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LATIN AMERICAN ECONOMIC REVIEW

Volume 34: Issue on Experimental Economics | Article 7 | 2025

Does the content and mode of delivery of information matter for electoral accountability? Evidence from a field experiment in Mexico*

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Submission received: March 14, 2024
Final version received: February 22, 2025
Accepted: March 7, 2025
Published: May 9, 2025

Abstract

Evidence that information campaigns help citizens elect better politicians is mixed. We investigate whether comparative performance information and public dissemination can enhance information's effects on electoral accountability, by respectively helping citizens to identify malfeasance by incumbent parties and facilitating coordination around the information provided. We test these mechanisms using a large-scale field experiment that provided citizens with the results of audit reports documenting mayoral malfeasance before the 2015 Mexican municipal elections. Although citizens used incumbent performance indicators to hold governments to account, we find that neither benchmarking incumbent performance against mayors from other parties within the state, nor accompanying leaflet delivery with loudspeakers announcing the leaflets' delivery, significantly moderated the effects of information on citizen beliefs or incumbent party vote share. Comparative performance information's ineffectiveness likely reflected citizens' limited updating from the particular comparison provided, while the loudspeaker generated somewhat greater common knowledge without meaningfully facilitating voter coordination. The results highlight challenges in designing informational campaigns to capture the theoretical conditions conducive to electoral accountability.

Keywords: elections; experiments; information; malfeasance; Mexico; political economy

JEL: D72, D83.

*We thank the steering committee and other team members of the EGAP Metaketa initiative for illuminating discussions and useful comments. We are extremely grateful to Anais Anderson, Adriana Paz, and Alejandra Rogel, and the Data OPM and Qué Funciona para el Desarrollo teams for their implementation of this project, as well as to Juan Carlos Cano Martínez, Executive Secretary of the Guanajuato Electoral Institute, for his assistance in responding to municipal governments that tried to prevent our treatment's dissemination. Ivan San Miguel provided excellent research assistance. We thanks Tommaso Nannicini, Francesco Trebbi, Frederico Finan, and Laura Schechter for sharing their survey instruments. This research was financed by the EGAP Metaketa initiative, and was approved by the Harvard Committee on the Use of Human Subjects (15-1068) and the New York University Committee on Activities Involving Human Subjects (15-10587). Larreguy gratefully acknowledges funding from the French Agence Nationale de la Recherche under the Investissement d'Avenir program ANR-17-EURE-0010. Our pre-analysis plan was pre-registered with EGAP, and is publicly available at http://egap.org/registration/760.

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1. Introduction

Theoretical models of political accountability suggest that incumbent performance information is essential for enabling citizens to identify and elect desirable politicians (Fearon, 1999; Manin et al., 1999; Rogoff, 1990). Such electoral accountability is especially important in developing contexts where weak political institutions may otherwise fail to constrain corruption, incompetence, and clientelism (Pande, 2011). However, the existing experimental and quasi-experimental evidence that informing citizens of poor performance results in electoral sanctioning is markedly mixed. On one hand, some studies—often disseminating information via broadcast or print media—report that the revelation of relatively good performance is indeed rewarded, while sufficiently bad performance results in electoral sanctions (Banerjee et al., 2011; Enríquez et al., 2024; Humphreys and Weinstein, 2012; Larreguy et al., 2020; Marshall, 2023). On the other hand, a number of other studies observe little effect of providing information (de Figueiredo et al., 2023; Dunning et al., 2019; Incerti, 2020), or even that poor incumbent performance disproportionately harms challengers when citizens disengage (Chong et al., 2015).

In this article, we test two potentially critical theoretical mechanisms that could help to account for the varying efficacy of informational interventions: the absence of benchmarked performance indicators across similar locations; and the use of public, rather than private, modes of information dissemination. While both components are frequently touted as central ingredients in supporting electoral accountability, and are often included as part of multifaceted information campaigns, we are aware of no prior study that experimentally separates the causal contributions of each component.

First, if citizens only receive information pertaining to their incumbent's performance (what we call *local* performance information), they may struggle to distinguish their incumbent's aptitude from common shocks, such as budgetary reforms, that affect the performance of all incumbents holding similar offices at the same time. This could lead naive voters to excessively punish (reward) bad (good) performance, or Bayesian voters to largely disregard performance signals driven by factors beyond their particular incumbent's control. However, by providing a relevant cross-sectional benchmark, our simple theoretical model shows how *comparative* performance information may influence voters through two channels: (a) enabling voters to more accurately evaluate their incumbent's performance by filtering out the common component of performance that is expected from both incumbent and challenger parties; and (b) directly informing voters about other parties' performance (Aytaç, 2018; Besley, 2006; Holmstrom, 1982; Kayser and Peress, 2012; Meyer and Vickers, 1997). For example, suppose that municipalities governed by other parties performed better than expected. We then expect providing citizens with comparative performance information will lead to less favorable evaluations of the incumbent's competence than when only local performance information is provided because Bayesian citizens would conclude that there was a more favorable common shock than they would otherwise have believed. Furthermore, to the extent that comparison units are perceived to resemble local challenger parties, better-than-expected performance by other parties will differentially decrease incumbent vote share, relative to non-benchmarked incumbent performance information.

Second, theories of citizen coordination suggest that informational interventions may fail if citizens do not know that other citizens have also received such information or do not expect other citizens to change their voting behavior accordingly. Consequently, public dissemination of performance information could induce tacit coordination among voters by establishing greater common knowledge or induce explicit coordination by stimulating discussion within communities (Bhandari et al., 2023; Chwe, 2000; Cornand and Heinemann, 2008; Enríquez et al., 2024; Morris and Shin, 2002), and thus amplify the effects of information dissemination on electoral accountability. Without such coordination, there may be little incentive for individual citizens to forgo clientelistic benefits to hold politicians to account. Alternatively, by increasing attention or discussion, public dissemination could increase the likelihood that citizens receive and engage with performance indicators. Both the coordination and diffusion mechanisms are often attributed to mass media (Adena et al.,

2015; Arias, 2019; Enikolopov et al., 2020; Manacorda and Tesei, 2020; Yanagizawa-Drott, 2014), which may explain why extant mass dissemination studies generally report larger accountability-enhancing effects.

To test the relevance of these potential moderators of the effects of information campaigns, we conducted a large-scale field experiment around Mexico's 2015 municipal elections. We randomly varied the *content* and *delivery mode* of leaflets containing incumbent performance information—relating to mayoral malfeasance—that were disseminated in treated electoral precincts. The experiment, which was conducted across 678 electoral precincts from 26 municipalities in four states of central Mexico, informed registered voters of independent audit reports documenting unauthorized spending and illegal misallocation of funds to municipal projects not benefiting impoverished localities. Beyond the door-to-door delivery of leaflets reporting on the performance of the incumbent mayor, we randomized two additional components of the intervention that further: (i) compared the incumbents' performance to the average performance of mayors from other parties within their state (comparative treatment); and (ii) accompanied leaflet delivery with a loudspeaker announcing that the leaflets were being delivered throughout the community (public treatment).

We build on Arias et al. (2022) analysis of the same experiment, which focused on demonstrating how citizens' priors beliefs moderate their responses to receiving *any* of these information treatments. Importantly for interpreting our findings, Arias et al. (2022) showed that informing citizens about mayoral malfeasance *increased* the incumbent party's vote share on average, and especially where performance exceeded prior expectations. Since most citizens had highly pessimistic prior beliefs, revealing even severe levels of mayoral malfeasance did not worsen—and in rare cases even improved—citizens' perceptions of the incumbent party. However, Arias et al. (2022) pool across all treatment conditions and thus estimate the effect of receiving any information, rather than separately estimating the effect of each form of information delivery. Their findings may then mask substantial heterogeneity across treatment conditions. Instead, this article's contribution is to explore how benchmarked and publicly disseminated incumbent performance information differentially shapes information provision's impact on citizens' posterior beliefs and voting behavior. To our knowledge, this article is the first to experimentally unpack the roles of cross-sectional benchmarks and common knowledge signals in examining the effects of incumbent performance information on accountability and electoral outcomes.

However, we find little evidence that either variant of the basic information treatment differentially influenced voting behavior. Failing to support our pre-registered theoretical expectations, comparative performance information—that generally detailed lower-than-expected levels of malfeasance among other parties within the state—was not more likely to decrease the vote share for the incumbent party than simply providing incumbent-only information, even when challengers' performance notably exceeded expectations. Our survey data indicate that citizens did not differentially update their beliefs from such comparative information, suggesting that they either did not understand the benchmark or did not regard it as relevant. Similarly, we find no evidence that public dissemination amplified, or otherwise moderated, the effect of distributing leaflets or that the loudspeaker enhanced citizen coordination.

We can rule out even relatively small effects of adding cross-sectional benchmarks and loudspeaker announcements to incumbent-only performance information. Specifically, the 95% confidence interval around our differential treatment effect estimate indicates that providing comparative information did not alter the incumbent's vote share by more than 2 percentage points relative to providing a treatment without benchmarked information—a maximum difference of less than 0.24 standard deviations of the dependent variable. Similarly, the 95% confidence interval around the estimated differential effect of the loudspeaker shows that it did not alter the incumbent's vote share by more than 0.1 percentage points compared to a privately-delivered treatment—a difference of less than 0.05 standard deviations.

Our primary contribution is thus to show that these theoretically-motivated interventions might be insufficient to break voters out of low-accountability political equilibria. As Khemani et al. (2016) optimistically

argue, informed participation on the part of citizens has the potential to "make politics work for development" by changing the incentives for politicians to serve their constituents and helping citizens to select those most likely and able to do so. Despite their theoretical promise in mitigating these agency problems, we find little evidence to suggest that either of our interventions—which are somewhat common in election campaigns and could be easily scaled—meaningfully altered voter behavior. These rather pessimistic findings—which challenge widely-held assumptions about how information influences electoral accountability—suggest that information interventions may need to be more specifically targeted to ensure their relevance and comprehensibility, while more powerful and larger-scale interventions may be required to induce coordinated efforts. Nevertheless, our null findings advance the study of accountability and inform the design of future information dissemination campaigns by highlighting types of interventions unlikely to moderate the effects of simply providing incumbent performance indicators.

2. Theoretical framework

An influential theoretical literature argues that signals of incumbent performance can update citizen beliefs, and thereby help voters to prospectively identify and elect competent politicians more likely to represent their interests (Fearon, 1999; Rogoff, 1990), while also potentially mitigating moral hazard once in office (Barro, 1973; Ferejohn, 1986).[1] Several studies provide empirical evidence consistent with such a belief updating channel (e.g. Arias et al., 2022; Banerjee et al., 2011; Bhandari et al., 2023; Ferraz and Finan, 2008; Humphreys and Weinstein, 2012; Kendall et al., 2015). In this article, we extend the learning framework to incorporate the provision of comparative performance information and the public dissemination of performance information. The following subsections discuss the theoretical rationale for these extensions to the basic Bayesian framework, and state the implications for citizen beliefs and voting behavior that we pre-registered ahead of implementing our experiment.

2.1. Comparative performance information

We consider the role of comparative performance information—in our setting, the provision of information that compares the incumbent party's observed malfeasance to that of potential challenger parties that are incumbents in comparable municipalities—in the context of a learning framework where government malfeasance reflects: (a) the unobserved underlying malfeasance of an incumbent party, and (b) unobserved factors equally influencing the malfeasance indicators of all incumbents. Our framework characterizes citizens as Bayesians seeking to select the least malfeasant politician on the basis of the information available, and emphasizes the importance of how citizens' prior beliefs relate to indicators of malfeasance for determining the effect of information on support for the incumbent party. This generates our hypotheses regarding how the effect of comparative performance information differs from that of providing information only about the incumbent without a benchmark (local performance information). Appendix A.1 formalizes the theoretical expectations we describe below.

The signal extraction problem for citizens is to separate an incumbent party's propensity to be malfeasant from the effects of "common shocks" that afflict multiple municipalities (Holmstrom, 1982; Meyer and Vickers, 1997). For example, nationwide budgetary shifts, decentralization reforms, bureaucratic quality, or economic pressures could represent common shocks that influence malfeasance indicators in many municipalities without reflecting an individual incumbent party's malfeasance. In the absence of performance benchmarks, citizens' evaluations of the incumbent become less favorable to the extent that reported malfeasance indica-

[1] However, prospective voters may not be able to commit to punishing low effort in office (Fearon, 1999).

tors, adjusted for prior expectations of common shocks affecting all incumbents, exceed prior expectations.

Information about the malfeasance of incumbents from other municipalities gives citizens a second signal of performance related to challengers. This signal most obviously informs citizens about the malfeasance of alternative parties, and how this compares with indicators of incumbent malfeasance. However, it also enables citizens to more accurately update their posterior beliefs about their incumbent party's underlying malfeasance by filtering out their updated belief about the common component of malfeasance driving the observed performance of all incumbents. In particular, if challengers are less malfeasant than citizens expected, then Bayesian citizens will infer that the common shock partly driving the observed malfeasance level of the incumbent is lower than they had expected. Because of this, comparative performance information would thus lead citizens in this example to update more unfavorably about the incumbent than if they had only received a signal of incumbent malfeasance.

