter evaluations of candidates reflect voters' rational use of information about candidates' current policy promises and whether they kept previous policy promises.

The field experiment is the first, in either a developed or developing country, to examine the effect of information about both past and current policy promises. Prior to the 2013 and 2016 elections in the Philippines, we asked all mayoral candidates in seven municipalities to state how they would allocate their substantial local discretionary funds across ten spending categories. We use their responses to provide two types of information to voters before the 2016 elections. Voters in one treatment group of villages (*barangays*) receive information about the candidates' *current 2016 promises* regarding their proposed spending allocations. Voters in the second treatment group of villages receive identical information about candidates' current 2016 promises, plus information about candidates' *past promises from the 2013 elections*. Both groups of voters can evaluate the current campaign promises of the candidates in the 2016 races; the second group of voters could also determine whether current incumbent mayors fulfilled their 2013 promises.

Although policy-based campaigning is rare in the Philippines, where campaigns center around vote buying and clientelism, these interventions still had important effects on voter behavior. First, voters who received information about 2016 policy platforms were more likely to vote for candidates whose 2016 promises were closer to their ideal policy preferences than those of more distant candidates.[1] Second, voters who received information about the

[1] We collect individual-level data on policy preferences, allowing us compute distance with policy platforms, and vote choice. Most of the variation in policy preferences is within villages and thus we run our vote choice

2013 promises of the candidates are significantly more likely than control voters to vote for incumbents who fulfilled past promises.

Our structural model of vote choice further allows us to parse the underlying mechanisms activated by the treatment. In a setting where policy promises are not a common feature of campaigns, information about policy promises can affect voter evaluations of candidates by: (i) changing the salience of policy in vote choice; and (ii) affecting voter beliefs about candidate valence and policy positions. By comparing individuals with identical beliefs across treatment and control groups and looking at how preference shifts systematically match different vote choices, the structural framework cleanly separates the role of beliefs and preference parameters, to show that the information treatments operate through both increased policy salience and changes in beliefs among voters.[2]

Both the field experiment and structural model yield ample evidence that voters use information rationally. First, voters who are informed of candidate promise-keeping should change their evaluations of candidate quality (valence) and their subsequent probability of supporting the candidate. This is the case: although the experiment conveys no information on candidate quality, voters who received information about past promises perceived incumbents who kept their promises as more honest and competent. In addition, counterfactual exercises suggest that these assessments of candidate honesty and competence have substantial effects on incumbent support.[3] Second, consistent with rational updating, if information changes voter evaluations of candidates, voters should be more certain about candidate policy platforms. Our findings confirm that the second moments of respondents' subjective belief distributions tighten compared to those of control voters. Their beliefs about candidate policies are also closer to the actual policy promises that candidates had announced. We can further rule out that our results are driven by voters adjusting their own preferences to match those of their preferred candidate.[4]

Finally, the paper addresses a key puzzle posed by these findings: if programmatic

analyses at the individual-level rather than at the precinct-level. Importantly, votes reported by respondents are highly correlated with official votes in precinct-level results. Specifically, the correlation between official incumbent vote share at the village level and incumbent vote share computed from our sample is 0.77 and the correlations are identical in the treatment and control groups.

[2]Psychological dimensions of electoral campaigns (salience, awareness, etc.) are notoriously hard to pin down quantitatively and disentangling the effects of informational treatments in beliefs versus preferences is subject to nontrivial identification issues. Intuitively, the parameters governing preferences and those governing subjective beliefs typically appear in the form of interactions in a voter's expected utility and cannot be generally separated in standard discrete choice models of vote without additional information. What is crucial to our approach is the direct elicitation of the beliefs from voters, which provide the additional data anchoring identification along the beliefs dimension.

[3]For example, switching to an honest and competent incumbent increases the average probability of voting for the incumbent from a baseline of 37 percent to 47 percent across voters not already persuaded.

[4]There is no evidence that individual policy preferences are affected by our treatments. If they had been, we would expect to see the determinants of policy preferences in control households (e.g., farmer versus non-farmer preferences for agricultural extension services, or preferences for education among families with children versus no children) to be attenuated in treated households. In fact, as we discuss below in detail, these determinants are the same.

messages are effective in shifting vote shares, why do elections in the Philippines and other less consolidated democracies revolve around clientelism and vote buying rather than policy information? It turns out that although providing policy information is cheap and electorally effective, vote buying is more cost-effective based on the estimates presented in the paper.[5] These results suggest that private incentives may be insufficient to sustain the emergence of informational campaigns, giving rise to the systematic under-provision of policy information that seem to be endemic in political discourse across the world. At the same time, more optimistically, our results also point to a crucial role for credible, independent media or NGOs to provide the policy information necessary to create political environments conducive to programmatic politics and policy-based campaigns.

Our work addresses gaps in several strands of the literature. A large body of empirical research on electoral information and voter persuasion, summarized in DellaVigna and Gentzkow (2010), considers information in general (e.g. access to specific media sources) and has largely focused on established democracies.[6] In particular, our study extends previous work by Kendall et al. (2015) on the policy and valence effects of campaign messaging.

We embed the empirical analysis in a more sophisticated model of voter behavior that takes into account the influence of vote buying and information about past promises. As a consequence, we are able to test whether the introduction of policy platforms in campaign messaging affects policy salience and beliefs about candidates in a consolidating democracy, where policy promises are rare and vote buying has been the main currency of campaigning. Finally, by allowing the policy space to be multidimensional, we can show how the effects of information operate through policy preferences rather than relying on party affiliation or other factors that are difficult to disentangle from policy platforms.

Our findings are also relevant to the literature examining how politicians exploit the information deficiencies of voters in the developing world (Banerjee et al., 2011).[7] This large and rich literature is silent on the central issues that we consider in this paper: the impact of information on past and current promises on voter behavior. Rather, it focuses on how voters update evaluations based on information on incumbent actions, and how incumbents take advantage of ignorance of their behavior to shirk. Work in the Philippines has already documented that mayors take advantage of voter imperfect information by claiming credit for central government projects (Labonne, 2013; Cruz and Schneider, 2017) or by ramping up visible infrastructure projects before elections (Labonne, 2016). Other research

[5] An extensive literature documents the enforceability of vote buying in a number of contexts (see, e.g., Brusco et al. 2009; Finan and Schechter 2012; Nichter 2008; Stokes 2005). Vote buying is similarly enforceable in the Philippines (Canare et al., 2018; Cruz, 2018; Hicken et al., 2018, 2015; Ravanilla et al., 2017).

[6] Voter persuasion is also the subject of an active theoretical literature. For example, see Alonso and Camara (2016a,b) who study a Bayesian persuasion framework a la Kamenica and Gentzkow (2011), with and without uncertainty about voters preferences.

[7] For example, the coordinated field experiments in the Experiments in Governance and Politics *Metaketa I* initiative suggest that providing information to voters leads to mixed results for accountability (Dunning et al., eds, 2019).

has examined the effects of providing information on politician performance, attributes and campaign activities, though not campaign promises (Ferraz and Finan, 2008; Chong et al., 2015; Humphreys and Weinstein, 2013; Larreguy et al., 2015; Bidwell et al., 2015; Banerjee et al., 2018a; Arias et al., 2018; Dunning et al., eds, 2019). Other studies have focused on direct appeals to reduce clientelism and vote buying (Vicente, 2014; Hicken et al., 2015). A third set of studies has elements of both direct appeals to voters and information about politicians (Fujiwara and Wantchekon, 2013; Gottlieb, 2014), but again does not examine the effects of information about policy promises.

Two papers, Bidwell et al. (2015) and Brierley et al. (2018), examine how exposure to candidate debates influences voters. In Bidwell et al. (2015), debate exposure increases voter knowledge about the candidates, shifts voter policy preferences to those of their preferred candidate, and increases the vote share of candidates who performed well during the debate. In Brierley et al. (2018), debate exposure improves voters' evaluations of candidates. However, the striking effects of these debates on voter behavior do not allow us to make inferences on the impact of policy promises and promise-keeping, nor about the mechanisms through which information about promises influences voter behavior. Debates expose voters to many types of information and it can be difficult to infer which of these drive the effects. Even if candidates discussed their current and past policy promises, the debates' effects on voters could have instead been swayed by other debate features, such as the candidates' manner of speaking; the way in which they interact with other candidates; or the audience response to some candidates relative to others. Our structural approach also gives us greater ability, compared to this earlier work, to quantify the mechanisms through which information influences voter behavior–whether by affecting voter beliefs about candidate policy positions and valence; or by shifting the salience of policy. In the same way, we can also calculate policy and electoral counterfactuals.

The analysis here also raises interesting issues regarding foundational work in political economy, which assumes that campaign platforms are central to voter decision making (e.g., Downs (1957)). Consistent with Downs, the policy promises of competing mayoral candidates appear to converge. However, the foundational analysis assumes that convergence occurs under the following conditions associated with established democracies: partisan divisions corresponding to socio-economic cleavages and institutional arrangements that increase the likelihood that candidates will carry out their promises. In these settings, political parties have policy platforms, new parties emerge infrequently, and party-switching among politicians is rare. The Philippines and other clientelist democracies lack these institutional arrangements and political commitment. Our finding that information about promises can matter even in less consolidated democracies, where Downsian assumptions appear not to hold, raises interesting issues for future theoretical and empirical research.

Although set in the Philippines, the analysis also informs research on voter responses to political promises in established democracies. Two obstacles confront this work. One is the difficulty of disentangling incumbents' past policy decisions from candidates' promises regarding future policies.[8] Another is that when choosing between well-established parties, it may not be the information conveyed by a party's label about the policy commitments of its candidates that persuades them, but rather deeply-rooted psychological attachments, influenced by social identity, that are affected by party affiliation (e.g., Lenz, 2013). Our research design reduces these obstacles. First, we can distinguish the impacts of past and future promises. Second, municipal elections in a country in which parties are weak and evanescent allow us to discount the party identification effect and isolate the influence of campaign promises on voter behavior.[9]

Finally, the research here complements work by Cruz et al. (2021). Taken together, the two experiments reveal new dynamics about the move from clientelist to programmatic politics.[10] Their experiment took place in a group of municipalities in the Philippines that includes the seven municipalities examined here. Just before the 2013 mayoral elections, they distributed similar information about public spending and candidates' intended allocations. This was, however, the *first* time that voters had been systematically exposed to information either about local public spending or about candidate promises regarding allocations. In this context, where voters had no prior exposure to policy promises, the information about the basic capabilities of the local government raised voter expectations of incumbent politicians. Promises, though, did not influence voter behavior.

Instead, consistent with Aragones et al. (2007), the first round of flyer distribution prior to the 2013 municipal elections may have led to a shift to an equilibrium in which candidates could subsequently make credible policy promises.[11] Subsequent to the 2013 intervention, voters knew of the resources available to provide public goods and of incumbent intentions regarding public good allocations. The 2016 intervention analyzed here therefore took place in a much different environment, in which it was possible for past and future policy promises to have a more substantial impact on voter behavior.[12] By the time information about candidate promises was distributed in our 2016 experiment, the electoral equilibrium had plausibly shifted to one in which it was not unfeasible to explore the effects on voter behavior of information about past and future policy promises.

[8]For example, Ansolabehere and Jones (2010) provide evidence that the past policy votes of legislators affect voter intentions to support them.

[9]Elinder et al. (2015) show that parents of young children responded more negatively than parents of older children to a promise by the Swedish Social Democrat party to cut subsidies to parents with young children and more positively to promises to cap childcare fees. We show responsiveness to promises using experimental methods, along multiple policy dimensions and in a setting where promises are not supposed to matter.

[10]Further comparison of the two experiments is available in Appendix A.3.1.

[11]Aragones et al., 2007 show that equilibria can emerge in which candidates make credible policy promises even in the absence of institutional arrangements that facilitate their enforcement by voters.

[12]Incumbents put more effort into providing public goods, proposed budgetary allocations became more salient, and voters and incumbents had reason to believe that voters would punish incumbents who did not fulfill their promises.

2 Empirical Model

We consider a first-past-the-post election between electoral (mayoral) candidates A and B. Consistent with municipal decision making in the Philippines, elected mayors are assumed to be budget dictators, allocating resources across K categories of public goods and administering the locality based on their overall ability/valence. Voters are assumed to obtain utility from private consumption and a vector of K public goods.[13] Voters also care about an M-dimensional vector of valence characteristics (competence, honesty, experience, etc.) $\mathbf{v}_j$ for each candidate $j = A, B$.[14]

Let us express each $k = 1, ..., K$ policy variable in terms of its share of total budget $1 \geq p^k \geq 0$ (measured at 0.05 discrete increments in our application). The size of the total municipal budget is assumed exogenous (almost entirely financed by the central government).[15] We normalize total budget to 1. A policy vector $\mathbf{p} = \left[p^1, ..., p^K\right]$ belongs to the finite discrete policy/ideology simplex:

$$\mathcal{P} = \left\{ \mathbf{p} \in \mathbb{R}^K : p^k \geq 0, \sum_{k=1}^{K} p^k = 1 \right\}.$$

Once elected a mayor j implements a specific policy vector $\mathbf{p}_j \in \mathcal{P}$, which may be interpreted as the candidate's type within a citizen-candidate framework. For evidence on the realism of this assumption we refer to Ansolabehere et al. (2001) or Lee et al. (2004).

Voters are assumed to be heterogeneous in preferences, with each voter i evaluating policies relative to her ideal point $\mathbf{q}_i \in \mathcal{P}$ and caring about the valence characteristics of the candidate $\mathbf{v}_j$. Before being elected, j may transfer to voter i $z_{ij} \geq 0$ monetary value (in exchange of their vote, a patronage transfer, etc.).

Let the utility of voter i of type $\mathbf{q}_i$ be defined in the following additively separable form:

$$U_i(z, \mathbf{v}, \mathbf{p}) = \alpha_i z_{ij} + \gamma_i \mathbf{v}_j - \omega_i \times \| \mathbf{p}_j - \mathbf{q}_i \|^{\zeta_i} + \varepsilon_{ij}, \tag{1}$$

where $\mathbf{p}_j$ is policy implemented by the elected mayor j; α_i, γ_i, ζ_i, ω_i are individual utility weights to be estimated and $\| \, . \, \|^{\zeta_i}$ indicates a generic loss function with curvature $\zeta_i \geq 0$, not necessarily larger than 1 (i.e. we do not impose quadratic or even convex losses). The deterministic component of preferences is augmented by a random utility component ε_{ij} specific to the i, j match. This specification may be extended for the interaction of valence and policy platforms.[16]

[13] In our empirical section we have $K = 10$.

[14] In our empirical section we have $M = 6$.

[15] For the average municipality, fixed transfers from the central government pay for 85 percent of municipal spending (Troland, 2014).

[16] In previous research, Kendall et al. (2015) show that interactive elements of preferences (1) (for example, between valence and policy position of a candidate) can be easily introduced in an analogous setting, but find them to be not statistically significant. For this reason, we omit interactions from the current analysis of (1). Instead, in the same paper, a generic form for the loss function $\| \, . \, \|^{\zeta_i}$ plays a relevant role, with loss parameters

We now specify the voters' information set. Let us indicate with $\phi_j = \left[\phi_j^1, ..., \phi_j^K\right] \in \mathcal{P}$ the policy platform that candidate j declares in his electoral campaign (in our empirical application these are the campaign platforms announced in 2016). Indicate with $\phi_j^0 \in \mathcal{P}$ the previous term's electoral promises, available if j is a repeat candidate (in our empirical application these are the campaign platforms announced in 2013). Voters are assumed to know $\mathbf{p}_j^0 \in \mathcal{P}$, that is the previous term's implemented policy in their municipality. $\mathbf{p}_j^0$ is only available if j is the incumbent.

Individuals are uncertain about the likelihood of the actual $\mathbf{p}_j$ that candidate j will implement once in office. Subjective beliefs have some dispersion over the policy-valence support because voters may be uninformed about certain policy dimensions, or because of vagueness or inconsistency of campaign promises ϕ_j, or because platforms may not be fully credible.

Let us indicate with $f^{i,j}(\mathbf{v}, \mathbf{p})$ voter i's prior joint distribution function for $j = A, B$. $f^{i,j}(\mathbf{v}, \mathbf{p})$ is to be thought of as a discrete, but highly dimensional, subjective belief distribution. To see this, recall that each p_j^k can take 20 values, for 10 public goods categories. Possible budget allocations are then elements of the simplex $\mathcal{P}$, which has high cardinality.[17] Priors are different for each voter i and for each candidate j and are allowed to depend on individual covariates and are not required to be independent across candidates.

Our experimental strategy affects voter priors, by inducing exogenous variation in voters' information set. Exact details of the experimental design are provided in Section 3, but to fix ideas, let us consider randomly dividing voters into treatment and control groups $H \in$ {T1;T2;C}. Three experimental arms are defined. T1 voters receive a message about current policy platforms $\left\{\phi_j\right\}_{j=A,B}$. T2 voters receive a message about current $\left\{\phi_j\right\}_{j=A,B}$ and past platforms $\left\{\phi_j^0\right\}_{j=A,B'}$, where without loss of generality A is indicated as the incumbent and B, B' the current and past challenger. Voters in group C receive no electoral message.

Finally, let us indicate with $f^{i,j}(\mathbf{v}, \mathbf{p}|H = h)$ a voter's joint posterior distribution function, conditional on the information received experimentally (h).

2.1 Additional Components of Voting Behavior and Likelihood

Before defining the likelihood function for our problem, we allow voters an additional margin of response to H, namely through their preferences. By heightening awareness or the salience of specific choice dimensions, the information treatment may also affect voter policy preferences, reflected, for instance, in a higher utility weight ω_i for a treated voter i (exoge-

statistically different from commonly assumed quadratic losses (an assumption typically imposed for analytical convenience). We maintain flexibility along this margin.

[17]Even limiting $K = 3$ policy dimensions and no valence, full elicitation for each candidate j would require 231 questions ($= 21 * (21 + 1)/2$). Direct elicitation of the individual belief distributions is, even for expert responders, unfeasible with $K = 10$.

nously made aware of, say, the mayor's role in education or health services provision) relative to a control voter (unaware of such role in policymaking). This psychological dimension of choice has a long tradition in the literature on political opinion, salience, and the importance of attributing credit or blame to politicians (Achen and Bartels, 2004; Cruz and Schneider, 2017; Grimmer et al., 2012).

We allow i, conditional on treatment status $H = h$, to have preference parameters:

$$\begin{aligned}
\alpha_i &= \alpha^0 + \alpha^1(h) \\
\gamma_i &= \gamma^0 + \gamma^1(h) \\
\omega_i &= \omega^0 + \omega^1(h)
\end{aligned}$$

where we normalize $\alpha^1(C) = \gamma^1(C) = \omega^1(C) = 0$.

Within our empirical environment this specification element can be tested formally. Anticipating some of our results, we will see below that a restricted model not allowing for salience can be statistically rejected against this more general structure of preferences allowing this psychological response. In our structural estimation section we also tackle potential misspecifications of salience.[18]

The expected utility for voter i from the election of candidate j can now be defined as:

$$\mathbb{E}U_j^i(h) = \alpha_i z_{ij} + \sum_{\mathbf{v},\mathbf{p}} f^{i,j}(\mathbf{v},\mathbf{p}|h) \times \left(\gamma_i \mathbf{v}_j - \omega_i \times \parallel \mathbf{p} - \mathbf{q}_i \parallel^{\zeta_i}\right) + \varepsilon_{ij}$$

Making use of the random utility components ε_{ij}, the probability that voter i votes for A (i.e. i chooses action $Y_i = A$) can be defined as:

$$\Pr(Y_i = A) = \Pr\left[\mathbb{E}U_A^i(h) \geq \mathbb{E}U_B^i(h)\right],$$

which is used to construct the likelihood function of our problem.

