Misperceived Social Norms and Political Accountability: Evidence and Theory*

Shuhei Kitamura† Ryo Takahashi‡ Katsunori Yamada§

March 25, 2026

Abstract

Elections can deter corruption only if voters punish tainted incumbents. We study whether punishment depends on second-order beliefs—beliefs about how other voters will react. Before Japan's October 2024 general election amid a funding scandal, we ran a pre-registered online experiment. To isolate this channel, we provided no new factual information about the scandal itself and instead reported perceived public disapproval: treated respondents learned that, in our baseline survey, the average respondent estimated that 67% of other voters viewed the scandal as unacceptable. The message increased turnout by 6 percentage points and support for opposition challengers by 7 points. Effects were sharply heterogeneous. Swing voters, especially those who initially overestimated how widely others would punish, became more likely to vote and back challengers. By contrast, ruling-party supporters, especially those who initially underestimated how widely others would punish, shifted toward the incumbent when they learned disapproval was higher than expected. More broadly, anti-corruption messages may affect voting not only by changing beliefs about wrongdoing, but also by changing beliefs about others' reactions, helping explain why such campaigns often have mixed effects.

*We are especially grateful to Ricardo Perez-Truglia, whose insightful and invaluable feedback on an earlier draft greatly improved this manuscript. We also thank Gustavo Bobonis, Gwen-Jiro Clochard, Hülya Eraslan, Daiki Kishishita, Tetsuro Kobayashi, Takeshi Murooka, Pablo Querubin, Teppei Yamamoto, and Leeat Yariv for their helpful comments and suggestions. We further appreciate the feedback from seminar participants at Hitotsubashi, Osaka, and Waseda, as well as from participants at the 2025 Asia-Pacific Meeting of the ESA (Osaka), the 2025 Frontiers in Behavioral and Experimental Economics (Osaka), the 2026 ASSA Annual Meeting (Philadelphia), the 2026 JSQPS Winter Meeting (Waseda), the 40th Meeting of the European Economic Association (Bordeaux), and the 8th Empirical Microeconomics Workshop (Banff). The experiment and pre-analysis plan (PAP) were pre-registered in the American Economic Association RCT Registry (RCT ID: AEARCTR-0014635). The experiment was approved by the Ethics Review Committee on Research with Human Subjects of Waseda University (ID: 2024-402). Takahashi acknowledges the financial support from Waseda University. The authors have no conflicts of interest to disclose. Corresponding author: Shuhei Kitamura (kitamura@iser.osaka-u.ac.jp).

†Associate Professor. Institute of Social and Economic Research, The University of Osaka, 6-1 Mihogaoka, Ibaraki, Osaka, 567-0047 Japan. Email: kitamura@iser.osaka-u.ac.jp.

‡Associate Professor. Graduate School of Economics, Waseda University. 1-6-1 Nishi-Waseda, Shinjuku, Tokyo, 169-0051 Japan. Email: ryo@waseda.jp.

§Professor. Faculty of Economics, Kindai University. 228-3, Shin-Kamikosaka, Higashi-Osaka city, Osaka, 577-0813 Japan. Email: kyamada@kindai.ac.jp.

Keywords: Political accountability, corruption, misperception, social norm, strategic voting, moral, partisan identity, information campaign

1 Introduction

Elections are meant to hold public officials accountable. By giving voters the opportunity to remove corrupt or poor performers, elections should discipline incumbents and reward good governance. In standard agency models, when credible evidence of wrongdoing becomes public, support for the incumbent should fall (Barro, 1973; Ferejohn, 1986; Persson and Tabellini, 2000; Besley, 2006).

Yet accountability often breaks down. In many settings, corruption does not end political careers even when allegations are widely known.[1] Consistent with this pattern, policy shocks that disclose audits and field experiments that publicize misconduct find heterogeneous and sometimes perverse effects: some interventions reduce incumbents' vote shares, others have little impact, and some even backfire (Ferraz and Finan, 2008; Chong et al., 2015; Bobonis et al., 2016; Avis et al., 2018; Dunning et al., 2019).

Why is electoral sanctioning so fragile?[2] We argue that a central reason is that accountability is not a purely individual act. Voting is often strategic. Citizens' turnout and vote choice depend not only on their own views, but also on expectations about how others will respond. Because participation is costly, even voters who would like to replace a scandal-tainted incumbent may stay home when they believe the "punisher" side is already strong enough to prevail (Harsanyi, 1980; Feddersen and Sandroni, 2006; Coate and Conlin, 2004). At the same time, party attachments often operate as social identities, shaping moral judgments and making co-partisan misconduct easier to excuse than out-partisan misconduct (Green et al., 2002; Mason, 2018; Anduiza et al., 2013; Boas et al., 2019; Solaz et al., 2019). What we know much less about is how these identity-linked reactions interact with

[1] The reelection of politicians linked to corruption is well documented in countries such as Italy, Japan, and the United States (see, e.g., Rundquist et al., 1977; Reed, 1996; Chang et al., 2010).

[2] The literature offers potential explanations, including lack of credible information (e.g., Ferraz and Finan, 2011; Weitz-Shapiro and Winters, 2017), corruption–competence tradeoffs (e.g., Winters and Weitz-Shapiro, 2013; Vera, 2020), group loyalty (e.g., Banerjee and Pande, 2009; Anduiza et al., 2013; Solaz et al., 2019), absence of credible clean alternatives (e.g., Pavão, 2018; Agerberg, 2020), clientelism and vote-buying (e.g., Bobonis et al., 2025; Bøttkjær and Justesen, 2021), and cynicism or tolerance of corrupt norms (e.g., Klašnja and Tucker, 2013; Vera, 2020).

second-order beliefs: beliefs about other voters' intolerance of corruption and about how much effort each camp will exert. These beliefs can shape both turnout and vote choice even when information about the underlying offense is held fixed.[3]

We study this mechanism in the context of a major corruption scandal involving Japan's ruling Liberal Democratic Party (LDP) in the run-up to the October 2024 general election. In a pre-registered online survey experiment fielded in the days immediately before the election, we randomly provided voters with information about a prevailing social norm: the aggregate level of public intolerance toward the scandal, measured in our baseline data. Importantly, the treatment does not add new facts about the scandal. It is designed to shift beliefs about other voters' views and likely behavior: how many people disapprove and how many will punish, while holding constant information about wrongdoing. By varying only common information about others' attitudes, the design isolates the role of second-order beliefs from first-order learning about the scandal itself.

The intervention yields two main results. First, intolerance-norm information increases turnout and shifts vote choice toward opposition challengers, especially among swing voters, most strongly among those who initially *overestimated* how widespread electoral punishment would be. Second, the same information increases support for the incumbent among ruling-party supporters, most strongly among those who initially *underestimated* how widespread punishment would be. A framework in which voters respond only to their own preferences or to new performance signals struggles to account for this combination of sanctioning and backlash. The patterns are natural once accountability is viewed as a contest between camps, where perceived size and resolve shape strategic incentives on both sides.

More broadly, our findings show that accountability can operate through beliefs about others, not only through new facts about performance. Any disclosure about corruption or performance that changes what citizens expect others to do can activate strategic considerations, shifting incentives to turn out and to coordinate on punishment or defense. This perspective helps reconcile why information interventions often produce mixed effects across settings: the same disclosure can discipline incumbents on average while also mobilizing

[3] A growing literature shows that beliefs about others shape political behavior, including political contributions (Perez-Truglia and Cruces, 2017), protest (Cantoni et al., 2019), activism (Hager et al., 2023), and climate action (Andre et al., 2024).

loyalist defense, depending on heterogeneity in priors and on the endogenous composition of the camps.

To interpret this heterogeneity, we develop a simple camp-contest framework. There are two latent camps. A *punisher* camp prefers replacing the incumbent after misconduct, while a *loyalist* camp prefers defending the incumbent. Camp membership is endogenous: voters trade off the moral cost of supporting corruption against the identity and material benefits of keeping "their" side in office. Turnout and vote choice depend on beliefs about each camp's latent strength and effort. A common signal about public intolerance can therefore (i) reduce free riding among potential punishers when they learn that punishment is less widespread than they thought, and (ii) trigger loyalist counter-mobilization when supporters learn that the opposing camp is larger or more resolute than they anticipated. In short, the same truthful information about perceived norms can either help or hurt accountability, depending on voters' prior expectations and partisan identity.

Related literature and contribution

Accountability and information. A large literature studies electoral accountability when voters receive credible information about performance and malfeasance. Empirically, audits and information campaigns can discipline incumbents, but their effects vary across settings and interventions (Ferraz and Finan, 2008; Chong et al., 2015; Avis et al., 2018; Dunning et al., 2019).[4] Most of this work changes voters' first-order beliefs by revealing additional information about performance or wrongdoing. By contrast, our intervention holds information about the scandal itself fixed and provides only *social* information, an aggregate intolerance norm, that shifts second-order beliefs about how other voters will respond. This design isolates how perceived camp strength and identity considerations jointly shape electoral sanctioning, helping rationalize why accountability interventions can strengthen punishment for some voters while provoking defensive responses from others.

[4] Related work studies how information about politicians' past performance shapes voter behavior through field and survey experiments, yielding mixed results (e.g., Banerjee et al., 2011; Bobonis et al., 2016; Weitz-Shapiro and Winters, 2017; Buntaine et al., 2018; Adida et al., 2020; Cruz et al., 2020; Arias et al., 2022; de Figueiredo et al., 2023; Acemoglu et al., 2024).

Partisanship, identity, and corruption tolerance. A second strand emphasizes that voters often evaluate corruption through the lens of partisan and group identity. Co-partisans may excuse misconduct, and partisan asymmetries can blunt sanctioning (Anduiza et al., 2013; Boas et al., 2019; Solaz et al., 2019). Related evidence shows that loyalty and other non-policy attachments can sustain support for corrupt politicians (Banerjee and Pande, 2009; Eggers, 2014; Bøttkjær and Justesen, 2021). Our contribution is to add an explicitly strategic layer to these identity-based accounts: partisan asymmetries need not reflect only motivated reasoning; they can also arise from rational conditioning on perceived out-group strength and in-group effort.

Misperceptions, social norms, and strategic participation. A growing literature documents systematic misperceptions about others' attitudes and behavior, and shows that correcting these misperceptions can change real actions (Bursztyn et al., 2020; Bursztyn and Yang, 2022).[5] We bring this perspective to electoral accountability, where strategic interdependence is central: turnout and vote choice depend on expectations about how other voters will behave. This feature links our setting to strategic voting and participation models in which individuals condition on expected aggregate behavior (Cox, 1997; Kawai and Watanabe, 2013; Eggers and Vivyan, 2020) and to group-ethical approaches to turnout in which individuals "do their part" rather than maximize pivotality (Harsanyi, 1980; Feddersen and Sandroni, 2006; Coate and Conlin, 2004). Empirically, we show that providing an intolerance-norm statistic, without adding new information about wrongdoing, causally shifts both turnout and vote choice, with the sign and magnitude of the response shaped by voters' priors and camp affiliation.

2 Background

Since its founding in 1955, the Liberal Democratic Party (LDP) has dominated Japanese politics, losing power only briefly in 1993–1994 and 2009–2012. Between 2023 and 2024,

[5] In politics, related work documents misperceptions about polarization, public opinion, ideological extremity, and partisan stereotyping (Levendusky and Malhotra, 2016; Mildenberger and Tingley, 2019; Ahler, 2014; Ahler and Sood, 2018; Chambers et al., 2005), as well as misperceptions about peers' vote choices (Carlson and Hill, 2022) and biased perceptions among political elites (Broockman and Skovron, 2018).

the LDP was at the center of a major political scandal. The scandal involved allegations that three major party factions (the Abe, Nikai, and Kishida factions) failed to properly disclose over 970 million yen (about 6.5 million USD) in political funds, violating mandated finance disclosure regulations and raising suspicions that the funds were used as personal slush funds. The scandal is widely regarded as the most serious case of political corruption in modern Japanese politics (e.g., Yomiuri Shimbun, 2024; NPR, 2023).

Public backlash was swift. By February 2024, the LDP's approval rating had dropped to 21%—its lowest since returning to power in 2012 (Asahi Shimbun, 2024). In the October 2024 general election, the LDP and its coalition partner Komeito lost their Lower House majority for the first time since 2009. The LDP's seats fell from 237 to 191, and 28 of the 46 politicians implicated in the scandal lost their races. The result dealt a significant blow to Prime Minister Shigeru Ishiba, who had taken office just weeks earlier on October 1, 2024.

3 Experimental Design

3.1 Overview

Our pre-registered experiment (AEARCTR-0014635) took place between October 18th and 31st, 2024, while the election occurred on October 27th. Our study consisted of three surveys: a baseline survey, a post-treatment survey, and an endline survey. Figure 1 presents an overview of the survey timeline. The baseline survey was conducted between the 18th and 23rd, the information intervention and post-treatment survey took place between the 25th and 26th, and the endline survey was administered between the 28th and 31st. The election occurred on the 27th (Sunday).

The primary objective of this study was to investigate how voters' perceptions of others' attitudes influence their own voting behavior. To this end, we implemented a randomized information intervention between the baseline and post-treatment surveys. The core feature of this intervention was the provision of information about the prevailing social norm; specifically, participants were shown aggregate attitudes toward corruption, as measured in the baseline survey.

Under strategic considerations, voters form beliefs about how intolerant their fellow citi-

zens are of corruption and adjust their turnout and ballot choices accordingly. Our information treatment targets precisely those beliefs: by revealing the aggregate norm, it changes voters' perceptions of others' attitudes. That updated view, in turn, produces the observed shifts in turnout and vote choice. Examining this process is essential for understanding how voters' considerations of others shape electoral outcomes.

Figure 2 reproduces the intervention screen shown to participants. Combining text with a simple graphic, the screen reported the baseline social norm: on average, respondents believed that 67% of other voters regarded the scandal as unacceptable.

Further details of the overall survey design are provided below.

3.2 Survey procedure and questionnaires

Participants were informed at the outset of the purpose of the surveys, their relation to the upcoming election, that they were conducted for academic research by university researchers, and that no moral judgment would be made based on their responses. To avoid the experimenter demand effect, the purpose was stated in general terms.[6] Participant anonymity was guaranteed, and they were free to withdraw from the experiment at any time.

To facilitate completion, we limited the number of survey questions and kept the questions clear and straightforward to avoid complications or confusion.[7] Furthermore, in addition to a baseline participation reward, payments increased toward the end of the experiment. By completing all surveys, participants could earn up to 65 points (one point is equivalent to one JPY).

We also offered small performance-based bonuses for belief-elicitation items to encourage careful reporting and to reduce potential experimenter-demand and social-desirability concerns. Incentivized elicitation is commonly used in the social-norm and belief-elicitation literature to make responses payoff-relevant and limit such concerns (e.g., Krupka and Weber, 2013). Questions with a special icon were designated as bonus questions, and respondents were informed that accurate answers would earn an additional 100 points (awarded to up to

[6] The survey introduction stated: "This survey aims to academically investigate your thoughts and opinions leading up to the House of Representatives election." The Online Appendix includes an English-translated version of the survey questionnaire, including the instructions.

[7] The baseline survey, which includes the most questions, contains only 11 questions and is designed to be completed within 5 minutes.

100 respondents).[8]

Screening & Baseline survey (October 18th). Before conducting the baseline survey, screening questions were administered. The screening questions included postal code, whether the respondent had already voted in early voting, and an attention-check question. Those who did not live in the target constituencies, had already voted, or did not pass the attention-check question did not continue to the baseline survey. For those who proceeded to the baseline survey, we asked about their demographics and questions related to politics and policy. The baseline survey contained 11 questions in total.

- *Measuring prior beliefs for the intervention screen.* In the baseline survey, one question asked about respondents' perceptions regarding the proportion of other voters who consider the slush fund issue unacceptable:

> This survey project brings together a diverse group of participants, whose responses can be said to represent the views and attitudes of the average voter in Japan.
>
> Regarding the following question, how many out of 100 participants do you think answered "Completely unacceptable" or "Somewhat unacceptable"?
>
> <Question> To what extent can you tolerate issues such as omissions in political fund balance reports or kickbacks from political funds?
>
> <Answer> 1. Completely unacceptable 2. Somewhat unacceptable 3. Somewhat acceptable 4. Completely acceptable
>
> To the above question, I believe that ( ) out of 100 people answered "1. Completely unacceptable" or "2. Somewhat unacceptable."

Respondents entered a number from 0 to 100, and these values were used to create the intervention screen.

[8] The decision to limit incentives to 100 participants was made solely for practical reasons. Specifically, when contracting with the survey firm, it was necessary to determine the total payment amount, including participant compensation, in advance. By capping the number of incentivized respondents at 100, we were able to fix the overall cost. Although participants were not informed of this operational constraint, the incentive structure remained intact in that each respondent's compensation was still contingent on their truthful reporting.

We intentionally designed the survey question to incentivize respondents to provide their best estimate of social norms, regardless of their personal attitudes toward corruption; that is, respondents were encouraged to make their best guess even if they personally viewed the slush fund issue as acceptable or unacceptable. We then averaged these perceptions to define the prevailing social norm (67%) used in the subsequent analyses.

Intervention & Post-treatment survey (October 25th). We first showed the treatment group our intervention screen explained above. In contrast, the control group proceeded without receiving any additional information, as it is called passive design by Haaland et al. (2023). The post-treatment survey was administered immediately afterward and asked about the intended voting behavior and posterior beliefs. There were a total of eight questions. Both the treatment and control groups were asked the same set of questions.

Endline survey (October 28th). One day after the election, the endline survey asked both groups two questions about actual voting behavior: whether respondents voted, and if so, for which political party candidate.

Table A.1 in the Online Appendix shows the variables used in the analysis based on surveys administered after the treatment. All survey questions (originally in Japanese) and their English translations are also provided in the Online Appendix.

3.3 Sample

We conducted the experiment in collaboration with Myvoice Inc. The target population consisted of 2,800 registered individuals aged between their 20s and 60s in selected single-member constituencies in Japan. We determined this target sample size through a power calculation described in the pre-analysis plan (PAP).

The target constituencies consist of two types, both of whose incumbent candidates are from the LDP: *fraud districts*, where the incumbent politicians were involved in the slush fund issue, and *clean districts*, where the incumbent politicians were not implicated.[9] To enable

[9] Japan is divided into 289 single-member constituencies, each with roughly 364,000 registered voters on average (as of October 1st, 2024). A constituency is smaller than a prefecture (47 in total) but larger than a municipality (1,741).

clean comparisons and minimize confounding factors, we selected constituencies through a multi-step procedure described in detail in Section A.1 of the Online Appendix. Briefly, we began from an initial pool of LDP incumbents in the House of Representatives and then restricted attention to candidates running for re-election in single-member districts, excluding candidates elected through proportional representation, those connected to the former Unification Church scandal, and those not officially endorsed by the party due to the fraud scandal. We further restricted the sample to constituencies that do not include any municipality split across multiple constituencies and that were not significantly affected by the 2022 amendment to the Public Offices Election Law. In prefectures where at least one qualifying fraud district exists, we selected clean districts with incumbent LDP candidates who were not involved in the slush fund scandal and that met the same criteria; one fraud district was excluded because no qualifying clean district existed in that prefecture. The final sample comprises 10 fraud districts and 15 clean districts, geographically dispersed across multiple regions (Figure A.1 in the Online Appendix).

Due to limitations in the demographic characteristics of the survey company's registered sample pool, we conducted stratified sampling based on constituency types. Table A.2 in the Online Appendix compares baseline constituency characteristics. Fraud and clean constituencies are statistically similar across these baseline characteristics. When comparing the set of targeted constituencies (fraud and clean combined) to non-targeted constituencies nationwide, most baseline variables are balanced; however, targeted constituencies have a slightly smaller number of registered voters and a lower female-to-male ratio. First, the magnitudes of these differences are small. Second, as an additional exploratory check (outside the PAP), we also examine heterogeneity by gender. Table A.15 in the Online Appendix shows that the estimated treatment effects are qualitatively similar for women and men, and the difference between female and male effects is not statistically significant.

Taken together, these comparisons suggest that, at least on observable constituency characteristics, external-validity concerns stemming from constituency selection are limited.

3.4 Balance and attrition

We conducted a statistical test (t-test) using individual-level data to check the balance between the treatment and control groups. The variables used to check the balance were pre-specified in the PAP. Table A.3 in the Online Appendix shows that all variables, except for educational attainment, are balanced at a false discovery rate of 0.05. Following the PAP, we included this variable as a baseline control in the subsequent regression analyses.