Voting behavior then reflects a citizen's beliefs about the *relative* levels of incumbent and challenger party malfeasance. We assume that voters will re-elect the incumbent party to the extent that they believe—conditional on their partisan attachments—that the challenger would be less malfeasant than the incumbent. In addition to updating citizens' beliefs about the incumbent's malfeasance, comparative performance information also updates citizens' beliefs about the challenger's malfeasance. Thus, comparative performance information will induce greater electoral sanctioning of the incumbent, relative to only receiving a signal of incumbent malfeasance, when such information induces voters to update favorably about the challenger.[2]

This theoretical framework implies several testable implications. We focus on comparing the effects of comparative and local (i.e. incumbent-only) information, rather than on the overall effect of these treatments relative to the control group.[3] As detailed in Section 4, the typical citizen in our sample received information revealing significant incumbent malfeasance in comparison with zero or low malfeasance among challengers (municipalities governed by different parties within the same state). Applied to our framework, we assumed—at the point of pre-registering our hypotheses—that indicators of incumbent malfeasance would exceed prior expectations, while indicators of challenger malfeasance would fall below prior expectations. We thus conjectured that, for the average citizen, the provision of comparative information would lead them to update relatively more unfavorably about the incumbent party than the provision of local information. More concretely:

H1. *If information about incumbent malfeasance exceeds citizens' expectations while information about the challenger falls below a citizen's expectations, comparative malfeasance information will, on average, increase a citizen's posterior belief that the incumbent is malfeasant and decrease their posterior belief that the challenger is malfeasant relative to local malfeasance information.*

Furthermore, we expected to observe the following heterogeneous effects that account for the extent of the deviation from a citizen's prior beliefs:

H2. *The effect of comparative malfeasance information on a citizen's posterior beliefs about the incumbent's (challenger's) malfeasance, relative to only providing local malfeasance information, will be decreasing (increasing) in the difference between reported challenger (incumbent) party malfeasance and the citizen's prior expectations of such malfeasance.*

If hypotheses H1 and H2 hold, we then expect belief updating to, in turn, affect voting behavior. Based on

[2]To the extent that a benchmarked signal increases the precision of citizens' posterior beliefs, there is also a second-order that amplifies voter sanctioning of incumbent malfeasance which exceeds expectations.

[3]Appendix A.1 also derives testable implications for the effect of comparative information relative to a no-information control group.

the content of the information that voters would receive, we conjectured the following hypotheses pertaining to the average electoral effects of providing comparative performance information:

H3. *If information about incumbent malfeasance exceeds voters' expectations while information about the challenger falls below voters' expectations, comparative malfeasance information will, on average, decrease the incumbent party's vote share relative to local malfeasance information*

H4. *The effect of comparative malfeasance information on incumbent party vote share, relative to only providing local malfeasance information, will be increasing in the difference between reported challenger party malfeasance and citizens' prior expectation of such malfeasance.*

Previous non-experimental studies in predominantly developed contexts provide some evidence consistent with the importance of such cross-sectional benchmarks. Kayser and Peress (2012) show that media benchmarking of national economic performance relative to the international economy enables OECD voters to filter out common shocks and hold incumbents more electorally accountable for domestic than international components of economic growth. Similarly, Aytaç (2018) finds that incumbent vote shares in democracies increase with economic growth relative to both previous domestic and contemporaneous international growth rates. Focusing on U.S. governors, Besley and Case (1995) also provide evidence that relative performance evaluation by voters induces incumbents to engage in yardstick competition vis-à-vis neighboring incumbents when setting tax rates. These studies assume that performance signals are widely observed by voters and are uncorrelated with other factors driving incumbent support. Our approach instead compares voters randomly assigned to receive different information, and explicitly considers how these relate to voters' prior beliefs.

2.2. Public dissemination

While we expect the content of the information provided to affect citizens' posterior beliefs and their voting behavior, the mode of information delivery may be equally important in influencing such behavior. In particular, we consider the potential role of public forms of dissemination, which increase the likelihood that citizens not only receive the information themselves but also know that other citizens in their community also received the same information.

Providing such a public signal could affect voter behavior through several coordination mechanisms. First, the greater common knowledge induced by a public signal could tacitly coordinate voter behavior. For example, Morris and Shin (2002) use a model based on a Keynesian beauty contest to demonstrate that a public signal can coordinate behavior around that signal when there exist complementarities to individuals acting as they expect others to; Cornand and Heinemann (2008) further show this effect increases with the share of people receiving the signal. In our electoral context, this could reflect voters becoming more likely to vote in accordance with their posterior beliefs about malfeasance because they believe that others will also do so, which will entail sending a clearer message to politicians that malfeasance will not be tolerated (e.g. (Lohmann, 1993)).

Second, a public signal could similarly increase communication between citizens that helps them solve the problem of coordinating on the best candidate by establishing the expectation that other voters are also willing to act on the information provided (Chwe, 2000). This could entail coordinating on a costly but beneficial action in response to the information's content. Communication could also explicitly induce coordination, e.g. where citizens meet in response to the information and debate until an explicit agreement pertaining to a common response is reached.

Third, even without inducing tacit or explicit coordination, the public signal could enhance discussion and engagement with the information, which increases the probability that the information is internalized and that

beliefs are consolidated. In this respect, a public signal could instigate information diffusion within a social network (e.g. (Alatas et al., 2016; Jackson, 2010; Enríquez et al., 2024)), and induce a seemingly collective response because any given individual becomes more likely to respond privately. Ultimately, each mechanism likely implies that the public signal will amplify the effect of the information's content.

Applied to our empirical setting, we expected that—relative to a private mode of information provision—a public mode of information dissemination would increase the magnitude of responses to the information provided. We thus hypothesized that:

H5. *The magnitude of the* average effect of providing malfeasance information on the incumbent party's vote share is greater when the information is delivered through public than private dissemination mechanism.

H6. *The magnitude of the effect of providing malfeasance information through a public dissemination mechanism on the incumbent party's vote share, relative to providing information through a private mechanism, will be increasing in the difference between reported incumbent (challenger) party malfeasance and citizens' prior expectation of such malfeasance.*

Since public signals do not necessarily require that individuals differentially update their beliefs about incumbent malfeasance, such amplification effects may not apply to hypotheses H1 and H2.

Various previous studies, particularly those focusing on the media, suggest that a public signal can meaningfully alter citizen behavior. In the case of electoral accountability, the effects of performance information disseminated through the media are often larger in magnitude than the effects of providing similar information to individuals privately (e.g. (Banerjee et al., 2011; Chong et al., 2015; Ferraz and Finan, 2008; Larreguy et al., 2020)). Similarly, modern technologies such as cellphones and social media appear to have coordinated protest participation across African (Manacorda and Tesei, 2020), and in France (Larson et al., 2019) and Russia (Enikolopov et al., 2020). While public information dissemination may have contributed positively to social welfare in such cases, radio access may also have helped coordinate anti-Semitic acts and electoral support for the Nazis (Adena et al., 2015) and the Rwandan genocide (Yanagizawa-Drott, 2014). While such evidence is suggestive, no study of which we are aware has yet identified the differential effect of providing information publicly, rather than privately, to voters.

3. Mayoral malfeasance in Mexico

Mexico's federal system is divided into 31 states and the Federal District of Mexico City. The states, in turn, contain almost 2,500 municipalities, which receive state and federal transfers but often possess a limited capacity to raise their own revenues. Municipal governments are led by mayors who preside over almost 10% of total government spending, which they primarily use to deliver basic public services and manage local infrastructure. Although mayors were able to run for re-election for the first time in 2018 in most states, mayors were previously elected to non-renewable terms typically lasting for three years.

3.1. Independent federal audits of municipal spending

A major source of funds for investments in local infrastructure is the Municipal Fund for Social Infrastructure (FISM), which represents 24% of the average mayor's annual budget. FISM funds are direct federal transfers mandated exclusively for infrastructure projects—such as investments in water supply, drainage, electrification, health infrastructure, education infrastructure, housing, and roads—designed to improve public service

delivery in localities defined by the federal government as impoverished.[4] Within these restrictions, mayors have the discretion to choose where and what types of projects are pursued. However, citizens are poorly informed about mayoral responsibility for such provision (Chong et al., 2015).

FISM transfers are subject to independent audits by the Federal Auditor's Office (ASF). The ASF has constitutional authority to audit the spending, accounting, and management of federal funds, and is perceived to be neutral, autonomous, and professional (De La O and Martel García, 2015). It can impose fines, recommend economic sanctions, and file or recommend criminal prosecution on the basis of its reports. Each year, the ASF audits around 150 municipalities, selected on the basis of the share of FISM funds in the municipal budget, previous performance, factors increasing the risk of mismanagement, and the recency of the last audit (see Auditoría Superior de la Federación, 2014). Audits are not announced or conducted until the year after spending occurred, and reports were (until recently) presented to Congress in February two calendar years after audited spending occurred. The reports are publicly available online at www.asf.gob.mx.

ASF reports examine FISM administration across a variety of dimensions, but we focus on two key dimensions that form the basis of the information provided by our experiment: (1) the share of FISM funds spent on projects that did not benefit the poor; and (2) the share of FISM funds spent on unauthorized projects. Projects not benefiting the poor represent social infrastructure investments completed in localities that are not classified as impoverished. Unauthorized projects are non-social infrastructure projects, which in practice are often similar to the corrupt practices documented in Ferraz and Finan (2008), e.g. procurement violations and electorally-targeted projects. We refer to both violations as malfeasance.

Municipal malfeasance is not uncommon in Mexico. ASF reports released between 2007 and 2015 document that, on average, 8% of audited funds were spent on projects that did not benefit the poor, while 6% were spent on unauthorized projects. Malfeasance is often concentrated in particular municipalities, and instances of FISM violations can be egregious. For example, nine mayors across the state of Tabasco diverted FISM toward the 2012 electoral campaigns of their parties' candidates,[5] the mayor of Oaxaca de Juárez created a fake union to collect illegal payments,[6] and 12 projects followed irregular tenders in Altamira in 2014.[7]

While the results of FISM audits can be reported locally, and media coverage does influence electoral accountability in urban areas (Larreguy et al., 2020), citizens—as in many developing contexts (Keefer, 2007; Pande, 2011)—are generally poorly informed about mayoral use of FISM funds. Chong et al. (2015) note that only around 10% of citizens are aware of the FISM program. However, although citizens are not generally aware of the program, dissatisfaction with services is high; 53% are unsatisfied with service provision and 42% believe the municipal government to be dishonest. Knowledge of mayoral performance in other municipalities is also likely to be low.

3.2. Municipal electoral competition

In the 2015 election, electoral competition in most Mexican municipalities was between the country's three largest parties: the populist PRI, right-wing National Action Party (PAN), and the PRI's left-wing offshoot Party of the Democratic Revolution (PRD). Municipal election campaigns are generally oriented around political parties rather than specific candidates for various reasons. First, citizens are much better informed about parties than individual politicians (e.g. Chong et al., 2015; Larreguy et al., 2018). In our sample, for example, 80% of survey respondents correctly identify the party of their municipal incumbent; this substantially

[4] According to their marginalization index, the National Population Council (CONAPO) identified that 22.7% of citizens were living in impoverished localities in 2010.

[5] *Tabasco Hoy*, "Pagaron pobres campañas 2012," March 6th 2014.

[6] BBM Noticias, "ASF: desvió Ugartchechea 370.9 mdp," October 21, 2013, link here.

[7] Centro Noticias Tamaulipas, "ASF detectó anomalías del FISMDF en Altamira," March 18th 2016; link here.

exceeds individual politician name recognition at all levels of government. Second, voters may recognize that Mexico's main parties continue to use distinct candidate selection mechanisms that select candidates with similar characteristics over time (Langston, 2003). Third, voters have consistently been shown to hold parties responsible for the actions of individual politicians (e.g. Chong et al., 2015; Larreguy et al., 2018; Marshall, 2023). Consequently, despite the fact that mayors could not themselves seek re-election, there are good reasons to believe that citizens would hold their party responsible for their actions in office.

4. Experimental design

We conducted a field experiment around the June 7, 2015 Mexican municipal elections to test the theoretical predictions enumerated in Section 2. The following subsection describes sample selection, treatment conditions, outcome variables, and our estimation strategy.

4.1. Sample selection

The experiment was conducted across 26 municipalities in the central states of Guanajuato, Estado de México, San Luis Potosí, and Querétaro. Of the municipalities in these states where an audit report was released in February 2015, the 26 were chosen to minimize safety risks to our implementation team, to match the distribution of incumbent parties across municipalities in these four states, and to maximize variation in reported malfeasance across municipalities subject to the constraint that at least one of our two measures of reported malfeasance was at least two percentage points higher or lower than the state average among audited opposition parties.[8] Figure 1 shows the location of these municipalities.

Within each municipality, up to one third of electoral precincts—Mexico's lowest geographical level of electoral aggregation, containing around 1,250 registered voters on average—were then selected for our experimental sample. We oversampled precincts from municipalities with high or low levels of incumbent malfeasance and stark contrasts with other parties. Priority was given to small but accessible rural precincts and small urban precincts to minimize the number of neighboring precincts included in the experimental sample, thereby reducing the risk of cross-precinct spillovers.[9] Appendix Table A1 shows that the resulting sample of 678 precincts is broadly representative of Mexico sociodemographically.

4.2. Treatment conditions

Partnering with the Mexican NGO Borde Político, our baseline treatment disseminated leaflets documenting the results of the ASF audits. As the example from Guanajuato in Figure 2 illustrates, the leaflet explained that FISM funds were intended for social infrastructure projects benefiting the poor, before reporting the total amount of funds (29.2 million pesos) received by the municipal government and the share of those funds (28%) spent on either (but not both) projects not actually benefiting the poor (as in this example) or unauthorized projects (see the example in Appendix Figure A1). The front of the triptych notes that Borde

[8]The municipalities of Aquismón and Villa Victoria were replaced by Atlacomulco, Temoaya, and additional precincts in Tlalnepantla de Baz because our team immediately received threats upon entering these municipalities. Combined with our block randomization design (see below), the risk of bias is likely to be minimal because replacement is uncorrelated with treatment due to the teams leaving before leaflets were delivered.

[9]In urban areas, we restricted our sample to precincts with at most 1,750 registered voters and designed an algorithm to minimize the number of neighboring precincts. This entailed identifying all neighboring precincts that were eligible for our sample and iteratively removing the precinct with the most in-sample neighbors until we reached the specified number of precincts for that municipality.

