DISCUSSION PAPER SERIES

DP14608

VOTING OR ABSTAINING IN "MANAGED" ELECTIONS? A FIELD EXPERIMENT IN BANGLADESH

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DEVELOPMENT ECONOMICS

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Voting or abstaining in "managed" elections? A field experiment in Bangladesh[*]

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March 24, 2020

Abstract

Many governments in weak democracies countries "manage" the electoral process to make their defeat very unlikely. We aim to understand why citizens decide to vote or abstain in managed elections. We focus on the 2018 general election in Bangladesh and randomize the salience of the citizens' views (i) that election outcomes matter for policy outcomes and (ii) that high voting participation increases the winning party's legitimacy. These treatments increase voting participation in government strongholds and decrease participation in opposition strongholds. The legitimacy treatment has stronger effects. These results have important implications for get-out-the-vote and information campaigns in weak democracies.

Keywords: Electoral authoritarianism, managed/authoritarian elections, voting behavior, field experiment, Bangladesh.

JEL classification: C93, D72.

1   Introduction

Elections have become more common across the globe since the end of the Cold War and the dissolution of the Soviet Union. However, in many countries, elections are not complemented by checks and balances on the executive's use of power. As a result, there is a high and increasing number of elections in which the incumbent government takes actions in the run-up to the election or on election day to make its defeat very unlikely (Collier and Vicente, 2012, 2014; Cheeseman and Klaas, 2018). We describe such elections as "managed" elections. Prominent examples include the 2018 presidential elections in Egypt, Russia and Venezuela, and the 2018 general election in Bangladesh. Little is known about how people decide whether to vote or abstain in managed elections.

In this paper, we study two motives for voting participation in managed elections. First, people may vote to increase the legitimacy of the electoral process and the elected government, or abstain in order not to contribute to the legitimacy of a process and a government that they view as a flawed and illegitimate. Second, people may vote in the hope that voting outcomes matter for policy outcomes, e.g., local public goods provision. To study the importance of these motives, we focus on the general election in Bangladesh on December 30, 2018, and randomize the salience of (i) the citizens' view that their voting participation and high turnout increase the elected government's legitimacy or (ii) their view that election outcomes matter for policy outcomes.

The 2018 general election in Bangladesh is an ideal testing ground to investigate voting behavior in managed elections for at least two reasons. First, while Bangladesh is officially a parliamentary representative democratic republic, its politics has long been dominated by Sheikh Hasina from the Awami League (AL) and Khaleda Zia from the Bangladesh Nationalist Party (BNP) in a way such that Bangladesh is by now a weak democracy at best.[1] Bangladesh is not just any weak democracy, but one of the largest – if not the single largest – weak democracy in the world.[2]

Second, it was easy to foresee that the incumbent government of Ms Hasina and the AL would "manage" the 2018 general election. They had already "managed" the run-up to the 2014 general election by arresting thousands of opposition party members, preventing BNP pre-election rallies, and putting Ms Zia under house arrest. At the end, the BNP and other opposition parties decided to boycott the 2014 general election in order not

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[1]Riaz (2019) provides an excellent overview of Bangladesh's political development since the early 1990s and the 2018 general election. He describes its current regime as "hybrid," i.e., "ostensibly democratic but essentially authoritarian."

[2]Bangladesh is the eighth most populous country in the world. The Polity2 score by the Policy IV Project is a well-known indicator for the quality of a country's de facto political institutions, ranging from -10 to 10. Bangladesh's Polity2 score for 2017 (i.e., prior to the 2018 general election) was 1, which indeed suggests that Bangladesh is a weak democracy (or an anocracy or a hybrid regime). Looking at the seven more populous countries, Brazil, India, Indonesia and the United States are reasonably strong democracies with Polity2 scores of 8 or above, while China is an autocracy with a Polity2 score of -7. Nigeria and Pakistan have Polity2 scores of 7, but may arguably be weak democracies as well.

to legitimize the flawed electoral process and the reelected Hasina government. As a result of this boycott, the AL then won 79 percent of the vote. In early 2018, Ms Zia was sentenced to five years in prison on corruption charges, which prohibited her from participating in the general election. Other high-profile BNP candidates were sentenced to prison or accused of corruption as well (Riaz, 2019). Moreover, there were irregularities in city corporation elections in May 2018, with supporters of the AL forcibly taking control of polling stations.[3] During 2018, the freedom of the press worsened, and there were politically motivated incidences of extrajudicial killings and enforced disappearances (Riaz, 2019). The BNP decided to participate in the general election even though the government's promise of free and fair elections was hardly credible.[4] We too were, of course, aware of the high likelihood of managed elections when registering our pre-analysis plan and implementing our interventions in December 2018.[5]

The core idea of the experiment was to deliver two different treatments to individuals in two different types of villages. The legitimacy treatment focused on the message that high turnout may increase the winning party's legitimacy. The policy treatment focused on the message that voting outcomes matter for policy outcomes. We focused on villages that are either government strongholds that traditionally voted for the AL, or opposition strongholds that traditionally voted for the BNP. We selected around 150 AL and 150 BNP villages in south-western Bangladesh. We randomly assigned around 50 villages of each type to the control group, the policy treatment group and the legitimacy treatment group, respectively. Within each village, we surveyed around 40 randomly selected individuals. In the two weeks prior to the 2018 general elections, we delivered the policy and the legitimacy treatment messages multiple times to the selected individuals in the corresponding treatment villages by door-to-door visits and by distributing leaflets, stickers, and newspapers with advertorials. Importantly, we conducted a post-vote survey directly after the election to ask each of the almost 12,000 respondents whether they and the other household members participated in the election and to check for an ink mark on their fingers.

We split the sample into AL and BNP villages because we predict different effects of our treatments on government- and opposition-leaning citizens (see the theoretical frame-

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[3]The Daily Star, "Khaleque wins: Khulna city polls see irregularities in many centres," May 16, 2018.

[4]Dhaka Tribune, "After much deliberation, BNP decides to come to polls," November 11, 2018.

[5]At that time, we could not know whether and to what extent the incumbent government would "manage" the proceedings on election day itself. There is plenty of anectodal evidence for vote rigging. First and foremorst, there are news reports of fake votes and ballot stuffing, and complaints that voters and opposition polling agents were barred from entering polling centers (e.g., BBC, "Bangladesh elections: Deadly clashes mar vote," December 30, 2018; The Economist, "Elections in Bangladesh: Leaving nothing to chance," January 5, 2019.) Second, the official election outcomes are hardly credible: The official turnout was 89 percent and the official AL vote share 82 percent, implying an increase since the 2014 general election boycotted by the opposition. In our sample, there are many polling stations where the official AL vote share increased from less than 30 percent in the last reasonably free and fair general election in 2008 to more than 90 percent in 2018 general election.

work in Online Appendix A). We predict that increasing the salience of the view that high turnout increases the legitimacy of the elected government, raises voting participation among citizens who see the elections and the incumbent government as legitimate, but lowers voting participation among citizens who question the legitimacy of the "managed" electoral process and the sure-to-be-reelected incumbent government. Consistent with these predictions, we find that the legitimacy treatment increases voter turnout, as measured by ink marks on the respondents' fingers, by around 15 percentage points (p.p.) in AL villages, but decreases voter turnout by around 21 p.p. in BNP villages.

The predictions for the policy treatment are less straightforward. The reason is that the relation between election outcomes and policy outcomes depends on the government's mindset. If its approach to public goods provision is reasonably benevolent, it can use local election outcomes as signals of policy preferences that can guide the allocation of local public goods. In this case, people have an incentive to vote according to their true policy and party preferences. Alternatively, if the government has a clientelistic or favoritism mindset, it may use local public goods to reward loyalists. This approach to local public goods provision incentivizes people to vote for the government party, no matter their true preferences. For government-leaning citizens, voting according to their preferences and voting for the government party is the same. An increase in the salience of the view that election outcomes matter for policy outcomes thus makes them more likely to participate in the election and to vote for this party. Thus, we predict that the policy treatment increases turnout in government-leaning villages. Our results confirm this prediction: The policy treatment increases voter turnout by around 7 p.p. in AL villages.

The voting decision of citizens with a preference for the opposition party depends on their beliefs about the government's mindset. They have an incentive to vote for the opposition party if they expect a reasonably benevolent approach to local public goods provision, but for the government party if they expect a clientelistic approach. Moreover, the policy treatment may not only increase the salience of the view that the election outcome matters for policy outcomes, but also change the citizens' beliefs about the government's mindset. From a theoretical point of view, the policy treatment's effect on voting participation of opposition-leaning citizens is therefore ambiguous in general. In our field experiment, we find that the policy treatment lowers voting participation by 10 p.p. in BNP villages.[6]

To summarize, our main findings are that the legitimacy and the policy treatment

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[6]In the pre-analysis plan, we predict a positive effect of the policy treatment on turnout in BNP villages. Our theoretical framework too would suggest a positive effect of the policy treatment on turnout in opposition-leaning villages under the assumption that the policy treatment does not change the citizens' beliefs about the government's mindset (see Prediction 5 in Online Appendix A). Below, we provide evidence that this assumption was probably too strong and provide an explanation for why the effect may be negative based on this evidence and our theoretical framework.

both substantially increase turnout in the (government-leaning) AL villages and decrease turnout in the (opposition-leaning) BNP villages, with the effects of the legitimacy treatment being at least twice as strong in both types of villages. These results are robust to the use of self-reported voting participation (instead of ink marks), and they spill over to the spouses and other household members of the treated individuals. They imply that even neutrally framed get-out-the-vote campaigns may simultaneously reduce the number of votes of some parties, while increasing the number of votes of other parties, potentially leading to dramatic changes in vote shares.

