PRIORS RULE: WHEN DO MALFEASANCE REVELATIONS HELP OR HURT INCUMBENT PARTIES?*

ERIC ARIAS† HORACIO LARREGUY‡ JOHN MARSHALL§ PABLO QUERUBÍN¶

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Effective policy-making requires that voters avoid electing malfeasant politicians. However, informing voters of incumbent malfeasance in corrupt contexts may not reduce incumbent support. As our simple learning model shows, electoral sanctioning is limited where voters already believed incumbents to be malfeasant, while information's effect on turnout is non-monotonic in the magnitude of reported malfeasance. We conducted a field experiment in Mexico, which informed voters about malfeasant mayoral spending before municipal elections, to test whether these Bayesian predictions apply in a developing context where many voters are poorly informed. Consistent with voter learning, the intervention increased incumbent vote share where voters possessed unfavorable prior beliefs and when audit reports caused voters to favorably update their posterior beliefs about the incumbent's malfeasance. Furthermore, we find that low and, especially, high malfeasance revelations increased turnout, while less surprising information reduced turnout. These results suggest that improved governance requires greater transparency and citizen expectations.

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†Independent researcher. Email: ericarias302@gmail.com.

‡Departments of Economics and Political Science, ITAM. Email: horacio.larreguy@itam.mx.

§Department of Political Science, Columbia University. Email: jm4401@columbia.edu.

¶Department of Politics, New York University. Email: pablo.querubin@nyu.edu.

1 Introduction

Elected politicians around the world are expected to implement policies to support economic development and alleviate poverty. The median voter in developing countries is generally poor, and thus often stands to benefit substantially from anti-poverty programs. However, such programs can be beset by bribery (e.g. Hsieh and Moretti 2006), procurement and invoicing fraud (e.g. Ferraz and Finan 2008), and misallocated spending (e.g. Larreguy, Marshall and Snyder 2020). While policy-makers and NGOs have increasingly sought to design institutions to mitigate these lasting concerns (Khemani et al. 2016), effective political accountability ultimately requires citizens to elect honest politicians. A key question is thus: when will voters hold their governments to account by punishing incumbent parties for malfeasant behavior in office?

A growing political economy literature has emphasized the importance of providing voters with information about incumbent performance in office. Exposure to negative information, such as reports revealing corruption, is expected to induce the electorate to screen out (e.g. Fearon 1999; Rogoff 1990) or sanction (e.g. Barro 1973; Ferejohn 1986) those responsible when it is believed that politicians or parties vary in competence or their efforts to represent voters' interests.

However, while several prominent studies have found that incumbent performance information promotes electoral accountability, the evidence supporting the voter learning logic is mixed. On one hand, Chang, Golden and Hill (2010), Ferraz and Finan (2008), and Larreguy, Marshall and Snyder (2020) find that media revelations of mayoral malfeasance reduce incumbent support in Italy, Brazil, and Mexico, respectively. Experimental studies by Banerjee et al. (2011) and Buntaine et al. (2018) further find that disseminating scorecards reporting incumbent activity can reduce the vote share of poorly performing elected officials and increase the vote share of highly performing elected officials in India and Uganda. On the other hand, other recent field experiments by Adida et al. (2017), Boas, Hidalgo and Melo (2019), Chong et al. (2015), and de Figueiredo, Hidalgo and Kasahara (2014) find that disseminating information about national and local incumbent performance in Benin, Brazil, and Mexico did little to affect incumbent electoral prospects. The effects on turnout of revealing incumbent malfeasance are similarly mixed: while Chong et al. (2015) suggest that unfavorable information may induce systemic disengagement in Mexico, Banerjee et al. (2011) observe increased turnout in India.

Even among the findings that information induces sanctions (rewards) for low (high)-performing incumbents, it is not obvious that information's effects actually reflect the learning mechanism underpinning theories of electoral accountability.[1] Since the studies reporting the largest effects of

[1] In fact, the studies that administer post-election surveys suggest that voting behavior changed *without* substan-

information campaigns typically involve mass media, it remains possible that information provision instead generates a public signal coordinating voters in favor of better candidates and against worse candidates without significantly updating their beliefs (e.g. Morris and Shin 2002). Another recent strand of literature shows that informational interventions may impact electoral outcomes by triggering responses from incumbent and challenger parties or inducing a strategic reallocation of campaign resources (Banerjee et al. 2011; Bidwell, Casey and Glennerster 2020; Bowles and Larreguy 2020; Cruz, Keefer and Labonne 2021). Beyond its theoretical importance, whether belief updating or these alternative mechanisms drive the effects of providing information has important implications for the design and scale of information dissemination campaigns.

We argue that voters' *prior* beliefs can play a key role in rationalizing these mixed findings, and ultimately help to explain when and how providing information about incumbent performance in office impacts turnout and vote choice. We illustrate the importance of the direction and magnitude of belief updating in response to signals of incumbent malfeasance in a two-party model where expressive voters learn about the incumbent party's underlying malfeasance. Our simple model emphasizes that, if voters already believe that their incumbent party is malfeasant, even revelations of relatively severe malfeasance can fail to decrease incumbent support because voters do not update their posterior beliefs unfavorably. Accordingly, well-intentioned interventions can produce seemingly perverse consequences in terms of supporting malfeasant politicians.

Furthermore, the implications for turnout imply a testable non-monotonicity. Under relatively general and empirically plausible distributions of partisan attachments, we show that signals which induce moderate levels of updating can reduce turnout by shifting a large mass of weak supporters of one party to abstain when turning out is costly. However, sufficiently surprising revelations—whether favorable or unfavorable—increase turnout by shifting voters who previously abstained, and even supporters of the other party, to turn out for the party shown to be less malfeasant.

We test these theoretical predictions, which we pre-registered, using a field experiment conducted in Mexico around the 2015 municipal elections. Beyond its large population and recent shift towards a more pluralistic democracy, Mexico's relatively high—but substantially varying—levels of corruption and distrust in elected politicians across municipalities make it a well-suited location to test our argument. Although municipal mayors could not seek re-election at the time, voters hold parties responsible for incumbent performance in office in Mexico's party-centric system. Extending two recent studies examining electoral responses to municipal audit reports, but with markedly different findings (Chong et al. 2015; Larreguy, Marshall and Snyder 2020), we examine how voters respond to leaflets revealing the extent to which municipal governments correctly

tially altering voter beliefs about incumbent performance (Banerjee et al. 2011; Buntaine et al. 2018).

spent federal transfers earmarked for social infrastructure projects benefiting the poor.

We partnered with a local civil society organization to disseminate leaflets documenting the results of independent municipal audit reports across 678 rural and urban electoral precincts in 26 municipalities from four central Mexican states in the weeks just before the election. Voters in treated precincts, where leaflets were delivered to up to 200 households, received one of two measures of incumbent malfeasance: the share of funds earmarked for social infrastructure projects that was spent on projects that did not benefit the poor, or the share of such funds spent on unauthorized projects. These measures ranged from 0% to 58% in our sample, with significant variation around the mean of 21%. Since a baseline survey was not financially feasible, we use the municipal control group's post-election beliefs to proxy for the pre-treatment prior beliefs of treated and control voters within each municipality. We further proxy for belief updating among treated voters within each municipality by measuring belief updating by control group respondents when exposed to the leaflet information during the post-election survey. A variety of tests validate these proxies.

Consistent with the theory, we find that the impact of revealing municipal audit reports on voters' support for the incumbent party depends on how the information relates to their prior beliefs. On average, information did not affect voters' posterior beliefs regarding incumbent party malfeasance. Most likely by increasing the certainty of risk averse voters, treatment ultimately increased the incumbent party's vote share by two percentage points. However, our key finding is that voter learning is a central force driving voting behavior. At both the individual and precinct levels, we show that the average effects mask substantial heterogeneity in the response of a Mexican electorate generally skeptical that local politicians allocate funds as legally mandated. Specifically, the increase in incumbent support induced by our treatment is concentrated in municipalities in which audit reports revealed low malfeasance, and where voters already believed that the incumbent party was malfeasant, voters possessed less precise prior beliefs, and voters most favorably updated their posterior beliefs regarding incumbent party malfeasance upon receiving the information.

The non-monotonic effect of malfeasance revelations on electoral turnout is also supported, though changes in turnout were relatively small. Information provision produced heterogeneous effects on turnout, with relatively unsurprising information—20-30% of funds spent on projects that did not benefit the poor or on unauthorized projects—depressing turnout by around 1 percentage point and extreme cases of malfeasance—both 0% and above 50%—mobilizing turnout by around 0.5 percentage points. In contrast with the view that malfeasance revelations breed generalized disengagement (Chong et al. 2015), we find little evidence to suggest that revealing more severe cases of malfeasance to voters reduces confidence in the capacity of elections to select com-

petent politicians.

Several further analyses suggest that these changes in beliefs and voting behavior were largely driven by voter learning. First, a number of robustness checks show that heterogeneity in response to treatment is not driven by potential confounds of voters' prior beliefs or the level of malfeasance reported. Second, although incumbent and especially challenger parties discredited or incorporated malfeasance reports into their election campaigns, these reactions are unlikely to be the primary determinant of voters' response to treatment. This is because the information treatment increased incumbent support on average and politician reactions did not respond differentially in municipalities where voters had more favorable prior beliefs or updated more unfavorably about the incumbent after receiving the information. Third, voter coordination was also second-order for understanding the intervention's positive effect on incumbent support overall as well as heterogeneity in its effect by voters' prior beliefs, voters' belief updating, and the level of malfeasance reported. As we show in a separate paper, electoral precincts containing more highly-connected networks did respond to treatment by coordinating votes for challengers, but this occurred where voters already believed that challenger parties were less malfeasant than incumbent parties rather than in response to new malfeasance information (Arias et al. 2019).

By documenting electoral accountability and sophisticated learning by voters in response to receiving incumbent performance information, this article makes three main contributions. First, we provide the first clear evidence from a developing country of the Bayesian interaction between the provision of non-partisan information and prior beliefs for understanding voting behavior. While previous studies have highlighted the potential importance of voters' prior beliefs about incumbent performance (Banerjee et al. 2011; Buntaine et al. 2018; Chong et al. 2015; Ferraz and Finan 2008; Humphreys and Weinstein 2012; Larreguy, Marshall and Snyder 2020), such studies either did not measure prior beliefs and updating or did not detect effects of information provision on posterior beliefs and actual vote choices.[2] By illustrating the voter learning channel, our findings help rationalize why Brazilian voters only punish incumbents responsible for more than one corruption violation (Ferraz and Finan 2008) and performance scorecards affect support for the best- and worst-performing Indian politicians (Banerjee et al. 2011). As well as differences in dissemination technologies and the relevance of the information provided, voters' low expectations of elected politicians may then explain the mixed impact of disseminating credible indicators of what may objectively be regarded as poor incumbent performance on electoral accountability.

[2]Other studies in the EGAP Metaketa initiative, which this study was part of, also examined the updating of posterior beliefs (see Dunning et al. 2019). However, the other studies generally yielded relatively inconclusive evidence, possibly because they focused primarily on the *direction* of updating (rather than its extent) and took a different theoretical approach to turnout.

Second, our focus on voters' prior beliefs in the context of non-partisan information provision complements previous studies highlighting the significance of prior beliefs for understanding voter responses to partisan campaign messages. In particular, Kendall, Nannicini and Trebbi (2015) indicate that Italian voters in a single municipality updated from both valence and ideological messages delivered by an incumbent's canvassing team during an election campaign, although only the valence message—the high regional ranking of the mayor's development plan—influenced vote choice.[3] Our results imply that such sophisticated learning in a developed context extends to performance indicators delivered by non-partisan sources in a developing context. This may be especially important for policy-makers and civil society organizations seeking to maintain credibility by avoiding partisan messaging.

Third, we provide an alternative interpretation for extant results suggesting that revelations of malfeasance motivate voters to disengage from the political system and reduce turnout (Chong et al. 2015; de Figueiredo, Hidalgo and Kasahara 2014). Most notably, Chong et al. (2015) surprisingly find that revealing severe malfeasance reduced challenger turnout more than incumbent turnout. However, since this does not account for how the information provided related to voters' prior beliefs, it can be rationalized within our theoretical framework by voters expecting particularly high levels of malfeasance by the incumbent party. Although we do not preclude disengagement, at least in theory, our approach nevertheless substantiates the claim that the mixed extant findings with respect to turnout may to a significant degree reflect Bayesian updating. The importance of belief updating in making turnout decisions also accords with Leon's (2017) finding that experimentally reducing voters' perception of fines for abstention reduced turnout in Peru, especially among the voters most indifferent between parties.

The article is structured as follows. Section 2 describes the Mexican municipal context motivating our argument. Section 3 presents a simple model highlighting the conditions under which information increases or decreases a voter's propensity to turn out and cast a ballot for the incumbent party. Section 4 explains and validates our experimental design. Sections 5 and 6, respectively, present the individual- and precinct-level results. Section 7 concludes.

2 Malfeasance, audits, and elections in Mexican municipalities

Mexico's federal system is divided into 31 states (and the Federal District of Mexico City), which contain around 2,500 municipalities and 67,000 electoral precincts. Following major decentraliza-

[3]Other natural or field experiments also document the effectiveness of partisan campaign messaging (e.g. Larreguy, Marshall and Snyder 2018; Pons 2018; Spenkuch and Toniatti 2018), but without examining voter beliefs.

tion reforms in the 1990s (see Wellenstein, Núñez and Andrés 2006), municipal governments—the focus of this article—have played an important role in delivering basic public services and managing local infrastructure. Municipalities, which account for almost 10% of total government spending, are governed by mayors who were typically elected to three-year non-renewable terms.[4]

2.1 Independent audits of municipal spending

A key component of a mayor's budget is the Municipal Fund for Social Infrastructure (FISM), which represents 24% of the average municipality's budget. According to the 1997 Fiscal Coordination Law, FISM funds are direct federal transfers mandated exclusively for infrastructure projects that benefit populations living in poverty, as defined by localities deemed to be marginalized by the National Population Council (CONAPO). Eligible projects include investments in the water supply, drainage, electrification, health infrastructure, education infrastructure, housing, and roads. However, citizens are poorly informed about both the resources available to mayors and their responsibility to provide basic public services (Chong et al. 2015).

The use of federal funds, including FISM transfers, is subject to independent audits by the Federal Auditor's Office (ASF). Although the ASF reports to Congress, its autonomy is enshrined in the constitution, and it has the power to impose fines, recommend economic sanctions, and file or recommend criminal lawsuits against public officials. The ASF selects around 150 municipalities for audit each year, based primarily on the relative contribution of FISM transfers to the municipal budget, historical audit outcomes, factors that raise the likelihood of mismanagement, and whether the municipality has recently been audited (including concurrent federal audits of other programs—see Auditoría Superior de la Federación 2014). The municipalities to be audited in a given year are announced after the funds disbursed for a given fiscal year have been spent.

Audits address the spending, accounting, and management of FISM funds from the previous fiscal year. We focus on two key dimensions of mayoral malfeasance documented in these audit reports, which are not necessarily mutually exclusive: (i) the share of funds spent on social infrastructure projects that did not directly benefit the poor; and (ii) the share of funds spent on unauthorized projects, which includes the diversion of resources to non-social infrastructure projects (e.g. personal expenses and election campaigns) and funds that are not accounted for. Between 2007 and 2015, 8% of audited funds were spent on projects that did not benefit the poor, while 6% were spent on unauthorized projects. Since ASF reports capture only one dimension of malfeasance, it is not surprising that 42% of voters do not believe that municipal governments use public resources honestly (Chong et al. 2015).

[4]Re-election became possible for incumbents in most states as of July 2018.

At the time of this study, the results for each audited municipality were reported to Congress in February the year after the audit was conducted. All reports are available on the ASF's website, asf.gob.mx. Despite their public release, most voters are poorly informed about the ASF and media coverage of individual municipalities is mixed.

2.2 Municipal elections

Traditionally, local political competition has been between either the populist Institutional Revolutionary Party (PRI) and the right-wing National Action Party (PAN), or between the PRI and its left-wing offshoot, the Party of the Democratic Revolution (PRD). Due to regional bases of political support and highly localized influence within municipalities, local politics is typically dominated by one or two main parties.[5] In the municipal elections that we study, the average effective number of political parties by vote share at the precinct and municipal levels remains consistently around 2.5.[6] Moreover, as Appendix Figure A1 shows, this two-party dominance is reflected in the generally bimodal distribution of voter partisanship within municipalities.

Although economic and criminal punishments for misallocating funds are relatively rare, there are good reasons to believe that voters might hold the incumbent party responsible, even before mayors could seek re-election. First, voters are considerably better informed about political parties than about individual politicians (e.g. Chong et al. 2015; Larreguy, Marshall and Snyder 2018). Crucially for political accountability, 80% of voters in our survey can correctly identify the party of their municipal incumbent. Second, Mexico's main parties have differentiated candidate selection mechanisms that deliver candidates with similar attributes (Langston 2003). For example, 74% of voters in our survey believe that if the current mayor is malfeasant, then another candidate from the same party is likely to also be malfeasant. Third, citizens care about how their governments allocate resources. The surveys we conducted for this study show that 74% and 72% of respondents in control precincts, respectively, regard fighting poverty and honesty as important or very important when deciding which candidate to vote for.

However, extant evidence of electoral sanctioning in response to revelations of malfeasant behavior in Mexico is mixed. Among voters with access to more broadcast media outlets incentivized to report local news, Larreguy, Marshall and Snyder (2020) observe larger electoral penalties (rewards) in municipalities where the ASF reported high (low) levels of malfeasance just before mu-

[5] In order to get elected, the three large parties often subsume smaller parties into municipal-level coalitions. The National Regeneration Movement (MORENA) also stood for the first time in 2015, and made headway against this hegemony at the national level, obtaining 9% of the federal legislative vote. However, it was not until 2018 that MORENA obtained large vote shares across the country.

[6] The effective number of parties is given by $\frac{1}{\sum_{j \in \mathbb{J}} V_j^2}$, where V_j is party j's vote share (Laakso and Taagepera 1979).

nicipal elections. Conversely, in a field experiment conducted in 12 municipalities across three states, Chong et al. (2015) suggest that flyers documenting severe incumbent malfeasance breed generalized disengagement: while incumbent support declined when the incumbent was revealed to be highly malfeasant, challenger support also declined at least as much. The disjuncture between these accountability and disengagement findings, which cover the same information over the same period, exemplifies the need for a more refined theory capable of explaining when and why different types of information impact voters differently.

3 Information, prior beliefs, and voting behavior

Our theoretical framework explores the implications for electoral accountability of providing information about incumbent malfeasance. A simple learning model first shows that the impact of information on voters' posterior beliefs—and ultimately their vote choice—depends on how the information revealed relates to voters' prior beliefs. Our second insight concerns turnout: with a positive cost of voting and an empirically plausible distribution of voter partisan attachments, information relatively close to voters' prior beliefs may reduce turnout, while major departures can increase turnout by causing wholesale shifts in support between incumbent and challenger parties.

3.1 Model

We consider a simple decision-theoretic model in which voters in a given municipality—or part of a municipality—update their posterior beliefs about a party's malfeasance based on informative signals, and choose between voting for incumbent party I, voting for challenger party C, and abstaining.[7] Since two-party competition predominates in most parts of Mexico, this assumption approximates political competition in most Mexican municipalities.

We assume that voters receive expressive utility from voting for the *relatively* less malfeasant party, and only turn out if parties are sufficiently different in terms of the utility that voters expect to obtain from either of them.[8] For analytical simplicity, we model the expected utility that voter i receives from voting for party $j \in \{I, C\}$ as the sum of a fixed partisan benefit deriving from voting

[7] In the model, we abstract from party attempts to counteract the effect of scandal exposure. Empirically, we find some evidence of such responses. However, as explained below, this operates alongside, rather than in place of, voter updating of posterior beliefs.

[8] In the relatively large municipalities of our sample, voters are unlikely to perceive themselves as pivotal. In such contexts, expressive voting is a standard motive for voting (e.g. Brennan and Hamlin 1998).

for I and j's expected level of malfeasance:[9]

$$U_{ij} = \begin{cases} \delta_i + \mathbb{E}\left[-\theta_I\right] & \text{if } j = I \\ \mathbb{E}\left[-\theta_C\right] & \text{if } j = C \end{cases} \tag{1}$$

where $\delta_i \in \Gamma \subseteq \mathbb{R}$ is a partisan bias towards the incumbent that can be positive or negative, and party j's type $\theta_j \in \mathbb{R}$ captures its underlying level of malfeasance.[10] The partisan bias δ_i is independently and identically distributed across voters according to a twice-differentiable cumulative distribution function F, and could reflect durable partisan attachments or shocks occurring before the election that are uncorrelated with prior beliefs and signals of malfeasance. For simplicity, voters are assumed to be risk-neutral regarding the costs of expected malfeasance θ_j, although similar results hold when voters are risk averse. Finally, $c > 0$ is a constant cost of turning out to vote.

A voter only turns out to vote if the difference in expected utility between the two parties is large enough. Conditional on voting, individuals vote for their most preferred party: i votes for incumbent party I if $\Delta_i := U_{iI} - U_{iC} \geq c$, votes for challenger party C if $-\Delta_i \geq c$, and abstains if $|\Delta_i| < c$.[11]

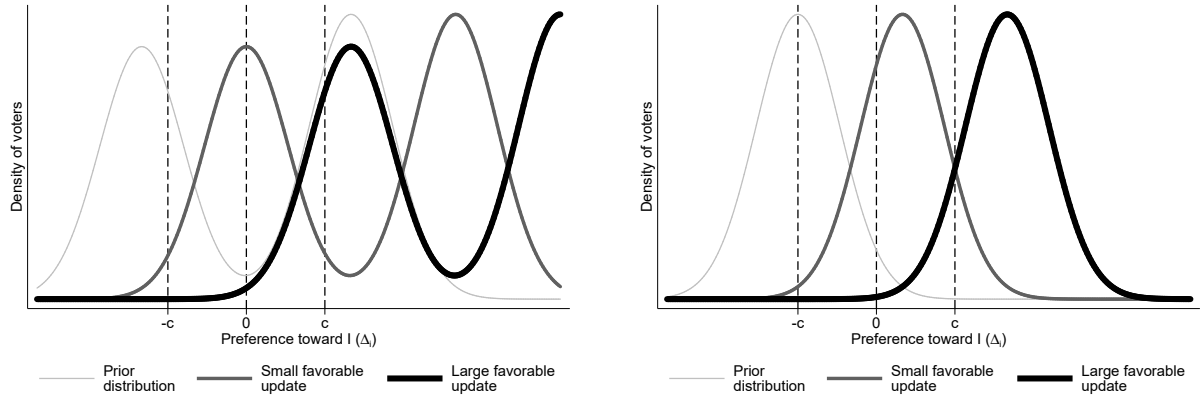
Voters are uncertain about the underlying malfeasance θ_j of both the incumbent and challenger parties, and learn from a signal about incumbent party malfeasance in a Bayesian fashion. We assume that all voters in a municipality share a prior belief about the malfeasance of each party j that is normally distributed according to $N(\mu_j, \sigma_j^2)$, where $\lambda_j := 1/\sigma_j^2$ denotes the precision of the prior beliefs. Heterogeneity in prior beliefs across municipalities could emanate from differences in the (realization or number of) private signals pertaining to incumbent and challenger party malfeasance that voters were previously exposed to. When voters receive an audit report documenting malfeasance that pertains to the incumbent, they observe a common signal s_I drawn from a normal distribution $N(\theta_I, \tau_I^2)$ centered on the incumbent's true (but unknown) malfeasance level θ_I. The known precision of this signal, $\rho_I := 1/\tau_I^2$, could derive from the audit report only capturing one dimension of an incumbent's malfeasance. For simplicity, we consider the case where the malfeasance of each party is known to be independently distributed, but show similar results for correlated prior beliefs in Appendix section A.6.

After receiving a signal s_I of underlying incumbent malfeasance, voters' posterior beliefs about

[9] The theory could be extended to incorporate a ban on re-election by allowing for imperfect within-party candidate correlations. Provided that candidates within parties are sufficiently similar, the forces underpinning our results remain.

[10] The latent malfeasance dimension, and the signal described below, are modeled with unbounded support to simplify the analysis using a normal learning framework.

[11] An alternative specification of expressive utility, in which voters vote for j if $U_{ij} > \max\{U_{i,-j}, c\}$, would complicate our analysis but yield qualitatively similar comparative statics.



(a) Bimodal distribution, where each mode in the prior distribution votes for a different party

(b) Symmetric unimodal distribution

Figure 1: Vote choice for different signals and distributions of voters

θ_I become:

$$N\left(\mu_I + \kappa_I(s_I - \mu_I), \frac{1}{\lambda_I + \rho_I}\right) \tag{2}$$

where $\kappa_I := \frac{\rho_I}{\lambda_I + \rho_I}$ captures the relative precision of the signal. Higher values of κ_I increase voter updating because the signal is precise relative to voters' prior beliefs, while positive values of $s_I - \mu_I$—which we henceforth refer to as the extent of unfavorable updating—indicate that the signal exceeds the prior expectation of incumbent malfeasance among voters. The difference in the expected utility of voting for I relative to C for voter i then becomes: $\Delta_i = \delta_i - \mathbb{E}[\theta_I | s_I, \mu_I] + \mathbb{E}[\theta_C | \mu_C] = \delta_i - [\mu_I + \kappa_I(s_I - \mu_I)] + \mu_C$.

Integrating over the distribution of voter partisan biases, we obtain the following results pertaining to the share of voters V_I that turn out for the incumbent party:

Proposition 1 (**Incumbent vote share**). *Receiving a signal s_I of incumbent malfeasance increases incumbent party vote share V_I, relative to receiving no signal, if and only if $s_I < \mu_I$. This difference in V_I is decreasing in s_I and increasing in μ_I (provided that κ_I is sufficiently large), and the magnitude of the difference is decreasing in λ_I.*

Proof. All proofs are in Appendix section A.1. ■

The effect of different signals on the incumbent party's vote share is illustrated in Figure 1, which plots the distribution of voters by their relative preference Δ_i for the incumbent for bimodal

and unimodal cases of F. Voters for whom $\Delta_i > c$ turn out for I, while voters for whom $\Delta_i < -c$ vote for C; the voters for whom $\Delta_i \in [-c, c]$ abstain. We analyze how the key parameters in our model affect voting behavior by shifting the distribution of voters along the Δ_i axis.

The overall effect of information dissemination depends on how the signal relates to voters' prior beliefs. As illustrated by the three distributions of voter preferences, a signal that the incumbent is less malfeasant than voters initially believed (i.e. $s_I < \mu_I$) raises Δ_i to increase I's vote share by producing a commensurate shift in the distribution of relative voter preferences to the right. A comparison of the medium and thickest distributions shows that a larger favorable update—due to an especially low s_I—further increases I's vote share. The magnitude of the distribution shift, in either direction, is decreasing in λ_I because relatively precise prior beliefs reduce the weight attached to the signal in a voter's posterior belief. Finally, the degree to which the distribution shifts after voters receive the public signal is also influenced by μ_I: where voters possess more unfavorable prior beliefs, the effect of the signal is more favorable toward the incumbent because a larger mass of voters can be persuaded to support the incumbent party.

While the incumbent vote share results hold for any distribution F of partisan attachments, the effect of providing information about the incumbent on overall turnout T depends on the shape and position of F and the extent to which information induces updating:

Proposition 2 (Turnout). *Receiving a signal s_I of incumbent malfeasance ambiguously affects turnout: T increases (decreases) when $F(\bar{\delta}_C) - F(\widehat{\delta}_C) - [F(\bar{\delta}_I) - F(\widehat{\delta}_I)] > (<) 0$, where $\bar{\delta}_j$ and $\widehat{\delta}_j$ denote the points of indifference between voting for party j and not voting, respectively, with and without the signal. This effect is increasing (decreasing) in s_I when $F'(\bar{\delta}_C) - F'(\bar{\delta}_I) > (<) 0$.*

To illustrate the intuition, consider the case where voters receive $s_I < \mu_I$. This signal of lower-than-expected incumbent malfeasance produces two effects: (i) it induces some voters who would not otherwise have voted to turn out for I; and (ii) it induces some voters who would otherwise have voted for C not to turn out. The relative masses of voters associated with these conflicting effects on turnout determine whether turnout increases or decreases. Intuitively, a sufficiently extreme realization of s_I in either direction will eventually increase turnout because all voters will support or oppose the incumbent party. However, the effect of more moderate signals depends on both F and $s_I - \mu_I$.

To produce sharper empirical predictions, we gain insight by focusing on two empirically-plausible distributions. Motivated by the distribution of partisan attachments in the two-party races that are common in Mexican municipalities (see Appendix Figure A1), Figure 1a first considers the case where F is bimodally distributed and, absent a signal, voters at each mode turn out for different parties. Signals that constitute small and large favorable updates demonstrate how the

effect on turnout can be non-monotonic: while the medium thickness distribution associated with a small favorable update reduces turnout by shifting the mode where voters initially supported C to a point of abstention, the thickest distribution associated with a large favorable update increases turnout by shifting the same mode to vote for I. In less polarized contexts, the distribution of partisanship could be unimodal. Figure 1b considers a symmetric unimodal distribution where the distribution is not centered on zero. This case shows that a sufficiently moderate signal again decreases turnout.

More generally, providing incumbent performance information can produce a non-monotonic effect on turnout whenever the initial distribution of voters allows for a sufficiently large mass of voters to instead abstain—inducing an aggregate decrease in turnout—in response to at least some signals. Building from our two example distributions, the following proposition establishes sufficient conditions for a non-monotonic effect of providing information on incumbent malfeasance on turnout that are relatively general:

Proposition 3 (**Non-monotonic effects on turnout**). *The following conditions guarantee that the effect of receiving a signal s_I of incumbent malfeasance on turnout is positive for $s_I \leq s^*$ and $s_I \geq s^{**} > s^*$, and is negative for some $s_I \in (s^*, s^{**})$:*

- *F is unimodal and the distribution of voter prior beliefs does not minimize or maximize T.*
- *F is bimodal with modes m_C and m_I, where $m_C \leq \widehat{\delta}_C < \widehat{\delta}_I \leq m_I$ and $F'(\widehat{\delta}_C) \neq F'(\widehat{\delta}_I)$.*

Where F has more than two modes, which is rare in the empirical context of this study, the effect of a common signal may vary across intervals of s_I.

3.2 Empirical implications

We focus our comparative static predictions on the effect of providing voters with a common signal of incumbent malfeasance, s_I, via a treatment containing information pertaining to mayoral malfeasance. We now enumerate the core hypotheses that motivate our field experiment and that we registered in our pre-analysis plan.