Figure 1: *The municipalities in our experimental sample*

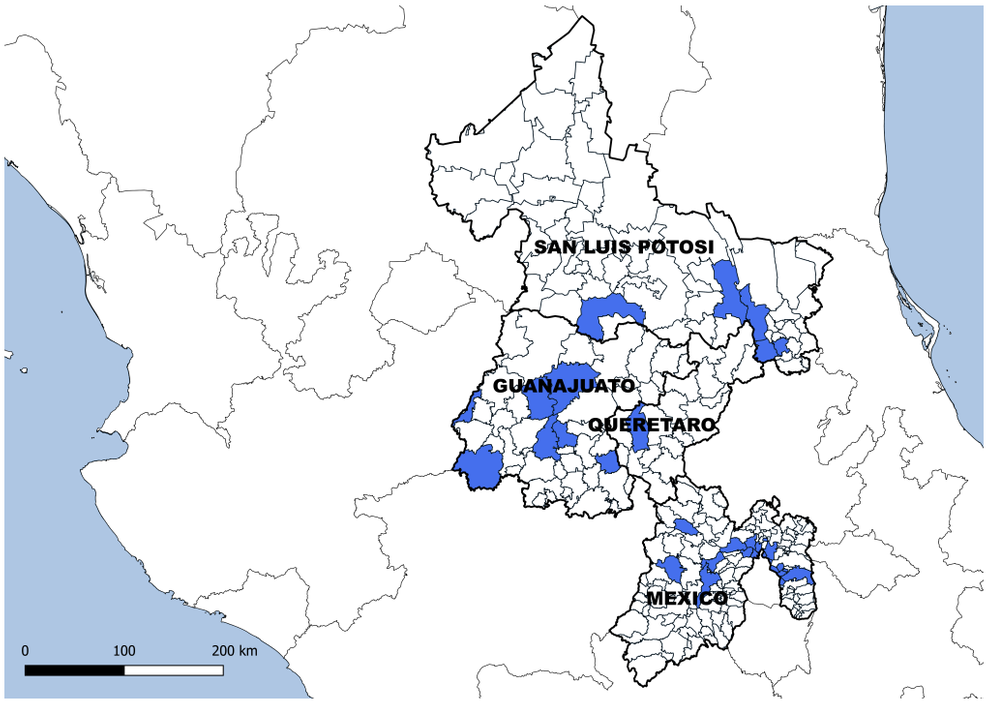


Figure 2: *Example comparing local and comparative leaflets from Guanajuato, Guanajuato*

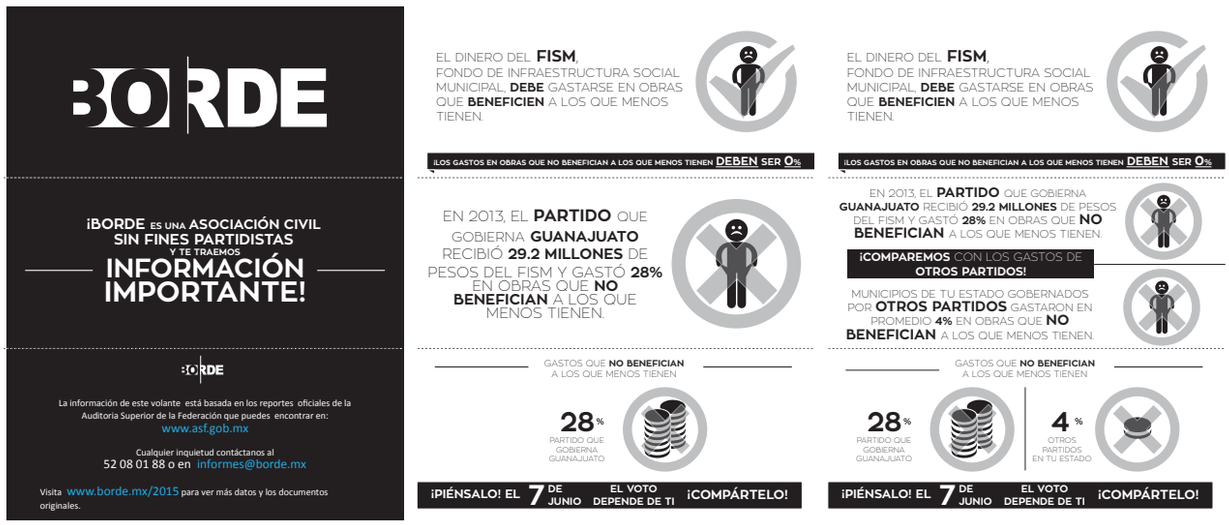
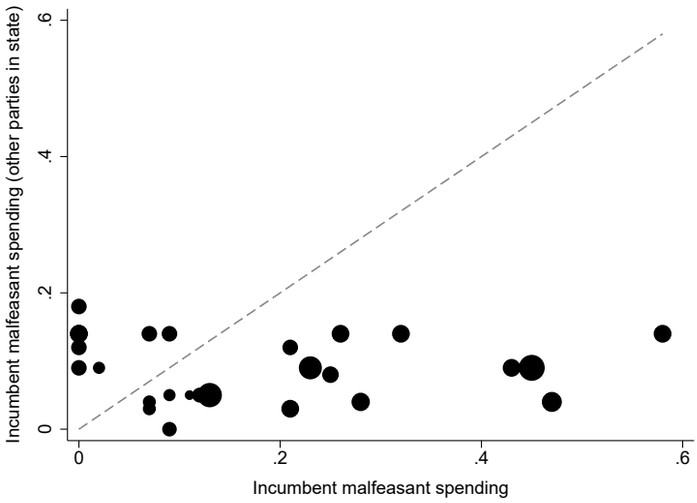


Figure 3: *Precincts by share of malfeasant spending in our sample*



Notes: The dotted line is the 45^o line. Each point is one of our 26 municipalities. The size of points corresponds to the number of precincts in our sample from that municipality.

Político is a non-partisan NGO and that the information provided can be accessed on the ASF's website. The leaflet refers to the incumbent governments without explicitly naming the incumbent parties, and were designed in black and white colors, to minimize association with any particular political party.[10]

Up to 200 leaflets were delivered to households in each precinct either by hand or by placing them in a mailbox or pinning them to doors without mailbox. We reached 57% of households in the average precinct. Leaflets were delivered over the month before the election, and compliance with our delivery protocols was generally very good.[11] Leaflet delivery locations were logged by our enumerators so that our post-election survey team could interview only leaflet recipients in treated precincts.

To investigate how the content and mode of information provision affected citizens, we varied our intervention along two dimensions corresponding to the hypotheses generated by the theoretical considerations outlined above. First, to identify the effect of providing citizens with a benchmark against which to compare their incumbent party's malfeasance, we also delivered comparative leaflets. In contrast with the *local* leaflet in the middle panel of Figure 2, the *comparative* leaflet in the third panel also provided information about the mean outcome among all audited municipalities within the same state that were governed by a different political party.[12] While the comparative benchmark could directly refer to malfeasance levels in local governments led by the municipality's main challenger party, challengers and party coalitions regularly change and explicitly naming challenger parties might have undermined citizens' trust in the information provided by a leaflet perceived to be partisan. In this example, the local information shows 28% while the benchmark shows 4%.

Figure 3 documents the distribution of malfeasant spending in our sample. Importantly, the average

[10]These efforts to minimize perceived bias were largely successful, given that citizens generally recognized the leaflets as non-partisan (see Appendix Table A11).

[11]A few leaflets were delivered to households outside the precinct, while adverse weather conditions and poor road conditions prevented us from reaching one precinct. We preserve the randomization by estimating intent to treat effects.

[12]Given that a within-municipality comparison was not always recent, available, or from a different political party, a spatial comparison offered the most electorally-relevant contrast with a municipality's incumbent party. Bhandari et al. (2023) find that such temporal benchmarks helped voters hold members of parliament to account in Senegal.

Table 1: *Factorial design with a pure control*

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

precinct was informed of 21% malfeasant spending within their municipality and 9% in municipalities within their state governed by other parties. As the figure illustrates, only 7 of our 26 municipalities learned that their incumbent's malfeasance fell below the average across incumbents from other parties audited within the same state. Although respondents in control precincts generally already viewed the incumbent as somewhat more malfeasant than challenger parties, as shown in Appendix Figure A3, these malfeasance indicators are likely to accentuate this difference.

Second, we varied whether the leaflet was delivered in a private or public manner. For the *public* mode of delivery, door-to-door delivery of the leaflet—our *private* mode of delivery—was augmented by a powerful portable loudspeaker carried on the back of a team member.[13] While this mode of delivery may have a relatively limited range, portable loudspeakers are common in Mexico and are often used by street vendors and political campaigns to broadcast messages throughout neighborhoods. Akin to the vehicles commonly driving around before Mexican elections blaring campaign messages, a single *perifonista* walked through the streets of each precinct alongside other team members distributing leaflets while playing a 30-second message on loop. The message informed citizens that their neighbors would also receive information concerning the malfeasance of their municipal mayor, and encouraged them to share and discuss the information provided.

Treatments were randomly assigned within 100 blocks (stratified by rural/urban within a municipality) containing six or seven similar precincts according to the 2×2 factorial design with a pure control shown in Table 1.[14] Each block contained one precinct receiving each treatment condition, as well as two or three control precincts (depending on precinct availability). Block randomization ensures that all respondents within a block are subject to the same electoral race and receive the same information pertaining to their mayor, and can substantially increase statistical power. Appendix Table A2 demonstrates that pre-determined precinct- and individual-level covariates are well-balanced across treatment conditions.

4.3. Measurement of key variables

We examine two main classes of outcomes. First, we collected precinct-level electoral results from state electoral institutes to measure incumbent party vote share as a proportion of registered voters.[15] Second, we surveyed ten citizens per precinct in the weeks after the election in all treated precincts and in one control precinct from each block. We use this survey, which only visited households where leaflets were delivered, to measure posterior beliefs about incumbent and challenger party malfeasance and voter coordination.[16]

Although financial constraints prevented us from conducting a baseline survey, we measure the direction

[13] We purchased these modified rucksack loudspeakers from a vendor in Mexico City that also serves political campaigns similarly seeking to broadcast their message. See Appendix Figure A2.

[14] Blocks were created using the R package `blockTools`, which sequentially creates the most similar blocks possible, based on 23 social, economic, demographic, and political variables.

[15] We obtain similar results using vote share as a proportion of turnout, but prefer the registered voters denominator because turnout could also be affected by treatment.

[16] In control precincts, enumerators were instructed to survey respondents using the same protocols that would have occurred had the precinct been treated.

and extent of updating by showing respondents the leaflet at the end of our survey and eliciting beliefs about incumbent and challenger malfeasance before and after seeing the leaflet. We then construct a municipality-level measure of how respondents updated their beliefs in response to the information, using the average change in beliefs upon seeing the leaflet among respondents in control precincts within the municipality. Using post-election surveys in this way requires: (1) that control group respondents are similar to treatment group respondents, (2) that control respondent beliefs are consistent across the month between the intervention and the post-election survey, and (3) that control group respondents internalized the information similarly to those in treated precincts. Appendix section A.5 supports these assumptions.

4.4. Estimation

To identify how our experimental variation in the content of leaflet information and the mode of leaflet delivery affected voters, we estimate the following baseline regressions:

$$Y_{pbm} = \alpha_{bm} + \tau_L Local_{pbm} + \tau_C Comparative_{pbm} + \varepsilon_{pbm}, \quad (1)$$

$$Y_{pbm} = \alpha_{bm} + \tau_P Private_{pbm} + \tau_K Public_{pbm} + \varepsilon_{pbm}, \quad (2)$$

where Y_{pbm} is an outcome for precinct p within block b of municipality m; we add an i subscript for individual-level survey responses. In all specifications, block fixed effects α_{bm} adjust for the differential treatment probabilities across blocks arising from different block sizes. Standard errors are clustered by treatment-municipality. We depart from our pre-analysis plan by weighting precinct-level observations by the share of registered voters within the precinct to whom we delivered a leaflet.[17] This is likely to increase precision by downweighting precincts where treatment delivery was more limited, although Appendix Tables A9-A10 show that unweighted regressions produce similar results.[18]

The coefficient τ_L in equation (1) estimates the effect of our *local* treatment condition—which did not include any benchmark against which to compare the incumbent's malfeasance—relative to control precincts. Coefficient τ_C estimates the effect of our *comparative* treatment condition—which included information about malfeasance by other parties—relative to the control group. Likewise, in specification (2), coefficient τ_P estimates the effect of the *private* treatment condition, and coefficient τ_K estimates the effect of the *public* treatment condition—in which leaflet delivery was supplemented by a powerful loudspeaker informing citizens about the leaflets—relative to control precincts. Since the *differential* effect of our treatment variants is the main focus of this article, we test for differences across treatment conditions as well as drop the control group to explicitly estimate differences between comparative and local, or public and private, treatment conditions.

To examine how the effects of the treatments vary with reported levels of malfeasance and, most importantly, how voters updated their beliefs in response to the information and the content of the information provided, we also estimate heterogeneous effects of the form:

$$Y_{pbm} = \alpha_{bm} + \tau_L Local_{pbm} + \tau_{LX}(Local_{pbm} \times X_{bm}) + \tau_C Comp_{pbm} + \tau_{CX}(Comp_{pbm} \times X_{bm}) + \varepsilon_{pbm}, \quad (3)$$

$$Y_{pbm} = \alpha_{bm} + \tau_P Private_{pbm} + \tau_{PX}(Private_{pbm} \times X_{bm}) \tau_K Public_{pbm} + \tau_{KX}(Public_{pbm} \times X_{bm}) + \varepsilon_{pbm}, \quad (4)$$

where X_{bm} is a block- or municipality-level moderator described below.

[17] In control precincts, we use the share of leaflets delivered to the average treated precinct within their block.

[18] The few deviations from our pre-analysis plan are justified in Appendix A.3.