We further study heterogeneity in treatment effects along various dimensions. The most striking results is effect heterogeneity by gender: Women are more likely than men to respond with abstention to both treatments in both types of villages.

We contribute to various strands of the literature on voting and turnout in both economics and political science. First, there is an established literature on why many people vote even though the chances that any single individual is pivotal is vanishingly small (e.g., Aldrich, 1993; Blais, 2000; Feddersen, 2004). Group-based models of turnout offer a prominent explanation (Feddersen, 2004). In these models, individual group members may vote because they know that they as a group can influence election outcomes. These models are consistent with high voter turnout in free and fair elections. However, they cannot explain high turnout in managed elections in which the chances that an entire group is pivotal is vanishingly small given the incumbent governments' interference in the electoral process. Hence, we complement this literature by shedding light on why people vote in elections that are neither free nor fair.

Second, we contribute to the experimental literature on whether and how get-out-the-vote campaigns and information campaigns (or particular features thereof) can rise voter turnout. While many studies focus on the United States (e.g., Gerber and Green, 2000; Gerber et al., 2011; Green et al., 2013), there has recently been a surge in studies in developing countries (e.g., Chong et al., 2014; Aker et al., 2017; Giné and Mansuri, 2018; Chong et al., 2019; Dunning et al., 2019). To this literature, we add a novel treatment and novel results. To the best of our knowledge, we are the first to experimentally raise the salience of the view that high turnout increases the winning party's legitimacy and, therefore, the first to document that the corresponding treatment effects go in opposite directions in government- and opposition-leaning villages, but are large in absolute value in either case. The result that the same message can have different effects depending on the party preferences of the local population may also explain why some studies find surprisingly small or zero effects on average (e.g., Dunning et al., 2019). This result further highlights that the main effect of get-out-the-vote campaigns may not be a change in turnout, but a change in the composition of the population that actually votes.

Another novel result is that women are more likely than men to respond with abstaining from voting to both our treatments. This result implies that get-out-the-vote

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campaigns may worsen the gender vote gap. Giné and Mansuri (2018) study an information campaign that successfully increased female voter turnout in Pakistan. In our view, the appropriate take-away from their and our study is that the success of such campaigns depends on the political and legal environment before the elections, the exact messages, as well as the target population.[7]

Finally, there is a literature on voting in (semi-)authoritarian regimes, which intersects the two strands of the literature discussed above. Related to our ideas, Croke et al. (2016) argue that educated voters may deliberately abstain from voting as participation may legitimize such regimes. They exploit a natural experiment in Zimbabwe and find evidence in support of their argument. Most other recent studies in this literature use field experiments to study, e.g., the effect of information campaigns (see above) or campaigns aimed at reducing electoral misconducts (Vicente and Wantchekon, 2009; Vicente, 2014; Hicken et al., 2018) or electoral violence (Collier and Vicente, 2014). Unlike these experiments, we study the determinants of voting participation and focus on the role of high turnout for the regime's legitimacy as well as the different effects in government and opposition strongholds.

The reminder of the paper is structured as follows: Section 2 presents the experimental design, Section 3 the results, and Section 4 our concluding remarks.

2 Experimental design

The core idea of the experiment was to deliver two different treatments to individuals of two different types of villages. The "policy treatment" focused on the message that voting outcomes may affect policy outcomes, and the "legitimacy treatment" on the message that high turnout may increase the political legitimacy of the winning party's government. We are interested in the potentially differential effects of these treatments on voting turnout in government strongholds, i.e., villages that supported the AL in past elections, and opposition strongholds, i.e., villages that supported the BNP in past elections.

2.1 Sampling and randomization

The study area includes the five Upazilas (sub-districts) Assasuni, Dumuria, Koyra, Paikgachha, and Tala, which belong to the south-western districts Khulna and Satkhira: It intersects with four electoral constituencies of the national parliament: Khulna 5, Khulna 6, Satkhira 1, and Satkhira 3. These four constituencies contain 563 polling stations. Many of these polling stations consist of more than one village.

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[7]Noteworthy differences between the campaign in Giné and Mansuri (2018) and our treatments include: They focus exclusively on women, while we aim for gender balance. They focus on mostly illiterate women, while we target literate individuals. Their treatment also includes information about registration, while there is universal registration six months before elections in Bangladesh.

Our sampling and randomization strategy contained the following stages: First, we collected the vote share of the AL and the BNP at the level of polling stations in the 2001 and 2008 general elections.[8] For polling stations consisting of multiple villages, we complemented this information with local knowledge about the support for the AL and the BNP across villages within polling stations (talking to village elders, leaders, and party members, and verifying from multiple such sources). We chose 302 rural villages from the same number of polling stations. 154 of these villages had supported the AL in past elections, and 148 had supported the BNP.[9] Figure 1 shows a map of the study area. It indicates AL and BNP villages using red and green color, respectively.

Figure 1 around here.

Second, we randomly assigned the villages of each type into roughly equal numbers of control villages, villages where we would deliver the policy treatment, and villages where we would deliver the legitimacy treatment. More specifically, we randomly assigned the 154 AL (148 BNP) villages into 52 (48) control villages, 51 (50) policy treatment villages, and 51 (50) legitimacy treatment villages. The map in Figure 1 represents control villages by squares, policy treatment villages by stars, and legitimacy treatment villages by triangles.

Third, we randomly selected 40 households per village, with a focus on the married and literate individuals between the age of 20 and 55 years (who can read and understand our treatment messages). We surveyed these households with gender balance among respondents, who are also our target subjects for the treatments.

2.2 Treatment delivery

To each randomly selected individual from a policy or a legitimacy treatment village, the corresponding message was delivered in three different ways: First, we conducted door-to-door visits from December 17–19, 2018. During these visits we talked directly with the randomly selected individuals to make the main message of the treatment clear and gave them a leaflet underscoring the message. The leaflet for the policy treatment listed several key points indicating how a vote can play an important role in shaping development policies that may affect the area, and highlighted that the importance of local development implies that one should vote for her/his preferred candidate or party. The leaflet for the legitimacy treatment discussed how one's voting participation gives

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[8]The 2014 general election is unsuitable, as the BNP boycotted the election. We added the vote shares of the AL's (BNP's) coalition parties to the vote share of the AL (BNP).

[9]We aimed for 150 AL villages and 150 BNP villages. We started with collecting information for slightly more than 300 villages (to avoid having villages without a clear voting record for either party) and ended up with 154 that had supported the AL in past elections and 148 that had supported the BNP.

more legitimacy to winning party's government (see Figure B.2 and the translation in Online Appendix B).

Second, we provided stickers with the respective messages to the individuals from December 22–23. These stickers contained the same key points as the leaflets along with some pictures showing schools, hospitals, bridges, rail lines, etc., for the policy treatment, and the parliament and the supreme court for the legitimacy treatment (see Figure B.3 in Online Appendix B).

Third, we published advertorials conveying these messages in a well-known, non-partisan local newspaper (see Figure B.4 in Online Appendix B). The advertorials with these two messages were published on two subsequent days, and we distributed the newspaper with the policy (legitimacy) advertorial to the individuals who got the policy (legitimacy) treatment from December 26–27. In our study area, newspapers are mainly read in urban areas, whereas the people living in the rural villages that constitute our sample are unable or unwilling to buy newspapers. In our survey, only six percent of the respondents claim to read newspapers regularly. Hence, informational spillovers from these advertorials are unlikely to contaminate our treatments.

These treatment were delivered by a local, non-political and non-partisan NGO, which is known in the study area for activities promoting education and social services.[10] From December 28–29, we made phone calls to the treated individuals to verify their understanding our our treatment mes sages.