Specifically, defining an indicator variable $d_{ij} = 1$ for i voting for j, and 0 otherwise, under the assumption of Type I extreme value distribution for $\varepsilon_{i,j}$, i.i.d. with CDF $F(\varepsilon_{ij}) =$

[18] The treatment could also affect voter bliss points $\mathbf{q}_i(h)$. Indeed, the literature on political behavior has sometimes focused on how candidate announcements may shape voters' ideal policy positions. We do not find any direct evidence of this phenomenon in the analysis of Section 4 and therefore omit it from the presentation of the model.

$\exp\left(-e^{-\varepsilon_{ij}}\right)$, we obtain:

$$\begin{aligned}\ln L(\theta) &= \sum_{i=1}^{N}\sum_{j} d_{ij}\ln \Pr\left(Y_i = j\right)\\ &= \sum_{i=1}^{N}\sum_{j} d_{ij}\ln \frac{e^{\left(\alpha_i z_{ij}+\sum\limits_{\mathbf{v},\mathbf{p}} f^{i,j}(\mathbf{v},\mathbf{p}|h)\times\left(\gamma_i \mathbf{v}_j-\omega_i\times\|\mathbf{p}-\mathbf{q_i}\|^{\zeta_i}\right)\right)}}{\sum_{l=A,B} e^{\left(\alpha_i z_{il}+\sum\limits_{\mathbf{v},\mathbf{p}} f^{i,l}(\mathbf{v},\mathbf{p}|h)\times\left(\gamma_i \mathbf{v}_l-\omega_i\times\|\mathbf{p}-\mathbf{q_i}\|^{\zeta_i}\right)\right)}}.\end{aligned}$$

There is a final specification improvement that we add to the log-likelihood above. The estimation of this log-likelihood relies for unbiasedness on a “missing completely at random" (MCAR) assumption for voter non-response. Non-response rates are about 8 percent in our full sample. Voters supporting winning candidates, however, typically reveal their vote at differential rates relative to voters supporting losing candidates. We provide evidence in Section 5 that indicates that the sub-sample of voters choosing to hide their votes is predictable, so direct estimation of the model would lead to biased estimates in our setting. MCAR appears violated. This is shown through an application of the choice-based approach suggested by Ramalho and Smith (2013) that allows to incorporate non-random non-response under weak assumptions. The assumption is that, conditional on the voter’s actual voting decision (his/her choice), the probability with which a voter chooses to respond to the survey is constant. The probability of non-response is therefore allowed to depend on vote choice and can be estimated. Under this assumption, the log likelihood is:

$$\ln L(\theta) = \sum_{i=1}^{N}\left[o_i \sum_{j} d_{ij}\ln \beta_j \Pr\left(Y_i = j\right) + (1-o_i)\ln\left(1-\sum_{j}\beta_j \Pr\left(Y_i = j\right)\right)\right],$$

where o_i is an indicator function taking value 1 if i discloses the vote, and 0 otherwise.

The additional β_j parameters are the probabilities with which a voter discloses the vote for j. The first term of the log likelihood is the probability that a voter votes for j and discloses her vote. The second term reflects the probability that the voter votes for one of the candidates, but chooses not to disclose his/her vote. This is the log likelihood that we estimate.

2.2 Elicitation of Subjective Posteriors

In our setting, direct nonparametric elicitation of individual belief distributions $f^{i,j}(\mathbf{v},\mathbf{p}|h)$ (e.g., Manski, 2004) is not feasible due to issues of dimensionality. This would be true even for expert respondents, let alone regular voters in the Philippines.

The approach we follow is different and it is designed to integrate data derived from direct elicitation with flexible structural econometric elements. As this approach may be of methodological value in the design of complex multidimensional belief elicitation surveys beyond the context of voting, we present the details of its implementation in Appendix A.1.

2.3 Subjective Updating

Our predictions about the effects of our interventions on voter behavior are based on assumptions we make about how voters learn from the information we provide them. We outline these assumptions here, with full exposition and examples in Appendix A.2.

First, our starting assumption is rational use of information. The policy platforms elicited from candidates reach voter *i* and are incorporated in her beliefs.

Second, we impose no restrictions on the underlying signaling game between politicians and the voters. The game may take a variety of theoretical forms, many of which have been discussed in the political economy literature (Chappell, 1994; Callander and Wilkie, 2007; Bernhardt et al., 2011). For instance, one could allow for voter beliefs about politician valence to respond to information about policy, or even allow for cross-learning about all candidates from the policy choices of each of them. Given that such restrictions are not necessary for our empirical approach, we allow the strategic interaction between candidates and voters to be general.

Third, we allow voters to update on relevant political events occurring in parallel to our treatment. These events would include campaign activities naturally occurring in each electoral race and affecting voters independent of assignment to treatment status.

Fourth, we assume that information provided to treated subjects does not spill over to subjects in the control areas. The stable unit treatment value assumption (SUTVA) is especially important in informational experiments, because of the possibility that information can diffuse through social networks (Banerjee et al., 2018b). We validate SUTVA empirically and do not detect substantial violations.[19]

3 Institutional Setting, Experiment, and Data

There are 1,489 Philippine municipalities, each governed by a mayor, elected at-large every three years.[20] The Local Government Code passed in 1991 devolved a number of responsibilities to municipalities, including local infrastructure projects, health and nutrition initiatives, and other client-facing services (Khemani, 2015; Llanto, 2012). In turn, the federal government implemented a system of fixed transfers to the municipalities, which constitute 85

[19]Our design treats entire villages precisely because of likely contamination arising within village, avoiding the most plausible source of violation. Furthermore, we do not detect a gradient in similarity of behavior when focusing on the differential behavior of subjects residing in different control villages with more or less social connections to treatment villages.

[20]Municipalities are composed of villages (about 20-25 on average).

percent of municipal spending (Troland, 2014). Laws governing transfers to municipalities encourage municipalities to allocate 20 percent of transfers to development projects.

Mayors exercise broad budgetary discretion and control over municipal spending priorities and are often characterized as 'budget dictators' who are not subject to any meaningful institutional checks and balances (Capuno, 2012; Sidel, 1999). As a result, unlike politicians in the national legislature or local politicians in other countries without executive powers, voters in the Philippines can reasonably attribute municipal spending and programs to the efforts of their mayor (Abinales and Amoroso, 2017; Rogers, 2004).

As in many other democracies in the developing world, Philippine politics is characterized by clientelist politics (Abinales and Amoroso, 2017; Timberman, 1991). Campaigns tend to have little or no policy content and parties are more likely to be known for personalities or family alliances than for platforms and programs (Hutchcroft and Rocamora, 2003; Kerkvliet, 2002; Montinola, 1999; Mendoza et al., 2014).

Vote buying is prevalent and widely accepted, and the price per vote generally ranges from PHP 100 to PHP 1,500 per household (approximately \$ 1.96 to \$ 29.50 USD). The price per vote varies with local economic conditions and the competitiveness of local elections. In our study area, the vote buying rates tend to be higher than in other parts of the country: around \$20-\$50 per household (which typically includes at least 4 voting age individuals). These are significant amounts, given that the poverty threshold in 2015 was PHP 302 (\$ 5.83) per day for a family of 5. Twenty-one percent of the population falls below that threshold.[21]

3.1 Design of the Experiment

Our experimental design spans two consecutive mayoral elections in the Philippines, in 2013 and 2016. A few weeks before each of the elections, survey enumerators collected data from every mayoral candidate in order to produce flyers that described candidate spending priorities and policy promises.[22]

A non-governmental organization, the Parish Pastoral Council for Responsible Voting (PPCRV), distributed the flyers containing the information collected from candidates to all households in randomly selected villages in the days leading up to the elections.[23]

[21] Source: https://psa.gov.ph/content/poverty-incidence-among-filipinos-registered-216-2015-psa visited on May 4, 2018.

[22] Candidates were identified using the official list of registered candidates produced by the Commission on Elections (COMELEC). All incumbents ran and between 1 and 3 candidates challenged them. In one municipality the incumbent wasn't the candidate elected in 2013 as he was removed from office due to corruption (and was replaced by his vice-mayor). Results are robust to excluding that municipality. The information campaign was designed to incentivize participation: most candidates were eager to participate (only one refused in 2013 and all agreed in 2016), even contacting PPCRV to ensure that they would be included. Incumbent willingness to participate may appear puzzling, given that the effect of the information treatment was to decrease incumbent support in 2013 (Cruz et al., 2021). However, since incumbents knew that the flyer would be distributed regardless of their participation, their preferred response was to ensure that at least their own spending priorities and programs would also be shared with voters.

[23] A copy of the 2016 flyer is included as Figures A.1 and A.2. The translation is available in Table A.3.

The two-arm treatment design allows us to assess the effect of two specific types of information necessary for voters to evaluate the candidates for office: (i) what candidates propose to do if elected; and (ii) whether the incumbent politician fulfilled her previous policy promises. Households in the first treatment arm (T1) received only the flyers produced in 2016, containing the information provided by the current candidates for office. Households in the second treatment arm (T2) received *both* the 2016 and the 2013 flyers for their municipality. The 2013 and 2016 flyers have identical formatting. Since 2016 incumbents were necessarily candidates in 2013, the 2013 flyers contain the proposed budget allocations that were made by the current incumbent mayor.

We did not explicitly inform voters whether their incumbent mayor kept budgetary promises made in 2013. This intervention was perceived as excessively intrusive by candidates according to our preliminary interaction phase with them. Instead, we decided to provide voters only with 2013 campaign promises information necessary to make this assessment, in combination with their own knowledge of their municipality during the 2013-2016 period.

The candidate data collection process was identical in 2013 and 2016. Candidates were told that the information they provided would be given to randomly-selected villages in their municipality prior to the election, but not which ones. In the course of the interview, we gave each candidate a picture worksheet with a list of ten sectors. Candidates were asked to allocate money across sectors. To facilitate this exercise, candidates received 20 tokens to place on the worksheet and were told that each token represented five percent of the total budget.[24] Descriptive statistics on the promises are available in Tables A.4 and A.5.

Villages were allocated to T1, T2, and control using a matching algorithm.[25] The final sample includes 158 villages: 54 T1, 50 T2 and 54 control villages in seven municipalities. (cf. Table A.1).

PPCRV prepared flyers showing the proposed allocations of all candidates in each municipality for both the 2013 and 2016 elections. Then, in the week leading up to the election, trained PPCRV volunteers distributed the flyers to all households in target villages through door-to-door visits. The teams were instructed to visit all households in the village and give the flyer (or two flyers in the case of T2) to the head of household or spouse, and in his or her absence, a voting-age household member.[26]

[24]Candidates took this task seriously, considering their allocations carefully and often moving tokens (poker chips) around several times before being satisfied with their allocation.

[25]First, for all potential triplets of villages, the Mahalanobis distance was computed using number of registered voters, number of precincts, an urban/rural dummy, incumbent vote share in the 2013 elections, prevalence of vote-buying in 2013, salience of budget allocations in 2013 and knowledge of electoral promises in 2013. Second, the partition that minimized the total sum of Mahalanobis distance between villages in the same triplets was selected. Third, within each triplet, a village was randomly selected to be allocated to T1, a village was randomly selected to be allocated to T2; the other one serving as control. In two cases, the number of villages was not a multiple of 3 and we created a pair instead of a triplet. In those cases, a village was randomly allocated to T1; the other serving as control.

[26]Due to time constraints, there were no additional visits on different days if no voting-age household member

For each household visit, volunteers used a detailed script to explain the information campaign to voters. The script emphasized the following: (i) the distribution of flyers was part of the PPCRV's non-partisan voter education campaign; and (ii) the information contained in the flyers came directly from the candidates themselves. 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 instructed voters on what conclusions to draw from the information. A detailed timeline of the experiment is available in Table A.2. The experiment was registered on the AEA RCT registry on May 5, 2016.[27]

The results in Table A.7 indicate that the village-level variables used to carry out the 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. Overall the groups are well balanced.[28]

3.2 Data

We implemented a detailed household survey in 158 villages shortly after the May 2016 elections. In each village, the field team obtained the official list of registered voters and randomly selected 22 individuals to be interviewed for a total sample size of 3,476. Descriptive statistics for the variables not displayed in the balance tests tables are available in Table A.10.

Vote Choice. Across the seven municipalities where our experiment took place, incumbents won 68.5 percent of the vote, on average. We collected data on respondents' vote choice. In order to reduce the tendency of respondents to claim they voted for the winner when they did not, we used a secret ballot protocol.[29]

The vote choice data collected using this module appear reliable and unaffected by the treatment. The votes reported by subjects are highly correlated with official votes in precinct-level results that correspond to respondent villages. Specifically, the correlation between official incumbent vote share at the village level and incumbent vote share computed from our sample is 0.77 (See Figure A.3). The correlations are identical in the treatment (0.77) and control groups (0.78). In addition, the likelihood of refusing to answer the vote choice question is similar between the treatment (6.9 percent) and control (8.1 percent) group (p-value 0.243).

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was present on the day of the visit. Our enumerators did not report problems with contacting households with the flyers.

[27]Relevant documents are available at https://www.socialscienceregistry.org/trials/1210

[28]This set of results is available in Table A.6-A.9.

[29]The protocol was implemented as follows. Respondents were given ballots with only ID codes corresponding to their survey instrument. The ballots contained the names and parties of the mayoral candidates in the municipality, in the same order and spelling 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.

Voter preferences over budget spending. We used the same method as the one used to elicit candidate promises to ask respondents about their ideal policy allocations, $\mathbf{q}$. Respondents were given a picture worksheet with a list of ten sectors. Enumerators informed them of the amount of their local development fund and that local governments face a number of options in terms of how to allocate a budget. Then respondents were asked to consider their own preferences for allocation. This approach was developed by Cruz (2018) to reduce the cognitive demands of expressing preferences in situations where there are multiple choices with explicit and clear trade-offs. The combination of picture worksheets and tokens is especially helpful for respondents with lower levels of literacy and numeracy. As in the candidate surveys, respondents took this task seriously, considering their allocations carefully and often moving tokens around several times before being satisfied with their allocation.

Voter beliefs about candidate policies. We then collected data on voter beliefs about the proposed policies of candidates, $\mathbf{p}$. Direct elicitation of those beliefs is not possible in this context, as they are high dimensional objects, necessitating adjustments to reduce the cognitive demands of the survey modules.

To collect data on voter beliefs about candidates' policies, after voters expressed their own policy preferences (as described in the previous section), enumerators asked them to repeat the exercise, this time indicating what budget allocations they thought the candidate would choose (for voter i and candidate j it is a vector $\pi_{i,j} = [\pi^1_{i,j}, ..., \pi^K_{i,j}]$). To facilitate direct comparison across candidates and reduce bias resulting from the order in which the candidates were considered, respondents were asked to consider allocations for all candidates, one sector at a time (sectors were also shuffled to reduce concerns with question order). Respondents were given a set of tokens that they could allocate to each sector, with a different color for each candidate. As in the previous exercise, once respondents completed the worksheet, they were given an opportunity to review and reallocate their poker chips as needed.

After respondents completed the exercise, enumerators then asked them how certain they were, across all candidates, of their allocations. The procedure described in detail at the end of Section 2.3 shows how this information can be used to recover subjective beliefs distributions for all voters and, for each voter, a different distribution for each candidate, making our approach both unrestrictive and flexible.

Voter beliefs about candidate valence. We collected data on voters' beliefs about candidate valence along the following dimensions: (i) Approachable/Friendly;[30] (ii) Experienced in

[30] In the Philippine context, "approachability" refers to a general friendliness or helpfulness of politicians, compared to politicians that may be considered more aloof. While this may call to mind the ability to approach politicians for favors, extensive pre-testing of this question suggests that respondents differentiate between approachability or helpfulness in general and clientelist access that is specific to those that are part of the politician's network.

politics; (iii) Honest; (iv) Politically well-connected; (v) Gets things done; (vi) Understands the problems of citizens like me. Again, in order not to excessively load the cognitive requirements of our survey, we avoided eliciting from voters the full distribution for $\mathbf{v}$, which would have been as demanding as the distribution of $\mathbf{p}$. We opted for a simpler elicitation for valence, by focusing on which j candidate dominates in expectation along each of the six dimensions.[31] As voter preferences are linear and monotonically increasing along all valence dimensions, this is in fact the relevant information needed for $\mathbf{v}$ in the computation of $\Pr(Y_i = j)$.

Similarity. We expect the treatment to cause voters to select candidates whom they believe will pursue policies that are closer to how they want the budget to be allocated. For use in the reduced-form analysis, we first compute the similarity between voter i ideal point $\mathbf{q}_i$ and modal candidate j's policy $\pi_{i,j}$ as a measure of distance between the two vectors:

$$Similarity_{ij} = 1 - \sqrt{\frac{1}{2}\sum_{k=1}^{K}\left|\pi_{i,j}^{k} - q_i^k\right|^2} \tag{2}$$

For robustness purposes, the similarity measure can be constructed based on a number of different sectors: for individual i's top sector, top 2 sectors, top 3 sectors as well as for health, education and agriculture (the three main sectors), and all sectors. These alternative measures are useful to ascertain the possible fragility of our results to imposing excessive policy detail or cognitive overload from focusing on irrelevant dimensions.

Spatial voting theory generically implies that likelihood of the decision of i to support incumbent politician A versus challenger B must be increasing in the difference between how close voter i is to A relative to B. We therefore define:

$$\Delta Similarity_{i,A,B} = Similarity_{iA} - Similarity_{iB} \tag{3}$$

Within each electoral race, the notation can be simplified to $\Delta Similarity_{i,A,B} = \Delta Similarity_i$.

Clientelist ties and vote buying. We also expect that voters with clientelist ties to one of the candidates will respond more weakly to our information treatment. To capture clientelist ties, we asked respondents to report information on their links to the mayor. 18 percent of survey respondents report having a direct link with the mayor (such as family ties).

[31] The question was worded as follows: "Now we're going to show you a set of worksheets—one for each candidate—as well as some flashcards containing some traits 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 doesn't apply to any of the candidates." To reduce concerns about question ordering effects, the candidate worksheets were presented at the same time and the flashcards were shuffled for each respondent.

41 percent of respondents report an indirect link to the mayor through one intermediary (distance of two from the incumbent). We code individuals as clients if they are connected to the mayor through a politician (e.g. barangay captain, councilor, etc.). About 15 percent of our respondents fall into that category.

In addition, we asked respondents how easily they can access a series of common clientelist goods (on a 10-point scale). We then classify voters as clients along that dimension if they are above the median. We measure this along the following dimensions: ease with which they can ask politicians to provide an endorsement letter for a job, to pay for funeral expenses, or to pay for medical expenses.

We also collected data on whether survey respondents were offered money for their vote, as well as the amounts of money that were offered. Although most research relies on survey evidence for vote buying, one concern with such measures is social desirability bias. However, such questions are not considered sensitive in the Philippines, and direct questions on vote buying are commonly included in surveys on politics.[32] Cruz (2018) finds that the estimated rate of vote buying using an unmatched count technique is statistically indistinguishable from the estimate calculated using the direct question, suggesting that vote buying can be elicited directly.

Fulfilling promises. We use data from the household survey to measure the incumbent mayors' sectoral allocations during the 2013/2016 term. Each respondent can list up to 5 projects implemented by the municipality between 2013 and 2016. We start by matching those responses to the 10 sectors included in the flyers and count the number of projects in each sector by each respondent. We then aggregate the individual-level responses to the village-level and compute the share of projects in each sector (p_k^0). As in equation (2), this allows us to measure similarity between projects implemented between 2013 and 2016 in each village and incumbent promises made prior to the 2013 elections (ϕ_k^0) as the distance between the two vectors:

$$Similarity_p^{\phi^0} = 1 - \frac{1}{\mu}\sqrt{\sum_{k=1}^{K} \mid p_k^0 - \phi_k^0 \mid^2}$$

where μ ensures that the measure is between 0 and 1.[33] To indicate incumbents who have fulfilled their promises in a village, we created a dummy variable, *Kept*, which equals 1 when $Similarity_p^{\phi^0}$ is greater than 0.5, 0 otherwise.

