

MOTIVATING POLITICAL CANDIDACY AND PERFORMANCE: EXPERIMENTAL EVIDENCE FROM PAKISTAN*

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Abstract

In developing countries, politics is considered the domain of the privileged – those driven by a desire to further their own interests. We analyze a field experiment that mobilizes ordinary citizens to become village politicians in Pakistan by varying how political office is portrayed. When pro-social versus personal benefits of elected office are highlighted – like the ability to help others instead of gaining respect and status – the probability that a randomly selected citizen will run and win more than doubles. We also find that a year after elections official budgetary spending in these villages is more aligned with citizen preferences. There is evidence that changes in the types of candidates is a potential mechanism: those who think political office is consistent with pro-sociality run more with pro-social messaging and less with personal messaging. Overall, our results show that the way political office is portrayed affects who decides to become a politician, as well as subsequent policy outcomes.

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1 Introduction

Democracies give citizens a say in who represents them, which makes elections an important channel for aggregating preferences. What is often missed, and no less important, in determining the quality and motivation of elected representatives, is the other side of the democratic coin: who decides to run for political office (Dal Bó et al. 2017; Besley 2005).

Who becomes a political candidate is particularly important in developing countries, where being a politician is often considered the domain of elites. This affects the demand for ordinary people to run for office through political parties, but it also affects the supply of who decides to enter the race to begin with. As a result, democratic performance may suffer because those elected may not represent interests of all citizens (Manin 1997; Chattopadhyay and Duflo 2004; Fujiwara 2015; Martinez-Bravo et al. 2017; Banerjee et al. 2017).

We use the opportunity afforded by a new set of local elections in Pakistan. In 2015 the Supreme Court of Pakistan announced that elections at the village level be carried out for the first time in the country’s history.¹ This ruling potentially created a ‘critical juncture’ in consolidating democracy in the area (Acemoglu and Robinson 2013; Banerjee and Duflo 2014). Roger Myerson (2009), writing specifically about Pakistan, notes: “just as economic competition should motivate suppliers to offer better values in the market, so democratic competition in the political arena should motivate political leaders to promise better public services and more efficient government.” Opening up opportunities for politics can therefore affect the types of people who run for office, possibly making politics less elite and democracy more responsive.

To confront these issues, we run a randomized controlled trial before elections for the new village councils in Pakistan. We put effort into recruiting a randomly selected subset of

¹Unlike neighboring India, Pakistan has never had elected government at the village level. The scope of the institutional shakeup can be gauged from the fact that before 2015, there were 125 elected officials representing a population of 30 million. The reform increased the number of directly elected officials to 41,762 (Democracy Reporting International 2015). Besides this many-fold increase in the number of elected officials, the province also committed that 30% of the development budget was to be spent through these new local governments.

villagers to run for office by providing them with basic information on how to file papers to declare candidacy in public and private meetings. During these meetings, we randomize the portrayal of political office by highlighting either social benefits of office, such as the ability to help others, or personal benefits, such as gaining respect and influence.² We randomize these at the village level in one-on-one meetings with enumerators as well as village-level public meetings, such that social *or* personal benefits are highlighted in a village only in private, only in public, at both, or at neither.

While mobilization is frequently deployed as a tool for increasing voter participation across the developed (Gerber and Green 2000; Pons 2018) and developing world (Giné and Mansuri 2011), it is not obvious if mobilization will work on costlier political actions like candidacy. In particular, it is unclear how candidacy will be affected when public office is portrayed in terms of its social or personal benefits. While, personal motivations can crowd out socially minded people from running for office (Bénabou and Tirole 2006), they can also induce more competent people to seek office (Ferraz and Finan 2011). Similarly, social motivations can prime more socially minded people to contest elections (Besley and Ghatak 2005a), but they may also give public cover to more personally-motivated people to run (Fisman et al. 2014; Truex 2014). Besides showing how these motivations change the probability of running for office, we also explore heterogeneous effects on the types of people who respond to these treatments.

We report three results that trace treatment effects from the candidacy decision to performance impacts. First, social messages, that highlight the ability of elected councilors to help others, increase candidacy by 2.4 percentage points ($p = 0.002$) versus messages that prime people to think about personal benefits from office. When we compare these messages

²This closely follows Ashraf et al. (2018) who make salient career or pro-social benefits of a health worker job in Zambia through posters in the village center. We differ in our comparisons by retaining a neutral condition so that the effect of benefits can be tested individually. ‘Personal’ benefits from office can include respect (Bénabou and Tirole 2003), ambition for a political career (Fowler and McClure 1990; Fowler 1993), wages (Dal Bó et al. 2013), and rents (Fisman et al. 2014; Eggers and Hainmueller 2013; Truex 2014). ‘Social’ benefits for running may include a commitment to a pro-social goal (Handy and Katz 1998), intrinsic utility (Broockman 2013; Andreoni and Bernheim 2009), or altruism (Bénabou and Tirole 2006).

against a neutral condition, we find that they work in opposite directions: social messaging increases the probability of candidacy (0.015 percentage points, $p = 0.081$), while personal messages reduces the probability of running (-0.009 percentage points, $p = 0.124$). Beyond candidacy, these effects persist in the probability of getting elected to the village councils. We also show that mobilization is most effective when messaging reinforces the benefits of office both in private and public village meetings. Overall, this first set of results show that the way in which politics is portrayed influences people’s decision to enter the race in opposite directions.

Second, we ask if a change in candidacy leads to changes in in-office outcomes, or whether in-office incentives wash out any changes in who decides to run or not run for office (Fearon 1999)? We find that politicians that emerge in villages where we highlighted social messages are more responsive to citizen preferences than those motivated by personal messages. Official budgets at the end of the first year in office are more aligned with citizen preferences in the former types of villages, and less aligned in the latter case. This result is important because it demonstrates that how political office is depicted can directly affect the performance of democracy.

Third, we conduct an exploratory analysis on whether a potential mechanism for the treatment effects we observe is changes in the type of people who run for office. Consistent with the theoretical and empirical literature that highlights the potential for extrinsic benefits to crowd out intrinsic motivations (Bénabou and Tirole 2006; Ashraf et al. 2018), we find that making salient the personal benefits of political office reduces candidacy among pro-social types – those who ex-ante were more likely to consider village councilors’ jobs to be pro-social service. Conversely, similar pro-social people are more likely to run when social benefits of office are highlighted in the village (Handy and Katz 1998; Besley and Ghatak 2005b).

Taken together, our experiment provides compelling evidence that, in addition to the demand for politicians, we need to think about the supply of politicians – that is, we need

to account for both citizens and their preferences, but also politicians and their preferences in our understanding of democratic performance (Dal Bó and Finan 2018).

There is a large literature on elections and how they allow citizens to express their preferences (Downs 1957; Manin 1997; Besley and Coate 1997; Osborne and Slivinski 1996; Fearon 1999; Caselli and Morelli 2004). However, there exists relatively little empirical work, almost all on developed countries, on how politicians decide to run for elections (Dal Bó et al. 2017; Preece and Stoddard 2015).³ Understanding the mechanics of the decision to enter politics is perhaps more important in developing countries where races are often closed to broader participation and potentially captured by elites (Bardhan and Mookherjee 2000). We study these questions in the case of new local governments in Pakistan, where similar risks are anticipated.

Previous work on the performance of democratic systems has analyzed the effects of aligning the preferences of elected representatives with those of the electorate (Chattopadhyay and Duflo 2004; Fujiwara 2015; Martinez-Bravo 2017). Chattopadhyay and Duflo (2004) study that more representation of women in India improves preference alignment between women in the electorate and the elected representatives. The results in this paper are similar in spirit. When politics is portrayed as a pro-social task, councils' spending is more aligned with citizen preferences. However, unlike Chattopadhyay and Duflo, who consider equally aligning preferences between a relatively equal sized electorate of men and women, we are interested in improving the match of preferences between a large electorate and a relatively small elected elite. In our case, therefore, the political mobilizing of average citizens is important.

A key contribution of this paper compared to previous work is that we can study the performance effects of political selection. In this sense, Ferraz and Finan (2011) is the closest paper to ours as they study the effect of higher wages on who decides to run for office, but are able to examine the effects of higher wages on legislative performance and

³Notable exceptions include Cruz et al. (2017) in the Philippines, Beath et al. (2016) in Afghanistan, and Banerjee et al. (2013) in India.

public goods provision. Our analysis of performance extends this work by also incorporating citizen preferences in the analysis to benchmark whether an improvement in one direction is potentially welfare enhancing.⁴

Finally, the results in the paper are also relevant to the policy community working on democracy promotion in consolidating democracies. International actors, such as the OECD,⁵ spend billions of dollars promoting inclusive and responsive institutions around the world.⁶ We provide, to our knowledge, the first experimental evidence that mobilization can have a large effect on candidacy and performance of local government.

The rest of the paper proceeds as follows. In Section 2 below we provide details of the context as well as background information on the reform. Section 3 provides details of the experiment. We present experimental results in two sections: Section 4 analyzes the effect of treatments on candidacy and election to the village council, and performance impacts. Next, Section 5 analyzes mechanisms. Finally, Section 6 concludes the paper.

2 Context

This section briefly reviews the history of devolution in Pakistan to help place the new reform in context. Next, it provides specifics of how village councils are formed, as well as information on the candidacy process. Finally, it provides some information on the area where we conduct the experiment.

2.1 The Local Government Reform of 2015

Local governments in Pakistan have been associated with military rule. This has made the concept of devolution of power unappealing to political parties and political elites. Conse-

⁴Note that we are unable to study effects on more downstream development outcomes like child birth weight (Fujiwara 2015).