2.3 Surveys and measurement

We designed two pre-treatment and two post-vote surveys, which were conducted by the local NGO in all 302 villages. The first pre-treatment survey was conducted about six weeks prior to the election and was used to collect background information about the respondents and their family as well as their views on democracy and the role of members of parliament (MPs). The second pre-treatment survey was conducted from December 12–16 and was used to collect information about the respondents' knowledge on political parties and the general environment in which they expected the election to take place. By this time, the political parties had chosen their official candidates and communicated

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[10]Before starting the intervention and surveys, the two authors who hail from the study area and the representatives of this NGO held separate meetings with the selected enumerators (field coordinators) to inform them about the data collection process, the importance of maintaining the anonymity and confidentiality of the information, and the importance of keeping a low profile (e.g., to avoid misunderstanding about working for any agency or party). Before the delivery of the treatment messages, the NGO's enumerators were trained by two of the authors. The training was arranged in each constituency before each intervention or survey. To avoid confusion between the two treatments, separate groups of enumerators were involved and trained on separate days. Two authors and the representatives of the NGO directly monitored the interventions and surveys (but the authors did not directly take part in the interventions and surveys). The messages of the treatments and the survey questionnaires were pre-tested to ensure that the messages were understandable for the local population and that questions are not too sensitive.

their election manifestos to the people in these constituencies.[11]

The first post-vote survey took place in the late afternoon and evening of the election day (and the subsequent day if we could not finish the work or did not find the person at home on election day). We used this survey to collect information on voting participation. We asked the respondents about their own and their household members' voting participation and visually checked for an ink mark on their and their spouses' fingers. The presence of an ink mark is the main dependent variable in our analysis.[12] Given the enormous time pressure to collect this information for almost 12,000 households across 302 villages, we conducted a second post-vote survey within a week after the election to collect additional information about the respondents' experience on election day and their views on democracy and the role of MPs.[13]

We collected responses to the first pre-treatment survey from 11,961 respondents, of whom 11,843 responded to the three subsequent surveys as well. Hence, the rate of attrition is only 1.0 percent.[14] The reasons for this low level of attrition are threefold. First, the time period was relatively short, with only around seven weeks between the first and the last survey. Second, in this rural part of Bangladesh, it is generally easy to find the same respondents again. Third, our fieldworkers returned on a subsequent day in case they missed a respondent. We conclude that attrition is not a concern in our study. The final sample size of 11,843 respondents implies that we have information from 39.2 individuals in an average village.

3   Analysis

3.1   Balance tests

Table 1 tests for balance along individual and household-level pre-treatment characteristics. These characteristics, which were all collected in the first pre-treatment survey, are the respondents' gender, age, and years of schooling in columns (1)–(3); the household head's occupation as farmer, laborer, owner of a (typically small) business, or professional in columns (4)–(7); the number of voters living in the household and household income (in 1,000 Taka) in columns (8) and (9). We conduct the balance tests separately for AL and BNP villages, as the random assignment of villages into control and treatment villages was done separately for these two types of villages.

Table 1 around here.

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[11]The election commission declared the candidates' validity and decided on the symbols representing the various parties on December 10.

[12]We did not ask the respondents about the party they voted for, as our pre-tests suggested that this question would have been too sensitive.

[13]Figure B.1 in Online Appendix B summarizes the timing of our treatment deliveries and surveys.

[14]Table B.1 in Online Appendix B provides more detailed information on attrition.

Panel A shows the balance tests for AL villages. In each column, the constant indicates the sample mean in the control group, and the coefficients on the treatment variables the deviation in these treatment groups from the control group. We see that individual pre-treatment characteristics are well balanced across control, policy treatment and legitimacy treatment villages of the AL sample. Panel B shows that individual pre-treatment characteristics are also well balanced in the BNP sample, except that households in the treatment groups have on average 0.2 more voters than households in the control group (with the p-value of the F-test that both treatment variables are jointly 0 being 0.05). These results are robust to the inclusion of constituency fixed effects (see Table C.1 in Online Appendix C).

We also test for balance in village-level characteristics, namely the numbers of voters, the presence of a polling station and schools of various types as well as the distance to the subnational (Upazila) capital and the closest bus stations (see Table C.2 in Online Appendix C). We find that these characteristics are well balanced across control, policy treatment and legitimacy treatment villages in both the AL and the BNP sample.

3.2   Main results

Figure 2 presents average voter turnout, measured by the presence of ink marks on the respondents' fingers directly after the election, across control and treatment groups as well as across AL and BNP villages.

Figure 2 around here.

The two left most bars show that turnout is 65.7 percent in the AL control villages and 56.6 percent in the BNP control villages. The remaining bars show turnout in the treatment villages. In AL villages, the policy and the legitimacy treatment increase turnout by 7.0 p.p. and 15.6 p.p., respectively. In contrast, in BNP villages, the policy and the legitimacy treatment reduce turnout by 10.2 p.p. and 20.7 p.p, respectively. Hence, the legitimacy treatment increase the turnout gap between AL and BNP villages from less than 10 p.p. to more than 40 p.p.

Table 2 reports the effects of our two treatments on voter turnout when adding individual-, household- and village-level controls (including the characteristics reported in the balance tests) as well as constituency-fixed effects.

Table 2 around here.

Panel A presents results for AL villages, and panel B for BNP villages. In column (1), we again measure turnout based on the respondents' ink marks. The dependent variable is equal to 100 if the respondent had an ink mark directly after the election, and zero

otherwise. Panel A confirms the sizable positive effects of both treatments in AL villages, and panel B the sizable negative effects of both treatments in BNP villages. The effect sizes are very similar to the differences in average turnout between control and treatment groups shown in Figure 2, and the corresponding p-values are all smaller than 0.001. Hence, we find strong evidence (i) for opposite effects across village types for both treatments and (ii) for larger effects (in absolute value) of the legitimacy treatment in any village type.

In column (2), we see that the effects are slightly smaller in absolute value, but still sizable when focusing on the ink marks of the respondents' spouses. This finding provides strong evidence for intra-household spillovers of our treatments.

In columns (3)–(5), we look at self-reported (non-verifiable) voting participation by the respondents, their spouses and all household members other than the respondent who are eligible to vote, respectively.[15] We again find sizable treatment effects that are positive in AL villages but negative in BNP villages, and larger (in absolute values) for the legitimacy treatment than the policy treatment. We also find further evidence for substantial intra-household spillovers of our treatments.[16]

In the full sample including both AL and BNP villages, the treatment effects are basically zero (see panel A of Table C.3 in Online Appendix C), implying that the opposing effects across village types almost offset one another. This finding suggests that the effect of simply receiving any treatment (irrespective of its message) is probably small.[17]

The strong positive effect of the legitimacy treatment in AL villages and its strong negative effect in BNP villages are in line with our predictions that the legitimacy treatment promotes voting participation among government supporters but abstention among supporters of the opposition. The positive effect of the policy treatment on voter turnout in AL villages is consistent with our predictions as well. The effect of the policy treatment on voter turnout in BNP villages is theoretically ambiguous (as discussed in the Introduction and shown in Online Appendix A) but empirically negative. We offer an explanation for this negative effect below.

Table 3 shows how our treatments change the respondents' views on the role of MPs and the desirability of multi-party elections. To focus on changes, we use the respondents' views from the second post-election survey as dependent variables and control for their

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[15]Our enumerators asked these other household members directly about their voting participation if they were present, but asked the respondent otherwise.

[16]In the pre-analysis plan, we proposed two additional approaches to study spillovers. The first is based on the use of official data on election outcomes. We decided not to pursue this approach because of the widespread vote rigging on election day (see footnote 5). The second approach is based on exploiting differences in the village-level population and, therefore, the share of treated individuals per village to estimate between-household within-village spillovers. Given that we treat (at most) 40 individuals per village while the average (median) village has 1338 (1380) voters, perhaps this non-finding is not surprising.

[17]Panel B of Table C.3 shows that results are almost identical to those presented in Table 2 when using interaction terms instead of sample splits to estimate separate treatment effects for AL and BNP villages.

views expressed in the first pre-treatment survey.

Table 3 around here.

The policy treatment has strong effects on the respondents' views about whether MPs are responsible for local public goods and income earning opportunities, but not on their views on the desirability of multi-party elections. This pattern confirms that the policy treatment is indeed perceived as relating to policy issues rather than fundamental questions about the value of democracy. The strong effects of the policy treatment on the views about the role of MPs again differ across village types: The policy treatment increases the prevalence of the view that MPs are responsible for local public goods and income earning opportunities in AL villages, but decreases the prevalence of this view in BNP villages.

The legitimacy treatment has considerably weaker effects on the respondents' views on the role of MPs (as compared to the policy treatment), but in AL villages it has a strong negative effect on the respondents' views on the desirability of multi-party elections. This pattern suggests that the legitimacy treatment indeed triggers the respondents to think about the political process and the value of democracy. One might expect a positive effect of the legitimacy treatment on the respondents' views on the desirability of multi-party elections in BNP villages. We do not find such an effect, probably because 94 percent of the respondents in BNP control villages view multi-party elections as desirable, leaving little room for substantial increases in this share.

Let us now return to the negative effect of the policy treatment on voter turnout in BNP villages. Our theoretical framework suggests that the effect of the policy treatment on voting participation of opposition-leaning citizens is ambiguous in general and depends on how treatment affects the citizens' beliefs about the government's approach to public goods provision. It predicts that the policy treatment lowers voting participation if these citizens used to think that the government is reasonably benevolent in its approach to local public goods provision, but start to doubt that once they receive our treatment (see Prediction 6 in Online Appendix A). Such citizens may have planned to vote for the opposition, but the change in beliefs makes them fear that electoral support for the BNP may negatively affect local public goods provision. As a result, they may decide to abstain. The negative effect of the policy treatment on the view that local MPs are responsible for local public goods in BNP villages indeed suggests that the policy treatment changes the respondents' beliefs about how public goods are provided. Therefore, the negative effect of the policy treatment on voter turnout in BNP villages could indeed be driven by a change in beliefs about the government's approach to public goods provision.

3.3 Heterogenous treatment effects

Table 4 studies heterogeneity in treatment effects on voter turnout, as inferred from the respondents' ink marks, along various individual characteristics, using interaction terms between these characteristics and our treatment variables. As these interaction terms vary at the individual level, we can add village fixed effects to absorb any potential differences in treatment effects due to differences across villages.[18]

Table 4 around here.