Comparing the expected prior belief (μ_I) and posterior belief ($\mu_I + \kappa_I(s_I - \mu_I)$) shows that the average effect of providing information on voters' posterior beliefs—and vote choice, as Proposition 1 shows—depends on how the signal relates to voters' prior expectations of malfeasance (i.e. $s_I - \mu_I$) in the average municipality. While the average treatment effect is context-dependent and hard to anticipate until prior beliefs are measured in the municipalities under study, there

are clear second-order predictions for the effect of information dissemination on voters' posterior beliefs. First, this effect is smaller where voters already believe that the incumbent party is malfeasant (i.e. high μ_I), since the signal drags a voter's posterior belief away from their prior belief. Second, the posterior beliefs of voters where voters already possess precise prior beliefs about incumbent malfeasance (i.e. low κ_I, or high λ_I) are less responsive to new information in either direction. Third, voters update their posterior beliefs more favorably (unfavorably) about the incumbent party's malfeasance upon learning that the incumbent is relatively clean (malfeasant) (i.e. low (high) s_I).

These effects on voter' posterior beliefs are summarized in the following hypothesis:

H1 (**Posterior beliefs**). *The effect of providing information about an incumbent's malfeasance on voters' posterior beliefs about whether the incumbent party is malfeasant is:*

(a) Decreasing in voters' prior beliefs that the incumbent party is malfeasant.

(b) Decreasing in magnitude with the precision of voters' prior beliefs.

(c) Increasing in the severity of the reported malfeasance.

In addition to establishing the extent to which voters update their beliefs, these empirical predictions regarding voters' posterior beliefs imply the following effects on the incumbent party's vote share:

H2 (**Incumbent party vote share**). *The effect of providing information about an incumbent's malfeasance on the incumbent party's vote share is:*

(a) Increasing in voters' prior beliefs that the incumbent party is malfeasant.

(b) Decreasing in magnitude with the precision of voters' prior beliefs.

(c) Decreasing in the severity of the reported malfeasance.

(d) Decreasing in the extent to which the information unfavorably updates voters' prior beliefs.

As shown above, new information is predicted to produce non-monotonic effects on turnout under empirically plausible distributions of voter partisanship. In particular, highly favorable or unfavorable revelations motivate voters who previously abstained to turn out to vote and induces voters to switch parties, while relatively unsurprising—but nevertheless informative—favorable (unfavorable) information induces challenger (incumbent) partisans to become relatively indifferent between the parties and abstain from voting. While this logic does not yield clear predictions

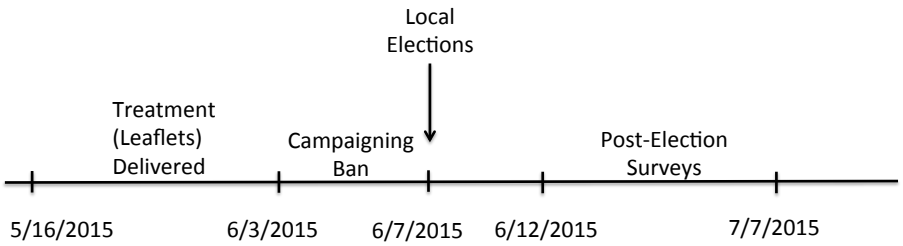


Figure 2: Timeline of the experiment's implementation

for the average effect of new information or its linear interaction with the level of malfeasance reported, it clearly predicts that:

H3 (Turnout). *Providing information reporting sufficiently high and low levels of incumbent malfeasance increases electoral turnout, while some intermediate levels of reported malfeasance decrease turnout.*

4 Experimental design

We designed a field experiment to test the model's predictions around Mexico's June 7, 2015 municipal elections, which were held concurrently with state and federal legislative elections. We examine the effect of providing voters in 678 electoral precincts across 26 municipalities with the results of audit reports documenting the municipal use of federal transfers designated for infrastructure projects benefiting the poor. We first explain our sample selection, and then outline the intervention, randomization, and estimation strategy. Figure 2 summarizes the project's timeline.

4.1 Sample selection

Our study focused on 26 municipalities in the central states of Guanajuato, México, San Luis Potosí, and Querétaro. In addition to the fact that they held elections in 2015,[12] these four states were chosen for security and logistical reasons, and because they exhibit variation in the municipal incumbent party. The municipalities were selected to ensure: (i) the safety of voters and our implementing team; (ii) that the level of malfeasance reported by the ASF differed from the average

[12]Municipal elections reflect state electoral cycles, which are staggered across years. On June 7, 2015, 15 states and the federal district held simultaneous local elections.

of other incumbent parties elsewhere in the state; and (iii) that the proportion of municipal governments run by each party matched the proportion of municipalities audited by the ASF. The average municipality contained 259,000 registered voters. Appendix section A.3.3 explains the selection of municipalities in detail.

After immediately receiving threats upon entering Aquismón and Villa Victoria, these municipalities were replaced by Atlacomulco, Temoaya, and an additional block from Tlalnepantla de Baz in the state of México. Since our blocking strategy—explained below—ensured that treatments were randomized within municipalities, excluding these problematic municipalities does not affect the study's internal validity.

Within each municipality, we selected up to one third of the electoral precincts. To generate variation in the level of malfeasance reported, we oversampled precincts from municipalities with particularly high or low levels of incumbent malfeasance and starker contrasts with opposition party malfeasance within the state. Within municipalities, we first prioritized accessible rural precincts to mitigate the risk of cross-precinct spillovers and the possibility that voters had already encountered the audit information. Moreover, to maximize the share of households that we could reach with a fixed number of leaflets, attention was restricted to precincts with fewer registered voters. In urban precincts, which constitute 49% of our sample, we restricted attention to precincts with at most 1,750 registered voters, and minimized the number of neighboring urban precincts in our sample. Appendix Table A2 shows that our final sample of precincts is similar to the national distribution according to various socioeconomic indicators from the 2010 Census.

4.2 Information treatment

In partnership with the Mexican NGO Borde Político,[13] we sought to evaluate the impact of distributing leaflets to voters that documented the use of FISM funds in their municipality. For each municipality, the leaflet focused on one of two indicators of incumbent malfeasance—which represent signals of θ_I in our model—documented by the ASF that observational studies find voters care about (Larreguy, Marshall and Snyder 2020): the proportion of unauthorized spending or the proportion of spending that did not benefit the poor. For each municipality, we chose the malfeasance measure that maximized the difference from other parties within the municipality's state. All treatments were delivered at the electoral precinct level, Mexico's lowest level of electoral aggregation.

[13]Borde Político is a leading NGO seeking to increase voter knowledge about the actions of their politicians in office, with significant experience in developing web-based platforms to provide politically relevant information to voters (see borde.mx).

Figure 3: Example of local information leaflet in Ecatepec de Morelos, México

The leaflet was designed to be non-partisan, accessible, and sufficiently intriguing that voters would not discard it.[14] Figure 3 provides an example of a leaflet focusing on a severe case of unauthorized spending in the municipality of Ecatepec de Morelos in the state of México. The front page explains that Borde Político is a non-partisan organization and that the information contained in the leaflet is based on the ASF's official audit reports, which are available online. The main page first states that FISM funds should only be spent on social infrastructure projects, and provides graphical examples of such projects on the right. The leaflet then informs recipients of the total amount of money their municipality received (146.3 million pesos, in this case), and the percentage of this money that was spent in an unauthorized way (45%). To avoid suspicions of political motivation, neither the incumbent mayor nor their party is referred to directly. Figure 4 shows that the average precinct in our sample was informed of 21% malfeasant spending within their municipality.

The experiment also incorporated two variants of this information treatment. First, to examine the effect of providing voters with a benchmark against which to compare their municipality's

[14]The leaflet was produced by a local graphic designer based on feedback from multiple focus groups. We also sought legal advice to ensure that the leaflets did not constitute political advertisements, and thus were not subject to distribution restrictions stipulated in Mexican electoral law.

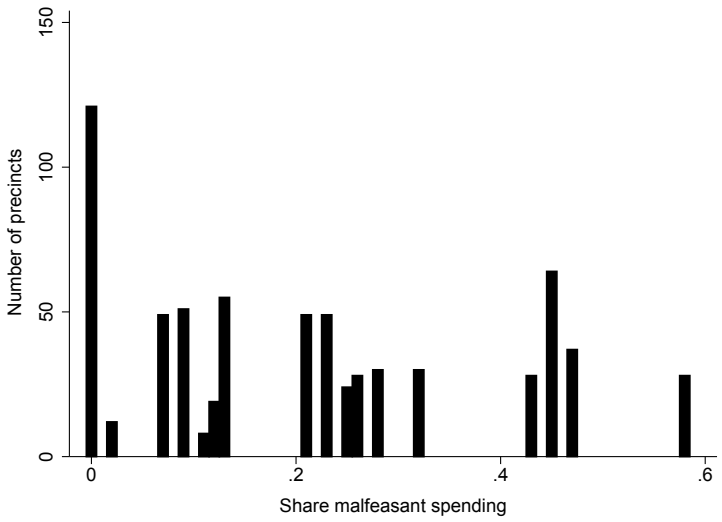


Figure 4: Precincts by share of malfeasant spending in our sample

malfeasance, we supplemented the leaflet by providing the mean outcome among all audited municipalities within the same state governed by a different political party; Appendix Figure A3 provides an example of such a leaflet. Second, to vary the extent to which the distribution of the leaflets was common knowledge among voters within the precinct, we also varied whether leaflet delivery was accompanied by a loudspeaker informing voters that their neighbors would also receive the information and encouraging them to share and discuss it. These treatment variants did not generate different effects, as shown in Tables A32 and A33. We present more details about the treatment variants and additional results in a short companion paper (Arias et al. 2018).

4.3 Block randomization and implementation

Our sample of 678 precincts was randomly assigned to receive treatments according to the factorial design described in Table 1. The 400 treated precincts were divided equally between the four versions of the information treatment. Given that neither the comparative nor public information components significantly moderated our treatment effects, and all leaflets contained the same baseline information pertaining to incumbent malfeasance, we proceed by pooling all treatment conditions. The control group, comprising 278 electoral precincts, received no leaflets.

For the randomization, precincts were first stratified into blocks each containing six or seven similar precincts *within* a given municipality.[15] Within each block, we then randomly assigned

[15]If there were sufficient precincts, and the total number of treated precincts did not exceed one-third of all

Table 1: Factorial design with a pure control

	Control	Private	Public
Control	278 precincts		
Local		100 precincts	100 precincts
Comparative		100 precincts	100 precincts

precincts to each of the treatment conditions and, depending on the availability of an additional precinct, either two or three pure control precincts. Because blocks lie strictly within municipalities, malfeasance information always pertains to the same municipal incumbent party and dimension of malfeasance for all precincts within a block.

Our distribution teams delivered one leaflet to a maximum of 200 randomly selected households in the largest locality in rural blocks and randomly selected city blocks in urban blocks within each treated precinct.[16] Within our sample, the median precinct contained 353 households (according to the 2010 Census) and 1,056 voters registered for the 2015 election. Where possible, leaflets were delivered in person with a short verbal explanation of the leaflet's provenance. When no adult was available, leaflets were left in mailboxes or taped to the recipient's front door in a waterproof bag. Leaflet delivery took several hours per precinct, and was conducted over a period of three weeks, concluding at the legally designated end of the election campaign four days before the election.

While compliance with the delivery of our treatments was very good in general, we also encountered several issues in the field. In a couple of cases, some leaflets were delivered to voters outside the precinct or adverse weather conditions and poor road conditions prevented us from reaching a precinct.[17] To preserve the randomization, we focus on estimating intent to treat (ITT) effects, which are arguably also the most policy relevant estimand.

4.4 Precinct- and individual-level data

We collected two sources of data to measure our main outcomes. First, using publicly available results and freedom of information requests, we collected official precinct-level electoral returns from each state's electoral institute to compute three pre-registered precinct-level outcomes: in-

precincts, we used blocks of seven precincts. Precinct similarity was defined by the Mahalanobis distance between 23 social, economic, demographic, and political variables provided by Mexico's National Statistical Agency and the National Electoral Institute (INE).

[16] Since randomization blocks consist of either only rural or only urban precincts, block fixed effects fully account for any sampling differences across rural and urban precincts.

[17] The results are robust to dropping the misassigned precincts from our sample.

cumbent party vote share (as a share of turnout), incumbent party vote share (as a share of registered voters), and turnout. Measuring incumbent party vote share using the share of registered voters allows us to abstract from changes in turnout. We drop the three precincts in our sample that the INE merged with another precinct because they contained fewer than 100 registered voters, which produces a final sample of 675 electoral precincts.[18] We complement the 2015 precinct-level electoral returns with covariates from the 2010 Census and 2012 electoral returns.

Second, we conducted a post-election survey that interviewed ten voters from each of the treated precincts and ten voters from a randomly selected control precinct within each block.[19] At the beginning of the survey, we measured voters' posterior beliefs about each major party's level of corruption or level of interest in supporting the poor (depending on the measure of malfeasance that the leaflets reported on in that municipality) on a five-point scale from very low (-2) to very high (2).[20] Higher values of this variable indicate that voters believed a party was more malfeasant.[21] To gauge the precision of these beliefs we then asked respondents to report how certain they were about this belief on a four-point scale ranging from very uncertain (1) to very certain (4). Summary statistics for the main variables are provided in Appendix Table A3.

4.5 Estimation and balance

Following our pre-analysis plan, we estimate the average ITT effect of providing any type of information using OLS regressions of the form:

$$Y_{pbm} = \alpha_{bm} + \beta Treatment_{pbm} + \varepsilon_{pbm}, \tag{3}$$

where Y_{pbm} is an outcome for electoral precinct p within randomization block b in municipality m. For individual-level survey outcomes, Y_{ipbm} also includes an i subscript. Block fixed effects, α_{bm}, are included to adjust for the differential probabilities of treatment assignment across blocks, arising from different block sizes, and to increase efficiency by absorbing block-specific characteristics, such as race-specific differences across municipalities. Including block fixed effects also

[18] In two of these cases, the precinct was merged with another precinct that remains in our sample; where the treatment condition conflicts, we retain the larger precinct's treatment status. We were not aware of these merges when the experiment was designed.

[19] For treated precincts, enumerators were instructed to survey the localities and city blocks where our informational treatment was delivered. In control precincts, respondents were chosen according to the same protocol used to determine the delivery of leaflets in treated rural and urban blocks.

[20] We did not ask about Movimiento Ciudadano, which was the incumbent party only in Apaseo el Alto. Consequently, the 24 precincts from this municipality are dropped from analyses examining prior beliefs.

[21] We did not elicit perceptions of the exact share of funds that respondents believe each party spends in a malfeasant way, as we believed this would be hard for respondents to understand.

ensures that we only compare precincts that chose between the same candidates. Throughout, standard errors are clustered at the municipality-treatment level.

We use equation (3) to validate the randomization. Appendix Table A4 demonstrates that the treatment is well-balanced across 46 precinct and survey respondent-level covariates. As usual, there are some significant differences, most notably with respect to incumbent vote share in the previous elections in 2012. However, Appendix Table A26 shows that our estimates are robust—and, if anything, more precisely estimated—when we adjust for the 40 precinct-level pre-treatment variables.

4.6 Heterogeneous effects

To test our core hypotheses examining how the effects of providing malfeasance information vary with voters' prior beliefs, the level of malfeasance reported, and the extent of voter updating, we further estimate interactive specifications of the form:

$$Y_{pbm} = \alpha_{bm} + \beta Treatment_{pbm} + \gamma\big(Treatment_{pbm} \times X_m\big) + \varepsilon_{pbm}, \tag{4}$$

where X_m is a municipality-level variable capturing the heterogeneous effects enumerated in hypotheses H1-H3. Since X_m is not randomly assigned, we also show the robustness of these specifications to interacting our treatment with potential confounders of X_m.

Measuring the prior beliefs and voter updating required to test parts of H1 and H2 is challenging in our context. Since we could not conduct a baseline survey due to financial constraints, we use the post-election responses from each municipality's surveyed control precincts to proxy for the average pre-treatment beliefs of the treated and control voters within the same municipality. Specifically, to measure the level of voters' prior beliefs—a proxy for the parameter μ_I in the model—we use the mean belief about the incumbent party's malfeasance reported in a municipality's control precincts. For the precision of such prior beliefs (λ_I), we similarly use the mean precision of the incumbent malfeasance perceptions reported in a municipality's control group.

To proxy for the overall extent to which voters in a given municipality updated their posterior beliefs about incumbent party malfeasance in response to treatment, we measure the average change in the control group's beliefs after being exposed to the treatment information during the post-election survey. Specifically, we showed all voters the leaflet corresponding to their municipality at the end of the survey and asked them again how they perceived the incumbent party on the same five-point scale. The average change within each municipality approximates $\mathbb{E}[\theta_I|s_I,\mu_I] - \mathbb{E}[\theta_I|\mu_I]$ in our model, where positive (negative) values imply that voters updated

unfavorably (favorably) relative to their prior beliefs. Given that control group respondents had less time to internalize the information than those in treated precincts, we focus on the *slope* with respect to updating, rather than relying on the levels of updating to categorize favorable and unfavorable updating.

Using post-election surveys from the control group to proxy for pre-treatment beliefs and belief updating by treated voters in a municipality relies on two assumptions: (i) that control group respondents are similar to treatment group respondents; and (ii) that control group respondent beliefs are persistent and not subject to spillovers between the intervention and the post-election survey. Appendix section A.4.3 provides extensive support for these assumptions. In short, our randomization and the lack of selection into the endline sample support assumption (i), while our blocking strategy ensures that treated and control respondents within municipalities are similar in practice. In support of assumption (ii), we show that municipal-level electoral outcomes do not influence control group beliefs, that there is no evidence of cross-precinct spillovers, that control group respondents update more than treated respondents upon being shown the leaflet, and that a validation exercise we conducted using a panel survey in Brazil suggests limited changes in politician assessments just before and after elections among control voters. Provided these assumptions hold, we further prove in Appendix section A.4.3 that our estimates represent a lower bound on the magnitude of the precinct- and individual-level heterogeneous effects, to the extent that aggregating to the municipal level adds classical measurement error to precinct- and individual-specific prior beliefs.

5 How do voters interpret the information treatment?

Before examining the precinct-level electoral results, we start by examining how the information treatment affected voters' actions and posterior beliefs using our post-election survey.

5.1 Manipulation checks

The four self-reported outcomes in Table 2 show that treated voters received and engaged with the information distributed. Column (1) finds that treated voters were 25 percentage points more likely to remember receiving our leaflet, relative to a control mean of 9% of voters.[22] Column (2) next shows that voters in treated precincts were 17 percentage points more likely to report having read the leaflet, while column (3) demonstrates that treated voters were 14 percentage points more

[22]The non-zero control mean likely reflects respondents mistaking our leaflet for another leaflet. Appendix Tables A6 and A7 find no evidence to suggest that this is explained by cross-precinct spillovers.

Table 2: Effect of information treatment on self-reported engagement with leaflet

	Remember leaflet	Remember reading leaflet	Correctly remember content	Leaflet influenced vote
	(1)	(2)	(3)	(4)
Information treatment	0.247***	0.171***	0.138***	0.051***
	(0.022)	(0.018)	(0.019)	(0.010)
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}
Control outcome mean	0.09	0.05	0.06	0.02
Control outcome std. dev.	0.28	0.22	0.25	0.14
R^2	0.11	0.09	0.10	0.06
Observations	4,958	4,958	4,958	4,958

Notes: All specifications include block fixed effects, and are estimated using OLS. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

likely to correctly answer a multiple choice question asking what issue was covered in the leaflet. Finally, column (4) indicates that 7% of treated voters reported that the leaflet influenced their vote choice, which is 5 percentage points higher than for voters located in control precincts.

Voters generally did not believe that the leaflet was politically motivated. Among treated precincts, 44% of voters correctly believed that the leaflet came from a non-partisan NGO. This response was more than twice as likely as any particular political party, while 33% did not know. The difference was even greater among those who remembered the leaflet. Moreover, neither the comparative nor public treatment variants—which could have been perceived as more political—differentially affected the perception that the treatment emanated from a government or political source. Finally, as Appendix Tables A10 and A11 show, such perceptions about the leaflet are not correlated with municipal-level prior beliefs, the precision of those beliefs, or belief updating.

5.2 The effect of information on voters' posterior beliefs

The distribution of prior beliefs about the municipal incumbent party's malfeasance in the control group indicates that voters had relatively low expectations of incumbent parties. Figure 5 shows that around 60% of respondents reported that they believe the incumbent party engaged in medium to very high levels of corruption or misallocated spending. These expectations of non-trivial incumbent malfeasance in office are consistent with the prior beliefs of Mexican voters reported in Chong et al. (2015). Nevertheless, a significant fraction of voters also perceived that their incum-

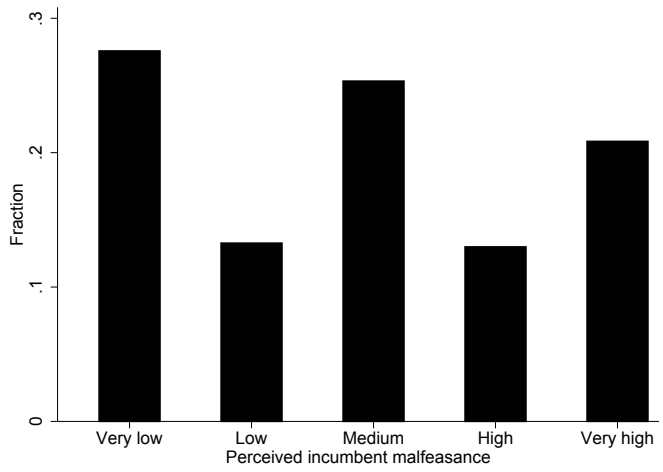


Figure 5: Perceived incumbent party malfeasance in control precincts

bent party engaged in limited malfeasant behavior. Interestingly, however, voters' prior beliefs are not significantly correlated with the malfeasance levels documented in the ASF reports.

Given voters' low expectations, it is important to assess whether voters favorably or unfavorably update their posterior beliefs about the incumbent party's malfeasance to understand how the information treatment will affect incumbent party support on average. Taking such posterior beliefs as our outcome, Table 3 presents estimates from equations (3) and (4). The negligible and far from statistically significant, coefficient in column (1) shows that treated voters did not increase their posterior beliefs about their incumbent party's malfeasance upon learning of relatively high levels of malfeasance, on average. As in Banerjee et al. (2011), this finding suggests that the information provided broadly aligned with what the *average* voter already believed.

However, the lack of updating among treated voters *on average* masks substantial heterogeneity in responses across voters that possessed different prior beliefs. Consistent with hypothesis H1, column (2) indicates that treated voters in municipalities that had unfavorable prior beliefs (i.e. pre-existing expectations of high levels of malfeasance) about the incumbent favorably updated those beliefs about the incumbent, while treated voters in municipalities that had favorable prior beliefs (i.e. expectations of low malfeasance) were more likely to report perceiving their incumbent as corrupt or neglectful of the poor.[23] For the average leaflet, the difference in responses to the treatment between the municipalities with the most favorable and most unfavorable prior beliefs is

[23] Appendix Table A30 shows that this finding is robust to splitting the sample between municipalities with above- and below-median prior beliefs. This check addresses the concern that the results in column (2) could arise mechanically because the municipal-level incumbent malfeasance priors regressor is an aggregation of the individual level posterior belief outcomes in the control group.

Table 3: Effect of information treatment on voters' posterior beliefs about incumbent party malfeasance

	Perceived incumbent party malfeasance (very low to very high)					
	(1)	(2)	(3)	(4)	(5)	(6)
Information treatment	-0.001	-0.015	0.427	0.016	0.848*	-0.096**
	(0.040)	(0.037)	(0.476)	(0.067)	(0.452)	(0.047)
× Incumbent malfeasance prior		-0.126***			-0.151***	
		(0.035)			(0.033)	
× Incumbent prior precision			-0.132		-0.258*	
			(0.149)		(0.139)	
× Incumbent malfeasant spending				-0.083	-0.137	
				(0.214)	(0.165)	
× Unfavorable incumbent updating						0.102***
						(0.030)
Outcome range	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}
Control outcome mean	-0.14	-0.14	-0.14	-0.14	-0.14	-0.14
Control outcome std. dev.	1.48	1.48	1.48	1.48	1.48	1.48
Interaction range		[-1.4,1.1]	[2.4,3.8]	[0,0.58]		[-0.6,2.7]
Interaction mean		-0.09	3.23	0.21		0.91
Interaction std. dev.		0.82	0.26	0.17		1.00
R^2	0.29	0.29	0.29	0.29	0.30	0.29
Observations	4,624	4,624	4,624	4,624	4,624	4,624

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

almost one third of a standard deviation in the posterior belief. Moreover, column (3) shows that treated voters within municipalities with relatively weak prior beliefs are not significantly more likely to unfavorably update their posterior beliefs about their incumbent party. Given the lack of an effect on the average posterior belief, this null finding is also consistent with the model's prediction that the magnitude of the average effect only significantly varies with the precision of voters' prior beliefs when the magnitude of the average effect is not zero. Column (5) reports similar estimates when each of the main interactions are included simultaneously.

The insignificant interaction in column (4) initially provides surprisingly little evidence that the share of misspent funds differentially influences the posterior beliefs of treated voters, as predicted by part (c) of hypothesis H1. However, the precinct-level electoral results described below strongly support this hypothesis. Moreover, posterior beliefs do change once we account for how the information provided relates to prior beliefs. The statistically significant positive coefficient on the interaction between the treatment indicator and our measure of voter updating in column (6) demonstrates that treated voters in municipalities where voters unfavorably (favorably) update their posterior beliefs about the incumbent display substantially more unfavorable (favorable) opinions

of the incumbent party. Substantively, a one-standard-deviation difference in updating translates to around a 0.1-standard-deviation change in posterior beliefs among treated voters.[24]

Our information treatment could, in theory, have also affected posterior beliefs about challengers (e.g. Kendall, Nannicini and Trebbi 2015). Appendix Tables A16 and A17 show that treated voters in municipalities with unfavorable prior beliefs about the challenger were also more likely to favorably update their posterior beliefs about the main challenger's malfeasance. Given that such effects are similar across the local and comparative variants of the treatment (Arias et al. 2018), this suggests that voters in our sample primarily updated their posterior beliefs about challengers from the information they received about the incumbent, and that voters believed incumbent and challenger types to be positively correlated.[25] To the extent that voters updated similarly about challengers, our estimates are likely to understate the effect of information only inducing voters to update about the incumbent party. However, justifying our focus on incumbent parties, Tables A18 and A19 show that voting behavior is driven primarily by how the treatment relates to voters' prior beliefs about the incumbent party rather than challenger parties. Appendix section A.6 discusses these results in greater detail.

Together, these results confirm that voters meaningfully updated their posterior beliefs about the incumbent party in response to our information treatment. Although reported malfeasance conformed with prior beliefs on average, voters nevertheless updated in a direction that depended on how the information received related to their prior beliefs. We next examine whether such belief updating translates into precinct-level vote choices.

6 Precinct-level election results

We now present our three main precinct-level findings. First, reflecting a combination of voters' low expectations, as well as uncertainty-reduction and potentially differentially effective incumbent responses, we first show that the information treatment increased the incumbent's vote share on average. Second, and consistent with our theoretical model, this effect is greatest where voters updated their posterior beliefs about the incumbent party most favorably based on the information received. Third, we find a non-monotonic effect of information on electoral turnout where revelations of intermediate levels of malfeasance reduced turnout, but extreme levels—low and

[24] This result is not mechanical because municipal-level measures of unfavorable updating are based only on responses from voters in control precincts upon receiving the leaflet.

[25] Using a 5-point scale of whether voters believed other candidates of the same party would behave similarly to the incumbent, ranging from not all probable (1) to extremely probable that they will behave similarly in office (5), we find that voters on average believed candidates of all parties to be very similar to the incumbent, though this perceived similarity was slightly higher for incumbent party candidates (3.12) than for for those of challenger parties (3.06).

particularly high malfeasance—increased turnout.

6.1 Average effects of information on incumbent vote share

We first document that information about incumbent malfeasance *increased* the incumbent party's vote share, on average within our sample. Column (1) of panel A in Table 4 reports that our intervention significantly increased the incumbent party's vote share, as a proportion of those that turned out, by an average of 2 percentage points. Column (1) of panel B similarly shows that this translates into a 0.8 percentage point increase in the incumbent party's vote share, as a proportion of all registered voters in the precinct. The latter estimate indicates that the information caused the incumbent party to gain more voters, rather than simply demobilized challenger supporters. Relative to the mean vote share in the control group, the information treatment increased the incumbent party's vote share by 5%, or around a sixth of a standard deviation.

Although voters' expectations were sufficiently low that malfeasance revelations did not shift their posterior beliefs on average, there are several reasons why the incumbent party might still benefit from information provision. Specifically, voter risk aversion or political campaign responses to information dissemination could account for the increased incumbent party vote share. We conduct several exploratory analyses, which were not prespecified, to assess these potential mechanisms.

Incorporating voter risk aversion into our stylized model generates another channel through which signals of incumbent malfeasance could influence voters.[26] In particular, such information could have increased incumbent party support by reducing posterior uncertainty about the party's type (see also Kendall, Nannicini and Trebbi 2015). We find evidence consistent with this risk-reduction interpretation in Table 5. While the average effect of providing information on posteriors' precision reported in column (1) is zero—likely due to a ceiling effect on high reported levels of precision (mean precision was 3.25 on the four-point scale in control precincts)—column (2) intuitively shows that the greatest increases in posterior precision indeed occurred among respondents in municipalities where voters possessed less precise prior beliefs. Furthermore, columns (3) and (4) shows that there was a significant increase in incumbent vote share among respondents in municipalities with below-median prior precision (i.e. below 3.25) and no detectable effect in municipalities where the precision of prior beliefs was greatest.[27] Together, this evidence suggests that, given their low expectations of politicians, voters did not substantially improve their perception of incumbent party malfeasance, on average, but nevertheless became more likely to vote for

[26] Risk aversion could be incorporated into the model by allowing the politician type component of a voter's utility function to be represented by $\mathbb{E}[-\exp(\theta_j)]$. Our pre-analysis plan noted this theoretical extension may prove relevant.

[27] As with column (2) of Table 3, this split sample approach addresses the concern that a combination of block fixed effects perfectly explain control group responses.