⁵OECD/DAC (2009) estimate that 20% of all official Development Assistance by OECD is devoted to capacity development.

⁶The U.S. spends \$2.5 billion annually on democracy promotion (Carothers 2009).

quently, existing party systems in Pakistan have become increasingly centralized, with the party leadership exercising strict control over party cadres (Cheema et al. 2010). While political parties do proclaim the principles of democracy within their parties, they seldom hold intra-party elections, preferring to assign party offices to loyalists as rewards (Salim 2005). Unsurprisingly, basic village and neighborhood levels are marked by the relative absence of political workers who can be called upon to run for offices of local government. This has suited political elites interested in consolidating power at the higher central and/or provincial levels.

This paper focuses on Khyber Pakhtunkhwa, a province of thirty million people in Pakistan's northwest. Under the direction of the Supreme Court of Pakistan, the KP government promulgated "the Local Government Act (LGA) of 2013" (United Nations Development Program 2014). Village council elections were held for the first time in Pakistan's history on May 30th, 2015 under this law.

As shown in Figure 1, Village Councils (together with Neighborhood Councils for urban areas), constitute the lowest tier of the local government under the LGA 2013. Adult residents of villages who are eligible to vote can run for the office of a Village Councilor. While there are almost no explicit restrictions (other than no criminal record and a clean financial history), the process of declaring candidacy requires an understanding of the system and an ability to navigate the bureaucratic requirements for paperwork. As described in detail in Appendix A, citizens have to collect papers, prepare legal declarations, and deposit approximately USD 10 through bank draft to have their application accepted. Significantly, elections for Village Councils were conducted on a non-party basis. This bars political party workers from using the party name and platform in their campaigns. Unofficially, however, candidates could invoke party platforms if the need arises.

The 2015 Local Government elections are important for at least two reasons. First, they introduced the principles of democratic representation at the most local level. Second, they provided opportunities for local prospective politicians to get hands-on training in

politics, and to appear on party platforms for subsequent elections. Thus, these elections have potentially democratized political parties in the province.

2.2 Composition of the Council

The law follows the principle of equal representation, which translates into council sizes equal in proportion to the size of the villages. Each council has general (open) and reserved seats that are elected through a direct ballot for at-large constituencies comprising the village. The number of general seats varies between five and ten, depending on the population of the village. In addition, each village has two women, one youth (less than 30 years of age), one farmer/worker, and one minority seat that is reserved. Any eligible person can run for the election on a general seat, while the reserved seats require the candidate to meet specific criteria. In line with the types of open and reserved seats in the village, voters cast five ballots: one for a general seat candidate, one for peasant/worker, one for youth, and two for women. The person receiving the highest number of votes on a general seat is elected as the Nazim (chairperson) of the village council, and the candidate securing the second highest number of votes is appointed as their deputy.

Village Councils are the lowest tier of the local government. Compared to the previous lowest existing tier of elected government - the Union Council, which was elected by an average of 26,000 citizens - local councils, on average, represent 7,500 people across the province, and 4,300 people in our sample. They are responsible for ensuring the provision of basic public services in their areas of operation including preparation of annual development budgets which form a core activity that councils perform.

2.3 Haripur and Abbottabad

Our experiment focuses on two districts of the Khyber-Pakhtunkhwa province in Pakistan: Haripur and Abbottabad. These districts, shown in Figure 2, were chosen for the experiment after careful logistical considerations and owing to the availability of an able local NGO

partner to implement the intervention. These two districts have better health and education indicators compared to the provincial average. The percentage of households with access to ‘improved drinking water sources’ in Abbottabad and Haripur is 81.7% and 75% respectively, whereas the provincial average is 74.6% (*Multiple Indicator Cluster Survey* 2008).

While there are no systematic data on informal governance institutions in these areas prior to the reform, our fieldwork suggests that local decisions were typically taken by village elites who would organize around informal groups called ‘jhanbas’. These groups would form around issues that require collective action but, interestingly, would not necessarily form along class and caste lines. Overall, it is important to note that the treatment effects we see are originating from a baseline of some informal organization rather than a complete lack of institutions. Of course, random assignment means that the effect of these institutions is balanced across treatment arms.

3 Experiment

We design an experiment in 192 randomly sampled villages of Haripur and Abbottabad districts. We collaborate with a local partner NGO, Sangum Development Organization, henceforth Sangum, that is based in Haripur and has vast experience in conducting field-based projects.

In the experiment, we canvass the village (private meeting), and invite locals to attend a public meeting in the village the same afternoon or the following day (public meeting). In this section we i) explain how field activities unfolded ii) describe the treatments, iii) present details of the randomization, and iv) present a timeline for the project.

3.1 Order of Field Activities

Activities on the ground proceed as follows. First, the **private meetings**. A pair of social mobilizers from our field partners, Sangum, canvass on average 48 randomly sampled

households in every village.⁷ Once a household is approached, enumerators conduct a short survey with a male respondent.⁸ After the survey, enumerators deliver a neutral, personal, or a social message in private one-on-one meetings (see section 3.2). Finally, subjects are invited to a public meeting in the village, and the time and location details for these are shared. It is made clear that the public meeting is open to others who may be interested in finding out more about the upcoming elections.⁹ Importantly, the private meetings are usually held at the respondent’s dwelling. On average, the interaction between our enumerators and subjects lasts between 10-15 minutes. Figure 3 shows examples of these interactions between enumerators and citizens.

Second, the **public meetings**. Enumerators then proceed to prepare for the public meeting. Based on the experience of the enumerators, in some villages the same field team carries out the session. In other villages, more experienced persons of the team conduct the public meetings. All the public sessions are organized within the same village to make them accessible for citizens. During the session, the field staff follows the guidelines discussed in section 3.2. As participants arrive, enumerators note their attendance. Then the sessions begin (more details below). Figure 4 shows examples of these sessions in three villages. On

⁷As no household rosters or maps of villages exist, field teams are instructed to begin at the center of the village. The center is identified as a key landmark at the geographic center of the village, by the survey team supervisors. The teams start the activity by talking to every 5th household in the direction of North and repeat this process in different directions interviewing about 10 households in one direction on average. An advantage of drawing a random sample is that we capture the effect of treatments for the average household office-eligible male member. Most research on candidates focuses on citizens that have already selected into some aspects of political life such as membership of a political party (Preece and Stoddard 2015), or an interest group (Broockman 2014).

⁸We focus on male respondents for three reasons: First, field research with women in most areas of Pakistan requires women enumerators. Due to funding constraints, we were unable to double team sizes to canvass women respondents. Second, discussions in the pilots suggested that womens’ political participation through candidacy was expected to be low, mostly restricted to the two reserved seats for women. In fact, 45 of the 384 reserved women’s seats in our sample remained uncontested, and General (open) seats did not have a single woman contestant across 48 villages where we carry out no treatment exercise. Finally, research has shown that womens’ political preferences in Pakistan tend to align with male members of the household (Bari 2005).

⁹We decided to not make public meetings exclusive to those we invited for two reasons. First, since the treatments involve encouraging people to run for office, we wanted to ensure that at the village level, people had the opportunity to receive information on how to contest if they were interested. Second, logistically, it is difficult and unpleasant to deny permission to people who are interested in finding out more about the elections. To maintain the good rapport our partners enjoy in the area, we decided to not have exclusivity in the public meetings.

average, a public session lasts 30-40 minutes in the village.

3.2 Details of Treatments

The treatments focus on highlighting the benefits accruing to prospective politicians should they be elected to political office. To difference out the mode of delivering information on the benefits of office, we always retain what we label a ‘neutral’ condition. A key point to note here is that the content of the ‘neutral’ condition is *always* delivered – it can be thought of as our excuse to talk to people. We make benefits from office salient *on top* of the neutral condition so that comparisons between the two are the effect of this extra message that is made salient.

There are many aspects of the treatment that are similar across the two modes of delivery. First, we focus on keeping the interaction between enumerators and citizens natural. A consequence of this is that we kept our pre-treatment survey as well as the training roster relatively short. Second, while we use the term ‘scripts’ to describe the content of the treatments below, the treatments were actually delivered in a conversational manner to make the exercise natural – encouraging people to run for office while reading from a paper is unlikely to work, or be received well. This is one reason we decided to partner with Sangum, as their staff includes experienced fieldworkers. The scripts we describe below were used extensively in training the enumerators, and the enumerators had a copy of these scripts in the field to refresh the key points they had to make. The conversational delivery of the treatment also makes the delivery of information across the two treatments comparable. Third, the scripts were developed after detailed piloting with focus groups before fieldwork commenced. Therefore, the benefits mentioned in the scripts come from, and are directly relevant to, the population where we conduct our experiment. Fourth, the private meetings were often delivered at a person’s residence (and always in a private space), while the public sessions were conducted in a central area in the village. It was public in the sense that a relatively larger number of people could be accommodated - it was still a ‘venue’ however, and

access to the public ceased once the training began. Usually, the venue would be someone's house or a school building in the village, access to which was granted to our field partners by locals.

Private Meeting Scripts

The first way of delivering treatments is through one-on-one meetings with enumerators. One of three scripts is used during these meetings: i) neutral, ii) personal benefits, or iii) social benefits.

