Partisan effects of information campaigns in competitive authoritarian elections: Evidence from Bangladesh *

Short title: Partisan effects of information campaigns

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September 8, 2023

Abstract

To study the effects of non-partisan information and get-out-the-vote (GOTV) campaigns on the partisan composition of the voting population in competitive authoritarian elections, we conducted a large-scale field experiment prior to the 2018 Bangladeshi general election. Our two treatments highlight that high turnout increases the winning party's legitimacy and that election outcomes matter for policy outcomes. Both treatments increase turnout (measured by ink marks) in government strongholds but decrease turnout in opposition strongholds. We explain the withdrawal of treated opposition supporters and conclude that non-partisan information and GOTV campaigns can further tilt the uneven playing field in competitive authoritarian elections.

Keywords: Competitive authoritarian elections, information campaigns, voting behavior, field experiment, Bangladesh.

JEL classification: C93, D72.

*We acknowledge helpful comments by the editor, Ekaterina Zhuravskaya, four anonymous referees, Annatina Aerne, Farzana Afridi, Eri Bertsou, Chongwoo Choe, Paul Schaudt and Hannah Smidt as well as conference and seminar participants at the Australasian Public Choice Workshop, the Bruneck Workshop in Political Economy, the Monash Workshop on Sustainable Development, the Swiss Development Economics Workshop, the University of Aix-Marseille, the University of Graz, and the University of St.Gallen. We acknowledge financial support by Monash University. The project was approved by the Monash University Human Research Ethics Committee (Project ID: 17640). The pre-analysis plan is available at the American Economic Association's RCT Registry (AEARCTR-0003509).

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1 Introduction

Since the end of the Cold War, the number of countries holding elections has increased, but many elections take place in countries that are not well-functioning democracies. These elections are often best described as "competitive authoritarian elections" (Levitsky and Way, 2002; Howard and Roessler, 2006), where opposition parties are allowed to compete but the incumbent government takes actions "to create an uneven playing field" (Levitsky and Way, 2002, p. 53) in order to make its own defeat unlikely. Western donor agencies and NGOs often view democracy with free and fair elections as desirable. They have various options for supporting democracy and strengthening the electoral process. One option is to support non-partisan information campaigns that remind citizens of the benefits of democracy and the importance of voting participation.

Consider the case of the Bangladeshi Election Working Group (EWG), which is a non-partisan network of around 30 NGOs committed to free and fair elections and good governance in Bangladesh. The EWG is supported by the Asia Foundation, which has a long history of close ties to the U.S. government and has received funding from the development agencies of Denmark, Sweden, Switzerland, and the United Kingdom for its work in Bangladesh. A core activity of the EWG are information programs, typically run by local NGOs, with the objective of increasing voter turnout.[1]

Non-partisan information campaigns that aim to encourage people to vote – so-called get-out-the-vote (GOTV) campaigns (Gosnell, 1927; Gerber and Green, 2000) – vary in their specific messages, but many emphasize the importance of high turnout for ensuring the legitimacy of the winning party or for promoting effective policy making. However, little is known about the effect of such campaigns on voting participation of government and opposition supporters and, therefore, the partisan composition of the voting population in competitive authoritarian elections.

In this paper, we present the results of a large-scale field experiment that examines the effect of non-partisan GOTV campaigns on voter turnout in government and opposition strongholds during a competitive authoritarian election. Specifically, we conducted the experiment during the Bangladeshi general election in December 2018, which presented an ideal testing ground,

[1] See https://bd.usembassy.gov/upcoming-election-in-bangladesh/ and the Asia Foundation (2016).

given the incumbent government's efforts to tilt the playing field in its favor. These efforts included the imprisonment of high-profile opposition politicians, restrictions on press freedom, and partisan appointments to the Election Commission (see Section 2.2 for details). We were aware of the uneven playing field when applying for IRB approval, when registering our pre-analysis plan, and when implementing our interventions in December 2018.

The core idea of our field experiment was to deliver two different treatments (or GOTV campaigns) to individuals of two different types of villages. The "policy treatment" focused on the message that voting outcomes affect policy outcomes, and the "legitimacy treatment" on the message that high voter turnout increases the legitimacy of the winning party's government. Similar messages are regularly delivered by non-partisan information and get-out-the-vote campaigns in Bangladesh and elsewhere.[2] Taken at face value, the wording of these messages is neutral; they simply encourage people to participate in the election without naming any parties or candidates or even suggesting whom to vote for.[3] We were interested in the potentially differential effects of these treatments on turnout in government and opposition strongholds. Therefore, before running our experiment, we collected information that allowed us to classify villages as either government or opposition strongholds. A locally known, non-partisan NGO delivered the treatment messages multiple times to selected individuals in the corresponding treatment villages by conducting door-to-door visits and by distributing leaflets, stickers, and newspapers with advertorials. Convinced that survey answers cannot provide reliable information on such a sensitive topic as voting behavior in a competitive authoritarian election, we opted for a logistically challenging data gathering effort: we checked for an ink mark on the fingers of our almost 12,000 respondents and their spouses directly after the election.

We specified in the pre-analysis plan that we are particularly interested in the potentially differential effects of the two treatments on voter turnout in government and opposition villages. We expected that the policy treatment would increase turnout in both types of villages, whereas the legitimacy treatment would increase turnout in government villages, but decrease turnout in opposition villages. Our findings only partially confirm our expectations. We find that the policy and the legitimacy treatment increase turnout, as measured by ink marks, by

[2]Other NGOs that run campaigns promoting voting participation, political awareness and good governance in Bangladesh include Shushashoner Jonno Nagorik (SHUJAN), Transparency International Bangladesh as well as NGOs affiliated with the Bangladeshi Election Working Group (EWG).

[3]We discuss the neutral and non-partisan framing of our treatment messages in more detail in Section 3.3.

around 7 and 15 percentage points in government villages, but decrease turnout by around 10 and 21 percentage points in opposition villages. These effects spill over to the spouses of the treated respondents. Therefore, each of our treatments substantially altered the partisan composition of the voting population to the government party's favor, with no noteworthy changes in total turnout. This finding highlights that non-partisan GOTV campaigns run prior to competitive authoritarian elections can further tilt the already tilted playing field to the incumbent government's advantage.

We present a theoretical framework inspired by Fiorina (1976) to offer a well-structured discussion about why our treatments led to a withdrawal from voting in opposition villages. This framework highlights rational individuals who face both instrumental and expressive voting motives and incur voting costs.[4] The substantive argument for why the legitimacy treatment reduced turnout in opposition villages builds on Croke et al. (2016), who argue that opposition supporters may deliberately disengage and abstain from voting in order to de-legitimize the regime in (competitive) authoritarian elections. We posit that the legitimacy treatment makes this option more salient, thereby reducing the opposition supporters' expressive benefit from the act of voting itself. As a result, fewer opposition supporters may find it worthwhile to pay the voting costs, leading to lower turnout in opposition villages. Croke et al. (2016) further suggest that deliberate abstention is a sophisticated strategy that requires good education and political awareness. We find that the withdrawal from voting in opposition villages indeed tends to be stronger for respondents with more years of schooling, access to television (as a proxy for political information), and first-hand political experience. These findings provide empirical support for our argument that some respondents from opposition villages withdraw from voting in response to the legitimacy treatment in order to de-legitimize the regime.[5]

To understand why the policy treatment reduced turnout in opposition villages, it is important to notice that Bangladeshi politics is clientelistic and polarized (see Section 2.1 for details).[6] Our argument consists of three steps. First, the policy treatment makes instrumental

[4] An individual's instrumental motive is based on the expected benefit from getting the party with the more attractive (economic) policy into power. In contrast, their expressive motives are "more directly and immediately linked to the act of voting, or of voting for a particular candidate or option, itself" (Hamlin and Jennings, 2011, p. 645).

[5] Consistent with this argument, the legitimacy treatment also lowers the share of respondents from opposition villages who thought the election was free and fair.

[6] See Wantchekon (2003) and Vicente and Wantchekon (2009) for early experimental work on how clientelism in general and clientelistic election campaigns in particular shape voting behavior.

voting motives more salient. Second, given the tilted playing field and the clientelistic nature of politics in Bangladesh, the policy treatment makes opposition supporters realize that voting for opposition party candidates does not provide any instrumental benefits (as these candidates are unlikely to win and would lack access to the government even if they won). These opposition supporters also realize that their instrumental benefits may be higher when voting for government party candidates, but still limited (as opposition supporters are not part of a government party-based patron-client network). In support of these claims, we document that the policy treatment indeed leads to a substantial decrease in the share of respondents from opposition villages who think that members of parliament (MPs) can actually provide local public goods and income earning opportunities. Third, due to the polarized nature of Bangladeshi politics, opposition supporters obtain considerably higher expressive benefits from voting for their party than the government party they dislike. The policy treatment thus leads to a situation in which instrumental and expressive voting motives point in different directions. As a result of this "cross-pressure," some opposition supporters are no longer willing to pay the voting costs and withdraw from voting.

We contribute to the experimental literature on information and GOTV campaigns. Most early contributions to this literature focus on whether and how such campaigns (or particular features thereof) can increase voter turnout in elections in the United States (e.g., Gerber and Green, 2000; Arceneaux and Nickerson, 2009; Gerber et al., 2011; Enos et al., 2014). An exception is the early experimental study of GOTV campaigns in competitive authoritarian elections by Guan and Green (2006). They randomize their treatments at the level of dorm rooms at Peking University and find positive effects of their door-to-door campaign on the students' voting participation. Like us, they find treatment effects that are large (compared to those documented for the United States).[7]

More recently, there has been a surge in experimental studies evaluating information campaigns in a diverse set of weak democracies and electoral autocracies. However, few of these studies explicitly focus on GOTV campaigns highlighting the importance of voting participation. Notable exceptions include Giné and Mansuri (2018) and Marx et al. (2021) who evaluate treatments with GOTV messages – with the one in Giné and Mansuri (2018) being very sim-

[7]Enikolopov et al. (2011) argue that new information – in their case media access – tends to have particularly large effects on voter turnout and voting choices in countries with weak democratic institutions, where parties often run on platforms with vaguely defined ideologies. This is the case in Bangladesh as well (see Section 2.1).

ilar to our policy treatment – as well as separate treatments providing information about the electoral process. In addition, Aker et al. (2017) and Chong et al. (2019) study campaigns that combine information about the electoral process with standard GOTV messages.[8]

We contribute to this literature in two ways. First, to the best of our knowledge, we are the first to experimentally study the message that high turnout increases the winning party's legitimacy. Studying this message is important. Many governments around the world have (or develop) autocratic tendencies and may strategically hold competitive authoritarian elections exactly because they want to legitimize their regime (e.g., Croke et al., 2016; Levitsky and Way, 2002). Thus, it is of prime importance to understand the consequences of GOTV campaigns promoting this motive for voting in the context of competitive authoritarian elections.

Second, we contribute to this literature by showing that non-partisan GOTV campaigns can have differential effects on the actual voter turnout of government and opposition supporters in a weak democracy or electoral autocracy. While many of these studies focus on average treatment effects, we deliberately focus on the differential effects of our two treatments in government and opposition strongholds. We indeed find large positive effects of both treatments on voter turnout in government strongholds and large negative effects in opposition strongholds. These differential effects have important implications for Western donors and NGOs that aim to strengthen democracy and increase voter turnout in countries with weak democratic institutions, but instead run the risk of further tilting already tilted playing fields with their information and GOTV campaigns.

It is well known that partisan information can have differential effects on the political behavior of government and opposition supporters in weak democracies and autocracies. For example, propaganda can have such differential effects (e.g., Adena et al., 2015; Peisakhin and Rozenas, 2018; Caesmann et al., 2021). More closely related to our paper, Baysan (2022) evaluates a randomized information campaign run by the largest party opposing the 2007 constitutional referendum in Turkey. She finds that this partisan campaign increased the share of "no" votes in regions where this party was relatively strong in past elections, but decreased

[8]Other experimental studies (conducted in a wide range of different developing countries) evaluate information campaigns providing information about the honesty or performance of incumbent politicians (Chong et al., 2015; Dunning et al., 2019; John and Sjoberg, 2020) or the funding they have available (Cruz et al., 2021); information campaigns highlighting campaign promises (Cruz et al., 2018) or the candidates' profiles more generally (Platas and Raffler, 2021); anti-vote-buying campaigns (Blattman et al., 2019; Hicken et al., 2018; Vicente, 2014); anti-violence campaigns (Collier and Vicente, 2014); and partisan information campaigns run by a political party (Baysan, 2022; Wantchekon, 2003).

this share in regions where this party was relatively weak.[9] The main difference of our study is to show that even non-partisan GOTV campaigns can have such differential effects.

Thereby our paper relates to two recent studies on Kenyan politics that also document partisan effect in response to non-partisan information. Marx et al. (2021) show that their treatments eroded trust in the Electoral Commission after a contentious election, but only among respondents from ethnic groups linked to the losing coalition. John and Sjoberg (2020) document that a non-partisan parliamentary monitoring website (rather than specific GOTV campaigns) increased intended future political participation of self-declared government supporters but not self-declared opposition supporters. Moreover, we relate to studies documenting how different types of information campaigns can change the vote shares of different political parties or candidates. For example, Aker et al. (2017) find that their civic education treatment, which was centered around flyers with information about the political process but also contained standard GOTV messages, increased the vote share of the government candidate at the expense of the main opposition candidate.[10] We differ from these studies by focusing on specific GOTV messages and by relying on our pre-treatment classification of government and opposition strongholds (similar to Baysan, 2022) rather than post-treatment vote shares. This pre-treatment classification allows us to identify differential effects of our GOTV campaigns on the voter turnout among government and opposition supporters.[11]

The remainder of the paper is structured as follows: Section 2 provides background information on politics in Bangladesh and the 2018 general election. Section 3 discusses our experimental design, its implementation, and ethical considerations. Section 4 presents balance tests, our main results, and various robustness tests. Section 5 explains why our two treatments may have reduced voter turnout in opposition villages. Section 6 briefly concludes.

[9]See Pons (2018) for a randomized study on the effects of a partisan campaign conducted on a national scale in France, i.e., in a strong rather than a weak democracy.