Table 4: Effect of information treatment on incumbent party vote share

	Incumbent party vote share					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Incumbent party vote share (share of turnout)						
Information treatment	0.020***	0.019***	0.146***	0.031***	0.137***	0.026***
	(0.004)	(0.004)	(0.044)	(0.006)	(0.036)	(0.004)
× Incumbent malfeasance prior		0.009*			0.005	
		(0.005)			(0.003)	
× Incumbent prior precision			-0.040***		-0.033***	
			(0.014)		(0.010)	
× Incumbent malfeasant spending				-0.052**	-0.051***	
				(0.023)	(0.016)	
× Unfavorable incumbent updating						-0.009**
						(0.004)
Outcome range	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]
Control outcome mean	0.38	0.39	0.39	0.38	0.39	0.39
Control outcome std. dev.	0.12	0.12	0.12	0.12	0.12	0.12
R^2	0.61	0.59	0.59	0.61	0.59	0.59
Panel B: Incumbent party vote share (share of registered voters)						
Information treatment	0.008***	0.008***	0.054**	0.014***	0.047**	0.012***
	(0.002)	(0.002)	(0.025)	(0.003)	(0.019)	(0.003)
× Incumbent malfeasance prior		0.005**			0.004*	
		(0.002)			(0.002)	
× Incumbent prior precision			-0.014*		-0.010*	
			(0.008)		(0.006)	
× Incumbent malfeasant spending				-0.029**	-0.028***	
				(0.013)	(0.010)	
× Unfavorable incumbent updating						-0.005***
						(0.002)
Outcome range	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]
Control outcome mean	0.19	0.20	0.20	0.19	0.20	0.20
Control outcome std. dev.	0.07	0.07	0.07	0.07	0.07	0.07
R^2	0.62	0.61	0.61	0.62	0.61	0.61
Interaction range		[-1.4,1.1]	[2.4,3.8]	[0,0.58]		[-0.6,2.7]
Interaction mean		-0.10	3.23	0.21		0.91
Interaction std. dev.		0.83	0.26	0.17		1.00
Observations	675	651	651	675	651	651

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in Columns (2), (3), and (5) reflect the lack of data on prior beliefs about the incumbent party in Apaseo el Alto. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table 5: Effect of information treatment on the precision of voters' posterior beliefs about incumbent party malfeasance

	Precision of perceived incumbent party malfeasance (very low - very high)			
	(1)	(2)	(3) Above-mean incumbent prior precision	(4) Below-mean incumbent prior precision
Information treatment	0.016	0.675**	-0.020	0.050*
	(0.024)	(0.265)	(0.041)	(0.026)
× Incumbent prior precision		-0.204**		
		(0.084)		
Outcome range	{1,2,3,4}	{1,2,3,4}	{1,2,3,4}	{1,2,3,4}
Control outcome mean	3.25	3.25	3.51	2.94
Control outcome std. dev.	0.85	0.85	0.72	0.88
Interaction range		[2.4,3.8]		
Interaction mean		3.23		
Interaction std. dev.		0.26		
R^2	0.11	0.11	0.06	0.06
Observations	4,673	4,673	2,429	2,244

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

an incumbent party that represented a less risky option.

Another possible explanation is that voting behavior reflected general equilibrium considerations, including incumbent and challenger parties reactions to the information's provision. Previous studies have found that information provision reduced vote buying in India (Banerjee et al. 2011), although the opposite occurred in the Philippines (Cruz, Keefer and Labonne 2021). Bowles and Larreguy (2020) and Bidwell, Casey and Glennerster (2020) also suggest that candidates adjusted their on-the-ground campaigning after debates in Liberia and Sierra Leone. While incorporating such reactions in our overall point estimates may capture the primary parameter of policy interest, it remains important to understand whether the mechanism reflects belief updating in responses to the information's content or campaign responses to the information disseminated.

We examine this systematically by asking voters whether incumbents and challengers referred to the information reported in our leaflets in any of the following (non-exclusive) ways: (i) cam-

paign activities; (ii) partisan leaflets; (iii) visits from local political actors; (iv) advertisements; or (v) through the media. Around 17% of voters reported experiencing at least one type of incumbent response, and 16% reported at least one type of challenger response. According to our respondents, incumbents most frequently claimed that all parties were equally bad, while opposition parties were more likely to emphasize the content of the leaflets. Our outcome of interest is the total number of politician responses reported by the respondent, ranging from 1 to 5.

Column (1) of Table 6 shows that incumbents, and especially challengers, engaged in more campaign activities in treated precincts.[28] The increase is only statistically significant among challengers, and the effect magnitude is relatively small in each case. For politician responses to explain the positive average effect, the incumbent's resource advantage would need to make their responses substantially more effective (e.g. Cruz, Keefer and Labonne 2021) or challenger reactions would need to have backfired. Thus, while campaigns did respond somewhat to information dissemination, it is unlikely that these responses drove average voter behavior. Moreover, our evidence of voter learning—to which we soon turn—suggests that political responses cannot account for heterogeneity in treatment effects by prior beliefs, updating and incumbent-malfeasance levels.

It is also unlikely that the positive average effect reflects other potential explanations. One possibility is that voters (wrongly) credited the incumbent party for attracting FISM resources to their municipality. However, we find little support for this interpretation in Appendix Table A20, which shows no heterogeneous effects by the quantity of FISM funds received by the municipality, in either absolute or per voter terms. Another possibility is that the intervention may have been perceived as a smear campaign against the incumbent party. However, as shown above, voters nevertheless updated their beliefs and generally thought the information came from a non-partisan source. Finally, the treatment could have altered the weight that voters attached to different issues when deciding how to cast their ballot. However, Appendix Table A21 finds no evidence to suggest that a candidate's honesty or likelihood of addressing poverty became more important to treated voters.

6.2 Heterogeneous effects of information on incumbent vote share

Although treated precincts somewhat surprisingly rewarded incumbent parties *on average*, we next demonstrate that—as theorized (and prespecified)—voting behavior on the margin varies with the information content received and in line with the changes in posterior beliefs documented in our survey data.

[28]The non-zero number of activities in the control group likely reflects recall failures.

Table 6: Effect of information treatment on political party responses

	Total party activities					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Incumbent reactions						
Information treatment	0.032	0.034	0.681*	-0.131*	0.439	-0.001
	(0.043)	(0.043)	(0.348)	(0.077)	(0.296)	(0.069)
× Incumbent malfeasance prior		0.024			0.018	
		(0.038)			(0.032)	
× Incumbent prior precision			-0.204*		-0.177*	
			(0.111)		(0.096)	
× Incumbent malfeasant spending				0.766***	0.755***	
				(0.258)	(0.230)	
× Unfavorable incumbent updating						0.036
						(0.040)
Control outcome mean	0.43	0.46	0.43	0.43	0.43	0.43
Control outcome std. dev.	1.18	1.17	1.18	1.18	1.18	1.18
R^2	0.12	0.12	0.12	0.12	0.12	0.12
Panel B: Challenger reactions						
Information treatment	0.102**	0.105***	0.609	-0.024	0.400	0.089
	(0.039)	(0.039)	(0.398)	(0.060)	(0.384)	(0.060)
× Incumbent malfeasance prior		0.033			0.029	
		(0.043)			(0.038)	
× Incumbent prior precision			-0.159		-0.132	
			(0.122)		(0.116)	
× Incumbent malfeasant spending				0.591***	0.588***	
				(0.204)	(0.187)	
× Unfavorable incumbent updating						0.014
						(0.036)
Control outcome mean	0.40	0.48	0.40	0.40	0.40	0.40
Control outcome std. dev.	1.17	1.24	1.17	1.17	1.17	1.17
R^2	0.12	0.12	0.12	0.12	0.12	0.12
Outcome range	{0,1,2,3,4,5}	{0,1,2,3,4,5}	{0,1,2,3,4,5}	{0,1,2,3,4,5}	{0,1,2,3,4,5}	{0,1,2,3,4,5}
Interaction range		[-1.4,1.1]	[2.0,3.8]	[0,0.58]	[-0.6,2.7]	[0,0.58]
Interaction mean		-0.09	3.18	0.21		0.90
Interaction std. dev.		0.80	0.35	0.17		0.97
Observations	4,958	4,958	4,958	4,808	4,958	4,958

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

First, supporting hypothesis H2, the information treatment's largest positive effects were detected where voters initially believed that their incumbent was more malfeasant. Across both panels in Table 4, column (2) shows that the treatment's positive effect on incumbent party vote share was significantly greater (smaller) in precincts within municipalities where the control group had more unfavorable (favorable) prior beliefs regarding the incumbent party's level of malfeasance. Our estimates indicate that moving from the municipality with the most favorable prior beliefs about the incumbent party (-1.4) to the municipality with the most unfavorable prior beliefs (1.1) increased the effect of providing information on the incumbent party's vote share from 0.6 to 2.9 percentage points and the effect on the incumbent's share of registered votes from 0.1 to 1.4 percentage points. Also consistent with H2 and the risk-reduction explanation for the positive average effect on the incumbent's vote share, column (3) reports a significantly smaller positive effect of the information in precincts where the municipality's control respondents had more precise prior beliefs.

Second, and further supporting H2, treated voters were more likely to vote for incumbents overseeing lower levels of malfeasance. The significant negative interaction in column (4) between the treatment and the share of malfeasant spending reported in the leaflet implies that a one-standard-deviation increase in the share of malfeasance reduced the positive effect of treatment on the incumbent party's vote share (as a share of turnout) by 0.9 percentage points. As illustrated in Figure 6, revealing any level of malfeasant spending below 35% significantly increased the incumbent's vote share. The effect of providing information is never meaningfully negative in our sample, where the highest level of reported malfeasance is 58%. However, between 2007 and 2015, 46 audited municipalities (3.4% of all audited municipalities) across the country registered malfeasance rates exceeding 60%. Electoral sanctions could occur in such settings where more extreme levels of malfeasance are reported. Column (5) demonstrates that these results are robust to simultaneously adjusting for interactions with voters' prior beliefs.

Third, and combining the preceding heterogeneous effects, the effect of revealing incumbent malfeasance information decreases with the overall extent to which voters unfavorably updated their beliefs about the incumbent party's malfeasance. Column (6) of both panels reports a significant negative interaction between the treatment and our measure of unfavorable updating of posterior beliefs in each municipality's control precincts. A one-standard-deviation increase in unfavorable updating induced by the information reduced the incumbent party's vote share (as a share of turnout) by 0.9 percentage points in treated precincts.

We observe broadly similar responses to revelations concerning spending that did not benefit the poor and revelations pertaining to unauthorized spending. Appendix Table A22 splits the sam-

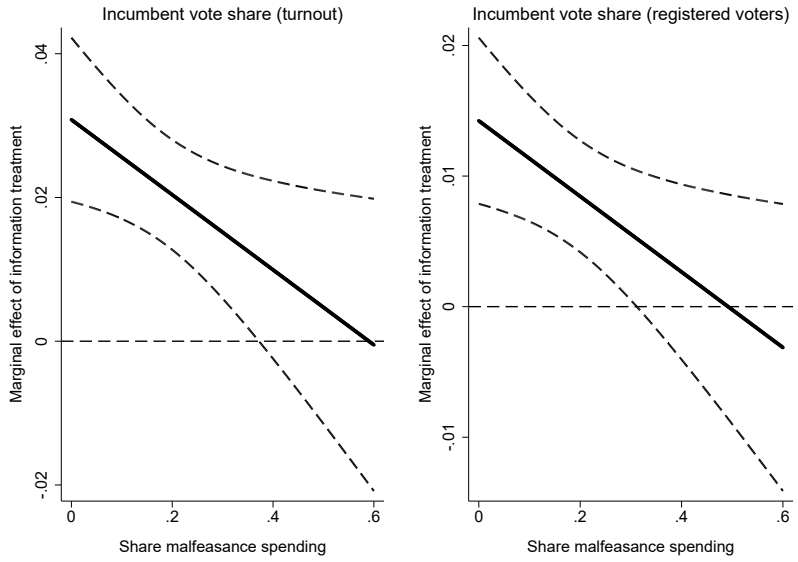


Figure 6: Marginal effect of information treatment on incumbent party vote share, by share of malfeasant spending (95% confidence intervals)

ple between municipalities that received information about not spending FISM funds on projects that benefited the poor and spending on unauthorized projects. There is a clear positive average effect of our treatment across both types of malfeasant spending, while the interactions with the share of malfeasant spending and the extent of unfavorable updating point in the same direction across subsamples.[29] Moreover, we show in Appendix Table A25 that the results in the full sample are driven by the specific dimension of malfeasance that was reported to voters, rather than treatment raising the salience of other dimensions of malfeasance more generally.

These findings fit closely with voting behavior reflecting updated beliefs driven by signals of incumbent malfeasance. However, it is also possible that vote choices could instead reflect voter reactions to differences in party campaign strategies across different types of treated and control precincts. Indeed, the modest increases in political activity documented in column (1) of Table 6 mask significant heterogeneity in response to the distribution of Borde Político's leaflets. The large and significant positive interactions in column (4) demonstrate that, for both incumbents and challengers, party activity increased substantially in municipalities in which high levels of malfea-

[29]The lack of heterogeneity in electoral response by precinct socioeconomic development also indicates that misallocating funds to projects that did not benefit the poor is no less salient where voters were less likely to directly benefit from FISM projects themselves. This suggests that voters, at least in our experimental sample, primarily worry about malfeasance in terms of incumbent integrity or competence, rather than its distributive implications.

sance were revealed. In a treated precinct within a municipality with 50% malfeasant spending, activity almost doubled relative to a municipality with 0%. However, these party responses cannot fully account for the heterogeneous effects of treatment attributed to voters learning from the leaflets. A comparison of column (4) with columns (2), (3), (5), and (6) in Table 6 shows that political responses are driven by the *level* of malfeasance reported, rather than voters' prior beliefs or the extent to which these were updated based on this information. This suggests that incumbent parties and their operatives may not know the extent to which voters expect their representatives to engage in minimal malfeasant spending while in office. The preceding evidence of increased party activity thus suggests that party responses could play a role in shaping how information dissemination impacts incumbent party support, but an important component is nevertheless explained by voters updating directly from the leaflets.

6.3 Robustness tests

Table 7 demonstrates that the incumbent party vote share results are robust to several alternative specifications. We focus on vote share as a share of turnout, but Appendix Table A28 reports similar results for incumbent party vote share using registered voters in the denominator.

First, we address the concern that aggregating prior beliefs and belief updating at the municipal level fails to capture meaningful variation in beliefs across precincts within municipalities. To the extent that using municipal-level aggregates of the prior beliefs introduces classical error in the precinct level analysis, such aggregation may lead to the underestimation of heterogeneous effects across precincts. We combat this issue by using our survey data from control precincts, which are uncontaminated by treatment, to impute precinct-level prior beliefs across the sample. The 14 covariates underpinning our prediction model are described in Appendix section A.9.2, and explain at least 50% of variation in precinct-level prior beliefs, prior belief precision, and belief updating upon viewing the leaflet. Panel A of Table 7 shows that these precinct-specific predicted beliefs yield similar results to our main estimates, suggesting that measurement error in the precinct-level regressions due to municipal-level aggregation of prior beliefs is not a major challenge for estimation. Appendix Table A27 shows that a similar individual-level prediction exercise also generates similar results.

Second, since only the provision of audit information was randomized, it is possible that our heterogeneous effects could be confounded by correlates of voters' prior beliefs and the level of municipal malfeasance. In particular, our estimates could be biased if voters' prior beliefs correlate with potential confounds relating to the extent of treatment dissemination, the ease with which our information treatment could be relayed through local networks, alternative sources of

Table 7: Robustness of information treatment on incumbent party vote share (share of turnout)

	Incumbent party vote share (share of turnout)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Predicted precinct-level prior beliefs and updating						
Information treatment	0.020***	0.022***	0.135***	0.031***	0.128***	0.033***
	(0.004)	(0.004)	(0.045)	(0.006)	(0.037)	(0.005)
× Incumbent malfeasance prior (predicted)		0.009*			0.006	
		(0.005)			(0.004)	
× Incumbent prior precision (predicted)			-0.036**		-0.030**	
			(0.014)		(0.011)	
× Incumbent malfeasant spending				-0.052**	-0.052***	
				(0.023)	(0.017)	
× Unfavorable incumbent updating (predicted)						-0.009**
						(0.004)
Panel B: Adjusting for (demeaned) precinct-level covariates interacted with information treatment						
Information treatment	0.018***	0.018***	0.105**	0.030***	0.131**	0.026***
	(0.003)	(0.003)	(0.052)	(0.006)	(0.052)	(0.004)
× Incumbent malfeasance prior		0.008**			0.007*	
		(0.004)			(0.004)	
× Incumbent prior precision			-0.027*		-0.031*	
			(0.016)		(0.016)	
× Incumbent malfeasant spending				-0.058**	-0.068***	
				(0.025)	(0.021)	
× Unfavorable incumbent updating						-0.010***
						(0.003)
Panel C: Adjusting for (demeaned) municipal-level covariates interacted with information treatment						
Information treatment	0.020***	0.019***	0.146***	0.040***	0.151***	0.031***
	(0.004)	(0.003)	(0.037)	(0.005)	(0.032)	(0.006)
× Incumbent malfeasance prior		0.011			0.003	
		(0.008)			(0.006)	
× Incumbent prior precision			-0.040***		-0.035***	
			(0.012)		(0.010)	
× Incumbent malfeasant spending				-0.093***	-0.093***	
				(0.020)	(0.016)	
× Unfavorable incumbent updating						-0.013**
						(0.006)
Panel D: Weighting observations by the (expected) share of the precinct that received a leaflet						
Information treatment	0.026***	0.024***	0.168***	0.042***	0.144***	0.034***
	(0.006)	(0.005)	(0.062)	(0.007)	(0.046)	(0.006)
× Incumbent malfeasance prior		0.012**			0.008*	
		(0.006)			(0.004)	
× Incumbent prior precision			-0.045**		-0.033**	
			(0.019)		(0.014)	
× Incumbent malfeasant spending				-0.072***	-0.071***	
				(0.027)	(0.018)	
× Unfavorable incumbent updating						-0.012**
						(0.004)

Notes: All specifications include block fixed effects, and are estimated using OLS. See text for interactive covariates included in panels B and C. Observations in panel D are weighted by the share of the precinct that was treated. Lower-order interaction terms are omitted. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

our information, and the level of political polarization. Similarly, the content of the ASF's report could be correlated with structural factors that affect voters' expectations of government service provision or the welfare consequences of malfeasant spending, and in turn shape their response to treatment.

To address these concerns, we adjust for the interaction between our information treatment and potential confounders. We start with the following (demeaned) precinct-level covariates: share of the precinct electorate that received a leaflet, distance to the municipality center, whether a precinct is rural, population density, number of radio and television stations covering the precinct that transmit from within the municipality, percentage of households with access to a television, percentage of households with access to the internet at home, and municipal winning margin in the previous election. An important caveat is that some of these variables could themselves determine voters' prior beliefs, and this could contaminate our estimates by partialing out part of the effect of prior beliefs. In addition, we examine robustness to including the following (demeaned) municipal-level covariates: the number of registered voters; population density; working age share of the population; average years of schooling; the share of households with televisions; the share of households with access to the internet; and municipal incumbent vote share and victory margin at the previous election.

The results in panels B and C demonstrate that our heterogeneous effects are generally robust, supporting our interpretation that the results reflect voter learning. Only in the case of the interaction with the precision of prior beliefs in panel B are the results somewhat sensitive to the inclusion of interactive controls. Further analyses suggest that this sensitivity primarily reflects interactively adjusting for our proxies for ruralness, which could be determinants of prior precision as well as information flows within voter networks.

Third, we show that the effects become somewhat larger when we account for heterogeneity in the share of voters that actually received leaflets. Specifically, we weight precinct-level observations by the share of voters to whom we delivered a leaflet. In control precincts, we compute this share based on the average number of leaflets delivered to treated precincts within the same block. This weighting scheme downweights large precincts in which only a small fraction of voters could receive the leaflet. Consistent with our results being driven by exposure to treatment, panel D reports larger point estimates across all specifications. Similarly, we show in Appendix Table A29 that the magnitude of the average and heterogeneous effects of information provision generally increase with the share of voters in a precinct that received a leaflet.

6.4 Non-monotonic effects of information on turnout

A distinctive feature of our theory is the non-monotonic relationship between the extent of malfeasance and turnout captured in H3. In particular, we predicted that revelations of either extremely low or high levels of malfeasance would induce significant masses of voters to strongly prefer a particular party and thereby reduce the number of largely indifferent voters that abstain. Signals of incumbent malfeasance that induce small shifts in the distribution of voter preferences could instead reduce turnout when a mode of voters become relatively indifferent between parties.

The heterogeneous effects reported in Table 8 find support for these predictions. Since we expected non-monotonic effects, it is not surprising to observe in columns (1) and (2) that the average ITT effect is close to zero and does not vary linearly with the share of malfeasant spending that was reported. Rather, we first focus on the interaction between treatment and a quadratic operationalization of reported malfeasance that allows us to detect the prespecified non-monotonic effect. At malfeasance levels close to 0%, the lower-order treatment term in column (3) shows that turnout increased by 0.4 percentage points. The negative linear and positive quadratic interactions with the share of malfeasant spending demonstrate that turnout decreased at interim levels of malfeasance—which conform more closely with voters' prior beliefs—but increased by more than a percentage point at high levels of malfeasance. In this specification, the positive effect at the lowest and highest levels of malfeasance in our sample is not statistically significant, although we observe significant increases of at least a percentage point at each extremity when observations are weighted by the share of the precinct that received a leaflet, as Appendix Table A31 shows. Column (4) reports similar results—with a statistically significant increase in turnout for high levels of malfeasance—when splitting the sample into quartiles by level of reported malfeasance, and thus demonstrates that the results are not an artifact of imposing a quadratic specification. Figure 7 depicts both the quadratic and non-parametric non-monotonic relationships graphically.

While the preceding results support our model's emphasis on the importance of voters' prior expectations, it is possible that malfeasance revelations could induce voters to disengage with politics. This is most plausible where high levels of malfeasance are reported (Chong et al. 2015). However, we find no evidence to suggest that information about an incumbent's malfeasance induced a general form of disengagement with the political system. As previously noted, column (2) of panel A in Table 8 shows that turnout does not linearly decrease with the level of malfeasant spending. Furthermore, we turn to our survey data to examine voter responses on a five-point scale rating their belief that elections help to select honest and competent politicians. Columns (4) and (6) of panel B show that neither high levels of malfeasance nor unfavorable updating significantly

Table 8: Effect of information treatment on turnout and confidence in the electoral process

Panel A: Turnout	Turnout					
	(1)	(2)	(3)	(4)		
Information treatment	-0.004	-0.005	0.004	0.002		
	(0.003)	(0.004)	(0.004)	(0.005)		
× Incumbent malfeasance spending		0.002	-0.126**			
		(0.012)	(0.059)			
× Incumbent malfeasance spending squared			0.251**			
			(0.111)			
× Incumbent malfeasance spending quartile 2				-0.000		
				(0.006)		
× Incumbent malfeasance spending quartile 3				-0.028***		
				(0.008)		
× Incumbent malfeasance spending quartile 4				0.003		
				(0.005)		
Outcome range	[0.21,0.79]	[0.21,0.79]	[0.21,0.79]	[0.21,0.79]		
Control outcome mean	0.50	0.50	0.50	0.50		
Control outcome std. dev.	0.10	0.10	0.10	0.10		
Interaction range		[0,0.58]	[0,0.58]			
Interaction mean		0.21	0.21			
Interaction std. dev.		0.17	0.17			
R^2	0.68	0.68	0.68	0.68		
Observations	675	675	675	675		
Panel B: Confidence in the system	Elections help to select competent candidates (did not help at all - helped a lot)					
	(1)	(2)	(3)	(4)	(5)	(6)
Information treatment	0.008	-0.000	0.389	0.052	0.712	-0.044
	(0.042)	(0.041)	(0.511)	(0.078)	(0.517)	(0.054)
× Incumbent malfeasance prior		-0.078			-0.100**	
		(0.049)			(0.048)	
× Incumbent prior precision			-0.118		-0.205	
			(0.158)		(0.163)	
× Incumbent malfeasant spending				-0.209	-0.247	
				(0.255)	(0.229)	
× Unfavorable incumbent updating						0.057
						(0.038)
Outcome range	{1,2,3,4,5}	{1,2,3,4,5}	{1,2,3,4,5}	{1,2,3,4,5}	{1,2,3,4,5}	{1,2,3,4,5}
Control outcome mean	2.86	2.86	2.84	2.86	2.86	2.86
Control outcome std. dev.	1.40	1.40	1.41	1.40	1.40	1.40
Interaction range		[-1.4,1.18]	[2.4,3.8]	[0,0.58]		[-0.6,2.7]
Interaction mean		-0.10	3.23	0.21		0.91
Interaction std. dev.		0.82	0.26	0.17		1.00
R^2	0.06	0.06	0.06	0.06	0.06	0.06
Observations	4,615	4,615	4,615	4,615	4,615	4,615

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

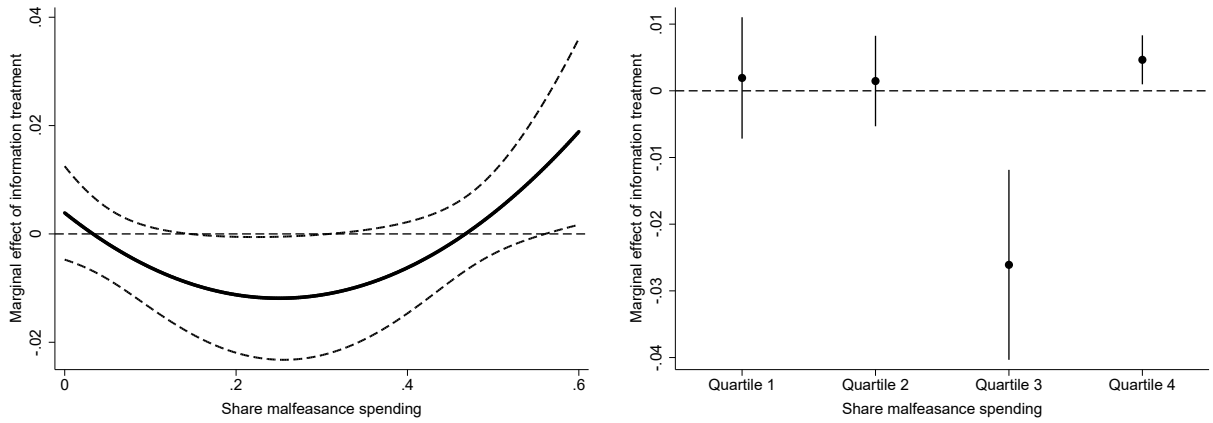


Figure 7: Marginal effect of information treatment on turnout, by share of malfeasant spending (95% confidence intervals)

altered treated voters' faith that elections can select good candidates.[30]

Another possibility is that the non-monotonic effects on turnout could reflect party mobilization strategies, rather than voter updating. However, the results in Table 6 indicate that party campaign responses were concentrated in high-malfeasance municipalities, and thus cannot explain how the treatment could induce high turnout in low-malfeasance municipalities. In sum, our findings suggest that malfeasance revelations which substantially deviate from the average voter's prior belief can increase turnout by inducing voters to shift not just towards indifference but to support other parties instead.

7 Conclusion

This article demonstrates the importance of voters' prior beliefs in understanding when incumbent malfeasance revelations affect electoral accountability. We find that Mexican voters—who, like voters in many developing contexts, have low expectations that their incumbents will correctly allocate resources—on average actually *reward* municipal incumbent parties revealed to have engaged in non-trivial levels of malfeasance in office. Although the increase in incumbent support on average likely reflects uncertainty-reduction among risk averse voters, we also document considerable support for our simple learning model. In particular, information provision was significantly more likely to increase incumbent support among voters who possessed unfavorable prior beliefs

[30]Unreported results including a quadratic interaction with incumbent malfeasant spending provide no evidence to suggest that confidence mirrors the non-monotonic relationship with turnout.

about incumbent party malfeasance, who learn of lower incumbent malfeasance, and who update their beliefs about the incumbent most favorably. Furthermore, and consistent with our theoretical model, the effect of information provision on turnout varies non-monotonically with the signal: surprising information increases turnout by shifting voters between parties, and relatively unsurprising information shifts voters towards indifference. By emphasizing voters' prior beliefs, and their relationship with the content of the information, these findings can help explain the mixed evidence that information induces electoral sanctioning or impacts political participation in developing democracies.

The implications of our findings for using information interventions to improve governance are mixed. A clear reason for optimism is that voters are able to understand signals of incumbent malfeasance and incorporate them into their voting behavior in an approximately Bayesian manner. Fixing voters' expectations of the parties, information thus helps voters to choose between candidates. However, the fact that some voters are so pessimistic that the misallocation of up to 40% of funds is considered good news is worrying for proponents of good governance. As the mixed evidence from previous studies suggests, such beliefs may not be uncommon in developing contexts—and may be consistent with incumbent behavior (e.g. Caselli and Morelli 2004).

In this light, our findings suggest a need to improve voters' expectations of their elected representatives, which could also induce politicians to perform better in office in the long run (Barro 1973; Ferejohn 1986), the need for better politicians to stand for office, or the need for more effective audits and legal sanctions. Civic education or a critical media may be required to help voters understand what good performance entails (e.g. Adida et al. 2017; Botero et al. 2015; Gottlieb 2016). Higher-quality candidates should also be encouraged to stand for office; some evidence suggests that increased wages can help (Caselli and Morelli 2004; Gagliarducci and Nannicini 2013). More effective audits and legal sanctions may also help improve politicians' performance by causing parties to believe that they will be electorally sanctioned for malfeasance in office (Avis, Ferraz and Finan 2018; Bobonis, Fuertes and Schwabe 2016; Olken 2007; Zamboni and Litschig 2018).

Finally, our study underscores the importance of investigating equilibrium political responses for understanding the impact of informational interventions. As with several other recent studies (Banerjee et al. 2011; Bowles and Larreguy 2020; Bidwell, Casey and Glennerster 2020; Cruz, Keefer and Labonne 2021), we document evidence that politicians respond to such interventions. Although the patterns of political responses in this study do not confound our capacity to isolate effects attributable to belief updating, such responses could be consequential in terms of partially explaining the average effects. Moreover, it is interesting to find that, while politicians do respond

to informational interventions in an attempt to counteract their electoral consequences, their responses do not address the sophisticated way in which voters process the information provided. To better understand when informational interventions are effective, political responses to non-partisan informational dissemination demand further attention.

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A Appendix (For Online Publication)

Contents

A.1 Proofs of results in the main paper

A.1.1 Proof of Proposition 1

Upon receiving a signal s_I, the share of voters that votes for each party is obtained by integrating over δ_i:

$$\bar{V}_I = 1 - F(\bar{\delta}_I), \tag{A1}$$
$$\bar{V}_C = F(\bar{\delta}_C), \tag{A2}$$

where:

$$\bar{\delta}_I := [\mu_I + \kappa_I(s_I - \mu_I)] - \mu_C + c, \tag{A3}$$
$$\bar{\delta}_C := [\mu_I + \kappa_I(s_I - \mu_I)] - \mu_C - c. \tag{A4}$$

Similarly, without receiving a signal,

$$\widehat{V}_I = 1 - F(\widehat{\delta}_I), \tag{A5}$$
$$\widehat{V}_C = F(\widehat{\delta}_C), \tag{A6}$$

where the vote shares are defined by the following cut points:

$$\widehat{\delta}_I := \mu_I - \mu_C + c, \tag{A7}$$
$$\widehat{\delta}_C := \mu_I - \mu_C - c. \tag{A8}$$

The differences in vote share between receiving and not receiving a signal are then given by $\bar{V}_I - \widehat{V}_I$ and $\bar{V}_C - \widehat{V}_C$.

