Social Motivation, Political Candidacy, and Performance: Experimental Evidence From Pakistan*

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Abstract

While interventions to improve democratic performance generally focus on incentives of politicians who have already been elected, there is limited evidence on how changes in the supply of politicians might impact policy. We experimentally vary how political office is portrayed to ordinary citizens and find that emphasizing pro-social motives instead of personal benefits of holding office – such as the ability to help others versus enhancing one's own respect and status – raises the likelihood that individuals run for office and that voters elect them, and improves the alignment of subsequent policies with citizens' preferences. These effects arise when encouragements are delivered in public but not private meetings, suggesting that social context particularly shapes the political class.

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1 Introduction

Many democratic governments have failed to deliver effectively to populations that elect them. While, scholars have spent considerable effort in examining how democracies may be improved through politicians who have already been elected,[1] what is often missed, and no less important, is how improving the *supply* of politicians affects democratic performance. Who runs for political office affects policy, independent of, and prior to the rules under which politicians operate once elected. This question is perhaps of central importance in developing countries where "bad" politicians may dominate the political class.[2]

How can we get "good" politicians – those that will carry out policy that is responsive to citizen preferences (Dahl 1973; Lipset 1959; Caselli and Morelli 2004; Besley and Coate 1997) – to enter politics? Theoretical work on political entry suggests that answering this question is not straightforward for at least two reasons. First, it is unclear who might be a 'good' politician to begin with. A key puzzle in the literature – and the one where we provide new evidence – relates to question of what motivations might enable political entry of prospective candidates who benefit society at large (Dal Bó and Finan 2018; Bandiera et al. 2019). For instance, we might want to encourage people motivated by ego-rents like respect, status, and influence (Caselli and Morelli 2004; Gagliarducci and Nannicini 2013) to be in office because not only can societal improvement align with their career ambitions (Schlesinger 1966), but such people may also posses higher 'ability' and 'competence' (Dal Bó et al. 2017; Ashraf et al. 2020). On the other hand, people motivated to help improve their communities are perhaps best suited for a political job as they will derive private utility from improving outcomes for broader society (Besley and Ghatak 2005).

Second, even if we agree on the types of people we want in politics – defined by the motivations they carry – it remains unclear how such people may be mobilized into running for political office (Blair et al. 2019). While, highlighting personal benefits

[1] See, for example, Ferraz and Finan (2011a); Pande (2011); Humphreys and Weinstein (2012); Gagliarducci and Nannicini (2013); Martinez-Bravo et al. (2017); Grossman and Michelitch (2018); Dunning et al. (2019); Arias et al. (2019).

[2] For example, politicians may not be representative (Cruz et al. 2017; Chattopadhyay and Duflo 2004; Querubin et al. 2016; Butler and Nickerson 2011; Broockman and Skovron 2018); motivated by private returns (Fisman et al. 2014; Ferraz and Finan 2011b; Reinikka and Svensson 2004; Prakash et al. 2019; Eggers and Hainmueller 2009; Folke et al. 2017); and have criminal backgrounds (Vaishnav 2017; Blaydes 2010).

can crowd out socially-minded people from running for office (Bénabou and Tirole 2006; Deci 1971; Frey 1997), they can also induce more competent, perhaps career-minded, people to seek political office (Schlesinger 1966; Ferraz and Finan 2011b). Similarly, social motivations can prime more socially minded people to seek office (Deci 1972; Besley and Ghatak 2005), but they may also give public cover to more personally-motivated people to run for office.

We provide new evidence on these puzzles through a field experiment in Pakistan where randomly-sampled citizens are encouraged to consider running for new village councils. Encouragements vary in how political office is portrayed to prospective candidates: in some villages the ability to help the community through elected office is emphasized (*social villages*), while, in other villages, personal benefits of political office, like enhancing one's respect and status, are made salient (*personal villages*).[3] We study the impacts of these encouragements on candidacy decisions, voting decisions, and, perhaps most importantly, the alignment of subsequent policy outcomes with the preferences of the electorate.

We begin by studying political entry decisions of citizens and show that when politics is portrayed as enabling community-minded policy, relative to yielding personal benefits, people are more likely to run for office. Furthermore, this average increase is larger for people who are more pro-social to begin with. This first result shows that how politics is portrayed to ordinary citizens can be an important determinant of who becomes a political candidate.