Table 2: *Manipulation tests*

	Remember leaflet		Correctly remember content		Remember comparative content	Remember loud speaker	Share of community received
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Local treatment	0.234***		0.128***		0.046***		
	(0.023)		(0.019)		(0.010)		
Comparative treatment	0.260***		0.148***		0.066***		
	(0.023)		(0.020)		(0.010)		
Private treatment		0.231***		0.130***		0.006	0.483***
		(0.023)		(0.020)		(0.007)	(0.062)
Public treatment		0.263***		0.145***		0.057***	0.566***
		(0.024)		(0.020)		(0.008)	(0.064)
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{1,2,3,4,5}
Control outcome mean	0.09	0.09	0.06	0.06	0.03	0.03	1.45
Control outcome std. dev.	0.28	0.28	0.25	0.25	0.18	0.16	1.01
Test: same treatment effect (p value)	0.08	0.06	0.04	0.19	0.03	0.00	0.06
Observations	4,958	4,958	4,958	4,958	4,958	4,958	4,929

Notes: All specifications include block fixed effects, and are estimated using OLS. Standard errors clustered by municipality-treatment are in parentheses. "Remember leaflet" indicates whether the respondents recalls receiving the leaflet. "Correctly remember content indicates respondents that correctly recall the issue discussed in the leaflet (i.e. unauthorized spending or not spending on the poor, from among four options). "Remembers comparative content" indicates whether the respondent reports that the leaflet also included information on other parties in the state. "Remember loud speaker" indicates whether the respondent recalled listening to a loudspeaker with a recording accompanying the leaflet distribution. "Share of community received" measures on a five-point scale the fraction of community members that the respondent believes received a leaflet—1=very few, 2=less than half, 3=approx. half, 4=more than half and 5=almost everyone. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

5. Results

We first demonstrate that the treatments were indeed received and internalized by citizens, before turning to our main estimates of the effects of comparative performance information and public dissemination.

5.1. Manipulation tests

To verify that our treatment reached targeted citizens and produced the intended effects, Table 2 reports several manipulation tests. Based on our post-treatment survey, columns (1)-(4) confirm that citizens in precincts assigned to receive any treatment were more than three times more likely to remember the leaflet and correctly recall the issue discussed in the leaflet. For example, the share of respondents who remembered the leaflet rose from 9% in the control group to 32% in the local treatment and 35% in the comparative treatment group. The comparative and public treatments were slightly more effective in this regard, as shown by the statistically significant differences reported in the coefficient equality tests at the foot of the table.

Column (5) shows that 10% of citizens in precincts assigned to receive the comparative leaflets were recalled receiving information about other parties in their state, compared with only 3% in control precincts. Given the lack of cross-precinct spillovers documented in Appendix Section A.6, the positive effect among citizens in precincts receiving only information about their own municipal government (local treatment) in-

dicates fuzzy recall. Nevertheless, citizens in precincts receiving the comparative information were two percentage points—or around 40%—significantly more likely to recall receiving information about opposition incumbents in their state.

The public dissemination treatment also elicited the expected responses. Column (6) shows that respondents in precincts receiving the private information treatment were as likely as the 3% of respondents in control precincts to recall a loudspeaker. However, citizens in precincts subject to public dissemination were six percentage points more likely to correctly recall that a loudspeaker accompanied the leaflet delivery. Moreover, the test at the bottom of column (7) indicates that respondents in such precincts were also significantly more likely to believe that a large fraction of their community received the leaflets. This was a central message of the loudspeaker script and is a key mechanism through which we expected public dissemination could generate coordination.

5.2. The effect of providing comparative malfeasance information

To test H1 and H2, we first examine the differential effect of providing comparative performance information on citizens' posterior beliefs about incumbent and challenger malfeasance. To measure perceptions of malfeasance, respondents were asked to rate the malfeasance of the three main parties—PAN, PRD, and PRI—on a five-point scale where high values represent high malfeasance on the dimension about which their municipality received information. While the incumbent party can always be matched to a particular party, the challenger party is not always well defined.[19] Accordingly, we consider three possible definitions of challenger: the party receiving the second largest vote share at the previous municipal election in 2012; a respondent's second most preferred party; and the average across whichever of the PAN, PRD, and PRI were not in office before the 2015 election.[20] We focus on the first definition in the main text, and provide similar results using the second and third definitions in the Appendix.

Table 3 first explores how the provision of information induced respondents to update their posterior beliefs about the incumbent party. In line with Arias et al. (2022), column (1) shows no statistically significant average treatment effect of either the local or comparative treatments relative to the control group. Moreover, and contrary to H1, the t tests at the foot of panel A and the point estimates in panel B provide no evidence that comparative information induced more unfavorable updating than local information. In fact, our design is powered to precisely estimate this null effect: the 95% confidence interval—$(-0.077, 0.095)$— implied by the estimate in panel B indicates that we can reject even a 0.07 standard deviation increase in perceived incumbent malfeasance due to the differential effect of providing a cross-sectional performance benchmark. This is surprising given that citizens assigned to this treatment learned of relatively low malfeasance levels for challenger parties, and should have been more likely to attribute the often high levels of reported incumbent malfeasance to the incumbent party's particularly low underlying level of quality, rather than to a common shock affecting all parties similarly.

We further find little evidence that the effects of providing comparative performance information vary with either the benchmarked component of the information's content or voter belief updating about challengers. First, the estimates in column (2) show that neither the effect of local nor comparative information significantly varied by the level of malfeasance reported for the incumbent and challenger parties. Second, column (3) shows that—consistent with our Bayesian framework, and the results in Arias et al. (2022)—treated citizens in municipalities that updated most unfavorably about the incumbent from the information provided were

[19]To minimize associations with any political party, the comparative leaflets did not explicitly name any party. Instead, the benchmark provided information about the average malfeasance among all audited municipalities within the same state that were governed by a different political party.

[20]For instance, in municipalities governed by the PRI, we use the average party malfeasance of the PAN and PRD.

Table 3: *Effect of local and comparative information treatments on citizens' posterior beliefs about incumbent and challenger party malfeasance*

	Perceived incumbent party malfeasance (very low - very high)			Perceived challenger party malfeasance (very low - very high)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Control group as baseline						
Local treatment	0.000	0.041	-0.090	0.027	-0.064	-0.011
	(0.045)	(0.095)	(0.056)	(0.042)	(0.103)	(0.044)
Comparative treatment	-0.003	-0.027	-0.094*	-0.042	-0.029	-0.080*
	(0.046)	(0.093)	(0.053)	(0.043)	(0.091)	(0.042)
Local treatment × Incumbent malfeasant spending		-0.051			0.217	
		(0.237)			(0.242)	
Comparative treatment × Incumbent malfeasant spending		-0.117			-0.173	
		(0.234)			(0.245)	
Local treatment × Challenger malfeasant spending		-0.328			0.488	
		(0.892)			(0.962)	
Comparative treatment × Challenger malfeasant spending		0.557			0.265	
		(0.925)			(1.007)	
Local treatment × Unfavorable incumbent updating			0.131**			0.014
			(0.064)			(0.075)
Comparative treatment × Unfavorable incumbent updating			0.155**			-0.051
			(0.071)			(0.074)
Local treatment × Unfavorable challenger updating			-0.040			0.031
			(0.073)			(0.079)
Comparative treatment × Unfavorable challenger updating			-0.071			0.118
			(0.087)			(0.087)
Control outcome mean	-0.14	-0.14	-0.14	-0.30	-0.30	-0.30
Control outcome std. dev.	1.48	1.48	1.48	1.36	1.36	1.36
Test: same treatment effect (p value)	0.94	0.53	0.96	0.08	0.68	0.09
Test: same interaction (1) effect (p value)		0.76	0.66		0.78	0.14
Test: same interaction (2) effect (p value)		0.37	0.62		0.07	0.29
Observations	4,624	4,624	4,624	4,958	4,958	4,958
Panel B: Local treatment group as baseline						
Comparative treatment	0.009	-0.071	0.002	-0.059	0.044	-0.064
	(0.044)	(0.110)	(0.061)	(0.041)	(0.085)	(0.040)
Comparative treatment × Incumbent malfeasant spending		-0.053			-0.427*	
		(0.214)			(0.227)	
Comparative treatment × Challenger malfeasant spending		1.023			-0.131	
		(1.002)			(0.872)	
Comparative treatment × Unfavorable incumbent updating			0.017			-0.084
			(0.061)			(0.065)
Comparative treatment × Unfavorable challenger updating			-0.014			0.117
			(0.069)			(0.069)
Local treatment outcome mean	-0.10	-0.10	-0.10	-0.22	-0.22	-0.22
Local treatment outcome std. dev.	1.49	1.49	1.49	1.36	1.36	1.36
Observations	3,555	3,555	3,555	3,819	3,819	3,819
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}

Notes: All specifications include block fixed effects, and are estimated using OLS. The challenger is the party that received the second-largest vote share in the last municipal election. The smaller sample in columns (1), (2) and (3) reflects the lack of data on prior beliefs about *Movimiento Ciudadano*, the incumbent party in the municipality of Apaseo el Alto. Thus, the 24 precincts from this municipality are dropped from analyses examining incumbent beliefs. The coefficients in columns 4-6 remain very similar if we drop Apaseo el Alto from the analysis. 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 $p < 0.01$.

significantly more likely to believe that incumbent parties were more malfeasant.[21] However, contrary to H2, the extent of updating *about the challenger*—which helps citizens to filter out common shocks—did not significantly moderate the influence of the comparative treatment or differentially do so relative to the local information treatment. Indeed, while the interaction coefficient in panel B is negative (consistent with our theory), it is negligible and far from being statistically significant: the 95% confidence interval implied by the interaction with respect to unfavorable challengers updating in column (3) of panel B does not contain standardized differential effects of more than 0.1 standard deviations.

We find only slightly stronger support for hypotheses H1 and H2 in the case of updating about challenger parties. Consistent with H1, the negative coefficient in column (4) of panel B in Table 3 indicates that the comparative treatment induced more favorable updating about the challenger, relative to incumbent-only information. However, the point estimate is statistically insignificant and the 95% confidence interval—$(-0.139, 0.0214)$—does not include negative effects exceeding 0.11 standard deviations of the challenger posterior belief. Consistent with Bayesian learning, the interaction of the comparative treatment with our measure of unfavorable challenger updating is positive, though also not statistically significant. A similar pattern emerges regarding H2: the interactions between the comparative treatment and reported malfeasant spending (column (5) in panel B) and incumbent and challenger unfavorable updating (column (6) in panel B) are also negative, though the latter is not statistically significant. Appendix Tables A7 and A8 report similar patterns across our other definitions of the challenger. In sum, while the signs of the coefficients are consistent with the predictions in hypotheses H1 and H2, the estimates are generally small and statistically insignificant. These results suggest that any effect of benchmarked performance information on posterior beliefs about challengers was limited.

We next examine the effects of providing comparative information on incumbent vote share, as a share of registered voters. Hypotheses H3 and H4 anticipated that providing citizens with a benchmark—especially one that contrasts the incumbent party's malfeasance with that of challenger parties in office elsewhere in the state (see Figure 3)—would elicit stronger sanctioning of the incumbent. Unsurprisingly, in light of the preceding results with respect to beliefs about malfeasance, the findings in Table 4 indicate that comparative malfeasance information did not have a differential effect on electoral outcomes.

In particular, column (1) first shows that comparative information did not differentially affect voting behavior on average. The positive treatment effect on incumbent vote share, relative to the control group, may initially seem surprising. However, Arias et al. (2022) show that while the malfeasance reports did not affect the level of posterior beliefs *on average*, the reports reduced voter uncertainty about incumbent party malfeasance and elicited responses from incumbent and challenger parties that were likely to have differentially benefited the incumbent party and increased its vote share. More importantly for this article's focus on benchmarking incumbent performance, the coefficient in column (1) of panel B demonstrates that the effects of local and comparative performance information are statistically indistinguishable. Furthermore, this null effect is fairly precisely estimated: the 95% confidence interval—$(-0.009, 0.019)$—does not include negative effects exceeding 0.24 standard deviations of the incumbent party vote share. In contrast with hypothesis H3, which was premised on the substantially lower malfeasance reported about the average incumbent from a different party, this suggests that both types of information affected voter behavior similarly, on average.

Consistent with Bayesian learning, columns (2) and (3) further show that the electoral reward for the incumbent is lower in municipalities in which greater incumbent malfeasance was reported, and in which voters updated more unfavorably about the incumbent based on the information reported. This was the main finding in Arias et al. (2022). However, we find no evidence of a *differential* impact of providing comparative information. In particular, and contrary to H4, column (3) reports relatively small and statistically insignificant variation in the effect of our comparative treatment by the extent of unfavorable updating about the

[21] This result is not mechanical, since municipality-level measures of unfavorable updating are based only on responses from citizens in control precincts upon receiving the leaflet.