For $s_I - \mu_I < (>)0$, $\bar{V}_I - \widehat{V}_I = F(\widehat{\delta}_I) - F(\bar{\delta}_I) > (<)0$ because F is increasing and the specified condition ensures that $\widehat{\delta}_I > (<)\bar{\delta}_I$. Differentiating this difference yields the following comparative statics:

$$\frac{\partial[\bar{V}_I - \widehat{V}_I]}{\partial s_I} = -F'(\bar{\delta}_I)\kappa_I < 0, \tag{A9}$$
$$\frac{\partial[\bar{V}_I - \widehat{V}_I]}{\partial \mu_I} = F'(\widehat{\delta}_I) - F'(\bar{\delta}_I)[1 - \kappa_I], \tag{A10}$$

which follow from $F' > 0$ (because $F'(\cdot)$ is a density function), and where a sufficient condition for

the second comparative static to be positive is that $(1-\kappa_I)$, which is positive because $\kappa_I \in (0,1)$, is sufficiently small. Finally, differentiating the magnitude of the vote share differential with respect to λ_I yields:

$$\frac{\partial[\bar{V}_I - \widehat{V}_I]}{\partial \lambda_I} = F'(\bar{\delta}_I)(s_I - \mu_I)\frac{\rho_I}{(\lambda_I + \rho_I)^2}, \tag{A11}$$

which follows the sign of $s_I - \mu_I$, and is decreasing (increasing) in λ_I when $\bar{V}_I > (<)\widehat{V}_I$. ■

A.1.2 Proof of Proposition 2

For turnout, the sign of $\overline{T} - \widehat{T} = F(\bar{\delta}_C) - F(\widehat{\delta}_C) - [F(\bar{\delta}_I) - F(\widehat{\delta}_I)]$ depends on F, where

$$\frac{\partial[\overline{T} - \widehat{T}]}{\partial s_I} = \kappa_I[F'(\bar{\delta}_C) - F'(\bar{\delta}_I)]. \tag{A12}$$

The direction thus depends on the densities at the cut points after receiving information: since $\kappa_I > 0$, s_I increases turnout when $F'(\bar{\delta}_C) - F'(\bar{\delta}_I) > 0$ and decreases turnout when $F'(\bar{\delta}_C) - F'(\bar{\delta}_I) < 0$. ■

A.1.3 Proof of Proposition 3

We start with the first, and easier, case where F is unimodal. If the distribution of voter prior beliefs minimizes (maximizes) $\hat{T}$, turnout obviously cannot decrease (increase) further. If this is not the case, there must exist some s for which $\overline{T} - \widehat{T} < 0$ and some $s' \neq s$ for which $\overline{T} - \widehat{T} > 0$. By unimodality of F and the fact that s_I is unbounded, $\frac{\partial[\overline{T} - \widehat{T}]}{\partial s_I} < 0$ for s_I sufficiently small and $\frac{\partial[\overline{T} - \widehat{T}]}{\partial s_I} > 0$ for s_I sufficiently large. Given this, that $\hat{T}$ neither maximizes nor minimizes turnout, and that $\lim_{s_I \to -\infty} \overline{T} = 1$ and $\lim_{s_I \to \infty} \overline{T} = 1$, the unimodality of F implies that there must exist an $s^* \leq \mu_I$ for which $\overline{T} - \widehat{T} > 0$ for all $s_I < s^*$ and an $s^{**} \geq \mu_I$ for which $\overline{T} - \widehat{T} > 0$ for all $s_I > s^{**}$. By the unimodality of F, $\overline{T} - \widehat{T} < 0$ for either $s_I \in [s^*, \mu_I]$ or $s_I \in [\mu_I, s^{**}]$.

For the case where F is bimodal with modes m_C and m_I, we first prove two preliminary results:

Lemma 1. *Assume that F is bimodal with modes m_C and m_I, where $m_C \leq \widehat{\delta}_C < \widehat{\delta}_I \leq m_I$. Then, $\frac{\partial[\overline{T} - \widehat{T}]}{\partial s_I} \leq 0$ for $s_I \leq \underline{s}$ and $\frac{\partial[\overline{T} - \widehat{T}]}{\partial s_I} \geq 0$ for $s_I \geq \bar{s}$, where $\underline{s} < \bar{s}$.*

Proof: First note that $\bar{\delta}_C$ and $\bar{\delta}_I$ are increasing in s_I. Given that $m_C \leq \widehat{\delta}_C < \widehat{\delta}_I \leq m_I$ and s_I has unbounded support, there must then exist an $s' > \mu_I$ such that $\bar{\delta}_C(s') = m_I < \bar{\delta}_I(s')$. Given that bimodality requires that $F'(m_I) > F'(\delta)$ for all $\delta > m_I$, $\frac{\partial[\overline{T} - \widehat{T}]}{\partial s_I} > 0$ evaluated at s'. Given continuity of F and that bimodality also implies $F''(\delta) < 0$ for all $\delta > m_I$, there must exist a smallest

$\bar{s} = s' - \bar{\varepsilon}$, where $\bar{\varepsilon} \geq 0$, such that $\frac{\partial[\overline{T}-\widehat{T}]}{\partial s_I} \geq 0$ at $\bar{s}$ and $\frac{\partial[\overline{T}-\widehat{T}]}{\partial s_I} > 0, \forall s > \bar{s}$. By analogous arguments, there exists an $s'' < s'$ such that $\bar{\delta}_I(s'') = m_C > \bar{\delta}_C(s')$, and thus a largest $\underline{s} = s'' + \underline{\varepsilon}$, where $\underline{\varepsilon} \geq 0$, such that $\frac{\partial[\overline{T}-\widehat{T}]}{\partial s_I} \leq 0$ at $\underline{s}$ and $\frac{\partial[\overline{T}-\widehat{T}]}{\partial s_I} < 0, \forall s < \underline{s}$. ■

Lemma 2. *Assume that F is bimodal with modes m_C and m_I, where $m_C \leq \widehat{\delta}_C < \widehat{\delta}_I \leq m_I$. Provided $F'(\widehat{\delta}_I) \neq F'(\widehat{\delta}_C)$, there exists some s_I for which $\overline{T} - \widehat{T} < 0$.*

Proof: Given the linearity of $\bar{\delta}_C$ and $\bar{\delta}_I$ in s_I, we can define the continuous functions $\varepsilon^+(s_I) = \overline{\delta}_C(s_I) - \widehat{\delta}_C = \overline{\delta}_I(s_I) - \widehat{\delta}_I > 0$ for $s_I > \mu_I$ and $\varepsilon^-(s_I) = \overline{\delta}_C(s_I) - \widehat{\delta_C} = \overline{\delta}_I(s_I) - \widehat{\delta}_I < 0$ for $s_I < \mu_I$. Then, given the continuity of F:

$$\overline{T}(\varepsilon^+(s_I)) - \widehat{T} = F(\widehat{\delta}_C + \varepsilon^+(s_I)) - F(\widehat{\delta}_C) - [F(\widehat{\delta}_I + \varepsilon^+(s_I)) - F(\widehat{\delta}_I)] \tag{A13}$$

$$\lim_{\varepsilon^+(s_I)\to 0} \frac{\overline{T}(\varepsilon^+(s_I)) - \widehat{T}}{\varepsilon^+(s_I)} = \lim_{\varepsilon^+(s_I)\to 0} \frac{F(\widehat{\delta}_C + \varepsilon^+(s_I)) - F(\widehat{\delta}_C) - [F(\widehat{\delta}_I + \varepsilon^+(s_I)) - F(\widehat{\delta}_I)]}{\varepsilon^+(s_I)} \tag{A14}$$

$$= F'(\widehat{\delta}_C) - F'(\widehat{\delta}_I), \tag{A15}$$

where the second line divides by $\varepsilon^+(s_I)$ and takes the limit to 0. Similarly, for $\varepsilon^-(s_I)$:

$$\lim_{\varepsilon^-(s_I)\to 0} \frac{\overline{T}(\varepsilon^-(s_I)) - \widehat{T}}{\varepsilon^-(s_I)} = F'(\widehat{\delta}_I) - F'(\widehat{\delta}_C) = -\lim_{\varepsilon^+(s_I)\to 0} \frac{\overline{T}(\varepsilon^+(s_I)) - \widehat{T}}{\varepsilon^+(s_I)}. \tag{A16}$$

Provided $F'(\widehat{\delta}_I) \neq F'(\widehat{\delta}_C)$, it must then be the case that either $\lim_{\varepsilon^+(s_I)\to 0} \frac{\overline{T}(\varepsilon^+(s_I)) - \widehat{T}}{\varepsilon^+(s_I)} < 0$ or $\lim_{\varepsilon^-(s_I)\to 0} \frac{\overline{T}(\varepsilon^-(s_I)) - \widehat{T}}{\varepsilon^-(s_I)} < 0$. ■

We can now prove the second case in the proposition. Lemma 2 establishes that $\overline{T} - \widehat{T} < 0$ some s_I. However, Lemma 1, the linearity of $\bar{\delta}_C$ and $\bar{\delta}_I$ in s_I, and the unbounded support of s_I ensure that there must exist an s^* sufficiently small that $\overline{T} - \widehat{T} > 0$ and an $s^{**} > s^*$ sufficiently large that $\overline{T} - \widehat{T} > 0$. ■

A.2 Bimodal distribution of voter partisan attachments in Mexico

Figure A1 provides evidence to suggest that the municipal partisan attachments of voters in Mexico are generally bimodally distributed. The figures were constructed using the Comparative Study of Electoral Systems 2009 survey in Mexico. We first constructed a 7-point ideological scale based on which parties voters sympathize with: if individuals only mentioned one party, we assigned them values -3 (for left parties: PRD, Labor Party (PT), Citizen's Movement (MC), and Social

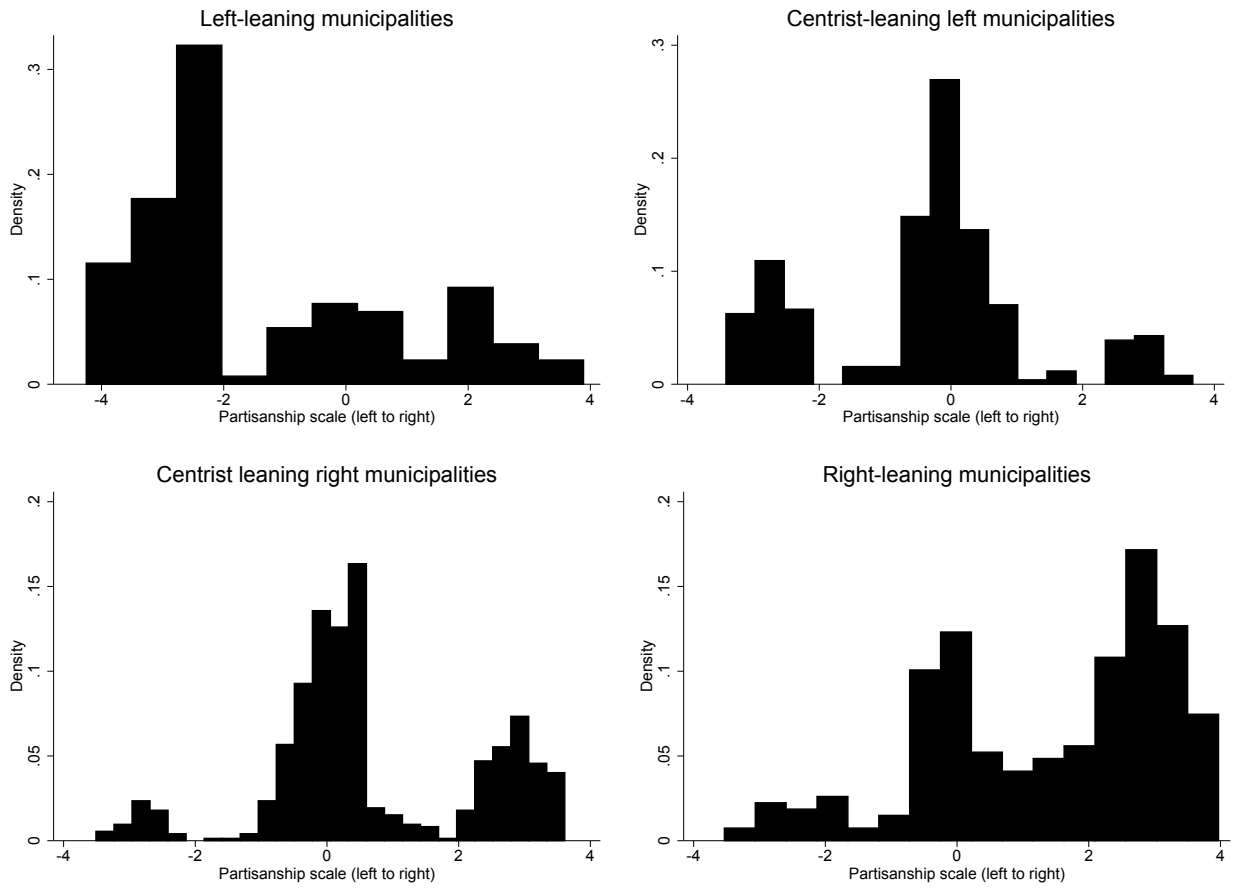

Figure A1: Distribution of voter partisanship, by type of municipality

Democratic Party (PSD)), 0 (for centrist parties: PRI, Ecological Green Party (PVEM), and New Alliance Party (PNA)), or 3 (for right parties: PAN) depending on the ideology of the chosen party. If an individual mentioned more than one party, they were asked about their second preferred party, and we coded the individual as the average of the two. We then demeaned individual responses using the municipality mean. Finally, the graphs are split according to "left-leaning municipalities" with modes between -3 and -2, "center left-leaning municipalities" with modes between -2 and 0, "center right-leaning municipalities" with modes between 0 and 2, and "right-leaning municipalities" with modes between 2 and 3. Each graph is centered around the mean ideology across municipalities within that graph.

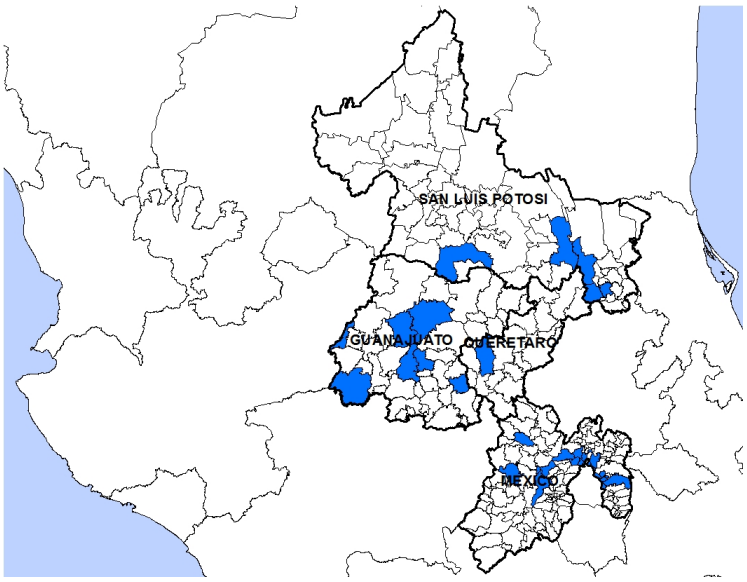

Figure A2: The 26 municipalities in our sample

A.3 Additional information about the intervention

A.3.1 Additional information about municipality selection

The 26 municipalities were selected from the 56 municipalities (within the four states under study) in which an audit was released in 2015. Our selection reflected three criteria. The first criteria relates to the safety of voters and our distribution and survey teams. This entailed eliminating 12 municipalities. Second, to ensure that there is variation in audit outcomes between incumbent and challenger parties, we only selected municipalities in which the ASF's audit revealed that at least one of the two measures of reported malfeasance (percentage of FISM funds not spent on the poor or spent on unauthorized projects) was at least two percentage points lower (or, more often, higher) than the state average of opposition parties. This excluded three of the remaining audited municipalities. Third, of 41 remaining municipalities, we selected municipalities to match the distribution of incumbent parties across audited municipal governments in our four states. Of our 26 municipalities, 17 were governed by the PRI (including 16 in coalition with the Teacher's (PNA) and Green (PVEM) parties), five by the PAN (including two in coalition with the PNA), two by the PRD, and one by the Citizen's Movement (MC). Figure A2 shows a map of the municipalities included in the study.

Figure A3: Example of a comparative information leaflet in Salamanca, Guanajuato

A.3.2 Example of comparative information leaflet

Figure A3 provides an example of the comparative leaflet from the municipality of Salamanca, Guanjuato. In addition to the content provided by the basic leaflet, this leaflet further provides audit information pertaining to the average municipal government that was both audited and governed by another party within the same state.

A.3.3 Experimental protocols

In treated precincts, leaflets were delivered to the largest localities (in rural precincts) and to compact sets of city blocks (in urban precincts). Within these areas, the starting point for leaflet distribution was randomly determined by the leaflet distribution teams. Distributors then followed a random walk to distribute leaflets to all available households until all of the 200 available leaflets were distributed. If possible, leaflets were given to the person that answered the door; when a door was opened by a child, the distribution team requested that they call the head of the household or

another adult. Where nobody answered the door, leaflets were taped to the door in rain-proof bags. The leaflets were distributed by a team hired by Data OPM (www.dataopm.net), who wore t-shirts emblazoned with Borde Político's logo and informed citizens that they contacted that they were delivering leaflets on Borde Político's behalf.

The post-election survey was administered to a sample of 5,000 individuals, with 1,000 surveys allocated to each treatment arm and 1,000 surveys in control precincts. We surveyed 10 households per precinct, which were randomly sampled using a constrained random walk protocol. In treated precincts, enumerators were instructed to survey the areas where treatments were delivered. In control precincts, respondents were chosen according to the same protocol used to determine the delivery of leaflets in treated rural and urban blocks. Within these constraints, the starting point was randomly determined, and enumerators were instructed to follow a random walk to contact one household for every few households they encountered. Our survey team approached voters at home to conduct interviews in their homes, proceeding with interviews only if the participant was a registered voter in the precinct. All potential survey participants were provided with information about the study as part of our informed consent protocol. The introduction and informed consent script is included in Figure A4. The survey was also conducted by enumerators from Data OPM, but in the name of Harvard and NYU, rather than Borde Político.

Figure A4 shows the six-page survey instrument, including the instructions followed by enumerators in the field. This version pertains to unauthorized spending; an analogous version of the survey was used in the municipalities that received information about spending that did not benefit the poor, with the exception of wording to reflect this difference for the questions that did not specifically ask about each dimension of municipal malfeasance. The scales shown to respondents as aides to answer some questions are available upon request.

Appendix A: Survey Instrument Type C

CUESTIONARIO TIPO C

Número de folio: Letra |__| *Entidad*|__|__| Municipio|__|__|__| Localidad|__|__|__|__| Folio|__|__|

Fecha (DD/MM/AAAA): |__|__|-|__|__|- |__|__|__|__| Estado:

Municipio: Localidad:

Sección: Manzana:

Nombre del encuestador: ID del encuestador: |__|__|__|

Inicio (HH:MM): |__|__|:|__|__| Termino (HH:MM): |__|__|:|__|__|

Instrucciones generales para el encuestador: Encuestador, seleccione al jefe de familia u otro responsable del hogar mayor de 18 años.

ANOTE CONDICIÓN DEL INFORMANTE:

1. Jefe de familia 2. Jefa de familia/ama de casa
3. Otro adulto que resida en el hogar

COMENTARIOS DEL ENCUESTADOR: _____

_____

ANOTE ANTES DE INICIAR:

¿En cuántos hogares intentó antes de realizar esta entrevista?: |__|__|

CONSENTIMIENTO INFORMADO

Hola mi nombre es (…), estoy realizando una encuesta para Qué Funciona para el Desarrollo, en colaboración con varias universidades. Esta es mi identificación. No representamos al gobierno ni a ningún partido político. Estamos preguntando a más de mil personas en todo el país sus opiniones sobre política y elecciones. Esta encuesta es anónima, confidencial y para usos académicos, por lo que nadie va a saber lo que respondió usted en específico. Puede elegir no responder ciertas preguntas. En general, la encuesta permitirá que se conozca mucho mejor lo que opinan y desean los mexicanos. La encuesta dura 20 minutos. También, si lo desea, le puedo dar un número de teléfono donde puede contactarnos si tiene alguna pregunta luego de la encuesta. ¿Me permite encuestarlo?

¿Tiene credencial para votar vigente en el lugar donde vive?

1. SI → CONTINUAR ENTREVISTA
2. NO → TERMINAR ENTREVISTA

1. VOTACIÓN

1a. El pasado 7 de junio hubo elecciones para Presidente Municipal y, como en cualquier elección, siempre hay personas que no tienen tiempo de ir a votar y otras a las que no les interesa. ¿Usted votó en las elecciones del 7 de junio?

1: Si 88: NS → **IR A PREGUNTA 3**
2: No → **IR A PREGUNTA 3** 99: NR → **IR A PREGUNTA 3**

1b. **(ENCUESTADOR: ENTREGUE "BOLETA A")** ¿Podría indicarme el partido por el cual votó para Presidente Municipal? Para su respuesta, use esta boleta que se parece a la de las elecciones, anote su respuesta y dóblela sin mostrármela y colóquela **aquí** en este sobre. **(ENCUESTADOR. REGISTRE LA RESPUESTA CUANDO TERMINE LA ENTREVISTA)**

1: PAN 8: MORENA
2: PRI 9: Encuentro Social
3: PRD 10: Partido Humanista
4: Partido Verde (PVEM) 11: Partido Futuro Democrático
5: Partido del Trabajo (PT) 12: Anuló/Tachó
6: Partido Nueva Alianza (PANAL) 13: Escribió algo
7: Movimiento Ciudadano 14: Dejo en blanco 99: NR

1c. ¿Qué tan seguro estaba usted sobre su elección? **(ENCUESTADOR: LEA OPCIONES DE 1 A 4)**

1: Muy seguro 4: Muy inseguro
2: Seguro 88: NS
3: Inseguro 99: NR

2. Si usted hubiera cambiado su voto por algún otro candidato, ¿por quién hubiera votado?

(ENCUESTADOR: NO LEA OPCIONES, REGISTRE LA RESPUESTA ESPONTÁNEA)

1: PAN
2: PRI
3: PRD
4: Partido Verde (PVEM)
5: Partido del Trabajo (PT)
6: Partido Nueva Alianza (PANAL)
7: Movimiento Ciudadano
8: MORENA
9: Encuentro Social
10: Partido Humanista
11: Partido Futuro Democrático
12: Independiente
12: Otro **(especificar)**: _____
13: No hubiera cambiado
88: NS 99: NR

TODOS IR A 5A

3. Si usted hubiera votado, ¿por cuál partido lo habría hecho?

(ENCUESTADOR: NO LEA OPCIONES, REGISTRE LA RESPUESTA ESPONTÁNEA)

1: PAN 8: MORENA
2: PRI 9: Encuentro Social
3: PRD 10; Partido Humanista
4: Partido Verde (PVEM) 11: Partido Futuro Democrático
5: Partido del Trabajo (PT) 12. Ninguno
6: Partido Nueva Alianza (PANAL) 13: Otro **(especificar)**: _____
7: Movimiento Ciudadano 88: NS 99: NR

4. **(ENCUESTADOR: PREGUNTAR <u>SOLO A QUIENES NO VOTARON</u>)**

¿Cuáles fueron las razones por las que usted no votó en las elecciones del 7 de junio? **(ENCUESTADOR: NO LEA LAS OPCIONES. ANOTE HASTA TRES MENCIONES EN EL ORDEN EL QUE FUERON MENCIONADAS. CUANDO TERMINE LA ENTREVISTA. CODIFIQUE LAS RESPUESTAS CON BASE EN LOS NUMEROS DE LA TABLA DE RAZONES.)**

ANOTE TEXTUAL Y CODIFIQUE EN LA TABLA:

1._____
2._____
3._____

Razón	
1. No renovó credencial de elector a tiempo	9. Todos los políticos son iguales
2. Problemas con la credencial o registro electoral	10. Todos los políticos son corruptos
3. Tuvo que trabajar	11. Estaba de viaje
4. Tuvo otros compromisos o actividades	12. Se gasta mucho dinero en las elecciones
5. No le dio tiempo	13. Por violencia o inseguridad
6. No supo dónde votar	14. Yo nunca voto
7. No le interesó votar	15. Otra razón **(especificar)** _____

Figure A4: The survey instrument used by enumerators

5a. Ahora le voy a preguntar acerca de la elección de Presidente Municipal que se celebró hace tres años en el 2012. ¿Usted votó en esas elecciones?

1: Si — 88: NS → **IR A PREGUNTA 7a**
2: No → **IR A PREGUNTA 7a** — **99:** NR → **IR A PREGUNTA 7a**

5b. **(ENCUESTADOR: ENTREGUE "BOLETA B")** ¿Podría indicarme el partido por el cual votó para Presidente Municipal en el 2012? Para su respuesta, use esta boleta que se parece a la de las elecciones, anote su respuesta y dóblela sin mostrármela y colóquela aquí en este sobre.**(ENCUESTADOR. REGISTRE LA RESPUESTA CUANDO TERMINE LA ENTREVISTA)**

1: PAN — 6: Partido Nueva Alianza (PANAL)
2: PRI — 7: Movimiento Ciudadano
3: PRD — 12: Otro **(especificar)**: _____
4: Partido Verde (PVEM) — 88: NS
5: Partido del Trabajo (PT) — 99: NR

5c. ¿Qué tan seguro estaba usted sobre su elección en el 2012? **(ENCUESTADOR: LEA OPCIONES DE 1 A 4)**

1: Muy seguro — 4: Muy inseguro
2: Seguro — 88: NS
3: Inseguro — 99: NR

6. Si usted hubiera cambiado su voto por otro partido, ¿por qué partido hubiera votado? **(ENCUESTADOR: NO LEA OPCIONES, REGISTRE LA RESPUESTA ESPONTÁNEA)**

1: PAN — 7: Movimiento Ciudadano
2: PRI — 8. Partido Conciencia Popular (PCP)
3: PRD — 9: Otro: _____
4: Partido Verde (PVEM) — 10. No hubiera cambiado
5: Partido del Trabajo (PT) — 88: NS
6: Partido Nueva Alianza (PANAL) — 99: NR

2. IDEOLOGÍA

7a. Generalmente, ¿usted con qué partido político simpatiza? **(ENCUESTADOR: NO LEA LAS OPCIONES. REGISTRE LA RESPUESTA ESPONTÁNEA.)**

1: PAN — 8: MORENA
2: PRI — 9: Encuentro Social
3: PRD — 10: Partido Humanista
4: Partido Verde (PVEM) — 11: Partido Futuro Democrático
5: Partido del Trabajo (PT) — 12: Otro **(especificar)**: _____
6: Partido Nueva Alianza (PANAL) — 13: Ninguno → **IR A 8a**
7: Movimiento Ciudadano — 99: NS/NR → **IR A 8a**

[MK] 7b. **MUESTRE TARJETA "A" AL ENCUESTADO (ENCUESTADOR: UTILICE LA RESPUESTA A LA PREGUNTA ANTERIOR COMO EL PARTIDO CORRESPONDIENTE EN LA SIGUIENTE PREGUNTA)**

En una escala de 1 a 7, donde 7 significa que siente mucha simpatía por el **[PARTIDO CORRESPONDIENTE]** y 1 significa que no siente mucha simpatía por el **[PARTIDO CORRESPONDIENTE]**, ¿qué grado de apego siente por el **[PARTIDO CORRESPONDIENTE]**?

1: No mucha simpatía — 5:
2: — 6:
3: — 7: Mucha simpatía
4: — 88: NS 99: NR

TODOS IR A P9

8a. **(ENCUESTADOR: PREGUNTAR SÓLO A LOS QUE CONTESTARON NINGUNO O NS/NR EN 7a)**

Pero, ¿siente un poco más de simpatía por algún partido que por los otros?

1: Si — 88: NS → **ir a pregunta 9**
2: No, por ninguno → **ir a pregunta 9** — 99: NR → **ir a pregunta 9**

8b. **(ENCUESTADOR: SI LA RESPUESTA ES "SI", ENTONCES PREGUNTAR:)** ¿Por cuál partido? **(ENCUESTADOR: NO LEA LAS OPCIONES. REGISTRE LA RESPUESTA ESPONTÁNEA.)**

1: PAN — 8: MORENA
2: PRI — 9: Encuentro Social
3: PRD — 10: Partido Humanista
4: Partido Verde (PVEM) — 11: Partido Futuro Democrático
5: Partido del Trabajo (PT) — 12: Otro **(especificar)**: _____
6: Partido Nueva Alianza (PANAL) — 88: NS
7: Movimiento Ciudadano — 99: NR

[ENCUESTADOR: HACER CADA UNA DE LAS SIGUIENTES TRES PREGUNTAS PARA LOS PARTIDOS PAN PRI Y PRD. UNA VEZ TERMINADA LA PREGUNTA PARA TODOS ESTOS PARTIDOS, PASAR A LA SIGUIENTE. SI LA RESPUESTA A LA PREGUNTA 7a o 8b ES DISTINTA A ALGUNO DE ESTOS PARTIDOS, ENTONCES TAMBIÉN APLIQUE LAS MISMAS PREGUNTAS PARA EL PARTIDO CORRESPONDIENTE] Ahora le voy a hacer una serie de preguntas sobre algunos partidos políticos.

PREGUNTE EN EL SIGUIENTE ORDEN: PRIMERO DE 9 A 11 PARA EL PRI, LUEGO PARA EL PAN, DESPUÉS DEL PRD Y AL FINAL EL PARTIDO CON EL QUE SIMPATICE EN 7a/8b

9. **(ENCUESTADOR: MOSTRAR TARJETA "BC")** En una escala del 1 a 5, donde 1 es muy baja y 5 es muy alta, ¿cuál es su percepción del nivel de **corrupción** de los **políticos locales** del **[PARTIDO CORRESPONDIENTE]?**: 1: Corrupción muy baja, 2: Corrupción baja, 3: Corrupción regular/intermedia, 4: Corrupción alta, 5: Corrupción muy alta, 88: NS, 99:NR

Ejemplos de corrupción incluyen el desvío de recursos públicos para usos indebidos como: usos personales, para beneficiar a amigos o familiares, o para fines electorales.