We also checked the balance between attritors and non-attritors using the same set of variables. Table A.4 in the Online Appendix shows the result. First, the number of attritors after the treatment is impressively low (95/2280 or 4%). Second, the table shows that all variables, except gender and marital status, are balanced at a false discovery rate of 0.05. As pre-specified in the PAP, we computed bounds using the method proposed by Lee (2009).

The summary statistics of the final sample are provided in Table A.5 in the Online Appendix.

3.5 Estimation strategy

In the main regression analyses, we estimate the following model using Ordinary Least Squares (OLS):

$$y_i = \alpha + \beta T_i + \mathbf{X}_i \gamma + \varepsilon_i, \tag{1}$$

for individual i, where y_i is the outcome, T_i is the treatment indicator, $\mathbf{X}_i$ is a vector of control variables, and ε_i is the error term. The control variables include the baseline control variable and prefecture fixed effects. We use a randomized experiment to estimate the causal effect β. As specified in the PAP, we use robust standard errors in the main analyses and conduct robustness checks using the Wild bootstrap and randomization inference.

4 Empirical Results

4.1 Perceptions of intolerance norms

Panel (a) in Figure 3 shows the actual distribution of respondents' prior beliefs about other voters' corruption intolerance. The average value, our definition of intolerance norms, is

67%. The distribution is wide and markedly skewed, with many respondents perceiving other voters as less tolerant of corruption.

Panel (b) displays the cumulative distribution functions (CDFs) for three groups: swing voters (defined as those who do not support any particular party), opposition-party supporters, and LDP supporters. The CDFs for swing voters and opposition-party supporters first-order stochastically dominate that of LDP supporters, indicating that, at every level of prior belief, a greater share of swing voters and opposition supporters believe that a larger proportion of others are intolerant of corruption, relative to LDP supporters.

Overall, although some LDP supporters hold stringent views about the scandal, such views are more commonly found among swing voters and opposition-party supporters, whereas LDP supporters tend to hold more lenient views. We later exploit this important variation in prior beliefs across political alignment to examine heterogeneous treatment effects.

Because the treatment reveals a common norm statistic, heterogeneity in baseline beliefs implies that the same message can induce different, and potentially opposing, belief revisions across voters; Section 6 formalizes this logic.

4.2 Effects of the information treatment

4.2.1 Effects on voting outcomes

Figure 4 summarizes the effects of the information treatment on voting outcomes. Our main analyses focus on voting outcomes reported in the post-election survey. Table A.6 in the Online Appendix reports regression results based on these outcomes as well as on the pre-election intention measures.

The results indicate that the treatment increased voter turnout by 6.4 percentage points (left panel). Because the turnout indicator equals the sum of the incumbent-vote and challenger-vote indicators, this effect can be decomposed into changes in the probability of casting an incumbent vote versus a challenger vote. We find that the treatment increases the probability of voting for a challenger by 7.3 percentage points (right panel), while the probability of voting for the incumbent changes little and is statistically indistinguishable from zero (middle panel). Taken together, these results suggest that providing information about actual intolerance norms mobilized voters and shifted votes toward challengers.

These findings connect to a broader literature on voter information campaigns. Banerjee et al. (2011) show that providing information on candidate performance and qualifications increased turnout in India. Arias et al. (2022) document a non-monotonic effect of malfeasance revelations on turnout in Mexico, with turnout increasing at both low and high levels of malfeasance. By contrast, Chong et al. (2015) find that exposing corruption reduced both turnout and challenger support. Unlike Chong et al. (2015), our results indicate that information about intolerance norms not only mobilized voters but also increased challenger support.

Importantly, voter responses varied by political alignment. In Table A.7 in the Online Appendix, we find that swing voters were mobilized and became more likely to vote for a challenger. In contrast, while the treatment did not affect turnout among LDP supporters, it significantly increased their support for the incumbent. Finally, we find no significant effects among supporters of opposition parties. The results for swing voters and LDP supporters are summarized in Figure 5.

To further characterize this heterogeneity, as prespecified in the PAP, we examine whether treatment effects vary with respondents' baseline beliefs about others' intolerance norms, measured in the baseline survey (Section 5.1). Specifically, Table 3 splits respondents within each political-alignment group by whether their baseline belief about the share of other voters who regard the scandal as unacceptable is above the sample mean ("stringent priors") or at/below the mean ("lenient priors").

Two patterns stand out. First, among swing voters, the turnout response is concentrated among those with stringent priors, while challenger support rises for both stringent- and lenient-prior swing voters. Second, among LDP supporters, treatment effects on vote choice are concentrated among those with lenient priors: the treatment increases incumbent support and reduces challenger support in this subgroup, while aggregate turnout among LDP supporters remains largely unchanged. We interpret these prior-dependent and alignment-dependent responses through a simple model of heterogeneous priors and belief updating in Section 6.

4.2.2 Effects across district types

We next examine whether the treatment effects differ across district types. Our sample includes constituencies where the incumbent was implicated in corruption ("fraud" districts) and those where no wrongdoing was detected ("clean" districts). The question is whether the information treatment increases turnout and challenger support only where incumbents are personally tainted, or whether it also spills over to clean districts by affecting attitudes toward the ruling LDP more broadly. Table 2 addresses this question.

Panel A of Table 2 shows that turnout increases in clean districts (Column (4)), driven largely by swing voters (Column (5)), whereas the net turnout effect in fraud districts is small (Column (1)). This pattern reflects how the treatment reallocates voting probabilities across candidates. In fraud districts, the message mainly induces a shift away from the implicated incumbent: the probability of voting for the incumbent falls (Panel B, Column (1)) while the probability of voting for the challenger rises (Panel C, Column (1)). Because turnout is the sum of these two components, these movements largely offset each other, leaving only a modest change in turnout. In clean districts, by contrast, we do not see a corresponding decline in incumbent voting, so the increase in challenger voting translates more directly into higher turnout. Importantly, challenger support rises in both district types, including clean districts where the local LDP incumbent was not personally implicated. This suggests that the treatment shifts attitudes toward the ruling-party brand, not only toward implicated individuals.

In addition, we observe a different pattern among LDP loyalists. Treated loyalists increase their support for the incumbent, especially in clean districts. They are also somewhat less likely to support challengers, though this effect is statistically insignificant.

4.2.3 Robustness checks

Finally, we conduct a series of robustness checks as pre-specified in the PAP, with results reported in the Online Appendix. Table A.16 reports p-values using randomization inference, considering multiple hypothesis testing (Young, 2019), and applying the Wild bootstrap (e.g., Cameron et al., 2008). Table A.17 presents Lee bounds (Lee, 2009) that account for attrition. Table A.18 shows estimation results that include additional controls (gender and

marital status) which were statistically significant in the attrition check. Finally, Table A.19 presents estimates using Poisson pseudo-maximum likelihood (Correia et al., 2020). Overall, the qualitative results are similar across these exercises.

Taken together, these heterogeneous responses motivate the model in Section 6, which formalizes how updating about others' intolerance can generate both mobilization and loyalist backlash.

5 Model

Our experiment provides voters with information about *others*: treated respondents learn a norm statistic describing how many citizens regard the corruption scandal as unacceptable. The central theoretical question is therefore not whether a given voter is pivotal, but how new information changes beliefs about the *latent strength* of the set of voters who would punish the incumbent, and how those beliefs feed into strategic turnout. Consistent with this focus, we find no treatment effect on perceived pivotality among swing voters; see the discussion in Section 6.

We adapt the group-rule utilitarian approach to turnout in Coate and Conlin (2004), which treats participation as a collective-action problem within politically aligned camps. In its canonical formulation, this framework typically takes camp membership as exogenous and abstracts from within-camp heterogeneity in beliefs about the aggregate political state (e.g., the relative strength of the sides). This baseline structure is well suited to our setting because the information treatment shifts beliefs about the relative size and resolve of the camps, and turnout decisions are naturally interdependent when voters care about group outcomes rather than pivotality alone. It also delivers transparent comparative statics for how belief updates map into turnout and vote choice, which matches the empirical patterns we document. To bring the framework in line with electoral accountability in our setting, we add two ingredients: (i) voters can hold imperfect and heterogeneous beliefs about the latent size of the punisher camp, and the information treatment shifts those beliefs; and (ii) vote choice incorporates a moral cost of tolerating corruption and (for loyalists) an identity payoff from incumbent rule, so that voters endogenously sort into an incumbent camp and a challenger camp (see Appendix for the full microfoundation).

5.1 A two-camp turnout game

There are two candidates: an incumbent I (scandal-tainted) and a challenger C (clean). The electorate is a unit-mass continuum of voters. Conditional on turning out, each voter votes *sincerely*: some voters support the challenger and would vote for C (the "punisher" camp, denoted S), while the rest support the incumbent and would vote for I (the "loyalist" camp, denoted L). Let $\pi \in (0,1)$ denote the latent population share of the punisher camp S; the remaining share $1-\pi$ belongs to camp L.[10]

Each voter draws an idiosyncratic voting cost $c_i \sim \text{Unif}[0,1]$.[11] Following the group-ethical turnout logic, voters in camp $g \in \{L,S\}$ use a cutoff rule

$$t_i = \mathbf{1}\{c_i \le \tau_g\},$$

so turnout in camp g equals $\Pr(c_i \le \tau_g) = \tau_g$. Because costs are uniform, the cutoff τ_g is numerically equal to the turnout rate in that camp, and the expected per-capita turnout cost under cutoff τ_g is

$$\mathbb{E}[c_i \mathbf{1}\{c_i \le \tau_g\}] = \int_0^{\tau_g} c\,dc = \frac{\tau_g^2}{2}.$$

In this model, information that increases (decreases) τ_g increases (decreases) turnout in camp g one-for-one.

Given (τ_L, τ_S) and the latent share π, vote shares are

$$V_I = (1-\pi)\tau_L, \qquad V_C = \pi\tau_S,$$

and the incumbent wins if $V_I \ge V_C$, equivalently

$$\pi \le k(\tau_L, \tau_S) \equiv \frac{\tau_L}{\tau_L + \tau_S}.$$

We summarize each camp's stakes with two constants: $x > 0$ is the value to camp L of an

[10] Equivalently, one can parameterize beliefs by the loyalist share $q \equiv 1-\pi$; this is a one-to-one transformation, so the comparative statics and predictions are unchanged.

[11] To rule out the degenerate case in which both camps abstain ($\tau_L = \tau_S = 0$) in which case the winning threshold $k(\tau_L, \tau_S) \equiv \tau_L/(\tau_L + \tau_S)$ is undefined—we focus throughout on equilibria with strictly positive turnout in both camps. Formally, we restrict turnout cutoffs to $\tau_g \in [\varepsilon, 1]$ for some $\varepsilon > 0$. This is a technical convenience and all comparative statics in the paper consider such interior behavior.

incumbent victory, and $b > 0$ is the value to camp S of a challenger victory. In the Appendix microfoundation, with $\Delta_i \equiv \theta_i - \eta_i \phi_i$ (moral cost $\theta_i > 0$, loyalty indicator $\eta_i \in \{0,1\}$, identity payoff $\phi_i > 0$), these correspond to $b = \mathbb{E}[\Delta_i \mid \Delta_i \geq 0] \geq 0$ and $x = \mathbb{E}[-\Delta_i \mid \Delta_i < 0] \geq 0$. For this stylized formulation, we treat (x, b) as primitives. Given beliefs about π, camps evaluate cutoffs by

$$U_L(\tau_L; \tau_S) = x \cdot \mathbb{P}\big(\pi \leq k(\tau_L, \tau_S)\big) - \frac{\tau_L^2}{2}, \tag{2}$$

$$U_S(\tau_S; \tau_L) = b \cdot \mathbb{P}\big(\pi > k(\tau_L, \tau_S)\big) - \frac{\tau_S^2}{2}. \tag{3}$$

A (pure-strategy) cutoff equilibrium is a pair (τ_L^*, τ_S^*) such that each camp's cutoff is a best response to the other's.

5.2 Information about π and the treatment

Let P_g denote camp g's prior over π, and let $\mathcal{H}$ denote an individual's information set. For turnout decisions, the relevant belief object is the one-dimensional posterior mean

$$m_g \equiv \mathbb{E}_{P_g(\cdot \mid \mathcal{H})}[\pi].$$

Because priors can differ across camps, their posterior means can differ even after conditioning on the common signal: $m_L \neq m_S$.

To interpret the RCT, let $\mathcal{H}^{\text{ctrl}}$ and $\mathcal{H}^{\text{tr}}$ denote the information sets in control and treatment. The treatment corresponds to an information refinement $\mathcal{H}^{\text{ctrl}} \rightsquigarrow \mathcal{H}^{\text{tr}}$ for treated individuals, and its behavioral effects operate through the induced shift in m_g.

5.2.1 A simple parametric private information structure

We represent the treatment information as a private message

$$s \in \left\{0, \frac{1}{n}, \ldots, 1\right\},$$

which is a scalar signal derived from the displayed statistic. In the experiment, the displayed statistic is the average reported belief about the share of others who are intolerant (e.g. 0.67);

we interpret it as a noisy signal about the latent share of "punishers." For tractability, we model this signal as binomial: conditional on π, let $K \mid \pi \sim \text{Binomial}(n, \pi)$ and set $s \equiv K/n$, i.e.

$$\mathbb{P}\Big(s = \frac{k}{n} \;\Big|\; \pi\Big) = \binom{n}{k} \pi^k (1-\pi)^{n-k}, \qquad k = 0, 1, \ldots, n.$$

Treated individuals observe s; control individuals do not.

Allow camps to start from different subjective priors,

$$\pi \sim \text{Beta}(\alpha_g, \beta_g), \qquad \mu_g \equiv \mathbb{E}_{P_g}[\pi] = \frac{\alpha_g}{\alpha_g + \beta_g}.$$

After observing s, Bayes' rule implies the posterior

$$\pi \mid s \sim \text{Beta}(\alpha_g + ns, \beta_g + n(1-s)), \qquad m_g(s) \equiv \mathbb{E}_{P_g(\cdot|s)}[\pi] = \frac{\alpha_g + ns}{\alpha_g + \beta_g + n}.$$

5.2.2 Opposite belief updating

The key feature of heterogeneous priors is that the same realized message can move beliefs in opposite directions across camps.

Lemma 1 (Opposite belief updating under heterogeneous priors) *If the realized private message satisfies*

$$\mu_L < s < \mu_S,$$

then $m_L(s) > \mu_L$ *and* $m_S(s) < \mu_S$.

Proof. Fix a camp g and compute

$$m_g(s) - \mu_g = \frac{\alpha_g + ns}{\alpha_g + \beta_g + n} - \frac{\alpha_g}{\alpha_g + \beta_g} = \frac{n(\alpha_g + \beta_g)(s - \mu_g)}{(\alpha_g + \beta_g + n)(\alpha_g + \beta_g)}.$$

Hence $\text{sign}\{m_g(s) - \mu_g\} = \text{sign}\{s - \mu_g\}$. So if $\mu_L < s$ then $m_L(s) > \mu_L$, and if $s < \mu_S$ then $m_S(s) < \mu_S$. □

Figure 6 visualizes this logic. When a common signal s lies between the two camps' prior means, $\mu_L < s < \mu_S$, it pulls both posterior beliefs toward s, implying an upward revision for the camp with the lower prior mean and a downward revision for the camp with the

higher prior mean. The figure plots an illustrative example with dashed (prior) and solid (posterior) densities.

5.3 From belief updating to turnout predictions

Lemma 1 is a statement about belief revision. To translate belief movements into turnout, we summarize how equilibrium cutoffs react to a change in the camp's perceived punisher strength:

$$\kappa_L \equiv \frac{\partial \tau_L^*}{\partial m_L}, \qquad \kappa_S \equiv \frac{\partial \tau_S^*}{\partial m_S}.$$

An increase in m_g means that camp g assigns a higher expected value to π, i.e. expects a larger punisher camp.

The signs of (κ_L, κ_S) encode four qualitative strategic-response regimes. For loyalists, $\kappa_L > 0$ corresponds to *across-side complementarity*: perceiving a stronger punisher side reduces the incumbent's winning chances, raising the marginal value of loyalist turnout. Conversely, $\kappa_L < 0$ captures *across-side substitutability*: a stronger expected punisher side reduces the perceived return to loyalist turnout. For punishers, $\kappa_S < 0$ corresponds to *within-side substitutability*: if the punisher side is expected to be larger, each punisher can exert less effort; while $\kappa_S > 0$ corresponds to *within-side complementarity*, where a larger expected punisher side increases the incentive to turn out. Table 1 summarizes the resulting 2×2 turnout predictions.

Under the opposite updating configuration $\mu_L < s < \mu_S$, the signal lies between the camps' prior means, so both update toward it but in opposite directions. Loyalists, who start from more lenient priors, revise upward about the expected size of the punisher camp, while punishers, who start from more stringent priors, revise downward. This is the natural case when groups hold systematically different priors yet receive a common statistic. Formally, the treatment moves posterior means in opposite directions:

$$\Delta m_L \equiv m_L(s) - \mu_L > 0, \qquad \Delta m_S \equiv m_S(s) - \mu_S < 0.$$

Thus, to first order, small belief shifts translate into proportional changes in the equilibrium turnout cutoffs: $\Delta\tau_L \approx \kappa_L \Delta m_L$ and $\Delta\tau_S \approx \kappa_S \Delta m_S$. Because $\Delta m_L > 0$ while $\Delta m_S < 0$, the

loyalist response has the same sign as κ_L, whereas the punisher response has the opposite sign of κ_S. This sign logic gives a clean mapping from the strategic regime to the direction of turnout changes.

5.4 Mapping to the empirical patterns

In the Appendix microfoundation, camp membership is endogenous through $\Delta_i = \theta_i - \eta_i \phi_i$, where $\theta_i > 0$ is the moral cost from the incumbent remaining in office, $\eta_i \in \{0,1\}$ indexes partisan loyalty, and $\phi_i > 0$ is a loyalist's identity payoff from incumbent rule. Because $\eta_i = 0$ (swing) implies $\Delta_i = \theta_i \geq 0$, swing voters map naturally to the challenger/punisher camp S. By contrast, loyalists ($\eta_i = 1$) tend to fall in the incumbent camp L unless θ_i is large enough to outweigh identity payoffs ϕ_i. Empirically, this mapping aligns closely with political identity: swing voters behave like camp S, while LDP supporters behave like camp L.

The key ingredient needed to interpret our heterogeneous treatment effects is heterogeneous priors. As shown in Figure 3, swing voters' baseline beliefs about intolerance norms are stochastically higher than those of LDP supporters. If voters treat intolerance as informative about others' willingness to punish, this pattern suggests a configuration in which many swing voters enter with priors satisfying $\mu_S > s$ while many LDP supporters, especially those with lenient priors, satisfy $\mu_L < s$. Under the Beta-Binomial structure above, this yields *opposite belief updating* (Lemma 1): the same realized signal shifts perceived punisher strength in opposite directions across groups.

Swing voters. Table 3 shows that the turnout effect is concentrated among stringent-prior swing voters. In the model, stringent swing voters have $\mu_S > s$ and therefore update downward, $\Delta m_S < 0$. Under within-side substitutability ($\kappa_S < 0$), a fall in m_S increases punisher turnout by reducing free riding: when punishers perceive the punisher side as weaker than they previously believed, each individual's marginal contribution is larger. This delivers higher turnout and higher challenger support among stringent swing voters, consistent with the data. By contrast, lenient-prior swing voters have μ_S closer to (or below) s, so their belief revision is smaller and the turnout response is correspondingly attenuated. Consistent

with a complementary payoff shift for swing voters, Table A.9 in the Online Appendix shows that the treatment increases stated support for transparency-enhancing reforms in political funding. In the microfoundation, this pattern can be represented as an upward shift in the effective moral-cost parameter θ_i (or, equivalently, a larger perceived benefit from replacing the incumbent), which would reinforce challenger-oriented behavior. We do not detect a corresponding increase in costly donations, suggesting that this channel is expressed primarily through policy demand rather than private contributions.

LDP supporters. Among LDP supporters, Table 3 shows that vote-choice effects are concentrated among those with lenient priors: incumbent support rises and challenger support falls in this subgroup, while aggregate turnout among LDP supporters changes little. The model separates two channels. The belief/turnout channel works through κ_L and delivers an ambiguous turnout response (Cases A–D in Table 1); empirically, a near-zero turnout effect is consistent with either a small κ_L and/or a ceiling effect because baseline loyalist turnout is already high. The shift in loyalist *vote choice*, however, points to the additional Δ-shift channel: the information can shift the effective payoff difference Δ_i for partisan voters, for example by increasing the perceived benefit of incumbent rule (a higher ϕ_i) and/or reducing the effective moral cost of supporting the incumbent (a lower θ_i), making Δ_i more negative for marginal loyalists and thereby increasing incumbent support even when turnout barely moves. Consistent with this interpretation, Table A.10 in the Online Appendix shows that treated LDP supporters, especially those with lenient priors, report higher perceived incumbent valence (competence and trustworthiness), which in the microfoundation corresponds to a higher effective incumbent-rule benefit and hence a lower Δ_i for marginal loyalists.