Inspired by studies on the gender vote gap and information campaigns aimed at raising female voting participation (e.g., Giné and Mansuri, 2018; Chong et al., 2019), we first focus on heterogeneity in treatment effects by gender. In column (1), we see that the gender vote gaps in the control groups are between 3 and 4 p.p. More interesting are the gender differences in the treatment effects: The positive effects of both treatments in AL villages are smaller for women than men, and the negative effects of both treatments in BNP villages are larger in magnitude for women as well. These results suggest that women are more likely than men to respond with voting abstention to any of the two treatments in any of the two village types. These gender differences in the treatment effects are sizable, ranging from 2.7 to 5.6 p.p. (with p-values ranging from 0.00 to 0.05). Hence, these treatments have increased the gender vote gap in both village types. One possible reason is that woman may be more likely to support the BNP.[19] Indeed, the differences in treatment effects between men and women are reminiscent of the differences in treatment effects between AL and BNP villages. Another possible reason is that our treatments could have triggered a sense of insecurity, and that female voting participation may be more sensitive to personal security concerns on election day (e.g., Ahmed, 2008; Giné and Mansuri, 2018).[20] However, independent of the main reason, this gender difference in treatment effects has strong implications for get-out-the-vote campaigns, in particular those that aim at increasing female voting participation.

In column (2), we look for heterogenous treatment effects based on schooling, by adding interaction terms between the treatment variables and the number of years of schooling. We find that respondents with more schooling tend to be more likely to respond with

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[18]The village fixed effects absorb the (uninteracted) treatment variables too. Table C.4 in Online Appendix C presents results without village fixed effects.

[19]According to a survey run by an NGO before the 1996 national elections, the BNP is particularly popular among housewives in rural areas (Daily Ajker Kagoj, June 9, 1996).

[20]Table C.5 in Online Appendix C offers several more results on these gender differences in the treatment effects, which rule out some alternative explanations. First, we show that the results carry over to the respondents' spouses: female spouses of male respondents are typically also more likely to respond with abstentions to the treatment of their spouses than male spouses of female respondents. Second, we show that the gender differences in the treatment effects do not systematically vary with the gender of the enumerator. Third, we show that these gender differences are larger when restricting the sample to respondents who cannot take their voting decision by themselves.

abstention to any type of treatment (just as female respondents do), but these effects are small and imprecisely estimated with the exception of the legitimacy treatment's negative effect in BNP villages. Consistent with Croke et al. (2016), this latter finding suggests that better educated individuals are more likely to understand the rather complicated argument that supporters of the opposition should abstain from voting to reduce the winning party's legitimacy.

In columns (3)–(7), we study heterogeneous treatment effects along the lines of pre-treatment differences in media (radio) access, political knowledge, political activities, views on democracy, and the respondents expectation of whether the elections would be free and fair. We find no evidence for heterogeneous treatment effects along these dimensions. The only exception is that politically active citizens in BNP villages show a stronger response to the (hard-to-understand) legitimacy treatment than politically less experienced citizens.

4   Conclusions

Many governments in weak democracies "manage" the electoral process to make their defeat very unlikely. In this paper, we have presented the result from a field experiment conduct around the 2018 general election in Bangladesh to shed light on the determinants of voting participation in managed elections. We have randomized the salience of (i) the citizens' view that election outcomes matter for policy outcomes and (ii) their view that their voting participation and high turnout increase the winning party's legitimacy. These treatments both increase voting participation in government strongholds, but decrease voting participation in opposition strongholds, with the legitimacy treatment having much stronger effects. We have further shown that there are strong within-family spillovers of these treatments and that women are more likely to abstain in response to any treatment than men.

Our results have important implications for get-out-the-vote and information campaigns in weak democracies and low-income countries more generally. First, even neutrally framed campaigns can simultaneously increase electoral support for the incumbent party and decrease support for opposition parties, further tilting the already tilted playing field in the incumbent's favor. Second, these campaigns may widen the gender vote gap.

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Figures and Tables

Figure 1: Map showing all 302 villages in our sample
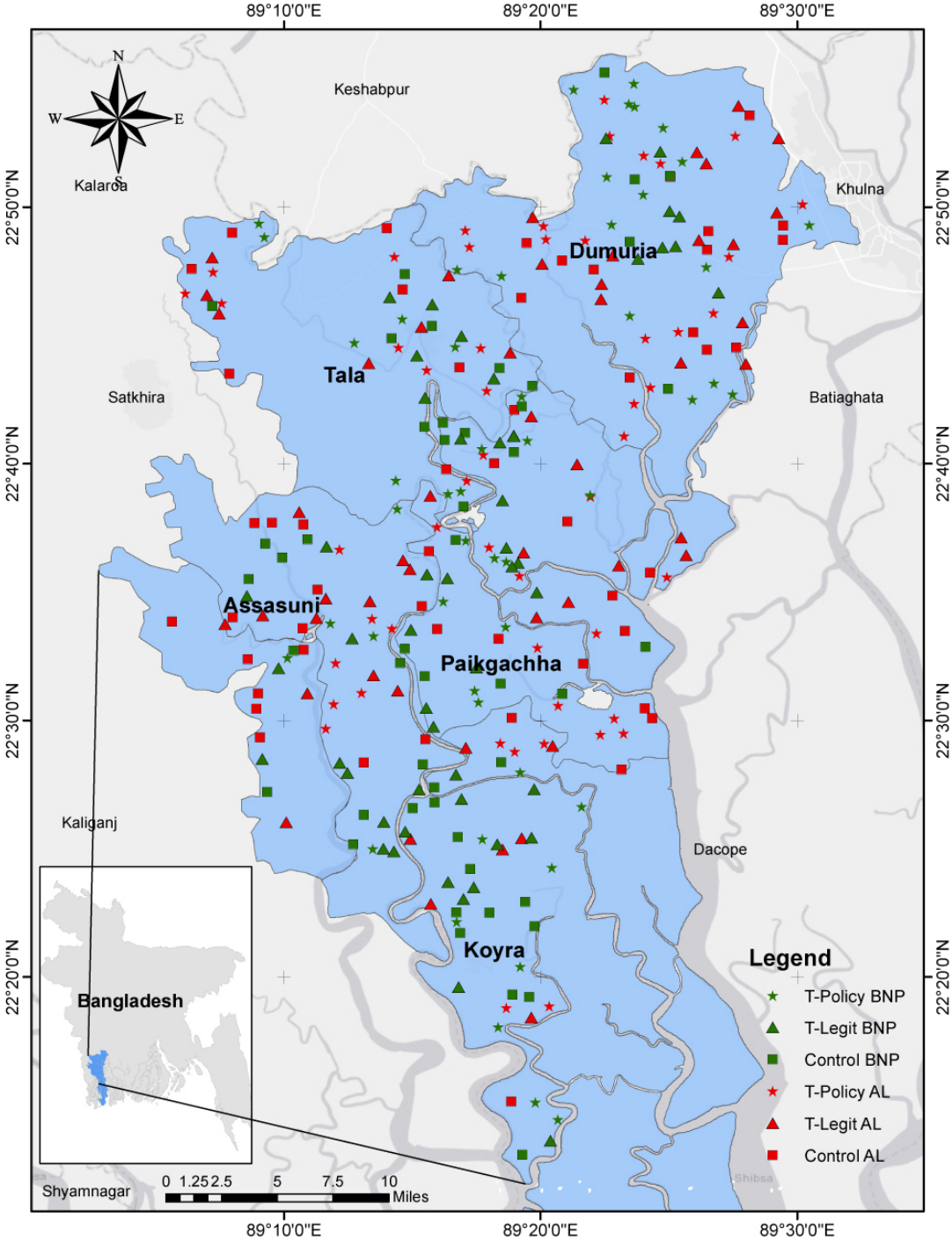
Figure 2: Turnout across treatment groups and village types
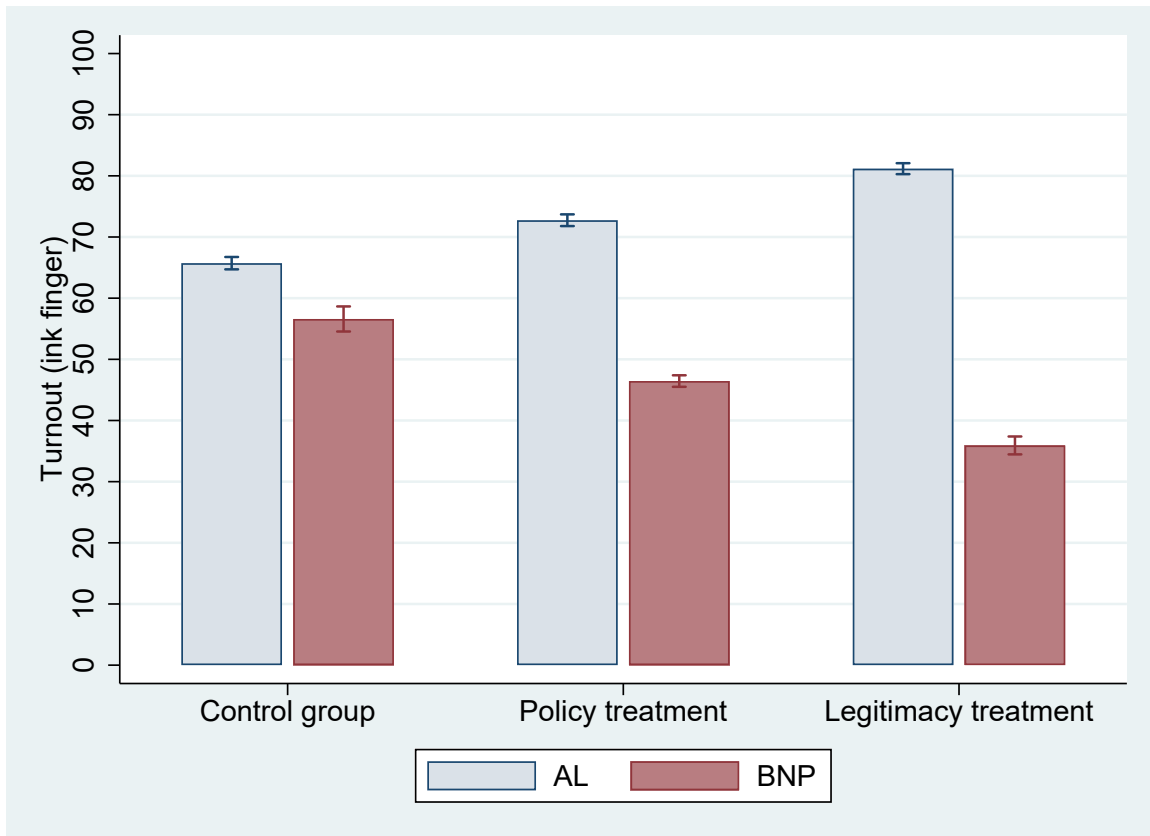
Notes: Bar plots show average voter turnout (in %) for control groups, policy treatment groups and legitimacy treatment groups in AL and BNP villages, respectively. Turnout is based on the ink marks on the respondents' fingers (as observed directly after the election). Vertical lines show 95% confidence intervals.