10. ¿Qué tan seguro está de su respuesta? **(ENCUESTADOR: LEER LAS OPCIONES)** 1: Muy seguro, 2: Seguro, 3: Inseguro, 4: Muy inseguro, 88: NS, 99: NR

11. En el último mes, ¿cómo cambio esta percepción sobre el nivel de corrupción de los políticos locales del **[PARTIDO CORRESPONDIENTE]**? **(ENCUESTADOR: LEER LAS OPCIONES)**1: Aumentó mucho2: Aumentó3: Permaneció igual, 4: Disminuyó, 5: Disminuyó mucho, 88: NS, 99: NR

	PRI	PAN	PRD	PARTIDO DE PREG 7a/8b
P9. Nivel de corrupción				
P10. Seguro de respuesta				
P11. Cambio percepción				

3. ASOCIACION DE CARACTERISTICAS DE CANDIDATOS

12. Si usted recibe información que el Presidente Municipal ha estado involucrado en actos de corrupción, ¿qué tan probable es que otro candidato a Presidente Municipal del **mismo partido** haga lo mismo? **(ENCUESTADOR: LEA LAS OPCIONES Y HACER ÉNFASIS EN QUE SE TRATA DEL PARTIDO QUE GOBIERNA AL MUNICIPIO)**

1: Nada probable — 4: Muy probable
2: Poco probable — 5: Extremadamente probable
3: Algo Probable — 88: NS 99: NR

13. Si usted recibe información que el Presidente Municipal ha estado involucrado en actos de corrupción, ¿qué tan probable es que candidatos a Presidente Municipal de los **otros partidos** hagan lo mismo? **(ENCUESTADOR: LEA LAS OPCIONES Y HACER ÉNFASIS EN QUE SE TRATA DE OTROS PARTIDOS)**

1: Nada probable — 4: Muy probable
2: Poco probable — 5: Extremadamente probable
3: Algo Probable — 88: NS 99: NR

4. TEMAS IMPORTANTES

[MK] 14. **(ENCUESTADOR: MUESTRE LA TARJETA "C".)** A la hora de decidir por qué candidato votar para Presidente Municipal… Siendo 1 nada importante y 5 muy importante, **¿qué nivel de importancia le asignaría a las siguientes características?**
(ENCUESTADOR: LEA TODAS LAS OPCIONES Y MARQUE EN EL RECUADRO EL NIVEL DE IMPORTANCIA QUE SEÑALÓ EL ENTREVISTADO)

1. Su partido político	1	2	3	4	5	8:NS	9:NR
2. Su honestidad	1	2	3	4	5	8:NS	9:NR
3. Su formación y experiencia en política	1	2	3	4	5	8:NS	9:NR
4. Sus propuestas respecto al crimen e inseguridad	1	2	3	4	5	8:NS	9:NR
5. Sus propuestas respecto a la lucha contra la pobreza	1	2	3	4	5	8:NS	9:NR
6. Sus propuestas respecto a la creación de empleos	1	2	3	4	5	8:NS	9:NR
7. Los recursos que traería a su colonia/comunidad	1	2	3	4	5	8:NS	9:NR

5. ADQUISICION DE INFORMACION

15a. En general, ¿está usted interesado en informarse sobre política?
1: Si
2: No → **ir a pregunta 16a**
9: NS/NR → **ir a pregunta 16a**

15b. En su opinión, ¿cuáles son las razones por las cuales es importante para usted informarse sobre política?
(ENCUESTADOR: LEER TODAS LAS OPCIONES, E INDICAR SÍ O NO SEGÚN LA RESPUESTA DEL ENTREVISTADO).

	Sí	No	NS/NR
1: Para poder elegir a mejores candidatos	1	2	9
2: Para poder hablar sobre política en el trabajo o con familiares y conocidos	1	2	9
3: Porque es un deber cívico	1	2	9
4. Porque me interesan los temas políticos	1	2	9
5: **(ENCUESTADOR: LEER SOLO SI DIJO NO DE 1 a 4)** No hay razón importante	1	2	9

6. VERIFICACION

16a. ¿Usted recuerda si usted o alguien de su hogar recibió un tríptico como este? **(ENCUESTADOR: MOSTRAR UN TRIPTICO COMO EJEMPLO)**
1: Si — 88: NS → **Ir a pregunta 18**
2: No → **Ir a pregunta 18** — 99: NR → **Ir a pregunta 18**

16b. ¿Cómo fue que usted o alguien de su hogar recibió este tríptico?
1: Se lo entregaron en mano — 4. Un vecino se lo hizo llegar
2: Lo recogió en la puerta de su casa — 5: Otro (especificar): _____
3: Alguien de su hogar me lo hizo llegar — 88: NS — 99:NR

17a. ¿Recuerda usted haber leído la información en este tríptico?
1: Si — 3: No recuerdo → **ir a pregunta 18**
2: No → **ir a pregunta 18** — 9: NS/NR → **ir a pregunta 18**

17b. ¿Qué fue lo que le hizo leer el tríptico? **(ENCUESTADOR: LEER TODAS LAS OPCIONES, Y SELECCIONAR TODAS LAS QUE APLIQUEN.)**

	Sí	No
1. Por curiosidad.	1	2
2: Para estar más informado.	1	2
3: Porque esperaba que otros también supieran o me hablaran de esto.	1	2
88: NS	1	2
99: NR	1	2

18. ¿Sabe usted o recuerda qué información contenía este tríptico?
(ENCUESTADOR: LEER TODAS LAS OPCIONES DEL 1 AL 4, Y SELECCIONAR TODAS LAS QUE APLIQUEN.
SI RESPONDIO "NO" o "NR" DE 1 A 4 → Pasar a 21)

	Si	No	NR
1: Gastos del partido del gobierno municipal en cosas que no debían gastar	1	2	9
2: Nivel de desempleo	1	2	9
3: Gastos del partido del gobierno municipal que no beneficiaron a los que menos tienen	1	2	9
4: Estadísticas sobre crimen e inseguridad	1	2	9

19. ¿Este tríptico contenía información solamente sobre el partido que gobierna su municipio o también sobre otros partidos en su estado?
(ENCUESTADOR: LEER LAS OPCIONES)
1: Sólo del partido que gobierna su municipio — 88: NS
2: También de otros partidos en su estado — 99: NR

20. ¿Cambió su decisión de voto como consecuencia de la información presentada en los trípticos?
1: Si — 2: No — 88: NS — 99: NR

21. ¿Quién cree usted que distribuyó los trípticos?
(ENCUESTADOR: LEER TODAS LAS OPCIONES, Y SELECCIONAR TODAS LAS QUE APLIQUEN.)

	Sí	No	NS/NR
1: Una asociación civil no-partidista	1	2	9
2: El gobierno Federal	1	2	9
3: El gobierno Estatal	1	2	9
4: El gobierno Municipal	1	2	9
5: El PRI	1	2	9
6: El PAN	1	2	9
7: El PRD	1	2	9
8: Otro (ESPECIFIQUE) _____	1	2	9

22. ¿Cuántas personas de su colonia/comunidad cree usted que recibieron un tríptico con información sobre cómo se utilizó el presupuesto municipal? **(ENCUESTADOR: LEER LAS OPCIONES)**
1: Muy pocas — 4: Más de la mitad
2: Menos de la mitad — 5: Casi todos
3: Aproximadamente la mitad — 88: NS — 99: NR

23a. ¿Usted o alguien de su hogar escuchó una grabación, o sabe de otros que hayan escuchado una grabación, sobre la distribución de información vía trípticos?
1: Si — 88: NS → **ir a pregunta 24a**
2: No → **ir a pregunta 24a** — 99: NR → **ir a pregunta 24a**

23b. ¿Cuántas personas de su colonia/comunidad cree usted que escucharon dicha grabación sobre la distribución de información vía trípticos? **(ENCUESTADOR: LEER LAS OPCIONES)**
1: Muy pocas — 4: Más de la mitad
2: Menos de la mitad — 5: Casi todos
3: Aproximadamente la mitad — 88: NS — 99: NR

7. RESPUESTAS DE LOS PARTIDOS

[MK] 24a. Durante el último mes, ¿el **partido que gobernaba su municipio** hizo referencia a la información presentada en los trípticos sobre gastos indebidos a través de…? **(ENCUESTADOR: LEA LAS OPCIONES Y MARQUE TODAS LOS QUE APLIQUEN)**

	Sí	No	NS/NR
1: Volantes	1	2	9
2: Actos de campaña	1	2	9
3: Visitas de miembros locales de partido	1	2	9
4: Anuncios en espectaculares	1	2	9
5: Medios de comunicación como radio, TV o prensa	1	2	9

(ENCUESTADOR:SI RESPONDIO "NO" o "NS/NR" DE 1 A 5 → Pasar a 25a)

24b. ¿Cuál fue el mensaje **del partido que gobernaba su municipio** sobre la información contenida en los trípticos? **(ENCUESTADOR: LEA LAS OPCIONES Y MARQUE ÚNICAMENTE UNA)**

1: Le restó importancia a la Información en los trípticos
2: Llamó la atención o destacó la información contenida en los trípticos
3: Presentó excusas para la información dada en los trípticos
4: Argumentó que todos los partidos son iguales
88: NS 99: NR

25a. Durante el último mes, ¿**partidos opositores** hicieron referencia a la información presentada en los trípticos sobre gastos indebidos a través de…? **(ENCUESTADOR: LEA LAS OPCIONES Y MARQUE TODAS LOS QUE APLIQUEN)**

	Sí	No	NS/ NR
1: Volantes	1	2	3
2: Actos de campaña	1	2	3
3: Visitas de miembros locales de partido	1	2	3
4: Anuncios en espectaculares	1	2	3
5: Medios de comunicación como radio, TV o prensa	1	2	3

(ENCUESTADOR: SI RESPONDIO "NO" DE 1 A 5 → Pasar a 26)

25b. ¿Cuál fue el mensaje de dichos partidos opositores sobre la información contenida en los trípticos? **(ENCUESTADOR: LEA LAS OPCIONES Y MARQUE ÚNICAMENTE UNA)**

1: Restaron importancia a la Información en los trípticos
2: Llamaron la atención o destacaron la información contenida en los trípticos
3: Presentaron excusas para la información dada en los trípticos
4: Argumentaron que todos los partidos son iguales
88: NS 99: NR

8. MECANISMOS DE TRANSMISIÓN SOCIAL

26. ¿Platicó con su familia o conocidos sobre los trípticos y la información contenida en ellos? **(ENCUESTADOR: LEER TODAS LAS OPCIONES, Y SELECCIONAR TODAS LAS QUE APLIQUEN.)**

1: Si, fue algo que yo le mencione a otros
2: Si, fue algo que otros hablaron conmigo
3: No → **ir a pregunta 29**
88: NS → **ir a pregunta 29** 99: NR → **ir a pregunta 29**

27. ¿Durante estas pláticas, se pusieron de acuerdo para votar todos por el mismo partido?

1: Si 2: No 88: NS 99: NR

28a. ¿Cambió su decisión de voto como consecuencia de estas pláticas?

1: Si 2: No → **ir a pregunta 29**
88: NS → **ir a pregunta 29** 99: NR → **ir a pregunta 29**

28b. ¿Podría decirnos por qué cambió su decisión de voto como consecuencia de estas pláticas? **(ENCUESTADOR: LEER TODAS LAS OPCIONES, Y SELECCIONAR TODAS LAS QUE APLIQUEN.)**

	Sí	No
1: Porque me hizo pensar con más cuidado mi decisión	1	2
2: Porque pensó que otros votantes en su localidad/colonia cambiarían su decisión de voto	1	2
3: Porque se puso de acuerdo con otras personas de su colonia/localidad para votar todos por el mismo partido	1	2
88: NS	1	2
99: NR	1	2

29. **(ENCUESTADOR: MOSTRAR TARJETA "D")** ¿Cuántas personas de su localidad/colonia **cree** que cambiaron su percepción sobre el grado de corrupción del **partido que gobernaba su municipio** antes de estas pasadas elecciones, debido a la información contenida en los trípticos? **(ENCUESTADOR: LEER TODAS LAS OPCIONES)**

1: Muy pocas 4: Más de la mitad
2: Menos de la mitad 5: Casi todos
3: Aproximadamente la mitad 88: NS 99: NR

30. **(ENCUESTADOR: MOSTRAR TARJETA "D") ENFATICE LA PARTE SUBRAYADA** ¿Cuántas personas de su localidad/colonia cree que cambiaron su percepción sobre el grado de corrupción de los **partidos opositores**, debido a la información contenida en los trípticos? **(ENCUESTADOR: LEER TODAS LAS OPCIONES**

1: Muy pocas 4: Más de la mitad
2: Menos de la mitad 5: Casi todos
3: Aproximadamente la mitad 88: NS 99: NR

31. (ENCUESTADOR: MOSTRAR TARJETA "D") ¿Cuántas personas de su localidad/colonia cree que cambiaron su decisión de voto debido a la información contenida en los trípticos? **(ENCUESTADOR: LEER TODAS LAS OPCIONES)**

1: Muy pocas 4: Más de la mitad
2: Menos de la mitad 5: Casi todos
3: Aproximadamente la mitad 88: NS 99: NR

9. CLIENTELISMO – LIST EXPERIMENT

32a. **(ENCUESTADOR: SI EL FOLIO DEL CUESTIONARIO TERMINA EN NÚMERO "PAR" PREGUNTAR:)**

Aplicar FOLIO **PAR**

[MOSTRAR Y LEER TARJETA "E.1"] Le voy a leer una lista de **tres** actividades que aparecen en esta tarjeta y quisiera que me diga cuántas de estas actividades ha hecho usted en las últimas semanas. Por favor, no me diga CUÁLES sino CUÁNTAS. Las tres actividades son…

a. Ver noticias en la televisión que mencionan algún candidato
b. Asistir a un acto de campaña
c. Hablar de política con otras personas

|____| actividades que ha hecho

32b. **(ENCUESTADOR: SI EL FOLIO DEL CUESTIONARIO TERMINA EN NÚMERO "IMPAR" PREGUNTAR:)**

Aplicar FOLIO IM**PAR**

[MOSTRAR Y LEER TARJETA "E.2"] Le voy a leer una lista de **cuatro** actividades que aparecen en esta tarjeta y quisiera que me diga cuántas de estas actividades ha hecho usted en las últimas semanas. Por favor, no me diga CUÁLES sino CUÁNTAS. Las cuatro actividades son…

a. Ver noticias en la televisión que mencionan algún candidato
b. Asistir a un acto de campaña
c. Recibir un regalo, favor o acceso a un servicio a cambio de su voto
d. Hablar de política con otras personas

|____| actividades que ha hecho

10. CONOCIMIENTO SOBRE POLÍTICA

33. Antes de la elección, ¿qué tan frecuentemente habló usted sobre política, por ejemplo, con su familia o conocidos? (**ENCUESTADOR: LEER LAS OPCIONES)**

1: No habló 4: Varias veces por semana
2: Habló alguna que otra vez 5: Diario
3: Una vez por semana 88: NS 99: NR

34. Por lo que sabe o ha oído ¿cuántos años dura un Presidente Municipal en su cargo? **(ENCUESTADOR: NO LEA OPCIONES, REGISTRE LA RESPUESTA ESPONTÁNEA) Respuestas correcta: 3 años**

1: 3 años 88: NS
2: Otro (especificar): _____ 99: NR

35. ¿Me podría decir el partido del Presidente Municipal que está terminando su periodo de gobierno en su municipio?
(ENCUESTADOR: ANOTE EL PARTIDO, Y AL FINALIZAR LA ENTREVISTA, VERIFIQUE SI SON CORRECTOS O INCORRECTOS USANDO LA TARJETA "F", Y MARQUE LA RESPUESTA CORRESPONDIENTE)

1: Respuesta correcta 88: NS
2: Respuesta incorrecta 99: NR _____

36. ¿Me podría decir el partido que ganó las elecciones para Presidente Municipal el pasado 7 de junio? **(ENCUESTADOR: ANOTE EL PARTIDO, Y AL FINALIZAR LA ENTREVISTA, VERIFIQUE SI SON CORRECTOS O INCORRECTOS USANDO LA TARJETA "F", Y MARQUE LA RESPUESTA CORRESPONDIENTE)**

1: Respuesta correcta 88: NS
2: Respuesta incorrecta 99: NR _____

37. Quitando el **[PARTIDO QUE GOBERNABA ANTES DE LA ELECCION]**, ¿qué **otro** partido cree usted que gobierna en la mayoría de los municipios de su estado? **(ENCUESTADOR: NO LEA LAS OPCIONES. REGISTRE LA RESPUESTA ESPONTÁNEA. LA RESPUESTA NO PUEDE SER IGUAL AL PARTIDO QUE GOBERNÓ EL MUNICIPIO DE 2012 A 2015)**
SI RESPONDE "NO SABE" INSISTIR ¿qué otro partido cree que gobierna?

1: PAN 8: MORENA
2: PRI 9: Encuentro Social
3: PRD 10; Partido Humanista
4: Partido Verde (PVEM) 11: Partido Futuro Democrático
5: Partido del Trabajo (PT) 12: Otro (especificar): _____
6: Partido Nueva Alianza (PANAL) 88: NS
7: Movimiento Ciudadano 99: NR

38. **MOSTRAR TARJETA "G". (ENCUESTADOR: LEER CADA MEDIO, Y LEER LAS OPCIONES DE RESPUESTA USANDO LA TARJETA. ANOTAR LA RESPUESTA QUE CORRESPONDA):** En el mes antes de las elecciones, siguió usted noticias de las campañas electorales por…

	Diario	Varias veces a la semana	Una vez a la semana	De vez en cuando	Nunca	NS / NR
1. Televisión	1	2	3	4	5	9
2. Radio	1	2	3	4	5	9
3. Periódicos	1	2	3	4	5	9
4. Internet o Redes Sociales	1	2	3	4	5	9

11. CONFIANZA EN INSTITUCIONES

39. **(ENCUESTADOR: MOSTRAR TARJETA "H" A LOS ENCUESTADOS)** Hay quienes creen que los procesos electorales permiten elegir candidatos honestos y competentes. Otros, creen lo contrario. Usando la siguiente escala, donde 1 significa que las elecciones **No ayudan nada** y 5 que las elecciones **Ayudan mucho**, ¿cuánto cree que las pasadas elecciones del 7 de junio ayudaron a elegir candidatos honestos y competentes?

1: No ayudaron nada 4:
2: 5: Ayudaron mucho
3: 88: NS 99: NR

[MK] 40. En su opinión, ¿qué tan probable es que gente con poder pueda enterarse de su voto, a pesar de que se supone que el voto es secreto? **(ENCUESTADOR: LEER LAS OPCIONES)**

1: Nada probable 4: Muy probable
2: Algo probable 5: Extremadamente probable
3: Probable 88: NS 99: NR

[MK] 41. Y, ¿qué tan probable es que el conteo de votos de la elección del 7 de junio haya sido limpio? **(ENCUESTADOR: LEER LAS OPCIONES)**

1: Nada probable 4: Muy probable
2: Algo probable 5: Extremadamente probable
3: Probable 88: NS 99: NR

42. En su colonia/comunidad, ¿qué tan frecuente es que representantes del partido del gobierno municipal ofrezcan regalos, favores o servicios a cambio de su voto? **(ENCUESTADOR: LEER OPCIONES)**

1: Nada frecuente 4: Muy frecuente
2: Poco frecuente 88: NS
3: Frecuente 99: NR

12. CLASIFICACIÓN DEL GOBIERNO CON BASE EN LA INFOMACIÓN DE LOS TRÍPTICOS

Para ir terminando, antes de las elecciones Borde Político, una asociación civil sin fines partidistas, estuvo distribuyendo trípticos entre algunos votantes de su municipio. Esto fue parte de un estudio conjunto entre Borde Político, la Universidad de Harvard y la Universidad de Nueva York para analizar el efecto de la información provista en los trípticos sobre el comportamiento de los votantes. En el tríptico se reportó información oficial de la Auditoría Superior de la Federación sobre la forma en la que el partido que gobierna su municipio gastó los recursos del Fondo de Infraestructura Social Municipal durante el año 2013. Estos recursos deben gastarse en obras de infraestructura. Los gastos que no sean en obras de infraestructura deben ser 0%.

43. **(ENCUESTADOR: MOSTRAR TARJETA "IC")** En 2013, el partido que gobernaba su municipio gastó el **(ENCUESTADOR: LEER Y MOSTRAR EL NÚMERO DEL TRÍPTICO PARA EL PROPIO MUNICIPIO)%** en cosas que no debe.
Con base en esta información, ¿cómo evaluaría usted a dicho partido en cuanto al grado de corrupción de sus políticos?
(ENCUESTADOR: LEER LAS OPCIONES Y REGISTRAR LA RESPUESTA)

1: Corrupción nula o muy baja 4: Alta
2: Baja 5: Muy alta
3: Regular/ intermedia 88: NS 99: NR

44. Respecto a lo que usted esperaba **antes** de que comenzara la campaña electoral, este porcentaje de dinero gastado por el partido que gobernaba su municipio en cosas que no debe fue… **(ENCUESTADOR: LEA LAS OPCIONES Y REGISTRE LA RESPUESTA)**

1: Mucho más alto 4: Más bajo
2: Más alto 5: Mucho más bajo
3: Igual 88: NS 99: NR

45. **(ENCUESTADOR: MOSTRAR TARJETA "IC")** En el 2013, partidos diferentes al de su municipio, que gobernaron en otros municipios en su estado, gastaron en promedio **(ENCUESTADOR: LEER Y MOSTRAR EL NÚMERO DEL TRÍPTICO PARA OTROS MUNICIPIOS)%** en cosas que no deben.
Con base en esta información, ¿cómo evaluaría usted a estos otros partidos en cuanto al grado de corrupción de sus políticos?
(ENCUESTADOR: LEER LAS OPCIONES Y REGISTRE LA RESPUESTA)

1: Corrupción nula o muy baja 4: Alta
2: Baja 5: Muy alta
3: Regular/ intermedia 88: NS 99: NR

46. Respecto a lo que usted esperaba **antes** de que comenzara la campaña electoral, este porcentaje de dinero gastado por estos otros partidos en cosas que no deben fue…
(ENCUESTADOR: LEA LAS OPCIONES Y REGISTRE LA RESPUESTA)

1: Mucho más alto 4: Más bajo
2: Más alto 5: Mucho más bajo
3: Igual 88: NS 99: NR

13. DEMOGRÁFICOS

47. Género (sexo) del entrevistado **(ANOTAR SIN PREGUNTAR)**:
1: Masculino 2: Femenino 88: NS 99: NR

48. [MK]: ¿En qué año nació? _____ 88: NS 99: NR

49. [MK] ¿Cuántos años de educación, o cuál grado máximo, completó usted? **(ENCUESTADOR: APÓYESE DE LA TARJETA "X" PARA EL CÁLCULO)**

Ninguno: 0					
Primaria Incompleta	1	2	3	4	5
Primaria completa	6				
Secundaria incompleta	7	8			
Secundaria completa	9				
Preparatoria o carrera técnica incompleta	10	11			
Preparatoria o carrera técnica completa	12				
Universidad incompleta	13	14	15		
Universidad completa y más	16 y más				
NS/NR	88	99			

50. ¿Cuál es su actividad principal? **(ENCUESTADOR: NO LEA LAS OPCIONES. REGISTRE LA RESPUESTA ESPONTÁNEA.)**

1: Trabajo
2: Tiene trabajo, pero no trabajó (por vacaciones, incapacidad o enfermedad)
3: Hogar
4: Estudiante
5: Trabaja y estudia
6: Jubilado o pensionado
7: Desempleado (no trabajó, pero buscó trabajo)
8: Está incapacitado permanentemente
9: Otro: _____
88: NS 99: NR

[MK] 51. En general, ¿cómo evalúa su nivel de vida en comparación con otros mexicanos? Diría usted que esta mucho peor, peor, igual, mejor, o mucho mejor

1: Mucho peor	4: Mejor
2: Peor	5: Mucho mejor
3: Igual	88: NS 99: NR

52. **(ENCUESTADOR: MOSTRAR TARJETA "J" A LOS ENCUESTADOS)**
Ahora, dígame, tomando en cuenta los siguientes rangos, ¿en cuál ubicaría el ingreso familiar mensual de su hogar?

1: 0-1 Sal. Mín (0 - 2,100)	12: 14-16 Sal. Mín (29,401 – 33,600)
2: 1-2 Sal. Mín (2,101 – 4,200)	13: 16-18 Sal. Mín (33,601 – 37,800)
3: 2-3 Sal. Mín (4,201 – 6,300)	14: 18-20 Sal. Mín (37,801 – 42,000)
4: 3-4 Sal. Mín (6,301 – 8,400)	15: 20-22 Sal. Mín (42,001 – 46,200)
5: 4-5 Sal. Mín (8,401 – 10,500)	16: 22-24 Sal. Mín (46,201 – 50,400)
6: 5-6 Sal. Mín (10,501 – 12,600)	17: 24-26 Sal. Mín (50,401 – 54,600)
7: 6-7 Sal. Mín (12,601 – 14,700)	18: 26-28 Sal. Mín (54,601 – 58,800)
8: 7-8 Sal. Mín (14,701 – 16,800)	19: 28-30 Sal. Mín (58,801 – 63,000)
9: 8-10 Sal. Mín (16,801 – 21,000)	20: 30+ Sal. Mín (63,001 - +)
10: 10-12 Sal. Mín (21,001 – 25,200)	88: NS
11: 12-14 Sal. Mín (25,201 – 29,400)	99: NR

53. Por favor indíquenos cuantos adultos (incluyéndose a usted) y cuantos menores de edad forman parte de su hogar.

|____|____| adultos (mayores de 18 años)

|____|____| menores (menores de 18 años)

88: NS 99: NR

54. Para finalizar, ¿podría darme un número telefónico para contactarlo en caso de que mi supervisor necesite revisar mi trabajo?

1: Sí fijo: _____
2 Sí celular: _____
3: NO
4: No tengo teléfono
88: NS 99: NR

55. **(ENCUESTADOR: AL DESPEDIRSE, PREGUNTAR INFORMALMENTE "CON QUIEN TUVE EL GUSTO" Y ANOTAR EL PRIMER NOMBRE)**

Table A1: Mapping pre-specified hypotheses to the results in this paper

PAP hypothesis	Location in paper	Notes
PAP-H1	Column 1 - Table 4	ATE - Vote Share
PAP-H3	Column 4 - Table 4	Interaction w/ malfeasant spending - Vote share
PAP-H7	Column 6 - Table 4	Interaction w/ unfavorable updating - Vote share
PAP-H8	Column 3 - Table 4	Interaction w/ prior precision - Vote share
PAP-H17	Figure 7 and Table 8	Non-monotonic effects on turnout
PAP-H19	Columns 1 and 4 - Table 3	ATE and Interactions - Posterior beliefs
PAP-H29	Table 2	Prob. of receiving information via leaflets
PAP-H41	Table 6 - Panel A	Politician responses - Incumbent
PAP-H42	Table 6 - Panel B	Politician responses - Challenger

A.3.4 Relationship to the pre-analysis plan

The experiment, together with a pre-analysis plan (PAP), were registered with EGAP before outcome data were collected; the registration is publicly available at www.egap.org/registration/760. Here, we explain how the final analyses relate to the PAP.

The PAP distinguished primary and secondary outcomes, as well as highlighting possible mediators and moderators. The main paper principally focuses on the primary precinct-level outcomes—incumbent party vote share and turnout. Hypothesis H2 in the main paper corresponds to hypotheses PAP-H1, PAP-H3, PAP-H7, and PAP-H8 in the PAP. Hypothesis H1 in the main paper, which is the analog of H2 for belief updating, corresponds to the mediation hypothesis PAP-H19 in the PAP. The non-monotonic effect on turnout in hypothesis H3 in the main paper corresponds to PAP-H17 in the PAP. Table A1 summarizes the mapping from the hypotheses in the PAP to the regression tables reported in the paper. The other primary hypotheses specified in the PAP mostly relate to the treatment variants, and the results are reported in our companion working paper (Arias et al. 2018).

In addition to belief updating, the other mediators that we consider in the main paper connect to the following secondary outcomes: our "first stage" examination of engagement with the treatment (hypotheses PAP-H29 in the PAP) and politician responses to the intervention (hypotheses PAP-H41 and PAP-H42 in the PAP). The other secondary hypotheses not covered in this paper relate to considering correlation of candidate types and vote buying (where the list experiment yields noisy estimates in line with the predictions in PAP-H33) as outcomes and considering heterogeneity in treatment effect by second-order moderators and treatment variants. The latter heterogeneous effects are reported in Arias et al. (2018) and Dunning et al. (2019). In short, this article covers

the primary pre-specified hypotheses of interest, but only reports results relating to secondary outcomes and mediators that are relevant for understanding the results of the primary hypotheses.

We follow the prespecified estimation strategy outlined at the beginning of section 6 of the PAP. The PAP specifically covered both the average and heterogeneous effect specifications. However, while the PAP specified a non-monotonic effect on turnout, it did not propose a specification for this. Accordingly, Table 8 consider two natural approaches: fitting a quadratic form and non-parametrically splitting the reported malfeasance distribution into quartiles.

A.4 Validation of the research design

A.4.1 Summary statistics

Table A2 compares our final sample of precincts to the national distribution according to a variety of 2010 Census characteristics. The statistics suggest that our sample of electoral precincts is relatively similar to the national average in terms of all characteristics, with the exception of being slightly less educated and having slightly lower internet access at home. Moreover, as the standard deviations indicate, the distribution is also broadly similar.

Table A3 provides summary statistics for the main variables that appear in our analysis, both at the precinct and individual levels.

A.4.2 Balance tests

Table A4 presents the results of our balance tests, at both the precinct and individual levels. The final eight variables are from our post-treatment survey.

A.4.3 Validation of measures of voters' prior beliefs

In this section, we report theoretical and empirical analyses to validate our municipal-level measures of prior beliefs and updating based on aggregating responses of subjects in the control group. First, under several assumptions that are plausible in this context, we present an econometric analysis to show that municipal-level aggregation of prior beliefs that vary across precincts within municipalities introduces classical error in the precinct level analysis. Second, we provide additional evidence in support of the key assumptions under which endline beliefs for control group respondents can be used as proxies of the prior beliefs of treated subjects.