Opposition-party supporters. Finally, we find little effect among opposition-party supporters. A natural interpretation in this framework is a corner: if opposition supporters already have high baseline turnout and challenger support, the same information induces little marginal adjustment in either cutoffs or vote choice.

6 Alternative explanations

The main findings of the paper are that informing voters about intolerance norms increases turnout and support for challengers, especially among swing voters, while it increases support for incumbents among ruling party loyalists. Although the model above fits the empirical patterns, we discuss alternative explanations in this section.

Instrumental voting. A standard benchmark is the calculus of voting (Downs, 1957; Riker and Ordeshook, 1968), in which an individual votes when the expected return is positive:

$$R_i = p_i B_i - C_i + D_i,$$

where p_i is the pivotal probability, B_i is the utility gain from the preferred electoral outcome, C_i is the cost of voting, and D_i captures non-instrumental benefits (such as civic duty). In this framework, norm information could increase turnout if it makes the election appear more competitive, thereby increasing perceived pivotality p_i and the instrumental component $p_i B_i$. We assess this implication using the post-treatment question, "How much influence do you think your vote has on the election outcome?" Figure 7 shows that the estimated treatment effect on perceived influence is not statistically significant for swing voters (or in the full sample), despite a sizable turnout increase among swing voters. By contrast, perceived influence increases among ruling-party loyalists, yet their turnout does not rise. Tables A.11 and A.12 further show that this increase in perceived influence among loyalists is concentrated in particular prior-defined subgroups.[12] Overall, these patterns provide limited support for an instrumental pivotality-based explanation of the turnout effect.[13]

[12] In Appendix Section A.3, the alternative proxy for perceived pivotality shows a positive effect concentrated among lenient loyalists. One interpretation is that the information increases perceived incumbent viability (or shifts valence perceptions), leading them to revise expected support upward; another is that responses partly reflect expressive reporting about a preferred outcome. Either way, the result is not a clean measure of pivotality.

[13] Because we do not have a within-respondent measure of how precise each person considers their own pivotality belief to be, we also conduct an exploratory across-respondent dispersion test. Specifically, among stringent swing voters (the subgroup with the strongest turnout response), we apply the Brown-Forsythe test to residuals from a regression of perceived pivotality on baseline controls and prefecture fixed effects. The median-based statistic is $W_{50} = 4.82$ ($p = 0.028$), with a lower residual standard deviation in the treatment group ($s = 0.725$) than in the control group ($s = 0.798$). For lenient loyalists, we find no significant difference in residual dispersion ($W_{50} = 1.35$, $p = 0.25$). We also repeat these exercises using the alternative pivotality proxy described in Section A.3 of the Online Appendix and obtain qualitatively similar conclusions. Since this analysis compares dispersion across individuals rather than identifying a within-individual reduction in

Expressive voting. The same calculus of voting highlights an expressive or duty component through D_i (Downs, 1957; Riker and Ordeshook, 1968). In our context, norm information could raise D_i by increasing the expressive value of participating and of sanctioning corruption, for example by making it feel more normatively appropriate or personally meaningful to turn out and support a challenger. This mechanism can generate higher turnout even if perceived pivotality p_i does not change. However, a purely expressive account has difficulty matching the full heterogeneity of our findings. In particular, if the treatment mainly increases the expressive value of punishing corruption, it does not naturally explain why ruling party loyalists become more likely to support incumbents after receiving the same intolerance signal.

Issue salience. We next consider issue salience. Our treatment might have made the corruption issue more salient relative to other issues. In our setting, however, the scandal was already highly salient during the election campaign, with extensive media coverage beforehand. Given this baseline salience, it is unlikely that our treatment added a first-order salience effect large enough to account for the turnout and vote-choice shifts. Moreover, salience alone does not naturally explain why swing voters move toward challengers while ruling party loyalists move toward incumbents. Empirically, we test the salience channel by examining heterogeneity by whether respondents listed the slush fund issue as one of the three most important issues in the baseline survey. Columns (1) and (2) in Table A.14 show significant effects among *both* those who did not list the issue as important and those who did, with slightly larger estimates among those who *already* regarded it as important. These patterns are difficult to reconcile with a pure salience account.

Perceived competence of incumbents. We also consider whether the treatment changed perceived incumbent valence, in particular perceived competence and trustworthiness. One possibility is that respondents infer competence-related information from the norm statistic itself, even though the treatment does not provide new facts about the scandal. However, the evidence suggests that this channel is unlikely to account for the main punishment response among swing voters. As shown in Table A.10 and discussed in Section 5.4, the treatment

uncertainty, we view it as imperfect and exploratory.

has no statistically significant effect on swing voters' assessments of incumbent competence or trustworthiness, while it increases these assessments among ruling-party loyalists, with the largest effects among those with lenient priors. In line with the interpretation in Section 5.4, we do not view this pattern as primarily reflecting competence learning. Rather, we interpret it as the empirical counterpart of a preference shift in the model, captured by a higher identity payoff from incumbent rule, ϕ_i, which makes $\Delta_i = \theta_i - \phi_i$ more negative for marginal loyalists and thereby raises incumbent support.

Perceived corruption prevalence. Finally, we consider perceptions of corruption prevalence. Prior studies suggest that the perceived prevalence of corruption can shape whether voters punish malfeasance (Klašnja and Tucker, 2013; Vera, 2020; Arias et al., 2022). Since our treatment concerns others' intolerance of corruption while holding information about the scandal itself constant, it should not affect beliefs about how widespread the scandal is. We test this by splitting the sample using respondents' baseline beliefs about the share of politicians involved in the slush fund issue (above versus below the median). Columns (3) and (4) in Table A.14 show no systematic differences across these groups. Taken together, issue salience and perceived corruption prevalence do not account for our main findings.

7 Conclusion

Electoral accountability depends on voters sanctioning misconduct. But sanctioning is not a purely individual decision: turnout and vote choice often depend on what voters think other citizens will do. In this sense, accountability is a coordination problem. Beliefs about others' intolerance—and about how much effort each side will exert—can shift both participation and electoral choices.

We study this mechanism during Japan's 2024 House of Representatives election amid a major political-funding scandal. In a pre-registered online survey experiment fielded in the days before the election, we randomly provided respondents with a single piece of aggregate social information. The message reported that, in our baseline survey, the average respondent estimated that 67% of citizens viewed the scandal as unacceptable. Importantly, the message added no new facts about wrongdoing; it was designed to shift beliefs about others' views

and likely reactions while holding information about the scandal itself fixed.

On average, the norm statistic increased participation and strengthened accountability. Turnout rose by 6.4 percentage points and support for challengers rose by 7.3 points. The response, however, was sharply heterogeneous. Swing voters shifted toward challengers, whereas ruling-party loyalists shifted toward incumbents. This heterogeneity is systematically related to priors: respondents whose priors were above the signal revised downward and mobilized to punish, while respondents whose priors were below the signal revised upward and defensively aligned.

We interpret these patterns with a simple camp-based framework in which (i) voters hold heterogeneous priors about others' reactions and (ii) camp membership can be endogenous. A common signal about perceived intolerance shifts beliefs about each camp's latent strength. Among potential punishers, learning that punishment is less widespread than expected reduces free riding and raises effort. Among loyalists, learning that punishment is more widespread than expected increases the perceived threat and triggers defensive alignment. Thus the same truthful statistic about perceived norms can simultaneously increase sanctioning and generate backlash.

Our analysis is positive rather than normative. We do not conduct a welfare analysis of norm-information interventions or claim that publicizing intolerance norms is socially optimal. Nevertheless, the results imply a practical design lesson for accountability campaigns: because the same truthful message can mobilize potential punishers while inducing defensive alignment among partisan loyalists, the net effect depends on the distribution of prior beliefs and partisan identities, quantities that can be measured ex ante and should inform whether, and how, information is deployed.

More broadly, our findings show that accountability can operate through beliefs about others, not only through new facts about performance. This logic extends beyond norm messages: any information about corruption or performance that changes what citizens infer about how others will react can activate strategic considerations, shifting incentives to turn out and to coordinate on punishment or defense. This perspective helps reconcile why information interventions often produce mixed effects across settings: the same information can discipline incumbents on average while also mobilizing loyalist defense, depending on heterogeneity in priors and on the endogenous composition of the camps.

A caveat is that the asymmetry we document is shaped by the distribution of priors in our setting. Among LDP supporters, priors about public intolerance are dispersed and skewed toward expecting relatively lenient reactions, which makes an upward update—learning that disapproval is more widespread than expected—more likely to be experienced as a threat and to induce defensive alignment. In other contexts, loyalists may hold tighter priors or priors skewed toward stringent punishment, or the scandal may cross a broadly shared threshold of intolerability, in which case even loyalists may sanction incumbents regardless of what they expect others to do. We cannot test these counterfactuals with the current evidence, and understanding what makes misconduct "tolerable" versus "intolerable" for partisan supporters is an important question for future work beyond the scope of this study.

The framework is not specific to this scandal or to Japan. Camp politics is common, participation is costly, and voters rarely observe the latent strength of each side. A natural next step is to test how these belief-based mechanisms operate with repeated exposure, in lower-salience contexts, and in environments with stronger polarization or weaker trust in information sources.

References

Acemoglu, D., C. G. Aksoy, C. Baysan, C. Molina, and G. Zeki (2024). Misperceptions and Demand for Democracy under Authoritarianism. *NBER Working Paper*.

Adida, C. L., J. Gottlieb, E. Kramon, and G. McClendon (2020). Information, accountability, and cumulative learning: A field experiment in nigeria. *American Political Science Review 114*(3), 760–774.

Agerberg, M. (2020). The Lesser Evil? Corruption Voting and the Importance of Clean Alternatives. *Comparative Political Studies 53*(2), 253–287.

Ahler, D. J. (2014). Self-Fulfilling Misperceptions of Public Polarization. *The Journal of Politics 76*(3), 607–620.

Ahler, D. J. and G. Sood (2018). The Parties in Our Heads: Misperceptions about Party Composition and Their Consequences. *The Journal of Politics 80*(3), 964–981.

Andre, P., T. Boneva, F. Chopra, and A. Falk (2024). Misperceived Social Norms and Willingness to Act Against Climate Change. *The Review of Economics and Statistics*, forthcoming.

Anduiza, E., A. Gallego, and J. Muñoz (2013). Turning a Blind Eye: Experimental Evidence of Partisan Bias in Attitudes toward Corruption. *Comparative Political Studies 46*(12), 1664–1692.

Arias, E., H. Larreguy, J. Marshall, and P. Querubín (2022). Priors Rule: When Do Malfeasance Revelations Help Or Hurt Incumbent Parties? *Journal of the European Economic Association 20*(4), 1433–1477.

Asahi Shimbun (2024). Cabinet support at 21%, lowest since return to power; LDP support also at 21% — Asahi poll. `https://www.asahi.com/articles/ASS2L7FLZS2HUZPS004.html`. (Published: 2024-2-18; accessed: 2024-12-26).

Avis, E., C. Ferraz, and F. Finan (2018). Do Government Audits Reduce Corruption? Estimating the Impacts of Exposing Corrupt Politicians. *Journal of Political Economy 126*(5), 1912–1964.

Banerjee, A. and R. Pande (2009). Parochial Politics: Ethnic Preferences and Politician Corruption. Unpublished manuscript, Harvard University.

Banerjee, A. V., S. Kumar, R. Pande, and F. Su (2011). Do Informed Voters Make Better Choices? Experimental Evidence from Urban India. *Working paper*.

Barro, R. J. (1973). The control of politicians: An economic model. *Public Choice 14*(1), 19–42.

Besley, T. (2006). *Principled Agents? The Political Economy of Good Government*. Oxford: Oxford University Press.

Boas, T. C., F. D. Hidalgo, and M. A. Melo (2019). Norms versus Action: Why Voters Fail to Sanction Malfeasance in Brazil. *American Journal of Political Science 63*(2), 385–400.

Bobonis, G. J., L. R. C. Fuertes, and R. Schwabe (2016). Monitoring Corruptible Politicians. *American Economic Review 106*(8), 2371–2405.

Bobonis, G. J., P. J. Gertler, M. Gonzalez-Navarro, and S. Nichter (2025). Does Combating Corruption Reduce Clientelism? *The Economic Journal*, forthcoming.

Broockman, D. E. and C. Skovron (2018). Bias in Perceptions of Public Opinion among Political Elites. *American Political Science Review 112*(3), 542–563.

Buntaine, M. T., R. Jablonski, D. L. Nielson, and P. M. Pickering (2018). SMS texts on corruption help Ugandan voters hold elected councillors accountable at the polls. *Proceedings of the National Academy of Sciences 115*(26), 6668–6673.

Bursztyn, L., A. L. González, and D. Yanagizawa-Drott (2020). Misperceived Social Norms: Women Working Outside the Home in Saudi Arabia. *American Economic Review 110*(10), 2997–3029.

Bursztyn, L. and D. Y. Yang (2022). Misperceptions About Others. *Annual Review of Economics 14*(1), 425–452.

Bøttkjær, L. and M. K. Justesen (2021). Why Do Voters Support Corrupt Politicians? Experimental Evidence from South Africa. *The Journal of Politics 83*(2), 788–793.

Cameron, A., J. Gelbach, and D. Miller (2008). Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics 90*(3), 414–427.

Cantoni, D., D. Y. Yang, N. Yuchtman, and Y. J. Zhang (2019). Protests as Strategic Games: Experimental Evidence from Hong Kong's Antiauthoritarian Movement. *The Quarterly Journal of Economics 134*(2), 1021–1077.

Carlson, T. N. and S. J. Hill (2022). Experimental Measurement of Misperception in Political Beliefs. *Journal of Experimental Political Science 9*(2), 241–254.

Chambers, J. R., R. S. Baron, and M. L. Inman (2005). Misperceptions in Intergroup Conflict: Disagreeing About What We Disagree About. *Psychological Science 17*(1), 38–45.

Chang, E. C. C., M. A. Golden, and S. J. Hill (2010). Legislative Malfeasance and Political Accountability. *World Politics 62*(2), 177–220.

Chong, A., A. L. De La O, D. Karlan, and L. Wantchekon (2015). Does Corruption Information Inspire the Fight or Quash the Hope? A Field Experiment in Mexico on Voter Turnout, Choice, and Party Identification. *The Journal of Politics 77*(1), 55–71.

Coate, S. and M. Conlin (2004). A Group Rule—Utilitarian Approach to Voter Turnout: Theory and Evidence. *American Economic Review 94*(5), 1476–1504.

Correia, S., P. Guimarães, and T. Zylkin (2020). Fast Poisson estimation with high-dimensional fixed effects. *The Stata Journal 20*(1), 95–115.

Cox, G. W. (1997). *Making Votes Count: Strategic Coordination in the World's Electoral Systems*. Cambridge: Cambridge University Press.

Cruz, C., P. Keefer, and J. Labonne (2020). Incumbent performance, information, and elections: Evidence from malabon, philippines. *Journal of Development Economics 146*, 102511.

de Figueiredo, M. F., F. D. Hidalgo, and Y. Kasahara (2023). When Do Voters Punish Corrupt Politicians? Experimental Evidence from a Field and Survey Experiment. *British Journal of Political Science 53*(2), 728–739.

Downs, A. (1957). *An Economic Theory of Democracy*. New York: Harper.

Dunning, T., G. Grossman, M. Humphreys, S. D. Hyde, C. McIntosh, G. Nellis, C. L. Adida, E. Arias, C. Bicalho, T. C. Boas, M. T. Buntaine, S. Chauchard, A. Chowdhury, J. Gottlieb, F. D. Hidalgo, M. Holmlund, R. Jablonski, E. Kramon, H. Larreguy, M. Lierl, J. Marshall, G. McClendon, M. A. Melo, D. L. Nielson, P. M. Pickering, M. R. Platas, P. Querubín, P. Raffler, and N. Sircar (2019). Voter information campaigns and political accountability: Cumulative findings from a preregistered meta-analysis of coordinated trials. *Science Advances 5*, eaaw2612.

Eggers, A. C. (2014). Partisanship and Electoral Accountability: Evidence from the UK Expenses Scandal. *Quarterly Journal of Political Science 9*(4), 441–472.

Eggers, A. C. and N. Vivyan (2020). Who Votes More Strategically? *American Political Science Review 114*(2), 470–485.

Feddersen, T. and A. Sandroni (2006). A Theory of Participation in Elections. *American Economic Review 96*(4), 1271–1282.

Ferejohn, J. (1986). Incumbent Performance and Electoral Control. *Public Choice 50*(1-3), 5–25.

Ferraz, C. and F. Finan (2008). Exposing Corrupt Politicians: The Effects of Brazil's Publicly Released Audits on Electoral Outcomes. *The Quarterly Journal of Economics 123*(2), 703–745.

Ferraz, C. and F. Finan (2011). Electoral Accountability and Corruption: Evidence from the Audits of Local Governments. *American Economic Review 101*(4), 1274–1311.

Green, D. P., B. Palmquist, and E. Schickler (2002). *Partisan Hearts and Minds: Political Parties and the Social Identities of Voters*. Yale University Press.

Haaland, I., C. Roth, and J. Wohlfart (2023). Designing Information Provision Experiments. *Journal of Economic Literature 61*(1), 3–40.

Hager, A., L. Hensel, J. Hermle, and C. Roth (2023). Political Activists as Free Riders: Evidence from a Natural Field Experiment. *The Economic Journal 133*(653), 2068–2084.

Harsanyi, J. C. (1980). Rule utilitarianism, rights, obligations, and the theory of rational behavior. *Theory and Decision 12*(2), 115–133.

Kawai, K. and Y. Watanabe (2013). Inferring Strategic Voting. *American Economic Review 103*(2), 624–662.

Klašnja, M. and J. A. Tucker (2013). The economy, corruption, and the vote: Evidence from experiments in Sweden and Moldova. *Electoral Studies 32*(3), 536–543.

Krupka, E. L. and R. A. Weber (2013). Identifying Social Norms Using Coordination Games: Why Does Dictator Game Sharing Vary? *Journal of the European Economic Association 11*(3), 495–524.

Lee, D. S. (2009). Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects. *The Review of Economic Studies 76*(3), 1071–1102.

Levendusky, M. S. and N. Malhotra (2016). (Mis)perceptions of Partisan Polarization in the American Public. *Public Opinion Quarterly 80*(S1), 378–391.

Mason, L. (2018). *Uncivil Agreement: How Politics Became Our Identity*. University of Chicago Press.

Mildenberger, M. and D. Tingley (2019). Beliefs about Climate Beliefs: The Importance of Second-Order Opinions for Climate Politics. *British Journal of Political Science 49*(4), 1279–1307.

NPR (2023). Party bosses fall in Japan's worst political corruption scandal in decades. `https://www.npr.org/2023/12/22/1221230635/japan-alleged-political-corruption-ldp-slush-fund`. (Published: 2023-12-22; accessed: 2025-06-03).

Pavão, N. (2018). Corruption as the Only Option: The Limits to Electoral Accountability. *The Journal of Politics 80*(3), 996–1010.

Perez-Truglia, R. and G. Cruces (2017). Partisan Interactions: Evidence from a Field Experiment in the United States. *Journal of Political Economy 125*(4), 1208–1243.

Persson, T. and G. Tabellini (2000). *Political Economics: Explaining Economic Policy*. Cambridge, MA: MIT Press.

Reed, S. R. (1996). Political corruption in Japan. *International Social Science Journal 48*(149), 395–405.

Riker, W. H. and P. C. Ordeshook (1968). A Theory of the Calculus of Voting. *American Political Science Review 62*(1), 25–42.

Rundquist, B. S., G. S. Strom, and J. G. Peters (1977). Corrupt Politicians and Their Electoral Support: Some Experimental Observations. *American Political Science Review 71*(3), 954–963.

Solaz, H., C. E. D. Vries, and R. A. d. Geus (2019). In-Group Loyalty and the Punishment of Corruption. *Comparative Political Studies 52*(6), 896–926.

Vera, S. B. (2020). Accepting or Resisting? Citizen Responses to Corruption Across Varying Levels of Competence and Corruption Prevalence. *Political Studies 68*(3), 653–670.

Weitz-Shapiro, R. and M. S. Winters (2017). Can Citizens Discern? Information Credibility, Political Sophistication, and the Punishment of Corruption in Brazil. *The Journal of Politics 79*(1), 60–74.