To validate data from the household survey, we gathered data from municipal accountants

[32]See, e.g., Canare et al. (2018) and Khemani (2015). Prior to this study, vote buying questions have been included in surveys conducted by Cruz in 2010, 2011, 2013, and 2015. Both pre-testing and enumerator reports suggest that respondents are comfortable asking whether vote buying occurs in their village, whether they were offered money for their vote, and even the amounts of money offered.

[33]It is the maximum of $\sqrt{\sum_{k=1}^{K} \mid p_k^0 - \phi_k^0 \mid^2}$ for incumbents in our sample.

and engineers on projects implemented by the municipality between 2013 and 2016 (and their cost).

This audit data was collected at the municipal-level. We then computed budget shares and compare them to the project shares computed from the household survey. Table A.11 presents these comparisons in detail. The shares are remarkably similar across the two methodologies and for two different methods of aggregating the household survey data.

4 Reduced-Form Estimation

This section shows that voters respond to the information provided by the experiment in ways consistent with the intuition of the model presented in Section 2. First, treated voters incorporate complex information about candidate promises when deciding for whom to vote. Information changes their subjective beliefs about candidate policy positions. Second, information about incumbents' past policy promises allows voters to infer whether incumbents have fulfilled their promises. Because this changes their beliefs about valence–whether incumbents are more honest and competent–informed voters are more likely to support incumbents who fulfill their promises. Third, clientelist ties substantially attenuate the effect of informational treatments.

4.1 Treated voters are more likely to vote for the candidate whose policies are closer to their own preferences

We start by estimating a spatial voting regression of the form:

$$Y_{ivl} = \delta^0 T_{vl} + \delta^1 \Delta Similarity_{ivl} + \delta^2 T_{vl} \times \Delta Similarity_{ivl} + v_l + u_{ivl} \tag{4}$$

where Y_{ivl} is a dummy equal to one if individual i in village v in triplet l reported voting for the incumbent in the 2016 elections.T_{vl} is a dummy equal to one if the intervention was implemented in village v. Recall that $\Delta Similarity_{ivl}$ is defined in equation (3) and refers to the difference between the similarity of the voter i's ideal point to incumbent expected policies and the similarity of voter i's ideal point to challenger expected policies.[34] For example, $\Delta Similarity_{ivl}$ would be large if the voter is much more similar in policy preferences to the incumbent than the challenger, and would be small if the voter were equally distant or close to both.

It is important to note that most of the variation in $\Delta Similarity_{ivl}$ is within rather than across villages: if we regress $\Delta Similarity_{ivl}$ on a set of village fixed effects, the R-square of the regression is 0.07. This is to be expected: villages are similar in the distribution of

[34]In cases where we have more than one challenger, we take the difference between the incumbent and the challenger to which voter i is the closest. This happens in two out of seven municipalities, while the remaining five elections have two candidates.

households that reside in them, but within villages households are heterogeneous. It is therefore not meaningful to aggregate data to the village-level and to use the official voting data as an outcome. Instead, it underscores how focusing on individual vote choices is the most informative direction of analysis.

The analysis focuses on individual, survey-reported vote choices rather than official voting data at the village level because village level comparisons are uninformative: $\Delta Similarity_{ivl}$ varies significantly within villages, but not across villages (regressing $\Delta Similarity_{ivl}$ on a set of village fixed effects yields an R-square of 0.07). This is to be expected, given that villages in the sample are similar. As a consequence, average households exhibit less variation across villages than do individual households within villages.

Given that we randomized within triplets and assigned treatments at the village-level, all regressions include a full set of triplet fixed effects and standard errors are clustered at the village-level. The coefficient δ^1 in equation (4) captures the extent to which respondents in the control group vote spatially–vote for incumbents who are closer to them in the policy space. The coefficient of interest is δ^2, measuring the degree to which the informational treatment increases the effect of policy promises on vote choice.

Consistent with the model discussed in Section 2, Panel A of Table 1 shows that voters treated with information about candidate promises are more likely to vote for the candidate whose promises are closest to them in the policy space.[35] Based on the estimates of δ^2, a one standard deviation increase in the measure of $\Delta Similarity$ increases the likelihood of voting for the incumbent by 3-4 percentage points. This is a noticeable effect given that the control group mean of the outcome variable is 68.9 percent. This is true whether we restrict the similarity measures to the voter's preferred sector (Column 1), two preferred sectors (Column 2), or three preferred sectors (Column 3). We find similar results if we only look at similarity for health, education and agriculture assistance (Column 4) or for the 10 sectors jointly (Column 5).

An estimated coefficient $\delta^2 > 0$ could be significant and positive for two reasons: (i) voters learning about different campaign promises and voting for the candidate whose policies are more similar to theirs (learning); or (ii) voters increase the importance they place on policies in their vote choice (salience). Further analysis below leverages the structural model to demonstrate that the treatment effect in fact operates through both mechanisms.

[35]We also provide evidence that allows us to rule out violations of the stable unit treatment value assumption (SUTVA) as discussed in Section 2. Using a measure of potential information diffusion between treatment and control villages, we show that outcomes of interest do not differ between control villages that are well connected to treated villages and control villages that are not as well connected to treated villages. We measure connections between villages using survey data in which respondents were asked to list (up to 10) villages in their municipality where their family and friends reside. We use this information to proxy for information flows between the villages, by creating a dummy equal to one for villages that are more connected to treated villages (above the median number of links). If spillovers are present we expect the diffusion of information to be larger in villages that are more connected to treated villages. The results regressing our outcomes of interest for the control group on a set of municipal fixed effects and our dummy variable (available in Tables A.12 and A.13) rule out large SUTVA violations.

Last, the effects tend to be stronger for T1 than for T2 (Panel B of Table 1). Recall that in T1 voters are only provided with information on promises made by candidates in the 2016 elections, whereas T2 voters are also provided with information on promises made in 2013. While we cannot reject the null that the effects are identical for T1 and T2, the point estimates for T1 tend to be larger than the point estimates for T2. Only the point estimates for T1 are consistently significant and precisely estimated. In Section 4.2, we explain that the effects of 2013 information on voter beliefs about candidate honesty and competence attenuates the effects of 2016 information on voter beliefs about candidate policy preferences.

Treated voters are more knowledgeable and certain about candidate promises As discussed in Section 2, in addition to collecting data on the modes of posterior beliefs, we asked respondents to indicate their overall degree of certainty over candidates' positions. Information on second moments is useful not just for the structural analysis that follows in Section 5, but also because it provides direct evidence of treatment effects operating though subjective beliefs.

Table 2 shows that treated voters are overall more certain about their assessment of policy positions of candidates (coefficient 0.066). The effect is significant at standard confidence levels. The effect is stronger for voters treated with T1, who exhibit a statistically precise response (coefficient 0.081). The effect of T2 is not distinguishable from that of T1 in terms of magnitudes, but it is less precise.[36]

We also report evidence that treated voters are indeed better informed based on the accuracy of their belief modes. For this purpose, we compute the distance between candidate actual promises and voter's beliefs about what the incumbent will do if elected. Table 3 shows that this distance tends to be systematically lower in treated villages and it presents results across different subgroups of sectors for robustness. Again, consistent with the previous set of results, treatment effects on accuracy of beliefs tend to be stronger for T1 than for T2.

These findings provide intuitive reduced-form evidence of experimental effects through beliefs. The structural model in Section 5 further demonstrates that these treatment effects are quantitatively substantial.

4.2 Voters who are reminded of past promises reward incumbents who fulfilled them

Recall that voters in T2 are informed both about the promises of 2016 candidates and the promises that past candidates made in 2013, which, by construction, includes their current incumbent. Voters in T2 can use information available to them to assess whether incumbents fulfilled promises between 2013-2016. We can test this by estimating equations of the form:

[36] Again we refer to Section 4.2 for a full discussion of the rationale behind attenuated T2 effects.

$$Y_{ivl} = \alpha T_{vl} + \beta Kept_{vl} + \gamma T_{vl} \times Kept_{vl} + v_l + u_{ivl} \tag{5}$$

where $Kept_{vl}$ is a dummy equal to one if the incumbent fulfilled her 2013 promises.[37] As before, all regressions include a full set of triplet fixed effects and the standard errors are clustered at the village-level. We are interested in γ. If voters care about incumbents fulfilling their promises, γ should be greater than zero. To account for the potential differences between T1 and T2 we also estimate those effects separately.

Treated voters are more likely to vote for the incumbent when she fulfilled past promises (Column 1 of Table 4). The entire effect comes from voters informed of past promises: the point estimate for T2×*Kept* is 0.13 while it is only -0.0025 for T1×*Kept* (Column 2 of Table 4).[38] This effect is large: the point estimates on T2 is -0.015, which suggests that voters penalize candidates who do not fulfill their promises (*Kept* = 0). However, these estimates are noisy and we are unable to reject the null of no effect. These results are not merely capturing the possibility that mayors who keep promises may allocate more projects to a village: results are robust to controlling for the number of projects provided by the mayor during her term and its interaction with the treatment dummies (Table A.15).

The effects of T2 for incumbents who fulfilled their promises appear to work through valence beliefs. We re-estimate equation (5) replacing Y_{ivl} with respondent's beliefs about all the different dimensions of incumbent valence. In villages where the incumbent fulfilled her promises voters who received information about the earlier 2013 promises were more likely to rate her as more honest and as more capable (Table 5). These are the two valence dimensions conceptually closest to keeping one's promises. No other valence dimension is precisely affected, nor it is conceptually clear why it should be (e.g. in the case of approachability). These results are robust to controlling for the number of projects provided by the mayor during her term and its interaction with the treatment dummies (Table A.16).

These treatment effects for T2 also explain why the effect sizes in the previous section are smaller in T2. In T2, providing voters with information that changes their beliefs about candidate honesty and competence can lessen the importance of subsequent information about policy positions. Intuitively, the T2 voters informed about campaign promises in 2016 and 2013 may discount the 2016 policy promises in areas where incumbents did not fulfill them.

[37] In those regressions we drop the municipality of Bangui as Diosdado Garvida, the mayor elected in 2013, was suspended from his post halfway into his term.

[38] Importantly, this set of results is robust to controlling for our similarity measures and their interactions with the two treatments. Those results are available in Table A.14. The point estimates on both T1×Δ*Similarity* and T2×*Kept* are very stable and we can comfortably reject the null of no effects.

4.3 Voters do not shift their ideal points to match the policies of their preferred candidate

A further concern is that treated voters might respond to the treatments by shifting their ideal points $\mathbf{q}_i$ and stating policy preferences that match the promises of their preferred candidate. While it is not possible to compare voter policy positions before and after the experiment,[39] other evidence is entirely inconsistent with this possibility.

First, there are no differences in the determinants of respondent policy preferences between individuals in treatment and control villages. Policy preferences are generally correlated with household characteristics–households with young children prefer health and education policies, farmers prefer spending on agriculture, etc. If voters in the treatment group were changing their preferences to match the preferences of their preferred candidate, then the relationship between these determinants and preferences should be attenuated in treated households. They are not. We regress individuals preferences on a set of household characteristics, the treatment dummy and its interactions with the household characteristics. There is no statistical evidence that the determinants of the ideal points differ between treatment and control (Table A.17 and A.18), suggesting that the determinants of policy preferences were similar for respondents in both treatment and control.

Second, if treated voters were to switch their preferences to match those of their preferred candidate, then they should be more likely to report policies similar to those of the most popular candidates in their areas. Specifically, (i) since incumbents command high support everywhere, treated voters should be more likely to report policy preferences similar to incumbents; (ii) this effect should be especially strong in incumbent strongholds; and (iii) treated voters should be more likely to report policy preferences similar to challengers in challenger strongholds.

We find no evidence for any of these patterns. Table A.19 shows that respondents are no more likely to report policies similar to the incumbent's policies, despite higher baseline levels of support for incumbents in general. For the latter two patterns, we use results from the 2013 elections to identify incumbent and challenger strongholds in order to regress our measure of similarity between voters preferences and expected policies of the incumbent on those two subamples. Tables A.20 and A.21 similarly show no evidence of treated voters shifting their policy preferences towards the incumbent in incumbent strongholds nor shifting their reported preferences towards the challenger in challenger strongholds.

Third, in anticipation of the structural analysis, we regress the closeness between the structurally estimated mean of beliefs for each candidate and the voter's ideal point vector on the treatment dummy, a vote choice dummy, and its interaction with a vote choice dummy. Again, there is no statistical evidence that voters move closer to their most preferred candidate

[39] Unfortunately the timing of pre-campaign activities by the various candidates precluded the inclusion of a baseline survey.

or that this closeness is dependent on the treatment or on the interactions between treatment and the candidate choice of the voter (Table A.22). Voters do not appear to have shifted their ideal points as a result of the informational treatment, even when one controls for the full structure of beliefs recovered from our theory.

4.4 Voters that are not part of clientelist networks respond more to the treatment

Clientelist voters have the most to lose by switching to programmatic voting, which suggests that voters who have easy access to clientelist goods should be less responsive to information about policy promises than voters who do not have access to such goods. One way to capture access to such goods is through networks: voters with closer ties to the mayor should have more opportunities to receive clientelist goods. Hence, as described in Section 3, we identify clients as those with a predetermined political link to the mayor, such as family ties, and estimate equation (4), but estimate different treatment effects for clients and non-clients. As we discuss in the introduction, this heterogeneity is an important consideration for the study of less-consolidated or hybrid democracies. In Table 6 we show that non-clients respond strongly to the treatment. Clients respond more weakly to the treatment, indistinguishable from 0. In the appendix, we show that our results hold if we measure clientelism with the ease with which the respondent can obtain: (i) an endorsement letter for a job (Table A.23); (ii) support to pay for funeral expenses (Table A.24); or (iii) assistance for medical expenses. (Table A.25).

5 Structural Estimation

The structural model extends the previous section's reduced-form representation of the multiple causal relationships operating in our setting, by accommodating and quantifying these different mechanisms within a unified econometric framework. We estimate the empirical model from Section 2, quantifying preference weights on clientelist transfers, valence and policy. We first exclude a role for salience, and then estimate how information changes the preference weights assigned to policy. We then discuss two tests of model fit and conduct four sets of counterfactual exercises.

Table 7 presents maximum likelihood estimates for a baseline random utility model of vote choice where preference parameters are restricted to be identical across treated and control voters. That is, we impose, for any i, the restriction $\{\alpha_i, \gamma_i, \omega_i\} = \{\alpha, \gamma, \omega\}$. The specification corresponds to a standard vote choice environment of the type analyzed by Kendall et al. (2015), where any role for salience is excluded. As is standard in these environments, the units of measurement for the parameters are expressed in terms of standard deviations of the random utility shock ε_{ij}. To keep the dimensionality of the problem tractable in the structural estimation, we perform our analysis on $K = 4$, with health, education, agricultural assistance (the three largest expenditure categories), and one residual (other) category.

In this restricted version of our model, MLE delivers a precise estimate of 0.37 for preference weights on clientelist transfers α, where z_i is approximated by an indicator of whether i has a predetermined patron-client tie to the incumbent in the form of family or personal friendship connections. Interestingly, valence parameters γ (representing a vector of weights on incumbent approachability, experience, honesty, connections, competence, and empathy) are typically larger in magnitude than α and precisely estimated (we estimate γ_1 =1.54, approachability, three times larger, and only γ_4 =0.13 , politician's connections, is smaller). This finding holds across specifications, with valence beliefs representing the most significant driver of vote choice across all municipalities. However, as one would have anticipated from Section 4, policies matter, with an estimated utility parameter $\omega = 0.71$ and an asymptotic standard error of 0.22.[40]

The loss function coefficient ζ is estimated to be statistically below 1 (0.22, s.e. 0.05), in line with results in Kendall et al. (2015). This indicates that voters are more sensitive to policy differences in the neighborhood of their ideal points than to policy differences occurring far away from their ideal points. This finding cautions against operating under the analytically convenient, but empirically unsupported, assumption of quadratic losses common in the literature.[41]

Finally, we verify that the Ramalho and Smith correction for the "missing completely at random" (MCAR) violation is in fact necessary. We obtain statistically different parameters for the probability of nonresponse for supporters of incumbent candidates (0.03) versus the probability of nonresponse for supporters of challengers (0.13). Incumbents in the Philippines are typically at an electoral advantage and the evidence validates the concern that voters may be reluctant to explicitly state their support of challengers in the races that we study.

Table 8 extends the empirical analysis to the psychological effects of the treatment on voters preferences. We define salience effects (also indicated as awareness effects in the literature) as the causal effects of informational treatments on preference weights on transfers, valence, and public policy weights in voters' utility functions, $\{\alpha_i, \gamma_i, \omega_i\}$. These effects do not operate through subjective beliefs, but are akin to state-dependent preferences.

Salience effects are quantitatively relevant on important margins. In Table 8 voters made aware of policy platforms by either T1 or T2 (both treatments include policy allocations, and thus both make policy salient) increase voters' weight on policy ω from 0.71 to 0.99 and reduce the weight α placed on clientelist transfers from 0.37 to 0.28. This finding is consequential. Policy information in our flyers, by raising awareness and increasing policy salience, appears to have affected voters' decision making, inducing them to place higher

[40] Asymptotic standard errors are computed by Outer Product of Gradients.

[41] Given estimate of ζ below 2, we also checked the robustness of our reduced form analysis to a measure of similarity allowing for concavity in losses (ζ = .2). We found our reduced-form results qualitatively robust to this correction.

weight on programmatic politics as opposed to clientelist handouts. This policy salience effect is identified in practice by comparing two voters with identical beliefs, but one in the control and the other in the treatment group, and verifying that the treated individual places more weight on public policy when voting than the control voter. Importantly, this happens regardless of the amount of learning (i.e. holding constant the beliefs about candidate policy intentions of the two voters).

In contrast to the previous specification, parameters governing the probability mass on the mode for individual beliefs ($\psi(3)$ for rather uncertain and $\psi(2)$ for very uncertain) are now precisely estimated. Also, all valence dimensions weights remain precisely estimated and valence maintains an important role in explaining vote choice. The Ramalho and Smith correction for the MCAR violation appears necessary under this specification as well. The non-response probabilities are in fact statistically different between voters supporting the incumbent and those supporting the challenger.

The model allowing for salience effects in Table 8 can be statistically tested against the restricted model in Table 7, where preferences are not allowed to respond to treatment. A Likelihood Ratio test supports the salience model at standard confidence levels. Comparing the two log-likelihoods indicates a superior fit of the salience model and a Likelihood Ratio test statistic favors the salience model specification relative to the restricted specification with a $\chi^2(7)$ p-value of 0.038.

We further assess the model for misspecification. Table 9 allows for voter psychological responses to our treatments in the form of salience, but imposes $\omega^0 = 0$, that is no weight on policy for the control group. This restriction assuages the concern that control voters may induce inconsistency in the estimation through their policy-related parameters ω^0,ζ, $\psi(2)$, $\psi(3)$. To see how this would induce problems of inference, consider, as a form of misspecification, the case of control group voters so completely unaware of policy as to not even have properly defined beliefs or preferences over it. For those voters, no information from the control voters should be used to estimate of policy or beliefs parameters. In Table 9 we exclude this possible source of fragility by eliminating any role for policy in the control group. Reassuringly, we find estimates consistent with Table 8 for all parameters shared across the two models. The results from Table 8, therefore, appear robust to this potential misspecification issue. For completeness, a Vuong test for non-nested model selection of Table 9 relative to the specification of Table 7 again supports the presence of salience effects at standard confidence levels.