To estimate heterogeneous effects by voter prior beliefs, we would ideally have estimated the

Table A2: Precinct-level comparison of Census 2010 characteristics between our sample and the nation

Variable	Observations	Mean	Std. dev.	Observations	Mean	Std. dev.
Population	678	1,633.18	997.00	66,740	1,683.20	1,878.04
Share working age	678	0.63	0.06	66,685	0.63	0.06
Average children per woman	678	2.49	0.58	66,740	2.50	0.62
Share indigenous speakers	678	0.05	0.15	66,682	0.06	0.19
Average years of schooling	678	7.98	2.39	66,740	8.27	2.47
Share economically active	678	0.38	0.07	66,685	0.39	0.07
Average occupants per room	678	1.16	0.28	66,740	1.11	0.35
Share of homes with water, drainage, and electricity	678	0.77	0.28	66,681	0.78	0.30
Shares of homes with a television	678	0.91	0.14	66,681	0.90	0.15
Share of homes with internet	678	0.16	0.19	66,681	0.19	0.20

Table A3: Summary statistics

Variable	Observations	Mean	Standard deviation	Minimum	Maximum
Panel A: precinct-level covariates					
Incumbent party vote share (share of turnout)	675	0.39	0.12	0.07	0.85
Incumbent party vote share (share of registered voters)	675	0.20	0.07	0.03	0.47
Turnout	675	0.50	0.10	0.21	0.79
Information treatment	675	0.59	0.49	0.00	1.00
Share that (would have) received a leaflet	675	0.77	0.41	0.08	5.36
Share that (would have) received been delivered a leaflet by hand	675	0.00	0.00	0.00	0.02
Incumbent malfeasance prior	651	-0.08	0.89	-1.60	1.80
Incumbent prior precision	651	3.24	0.37	2.40	4.00
Incumbent malfeasant spending	675	0.21	0.17	0.00	0.58
Unfavorable incumbent updating	651	0.89	1.08	-1.20	2.90
Rural	675	0.51	0.50	0.00	1.00
Area	675	10.80	19.98	0.02	212.62
Population	675	1,640.44	993.55	178.00	10,946.00
Population density	675	5,892.79	7,236.21	0.91	27,462.40
Distance from municipal centroid	675	8,060.54	6,672.66	185.79	53,502.60
Number of households	675	391.41	231.19	37.00	3,136.00
Number of private dwellings	675	473.09	344.15	45.00	5,203.00
Average occupants dwelling	675	4.14	0.49	2.61	5.83
Average occupants per room	675	1.16	0.28	0.47	1.92
Share of homes with 2+ rooms	675	0.66	0.13	0.36	0.98
Share of homes with 3+ rooms	675	0.76	0.14	0.40	1.00
Average years of schooling	675	8.00	2.38	2.99	14.69
Share married	675	0.55	0.04	0.38	0.67
Share working age	675	0.63	0.06	0.44	0.80
Share economically active	675	0.38	0.06	0.13	0.53
Share without health care	675	0.35	0.12	0.07	0.84
Share with state workers health care	675	0.04	0.05	0.00	0.36
Share old	675	0.06	0.03	0.01	0.21
Average children per woman	675	2.49	0.58	1.27	4.84
Share of households with male head	675	0.77	0.06	0.49	0.97
Share born out of state	675	0.27	0.26	0.00	0.88
Share indigenous speakers	675	0.05	0.15	0.00	0.94
Share of homes without a dirt floor	675	0.93	0.09	0.27	1.00
Share of homes with a toilet	675	0.89	0.16	0.07	1.00
Share of homes with water	675	0.87	0.23	0.00	1.00
Share of homes with drainage	675	0.84	0.22	0.01	1.00
Share of homes with electricity	675	0.97	0.07	0.30	1.00
Share of homes with water, drainage, and electricity	675	0.77	0.28	0.00	1.00
Share of homes with a washing machine	675	0.60	0.24	0.00	0.99
Share of homes with a landline telephone	675	0.41	0.29	0.00	0.99
Share of homes with a radio	675	0.83	0.10	0.47	0.99
Share of homes with a fridge	675	0.76	0.20	0.00	1.00
Share of homes with a cell phone	675	0.56	0.24	0.00	0.97
Share of homes with a television	675	0.91	0.13	0.11	1.00
Number of local media stations	675	2.46	3.17	0.00	13.00
Share of homes with a car	675	0.39	0.17	0.01	0.98
Share of homes with a computer	675	0.23	0.22	0.00	0.91
Share of homes with internet	675	0.16	0.19	0.00	0.87
Turnout in 2012	675	0.63	0.08	0.25	0.89
Incumbent party vote margin in 2012	675	-0.18	0.14	-0.82	0.00
Incumbent party vote share in 2012	675	0.42	0.12	0.11	0.87
Municipal-level incumbent party vote share in 2012	675	0.11	0.10	0.00	0.47
Panel B: survey-level covariates					
Remember leaflet	4,635	0.27	0.44	0.00	1.00
Remember reading leaflet	4,635	0.18	0.38	0.00	1.00
Correctly remember content	4,635	0.17	0.38	0.00	1.00
Leaflet influenced content	4,635	0.06	0.24	0.00	1.00
Perceived incumbent party malfeasance	4,635	-0.10	1.48	-2.00	2.00
Precision of perceived incumbent party malfeasance	4,626	3.25	0.84	1.00	4.00
Elections help to select competent candidates	4,517	2.85	1.40	1.00	5.00
Total incumbent party activities	4,635	0.48	1.20	0.00	5.00
Total challenger party activities	4,635	0.51	1.28	0.00	5.00
Information treatment	4,635	0.77	0.42	0.00	1.00
Female	4,635	0.64	0.48	0.00	1.00
Age	4,560	44.40	15.98	17.00	95.00
Education	4,628	8.14	4.13	0.00	16.00
Income	4,130	2.54	1.97	1.00	20.00
Income (log)	4,130	1.16	0.44	0.69	3.04
Employed	4,627	0.42	0.49	0.00	1.00
Turnout in 2012	4,635	0.64	0.48	0.00	1.00
Incumbent vote in 2012	2,974	0.55	0.50	0.00	1.00
Political knowledge Index	4,635	2.41	0.85	0.00	3.00

Table A4: Effect of information treatment on 40 precinct-level and 8 individual-level pre-treatment variables

	Control mean	Treatment mean	Treatment effect	Standard error	Observations
Panel A: precinct-level covariates					
Area	10.0	10.5	-0.637	(0.717)	675
Population	1372.6	1392.7	-26.24	(36.34)	675
Population density	6126.5	5491.7	90.93	(231.8)	675
Distance from municipal centroid	7645.4	8839.5	438.7	(273.2)	675
Number of households	329.4	330.9	-6.831	(8.787)	675
Number of private dwellings	395.9	398.6	-9.930	(11.06)	675
Average occupants per dwelling	4.10	4.16	0.014	(0.016)	675
Average occupants per room	1.15	1.19	0.006	(0.008)	675
Share of homes with 2+ rooms	0.66	0.65	0.001	(0.006)	675
Share of homes with 3+ rooms	0.76	0.75	0.001	(0.006)	675
Average years of schooling	8.12	7.73	-0.107*	(0.054)	675
Share married	0.55	0.55	0.001	(0.002)	675
Share working age	0.63	0.63	-0.001	(0.001)	675
Share economically active	0.38	0.37	0.000	(0.002)	675
Share without health care	0.34	0.35	0.011**	(0.005)	675
Share with state workers health care	0.04	0.04	0.000	(0.002)	675
Share aged 65+	0.06	0.06	0.001	(0.002)	675
Average children per woman	2.47	2.58	0.042***	(0.015)	675
Share of households with male head	0.77	0.77	0.001	(0.003)	675
Share born out of state	0.27	0.27	0.006	(0.006)	675
Share indigenous speakers	0.06	0.06	0.008**	(0.004)	675
Share of homes without a dirt floor	0.92	0.92	-0.001	(0.003)	675
Share of homes with a toilet	0.89	0.88	-0.001	(0.005)	675
Share of homes with water	0.84	0.84	0.002	(0.009)	675
Share of homes with drainage	0.83	0.82	-0.004	(0.006)	675
Share of homes with electricity	0.96	0.96	0.002	(0.003)	675
Share of homes with water, drainage, and electricity	0.76	0.74	-0.008	(0.009)	675
Share of homes with a washing machine	0.58	0.57	0.004	(0.005)	675
Share of homes with a landline telephone	0.42	0.38	-0.016**	(0.007)	675
Share of homes with a radio	0.82	0.82	0.000	(0.003)	675
Share of homes with a fridge	0.75	0.74	-0.001	(0.006)	675
Share of homes with a cell phone	0.55	0.53	0.008	(0.005)	675
Share of homes with a television	0.90	0.89	-0.004	(0.003)	675
Number of local media stations	2.32	2.33	0.05	(0.030)	675
Share of homes with a car	0.39	0.37	-0.005	(0.006)	675
Share of homes with a computer	0.25	0.21	-0.007	(0.006)	675
Share of homes with internet	0.17	0.14	-0.006	(0.006)	675
Turnout in 2012	0.63	0.63	0.007**	(0.003)	675
Incumbent party vote share in 2012	-0.17	-0.20	-0.017***	(0.006)	675
Incumbent party vote margin in 2012	0.42	0.44	0.014***	(0.005)	675
Panel B: survey-level covariates					
Female	0.62	0.64	0.020	(0.018)	4,958
Age	44.6	44.4	-0.528	(0.531)	4,869
Education	8.13	8.00	-0.062	(0.133)	4,948
Income	2.55	2.48	-0.043	(0.081)	4,402
Income (log)	1.16	1.14	-0.010	(0.017)	4,402
Employed	0.42	0.42	-0.006	(0.014)	4,950
Turnout in 2012	0.63	0.63	0.004	(0.012)	4,958
Incumbent vote in 2012	0.55	0.54	-0.007	(0.021)	3,122
Political knowledge index	2.39	2.40	0.006	(0.025)	4,958

Notes: Specifications include block fixed effects and are estimated using OLS. Two variables used as controls—rural and previous municipal incumbent party vote share—are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

following equation:

$$Y_{pbm} = \alpha_{bm} + \beta T_{pbm} + \gamma(T_{pbm} \times X_{pbm}) + \varepsilon_{pbm} \tag{A17}$$

where this differs from equation (4) in the main paper because X_{pbm} is measured at the precinct level. For individual-level survey outcomes, Y_{ipbm} and X_{ipbm} also includes an i subscript.

However, due to our inability to measure individual prior beliefs, we estimated equation (4) where we instead replaced X_{pbm} and X_{ipbm} with $X_m = \frac{1}{\left|i \in m,\, Treatment_{ipbm}=0\right|} \sum_{i \in m, Treatment_{ipbm}=0} X_{ipbm}$, i.e., the average belief among post-election survey respondents in the control precincts.

To econometrically validate this approach, we next show that our estimates represent a lower bound on the magnitude of the precinct- and individual-level heterogeneous effects under plausible conditions. In particular, this is the case where: (i) control group respondents are similar to treatment group respondents; and (ii) control group respondents' beliefs are not subject to spillovers between the intervention and the post-election survey. Intuitively, this is because these conditions ensure that our municipal-level aggregation simply adds classical measurement error to precinct- and individual-specific prior beliefs. We illustrate this for the precinct-level estimating equation, although the proof extends to the individual-level analog.

Applying the Frisch-Waugh Theorem, and abusing notation in terms of using the same variables to now refer to their partialled out analogs, denote the OLS estimate as:

$$\begin{pmatrix} \mathbb{E}[\widehat{\beta}] \\ \mathbb{E}[\widehat{\gamma}] \end{pmatrix} = \begin{pmatrix} \mathbb{E}\left[T_{pbm}^2\right] & \mathbb{E}\left[T_{pbm}^2 X_m\right] \\ \mathbb{E}\left[T_{pbm}^2 X_m\right] & \mathbb{E}\left[T_{pbm}^2 X_m^2\right] \end{pmatrix}^{-1} \begin{pmatrix} \mathbb{E}\left[T_{pbm} Y_{pbm}\right] \\ \mathbb{E}\left[T_{pbm} X_m Y_{pbm}\right] \end{pmatrix}. \tag{A18}$$

Under assumption (i), which holds thanks to our block randomization and the lack of selection into the endline, and assumption (ii), which we next show supporting evidence of:

$$X_m = \mathbb{E}\left[X_{ipbm}\right] + \nu_m, \tag{A19}$$

with $\mathbb{E}\left[\nu_m\right] = 0$, $\mathbb{E}\left[T_{pbm}^k \nu_m\right] = 0$ for $k = \{1,2\}$, $\mathbb{E}\left[X_{ipbm} \nu_m\right] = 0$, and $\mathbb{E}\left[T_{pbm}^2 X_{ipbm} \nu_m\right] = 0$.

Further using that, thanks to our randomization, $\mathbb{E}\left[T_{pbm}^k X_{ipbm}^s\right] = \mathbb{E}\left[T_{pbm}^k\right] \mathbb{E}\left[X_{ipbm}^s\right]$ for k and

$s = \{1,2\}$ and $\mathbb{E}\left[T^2_{pbm} v^2_m\right] = \mathbb{E}\left[T^2_{pbm}\right]\mathbb{E}\left[v^2_m\right]$, it follows that

$$\begin{pmatrix} \mathbb{E}[\widehat{\beta}] \\ \mathbb{E}[\widehat{\gamma}] \end{pmatrix} = \begin{pmatrix} \beta \\ \gamma \end{pmatrix} + \begin{pmatrix} \frac{\mathbb{E}\left[X_{ipbm}\right]\mathbb{E}\left[v^2_m\right]}{\mathbb{E}\left[X^2_{ipbm}\right] - \mathbb{E}^2[\mu_i] + \mathbb{E}[v^2_m]} \\ -\frac{\mathbb{E}\left[v^2_m\right]}{\mathbb{E}\left[X^2_{ipbm}\right] - \mathbb{E}^2[\mu_i] + \mathbb{E}[v^2_m]} \end{pmatrix} \gamma. \tag{A20}$$

This shows that γ—the key coefficient of interest—is biased toward zero, while the bias of β depends in turn on the sign of β, γ, and $\mathbb{E}[X_{ipbm}]$.

Next, we provide evidence to support our claim that post-treatment beliefs in the control precincts proxy for pre-treatment prior beliefs in the treated precincts within the same municipality. To do so, we show that the two key assumptions—(1) that control group respondents are similar to treatment group respondents and (2) that control group respondent beliefs are consistent across the month between the intervention and the post-election survey—are plausible in the context of this study.

First, our randomization ensures that treated and control precincts are identical in expectation. The balance over individual-level characteristics observed in Table A4 is particularly important because it indicates that our treatment did not affect the willingness of different types of voters to participate in the endline survey. Moreover, our blocking strategy ensures substantial within-block similarity in practice: block fixed effects account for 60% of the variation in precinct-level incumbent vote share and 29% of the variation in individual-level beliefs within our samples.

Second, we examine whether the election itself influenced beliefs between the dissemination of the treatment and the post-election survey. Table A5 shows that the 2015 *municipal*-level election outcomes are generally uncorrelated with the level of beliefs about incumbent party malfeasance among respondents in the control group, conditioning on the municipal incumbent party's vote share in the previous election—a pre-treatment proxy for prior beliefs in the control group. The exception is in column (4), where the municipal incumbent party's vote share is positively correlated with the precision of prior beliefs in the control group. However, the magnitude is small: a 70 percentage point increase in vote share is required to increase the precision of beliefs in the control group by a standard deviation. Moreover, the election outcome itself is not significantly correlated with belief precision in the control group. The results suggest that the intervening election outcomes themselves did not substantially influence voter beliefs (and thus violate our second assumption). This is not surprising, since electoral expectations were likely to be relatively fixed in advance and the scale of our intervention was specifically designed not to influence electoral outcomes.

Third, and more generally, the 2012 Mexican Panel Survey shows that voter assessments of

Table A5: Correlation between municipal-level election outcomes and prior beliefs in the control group

	Incumbent malfeasance prior		Incumbent prior precision	
	(1)	(2)	(3)	(4)
Municipal incumbent won election (2015)	-0.516		0.197	
	(0.382)		(0.127)	
Municipal incumbent vote share (2015)		-1.713		1.207**
		(1.661)		(0.481)
Municipal incumbent vote share (2012)	3.307*	3.723**	-0.865	-1.027
	(1.690)	(1.767)	(0.695)	(0.697)
Constant	-1.198	-1.110	3.482***	3.238***
	(0.779)	(1.007)	(0.368)	(0.381)
Control outcome mean	-0.14	-0.14	3.25	3.25
Control outcome std. dev.	1.48	1.48	0.85	0.85
2015 election outcome mean	0.75	0.38	0.74	0.38
2015 election outcome std. dev.	0.44	0.08	0.44	0.08
R^2	0.06	0.04	0.02	0.02
Observations	1,070	1,070	1,081	1,081

Notes: Specifications are estimated using OLS. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

politicians are relatively persistent in the months prior to the election. Voters' opinions of the presidential candidates before and after the election—three months apart, in contrast to the 3–4 weeks apart we examine—exhibit a 0.4 correlation.

Fourth, we test for whether control precincts were subject to information spillovers. Table A6 reports the effects of spillovers from precincts in our experimental sample to neighboring precincts (any precinct that partially borders a precinct in our experimental sample) that were not in our experimental sample. Here, the unit of observation is the precinct-neighbor level; precincts are inversely weighted by the number of neighbors in the experimental sample. While the interaction with the precision of prior beliefs is consistent with the predictions of our model, this is not supported in our main specifications reported in the main paper. Moreover, the positive interaction with the malfeasance level reported is exactly opposite to our findings and model's prediction. It is then hard to see how these results could reflect our information treatment. Table A7 shows that leaflet recall is unaffected by the share of treated neighbors among respondents in control precincts. In addition, columns (5) and (6) show that the increased political responses in treated precincts do not spill over into neighboring control precincts. These checks indicate that information from treated precincts did not influence beliefs in the control group in the three weeks between the treatment and the post-election survey, and thus violate our second assumption.

Fifth, if the information was indeed novel to the control group, then the control group should have updated its beliefs substantially more than the treatment group after being shown the leaflet at the end of the post-election survey. Table A8 shows that control respondents perceived their incumbent to be more malfeasant when shown a leaflet revealing high levels of malfeasance for the first time at the end of the post-election survey. While not reaching statistical significance, the interactions in columns (2) and (6) also align with the results in the main paper. Control respondents thus seem to react similarly to treated respondents, suggesting that treated respondents possessed similar prior beliefs.

Finally, we use data from a similar intervention to ours conducted around the October 2016 Brazilian municipal elections by Boas, Hidalgo and Melo (2019). Critical for our purposes, their study collected voters' beliefs on local governments' performance at both baseline and endline, which allows us to look directly at the extent to which endline beliefs of respondents in control units are valid proxies for the prior beliefs of respondents in treated units.

This Brazilian study informed voters about the local government's use of funds (that we refer to as the "accounts" treatment) and about educational performance in the municipality (that we refer to as the "education" treatment). In addition, there was a pure control group. Assignment to treatment was randomized at the census tract level, which were treated as randomization blocks. The

Table A6: Neighbor spillover effects of information treatment on incumbent party vote share

	Incumbent party vote share					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Incumbent party vote share (share of turnout)						
Neighbor information treatment	-0.003	-0.003	0.055*	-0.011***	0.067*	-0.004
	(0.003)	(0.002)	(0.028)	(0.004)	(0.033)	(0.003)
× Neighbor incumbent malfeasance prior		-0.000			-0.003	
		(0.003)			(0.003)	
× Neighbor incumbent prior precision			-0.017**		-0.024**	
			(0.008)		(0.010)	
× Incumbent malfeasant spending				0.035***	0.037***	
				(0.011)	(0.010)	
× Neighbor unfavorable incumbent updating						0.002
						(0.002)
Outcome range	[0.05,0.89]	[0.05,0.89]	[0.05,0.89]	[0.05,0.89]	[0.05,0.89]	[0.05,0.89]
Control outcome mean	0.40	0.40	0.40	0.40	0.40	0.40
Control outcome std. dev.	0.11	0.11	0.11	0.11	0.11	0.11
R^2	0.51	0.50	0.50	0.51	0.50	0.50
Panel B: Incumbent party vote share (share of registered voters)						
Neighbor information treatment	-0.003**	-0.003**	0.029*	-0.009***	0.039***	-0.005**
	(0.001)	(0.001)	(0.015)	(0.002)	(0.014)	(0.002)
× Neighbor incumbent malfeasance prior		-0.001			-0.002**	
		(0.001)			(0.001)	
× Neighbor incumbent prior precision			-0.010**		-0.014***	
			(0.005)		(0.004)	
× Incumbent malfeasant spending				0.023***	0.024***	
				(0.006)	(0.005)	
× Neighbor unfavorable incumbent updating						0.001
						(0.001)
Outcome range	[0.03,0.46]	[0.03,0.46]	[0.03,0.46]	[0.03,0.46]	[0.03,0.46]	[0.03,0.46]
Control outcome mean	0.19	0.19	0.19	0.19	0.19	0.19
Control outcome std. dev.	0.06	0.06	0.06	0.06	0.06	0.06
R^2	0.53	0.53	0.53	0.54	0.53	0.53
Interaction range		[-1.4,1.1]	[2.4,3.8]	[0,0.58]		[-0.6,2.7]
Interaction mean		-0.18	3.31	0.24		1.01
Interaction std. dev.		0.87	0.22	0.19		1.05
Observations	2,302	2,268	2,268	2,302	2,268	2,268

Notes: The sample contains all precinct-neighboring precincts pairs for which the neighboring precinct (which partially shares a border with a precinct in the experimental sample) is included in the experimental sample, but the spillover precinct is not. Specifications include neighbor-level block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in Columns (2), (3), and (5) reflect the lack of data on prior beliefs about the incumbent party in Apaseo el Alto. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A7: Neighbor spillover of information treatment on self-reported engagement with leaflet and political responses in control precincts

	Remember leaflet (1)	Remember reading leaflet (2)	Correctly remember content (3)	Leaflet influenced vote (4)	Total incumbent activities (5)	Total challenger activities (6)
Share of treated neighbors	-0.014	-0.013	-0.017	0.007	-0.396*	-0.254
	(0.040)	(0.024)	(0.022)	(0.011)	(0.193)	(0.183)
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1,2,3,4,5}	{0,1,2,3,4,5}
Outcome mean	0.09	0.05	0.06	0.02	0.43	0.40
Outcome std. dev.	0.28	0.22	0.25	0.14	1.18	1.17
Share of treated neighbors mean	0.41	0.41	0.41	0.41	0.41	0.41
Share of treated neighbors std. dev.	0.42	0.42	0.42	0.42	0.42	0.42
R^2	0.00	0.00	0.00	0.00	0.02	0.01
Observations	1,139	1,139	1,139	1,139	1,139	1,139

Notes: The sample includes all control precincts within our experimental sample. All specifications are estimated using OLS. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

study surveyed around 3,000 individuals at baseline (before the intervention and the elections) and endline. One-third were exposed to the accounts treatment, one-third to the education treatment, and the remaining third constituted a control group.

All respondents were asked to evaluate the accounts management and educational performance of local governments in both baseline and endline, irrespective of which treatment they were assigned to. We simply pool the accounts and education treatments, though the patterns described below are very similar if we consider each treatment separately.[1]

Recall that our approach of using the beliefs of the control group at endline as proxies for the prior beliefs of the treated group requires two conditions:

1. The pre-treatment beliefs of control and treatment respondents are similar (on average).

2. Absent any intervention, individual beliefs are fairly consistent over short periods of time. That is, there is persistence in the beliefs of control subjects before and after the implementation of the intervention.

We conduct some basic correlation tests to assess the extent to which these two conditions held in the context of the Brazilian experiment. We first generate average values of treated and control responses within municipalities for both endline and baseline. The notation of variables

[1]Since we pool treatments, each control individual appears twice: as control for the educational and accounts treatment.

Table A8: Effect of showing voters the leaflet in the post-treatment survey

	Perceived incumbent party malfeasance (very low - very high)					
	(1)	(2)	(3)	(4)	(5)	(6)
Shown leaflet for first time	0.061*	0.059*	0.065	-0.008	0.034	0.025
	(0.031)	(0.035)	(0.355)	(0.043)	(0.401)	(0.057)
× Incumbent malfeasance prior		-0.023			-0.020	
		(0.049)			(0.049)	
× Incumbent prior precision			-0.001		-0.013	
			(0.107)		(0.117)	
× Incumbent malfeasant spending				0.329*	0.322	
				(0.171)	(0.192)	
× Unfavorable incumbent updating						0.040
						(0.036)
Perceived incumbent party malfeasance (pre-leaflet)	-0.001	-0.001	-0.001	-0.002	-0.001	-0.001
	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)
Outcome range	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}
Control outcome mean	0.75	0.75	0.75	0.75	0.75	0.75
Control outcome std. dev.	1.07	1.07	1.07	1.07	1.07	1.07
Interaction range		[-1.4,1.1]	[2.4,3.8]	[0,0.58]		[-0.6,2.7]
Interaction mean		-0.09	3.23	0.21		0.91
Interaction std. dev.		0.82	0.26	0.17		1.00
R^2	0.09	0.09	0.09	0.09	0.09	0.09
Observations	4,624	4,624	4,624	4,624	4,624	4,624

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A9: Correlation analysis of beliefs over time from Brazilian study (Boas, Hidalgo and Melo 2019), both treatments pooled

Variables	av_bl_c	av_bl_t	av_el_c	av_el_t
av_bl_c	1.000			
av_bl_t	0.858	1.000		
av_el_c	0.859	0.779	1.000	
av_el_t	0.766	0.784	0.876	1.000

is straightforward. The middle two letters refer to baseline (bl) or endline (el), and the last letter indicates whether the statistic refers to control respondents only (c) or treatment respondents only (t). Correlations are reported in Table A9.

The first thing to note is that the correlation of baseline priors for treatment and control (*av_bl_t* and *av_bl_c*) is large and positive (0.86). This is probably not surprising, given that treatment was randomly assigned. Moreover, this correlation would most likely become larger as the survey sample size increases.

Next we look at the second condition. The correlation between the control group at baseline and endline is 0.86. Survey responses are noisy, and thus we would not expect a perfect serial correlation even absent any treatment, as other events between baseline and endline (i.e. the election) may change some people's preferences. So a positive correlation of around 0.9 is consistent with condition 2.

Finally, we look at our object of interest: the extent to which the prior beliefs of the treated group (*av_bl_t*) are correlated with the endline evaluations of the control group (*av_el_c*). The correlation here is 0.78. This strong correlation is consistent with the correlations documented above in support of conditions 1 and 2, and suggests that the endline responses of the control group may be used as valid proxies for baseline responses of the treated.

Since this exercise was conducted in the context of a different country and a different intervention, it is hard to assess the extent to which these correlations would be similar in the context of our experiment had we conducted a baseline survey. However, together with the evidence reported in Tables A5-A8, these results are encouraging regarding the use of our approach to proxy for voters' prior beliefs.

A.4.4 Perceived origin of the leaflet

Tables A10 and A11 examine the correlates of beliefs about the origins of the leaflets among treated voters. Respondents were asked to answer yes or no with regard to whether they believed that the leaflet was disseminated by eight possible sources: a non-partisan NGO, the federal government, the state government, the municipal government, the PAN, the PRI, the PRD, or other. Respondents were able to select more than one option.

Column (1) of panels A and B in Table A10 shows that neither the public nor comparative version of our information treatment significantly affected the belief that the treatment came from an NGO or a political party. As the outcome mean at the foot of the table indicates, more voters—43%—believed that the leaflet was distributed by a non-partisan NGO than the total number of voters who believed that the leaflet originated by the PAN, PRD, or PRI. Columns (2)-(4) show that these beliefs are mostly uncorrelated with municipal-level prior beliefs, although voters in the municipalities that had the strongest prior beliefs and updated most unfavorably were less likely to believe the leaflet came from an NGO. Columns (5)-(8) show similar results when restricting the sample to those who recalled receiving the treatment. The results in Table A11 similarly show that the belief that the information was disseminated by the incumbent party or a challenger—both of which are rare in comparison to the belief that the information was distributed by a non-partisan NGO—was uncorrelated with the information treatment form and voters' prior beliefs and updating.

A.5 Heterogeneity in beliefs by perceived origins of the leaflet

It is possible that voters' perception of who distributed the leaflet shapes the degree to which voters updated their beliefs. Following Alt, Lassen and Marshall (2016), NGOs might be regarded as more credible than political parties, while favorable (unfavorable) reports might be more influential if they are perceived to have been disseminated by challenger (incumbent) parties. Since control respondents were shown the leaflet during the endline survey, but *after* their posterior beliefs were elicited, we are able to measure (post-treatment) perceptions of the leaflet's provenance for respondents in both treated and control precincts. To examine heterogeneity in treatment effects across perceptions of the leaflet's provenance, we further interact our estimates in Table 3 with (demeaned) indicators for whether the survey respondent believed the leaflet was distributed by an NGO, believed the leaflet was distributed by the incumbent party, believed the leaflet was distributed by an opposition party, or did not know who distributed the leaflet.

The survey-level results by perceived source of the leaflet are reported in Tables A12-A15.

Table A10: Correlates of voter beliefs about the leaflet's author, NGO and political parties

	All treated respondents				Treated respondents that remember the leaflet			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Outcome: believe leaflet was distributed by an NGO								
Public information treatment	0.021				0.034			
	(0.019)				(0.031)			
Comparative information treatment	-0.015				0.013			
	(0.018)				(0.026)			
Incumbent malfeasance prior		0.031				0.035*		
		(0.023)				(0.020)		
Incumbent prior precision			-0.164*				-0.312***	
			(0.083)				(0.079)	
Unfavorable incumbent updating				-0.031**				-0.032*
				(0.015)				(0.016)
Fixed effects	block	state	state	state	block	state	state	state
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.43	0.43	0.43	0.43	0.64	0.64	0.64	0.64
Outcome std. dev.	0.50	0.50	0.50	0.50	0.48	0.48	0.48	0.48
R^2	0.05	0.01	0.01	0.01	0.11	0.01	0.02	0.01
Observations	3,659	3,659	3,659	3,659	1,186	1,186	1,186	1,186
Panel B: Outcome: believe leaflet was distributed by any political party (PAN, PRD, PRI)								
Public information treatment	-0.012				-0.041			
	(0.016)				(0.027)			
Comparative information treatment	-0.023				0.002			
	(0.015)				(0.023)			
Incumbent malfeasance prior		0.028				0.034		
		(0.023)				(0.031)		
Incumbent prior precision			-0.189*				-0.104	
			(0.110)				(0.178)	
Unfavorable incumbent updating				-0.025				-0.029
				(0.016)				(0.021)
Fixed effects	block	state	state	state	block	state	state	state
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.25	0.25	0.25	0.25	0.23	0.23	0.23	0.23
Outcome std. dev.	0.43	0.43	0.43	0.43	0.42	0.42	0.42	0.42
R^2	0.06	0.01	0.01	0.01	0.13	0.01	0.00	0.01
Observations	3,659	3,659	3,659	3,659	1,186	1,186	1,186	1,186

Notes: Some respondents noted that they believed the information came from multiple types of sources (see description above). All specifications contain only treated observations, and are estimated using OLS. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A11: Correlates of voter beliefs about the leaflet's author, incumbent and challenger parties

	All treated respondents				Treated respondents that remember the leaflet			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Outcome: believe leaflet was distributed by the municipal incumbent party								
Public information treatment	0.009				-0.019			
	(0.018)				(0.026)			
Comparative information treatment	-0.031				-0.027			
	(0.021)				(0.026)			
Incumbent malfeasance prior		0.014				0.014		
		(0.019)				(0.026)		
Incumbent prior precision			-0.143*				-0.149	
			(0.083)				(0.125)	
Unfavorable incumbent updating				-0.014				-0.012
				(0.013)				(0.019)
Fixed effects	block	state	state	state	block	state	state	state
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.26	0.26	0.26	0.26	0.24	0.24	0.24	0.24
Outcome std. dev.	0.44	0.44	0.44	0.44	0.43	0.43	0.43	0.43
R^2	0.05	0.01	0.01	0.01	0.12	0.02	0.02	0.02
Observations	3,659	3,659	3,659	3,659	1,186	1,186	1,186	1,186
Panel B: Outcome: believe leaflet was distributed by a municipal challenger party								
Public information treatment	-0.005				-0.030			
	(0.013)				(0.023)			
Comparative information treatment	-0.011				-0.008			
	(0.012)				(0.026)			
Incumbent malfeasance prior		0.020				0.002		
		(0.015)				(0.021)		
Incumbent prior precision			-0.096				-0.013	
			(0.102)				(0.156)	
Unfavorable incumbent updating				-0.021*				-0.007
				(0.011)				(0.016)
Fixed effects	block	state	state	state	block	state	state	state
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.16	0.16	0.16	0.16	0.14	0.14	0.14	0.14
Outcome std. dev.	0.36	0.36	0.36	0.36	0.35	0.35	0.35	0.35
R^2	0.06	0.01	0.01	0.01	0.13	0.01	0.01	0.01
Observations	3,659	3,659	3,659	3,659	1,186	1,186	1,186	1,186

Notes: Some respondents noted that they believed the information came from multiple types of source. (see description above). All specifications contain only treated observations, and are estimated using OLS. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Although the estimates are relatively small in magnitude and statistically imprecise, a comparison between column (1) in Tables A13 and A14 suggests that voters may have been more likely to update favorably (unfavorably), on average, when they believed that the leaflet was delivered by a challenger (incumbent) party. The estimates in columns (2) and (6) also tentatively suggest that belief updating may have been greater among respondents that believed the leaflet was distributed by an NGO, the incumbent party, or an opposition party. The point estimates are broadly similar in magnitude in each case, suggesting that even sources with political incentives were regarded as providing more credible information. In contrast, the effects in Table A15 are smaller in magnitude among respondents that did not know who distributed the leaflet. However, these triple interaction coefficients are not statistically significant, and should thus be treated with appropriate caution. Analogous results hold among the precincts where surveys were conducted, although the estimates are again relatively imprecise.