Table 4: *Effect of local and comparative information treatments on incumbent party vote share*

	Incumbent party vote share (share of registered voters)		
	(1)	(2)	(3)
Panel A: Control group as baseline			
Local treatment	0.012***	0.031***	0.022***
	(0.004)	(0.010)	(0.004)
Comparative treatment	0.015***	0.019*	0.015***
	(0.005)	(0.011)	(0.005)
Local treatment × Incumbent malfeasant spending		-0.051**	
		(0.025)	
Comparative treatment × Incumbent malfeasant spending		-0.036*	
		(0.021)	
Local treatment × Challenger malfeasant spending		-0.096	
		(0.080)	
Comparative treatment × Challenger malfeasant spending		0.044	
		(0.088)	
Local treatment × Unfavorable incumbent updating			-0.012*
			(0.006)
Comparative treatment × Unfavorable incumbent updating			-0.009
			(0.006)
Local treatment × Unfavorable challenger updating			0.000
			(0.006)
Comparative treatment × Unfavorable challenger updating			0.008
			(0.007)
Control outcome mean	0.19	0.19	0.19
Control outcome std. dev.	0.07	0.07	0.07
Interaction 1 mean		0.21	0.91
Interaction 1 std. dev.		0.17	1.00
Interaction 2 mean		0.09	0.71
Interaction 2 std. dev.		0.04	0.95
Test: same treatment effect (p value)	0.61	0.42	0.31
Test: same interaction (1) effect (p value)		0.68	0.81
Test: same interaction (2) effect (p value)		0.26	0.42
Observations	675	675	651
Panel B: Local treatment group as baseline			
Comparative treatment	0.005	-0.010	-0.004
	(0.007)	(0.017)	(0.007)
Comparative treatment × Incumbent malfeasant spending		0.007	
		(0.038)	
Comparative treatment × Challenger malfeasant spending		0.142	
		(0.136)	
Comparative treatment × Unfavorable incumbent updating			-0.000
			(0.010)
Comparative treatment × Unfavorable challenger updating			0.010
			(0.010)
Local treatment outcome mean	0.21	0.21	0.21
Local treatment outcome std. dev.	0.08	0.08	0.08
Interaction 1 mean		0.22	0.92
Interaction 1 std. dev.		0.17	1.00
Interaction 2 mean		0.09	0.72
Interaction 2 std. dev.		0.04	0.94
Observations	398	398	382
Outcome range	[0.03,0.40]	[0.03,0.40]	[0.03,0.40]

Notes: All specifications include block fixed effects, weight by the share of the precinct that was treated, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in column (3) 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 $p < 0.01$.

challenger.[22] The interaction terms in panel B indicate that, for standard deviation increases in challenger malfeasant spending and unfavorable challenger updating respectively, the 95% confidence intervals imply that the differential effect of providing comparative performance information does not contain incumbent vote share increases exceeding 1.6 and 2.8 percentage points.

The lack of a meaningful differential effect associated with providing comparative information could reflect several possibilities. First, since voters in the control group already believed the main local challenger to be less malfeasant, information about challenger parties may have already corresponded with their prior beliefs. Second, voters may have simply failed to comprehend the benchmark component of the treatment. Third, the cross-sectional benchmark we provided may not have been relevant to citizens in our context. For example, Bhandari et al. (2023) show that citizens in Senegal use across-time, within-district, performance benchmarks to update their beliefs and decide who to vote for.

While it is difficult to disentangle the reasons behind null findings (e.g. Lieberman et al., 2014), our survey data can help separate between these explanations by examining whether the comparative treatments differentially affected voter beliefs. First, the estimates in Table 3 show no differential updating about the incumbent party's malfeasance across the local and comparative treatment conditions. These results indicate that citizens understood the information provided about the incumbent, but adding a benchmark did not adjust how they updated about their incumbent party.

Second, the limited updating about challengers shown in Table 3 indicates that respondents primarily updated from the information provided about the incumbent party. Together, this evidence suggests that citizens either struggled to comprehend the comparative component of the information, which some of our enumerators highlighted as they conducted the survey, or that citizens did not believe that the malfeasance of parties in other municipalities represents a good proxy for how such parties would perform in their municipality. In contrast, the fact that respondents did not differentially update about the challenger when information differed from prior beliefs suggests that malfeasance indicators conforming with prior expectations does not explain this intervention's limited effects. It is thus possible that, had they received what they considered more appropriate comparative information, citizens would have updated differentially about the incumbent and consequently changed their voting behavior.[23] Unfortunately, we cannot distinguish between these potential explanations.

5.3. Limited amplifying effect of public information dissemination

While the effects of our basic incumbent performance information are not altered by further providing comparative performance information, it is possible that public dissemination may more effectively stimulate voter responses. To investigate this, we examine whether the effects of information on voting behavior are amplified when the provision of performance information is accompanied by a loudspeaker announcing its dissemination.

However, despite being more likely to recall hearing a loudspeaker and believe that a large fraction of the community received the leaflets (see Table 2), public dissemination produced similar—if not weaker—responses from voters. Column (1) of Table 5 reports a smaller increase in incumbent vote share associated with public dissemination, relative to private treatment dissemination. Contrary to the expectation that public dissemination would amplify the positive average treatment effect, the 95% confidence implied by the estimate in column (1) of panel B—$(-0.028, 0.004)$—excludes an increase in incumbent vote share of 0.4 percentage points or 0.05 standard deviations or more. Columns (2) and (3) further show no significant difference in

[22] Unreported estimates for turnout also suggest that local and comparative information impacted turnout similarly.

[23] This finding raises the possibility that interventions using more intuitive comparative benchmarks, such as a municipality's ranking in terms of malfeasance, could be more informative for citizens

Table 5: *Effect of private and public information treatments on incumbent party vote share*

	Incumbent party vote share (share of registered voters)		
	(1)	(2)	(3)
Panel A: Control group as baseline			
Private treatment	0.020***	0.041***	0.023***
	(0.005)	(0.013)	(0.005)
Public treatment	0.007	0.009	0.013***
	(0.004)	(0.014)	(0.004)
Private treatment × Incumbent malfeasant spending		-0.065***	
		(0.021)	
Public treatment × Incumbent malfeasant spending		-0.021	
		(0.022)	
Private treatment × Challenger malfeasant spending		-0.079	
		(0.103)	
Public treatment × Challenger malfeasant spending		0.028	
		(0.096)	
Private treatment × Unfavorable incumbent updating			-0.015**
			(0.006)
Public treatment × Unfavorable incumbent updating			-0.005
			(0.004)
Private treatment × Unfavorable challenger updating			0.011
			(0.007)
Public treatment × Unfavorable challenger updating			-0.003
			(0.005)
Control outcome mean	0.19	0.19	0.19
Control outcome std. dev.	0.07	0.07	0.07
Interaction 1 mean		0.21	0.91
Interaction 1 std. dev.		0.17	1.00
Interaction 2 mean		0.09	0.71
Interaction 2 std. dev.		0.04	0.95
Test: same treatment effect (p value)	0.09	0.18	0.22
Test: same interaction (1) effect (p value)		0.18	0.15
Test: same interaction (2) effect (p value)		0.52	0.09
Observations	675	675	651
Panel B: Private treatment group as baseline			
Public treatment	-0.012	-0.027	-0.009
	(0.008)	(0.025)	(0.009)
Public treatment × Incumbent malfeasant spending		0.040	
		(0.034)	
Public treatment × Challenger malfeasant spending		0.059	
		(0.175)	
Public treatment × Unfavorable incumbent updating			0.010
			(0.007)
Public treatment × Unfavorable challenger updating			-0.016*
			(0.008)
Private treatment outcome mean	0.21	0.21	0.21
Private treatment outcome std. dev.	0.08	0.08	0.08
Interaction 1 mean		0.22	0.92
Interaction 1 std. dev.		0.17	1.00
Interaction 2 mean		0.09	0.72
Interaction 2 std. dev.		0.04	0.94
Observations	398	398	382
Outcome range	[0.03,0.40]	[0.03,0.40]	[0.03,0.40]

Notes: All specifications include block fixed effects, weight by the share of the precinct that was treated, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in Column (3) 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 6: Effect of variants of information treatment on social transmission

	Social discussion of leaflet (1)	Discussion created vote coordination (2)	Discussion of leaflet changed vote (3)
Private information treatment	0.111***	0.022***	0.028***
	(0.015)	(0.008)	(0.007)
Public information treatment	0.125***	0.030***	0.030***
	(0.014)	(0.008)	(0.008)
Outcome range	{0,1}	{0,1}	{0,1}
Control outcome mean	0.05	0.02	0.02
Control outcome std. dev.	0.23	0.13	0.12
Test: same treatment effect (p value)	0.22	0.26	0.82
Observations	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. "Social discussion of leaflet" indicates respondents that reported discussing the contents of the leaflets with other members of the community. "Discussion created vote coordination" indicates people in the community coordinating to vote for the same party as a result of discussing the leaflet. "Discussion of leaflet changed vote" captures that discussions about the leaflet with other members of the community changed the respondent's vote choice. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

heterogeneous responses to treatment by the level of incumbent and challenger malfeasance reported in the leaflets or by the degree to which citizens updated their beliefs in response to this information. In sum, these results provide little evidence to support hypotheses H5 or H6.

The limited effect of adding a loudspeaker is consistent with the results from a similar field experiment in India conducted in 2017 (George et al., 2021). They informed voters about the criminal background of legislative candidates, and also found no differential effect of a treatment variant which informed voters that many others living in their area were sent the same information.

However, our finding contrasts with the large effects of information disseminated by the media in similar contexts (Banerjee et al., 2011; Enríquez et al., 2024; Ferraz and Finan, 2008; Larreguy et al., 2020; Marshall, 2023). One potential explanation for the limited voter response is the greater capacity of broadcast media to foster either explicit or tacit coordination through common knowledge (Adena et al., 2015; Yanagizawa-Drott, 2014). Given loudspeakers' limited range, our public mode of delivery may have failed to sufficiently increase common knowledge to induce substantial coordination. Indeed, even though citizens in precincts exposed to the public treatment were significantly more likely to recall hearing our loudspeaker, the difference in recall was small: column (6) in Table 2 shows that respondents in precincts subject to public dissemination were only six percentage points more likely to correctly recall that leaflet delivery was augmented by a loudspeaker compared to respondents in precincts exposed to the private treatment. It is perhaps then relatively unsurprising to find little evidence that the public treatment condition translated into greater voter coordination: the t tests at the foot of columns (1)-(3) in Table 6 report no significant increase in discussion of the leaflet, vote coordination on the basis of the leaflet, or changes in voting behavior on the basis of discussions of the leaflet between the public and private forms of information dissemination.

An alternative possibility is that the loudspeaker led citizens to perceive our intervention as being more

partisan. Since political parties frequently use these loudspeakers as part of their campaigns, this could have led respondents to discount the information in the leaflets and explain the somewhat weaker effects. However, Appendix Table A11 reports no evidence that the public treatment increased voter perceptions that the leaflet was delivered by the municipal incumbent, municipal challengers, or the PAN, PRD, or PRI parties.

6. Conclusion

This article examines how comparative performance information and public dissemination moderate the effect of an NGO information campaign in the context of Mexican municipal elections. Leveraging a large-scale field experiment varying the provision of performance benchmarks and public dissemination by loudspeaker, we find little evidence of a differential effect on either voter belief updating or voting behavior beyond the pro-accountability effects of incumbent performance information documented by Arias et al. (2022). First, while citizens in treated precincts were significantly more likely to recall receiving information about other parties in their state than citizens in control precincts, they did not differentially update their beliefs about the incumbent. Second, while the loudspeaker increased the perception that the leaflets were widely delivered among treated citizens, citizens in precincts where leaflet delivery was accompanied by a loudspeaker were not more likely to coordinate their behavior around the treatment information. Given the widely-recognized problem of publication bias, we believe that these null findings of our well-powered field experiment have important implications for understanding political behavior, future research, and campaign design.

First, the provision of a cross-sectional benchmark could reduce citizen comprehension of the information and might prove irrelevant to voters. It is then essential for information campaigns to provide benchmarks that are both easy to comprehend and relevant. To that end, it is important to start by eliciting which comparative information citizens deem relevant to assessing the relative performance of their incumbents. While only contemporaneous information regarding mayors from other parties within the state was available in our case due to the infrequency of ASF audits, citizens might regard other comparative information, e.g. from previous incumbents in their municipalities, as more relevant.[24] Additionally, extensively piloting is important for determining the most effective way of depicting comparisons.

Second, public dissemination through devices such as loudspeakers might be insufficient to produce additional coordination beyond the small levels of coordination induced by leaflets. To the extent that the large effects of the media on political outcomes (e.g. Adena et al., 2015; DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Enríquez et al., 2024; Garbiras-Díaz and Montenegro, 2022; Larreguy et al., 2020; Marshall, 2023; Snyder and Strömberg, 2010; Yanagizawa-Drott, 2014) reflect coordination, our findings indicate that a loudspeaker cannot achieve this. However, further research is still needed to understand how to get a critical mass of voters to coordinate around the treatment information. In doing so, researchers might usefully assess whether making common knowledge that the treatment information was delivered, e.g. via directly communicating the extent to which other citizens also got the information, is a more effective way of inducing coordination. Our findings nevertheless demonstrate that leaflets can be effective, underscoring the importance of clear information about incumbent performance in office.

[24]The ASF had not recently audited many of the municipalities in our sample, which prevented us from using such comparative information.

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A. Supporting Information for "Does the content and mode of delivery of information matter for electoral accountability? Evidence from a field experiment in Mexico"

A.1. Formal derivation of the effect of providing incumbent-only performance information and comparative performance information

To formally establish the basis for our hypotheses regarding the effects of providing comparative performance information, we compare the Bayesian inferences that voters draw from incumbent-only and benchmarked signals using a Normal learning framework.[25] In our model, a voter seeks to learn about the unobservable malfeasance of incumbent party I, m_I, and the unobservable malfeasance of "neighboring" incumbent party/parties N, m_N, in the presence of an unobserved common shock m_C that equally impacts the observable performance of both I and N. We assume that our representative voter's prior beliefs over these quantities are given by $N(\theta_I, 1/p_I)$, $N(\theta_N, 1/p_N)$, and $N(\theta_C, 1/p_C)$ respectively. For simplicity, we assume that these prior beliefs are independently distributed.