[10]In addition, Collier and Vicente (2014) and Vicente (2014) show that anti-violence and anti-vote-buying campaigns can increase the electoral support for government candidates. In contrast, the anti-vote-buying campaign studied by Blattman et al. (2019) and the video-recorded candidate interviews studied by Platas and Raffler (2021) increased the electoral support for opposition candidates. Moreover, Giné and Mansuri (2018) find that their information campaigns aimed at increasing female voting participation also had an impact on the vote shares of different political parties.

[11]In contrast, a higher vote share for the government party could be driven by differential effects on voter turnout or by some (former) opposition supporters voting for the government party.

2 Politics in Bangladesh

2.1 From independence to 2018

Bangladesh, the eighth most populous country in the world, became independent in 1971. A popular uprising ended a period of military dictatorship in 1990, after which the political parties agreed to hold an election organized and supervised by a civilian, non-partisan caretaker government (NCG). The two main parties were the Awami League (AL) led by Sheikh Hasina and the Bangladesh Nationalist Party (BNP) led by Khaleda Zia. Sheikh Hasina is the daughter of Sheikh Mujibur Rahman, who was leader of the AL during the struggle for independence and the first president of Bangladesh. He is considered to be the founding father of Bangladesh. Khaleda Zia is the widow of Ziaur Rahman, who is the founder of the BNP and another former president, often credited for promoting stability and development.[12] The BNP won the 1991 general election and re-introduced a parliamentary system (Riaz and Parvez, 2021). The AL and the BNP have been the two main parties ever since, and they are still led by Sheikh Hasina and Khaleda Zia, respectively.

Further general elections organized and supervised by a NCG took place in 1996 and 2001. These elections were reasonably free and fair and led to changes in government from the BNP to the AL and back to the BNP (Jahan, 2015; Riaz, 2019; Riaz and Parvez, 2021). Prior to the scheduled 2006 general election, the BNP tried to bias the composition of the NCG in its favor, leading to violent protests by AL supporters. A military-backed technocratic government assumed power and ruled the country until early 2009. The AL won a two-thirds majority in parliament in a reasonably free and fair general election organized by the military-backed technocratic government in December 2008 (Riaz and Parvez, 2021).

After coming back to power, Sheikh Hasina and her AL government passed a constitutional amendment in 2011 to abolish the NCG system, which had previously ensured reasonably free and fair elections. This constitutional change is seen as a "watershed moment for the democracy in the country" (Riaz and Parvez, 2021, p. 5), as it paved the way for competitive authoritarian elections in which the incumbent government appoints officials of the Election Commission based on their party affiliation and loyalty. In the lead-up to the 2014 general

[12] Sheikh Mujibur Rahman and Ziaur Rahman are not consanguine. They were both assassinated by army officers while in power. See Jahan (2015) for a discussion of their roles and influence on the AL and the BNP, respectively.

election, the AL government took further measures to create an uneven playing field, including arresting thousands of opposition party members, preventing BNP rallies, and putting Khaleda Zia under house arrest. In response to these actions, the BNP and other opposition parties boycotted the election in order not to legitimize the electoral process and the regime (Jahan, 2015; Riaz and Parvez, 2021). The AL's official vote share was 72.1 percent in a largely uncontested election.

In the early years, there were some ideological differences between the AL and the BNP, with the AL being more committed to secularism and the BNP highlighting the country's Islamic identity (Jahan, 2015). However, as time passed, the two parties and their current leaders put ever less emphasis on ideology and the consolidation of democracy, but ever more emphasis on staying in power and winning elections by any means (Islam, 2013; Riaz, 2019). They both attempted to monopolize and abuse state power (Islam, 2013).

The two parties and their leaders compensated the decreasing importance of ideology by relying on a network of patron-client relations, personalized leadership, and the "past successes" of their founding fathers, Sheikh Mujibur Rahman and Ziaur Rahman. These patron-client relations are based on reciprocal exchange in personalized, hierarchical settings, in which patrons can extract labor, service, respect and political loyalty from their clients, while clients can expect material benefits and opportunities of various kinds in return (Rahaman, 2007; Islam, 2013). Each party can be seen as a web of patron-client relations: "In a pyramidal structure, this web connects the Prime Minister, through numerous links down the system, to the lowly peasant. Each client uses the resources received from the patron above them to build their own patronage empire" (Islam, 2013, p. 153). Furthermore, Bangladeshi citizens expect MPs to prioritize local development. MPs respond to this demand by promising material benefits during their campaigns and focusing on their local areas in their motions in parliament (Jahan, 2015). However, the system of patronage and the personalization of politics has polarized the country and led to "two separate 'tribes' in a homogeneous society" (Islam, 2013, p. 155). For example, there are regular complaints that MPs direct development goods and services to their own supporters and that MPs of opposition parties struggle to secure funding for the development of their constituency (Jahan, 2015).

2.2 The 2018 general election

The most recent general election in Bangladesh was held on December 30, 2018. This election took place on an uneven playing field, as the ruling AL government had taken steps to suppress the opposition in the months leading up to the election. For example, in early 2018, Khaleda Zia was sentenced to five years in prison on corruption charges in a trial that was widely seen as influenced by the government. Other high-profile BNP candidates were also sent to prison or accused of corruption. In addition, there were irregularities in city corporation elections close to our study area in May 2018, with supporters of the AL forcibly taking control of polling stations.[13] Freedom of the press worsened during 2018, and there were politically motivated incidents of extrajudicial killings and enforced disappearances. Later, the partisan Election Commission disqualified many opposition candidates. Despite these irregularities, the BNP and other opposition parties decided to participate in the general election, forming the Jatiya Oikya Front coalition to run against the AL-led Grand Alliance (Riaz, 2019; Riaz and Parvez, 2021). We were aware of this uneven playing field when we applied for IRB approval, registered our pre-analysis plan and implemented our interventions.

Obviously, we could not yet know what would happen on election day. Official voter turnout was 80.2 percent, and the AL's official vote share increased from 72.1 in 2014 to 74.6 percent in 2018, despite facing the major opposition parties. The BNP came second with an official vote share of only 13.1 percent. Riaz and Parvez (2021) discuss even more surprising results at the level of individual polling stations. Some polling stations recorded a voter turnout of 100 percent (or, implicitly, more), and others recorded zero votes for the BNP candidate. These results were partly due to vote rigging. There is plenty of anecdotal evidence of fake votes and ballot stuffing, and of voters and opposition polling agents being hindered from entering polling centers.[14] The BNP and its coalition partners rejected the official election results and demanded that fresh elections be held under a NCG, but to no avail.

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

[14]For example, BBC, "Bangladesh elections: Deadly clashes mar vote," December 30, 2018; The Daily Star, "As We Saw," December 31, 2018; The Daily Star, "Polls Anomalies in 47 of 50 Seats," January 16, 2019; The Economist, "Elections in Bangladesh: Leaving nothing to chance," January 5, 2019; The Guardian, "Bangladesh PM Hasina wins thumping victory in elections opposition reject as 'farcical'," December 31, 2018.

3 Experimental design and implementation

We aimed to investigate the impact of two treatments on voter turnout in government and opposition strongholds. Specifically, we delivered a policy treatment emphasizing the link between voting outcomes and policy outcomes, and a legitimacy treatment highlighting the importance of high turnout in enhancing the political legitimacy of the winning party's government.

3.1 Sampling and randomization

Our study area is located in the Khulna District and the Satkhira District, which are both part of the Khulna Division in south-western Bangladesh. It includes the five rural sub-districts (*upazila*) Assasuni, Dumuria, Koyra, Paikgachha, and Tala.[15] These sub-districts belong to four electoral constituencies of the national parliament: Khulna 5, Khulna 6, Satkhira 1, and Satkhira 3. These four constituencies contain 563 polling stations. Most polling stations comprise multiple villages, and most villages consist of several neighborhoods (*para*).

Our experimental design required a sample of villages or neighborhoods that we could classify as either government or opposition strongholds, i.e., as leaning towards either the AL or the BNP. For this purpose, we primarily relied on the results from the 2001 and 2008 general elections, which were the last two reasonably free and fair national elections.[16] We selected at most one village per polling station and applied a simple rule whenever possible to classify villages based on previous election results. A village was classified as a government (opposition) village if the AL (BNP) and its coalition parties got more votes than the BNP (AL) and its coalition parties in the polling station containing this village in both the 2001 and the 2008 general elections. We could apply this simple rule in about two thirds of the villages in our sample. If the relative electoral strength of the two parties changed from 2001 to 2008, we used information from focus group-based village questionnaires (rather than polling station-level information) to classify villages.[17]

[15] Online Appendix Figure A.1.1 shows a map of the study area, and Online Appendix Table A.2.1 uses data from the Bangladesh Household Income and Expenditure Survey 2016 to show that Khulna Division is representative of the entire country in terms of age, education, income, and household size.

[16] Earlier elections were unsuitable due to changes in polling stations prior to the 2001 general elections. The 2014 general election was unsuitable, as it was boycotted by the BNP (see Section 2.1).

[17] We administered a separate village questionnaire and collected information through focus group discussions with 5–6 people from each village, consisting of village leaders, teachers, (non-partisan) local government representatives, and village elders. These participants were selected based on their knowledge of local politics and their ability to represent diverse viewpoints in the village. We asked them to identify whether the majority

Our aim was to identify 150 villages with a clear leaning towards the AL and 150 villages with a clear leaning towards the BNP. To achieve this, we collected information for slightly over 300 villages. Ultimately, we identified 154 government villages that had supported the AL and its coalition partners in past elections and 148 opposition villages that had supported the BNP and its coalition partners.

We validated our classification of villages into government and opposition strongholds in multiple ways. First, we compared the results from the focus group-based village questionnaires with the election results in villages where the same party had won both the 2001 and the 2008 general election. For all these villages, the classification based on the village questionnaires confirmed our classification based on the election results. Second, for the villages where the relative electoral strength had changed from 2001 to 2008, we collected the vote shares from the 1996 general election and found that our focus group-based classification was consistent with a classification based on the 1996 vote shares for the villages belonging to polling stations that remained unchanged from 1996 to 2001. Third, we tested whether our classification was predictive for the difference in the official polling station-level vote shares of the AL and the BNP in the 2018 general election. Online Appendix Table A.3.1 does so based on three different samples: our full-sample, the sub-sample of our control villages, and a new sample with 184 polling stations from four other electoral constituencies in Khulna District. We indeed find that this difference in vote shares was higher in government villages/polling stations than opposition villages/polling stations.[18] These results may lend further support to our classification, but they must be taken with a large grain of salt because the official 2018 election data is untrustworthy (see Section 2.2). In addition, we later show that our results are robust when restricting the sample to villages from polling stations with large differences between the AL and the BNP vote share in the 2001 and 2008 general elections.

Our randomization strategy contained the following stages: First, we randomly assigned the 154 government (148 opposition) villages into 52 (48) control villages, 51 (50) villages where we would deliver the policy treatment, and 51 (50) villages where we would deliver the legitimacy

of the villagers were currently leaning more towards the AL or the BNP, taking into account the results from all previous elections and current numbers of active supporters of the different parties. The participants also provided information on neighborhoods that were particularly supportive of the party that was more popular in their village.

[18]The estimated coefficients are sizeable and statistically significant in the full and the new alternative sample, but not in the sub-sample of our control villages.

treatment. The map in Online Appendix Figure A.1.1 indicates government and opposition villages in red and green, respectively, and denotes control villages by squares, policy treatment villages by stars, and legitimacy treatment villages by triangles.

Second, we selected a neighborhood (*para*) in each village. This selection was non-random as we intended to target individuals in government and opposition strongholds. We used our village-level focus-group discussions to learn about the neighborhoods of government (opposition) villages with a particularly strong leaning towards the AL (BNP).

Third, we relied on systematic random sampling to select 40 individuals from within each of these neighborhoods. We always started from one side of the chosen neighborhood and selected every third household. In so doing, we alternated between male and female subjects and focused on literate and married individuals between the ages of 20 and 55 years. We focused on literate individuals to ensure that they could read and understand our treatment messages (e.g., the leaflets), and on married individuals in order to look at spillovers within couples.

We instructed the enumerators of our first pre-treatment survey to select the immediate neighbor's household if nobody was at home, if no married couple lived in the household, or if the targeted individual was not literate. However, these are all unlikely scenarios in our study area. First, it was very rare that nobody was at home, as women stay at home most of the time. Second, we were not informed of any instance in which no married couple lived in a household, which is not surprising given that women typically move out of their parents' household after getting married and live with their husbands' family. Third, most people are literate in our study area.[19] Moreover, we instructed our enumerators to make an appointment at a suitable time on the current or subsequent day if they needed to select the husband (wife) from a given household, but only their spouse was at home. We were able to survey 11,961 respondents in our first pre-treatment survey, which corresponds to 99.0 percent of our target sample size of 40 individuals in each of the 302 villages.[20]

[19] According to the Bangladesh Household Income and Expenditure Survey 2016, 88 percent of individuals aged 20–55 years living in Khulna Division are married. Moreover, there is at least one literate spouse in this age bracket in 87 percent of households in Khulna Division. Literate men and women in Khulna Division have 7.6 and 7.1 years of schooling on average (see Online Appendix Table A.2.1).

[20] In some villages the number of respondents was slightly less than 40 for a number of reasons, e.g., that the selected neighbourhood in that village was not large enough, that many households were away for seasonal work, or for festivals or religious or social gatherings outside the villages.

3.2 Organizational matters

Our field work consisted of two pre-treatment surveys, three different modes of delivery of the two treatment messages, and two-post vote surveys. We discuss all these interventions and surveys in detail in Sections 3.3 and 3.4, respectively. Figure 1 shows the timeline of our fieldwork.

Figure 1 about here

Given that we needed to complete four surveys and three interventions in 302 villages within a relatively short time-frame, we decided to collaborate with a local NGO: the Global Development and Research Initiative (GDRI). This NGO was an ideal partner for several reasons. First, it is well-known in the study area, where it has been working since 2009, and has a good reputation as a non-partisan, research-orientated NGO typically working on social issues such as education, health, and disaster relief. Second, it is experienced in conducting large-scale surveys within relatively short time frames. Moreover, some of the authors had worked with this NGO in earlier research projects.