A.6 Updating about challengers

Although our analysis focuses on the effect of the intervention on posterior beliefs about the incumbent party, our findings could also reflect changes in posterior beliefs about challengers. Indeed, two elements of the information disseminated could be informative about challenger parties in a voter's municipality. First, voters may update about challengers from the benchmarked information that reports the level of malfeasance found in other municipalities within the same state that are governed by different parties. Since Mexican parties normally field candidates in every municipality, voters might update about challenger parties in their municipality to the extent that they believe that candidates within a given party are similar across municipalities. We examine this case formally in a companion paper (Arias et al. 2018), where we model signals of underlying malfeasance as reflecting both the type of a given incumbent party and common shocks affecting incumbents from all parties equally. That paper shows that voters use signals of incumbent and challenger malfeasance to update about both parties, but also to learn about and then filter out common shocks. The data, however, indicate that voters did not differentially update their beliefs about either the incumbent or challenger party malfeasance from benchmarked information (or its interaction with the reported level of challenger malfeasance), relative to incumbent-only information, and did not significantly update about challenger parties relative to the control group. This suggests that voters either did not understand the benchmark or did not regard it as relevant.

Second, if voters perceive the type of incumbent and challenger parties in their municipality to be correlated, they could also draw inferences about challenger parties on the basis of the signal of incumbent party malfeasance. Our simple model abstracted from this possibility by assuming

Table A12: Heterogeneity in the effect of information on voters' posterior beliefs about incumbent party malfeasance, by belief that the leaflet was distributed by an NGO

	Perceived incumbent party malfeasance (very low to very high)					
	(1)	(2)	(3)	(4)	(5)	(6)
Information treatment	-0.004	-0.021	0.456	0.015	0.917*	-0.110**
	(0.039)	(0.036)	(0.476)	(0.068)	(0.469)	(0.046)
× NGO distribution	0.022	0.014	0.100	-0.038	0.323	-0.046
	(0.085)	(0.081)	(0.863)	(0.136)	(0.851)	(0.100)
× Incumbent malfeasance prior		-0.142***			-0.168***	
		(0.038)			(0.036)	
× Incumbent malfeasance prior × NGO distribution		-0.087			-0.093	
		(0.094)			(0.104)	
× Incumbent prior precision			-0.141		-0.279*	
			(0.148)		(0.144)	
× Incumbent prior precision × NGO distribution			-0.023		-0.106	
			(0.269)		(0.266)	
× Incumbent malfeasant spending				-0.090	-0.172	
				(0.219)	(0.174)	
× Incumbent malfeasant spending × NGO distribution				0.260	0.124	
				(0.418)	(0.419)	
× Unfavorable incumbent updating						0.115***
						(0.031)
× Unfavorable incumbent updating × NGO distribution						0.084
						(0.076)
Control outcome mean	-0.14	-0.14	-0.14	-0.14	-0.14	-0.14
Control outcome std. dev.	1.48	1.48	1.48	1.48	1.48	1.48
Interaction mean		-0.09	3.23	0.21		0.91
Interaction std. dev.		0.82	0.26	0.17		1.00
R^2	0.29	0.30	0.29	0.29	0.30	0.30
Observations	4,624	4,624	4,624	4,624	4,624	4,624

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A13: Heterogeneity in the effect of information on voters' posterior beliefs about incumbent party malfeasance, by belief that the leaflet was distributed by the incumbent party

	Perceived incumbent party malfeasance (very low to very high)					
	(1)	(2)	(3)	(4)	(5)	(6)
Information treatment	-0.001	-0.014	0.420	0.012	0.804*	-0.097**
	(0.040)	(0.037)	(0.470)	(0.067)	(0.451)	(0.048)
× Incumbent distribution	0.047	0.052	0.702	-0.117	0.769	-0.007
	(0.094)	(0.093)	(1.223)	(0.134)	(1.340)	(0.114)
× Incumbent malfeasance prior		-0.130***			-0.153***	
		(0.036)			(0.034)	
× Incumbent malfeasance prior × Incumbent distribution		-0.095			-0.102	
		(0.098)			(0.093)	
× Incumbent prior precision			-0.130		-0.246*	
			(0.147)		(0.138)	
× Incumbent prior precision × Incumbent distribution			-0.205		-0.272	
			(0.383)		(0.413)	
× Incumbent malfeasant spending				-0.052	-0.115	
				(0.219)	(0.167)	
× Incumbent malfeasant spending × Incumbent distribution				0.788	0.713	
				(0.489)	(0.501)	
× Unfavorable incumbent updating						0.106***
						(0.030)
× Unfavorable incumbent updating × Incumbent distribution						0.085
						(0.085)
Control outcome mean	-0.14	-0.14	-0.14	-0.14	-0.14	-0.14
Control outcome std. dev.	1.48	1.48	1.48	1.48	1.48	1.48
Interaction mean		-0.09	3.23	0.21		0.91
Interaction std. dev.		0.82	0.26	0.17		1.00
R^2	0.29	0.29	0.29	0.29	0.30	0.29
Observations	4,624	4,624	4,624	4,624	4,624	4,624

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A14: Heterogeneity in the effect of information on voters' posterior beliefs about incumbent party malfeasance, by belief that the leaflet was distributed by an opposition party

	Perceived incumbent party malfeasance (very low to very high)					
	(1)	(2)	(3)	(4)	(5)	(6)
Information treatment	-0.001	-0.014	0.447	0.020	0.856*	-0.095*
	(0.040)	(0.037)	(0.480)	(0.066)	(0.462)	(0.048)
× Opposition distribution	-0.135	-0.128	2.189	-0.168	2.504*	-0.241*
	(0.116)	(0.112)	(1.336)	(0.198)	(1.393)	(0.121)
× Incumbent malfeasance prior		-0.127***			-0.153***	
		(0.038)			(0.035)	
× Incumbent malfeasance prior × Opposition distribution		-0.123			-0.203	
		(0.139)			(0.132)	
× Incumbent prior precision			-0.138		-0.260*	
			(0.150)		(0.142)	
× Incumbent prior precision × Opposition distribution			-0.715*		-0.821*	
			(0.417)		(0.423)	
× Incumbent malfeasant spending				-0.098	-0.141	
				(0.216)	(0.164)	
× Incumbent malfeasant spending × Opposition distribution				0.170	0.157	
				(0.692)	(0.656)	
× Unfavorable incumbent updating						0.104***
						(0.031)
× Unfavorable incumbent updating × Opposition distribution						0.153
						(0.120)
Control outcome mean	-0.14	-0.14	-0.14	-0.14	-0.14	-0.14
Control outcome std. dev.	1.48	1.48	1.48	1.48	1.48	1.48
Interaction mean		-0.09	3.23	0.21		0.91
Interaction std. dev.		0.82	0.26	0.17		1.00
R^2	0.29	0.30	0.29	0.29	0.30	0.30
Observations	4,624	4,624	4,624	4,624	4,624	4,624

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A15: Heterogeneity in the effect of information on voters' posterior beliefs about incumbent party malfeasance, by belief that the leaflet was unknown

	Perceived incumbent party malfeasance (very low to very high)					
	(1)	(2)	(3)	(4)	(5)	(6)
Information treatment	-0.004	-0.017	0.279	0.012	0.745*	-0.101**
	(0.040)	(0.036)	(0.451)	(0.068)	(0.439)	(0.047)
× Don't know distributor	-0.053	-0.047	1.048	0.028	0.788	-0.011
	(0.105)	(0.110)	(1.152)	(0.130)	(1.170)	(0.158)
× Incumbent malfeasance prior		-0.138***			-0.157***	
		(0.038)			(0.038)	
× Incumbent malfeasance prior × Don't know distributor		0.108			0.087	
		(0.118)			(0.124)	
× Incumbent prior precision			-0.087		-0.228*	
			(0.141)		(0.133)	
× Incumbent prior precision × Don't know distributor			-0.338		-0.235	
			(0.350)		(0.359)	
× Incumbent malfeasant spending				-0.072	-0.128	
				(0.222)	(0.183)	
× Incumbent malfeasant spending × Don't know distributor				-0.348	-0.292	
				(0.388)	(0.442)	
× Unfavorable incumbent updating						0.108***
						(0.032)
× Unfavorable incumbent updating × Don't know distributor						-0.059
						(0.093)
Control outcome mean	-0.14	-0.14	-0.14	-0.14	-0.14	-0.14
Control outcome std. dev.	1.48	1.48	1.48	1.48	1.48	1.48
Interaction mean		-0.09	3.23	0.21		0.91
Interaction std. dev.		0.82	0.26	0.17		1.00
R^2	0.29	0.30	0.29	0.29	0.30	0.30
Observations	4,624	4,624	4,624	4,624	4,624	4,624

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

that types are independent. Allowing voters to believe that incumbent and challenger types are positively (negatively) correlated would induce voters to update in the same (opposite) direction about challengers as incumbents—at least when the means of voters' prior beliefs about the two are similar. Provided that the precision of a voter's prior belief about a challenger's type is not too much smaller than the precision of their belief about the incumbent's type, the magnitude of updating about the incumbent would exceed the magnitude of updating about challengers. To formalize these intuitions, we extend our normal learning framework to let prior beliefs be given by $N\left(\begin{bmatrix}\mu_I\\ \mu_C\end{bmatrix}, \begin{bmatrix}\sigma_I^2 & r\sigma_I\sigma_C\\ r\sigma_I\sigma_C & \sigma_C^2\end{bmatrix}\right)$, where r is the correlation in voters' prior beliefs about θ_I and θ_C. The resulting posterior beliefs are then given by:

$$N\left(\begin{bmatrix}\widehat{\mu}_I\\ \widehat{\mu}_C\end{bmatrix}, \begin{bmatrix}\frac{\sigma_I^2}{1+\sigma_I^2\rho_I} & \frac{r\sigma_I\sigma_C}{1+\sigma_I^2\rho_I}\\ \frac{r\sigma_I\sigma_C}{1+\sigma_I^2\rho_I} & \frac{\sigma_C^2+(1-r^2)\sigma_I^2\sigma_C^2\rho_I}{1+\sigma_I^2\rho_I}\end{bmatrix}\right), \tag{A21}$$

where the posterior means given below reflect a weighted average of voters' prior beliefs about a given party, how the signal s_I relates to their prior beliefs about that party, and voters' prior belief about the challenger party (which, due to the correlation, also constrains the degree to which voters update from s_I):

$$\widetilde{\mu}_I := \frac{\sigma_I^2(\sigma_I^2+r\sigma_C^2+\rho_I)}{1+\sigma_I^2\rho_I}\mu_I + \frac{\sigma_I\rho_I}{1+\sigma_I^2\rho_I}(s_I-\mu_I) + \frac{r\sigma_I\sigma_C(\sigma_I^2+\sigma_C^2)}{1+\sigma_I^2\rho_I}\mu_C \tag{A22}$$

$$\widetilde{\mu}_C := \frac{\sigma_C^2[\sigma_C^2+(1-r^2)\sigma_I^2\sigma_C^2\rho_I]+r\sigma_I\sigma_C(r\sigma_I\sigma_C+\rho_I)}{1+\sigma_I^2\rho_I}\mu_C + \frac{r\sigma_I\sigma_C\rho_I}{1+\sigma_I^2\rho_I}(s_I-\mu_C) + \frac{r\sigma_I\sigma_C[\sigma_I^2+\sigma_C^2+(1-r^2)\sigma_I^2\sigma_C^2\rho_I]}{1+\sigma_I^2\rho_I}\mu_I. \tag{A23}$$

The coefficients on μ_j, $(s_I-\mu_j)$, and μ_{-j} can be interpreted as weights. The incumbent party's vote share, following information provision, is then given by $\widetilde{V}_I = 1-F(\widetilde{\mu}_I-\widetilde{\mu}_C+c)$. Although the new weights provide more complex conditions, similar comparative statics for the comparison $\widetilde{V}_I-\widehat{V}_I$.

Given the limited additional impact of the benchmarked information (Arias et al. 2018), the following tests focus on how voter beliefs about the challengers in their municipality were affected by receiving malfeasance reports pertaining to the incumbent party. Tables A16 and A17 show our survey-level estimates of the effect of the information treatment on voters' posterior beliefs about challenger malfeasance, deploying two definitions of municipal challengers (the second largest party in the last municipal election and the average response across non-incumbent major parties).

The following results suggest that voters may have updated somewhat about challenger parties, although the effects on vote share are largely driven by updated beliefs about the incumbent party.[2]

As with the incumbent party, column (1) in Tables A16 and A17 show that treated voters did not update about challenger parties on average. More importantly, column (4) shows that treated voters did not update their beliefs about challengers in line with signals of incumbent malfeasance. In contrast, column (6) suggests that unfavorable updating about the challenger may have induced treated voters to increase their belief that the challenger is malfeasant.[3] Consistent with a positive correlation between prior beliefs (i.e. $r > 0$), the magnitude of updating is smaller than regarding incumbent parties. These, heterogeneous effects, which are driven by the differential response with respect to voters' prior beliefs about challengers in column (2), may thus reflect voters' correlated beliefs about incumbent and challenger parties. Indeed, the correlation between the incumbent malfeasance prior and our measures of challenger malfeasance priors is around 0.7.

We next show that, to the extent that posterior beliefs about challengers changed, they do not seem to influence electoral outcomes. For both our definitions of challenger parties, Tables A18 and A19 examine how incumbent party vote share varies with beliefs about challengers.[4] The results indicate that voters' prior beliefs about challengers and voter updating about challengers did not substantially impact incumbent party electoral performance. In particular, and in sharp contrast with Table 4, column (4) shows that we fail to detect a significant positive interaction with voters' unfavorable updating about challengers—which we would expect to observe if voters that updated unfavorably about the challenger started to vote for the incumbent party. Moreover, the positive interaction with prior beliefs about challenger malfeasance in column (2) indicates that treated precincts in municipalities with the least favorable prior beliefs about challengers rewarded the incumbent party the most. Given that updating by the level of malfeasance priors was similar across incumbent and challenger parties along this dimension, and that malfeasance prior beliefs are highly correlated across parties, this suggests that posterior belief updating about the incumbent was more important for vote choice than posterior belief updating about challengers. Again, the precision of prior beliefs about challenger malfeasance does not influence voter beliefs and behavior.

[2]The single block from Tamasopo is dropped for our second challenger definition because we did not ask about the second-placed party (MC) in that municipality.

[3]For the construction of unfavorable challenger updating, we again use the responses of control respondents, who received the leaflet at the end of the post-treatment survey.

[4]The interaction with incumbent malfeasant spending is excluded because it simply replicates column (4) of Table 4.

Table A16: Effect of information treatment on voters' posterior beliefs about challenger party malfeasance, where the challenger is the party that received the second-largest vote share in the last municipal election

	Perceived challenger party malfeasance (very low - very high)					
	(1)	(2)	(3)	(4)	(5)	(6)
Information treatment	-0.007	-0.037	-1.022*	-0.009	-0.686	-0.051
	(0.038)	(0.034)	(0.545)	(0.065)	(0.551)	(0.035)
× Challenger malfeasance prior		-0.115*			-0.084	
		(0.063)			(0.065)	
× Challenger prior precision			0.326*		0.208	
			(0.178)		(0.184)	
× Incumbent malfeasant spending				0.013	0.036	
				(0.225)	(0.194)	
× Negative challenger updating						0.060
						(0.041)
Outcome range	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}
Control outcome mean	-0.30	-0.30	-0.31	-0.30	-0.30	-0.30
Control outcome std. dev.	1.36	1.36	1.37	1.36	1.36	1.36
Interaction range		[-1.3,0.9]	[2.6,3.5]	[0,0.18]	[-0.2,0.4]	[-0.6,2.3]
Interaction mean		-0.25	3.10	0.21		0.71
Interaction std. dev.		0.58	0.20	0.17		0.91
R^2	0.19	0.19	0.19	0.19	0.19	0.19
Observations	4,958	4,958	4,908	4,958	4,958	4,958

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in Column (3) reflects a lack of data on prior beliefs about the challenger in Tamasopo. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A17: Effect of information treatment on voters' posterior beliefs about challenger party malfeasance, where the challenger is the average posterior belief across the PAN, PRD, and PRI where they are not the municipal incumbent

	Perceived challenger party malfeasance (very low - very high)					
	(1)	(2)	(3)	(4)	(5)	(6)
Information treatment	0.009	-0.027	-0.643**	0.066	-0.301	-0.052**
	(0.034)	(0.026)	(0.284)	(0.059)	(0.287)	(0.024)
× Challenger malfeasance prior		-0.125***			-0.100*	
		(0.045)			(0.051)	
× Challenger prior precision			0.203**		0.103	
			(0.091)		(0.094)	
× Incumbent malfeasant spending				-0.266	-0.238	
				(0.246)	(0.215)	
× Negative challenger updating						0.080**
						(0.030)
Outcome range	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}
Control outcome mean	-0.33	-0.33	-0.33	-0.33	-0.33	-0.33
Control outcome std. dev.	1.20	1.20	1.20	1.20	1.20	1.20
Interaction range		[-1.2,0.9]	[2.7,3.8]	[0,0.18]	[-0.2,0.4]	[-0.7,2.3]
Interaction mean		-0.27	3.20	0.21		0.73
Interaction std. dev.		0.68	0.25	0.17		1.00
R^2	0.30	0.30	0.30	0.30	0.30	0.30
Observations	4,958	4,958	4,958	4,958	4,958	4,958

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A18: Effect of information treatment on incumbent party vote share, using the challenger's prior beliefs and updating where the challenger is the party that received the second-largest vote share in the last municipal election

	Incumbent party vote share			
	(1)	(2)	(3)	(4)
Panel A: Incumbent party vote share (share of turnout)				
Information treatment	0.020***	0.024***	0.070	0.026***
	(0.004)	(0.004)	(0.083)	(0.004)
× Challenger malfeasance prior		0.016**		
		(0.006)		
× Challenger prior precision			-0.016	
			(0.027)	
× Unfavorable challenger updating				-0.008**
				(0.003)
Outcome range	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]
Control outcome mean	0.38	0.38	0.38	0.38
Control outcome std. dev.	0.12	0.12	0.12	0.12
R^2	0.61	0.61	0.61	0.61
Panel B: Incumbent party vote share (share of registered voters)				
Information treatment	0.008***	0.010***	0.035	0.010***
	(0.002)	(0.002)	(0.044)	(0.003)
× Challenger malfeasance prior		0.006*		
		(0.004)		
× Challenger prior precision			-0.009	
			(0.014)	
× Unfavorable challenger updating				-0.003
				(0.002)
Outcome range	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]
Control outcome mean	0.19	0.19	0.19	0.19
Control outcome std. dev.	0.07	0.07	0.07	0.07
R^2	0.62	0.62	0.61	0.62
Interaction range		[-1.3,0.9]	[2.6,3.5]	[-0.6,2.3]
Interaction mean		-0.25	3.11	0.72
Interaction std. dev.		0.59	0.20	0.93
Observations	675	675	668	675

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in Column (3) reflects a lack of data on prior beliefs about the challenger in Tamasopo. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A19: Effect of information treatment on incumbent party vote share, using prior beliefs about the challenger and updating where the challenger is the average posterior belief across the PAN, PRD, and PRI where they are not the municipal incumbent

	Incumbent party vote share			
	(1)	(2)	(3)	(4)
Panel A: Incumbent party vote share (share of turnout)				
Information treatment	0.020***	0.023***	0.114*	0.025***
	(0.004)	(0.004)	(0.059)	(0.004)
× Challenger malfeasance prior		0.012**		
		(0.005)		
× Challenger prior precision			-0.030	
			(0.018)	
× Unfavorable challenger updating				-0.007**
				(0.003)
Outcome range	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]
Control outcome mean	0.38	0.38	0.38	0.38
Control outcome std. dev.	0.12	0.12	0.12	0.12
R^2	0.61	0.61	0.61	0.61
Panel B: Incumbent party vote share (share of registered voters)				
Information treatment	0.008***	0.009***	0.050	0.010***
	(0.002)	(0.002)	(0.032)	(0.003)
× Challenger malfeasance prior		0.005		
		(0.003)		
× Challenger prior precision			-0.013	
			(0.010)	
× Unfavorable challenger updating				-0.002
				(0.002)
Outcome range	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]
Control outcome mean	0.19	0.19	0.19	0.19
Control outcome std. dev.	0.07	0.07	0.07	0.07
R^2	0.62	0.62	0.62	0.62
Interaction range		[-1.2,0.9]	[2.7,3.8]	[-0.7,2.3]
Interaction mean		-0.27	3.21	0.74
Interaction std. dev.		0.68	0.25	1.02
Observations	675	675	675	675

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

A.7 Alternative explanations for the positive average treatment effect on incumbent vote share

In the main text, we provide evidence suggesting that increased precision of posterior beliefs and politician responses could account for the positive average treatment effect on precinct-level incumbent vote share. However, we also consider alternative explanations for the positive average treatment effect in the aggregate data. We first consider the possibility that our results are explained by the effect of our information treatment on voter expectations of their incumbent's ability to extract federal funds. Columns (1) to (4) of Table A20 show that voters are no more likely to reward incumbent parties that received large quantities of FISM funds in absolute or per voter terms. These results then suggest that credit claiming is unlikely to be driving the average effect at the precinct level. In addition, we also examine the extent to which voters report ranking honesty and policies to address poverty as important—on a five-point scale—in determining their vote choices. The results in Table A21 indicate that neither characteristic was influenced by the information treatment.

A.8 Heterogeneity by the type of information reported

In Table A22, we split the sample between municipalities that received information about not spending FISM funds on projects that benefited the poor (panel A) and spending on unauthorized projects (panel B). In line with Larreguy, Marshall and Snyder (2020), we find broadly similar results across both sub-samples, although the response to the signal and the degree of updating differs a little in magnitude. This suggests that voters viewed unauthorized spending and spending that did not benefit the poor similarly as signals of incumbent quality.

Nevertheless, a potential concern when pooling across the two types of malfeasance information is that the type of information reported could correlate with other features of the municipality. Since this was not randomized, the type of information received could in turn explain heterogeneity in voters responses to the information provided. The comparison of mean characteristics in Table A23 indicates that the two types of municipalities are relatively similar in terms of population, age distribution, religion, and prior election outcomes. However, the 17 municipalities that received information about unauthorized spending have higher levels of development and smaller indigenous populations than the 9 municipalities that received information about spending that did not benefit the poor. To ensure that the dimension of malfeasance reported is not proxying for such potential confounds, we adjust for the interaction between treatment and a (demeaned) indicator for the dimension of malfeasance that is reported. Although the precision of our estimates declines, the results in Table A24 show similar point estimates. This indicates that our main findings

Table A20: Alternative explanations for the positive average effect of the information treatment on the incumbent party's vote share

	Incumbent party vote share			
	(share of turnout)		(share of registered voters)	
	(1)	(2)	(3)	(4)
Information treatment	0.015849*	0.020323***	0.004950	0.006682***
	(0.008398)	(0.004763)	(0.004608)	(0.002459)
× FISM pesos received (millions)	0.000059		0.000048	
	(0.000088)		(0.000047)	
× FISM pesos received per voter (1000s)		-0.001243		0.002824
		(0.003713)		(0.002242)
Outcome range	[0.07,0.71]	[0.07,0.71]	[0.03,0.44]	[0.03,0.44]
Control outcome mean	0.38	0.38	0.19	0.19
Control outcome std. dev.	0.12	0.12	0.07	0.07
Interaction range	[10,146.3]	[0.08,3.08]	[10,146.3]	[0.08,3.08]
Interaction mean	64.63	0.49	64.63	0.49
Interaction std. dev.	37.27	0.66	37.27	0.66
R^2	0.61	0.61	0.62	0.62
Observations	675	675	675	675

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A21: Effect of information treatment on the importance of different factors determining a respondent's vote choice

	Importance attached to characteristic		
	(1)	(2)	(3)
Panel A: Candidate's honesty			
Information treatment	0.014	0.011	0.027
	(0.033)	(0.059)	(0.065)
× Absolute updating		0.003	
		(0.035)	
× Share malfeasance spending			-0.062
			(0.190)
Outcome range	{1,2,3,4,5}	{1,2,3,4,5}	{1,2,3,4,5}
Control outcome mean	4.04	4.04	4.04
Control outcome std. dev.	1.22	1.22	1.22
Interaction range		[0,2.7]	[0,0.58]
Interaction mean		1.04	0.21
Interaction std. dev.		0.86	0.17
R^2	0.06	0.06	0.06
Observations	4,674	4,674	4,674
Panel B: Candidate's policies to address poverty			
Information treatment	0.037	0.054	0.067
	(0.031)	(0.050)	(0.051)
× Absolute updating		-0.016	
		(0.037)	
× Share malfeasance spending			-0.143
			(0.138)
Outcome range	{1,2,3,4,5}	{1,2,3,4,5}	{1,2,3,4,5}
Control outcome mean	4.11	4.11	4.11
Control outcome std. dev.	1.26	1.26	1.26
Interaction range		[0,2.7]	[0,0.58]
Interaction mean		1.04	0.21
Interaction std. dev.		0.86	0.17
R^2	0.07	0.07	0.07
Observations	4,697	4,697	4,697

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A22: Effect of information treatment on incumbent party vote share, by type of malfeasance information received

	Incumbent party vote share (share of turnout)			Incumbent party vote share (share of registered voters)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Municipalities receiving information regarding the share of spending not spent on the poor						
Information treatment	0.014**	0.027***	0.017	0.006*	0.014***	0.016**
	(0.006)	(0.006)	(0.014)	(0.003)	(0.004)	(0.007)
× Incumbent malfeasant spending		-0.063**			-0.039***	
		(0.025)			(0.013)	
× Unfavorable incumbent updating			-0.004			-0.007*
			(0.009)			(0.004)
Outcome range	[0.09,0.85]	[0.09,0.85]	[0.09,0.85]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]
Control outcome mean	0.40	0.40	0.41	0.20	0.20	0.20
Control outcome std. dev.	0.12	0.12	0.11	0.07	0.07	0.06
Interaction range		[0,0.58]	[0.1,2.7]		[0,0.58]	[0.1,2.7]
Interaction mean		0.22	1.62		0.22	1.62
Interaction std. dev.		0.18	0.67		0.18	0.67
R^2	0.54	0.54	0.49	0.54	0.54	0.51
Observations	407	407	383	407	407	383
Panel B: Municipalities receiving information regarding the share of unauthorized spending						
Information treatment	0.028***	0.034***	0.025***	0.011***	0.012*	0.008**
	(0.005)	(0.009)	(0.005)	(0.003)	(0.006)	(0.004)
× Incumbent malfeasant spending		-0.027			-0.006	
		(0.022)			(0.015)	
× Unfavorable incumbent updating			-0.037*			-0.027*
			(0.019)			(0.014)
Outcome range	[0.07,0.71]	[0.07,0.71]	[0.07,0.71]	[0.03,0.44]	[0.03,0.44]	[0.03,0.44]
Control outcome mean	0.35	0.35	0.35	0.19	0.19	0.19
Control outcome std. dev.	0.12	0.12	0.12	0.07	0.07	0.07
Interaction range		[0,0.45]	[-0.6,0.5]		[0,0.45]	[-0.6,0.5]
Interaction mean		0.21	-0.10		0.21	-0.10
Interaction std. dev.		0.15	0.24		0.15	0.24
R^2	0.69	0.69	0.69	0.72	0.72	0.72
Observations	268	268	268	268	268	268

Notes: Panel A includes only precincts from municipalities that received information about the share of spending on projects that did not benefit the poor; panel B includes only precincts from municipalities that received information about the share of unauthorized spending. All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in Columns (3) and (6) reflects the lack of data on prior beliefs about the incumbent party in Apaseo el Alto. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

are robust to exploiting only within-indicator variation in reported malfeasance levels.

Furthermore, since the two dimensions of malfeasance are correlated (albeit negatively), it remains possible that the treatment primed voters to think about the other dimension of corruption without acting on their updated beliefs relating to the indicator that was reported. For example, the leaflets may have caused changes in voting behavior by inducing voters to think about how the government has done a good job at spending resources on projects that benefited the poor, rather than responding to being told that the incumbent engaged in 0% unauthorized spending. This alternative interpretation is relatively unlikely because voters do not appear to possess such information (priors beliefs are not significantly correlated with measures of municipal malfeasance) and because the two indicators are negatively correlated in our sample, but also because there is limited variation in the other dimension (which ranges from 0% to 8% in our sample). Still, to assess this alternative interpretation more formally, we examine the interaction between treatment and each measure of malfeasance simultaneously. The results in Table A25 indicate that the interactions we observe are not altered by including the interaction with the level of malfeasance on the non-reported dimension. Moreover, the estimated coefficient is half the size of the comparable interaction in column (3) and with the opposite sign.