Next, we ask if voters care to elect these new politicians to office? Improving policymaking requires both that supply of politicians improves, but also that voters demand these politicians by voting them into office. It could be the case that deviations from status-quo candidacy do not matter as the new politicians never stood a chance of getting elected by voters. To the contrary, we find that people who run with social versus personal encouragements do in fact get elected, suggesting that while these people are electable, we do not see them in office because they do not put themselves forward as candidates. Moreover, voters too are more likely to choose pro-social candidates as their elected representatives, suggesting that voters care for the pro-sociality of the political class.

[3]Our formulation of treatments in the political sphere are inspired by Ashraf et al. (2020) who examine bureaucratic recruitment and demonstrate that career benefits attract talented individuals to apply for a new health care position in Zambia.

Finally, we return to the question of whether mobilizing candidates actually affects policy (Fearon 1999)? By benchmarking actual policy decisions made by elected politicians against the preferences of the electorate, we ask if social versus personal messaging align or widen the gap between the two? We find that in villages where people are encouraged to run to help their community instead of helping themselves, official spending is significantly more aligned with citizen preferences up to two years after the elections. We further show that this alignment is explained by changes in politicians' decisions on budgets instead of changes in citizen preferences themselves (which remain the same). This result on policy alignment provides direct evidence that social versus personal encouragements are yielding 'good' politicians to office.[4]

In addition to the main results on candidacy, voting, and policy alignment, we provide further evidence that helps us unpack one mechanism of the candidacy decision. Besides social versus personal encouragements, we also randomize whether these encouragements are provided (i) only in private one-on-one conversations to prospective candidates, or (ii) only in public meetings attended by other people in the village. Comparing these treatments to villages where meetings are held similarly but social or personal encouragements are not provided allows us to understand whether social versus personal motivations to seek political office interact with how prospective candidates evaluate the desirability those motivations in their communities (Tankard and Paluck 2016; Bursztyn and Jensen 2017). We find that social versus personal messaging increase candidacy, election, and policy alignment primarily when encouragements are provided in public but not in private. One interpretation of this result is that instead of providing new information to prospective candidates about what they can achieve through political office, social versus personal encouragements are operating through external channels where public signaling, common knowledge, and/or community coordination are important (Ashraf and Bandiera 2018). This result highlights how getting people who will perform well on policy to run may require an examination and appreciation of the social norms around the candidacy decision.

The new local government reform in Pakistan – the fifth most populous country – provides a good testing ground for this research. As opposed to state or national levels where other factors like parties and donors are perhaps more important, political entry decisions at the local level provide evidence on how one might broaden the

[4]In this sense, our approach to studying policy outcomes is consistent with political agency models like Besley (2006).

composition and performance of the political class (Martinez-Bravo 2014; Martinez-Bravo et al. 2017), potentially nurturing leaders for higher level politics at the very first step of the political career ladder. Building a base of evidence on political entry at the local level is therefore an important precursor to understanding the political pipeline.[5] Similar considerations are at play in many countries that have recently undertaken reforms to bring elected government closer to citizens, with the hopes that local policy can be made more responsive to citizen preferences.[6]

Social scientists have spent considerable energy to build a body of knowledge on how to move democracies to be more responsive to citizens.[7] While, prior work on aligning citizen preferences with policy tends to focus on the performance of politicians already in office, to our knowledge we report results from the first field experiment that mobilizes politicians and examines subsequent policy responsiveness. In doing so our work complements recent studies that show how representation can improve policy outcomes (Fujiwara 2015; Chattopadhyay and Duflo 2004).

The political economy literature has long examined which incentives are likely to yield politicians that are better at aligning policy with citizen preferences (Besley 2005; Caselli and Morelli 2004; Besley and Ghatak 2005). While previous work on political (Gagliarducci and Nannicini 2013) and bureaucratic selection (Ashraf et al. 2020) shows that pecuniary (Dal Bó et al. 2013) and career incentives (Bertrand et al. 2020) matter in recruiting agents who are more competent, and thus may improve policy, to our knowledge, our examination of pro-social motivation remains understudied. This is identified as an open question in a recent review of the political selection literature that says "while we have made progress in documenting some

[5]Indeed, 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."

[6]The most recent example of this is the case of Nepal that, following a large civil war, established a republic and elected local governments across the country in 2017. Another example is Kenya which also passed a recent local government reform. In addition, the institutional details of the reform in Pakistan are similar to many other systems, including Gram Panchayats in India as well as non-party elections of school boards in the United States.