Winters, M. S. and R. Weitz-Shapiro (2013). Lacking Information or Condoning Corruption? When Do Voters Support Corrupt Politicians? *Comparative Politics 45*(4), 418–436.

Yomiuri Shimbun (2024). Accounting officers of three factions indicted for failing to report 970 million yen in total; indictment of Abe faction executives dropped. `https://www.yomiuri.co.jp/national/20240119-OYT1T50211/`. (Published: 2024-01-19; accessed: 2024-12-26).

Young, A. (2019). Channeling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results. *The Quarterly Journal of Economics 134*(2), 557–598.

Figures and Tables

Figure 1: Timeline of the Experiment

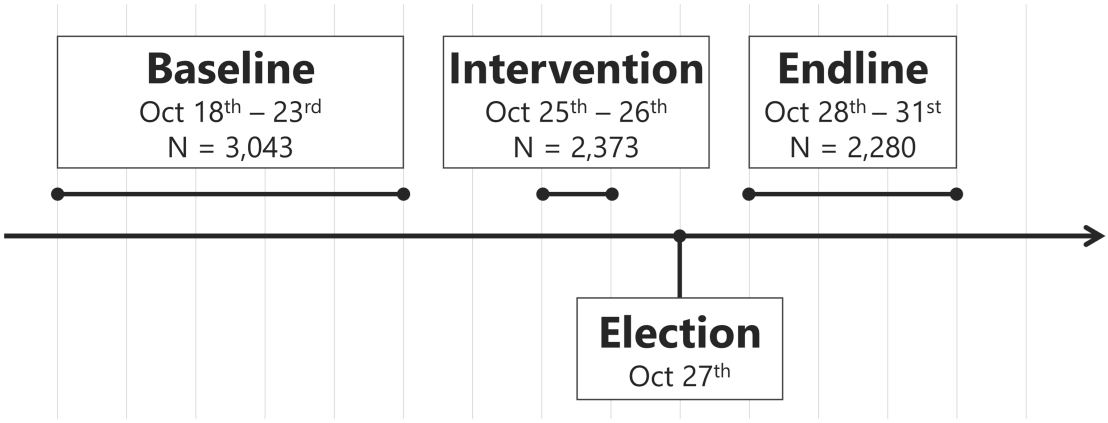


Figure 2: Information Treatment Screen

This survey project brings together a diverse group of participants, whose responses can be said to represent the views and attitudes of the average voter in Japan. In the previous survey, participants were asked how many of the other respondents considered that "omissions in political funding reports or kickbacks from political funds" are "completely unacceptable" or "somewhat unacceptable."

As a result, it was found that people believe that out of 100 participants, 67 considered "omissions in political funding reports or kickbacks from political funds" to be either "completely unacceptable" or "somewhat unacceptable."

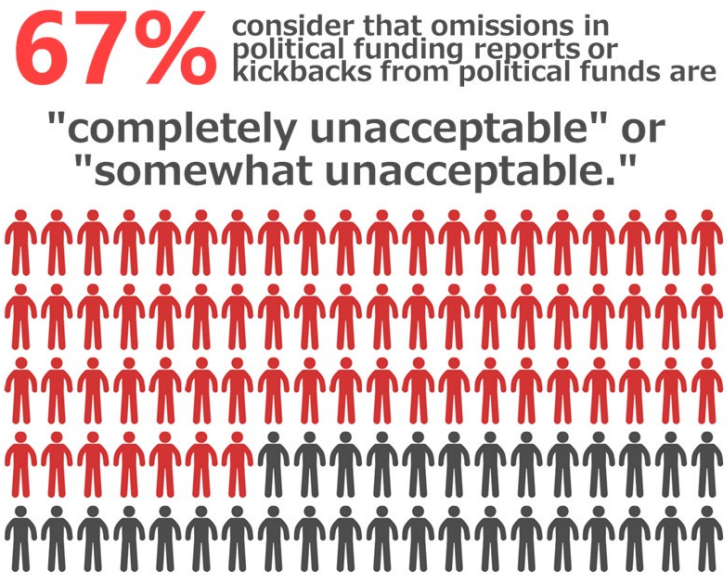
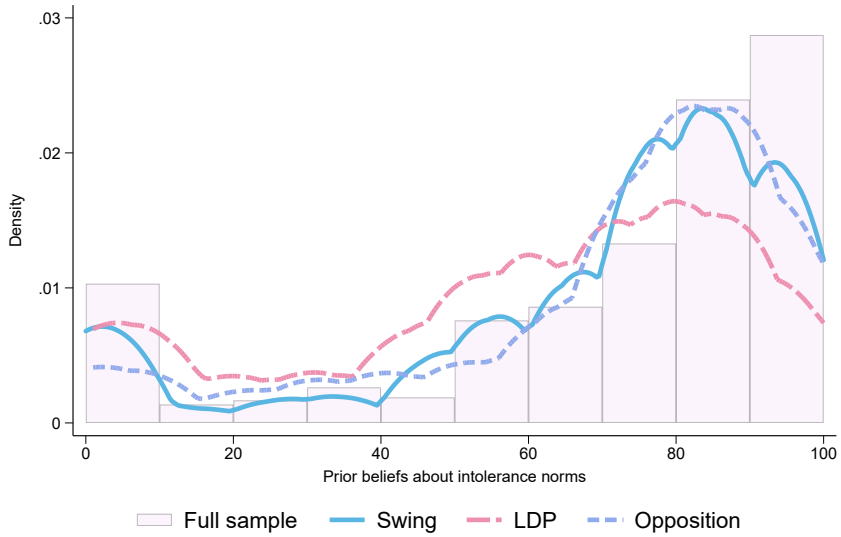
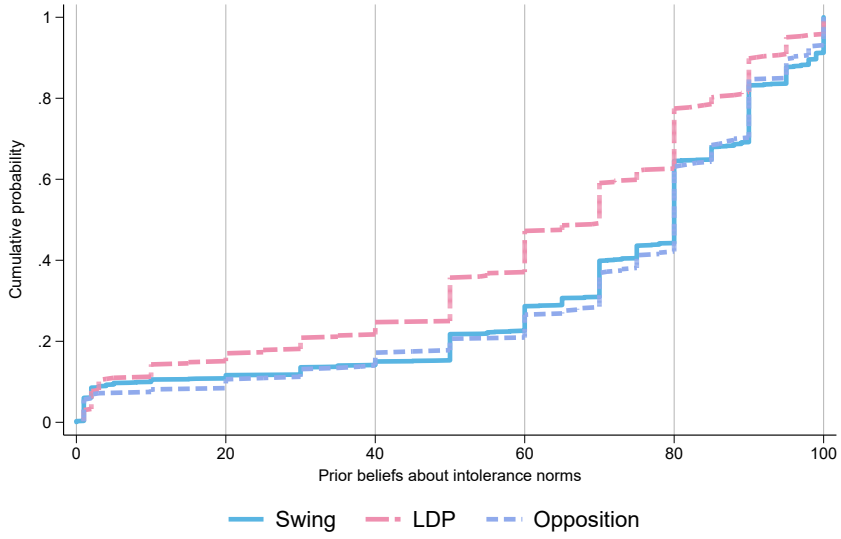


Figure 3: Distribution of Prior Beliefs about Intolerance Norms



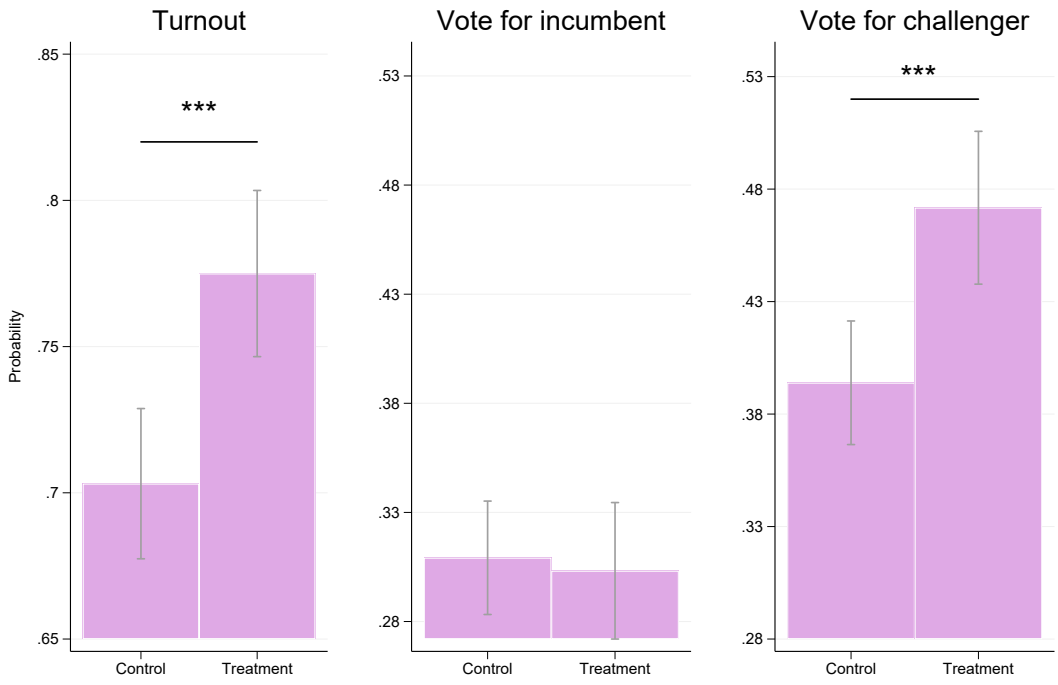
(a) Kernel density



(b) Cumulative distribution

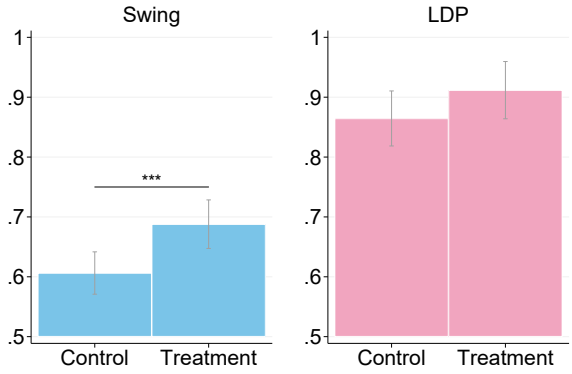
Note: The figures show the distributions of prior beliefs about intolerance norms. Panel (a) presents the kernel density estimates for swing voters, defined as those who do not support any particular party, LDP supporters, and opposition-party supporters. The background histogram reflects the full sample distribution. Panel (b) displays the cumulative distribution function for each group.

Figure 4: Main Effects of the Information Treatment on Voting Outcomes

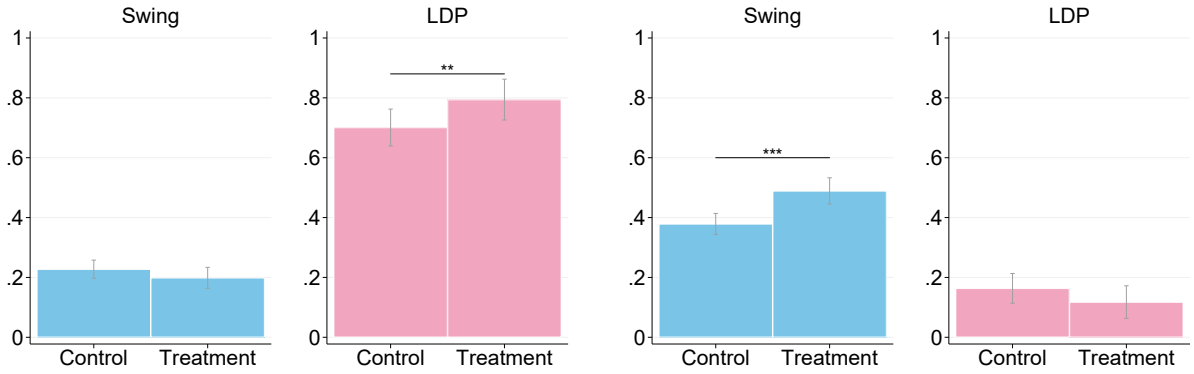


Note: Each bar represents the group average with a 95% confidence interval. The leftmost bars use a turnout dummy as the outcome; the middle bars use a dummy equal to one if the respondent voted for the incumbent; and the rightmost bars use a dummy equal to one if the respondent voted for a challenger. *, **, and *** indicate p <0.10, p <0.05, and p <0.01, respectively.

Figure 5: Heterogeneous Effects of the Information Treatment on Voting Outcomes (Swing Voters and LDP Supporters)



(a) Turnout

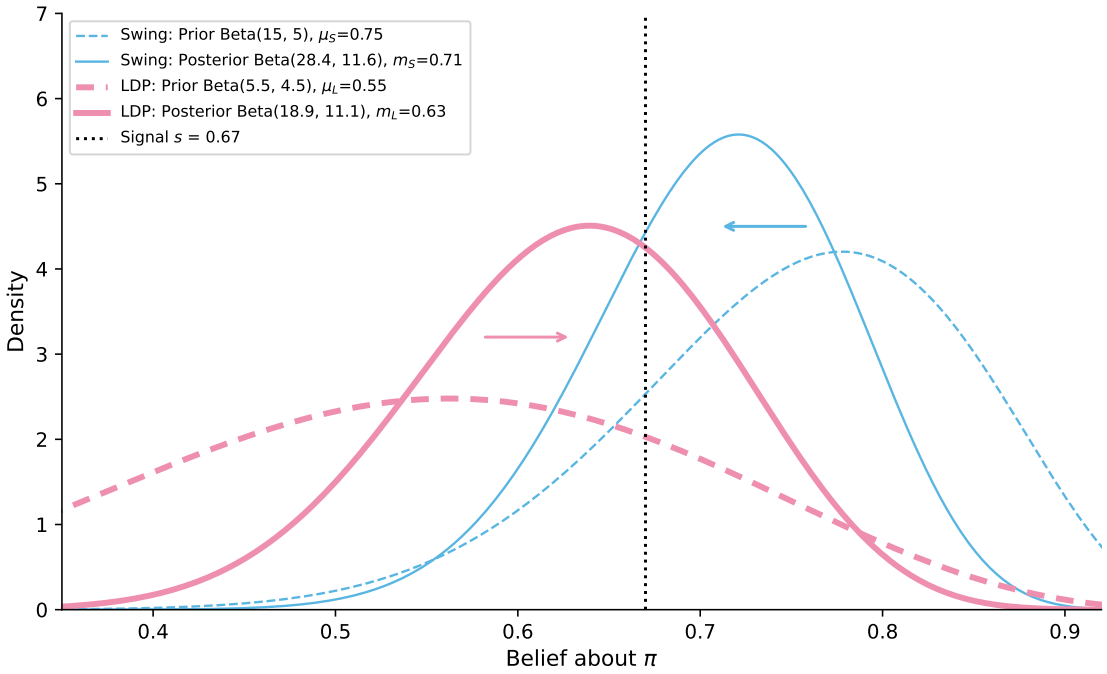


(b) Vote for incumbent

(c) Vote for challenger

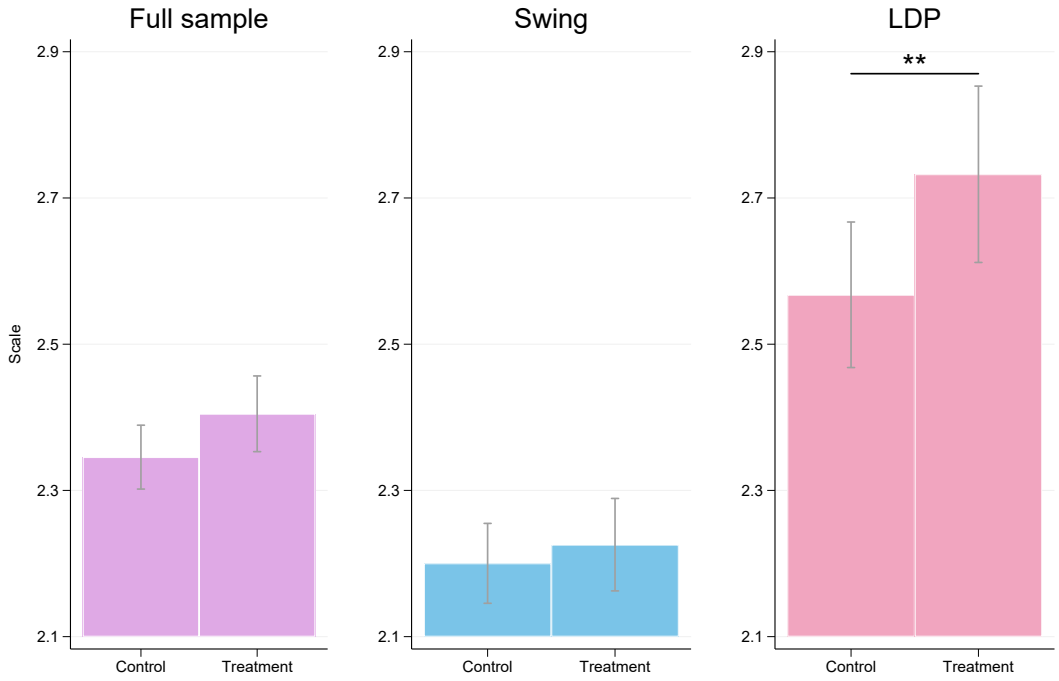
Note: Each bar represents the group average with a 95% confidence interval. Panel (a) uses a turnout dummy as the outcome; Panel (b) uses a dummy equal to one if the respondent voted for the incumbent; and Panel (c) uses a dummy equal to one if the respondent voted for a challenger. In each panel, the left two bars represent swing voters, and the right two bars represent LDP supporters. *, **, and *** indicate p <0.10, p <0.05, and p <0.01, respectively.

Figure 6: Opposite Belief Updating under Heterogeneous Priors



Note: The dashed curves show illustrative prior distributions for the punisher (swing) camp and the loyalist (LDP) camp. The vertical line indicates the common norm signal $s = 0.67$. Because the signal lies between the two prior means, the posterior for the group with $\mu_g > s$ shifts left while the posterior for the group with $\mu_g < s$ shifts right, as stated in Lemma 1.

Figure 7: Effects on Posterior Beliefs about Pivotality



Note: Each bar represents the group average with a 95% confidence interval. The outcome is posterior beliefs about pivotality, measured by responses to the question: "How much influence do you think your vote has on the election outcome?" (1 = Not at all, 2 = Not much, 3 = Some, 4 = A great deal) in the pre-election (post-treatment) survey. The leftmost bars represent the full sample, the middle bars represent swing voters, and the rightmost bars represent LDP supporters. *, **, and *** indicate p <0.10, p <0.05, and p <0.01, respectively.

Table 1: Turnout predictions under opposite belief updating ($\mu_L < s < \mu_S$).