Table 1: Balance tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	Female	Age	Schooling	Farmer	Laborer	Business	Profes.	Voters	Income
Panel A: AL villages									
Constant	50.2	36.9	9.2	50.7	17.0	21.0	9.2	2.7	97.2
	(0.3)	(0.3)	(0.1)	(3.3)	(2.6)	(2.1)	(1.0)	(0.1)	(4.2)
Policy treatment	-1.0	-0.8	0.1	-0.5	-0.1	0.7	-0.6	0.1	2.7
	(1.1)	(0.6)	(0.1)	(4.7)	(3.5)	(2.8)	(1.4)	(0.1)	(6.0)
Legitimacy treatment	-1.0	0.2	-0.0	-2.7	1.2	0.8	0.2	0.1	6.5
	(0.5)	(0.5)	(0.1)	(4.9)	(3.8)	(3.0)	(1.4)	(0.1)	(9.7)
F-test (p-value)	0.14	0.23	0.69	0.84	0.93	0.96	0.83	0.34	0.78
R-squared	-0.00	0.00	0.00	0.00	-0.00	-0.00	-0.00	0.00	0.00
Observations	6065	6065	6065	6065	6065	6065	6065	6065	6065
Panel B: BNP villages									
Constant	49.3	36.7	9.0	36.5	29.2	26.1	7.5	2.4	95.0
	(0.3)	(0.3)	(0.1)	(3.1)	(3.3)	(2.7)	(1.0)	(0.1)	(4.1)
Policy treatment	-0.6	-0.1	0.1	5.9	-5.4	-4.7	1.6	0.2	2.8
	(0.6)	(0.5)	(0.1)	(4.3)	(4.1)	(3.2)	(1.4)	(0.1)	(6.3)
Legitimacy treatment	0.4	0.1	0.1	1.2	-1.4	-2.8	1.3	0.2	6.9
	(0.5)	(0.5)	(0.1)	(4.3)	(4.4)	(3.6)	(1.4)	(0.1)	(7.3)
F-test (p-value)	0.26	0.92	0.61	0.36	0.37	0.36	0.46	0.05	0.64
R-squared	-0.00	-0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00
Observations	5778	5778	5778	5778	5778	5778	5778	5778	5778

Notes: Dependent variables are indicated in top row and explained in Section 3.1. Linear regressions without any fixed effects or controls. Standard errors (in parenthesis) are clustered at the village level. The p-values refer to the F-test that both treatment variables are jointly 0.

Table 2: Main results

Dependent variable:	(1) Ink mark respondent	(2) Ink mark spouse	(3) Voting respondent	(4) Voting spouse	(5) Voting household
Panel A: AL villages					
Policy treatment	7.1	6.1	12.7	9.6	8.3
	(1.4)	(1.4)	(1.2)	(1.3)	(1.1)
Legitimacy treatment	15.4	13.1	18.8	14.5	12.3
	(1.4)	(1.4)	(1.2)	(1.3)	(1.1)
R-squared	0.03	0.02	0.05	0.02	0.02
Observations	6065	6065	6065	6065	6064
Mean dep. variable	65.7	64.5	69.6	69.7	71.4
Panel B: BNP villages					
Policy treatment	-10.3	-9.3	-8.4	-7.0	-6.2
	(1.6)	(1.6)	(1.6)	(1.6)	(1.5)
Legitimacy treatment	-20.7	-18.2	-14.9	-15.8	-15.5
	(1.6)	(1.6)	(1.6)	(1.6)	(1.5)
R-squared	0.03	0.03	0.02	0.02	0.02
Observations	5778	5778	5778	5778	5778
Mean dep. variable	56.6	52.3	63.7	66.6	65.9

Notes: Dependent variables in columns (1-)–(4) are binary variables equal to 100 in case of voting participation, and zero otherwise, with voting participation based on ink marks for the respondents and their spouses in columns (1) and (2), and self-declared voting participation of the respondents and their spouses in columns (3) and (4). Dependent variable in column (5) is the share (in percent) of household members other than the respondents who are said to have voted. Linear regressions with individual-, household- and village-level controls as well as constituency fixed effects. Individual-level controls are gender, age and schooling of the respondent and her/his spouse. Household-level controls are dummies for the household head's occupation (farmer, laborer, business owner, professional), the number of voters, and household income (in logs). Village-level controls are the number of voters, indicators for the presence of a polling station and primary/secondary/higher schools, shares of the four different occupational categories, and the distance to the Upazila capital and the closest bus stop. Standard errors (in parenthesis) are clustered at the village level. Last row in each panel shows the mean of the dependent variables in the control group.

Table 3: Effects on views about the MPs' role and democracy

	(1)	(2)	(3)
Dependent variable:	MPs for local public goods	MPs for local incomes	Pro multi-party elections
Panel A: AL/government villages			
Policy treatment	16.19	16.78	2.11
	(3.21)	(4.24)	(4.02)
Legitimacy treatment	4.94	9.45	-18.56
	(2.06)	(4.45)	(3.67)
Pre-treatment view	0.61	0.31	0.37
	(0.03)	(0.03)	(0.03)
R-squared	0.48	0.16	0.18
Observations	6065	6065	6065
Mean dep. variable	65.9	57.7	62.3
Panel B: BNP/opposition villages			
Policy treatment	-16.16	-14.59	-4.08
	(4.46)	(4.78)	(1.46)
Legitimacy treatment	-9.07	-6.55	-1.37
	(3.91)	(4.70)	(1.64)
Pre-treatment view	0.44	0.51	0.17
	(0.04)	(0.03)	(0.02)
R-squared	0.26	0.31	0.08
Observations	5778	5778	5778
Mean dep. variable	61.9	52.7	93.6

Notes: Dependent variables are indicated in the top row and based on the respondents' views expressed in the second post-vote survey. "MPs for public goods" is equal to 100 if respondents state that MPs are responsible to arrange funding for local public goods (roads, bridges, schools, colleges, hospitals, etc.); and zero otherwise. "MPs for income" is equal to 100 if respondents state that MPs are responsible to generate income earning opportunities; and zero otherwise. "Pro multi-party elections" is equal to 100 if respondents disapprove with the statements that only one political party should be allowed to stand for election, and that there should be no elections and the prime minister should decide everything; and zero otherwise. In each column, "Pre-treatment view" is based on the same questions and the same coding as the dependent variable, but taken from the first pre-treatment survey. Linear regressions with the same individual-, household- and village-level controls as in Table 2 plus constituency fixed effects. Standard errors (in parenthesis) are clustered at the village level. Last row in each panel shows the mean of the dependent variables in the control group.