[7]Both Dahl and Lipset famously recognized that an important element of a good democracy is the government's ability and willingness to carry out policies that are aligned with constituent preferences. Dahl (1973) described a democracy as a government that "continue[s] over a period of time to be responsive to the preferences of its citizens" (p. 2). Lipset (1959) wrote that "Democracy...[is] a political system which supplies regular constitutional opportunities for changing the governing officials, and a social mechanism which permits the largest possible part of the population to influence major decisions by choosing among contenders for political office" (p. 45).

of the financial rewards of political office and how they affect political selection, we are still missing evidence on non-financial returns. Motives such as prestige or the desire to perform one's civic duty could play even larger roles in determining selection patterns" (Dal Bó and Finan 2018:p 566).

Further, "finding (such) ways to leverage non-pecuniary incentives for politicians may be particularly important in poor countries" (Bandiera et al. 2019:p. 8). In such conditions aligning an individual's motivations with the organizational mission can compensate for low powered incentives (Besley and Ghatak 2005). For instance, highlighting career concerns using job advertisements has been shown to be an effective strategy in recruiting talented bureaucrats in Zambia and Uganda (Ashraf et al. 2020; Deserranno 2019). Similarly, non-financial rewards have been shown to boost performance particularly on pro-social tasks (Ashraf et al. 2014). In the same spirit, our evidence from Pakistan shows that mobilizing citizens on pro-social aspects of political office can motivate candidacy and make politicians more responsive to citizens.

More broadly, contrary to the folk theory that people are primarily selfish, this paper also relates to a large body of literature spanning several disciplines that argues that intrinsic motivations such as pro-sociality and warm glow can shape civic and cooperative behavior (Andreoni 1990; Frey 1997; Andreoni 1995a; Broockman 2013; Bénabou and Tirole 2006). In this spirit, our research first extends prior work on how pro-social motivations can be mobilized (Blair et al. 2019) by extending analysis to the political class (Ravanilla 2016; Landmann and Vollan 2020), perhaps one of the most important agents of policy change. Second, our study brings field experimental evidence to demonstrate how messaging on pro-social features of political office can enhance coordination among voters around pro-social candidates, a question previously explored extensively in public goods games in lab studies (Andreoni 1995b; Ostrom 2000).

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, and provides a brief description of

status-quo politics.

2.1 The Local Government Reform of 2015

Local government reforms in Pakistan have been carried out by military regimes starting with dictator General Ayub Khan in 1962, usually with the aim of weakening the role of political parties over local politics. Consequently, 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 formal party 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" under which Village Council elections were held on May 30th, 2015. As shown in Figure 1, Village Councils (together with Neighborhood Councils for urban areas), constitute the lowest tier of local government. We conduct our experiment in Haripur and Abbottabad districts as shown in Figure 2. These districts have slightly better health, education, and public service outcomes compared to the provincial averages (MICS 2008). As we describe below, these districts were chosen once we identified a local partner.

2.2 Village Councils

We focus on village councils that represent about 6,500 voters on average (see Table A2 for descriptives).[8] Consistent with local elections in South Asia, voter turnout in 2015 was high at around 76% signaling their importance to local communities. This is about 21 percentage points higher than the national turnout in the general elections of 2013.

[8]The last local government reforms, between 2003 and 2007, brought local government to the Union Council level, an electorate of about 26,000