We collaborated with GDRI for all surveys and the delivery of the treatment messages. GDRI has a large pool of field workers and enumerators from the local area whom it can hire as and when needed. It hired separate sets of enumerators for the pre-treatment surveys, the treatment delivery, and the post-election surveys. This measure helps to reduce potential experimenter demand effects, even though such effects are not a major concern for our main outcome variable (ink marks). In order to avoid confusion, we also relied on separate sets of enumerators for the delivery of the legitimacy and the policy treatment.

The two authors who hail from the study area and the representatives of the NGO held separate training sessions with the enumerators in each constituency before each survey and each intervention (with the training sessions for the legitimacy and the policy treatment taking place on different days). These sessions emphasized the need for discretion and the importance of ensuring data confidentiality and avoiding any misunderstandings about working for a particular agency or party. The authors and NGO representatives also monitored the fieldwork without participating directly in the surveys or interventions.

At the beginning of the first survey, the respondents were fully informed about the survey process and the fact that the surveys and interventions were part of a research project. All

respondents provided their consent to participate in the study.

3.3 Treatment delivery

We had two treatments. The policy treatment focused on the message that voting outcomes would affect policy outcomes, and the legitimacy treatment on the message that high voter turnout would increase the legitimacy of the winning party's government. We framed both treatments in a neutral and non-partisan manner.[21] The wording – taken at face value – encouraged people to participate in the election without naming any parties or candidates or even suggesting whom to vote for. Moreover, the treatment messages were delivered by local enumerators from the locally known, non-partisan NGO, and the respondents were informed at the beginning of the first survey that the intervention was part of a research project.[22] We are thus confident that our treatments were generally considered to be non-partisan. While it is possible that some individuals interpreted our treatment messages as partisan,[23] it is important to highlight that none of our enumerators was met with objections or even aggression – despite the rather tense situation prior to the election. This absence of resistance in both government and opposition strongholds would have been highly unlikely if our interventions had been widely perceived as being partisan.

We delivered the treatment messages in three different ways to each randomly selected individual in policy or legitimacy treatment villages. First, we conducted door-to-door visits from December 17–19. 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. The leaflet for the legitimacy treatment discussed how one's voting participation gives greater legitimacy

[21] We pre-tested all treatment messages and survey questions to ensure that they were understandable for the local population and that the questions were not too sensitive and would not make the respondents feel uncomfortable.

[22] The leaflets and the stickers we used to deliver the treatment messages contained the logos of both Monash University and Khulna University (see Online Appendix Figures A.4.1 and A.4.2). Monash University is recognized as a world-class university in Bangladesh and, in particular, in the study area, where researchers from Monash University had collaborated with the same NGO on earlier research projects. Khulna University is located very close to the study area and is locally well-known as one of the best universities in Bangladesh.

[23] For example, it is possible that some individuals considered these messages to be pro-government, because they highlighted positive aspects of elections despite the chances that the upcoming election would be rigged. It is also possible that some individuals understood the legitimacy treatment message as non-partisan, because the incumbent government was likely to (officially) win the election.

to the winning party's government (see Online Appendix Figure A.4.1 and our translations in Online Appendix A.4).

Second, we provided stickers with the corresponding messages to these individuals from December 22–23. These stickers contained the key points of the leaflets and some illustrative pictures (see Online Appendix Figure A.4.2).

Third, we published advertorials conveying these messages in a well-known, non-partisan local newspaper (see Online Appendix Figure A.4.3). The advertorials with these two messages were published on two subsequent days, and we distributed the newspaper with the policy (legitimacy) advertorial to the randomly selected individuals in policy (legitimacy) treatment villages from December 26–27. In this part of Bangladesh, newspapers are mainly read in urban areas, whereas the people living in the rural villages that constitute our sample area are generally unable or unwilling to buy newspapers. Only six percent of our respondents read a newspaper regularly. Hence, it is unlikely that information spillovers from these advertorials contaminated our treatments.

We are confident that our treatment messages were well understood by most targeted individuals, as we delivered them three times in the two weeks leading up to the election. This repeated exposure allowed individuals to engage with the messages and reflect on their content, which is important to consider when interpreting our results.

3.4 Surveys and measurement

We conducted two pre-treatment and two post-election surveys. We used the first pre-treatment survey, which we conducted about six weeks prior to the election, to collect background information about the respondents and their views on democracy and the role of MPs.[24]

The second pre-treatment survey was conducted from December 12–16. By this time, the Election Commission had confirmed the official candidates' eligibility and decided on the symbols representing the various parties, and the parties themselves had communicated their manifestos. We used this survey to collect information on the respondents' knowledge of political parties and the general environment in which they expected the election to take place. We did

[24]Online Appendix Table A.2.1 compares our sample with the households from the Bangladesh Household Income and Expenditure Survey 2016. It suggests that our sample is fairly representative for the population in Khulna Division as well as the entire country in terms of age, education, household size, and income.

not ask the respondents whom they planned to vote for. Our pre-tests suggested that such questions would be too sensitive and would make respondents feel uncomfortable. Moreover, the respondents may not have responded truthfully or may have decided to stop participating in our study. However, we did ask which party's candidate the respondents believed was most likely to become the local MP. The shares of respondents suggesting the local AL (BNP) candidate was 82.1 (5.4) percent in government villages and 53.2 (33.6) percent in opposition villages. Hence, even in opposition villages most respondents viewed it as unlikely that a local BNP candidate could win. The share of respondents expecting free and fair elections was 42.1 percent in government villages and 30.8 in opposition villages at this point in time, i.e., two to three weeks prior to election day.

The primary objective of the first post-election survey was to inquire about the respondents' and their spouses' voting participation and, importantly, to visually check for an ink mark on their fingers. Although the ink used in this and many other elections in developing countries is typically indelible and remains visible for several days or even weeks (Ferree et al., 2020), we conducted the first post-election survey in the late afternoon and evening of election day to be extra cautious. If we could not complete the work or locate some respondents (or their spouses) at home on election day, we continued the survey on the following day. We also asked the respondents about their views on democracy and the role of MPs. However, we did not ask them about the party they voted for, as our pre-tests suggested that such questions would be too sensitive. Due to the tremendous time pressure of gathering all this information for nearly 12,000 households across 302 villages, we conducted a second post-election survey within a week after the election to collect additional information on their election day experience.

Attrition is not a major concern in our study. We collected responses from 11,961 respondents in the first pre-treatment survey. 11,843 of these respondents also answered the first post-election survey, and 11,763 answered all four surveys. Hence, the rate of attrition between the first pre-treatment survey and the first post-election survey was 1.0 percent and the total rate of attrition 1.7 percent (see Online Appendix Table A.5.1). These low attrition rates can be attributed to several factors: the relatively short duration of our study (see Figure 1), the ease of finding respondents or their spouses at home in this rural part of Bangladesh, and our enumerators' efforts to revisit respondents on the subsequent day in case they missed them.

The 11,843 respondents – corresponding to 39.2 respondents in an average village – who answered the first pre-treatment survey and the first post-election survey constitute our main sample, as our main results solely rely on information collected in these two surveys. We drop all other respondents. Hence, the number of observations is slightly smaller for variables based on the second pre-treatment survey or the second post-election survey. Online Appendix A.6 describes all the (survey-based) variables used in our analyses and presents summary statistics in Table A.6.1.

3.5 Ethical considerations

As previously discussed, we were aware of the uneven playing field created by the incumbent government when we applied for IRB approval, registered our pre-analysis plan and implemented our interventions. We expected the legitimacy treatment to increase voter turnout in government villages and decrease turnout in opposition villages, and the policy treatment to increase turnout in both village types. However, it turned out that both treatments increased turnout in government villages and decreased turnout in opposition villages (see Section 4.2). The question arises whether our interventions might have benefited an incumbent government with authoritarian tendencies.

We are convinced that the effect of our interventions on the election outcome is absolutely negligible. There are three reasons for this. First, similar non-partisan information and GOTV campaigns are common in Bangladesh (and elsewhere). Staging one more campaign of this type in four out of the 300 Bangladeshi electoral constituencies was unlikely to make a difference. Second, Bangladesh has more than 100 million registered voters and, therefore, around 350,000 registered voters in an average electoral constituency. The treatment of around 8,000 individuals spread across four electoral constituencies would be unlikely to make a difference even in a free and fair election. Third, informed observers and the majority of our respondents were aware of the uneven playing field and did not expect free and fair elections. The vote-rigging on election day indeed ensured that the AL easily won the four electoral constituencies in which our intervention took place. Averaged across these four consistencies, the AL candidates officially received 287,944 votes and the BNP candidates only 23,586 votes. This large difference makes it clear that our study could not have had an effect on official constituency- or country-level

election outcomes.

Our findings, however, have important ethical implications for future information and GOTV campaigns in weak democracies and electoral autocracies. After all, campaigns similar to ours could influence the election result to the benefit of the incumbent government if the number of targeted individuals were larger or the number of voters smaller, or if institutional constraints prevented the incumbent government from tilting the playing field as much as in the election we studied. We come back to these implications in the conclusions.

4 Analysis

4.1 Balance tests

Table 1 tests for balance along individual- and household-level characteristics collected in the first pre-treatment survey: the respondent's gender, age and years of schooling in columns (1)–(3), the household head's occupation as either a farmer, laborer, owner of a (typically small) business, or professional in columns (4)–(7); the number of voters living in the household in column (8); and the annual household income (in 1,000 Bangladeshi Taka) in column (9). We run separate tests for government and opposition villages, as the random assignment of villages into control and treatment groups was done separately for each village type. We cluster standard errors at the village level. We also report randomization inference (RI) p-values based on a permutation test at the village level by randomly shuffling the treatment status 1,000 times (Young, 2019).

Table 1 about here

We find that the individual- and household-level characteristics are well-balanced across control and treatment groups in both village types, except that households in control opposition villages have on average 0.2 fewer voters than households in treatment opposition villages. Online Appendix Table B.1.1 shows that results remain qualitatively and quantitatively similar when constituency-fixed effects are added.

We test for balance in village-level characteristics, such as the numbers of voters, the presence of a polling station and schools of various types, and the distance to the sub-district (*upazila*) capital or the nearest bus stop, in Online Appendix Table B.1.2. These characteris-

tics are well-balanced too.

4.2 Main results

Figure 2 presents the average voter turnout, measured by ink marks on the respondents' fingers, across control and treatment groups. Panel A pools all our respondents. Voter turnout is 61.4 percent among individuals in the control group and only slightly lower among individuals in the policy treatment group (59.8 percent) and the legitimacy treatment group (58.9 percent). Hence, the averaged treatment effects are very small.[25] Therefore, if our main goal had been to study the average effects of our two treatments on voter turnout in competitive authoritarian elections, we would have to conclude that these treatments have no noticeable effects.

Figure 2 about here

However, as highlighted already in the pre-analysis plan, we are particularly interested in the potentially differential effects of the two treatments on voter turnout in government and opposition strongholds. Panel B therefore presents voter turnout across control and treatment groups separately for government and opposition villages. The two leftmost bars show that turnout is 65.7 percent in control government villages and 56.6 percent in control opposition villages, implying a turnout gap of 9.1 percentage points (pp). In government villages, the policy and the legitimacy treatment further increase turnout by 7.0 pp and 15.5 pp, respectively. In contrast, these two treatments reduce turnout in opposition villages by 10.2 pp and 20.7 pp, respectively.

These are large effects, but large effects of information and GOTV campaigns are not uncommon in competitive authoritarian elections. For example, Guan and Green (2006) find that their one-time delivery of GOTV messages to dorm rooms at Peking University increased the students' voting participation by 13 pp (and even by 18 pp when excluding students from university departments with a baseline voting participation of more than 90 percent). Chong et al. (2015) provide an example of an information campaign that had a large negative effect on voter turnout. They study a campaign distributing flyers with information about local levels

[25] Online Appendix Table B.2.1 confirms that these average treatment effects are not statistically significant at conventional levels.

of corruption prior to local elections in Mexico and find that exposing the median level of corruption reduces voter turnout by 12 pp.[26] Moreover, the effects we find may also be large because our campaign was fairly intense given that we delivered the treatment messages three times (and in three different ways) within the final two weeks prior to the election.

The large differences between the treatment effects in government and opposition villages imply that the policy and the legitimacy treatment increase the turnout gap between government and opposition villages from 9.1 pp to 26.3 pp and 45.3 pp, respectively. These results demonstrate that information and GOTV campaigns can have a large impact on the partisan composition of the voting population, even if they do not affect total turnout.

Table 2 reports the results from linearly regressing voter turnout – again measured based on ink marks – on our two treatments. Panel A presents the results for government villages, and panel B those for opposition villages. We again cluster standard errors at the village level and further report randomization inference (RI) p-values.

Table 2 about here

In column (1), we show the results from regressions that do not include any control variables. The coefficient estimates correspond to the effects shown graphically in Figure 2 and are statistically significant. In column (2), we add individual-, household- and village-level control variables as well as constituency-fixed effects. It is reassuring that the coefficient estimates and the standard errors remain almost identical. To summarize, we find sizeable positive effects of both treatments in government villages and sizeable negative effects of both treatments in opposition villages. Moreover, the legitimacy treatment's effects are larger (in absolute values) in both types of villages.

To study intra-household spillovers, we focus on the ink marks of the respondents' spouses in columns (3) and (4). We find that the effects of our treatments on the spouses' voter turnout are slightly smaller in absolute value than their effects on the respondents' turnout, but still

[26]Enikolopov et al. (2011) argue that new information has particularly large effects on voting behavior in countries with weak democratic institutions, where parties often run on platforms with vaguely defined ideologies. They show that the availability of the only independent national TV channel (NTV) decreased voter turnout by 3.8 pp in the 1999 Russian parliamentary elections. Furthermore, NTV availability decreased the vote share of the government party by 8.9 pp and increased the combined vote share of the major opposition parties by 6.3 pp. The authors find even larger effects when using survey data and focusing on respondents who watched NTV.

sizeable. We conclude that there are strong intra-household spillovers.