Table 4: Heterogeneity in treatment effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Z variable:	Female	Schooling	Radio	Political knowledge	Political activities	Pro democracy	Free/fair
Panel A: AL villages							
Z	-3.7	0.6	-1.8	1.4	0.1	-3.0	0.7
	(2.0)	(0.5)	(3.5)	(3.2)	(3.0)	(3.1)	(3.1)
Policy treatment × Z	-3.3	-0.4	-0.7	5.8	-0.1	0.0	-1.8
	(1.3)	(0.8)	(4.4)	(4.7)	(4.8)	(4.1)	(4.8)
Legitimacy treat. × Z	-4.7	-0.4	4.0	4.3	4.4	0.7	1.6
	(1.4)	(0.7)	(4.2)	(4.1)	(3.8)	(3.9)	(3.8)
R-squared	0.01	0.01	0.01	0.01	0.01	0.01	0.01
Observations	6065	6065	6065	6058	6065	6065	6051
Panel B: BNP villages							
Z	-3.2	1.0	0.3	4.5	7.6	3.8	0.5
	(2.3)	(0.8)	(3.5)	(3.8)	(3.8)	(3.6)	(3.6)
Policy treatment × Z	-5.6	-1.4	0.5	-4.8	-7.0	-3.3	1.6
	(1.4)	(1.0)	(4.5)	(4.8)	(4.7)	(4.6)	(5.2)
Legitimacy treat. × Z	-2.7	-2.5	-1.9	-2.0	-13.6	-4.9	7.5
	(1.4)	(1.0)	(5.2)	(5.7)	(4.8)	(5.1)	(4.7)
R-squared	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Observations	5778	5778	5778	5772	5778	5778	5771
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is the ink mark on the respondent's finger in all columns. Linear regressions with the same individual- and household-level controls as in Table 2 plus village fixed effects. The village fixed effects absorb variation in the (uninteracted) treatment variables and prevent the estimation of separate coefficients for these. Z variables are all based on our pre-treatment surveys. They are dummy variables for female respondents in column (1); access to radio in column (3); basic knowledge on the main political parties, their official symbols and their local candidates in column (4); political activities such as attending community or budget meetings, contacting local government or party officials, or participating in protests in column (5); views against a one-party state and in support of elections in column (6); and expectation of free and fair elections in column (7); and the (non-binary) years of schooling in column (2). Standard errors (in parenthesis) are clustered at the village level.

Online Appendix: Voting or abstaining in "managed" elections? A field experiment in Bangladesh

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Content:

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Appendix A: Theoretical framework

This appendix presents a (deliberately) simple theoretical framework that offers some guidance for understanding the decision to vote or abstain in "managed" elections and to predict how our treatments affect these decisions.

Setup: Citizen i is characterised by $p_i \in \{0, 1\}$, which is 1 (0) if she prefers the government (opposition) party; $l_i \in \{0, 1\}$, which is 1 (0) if she considers the regime to be legitimate (illegitimate); and $\mu_i \in [0, 1]$, which is her belief that the government is benevolent and catering to local preferences rather than clientelistic and rewarding loyalists.

Citizen i takes two related voting decisions: participation decision $v_i \in \{0, 1\}$, with 1 (0) representing participation (abstention) in the election; and party choice $g_i \in \{0, 1\}$, with 1 (0) representing a vote for the government (opposition) party.[2]

Citizen i's "warm glow" utility from participating in the election ($v_i = 1$) is

$$\alpha_i + \beta_i I(l_i = 1) + \gamma_i [\mu_i I(g_i = p_i) + (1 - \mu_i) I(g_i = 1)], \tag{1}$$

where $I(x) = 1$ if statement x is correct, and $I(x) = -1$ otherwise, and where $\beta_i \geq 0$ and $\gamma_i \geq 0$. Her utility from abstaining ($v_i = 0$) is zero and, therefore, independent of g_i. The first component of utility function (1) captures all considerations that affect citizen i's turnout, but are unrelated to her legitimacy and policy considerations, e.g., voting costs, social norms and peer pressure, or pressure from the government to either vote or abstain. The second component captures her benefit from contributing to the regime's legitimacy by participating rather than abstaining. This benefit is positive if she considers the regime to be legitimate ($l_i = 1$), and negative otherwise ($l_i = 0$). The third component captures her benefits from trying to improve local public goods provision. The vote choice g_i that best serves this purpose depends on her beliefs μ_i: She is better off voting according to her preferences ($g_i = p_i$) if $\mu_i \geq \frac{1}{2}$, i.e., if she believes that the government is well-intentional and makes "good use" of knowing the policy preferences of the local population, but better off voting for the government party ($g_i = 1$) if $\mu_i < \frac{1}{2}$, i.e., if she believes that the government has a clientelistic mindset and rewards loyal villages.

Legitimacy treatment: Suppose the legitimacy treatment increases preference parameter β_i. It is easy to see that citizen i becomes more likely to participate (in the sense that she participate in a larger part of the parameter space) if $l_i = 1$, and less likely to participate if $l_i = 0$. This leads to the following prediction:

Prediction 1: The legitimacy treatment increases turnout in villages where most citizens consider the regime to be legitimate (as in government-leaning villages).

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[2]In our field experiment, we observe participation decisions but not vote choices.

Prediction 2: The legitimacy treatment decreases turnout in villages where most citizens consider the regime to be illegitimate (as in opposition-leaning villages).

Policy treatment: Suppose the policy treatment increases preference parameter γ_i and may change belief μ_i. We first look at a citizen i who prefers the government party ($p_i = 1$). This citizen is better off voting for the government party rather than the opposition party independently of her belief μ_i (as $g_i = p_i$ and $g_i = 1$ coincide). Hence, she becomes more likely to participate if γ_i increases, independently of whether and how μ_i changes. This leads to the following prediction:

Prediction 3: The policy treatment increases turnout in government-leaning villages.

We now turn to a citizen i who prefers the opposition party ($p_i = 0$). This citizen votes for the opposition party ($g_i = 0$) if $\mu_i \geq \frac{1}{2}$, and for the government party ($g_i = 1$) otherwise.[3] Given this vote choice, her utility (1) from participating in the election simplifies to

$$\alpha_i + \beta_i I(l_i = 1) + \gamma_i |1 - 2\mu_i|.$$

Therefore, the policy treatment makes her more likely to participate in the election if it increases $\gamma_i |1 - 2\mu_i|$, and less likely to participate otherwise. This result and the assumption that the policy treatment increases preference parameter γ_i and may change belief μ_i leads to the following three predictions:

Prediction 4: The effect of the policy treatment on turnout in opposition villages is ambiguous in general.

Prediction 5: The policy treatment increases turnout in opposition villages if it does not change the citizens' belief about the government's approach to local public goods provision.

Prediction 6: The policy treatment decreases turnout in opposition villages if citizens believe that the government's approach to local public goods provision is relatively benevolent ($\mu_i > \frac{1}{2}$), but the treatment lowers this belief (μ_i closer to $\frac{1}{2}$).[4]

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[3]Without loss of generality, we assume that citizen i chooses $g_i = p_i$ if indifferent.

[4]There exists a symmetric prediction: The policy treatment decreases turnout in opposition villages if citizens believe that the government's approach to local public goods provision is relatively clientelistic ($\mu_i < \frac{1}{2}$), but the treatment lower this belief (μ_i closer to $\frac{1}{2}$).

Appendix B: Further information on field experiment

B.1 Timeline

Figure B.1: Timeline
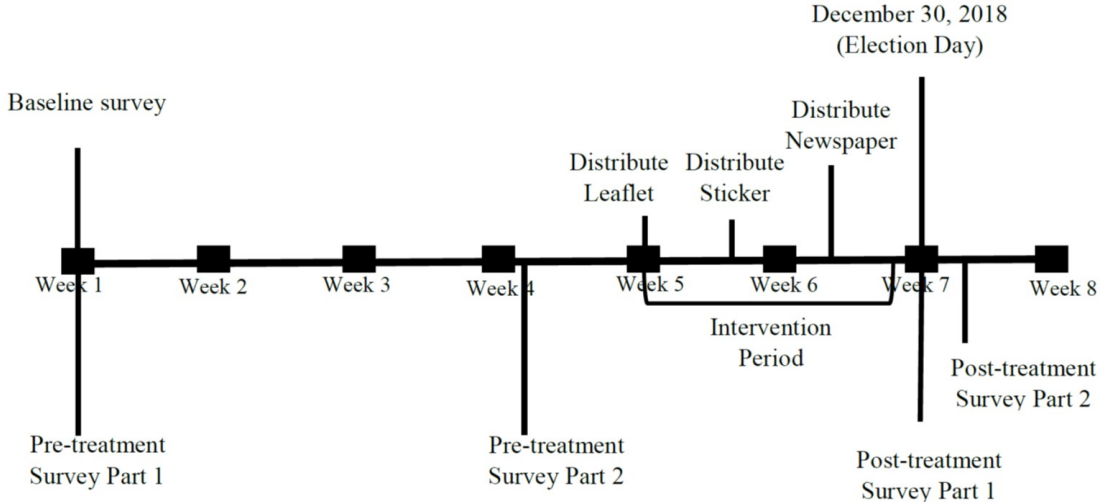
B.2 Treatment messages