Neutral Script: *“You may be aware that for the first time elections on May 30th will elect a 10-15 member council at the village level. People above the age of 21 can contest these elections. There isn't even an education requirement to contest. All you have to do is collect papers from the district office of the Election Commission, and submit them along with two references.”*

Personal Benefits Script: Neutral Script and *“People who are elected to the village election will be given a excellent opportunity to move forward in politics, and gain respect and influence in the area. Members of the village council will be able to build connections with tehsil and district level politicians, which will open avenues for advancing in politics. Besides this, council members will also be able to enhance their influence in the village. They will be known as leaders in their neighborhoods, and this get them more recognition. Their children will be able to build a network in the area, which will make their entry into politics easier.”*

Social Benefits Script: Neutral Script and *“People who are elected to the village election will be given a excellent opportunity to do their part for the development of their area. Members of the village council will play an important role in improving the quality of government services in the village. They will work towards securing the welfare and rights of the poor. Working together with the district governments, they will improve village school and*

health facilities. An elected councillor will have a unique opportunity to address the problems of his neighborhood, and this will make him the standard-bearer of social development for the village.”

Public Meeting Outline

We also deliver treatments by offering one public meeting in every village. The public meeting focuses on delivering information regarding the election to anyone who attends. This forms the ‘neutral’ public session. Based on the randomization that we describe below, a section was added at the end of the public meeting to tell attendees about either personal or social benefits that can accrue to people who win the election. Just like the private meetings, the public sessions follow a broad script developed by the research team in collaboration with master trainers at Sangum. Below, we provide an outline that was followed for the public sessions.

1. Welcome and Introductions
2. Overview of Local Elections, including information on Village Councils
3. Provide details of:
 - Composition of councils (Chairpersons, General Seats, Reserved Seats)
 - Pre-requisites for Filing Papers (age, nationality, etc)
4. Detail Process of Declaring Candidacy
5. Discussion on **Personal** *or* **Social** Benefits to councilors
6. Questions and Discussion
7. End

3.3 Details of Randomization

Figure 5 presents the overall design of the experiment across 192 villages. All treatments are block-randomized at the village level. Blocks are manually created by our field partner

and are based on geographic proximity and access constraints. Treatments are randomized across three types of villages. In 48 “neutral villages” a neutral message is delivered both in private and public meetings. In 72 “social village”, a social message is delivered in private or public meeting or both. Finally, 72 “personal village” are similarly selected. Villages where a social or personal benefits message is highlighted only in private or public, but not both, are labeled as “low” intensity treatments, while villages where the personal or social benefits message is highlighted in both private and public meetings are labeled as “high” intensity treatments. Critically, the social and personal benefits of office are not cross randomized, such that, a village can only receive one type of message but not both. This is shown by the missing cells in Table I. Finally, cross-randomizing messages regarding benefits from office and retaining a neutral condition where no benefits are mentioned allows us to difference out the direct effect of approaching citizens and having meetings with them.

3.4 Timeline

Below, we provide a condensed timeline for the project.

1. March 3-14, 2015: Pilot for treatment design in Haripur District, KP
2. Last week of March - April 13, 2015: Administering Treatments
3. April 13-17, 2015: Candidates file their papers
4. May 3-28, 2015: Survey of all candidates
5. May 30, 2015: Election Day
6. June 23 - July 31, 2016: Performance surveys of council members and citizens
7. June 25, 2015: Initial notification of results
8. August 30, 2015: Oaths of office begin

3.5 Data

Pre-candidacy surveys: Initial fieldwork comprises three data collection efforts: first, during the private meeting stage, a survey is fielded before enumerators deliver the treatment scripts. The respondents are asked questions about demographics, their preferences regarding politics, their beliefs about the process of candidacy, as well as their own interest in it. These data provide us with pre-treatment covariates on a representative sample of the village. However, the scope of these data is limited as we canvass about 9,310 people over a short period of time. Second, our field teams collect information on the number of settlements, population and remoteness of the village in a short village survey with key informants. We use these data to test for the balance of our randomization. Finally, when individuals show up for the public meeting, they fill out an attendance roster, which notes their names so that they can be matched to the remaining data.

Electoral Data: Once subjects file papers for candidacy, we retrieve this information from the Election Commission of Pakistan and match the list of candidates for every village to our subjects. We also obtain data on the election results from the local offices of the Election Commission. These result sheets contain information on the number of votes secured by each candidate, the total tally of votes for the village, and the total registered votes in the village.

Performance Data: For performance related outcomes, we return to villages one year after the elections in June and July of 2016 and collect information from two sources. First, we collect information on spending decisions from the first budget documents prepared by each village (at the end of their first year). This information is available with the village Secretary.¹⁰ Second, we survey a sample of citizens in each village to collect their preferences over budgets. In total we interview 1318 citizens in this survey.

¹⁰We were able to collect this information from all villages except three that were facing a gridlock over spending decisions.

3.6 Balance

Appendix Table A2 presents the summary statistics for the subject pool. Table II checks randomization balance from a joint orthogonality test of the null hypothesis that the treatment arms do not predict village level pre-treatment variables, including population, number of settlements, distance to a main road and the local bureaucracy headquarters, and the size of the village council. We also report p-values of the balance test between pure control group and each of the treatment groups in the table. Overall, the tests suggest that the randomization was successful.

3.7 Pre-Analysis Plan

We pre-registered the main analysis of this paper with American Economic Association RCT Registry (AEARCTR-0000685) and Evidence in Governance and Politics registry (20151102AA).¹¹ In Appendix B, we describe how the analysis in this paper relates to the PAP and list any changes we made to the variables. There are two main points to note. First, candidacy and election to the village council, the outcomes in Section 4.2, are registered as the primary outcomes of interest in the pre-analysis plan. Second, the performance outcomes reported in Section 4.3 are not pre-registered, though our main results in that section makes use of official data on budgets.

4 The Effect of Making Benefits from Office Salient

In this section, we focus on studying the effects of making specific benefits from office salient to the experimental population. Does it matter is social benefits are highlighted over personal benefits from office?

¹¹These can be accessed via <https://www.socialscienceregistry.org/trials/685> and <http://egap.org/registration/1576>

4.1 Estimation

To examine answers to these questions, we focus on our subject pool of 9,310 individuals in the 192 treatment villages. In addition to comparing the effects of the two benefits from office against each other, we compare villages where benefits from office are made salient against villages where neutral messages are highlighted. Comparing against each other and against the neutral condition allows us to difference out the effect of having a conversation and focus specifically on the effect of making benefits from office salient.

We run regressions of the following form:

$$Y_{iv} = \beta_1 \text{Neutral}_v + \beta_2 \text{Social}_v + \beta_3 \text{Personal}_v + \gamma_v + \varepsilon_{iv} \quad (1)$$

where Neutral_v is an indicator variable that correspond to labeled cell N in Table I where only a neutral message was delivered in both private and public; Social_v is an indicator variable for villages where a social message was delivered in either public or private, corresponding to cells D , E , and F in Table I; and Personal_v is an indicator variable for villages where personal benefits were made salient in either public or private, corresponding to cells A , B , and C in Table I.

As we have random variation in the intensity of treatment, where some villages receive the same message in both private and public meetings, while others only receive those messages once, we also run regressions of the following form:

$$Y_{iv} = \beta_1 \text{Neutral}_v + \beta_{2L} \text{SocialLow}_v + \beta_{2H} \text{SocialHigh}_v + \beta_{3L} \text{PersonalLow}_v + \beta_{3H} \text{PersonalHigh}_v + \gamma_v + \varepsilon_{iv} \quad (2)$$

where SocialLow is an indicator variable for villages where social benefits of office are highlighted in either the private or public condition, corresponding to D and E cells in Table I. SocialHigh is an indicator variable for villages where social benefits are highlighted in both private and public, corresponding to cell F in Table I. The coefficients for personal benefits

are similarly defined. γ_v represents a vector of village-level block fixed effects. Standard errors are clustered at the village level, the unit of treatment assignment.

As we estimate the above models without an intercept, the β coefficients denote the means for outcomes for each group. With this set up, we can impose linear restrictions to compute the effect of making social or personal benefits from office salient. We compute the following effects from specification 1:

$$\text{Effect of Any Social Benefits vs Any Personal Benefits: } \beta_2 - \beta_3 = 0$$

$$\text{Effect of Any Social Benefits vs Neutral: } \beta_2 - \beta_1 = 0$$

$$\text{Effect of Any Personal Benefits vs Neutral: } \beta_3 - \beta_1 = 0$$

Similarly, to test for whether the intensity of treatment matters, we can impose the following restricts on Specification 2:

$$\text{Effect of High Social Benefits vs High Personal Benefits: } \beta_{2H} - \beta_{3H} = 0$$

$$\text{Effect of High Social Benefits vs Neutral: } \beta_{2H} - \beta_1 = 0$$

$$\text{Effect of High Personal Benefits vs Neutral: } \beta_{3H} - \beta_1 = 0$$

$$\text{Effect of Low Social Benefits vs Low Personal Benefits: } \beta_{2L} - \beta_{3L} = 0$$

$$\text{Effect of Low Social Benefits vs Neutral: } \beta_{2L} - \beta_1 = 0$$

$$\text{Effect of Low Personal Benefits vs Neutral: } \beta_{3L} - \beta_1 = 0$$

We use the effects explained above as the estimands of interest in this section. Importantly, we use inverse probability weights in the estimation of model 1 such that ‘low’ and ‘high’ type villages receive the same weight when calculating the overall effect of making benefits salient.¹² For completeness we also show the treatment effects across all cells in

¹²For example, the 24 villages that receive the High Social treatment are weighted twice the 48 Low Social treatment villages. This means that the ‘Any’ Social treatment in Table III can be interpreted as a simple

Table I in Appendix C.1.