A.1.1 Incumbent-only malfeasance information

We first consider the simpler case where a voter receives any given realization of the incumbent-only malfeasance signal, $\hat{s}_I$, drawn from signal distribution $N(m_I + m_C, 1/\rho_I)$, where the signal's precision ρ_I is known. This indicator is a noisy signal of the combined effects of the underlying malfeasance of the incumbent party in a voter's own municipality and the common shock. The following proposition establishes voters' posterior inferences about I's malfeasance and the common shock:

Theorem 1. *(Incumbent-only performance information) Upon receiving realized signal $\hat{s}_I$, a voter's posterior expectation of incumbent party I's malfeasance is $w_I(\hat{s}_I - \theta_C) + (1 - w_I)\theta_I$ and of the common shock is $w_C(\hat{s}_I - \theta_I) + (1 - w_C)\theta_C$, where w_I and w_C are weights (defined within the proof) that both increase with ρ_I and respectively increase in p_C and p_I.*

Proof: We first define $\mathbf{m} = [m_I, m_C]'$, $\boldsymbol{\mu} = [\theta_I, \theta_C]'$, $\mathbf{\Lambda}^{-1} = \begin{bmatrix} 1/p_I & 0 \\ 0 & 1/p_C \end{bmatrix}$, $\mathbf{A} = [1, 1]$, and $\mathbf{L}^{-1} = [1/\rho_I]$. Applying a standard multivariate updating result (e.g. Bishop, 2006:93) implies that posterior beliefs are distributed according to:

$$\mathbf{m}|\hat{s}_I \sim N\Big((\mathbf{\Lambda} + \mathbf{A}'\mathbf{L}\mathbf{A})^{-1}(\mathbf{A}'\mathbf{L}\hat{s}_I + \mathbf{\Lambda}\boldsymbol{\mu}), (\mathbf{\Lambda} + \mathbf{A}'\mathbf{L}\mathbf{A})^{-1}\Big), \tag{A1}$$

where the application of matrix operations to the model in hand implies:

$$\begin{aligned} (\mathbf{\Lambda} + \mathbf{A}'\mathbf{L}\mathbf{A})^{-1} &= \begin{bmatrix} p_I + \rho_I & \rho_I \\ \rho_I & p_C + \rho_I \end{bmatrix}^{-1} \\ &= \frac{1}{p_I p_C + p_I \rho_I + p_C \rho_I} \begin{bmatrix} p_C + \rho_I & -\rho_I \\ -\rho_I & p_I + \rho_I \end{bmatrix} := \mathbf{\Sigma}, \qquad \text{(A2)} \\ (\mathbf{A}'\mathbf{L}\hat{s}_I + \mathbf{\Lambda}\boldsymbol{\mu}) &= \begin{bmatrix} \rho_I \hat{s}_I + p_I \theta_I \\ \rho_I \hat{s}_I + p_C \theta_C \end{bmatrix}. \qquad \text{(A3)} \end{aligned}$$

[25] Since our model assumes full turnout, we will use citizens and voters interchangeably in this model.

Combining these results yields probability distribution:

$$p(\mathbf{m}|\hat{s}_I) \sim N\left(\begin{bmatrix} w_I(\hat{s}_I - \theta_C) + (1-w_I)\theta_I \\ w_C(\hat{s}_I - \theta_I) + (1-w_C)\theta_C \end{bmatrix}, \mathbf{\Sigma}\right), \tag{A4}$$

where $w_I := \frac{p_C\rho_I}{p_Ip_C+p_I\rho_I+p_C\rho_I}$ and $w_C := \frac{p_I\rho_I}{p_Ip_C+p_I\rho_I+p_C\rho_I}$. ■

This result shows that incumbent performance information influences voter beliefs to the extent that the common shock-adjusted signal $(\hat{s}_I - \theta_C)$ differs from the voter's prior belief θ_I. Since the common shock is also uncertain, voters have limited capacity to update about the value of this shock, and thus rely on their prior belief θ_C. Relative to receiving no information about incumbent performance, and thus retaining the prior belief θ_I, a voter upwardly (downwardly) updates their expectation of I's malfeasance when $\theta_I < (>) w_I(\hat{s}_I - \theta_C) + (1-w_I)\theta_I \iff \theta_I < (>) \hat{s}_I - \theta_C$. Intuitively, this implies that, after netting out prior expectations of the common shock, voters update unfavorably about the incumbent party when the signal exceeds the voter's prior expectation. The same expression pertains to evaluating the posterior belief regarding the expected *difference* in I's malfeasance (or, more generally, "quality") relative to N's malfeasance—a common assumption in models of vote choice, which seems appropriate to our model to the extent that incumbent parties in other municipalities within the same state approximate challenger parties within our voter's own municipality.

A.1.2 Benchmarked malfeasance information

We now consider the more demanding case where a voter receives benchmarked signal, $\hat{s}_N$, *in addition to* the incumbent malfeasance signal $\hat{s}_I$ previously analyzed. We similarly assume that $\hat{s}_N$ is drawn from signal distribution $N(m_N + m_C, 1/\rho_N)$, where the signal's precision ρ_N is also known. The presence of a second signal enables a voter to draw more precise inferences by filtering out more precisely estimated common shocks, as well as learn more about the performance of incumbent parties in other municipalities that—to the extent that parties are believed to be correlated across municipalities—is informative about local challenger parties.

Extending our first proposition, our main proposition establishes voters' posterior beliefs following the provision of comparative performance information:

Theorem 2. *(Comparative performance information) Upon receiving realized signals $\hat{s}_I$ and $\hat{s}_N$, a voter's posterior expectation of incumbent party I's malfeasance is $w_{I,s}\hat{s}_I - w_{I,C}\theta_C - w_{I,\Delta}(\hat{s}_N - \theta_N) + w_{I,I}\theta_I$, of the neighboring incumbent party N's malfeasance is $w_{N,s}\hat{s}_N - w_{N,C}\theta_C - w_{N,\Delta}(\hat{s}_I - \theta_I) + w_{N,N}\theta_N$, and of the common shock is $w_{C,I}(\hat{s}_I - \theta_I) + w_{C,N}(\hat{s}_N - \theta_N) + w_{C,C}\theta_C$, where the weights are defined within the proof.*

Proof: We first define $\hat{\mathbf{s}} = [\hat{s}_I, \hat{s}_N]'$, $\mathbf{m} = [m_I, m_N, m_C]'$, $\boldsymbol{\mu} = [\theta_I, \theta_N, \theta_C]'$, $\mathbf{\Lambda}^{-1} = \begin{bmatrix} 1/p_I & 0 & 0 \\ 0 & 1/p_C & 0 \\ 0 & 0 & 1/p_C \end{bmatrix}$, $\mathbf{A} = \begin{bmatrix} 1 & 0 & 1 \\ 0 & 1 & 1 \end{bmatrix}$, and $\mathbf{L}^{-1} = \begin{bmatrix} 1/\rho_I & 0 \\ 0 & 1/\rho_N \end{bmatrix}$. We then apply the same theorem as in the previous proof, where

the application of matrix operations to the model in hand implies:

$$
\begin{aligned}
(\mathbf{\Lambda} + \mathbf{A}'\mathbf{L}\mathbf{A})^{-1} &= \begin{bmatrix} p_I + \rho_I & 0 & \rho_I \\ 0 & p_N + \rho_N & \rho_N \\ \rho_I & \rho_N & p_C + \rho_I + \rho_N \end{bmatrix}^{-1} \\
&= \frac{1}{p_I\rho_I(p_N + \rho_N) + p_N\rho_N(p_I + \rho_I) + p_C(p_I + \rho_I)(p_N + \rho_N)} \\
&\quad \times \begin{bmatrix} (p_N + \rho_N)(p_C + \rho_I) + p_N\rho_N & \rho_I\rho_N & -\rho_I(p_N + \rho_N) \\ \rho_I\rho_N & (p_I + \rho_I)(p_C + \rho_N) + p_I\rho_I & -\rho_N(p_I + \rho_I) \\ -\rho_I(p_N + \rho_N) & -\rho_N(p_I + \rho_I) & (p_I + \rho_I)(p_N + \rho_N) \end{bmatrix} \\
&:= \mathbf{\Sigma}_B, \qquad \text{(A5)} \\
(\mathbf{A}'\mathbf{L}\hat{\mathbf{s}} + \mathbf{\Lambda}\boldsymbol{\mu}) &= \begin{bmatrix} \rho_I\hat{s}_I + p_I\theta_I \\ \rho_N\hat{s}_N + p_N\theta_N \\ \rho_I\hat{s}_I + \rho_N\hat{s}_N + p_C\theta_C \end{bmatrix}. \qquad \text{(A6)}
\end{aligned}
$$

Combining these results yields the probability distribution:

$$
p(\mathbf{m}|\hat{s}_I, \hat{s}_N) \sim N\left(\begin{bmatrix} w_{I,s}\hat{s}_I - w_{I,C}\theta_C - w_{I,\Delta}(\hat{s}_N - \theta_N) + w_{I,I}\theta_I \\ w_{N,s}\hat{s}_N - w_{N,C}\theta_C - w_{N,\Delta}(\hat{s}_I - \theta_I) + w_{N,N}\theta_N \\ w_{C,I}(\hat{s}_I - \theta_I) + w_{C,N}(\hat{s}_N - \theta_N) + w_{C,C}\theta_C \end{bmatrix}, \mathbf{\Sigma}_B\right), \qquad \text{(A7)}
$$

where the weights are given by $w_{I,s} := \frac{\rho_I(p_Np_C+p_C\rho_N+p_N\rho_N)}{D}$, $w_{I,C} := \frac{p_C\rho_I(p_N+\rho_N)}{D}$, $w_{I,\Delta} := \frac{p_N\rho_I\rho_N}{D}$, $w_{I,I} := \frac{p_I(p_Np_C+p_C\rho_N+p_N\rho_N+p_N\rho_I+\rho_I\rho_N)}{D}$, $w_{N,s} := \frac{\rho_N(p_Ip_C+p_C\rho_I+p_I\rho_I)}{D}$, $w_{N,C} := \frac{p_C\rho_N(p_I+\rho_I)}{D}$, $w_{N,\Delta} := \frac{p_I\rho_I\rho_N}{D}$, $w_{N,N} := \frac{p_N(p_Ip_C+p_C\rho_I+p_I\rho_I+p_I\rho_N+\rho_I\rho_N)}{D}$, $w_{C,I} := \frac{p_I\rho_I(p_N+\rho_N)}{D}$, $w_{C,N} := \frac{p_N\rho_N(p_I+\rho_I)}{D}$, and $w_{C,C} := \frac{p_C(p_I+\rho_I)(p_N+\rho_N)}{D}$, where $D := [p_I\rho_I(p_N + \rho_N) + p_N\rho_N(p_I + \rho_I) + p_C(p_I + \rho_I)(p_N + \rho_N)]^{-1}$ and all weights are positive. ■

This proposition clearly illustrates that voter posterior beliefs about the level of incumbent malfeasance increase with the extent to which indicators of incumbent malfeasance exceed expectations that now explicitly adjust for updated beliefs about the common shock. Specifically, like incumbent-only information, the expected level of incumbent malfeasance increases in the difference between the signal and the prior expectation of the common shock, i.e. $\hat{s}_I - \theta_C$. However, a Bayesian voter now also uses the benchmarked signal to further account for the possibility that high incumbent malfeasance could reflect a high realization of the common shock, i.e. $\hat{s}_N - \theta_N$. Relative to receiving no information, benchmarked performance information will induce upward (downward) updating when: $\theta_I < (>) w_{I,s}\hat{s}_I - w_{I,C}\theta_C - w_{I,\Delta}(\hat{s}_N - \theta_N) + w_{I,I}\theta_I$. This will hold when malfeasance indicators, adjusted for updated expectations of the common shock, exceed prior expectations of malfeasance. The same logic applies to evaluations of neighboring incumbent parties. The voter's belief about the common shock itself, $w_{I,C}\theta_C + w_{I,\Delta}(\hat{s}_N - \theta_N)$, is intuitively increasing in the extent to which the signal exceeds the voter's prior expectations.

Combining the two propositions, benchmarked information induces a more unfavorable (favorable) posterior expectation of incumbent party malfeasance than an incumbent-only signal when:

$$
w_I(\hat{s}_I - \theta_C) + (1 - w_I)\theta_I < (>) w_{I,s}\hat{s}_I - w_{I,C}\theta_C - w_{I,\Delta}(\hat{s}_N - \theta_N) + w_{I,I}\theta_I. \qquad \text{(A8)}
$$

Where the weights attached to the signal and prior beliefs do not drastically differ (and thus $\hat{s}_I$, θ_C, and θ_I cancel out), this expression demonstrates that voters will generally update unfavorably when $\hat{s}_N - \theta_N < 0$, i.e. when neighboring incumbent parties perform better than expected. This reflects the second signal inducing the voter to infer that there was a smaller common shock, due to such better performance, and thus becoming more likely to attribute underlying malfeasance to any high signal realization. The condition that the weights do

not drastically differ implies that the additional precision imparted by the second signal does not substantially increase the weight attached to the signal vis-à-vis prior beliefs.

When it comes to vote choice, voters may instead rely on a *relative* comparison between local incumbent parties and incumbent parties elsewhere. This relative comparison contrasts with updating about beliefs about the level of incumbent malfeasance, because the signal about the challenger now serves the function of both updating about common shocks and updating about levels of challenger malfeasance. In the case where vote choice reflects a preference for the less malfeasant party, benchmarked information induces a larger difference in expected malfeasance between local incumbent and neighboring (and, thus, challenger) incumbent parties relative to incumbent-only information when:

$$w^*_{I,s}\hat{s}_I + w_{I,I}\theta_I - w^*_{N,s}\hat{s}_N - w_{N,N}\theta_N > w_I(\hat{s}_I - \theta_C - \theta_N) + (1 - w_I)(\theta_I - \theta_N), \tag{A9}$$

where the common shock is identically accounted for when comparing posterior beliefs about the incumbent and neighboring incumbents (but adjusts the weighting coefficients to account for extracting the common shock). When the weight coefficients on comparable terms are similar in magnitude, this condition is positive when $\hat{s}_N - (\theta_N + \theta_C) < 0$. To the extent that the additional signal decreases the weight attached to prior beliefs, support for the incumbent will also increase in $\hat{s}_I - \theta_I$.