While the maximum likelihood estimates reported in Table 8 are informative about the effect of treatment on preferences and report precise parameters for both beliefs and preferences in the treated sample, they are not as informative about the tightening of the posterior beliefs that occurs due to rational learning. We know from the reduced-form analysis of Table 2 that voters become more certain about candidate policies upon receiving our informational treatments. Their beliefs also become more accurate. However, the dispersion of voter beliefs regarding multiple candidates and policy dimensions should be assessed through the full variance-covariance matrices associated with the individual subjective belief distributions. Such matrices depend, in fact, on which policy dimensions voters are most uncertain about and on where each multivariate distribution locates the bulk of its mass over the simplex (at the boundary or at its center). Beliefs, for example, may be highly asymmetrical for incumbents versus challengers and display different second moments, skewness, etc. even for identical answers to survey questions on uncertainty (question $Q2$ in the appendix).[42]

The variance-covariance matrices of beliefs generated based on subjective priors elicited from survey respondents (questions $Q1$, $Q2$, $Q3$ in the appendix) and the MLE estimates of $\psi(2)$, $\psi(3)$, show lower dispersion for the treated voters than for control voters. That is, the estimated variance-covariance matrix of beliefs of voters are generally tighter for treated than for control voters in the sense that the difference between these two variance-covariance matrices is positive semi-definite.[43] The intuition that posterior beliefs should tighten in presence of rational learning is appropriate for a large class of learning models and we find evidence of it in five municipalities.[44]

In terms of overall reduction of second moments of individual beliefs, averaging across all policies, all municipalities, and all voters relative to the control group, T1 reduces belief dispersion by 13.2 percent of the control standard deviation level, while T2 reduces the standard deviation by 11.5 percent based on the model estimated belief distributions for the incumbent (results for the challenger are quantitatively and qualitatively similar). In the municipalities of Bangui, Burgos, Paoay, San Juan, Pasuquin second moments tighten as result of the experiment, while in Dingras and Lidlidda our treatment increases dispersion. We do not observe systematic asymmetry in terms of variance reductions for challenger and incumbents, possibly related to the paucity of information about all candidates, as discussed above.

San Juan, Paoay and Pasuquin are the municipalities where the informational effects appear the strongest in terms of second moments. Reductions by each treatment arm, municipality, and policy category for the incumbent are reported in Table 10, which shows

[42]In essence, it is not sufficient to simply rely on survey answers to uncertainty ($Q2$ and $Q3$ in the appendix) individually or to look at the relative positions indicated by the modes in the assessment of candidate policies ($Q1$ in the appendix). Rather, this information has to be jointly assessed within the structure of the model.

[43]The procedure we follow to assess variance-covariance matrices involves four steps. We first calculate the estimated variance-covariance matrix of beliefs for all voters about the candidates under their consideration. We then average the variance-covariance matrices for all voters within T1, T2, or C. We then take the element by element difference of the average variance-covariance matrix for the control group and the variance-covariance matrix for each treatment arm and compute its value in standard deviation units of the corresponding element of the average variance-covariance matrix for the control group. We report the diagonal elements of the resulting matrix of differences.

[44]It is important to emphasize that it is possible to construct specific theoretical cases where more campaign information may in fact increase voters' posterior dispersion. This depends on the structure of the priors and the updating, but it may happen, for instance, if information confuses voters who are initially certain.

that belief tightening along each dimension is not due to a single outlier municipality, a specific candidate, or an influential policy dimension. Rather, belief tightening occurs fairly homogeneously across all categories and the variance reduction appears stronger for T1 rather than T2 (consistent with the reduced-form evidence presented in the previous section). The evidence supports the view that a relevant amount of learning about policy, in addition to the increase salience documented above, occurs in this experiment.

5.1 Model Fit

The in-sample fit of the model is reported for each municipality in Table 11. All municipal election winners are correctly predicted by the model.

In terms of fit of individual voter choices, we capture well above sixty percent of individual vote choices in most municipalities. In Dingras and San Juan we predict correctly over 90 percent of individual choices. Lidlidda, Pasuquin and Burgos also have correct predictions between 61 and 76 percent. There are two municipalities where the fit is less good: Paoay and Bangui. In Paoay the race was extremely close (and the incumbent eventually lost) and many individual choices appear fairly close in terms of expected utility between challenger and incumbent. It was, in essence, a difficult race to predict. In Bangui, a less accurate fit had to be expected, as Diosdado Garvida, the mayor elected in 2013, was in fact suspended and removed from his post in the middle of his term on charges of corruption. He was replaced by his deputy, who then ran in 2016.

For the out-of-sample fit assessments in Table 11, we perform a leave-one-out predictive exercise. We estimate the model for six municipalities at a time and then predict vote decisions based on the estimated parameters for the remaining (seventh) municipality.

Table 11 reports the proportion of correct votes for this out-of-sample exercise. We repeat this exercise for all seven races. The model's performance remains solid across all seven and it appears of equivalent quality as the in-sample fit. Our results do not appear driven by a specifically influential or larger municipality; they are stable across sub-samples of municipalities, and useful for prediction in this context. This robustness in fit not only confirms the predictive value of our framework, but provides reassurance about the stability of the structural estimates across the various municipalities.

5.2 Counterfactual Exercises

We present four sets of counterfactual exercises in Tables 12 and 13. In the first three sets of counterfactual exercises (Table 12), incumbent vote shares at the municipality level are the main outcome variable of interest: this allows us to assess the relative importance of the various drivers of voting behavior on a statistic of immediate political relevance. The final counterfactual exercise (Table 13) assesses the effect of incumbent promise-keeping on their vote share, by calculating the subsequent *persuasion rate*: the counterfactual probability of voting for the incumbent among voters who are not already "persuaded."

5.2.1 Vote Buying

We consider first a counterfactual election where vote-buying is excluded from voter utility. One can think of it as a perfectly clean election where $z_{ij} = 0 \; \forall i, j$. This is implemented within our setting by imposing $\alpha = 0$, thus making voters insensitive to clientelist ties or eliminating such ties altogether.

Comparing columns 1 and 2 in Table 12, across all municipalities vote shares for the incumbent would have fallen by 6 percentage points on average comparing actual and counterfactual vote shares and 2 percentage points when looking at the difference between model estimates in column 2 of Table 11 and counterfactual shares in column 2 of Table 12, with the largest effect in Lidlidda. This seems to suggest that vote buying is quantitatively relevant in our context, but possibly not the be-all-end-all of voter support for candidates in the Philippines.

To further illustrate this consideration, we study a second counterfactual election where *only vote buying matters* for voters. Here we impose $\gamma_i = \mathbf{0}; \omega_i = 0 \; \forall i$. Comparing column 1 and 3, this exercise shows that across all municipalities vote shares for the incumbent would have fallen by 13 percentage points between the model estimates and the counterfactual shares. This seems to suggest that valence and policy also play a substantial quantitative role, if not larger, in explaining high incumbent support in this context.

5.2.2 Awareness of Policy

Next, we assess the change in vote shares for the incumbent in presence of an increase in awareness about public policy. Here, voter utility incorporates the salience-enhanced policy weights estimated for the treatment group in Table 8 for all individuals, including control voters. That is, we perform this exercise by imposing for any voter i a utility weight on policy given by $\omega_i = \omega^0 + \omega^1$, independently of their treatment or control status and without changing those voters' posterior distributions. This is the sense in which the counterfactual focuses purely on psychological salience of policy, as it leaves beliefs unchanged.

As can be seen in column 4 of Table 12, increasing policy awareness in itself has little quantitatively effect on incumbent vote shares in these elections (almost no difference compared to the model estimates in column 2 of Table 11). This may appear unsurprising: policy salience does not imply an *a priori* bias in favor of the incumbent or in favor of the challenger. This is because voters are essentially uninformed about policy in the control group and therefore, even when policy is salient, they consider the two candidates as equivalent in terms of expected utility from the policy dimension. This result is relevant in establishing that "pure salience" campaigns, by making voters aware of public goods provision, but without delivering the necessary information to differentiate candidates, are likely to be electorally ineffectual in this context.

5.2.3 Optimal Choice of Policy Platforms

The third exercise focuses on candidates and their optimal choice of platforms. We consider an election where the incumbent announces a policy platform moving to the geometric median of the voters policy preferences in a municipality (i.e. to the geometric median of the set of ideal points $\{\mathbf{q}_i\}_{i=1}^{N}$) under the assumption that this campaign promise is fully credible and effective. In this exercise we maintain the modes at their actual values for the challenger.

This counterfactual election *prima facie* seems to suggest a productive deviation for the incumbent, as the politician moves his platform towards the median voter. This view, however, contrasts with a few important theoretical considerations. The main one is that in equilibrium the optimality of the initial platforms selected by the candidates should imply no obvious electoral gain from a deviation such as the one we induce.[45] If campaign positions are set (approximately) optimally in this context, policy adjustments in one direction have the potential to make fewer voters switch in favor of the incumbent than those moving away from him, producing ambiguous effects on vote shares (and, in fact, weakly negative effects if platforms are set optimally).

In addition, in the actual data we observe that both incumbent and challenger place their allocations in proximity of the geometric median of their municipality to begin with. The average adjustment to the median voter for each policy dimension across all municipalities is about 5 percent of the budget for the incumbent and 6 percent for the challenger. This is not only an interesting fact per se –as candidates display convergence to the median in the first place in this game[46]–, but it also suggests that the gains from further convergence to the geometric median of a municipality may be limited in terms of magnitudes.

The counterfactual shows that, across all municipalities, these considerations find support. Counterfactual incumbent vote shares appear essentially unaffected by moving closer to the geometric median of the electorate in column 5 of Table 12 relative to the model estimates. Further convergence to the median of their municipalities does not offer consistent and positive electoral gains to incumbents.

[45]Note that in a generic theoretical environment with multidimensional policy competition between two candidates there is no guarantee of convergence to the generalized median of the ideal voter position. This exercise should be considered illustrative of the potential of the model in quantifying electoral effects of realistic informational campaigns, rather than a simulation of the actual game played by candidates (which we do not study).

[46]This is a fact that holds in all seven electoral races. Detailed information on the relative spatial placement of all candidates is available from the authors upon request.

5.2.4 Politician Honesty and Capability

The last counterfactual exercise parallels the reduced form analysis of the effect of the incumbent promise-keeping on her vote share and voter beliefs about her valence. Recall that Table 5 shows that in villages reminded of promises in municipalities where the incumbent kept her promises, the valence assessment of the incumbent increases by 5.2 percentage-points along the honesty dimension and 7 percentage-points along the capability dimension. This exercise extends the reduced form analysis by adjusting voter beliefs "as if" voters received information that the incumbent fulfilled promises. This requires taking a stand on how promise-keeping affects beliefs about incumbent type. We implement this by imposing the maximum incumbent honesty and capability belief levels on all voters, a change that would affect 5 and 6 percent of the voters in our sample.[47] In this context, it is helpful to consider the effect size in terms of a *persuasion rate*, which scales the effect of the treatment with the share of the electorate that would not have voted for the incumbent otherwise. The latter is particularly important in the Philippines because incumbents typically receive high rates of support, necessitating that treatment effects take into account the smaller share of voters who are *persuadable*.[48]

For the sample of individuals switching to an honest and capable incumbent, the average probability of voting for the incumbent goes from a baseline average of 37 percent (with a standard deviation of 31 percent across voters not already persuaded) to an average of 47 percent (with a standard deviation of 32 percent). Calculating an individual specific persuasion rate (given by the difference between the counterfactual probability of voting for the incumbent and the same probability at baseline divided by the probability of voting for the challenger at baseline) produces a persuasion rate of 23 percent (with a standard deviation of 19 percent) in the counterfactual for voters who are not already persuaded.

By municipality, these persuasion rates range from 16.4 in Bangui, where the incumbent mayor was suspended from his post halfway into his term on corruption charges, to 32.1 percent in Pasuqin (Table 13). Looking at the dispersion of individual persuasion rates within municipality, coefficients of variation are around 1 in all municipalities, but in Pasuqin, where the coefficient of variation is 0.52. This indicates a relatively strong dispersion within municipality in the effectiveness of changing voter beliefs as function of promises kept.

6 Assessing Cost Effectiveness

Our results highlight an important puzzle: if information about policies can be effective in changing voter evaluations of candidates, why don't candidates use policy information as

[47] Note that in the Philippines, declaring an individual dishonest or incapable is a strong signal that counters social norms. Consequently, it is not surprising that a switch in incumbency honesty and capability beliefs from the lowest to the maximum level (0 to 1) results in a large effect on the probability of supporting the incumbent.

[48] This is the term used by Della Vigna and Gentzkow (2010) to describe voters who are not already persuaded.

a campaign strategy? Why do mayoral candidates engage in vote buying and clientelist practices instead?

It is certainly not for lack of information about the relative merits of different electoral strategies. Interviews with Philippine mayoral candidates suggest that they assess costs and electoral gains in sophisticated ways.[49] Given that Philippine mayors are sophisticated political actors and distributing flyers with policy information is relatively straightforward, why has it not occurred to them to publicize policy information for electoral gain?

Our field experiment and the analysis above provide accurate per-vote cost estimates of implementing an information campaign in this context. In this section we also use additional survey data and unique data sources in order to collect similar information on the range of price per vote for vote buying. This allows us to offer a comparison of costs between the two different electoral strategies.[50]

Distributing flyers to all treatment villages within a municipality costs $5,700 (current USD) on average, or about $3-$5 per flyer. This amount includes all costs of collecting policy data from candidates, professionally printing the flyers, training enumerators about the flyers, and hand-delivering flyers to households.[51]

These are non-trivial costs for a country like the Philippines where income per capita in 2016 was $2,951 (according to the World Bank). However, compared to the average cost of running for mayor (as reported by candidates in our surveys) in the 2016 elections, distribution of flyers is significantly less expensive. According to mayoral candidates in 2016, the average amount needed to run for mayor was $38,550,[52] almost six times higher than our informational treatment costs. This difference in scale reinforces the puzzle of why mayors do not use information-based campaign strategies, given that the campaign budgets could certainly accommodate them in terms of magnitudes.

An analysis of the electoral returns of the different political tools sheds more light on how to interpret these differences. According to our household survey data, conditional on having received any money for their vote, the average amount given to voters was $31

[49]For example, one candidate we interviewed had a spreadsheet tracking allocation of funds for vote buying for the different villages in the municipality. Other candidates explained cost-saving measures that they have taken: engaging in wholesale vote buying to target identifiable groups, or collaborating with provincial and national level candidates to pool vote buying money to purchase a single slate of votes.

[50]Note that we do not need to assume that candidates coordinate around the release of policy information. Suppose a voter is deciding whether to vote for candidate A or B. Voters have priors about what the candidates will do. Let's assume that candidate A provide some information about herself and/or her programs. Voters will then update their beliefs about both candidates (including from the fact that B is not responding if she isn't). Then voters can decide which candidate to vote for. Such a game can have a symmetric equilibrium where both candidates disclose or asymmetric ones where only one discloses.

[51]Note that our flyers were delivered in partnership with a credible non-partisan organization, while politicians may face additional challenges or costs when delivering information through their campaign or coordinating an information campaign with the other candidates.

[52]Note that these figures are taken from the survey question asking about the general cost for running for mayor in their municipality; by contrast, candidates tended to report that their own campaign expenditures were less.

(removing the top percentile of reported amounts, the average amount drops to $22).[53]

To further corroborate this evidence, we received permission from the bishops and archbishops of the Archdiocese of Nueva Segovia to collect vote buying data from semi-structured interviews with parish priests in Ilocos Norte and Ilocos Sur. Because priests are in a position to obtain sensitive information given their central role in their community, we collected information only about general trends or averages in their parishes. We did not collect any information about specific individuals, such as which individuals received illegal payment or how much certain individuals received - information to which priests have access through confessions. We used preliminary information from these additional surveys of parish priests in order to verify the price data that we received from both the surveys and the mayors. According to this approach, price per vote varies by municipality, often as a function of local economic conditions, but is generally in the range of $20-$50 per household for local elections across municipalities in our study area.

At the high bound of the range and assuming that votes are delivered, the per vote cost of vote buying reaches $12.50 (i.e. at $50 per household, considering 4 people on average per household). These are reasonable valuations, as vote buying is commonly known to be enforceable in the Philippines.[54] Even so, we use the high end of the vote buying range in order to account for these additional monitoring and logistical costs associated with vote buying, assuming that they can be included in higher prices per vote, as in Figure 2.[55]

The rationale for why buying votes may be electorally appealing becomes apparent at this point. Assume that candidates can micro-target our information treatments exactly to the subset of voters whose policy interests are aligned with them and produce a one standard deviation shift in similarity along the policy dimension. Based on our estimates from Section 4, we obtain that a one standard deviation increase in similarity yields an additional vote share of 4 percent. Treating 100 households (about 400 people) at $5 per flyer produces an expenditure of $500 and yields 16 votes. This implies a per vote cost of information of $31.25 per vote, or about 2.5 the cost of buying a vote. Even assuming that only 1 in 2 votes is

[53] While the survey data are noisy, the averages are broadly in line with the ranges given to us by key informants in separate interviews. The conversion USD/PHP at May 9, 2016 exchange rate (election day)

[54] Politicians and brokers use a wide range of strategies for ensuring that voters vote accordingly. The most straightforward are direct means of violating ballot secrecy, such as removing the discretion of voters by providing pre-filled ballots or requiring proof of vote choice (Cruz, 2015). Examples include instructing voters to mark the ballot in a certain way, or use cell phone pictures or carbon paper to record the markings made on the ballot.In the Philippines, these direct methods are less common (survey data indicates that less than 20% of voters targeted for vote buying report having to provide proof of their vote). Philippine brokers prefer to target voter buying to individuals that do not need to be monitored, either because of adherence to norms of reciprocity or the use of indirect monitoring through voter social networks (Cruz, 2018).

[55] We are documenting common features of vote buying that are not limited to the Philippine context—a broad literature covers the mechanics of vote buying across a number of other countries: (i) including aggregate methods of monitoring both brokers (Larreguy, 2013; Larreguy et al., 2016; Bowles et al., 2017) and voters (Rueda, 2017); (ii) targeting vote buying based on personal connections (Stokes, 2005; Szwarcberg, 2014) and individual characteristics such as reciprocity (Finan and Schechter, 2012; Lawson and Greene, 2014) and social persuasion (Lehoucq, 2007; Schaffer and Baker, 2015); or (iii) using forms of vote buying that require less monitoring (Schaffer and Schedler, 2007; Nichter, 2008).

delivered when bought, the informational treatments falls behind vote buying in terms of electoral returns.[56]

In fact, as illustrated quantitatively in Figure 2, even at compliance rates outside the normal range for the Philippines, vote buying is still the more cost-effective electoral strategy.[57] Vote buying only becomes less cost effective when both lack of compliance and price per vote approach unrealistic levels. Even when these prices are well outside a normal range for vote buying, vote buying is a comparatively more cost-efficient strategy as long as compliance rates are above 80-85 percent. Furthermore, the price per vote and compliance rates are positively correlated, making it even more unlikely that we would observe high prices per vote and low compliance rates: Survey data on vote prices suggest that politicians are more likely to use monitoring methods in areas where the price per vote is higher, a relationship that is confirmed by interviews with political operatives.

This analysis speaks to the mechanisms behind the under-provision of political information in consolidating democracies. The gap between information campaigns and clientelist electoral strategies highlights a valid rationale for the absence of policy content in these regimes. Even if campaign information on policy is effective, as we have shown, for politicians vote buying is more cost-effective. A lack of engagement in programmatic discourse and absence of information dissemination follows from this calculation. We are not aware of analogous quantitative assessments similar to the one performed here in the literature.

In terms of policy implications, this analysis suggests a possible role of free media and non-governmental organizations to provide this information in places where the private electoral incentives of politicians may be insufficient.

7 Conclusion

We build on previous research that has examined the impact of voter information by combining a structural model with a large-scale field experiment to provide voters with information they can use to evaluate their candidates on both policy and valence dimensions. We show that preferences over candidates follow standard spatial voting theory, even in a political context where we would not expect it. Voters given information about candidate platforms prefer candidates whose budgetary allocations are closest to their ideal points. Voters given information about previous campaign promises were significantly more likely to vote for incumbents who fulfilled past promises.

The structural model shows that these treatment effects operate in two ways: (i) increasing the salience of policy in vote choice; and (ii) affecting voter beliefs about candidate valence

[56] A low compliance rate, considering that compliance with vote buying ranges from 70% at the low end to 100% at the high end, according to conversations with local political intermediaries who discussed the matter anonymously.