The results shown in the Figure 2 and Table 2 report average treatment effects in government and opposition villages, respectively. The effects of these treatments on individuals known to be government or opposition supporters could be of interest as well, but we cannot directly estimate these effects because asking our respondents about their party preferences would have been too delicate. Instead we targeted neighborhoods in government (opposition) villages whose residents were known for strongly favoring the AL (BNP). Our estimates thus correspond to the effects for individual government or opposition supporters if and only if our identified government and opposition neighborhoods were homogeneous in terms of party preferences. Anecdotally, party preferences are largely determined by neighbourhood head or elders' dictate, and residents of the same neighborhood are often linked to the same patron. As a result, party preferences are indeed fairly homogeneous within these neighborhoods.[27]

However, we expect some degree of heterogeneity in party preferences even within these neighborhoods. In this case, we can use the estimated treatment effects in government and opposition strongholds to calculate the differences in treatment effects for government and opposition supporters. Table 2, column (1), suggests differences in the effects of the policy and the legitimacy treatment between government and opposition strongholds of 17.4 pp and 36.1 pp, respectively. To illustrate, let us assume that there are around 90 percent government supporters and 10 percent opposition supporters in government strongholds and vice versa in opposition strongholds. We then have to multiply the differences in the treatment effects between government and opposition strongholds by a factor of $1/(0.9 - 0.1) = 1.25$ to obtain the differences in the treatment effects between government and opposition supporters. Hence, the implied differences in the effects of the policy and the legitimacy treatment between government and opposition supporters would be around 22 pp and 45 pp, respectively. These differences would be larger if there was more heterogeneity in party preferences within these neighborhoods,[28] and smaller if there was less heterogeneity.

[27] Our focus-group discussions confirmed that the identified neighborhoods are generally homogeneous in terms of party preferences.

[28] For example, these differences would be around 29 pp and 60 pp if we assumed more conservatively that there are around 80 percent government supporters and 20 percent opposition supporters in government strongholds and vice versa in opposition strongholds.

4.3 Robustness tests

We conduct various robustness tests. In Online Appendix Table B.3.1, we use interaction terms in the full sample of government and opposition villages (instead of relying on sample splits) and find that the treatment effects are almost identical. In Online Appendix Table B.3.2, we measure turnout based on self-reported voting participation (rather than ink marks) and again find similar effects.

We next narrow our samples to villages and respondents who are particularly likely to support one of the two main parties. In Online Appendix Table B.3.3, we first restrict the sample to villages from polling stations where the same party won both the 2001 and the 2008 general election and then to villages from polling stations where this party's vote share exceeded the other party's vote share by 25 pp on average. In Online Appendix Table B.3.4, we first restrict the sample of respondents from government (opposition) villages to those who attended an AL (BNP) rally prior to the 2014 or 2018 election, and then to those who expected the AL (BNP) to win the 2018 election.[29] These two tables document that our estimates remain similar when restricting the sample to villages and respondents who are particularly likely to support one of the two main parties. This finding suggests that our identified government and opposition strongholds are indeed mostly populated by supporters of the corresponding party.

In our pre-analysis plan, we also proposed two exercises based on the official election data provided by the Election Commission. First, we intended to use these data to test whether our treatments have an effect on polling station-level voter turnout. We were aware that detecting a statistically significant effect would require very large spillovers across households within villages or even within polling stations due to the small share of treated respondents per polling station.[30] Second, we planned to use the official election data to study how the treatments impact upon the polling station-level vote shares of the two main parties.[31] However, as discussed in Section 2.2, there is substantial anecdotal evidence of fake votes and ballot

[29]The rally-based sample restriction may be conceptually more convincing than the expectation-based sample restriction, but it is also much more restrictive. In particular, there are very few respondents who attended a BNP rally, which is not surprising given the BNP's boycott of the 2014 general election and the harassment of BNP candidates and supporters prior to both the 2014 and 2018 general elections. The treatment effects in this very small (and selected) sample are large in absolute values but not statistically significant.

[30]Remember that most polling stations cover multiple villages and that we select 40 respondents from at most one village per polling station.

[31]Remember that we cannot investigate the treatment effects on individual voting choices because it was deemed too sensitive to ask our respondents about their party preference or voting choice.

stuffing, making the official election data untrustworthy and unreliable. For completeness, we nevertheless present the results of these two exercises in Online Appendix B.4.1. While we find no significant effect of the treatments on voter turnout or party vote shares in government villages, we find some evidence that the policy treatment increases the vote share of the BNP at the expense of the AL in opposition villages. These effects become smaller in absolute values and are no longer statistically significant once we add constituency-fixed effects. We are hesitant to draw any (strong) conclusions from these exercises, given the untrustworthiness and unreliability of the official election data.

5 Understanding the backlashes in opposition villages

The most striking finding of our experiment is that both treatments reduce voter turnout in opposition villages. While we expected this reduction for the legitimacy treatment, we did not expect it for the policy treatment. In this section, we present a theoretical framework and additional empirical evidence to explain the backlashes in voting participation observed in treated opposition villages.

5.1 Same or different treatments?

Before presenting our explanations, we want to make sure that the two treatments were indeed perceived and processed differently by the respondents (as we intended) and, therefore, reduced voter turnout via different channels. For this purpose, we investigate in Table 3 how our treatments shape the respondents' views on the role of MPs in policy making, the value of democracy, and the fairness of the elections. More specifically, we focus on the respondents' view as to whether MPs are actually responsible for local public goods provision and for generating local income earning opportunities in columns (1) and (2), their views on the desirability of having multiple parties and holding elections in columns (3) and (4), and their view as to whether the election was free and fair in column (5). These dependent variables are based on their views expressed in our post-election surveys. To focus on treatment-induced changes, we control for the respondents' views expressed in our pre-treatment surveys.

Table 3 around here.

We find that the policy treatment has strong and statistically significant effects on the respondents' views about the role of MPs in both government and opposition villages (but with opposite signs across the two village types), but no effect on their views about the importance of having multiple parties and holding elections. It is reassuring that the policy treatment is indeed perceived as relating to policy issues rather than fundamental questions about the value of democracy. The legitimacy treatment, in contrast, has strong and statistically significant effects on the respondents' view as to the importance of having multiple parties in government villages and the respondents' view as to the fairness of the election in opposition villages.[32] It is reassuring that the legitimacy treatment triggers thoughts about the political process and the value of democracy.

This analysis suggests that the two treatments were perceived and processed differently by the respondents in both government and opposition villages, suggesting that they reduced voter turnout via different channels. We next introduce a theoretical framework and then discuss the effects of each treatment separately (thereby referring to the results presented in Table 3).

5.2 Theoretical framework

To structure our discussion, we use a simple theoretical framework inspired by Fiorina (1976) in which rational individuals have both instrumental and expressive voting motives.[33] An individual's instrumental motive is driven by the expected benefit from getting the party with the more attractive (economic) policy into power. In contrast, their expressive motive is "more directly and immediately linked to the act of voting, or of voting for a particular candidate or option, itself" (Hamlin and Jennings, 2011, p. 645). For example, "[o]ne may vote to express solidarity with one's class or peer group, to affirm a psychic allegiance to a party, or simply to enjoy the satisfaction of having performed one's civic duty" (Fiorina, 1976, p. 393).

Think about an individual i, who can vote for either government party A (think AL) or opposition party B (think BNP) or abstain. This individual gets an instrumental benefit D_i from getting party A rather than party B into office, an expressive benefit E_i from voting for

[32] One may also have expected a positive effect of the legitimacy treatment on views about the importance of having multiple parties and holding elections in opposition villages. We do not find such effects, possibly because 98.3 and 95.0 percent of the respondents in control opposition villages view multiple parties and elections as desirable, leaving little room for any treatment to substantially increase these shares.

[33] Brennan and Hamlin (1998) also argue in favor of integrating instrumental and expressive voting motives to obtain a more general account of rational voting behavior.

party A rather than party B, an expressive benefit F_i from the act of voting itself, and incurs voting costs C_i unless she abstains. When neglecting the option to abstain, individual i would vote for party A if $D_i + E_i > 0$ and for party B otherwise. Therefore, we call individual i a government supporter if $D_i + E_i > 0$, and an opposition supporter if $D_i + E_i < 0$. However, our focus is on the decision of individuals to abstain or participate in the vote. Individual i participates if and only if

$$|D_i + E_i| + F_i > C_i. \tag{1}$$

Below we use this framework to understand why the two treatments cause opposition supporters to withdraw from voting. However, let us first illustrate this framework by looking at how the treatments impact the voting calculus of government supporters. It seems reasonable to assume that the policy treatment, which focuses on the effect of election outcomes on policy outcomes, mainly increases the governments supporters' instrumental benefit from voting for the government party D_i, while the legitimacy treatment, which focuses on the legitimizing effect of high turnout, mainly increases their expressive voting benefit F_i (or E_i).[34] Given that $D_i + E_i > 0$ holds for government supporters even prior to any treatment, it follows that both treatments increase $|D_i + E_i| + F_i$, such that more government supporters find it worthwhile to incur voting costs C_i. Therefore, this framework predicts that the two treatments both increase turnout among government supporters, which is consistent with our expectations specified in the pre-analysis plan as well as our empirical findings.

5.3 Legitimacy treatment and the backlash in opposition villages

As specified in our pre-analysis plan, we expected the negative effect of the legitimacy treatment on voter turnout in opposition villages. Croke et al. (2016) argue that opposition supporters may deliberately disengage and abstain from voting in order to de-legitimize the regime in (com-

[34]These assumptions are consistent with our more detailed discussion of the effects of our treatments on the instrumental and expressive voting benefits of opposition supporters in Sections 5.3 and 5.4. The assumption that the policy treatment increases the government supporters' instrumental benefit from voting is also consistent with the positive effect of this treatment on the respondents' views that local MPs can provide local public goods and generate local income earning opportunities in government villages (see Table 3, panel A, columns (1) and (2)). Moreover, we have seen that the legitimacy treatment fosters anti-democratic views among respondents from government villages who start questioning the usefulness of having multiple parties (see Table 3, panel A, column (3)). Possible explanations are that government supporters who get the legitimacy treatment may find it unacceptable that high turnout is seen as necessary for their party to be considered the legitimate winner or may think that their party would have a high legitimacy on its own if elections were uncontested.

petitive) authoritarian elections. It seems reasonable to assume that the legitimacy treatment makes this option more salient. Therefore, within our theoretical framework, the most plausible change in the voting calculus of opposition supporters exposed to the legitimacy treatment is a decrease in their expressive benefit F_i from the act of voting itself, which could well become negative. As a result, fewer opposition supporters find it worthwhile to incur voting costs C_i, leading to lower turnout in opposition villages.

We offer an indirect test for this explanation. Croke et al. (2016) argue that deliberate abstention is a fairly elaborate strategy that requires "critical capacities, political awareness" (p. 579) and, therefore, good education. Hence, if the legitimacy treatment indeed reduced turnout in opposition villages because some treated respondents deliberately abstained in order to de-legitimize the regime, we would expect more pronounced effects for respondents with better education, better political information (proxied by access to radio and TV), better political knowledge and more first-hand political experience.

We investigate heterogeneity in treatment effects along these dimensions in Table 4, thereby using various measures collected in our pre-treatment surveys.

Table 4 around here.

The legitimacy treatment's negative effect on voter turnout in opposition villages (panel B) indeed tends to be more pronounced for respondents who have more years of schooling (column (1)), have access to a TV (column (3)), have basic knowledge about the main parties and their local candidates (column (4)), have been politically active in the year prior to the election (column (5)), have participated in political rallies in the run-up to the election (column (6)), and expected the election not to be free and fair when asked two to three weeks prior to election day (column (7)). Although not all interaction terms are statistically significant at conventional levels, the resulting pattern aligns with the arguments that some treated respondents in opposition villages deliberately abstain from voting to de-legitimize the regime and that those with more critical capacities and political awareness are more likely to do so.[35]

[35]Table 4 reveals no such effect heterogeneity for the policy treatment in opposition villages, except that the effect of the policy treatment is more negative for respondents with access to a TV. Table 4 also reveals no such effect heterogeneity for either of the two treatments in government villages (again with the exception of access to a TV). Online Appendix Table B.5.1 tests for effect heterogeneity along various other dimensions. The most notable finding – contributing to the recent literature on information campaigns aimed at raising female voting

We have seen, moreover, that the legitimacy treatment lowered the share of respondents from opposition villages who perceived the election as free and fair (see Table 3, panel B, column (5)). This result suggests that the legitimacy treatment causes opposition supporters to think about the electoral process and to conclude that this process is unfair. Like the strategy of deliberate abstention to de-legitimize the regime, the insight that the electoral process is unfair also reduces the opposition supporters' expressive benefit from voting F_i, prompting some of them to withdraw from voting.

5.4 Policy treatment and the backlash in opposition villages

While we expected the policy treatment to increase voter turnout in opposition villages (as specified in the pre-analysis plan), we have found that it actually lowers turnout in these villages. This discrepancy demands an explanation. Building on our framework, we argue that the policy treatment raises the tension between the opposition supporters' instrumental and expressive voting motives and that some opposition supporters withdraw from voting in response to this tension.

This argument consists of three steps of reasoning. First, the policy treatment raises the salience of the notion that local voting outcomes shape local economic policy outcomes. Hence, it should mainly impact the voting calculus by changing instrumental voting benefits.

Second, it is important to understand how the policy treatment changes the opposition supporters' instrumental benefits from voting for the government or the opposition party. In a well-functioning democracy with a well-intentioned government, opposition supporters may benefit from voting for an opposition party because this party could win or because voting for this party allows them to signal their policy preferences to a government that may take these preferences into account.[36] In the 2018 Bangladeshi general election, however, the opposition party was very unlikely to win, given the tilted playing field, and the winning party was unlikely to take opposing views into account. After all, Bangladeshi politics is clientelistic and polarized, and parties resemble pyramidal structures of patron-client relations (see Section 2.1). Therefore, individuals who are clients of a patron linked to the opposition party, which is likely to lose,

participation (e.g., Giné and Mansuri, 2018; Chong et al., 2019) – is that women are more likely than men to withdraw from voting in response to the two treatments in both village types.

[36] In such a setting, the policy treatment may lead to a decrease in D_i and, consequently, higher turnout in opposition villages (as specified in the pre-analysis plan).

cannot expect to benefit from local public goods or local government jobs. If anything, they could hope to be rewarded for switching their alliance and voting for the government party. However, these rewards may be limited by the fact that opposition supporters are not yet part of a government party-based patron-client relation. In sum, the policy treatment should lead to a modest increase in the opposition supporters' instrumental benefits D_i from voting for the government party rather than the opposition party.