4.2 Results on Candidacy and Election

Table III presents the main results. First, relative to personal benefits, social benefits increase the probability of candidacy by 2.4 percentage points ($p=0.002$), an increase of more than a hundred percent. These effects are the result of social and personal benefits changing behavior in opposite directions. Highlighting social benefits increases the candidacy by 1.5 percentage points ($p=0.081$) while highlighting personal benefits reduces the probability of candidacy by 0.9 percentage points ($p=0.124$).

Second, the effects are larger and stronger when treatment intensity is high. High social benefits versus high personal benefits increase the probability of candidacy by 3.8 percentage points ($p=0.004$). Highlighting social benefits versus a neutral message seems to be contributing to about two-thirds of this magnitude.

Third, these effects also translate into the probability of getting elected to office. When social benefits are made salient versus personal benefits the unconditional probability of getting elected to office is 1.5 percentage points ($p = 0.003$). This can be decomposed into a 0.8 percentage point ($p = 0.165$) increase in the probability of getting elected when social benefits are made salient and a 0.8 percentage point ($p = 0.035$) decrease when personal benefits are highlighted. Relative to an unconditional probability of election to office of 1.7% these are relatively large effects. As before, the effects on election are primarily concentrated in the high treatment.

Overall, the results in this section suggest that the experiment had large effects on costly decisions that citizens take in terms of political participation related to candidacy. The way in which office is portrayed shapes these decisions in substantively important and opposite directions: portraying elected office as consistent with pro-social work positively influences peoples' decision to run relative to the case where political office is portrayed as a vehicle

average of high and low.

that yields personal benefits.

4.3 Results on Performance

What is the effect of changes in candidacy on the actual performance of politicians? It could be the case that changes in who runs for office matter little for the way in which democracy functions because critical decisions are shaped by in-office incentives rather than the identity of winning politicians. On the other hand, who wins might have a direct bearing on policy making process, thereby affecting the outcomes that democracies deliver. It is therefore important that we trace the effects we observe on candidacy and election to office on the actual performance of the village councils.

It remains unclear, however, how the performance of politicians should be judged. Existing research proxies for politician quality using human capital measures such as education (Dal Bó et al. 2017). While a higher quality politician may be able to affect performance on the extensive margin, such as negotiating a higher budget from the center, it is not clear if such politicians are also better at responding to citizen preferences. Indeed, research has shown that descriptively representative politicians may be better at responding to citizens than status-quo politicians (Chattopadhyay and Duflo 2004).

In our context, it may be the case that people motivated by social benefits are actually not better at their job than status quo politicians because they might have less human capital. In contrast, it could be the case that these people are in fact better at their job because they see political office as a means to achieve a pro-social end and are therefore better able and motivated to ascertain the needs of citizens.

Since we can only see the performance of elected individuals in office, and have no way of measuring how unelected politicians would have performed had they been elected, we cannot analyze individual level performance of our experimental sample. Therefore, in this section we turn our attention to performance of the entire elected council. We can use our village level randomization to estimate these effects causally. This is directly relevant for our case

where each elected village council receives a fixed annual budget that the council may spend as they see fit. As the budget amount is based on a formula and therefore not dependent on the efforts of politicians themselves, we can study the intensive margin of budget spending to ascertain if elected politicians' behavior is closer to or farther from the preferences of citizens.

This section uses two sources of data: a survey of citizens to measure their preferences on budget spending and official budget data from the village councils. We first discuss the difference between citizen preferences and how the councils decide to spend their development budgets descriptively. Then we dig deeper into differences between the council behavior as a result of treatments.

4.3.1 Citizen Preferences

In the survey of citizens, we ask them to divide hypothetical Rs. 100 village development budget over a set of sectors. All councils are assigned an annual budget starting from their second year in office. We conduct this exercise before the actual money arrives.

Citizen responses are collapsed into four broad categories based on the nature of the spending item. These categories are Municipal Services, Infrastructure, Community, and a residual category that stores preferences that are officially not the primary responsibility of the village council. *Municipal Services* include allocation to education, health, water, sewerage and waste disposal. *Infrastructure* includes construction and rehabilitation of roads, streets, retainer walls, and street lights. *Community* includes spending money on sports, graveyard, mosque, and the community center. *Not Primary Responsibility* includes provision of electricity, transport service, security, skills development, and a residual other category.

4.3.2 Summary Statistics on Preferences and Budgets

We compare citizen responses with how the elected councils actually decide to spend the money they were allocated in the official budgets they draw and submit to the Local Government office as described in section 3.5. Table IV reports the descriptive statistics from this exercise for villages which did not receive any appeal on the benefits from office (that is, the 48 neutral villages). We can see that the main difference between citizens' preferences and actual spending arises from the two categories of municipal services and infrastructure. Citizens want about 65% of the budget spent on the provision of municipal services in neutral villages, whereas councils spend about that much on building infrastructure in the village. This situation is reversed for infrastructure projects. Speculatively, one reason why village councils may want to spend less on municipal services and more on infrastructure is the opportunity for pilferage offered by infrastructure projects (Lehne et al. 2018). Since these statistics are only for the neutral condition villages, they present a picture of the differences between citizen preferences and politician decisions after the election.

By themselves, regressions on differences in spending by treatment group would be hard to interpret as it would be unclear if spending more on one sector is necessarily 'better' than spending more on another. However, as discussed above, the large dichotomy in how councils are spending their money in the neutral and control condition motivate us to investigate if there is a difference in how councilors spend their development budget relative to citizen preferences based on the treatment status of the village council.

4.3.3 Results on Euclidean Distance Between Preferences and Budgets

We are interested in checking if treatments improve the alignment of council spending with citizen preferences. To do this we measure the Euclidean distance between the two as follows. We calculate the distance for each budget category $j \in J$ by using the formula $\sqrt{(B_{ji} - \bar{C}_{ji})^2}$, where B_{ji} refers to the percentage of the budget spent on j in village i and \bar{C}_{ji} is the average of citizen preferences for spending on that category in village i . We also

calculate the overall difference in council spending and citizen preferences by summing over all four budget categories as follows: $\sqrt{\sum_J (B_{ji} - \bar{C}_{ji})^2}$.

Table V shows the effects of treatment on the Euclidean distance between citizen preferences and council budgets as a sum in column (1) and decomposed across the four budget categories in columns (2) - (5).

First, elected councils in villages where public office was portrayed with a social message versus a personal benefits message spend their budgets in a manner that is more aligned with citizen preferences. The effect on the Euclidean distance between the two is 13.05 points ($p = 0.01$). Importantly the primary contributor to this decrease in distance are spending on municipal and infrastructure categories which were the primary non-aligned categories in Table IV. Second, as before the effects are larger and stronger where treatment intensity is high. The effect of high social versus high personal on the Euclidean distance is 21.2 points ($p = 0.009$). Decomposing this overall effect in panel B and panel C, reveals that as with the candidacy and election results the degree of alignment between council spending and citizen preferences moves in opposite directions depending on what feature of political office is highlighted. Although, we have less power to detect these effects, spending in social versus neutral villages is more aligned while spending in personal villages is more misaligned.

Overall, this result shows that how political office was portrayed before the process of candidacy was announced has a large effect on policies undertaken by elected village councils. This suggests that indeed who runs for office has a potential to have a direct bearing on how democracies perform.

5 Mechanisms

The results on candidacy and performance are consistent with two classes of explanations. The first explanation focuses on politicians: it could be the case that treatments induced different types of politicians to run for office or that they primed politicians on what to do

once in office, which resulted in different policies by village councils. The second explanation focuses on citizens: it could be the case that citizens updated their expectations of the elected councils based on the nature of treatments delivered in the village such that changes in citizens' preferences are the primary contributor to the alignment of policies. In this section, we present further evidence on these explanations.

5.1 Types

Measuring the ex-ante type of a prospective politician is not straight forward. Before treatments are delivered, we carryout a short survey where we measure the degree to which a person associates political office with pro-social goals. We used responses to these questions as one measure of a person's pro-sociality. Specifically, we asked how much respondents agreed with four statements on a scale of one to five: 'Elected representatives serve people by solving their problems'; 'Helping others brings peace'; 'Publicly provided services are very important for ordinary people'; 'Improving village schools is directly linked to the performance of public representatives'. We take the average of responses across these four questions and split the data at the median to study if higher than median response on this variable predicts the treatment effects discussed in the previous section. The benefit of this strategy is that it maximizes the power by splitting respondents equally across the high and low types. The drawback, of courses, is that these are stated instead of behavioral measures of a person's pro-sociality but responses to these questions are not affected by the treatments as shown in Appendix Table A5.

Table VI presents heterogeneous effects of treatments by our measure of pro-sociality. Column (1) and (2) show the effect of treatment on low and high types separately, while column (3) shows the difference between the two. Column (4) to (9) show effects in the same way. We find that in villages where social benefits are highlighted the probability of candidacy increases by 3.5 percentage points among the high pro-social types versus a smaller increase of 1 percentage point among the low types (the difference is 2.5 percentage points,

$p = 0.027$). As before there is suggestive evidence that the high types are more likely to run when social versus neutral benefits are highlighted and less likely to run when personal versus neutral benefits are highlighted, though our power to study this decomposition is limited. In addition, these heterogeneous effects by pro-social types are concentrated primarily in the high intensity treatments.

Overall, this presents suggestive evidence that pro-social types are more likely to respond to social messaging versus messaging on the direct benefits of office. At least part of the effect on performance may, therefore, relate to changes in the types of people who are running for office.

5.2 Council Behavior

The main results on performance shows that social versus personal messaging improves alignment of budgetary spending with citizen preferences. This improved alignment can come about through changes in councils behavior in terms of budgetary spending and/or through changes in citizens' preferences themselves.