	Punisher camp S: response to m_S	
Loyalist camp L: response to m_L	**Within-side substitutability** ($\kappa_S < 0$)	**Within-side complementarity** ($\kappa_S > 0$)
Across-side complementarity ($\kappa_L > 0$)	**Case A:** $\Delta\tau_L > 0$, $\Delta\tau_S > 0$ (both up)	**Case B:** $\Delta\tau_L > 0$, $\Delta\tau_S < 0$ (L up, S down)
Across-side substitutability ($\kappa_L < 0$)	**Case C:** $\Delta\tau_L < 0$, $\Delta\tau_S > 0$ (L down, S up)	**Case D:** $\Delta\tau_L < 0$, $\Delta\tau_S < 0$ (both down)

Table 2: Effects of the Information Treatment on Voting Outcomes, by District Type

Sample:	Fraud district			Clean district		
	Full	Swing	LDP	Full	Swing	LDP
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Dependent variable: Turnout					
Treatment	0.029	0.020	0.007	0.099	0.128	0.067
	(0.028)	(0.039)	(0.053)	(0.027)***	(0.038)***	(0.046)
Baseline controls	yes	yes	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes	yes	yes
Control mean	0.735	0.667	0.857	0.670	0.545	0.872
Control std.	(0.442)	(0.472)	(0.352)	(0.471)	(0.499)	(0.336)
R^2	0.04	0.06	0.08	0.05	0.06	0.03
N	994	597	163	1053	640	187
Panel B	Dependent variable: Vote for incumbent					
Treatment	−0.058	−0.100	0.059	0.031	0.031	0.128
	(0.028)**	(0.031)***	(0.070)	(0.030)	(0.035)	(0.062)**
Baseline controls	yes	yes	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes	yes	yes
Control mean	0.288	0.220	0.686	0.332	0.236	0.716
Control std.	(0.453)	(0.414)	(0.466)	(0.471)	(0.425)	(0.453)
R^2	0.02	0.03	0.12	0.02	0.03	0.08
N	994	597	163	1053	640	187
Panel C	Dependent variable: Vote for challenger					
Treatment	0.086	0.120	−0.052	0.068	0.097	−0.061
	(0.032)***	(0.041)***	(0.057)	(0.030)**	(0.038)**	(0.047)
Baseline controls	yes	yes	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes	yes	yes
Control mean	0.447	0.447	0.171	0.338	0.310	0.156
Control std.	(0.498)	(0.498)	(0.379)	(0.474)	(0.463)	(0.364)
R^2	0.04	0.07	0.10	0.03	0.05	0.09
N	994	597	163	1053	640	187

Note. Robust standard errors are in parentheses. In Panel A, the dependent variable is whether the respondent turned out to vote; in Panel B, whether the respondent voted for the incumbent; and in Panel C, whether the respondent voted for a challenger. All columns use actual voting outcomes from the post-election survey. Columns (1)-(3) include individuals in fraud constituencies, while Columns (4)-(6) include individuals in clean constituencies. Columns (1) and (4) report estimates for the full sample within each district type. Columns (2) and (5) restrict the sample to "Swing" respondents (those who do not support a particular party), whereas Columns (3) and (6) restrict the sample to "LDP" supporters. Baseline controls include educational attainment. *, **, and *** indicate $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Table 3: Effects of the Information Treatment on Voting Outcomes, by Political Alignment and Prior Beliefs about Intolerance Norms

Sample:	Swing		LDP	
	Stringent	Lenient	Stringent	Lenient
	(1)	(2)	(3)	(4)
Panel A	Dependent variable: Turnout			
Treatment	0.083	0.054	0.051	0.023
	(0.032)***	(0.051)	(0.047)	(0.052)
Baseline controls	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes
Control mean	0.614	0.591	0.840	0.886
Control std.	(0.487)	(0.493)	(0.368)	(0.319)
R^2	0.06	0.07	0.10	0.05
N	861	376	179	171
Panel B	Dependent variable: Vote for incumbent			
Treatment	−0.017	−0.054	0.099	0.123
	(0.028)	(0.047)	(0.070)	(0.068)*
Baseline controls	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes
Control mean	0.211	0.260	0.660	0.737
Control std.	(0.409)	(0.440)	(0.476)	(0.442)
R^2	0.02	0.02	0.05	0.09
N	861	376	179	171
Panel C	Dependent variable: Vote for challenger			
Treatment	0.100	0.108	−0.048	−0.100
	(0.034)***	(0.053)**	(0.059)	(0.049)**
Baseline controls	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes
Control mean	0.402	0.331	0.180	0.149
Control std.	(0.491)	(0.471)	(0.386)	(0.358)
R^2	0.05	0.07	0.06	0.10
N	861	376	179	171

Note. Robust standard errors are in parentheses. In Panel A, the dependent variable is whether the respondent turned out to vote; in Panel B, whether the respondent voted for the incumbent; and in Panel C, whether the respondent voted for a challenger. All columns use actual voting outcomes from the post-election survey. Columns labeled "Swing" comprise respondents who do not support a particular party, whereas columns labeled "LDP" comprise LDP supporters. Columns labeled "Stringent" contain respondents whose prior beliefs about intolerance norms are above the sample mean, whereas columns labeled "Lenient" contain respondents whose prior beliefs are at or below the mean. Baseline controls include educational attainment. *, **, and *** indicate $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Online Appendix for *Misperceived Social Norms and Political Accountability: Evidence and Theory*

Shuhei Kitamura* Ryo Takahashi† Katsunori Yamada‡

Abstract: This supplementary material includes additional text, tables, figures, and the survey questionnaire. For references to the tables and figures, please refer to the main text.

Contents

A.1 Sampling **2**

A.2 Theoretical Framework **3**
A.2.1 Model with endogenous camp membership 3
A.2.1.1 Environment 3
A.2.1.2 Sincere vote choice and endogenous camps 4
A.2.1.3 Turnout rules and election outcome 5
A.2.1.4 Group-ethical objectives 5
A.2.1.5 Baseline best-response conditions 6
A.2.2 Information and beliefs 6
A.2.2.1 RCT interpretation 6
A.2.2.2 Parametric signal structure 7
A.2.2.3 Opposite belief updating 7
A.2.3 Equilibria 8
A.2.3.1 Assumptions 8

A.3 Alternative proxy for perceived pivotality **9**

A.4 Figures and Tables **11**

A.5 Survey Questionnaire **32**
A.5.1 English-translated Version 32
A.5.2 Original Version 48

*Associate Professor. Institute of Social and Economic Research, The University of Osaka, 6-1 Mihogaoka, Ibaraki, Osaka, 567-0047 Japan. Email: kitamura@iser.osaka-u.ac.jp.

†Associate Professor. Graduate School of Economics, Waseda University. 1-6-1 Nishi-Waseda, Shinjuku, Tokyo, 169-0051 Japan. Email: ryo@waseda.jp.

‡Professor. Faculty of Economics, Kindai University. 228-3, Shin-Kamikosaka, Higashi-Osaka city, Osaka, 577-0813 Japan. Email: kyamada@kindai.ac.jp.

A.1 Sampling

The target constituencies consist of two types, both of whose incumbent candidates are from the LDP: fraud districts, where the incumbent politicians were involved in the slush fund issue, and clean districts, where the incumbent politicians were not implicated.

We selected target constituencies to enable clean comparisons by minimizing the influence of confounding factors. The initial pool comprised 43 LDP incumbents in the House of Representatives, excluding eight individuals who did not run for re-election due to suspension of civil rights, retirement, or health reasons.[1]

First, we excluded candidates who won through the proportional representation (PR) system (8) and focused on those who had won in single-member districts (SDs) in the previous election.[2] Second, we excluded candidates connected to another scandal involving the former Unification Church (16).[3] Third, we excluded those who would not be officially endorsed by the party in the election due to the fraud scandal (4) to avoid capturing this additional shock in our estimates. These exclusions left 15 candidates.

Each of these 15 LDP candidates represents a single SD constituency and is running for re-election.[4] We further restricted the sample to constituencies that (a) do not include any municipality split across multiple constituencies (3 excluded), and (b) were not significantly affected by the 2022 amendment to the Public Offices Election Law (1 excluded).

In prefectures where at least one fraud district exists, we selected clean districts with incumbent LDP candidates who were not involved in the slush fund scandal and that met the same criteria. We also excluded one fraud district located in a prefecture where no qualifying clean districts exist.

The final sample comprises 10 fraud districts and 15 clean districts. As shown in Figure A.1, these constituencies are geographically dispersed rather than clustered in a single

[1] This also excludes three individuals who ran for office but were not incumbent candidates in the House of Representatives: Younosuke Kano, Tamayo Marukawa, and Yasuhide Nakayama.

[2] Some PR election winners actually *lost* in their SD contests, making comparisons difficult. PR candidates also represent large multi-prefecture constituencies, which differ substantially from the local constituencies considered here. Furthermore, PR outcomes depend on the party's list rather than local constituents' preferences.

[3] This scandal attracted significant public attention in the 2020s, particularly after the assassination of former Prime Minister Shinzo Abe in July 2022. The LDP has been accused of maintaining close ties with the Unification Church.

[4] Japan does not impose term limits for political office.

region: Iwate (fraud: 1; clean: 1), Tochigi (1,1), Saitama (1,4), Chiba (1,2), Tokyo (1,3), Ishikawa (1,1), Nagano (1,1), Aichi (2,2), and Mie Prefectures (1,1), where the first and second numbers in parentheses indicate the number of fraud and clean districts, respectively.

Table A.2 compares these constituencies with others, showing that they are similar to each other and also to non-targeted constituencies, whether or not the incumbents were implicated in the slush fund issue.

The target population for the study consists of 2,800 registered voters aged between their 20s and 60s in these target constituencies. We determined this target number based on the power calculations described in the Pre-Analysis Plan (PAP).

A.2 Theoretical Framework

A.2.1 Model with endogenous camp membership

A.2.1.1 Environment

The electorate is a unit-mass continuum of voters indexed by $i \in [0,1]$. There are two candidates: an incumbent I (scandal-tainted) and a challenger C (clean).

Each voter draws a type $(\theta_i, \eta_i, \phi_i, c_i)$ where

- $\theta_i > 0$ is the moral cost from the incumbent remaining in office;
- $\eta_i \in \{0,1\}$ indicates partisan loyalty ($\eta_i = 1$ loyalist, $\eta_i = 0$ swing);
- $\phi_i > 0$ is the identity payoff a loyalist receives if the incumbent remains in office;
- $c_i \in [0,1]$ is the opportunity cost of turning out.

Assume c_i is independent of $(\theta_i, \eta_i, \phi_i)$ and is uniformly distributed on $[0,1]$.

Let $w \in \{I, C\}$ denote the election winner and $t_i \in \{0,1\}$ denote turnout. The baseline payoff is

$$u_i \;=\; -c_i t_i \;+\; \mathbf{1}\{w = I\}\,(\eta_i \phi_i - \theta_i). \tag{1}$$

Remark 1. A "civic duty" benefit d can be added as $(d - c_i)t_i$ without affecting vote choice. It raises baseline turnout in both camps (by shifting effective costs down uniformly), but

does not change the comparative-statics predictions of the information treatment. Because our treatment provides information about the fraction of others rather than civic duty *per se*, it is more natural to model it as shifting beliefs that map into τ_g rather than as a change in d; a pure d-channel would predict more uniform effects across camps and would not generate the observed heterogeneity. See Section 6 in the main text for discussion.

A.2.1.2 Sincere vote choice and endogenous camps

Define the utility advantage of the challenger over the incumbent conditional on turning out:

$$\Delta_i \equiv \theta_i - \eta_i \phi_i. \tag{2}$$

Conditional on turning out, the voter votes sincerely:

$$v_i = \begin{cases} C & \text{if } \Delta_i \geq 0, \\ I & \text{if } \Delta_i < 0. \end{cases}$$

This partitions voters into two *endogenous camps*:

$$S\colon\ \Delta_i \geq 0 \quad \text{(challenger camp / punishers)}, \qquad L\colon\ \Delta_i < 0 \quad \text{(incumbent camp / loyalists)}.$$

Let

$$\pi \equiv \mathbb{P}(\Delta_i \geq 0) \tag{3}$$

denote the population share of the challenger camp S.[5] This is the key latent object that summarizes "others' willingness to punish" in reduced form.

Mapping to swing/loyalist labels. Because $\eta_i = 0$ implies $\Delta_i = \theta_i \geq 0$, all swing voters are in camp S. Loyalists ($\eta_i = 1$) are in camp L unless θ_i is large enough to overcome identity payoffs ϕ_i.

[5] Equivalently, one can parameterize beliefs by the loyalist share $q \equiv 1 - \pi$; this is a one-to-one transformation, so the comparative statics and predictions are unchanged.

A.2.1.3 Turnout rules and election outcome

Each camp uses a cutoff rule in voting costs:

$$t_i = \mathbf{1}\{c_i \le \tau_g\} \quad \text{for } i \in g,\ g \in \{L,S\}, \tag{4}$$

where $\tau_L, \tau_S \in [\varepsilon, 1]$ for some $\varepsilon > 0$; we focus on interior solutions. Since $c_i \sim \text{Unif}[0,1]$, turnout in camp g equals τ_g.

Vote totals (shares) for each candidate are

$$V_I = (1-\pi)\tau_L, \qquad V_C = \pi\tau_S.$$

The incumbent wins iff $V_I \ge V_C$, equivalently

$$\pi \le k(\tau_L, \tau_S) \equiv \frac{\tau_L}{\tau_L + \tau_S}. \tag{5}$$

Expected per-capita turnout cost under cutoff τ_g is

$$\mathbb{E}[c_i \mathbf{1}\{c_i \le \tau_g\}] = \int_0^{\tau_g} c\,dc = \frac{\tau_g^2}{2}.$$

A.2.1.4 Group-ethical objectives

Let $x > 0$ be the value of an incumbent victory for camp L and $b > 0$ the value of a challenger victory for camp S. In the microfoundation, these can be interpreted as $b = \mathbb{E}[\Delta_i \mid \Delta_i \ge 0] \ge 0$ and $x = \mathbb{E}[-\Delta_i \mid \Delta_i < 0] \ge 0$. Given a belief about π, camp payoffs are

$$U_L(\tau_L; \tau_S) = x \cdot \mathbb{P}(\pi \le k(\tau_L, \tau_S)) - \frac{\tau_L^2}{2}, \tag{6}$$

$$U_S(\tau_S; \tau_L) = b \cdot \mathbb{P}(\pi > k(\tau_L, \tau_S)) - \frac{\tau_S^2}{2}. \tag{7}$$

A cutoff equilibrium is a pair (τ_L^*, τ_S^*) in which each camp's cutoff maximizes its own payoff given the other's.

A.2.1.5 Baseline best-response conditions

Fix any information realization and let camp g's posterior CDF for π be $G_g(\cdot)$ with density $g_g(\cdot)$. Then the interim payoffs can be written as

$$U_L(\tau_L;\tau_S) = x\,G_L\big(k(\tau_L,\tau_S)\big) - \frac{\tau_L^2}{2}, \qquad U_S(\tau_S;\tau_L) = b\Big(1 - G_S\big(k(\tau_L,\tau_S)\big)\Big) - \frac{\tau_S^2}{2}.$$

If an interior best response exists and g_g is continuous, the (interior) first-order conditions are

$$\tau_L = x\,g_L\big(k(\tau_L,\tau_S)\big)\cdot\frac{\tau_S}{(\tau_L+\tau_S)^2}, \tag{8}$$

$$\tau_S = b\,g_S\big(k(\tau_L,\tau_S)\big)\cdot\frac{\tau_L}{(\tau_L+\tau_S)^2}, \tag{9}$$

with $k(\tau_L,\tau_S) = \tau_L/(\tau_L+\tau_S)$. These are *necessary* for interior optima but do not by themselves guarantee existence or uniqueness of a pure equilibrium, since global concavity of U_g in τ_g is not automatic.

A.2.2 Information and beliefs

A.2.2.1 RCT interpretation

Let P_g denote camp g's prior over π, and let $\mathcal{H}$ denote an individual's information set. For turnout decisions, the relevant belief object is the one-dimensional posterior mean

$$m_g \equiv \mathbb{E}_{P_g(\cdot|\mathcal{H})}[\pi].$$

Because priors can differ across camps, their posterior means can differ even after conditioning on a common signal: $m_L \neq m_S$.

To interpret the RCT without introducing an explicit treatment dummy in the model, let $\mathcal{H}^{\text{ctrl}}$ and $\mathcal{H}^{\text{tr}}$ denote the information sets in control and treatment. The treatment corresponds to an information refinement $\mathcal{H}^{\text{ctrl}} \rightsquigarrow \mathcal{H}^{\text{tr}}$ for treated individuals, and its behavioral effects operate through the induced shift in m_g.

A.2.2.2 Parametric signal structure

We represent the treatment information as a private message $s \in \{0, 1/n, \ldots, 1\}$, which is a scalar signal derived from the displayed statistic. In the experiment, the displayed statistic is the average reported belief about the share of others who are intolerant; we interpret it as a noisy signal about the latent share of "punishers." For tractability, we model this signal as binomial: conditional on π, let $K \mid \pi \sim \text{Binomial}(n, \pi)$ and set $s \equiv K/n$, i.e.

$$\mathbb{P}\Big(s = \frac{k}{n} \;\Big|\; \pi\Big) = \binom{n}{k} \pi^k (1-\pi)^{n-k}, \qquad k = 0, 1, \ldots, n. \tag{10}$$

Treated individuals observe s; control individuals do not.

Allow camps to start from different subjective priors,

$$\pi \sim \text{Beta}(\alpha_g, \beta_g), \qquad g \in \{L, S\}, \tag{11}$$

with prior mean $\mu_g = \mathbb{E}_{P_g}[\pi] = \alpha_g/(\alpha_g + \beta_g)$. After observing s, Bayes' rule implies the posterior

$$\pi \mid s \sim \text{Beta}(\alpha_g + ns, \beta_g + n(1-s)). \tag{12}$$

The posterior mean is

$$m_g(s) \equiv \mathbb{E}_{P_g(\cdot|s)}[\pi] = \frac{\alpha_g + ns}{\alpha_g + \beta_g + n}. \tag{13}$$

A.2.2.3 Opposite belief updating

Define the prior mean $\mu_g = \mathbb{E}_{P_g}[\pi] = \alpha_g/(\alpha_g + \beta_g)$ and the posterior mean $m_g(s)$ in (13).

Lemma A1 (Opposite belief updating under heterogeneous priors)**.** *If the realized message satisfies*

$$\mu_L < s < \mu_S,$$

then $m_L(s) > \mu_L$ *and* $m_S(s) < \mu_S$.

Proof. Fix a camp g and write $\mu_g = \alpha_g/(\alpha_g + \beta_g)$ and $m_g(s) = (\alpha_g + ns)/(\alpha_g + \beta_g + n)$. Compute

$$m_g(s) - \mu_g = \frac{\alpha_g + ns}{\alpha_g + \beta_g + n} - \frac{\alpha_g}{\alpha_g + \beta_g} = \frac{n(\alpha_g + \beta_g)(s - \mu_g)}{(\alpha_g + \beta_g + n)(\alpha_g + \beta_g)}.$$

Hence $\text{sign}\{m_g(s) - \mu_g\} = \text{sign}\{s - \mu_g\}$. If $\mu_L < s$ then $m_L(s) > \mu_L$; if $s < \mu_S$ then $m_S(s) < \mu_S$. □

Under the opposite-updating configuration $\mu_L < s < \mu_S$, Lemma A1 implies

$$\Delta m_L > 0, \qquad \Delta m_S < 0.$$

Let

$$\kappa_L \equiv \frac{\partial \tau_L^*}{\partial m_L}, \qquad \kappa_S \equiv \frac{\partial \tau_S^*}{\partial m_S}.$$

Then the four sign combinations yield the turnout predictions summarized in Table 1 of the main text.

A.2.3 Equilibria

The first-order conditions (8)–(9) characterize *interior* best responses when they exist, but they do not by themselves guarantee that a cutoff equilibrium exists (global concavity of each camp's objective in its own cutoff is not automatic). This section therefore records a simple high-level condition under which the turnout subgame admits a *pure-strategy* equilibrium in cutoff rules. Throughout, we maintain the interior restriction $\tau_g \in [\varepsilon, 1]$. In particular, this keeps $k(\tau_L, \tau_S)$ in (5) well defined because $\tau_L + \tau_S \geq 2\varepsilon > 0$.

A.2.3.1 Assumptions

For each camp $g \in \{L, S\}$, let s_g denote a one-dimensional private signal (type) summarizing camp g's information about π (which also allows $s_g = s$).

Assumption A1 (Regularity and strict concavity)**.** *For each camp g and each signal s_g:*

(1) The action set is the compact interval $[\varepsilon, 1]$.

(2) The interim payoff $U_g(\tau_g, \tau_{-g} \mid s_g)$ is bounded and continuous in (τ_g, τ_{-g}) and measurable in s_g.

(3) (Strict concavity in own action.) For each fixed (s_g, τ_{-g}), the map $\tau_g \mapsto U_g(\tau_g, \tau_{-g} \mid s_g)$ is strictly concave on $[\varepsilon, 1]$.

Assumption A1(1) matches the maintained restriction $\tau_g \in [\varepsilon, 1]$ in (4). Assumption A1(2) is satisfied in the baseline model because the payoff functions in (6)–(7) are bounded (probabilities minus turnout costs) and continuous on $[\varepsilon, 1]^2$, where $k(\tau_L, \tau_S)$ is well defined and continuous. Assumption A1(3) is a sufficient curvature condition: it is imposed here to obtain a clean existence/purity result.

Proposition A1 (Existence of a pure-strategy cutoff equilibrium)**.** *Maintain Assumption A1. Then the Bayesian game admits a Bayesian Nash equilibrium in measurable* pure *cutoff rules* $(\tau_L^*(\cdot), \tau_S^*(\cdot))$ *with* $\tau_g^*(s_g) \in [\varepsilon, 1]$ *for each* $g \in \{L, S\}$.