Third, we must understand how the increase in the instrumental benefits D_i resulting from the policy treatment shapes the opposition supporters' decision to participate or abstain. Remember that – within our theoretical framework – opposition supporters are characterized by $D_i + E_i < 0$ and participate in the election if and only if $|D_i + E_i| + F_i > C_i$. The first inequality implies that they would rather vote for the opposition party than the government party in the absence of any treatment. The increase in D_i resulting from the policy treatment obviously leads to an increase in $D_i + E_i$, making opposition supporters less keen on voting for the opposition. However, they may not want to vote for the government party either. After all, given the polarized nature of Bangladeshi politics, resulting in "two tribes" (Islam, 2013), we expect the expressive benefits from voting for one's "own" party to be large and, therefore, E_i to be negative and large in absolute value. Hence, the policy treatment may well result in a decrease of $|D_i + E_i|$ for opposition supporters, such that some of them find it no longer worthwhile to incur voting costs C_i.[37] Hence, our theoretical framework predicts that the policy treatment reduces voter turnout in opposition villages, which is indeed what we find.

The essence of this reasoning should be intuitive. After all, Fiorina (1976) already discusses that abstention may be the rational choice of individuals who feel "cross-pressured" because expressive and instrumental voting motives push in opposite directions. Moreover, there are also psychological reasons for why the policy treatment may reduce voter turnout in opposition villages – despite increasing the instrumental benefit from voting for the government party. According to cognitive dissonance theory, individuals experience discomfort when new information or insights clash with their attitudes or beliefs (Festinger, 1957). This discomfort can

[37]Formally, denote by D_i^0 an opposition supporter i's instrumental voting benefit in the absence of any treatment and by $\Delta_i > 0$ the increase thereof in response to the policy treatment. The policy treatment makes opposition supporter i more likely to abstain if and only if $|D_i^0 + E_i| > |D_i^0 + \Delta_i + E_i|$. Given $D_i^0 + E_i < 0$ (as i is an opposition supporter) and $\Delta_i > 0$, this condition holds if and only if $-(D_i^0 + E_i) > D_i^0 + \Delta_i + E_i$ or, equivalently $\Delta_i < -2(D_i^0 + E_i)$. While $\Delta_i \geq -2(D_i^0 + E_i)$ is a theoretical possibility, we consider it to be unlikely in our context, in which the political polarization and tribalization implies that the opposition supporters' expressive voting benefits E_i are negative and large in absolute value.

lead them to avoid behavior that exacerbates the cognitive dissonance. Stevens (2007) therefore argues that it is psychologically less difficult to abstain than to switch from the one party to another.

Table 3 provides indirect evidence supporting our explanation and the three steps of reasoning. First, the finding that the policy treatment shapes the respondents' views about the actual role of MPs in providing local public goods and generating local income earning opportunities suggests that this treatment indeed shapes instrumental voting motives. Second, the finding that these effects are negative and sizeable in opposition villages suggests that, once we trigger opposition supporters to think about the nexus between voting and economic policy outcomes, some of them conclude that the local MP can do relatively little for their economic well-being, especially if it were a local MP from the opposition party without access to the government. These opposition supporters may thus conclude that instrumental voting benefits are basically zero when voting for the opposition party, but possibly positive when voting for the government party. Third, many opposition supporters may thus revise their calculus of voting and decide to abstain (as expressive voting motives or psychological reasons prevent them from switching to the government party). Given that the policy treatment reduces the share of respondents who think that MPs can actually provide local public goods and generate local income earning opportunities by around 17 pp in opposition villages, it is possible that these changing views are responsible for the 10 pp reduction in voter turnout in response to the policy treatment in these villages.

6 Conclusions

We have conducted a large-scale field experiment during the 2018 Bangladeshi general election to study how neutrally framed information and GOTV campaigns impact the partisan composition of the voting population in competitive authoritarian elections. Both of our treatments (or campaigns) increased voter turnout in government strongholds but reduced turnout in opposition strongholds. In so doing, they tilted the already uneven playing field even further to the incumbent government's benefit.

We have provided an explanation for the (perhaps surprising) backlashes that the treatments caused in opposition strongholds. The legitimacy treatment may have led to a backlash because

it made educated and politically experienced opposition supporters withdraw from voting in order to de-legitimize the regime. The policy treatment may have caused a backlash because it made opposition supporters realize that instrumental voting benefits are higher from voting for the government party, given the uneven playing field and the clientelistic nature of Bangladeshi politics. However, these voters get a much higher expressive benefit from voting for their party given the polarized nature of Bangladeshi politics. As a result of this cross-pressure, many opposition supporters exposed to the policy treatment decided to abstain.

No two competitive authoritarian elections are alike. We nevertheless think that the Bangladeshi 2018 general election was a fairly typical competitive authoritarian election, as similar tactics are used to create an uneven playing field in many countries around the globe. Moreover, many of these countries also feature clientelistic and polarized politics. We therefore expect non-partisan information and GOTV campaigns to have similar partisan effects on the composition of the voting population in many other competitive authoritarian elections. Whether this is true is ultimately an empirical question that needs to be addressed in future research conducted during competitive authoritarian elections. Moreover, future research could also study whether the novel legitimacy treatment also has partisan effects in countries with (reasonably) free and fair elections, but very polarized polities.

Our findings from one of the world's most populous countries with competitive authoritarian elections serve as a warning: Western donors and NGOs should be very careful when resorting to information and GOTV campaigns to support democracy and promote voting participation. Otherwise, their campaigns can backfire and benefit the incumbent government that is already creating an uneven playing field.

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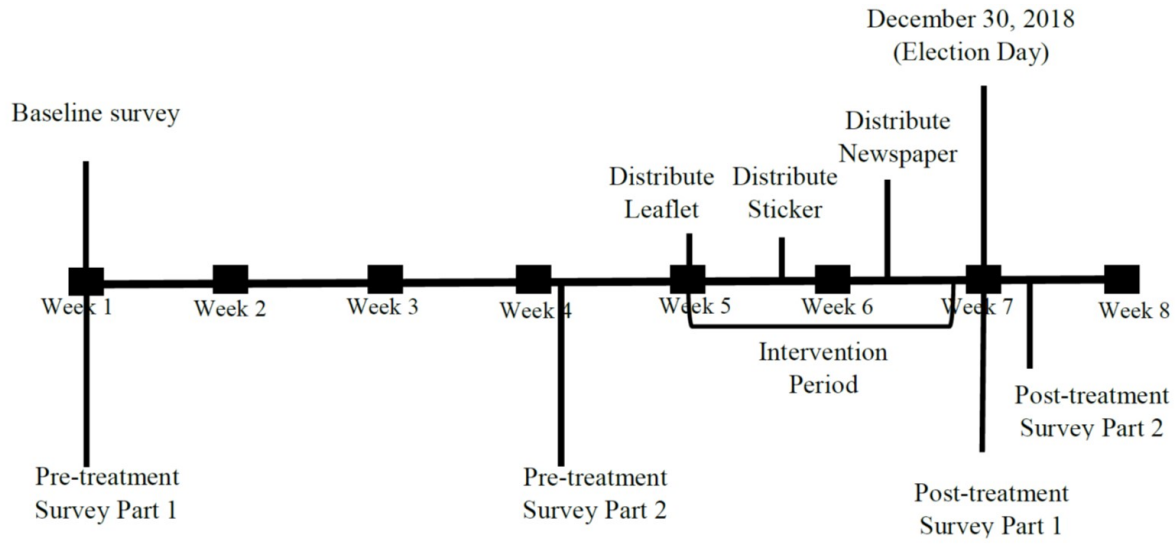
Figures and Tables

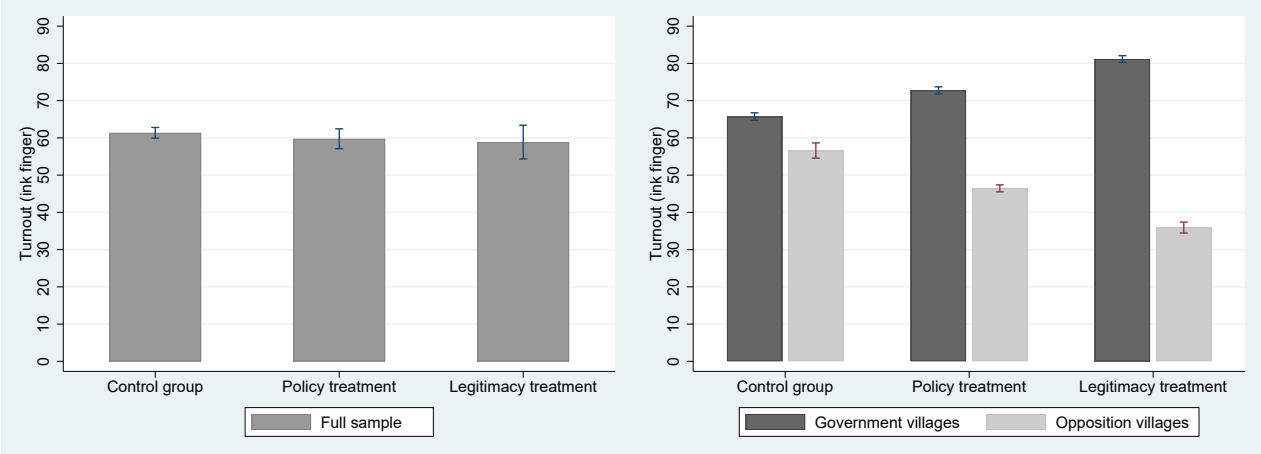
Figure 1: Timeline



Notes: Timeline shows election day, the timing of our four surveys, and the timing of our three interventions.

Figure 2: Voter turnout across treatment groups and village types

(a) Full sample (b) Government vs opposition villages



Notes: Panel (a) shows the average voter turnout for the control group, the policy treatment group, and the legitimacy treatment group in the full sample including both government and opposition villages. Panel (b) shows the average voter turnout for the control and treatment groups separately for government and opposition villages. Voter turnout is the share of respondents with an ink mark on their finger (in %). Vertical lines show 95% confidence intervals.

Table 1: Balance tests at the level of individuals and households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	Female	Age	Schooling	Farmer	Laborer	Business	Profes.	Voters	Income
Panel A: AL/government villages									
Constant/control mean	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)
RI p-values (policy)	0.35	0.17	0.46	0.91	0.99	0.80	0.67	0.27	0.67
RI p-values (legitimacy)	0.07	0.68	0.91	0.60	0.76	0.81	0.86	0.15	0.54
F-test p-values	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/opposition villages									
Constant/control mean	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)
RI p-values (policy)	0.28	0.84	0.38	0.19	0.22	0.15	0.23	0.04	0.67
RI p-values (legitimacy)	0.40	0.84	0.42	0.76	0.74	0.41	0.35	0.03	0.34
F-test p-values	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 4.1 and Online Appendix A.6. OLS regressions without control variables or fixed effects. Standard errors (in parentheses) are clustered at the village level. ***/**/* indicate statistical significance at the 1%/5%/10%-level. RI p-values are the Young (2019) randomization inference based p-values (with 1,000 replications) for each treatment variable. F-test p-values are for the F-tests that both treatment variables are jointly zero.

Table 2: Main results

	(1)	(2)	(3)	(4)
Dependent variable:	Ink mark respondent		Ink mark spouse	
Panel A: AL/government villages				
Policy treatment	7.0***	7.1***	6.0***	6.1***
	(0.7)	(0.7)	(0.8)	(0.8)
Legitimacy treatment	15.5***	15.4***	12.9***	13.1***
	(0.7)	(0.7)	(0.7)	(0.7)
RI p-values (policy)	0.00	0.00	0.00	0.00
RI p-values (legitimacy)	0.00	0.00	0.00	0.00
R-squared	0.02	0.03	0.01	0.02
Observations	6065	6065	6065	6065
Mean dep. var. (control)	65.7	65.7	64.5	64.5
Panel B: BNP/opposition villages				
Policy treatment	-10.2***	-10.3***	-9.2***	-9.3***
	(1.1)	(1.2)	(1.0)	(1.0)
Legitimacy treatment	-20.7***	-20.7***	-18.1***	-18.2***
	(1.3)	(1.3)	(1.0)	(1.1)
RI p-values (policy)	0.00	0.00	0.00	0.00
RI p-values (legitimacy)	0.00	0.00	0.00	0.00
R-squared	0.03	0.03	0.02	0.03
Observations	5778	5778	5778	5778
Mean dep. var. (control)	56.6	56.6	52.3	52.3
Individual controls	no	yes	no	yes
Village controls	no	yes	no	yes
Constituency-fixed effects	no	yes	no	yes

Notes: Dependent variables are binary variables equal to 100 in case of an ink mark on the finger and zero otherwise, with the individuals under consideration being the respondents in columns (1) and (2) and their spouses in columns (3) and (4). Linear regressions without control variables or fixed effects in columns (1) and (3), but with individual-, household- and village-level controls and constituency-fixed effects in columns (2) and (4). Individual-level controls are gender, age and schooling of the respondent and their 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 shares of the four different occupational categories, the number of voters, indicators for the presence of a polling station and primary/secondary/higher schools, and distance to the sub-district capital and closest bus stop. Standard errors (in parentheses) are clustered at the village level. ***/**/* indicate statistical significance at the 1%/5%/10%-level. RI p-values are the Young (2019) randomization inference based p-values (with 1,000 replications). The last row in each panel shows the mean of the dependent variable in the control group.