A.9 Robustness tests

A.9.1 Adjusting for lower-order covariates

Since Table A3 documented some imbalances in treatment assignment across precincts, we adjust for the 40 precinct-level pre-treatment covariates to demonstrate that our results are not driven by imbalances that remain after random assignment. Table A26 reports these estimates, showing substantively similar findings that are, if anything, more precisely estimated. While this robustness check pertains to lower-order covariates, Table 7 in the main paper further shows that our findings are robust to including interactions between treatment and covariates that could confound our heterogeneous effects.

A.9.2 Imputing individual and precinct prior beliefs

As described above, we measure prior beliefs for our main estimates—used throughout the tables in the paper—using municipal averages drawn from the post-treatment survey responses of individuals in the control group. This was our pre-specified approach. However, while municipal aggregation offers a fairly precise estimate of prior beliefs given the numerous respondents per municipality, aggregating the data in this way also masks within-municipality heterogeneity

Table A23: Mean characteristics of municipalities that received information about spending that did not benefit the poor and unauthorized spending

	Municipalities that received information about spending that did not benefit the poor (1)	Municipalities that received information about unauthorized spending (2)	Difference (3)
Number of registered voters in 2015	257,137	260,026	-2,889
Share female	0.51	0.51	0.00
Share working age	0.62	0.64	-0.03
Share aged 65+	0.06	0.05	0.00
Share married	0.57	0.55	0.01
Average children per woman	2.57	2.35	0.22
Share of households with male head	0.79	0.77	0.02
Share born out of state	0.18	0.30	-0.12
Share Catholic	0.85	0.88	-0.03
Share non-Catholic Christian	0.09	0.06	0.04
Share non-religious	0.03	0.03	0.00
Share indigenous speakers	0.15	0.03	0.12
Average years of schooling	7.58	8.38	-0.79
Average years of schooling for women	7.43	8.24	-0.80
Average years of schooling for men	7.75	8.53	-0.78
Share illiterate	0.08	0.06	0.02
Share with higher education	0.24	0.32	-0.08
Share disabled	0.04	0.04	0.00
Share economically active	0.37	0.39	-0.02
Share without health care	0.34	0.36	-0.02
Share with state workers health care	0.04	0.05	-0.01
Average occupants per dwelling	4.39	4.11	0.28
Average occupants per room	1.25	1.15	0.09
Share of homes with 2+ rooms	0.65	0.64	0.00
Share of homes with 3+ rooms	0.74	0.74	0.00
Share of homes without a dirt floor	0.90	0.95	-0.05
Share of homes with a toilet	0.94	0.93	0.00
Share of homes with water	0.86	0.90	-0.04
Share of homes with drainage	0.82	0.91	-0.09
Share of homes with electricity	0.96	0.97	-0.01
Share of homes with water, drainage, and electricity	0.74	0.85	-0.10
Share of homes with a washing machine	0.52	0.66	-0.14
Share of homes with a landline telephone	0.32	0.45	-0.13
Share of homes with a radio	0.78	0.83	-0.05
Share of homes with a fridge	0.69	0.80	-0.11
Share of homes with a cell phone	0.50	0.62	-0.12
Share of homes with a television	0.87	0.94	-0.07
Share of homes with a car	0.34	0.41	-0.07
Share of homes with a computer	0.17	0.27	-0.10
Share of homes with internet	0.11	0.18	-0.08
Incumbent party vote share in 2012	0.46	0.42	0.04
Incumbent party vote margin in 2012	0.13	0.11	0.02

Note: All numbers refer to municipal means across the 9 municipalities that received information about spending that did not benefit the poor and the 17 municipalities that received information about unauthorized spending.

Table A24: Effect of information treatment on incumbent party vote share, adjusting for the interaction between treatment and the dimension of malfeasance that was reported to voters

	Incumbent party vote share				
	(1)	(2)	(3)	(4)	(5)
Panel A: Incumbent party vote share (share of turnout)					
Information treatment	0.018***	0.118***	0.031***	0.122***	0.024***
	(0.004)	(0.040)	(0.005)	(0.040)	(0.008)
× Incumbent malfeasant spending			-0.052**	-0.053***	
			(0.020)	(0.016)	
× Incumbent malfeasance prior	-0.002			-0.003	
	(0.012)			(0.010)	
× Incumbent prior precision		-0.031**		-0.029**	
		(0.012)		(0.012)	
× Unfavorable incumbent updating					-0.007
					(0.008)
× Unauthorized spending reported (demeaned)	0.021	0.011*	0.014**	0.016	0.006
	(0.019)	(0.007)	(0.007)	(0.018)	(0.015)
Outcome range	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]
Control outcome mean	0.39	0.39	0.38	0.39	0.39
Control outcome std. dev.	0.12	0.12	0.12	0.12	0.12
R^2	0.59	0.59	0.61	0.59	0.59
Panel B: Incumbent party vote share (share of registered voters)					
Information treatment	0.008***	0.042*	0.014***	0.051**	0.015***
	(0.002)	(0.021)	(0.003)	(0.021)	(0.004)
× Incumbent malfeasant spending			-0.029**	-0.028***	
			(0.011)	(0.009)	
× Incumbent malfeasance prior	0.007			0.006	
	(0.006)			(0.005)	
× Incumbent prior precision		-0.011		-0.012*	
		(0.007)		(0.006)	
× Unfavorable incumbent updating					-0.009**
					(0.004)
× Unauthorized spending reported (demeaned)	-0.003	0.005	0.005	-0.005	-0.009
	(0.010)	(0.004)	(0.004)	(0.010)	(0.008)
Outcome range	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]
Control outcome mean	0.20	0.20	0.19	0.20	0.20
Control outcome std. dev.	0.07	0.07	0.07	0.07	0.07
R^2	0.61	0.61	0.62	0.61	0.61
Interaction range	[-1.4,1.1]	[2.4,3.8]	[0,0.58]		[-0.6,2.7]
Interaction mean	-0.10	3.23	0.21		0.91
Interaction std. dev.	0.83	0.26	0.17		1.00
Observations	651	651	675	651	651

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in Columns (1), (2), and (4) reflect the lack of data on prior beliefs about the incumbent party in Apaseo el Alto. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A25: Effect of information treatment on incumbent party vote share, adjusting for the interaction between treatment and the level of malfeasance that was detected on the dimension that was not reported to voters

	Incumbent party vote share				
	(1)	(2)	(3)	(4)	(5)
Panel A: Incumbent party vote share (share of turnout)					
Information treatment	0.019***	0.113***	0.028***	0.120***	0.025***
	(0.003)	(0.040)	(0.006)	(0.038)	(0.003)
× Incumbent malfeasance prior	0.007*			0.005	
	(0.004)			(0.003)	
× Incumbent prior precision		-0.029**		-0.028**	
		(0.012)		(0.011)	
× Incumbent malfeasant spending			-0.040	-0.044**	
			(0.024)	(0.018)	
× Unfavorable incumbent updating					-0.007**
					(0.003)
× Non-reported incumbent malfeasance spending (demeaned)	0.364***	0.286***	0.267**	0.149	0.336***
	(0.082)	(0.095)	(0.121)	(0.113)	(0.082)
Outcome range	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]
Control outcome mean	0.39	0.39	0.38	0.39	0.39
Control outcome std. dev.	0.12	0.12	0.12	0.12	0.12
R^2	0.59	0.59	0.61	0.59	0.59
Panel B: Incumbent party vote share (share of registered voters)					
Information treatment	0.007***	0.026	0.012***	0.026	0.011***
	(0.002)	(0.023)	(0.003)	(0.022)	(0.002)
× Incumbent malfeasance prior	0.004*			0.003*	
	(0.002)			(0.002)	
× Incumbent prior precision		-0.006		-0.005	
		(0.007)		(0.007)	
× Incumbent malfeasant spending			-0.020	-0.021*	
			(0.013)	(0.011)	
× Unfavorable incumbent updating					-0.004***
					(0.002)
× Non-reported incumbent malfeasance spending (demeaned)	0.242***	0.241***	0.191***	0.174***	0.225***
	(0.045)	(0.047)	(0.042)	(0.054)	(0.046)
Outcome range	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]
Control outcome mean	0.20	0.20	0.19	0.20	0.20
Control outcome std. dev.	0.07	0.07	0.07	0.07	0.07
R^2	0.61	0.61	0.62	0.61	0.61
Interaction range	[-1.4,1.1]	[2.4,3.8]	[0,0.58]		[-0.6,2.7]
Interaction mean	-0.10	3.23	0.21		0.91
Interaction std. dev.	0.83	0.26	0.17		1.00
Observations	651	651	675	651	651

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in Columns (1), (2), and (4) reflect the lack of data on prior beliefs about the incumbent party in Apaseo el Alto. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A26: Effect of information treatment on incumbent party vote share, adjusting for 40 balancing covariates

	Incumbent party vote share (share of registered voters)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Incumbent party vote share (share of turnout)						
Information treatment	0.015***	0.014***	0.165***	0.027***	0.153***	0.023***
	(0.004)	(0.004)	(0.048)	(0.006)	(0.040)	(0.006)
× Incumbent malfeasance prior		0.010**			0.006*	
		(0.004)			(0.004)	
× Incumbent prior precision			-0.047***		-0.040***	
			(0.014)		(0.012)	
× Incumbent malfeasant spending				-0.057***	-0.053***	
				(0.021)	(0.015)	
× Unfavorable incumbent updating						-0.011***
						(0.003)
Outcome range	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]
Outcome mean	0.38	0.39	0.39	0.38	0.39	0.39
Outcome std. dev.	0.12	0.12	0.12	0.12	0.12	0.12
R^2	0.68	0.68	0.68	0.68	0.68	0.68
Panel B: Incumbent party vote share (share of registered voters)						
Information treatment	0.005**	0.005**	0.088***	0.012***	0.083***	0.009***
	(0.003)	(0.002)	(0.028)	(0.004)	(0.026)	(0.003)
× Incumbent malfeasance prior		0.005*			0.003	
		(0.003)			(0.003)	
× Incumbent prior precision			-0.026***		-0.022***	
			(0.009)		(0.008)	
× Incumbent malfeasant spending				-0.030**	-0.029***	
				(0.013)	(0.010)	
× Unfavorable incumbent updating						-0.005**
						(0.002)
Outcome range	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]
Outcome mean	0.19	0.20	0.20	0.19	0.20	0.20
Outcome std. dev.	0.07	0.07	0.07	0.07	0.07	0.07
R^2	0.69	0.68	0.68	0.69	0.69	0.68
Interaction range		[-1.4,1.1]	[2.4,3.8]	[0,0.58]		[-0.6,2.7]
Interaction mean		-0.10	3.23	0.21		0.91
Interaction std. dev.		0.83	0.26	0.17		1.00
Observations	675	651	651	675	651	651

Notes: All specifications include block fixed effects and the 40 precinct-level covariates from Table A3 (uninteracted), and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in Columns (2), (3), and (5) reflect the lack of data on prior beliefs about the incumbent party in Apaseo el Alto. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

that could account for differences across individuals or precincts in response to treatment. In particular, the aggregation may downwardly bias our heterogeneous effects estimates by failing to capture variation in prior beliefs across precincts, as our econometric discussion in section A.4.3 demonstrates.

To allow prior beliefs to vary across individuals and precincts, we use prediction models to impute more fine-grained prior beliefs and belief updating induced by treatment. Our prediction models follow our main measurement approach in relying only on control group survey responses, to ensure that beliefs are not driven by treatment, but further leverage a variety of individual- and precinct-level predictors that may capture variation in beliefs. In particular, we estimated regressions of the form:

$$Y_{ipm} = \alpha_m + \beta P_{pm} + \gamma R_{ipm} + \varepsilon_{ipm}, \tag{A24}$$

where α_m are municipality fixed effects, P_{pm} is a vector of precinct-level covariates, and R_{ipm} is a vector of individual-level covariates (used only for imputing individual-level beliefs). The coefficients from these specifications are then used to predict prior beliefs in both the control and treatment groups based on the values of the covariates for a given individual or precinct.

At the individual level, we use observations from the control group to predict malfeasance prior beliefs, the precision of those beliefs, and belief updating for the entire sample using the predicted values from a regression of survey responses for these outcomes on municipality fixed effects and 14 precinct-specific covariates that may shape prior beliefs and 10 individual-level covariates.[5] While the prior belief and updating prediction models explain more than 30% of variation in these individual outcomes, the prior precision model only explains 15% of variation. For our analysis of electoral returns, we similarly use control group observations to predict precinct-level malfeasance priors, prior precision, and belief updating for the entire sample using the predicted values generated from a regression of the precinct-level average belief on municipality fixed effects and the same 14 precinct-specific covariates that may shape prior beliefs. Each of the three regressions

[5]The individual-level covariates are: an indicator for knowing the length of a mayor's term in office, an indicator for knowing which party the incumbent before the election was from, an indicator for knowing which party won the election, four five-point scales capturing the regularity of news consumption about the election campaigns in the month before the elections via television, radio, newspapers, or the internet, an indicator for being a woman, age, fixed effects for different levels of education, and indicator for being employed. The precinct-level covariates are: share that would have been treated, share of leaflets expected to be delivered by hand, distance from the municipality's centroid, an indicator for being rural, population density, the number of local media outlets that cover the precinct, the share of individuals with televisions, the share of individuals with access to internet at home, average years of schooling, the share of the population above 15 that is illiterate, average occupants per room, the share of the population with electricity, running water, and drainage at home, the incumbent party vote share at the previous election, and turnout at the previous election.

explains at least 57% of the variation in the control group outcomes at the precinct level.

The results using these more fine-grained imputed prior beliefs to exploit variation both within and across municipalities are similar to the results for the municipal-level control averages. Table A27 shows that our findings are qualitatively unaffected by including predicted individual-level priors. In fact, the point estimates notably increase in magnitude for the prior and updating variables, suggesting that the prediction model may have alleviated measurement error arising from the municipal-level aggregation. Panel A of Tables 7 and A28 further shows that the main results are also robust at the precinct level. Here, the point estimates are similar in magnitude to our main estimates, suggesting that the precinct-level measurement error is less of an issue for estimation. The unconditional effect and the interaction with the share of malfeasant spending are omitted from these tables because neither leverages variation in beliefs.

A.9.3 Robustness to alternative vote share denominator

Table A28 presents the robustness checks reported in Table 7 instead using incumbent vote share as a share of registered voters, as the outcome.

A.9.4 Interaction with the share of households treated

As noted in the main paper, one robustness check weights observations by the share of households that received (or, in the control group, would have received) a leaflet to account for the fact that the intensity of treatment was lower in precincts with many households. An alternative approach is to examine how the unweighted estimates vary with the (standardized) share of households that (would have) received a leaflet. While the triple interaction coefficients vary in their precision, the results in Table A29 consistently show that the average and heterogeneous effects increase in magnitude where a larger share of households received a leaflet.

A.9.5 Splitting the sample for the individual-level results

One issue with our proxy for prior beliefs used specifically for the results in column (2) of Table 3 is that posterior belief outcomes in the control group are almost perfectly explained by the regressor measuring the municipality-level average malfeasance prior beliefs in the control group. This is because the municipality-level proxy for prior beliefs is constructed as the average outcome in the control group. It is important to reiterate that this concern only applies when considering posterior beliefs as an outcome together with examining heterogeneous effects by priors beliefs, and consequently our main estimates focusing on vote shares as an outcome are not affected by

Table A27: Effect of information treatment on voters' posterior beliefs about incumbent party malfeasance, using predicted individual-level prior beliefs and updating

	Perceived incumbent party malfeasance (very low - very high)			
	(1)	(2)	(3)	(4)
Information treatment	-0.018	0.318	0.679	-0.135**
	(0.045)	(0.378)	(0.416)	(0.057)
× Incumbent malfeasance prior	-0.228***		-0.238***	
	(0.038)		(0.039)	
× Incumbent prior precision		-0.097	-0.204	
		(0.116)	(0.123)	
× Incumbent malfeasant spending			-0.144	
			(0.212)	
× Unfavorable incumbent updating				0.160***
				(0.033)
Outcome range	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}
Control outcome mean	-0.12	-0.12	-0.12	-0.12
Control outcome std. dev.	1.48	1.48	1.48	1.48
Interaction range	[-2.3,2.0]	[2.2,4.2]		[-2.5,4.0]
Interaction mean	-0.13	3.28		0.97
Interaction std. dev.	0.90	0.31		1.11
R^2	0.30	0.30	0.30	0.30
Observations	4,447	4,438	4,438	4,447

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A28: Robustness of information treatment on incumbent party vote share (share of registered voters)

	Incumbent party vote share (share of registered voters)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Predicted precinct-level prior beliefs and updating						
Information treatment	0.008***	0.008***	0.041	0.014***	0.034	0.012***
	(0.002)	(0.002)	(0.027)	(0.003)	(0.023)	(0.003)
× Incumbent malfeasance prior (predicted)		0.005*			0.004*	
		(0.003)			(0.002)	
× Incumbent prior precision (predicted)			-0.010		-0.006	
			(0.008)		(0.007)	
× Incumbent malfeasant spending				-0.029**	-0.031***	
				(0.013)	(0.010)	
× Unfavorable incumbent updating (predicted)						-0.005**
						(0.002)
Panel B: Adjusting for (demeaned) precinct-level covariates interacted with information treatment						
Information treatment	0.007***	0.007***	0.026	0.015***	0.042	0.012***
	(0.002)	(0.002)	(0.029)	(0.003)	(0.028)	(0.003)
× Incumbent malfeasance prior		0.004			0.004	
		(0.002)			(0.003)	
× Incumbent prior precision			-0.006		-0.008	
			(0.009)		(0.009)	
× Incumbent malfeasant spending				-0.035**	-0.039***	
				(0.014)	(0.013)	
× Unfavorable incumbent updating						-0.005***
						(0.002)
Panel C: Adjusting for (demeaned) municipal-level covariates interacted with information treatment						
Information treatment	0.008***	0.007***	0.007	0.018***	0.011	0.012***
	(0.002)	(0.002)	(0.025)	(0.003)	(0.020)	(0.004)
× Incumbent malfeasance prior		0.002			-0.002	
		(0.005)			(0.004)	
× Incumbent prior precision			0.000		0.002	
			(0.008)		(0.006)	
× Incumbent malfeasant spending				-0.045***	-0.050***	
				(0.009)	(0.008)	
× Unfavorable incumbent updating						-0.005
						(0.003)
Panel D: Unweighted precinct estimates						
Information treatment	0.013***	0.012***	0.084**	0.022***	0.067**	0.018***
	(0.003)	(0.003)	(0.035)	(0.004)	(0.027)	(0.003)
× Incumbent malfeasance prior		0.007***			0.005**	
		(0.002)			(0.002)	
× Incumbent prior precision			-0.022**		-0.014*	
			(0.011)		(0.008)	
× Incumbent malfeasant spending				-0.043***	-0.042***	
				(0.015)	(0.012)	
× Unfavorable incumbent updating						-0.007***
						(0.002)

Notes: All specifications include block fixed effects, and are estimated using OLS. See text for interactive covariates included in panels B and C. Observations in panel D are weighted by the share of the precinct that was treated. Lower-order interaction terms are omitted. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A29: Heterogeneous effects of information treatment on incumbent party vote share, by (standardized) share of households that (would have) received a leaflet

	Incumbent party vote share					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Incumbent party vote share (share of turnout)						
Information treatment	0.020***	0.019***	0.100	0.034***	0.039	0.027***
	(0.004)	(0.004)	(0.075)	(0.006)	(0.066)	(0.006)
× Share received	0.018**	0.016*	-0.032	0.038**	-0.114	0.022**
	(0.009)	(0.009)	(0.118)	(0.016)	(0.116)	(0.010)
× Incumbent malfeasance prior		0.008			0.008*	
		(0.005)			(0.004)	
× Incumbent malfeasance prior × Share received		0.009			0.012	
		(0.009)			(0.011)	
× Incumbent prior precision			-0.025		-0.001	
			(0.023)		(0.020)	
× Incumbent prior precision × Share received			0.016		0.048	
			(0.037)		(0.037)	
× Incumbent malfeasant spending				-0.065***	-0.075***	
				(0.023)	(0.017)	
× Incumbent malfeasant spending × Share received				-0.100	-0.120**	
				(0.061)	(0.059)	
× Unfavorable incumbent updating						-0.009**
						(0.004)
× Unfavorable incumbent updating × Share received						-0.008
						(0.007)
Panel B: Incumbent party vote share (share of registered voters)						
Information treatment	0.008***	0.007***	0.038	0.015***	0.001	0.012***
	(0.002)	(0.002)	(0.044)	(0.003)	(0.040)	(0.003)
× Share received		0.011*	0.025	0.023**	-0.033	0.017**
		(0.006)	(0.075)	(0.010)	(0.072)	(0.007)
× Incumbent malfeasance prior		0.004			0.005*	
		(0.003)			(0.003)	
× Incumbent malfeasance prior × Share received		0.008			0.010	
		(0.006)			(0.007)	
× Incumbent prior precision			-0.010		0.005	
			(0.013)		(0.012)	
× Incumbent prior precision × Share received			-0.004		0.018	
			(0.024)		(0.024)	
× Incumbent malfeasant spending				-0.034**	-0.040***	
				(0.013)	(0.010)	
× Incumbent malfeasant spending × Share received				-0.056	-0.062	
				(0.035)	(0.037)	
× Unfavorable incumbent updating						-0.005***
						(0.002)
× Unfavorable incumbent updating × Share received						-0.008*
						(0.004)

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are omitted to save space. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A30: Effect of information treatment on voters' posterior beliefs about incumbent party malfeasance, splitting the sample between municipalities with above- and below-median priors

	Perceived incumbent party malfeasance (very low - very high)		
	Above-median incumbent malfeasance prior	Below-median incumbent malfeasance prior	Pooled
	(1)	(2)	(3)
Information treatment	-0.067	0.062	0.062
	(0.040)	(0.066)	(0.065)
Information treatment × Above-median incumbent malfeasance prior			-0.128*
			(0.076)
Outcome range	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}
Control outcome mean	0.63	-0.80	-0.14
Control outcome std. dev.	1.30	1.29	1.48
R^2	0.13	0.05	0.29
Observations	2,321	2,303	4,624

Notes: All specifications include block fixed effects, and are estimated using OLS. See text for interacted controls included. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

this potential concern.

To examine the robustness of the result in column (2) of Table 3, we separately examine the average treatment effect using subsamples split between municipalities with above- and below-median prior beliefs (i.e. with a malfeasance prior score above and below -0.18). By splitting the sample in this way, we no longer include the municipality-level proxy for prior beliefs as an interacted regressor. The results in Table A30 support the main findings, showing that treated voters in municipalities where the control group perceived above (below)-sample mean incumbent malfeasance became less (more) likely to believe that the incumbent is malfeasant. The relatively large effects, which column (3) shows to be statistically significantly different, suggest that they are unlikely to reflect potential measurement error in terms of which municipalities are classified as above and below the sample median.

Table A31: Effect of information treatment on turnout, weighting by the (expected) share of the precinct that received a leaflet

	Turnout			
	(1)	(2)	(3)	(4)
Information treatment	-0.003	-0.003	0.010*	0.010*
	(0.004)	(0.006)	(0.006)	(0.006)
× Incumbent malfeasance spending		-0.001	-0.187***	
		(0.018)	(0.057)	
× Incumbent malfeasance spending squared			0.364***	
			(0.108)	
× Incumbent malfeasance spending quartile 2				-0.014
				(0.008)
× Incumbent malfeasance spending quartile 3				-0.032***
				(0.008)
× Incumbent malfeasance spending quartile 4				-0.000
				(0.006)
Outcome range	[0.21,0.79]	[0.21,0.79]	[0.21,0.79]	[0.21,0.79]
Control outcome mean	0.51	0.51	0.51	0.51
Control outcome std. dev.	0.10	0.10	0.10	0.10
Interaction range		[0,0.58]	[0,0.58]	
Interaction mean		0.22	0.22	
Interaction std. dev.		0.17	0.17	
R^2	0.70	0.70	0.70	0.70
Observations	675	675	675	675

Notes: All specifications include block fixed effects, and are estimated using OLS. Observations are weighted by the share of the precinct that was treated. Lower-order interaction terms are absorbed by the block fixed effects. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

A.9.6 Turnout estimates weighting by the (expected) share of the precinct that received a leaflet

A.10 The effect of treatment variants

Tables A32 and A33 report the precinct-level estimates distinguishing each of our four treatment configurations. As noted in greater detail in Arias et al. (2018), the results show that the treatment variants—public and comparative information—did not produce qualitatively different effects.

Table A32: Effect of information treatment variants on incumbent party vote share (share of turnout)

	Incumbent party vote share (share of turnout)					
	(1)	(2)	(3)	(4)	(5)	(6)
Private local treatment	0.024***	0.023***	0.110	0.038**	0.130	0.023**
	(0.008)	(0.008)	(0.106)	(0.014)	(0.093)	(0.009)
Public local information treatment	0.003	0.004	0.153	0.017	0.065	0.031***
	(0.012)	(0.009)	(0.154)	(0.021)	(0.103)	(0.010)
Private comparative information treatment	0.025***	0.024**	0.012	0.051***	0.003	0.038***
	(0.009)	(0.009)	(0.091)	(0.013)	(0.089)	(0.011)
Public comparative information treatment	0.027**	0.025**	0.311**	0.018	0.348***	0.014
	(0.010)	(0.009)	(0.147)	(0.015)	(0.120)	(0.011)
Private local × Incumbent malfeasance prior		-0.002			-0.005	
		(0.008)			(0.008)	
Public local× Incumbent malfeasance prior		0.040***			0.038***	
		(0.011)			(0.010)	
Private comparative × Incumbent malfeasance prior		0.013			0.011	
		(0.011)			(0.009)	
Public comparative × Incumbent malfeasance prior		-0.016**			-0.023***	
		(0.008)			(0.007)	
Private local × Incumbent prior precision			-0.027		-0.029	
			(0.033)		(0.030)	
Public local× Incumbent prior precision			-0.047		-0.015	
			(0.048)		(0.034)	
Private comparative × Incumbent prior precision			0.003		0.014	
			(0.029)		(0.027)	
Public comparative × Incumbent prior precision			-0.088*		-0.103***	
			(0.045)		(0.036)	
Private local × Incumbent malfeasant spending				-0.066	-0.068	
				(0.047)	(0.049)	
Public local × Incumbent malfeasant spending				-0.065	-0.051	
				(0.074)	(0.046)	
Private comparative × Incumbent malfeasant spending				-0.121***	-0.119***	
				(0.040)	(0.034)	
Public comparative × Incumbent malfeasant spending				0.043	0.035	
				(0.064)	(0.038)	
Private local × Unfavorable incumbent updating						-0.000
						(0.007)
Public local× Unfavorable incumbent updating						-0.033***
						(0.010)
Private comparative × Unfavorable incumbent updating						-0.017*
						(0.009)
Public comparative × Unfavorable incumbent updating						0.014*
						(0.008)
Outcome range	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]	[0.07,0.85]
Control outcome mean	0.38	0.39	0.39	0.38	0.39	0.39
Control outcome std. dev.	0.12	0.12	0.12	0.12	0.12	0.12
Interaction range		[-1.4,1.1]	[2.4,3.8]	[0,0.58]		[-0.6,2.7]
Interaction mean		-0.10	3.23	0.21		0.91
Interaction std. dev.		0.83	0.26	0.17		1.00
R^2	0.61	0.61	0.60	0.62	0.62	0.61
Observations	675	651	651	675	651	651

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in Columns (2), (3), and (5) reflect the lack of data on prior beliefs about the incumbent party in Apaseo el Alto. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A33: Effect of information treatment variants on incumbent party vote share (share of registered voters)

	Incumbent party vote share (share of registered voters)					
	(1)	(2)	(3)	(4)	(5)	(6)
Private local treatment	0.011***	0.011**	0.083	0.017**	0.092*	0.011*
	(0.004)	(0.004)	(0.053)	(0.008)	(0.049)	(0.006)
Public local information treatment	-0.003	-0.001	0.029	0.003	-0.026	0.012*
	(0.006)	(0.005)	(0.086)	(0.010)	(0.067)	(0.007)
Private comparative information treatment	0.011**	0.009*	-0.002	0.026***	-0.006	0.018**
	(0.005)	(0.005)	(0.054)	(0.008)	(0.046)	(0.008)
Public comparative information treatment	0.012**	0.011**	0.106	0.012	0.126*	0.007
	(0.005)	(0.005)	(0.074)	(0.008)	(0.066)	(0.006)
Private local × Incumbent malfeasance prior		-0.001			-0.003	
		(0.004)			(0.005)	
Public local× Incumbent malfeasance prior		0.022***			0.022***	
		(0.005)			(0.005)	
Private comparative × Incumbent malfeasance prior		0.007			0.006	
		(0.006)			(0.006)	
Public comparative × Incumbent malfeasance prior		-0.008*			-0.010***	
		(0.004)			(0.004)	
Private local × Incumbent prior precision			-0.022		-0.023	
			(0.016)		(0.016)	
Public local× Incumbent prior precision			-0.010		0.009	
			(0.027)		(0.021)	
Private comparative × Incumbent prior precision			0.003		0.009	
			(0.017)		(0.014)	
Public comparative × Incumbent prior precision			-0.029		-0.036*	
			(0.023)		(0.020)	
Private local × Incumbent malfeasant spending				-0.027	-0.029	
				(0.024)	(0.026)	
Public local × Incumbent malfeasant spending				-0.024	-0.016	
				(0.038)	(0.024)	
Private comparative × Incumbent malfeasant spending				-0.068***	-0.068***	
				(0.024)	(0.021)	
Public comparative × Incumbent malfeasant spending				0.002	-0.002	
				(0.031)	(0.019)	
Private local × Unfavorable incumbent updating						-0.000
						(0.004)
Public local× Unfavorable incumbent updating						-0.017***
						(0.005)
Private comparative × Unfavorable incumbent updating						-0.010**
						(0.005)
Public comparative × Unfavorable incumbent updating						0.006
						(0.004)
Outcome range	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]	[0.03,0.47]
Control outcome mean	0.19	0.20	0.20	0.19	0.20	0.20
Control outcome std. dev.	0.07	0.07	0.07	0.07	0.07	0.07
Interaction range		[-1.4,1.1]	[2.4,3.8]	[0,0.58]		[-0.6,2.7]
Interaction mean		-0.10	3.23	0.21		0.91
Interaction std. dev.		0.83	0.26	0.17		1.00
R^2	0.62	0.62	0.61	0.62	0.63	0.62
Observations	675	651	651	675	651	651

Notes: All specifications include block fixed effects, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in Columns (2), (3), and (5) reflect the lack of data on prior beliefs about the incumbent party in Apaseo el Alto. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.