First, in Table VII we show evidence that council behavior in terms of budgetary spending changes in response to treatment. We find that social versus personal villages spend more money on municipal services (9.3 percentage points, $p = 0.02$) and less money on infrastructure (8.2 percentage points, $p = 0.042$). These results when compared with discrepancies between budgetary spending and citizen preferences in the neutral villages, as shown in Table IV, suggests that social versus personal messaging moves elected councils behavior towards what citizens want, that is, more municipal services and less infrastructure. As before, these changes are larger in the high intensity treatments and are contributed by effects moving in opposite directions when we decompose social and personal messaging against the neutral condition.

Having shown evidence for changes in actual council behavior, we also show evidence that citizen preferences are stable across treatment conditions. In Table VIII, we find no overall

differences in citizen preferences across budget categories that are the primary responsibility of the councils. There is some evidence to suggest that in villages where personal messages are highlighted against neutral, citizen preferences lean more towards spending categories that are not the primary responsibilities of elected councils. As shown above, lower candidacy among pro-social types in these villages is one way in which citizens' awareness of council responsibilities is skewed towards sectors not in the primary official domain.

Overall, this section provides suggestive evidence that candidacy among pro-social people is affected by the way in which the nature of political office is portrayed. In addition, these treatments have a direct bearing on the in-office behavior of the elected politicians, particularly in the way in which they spend money allocated to them. Finally, there is also limited evidence on treatments shaping citizens' preferences on how councils should act.

6 Conclusion

Under the landmark 18th amendment to the constitution of Pakistan, the first-ever village council elections were held across the Khyber Pakhtunkwa (KP) province in May 2015. While previously citizens only directly elected 125 members of the provincial assembly, they now elect more than 48000 representatives across the province. In addition to looking after local service delivery, the village councils receive funds to undertake development work in their areas.

Who decides to contest these elections potentially shapes the very nature of representation, and what the government delivers to citizens. A common critique of decentralized systems is that local leaders have a lot of control over budgets and decision-making, and may use this power to capture resources intended for other beneficiaries. One way to address this type of behavior would be to encourage people who may be motivated to help the community to run for local political office. But evidence on the efficacy of encouraging people to run for office is thin. Further, we have a limited understanding of the degree to which pro-social

dimensions of candidacy and personal benefits from office motivate people to run, and of whether these incentives determine a politician's performance once they are elected.

We report results from a field experiment in 192 villages where we examine the determinants of candidacy using experimental variation in how political office is portrayed to prospective candidates. We find people are more likely to run when the social benefits of political office, that is, the ability of helping one's community improve its development, are highlighted. In fact, people who are most likely to be encouraged to contest elections are the ones who hold higher ex-ante pro-social motivations. In contrast, personal appeals - those that relate to the ambition and status benefits that accrue to people who hold political office - discourage candidacy and the people who are most likely to be dissuaded are those that are pro-socially motivated. The effects extend to actual elections to village councils as well as the performance of councils a year after they get elected. We find candidates running in response to social versus personal messaging spend village funds in a manner more aligned with the preferences of citizens.

While previous research has used theoretical tools to study strategic decision making vis-a-vis policy platforms (Osborne and Slivinski 1996; Besley and Coate 1997); argued that certain personality types are more likely to be leaders (Judge et al. 2002); and showed that individuals with family members in politics are more likely to run for office (Querubin 2016; Chandra 2016; Cruz et al. 2017), relatively little attention has been paid to individual motivations related to aspects of the political job. Here we provide experimental evidence that mobilization can get randomly sampled people to run, and that the message used to mobilize matters for performance.

The findings are important because they shed light on how the talent pool of politicians can be expanded in consolidating democracies. We know from previous research that effective politicians can improve policy (Ashworth 2012; Levitt 1996; Besley 2005), service provision (Brollo et al. 2013; Chattopadhyay and Duflo 2004; Iyer and Mani 2012), and notions of representation itself (Pitkin 1967). But if only a few people participate in politics, democracy

remains a blunt tool to improve citizen welfare.

The results have implications for the democracy assistance programs run by donor countries. The United States, for instance, spends nearly \$2.5 billion a year on promoting democracy in developing countries (Carothers 2009). We provide, to our knowledge, the first experimental evidence that mobilization programs that focus on providing information on the nature of political office, can have an impact on the number and profile of candidates running for office as well as policy outcomes. Combining the present research with the democracy assistance agenda, therefore, is a promising avenue for future work.

Finally, this experiment lays the groundwork for following a large set of first-time politicians over their political careers. First, how are the newly elected councilors learning on the job? The councilors are elected with a relatively rudimentary set of skills, presenting opportunities for policies that aim to enhance their skills. Second, this large set of elected politicians presents parties with both an opportunity and a challenge. On the one hand, parties for the first time have the opportunity to select from a group of people who have demonstrated electability and a common track record of performance. On the other hand, selecting competent people for party tickets also creates intra-party competition, something that senior party officials are averse to (Myerson 2009). Third, analyzing re-election rates in the next round of elections will help understand whether citizens reward policies that are more aligned with their preferences.

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Figures

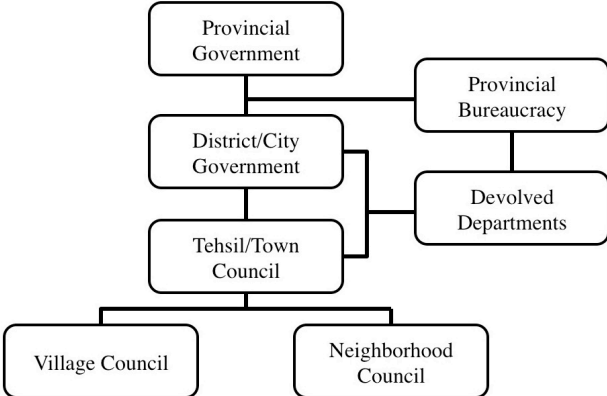


Figure 1: Village Councils in Political Hierarchy



Figure 2: Haripur and Abbottabad in Khyber Pakhtunkwa Province



Figure 3: **Private One-on-One Meetings**



Figure 4: **Public Meetings in Villages**

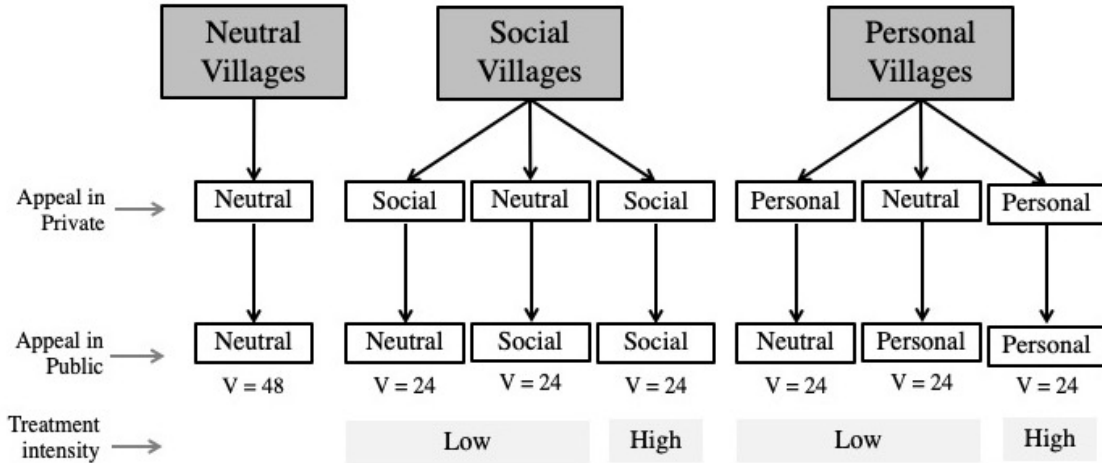


Figure 5: Design of Field Experiment

Notes: This figure shows the randomization scheme. All treatment randomizations are at the village level. V refers to the number of villages in a treatment category. The middle two layers of the figure show the type of appeal made to a person to run for office. ‘Neutral’ refers to an appeal with basic information about running for office. ‘Social’ refers to an appeal comprising the ability to improve services and development in the community. ‘Personal’ refers to an appeal that speaks to direct benefits that can accrue to a person, such as gaining respect and influence. Summary statistics are presented in Appendix C.

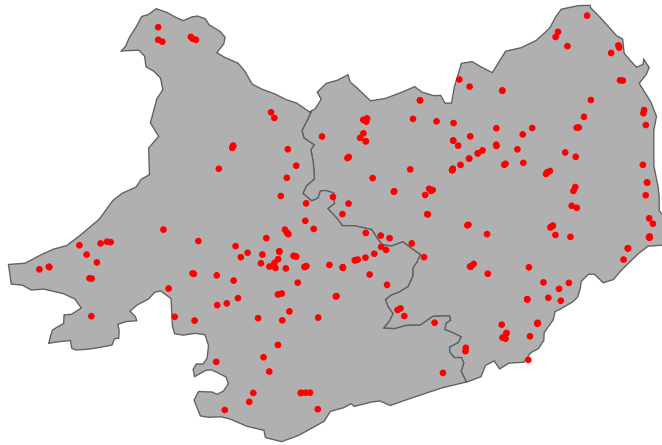


Figure 6: Sampled Villages in Haripur and Abbottabad

Tables

Table I: **Village Level Treatment Allocation**

		Private Treatments		
		Neutral Message	Personal Message	Social Message
Public Treatments	Neutral Message	N. 48	A. 24	D. 24
	Personal Message	B. 24	C. 24	-
	Social Message	E. 24	-	F. 24

Notes: This table presents the experimental design. Each cell reports the number of villages in the relevant treatment condition.