Table 3: Treatment effects on the respondents' views on policy making, democracy and elections

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	MPs for local public goods	MPs for local incomes	Pro multiple parties	Pro elections	Free & fair elections
Panel A: AL/government villages					
Policy treatment	17.3***	17.3***	0.7	7.1	5.3
	(3.3)	(4.2)	(2.4)	(4.0)	(6.9)
Legitimacy treatment	5.5**	10.0*	-11.2***	-10.1*	8.3
	(2.1)	(4.9)	(3.3)	(4.3)	(7.0)
Pre-treatment view	0.6***	0.3***	0.5***	0.3***	0.3***
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
RI p-values (policy)	0.00	0.00	0.77	0.09	0.44
RI p-values (legitimacy)	0.01	0.05	0.00	0.02	0.24
R-squared	0.46	0.13	0.23	0.12	0.09
Observations	6065	6065	6065	6065	6012
Mean pre-treatment view	64.4	55.9	84.3	77.9	42.1
Mean dep. var. (control)	65.9	57.7	79.4	71.7	58.8
Panel B: BNP/opposition villages					
Policy treatment	-17.4***	-16.7***	-1.4	-2.4	-11.4*
	(4.5)	(4.6)	(0.9)	(1.5)	(4.6)
Legitimacy treatment	-8.6*	-6.1	0.4	-1.2	-12.6**
	(4.3)	(5.0)	(0.6)	(1.4)	(4.6)
Pre-treatment view	0.5***	0.5***	0.1***	0.1***	0.1**
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
RI p-values (policy)	0.00	0.00	0.13	0.11	0.01
RI p-values (legitimacy)	0.05	0.22	0.48	0.40	0.00
R-squared	0.22	0.30	0.07	0.03	0.04
Observations	5778	5778	5778	5778	5751
Mean pre-treatment view	64.6	56.0	92.6	88.4	30.8
Mean dep. var. (control)	61.9	52.7	98.3	95.0	23.9

Notes: Dependent variables are indicated in the top row and based on the respondents' post-election views. They are equal to 100 if the respondents state that MPs are actually responsible for arranging funding for local public goods in column (1), if they state that MPs are actually responsible for generating local income earning opportunities in column (2), if they disagree with the statement that only one political party should be allowed to stand for election and hold office in column (3), if they disagree with the statement that there should be no election and that the prime minister should decide everything in column (4), if they perceived the election as being free and fair (column 5); and zero otherwise. In each column, "Pre-treatment view" is based on the same question and coding as the dependent variable, but taken from a pre-treatment survey. See Online Appendix A.6 for more information about these variables. OLS regressions without control variables or fixed effects, as in column (1) of Table 2. Standard errors (in parentheses) are clustered at the village level. ***/**/* indicate statistical significance at the 1%/5%/10%-level. RI p-values are the Young (2019) randomization inference based p-values (with 1,000 replications). The last two rows in each panel show the mean of the pre-treatment views in the given village type and the mean of the dependent variables in the control group of this village type.

Table 4: Heterogeneity in treatment effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Z variable:	Schooling	Radio	TV	Political knowledge	Political activities	Political rallies	Free & fair elections
Panel A: AL/government villages							
Z	0.8	0.6	-0.0	1.4	4.4	-1.2	1.2
	(0.5)	(1.9)	(1.7)	(2.1)	(2.5)	(1.8)	(1.7)
Policy treatment	7.0***	7.6***	4.5**	4.8*	7.7***	6.9***	8.1***
	(0.7)	(1.0)	(1.5)	(2.1)	(0.8)	(0.9)	(1.2)
Policy treatment × Z	-0.3	-2.1	3.8	3.2	-5.1	0.8	-2.4
	(0.7)	(2.4)	(2.3)	(2.8)	(3.7)	(2.7)	(2.3)
Legitimacy treatment	15.5***	14.8***	12.1***	14.0***	15.6***	15.3***	15.0***
	(0.7)	(1.0)	(1.5)	(2.0)	(0.8)	(0.9)	(1.2)
Legitimacy treatment × Z	-0.3	2.2	5.4*	1.9	-1.1	1.3	1.5
	(0.6)	(2.5)	(2.3)	(2.6)	(3.5)	(2.5)	(2.2)
R-squared	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Observations	6065	6065	6065	6051	6065	6051	6051
Panel B: BNP/opposition villages							
Z	1.1	0.6	8.0***	4.0	8.2*	10.0***	0.6
	(0.7)	(2.4)	(2.0)	(2.5)	(3.6)	(2.9)	(2.4)
Policy treatment	-10.3***	-10.1***	-6.4***	-7.0**	-9.7***	-9.8***	-10.5***
	(1.2)	(1.4)	(1.8)	(2.4)	(1.4)	(1.2)	(1.8)
Policy treatment × Z	-1.1	-0.4	-6.7**	-4.4	-5.0	-7.2	0.9
	(0.9)	(2.8)	(2.4)	(3.1)	(4.5)	(4.1)	(3.3)
Legitimacy treatment	-20.9***	-20.6***	-15.9***	-19.0***	-19.2***	-19.7***	-22.1***
	(1.3)	(1.6)	(2.0)	(2.6)	(1.5)	(1.4)	(1.7)
Legitimacy treatment × Z	-1.9*	-0.2	-8.4**	-2.3	-12.4*	-11.8**	5.4
	(0.9)	(3.3)	(2.7)	(3.3)	(4.9)	(3.8)	(3.3)
R-squared	0.03	0.03	0.03	0.03	0.03	0.03	0.03
Observations	5778	5778	5778	5771	5778	5771	5771

Notes: Dependent variable is equal to 100 if the respondents had an ink mark on their finger, and zero otherwise. OLS regressions without control variables or fixed effects, as in column (1) of Table 2. The Z variables are based on our pre-treatment surveys. Years of schooling (centered) in column (1) and dummy variables for access to radio in column (2); access to TV in column (3); basic knowledge on the main political parties, their official symbols and their local candidates in column (4); political activities during 2018 (e.g., attending community/budget meetings, contacting local government/party officials, or participating in protests) in column (5); participation in political rallies in the 2018 election campaign until two to three weeks prior to election day in column (6); and expectation of free and fair elections two to three weeks prior to election day in column (7). See Online Appendix A.6 for more information about these variables. Standard errors (in parentheses) are clustered at the village level. ***/**/* indicate statistical significance at the 1%/5%/10%-level.

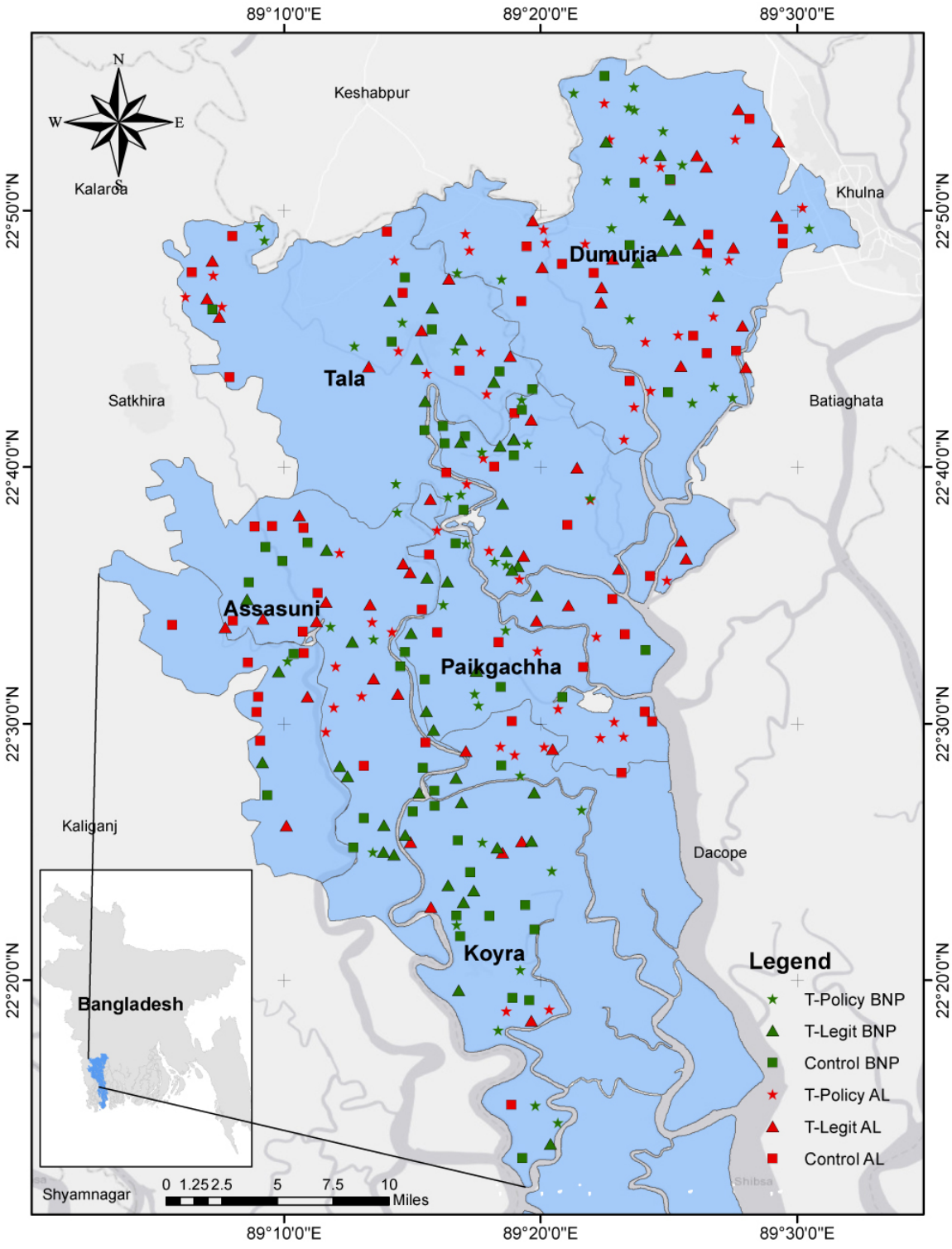
Online Appendix:
Partisan effects of information campaigns in competitive authoritarian elections: Evidence from Bangladesh

Content:

A. Additional information on the field experiment

A.1 Map of study area

Figure A.1.1: Map showing all 302 villages in our sample



A.2 Representativeness of study area and our sample

Table A.2.1: Representativeness of study area and our sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	HIES data for entire country			HIES data for Khulna Division			Our survey data		
Variable	Obs.	Mean	SD	Obs.	Mean	SD	Obs.	Mean	SD
Age men	18,787	37.84	8.88	3,196	38.35	8.86	11,843	39.99	8.53
Age women	22,791	32.87	8.63	3,961	33.45	8.59	11,843	33.11	7.83
Schooling man	18,787	7.68	3.83	3,196	7.56	3.71	11,843	9.15	2.13
Schooling woman	22,791	7.16	3.06	3,961	7.11	2.97	11,843	8.86	1.73
Household size	46,068	4.04	1.56	7,200	3.74	1.36	11,843	4.31	1.37
Agriculture	46,068	0.33	0.47	7,200	0.38	0.49	11,843	0.44	0.50
Income	46,068	10,738	11,862	7,200	9,757	10,109	11,843	8,273	7,053

Notes: Summary statistics are based on the Bangladesh Household Income and Expenditure Survey (HIES) 2016 in columns (1)–(3), the HIES 2016 entries from Khulna Division in columns (4)–(6), and our own survey data in columns (7)–(9). The samples are restricted to individuals who are aged 20–55 years and literate (can read and write). Age and schooling of the men correspond to age and schooling of head of households in HIES data (in case the household head is a female, we took the age and schooling of her spouse). Likewise, age and schooling of women correspond to age and schooling of spouses of household heads in HIES data (in case the spouses of household heads is a male, we took the age and schooling of the household head). Age and years of schooling are measured in years. Agriculture is a binary variable equal to one if the primary occupation of the household's main earner is in agriculture. Income is monthly household income (in Bangladeshi Taka).

A.3 Additional support for our village classification

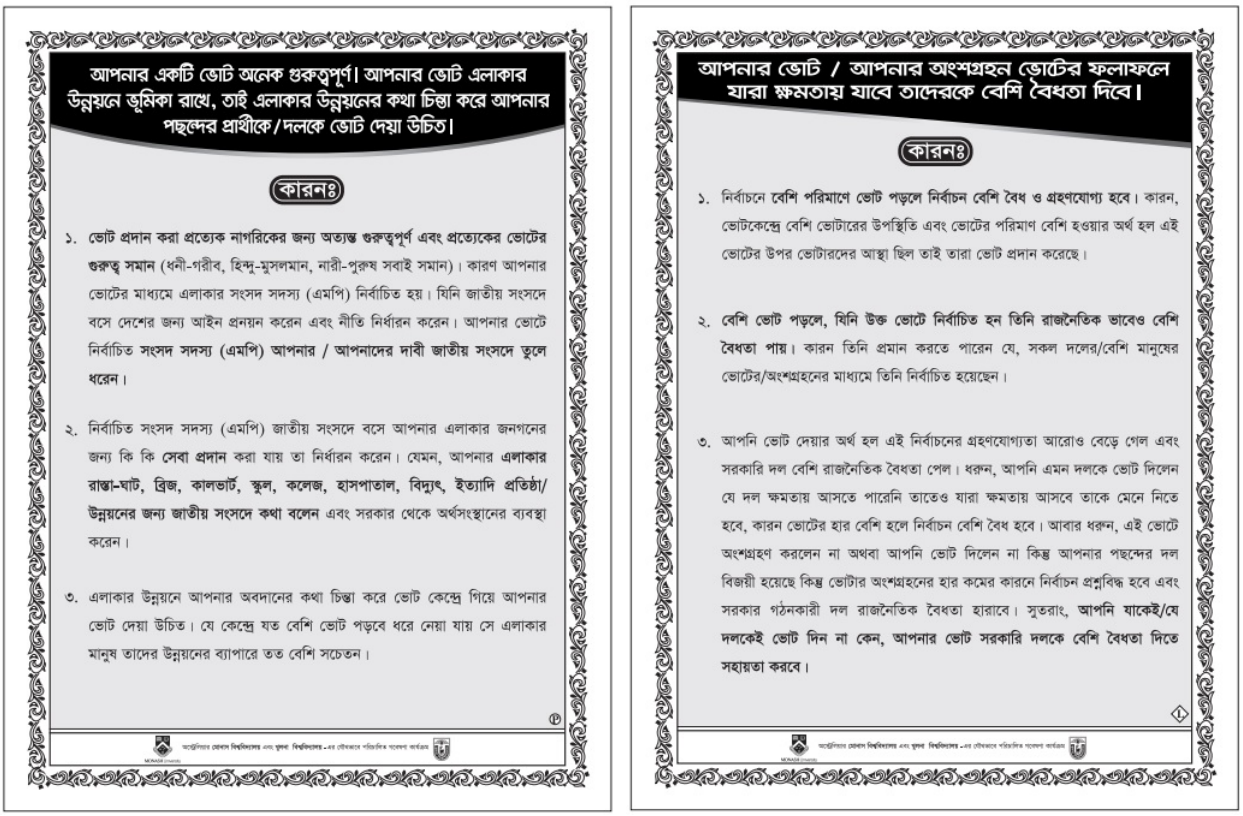
Table A.3.1: Village classification and the official AL/BNP vote gap in the 2018 election

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Official AL/BNP vote gap in 2018					
Sample:	Full sample		Control villages		Out of sample	
Government village/polling station	6.3***	7.8***	1.0	2.8	9.4**	9.4**
	(1.8)	(1.8)	(2.9)	(3.2)	(2.9)	(3.1)
R-squared	0.04	0.12	-0.01	0.04	0.05	0.19
Observations	302	302	100	100	184	184
Constituency-fixed effects	no	yes	no	yes	no	yes

Note: Dependent variable is the difference between the AL and the BNP vote shares (in p.p.) in the official polling station-level data from the 2018 general election provided by the Election Commission at http://www.ecs.gov.bd/page/parliament-election-result. Explanatory variable is a dummy variable for whether the corresponding village is coded as a government village. (Remember that we include at most one village per polling station.) The sample corresponds to the full sample of all 302 villages/polling stations in columns (1) and (2), the set of all 100 control villages in columns (3) and (4), and an alternative sample of 184 "out of sample" polling stations in columns (5) and (6). For this alternative sample, we collected official polling station-level election results for 2001, 2008 and 2018 from four electoral constituencies next to our study area: Khulna 1, Khulna 2, Khulna 3, and Khulna 4. We have full voting records for 250 of these polling stations. We classify them as government (opposition) polling stations if the AL (BNP) got more votes than the BNP (AL) in both the 2001 and the 2008 election. Thereby, we get 87 (97) government (opposition) polling stations and drop the remaining 66 polling stations. Linear regressions without control variables or fixed effects in odd columns, but with constituency-fixed effects in even columns. Robust standard errors in parenthesis. ***/**/* indicate statistical significance at the 1%/5%/10%-level.