[57] The examples are based on the average municipality in our sample and average number of adults per household.

and policy positions. Taken together, the evidence sheds new light on the effects of policy promises in elections: not only are voters capable of learning about policy promises, but they also incorporate this new information in their assessment of candidates in sophisticated ways.

Our work highlights the potential role of campaign information for democratic consolidation. While the counterfactual analysis shows that vote buying is still more cost-effective than providing policy information, it also suggests a possible role for non-governmental or media organizations to provide this type of policy information in the absence of politician incentives to do so. It also suggests that one possible way to incentivize candidates to pursue policy-based electoral strategies is to increase the targeting or monitoring costs of vote buying, thus decreasing the compliance rate and making it a less efficient strategy. These efforts can take relatively simple forms—procedural changes to improve voter privacy when casting ballots and additional safeguards to ensure ballot secrecy. The formal quantitative approach followed in this paper can help in calibrating them more precisely, with a view towards designing interventions to change the fundamental way in which voters evaluate candidates.

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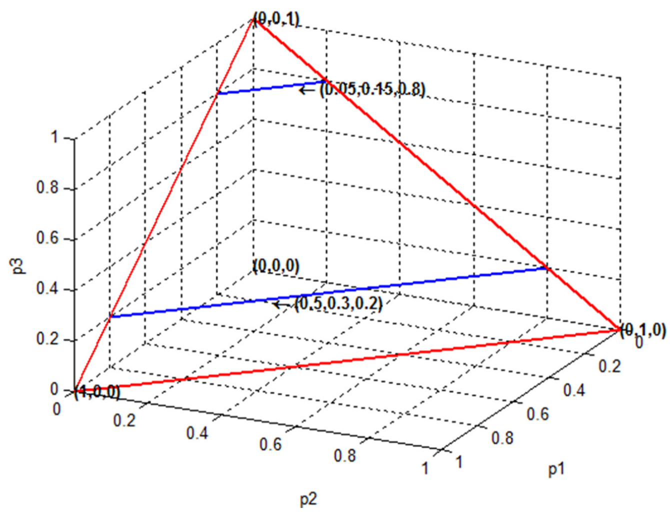

Figure 1: Example of the policy simplex with $K = 3$

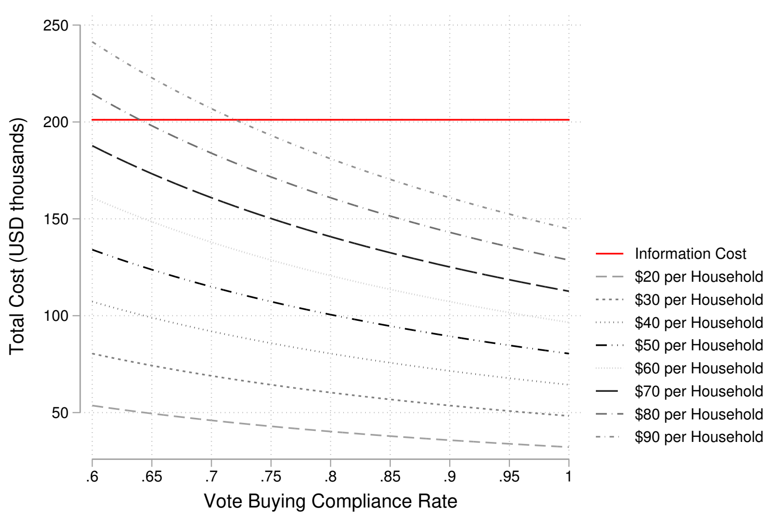

Figure 2: Cost Comparison for Vote Buying and Information Campaigns in a Municipality

Table 1: Treated voters are more likely to vote for the candidate whose policies are closer to their own preferences.

Dep var: vote for incumbent					
Similarity:		Top Sector		Health,	All
	1	2	3	Edu, Ag.	Sectors
Panel A: Overall effects					
Treatment	-0.00048	-0.00065	-0.00055	-0.00075	-0.00055
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
ΔSimilarity	0.011	0.049	0.034	-0.18	0.084
	(0.14)	(0.14)	(0.13)	(0.14)	(0.14)
Treat*ΔSimilarity	0.44**	0.40**	0.35*	0.56***	0.32*
	(0.18)	(0.20)	(0.19)	(0.19)	(0.17)
Observations	3155	3155	3155	3155	3155
R^2	0.30	0.30	0.30	0.30	0.30
Panel B: Separating the effects of T1 and T2					
T1	0.0033	0.0033	0.0035	0.0036	0.0038
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
T2	-0.0046	-0.0050	-0.0050	-0.0044	-0.0048
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
ΔSimilarity	0.011	0.048	0.034	-0.18	0.083
	(0.14)	(0.14)	(0.13)	(0.14)	(0.14)
T1*ΔSimilarity	0.59**	0.62**	0.54*	0.53*	0.40*
	(0.26)	(0.28)	(0.28)	(0.27)	(0.23)
T2*ΔSimilarity	0.31	0.23	0.22	0.57***	0.26
	(0.20)	(0.22)	(0.21)	(0.21)	(0.19)
Observations	3155	3155	3155	3155	3155
R^2	0.30	0.30	0.30	0.30	0.30

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is a dummy equal to 1 if the respondent voted for the incumbent in the 2016 mayoral elections. 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 2: Treated voters are more certain about candidate promises

Dep var:	Certainty	
Treatment	0.066**	
	(0.03)	
T1		0.081**
		(0.04)
T2		0.052
		(0.04)
Observations	3417	3417
R^2	0.03	0.03

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is certainty of beliefs about expected promises. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 3: Treated voters are better informed

Dep var: Distance between actual promises and expected policies					
		Top Sector		Health,	All
	1	2	3	Edu, Ag.	Sectors
Panel A: Overall effects					
Treatment	-0.0053	-0.0030	-0.0060*	-0.0019	-0.0059*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Obs.	3414	3414	3414	3414	3414
R^2	0.19	0.29	0.41	0.23	0.59
Panel B: Separating the effects of T1 and T2					
T1	-0.0089**	-0.0055	-0.0088**	-0.0048	-0.0084**
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
T2	-0.0020	-0.00072	-0.0035	0.00059	-0.0036
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
Obs.	3414	3414	3414	3414	3414
R^2	0.19	0.29	0.41	0.23	0.59

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is is distance between actual promises and voter expected policies. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 4: Voters who are reminded of past promises reward incumbents who fulfilled them

Dep var: vote for incumbent		
Treatment	-0.0019	
	(0.02)	
Kept	-0.031	-0.027
	(0.04)	(0.04)
Treat * Kept	0.077*	
	(0.04)	
T1		0.012
		(0.03)
T2		-0.015
		(0.03)
T1*Kept		-0.0025
		(0.05)
T2*Kept		0.13**
		(0.06)
Observations	2946	2946
R^2	0.26	0.26

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is a dummy equal to 1 if the respondent voted for the incumbent in the 2016 mayoral elections. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 5: Incumbents who fulfilled their promises are perceived to more honest and capable in T2 villages

Dep var:	Approachable	Experienced	Honest	Connected	Capable	Understands
	(1)	(2)	(3)	(4)	(5)	(6)
T1	0.011	0.011*	0.0063	0.016*	0.0046	0.0075
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
T2	-0.0020	-0.0022	-0.012	-0.0089	-0.011	-0.0050
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Kept	-0.0083	-0.012	-0.013	-0.0067	-0.032	-0.017
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
T1*Kept	0.018	0.0030	0.018	0.017	0.012	0.0013
	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
T2*Kept	0.037	0.030	0.052*	0.026	0.070**	0.031
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Observations	3130	3140	3109	3122	3129	3124
R^2	0.04	0.06	0.03	0.03	0.03	0.03

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is a dummy equal to one if incumbent is the candidate that the respondent most associate as being approachable/Friendly (Column 1), being experienced in politics (Column 2), being honest (Column 3), being politically well-connected (Column 4), getting things done (Column 5) understanding the problems of citizens like me (Column 6). 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 6: Effect of Treatment on links between Perceived Policy Similarity on Incumbent Vote Choice are attenuated for the incumbent's client (Political link)

Dep var: vote for incumbent					
Similarity:	Top Sector			Health,	All
	1	2	3	Edu, Ag.	Sectors
Client	-0.043	-0.043	-0.043	-0.043	-0.043
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
T*Client	0.045	0.044	0.044	0.044	0.044
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
T*Not Client	-0.0093	-0.0093	-0.0092	-0.0095	-0.0092
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Δ*Similarity*Client	0.28	0.064	0.019	0.030	-0.056
	(0.29)	(0.29)	(0.31)	(0.22)	(0.28)
Δ*Similarity*Not Client	-0.037	0.046	0.037	-0.22	0.10
	(0.16)	(0.16)	(0.14)	(0.16)	(0.15)
T*Δ*Similarity*Client	0.014	-0.0022	-0.0092	0.25	0.20
	(0.50)	(0.48)	(0.48)	(0.53)	(0.40)
T*Δ*Similarity*Not Client	0.51**	0.48**	0.43**	0.61***	0.34*
	(0.20)	(0.20)	(0.20)	(0.20)	(0.19)
Observations	3155	3155	3155	3155	3155
R^2	0.30	0.30	0.30	0.30	0.30

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is a dummy equal to 1 if the respondent voted for the incumbent in the 2016 mayoral elections. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table 7: Restricted model with salience

	Estimate	Standard Errors
ζ	0.22	0.05
α	0.37	0.09
ω	0.71	0.22
γ_1	1.54	0.12
γ_2	0.56	0.09
γ_3	0.82	0.17
γ_4	0.13	0.12
γ_5	0.21	0.17
γ_6	0.93	0.21
ψ_1	0.91	14.52
ψ_2	0.9	0.49
P(response | inc)	0.97	0.01
P(response | chal)	0.87	0.01

Notes: $LL = -2502$. Asymptotic standard errors computed with OPG. This model imposes equality of preference parameters across treatment and control groups. The valence parameters are as follows. γ_1: Approachable; γ_2: Experienced; γ_3: Honest γ_4: Connected; γ_5: Capable; γ_6: Understand citizens like me.

Table 8: Unrestrictred model with salience

	Estimate	Standard Errors
ζ	0.21	0.04
ψ_1	1	0.32
ψ_2	0.97	0.44
α_t	0.28	0.11
ω_t	0.99	0.26
γ_{1t}	1.51	0.15
γ_{2t}	0.59	0.1
γ_{3t}	0.78	0.22
γ_{4t}	0.27	0.15
γ_{5t}	0.32	0.21
γ_{6t}	1.06	0.27
α_c	0.54	0.15
ω_c	0.12	0.28
γ_{1c}	1.66	0.21
γ_{2c}	0.55	0.16
γ_{3c}	0.88	0.29
γ_{4c}	-0.11	0.21
γ_{5c}	-0.06	0.29
γ_{6c}	0.75	0.36
p(response|inc)	0.97	0.01
p(response|chal)	0.87	0.01
LR $\chi^2(7)$		14.82
	pval	0.04

Notes: $LL = -2494$. Asymptotic standard errors computed with OPG. Subscript indicates treatment (t) or control (c). Likelihood ratio test with 7 degrees of freedom performed against restricted model. The valence parameters are as follows. γ_1: Approachable; γ_2: Experienced; γ_3: Honest γ_4: Connected; γ_5: Capable; γ_6: Understand citizens like me.

Table 9: Quasirestrictred model with salience

	Estimate	Standard Errors
ζ_t	0.21	0.04
α_t	0.28	0.11
ω_t	0.99	0.26
γ_{1t}	1.51	0.15
γ_{2t}	0.59	0.1
γ_{3t}	0.78	0.22
γ_{4t}	0.27	0.15
γ_{5t}	0.33	0.21
γ_{6t}	1.06	0.27
ψ_{1t}	1	0.33
ψ_{2t}	0.95	0.44
α_c	0.55	0.15
γ_{1c}	1.66	0.21
γ_{2c}	0.55	0.16
γ_{3c}	0.88	0.29
γ_{4c}	-0.11	0.21
γ_{5c}	-0.07	0.29
γ_{6c}	0.75	0.36
p(response|inc)	0.97	0.01
p(response|chal)	0.87	0.01
Vuong Test		1.676
	pval	0.047

Notes: $LL = -2494$. Asymptotic standard errors computed with OPG. Subscript indicates treatment (t) or control (c). This model imposes that there are no utility effects of beliefs for the control group. Vuong test for non-nested models is performed against the restricted model. The valence parameters are as follows. γ_1: Approachable; γ_2: Experienced; γ_3: Honest γ_4: Connected; γ_5: Capable; γ_6: Understand citizens like me.

Table 10: Treatment effects on variance of beliefs about incumbent (in s.d. units)

	Bangui	Burgos	Dingras	Lidlidda	Paoay	Pasuquin	San Juan
Panel A: Combined treatment							
Health	0.01	0	-0.01	-0.03	0.02	0.02	0.01
Education	0.01	0	-0.01	-0.03	0.02	0.02	0.01
Agriculture	0.01	0	-0.01	-0.03	0.02	0.02	0.01
Other	0.01	0	-0.01	-0.03	0.02	0.02	0.01
Panel B: T1 (new promises only)							
Health	0.02	-0.01	-0.01	-0.02	0.02	0.02	0.02
Education	0.02	-0.01	-0.01	-0.03	0.02	0.02	0.02
Agriculture	0.02	-0.01	-0.01	-0.02	0.02	0.02	0.02
Other	0.02	-0.01	-0.01	-0.02	0.02	0.02	0.02
Panel C: T2 (new and old promises)							
Health	0	0	-0.01	-0.03	0.02	0.03	0.01
Education	0	0	-0.01	-0.04	0.02	0.03	0.01
Agriculture	0	0	0	-0.03	0.02	0.03	0.01
Other	0	0	-0.01	-0.03	0.02	0.03	0.01

Notes: The table reports reduction of second moments of individual beliefs. All changes by Treatment (any treatment, T1, T2), by municipality, and by public good category in units of standard deviation of the Control

Table 11: Out of sample fit

	Inc. Vote Share: Observed	Estimated	% Votes Correctly Predicted	Out-of-sample Inc. Vote Share
	(1)	(2)	(3)	(4)
Bangui	0.26	0.34	0.52	0.34
Burgos	0.84	0.73	0.76	0.73
Dingras	0.89	0.73	0.9	0.65
Lidlidda	0.73	0.48	0.61	0.47
Paoay	0.38	0.5	0.48	0.52
Pasuqin	0.53	0.69	0.61	0.77
San Juan	0.96	0.8	0.96	0.79

Notes: Column 1 reports the observed vote share for the incumbent. Column 2 reports the average of responding voters probabilities of voting for the incumbent, which represents expected incumbent vote share. Column 3 reports the % of votes correctly predicted, where for each voter the candidate with the highest estimated probability is chosen as that voter's choice. Column 4 report out of sample estimated incumbent vote share. The municipality is left out of the sample, the model is re-estimated, and the left-out municipality's incumbent vote share is predicted using the estimated parameters.

Table 12: Counterfactuals: Incumbent Vote Share

		Estimated under Counterfactual:			
	Observed	No Vote Buying	Only Vote Buying	Salience	Policy
	(1)	(2)	(3)	(4)	(5)
Bangui	0.26	0.33	0.34	0.33	0.34
Burgos	0.84	0.71	0.53	0.74	0.73
Dingras	0.89	0.71	0.52	0.73	0.73
Lidlidda	0.73	0.45	0.37	0.48	0.48
Paoay	0.38	0.48	0.52	0.5	0.5
Pasuqin	0.53	0.67	0.52	0.69	0.69
San Juan	0.96	0.79	0.54	0.8	0.8

Notes: Column 1 reports the observed vote share for the incumbent. Columns 2-5 report expected incumbent vote share under different counterfactuals. Column 2: counterfactual with only valence and policy effects. Column 3: counterfactual without valence and policy effects. Column 4: counterfactual with control group policy weight (ω) replaced with treatment group policy weight. Column 5: counterfactual where incumbent platform is shifted to the median voter of the municipality.

Table 13: Counterfactuals: Incumbent Valence and the Persuasion Rate

	Persuasion Rate
Bangui	0.16
Burgos	0.23
Dingras	0.29
Lidlidda	0.18
Paoay	0.18
Pasuqin	0.32
San Juan	0.28

Notes: Reports the persuasion rate by municipality. The persuasion rate is the treatment effect among voters who are not already persuaded. This is calculated as the difference between the counterfactual probability of voting for the incumbent and the same probability in the baseline scenario divided by the probability of voting for the challenger in the baseline scenario.

Appendix for Online Publication

A.1 Elicitation of Subjective Posteriors

Given that direct nonparametric elicitation of individual belief distributions would not be feasible even for expert respondents, we develop a new approach.

To operationalize the problem, we make a series of simplifying assumptions, while maintaining flexibility in representing complex belief structures. We first simplify the dependence structure between $\mathbf{v}$ and $\mathbf{p}$.[58] We assume that voter i's beliefs about candidate j's platform $f^{i,j}(\mathbf{p}|h)$ are unimodal and indicate the mode with $\pi_{i,j} = [\pi^1_{i,j}, ..., \pi^K_{i,j}]$. The vector $\pi_{i,j}$ is directly elicited by a set of survey questions, one for each j:

$$Q1: \textit{ Which budget allocation will each candidate } j \textit{ most likely choose?}$$

Figure 1 shows the representation of the policy simplex and two possible modal platforms $(0.05, 0.15, 0.8)$ and $(0.5, 0.3, 0.2)$ for the case $K = 3$.

We further assume that the distribution of beliefs is local around the mode. How spread out $f^{i,j}(\mathbf{p}|h)$ is around $\pi_{i,j}$ depends on the degree of i's subjective uncertainty about j's future policy choices. Second moments of high dimensional probability distributions are complex to elicit even for experts (Kadane and Wolfson, 1998; Garthwaite et al., 2012), so we follow a parsimonious, yet flexible approach.

To capture the amount of probability mass each individual places on the mode of their beliefs distributions, we ask the following question concerning their overall degree of uncertainty

$$Q2: \textit{How uncertain are you about the set } \left\{\pi_{i,j}\right\}_{j=A,B}\textit{?}$$
$$A2: \textit{Certain}; \textit{RatherUncertain}; \textit{VeryUncertain}; \textit{Don't know}. \quad x \in \{1, 2, 3, 4\}$$

Define the probability mass $\Psi(x)$ on the mode $\left\{\pi_{i,j}\right\}_{j=A,B}$ and impose, based on the amount of uncertainty declared in the answer, a lower modal mass the more uncertain the voter is: $\Psi(1) = 1 \geq \Psi(2) \geq \Psi(3) \geq \Psi(4)$. To see how this helps in the identification of voter beliefs, consider the answer *"Don't Know"* ($x = 4$). This answer indicates complete uncertainty, implying a well defined, uniform belief distribution. Similarly *"Certain"* ($x = 1$) indicates degenerate beliefs, with probability mass equal to 1 on the elicited mode and 0 everywhere

[58] Kendall et al. (2015) produce a framework where policy and valence beliefs $f^{i,j}(\mathbf{v}, \mathbf{p}|h)$ are allowed to take on a general dependence structure. The authors report, however, evidence in favor of independence as a valid working assumption in the context of Italian elections. Specifically, a copula-based method, which the authors develop, does not reject an independence assumption against alternative models with dependence. As we operate within a much more complex policy space than Kendall and coauthors, we will carry over this working assumption and allow voter beliefs on $\mathbf{v}$ to be independent from beliefs on $\mathbf{p}$ for each candidate.

else on the simplex.