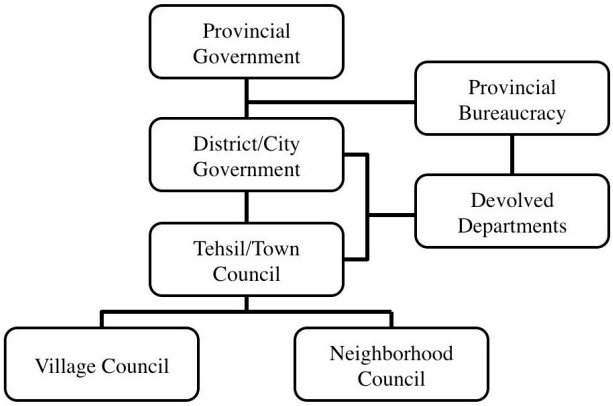

Figure 1: **Village Councils in Political Hierarchy**

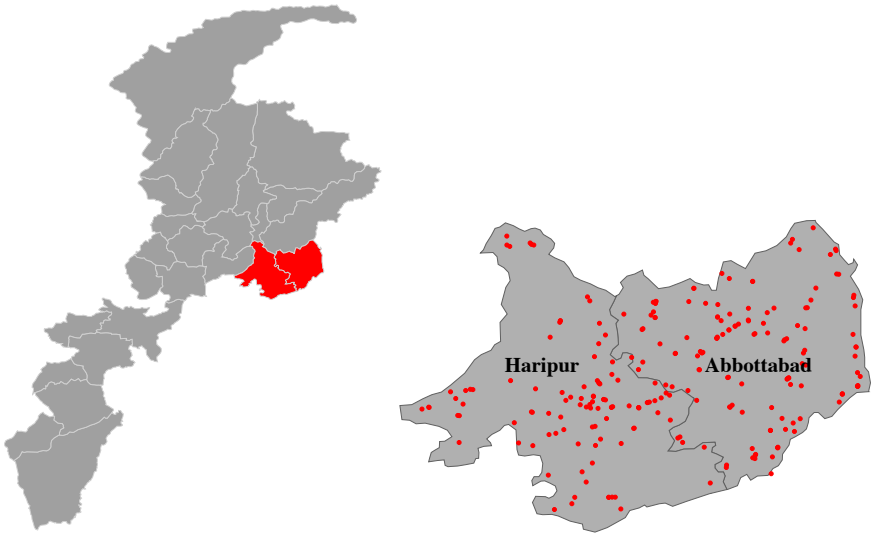

Figure 2: **Villages in Haripur and Abbottabad Districts (right) in Khyber Pakhtunkwa Province (left)**

Responsibilities Much like the rest of the developing world, local governments in KP have two major sets of responsibilities. The first, more substantial, responsibility relates to the council's annual budget. Each year village councils are allocated money by the province, based on a formula codified in law. The median budget allocation per Village council in our sample is approximately $20,000. The council has to decide how and where to spend the money. Each council draws up an annual budget, deciding which projects to undertake. Council members also oversee the implementation of these projects. Second, more informally, council members can also take up any issues that are of concern to their constituents. Related to this, council members look after the provision of public services in the village provided by the provincial government departments, such as health and education. This role is limited as the law only empowers the council to informally report on the performance of service providers without giving them any sanctioning authority.

Composition 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 an at-large constituency comprising the village. Any eligible person can run for the election on a general seat, while the reserved seats require the candidate to meet specific criteria. The number of open seats varies between five and ten, depending on the population of the village. Each village also has two women, one youth (less than 30 years of age), one farmer/worker, and one minority seat that is reserved.

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 seats. 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.

Candidacy All adults over the age of 21 that are eligible to vote can contest village elections. While there are no explicit restrictions, other than no criminal record and a clean financial history, the process of declaring candidacy requires an ability to navigate the bureaucratic apparatus. As described in detail in the Appendix A, citizens have to collect candidacy papers, prepare legal declarations, and deposit approximately USD 10 through bank draft to have their candidacy accepted. In this sense,

candidacy outcomes are costly actions that citizens take after careful deliberation.

Role of Parties Elections for Village Councils were conducted on a non-party basis. This barred political party workers from using the party name and platform in campaigns. As discussed earlier, however, all political parties lack representation at the village level given the historical milieu in which they have developed. Thus, while unofficially, some candidates invoked party platforms, there was limited *systematic* involvement of political parties in village elections.

Overall, studying village council elections is important for at least two reasons. First, local elections introduce principles of democratic representation at the most local level, brining elected government closer to citizens. Previous work shows that this carries important consequences for what democracy delivers to citizens, particularly in South Asia (Chattopadhyay and Duflo 2004; Gulzar et al. 2020). Second, local elections provide opportunities for local prospective politicians to get hands-on training in politics, and to appear on party platforms for subsequent elections. In fact, during fieldwork party leaders stated that village elections enabled them to identify viable and high performing candidates for party nominations in subsequent elections. Thus, studying candidacy at the local level is the first step in understanding the broader pipeline of political candidates.

3 Experiment

We design an experiment in 192 randomly sampled villages of Haripur and Abbottabad districts. There are two key variations in the experiment: how political office is portrayed through whether invitations emphasize social or personal benefits, and whether the portrayal is varied in private or public. In this section we i) explain how field activities unfolded ii) describe the treatments, and iii) present details of the randomization. Appendix Section D presents a timeline of the project. Finally, Appendix section E we discuss ethical considerations with respect to the experiment.