A.4 Treatment messages

Figure A.4.1: Leaflets used for treatment delivery



Notes: Left (right) picture shows the leaflet for the policy (legitimacy) treatment in Bengali. See below for translated versions.

Figure A.4.2: Stickers used for treatment delivery

Notes: Left (right) picture shows the stickers for the policy (legitimacy) treatment in Bengali. The stickers summarize and illustrate the text on the leaflets. See below for translated versions.

Figure A.4.3: Newspaper advertorials used for treatment delivery

Notes: Left (right) picture shows the newspaper front page with the advertorial for the policy (legitimacy) treatment in Bengali inside the red boxes on the bottom right. The advertorials summarize the text on 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."

Translation of sticker for policy treatment: In speech bubbles: "Your participation in the election accelerates the development of your area." "Elect MP's who will speak for you/your people." "He/she will raise your demands in parliament." Below pictures: "Your vote plays an important role in the development of your region, so considering the importance of your vote for the development of your region, you should vote for your favorite party/person."

Translation of sticker for legitimacy treatment: In sspeech bubbles: "Your vote gives validation to the election." "Whomever you vote for, your vote will provide more legitimacy to the winning party/government." Below pictures: "Your vote/participation in election will give more legitimacy to the party forming the government."

A.5 Attrition

Table A.5.1: Attrition

Village type	(1) Respondents answering S1	(2) Missing in S2/S3/S4	(3) Respondents answering all surveys	(4) Total rate of attrition	(5) Respondents answering S1 and S3	(6) Rate of attrition (S1 to S3)
Government/control	2,059	8/14/41	2,011	2.3	2,045	0.7
Government/legitimacy	2,029	12/16/15	1,999	1.5	2,013	0.8
Government/policy	2,052	27/45/12	2,002	2.4	2,007	2.2
Opposition/control	1,881	12/17/17	1,856	1.3	1,864	0.9
Opposition/legitimacy	1,967	6/10/8	1,947	1.0	1,957	0.5
Opposition/policy	1,973	8/16/16	1,948	1.3	1,957	0.8
Total	11,961	72/118/109	11,763	1.7	11,843	1.0

Notes: Surveys S1 and S2 are the first and second pre-treatment surveys; surveys S3 and S4 the first and second post-election surveys. The total rate of attrition in column (4) refers to the percentage share of respondents who answered S1 but missed one or more of the subsequent surveys S2–S4. Our main sample consists of the respondents who answered S1 and S3 (see column (5)). Therefore, we also report the rate of attrition from S1 to S3 in column (6).

A.6 Variable description and summary statistics

Variables based on first pre-treatment survey:

- "Age" is the respondent's age.
- "Female" is an indicator variable for whether the respondent is female.
- "Schooling" is the respondent's years of schooling.
- "Farmer/Laborer/Business owner/Professional" are indicator variables for whether the household head has the respective occupation.
- "Voters per household (hh)."
- "Income" is the annual household income in 1,000 Bangladeshi Taka.
- "Access to radio/TV" are indicator variables for whether the respondent has access to a radio/TV.
- "MPs for (local) public goods" is an indicator variable for whether the respondent states that MPs are actually responsible to arrange funding for local public goods (roads, bridges, schools, colleges, hospitals, etc.).
- "MPs for (local) income" is an indicator variable for whether the respondent states that MPs are actually responsible to generate local income earning opportunities.
- "Pro multiple parties" is an indicator variable for whether the respondent disagrees with the statement that only one political party should be allowed to stand for election and hold office.
- "Pro elections" is an indicator variable for whether the respondent disagrees with the statement that there should be no election and that the prime minister should decide everything.
- "Political activities" is an indicator variable for whether the respondent has attended community/budget meetings, contacted local government/party officials, or participated in protests during 2018, and zero otherwise.

Variables based on second pre-treatment survey:

- "Political knowledge" is an indicator variable for whether the respondent knows the main political parties, their official symbols and their local candidates two to three weeks prior to election day.
- "Political rallies" is an indicator variable for whether the respondent has attended political rallies in the 2018 election campaign until two to three weeks prior to the election.
- "Free & fair elections" is an indicator variable for whether the respondent expects the election to be free and fair two to three weeks prior to election day.
- "AL will win/BNP will win" are indicator variables for whether the respondent expects the local AL/BNP candidate to win two to three weeks prior to election day.

Variables based on first post-election survey:

- "Ink mark respondent" is an indicator variable for whether the respondent has an ink mark on their finger.
- "Ink mark spouse" is an indicator variable for whether the respondent's spouse has an ink mark on their finger.
- "Self-reported voting respondent" is an indicator variable for whether the respondent reports to have voted (before asked to show their ink mark).
- "Self-reported voting spouse" is an indicator variable for whether the respondent's spouse reports to have voted (before asked to show their ink mark).
- "MPs for (local) public goods/MPs for (local) income/Pro multiple parties/Pro elections:" These variables' post-election versions are based on the same questions and coding as their pre-treatment versions described above.

Variables based on second post-election survey:

- "Free & fair elections" is an indicator variable for whether the respondent perceived the election as completely free and fair when asked after the election.

Village characteristics:

- "Voters per village."
- "Polling station" is an indicator variable for whether the village hosts the polling station.
- "Distance capital/bus" are the distances to the sub-district (*upazila*) capital and the nearest bus station (in km).
- "Primary/second/higher school" are indicator variables for whether these types of schools are present in the village.
- "AL/BNP vote share 2001/2008" are based on the official polling station-level election results from the reasonably free and fair general elections in 2001 and 2008.

Table A.6.1: Summary statistics for government and opposition villages

	(1)	(2)	(3)	(4)	(5)	(6)
	AL/government villages			**BNP/opposition villages**		
Variable	Obs.	Mean	SD	Obs.	Mean	SD
Panel A: First pre-treatment survey						
Age	6,065	36.7	8.9	5,778	36.7	8.8
Female	6,065	49.5	50.0	5,778	49.2	50.0
Schooling	6,065	9.2	1.9	5,778	9.1	1.8
Farmer	6,065	49.6	50.0	5,778	38.9	48.8
Laborer	6,065	17.4	37.9	5,778	26.9	44.3
Business owner	6,065	21.5	41.1	5,778	23.5	42.4
Professional	6,065	9.1	28.7	5,778	8.4	27.8
Voters per hh	6,065	2.8	1.1	5,778	2.5	1.0
Income	6,065	100.3	91.3	5,778	98.2	77.1
Access to radio	6,065	29.5	45.6	5,778	26.7	44.3
Access to TV	6,065	60.5	48.9	5,778	57.3	49.5
MPs for local public goods	6,065	64.4	47.9	5,778	64.6	47.8
MPs for local incomes	6,065	55.9	49.7	5,778	56.0	49.6
Pro multiple parties	6,065	84.3	36.4	5,778	92.6	26.2
Pro elections	6,065	77.9	41.5	5,778	88.4	32.0
Political activities	6,065	13.0	33.6	5,778	12.7	33.3
Panel B: Second pre-treatment survey						
Political knowledge	6,051	71.0	45.4	5,771	72.3	44.7
Political rallies	6,051	18.7	39.0	5,771	10.3	30.3
Free/fair elections	6,051	42.1	49.4	5,771	30.8	46.2
AL will win	6,051	82.1	38.4	5,771	53.2	49.9
BNP will win	6,051	5.4	22.6	5,771	33.6	47.2
Panel C: First post-election survey						
Ink mark respondent	6,065	73.2	44.3	5,778	46.2	49.9
Ink mark spouse	6,065	70.8	45.5	5,778	43.0	49.5
Self-reported voting respondent	6,065	80.0	40.0	5,778	55.9	49.7
Self-reported voting spouse	6,065	77.6	41.7	5,778	58.9	49.2
MPs for local public goods	6,065	73.8	44.0	5,778	53.3	49.9
MPs for local incomes	6,065	66.7	47.1	5,778	44.7	49.7
Pro multiple parties	6,065	76.6	42.3	5,778	98.2	13.3
Pro elections	6,065	70.0	45.8	5,778	93.6	24.4
Panel D: Second post-election survey						
Free/fair elections	6,025	62.6	48.4	5,758	15.7	36.4
Panel E: Village characteristics						
Voters per village	154	1,336.8	478.0	148	1,336.0	405.1
Polling station	154	93.5	24.7	148	95.9	19.8
Distance bus	154	7.5	3.6	148	7.8	4.4
Distance capital	154	10.2	4.9	148	10.2	5.2
Primary school	154	96.1	19.4	148	93.2	25.2
Secondary school	154	53.2	50.1	148	49.3	50.2
Higher school	154	55.8	49.9	148	50.7	50.2
AL vote share 2001	154	58.7	15.3	148	29.9	10.8
BNP vote share 2001	154	38.0	15.2	148	66.1	10.4
AL vote share 2008	154	70.2	13.9	148	43.3	10.0
BNP vote share 2008	154	27.8	13.8	148	54.4	10.0

Notes: Separate summary statistics for the samples of government and opposition villages. As the assignment of villages into government and opposition villages was *not* randomized, we do *not* necessarily expect any characteristics to be balanced across these two samples.

B. Additional results

B.1 Additional balance tests

Table B.1.1: Balance tests at the level of individuals and households, with constituency-fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	Female	Age	Schooling	Farmer	Laborer	Business	Profes.	Voters	Income
Panel A: AL/government 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)
RI p-values (policy)	0.34	0.14	0.52	0.65	0.62	0.81	0.41	0.38	0.93
RI p-values (legit.)	0.06	0.75	0.74	0.43	0.43	0.82	0.91	0.26	0.52
F-test p-values	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/opposition 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)
RI p-values (policy)	0.40	0.61	0.29	0.30	0.56	0.15	0.44	0.17	0.71
RI p-values (legit.)	0.35	0.88	0.40	0.71	0.73	0.42	0.35	0.04	0.37
F-test p-values	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: Same tests as in Table 1, except that all regressions include constituency-fixed effects. See the notes to Table 1 for details.

Table B.1.2: Balance tests at the level of villages

	(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/government villages (without constituency-fixed effects)							
Constant/control mean	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)
RI p-values (policy)	0.78	0.17	0.63	0.77	0.35	0.40	0.39
RI p-values (legit.)	0.93	0.17	0.71	0.99	0.69	0.90	0.80
F-test p-values	0.95	0.31	0.71	0.94	0.59	0.64	0.49
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/government 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)
RI p-values (policy)	0.71	0.15	0.74	0.81	0.61	0.59	0.58
RI p-values (legit.)	0.93	0.17	0.69	0.99	0.80	0.99	0.67
F-test p-values	0.92	0.28	0.78	0.97	0.82	0.79	0.59
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/opposition villages (without constituency-fixed effects)							
Constant/control mean	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)
RI p-values (policy)	0.93	0.67	0.97	0.49	0.71	0.54	0.61
RI p-values (legit.)	0.76	0.38	0.50	0.63	1.00	0.38	0.44
F-test p-values	0.95	0.61	0.75	0.49	0.92	0.33	0.44
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/opposition 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)
RI p-values (policy)	0.60	0.70	0.97	0.64	0.84	0.80	0.86
RI p-values (legit.)	0.89	0.39	0.41	0.51	0.86	0.33	0.38
F-test p-values	0.77	0.61	0.64	0.52	0.95	0.45	0.55
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 4.1 and Online Appendix A.6. Linear regressions without control variables or fixed effects in panels A and C, but with constituency-fixed effects in panels B and D. Robust standard errors in parenthesis. ***/**/* indicate statistical significance at the 1%/5%/10%-level. RI p-values are the Young (2019) randomization inference based p-values (with 1,000 replications) for each treatment variable. F-test p-values are for the F-tests that both treatment variables are jointly zero.

B.2 Average effects across all villages

Table B.2.1: Average effects in full sample including both village types

	(1)	(2)	(3)	(4)
Dependent variable:	Ink mark respondent		Ink mark spouse	
Policy treatment	-1.6	-2.0	-1.7	-2.0
	(1.5)	(1.5)	(1.6)	(1.6)
Legitimacy treatment	-2.5	-2.7	-2.6	-2.7
	(2.4)	(2.4)	(2.3)	(2.3)
R-squared	0.00	0.01	0.00	0.01
Observations	11843	11843	11843	11843
Individual controls	no	yes	no	yes
Village controls	no	yes	no	yes
Constituency-fixed effects	no	yes	no	yes
Mean dep. var. (control)	61.4	61.4	58.7	58.7

Notes: Same specifications as in Table 2, except that the sample includes all respondents from both government and opposition villages. See the notes to Table 2 for details.