We further ask:

$$\begin{array}{lcl} Q3 & : & \textit{What budget areas are you most uncertain about?} \\ A3 & : & X = \textit{\{less than 4 areas listed\}} \end{array}$$

By focusing on a specific subspace of the simplex, this final question further differentiates any asymmetry between candidates in terms of voters beliefs. For example, in Figure 1 the two lines holding constant $p3 = 0.8$ and $p3 = 0.2$ identify the ranges of $p1, p2$ over which policy is uncertain for a voter answering $X = 3$. The candidate for which the voter expects $p3 = 0.8$ leaves uncertain a much lower share of the budget (20 percent) than the $p3 = 0.2$ candidate (80 percent). Therefore, the voter's belief distribution concerning the former candidate will be tighter than that for the latter.

More generally, suppose i indicates uncertainty about $k \in X_i = \{1, 2, K\}$ and i declares a $x_i = 3$ (*very uncertain*). Based on the answer to $Q1$ let us define the budget share allocated over policy dimensions that are not declared uncertain as:

$$\rho_{i,j} = \sum_{k=1, k \notin U_i}^{K} \pi_{i,j}^{k}.$$

We thus use $\rho_{i,j}$ to represent the share of a budget allocation presented by each candidate j about which voter i is relatively more certain. Let us further define the support of the belief distribution given answers to $Q1 - Q3$. We allow beliefs $f^{i,j}(\mathbf{p}_j|h)$ to have positive mass over the support:

$$\begin{aligned} \mathcal{S}_{i,j} &= \left\{ \begin{array}{c} \mathbf{p}_j = \left[p_j^1, ..., p_j^K\right] \in \mathcal{P} \\ \wedge f^{\,i,j}(\mathbf{p}_j|X_i, x_i, h) > 0 \end{array} \right\} \\ &= \left\{ \begin{array}{c} \mathbf{p} = \left[p_j^1, ..., p_j^K\right] \in \mathbb{R}^K : \\ if \;\; k \notin X_i, \; p_j^k = \pi_{i,j}^k \\ if \;\; k \in X_i, \; p_j^k > 0 : \left\{p_j^k\right\}_{k \notin X_i}, \sum_{s=1}^{\#(X_i)} p_j^s = 1 - \rho_{i,j} \end{array} \right\} \end{aligned}$$

That is, going back to our previous example, for the uncertain dimensions in $X_i = \{1, 2, K\}$, support $\mathcal{S}_{i,j}$ will include all possible policy combinations of $\left(p_j^1, p_j^2, p_j^K\right)$ such that $p_j^1 + p_j^2 + p_j^K = 1 - \rho_{i,j}$. All other dimensions $k \notin X_i$ will be left at the modal values. Notice that by definition $\pi_{i,j} \in \mathcal{S}_{i,j}$.

Concerning the beliefs probability distribution $f^{i,j}(\mathbf{p}_j|h)$ we assume a linear decay of a total $1 - \Psi_j(x_i)$ probability mass off the mode along all policy dimensions in X_i, while leaving $\Psi_j(x_i)$ probability mass on the mode. Notice that we are able to allow a different $\Psi_j(x_i)$ for any candidate j. More precisely, we employ:

$$
\begin{aligned}
f^{\,i,j}(\mathbf{p}_j|h) &= \begin{cases} 0 & if \;\; \mathbf{p}_j \notin \mathcal{S}_{i,j} \\ (1-\Psi_j(x_i)) \times w(\mathbf{p}_j) & if \;\; \mathbf{p}_j \in \mathcal{S}_{i,j}, \mathbf{p}_j \neq \pi_{i,j} \\ \Psi_j(x_i) & if \;\; \mathbf{p}_j = \pi_{i,j} \end{cases} \\
where \;\; w(\mathbf{p}_j) &= \frac{1-\left\|\mathbf{p}_j - \pi_{i,j}\right\|}{\Omega} \\
and \;\; \Omega &= \sum_{\mathbf{p}_j \in \mathcal{S}_{i,j}} \left(1-\left\|\mathbf{p}_j - \pi_{i,j}\right\|\right)
\end{aligned}
$$

and where $\|.\|$ indicates Euclidean distance.

This novel approach noticeably reduces the complexity of the elicitation process for a highly dimensional space. The presence of a detailed elicitation of $\pi_{i,j}$ and the additional information on X_i allow us to indirectly capture the perceived asymmetry across candidates in the i's beliefs distributions based on the different $\rho_{i,j}$. If, for example, voter i indicates $\rho_{i,A} > \rho_{i,B}$ and there is an identical probability mass on the mode $\Psi(x_i)$ for both A and B, it must follow that voter i's considerations about uncertainty mostly concern candidate B as the policy dimensions in X_i account for a larger share of policy budget for him/her. Going back, to Figure 1, assume, for the only three public goods $\left(p^1, p^2, p^3\right)$ in this simplified example, that voter i indicates $X_i = \{1, 2\}$ and modes $(0.05, 0.15, 0.8)$ and $(0.5, 0.3, 0.2)$ for the two candidates. Stating that i has uncertainty on policies X_i automatically informs us that his/her beliefs are much more spread out regarding the second candidate than the first.

A.2 Assumptions

Part of our experimental exercise is predicated on voter learning. We spell out here the set of assumptions necessary for its interpretation.

I. Rational updating. Rational use of information (but not necessarily Bayesian updating) is our starting assumption (which will be then validated empirically). The policy platforms elicited from candidates reach voter i and are incorporated in his/her beliefs. Using Bayesian updating for expositional purposes only, this means that for any candidates j:

$$
f^{i,j}(\mathbf{v}, \mathbf{p}|h) = \frac{\Pr^{i,j}\left(H = h|\mathbf{v}, \mathbf{p}\right)}{\Pr^{i,j}\left(H = h\right)} \times f^{i,j}(\mathbf{v}, \mathbf{p}) \quad h = T1, T2
$$

As an example, one can show empirically that $f^{i,j}(\mathbf{v}, \mathbf{p}|H = T1) \neq f^{i,j}(\mathbf{v}, \mathbf{p}|H = C)$, implying the new information triggers a change in beliefs. A plausible reason could be because voters did not know 2016 policy platforms.[59]

II. The underlying signaling game. We impose no restrictions on the signaling game between politicians A, B, and the voters. The game may take a variety of theoretical forms, many of which have been discussed in the political economy literature (Chappell, 1994; Callander and Wilkie, 2007; Bernhardt et al., 2011). Clearly, the details of such a game determine the likelihood $\frac{\Pr^{i,j}(H=h|\mathbf{v},\mathbf{p})}{\Pr^{i,j}(H=h)}$. For instance, one could allow for beliefs on $\mathbf{v}$ to respond to information on policy $\mathbf{p}$ and cross-learning about all candidates from the policy choice of each of them. In our setting, by focusing directly on the elicited posteriors $f^{i,j}(\mathbf{v},\mathbf{p}|h)$ in the estimation of $\ln L(\theta)$, we avoid imposing particular restrictions on $\frac{\Pr^{i,j}(H=h|\mathbf{v},\mathbf{p})}{\Pr^{i,j}(H=h)}$ altogether. Such restrictions are not necessary for our empirical approach and, in this sense, we allow the strategic interaction between candidates and voters to be general.

III. Updating on relevant events. We allow voters to update on relevant political events W occurring in parallel to our treatment. One can think of W as the set of events naturally occurring in each electoral race (at the margin of which we operate) and affecting all voters independently of treatment assignment. Independence between H and W, induced by the experimental design, allows us to incorporate voter updating based on W without complication. This requires that voter i and candidate j exhibit a likelihood of the form $\frac{\Pr^{i,j}(H=h|\mathbf{v},\mathbf{p})}{\Pr^{i,j}(H=h)} \times \frac{\Pr^{j}(W|\mathbf{v},\mathbf{p})}{\Pr^{j}(W)}$, instead of simply $\frac{\Pr^{i,j}(H=h|\mathbf{v},\mathbf{p})}{\Pr^{i,j}(H=h)}$.

Stable unit treatment value assumption (SUTVA). We assume information remains local to the treated subjects and does not affect control voters. This is a crucial assumption in informational experiments, as information tends to diffuse through social networks. The development economics literature has dedicated substantial effort to studying such spillovers (Banerjee et al., 2018b). Under SUTVA (Rubin, 1974, 1978), voter i posterior distribution on candidate j is:

$$f^{i,j}(\mathbf{v},\mathbf{p}|h,W) = \frac{\Pr^{i,j}(H=h|\mathbf{v},\mathbf{p})}{\Pr^{i,j}(H=h)} \times \frac{\Pr^{j}(W|\mathbf{v},\mathbf{p})}{\Pr^{j}(W)} \times f^{i,j}(\mathbf{v},\mathbf{p}) \qquad h = T1, T2$$

$$f^{i,j}(\mathbf{v},\mathbf{p}|H=C,W) = \frac{\Pr^{j}(W|\mathbf{v},\mathbf{p})}{\Pr^{j}(W)} \times f^{i,j}(\mathbf{v},\mathbf{p}).$$

[59] We will also show that $f^{i,j}(\mathbf{v},\mathbf{p}|H=T2) \neq f^{i,j}(\mathbf{v},\mathbf{p}|H=C)$ if $\left\|\mathbf{p}^0_j - \phi^0_j\right\|$ is low, that is when previous promises were kept so their distance from the implemented policy $\mathbf{p}^0_j$, which we measure, is low. In addition, $f^{i,j}(\mathbf{v},\mathbf{p}|H=T2) = f^{i,j}(\mathbf{v},\mathbf{p}|H=C)$ if $\left\|\mathbf{p}^0_j - \phi^0_j\right\|$ is high (i.e. when previous promises were not kept).

We validate SUTVA empirically and do not detect substantial violations. To begin with, our design treats entire villages precisely because of likely contamination arising within village, avoiding the most plausible source of violation. Furthermore, we do not detect a gradient in similarity of behavior when focusing on the differential behavior of subjects residing in different control villages with more or less social connections to treatment villages.

A.3 Background on the Experiment

A.3.1 Relationship with previous research

An advantage of our work relative to the literature is in the repeated intervention nature of our informational treatments, which may reduce the threat of confounding endogenous response by candidates. For example, in related work Cruz et al. (2021) document that this was indeed the case in 2013, with a systematic increase in vote buying efforts by politicians in response to the randomized informational treatments. The authors emphasize how this was a reaction to their RCTs, which employed flyers similar to the ones used in this paper. However, in 2016 we do not observe any systematic and targeted response in vote buying efforts by politicians in response to informational treatments on policy. This is possibly due to the fact that by 2016 political candidates had assumed familiarity with the informational treatments, they all remembered the 2013 intervention, and possibly had even begun to consider policy competition as a viable strategy to garner electoral support (we discuss its cost-effectiveness relative to more traditional electioneering tools like vote buying in Section 6). By the time of the 2016 elections, the electoral equilibrium had shifted: the number of projects financed by incumbent mayors during the 2013-2016 term increased drastically in the municipalities where the experiment was implemented. Respondents to the 2016 survey reported 58% more incumbent-financed projects between 2013-2016 than respondents from those municipalities reported between 2010-2013. In this sense our results for the 2016 campaigns are to be considered closer to steady state equilibrium effects.

A reduced-form specification worth exploring involves the role of candidates, not just voters in responding to our treatment. As discussed in the Introduction, we were wary of drastic experimental interventions within political contexts where policy information treatments would be deemed intrusive and deserving of immediate response. Cruz et al. (2021) show that this was indeed the case in 2013, where an experimental informational effort akin to T1 was implemented and a vote buying response by candidates ensued. Cruz et al. (2021) read this evidence as a response of candidates unprepared to the spotlight on public goods. In 2016 one of the first relationships we verified was that the strategic response by incumbents in terms of vote buying had disappeared. In Tables A.26 and A.27 we show that treated voters were neither more nor less likely to be targeted for vote buying.

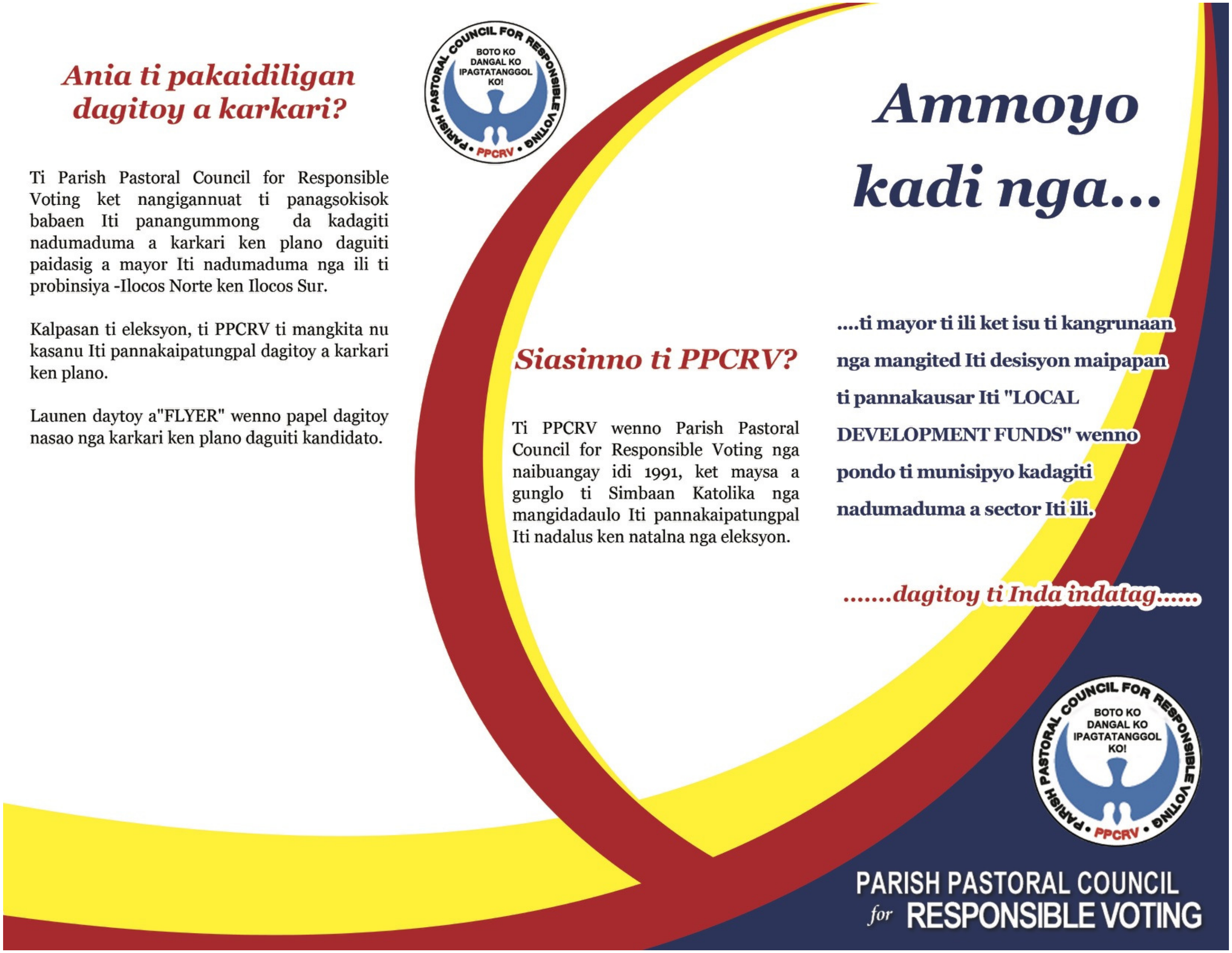

Figure A.1: Cover for the Flyer

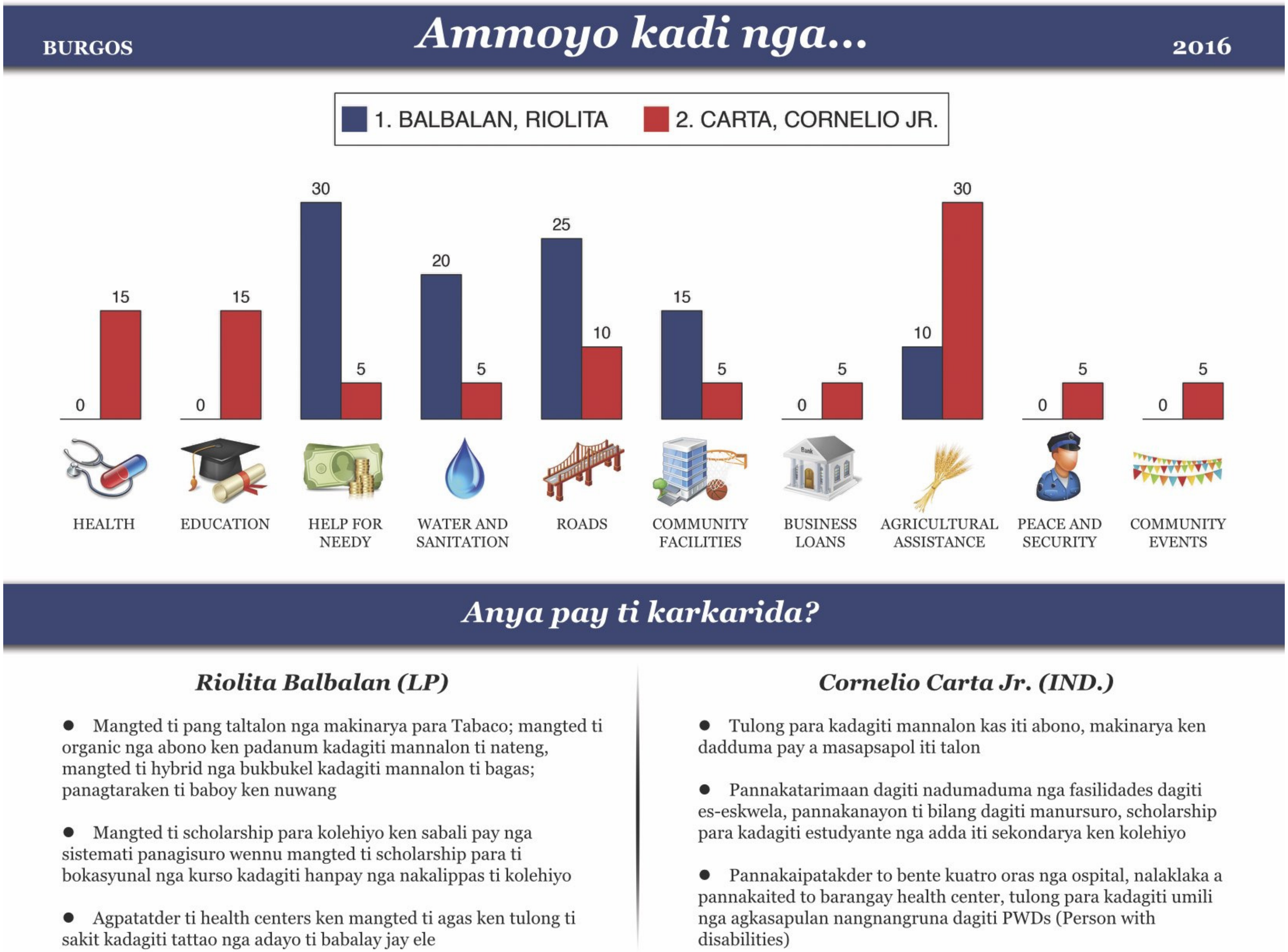

Figure A.2: Flyer for the Municipality of Burgos, Ilocos Sur

Table A.1: List of Intervention Municipalities

Province	Municipality	# Candidates
ILOCOS NORTE	BANGUI	4
	DINGRAS	2
	PAOAY	2
	PASUQUIN	2
ILOCOS SUR	BURGOS	2
	LIDLIDDA	3
	SAN JUAN (LAPOG)	2

Table A.2: Timeline

Date	Activity
April	Candidate interviews
May 3-6	Flyer distribution (door-to-door visits)
May 9	Elections
End of May/June	Household survey

Table A.3: Translation of Flyer for the Intervention (Fig. A.1)

Front Page	Inner Flap	Back
Did you know...	What makes these promises different?	About the PPCRV
[4ex] 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.4 Additional Results