Proof. In the Beta–Binomial specification, the message takes finitely many values $S = \{0, 1/n, \ldots, 1\}$, so $|S| = n+1$. A (pure) cutoff strategy for camp g is a vector $\tau_g = (\tau_g(s))_{s \in S} \in [\varepsilon, 1]^{|S|}$. Let $\mathscr{T}_g \equiv [\varepsilon, 1]^{|S|}$ and $\mathscr{T} \equiv \mathscr{T}_L \times \mathscr{T}_S$. Then $\mathscr{T}$ is a nonempty, compact, convex subset of $\mathbb{R}^{2|S|}$.

For each g, let $\Pi_g(\tau_L, \tau_S)$ denote camp g's ex ante expected payoff induced by $(\tau_L, \tau_S) \in \mathscr{T}$ (expectation taken over signals and π). By Assumption A1(2), Π_g is continuous in (τ_L, τ_S). By Assumption A1(3), $\Pi_g(\cdot, \tau_{-g})$ is strictly concave on $\mathscr{T}_g$ for each fixed τ_{-g}.

Fix τ_{-g}. By continuity and compactness, $\Pi_g(\cdot, \tau_{-g})$ attains a maximum on $\mathscr{T}_g$. Strict concavity implies that the maximizer is unique; denote it by $BR_g(\tau_{-g}) \in \mathscr{T}_g$. By Berge's maximum theorem, the argmax correspondence is upper hemicontinuous; since it is single-valued, BR_g is continuous.

Define the joint best-response map $BR : \mathscr{T} \to \mathscr{T}$ by $BR(\tau_L, \tau_S) = (BR_L(\tau_S), BR_S(\tau_L))$. This map is continuous and maps $\mathscr{T}$ into itself. By Brouwer's fixed point theorem, BR has a fixed point (τ_L^*, τ_S^*), which is a pure-strategy Bayesian Nash equilibrium in cutoff rules. □

A.3 Alternative proxy for perceived pivotality

As a robustness check, we consider an alternative proxy for perceived pivotality based on respondents' expectations about the share of voters in their constituency who will support the incumbent. In the model, beliefs about the incumbent's expected vote share map one for one into beliefs about the challenger's expected vote share, and thus speak directly to perceived electoral competitiveness. However, this proxy comes with an important caveat. Because the question explicitly refers to the incumbent, responses may partly reflect expressive reporting

or perceived viability rather than purely probabilistic beliefs about vote shares. In that case, any estimated treatment effect may mix belief updating with preference expression.

With this caveat in mind, Panel A of Table A.13 yields a pattern similar to our baseline pivotality measure in that the treatment effect is concentrated among loyalists. The sign, however, is positive: treated loyalists report that a larger share of voters will back the incumbent than do their control group counterparts. Consistent with an expressive component, we also find a positive treatment effect among loyalists on beliefs about the share of voters who *should* support the incumbent. Taken together, these results suggest that the intervention strengthens loyalists' normative support for the incumbent, and that this may in turn be reflected in their reported expectations about how others will vote.

For swing voters, Table A.13 shows only small increases in the belief that others will or should vote for the incumbent, with at best weak statistical support. One possible interpretation is modest downward updating about how widely electoral punishment will occur. Given the small magnitudes and the incumbent-referenced wording of these items, we view this interpretation as exploratory.

References

Correia, S., P. Guimarães, and T. Zylkin (2020). Fast Poisson estimation with high-dimensional fixed effects. *The Stata Journal 20*(1), 95–115.

Lee, D. S. (2009). Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects. *The Review of Economic Studies 76*(3), 1071–1102.

Roodman, D., J. MacKinnon, M. Nielsen, and M. Webb (2019). Fast and wild: bootstrap inference in Stata using boottest. *Stata Journal 19*(1), 4–60.

Tauchmann, H. (2014). Lee (2009) treatment-effect bounds for nonrandom sample selection. *Stata Journal 14*(4), 841–853.

Young, A. (2019). Channeling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results. *The Quarterly Journal of Economics 134*(2), 557–598.

A.4 Figures and Tables

Figure A.1: Target Constituencies

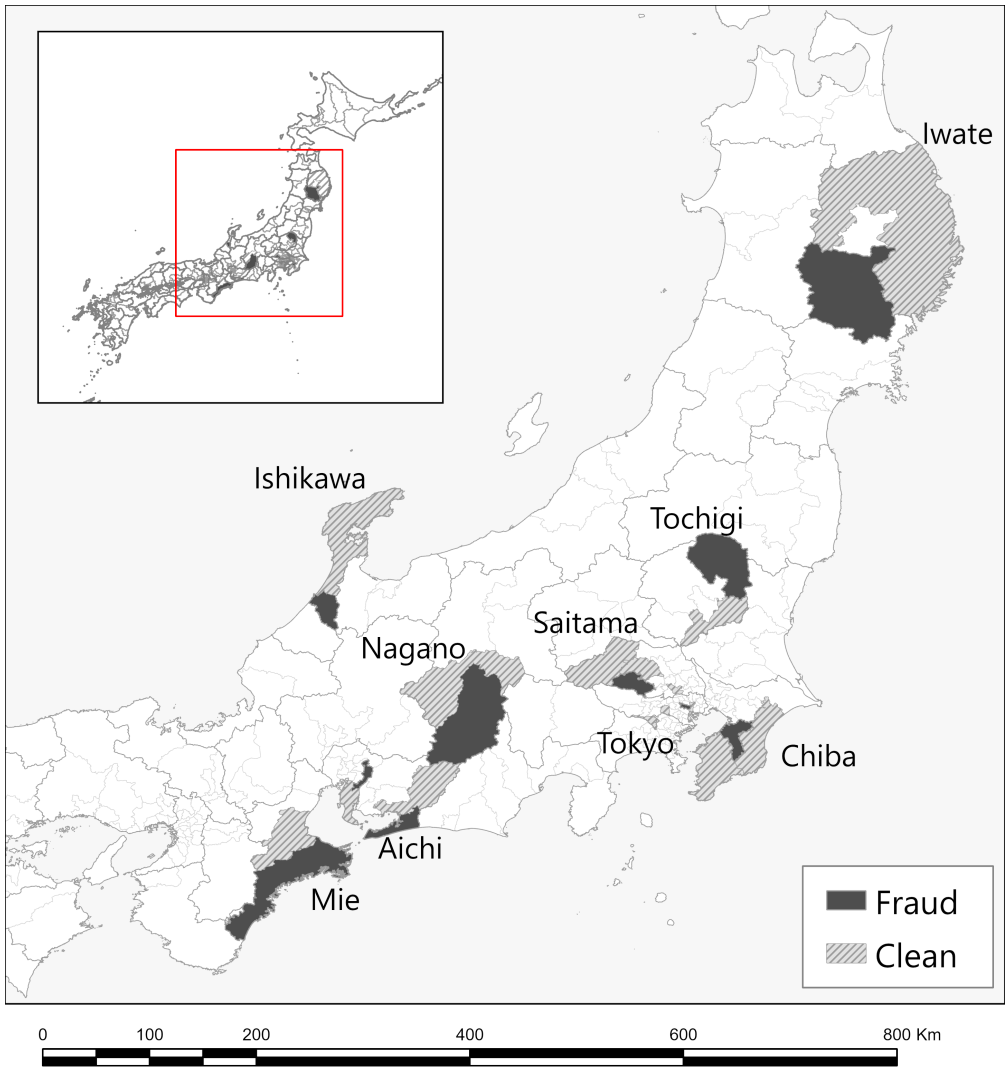


Table A.1: Post-treatment Variables

Variable	Definition	Incentivized?
Panel A: Pre-election		
Turnout (before election)	Intend to turn out = 1, abstain = 0	No
Vote for incumbent (before election)	Intend to vote for incumbent = 1, challenger = 0	No
Vote for challenger (before election)	Intend to vote for challenger = 1, incumbent = 0	No
Others will vote	Perceived share of other voters who *will* vote for incumbent	Yes
Others should vote	Perceived share of other voters who *should* vote for incumbent	Yes
Policy support	Intended support for transparency-enhancing policies (1 = Do not support at all ... 4 = Strongly support)	No
Donation	Intended amount of donation $\in [0, 50, 000]$	No
Competence	Perceived share of other voters who view the incumbent as "very competent" or "competent"	Yes
Trustworthiness	Perceived share of other voters who view the incumbent as "very trustworthy" or "trustworthy"	Yes
Panel B: Post-election		
Turnout (after election)	Turned out = 1, abstained = 0	No
Vote for incumbent (after election)	Voted for incumbent = 1, challenger = 0	No
Vote for challenger (after election)	Voted for challenger = 1, incumbent = 0	No

Table A.2: Target and Non-target Constituencies

	(1) Fraud	(2) Clean	(3) Target	(4) Non target	(5) (1) vs. (2)	(6) (3) vs. (4)
Turnout	0.533	0.528	0.530	0.522	0.762	0.270
	(0.036)	(0.040)	(0.037)	(0.037)		
Log(registered)	12.692	12.732	12.716	12.786	0.585	0.051
	(0.163)	(0.185)	(0.174)	(0.170)		
Female-male ratio	1.037	1.035	1.036	1.077	0.911	0.002
	(0.049)	(0.037)	(0.041)	(0.065)		
Vote share (LDP)	0.220	0.254	0.240	0.221	0.124	0.149
	(0.054)	(0.050)	(0.053)	(0.065)		
Vote share (DP)	0.191	0.228	0.212	0.209	0.227	0.822
	(0.062)	(0.070)	(0.068)	(0.066)		
N	10	15	25	264		

Note. Standard errors are in parentheses. Constituencies without candidates from the relevant parties were excluded when calculating vote shares. The last two columns report p values from pairwise tests of equality of means for Fraud versus Clean and Target versus Non target.

Table A.3: Balance Check

	(1) Control	(2) Treatment	(1) vs. (2)	N
Female	0.397	0.374	0.278	2280
	(0.013)	(0.016)		
20s	0.026	0.021	0.457	2280
	(0.004)	(0.005)		
30s	0.081	0.060	0.058	2280
	(0.007)	(0.008)		
40s	0.212	0.204	0.613	2280
	(0.011)	(0.013)		
50s	0.337	0.358	0.301	2280
	(0.013)	(0.016)		
60s	0.344	0.358	0.502	2280
	(0.013)	(0.016)		
Education years	14.044	14.318	0.001	2280
	(0.053)	(0.062)		
Married	0.576	0.595	0.366	2280
	(0.013)	(0.016)		
Log income	14.549	14.588	0.206	2280
	(0.019)	(0.023)		
N	1366	914		

Note. Standard errors are in parentheses. The third column shows p-values.

Table A.4: Attrition Check

	(1) Not attrited	(2) Attrited	(1) vs. (2)	N
Female	0.388	0.558	0.001	2375
	(0.010)	(0.051)		
20s	0.024	0.021	0.869	2375
	(0.003)	(0.015)		
30s	0.073	0.063	0.722	2375
	(0.005)	(0.025)		
40s	0.209	0.200	0.837	2375
	(0.009)	(0.041)		
50s	0.345	0.274	0.150	2375
	(0.010)	(0.046)		
60s	0.350	0.442	0.065	2375
	(0.010)	(0.051)		
Education years	14.154	14.242	0.662	2375
	(0.041)	(0.181)		
Married	0.584	0.695	0.031	2375
	(0.010)	(0.047)		
Log income	14.565	14.459	0.153	2375
	(0.015)	(0.067)		
N	2280	95		

Note. Standard errors are in parentheses. The third column shows p-values.

Table A.5: Summary Statistics

	Mean	Std	Min	p10	p50	p90	Max	N
A. Outcomes								
Turnout (before election)	0.60	0.49	0.00	0.00	1.00	1.00	1.00	2280
Turnout (after election)	0.73	0.44	0.00	0.00	1.00	1.00	1.00	2047
Vote for incumbent (before election)	0.23	0.42	0.00	0.00	0.00	1.00	1.00	2280
Vote for incumbent (after election)	0.31	0.46	0.00	0.00	0.00	1.00	1.00	2047
Vote for challenger (before election)	0.37	0.48	0.00	0.00	0.00	1.00	1.00	2280
Vote for challenger (after election)	0.43	0.49	0.00	0.00	0.00	1.00	1.00	2047
Perceived pivotality	2.37	0.81	1.00	1.00	2.00	3.00	4.00	2280
Others will vote	47.92	23.01	0.00	5.00	50.00	75.00	100.00	2280
Others should vote	45.07	25.33	0.00	2.50	50.00	80.00	100.00	2280
Policy support	2.84	0.97	1.00	1.00	3.00	4.00	4.00	2280
Donation	2790.38	7928.36	0.00	0.00	0.00	10000.00	50000.00	2280
Competence	32.15	21.75	0.00	3.00	30.00	60.00	100.00	2280
Trustworthiness	31.32	21.52	0.00	3.00	30.00	60.00	100.00	2280
B. Treatment dummy								
Treatment	0.40	0.49	0.00	0.00	0.00	1.00	1.00	2280
C. Covariates								
Female	0.39	0.49	0.00	0.00	0.00	1.00	1.00	2280
20s	0.02	0.15	0.00	0.00	0.00	0.00	1.00	2280
30s	0.07	0.26	0.00	0.00	0.00	0.00	1.00	2280
40s	0.21	0.41	0.00	0.00	0.00	1.00	1.00	2280
50s	0.35	0.48	0.00	0.00	0.00	1.00	1.00	2280
60s	0.35	0.48	0.00	0.00	0.00	1.00	1.00	2280
Education	14.15	1.93	9.00	12.00	14.00	16.00	16.00	2280
Married	0.58	0.49	0.00	0.00	1.00	1.00	1.00	2280
Log income	14.56	0.71	12.61	13.53	14.63	15.42	17.91	2280
D. Party identity								
Swing	0.64	0.48	0.00	0.00	1.00	1.00	1.00	2175
LDP	0.17	0.37	0.00	0.00	0.00	1.00	1.00	2175
Opposition	0.15	0.35	0.00	0.00	0.00	1.00	1.00	2175

Table A.6: Effects of the Information Treatment on Voting Outcomes

	Dependent variable:		
	Turnout	Vote for incumbent	Vote for challenger
	(1)	(2)	(3)
Panel A: Post-election			
Treatment	0.064	−0.009	0.073
	(0.019)***	(0.021)	(0.022)***
Baseline controls	yes	yes	yes
Prefecture F.E.	yes	yes	yes
Control mean	0.703	0.309	0.394
Control std.	(0.457)	(0.462)	(0.489)
R^2	0.04	0.00	0.02
N	2047	2047	2047
Panel B: Pre-election			
Treatment	0.100	0.017	0.084
	(0.021)***	(0.018)	(0.021)***
Baseline controls	yes	yes	yes
Prefecture F.E.	yes	yes	yes
Control mean	0.552	0.219	0.333
Control std.	(0.497)	(0.414)	(0.471)
R^2	0.03	0.01	0.03
N	2280	2280	2280

Note. Robust standard errors are in parentheses. In Panel A, the dependent variables are respondents' actual voting outcomes from the post-election survey; in Panel B, they are the stated vote intentions from the pre-election (post-treatment) survey. Column (1) reports turnout, Column (2) reports a vote for the incumbent candidate, and Column (3) reports a vote for a challenger. Baseline controls include educational attainment. *, **, and *** indicate $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Table A.7: Effects of the Information Treatment on Voting Outcomes, by Political Alignment

Survey:	Post-election			Pre-election		
Sample:	Swing	LDP	Opposition	Swing	LDP	Opposition
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Dependent variable: Turnout					
Treatment	0.074	0.044	−0.006	0.132	0.049	0.010
	(0.027)***	(0.034)	(0.028)	(0.027)***	(0.041)	(0.032)
Baseline controls	yes	yes	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes	yes	yes
Control mean	0.606	0.864	0.941	0.422	0.788	0.914
Control std.	(0.489)	(0.343)	(0.237)	(0.494)	(0.409)	(0.281)
R^2	0.06	0.02	0.03	0.05	0.02	0.03
N	1237	350	309	1392	364	320
Panel B	Dependent variable: Vote for incumbent					
Treatment	−0.031	0.103	−0.018	0.007	0.093	0.043
	(0.024)	(0.047)**	(0.041)	(0.019)	(0.050)*	(0.040)
Baseline controls	yes	yes	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes	yes	yes
Control mean	0.228	0.701	0.172	0.127	0.626	0.126
Control std.	(0.420)	(0.459)	(0.378)	(0.333)	(0.485)	(0.332)
R^2	0.01	0.04	0.04	0.01	0.05	0.06
N	1237	350	309	1392	364	320
Panel C	Dependent variable: Vote for challenger					
Treatment	0.105	−0.059	0.012	0.124	−0.045	−0.033
	(0.028)***	(0.037)	(0.047)	(0.026)***	(0.037)	(0.048)
Baseline controls	yes	yes	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes	yes	yes
Control mean	0.379	0.164	0.769	0.295	0.162	0.789
Control std.	(0.485)	(0.371)	(0.423)	(0.456)	(0.369)	(0.409)
R^2	0.05	0.05	0.04	0.05	0.07	0.05
N	1237	350	309	1392	364	320

Note. Robust standard errors are in parentheses. In Panel A, the dependent variable is whether the respondent turned out (or intends to turn out) to vote; in Panel B, whether the respondent voted (or intends to vote) for the incumbent; and in Panel C, whether the respondent voted (or intends to vote) for a challenger. Columns (1)–(3) use actual voting outcomes from the post-election survey, whereas Columns (4)–(6) use stated vote intentions from the pre-election (post-treatment) survey. Columns labeled "Swing" include respondents who do not support a particular party, those labeled "LDP" include LDP supporters, and those labeled "Opposition" include supporters of opposition parties. Baseline controls include educational attainment. *, **, and *** indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively.

Table A.8: Summary of Main Results

Sample:		Swing			LDP		
	Full sample	All	Stringent	Lenient	All	Stringent	Lenient
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Turnout	+	+	+	(+)	(+)	(+)	(+)
Vote for incumbent	(−)	(−)	(−)	(−)	+	(+)	+
Vote for challenger	+	+	+	+	(−)	(−)	−

Note: Rows labeled "Turnout," "Vote for incumbent," and "Vote for challenger" report results for, respectively, (i) whether the respondent turned out to vote, (ii) whether the respondent voted for the incumbent, and (iii) whether the respondent voted for a challenger. Column (1) reports results for the full sample. Columns labeled "Swing" include respondents who do not support any particular party, whereas columns labeled "LDP" include LDP supporters. Columns labeled "Stringent" comprise respondents whose prior beliefs about intolerance norms exceed the sample mean; columns labeled "Lenient" comprise those at or below the mean. Columns labeled "All" pool these sub-samples. Plus and minus signs indicate the direction of the estimated coefficients. Signs shown in parentheses indicate estimates that are not statistically significant at conventional levels. Regression results are shown in Tables A.6, A.7, 3, A.11, and A.12.