Figure B.2: Leaflets used for treatment delivery
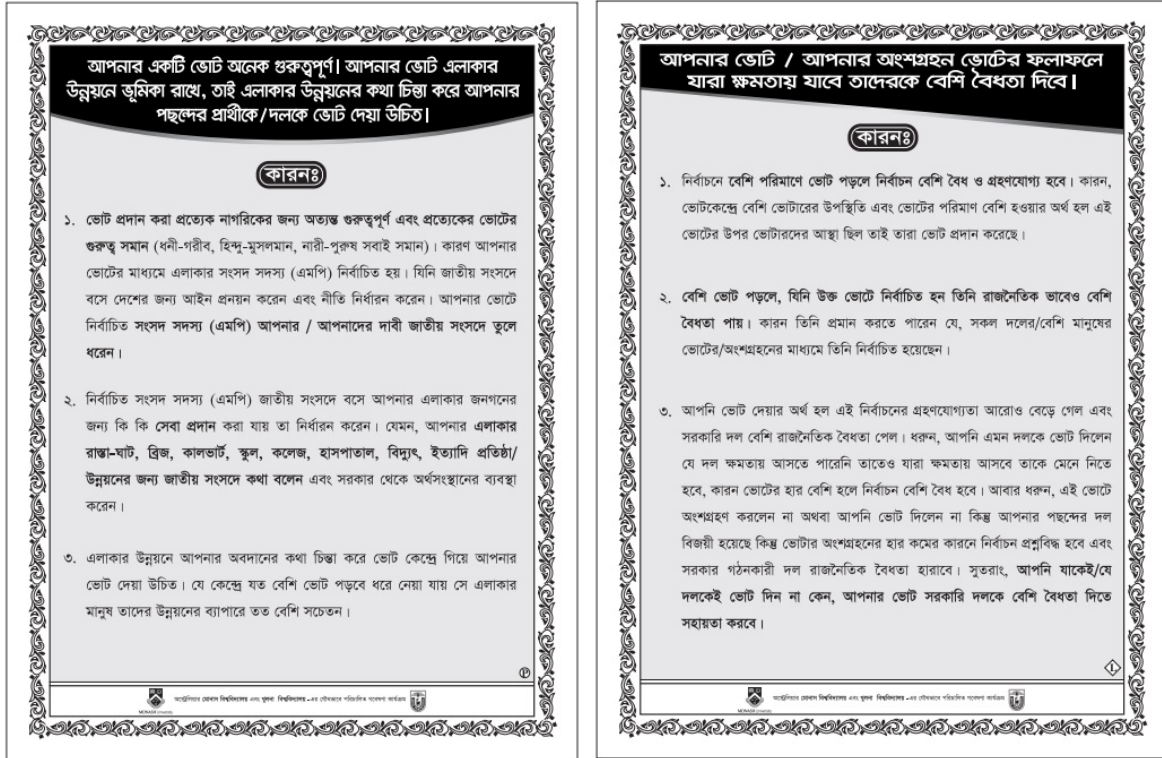
Notes: Top left (right) shows leaflet for policy (legitimacy) treatment in Bengali. See below for translated version.

Figure B.3: Stickers used for treatment delivery

Notes: Top left (right) shows stickers for policy (legitimacy) treatment in Bengali. The stickers summarize and illustrate the text on the leaflets. See below for translated version of the leaflets.

Figure B.4: Newspaper advertorials used for treatment delivery

Notes: Top left (right) shows newspaper front page with advertorial for policy (legitimacy) treatment in Bengali inside the red box. The advertorials summarize the text on the leaflets. See below for translated version of the leaflets.

Translation of leaflet for policy treatment: "Your vote is very important. Your vote plays an important role in the development of your area. So you should cast vote for your preferred candidate or party considering the importance of development of your area. Because:

1. Voting is important for every citizen and everyone's vote is equally important (...). The member of the parliament (MP) is elected by your vote. He/she legislates the laws and makes policy for the country through the parliament. MP, elected by your vote, presents your (people in your area) demands in the parliament.

2. The elected MPs help decide what services can be provided for the people of your area through National Parliament. For example, they demand for repair or construction or new roads, bridges, culvert, schools, colleges, hospitals, electricity, etc., in the parliament and arrange necessary funds from the government.

3. Considering the contribution to the development of your area, it is important to cast your vote. The people in the polling station (village/centre) with higher voter turnout are considered more conscious about the development of the area."

Translation of leaflet for legitimacy treatment: "Your vote/participation in the election will give more legitimacy to those who will win the election and be in power. Because:

1. The election will be more legitimate and acceptable, the more people casts their votes. Presence of more voters in the polling stations and increased number of voters casting their ballots indicate that the voters have confidence in this election, and hence they voted in the election.

2. When the voter turnout is high, then the person elected get more political legitimacy. Because the winning candidate/party can prove that the party has been elected by participation of all political parties/majority of the people.

3. Casting your vote in the election will help enhance the legitimacy of this election and the winning (governing) party will get more political legitimacy as a result of that. Suppose you cast vote for a party but that party lost in the election. If there are high turnout in the election then people will accept the results and the elected party, as higher voter's participation in the election will give more legitimacy to the election. If, on the other hand, you do not cast vote but your preferred party wins the election, then the results of the election will be questioned and the party forming the government will lose political legitimacy because of the low voter turnout. So, whoever/party you vote for, your vote will help the elected government to get more legitimacy."

B.3 Attrition

Table B.1: Attrition

Village type	Respondents answering survey 1	Missing in survey 2	Missing in survey 3	Missing in survey 4	Respondents answering all surveys	Rate of attrition (in %)
AL control	2,059	8	14	12	2,045	0.7
AL legitimacy treatment	2,029	11	16	15	2,013	0.8
AL policy treatment	2,052	27	45	41	2,007	2.2
BNP control	1,881	12	17	17	1,864	0.9
BNP legitimacy treatment	1,967	6	10	8	1,957	0.5
BNP policy treatment	1,973	8	16	16	1,957	0.8
Total	11,961	72	118	109	11,843	1.0

Notes: Surveys 1 and 2 are pre-treatment surveys, and surveys 3 and 4 are post-treatment surveys (see Section 2.3 for details).

Appendix C: Additional results

Table C.1: Balance tests based on individual data, with constituency fixed effects

Dependent variable:	(1) Female	(2) Age	(3) Schooling	(4) Farmer	(5) Laborer	(6) Business	(7) Profes.	(8) Voters	(9) Income
Panel A: AL villages									
Policy treatment	-1.0	-1.0	0.1	-2.0	1.7	0.7	-1.1	0.1	0.5
	(1.1)	(0.6)	(0.1)	(4.6)	(3.3)	(2.9)	(1.4)	(0.1)	(5.7)
Legitimacy treatment	-1.1	0.2	-0.0	-3.8	2.7	0.7	-0.1	0.1	6.3
	(0.5)	(0.5)	(0.1)	(4.6)	(3.4)	(3.0)	(1.3)	(0.1)	(9.6)
F-test (p-value)	0.12	0.16	0.67	0.71	0.72	0.96	0.71	0.48	0.80
R-squared	-0.00	0.00	0.01	0.01	0.04	0.00	0.00	0.02	0.01
Observations	6065	6065	6065	6065	6065	6065	6065	6065	6065
Panel B: BNP villages									
Policy treatment	-0.5	-0.3	0.1	4.9	-2.5	-5.1	1.1	0.1	2.5
	(0.6)	(0.5)	(0.1)	(4.4)	(4.0)	(3.5)	(1.5)	(0.1)	(6.3)
Legitimacy treatment	0.4	0.1	0.1	1.6	-1.4	-2.9	1.2	0.2	6.7
	(0.5)	(0.5)	(0.1)	(4.2)	(4.2)	(3.6)	(1.3)	(0.1)	(7.2)
F-test (p-value)	0.38	0.80	0.53	0.54	0.82	0.34	0.59	0.09	0.65
R-squared	-0.00	0.00	0.00	0.01	0.02	0.00	0.00	0.02	0.01
Observations	5778	5778	5778	5778	5778	5778	5778	5778	5778

Notes: Dependent variables are indicated in top row and explained in Section 3.1. Linear regressions with constituency fixed effects. Standard errors (in parenthesis) are clustered at the village level. The p-values refer to the F-test that both treatment variables are jointly 0.

Table C.2: Balance tests at village-level, with and without constituency fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Voters	Polling station	Distance to SN capital	Distance to bus station	Primary school	Secondary school	Higher school
Panel A: AL villages (without constituency fixed effects)							
Constant	1324.2	88.5	10.2	7.4	94.2	50.0	53.8
	(66.7)	(3.4)	(0.7)	(0.5)	(2.7)	(7.0)	(6.9)
Policy treatment	28.4	7.6	0.4	0.2	3.8	8.8	8.9
	(94.8)	(4.9)	(1.0)	(0.7)	(3.8)	(9.9)	(9.8)
Legitimacy treatment	9.8	7.6	-0.4	-0.0	1.8	1.0	-2.9
	(94.8)	(4.9)	(1.0)	(0.7)	(3.8)	(9.9)	(9.8)
F-test (p-value)	0.95	0.20	0.96	0.75	0.61	0.62	0.46
R-squared	-0.01	0.01	-0.01	-0.01	-0.01	-0.01	-0.00
Observations	154	154	154	154	154	154	154
Panel B: AL villages (with constituency fixed effects)							
Policy treatment	37.0	8.0	0.2	0.1	2.4	5.9	6.0
	(96.5)	(4.9)	(0.9)	(0.7)	(3.8)	(10.0)	(9.9)
Legitimacy treatment	10.7	7.6	-0.4	0.0	1.2	-0.5	-4.4
	(95.8)	(4.9)	(0.9)	(0.7)	(3.8)	(9.9)	(9.9)
F-test (p-value)	0.93	0.19	0.98	0.81	0.83	0.78	0.57
R-squared	-0.03	-0.00	0.08	0.05	0.01	-0.00	0.00
Observations	154	154	154	154	154	154	154
Panel C: BNP villages (without constituency fixed effects)							
Constant	1342.8	97.9	10.4	7.7	93.8	50.0	52.1
	(58.9)	(2.9)	(0.7)	(0.6)	(3.7)	(7.2)	(7.3)
Policy treatment	0.0	-1.9	0.0	0.7	-1.8	6.0	3.9
	(82.4)	(4.0)	(1.0)	(0.9)	(5.1)	(10.1)	(10.2)
Legitimacy treatment	-20.0	-3.9	-0.6	-0.3	0.2	-8.0	-8.1
	(82.4)	(4.0)	(1.0)	(0.9)	(5.1)	(10.1)	(10.2)
F-test (p-value)	0.96	0.63	0.47	0.81	0.91	0.38	0.48
R-squared	-0.01	-0.01	-0.01	-0.00	-0.01	-0.00	-0.00
Observations	148	148	148	148	148	148	148
Panel D: BNP villages (with constituency fixed effects)							
Policy treatment	50.8	-1.8	0.0	0.5	-1.2	2.2	0.6
	(85.2)	(4.2)	(1.1)	(0.9)	(5.3)	(10.5)	(10.6)
Legitimacy treatment	-6.5	-3.7	-0.7	-0.5	0.9	-8.9	-9.0
	(82.3)	(4.0)	(1.1)	(0.9)	(5.1)	(10.2)	(10.2)
F-test (p-value)	0.76	0.66	0.54	0.74	0.92	0.51	0.57
R-squared	-0.00	-0.00	-0.01	-0.00	-0.01	0.00	-0.01
Observations	148	148	148	148	148	148	148