3.1 Public and Private Meetings

Activities on the ground proceed as follows. First, the **private meetings**. A pair of enumerators from our partner NGO Sangum canvass on average 48 households se-

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

lected via random walk in every village for a total of 9,310 people across 192 villages in the experimental sample.[9] Once a household is approached, enumerators conduct a short survey with a male respondent.[10] This means the experimental sample represents the village community instead of picking people who have expressed a particular desire for political office. After the survey, enumerators deliver a neutral, personal, or a social encouragement in this private one-on-one meeting to the subject (see section 3.2 for description). Finally, subjects are invited to a public meeting in the village, and the time and location details for these are shared.[11] 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. 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

[9]Sangum was chosen with the help of a network of community organizations who identified Sangum Development Organization, an able NGO headquartered locally with a long history of implementing community level programs.

[10]Appendix B discusses the details of sampling, the challenges around working at the household level, and effectively yielding a male sample.

[11]It is made clear that the public meeting is open to others who may be interested in finding out more about the upcoming elections. We 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 public meetings. Table A15 shows that there is no evidence for differential selection into the public meeting by social versus personal treatments.

Figure 4: **Public Meetings in Villages**

discussed in section 3.2. As participants arrive, enumerators note their attendance. Then the public meetings begins and a social, personal, or neutral encouragement is offered to participants to run for office (more details below). Figure 4 shows examples of these sessions in three villages. On average, a public session lasts 30-40 minutes in the village.

Overall, the variation in delivering messages in public and private gives us leverage to study the question of how internal and external dynamics affect the candidacy decision.

3.2 Treatments on how Political Office is Portrayed

During the private and public meetings we vary how political office is portrayed in conversation with prospective politicians. There are three types of conversations: neutral, social, and personal.

During *private* one-on-one conversations that we described above, a **neutral message** provides basic information about when elections are going to be held as well as the eligibility criteria for candidacy. Critically this neutral message is always included in a conversation, and can thus be thought of as a premise for having a conversation with people. On top of a neutral message, some conversations, labeled **social messages**, portray political office as a vehicle for improving the quality of government services in the village, as well as working for the welfare of the community more broadly. Similarly, in addition to the neutral message, some conversations that we label **personal messages** highlight how political office can boost one's respect, status and influence. Similarly, *public* meetings always include a neutral message that

carries basic information on candidacy. In some meetings, social or personal benefits of office are discussed on top of this neutral conversation.

There are some additional aspects of the treatment that are important. First, we focus on keeping the interaction between enumerators and citizens natural. A consequence of this is that we kept pre-treatment surveys short. Second, though we extensively used scripts that highlighted the key points of each treatment in training the enumerators (see Appendix Section C), the treatments were actually delivered in a conversational manner to make the exercise natural – encouraging people to run for office while reading from a piece of paper is unlikely to work, or be received well. This is one reason we decided to partner with the NGO Sangum, as their staff includes experienced fieldworkers. The enumerators had a copy of the training scripts in the field to refresh the key points they had to make in conversation with people. Third, the treatments were developed after detailed piloting with focus groups before fieldwork commenced. The encouragements we use carry language that comes from, and is directly relevant to, the population where we conduct our experiment.