A.1.3 Empirical implications

With respect to absolute posterior beliefs, these performance metrics imply the following relationships:

- The effect of benchmarked information v. incumbent-only information on malfeasance beliefs is positive when, approximately, $\theta_N > \hat{s}_N$, and is decreasing in $(\hat{s}_N - \theta_N)$.
- The effect of incumbent-only information v. no information on malfeasance beliefs is positive when $\hat{s}_I > \theta_I + \theta_C$, and is increasing in $(\hat{s}_I - \theta_I)$.
- The effect of benchmarked information v. no information on malfeasance beliefs is positive when, approximately, $\hat{s}_I > \theta_I + \mathbb{E}[m|\hat{s}_I, \hat{s}_N]$, and is increasing in $(\hat{s}_I - \theta_I)$ and decreasing in $(\hat{s}_N - \theta_N)$.

Where relevant, relationships are approximate because we assume that the weights do not meaningfully differ.

With respect to relative comparisons between incumbent and "neighboring" incumbents (a proxy for challengers, empirically), which argue approximates vote choices, these performance metrics imply the following relationships:

- The effect of benchmark v. incumbent-only information on incumbent vote share is negative when, approximately, $\theta_N + \theta_C > \hat{s}_N$, and this ATE is thus increasing in $(\hat{s}_N - \theta_N)$.
- The effect of incumbent-only information v. control on incumbent vote share is negative when $\hat{s}_I > \theta_I + \theta_C$, and is decreasing in $(\hat{s}_I - \theta_I)$.
- The effect of benchmark information v. control on incumbent vote share is negative when, approximately, $\hat{s}_I > \hat{s}_N$, and is decreasing in $(\hat{s}_I - \hat{s}_N)$.

A.2. Additional treatment graphics

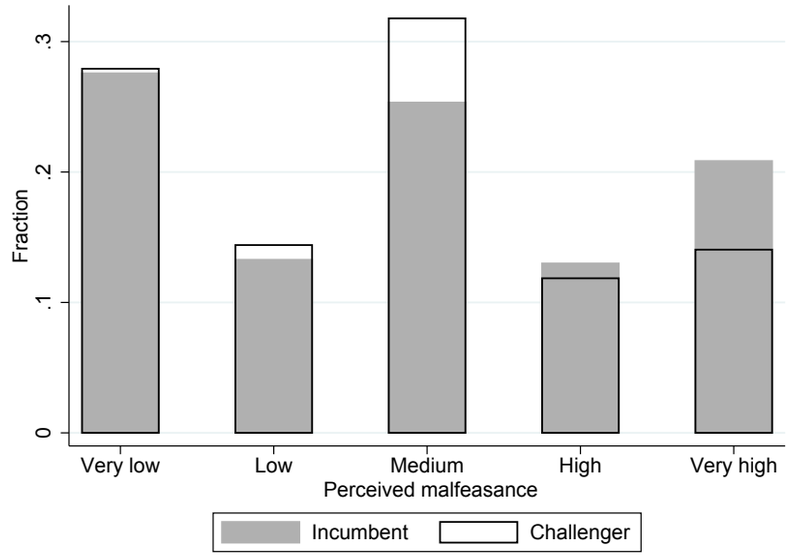
Figure A1: *Example comparing local and comparative leaflets from Ecatepec, Mexico*

Figure A2: *Loudspeaker accompanying leaflet delivery in the public treatment*

A.3. Deviations from pre-analysis plan

In our pre-analysis plan we mis-wrote our hypothesis regarding the differential effect of comparative performance information on beliefs on incumbent malfeasance. In H22 we wrote that the comparative treatment

Figure A3: *Perceptions of incumbent and challenger malfeasance in control precincts*



Note: The challenger is defined as the party receiving the second largest vote share at the previous municipal election in 2012.

would "on average, have a weaker effect on perceptions of corruption or lack of interest on marginalized populations of the incumbent than the local treatment". As section A.1 illustrates, this is a mistake as our Bayesian framework suggests that comparative performance information will induce more unfavorable incumbent updating where challenger party malfeasance falls below expectations—a likely condition to hold in our particular experimental context. Consistent with this logic, in the pre-analysis plan we hypothesized that the comparative treatment would have a more negative effect on the incumbent's vote share than the local treatment (see H9).

The other deviation is that we weight precinct-level observations by the share of registered voters within the precinct to whom we delivered a leaflet. Although we did not pre-register this weighting scheme, we ultimately believed that—in light of significant variation in the fraction of a precinct that 200 leaflets could reach—it was important to attach greater weight to precincts in which a larger fraction of the voting population received a leaflet and thus for which precinct-level electoral results are a better signal of citizen's behavior. However, as Tables A9-A10 show, all of our point estimates are similar if we run unweighted regressions.

A.4. Summary statistics

Table A1 uses 2010 Census characteristics to compare our sample of 678 precincts to the national distribution. As noted in the main text, our sample is broadly nationally representative with respect to these indicators.

Table A1: *Precinct-level comparison of Census 2010 characteristics between our sample and the nation*

	Experimental Sample			All Precincts in Mexico		
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	0.41	0.29	66,681	0.41	0.27
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

Note: All variables are unweighted.

A.5. Validation of measures of citizens' prior beliefs

We provide evidence to support our claim that post-treatment beliefs and updating in the control precincts proxy for pre-treatment prior beliefs and updating in the treated precincts within the same municipality. To do so, we show that the three key assumptions—(1) that control group respondents are similar to treatment group respondents, (2) that control group respondent beliefs are consistent across the month between the intervention and the post-election survey, and (3) that control group respondents internalized the information similarly to those in treated precincts—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 A2 is particularly important because it indicates that our treatment did not affect the willingness of different types of citizens 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.

Table A2: *Balance across 40 precinct-level variables and 8 individual-level variables*

	Control		Private local treatment		Public local treatment		Private comparative treatment		Public comparative treatment			Test: all treatment effects
	Mean	Std. dev.	Coefficient	Std. error	Coefficient	Std. error	Coefficient	Std. error	Coefficient	Std. error	Observations	=0 (p value)
Precinct-level covariates												
Area	10.020	[19.19]	-1.603	(1.47)	0.441	(1.83)	-1.751	(1.145)	-1.435	(1.299)	675	0.54
Population	1,372.550	[783.43]	21.385	(64.888)	15.329	(70.627)	-29.826	(75.527)	41.928	(62.579)	675	0.96
Population density	6,126.540	[7512.33]	186.065	(267.087)	120.194	(552.855)	-160.258	(500.067)	-516.456	(330.143)	675	0.33
Distance from municipal centroid	7,645.410	[6889.7]	242.372	(511.366)	526.728	(389.86)	1094.903**	(428.663)	641.529	(649.42)	675	0.11
Number of households	329.380	[174.73]	5.985	(15.58)	4.451	(16.339)	-6.479	(17.188)	9.382	(13.859)	675	0.95
Number of private dwellings	395.930	[214.92]	2.798	(18.574)	10.463	(19.573)	-8.505	(19.399)	2.465	(17.023)	675	0.97
Average occupants dwelling	4.100	[0.52]	0.010	(0.041)	0.025	(0.035)	0.003	(0.036)	0.028	(0.037)	675	0.89
Average occupants per room	1.150	[0.28]	0.025	(0.019)	-0.012	(0.019)	0.025	(0.02)	-0.001	(0.021)	675	0.36
Share of homes with 2+ rooms	0.660	[0.14]	-0.017*	(0.01)	0.023*	(0.012)	-0.004	(0.014)	-0.005	(0.011)	675	0.10
Share of homes with 3+ rooms	0.760	[0.13]	-0.020**	(0.01)	0.020*	(0.012)	-0.008	(0.014)	0.000	(0.012)	675	0.07
Average years of schooling	8.120	[2.47]	-0.198**	(0.091)	0.035	(0.127)	-0.216	(0.17)	-0.114	(0.097)	675	0.12
Share married	0.550	[0.05]	-0.003	(0.004)	0.003	(0.005)	-0.002	(0.006)	0.005	(0.004)	675	0.47
Share working age	0.630	[0.06]	-0.004	(0.004)	0.001	(0.004)	-0.004	(0.006)	-0.003	(0.004)	675	0.63
Share economically active	0.380	[0.07]	-0.001	(0.004)	0.000	(0.004)	-0.001	(0.004)	0.000	(0.005)	675	1.00
Share without health care	0.340	[0.12]	0.014	(0.012)	-0.008	(0.011)	0.021	(0.013)	0.019*	(0.011)	675	0.07
Share with state workers health care	0.040	[0.05]	-0.004	(0.003)	0.001	(0.005)	0.000	(0.004)	0.002	(0.003)	675	0.56
Share old	0.060	[0.03]	0.002	(0.003)	0.000	(0.003)	-0.001	(0.002)	0.003	(0.003)	675	0.55
Average children per woman	2.470	[0.62]	0.059*	(0.031)	0.043	(0.033)	0.081**	(0.04)	0.071**	(0.033)	675	0.02
Share of households with male head	0.770	[0.07]	-0.006	(0.006)	0.003	(0.007)	-0.007	(0.01)	-0.002	(0.007)	675	0.80
Share born out of state	0.270	[0.26]	0.001	(0.01)	0.014	(0.009)	0.008	(0.01)	0.013	(0.01)	675	0.52
Share indigenous speakers	0.060	[0.17]	-0.001	(0.013)	0.007	(0.007)	0.009*	(0.005)	0.015	(0.016)	675	0.50
Share of homes without a dirt floor	0.920	[0.11]	-0.007	(0.012)	0.001	(0.007)	0.003	(0.007)	-0.008	(0.018)	675	0.81
Share of homes with a toilet	0.890	[0.18]	0.000	(0.012)	0.009	(0.012)	0.003	(0.012)	0.007	(0.011)	675	0.88
Share of homes with water	0.840	[0.27]	0.032	(0.023)	0.018	(0.021)	0.003	(0.032)	-0.017	(0.027)	675	0.73
Share of homes with drainage	0.830	[0.24]	0.009	(0.012)	0.015	(0.02)	-0.001	(0.017)	-0.014	(0.022)	675	0.89
Share of homes with electricity	0.960	[0.09]	0.003	(0.006)	0.006	(0.007)	0.008*	(0.005)	0.001	(0.009)	675	0.24
Share of homes with water, drainage, and electricity	0.760	[0.31]	0.017	(0.019)	0.014	(0.022)	-0.013	(0.031)	-0.019	(0.026)	675	0.88
Share of homes with a washing machine	0.580	[0.26]	-0.001	(0.011)	0.009	(0.015)	-0.002	(0.014)	0.007	(0.011)	675	0.95
Share of homes with a landline telephone	0.420	[0.29]	-0.032***	(0.011)	-0.002	(0.013)	-0.032	(0.021)	-0.013	(0.012)	675	0.03
Share of homes with a radio	0.820	[0.1]	0.004	(0.006)	0.006	(0.007)	-0.011	(0.009)	-0.007	(0.008)	675	0.73
Share of homes with a fridge	0.750	[0.23]	-0.009	(0.013)	0.011	(0.014)	-0.018	(0.018)	0.006	(0.013)	675	0.55
Share of homes with a cell phone	0.550	[0.25]	-0.014	(0.013)	0.012	(0.013)	-0.001	(0.013)	0.008	(0.013)	675	0.78
Share of homes with a television	0.900	[0.15]	0.000	(0.007)	-0.005	(0.008)	-0.005	(0.008)	-0.006	(0.014)	675	0.87
Number of local media stations	2.320	[3.16]	0.055	(0.052)	0.008	(0.039)	0.043	(0.046)	0.105**	(0.048)	675	0.16
Share of homes with a car	0.390	[0.18]	-0.027*	(0.015)	-0.008	(0.017)	-0.010	(0.016)	-0.003	(0.011)	675	0.47
Share of homes with a computer	0.250	[0.24]	-0.021**	(0.01)	0.006	(0.013)	-0.015	(0.015)	-0.012	(0.009)	675	0.15
Share of homes with internet	0.170	[0.2]	-0.018*	(0.01)	-0.002	(0.012)	-0.011	(0.014)	-0.011	(0.008)	675	0.35
Turnout in 2012	0.630	[0.08]	0.006	(0.006)	0.007	(0.007)	0.013*	(0.008)	0.006	(0.005)	675	0.14
Incumbent vote party margin in 2012	-0.170	[0.13]	-0.031**	(0.015)	-0.009	(0.016)	-0.046	(0.028)	-0.018	(0.013)	675	0.13
Incumbent vote party share in 2012	0.420	[0.12]	0.017	(0.013)	0.004	(0.011)	0.032*	(0.019)	0.021*	(0.01)	675	0.10
Survey-level covariates												
Female	0.64	[0.48]	0.035	(0.022)	-0.009	(0.022)	0.013	(0.024)	0.044*	(0.023)	4,958	0.03
Age	44.42	[16.07]	-0.274	(0.697)	-0.986	(0.824)	-0.519	(0.667)	-0.309	(0.781)	4,869	0.81
Education	8.03	[4.14]	-0.277	(0.182)	0.11	(0.178)	-0.088	(0.183)	0.005	(0.171)	4,948	0.33
Ln(income)	1.14	[0.44]	-0.052**	(0.023)	0.008	(0.022)	-0.012	(0.021)	0.014	(0.024)	4,402	0.09
Employed	0.42	[0.49]	-0.024	(0.019)	0.015	(0.019)	0.002	(0.022)	-0.017	(0.021)	4,950	0.29
Turnout in 2012	0.63	[0.48]	-0.026	(0.019)	0.027	(0.018)	0.001	(0.018)	0.015	(0.017)	4,958	0.07
Voted for Incumbent in 2012	0.54	[0.5]	0.022	(0.03)	-0.016	(0.023)	-0.032	(0.026)	-0.001	(0.031)	3,122	0.23
Political knowledge index	2.4	[0.86]	0.004	(0.039)	0.024	(0.039)	0.047	(0.036)	-0.054	(0.037)	4,958	0.34

Notes: All specifications include block fixed effects, and precinct-level estimates are weight by the share of the precinct that was treated, and are estimated using OLS. Standard errors clustered by municipality-treatment are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A3: *Correlation between municipal-level election outcomes and prior beliefs in the control group*

	Incumbent malfeasance prior	
	(1)	(2)
Municipal incumbent won election (2015)	-0.516	
	(0.382)	
Municipal incumbent vote share (2015)		-1.713
		(1.661)
Municipal incumbent vote share (2012)	3.307*	3.723**
	(1.690)	(1.767)
Constant	-1.198	-1.110
	(0.779)	(1.007)
Control outcome mean	-0.14	-0.14
Control outcome std. dev.	1.48	1.48
2015 election outcome mean	0.75	0.38
2015 election outcome std. dev.	0.44	0.08
Observations	1,038	1,038

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

Second, we examine whether the election outcome itself influenced beliefs between the dissemination of the treatment and the post-election survey. Table A3 shows that the 2015 *municipal*-level election outcomes are generally uncorrelated with the level of citizen 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 respondents' 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 citizen assessments of politicians are relatively persistent in the months prior to the election. 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, if the information is indeed novel to the control group, then the control group should update its beliefs substantially more than the treatment group after being shown the leaflet at the end of the post-election survey. Table A4 shows that control respondents perceive 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. Control respondents thus seem to react similarly to treated respondents, suggesting that treated respondents likely possessed similar prior beliefs and that control group respondents responded similarly to reading the leaflet during the survey to how treated respondents responded to its delivery in the field.