Notes: Dependent variables are indicated in top row and explained in Section 3.1. Linear regressions without any controls and fixed effects in panels A and C, but with constituency fixed effects in panels B and D. Robust standard errors in parenthesis. The p-values refer to the F-test that both treatment variables are jointly 0.

Table C.3: Main results estimated in full sample including AL and BNP villages

Dependent var.:	(1) Ink mark respondent	(2) Ink mark spouse	(3) Voting respondent	(4) Voting spouse	(5) Voting household
Panel A: Average effects					
Policy treatment	-2.0	-2.0	1.9	1.2	0.9
	(1.1)	(1.1)	(1.0)	(1.0)	(1.0)
Legitimacy treatment	-2.7	-2.7	2.0	-0.6	-1.5
	(1.1)	(1.1)	(1.0)	(1.0)	(1.0)
R-squared	0.01	0.01	0.01	0.01	0.01
Observations	11843	11843	11843	11843	11842
Mean dep. variable	61.4	58.7	66.8	68.2	68.8
Panel B: Separate effects for AL and BNP villages					
BNP	-9.1	-12.2	-6.0	-3.1	-5.5
	(1.5)	(1.5)	(1.4)	(1.4)	(1.3)
Policy treatment × AL	7.0	6.0	12.6	9.5	8.3
	(1.5)	(1.5)	(1.4)	(1.4)	(1.3)
Policy treatment × BNP	-10.2	-9.2	-8.2	-6.9	-5.8
	(1.5)	(1.5)	(1.4)	(1.5)	(1.3)
Legitimacy treatment × AL	15.5	12.9	18.8	14.4	12.4
	(1.5)	(1.5)	(1.4)	(1.4)	(1.3)
Legitimacy treatment × BNP	-20.7	-18.1	-14.8	-15.8	-15.2
	(1.5)	(1.5)	(1.4)	(1.5)	(1.3)
R-squared	0.10	0.10	0.09	0.06	0.07
Observations	11843	11843	11843	11843	11842
Mean dep. variable	61.4	58.7	66.8	68.2	68.8

Notes: Sample includes all respondents from AL and BNP villages in both panels (unlike in all other tables). Dependent variables are indicated in top row and explained in Section 3.1. Linear regressions with the same individual-, household- and village-level controls as in Table 2 plus constituency fixed effects. AL is the omitted category in panel B. Standard errors (in parenthesis) are clustered at the village level. Last row in each panel shows the mean of the dependent variables in the control group.

Table C.4: Heterogeneity in treatment effects without village fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Z variable:	Female	Schooling	Radio	Political knowledge	Political activities	Pro democracy	Free/fair
Panel A: AL villages							
Z	-3.6	0.5	-0.5	0.5	0.3	-1.7	0.8
	(1.9)	(0.5)	(2.0)	(2.2)	(2.3)	(2.0)	(1.7)
Policy treatment	8.7	9.7	7.7	4.7	7.4	7.6	8.6
	(0.8)	(6.8)	(1.0)	(2.1)	(0.9)	(2.4)	(1.2)
Policy treatment $\times$ Z	-3.2	-0.3	-1.9	3.5	-1.3	-0.8	-3.3
	(1.3)	(0.7)	(2.5)	(3.0)	(3.3)	(3.1)	(2.5)
Legitimacy treatment	17.8	17.8	14.8	13.7	14.8	15.5	15.4
	(0.8)	(6.0)	(1.0)	(2.0)	(0.9)	(2.1)	(1.1)
Legitimacy treat. $\times$ Z	-4.7	-0.3	2.2	2.6	2.8	-0.1	0.6
	(1.4)	(0.6)	(2.4)	(2.7)	(2.8)	(2.8)	(2.2)
R-squared	0.03	0.03	0.03	0.03	0.03	0.03	0.03
Observations	6065	6065	6065	6058	6065	6065	6051
Panel B: BNP villages							
Z	-4.0	0.6	-0.1	3.9	2.9	3.6	0.5
	(2.0)	(0.7)	(2.3)	(2.4)	(2.8)	(3.3)	(2.4)
Policy treatment	-7.6	-0.2	-10.3	-6.4	-9.9	-7.4	-10.7
	(1.3)	(8.1)	(1.3)	(2.3)	(1.3)	(3.8)	(1.7)
Policy treatment $\times$ Z	-5.5	-1.1	0.2	-5.3	-2.4	-3.4	0.8
	(1.4)	(0.9)	(2.7)	(3.0)	(3.6)	(4.3)	(3.3)
Legitimacy treatment	-19.7	-3.2	-20.6	-19.1	-19.5	-15.0	-22.4
	(1.4)	(8.6)	(1.5)	(2.6)	(1.5)	(3.9)	(1.6)
Legitimacy treat. $\times$ Z	-2.6	-2.0	-1.3	-2.5	-7.4	-7.1	5.4
	(1.4)	(0.9)	(3.2)	(3.2)	(3.6)	(4.6)	(3.2)
R-squared	0.03	0.03	0.03	0.03	0.03	0.03	0.03
Observations	5778	5778	5778	5772	5778	5778	5771

Notes: Dependent variable is the ink mark on the respondent's finger in all columns. Linear regressions with the same individual-, household- and village-level controls as in Table 2 plus constituency fixed effects. Z variables are described in the table notes of Table 4. Standard errors (in parenthesis) are clustered at the village level.

Table C.5: More on heterogeneity in treatment effects by gender

Dependent variable:	(1) Ink mark respondent	(2) Ink mark spouse	(3) Ink mark respondent	(4) Ink mark respondent	(5) Ink mark respondent	(6) Ink mark respondent
Panel A: AL villages						
Female	-3.7	-4.7	-0.5	-3.9	-3.9	2.0
	(2.0)	(2.4)	(2.5)	(3.9)	(2.4)	(4.2)
Policy treatment × Female	-3.3	-3.8	-5.0	-1.6	-1.1	-8.4
	(1.3)	(1.9)	(1.6)	(2.0)	(2.0)	(3.5)
Legitimacy treat. × Female	-4.7	-5.3	-5.7	-2.9	-4.3	-7.9
	(1.4)	(1.5)	(1.8)	(2.7)	(1.8)	(3.6)
R-squared	0.01	0.71	0.74	0.72	0.74	0.72
Observations	6065	6065	4407	1658	3959	2106
Panel B: BNP villages						
Female	-3.2	-4.8	-1.3	-2.0	-2.7	3.8
	(2.3)	(2.3)	(2.6)	(4.5)	(3.2)	(4.9)
Policy treatment × Female	-5.6	-3.2	-4.7	-8.0	-4.8	-10.8
	(1.4)	(1.7)	(1.7)	(2.9)	(2.4)	(3.6)
Legitimacy treat. × Female	-2.7	-8.1	-1.2	-7.9	-3.0	-5.5
	(1.4)	(1.5)	(1.6)	(2.9)	(2.2)	(3.6)
R-squared	0.02	0.44	0.48	0.45	0.48	0.46
Observations	5778	5778	3776	2002	3834	1944
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample restriction	None	None	Female enum.	Male enum.	Own voting decision	Not own decision

Notes: Dependent variables are indicated in top row and explained in Section 3.2. Linear regressions with the same individual- and household-level controls as in Table 2 plus village fixed effects. The village fixed effects absorb variation in the (uninteracted) treatment variables and prevent the estimation of separate coefficients for these. Female refers to the gender of the respondent in columns (1) and (3)–(6), but the gender of the spouse in column (2). Columns (3) and (4) split the AL/BNP samples by the gender of the enumerator. Columns (5) and (6) split the AL/BNP samples by whether or not the respondents can decide whom to vote for by themselves. Standard errors (in parenthesis) are clustered at the village level.