Table A.9: Effects of the Information Treatment on Support for Transparency in Political Funding

Sample:	Swing			LDP		
	All	Stringent	Lenient	All	Stringent	Lenient
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Dependent variable: Policy support					
Treatment	0.156	0.113	0.207	0.079	0.174	−0.077
	(0.054)***	(0.065)*	(0.098)**	(0.079)	(0.114)	(0.109)
Baseline controls	yes	yes	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes	yes	yes
Control mean	2.725	2.837	2.500	2.865	2.933	2.803
Control std.	(0.999)	(1.005)	(0.952)	(0.724)	(0.737)	(0.710)
R^2	0.02	0.01	0.04	0.06	0.06	0.12
N	1392	965	427	364	187	177
Panel B	Dependent variable: Donation					
Treatment	−312.400	6.791	−1122.188	−153.346	−811.189	333.010
	(347.594)	(394.088)	(720.598)	(927.424)	(1582.643)	(1126.759)
Baseline controls	yes	yes	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes	yes	yes
Control mean	2248.588	1947.600	2857.105	3260.302	3281.143	3241.598
Control std.	(7462.502)	(6712.429)	(8767.616)	(7482.803)	(8471.179)	(6506.688)
R^2	0.00	0.01	0.03	0.02	0.04	0.09
N	1392	965	427	364	187	177

Note. Robust standard errors are in parentheses. In Panel A, the dependent variable is support for greater transparency in political funding, measured in the pre-election (post-treatment) survey by responses to the question: "To what extent do you support policies that aim to make the flow of political funds more transparent, including legal reforms?" (1 = Do not support at all, 2 = Do not support much, 3 = Somewhat support, 4 = Strongly support). In Panel B, the dependent variable is the amount (in Japanese yen) the respondent would donate to an NGO that promotes transparency in political funding, measured in the same survey by the question: "Suppose you have 50,000 yen at your disposal. How much of it would you donate to an NGO working to increase the transparency of political funding?" Columns labeled "Swing" comprise respondents who do not support a particular party, whereas columns labeled "LDP" comprise LDP supporters. Columns labeled "Stringent" contain respondents whose prior beliefs about intolerance norms are above the sample mean, whereas columns labeled "Lenient" contain respondents whose prior beliefs are at or below the mean. Columns labeled "All" pool these sub-samples. Baseline controls include educational attainment. *, **, and *** indicate $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Table A.10: Effects of the Information Treatment on Perceptions of the Incumbent's Valence (Competence and Trustworthiness)

Sample:	Swing			LDP		
	All	Stringent	Lenient	All	Stringent	Lenient
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Dependent variable: Competence					
Treatment	1.424	1.543	0.807	6.007	1.801	10.456
	(1.132)	(1.345)	(2.126)	(2.632)**	(3.576)	(3.849)***
Baseline controls	yes	yes	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes	yes	yes
Control mean	30.722	31.425	29.301	34.604	36.971	32.479
Control std.	(21.458)	(21.480)	(21.382)	(23.907)	(22.167)	(25.273)
R^2	0.01	0.01	0.02	0.05	0.05	0.10
N	1392	965	427	364	187	177
Panel B	Dependent variable: Trustworthiness					
Treatment	1.649	1.818	1.239	6.592	3.349	9.576
	(1.104)	(1.306)	(2.089)	(2.557)**	(3.540)	(3.654)***
Baseline controls	yes	yes	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes	yes	yes
Control mean	29.681	29.991	29.054	34.955	36.219	33.821
Control std.	(21.261)	(21.343)	(21.120)	(23.975)	(22.638)	(25.158)
R^2	0.01	0.01	0.02	0.05	0.07	0.08
N	1392	965	427	364	187	177

Note. Robust standard errors are in parentheses. Panel A uses respondents' beliefs about the incumbent's competence as the dependent variable. Specifically, each respondent estimates the share of other voters who view the incumbent as "very competent" or "competent" in the pre-election (post-treatment) survey; higher values indicate a belief that more people consider the incumbent competent. Panel B uses an analogous measure of perceived trustworthiness. Respondents estimate the share of voters who regard the incumbent as "very trustworthy" or "trustworthy" in the same survey; higher values indicate a belief that more people consider the incumbent trustworthy. Columns labeled "Swing" comprise respondents who do not support a particular party, whereas columns labeled "LDP" comprise LDP supporters. Columns labeled "Stringent" contain respondents whose prior beliefs about intolerance norms are above the sample mean, whereas columns labeled "Lenient" contain respondents whose prior beliefs are at or below the mean. Columns labeled "All" pool these sub-samples. Baseline controls include educational attainment. *, **, and *** indicate p <0.10, p <0.05, and p <0.01, respectively.

Table A.11: Effects of the Information Treatment on Posterior Beliefs about Pivotality

Dependent variable:	Perceived pivotality		
Sample:	Full sample	Swing	LDP
	(1)	(2)	(3)
Treatment	0.052	0.019	0.161
	(0.035)	(0.043)	(0.082)**
Baseline controls	yes	yes	yes
Prefecture F.E.	yes	yes	yes
Control mean	2.346	2.200	2.568
Control std.	(0.824)	(0.805)	(0.757)
R^2	0.01	0.01	0.06
N	2280	1392	364

Note. Robust standard errors are in parentheses. The dependent variable captures posterior beliefs about pivotality and is measured by responses to the question: "How much influence do you think your vote has on the election outcome?" (1 = Not at all, 2 = Not much, 3 = Some, 4 = A great deal) in the pre-election (post-treatment) survey. Column (1) uses the full sample. The column labeled "Swing" includes respondents who do not support a particular party, while the column labeled "LDP" includes LDP supporters. Baseline controls include educational attainment. *, **, and *** indicate $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Table A.12: Effects of the Information Treatment on Posterior Beliefs about Pivotality, by Political Alignment and Prior Beliefs about Intolerance Norms

Dependent variable:	Perceived pivotality			
Sample:	Swing		LDP	
	Stringent	Lenient	Stringent	Lenient
	(1)	(2)	(3)	(4)
Treatment	0.028	−0.028	0.118	0.236
	(0.050)	(0.085)	(0.116)	(0.118)**
Baseline controls	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes
Control mean	2.222	2.156	2.505	2.624
Control std.	(0.809)	(0.796)	(0.748)	(0.763)
R^2	0.02	0.02	0.07	0.09
N	965	427	187	177

Note. Robust standard errors are in parentheses. The dependent variable captures posterior beliefs about pivotality and is measured by responses to the question: "How much influence do you think your vote has on the election outcome?" (1 = Not at all, 2 = Not much, 3 = Some, 4 = A great deal) in the pre-election (post-treatment) survey. Columns labeled "Swing" comprise respondents who do not support a particular party, whereas columns labeled "LDP" comprise LDP supporters. Columns labeled "Stringent" contain respondents whose prior beliefs about intolerance norms are above the sample mean, whereas columns labeled "Lenient" contain respondents whose prior beliefs are at or below the mean. Baseline controls include educational attainment. *, **, and *** indicate p <0.10, p <0.05, and p <0.01, respectively.

Table A.13: Effects of the Information Treatment on Posterior Beliefs about Pivotality (Alternative Measures)

Sample:	Swing		LDP	
	Stringent	Lenient	Stringent	Lenient
	(1)	(2)	(3)	(4)
Panel A	Dependent variable: Others will vote			
Treatment	0.130	3.241	1.562	9.827
	(1.367)	(2.316)	(3.238)	(3.458)***
Baseline controls	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes
Control mean	48.925	41.388	54.333	47.692
Control std.	(22.693)	(24.954)	(23.308)	(23.235)
R^2	0.01	0.02	0.03	0.06
N	965	427	187	177
Panel B	Dependent variable: Others should vote			
Treatment	1.367	4.170	2.268	9.308
	(1.548)	(2.456)*	(3.758)	(3.961)**
Baseline controls	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes
Control mean	44.111	40.870	50.771	47.658
Control std.	(24.771)	(26.950)	(26.106)	(26.768)
R^2	0.02	0.02	0.06	0.06
N	965	427	187	177

Note. Robust standard errors are in parentheses. In Panel A, the dependent variable is the respondent's belief about the proportion of other voters in the same constituency who will vote for the incumbent candidate. In Panel B, the dependent variable is the respondent's belief about the proportion of other voters in the same constituency who should vote for the incumbent candidate. Columns labeled "Swing" comprise respondents who do not support a particular party, whereas columns labeled "LDP" comprise LDP supporters. Columns labeled "Stringent" contain respondents whose prior beliefs about intolerance norms are above the sample mean, whereas columns labeled "Lenient" contain respondents whose prior beliefs are at or below the mean. Baseline controls include educational attainment. *, **, and *** indicate $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Table A.14: Effects of the Information Treatment on Voting Outcomes: Alternative Mechanisms

Sample:	Importance		Prevalence	
	Important	Not important	Prevalent	Not prevalent
	(1)	(2)	(3)	(4)
Panel A	Dependent variable: Turnout			
Treatment	0.087	0.048	0.068	0.063
	(0.029)***	(0.025)*	(0.029)**	(0.026)**
Baseline controls	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes
Control mean	0.782	0.667	0.662	0.742
Control std.	(0.414)	(0.472)	(0.474)	(0.438)
R^2	0.05	0.04	0.04	0.04
N	671	1376	983	1064
Panel B	Dependent variable: Vote for incumbent			
Treatment	−0.018	0.003	0.010	−0.022
	(0.032)	(0.027)	(0.029)	(0.030)
Baseline controls	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes
Control mean	0.226	0.348	0.270	0.346
Control std.	(0.419)	(0.477)	(0.445)	(0.476)
R^2	0.01	0.01	0.01	0.01
N	671	1376	983	1064
Panel C	Dependent variable: Vote for challenger			
Treatment	0.105	0.045	0.057	0.085
	(0.038)***	(0.026)*	(0.032)*	(0.031)***
Baseline controls	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes
Control mean	0.556	0.319	0.391	0.396
Control std.	(0.498)	(0.466)	(0.488)	(0.490)
R^2	0.02	0.03	0.03	0.03
N	671	1376	983	1064

Note. Robust standard errors are in parentheses. In Panel A, the dependent variable is whether the respondent turned out to vote; in Panel B, whether the respondent voted for the incumbent; and in Panel C, whether the respondent voted for a challenger. All columns use actual voting outcomes from the post-election survey. Column (1) includes individuals who consider the slush fund issue to be one of the most important issues in the October election (listing it among the three issues they consider most important), while Column (2) includes those who do not. Column (3) includes individuals who believe that the prevalence of politicians involved in the slush fund issue is high (above the median value), while Column (4) includes individuals who believe it is low (below or equal to the median value). Baseline controls include educational attainment. *, **, and *** indicate $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Table A.15: Effects of the Information Treatment on Voting Outcomes, by Gender

Dependent variable:	Turnout		Vote for incumbet		Vote for challenger	
Sample:	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.085	0.052	−0.020	0.001	0.105	0.051
	(0.034)**	(0.023)**	(0.033)	(0.027)	(0.036)***	(0.028)*
Baseline controls	yes	yes	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes	yes	yes
Control mean	0.624	0.752	0.624	0.752	0.624	0.752
Control std.	(0.485)	(0.432)	(0.485)	(0.432)	(0.485)	(0.432)
Stat. difference (p-value)	0.421		0.619		0.236	
R^2	0.03	0.04	0.02	0.00	0.04	0.02
N	765	1282	765	1282	765	1282

Note. Robust standard errors are in parentheses. In Columns (1) and (2), the dependent variable is whether the respondent turned out to vote; in Columns (3) and (4), whether the respondent voted for the incumbent; and in Columns (5) and (6), whether the respondent voted for a challenger. All columns use actual voting outcomes from the post-election survey. Columns labeled "Female" and "Male" comprise female and male respondents, respectively. Baseline controls include educational attainment. *, **, and *** indicate $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Table A.16: Robustness Check: Wild Bootstrap and Robust Inference

	Dependent variable:			
	Turnout	Vote for incumbent	Vote for challenger	Perceived pivotality
	(1)	(2)	(3)	(4)
Treatment	0.064	−0.009	0.073	0.052
	(0.023)**	(0.021)	(0.017)***	(0.022)**
Baseline controls	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes
Control mean	0.703	0.309	0.394	2.346
Control std.	(0.457)	(0.462)	(0.489)	(0.824)
Wild bootstrap (p-value)	0.003	0.682	0.001	0.019
Randomization inference (p-value)	0.002	0.683	0.001	0.143
R^2	0.04	0.00	0.02	0.01
N	2047	2047	2047	2280

Note. Standard errors, clustered at the prefecture level, are reported in parentheses. In Column (1), the dependent variable is an indicator for whether the respondent turned out to vote; in Column (2), whether the respondent voted for the incumbent; and in Column (3), whether the respondent voted for a challenger. These columns use actual voting outcomes from the post-election survey. Column (4) uses a measure of posterior beliefs about pivotality, based on responses to the question: "How much influence do you think your vote has on the election outcome?" (1 = Not at all, 2 = Not much, 3 = Some, 4 = A great deal) in the pre-election (post-treatment) survey. Baseline controls include educational attainment. Wild bootstrap p-values are computed using the *boottest* package (Roodman et al., 2019) with 3000 replications and Webb weights. Randomized p-values are computed using the *randcmd* package (Young, 2019). *, **, and *** indicate $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Table A.17: Robustness Check: Lee (2009) bounds

	Dependent variable:			
	Turnout	Vote for incumbent	Vote for challenger	Perceived pivotality
	(1)	(2)	(3)	(4)
Panel A	Selection indicator is *Married*			
Upper	0.135	0.017	0.118	0.113
	(0.041)***	(0.032)	(0.036)***	(0.071)
Lower	0.084	−0.034	0.067	0.014
	(0.025)***	(0.038)	(0.036)*	(0.071)
Panel B	Selection indicator is *Female*			
Upper	0.106	0.010	0.144	0.114
	(0.042)**	(0.056)	(0.055)***	(0.110)
Lower	0.058	−0.038	0.096	−0.068
	(0.052)	(0.037)	(0.043)**	(0.096)
N	2047	2047	2047	2280

Note. Standard errors are reported in parentheses. In Column (1), the dependent variable is an indicator for whether the respondent turned out to vote; in Column (2), whether the respondent voted for the incumbent; and in Column (3), whether the respondent voted for a challenger. These columns use actual voting outcomes from the post-election survey. Column (4) uses a measure of posterior beliefs about pivotality, based on responses to the question: "How much influence do you think your vote has on the election outcome?" (1 = Not at all, 2 = Not much, 3 = Some, 4 = A great deal) in the pre-election (post-treatment) survey. Baseline controls include educational attainment. Upper and lower bounds are computed using the *leebounds* package (Tauchmann, 2014). *, **, and *** indicate p <0.10, p <0.05, and p <0.01, respectively.

Table A.18: Robustness Check: Additional Controls

	Dependent variable:			
	Turnout	Vote for incumbent	Vote for challenger	Perceived pivotality
	(1)	(2)	(3)	(4)
Treatment	0.061	−0.011	0.072	0.050
	(0.019)***	(0.021)	(0.022)***	(0.034)
Baseline controls	yes	yes	yes	yes
Additional controls	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes
Control mean	0.703	0.309	0.394	2.346
Control std.	(0.457)	(0.462)	(0.489)	(0.824)
R^2	0.07	0.02	0.03	0.02
N	2047	2047	2047	2280

Note. Standard errors are reported in parentheses. In Column (1), the dependent variable is an indicator for whether the respondent turned out to vote; in Column (2), whether the respondent voted for the incumbent; and in Column (3), whether the respondent voted for a challenger. These columns use actual voting outcomes from the post-election survey. Column (4) uses a measure of posterior beliefs about pivotality, based on responses to the question: "How much influence do you think your vote has on the election outcome?" (1 = Not at all, 2 = Not much, 3 = Some, 4 = A great deal) in the pre-election (post-treatment) survey. Baseline controls include educational attainment; additional controls include indicators for gender (female) and marital status (married). *, **, and *** indicate $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Table A.19: Robustness Check: Poisson pseudo-Maximum Likelihood

	Dependent variable:			
	Turnout	Vote for incumbent	Vote for challenger	Perceived pivotality
	(1)	(2)	(3)	(4)
Treatment	0.087	−0.028	0.169	0.022
	(0.026)***	(0.068)	(0.051)***	(0.015)
Baseline controls	yes	yes	yes	yes
Prefecture F.E.	yes	yes	yes	yes
Control mean	0.703	0.309	0.394	2.346
Control std.	(0.457)	(0.462)	(0.489)	(0.824)
pseudo-R^2	0.006	0.002	0.009	0.001
N	2047	2047	2047	2280

Note. Robust standard errors are reported in parentheses. In Column (1), the dependent variable is an indicator for whether the respondent turned out to vote; in Column (2), whether the respondent voted for the incumbent; and in Column (3), whether the respondent voted for a challenger. These columns use actual voting outcomes from the post-election survey. Column (4) uses a measure of posterior beliefs about pivotality, based on responses to the question: "How much influence do you think your vote has on the election outcome?" (1 = Not at all, 2 = Not much, 3 = Some, 4 = A great deal) in the pre-election (post-treatment) survey. Baseline controls include educational attainment. All models are estimated using the *ppmlhdfe* package (Correia et al., 2020). *, **, and *** indicate p <0.10, p <0.05, and p <0.01, respectively.

A.5 Survey Questionnaire

A.5.1 English-translated Version

Screening & Baseline Survey - October 18th

As of July 14, 2024, please provide the postal code of your address listed on your certificate of residence.
※Please respond with a 7-digit number without a hyphen.
(　　)

Next　Back

In this House of Representatives election (national election), have you already voted during early voting?
○ Voted during early voting
○ Did not vote during early voting

In social surveys, it is important for respondents to fully understand the questions and provide their answers accordingly.
From the following options, please select "None of the above."
○ Employment
○ AI
○ 42
○ Globalization
○ None of the above

Thank you for accessing this survey page. This survey is being conducted by the Institute of Social and Economic Research at Osaka University, the Faculty of Political Science and Economics at Waseda University, and the Faculty of Economics at Kinki University to understand public perceptions regarding elections.

This survey aims to academically investigate your thoughts and opinions leading up to the House of Representatives election.

The information you provide will be used only for academic research and not for any other purposes. The survey is anonymous, and your privacy will be fully protected. It is designed to understand your honest perspectives, without any judgment of right or wrong. We request your understanding in this regard and hope you are comfortable while answering the following questions.

In addition to this survey, some participants will be invited to complete additional surveys at approximately 6:00 PM on Friday, October 25, and Monday, October 28. This survey consists of 11 questions, while the survey on Friday, October 25, includes 8 questions, each expected to take about 5 minutes to complete. The survey on Monday, October 28, is a simple one with only 2 questions, which are expected to take no more than 2 minutes to complete.

Please note that if you leave the survey, any answers provided before your leave will still be submitted to the research team.

Furthermore, questions marked with the following icon are bonus questions. For these questions, after collecting responses from all participants in this survey for each question, 100 participants who answered correctly will be randomly selected to receive an additional 100 yen worth of points. Across the three surveys, there are a total of 5 bonus questions, allowing participants to earn up to 500 yen in bonus points.



If you are able to participate, we would greatly appreciate your cooperation in completing all the surveys.

Next Back

Q1. We would like to ask about your political values. Do you support any political party?

○ I support the Liberal Democratic Party.
○ I support another ruling party.
○ I support an opposition party.
○ I do not support any political party.
○ I prefer not to answer.

Q2. Regarding the upcoming House of Representatives election (national election), please select up to three issues that you personally consider important, in order of priority.

	Most important	Second most important	Third most important
	↓	↓	↓
Inflation	○	○	○
Declining birthrate	○	○	○
Aging population	○	○	○
Climate change and the environment	○	○	○
Fiscal policy	○	○	○
Foreign policy	○	○	○
Pension	○	○	○
Nuclear power and energy	○	○	○
	Most important	Second most important	Third most important
Constitutional amendment	○	○	○
Issues related to the former Unification Church	○	○	○
Politics and money/slush funds	○	○	○
Regional revitalization	○	○	○
Disaster relief	○	○	○
Others	○	○	○
There is no second/third important issues		○	○

Q3. Among the candidates in this House of Representatives election, which political party does the incumbent member of the House of Representatives in your constituency belong to?

○ Liberal Democratic Party
○ Constitutional Democratic Party of Japan
○ Japan Innovation Party
○ Komeito
○ Democratic Party for the People
○ Japanese Communist Party
○ Reiwa Shinsengumi
○ Social Democratic Party
○ Other parties/Independent
○ I don't know

Q4. In the previous House of Representatives election (2021), which party or independent candidate did you vote for?

○ Did not vote
○ Liberal Democratic Party
○ Constitutional Democratic Party of Japan
○ Japan Innovation Party
○ Komeito
○ Democratic Party for the People
○ Japanese Communist Party
○ Reiwa Shinsengumi
○ Social Democratic Party
○ Other parties/Independent
○ Other/Prefer not to answer

Q5. How frequently have you been exposed to election-related news since October of this year?
○ Almost every day
○ Several times a week
○ Rarely
○ Hardly at all

Q6. What percentage of Japanese politicians do you believe are involved in the issue of "omissions in political funding reports or kickbacks from political funds"?
(　　)%

Next　Back



Q7. This survey project brings together a diverse group of participants, whose responses can be said to represent the views and attitudes of the average voter in Japan.

Regarding the following question, how many out of 100 participants do you think answered "Completely unacceptable" or "Somewhat unacceptable"?

<Question>
To what extent can you tolerate the issue of “omissions in political funding reports or kickbacks from political funds”?

<Responses>
1. Completely unacceptable 2. Somewhat unacceptable 3. Somewhat acceptable 4. Completely acceptable

To the above question, I believe that (　　) out of 100 people answered "1. Completely unacceptable" or "2. Somewhat unacceptable."

Next　Back

Q8. What is your highest educational level?
○ Junior high school graduate
○ High school graduate
○ Junior college, technical college, or vocational school graduate
○ University or graduate school graduate

Next Back

Q9. What is your marital status?

○ Married

○ Single

○ Divorced/Widowed

Next Back

Q10. How many people live in your household?

○ 1 person (living alone)

○ 2 people

○ 3 people

○ 4 people

○ 5 people

○ 6 people

○ 7 or more people

Next Back

Q11. What was your household's annual income before taxes in 2023?