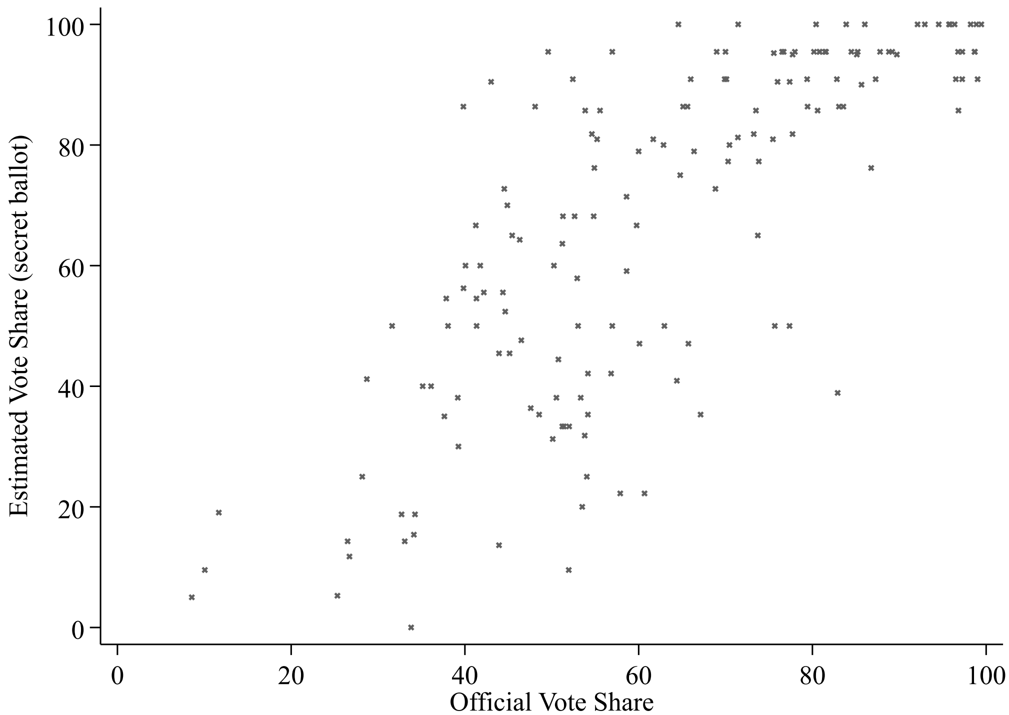

Figure A.3: Comparing village-level incumbent vote shares (official and estimated from survey data)

Table A.4: Candidate Promises

	Overall (1)	Incumbent (2)	Challenger (3)
Health	13.12	12.14	13.89
	(6.55)	(8.59)	(4.86)
Education	12.81	11.43	13.89
	(7.30)	(8.02)	(6.97)
Help for Needy	9.69	9.29	10.00
	(8.26)	(10.18)	(7.07)
Water and Sanitation	12.50	18.57	7.78
	(17.32)	(25.61)	(3.63)
Road	10.00	10.00	10.00
	(8.16)	(9.13)	(7.91)
Community Facilities	8.75	8.57	8.89
	(5.00)	(6.90)	(3.33)
Business Loan	6.25	6.43	6.11
	(6.19)	(8.52)	(4.17)
Agriculture	15.94	15.00	16.67
	(6.64)	(7.07)	(6.61)
Peace and Security	5.94	3.57	7.78
	(4.55)	(3.78)	(4.41)
Community Events	5.00	5.00	5.00
	(4.83)	(7.07)	(2.50)
Observations	16	7	9

Notes: Average promises for all candidates running in our 7 sample municipalities. The standard deviations are in (parentheses)

Table A.5: Distance Between Candidate Promises and Median Preferences

	Overall (1)	Incumbent (2)	Challenger (3)
Health	5.94	7.86	4.44
	(4.91)	(6.36)	(3.00)
Education	4.38	5.00	3.89
	(5.44)	(5.77)	(5.46)
Help for Needy	5.94	6.43	5.56
	(5.54)	(7.48)	(3.91)
Water and Sanitation	7.50	13.57	2.78
	(15.71)	(23.04)	(2.64)
Road	5.62	7.14	4.44
	(5.74)	(4.88)	(6.35)
Community Facilities	4.69	7.14	2.78
	(4.64)	(5.67)	(2.64)
Business Loan	4.38	4.29	4.44
	(4.79)	(7.32)	(1.67)
Agriculture	5.94	7.14	5.00
	(4.91)	(4.88)	(5.00)
Peace and Security	3.75	2.86	4.44
	(3.87)	(3.93)	(3.91)
Community Events	4.06	4.29	3.89
	(4.91)	(7.32)	(2.20)
Observations	16	7	9

Notes: Absolute value of the distance between candidate promises and median voter preferences.. The standard deviations are in (parentheses)

Table A.6: balance tests : voter preferences

	T1	T2	Control	β_T	β_{T1}	β_{T2}
	(1)	(2)	(3)	(4)	(5)	(6)
Health	18.34	18.25	18.09	0.10	0.26	-0.09
	(13.00)	(11.97)	(12.40)	[0.82]	[0.59]	[0.87]
Education	15.80	16.56	16.21	-0.19	-0.40	0.05
	(10.75)	(11.69)	(11.67)	[0.66]	[0.42]	[0.92]
Help for Needy	9.18	8.90	9.07	-0.03	0.12	-0.19
	(8.64)	(9.02)	(8.77)	[0.92]	[0.76]	[0.56]
Water and Sanitation	8.41	8.22	8.32	0.13	0.09	0.18
	(7.61)	(8.55)	(8.06)	[0.67]	[0.80]	[0.63]
Roads	11.02	10.05	10.45	0.20	0.57	-0.20
	(9.96)	(8.72)	(9.99)	[0.64]	[0.25]	[0.69]
Community Facilities	6.39	5.89	6.04	0.14	0.35	-0.10
	(6.57)	(6.18)	(6.37)	[0.57]	[0.23]	[0.70]
Business Loan	4.83	4.99	5.39	-0.47	-0.57	-0.37
	(6.51)	(6.44)	(7.09)	[0.03]	[0.02]	[0.14]
Agricultural Assistance	15.62	16.20	15.76	0.10	-0.15	0.37
	(12.75)	(12.48)	(13.10)	[0.85]	[0.80]	[0.58]
Peace and Security	6.56	7.06	6.56	0.27	-0.01	0.57
	(6.27)	(6.26)	(6.53)	[0.30]	[0.98]	[0.07]
Community Events	3.86	3.89	4.12	-0.24	-0.26	-0.22
	(5.24)	(4.79)	(4.79)	[0.15]	[0.17]	[0.25]

The standard deviations are in (parentheses) (Columns 1-3). Each cell in Columns 4-6 is either the coefficient on the dummy variable indicating whether the treatment (Column 4), treatment 1 (Column 5) or treatment 2 (Column 6) was implemented in the village from a different OLS regression with triplet fixed-effects or the associated p-value in [bracket].

Table A.7: balance tests : variables used for matching

	T1	T2	Control	β_T	β_{T1}	β_{T2}
	(1)	(2)	(3)	(4)	(5)	(6)
Registered voters	524.296	571.820	504.556	32.844	19.741	47.520
	(367.531)	(390.193)	(294.743)	[0.518]	[0.739]	[0.459]
Inc. Vote Share (2013)	51.844	52.668	50.535	1.627	1.310	1.982
	(16.307)	(15.211)	(14.376)	[0.340]	[0.502]	[0.329]
Nb precincts	1.074	1.100	1.111	-0.028	-0.037	-0.019
	(0.328)	(0.364)	(0.317)	[0.614]	[0.570]	[0.785]
Rural	0.907	0.940	0.926	-0.005	-0.019	0.011
	(0.293)	(0.240)	(0.264)	[0.920]	[0.735]	[0.842]
Vote buying (2013)	0.193	0.199	0.161	0.031	0.032	0.029
	(0.182)	(0.195)	(0.174)	[0.155]	[0.208]	[0.276]
Salience sectors (2013)	0.792	0.808	0.697	0.100	0.095	0.105
	(0.414)	(0.517)	(0.535)	[0.203]	[0.254]	[0.284]
Knowledge. promises (2013)	0.068	0.064	0.011	0.049	0.057	0.041
	(0.354)	(0.358)	(0.356)	[0.191]	[0.147]	[0.397]

The standard deviations are in (parentheses) (Columns 1-3). Each cell in Columns 4-6 is either the coefficient on the dummy variable indicating whether the treatment (Column 4), treatment 1 (Column 5) or treatment 2 (Column 6) was implemented in the village from a different OLS regression with triplet fixed-effects or the associated p-value in [bracket].

Table A.8: balance tests : HH variables

	T1	T2	Control	β_T	β_{T1}	β_{T2}
	(1)	(2)	(3)	(4)	(5)	(6)
Length stay	34.97	36.98	36.39	-0.46	-1.42	0.62
	(19.97)	(19.73)	(19.85)	[0.49]	[0.07]	[0.39]
HH size	5.00	5.15	5.04	0.05	-0.04	0.15
	(2.26)	(2.26)	(2.07)	[0.49]	[0.67]	[0.11]
Number kids (0-6)	0.47	0.44	0.46	0.00	0.02	-0.01
	(0.82)	(0.79)	(0.77)	[0.90]	[0.54]	[0.65]
Number kids (6-14)	0.58	0.59	0.64	-0.05	-0.05	-0.05
	(0.98)	(0.99)	(0.99)	[0.09]	[0.10]	[0.18]
Female	0.30	0.33	0.31	0.01	0.00	0.03
	(0.46)	(0.47)	(0.46)	[0.61]	[0.87]	[0.27]
Age	49.23	50.49	49.85	0.06	-0.55	0.76
	(15.58)	(14.57)	(15.18)	[0.93]	[0.50]	[0.28]
Education (years)	9.47	9.63	9.23	0.30	0.24	0.37
	(3.48)	(3.49)	(3.53)	[0.05]	[0.19]	[0.03]
Remittances abroad	0.31	0.34	0.32	0.01	-0.01	0.03
	(0.46)	(0.48)	(0.47)	[0.78]	[0.54]	[0.26]
CCT Beneficiary	0.19	0.19	0.20	-0.01	-0.01	0.00
	(0.40)	(0.40)	(0.40)	[0.72]	[0.67]	[0.85]

The standard deviations are in (parentheses) (Columns 1-3). Each cell in Columns 4-6 is either the coefficient on the dummy variable indicating whether the treatment (Column 4), treatment 1 (Column 5) or treatment 2 (Column 6) was implemented in the village from a different OLS regression with triplet fixed-effects or the associated p-value in [bracket].

Table A.9: balance tests : match preferences incumbent/voter vs. challenger/voter

	T1	T2	Control	β_T	β_{T1}	β_{T2}
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Beliefs						
Top sector	-0.002	0.002	0.001	-0.001	-0.004	0.001
	(0.072)	(0.068)	(0.073)	[0.558]	[0.181]	[0.606]
Top 2 sectors	-0.002	0.002	-0.001	0.000	-0.001	0.002
	(0.073)	(0.066)	(0.073)	[0.893]	[0.594]	[0.374]
Top 3 sectors	-0.003	0.001	-0.001	0.000	-0.002	0.002
	(0.073)	(0.066)	(0.076)	[0.866]	[0.403]	[0.507]
Health/Educ/Ag.	-0.004	0.002	0.000	-0.001	-0.004	0.003
	(0.073)	(0.063)	(0.074)	[0.775]	[0.192]	[0.317]
All sectors	-0.004	0.001	-0.001	0.000	-0.002	0.002
	(0.085)	(0.076)	(0.088)	[0.922]	[0.460]	[0.501]
Panel B: Stated Promises						
Top sector	-0.025	-0.018	-0.026	0.001	0.001	0.001
	(0.091)	(0.072)	(0.085)	[0.676]	[0.802]	[0.640]
Top 2 sectors	-0.034	-0.026	-0.036	0.001	0.002	0.001
	(0.094)	(0.076)	(0.093)	[0.635]	[0.593]	[0.802]
Top 3 sectors	-0.045	-0.034	-0.047	0.001	0.002	0.001
	(0.102)	(0.083)	(0.104)	[0.519]	[0.518]	[0.675]
Health/Educ/Ag.	-0.021	-0.021	-0.021	-0.001	0.000	-0.002
	(0.060)	(0.060)	(0.063)	[0.787]	[0.908]	[0.528]
All sectors	-0.060	-0.049	-0.061	0.000	0.001	-0.002
	(0.119)	(0.103)	(0.120)	[0.852]	[0.779]	[0.522]

The standard deviations are in (parentheses) (Columns 1-3). Each cell in Columns 4-6 is either the coefficient on the dummy variable indicating whether the treatment (Column 4), treatment 1 (Column 5) or treatment 2 (Column 6) was implemented in the village from a different OLS regression with triplet fixed-effects or the associated p-value in [bracket].

Table A.10: Further Descriptive Statistics

Variable	Obs.	Mean	Std. Dev.
Vote for incumbent	3,222	0.69	0.46
Certainty beliefs	3,189	2.93	0.85
Approachable	3,209	0.92	0.27
Experienced	3,217	0.97	0.17
Honest	3,187	0.95	0.23
Connected	3,197	0.95	0.22
Capable	3,205	0.95	0.22
Understand citizens like me	3,200	0.96	0.20
Vote buying	3,189	0.40	0.49

Notes: The sample is restricted to individuals who responded to the secret ballot question

Table A.11: Comparing Projects Data from the HH Survey and the Accountant/Engineer Survey

	Household Data		Accountant/Engineer data
	Village-level	Municipal-level	
	(1)	(2)	(3)
Health	6.54	6.42	4.66
Education	3.53	3.21	2.06
Help for Needy	1.45	1.24	0.56
Water and Sanitation	7.68	9.27	3.78
Roads	50.39	48.97	44.45
Community Facilities	18.47	18.52	18.68
Business Loan	0.53	0.47	1.71
Agricultural Assistance	5.85	5.75	6.84
Peace and Security	4.98	5.44	3.08
Community Events	0.59	0.71	0.03

Notes: Columns 1 and 2 report project shares across the 10 sectors computed from the household survey. Column 3 reports budget shares across the 10 sectors computed from the Accountant/Engineer survey.

Table A.12: Spillovers, certainty

Dep var:	Certainty
Better connected to treatment villages	0.069
	(0.08)
Observations	1167
R^2	0.03

Notes: Individual-level regressions with municipal fixed effects. The dependent variable is certainty of beliefs about expected promises. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.13: Spillovers, distance, similarity and voting for the incumbent

Sectors:	Top Sector			Health,	All
	1	2	3	Edu, Ag.	Sectors
Panel A: Distance between beliefs and actual promises					
Better connected to treatment villages	-0.011*	-0.011*	-0.0097*	-0.011**	-0.0074
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Observations	1167	1167	1167	1167	1167
R^2	0.18	0.28	0.40	0.25	0.58
Panel B: Vote for the incumbent					
Better connected to treatment villages	-0.10**	-0.10**	-0.10**	-0.10**	-0.10**
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Δ Similarity	0.0072	0.011	0.0096	-0.0083	0.013
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Δ Similarity *	-0.014	-0.016	-0.011	-0.019	-0.010
Better connected to treatment villages	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	1071	1071	1071	1071	1071
R^2	0.31	0.31	0.31	0.31	0.31

Notes: Individual-level regressions with municipal fixed effects. The dependent variable is certainty of beliefs about expected promises. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.14: Both stories hold when analyzed simultaneously

DV: vote for incumbent					
Similarity:	Top Sector			Health,	All
	1	2	3	Edu, Ag.	Sectors
T1	0.010	0.011	0.011	0.011	0.011
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
T2	-0.016	-0.016	-0.016	-0.016	-0.015
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
ΔSimilarity	-0.051	0.0099	0.000006	-0.22	0.050
	(0.14)	(0.16)	(0.15)	(0.15)	(0.15)
T1*ΔSimilarity	0.59**	0.63**	0.53*	0.51*	0.44*
	(0.28)	(0.31)	(0.31)	(0.29)	(0.25)
T2*ΔSimilarity	0.34	0.21	0.19	0.56**	0.22
	(0.24)	(0.26)	(0.25)	(0.25)	(0.22)
Kept	-0.025	-0.023	-0.024	-0.025	-0.023
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
T1*Kept	0.0033	0.0051	0.0050	0.0027	0.0036
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
T2*Kept	0.13**	0.12**	0.12**	0.12**	0.12**
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Observations	2885	2885	2885	2885	2885
R^2	0.27	0.27	0.26	0.26	0.27

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is a dummy equal to 1 if the respondent voted for the incumbent in the 2016 mayoral elections. 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: Voters who are reminded of past promises reward incumbents who fulfilled them (controlling for number of projects)

Dep var: vote for incumbent		
Treatment	-0.00068	
	(0.02)	
Kept	-0.041	-0.035
	(0.04)	(0.04)
T*Kept	0.090*	
	(0.05)	
# Projects (2013/16)	0.0059*	0.0060*
	(0.00)	(0.00)
T*# Projects (2013/16)	-0.0055	
	(0.00)	
T1		0.014
		(0.03)
T2		-0.013
		(0.03)
T1*Kept		0.022
		(0.06)
T2*Kept		0.14**
		(0.06)
T1*# Projects (2013/16)		-0.0032
		(0.00)
T2*# Projects (2013/16)		-0.0066*
		(0.00)
Observations	2946	2946
R^2	0.26	0.26

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is a dummy equal to 1 if the respondent voted for the incumbent in the 2016 mayoral elections. 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.16: Incumbents who fulfilled their promises are perceived to more honest and capable in T2 villages (controlling for number of projects)

Dep var:						
	Approachable	Experienced	Honest	Connected	Capable	Understands
	(1)	(2)	(3)	(4)	(5)	(6)
T1	0.00049	0.011*	0.0030	0.016*	0.0018	0.0056
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
T2	0.0030	-0.0024	-0.011	-0.0097	-0.011	-0.0047
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Kept	-0.030	-0.011	-0.019	-0.0061	-0.036*	-0.020
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
T1*Kept	0.039	-0.00065	0.024	0.0099	0.010	0.00044
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
T2*Kept	0.044	0.027	0.053**	0.022	0.068**	0.030
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
# Projects (2013/16)	-0.00087	-0.00089	-0.00075	-0.00056	-0.00064	-0.00030
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
T1*# Projects (2013/16)	-0.0065***	0.00036	-0.0014	-0.00075	-0.0022*	-0.0017
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
T2*# Projects (2013/16)	-0.0065***	0.0011	-0.0015	0.0016	-0.000043	-0.00017
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	3130	3140	3109	3122	3129	3124
R^2	0.05	0.06	0.03	0.03	0.03	0.03

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is a dummy equal to one if incumbent is the candidate that the respondent most associate as being approachable/Friendly (Column 1), being experienced in politics (Column 2), being honest (Column 3), being politically well-connected (Column 4), getting things done (Column 5) understanding the problems of citizens like me (Column 6). The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.17: Sectoral Preferences