Table II: Overall Balance for Experiment

	Village Pop (1)	Number Settlements (2)	Dist Road (3)	Dist HQ (4)	Dist Teh HQ (5)	Long (6)	Lat (7)	Num Gen Seats (8)
A. Neutral Canvass, Neutral Train	4188.384 (730.981)	2.363 (0.376)	1.738 (2.302)	20.863 (3.432)	12.617 (3.838)	34.055 (0.018)	73.167 (0.023)	6.487 (0.398)
B. Personal Canvass, Neutral Train	3970.954 (689.699)	2.332 (0.439)	-0.094 (2.472)	20.005 (3.304)	13.560 (4.151)	34.047 (0.022)	73.193 (0.024)	6.343 (0.378)
C. Neutral Canvass, Personal Train	4290.217 (670.607)	2.316 (0.422)	4.545 (3.945)	23.819 (3.387)	13.530 (4.363)	34.033 (0.019)	73.167 (0.023)	6.360 (0.370)
D. Personal Canvass, Personal Train	4187.863 (703.264)	2.611 (0.438)	1.862 (4.021)	17.463 (3.288)	8.431 (3.883)	34.033 (0.021)	73.187 (0.024)	6.378 (0.376)
E. Social Canvass, Neutral Train	4947.825 (662.900)	2.431 (0.358)	3.303 (2.331)	15.294 (3.294)	6.982 (3.674)	34.036 (0.021)	73.183 (0.024)	6.795 (0.360)
F. Neutral Canvass, Social Train	3723.231 (647.360)	2.022 (0.397)	2.528 (2.666)	16.009 (3.515)	8.753 (4.061)	34.038 (0.020)	73.181 (0.028)	6.171 (0.361)
G. Social Canvass, Social Train	3811.318 (705.307)	2.132 (0.424)	8.078 (5.398)	13.391 (3.590)	6.528 (4.291)	34.060 (0.022)	73.172 (0.026)	6.184 (0.399)
Hypothesis tests p-values								
Joint orthogonality p-value	0.241	0.848	0.811	0.258	0.422	0.823	0.699	0.298
A-B =0	0.623	0.939	0.517	0.850	0.829	0.708	0.185	0.549
A-C=0	0.799	0.891	0.542	0.525	0.840	0.244	0.994	0.572
A-D=0	0.999	0.540	0.979	0.452	0.298	0.324	0.320	0.645
A-E=0	0.126	0.833	0.601	0.262	0.181	0.425	0.383	0.238
A-F=0	0.194	0.333	0.802	0.314	0.359	0.395	0.589	0.135
A-G=0	0.398	0.537	0.306	0.127	0.132	0.827	0.834	0.225
# Villages	192	192	192	192	192	192	192	192

Notes: This table shows randomization balance by treatment arm. The bottom part reports p-values comparing indicated coefficients. The joint orthogonality test checks if all coefficients are equal.

Table III: Main Effects of Treatments

	Social Vs Personal		Social Vs Neutral		Personal Vs Neutral	
	Filed Papers (1)	Elected to Council (2)	Filed Papers (3)	Elected to Council (4)	Filed Papers (5)	Elected to Council (6)
Any	0.024*** (0.008)	0.015*** (0.005)	0.015* (0.008)	0.008 (0.006)	-0.009 (0.006)	-0.008** (0.004)
Low	0.009 (0.007)	0.006 (0.004)	0.002 (0.008)	0.001 (0.005)	-0.007 (0.006)	-0.005 (0.004)
High	0.038*** (0.013)	0.023*** (0.009)	0.026** (0.013)	0.013 (0.009)	-0.012 (0.008)	-0.010** (0.004)
Neutral Mean	0.030	0.017	0.030	0.017	0.030	0.017
# Observations	9310	9310	9310	9310	9310	9310
# Clusters	192	192	192	192	192	192

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports the effect of treatments on the probability of candidacy and the probability of winning the elections. The table uses a dataset of randomly selected individuals. “Filed Papers” takes a value of one if the individual appears on ballot and zero otherwise. “Elected to Council” takes a value one if the individual wins the election and zero otherwise. Columns (1) and (2) compare the outcomes for groups that receive the social benefits message versus the personal benefits message. Columns (3) and (4) report comparison of outcomes between social benefits message and neutral message, and Columns (5) and (6) report comparison of personal benefits message with the neutral message treatment. The rows specify the intensity of treatment. Row “Any” reports results as per linear restriction on equation 1. We use inverse probability weights in the estimation of this row such that ‘low’ and ‘high’ type villages receive the same weight when calculating the overall effect of making benefits salient. Rows “Low” and “Any” report results from linear combinations as per equation 2. “Any” compares villages receiving the indicated messages in private, in personal, or in both; “Low” refers to villages receiving messages in either private or in public but not both; and “High” refers to villages receiving the benefits message in both private and public. The main regression uses a dataset of 9310 individuals across 192 villages. Each regression uses block fixed effects. Standard errors are clustered at the village level and reported in parentheses. The effective number of observations are 144 villages with 6990 individuals in columns (1) and (2), 120 villages and 5773 individuals in columns (3) and (4), and 120 villages and 5857 individuals in columns (5) and (6).

Table IV: **Citizen Preferences and Actual Spending in Neutral Villages**

Variable	Mean (Percentage)	Std. Dev.	Min.	Max.	N
Panel A: Preferences of Citizens in Neutral Villages					
Municipal Services	65.64	18.76	7.774	96.484	357
Infrastructure	23.201	17.361	0	92.226	357
Community	3.948	5.803	0	31.376	357
Not Primary Responsibility	7.211	9.846	0	39.274	357
Panel B: Actual Spending in Neutral Villages					
Municipal Services	29.59	25.98	0	100	46
Infrastructure	66.07	26.4	0	100	46
Community	2.48	5.82	0	30.77	46
Not Primary Responsibility	1.86	6.18	0	35	46

Notes: This table presents summary statistics in neutral villages of citizen preferences for village budget spending, as well as the actual spending by the village councils. *Municipal Services* include allocation to education, health, water, sewerage and waste disposal. *Infrastructure* includes construction and rehabilitation of roads, streets, and street lights. *Community* includes spending money on sports, graveyard, mosque, and the community center. *Not Primary Responsibility* includes provision of electricity, transport service, security, skills development, and a residual other category. Panel A reports the percentage of the village budget that citizens want to be spent on each category in pure control. Panel B is calculated from a village dataset that comprises actual budget allocations to each category, converted to percentages.

Table V: Distance between Citizen Preferences and Council Budgets

	Euclidean Distance (1)	Municipal Services (2)	Infrastructure (3)	Community (4)	Not Primary Responsibility (5)
Panel A: Social Vs Personal					
Any	-13.049** (5.022)	-7.015* (3.798)	-10.195** (4.123)	-0.403 (1.190)	-4.585* (2.503)
Low	-4.619 (6.059)	-4.240 (4.799)	-4.038 (4.681)	1.370 (1.508)	0.617 (2.256)
High	-21.222*** (8.076)	-9.498 (6.031)	-16.111** (6.806)	-2.193 (1.851)	-10.006** (4.466)
Panel B: Social Vs Neutral					
Any	-7.908 (6.022)	-6.376 (4.650)	-6.369 (4.763)	0.877 (1.271)	0.501 (2.128)
Low	-4.950 (6.477)	-4.912 (5.023)	-4.157 (5.049)	1.626 (1.511)	1.133 (2.247)
High	-10.789 (7.742)	-7.614 (5.966)	-8.552 (6.192)	0.070 (1.454)	-0.401 (2.796)
Panel C: Personal Vs Neutral					
Any	5.140 (5.651)	0.639 (4.348)	3.826 (4.484)	1.280 (1.322)	5.086** (2.476)
Low	-0.297 (5.968)	-0.662 (4.798)	-0.094 (4.595)	0.263 (1.373)	0.542 (2.063)
High	10.424 (6.776)	1.880 (5.086)	7.551 (5.680)	2.260 (1.795)	9.601** (3.971)
Neutral Mean	67.425	42.500	48.448	4.797	7.218
# Observations	189	189	189	189	189

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports the effect of treatments on the performance of elected councils and how it relates to citizen preferences. The table uses a village level dataset that is constructed based on official budget data from the councils and the preferences of citizens regarding the budget. The dependent variable in each column is defined as the quadratic distance between the preferences of citizens regarding the category of budget mentioned in the column header and the actual spending by the council. The distance for each category is calculated using the formula $\sqrt{(B_{ji} - \bar{C}_{ji})^2}$, where B_{ji} refers to the percentage of the budget spent on category j in village i and \bar{C}_{ji} is the average of citizen preferences for spending on that category in village i . The overall difference in column (1) is calculated using the formula: $\sqrt{\sum_J (B_{ji} - \bar{C}_{ji})^2}$. Panel A compares the outcomes for villages that received social benefits message with those that received the personal benefits message. This comprises 144 village councils. Panel B reports comparison of outcomes between social benefits message and neutral message using data from 117 village councils, and Panel C reports comparison of personal benefits message with villages receiving neutral message treatments using data from 118 village councils. The rows specify the intensity of treatment. Row “Any” reports results as per linear restriction on equation 1. We use inverse probability weights in the estimation of this row such that ‘low’ and ‘high’ type villages receive the same weight when calculating the overall effect of making benefits salient. Rows “Low” and “Any” report results from linear combinations as per equation 2. “Any” compares villages receiving the indicated messages in private, in personal, or in both; “Low” refers to villages receiving messages in either private or in public but not both; and “High” refers to villages receiving the benefits message in both private and public. Each regression uses block fixed effects. Robust standard errors are reported in parentheses.