Finally, we use data from a similar randomized intervention to ours conducted around the October 2016

Table A4: *Effect of showing respondents the leaflet in the post-treatment survey*

	Perceived incumbent party malfeasance (very low - very high)	
	(1)	(2)
Shown leaflet for first time	0.061*	-0.008
	(0.031)	(0.043)
× Incumbent malfeasant spending		0.329*
		(0.171)
Perceived incumbent party	-0.001	-0.002
malfeasance (pre-leaflet)	(0.041)	(0.041)
Outcome range	{-2,-1,0,1,2}	{-2,-1,0,1,2}
Control outcome mean	0.75	0.75
Control outcome std. dev.	1.07	1.07
Interaction range		[0,0.58]
Interaction mean		0.21
Interaction std. dev.		0.17
Observations	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 $p < 0.01$.

Brazilian municipal elections by Boas et al. (2019). This study informed citizens about the local government's use of funds and about educational performance in the municipality. Critical for our purposes, their study collected citizens' 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. Our own analysis of the Brazilian data (additional details available upon request) reveals that:

1. Correlation of baseline priors for treatment and control is large and positive (0.86), which is perhaps not surprising, given that treatment was randomly assigned.

2. 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 and the preceding campaign) may change some people's preferences. So a positive correlation of around 0.9 is consistent with control group respondent beliefs being consistent across the month between the intervention and the post-election survey.

3. The correlation between prior beliefs of the treated group and the endline evaluations of the control group is 0.78, which suggests that the latter may be used as valid proxies for baseline responses of the treated.

Since this exercise was performed 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 A3-A4, these results are encouraging regarding the use of our approach to proxy for citizens' prior beliefs.

Table A5: *Neighbor spillover effects of information treatment on incumbent party vote share*

	Incumbent party vote share		
	(1)	(2)	(3)
Panel A: Incumbent party vote share (share of turnout)			
Neighbor information treatment	-0.001	-0.008**	-0.002
	(0.003)	(0.004)	(0.004)
× Incumbent malfeasant spending		0.028**	
		(0.011)	
× Neighbor unfavorable incumbent updating			0.001
			(0.003)
Outcome range	[0.05,0.89]	[0.05,0.89]	[0.05,0.89]
Control outcome mean	0.39	0.39	0.39
Control outcome std. dev.	0.12	0.12	0.12
Panel B: Incumbent party vote share (share of registered voters)			
Neighbor information treatment	-0.003*	-0.008***	-0.004
	(0.002)	(0.003)	(0.003)
× Incumbent malfeasant spending		0.022***	
		(0.007)	
× Neighbor unfavorable incumbent updating			0.001
			(0.001)
Outcome range	[0.03,0.46]	[0.03,0.46]	[0.03,0.46]
Control outcome mean	0.19	0.19	0.19
Control outcome std. dev.	0.06	0.06	0.06
Interaction range		[0,0.58]	[-0.6,2.7]
Interaction mean		0.24	0.97
Interaction std. dev.		0.19	1.05
Observations	2,297	2,297	2,263

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, weight by the share of the neighboring precinct that was treated divided by the number of precincts in the experimental sample that a precinct neighbors, and are estimated using OLS. Lower-order interaction terms are absorbed by the block fixed effects. The smaller sample in Column (3) 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 $p < 0.01$.

A.6. No evidence of cross-precinct spillovers

In this section, we test for whether control precincts were subject to information spillovers. Table A5 reports the effects of treatment 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. The positive interaction with the malfeasance level reported is exactly opposite to our findings and prediction from our theoretical framework. It is then hard to see how these results could reflect our information treatment. Table A6 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.

Table A6: *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
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 $p < 0.01$.

A.7. Additional results

Tables A7-A11 report additional results cited in the main text.

Table A7: *Effect of local and comparative information treatments on citizens' posterior beliefs about challenger party malfeasance, where the challenger is each respondent's second-choice party*

	Perceived challenger party malfeasance (very low - very high)		
	(1)	(2)	(3)
Panel A: Control group as baseline			
Local treatment	-0.005	-0.100	-0.035
	(0.037)	(0.095)	(0.044)
Comparative treatment	-0.054	-0.050	-0.111**
	(0.041)	(0.095)	(0.044)
Local treatment × Challenger malfeasant spending		0.436	
		(0.777)	
Comparative treatment × Challenger malfeasant spending		0.571	
		(0.899)	
Local treatment × Incumbent malfeasant spending		0.258	
		(0.209)	
Comparative treatment × Incumbent malfeasant spending		-0.257	
		(0.215)	
Local treatment × Unfavorable challenger updating			0.016
			(0.082)
Comparative treatment × Unfavorable challenger updating			0.196**
			(0.086)
Local treatment × Unfavorable incumbent updating			0.019
			(0.058)
Comparative treatment × Unfavorable incumbent updating			-0.075
			(0.058)
Control outcome mean	-0.19	-0.19	-0.19
Control outcome std. dev.	1.30	1.30	1.30
Test: same treatment effect (p value)	0.26	0.68	0.07
Test: same interaction (1) effect (p value)		0.89	0.00
Test: same interaction (2) effect (p value)		0.02	0.07
Observations	4,958	4,958	4,958
Panel B: Local treatment group as baseline			
Comparative treatment	-0.059	0.044	-0.087**
	(0.041)	(0.085)	(0.039)
Comparative treatment × Challenger malfeasant spending		-0.131	
		(0.872)	
Comparative treatment × Incumbent malfeasant spending		-0.427*	
		(0.227)	
Comparative treatment × Unfavorable challenger updating			0.163**
			(0.066)
Comparative treatment × Unfavorable incumbent updating			-0.083
			(0.052)
Local treatment outcome mean	-0.22	-0.22	-0.22
Local treatment outcome std. dev.	1.36	1.36	1.36
Observations	3,819	3,819	3,819
Outcome range	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}

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 A8: *Effect of local and comparative information treatments on citizens' posterior beliefs about challenger party malfeasance, where the challenger is the average respondent's 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)
Panel A: Control group as baseline			
Local treatment	0.032	-0.079	-0.009
	(0.037)	(0.087)	(0.037)
Comparative treatment	-0.014	-0.046	-0.044
	(0.039)	(0.084)	(0.036)
Local treatment × Challenger malfeasant spending		1.551**	
		(0.752)	
Comparative treatment × Challenger malfeasant spending		1.067	
		(0.816)	
Local treatment × Incumbent malfeasant spending		-0.137	
		(0.239)	
Comparative treatment × Incumbent malfeasant spending		-0.302	
		(0.240)	
Local treatment × Unfavorable challenger updating			0.109*
			(0.059)
Comparative treatment × Unfavorable challenger updating			0.150**
			(0.066)
Local treatment × Unfavorable incumbent updating			-0.049
			(0.062)
Comparative treatment × Unfavorable incumbent updating			-0.091
			(0.063)
Control outcome mean	-0.33	-0.33	-0.33
Control outcome std. dev.	1.20	1.20	1.20
Test: same treatment effect (p value)	0.17	0.56	0.38
Test: same interaction (1) effect (p value)		0.45	0.44
Test: same interaction (2) effect (p value)		0.34	0.34
Observations	4,958	4,958	4,958
Panel B: Local treatment group as baseline			
Comparative treatment	-0.038	0.035	-0.030
	(0.035)	(0.059)	(0.041)
Comparative treatment × Challenger malfeasant spending		-0.385	
		(0.691)	
Comparative treatment × Incumbent malfeasant spending		-0.180	
		(0.183)	
Comparative treatment × Unfavorable challenger updating			0.054
			(0.059)
Comparative treatment × Unfavorable incumbent updating			-0.051
			(0.047)
Local treatment outcome mean	-0.24	-0.24	-0.24
Local treatment outcome std. dev.	1.20	1.20	1.20
Observations	3,819	3,819	3,819
Outcome range	{-2,-1,0,1,2}	{-2,-1,0,1,2}	{-2,-1,0,1,2}

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: *Effect of local and comparative information treatments on incumbent party vote share, unweighted estimates*

	Incumbent party vote share (share of registered voters)		
	(1)	(2)	(3)
Panel A: Control group as baseline			
Local treatment	0.004	0.015*	0.012***
	(0.003)	(0.008)	(0.003)
Comparative treatment	0.012***	0.017*	0.012***
	(0.003)	(0.009)	(0.004)
Local treatment × Incumbent malfeasant spending		-0.027	
		(0.022)	
Comparative treatment × Incumbent malfeasant spending		-0.032*	
		(0.018)	
Local treatment × Challenger malfeasant spending		-0.053	
		(0.061)	
Comparative treatment × Challenger malfeasant spending		0.018	
		(0.071)	
Local treatment × Unfavorable incumbent updating			-0.007
			(0.005)
Comparative treatment × Unfavorable incumbent updating			-0.009*
			(0.005)
Local treatment × Unfavorable challenger updating			-0.003
			(0.005)
Comparative treatment × Unfavorable challenger updating			0.009*
			(0.005)
Control outcome mean	0.19	0.19	0.20
Control outcome std. dev.	0.07	0.07	0.07
Test: same treatment effect (p value)	0.15	0.86	1.00
Test: same interaction (1) effect (p value)		0.86	0.78
Test: same interaction (2) effect (p value)		0.47	0.12
Observations	675	675	651
Panel B: Local treatment group as baseline			
Comparative treatment	0.007	0.003	0.000
	(0.005)	(0.013)	(0.006)
Comparative treatment × Incumbent malfeasant spending		-0.006	
		(0.033)	
Comparative treatment × Challenger malfeasant spending		0.067	
		(0.104)	
Comparative treatment × Unfavorable incumbent updating			-0.002
			(0.008)
Comparative treatment × Unfavorable challenger updating			0.012
			(0.008)
Local treatment outcome mean	0.20	0.20	0.20
Local treatment outcome std. dev.	0.07	0.07	0.07
Observations	398	398	382
Outcome range	[0.03,0.40]	[0.03,0.40]	[0.03,0.40]

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) 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 A10: *Effect of private and public information treatments on incumbent party vote share, unweighted estimates*

	Incumbent party vote share (share of registered voters)		
	(1)	(2)	(3)
Panel A: Control group as baseline			
Private treatment	0.011***	0.024**	0.014***
	(0.003)	(0.010)	(0.004)
Public treatment	0.005	0.008	0.010**
	(0.003)	(0.010)	(0.004)
Private treatment × Incumbent malfeasant spending		-0.048***	
		(0.018)	
Public treatment × Incumbent malfeasant spending		-0.011	
		(0.018)	
Private treatment × Challenger malfeasant spending		-0.023	
		(0.071)	
Public treatment × Challenger malfeasant spending		-0.012	
		(0.072)	
Private treatment × Unfavorable incumbent updating			-0.013***
			(0.004)
Public treatment × Unfavorable incumbent updating			-0.002
			(0.004)
Private treatment × Unfavorable challenger updating			0.011**
			(0.005)
Public treatment × Unfavorable challenger updating			-0.005
			(0.003)
Control outcome mean	0.19	0.19	0.20
Control outcome std. dev.	0.07	0.07	0.07
Test: same treatment effect (p value)	0.20	0.34	0.54
Test: same interaction (1) effect (p value)		0.13	0.07
Test: same interaction (2) effect (p value)		0.92	0.01
Observations	675	675	651
Panel B: Private treatment group as baseline			
Public treatment	-0.006	-0.015	-0.004
	(0.005)	(0.017)	(0.007)
Public treatment × Incumbent malfeasant spending		0.037	
		(0.026)	
Public treatment × Challenger malfeasant spending		0.008	
		(0.120)	
Public treatment × Unfavorable incumbent updating			0.011
			(0.006)
Public treatment × Unfavorable challenger updating			-0.017**
			(0.006)
Private treatment outcome mean	0.20	0.20	0.20
Private treatment outcome std. dev.	0.07	0.07	0.07
Observations	398	398	382
Outcome range	[0.03,0.40]	[0.03,0.40]	[0.03,0.40]

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) 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 A11: *Effect of public treatment on belief about the leaflet's provenance*

	Believe that the leaflet was disseminated by...				
	...municipal incumbent party	...municipal challenger party	...PAN	...PRD	...PRI
	(1)	(2)	(3)	(4)	(5)
Public treatment	0.009	-0.005	-0.007	-0.016	-0.002
	(0.019)	(0.013)	(0.013)	(0.012)	(0.017)
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.26	0.16	0.14	0.12	0.17
Outcome std. dev.	0.44	0.36	0.35	0.33	0.38
Observations	3,659	3,659	3,659	3,659	3,659

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