○ Less than 2 million yen

○ 2 million to less than 4 million yen

○ 4 million to less than 6 million yen

○ 6 million to less than 8 million yen

○ 8 million to less than 10 million yen

○ 10 million to less than 12 million yen

○ 12 million to less than 15 million yen

○ 15 million to less than 20 million yen

○ 20 million to less than 30 million yen

○ 30 million to less than 50 million yen

○ 50 million yen or more

Intervention & Post-treatment survey - October 25th

Thank you for accessing this survey page. This survey is being conducted by the Institute of Social and Economic Research at Osaka University, the Faculty of Political Science and Economics at Waseda University, and the Faculty of Economics at Kinki University to understand public perceptions regarding elections.

This survey aims to academically investigate your thoughts and opinions leading up to the House of Representatives election.

The information you provide will be used only for academic research and not for any other purposes. The survey is anonymous, and your privacy will be fully protected. It is designed to understand your honest perspectives, without any judgment of right or wrong. We request your understanding in this regard and hope you are comfortable while answering the following questions.

Please note that if you leave the survey, any answers provided before your leave will still be submitted to the research team.

Furthermore, questions marked with the following icon are bonus questions. For these questions, after collecting responses from all participants in this survey for each question, 100 participants who answered correctly will be randomly selected to receive an additional 100 yen worth of points. Across the three surveys, there are a total of 5 bonus questions, allowing participants to earn up to 500 yen in bonus points.

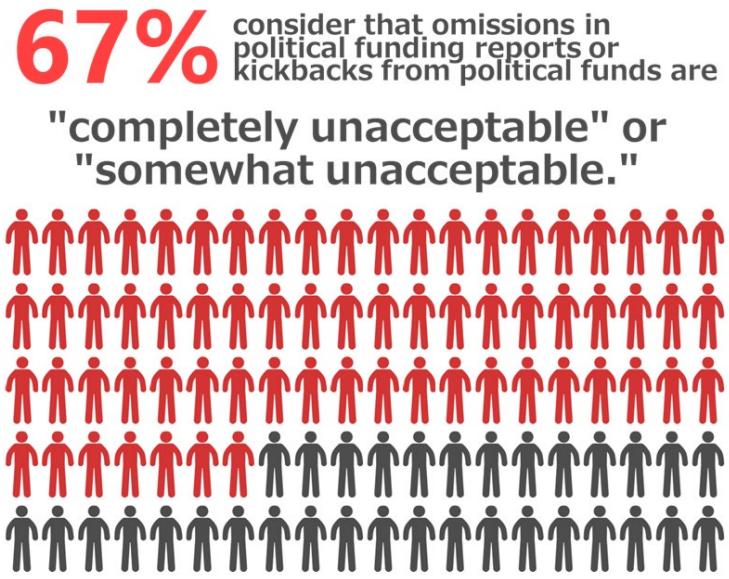


If you are able to participate, we would greatly appreciate your cooperation in completing all the surveys.

Next Back

This survey project brings together a diverse group of participants, whose responses can be said to represent the views and attitudes of the average voter in Japan. In the previous survey, participants were asked how many of the other respondents considered that "omissions in political funding reports or kickbacks from political funds" are "completely unacceptable" or "somewhat unacceptable."

As a result, it was found that people believe that out of 100 participants, 67 considered "omissions in political funding reports or kickbacks from political funds" to be either "completely unacceptable" or "somewhat unacceptable."



Next Back



This survey project brings together a diverse group of participants, whose responses can be said to represent the views and attitudes of the average voter in Japan.

Q1. We would like to ask your opinion.
Regarding the following question, how many people out of 100 in your constituency, whom we surveyed, do you think answered that they "will vote" for the incumbent candidate in your constituency?

<Question>
Will you vote for the incumbent candidate in your constituency in this election?

<Responses>
1. Will vote 2. Will not vote

To the above question, I believe that (　　) out of 100 people answered "1. Will vote".



Q2. Similarly, we would like to ask your opinion.
Regarding the following question, how many people out of 100 in your constituency, whom we surveyed, do you think answered that they "should vote" for the incumbent candidate in your constituency?

<Question>
Do you think you “should vote” for the incumbent candidate in your constituency in this election?

<Answers>
1. Should vote 2. Should not vote

To the above question, I believe that (　　) out of 100 people answered "1. Should vote."



Q3. Similarly, we would like to ask your opinion.
Regarding the following question, how many people out of 100 in your constituency, whom we surveyed, do you think answered that the incumbent House of Representatives member (elected in the previous 2021 election) in their constituency is "very competent" or "competent"?

<Question>
How competent do you think the incumbent House of Representatives member (elected in the previous 2021 election) in your constituency is?

<Answers>
1. Not competent at all 2. Not competent 3. Competent 4. Very competent

To the above question, I believe that ( ) out of 100 people answered “4. Very competent” or “3. Competent”.



Q4. Similarly, we would like to ask your opinion.
Regarding the following question, how many people out of 100 in your constituency, whom we surveyed, do you think answered that the incumbent House of Representatives member (elected in the previous 2021 election) in their constituency is "very trustworthy" or "trustworthy"?

<Question>
How trustworthy do you think the incumbent House of Representatives member (elected in the previous 2021 election) in your constituency is?

<Answers>
1. Not trustworthy at all 2. Not trustworthy 3. Trustworthy 4. Very trustworthy

To the above question, I believe that ( ) out of 100 people answered “4. Very trustworthy” or “3. Trustworthy”.

Next Back

Q5. How much influence do you think your vote has on the election outcome?
○ Not at all
○ Not much
○ Some
○ A great deal

Next Back

Q6. In this House of Representatives election, will you vote in the single-member district for the incumbent candidate (elected in the previous 2021 election) in your constituency?

○ Vote for the incumbent
○ Vote for another candidate from the same party as the incumbent
○ Vote for a candidate from a different party or an independent candidate
○ Do not plan to vote
○ Not sure

Next Back

Q7. To what extent do you support policies that aim to make the flow of political funds more transparent, including legal reforms?

○ Do not support at all
○ Do not support much
○ Somewhat support
○ Strongly support

Next Back

Q8. Suppose you have 50,000 yen at your disposal. How much of it would you donate to an NGO working to increase the transparency of political funding?

I would like to donate (　　) yen out of 50,000 yen.

Endline Survey - October 28th

Thank you for accessing this survey page. This survey is being conducted by the Institute of Social and Economic Research at Osaka University, the Faculty of Political Science and Economics at Waseda University, and the Faculty of Economics at Kinki University to understand public perceptions regarding elections.

This survey aims to academically investigate your thoughts and opinions leading up to the House of Representatives election.

The information you provide will be used only for academic research and not for any other purposes. The survey is anonymous, and your privacy will be fully protected. It is designed to understand your honest perspectives, without any judgment of right or wrong. We request your understanding in this regard and hope you are comfortable while answering the following questions.

Please note that if you leave the survey, any answers provided before your leave will still be submitted to the research team.

If you are able to participate, we would greatly appreciate your cooperation in completing all the surveys.

Next Back

Q1. Did you vote in the current House of Representatives general election (national election)?

○ Yes
○ No
○ Prefer not to answer

Next Back

Q2. In the single-member district of the current House of Representatives general election,

did you vote for the incumbent candidate (elected in the previous 2021 election)?

○ Did not vote

○ Voted for the incumbent candidate

○ Voted for another candidate from the same party as the incumbent candidate

○ Voted for a candidate from a different party or an independent candidate

○ Prefer not to answer

Next Back

A.5.2 Original Version

Baseline Survey - October 18th

本アンケートページにアクセスしていただきましてありがとうございます。本アンケート調査は、みなさまの「選挙に関する意識」を理解する目的のために、大阪大学社会経済研究所、早稲田大学政治経済学術院、近畿大学経済学部が共同で実施するものです。

この調査は、みなさまが今回の衆議院議員総選挙に先立ち、どのような考えを持っていらっしゃるのかを学術的に調査することを目的としています。

ご回答いただいた内容は学術調査にのみ使用され、その他の目的では一切使用されません。また、アンケートは匿名調査で、みなさまのプライバシーは完全に保護されます。アンケートはみなさまのありのままの姿を理解することを目的としており、それに関する善悪の判断をすることは一切ありません。その点をご理解いただき、気楽にお答え頂けますと幸いです。

今回の調査に加えて、一部の方には、10月25日(金)と28日（月）の午後18時ごろにアンケートのご案内を行います。今回の質問数は11問、10/25(金)の質問数は8問となっており、それぞれ5分程度の完了時間を想定しています。10/28（月）の質問数は2問の簡単な調査になっており、所要時間が2分を越えることはないと想定しています。

本調査では、回答を中断した場合も、中断までに回答されたデータは調査委託者に納品されます。

また、以下のマークがついている質問は、ボーナス質問です。これらの質問は、質問ごとにこの調査に参加されている皆さんの回答を集計した後、正解した方の中から100名様に追加で100円相当のポイント差し上げます。ボーナス質問は3回のアンケート合計で5問あり、最大で500円相当のポイントが獲得可能です。



ご参加頂ける方は、是非全てのアンケートにご協力くださいますよう、お願い申し上げます。

Q1 あなたの政治的価値観についてお尋ねします。あなたには支持している政党はありますか。

ひとつだけ 必須

- ○ 自民党を支持している
- ○ その他与党を支持している
- ○ 野党を支持している

◯ 支持している政党は無い

◯ 答えたくない

Q2 今回の衆議院議員総選挙(国政選挙)に関して、以下の点について、あなたが個人的に重視する順に1つから最大３つまで選んで下さい。

ひとつだけ　必須

	最も重視する	2番目に重視する	3番目に重視する
	↓	↓	↓
物価高	○	○	○
少子化	○	○	○
高齢化	○	○	○
気候変動・環境	○	○	○
財政	○	○	○
外交	○	○	○
年金	○	○	○
原発・エネルギー	○	○	○
	最も重視する	2番目に重視する	3番目に重視する
憲法改正	○	○	○
旧統一教会問題	○	○	○
政治とカネ・裏金	○	○	○
地方創生	○	○	○
被災地援助	○	○	○
その他	○	○	○
2番目／3番目に重視するものはない		○	○

Q3 今回の衆議院議員選挙の候補者のうち、あなたの選挙区の現在の衆議院議員(現職)の政党はどれですか。

ひとつだけ　必須

- ○ 自民党
- ○ 立憲民主党
- ○ 日本維新の会
- ○ 公明党
- ○ 国民民主党
- ○ 日本共産党
- ○ れいわ新選組
- ○ 社会民主党
- ○ その他の政党・無所属
- ○ わからない

Q4 前回(2021年)の衆議院議員選挙では、どの政党・無所属の議員に投票しましたか。

ひとつだけ　必須

- ○ 選挙に行っていない
- ○ 自民党
- ○ 立憲民主党
- ○ 日本維新の会
- ○ 公明党
- ○ 国民民主党
- ○ 日本共産党
- ○ れいわ新選組
- ○ 社会民主党
- ○ その他の政党・無所属
- ○ その他・答えたくない

Q5　今年の10月以降、どの程度の頻度で選挙関連のニュースに触れましたか。

ひとつだけ　必須

- ほぼ毎日
- 1週間に数回程度
- あまり触れていない
- ほとんど触れていない

Q6 あなたは、日本の政治家の何%ぐらいが「政治資金収支報告書への記載漏れや政治資金のキックバック」問題に関係していると思いますか。

必須

[ ] %



Q7 この調査プロジェクトには、様々な方々が集まっており、調査参加者の回答は、日本の平均的な有権者の見解と態度を表しているといえます。

以下の質問について、私たちが尋ねた中の100人のうち、何人が
「全く許容できない」または
「それほど許容できない」と答えたと思いますか。

＜質問＞
あなたは、「政治資金収支報告書への記載漏れや政治資金のキックバック」問題についてどれぐらい許容できますか。

＜回答＞
1.全く許容できない、2.それほど許容できない、3.まあ許容できる、4.全く許容できる

必須

上記の質問で、100人中 [ ]
人が、「1.全く許容できない」または「2.それほど許容できない」と答えたと思う

Q8 あなたの最終学歴をお答えください。

ひとつだけ 必須

- ○ 中学卒
- ○ 高校卒
- ○ 短大・高専・専門学校卒
- ○ 大学・大学院卒

Q9 あなたの婚姻状況をお答えください。

ひとつだけ 必須

- ○ 既婚
- ○ 未婚
- ○ 離死別

Q10 あなたのご家庭の同居人数をお答えください。

ひとつだけ　必須

- ○ 1人（1人暮らし）
- ○ 2人
- ○ 3人
- ○ 4人
- ○ 5人
- ○ 6人
- ○ 7人以上

Q11 2023年のあなたのご家庭の世帯年収（税引き前）をお答えください。

ひとつだけ　必須

- ○ 200万円未満
- ○ 200万円～400万円未満
- ○ 400万円～600万円未満
- ○ 600万円～800万円未満
- ○ 800万円～1000万円未満
- ○ 1000万円～1200万円未満
- ○ 1200万円～1500万円未満
- ○ 1500万円～2000万円未満
- ○ 2000万円～3000万円未満
- ○ 3000万円～5000万円未満
- ○ 5000万円以上

Intervention & Post-treatment survey - October 25th

本アンケートページにアクセスしていただきましてありがとうございます。本アンケート調査は、みなさまの「選挙に関する意識」を理解する目的のために、大阪大学社会経済研究所、早稲田大学政治経済学術院、近畿大学経済学部が共同で実施するものです。

この調査は、みなさまが今回の衆議院議員総選挙に先立ち、どのような考えを持っていらっしゃるのかを学術的に調査することを目的としています。

ご回答いただいた内容は学術調査にのみ使用され、その他の目的では一切使用されません。また、アンケートは匿名調査で、みなさまのプライバシーは完全に保護されます。アンケートはみなさまのありのままの姿を理解することを目的としており、それに関する善悪の判断をすることは一切ありません。その点をご理解いただき、気楽にお答え頂けますと幸いです。

本調査では、回答を中断した場合も、中断までに回答されたデータは調査委託者に納品されます。

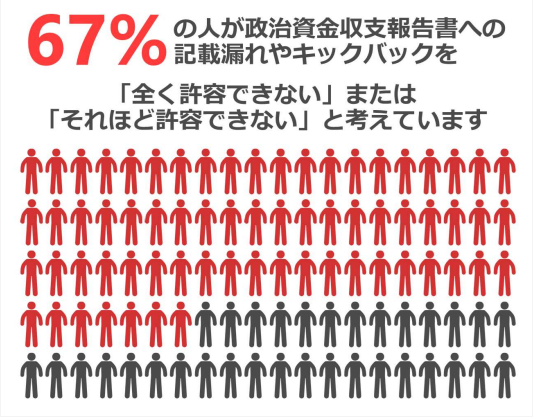
また、以下のマークがついている質問は、ボーナス質問です。これらの質問は、質問ごとにこの調査に参加されている皆さんの回答を集計した後、正解した方の中から100名様に追加で100円相当のポイント差し上げます。ボーナス質問は3回のアンケート合計で5問あり、最大で500円相当のポイントが獲得可能です。



ご参加頂ける方は、是非全てのアンケートにご協力くださいますよう、お願い申し上げます。

この調査プロジェクトには、様々な方々が集まっており、調査参加者の回答は、日本の平均的な有権者の見解と態度を表しているといえます。前回の調査では、「政治資金収支報告書への記載漏れや政治資金のキックバック」について、他の回答者の中に、「全く許容できない」または「それほど許容できない」と考えている方がどれぐらいいるかお聞きしました。

その結果、100人のうち、67 人が「政治資金収支報告書への記載漏れや政治資金のキックバック」を「全く許容できない」または「それほど許容できない」と考えていることがわかりました。





この調査プロジェクトには、様々な方々が集まっており、調査参加者の回答は、日本の平均的な有権者の見解と態度を表しているといえます。

Q1 あなたのお考えについてお聞きします。
以下の質問に関して、 私たちが尋ねた中の 、あなたの選挙区の人たち100人のうち、何人が自分たちの選挙区の現職候補に「投票する」と答えたと思いますか。

＜質問＞
あなたは今回の選挙で、自分の選挙区の現職候補に投票しますか。

＜回答＞
1．投票する　2．投票しない

必須

上記の質問で、100人中 [　　] 人が、「1．投票する」と答えたと思う



Q2 同様に、あなたのお考えについてお聞きします。
以下の質問に関して、 私たちが尋ねた中の 、 あなたの選挙区の人たち100人のうち、何人が自分たちの選挙区の現職候補に「投票すべき」と答えたと思いますか。

＜質問＞
あなたは今回の選挙で、自分の選挙区の現職候補に投票すべきだと思いますか。

＜回答＞
1．投票すべき　2．投票すべきではない

必須

上記の質問で、100人中 [ ] 人が、「1．投票すべき」と答えたと思う



Q3 同様に、あなたのお考えについてお聞きします。
以下の質問に関して、私たちが尋ねた中の、あなたの選挙区の人たち100人のうち、何人が自分たちの選挙区の現職の衆議院議員（前回の選挙（2021年）で当選した人）は「とても能力がある」または「能力がある」と答えたと思いますか。

＜質問＞
あなたは、自分の選挙区の現職の衆議院議員（前回の選挙（2021年）で当選した人）にはどれぐらい能力があると思いますか。

＜回答＞
1．全く能力がない　2．能力がない　3．能力がある　4．とても能力がある

必須

上記の質問で、100人中 [　　] 人が、「4．とても能力がある」または「3．能力がある」と答えたと思う



Q4 同様に、あなたのお考えについてお聞きします。
以下の質問に関して、私たちが尋ねた中の、あなたの選挙区の人たち100人のうち、何人が自分たちの選挙区の現職の衆議院議員（前回の選挙（2021年）で当選した人）は「とても信頼できる」または「信頼できる」と答えたと思いますか。

＜質問＞
あなたは、自分の選挙区の現職の衆議院議員（前回の選挙（2021年）で当選した人）はどれぐらい信頼できますか。

＜回答＞
1．全く信頼できない　2．信頼できない　3．信頼できる　4．とても信頼できる

必須

上記の質問で、100人中 [　　] 人が、「4．とても信頼できる」または「3．信頼できる」と答えたと思う

Q5 あなたの票は、選挙結果に対してどれぐらい影響力があると思いますか。

ひとつだけ 必須

- ◯ 全くない
- ◯ あまりない
- ◯ 少しある
- ◯ とてもある

Q6 あなたは今回の衆議院議員選挙の小選挙区で、自分の選挙区の現職候補(前回の選挙(2021年)で当選した人)に投票しますか。

ひとつだけ 必須

- ◯ 現職に投票する
- ◯ 現職と同じ政党だが別の候補者に投票する
- ◯ 現職とは別の政党・無所属の候補者に投票する
- ◯ 選挙に行く予定はない
- ◯ わからない

Q7 あなたは、法改正などを含め、政治資金の流れをより透明にする政策をどれぐらい支持しますか。

ひとつだけ　必須

- ◯ 全く支持しない
- ◯ それほど支持しない
- ◯ まあ支持する
- ◯ 強く支持する

Q8 仮に自由に使える5万円が手元にあるとします。そのうちの一部を、政治資金の透明性を高める活動をしているNGOに寄付できるとします。あなたは、5万円のうちいくらを寄付しますか。

必須

5万円のうち、 [　　] 円、寄付する

Endline Survey - October 28th

本アンケートページにアクセスしていただきましてありがとうございます。本アンケート調査は、みなさまの「選挙に関する意識」を理解する目的のために、大阪大学社会経済研究所、早稲田大学政治経済学術院、近畿大学経済学部が共同で実施するものです。

この調査は、みなさまが今回の衆議院議員総選挙に先立ち、どのような考えを持っていらっしゃるのかを学術的に調査することを目的としています。

ご回答いただいた内容は学術調査にのみ使用され、その他の目的では一切使用されません。また、アンケートは匿名調査で、みなさまのプライバシーは完全に保護されます。アンケートはみなさまのありのままの姿を理解することを目的としており、それに関する善悪の判断をすることは一切ありません。その点をご理解いただき、気楽にお答え頂けますと幸いです。

本調査では、回答を中断した場合も、中断までに回答されたデータは調査委託者に納品されます。

ご参加頂ける方は、是非全てのアンケートにご協力くださいますよう、お願い申し上げます。

Q1 あなたは今回の衆議院議員総選挙(国政選挙)で投票しましたか。

ひとつだけ 必須

- ◯ はい
- ◯ いいえ
- ◯ 答えたくない

Q2 あなたは今回の衆議院議員選挙の小選挙区で、現職候補(前回の選挙(2021年)で当選した人)に投票しましたか。

ひとつだけ 必須

- ◯ 選挙に行ってない
- ◯ 現職に投票した
- ◯ 現職と同じ政党だが別の候補者に投票した
- ◯ 現職とは別の政党・無所属の候補者に投票した
- ◯ 答えたくない