Table VI: **Heterogeneous Response by Pro-Social Type**

<i>Pro-social type level:</i>	Social Vs Personal			Social Vs Neutral			Personal Vs Neutral		
	Low only (1)	High only (2)	Diff: (2)-(1) (3)	Low only (4)	High only (5)	Diff: (5)-(4) (6)	Low only (7)	High only (8)	Diff: (8)-(7) (9)
Any	0.010 (0.007)	0.035*** (0.010)	0.025** (0.011)	0.007 (0.008)	0.020* (0.012)	0.013 (0.013)	-0.003 (0.006)	-0.014* (0.009)	-0.012 (0.010)
Low	0.007 (0.008)	0.012 (0.010)	0.007 (0.012)	0.004 (0.009)	0.003 (0.011)	0.000 (0.013)	-0.003 (0.005)	-0.009 (0.009)	-0.007 (0.010)
High	0.013 (0.011)	0.055*** (0.017)	0.041** (0.018)	0.011 (0.010)	0.035** (0.017)	0.024 (0.018)	-0.002 (0.009)	-0.020* (0.010)	-0.017 (0.011)
# Observations	9310	9310	9310	9310	9310	9310	9310	9310	9310
# Villages	192	192	192	192	192	192	192	192	192

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports heterogeneous effects by pro-social types. The dependent variable is whether a person ran for office. Type is measured by taking the mean response to the extent to which respondents agree with the following statements on a scale of 1 to 5: ‘Elected representatives serve people by solving their problems’; ‘Helping others brings peace’; ‘Publicly provided services are very important for ordinary people’; ‘Improving village schools is directly linked to the performance of public representatives’. We cut the mean of the responses at the median such that people whose responses are below the median are classified as “low” types, while people whose responses are above the median are classified as “high” types. Columns report regressions results restricting attention to low types and high types separately, before reporting the difference between them for each comparison. The rows specify the intensity of treatment. Row “Any” reports results as per a linear restriction on equation 1. We use inverse probability weights in the estimation of this row such that ‘low’ and ‘high’ type villages receive the same weight when calculating the overall effect of making benefits salient. Rows “Low” and “Any” report results from linear combinations as per equation 2. “Any” compares villages receiving the indicated messages in private, in personal, or in both; “Low” refers to villages receiving messages in either private or in public but not both; and “High” refers to villages receiving the benefits message in both private and public. The main regression uses a dataset of 9310 individuals across 192 villages, however the linear restrictions as specified in section 4.1 restricts the data to 144 villages with 3504 individuals in column (1), 144 villages and 3486 individuals in column (2), 144 villages and 6990 individuals in column (3); to 120 villages with 2723 individuals in column (4), 120 villages and 3050 individuals in column (5), 120 villages and 5773 observations in column (6); and to 120 villages with 2887 individuals in column (7), 120 villages and 2970 individuals in column (8), and 120 villages and 5857 individuals in column (9). Each regression uses block fixed effects. Standard errors are clustered at the village level and reported in parenthesis.

Table VII: Budget Spending

	Municipal (1)	Infrastructure (2)	Community (3)	Not Primary Responsibility (4)
Panel A: Social Vs Personal				
Any	9.257** (3.947)	-8.151** (3.982)	1.182 (0.956)	-2.288 (1.805)
Low	6.380 (4.861)	-5.652 (4.710)	1.234 (1.119)	-1.962 (1.754)
High	12.204* (6.326)	-10.550 (6.571)	1.070 (1.530)	-2.724 (3.367)
Panel B: Social Vs Neutral				
Any	5.154 (4.885)	-4.703 (4.875)	0.166 (1.171)	-0.617 (1.227)
Low	4.150 (5.369)	-3.661 (5.354)	0.310 (1.256)	-0.799 (1.262)
High	6.346 (5.868)	-5.865 (5.706)	-0.039 (1.541)	-0.442 (1.393)
Panel C: Personal Vs Neutral				
Any	-4.103 (4.676)	3.448 (4.841)	-1.016 (0.999)	1.671 (1.944)
Low	-2.242 (4.902)	2.001 (4.940)	-0.924 (1.079)	1.165 (1.938)
High	-5.855 (5.802)	4.681 (6.305)	-1.109 (1.148)	2.283 (3.273)
Neutral Mean	29.590	66.069	2.476	1.865
# Observations	189	189	189	189

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table tests whether the treatments led to a change in proportion of budget spent in different category compared to the neutral villages. Panel A compares the outcomes for villages that received social benefits message with those that received the personal benefits message. This comprises 144 village councils. Panel B reports comparison of outcomes between social benefits message and neutral message using data from 117 village councils, and Panel C reports comparison of personal benefits message with villages receiving neutral message treatments using data from 118 village councils. The rows specify the intensity of treatment. Row “Any” reports results as per linear restriction on equation 1. We use inverse probability weights in the estimation of this row such that ‘low’ and ‘high’ type villages receive the same weight when calculating the overall effect of making benefits salient. Rows “Low” and “Any” report results from linear combinations as per equation 2. “Any” compares villages receiving the indicated messages in private, in personal, or in both; “Low” refers to villages receiving messages in either private or in public but not both; and “High” refers to villages receiving the benefits message in both private and public. Each regression uses block fixed effects. Robust standard errors are reported in parentheses.

Table VIII: **Citizen Preferences on Budgetary Spending**

	Municipal (1)	Infrastructure (2)	Community (3)	Not Primary Responsibility (4)
Panel A: Social Vs Personal				
Any	0.613 (3.165)	2.316 (2.304)	0.034 (1.223)	-2.963 (2.209)
Low	-2.685 (3.438)	1.210 (2.741)	1.007 (1.455)	0.468 (2.218)
High	4.095 (5.436)	3.534 (3.875)	-1.003 (1.999)	-6.626* (3.816)
Panel B: Social Vs Neutral				
Any	-0.157 (3.732)	-1.423 (3.077)	0.744 (1.309)	0.836 (2.121)
Low	-0.337 (3.742)	-1.183 (3.279)	0.643 (1.483)	0.877 (2.228)
High	0.128 (4.957)	-1.556 (3.680)	0.853 (1.577)	0.575 (2.968)
Panel C: Personal Vs Neutral				
Any	-0.770 (3.597)	-3.739 (3.076)	0.711 (1.342)	3.799* (2.120)
Low	2.336 (3.680)	-2.397 (3.097)	-0.361 (1.328)	0.421 (1.957)
High	-3.960 (4.596)	-5.088 (3.843)	1.854 (1.856)	7.194** (3.094)
Neutral Mean	65.528	23.044	4.178	7.250
# Observations	192	192	192	192

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table tests whether the treatments led to a change in preferences of citizens. The table uses a village level dataset that is constructed by averaging the preferences of citizens collected through a survey. The table reports the effect of treatments on aggregate preferences compared to the neutral villages. Panel A compares the outcomes for villages that received social benefits message with those that received the personal benefits message. This comprises 144 village councils. Panel B reports comparison of outcomes between social benefits message and neutral message using data from 120 village councils, and Panel C reports comparison of personal benefits message with villages receiving neutral message treatments using data from 120 village councils. The rows specify the intensity of treatment. Row “Any” reports results as per linear restriction on equation 1. We use inverse probability weights in the estimation of this row such that ‘low’ and ‘high’ type villages receive the same weight when calculating the overall effect of making benefits salient. Rows “Low” and “Any” report results from linear combinations as per equation 2. “Any” compares villages receiving the indicated messages in private, in personal, or in both; “Low” refers to villages receiving messages in either private or in public but not both; and “High” refers to villages receiving the benefits message in both private and public. Each regression uses block fixed effects. Robust standard errors are reported in parentheses.

FOR ONLINE PUBLICATION: APPENDIX

Contents

- A Details of How Candidacy is Declared ii
- B Linkages and Deviations from Pre-Analysis Plan ii
- C Summary Statistics and Balance Tables iv
 - C.1 Decomposing the Treatment Effects viii

A Details of How Candidacy is Declared

To be eligible to run for election, prospective politicians must fill out a candidacy form, as well as declare income sources and wealth. Defaulters of bank loans and public servants are not eligible to contest the elections. The process is summarized as follows:

1. Collect and fill the candidacy declaration form, which includes details of at least two people who endorse the candidacy of the interested person
2. Prepare an affidavit, endorsed by a Public Notary, declaring that the candidate has not been a defaulter
3. Prepare an income and wealth declaration
4. Deposit a fee of Rs. 1000 (USD \$10) through a bank draft
5. Attach certified copies of educational certificates and the national identity card

B Linkages and Deviations from Pre-Analysis Plan

In this report, we summarize how our analysis relates to the Pre-Analysis Plan (PAP) that was specified before candidacy data was delivered to the authors. The PAP is registered at AEA RCT Registry (0000685) and at EGAP (20151102AA). Below, we report on linkages and deviations from the PAP by using the same section headings used in the PAP.

Experiment

The registered PAP is a comprehensive document for three separate experiments that were built into the design of the overall study. The focus of this paper is only on one of the experiments titled “Experiment 1” on page 12 in the PAP that relates to the one-on-one and public meetings in villages. The other experiments are smaller in nature and they are always cross-randomized so that their effects are differenced out.

Data collection

All data used in the paper are available to the authors after the PAP is registered. The results on candidacy and election in Section 4 are pre-registered. The results on performance effects reported in Section 4.3 are not pre-registered. However, that section makes use of administrative data on budgets and the collection of those data commenced before we had the final administrative data on elections in hand. In addition, we were not aware of the effects on candidacy and election by the time we started collecting data on performance.