3.3 Randomization

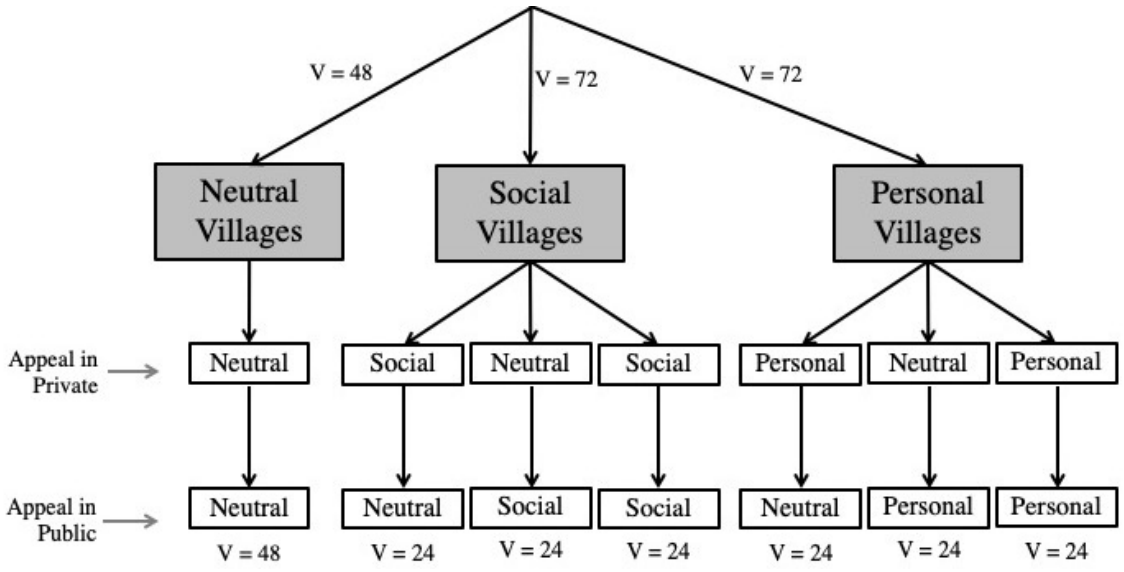

Figure 5: **Design of Field Experiment**

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.

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 villages", a social message is added on top of a neutral message in private or public meetings or both. 72 "personal villages" are similarly selected. As a reminder, a neutral message is delivered in all treatment conditions as baseline private and public conversations. Since all conditions include conversations comparing social villages against personal villages should yield the treatment effect of portraying political office in one way over the other. Finally, the social and personal benefits of office are not cross randomized: that is, a village can only receive one type of encouragement but not both. This is shown by the missing cells in Table 1.

Table 1: **Village Level Treatment Allocation**

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

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

3.4 Balance

Our field teams collected information on the population, number of settlements, distance to a main road and the local bureaucracy headquarters, and the size of the village council in a short village survey with key informants. We use these data to test for the balance of our randomization that we report in Appendix Table A4. Overall, the tests suggest that the randomization was successful. Appendix Table A2 presents the summary statistics for the subject pool.

3.5 Pre-Analysis Plan

We pre-registered the main analysis of this paper with the American Economic Association RCT Registry (AEARCTR-0000685) and the Evidence in Governance and Politics registry (20151102AA).[12] In Appendix F, we describe how the analysis in this paper relates to the PAP and list changes we made to the variables. There are two main points to note. First, the analysis on candidacy and election to the village council, the outcomes in Sections 4.2 and 4.3, are registered as the primary outcomes of interest in the pre-analysis plan. Second, the performance outcomes reported in Section 4.4 are not pre-registered, though our main results in that section make use of official data on budgets.

Importantly, as the experiment contains many treatment arms there are many ways to cut the data. The main hypotheses we pre-registered relate to testing the overall effects of making benefits from office salient, as are presented in the results on candidacy, election, and performance in sections 4.2, 4.3 and 4.4. As such, we treat these comparisons as the primary hypotheses of interest. We also decompose the main treatment effects in various ways to analyze how the main effects came to be. These are secondary analyses.

4 Results on Candidate Entry, Voter Selection, and Policy Outcomes

4.1 Estimation

We focus on our subject pool of 9,310 individuals in the 192 treatment villages. We run regressions of the following form:

$$Y_{iv} = \beta_1 Neutral_v + \beta_2 Social_v + \beta_3 Personal_v + \gamma_v + \varepsilon_{iv} \quad (1)$$

where Y_{iv} is an outcome, such as candidacy, for individual i in village v. $Neutral_v$ is an indicator variable that corresponds to labeled cell N in Table 1 where only a neutral message was delivered in both private and public meetings; $Social_v$ is an indicator variable for villages where a social message was delivered in either public or

[12] These can be accessed via `https://www.socialscienceregistry.org/trials/685` and `http://egap.org/registration/1576`

private, corresponding to cells A, B, and C in Table 1; and $Personal_v$ is an indicator variable for villages where personal benefits were made salient in either public or private, corresponding to cells D, E, and F in Table 1. γ_v are block fixed effects that also hold fixed the effect of enumeration teams that vary only across blocks. Standard errors are clustered at the village level, the unit of treatment assignment.