	Health	Education	Emergencies	Water	Road	ComFaci	EconProg	Agriculture	Peace	Festivals
Female	-0.96	0.40	0.69	-0.12	-1.05*	0.39	0.12	0.30	-0.17	0.40
	(0.75)	(0.64)	(0.53)	(0.48)	(0.62)	(0.35)	(0.46)	(0.90)	(0.43)	(0.33)
Yrs. Educ.	-0.21**	0.24**	-0.20***	-0.14**	-0.058	0.072	0.031	0.067	0.21***	-0.00027
	(0.10)	(0.11)	(0.06)	(0.06)	(0.08)	(0.06)	(0.06)	(0.09)	(0.07)	(0.04)
Farming	-3.13***	-1.00	-1.28**	0.85	0.73	0.26	0.10	4.37***	-0.52	-0.37
	(1.08)	(0.97)	(0.58)	(0.55)	(0.67)	(0.39)	(0.62)	(1.00)	(0.46)	(0.39)
Business	-0.88	-0.29	0.19	-0.15	-0.37	-0.35	2.00***	-0.27	0.12	0.0022
	(0.95)	(0.80)	(0.57)	(0.55)	(0.58)	(0.49)	(0.67)	(1.07)	(0.46)	(0.40)
HH Size	0.099	-0.18	-0.18	0.10	-0.098	0.033	-0.031	0.18	0.096	-0.016
	(0.19)	(0.14)	(0.13)	(0.11)	(0.16)	(0.10)	(0.11)	(0.24)	(0.08)	(0.07)
Kids 0-14	-0.093	1.02***	-0.25	-0.041	0.0013	-0.19	-0.22	-0.30	-0.019	0.083
	(0.29)	(0.31)	(0.19)	(0.19)	(0.24)	(0.14)	(0.14)	(0.27)	(0.20)	(0.12)
Treatment	-2.01	-0.85	-1.73	0.68	-0.71	1.32	0.68	0.91	1.89*	-0.17
	(2.27)	(1.96)	(1.45)	(1.34)	(1.62)	(1.08)	(1.33)	(2.06)	(1.00)	(0.82)
Treat*Female	0.52	0.88	0.67	0.45	0.46	-0.46	-0.35	-1.54	-0.33	-0.31
	(0.92)	(0.78)	(0.64)	(0.61)	(0.77)	(0.46)	(0.53)	(1.13)	(0.51)	(0.38)
Treat*Yrs. Educ.	0.13	0.055	0.076	0.020	-0.0073	-0.045	-0.12	-0.017	-0.100	0.0046
	(0.13)	(0.14)	(0.08)	(0.08)	(0.10)	(0.07)	(0.07)	(0.12)	(0.08)	(0.05)
Treat*Farming	1.90	0.45	0.016	-0.85	-0.33	-0.38	-0.97	0.50	-0.62	0.28
	(1.15)	(1.14)	(0.70)	(0.70)	(0.81)	(0.51)	(0.68)	(1.14)	(0.57)	(0.46)
Treat*Business	-0.43	-0.45	-0.26	0.43	0.53	0.75	-0.82	0.37	-0.13	0.025
	(1.16)	(1.02)	(0.74)	(0.71)	(0.76)	(0.63)	(0.79)	(1.29)	(0.58)	(0.51)
Treat*HH Size	-0.16	-0.088	0.12	-0.14	0.21	-0.062	0.15	-0.032	-0.00058	0.0051
	(0.24)	(0.18)	(0.16)	(0.14)	(0.19)	(0.12)	(0.14)	(0.27)	(0.10)	(0.10)
Treat*Kids 0-14	0.26	-0.37	0.052	0.22	-0.40	-0.0012	0.35*	0.16	-0.14	-0.14
	(0.39)	(0.37)	(0.23)	(0.25)	(0.30)	(0.18)	(0.18)	(0.35)	(0.22)	(0.15)
Mean	18.2	16.2	9.05	8.32	10.5	6.11	5.07	15.9	6.72	3.96
F-stat	1.00	0.63	0.42	1.01	0.64	0.60	2.08	0.33	0.76	0.65
p-value	0.43	0.73	0.89	0.42	0.72	0.76	0.049	0.94	0.62	0.72

Notes: Results from individual-level regressions with triplet fixed effects. 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 row " F-stat" provides the F-statistics of the joint test of the variable Treatment, Treat*Female, Treat*Yrs. Educ, Treat*Farming, Treat*Business, Treat*Kids 0-14. The associated p-value is on row " p-value" The standard errors (in parentheses) account for potential correlation within village. Standard errors, clustered by barangay, in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table A.18: Sectoral Preferences

	Health	Education	Emergencies	Water	Road	ComFaci	EconProg	Agriculture	Peace	Festivals
Female	-0.96	0.41	0.69	-0.13	-1.04*	0.40	0.11	0.30	-0.17	0.39
	(0.75)	(0.64)	(0.53)	(0.48)	(0.62)	(0.35)	(0.46)	(0.90)	(0.43)	(0.33)
Yrs. Educ.	-0.21**	0.24**	-0.20***	-0.14**	-0.061	0.069	0.031	0.069	0.21***	-0.00091
	(0.10)	(0.11)	(0.06)	(0.06)	(0.08)	(0.06)	(0.06)	(0.09)	(0.07)	(0.04)
Farming	-3.12***	-1.00	-1.27**	0.85	0.73	0.26	0.095	4.36***	-0.53	-0.37
	(1.08)	(0.98)	(0.58)	(0.55)	(0.67)	(0.39)	(0.62)	(1.00)	(0.46)	(0.39)
Business	-0.90	-0.30	0.18	-0.15	-0.37	-0.34	2.00***	-0.26	0.11	0.011
	(0.95)	(0.80)	(0.57)	(0.55)	(0.58)	(0.49)	(0.67)	(1.07)	(0.46)	(0.40)
HH Size	0.097	-0.18	-0.18	0.10	-0.097	0.035	-0.030	0.18	0.094	-0.015
	(0.20)	(0.14)	(0.13)	(0.11)	(0.16)	(0.10)	(0.11)	(0.24)	(0.08)	(0.07)
Kids 0-14	-0.095	1.02***	-0.26	-0.042	0.0012	-0.19	-0.22	-0.29	-0.019	0.083
	(0.29)	(0.31)	(0.19)	(0.20)	(0.24)	(0.14)	(0.14)	(0.27)	(0.20)	(0.12)
T1	-0.57	-0.36	-1.44	1.24	-1.90	0.80	1.00	-0.14	1.45	-0.061
	(2.82)	(2.51)	(1.67)	(1.81)	(1.94)	(1.19)	(1.53)	(2.37)	(1.31)	(0.92)
T1*Female	-0.097	1.23	-0.13	0.070	1.10	-0.015	-0.57	-0.78	-0.35	-0.46
	(1.11)	(0.93)	(0.74)	(0.73)	(0.90)	(0.53)	(0.61)	(1.23)	(0.57)	(0.43)
T1*Yrs. Educ.	0.099	0.035	0.12	0.027	-0.0015	-0.097	-0.11	-0.039	-0.024	-0.015
	(0.15)	(0.16)	(0.09)	(0.10)	(0.11)	(0.07)	(0.08)	(0.13)	(0.09)	(0.06)
T1*Farming	0.97	-0.39	-0.093	-0.61	-0.55	-0.18	-0.30	1.26	-0.73	0.63
	(1.24)	(1.38)	(0.79)	(0.89)	(0.98)	(0.54)	(0.75)	(1.27)	(0.63)	(0.48)
T1*Business	-0.85	-0.57	-0.38	0.59	1.09	1.25	-1.17	0.31	-0.22	-0.052
	(1.39)	(1.23)	(0.93)	(0.85)	(0.88)	(0.78)	(0.92)	(1.46)	(0.66)	(0.58)
T1*HH Size	-0.12	-0.078	0.056	-0.23	0.25	-0.040	0.056	0.12	0.016	-0.027
	(0.29)	(0.22)	(0.19)	(0.16)	(0.22)	(0.15)	(0.16)	(0.31)	(0.12)	(0.11)
T1*Kids 0-14	-0.10	-0.19	-0.066	0.15	-0.26	0.16	0.36	0.15	-0.19	-0.014
	(0.41)	(0.47)	(0.29)	(0.29)	(0.35)	(0.21)	(0.23)	(0.42)	(0.26)	(0.16)
T2	-3.11	-1.33	-1.92	0.29	0.039	1.53	0.50	1.87	2.45**	-0.31
	(2.48)	(2.05)	(1.72)	(1.45)	(1.83)	(1.27)	(1.39)	(2.31)	(1.09)	(1.00)
T2*Female	0.96	0.54	1.39*	0.77	-0.063	-0.79	-0.14	-2.20	-0.35	-0.13
	(1.04)	(0.89)	(0.76)	(0.71)	(0.88)	(0.56)	(0.59)	(1.38)	(0.58)	(0.44)
T2*Yrs. Educ.	0.14	0.068	0.034	0.012	0.010	0.029	-0.13*	-0.0049	-0.19**	0.030
	(0.16)	(0.15)	(0.09)	(0.09)	(0.12)	(0.08)	(0.08)	(0.15)	(0.09)	(0.07)
T2*Farming	2.74**	1.26	0.045	-1.07	-0.18	-0.62	-1.56**	-0.093	-0.45	-0.068
	(1.27)	(1.20)	(0.80)	(0.77)	(0.95)	(0.60)	(0.72)	(1.30)	(0.68)	(0.54)
T2*Business	-0.022	-0.30	-0.12	0.33	-0.052	0.18	-0.50	0.37	0.052	0.062
	(1.28)	(1.20)	(0.87)	(0.84)	(0.89)	(0.70)	(0.91)	(1.47)	(0.69)	(0.62)
T2*HH Size	-0.20	-0.096	0.17	-0.071	0.19	-0.070	0.23	-0.17	-0.024	0.036
	(0.25)	(0.21)	(0.19)	(0.16)	(0.20)	(0.13)	(0.15)	(0.27)	(0.11)	(0.12)
T2*Kids 0-14	0.57	-0.49	0.13	0.27	-0.50	-0.15	0.33	0.19	-0.084	-0.27
	(0.48)	(0.38)	(0.25)	(0.30)	(0.34)	(0.20)	(0.20)	(0.40)	(0.24)	(0.17)
Mean	18.2	16.2	9.05	8.32	10.5	6.11	5.07	15.9	6.72	3.96
F-stat (T1)	0.32	0.36	0.41	0.75	0.94	0.67	0.97	0.33	0.45	0.62
p-value (T1)	0.95	0.92	0.89	0.63	0.48	0.70	0.46	0.94	0.87	0.74
F-stat (T2)	1.61	0.70	0.83	0.87	0.55	0.82	2.89	0.64	1.09	0.68
p-value (T2)	0.14	0.67	0.57	0.53	0.79	0.58	0.0073	0.73	0.37	0.69

Notes: Results from individual-level regressions with triplet fixed effects. 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 row " F-stat" provides the F-statistics of the joint test of the variable Treatment, Treat*Female, Treat*Yrs. Educ, Treat*Farming, Treat*Business, Treat*Kids 0-14. The associated p-value is on row " p-value" The standard errors (in parentheses) account for potential correlation within village. Standard errors, clustered by barangay, in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

Table A.19: Treated voters do not appear to shift their preferences towards incumbents

	Top Sector 1	2	3	Health, Edu, Ag.	All Sectors
Panel A: Overall effects					
Treatment	0.0028	0.0021	0.0030	0.0011	0.0036
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Observations	3411	3411	3411	3411	3411
R^2	0.05	0.04	0.04	0.04	0.04
Panel B: Separating the effects of T1 and T2					
T1	0.0053	0.0043	0.0054	0.0036	0.0063
	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)
T2	0.00050	0.000075	0.00084	-0.0012	0.0011
	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)
Observations	3411	3411	3411	3411	3411
R^2	0.05	0.05	0.04	0.04	0.04

Notes: Individual-level regressions with triplet fixed effects. Dependent variable is our measure of similarity between voter preferences and perceived policies of the incumbent. 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.20: Treated voters do not appear to shift their preferences towards incumbent in incumbent strongholds

	Top Sector 1	2	3	Health, Edu, Ag.	All Sectors
Panel A: Overall effects					
Treatment	0.0091	0.0067	0.0075	0.0074	0.0067
	(0.008)	(0.008)	(0.008)	(0.007)	(0.009)
Observations	1801	1801	1801	1801	1801
R^2	0.04	0.04	0.04	0.04	0.04
Panel B: Separating the effects of T1 and T2					
T1	0.013	0.0088	0.0090	0.0094	0.0073
	(0.009)	(0.009)	(0.009)	(0.008)	(0.011)
T2	0.0045	0.0045	0.0060	0.0052	0.0061
	(0.009)	(0.009)	(0.009)	(0.008)	(0.011)
Observations	1801	1801	1801	1801	1801
R^2	0.04	0.04	0.04	0.04	0.04

Notes: Individual-level regressions with triplet fixed effects. Dependent variable is our measure of similarity between voter preferences and perceived policies of the incumbent. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.21: Treated voters do not appear to shift their preferences towards challengers in challenger strongholds

	Top Sector 1	Top Sector 2	Top Sector 3	Health, Edu, Ag.	All Sectors
Panel A: Overall effects					
Treatment	-0.0029	-0.0037	-0.0024	-0.0091**	-0.0046
	(0.005)	(0.006)	(0.005)	(0.004)	(0.006)
Observations	2029	2029	2029	2029	2029
R^2	0.07	0.06	0.06	0.07	0.05
Panel B: Separating the effects of T1 and T2					
T1	-0.0049	-0.0063	-0.0048	-0.013**	-0.0065
	(0.007)	(0.007)	(0.007)	(0.005)	(0.008)
T2	-0.0013	-0.0016	-0.00052	-0.0058	-0.0030
	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)
Observations	2029	2029	2029	2029	2029
R^2	0.07	0.06	0.06	0.07	0.05

Notes: Individual-level regressions with triplet fixed effects. Dependent variable is our measure of similarity between voter preferences and perceived policies of the challenger. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.22: Treated voters do not appear to shift their preferences away from those of the candidate they did not vote for (Structural estimates of beliefs means)

	Closeness to Structural Mean of Beliefs
Overall effects	
Treatment	0.01
	(0.01)
Vote Choice Dummy	-0.01
	(0.02)
Treatment x Vote Choice Dummy	-0.02
	(0.02)
Constant	0.41
	(0.01)

Notes: Dependent variable is the one-norm of the difference between voter's mean belief vector and voter's preference vector. Individual x Candidate regression. Standard errors, clustered by barangay, in parentheses. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.

Table A.23: Effect of Treatment on links between Perceived Policy Similarity on Incumbent Vote Choice are attenuated for the incumbent's client (Endorsement letter)

Dep var: vote for incumbent					
Similarity:	Top Sector			Health,	All
	1	2	3	Edu, Ag.	Sectors
Client	0.028	0.029	0.029	0.030	0.029
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
T*Client	-0.020	-0.019	-0.019	-0.019	-0.021
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
T*Not Client	0.0086	0.0088	0.0084	0.0090	0.0088
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
ΔSimilarity*Client	0.24	0.23	0.15	0.12	0.12
	(0.16)	(0.15)	(0.13)	(0.17)	(0.15)
ΔSimilarity*Not Client	-0.20	-0.15	-0.13	-0.49**	0.0094
	(0.20)	(0.21)	(0.22)	(0.20)	(0.22)
T*ΔSimilarity*Client	0.038	-0.019	0.0066	0.084	0.15
	(0.28)	(0.29)	(0.28)	(0.27)	(0.27)
T*ΔSimilarity*Not Client	0.71***	0.69***	0.61**	0.95***	0.45*
	(0.24)	(0.26)	(0.27)	(0.25)	(0.25)
Observations	3144	3144	3144	3144	3144
R^2	0.30	0.30	0.30	0.30	0.30

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is a dummy equal to 1 if the respondent voted for the incumbent in the 2016 mayoral elections. 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.24: Effect of Treatment on links between Perceived Policy Similarity on Incumbent Vote Choice are attenuated for the incumbent's client (Funeral expense)

Dep var: vote for incumbent					
Similarity:	Top Sector			Health,	All
	1	2	3	Edu, Ag.	Sectors
Client	0.028	0.028	0.029	0.029	0.028
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
T*Client	0.0055	0.0058	0.0064	0.0057	0.0055
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
T*Not Client	-0.0016	-0.0021	-0.0022	-0.0021	-0.0020
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Δ*Similarity*Client	0.018	0.082	0.049	-0.16	0.039
	(0.21)	(0.19)	(0.16)	(0.20)	(0.16)
Δ*Similarity*Not Client	-0.012	-0.020	-0.019	-0.26	0.092
	(0.22)	(0.24)	(0.25)	(0.23)	(0.23)
T*Δ*Similarity*Client	0.23	0.16	0.11	0.33	0.21
	(0.30)	(0.31)	(0.30)	(0.32)	(0.26)
T*Δ*Similarity*Not Client	0.56**	0.57**	0.51*	0.76***	0.37
	(0.26)	(0.27)	(0.28)	(0.27)	(0.26)
Observations	3145	3145	3145	3145	3145
R^2	0.30	0.30	0.30	0.30	0.30

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is a dummy equal to 1 if the respondent voted for the incumbent in the 2016 mayoral elections. 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.25: Effect of Treatment on links between Perceived Policy Similarity on Incumbent Vote Choice are attenuated for the incumbent's client (Medical expense)

Dep var: vote for incumbent					
Similarity:		Top Sector		Health,	All
	1	2	3	Edu, Ag.	Sectors
Client	0.026	0.026	0.026	0.028	0.026
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
T*Client	-0.00060	-0.00049	-0.00043	-0.00057	-0.00099
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
T*Not Client	0.00060	0.00038	0.000056	0.00019	0.0010
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
ΔSimilarity*Client	0.075	0.22	0.16	-0.059	0.15
	(0.18)	(0.18)	(0.15)	(0.16)	(0.14)
ΔSimilarity*Not Client	-0.098	-0.17	-0.16	-0.37	-0.028
	(0.23)	(0.25)	(0.26)	(0.24)	(0.25)
T*ΔSimilarity*Client	0.20	0.13	0.091	0.24	0.12
	(0.27)	(0.28)	(0.26)	(0.25)	(0.24)
T*ΔSimilarity*Not Client	0.65**	0.68**	0.62**	0.89***	0.50*
	(0.28)	(0.30)	(0.31)	(0.30)	(0.28)
Observations	3149	3149	3149	3149	3149
R^2	0.30	0.30	0.30	0.30	0.30

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is a dummy equal to 1 if the respondent voted for the incumbent in the 2016 mayoral elections. 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.26: Treated voters are not more likely to be targeted for vote buying.

Dep var: targeted for vote-buying				
Treatment	-0.0015		-0.023	
	(0.03)		(0.03)	
T1		0.0074		-0.013
		(0.03)		(0.04)
T2		-0.0096		-0.031
		(0.03)		(0.04)
Kept			-0.13**	-0.13**
			(0.06)	(0.06)
T*Kept			0.055	
			(0.08)	
T1*Kept				0.032
				(0.10)
T2*Kept				0.071
				(0.09)
Observations	3423	3423	3111	3111
R^2	0.12	0.12	0.12	0.12

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is a dummy equal to one if the respondent was targeted for vote-buying. 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.27: Treated voters are not more likely to be targeted for vote buying.

Dep var: targeted for vote-buying					
Similarity:		Top Sector		Health,	All
	1	2	3	Edu, Ag.	Sectors
Panel A					
Treatment	-0.00053	-0.00066	-0.00044	-0.00040	-0.00014
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
ΔSimilarity	-0.0050	0.095	-0.0015	0.0046	0.037
	(0.21)	(0.19)	(0.21)	(0.25)	(0.20)
T*ΔSimilarity	0.31	0.12	0.24	0.26	0.25
	(0.25)	(0.24)	(0.25)	(0.31)	(0.23)
Observations	3409	3409	3409	3409	3409
R^2	0.12	0.12	0.12	0.12	0.12
Panel B					
T1	0.0089	0.0086	0.0087	0.0084	0.0085
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
T2	-0.0071	-0.0073	-0.0073	-0.0079	-0.0072
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
ΔSimilarity	0.093	0.12	0.060	0.0061	0.035
	(0.19)	(0.20)	(0.18)	(0.19)	(0.15)
T1*ΔSimilarity	0.044	0.073	0.083	0.19	0.16
	(0.29)	(0.29)	(0.28)	(0.27)	(0.25)
T2*ΔSimilarity	-0.15	-0.23	-0.14	-0.22	-0.057
	(0.27)	(0.28)	(0.26)	(0.25)	(0.23)
Observations	3334	3334	3334	3334	3334
R^2	0.12	0.12	0.12	0.12	0.12

Notes: Individual-level regressions with triplet fixed effects. The dependent variable is a dummy equal to one if the respondent was targeted for vote-buying. The standard errors (in parentheses) account for potential correlation within village. * denotes significance at the 10%, ** at the 5% and, *** at the 1% level.