Variables

In Table A1 we report a mapping of main variables used in the paper with the relevant section of the PAP. As noted in the PAP, we also collect a host of outcomes for the candidate pool. However, a previous round of reviewer and editor comments recommended that we drop analysis on those outcomes as they were collected post-treatment. Consequently, in the present paper, we focus only on the main (primary) outcomes that were pre-registered: candidacy and election to council as measured through administrative data. Specifically, we measure candidacy with variable 4 (page 16 of PAP), which is not self-reported and is retrieved directly from the election commission. Using the administrative measure of candidacy removes possibility of survey response bias and allows comparisons with the probability of getting elected, which is only measured in administrative data.

In addition, we have made a change to the label of the Personal Benefits treatment which was labelled as Career Benefits in the PAP. The new label more accurately reflects the nature of the treatment.

Our main measure of pro-sociality (the type of candidate) was collected before treatments were administered and was pre-registered as an exploratory moderator.

Analysis

Table A1 shows the mapping of our outcomes to pre-registration status. Overall, the results reported in Section 4 are pre-registered and correspond to Analysis 2.1 in the PAP. This analysis focuses on the relevant sample for this experiment, that is, 9310 people approached in 192 villages.

The village level performance outcomes in Section 4.3 are not pre-registered but correspond to Analysis 2.5 in the PAP, that relates to calculating village level effects.

Table A1: **Mapping of Variables from main tables with Pre-Analysis Plan**

Variable	Type	Tables in Paper	Registered in PAP	PAP Section 5 variable #
Filed papers	Outcome	Table III	Yes	4
Elected to council	Outcome	Table III	Yes	5
Budget categories	Outcome	Table V	No	
	Outcome	Table VII	No	
	Outcome	Table VIII	No	
Pro-Social Type	Pre-treatment	Table VI	Yes	19

Notes: This table reports the mapping of main outcomes to PAP.

Hypotheses

Guided by Olken (2015), we consider effects on ‘primary outcomes’ of interest in Section 4: the results on candidacy and election to council. As noted above, other non-primary moderators are collected post treatment and as per the recommendation of previous reviewers and editor, we do not include them in the analysis.

The overall effect of comparing social and personal arms is pre-registered. The decomposition of these effects by high and low intensity is not pre-registered.

C Summary Statistics and Balance Tables

Table A2: Summary Statistics of Candidacy Stage Variables

Variable	Mean	Std. Dev.	Min.	Max.	N
Main Outcomes					
Filed Papers	0.03	0.16	0	1	9310
Elected to Council	0.02	0.13	0	1	9310
Village Characteristics					
Village Population	4366.505	1875.097	1831	12489	192
Number of Settlements	2.224	1.574	1	11	192
Distance to main road	8.105	16.944	0.5	100	192
Distance to District HQ	26.654	19.516	2	165	192
Distance to County HQ	22.872	17.575	1	110	192
Longitude	34.053	0.126	33.776	34.356	192
Latitude	73.120	0.222	72.593	73.489	192
Number of General Seats	6.073	0.957	5	10	192

Notes: This table reports summary statistics of data used in sections 4.

Table A3: Summary Statistics for Budget and Citizen Preferences

Variable	Mean	Std. Dev.	Min.	Max.	N
Citizens' Preferences					
Municipal	65.429	17.444	7.774	100	1318
Infrastructure	21.655	14.592	0	92.226	1318
Community	4.604	6.934	0	36.988	1318
Not Primary Responsibility	8.311	11.02	0	48.75	1318
Budget Spending					
Municipal	30.692	25.019	0	100	189
Infrastructure	64.846	25.268	0	100	189
Community	2.234	5.459	0	30.769	189
Not Primary Responsibility	2.228	9.012	0	80	189

Notes: This table reports summary statistics of data used in section 4.3. Three Village Councils did not prepare a budget due to gridlock. Table A4 provides evidence that treatments do not predict missing data.

Table A4: **Budget Data Missingness Balance**

	Budget Missing (1)
A. Neutral Canvass, Neutral Train	0.028 (0.024)
B. Personal Canvass, Neutral Train	0.026 (0.038)
C. Neutral Canvass, Personal Train	-0.014 (0.012)
D. Personal Canvass, Personal Train	-0.016 (0.013)
E. Social Canvass, Neutral Train	-0.012 (0.010)
F. Neutral Canvass, Social Train	-0.014 (0.012)
G. Social Canvass, Social Train	-0.014 (0.012)
Hypothesis tests p-values	
Joint orthogonality p-value	0.803
A-B =0	0.961
A-C=0	0.151
A-D=0	0.150
A-E=0	0.150
A-F=0	0.149
A-G=0	0.149
# Villages	192

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table checks for balance in missingness of data used in Table V. The dependent variable takes a value of 1 if data is missing and zero otherwise. We are missing budget data from three villages out of a sample of 192. The regression uses robust standard errors and are reported in parentheses. All regressions include block fixed effects.

Table A5: **Balance Test: Pro-social Type**

	Low (1)	High (2)
Neutral Canvass, Neutral Train	0.338 (0.058)	0.662 (0.058)
A. Personal Canvass, Neutral Train	0.396 (0.061)	0.604 (0.061)
B. Neutral Canvass, Personal Train	0.400 (0.056)	0.600 (0.056)
C. Personal Canvass, Personal Train	0.422 (0.064)	0.578 (0.064)
D. Social Canvass, Neutral Train	0.441 (0.059)	0.559 (0.059)
E. Neutral Canvass, Social Train	0.347 (0.063)	0.653 (0.063)
F. Social Canvass, Social Train	0.358 (0.067)	0.642 (0.067)
# Villages	192	192
# Observations	9310	9310
Joint orthogonality p-value	0.568	0.568

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table tests if the distribution of low and high pro-social types is balanced across treatment arms. Type is measured by taking the mean response to the extent to which respondents agree with the following statements on a scale of 1 to 5. ‘Elected representatives serve people by solving their problems’; ‘Helping others brings peace’; ‘Publicly provided services are very important for ordinary people’; ‘Improving village schools is directly linked to the performance of public representatives’. We cut the mean of the responses at the median such that people whose responses are below the median are classified as “low” types, while people whose responses are above the median are classified as “high” types. Therefore, the outcomes in columns 1 and 2 are just reverse coded versions of each other. The rows specify the intensity of treatment. Standard errors are clustered at the village level and reported in parentheses. All regressions include block fixed effects. Joint orthogonality test checks if all the treatment coefficients are equal.

C.1 Decomposing the Treatment Effects

Table A6: Decomposing Results by Treatment Arm

	Attended Pub Meet (1)	Filed Papers (2)	Elected to Council (3)
A. + C. Personal Private	0.024 (0.022)		
D. + F. Social Private	0.029 (0.023)		
A. Personal Private, Neutral Public		-0.005 (0.007)	-0.004 (0.005)
B. Neutral Private, Personal Public		-0.009 (0.006)	-0.007* (0.003)
C. Personal Private, Personal Public		-0.012 (0.008)	-0.010** (0.004)
D. Social Private, Neutral Public		-0.009 (0.009)	-0.002 (0.006)
E. Neutral Private, Social Public		0.013 (0.012)	0.004 (0.006)
F. Social Private, Social Public		0.026** (0.013)	0.013 (0.009)
Control Mean	0.815	0.030	0.017
# Villages	192	192	192
# Observations	9310	9310	9310
Hypothesis Tests p-values:			
Personal Message:			
Private Only ($A = 0$)		0.480	0.360
Public Only ($B = 0$)		0.141	0.055
Both ($C = 0$)		0.145	0.025
Private Any ($A + C = 0$)	0.259	0.194	0.068
Private Marginal ($A + C - B = 0$)		0.462	0.210
Public Any ($B + C = 0$)		0.092	0.019
Public Marginal ($B + C - A = 0$)		0.122	0.038
Personal Overall ($A + B + C = 0$)		0.137	0.046
Social Message:			
Private Only ($D = 0$)		0.306	0.705
Public Only ($E = 0$)		0.280	0.532
Both ($F = 0$)		0.039	0.130
Private Any ($D + F = 0$)	0.882	0.311	0.355
Private Marginal ($D + F - E = 0$)		0.812	0.568
Public Any ($E + F = 0$)		0.036	0.135
Public Marginal ($E + F - D = 0$)		0.007	0.089
Social Overall ($D + E + F = 0$)		0.188	0.309
Social vs Personal Message:			
Private Only ($D - A = 0$)		0.655	0.780
Public Only ($E - B = 0$)		0.054	0.072
Both ($F - C = 0$)		0.004	0.010
Private Any ($(D + F) - (A + C) = 0$)	0.220	0.033	0.027
Private Marginal ($(D + F - E) - (B + C - A) = 0$)		0.548	0.266
Public Any ($(E + F) - (B + C) = 0$)		0.001	0.001
Public Marginal ($(E + F - D) - (B + C - A) = 0$)		0.001	0.010
Overall Social vs Personal Message:			
$((D + E + F) - (A + B + C) = 0)$		0.004	0.005

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the village level and reported in parentheses. All regressions include block fixed effects. Dependent variable in column (1) takes value of 1 if the individual attended the public meeting held in the village and zero otherwise. Column (2) reports results for candidacy, where the dependent variable takes the value 1 a person ran for office, and zero otherwise. Dependent variable in Column (3) takes value 1 if a person got elected to the village council and 0 otherwise. The first part of the table breaks down the treatment effects by different combinations of treatments and reports standard errors clustered at the village level in parenthesis. The second part of the table reports p-values of various hypothesis tests that test various treatment conditions against each other.