As we estimate the above model 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 treatment effect of making social or personal benefits from office salient as follows:

$$\begin{aligned} \text{Effect of Social Benefits vs Personal Benefits:} \quad & \beta_2 - \beta_3 = 0 \\ \text{Effect of Social Benefits vs Neutral:} \quad & \beta_2 - \beta_1 = 0 \\ \text{Effect of Personal Benefits vs Neutral:} \quad & \beta_3 - \beta_1 = 0 \end{aligned}$$

Guided by Young (2019), we also report Fisher exact p-values that do not require a limiting distribution for inference (Gerber and Green 2012). This test assumes a null of no treatment effect for any unit.[13]

4.2 Results on the Decision to Run

We first study whether the experiment had any effect on actual candidacy decision. To do this, we match each of 9,310 subjects from the sample to the official lists of candidates released by the Election Commission of Pakistan as well as those elected to village councils. Table 2 shows that the experiment had large effects on candidacy decisions. Relative to personal benefits, social benefits increase the probability of candidacy by 1.8 percentage points (exact-p=0.004), an increase of about 85 percent. Though we have limited statistical power, we also see that the effects are the result of social and personal benefits changing behavior in opposite directions: highlighting social benefits increases candidacy by 1 percentage points (exact-p=0.1) while highlighting personal benefits reduces the probability of candidacy by 0.9 percentage points (exact-p=0.16). We also find that these changes in our experimental sam-

[13]We perform this test by creating a set of 5,000 artificial treatment assignments at the village level. The effect estimated using the actual treatment assignment is compared against the effects with these artificial treatments. The exact p-value is the share of artificial treatment effects that have a larger magnitude than the true treatment effect.

Table 2: **Effects on Candidacy**

	Dependent Variable: Candidate=1			
	Overall	Sub-group Effects by Pro-Social Type:		
	Effect	*Low only*	*High only*	*High vs Low*
	(1)	(2)	(3)	(4)
Social vs Personal	0.018***	0.010	0.027***	0.019
	(0.007)	(0.007)	(0.009)	(0.012)
	[0.007]	[0.083]	[0.011]	[0.069]
Social vs Neutral	0.010	0.010	0.010	0.002
	(0.008)	(0.008)	(0.012)	(0.014)
	[0.111]	[0.121]	[0.214]	[0.447]
Personal vs Neutral	-0.009	-0.000	-0.017*	-0.017
	(0.006)	(0.005)	(0.009)	(0.010)
	[0.153]	[0.475]	[0.103]	[0.121]
Neutral Mean	0.030			
# Villages	192	192	192	192
# Observations	9310	5056	4254	9310

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. The table uses a dataset of randomly selected individuals. The dependent variable "Candidate" takes a value of one if the individual appears on ballot and zero otherwise. Column 1 reports the causally identified effect of treatments on the probability of becoming a candidate for all individuals. Columns 2 and 3 report sub-group analysis based on pro-social type of the individual. Column 2 reports the effects of treatments on the probability of becoming candidate for the "low" pro-social type and column 2 reports the effects on "high" pro-social type. Column 4 reports the difference in effects between columns 2 and 3. Each regression uses block fixed effects. Standard errors are clustered at the village level and reported in parenthesis. Exact p-values are in square brackets.

ple also carry through to the size of the candidate pool at the village level where we document that about one additional person runs in social versus personal village (exact-p=0.142) (see Appendix H).

Besides the causal effect on the probability of running for office, we conduct exploratory analysis on which messages are likely to recruit more pro-social people – a question of interest for the theoretical literature on political candidacy. Before the treatments were delivered, we conducted a short survey with subjects where we measure the degree to which a person associates political office with pro-social goals. We use responses to these questions as our measure of the pro-social motivations of potential candidates.[14] The benefit of this strategy is that it maximizes the statistical power by splitting respondents equally across high and low types. The drawback which makes this variable imperfect is that it uses a stated instead of behavioral measure of community-mindedness. As such, we treat this analysis as exploratory.

We take the average of responses across the four questions we ask and split the data at the median to study if higher than median (*high*) response on this variable, compared to a lower than median (*low*) response, differentially predicts the treatment effects in column 1 of Table 2. We find that social versus personal messages increase the probability of candidacy particularly among the ex-ante pro-social types. High types run for office 2.7 percentage points more (column 3), while low types increase more by 1 percentage points (column 2). The difference is large at 1.9 percentage points (exact-p = 0.065) as shown in column 4. Interesting, and perhaps more speculatively, it seems that highlighting the personal benefits is most likely to dissuade high pro-social types to run for office – a result consistent with theoretical and empirical work that suggests that extrinsic motivation can sometimes crowd-out intrinsic motivation (Benabou and Tirole 2003; Ashraf et al. 2020).[15]

[14]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 internal peace'; 'Publicly provided services are very important for ordinary people'; 'Improving village schools is directly linked to the performance of public representatives'.