B.3 Robustness of main results

Table B.3.1: Robustness tests using interaction terms in full sample

	(1)	(2)	(3)	(4)
Dependent variable:	Ink mark respondent		Ink mark spouse	
BNP	-9.1***	-8.8***	-12.2***	-12.0***
	(1.2)	(1.1)	(1.0)	(1.0)
Policy treatment	7.0***	7.0***	6.0***	6.0***
	(0.7)	(0.7)	(0.8)	(0.8)
Policy treatment x BNP	-17.2***	-17.4***	-15.2***	-15.3***
	(1.3)	(1.3)	(1.3)	(1.3)
Legitimacy treatment	15.5***	15.4***	12.9***	13.1***
	(0.7)	(0.7)	(0.7)	(0.7)
Legitimacy treatment x BNP	-36.1***	-36.1***	-31.1***	-31.2***
	(1.4)	(1.4)	(1.2)	(1.2)
R-squared	0.10	0.10	0.10	0.10
Observations	11843	11843	11843	11843
Individual controls	no	yes	no	yes
Village controls	no	yes	no	yes
Constituency-fixed effects	no	yes	no	yes
Mean dep. variable (control)	61.4	61.4	58.7	58.7

Notes: Same specifications as in Table 2, except that (i) the sample includes all respondents from both government and opposition villages, (ii) an indicator variable for BNP/opposition villages is added, and (iii) interaction terms between this indicator variable and the two treatment variables are added. See the notes to Table 2 for details.

Table B.3.2: Robustness tests using self-reported voting behavior

	(1)	(2)	(3)	(4)
Dependent variable:	Self-reported voting respondent		Self-reported voting spouse	
Panel A: AL/government villages				
Policy treatment	12.6***	12.7***	9.5***	9.6***
	(0.8)	(0.8)	(0.9)	(0.9)
Legitimacy treatment	18.8***	18.8***	14.4***	14.5***
	(0.7)	(0.7)	(0.8)	(0.8)
R-squared	0.04	0.05	0.02	0.02
Observations	6065	6065	6065	6065
Mean dep. var. (control)	69.6	69.6	69.7	69.7
Panel B: BNP/opposition villages				
Policy treatment	-8.2***	-8.4***	-6.9***	-7.0***
	(1.1)	(1.2)	(1.0)	(1.0)
Legitimacy treatment	-14.8***	-14.9***	-15.8***	-15.8***
	(1.3)	(1.3)	(1.1)	(1.1)
R-squared	0.01	0.02	0.02	0.02
Observations	5778	5778	5778	5778
Mean dep. var. (control)	63.7	63.7	66.6	66.6
Individual controls	no	yes	no	yes
Village controls	no	yes	no	yes
Constituency-fixed effects	no	yes	no	yes

Notes: Same specifications as in Table 2, except that the dependent variables are based on the self-reported voting participation of the respondents and their spouses, respectively. See the notes to Table 2 for details.

Table B.3.3: Robustness tests restricting the sample to villages from polling stations with clear voting histories

	(1)	(2)	(3)	(4)
Dependent variable:	Ink mark respondent		Ink mark spouse	
Panel A: AL/government villages from PS always voting AL				
Policy treatment	6.7***	6.7***	5.8***	6.0***
	(0.9)	(0.9)	(1.1)	(1.1)
Legitimacy treatment	15.4***	15.1***	12.9***	12.9***
	(0.9)	(0.9)	(0.9)	(0.9)
R-squared	0.02	0.03	0.01	0.02
Observations	4108	4108	4108	4108
Mean dep. var. (control)	65.7	65.7	64.7	64.7
Panel B: AL/government villages from PS strongly voting AL				
Policy treatment	7.8***	7.8***	6.2***	6.7***
	(0.9)	(1.0)	(1.3)	(1.3)
Legitimacy treatment	15.4***	15.2***	12.8***	13.0***
	(0.9)	(0.9)	(0.9)	(1.0)
R-squared	0.02	0.03	0.01	0.02
Observations	3003	3003	3003	3003
Mean dep. var. (control)	65.8	65.8	64.8	64.8
Panel C: BNP/opposition villages from PS always voting BNP				
Policy treatment	-9.2***	-9.3***	-7.5***	-7.7***
	(1.6)	(1.6)	(1.3)	(1.3)
Legitimacy treatment	-19.9***	-19.8***	-17.8***	-17.9***
	(1.7)	(1.7)	(1.3)	(1.4)
R-squared	0.03	0.03	0.02	0.03
Observations	3870	3870	3870	3870
Mean dep. var. (control)	56.1	56.1	51.8	51.8
Panel D: BNP/opposition villages from PS strongly voting BNP				
Policy treatment	-8.6***	-8.7***	-6.8***	-6.9***
	(1.4)	(1.5)	(1.1)	(1.1)
Legitimacy treatment	-19.4***	-19.1***	-17.7***	-17.6***
	(1.7)	(1.7)	(1.2)	(1.2)
R-squared	0.03	0.03	0.02	0.03
Observations	2612	2612	2612	2612
Mean dep. var. (control)	55.7	55.7	51.7	51.7
Individual controls	no	yes	no	yes
Village controls	no	yes	no	yes
Constituency-fixed effects	no	yes	no	yes

Notes: Same specifications as in Table 2, except that the samples are restricted based on official polling station-level results from the 2001 and the 2008 general elections. (Remember that we include at most one village per polling station.) Panel A restricts the sample to government villages from polling stations where the AL got more votes than the BNP in both these elections, and Panel B adds the requirement that the average difference in vote shares exceeds 25 pp. Panel C restricts the sample to opposition villages from polling stations where the BNP got more votes than the AL in both these elections, and Panel D adds the requirement that the average difference in vote shares exceeds 25 pp. See the notes to Table 2 for details.

Table B.3.4: Robustness tests restricting the sample to respondents with likely party preferences

	(1)	(2)	(3)	(4)
Dependent variable:	Ink mark respondent		Ink mark spouse	
Panel A: Respondents from AL villages attending AL rallies				
Policy treatment	7.3**	6.8**	3.6	3.7
	(2.4)	(2.3)	(2.2)	(2.1)
Legitimacy treatment	17.3***	16.4***	12.1***	12.9***
	(2.0)	(1.8)	(1.8)	(1.9)
R-squared	0.02	0.03	0.01	0.01
Observations	1234	1234	1234	1234
Mean dep. var. (control)	65.3	65.3	64.6	64.6
Panel B: Respondents from AL villages expecting AL to win				
Policy treatment	7.1***	7.1***	6.6***	6.7***
	(0.9)	(0.9)	(0.8)	(0.8)
Legitimacy treatment	15.2***	15.1***	12.9***	13.0***
	(0.8)	(0.8)	(0.7)	(0.7)
R-squared	0.02	0.03	0.01	0.02
Observations	4967	4967	4967	4967
Mean dep. var. (control)	66.3	66.3	64.2	64.2
Panel C: Respondents from BNP villages attending BNP rallies				
Policy treatment	-13.5	-7.7	-8.7	-11.3
	(23.3)	(24.6)	(24.0)	(24.7)
Legitimacy treatment	-40.7	-33.6	-28.2	-32.3
	(22.4)	(23.0)	(21.3)	(21.5)
R-squared	0.06	-0.01	0.02	-0.08
Observations	88	88	88	88
Mean dep. var. (control)	75.0	75.0	62.5	62.5
Panel D: Respondents from BNP villages expecting BNP to win				
Policy treatment	-6.9**	-6.3**	-6.6**	-5.8**
	(2.3)	(2.3)	(2.2)	(2.2)
Legitimacy treatment	-21.4***	-20.9***	-16.8***	-16.1***
	(2.4)	(2.3)	(2.0)	(2.0)
R-squared	0.03	0.03	0.02	0.03
Observations	1937	1937	1937	1937
Mean dep. var. (control)	53.3	53.3	49.9	49.9
Individual controls	no	yes	no	yes
Village controls	no	yes	no	yes
Constituency-fixed effects	no	yes	no	yes

Notes: Same specifications as in Table 2, except that the samples are restricted based on selected responses from the second pre-treatment survey (conducted two to three weeks prior to election day). Panel A restricts the sample to respondents from AL/government villages who attended an AL rally prior to the 2014 or 2018 election; Panel B to respondents from AL/government villages who expected the local AL candidate to win in the 2018 election; Panel C to respondents from BNP/opposition villages who attended a BNP rally prior to the 2014 or 2018 election; and Panel D to respondents from BNP/opposition villages who expected the local BNP candidate to win in the 2018 election. See the notes to Table 2 for details.

B.4 Polling station-level results based on the untrustworthy official election data

Table B.4.1: Polling station-level results based on the untrustworthy official election data

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Voter turnout		AL vote share		BNP vote share	
Panel A: AL/government polling stations						
Policy treatment	-1.3	-0.4	0.4	-0.1	-0.5	0.0
	(1.2)	(1.2)	(1.3)	(1.3)	(1.2)	(1.2)
Legitimacy treatment	-0.3	0.4	0.7	0.6	-0.5	-0.4
	(1.3)	(1.1)	(1.3)	(1.3)	(1.2)	(1.2)
R-squared	-0.01	0.20	-0.01	0.04	-0.01	0.03
Observations	154	154	154	154	154	154
Mean dep. variable (control)	86.2	86.2	92.6	92.6	6.1	6.1
Panel B: BNP/opposition polling stations						
Policy treatment	-1.9	0.1	-5.1**	-2.1	4.6**	2.0
	(1.4)	(1.3)	(1.8)	(1.7)	(1.6)	(1.5)
Legitimacy treatment	-1.4	-1.0	-2.2	-1.3	1.8	1.0
	(1.3)	(1.1)	(1.8)	(1.6)	(1.6)	(1.5)
R-squared	-0.00	0.22	0.04	0.24	0.04	0.24
Observations	148	148	148	148	148	148
Mean dep. variable (control)	84.6	84.6	92.0	92.0	6.4	6.4
Constituency-fixed effects	no	yes	no	yes	no	yes

Notes: Units of observation are polling stations. Dependent variables are indicated in the top row, measured in %, and based on the official polling station-level data from the 2018 general election provided by the Election Commission at http://www.ecs.gov.bd/page/parliament-election-result. Treatment variables are equal to one whenever the corresponding treatment was administered to the respondents in the selected village of the given polling station, and zero otherwise. (Remember that we include at most one village per polling station.) Linear regressions without control variables or fixed effects in odd columns, but with constituency-fixed effects in even columns. Robust standard errors in parenthesis. ***/**/* indicate statistical significance at the 1%/5%/10%-level. The last row in each panel shows the mean of the dependent variable in the control group.

B.5 Additional results on effect heterogeneity

B.5.1 Heterogeneity in treatment effects II

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Z variable:	MPs for public goods	MPs for incomes	Pro multiple parties	Pro elections	AL will win	BNP will win	Female
Panel A: AL/government villages							
Z	1.4	2.7	-7.3**	1.2	2.9	-7.7**	-5.5***
	(1.4)	(1.8)	(2.5)	(2.0)	(2.2)	(2.6)	(1.0)
Policy treatment	9.8***	9.5***	6.7*	9.1**	5.8*	6.7***	8.6***
	(1.6)	(1.6)	(3.0)	(2.8)	(2.6)	(0.8)	(0.8)
Policy treatment × Z	-4.3	-4.3	0.5	-2.7	1.4	1.8	-3.2*
	(2.3)	(2.5)	(3.5)	(3.4)	(3.1)	(5.0)	(1.2)
Legitimacy treatment	15.7***	18.2***	14.1***	16.3***	16.8***	15.2***	17.7***
	(1.7)	(1.6)	(2.9)	(2.4)	(2.4)	(0.7)	(0.8)
Legitimacy treatment × Z	-0.4	-4.9	1.8	-1.1	-1.6	2.3	-4.8***
	(2.5)	(2.5)	(3.4)	(2.9)	(2.8)	(4.5)	(1.4)
R-squared	0.02	0.02	0.02	0.02	0.02	0.02	0.03
Observations	6065	6065	6065	6065	6051	6051	6065
Panel B: BNP/opposition villages							
Z	-0.0	0.6	8.2*	-0.2	3.3	-5.4*	-4.3***
	(2.1)	(2.2)	(3.3)	(4.4)	(2.3)	(2.2)	(1.2)
Policy treatment	-8.2***	-10.1***	-5.2	-9.5	-9.6***	-12.3***	-7.6***
	(1.9)	(1.9)	(4.3)	(4.9)	(1.9)	(1.4)	(1.4)
Policy treatment × Z	-3.1	-0.1	-5.5	-0.7	-1.6	5.4	-5.2***
	(2.7)	(2.8)	(4.6)	(5.3)	(2.8)	(2.8)	(1.4)
Legitimacy treatment	-22.5***	-20.4***	-14.8**	-18.6***	-22.6***	-21.1***	-19.5***
	(1.9)	(1.9)	(5.2)	(5.3)	(2.0)	(1.6)	(1.5)
Legitimacy treatment × Z	2.8	-0.6	-6.5	-2.3	2.6	-0.2	-2.3
	(2.7)	(2.8)	(5.7)	(5.9)	(3.1)	(3.0)	(1.3)
R-squared	0.03	0.03	0.03	0.03	0.03	0.03	0.03
Observations	5778	5778	5778	5778	5771	5771	5778

Notes: Dependent variable is equal to 100 if the respondent had an ink mark, and zero otherwise. Linear regressions without control variables or fixed effects, as in column (1) of Table 2. Z variables are based on our pre-treatment surveys. They are dummy variables for respondents stating that MPs are actually responsible to arrange funding for local public goods in column (1), respondents stating that MPs are actually responsible to generate income earning opportunities in column (2), respondents disagreeing with the statement that only one political party should be allowed to stand for election and hold office in column (3), respondents disagreeing with the statement that there should be no election and that the prime minister should decide everything in column (4), respondents expecting the local AL candidate to win in column (5), respondents expecting the local BNP candidate to win in column (6), and female respondents in column (7). Standard errors (in parentheses) are clustered at the village level. ***/**/* indicate statistical significance at the 1%/5%/10%-level.