[15]Since pro-social motivations are not randomized, as a robustness exercise, we check if the distribution of community-minded people is balanced across treatment conditions. We find that the personal vs neutral message treatment predicts responses to the questions measuring pro-social motivations in Appendix Table A6. This could be problematic for the heterogeneous effects we present here because it could be the case that more pro-social people are less likely to run under personal messaging because there are fewer of those people in those villages to begin with. Reassuringly, when we put a control for the proportion of pro-social people in the village, this imbalance disappears as shown in column (2) of Table A6. In the Appendix H.2, we re-generate Table A7 with the inclusion of this village level proportion of community-minded people as a control and find that the

Overall, the results on candidacy highlight that how political office is seen by prospective politicians influences the supply of candidates in elections and that pro-social motivations might be a powerful mobilizer particularly for people who are more pro-social to begin with.

4.3 Results on Voting

Next, we analyze voters' decisions. Changes in candidacy, while important on their own, may not reflect changes in the elected political class if voters have a preference for status-quo politicians. To test this empirically, we again make use of official electoral data to study the probability that a subject won an election and was elected to political office. We find in Table 3 that when social benefits are made salient versus personal benefits, the unconditional probability of getting elected to office is 1.2 percentage points higher (exact-p = 0.007). This can be decomposed into a 0.5 percentage point (exact-p = 0.17) increase in the probability of getting elected when social benefits are made salient and a 0.7 percentage point (exact-p = 0.096) decrease when personal benefits are highlighted. Relative to an unconditional probability of election to office of 1.7% these are relatively large effects. In addition, we also find that the increase in the probability of election is large for high versus low pro-social types: the difference is 1.4 percentage points (exact-p = 0.068).

4.4 Results on Policy

Next, we evaluate if these changes affect policy. Evaluating changes in policy outcomes is not straightforward. First, we can only observe the performance of elected individuals, 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. Randomizing treatments at the village level helps with this as we can study the performance of the entire elected council causally. Second, there are two dimensions on which policy can be affected. One is the extensive margin, where local political effort can generate more resources for the community (Burgess et al. 2015; Malik 2019). The other is on the intensive margin, which refers to how a given

point estimates hardly move. This suggests that the differences in treatments across high and low community-minded types are not necessarily originating because of differences in the distribution of these community-minded people across village.

Table 3: **Effects on Election**

	Dependent Variable: Elected to Council=1			
	Overall	Sub-group Effects by Pro-Social Type:		
	Effect	*Low only*	*High only*	*High vs Low*
	(1)	(2)	(3)	(4)
Social vs Personal	0.012***	0.006	0.019***	0.014
	(0.004)	(0.004)	(0.007)	(0.008)
	[0.007]	[0.111]	[0.014]	[0.068]
Social vs Neutral	0.005	0.004	0.006	0.003
	(0.005)	(0.005)	(0.009)	(0.011)
	[0.173]	[0.217]	[0.265]	[0.410]
Personal vs Neutral	-0.007**	-0.002	-0.013**	-0.011
	(0.003)	(0.004)	(0.006)	(0.008)
	[0.096]	[0.382]	[0.088]	[0.146]
Neutral Mean	0.017			
# Villages	192	192	192	192
# Observations	9310	5056	4254	9310

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. The table uses a dataset of randomly selected individuals. Dependent variable "Elected" takes a value of one if the individual is reported as elected by the Election Commission of Pakistan. Column 1 reports the causally identified effects of treatments on the probability of getting elected for all individuals. Columns 2 and 3 report sub-group analysis based on pro-social type of the individual. Column 2 reports the effects of treatments for the "low" pro-social type and column 2 reports the effects for "high" pro-social type. Column 4 reports the difference in effects between columns 2 and 3.Each regression uses block fixed effects. Standard errors are clustered at the village level and reported in parenthesis. Exact p-values are in square brackets.

amount of resources are distributed within the community.

In our context, the extensive margin is officially fixed as the amount of resources available to the Village Councils is determined by a legal fiscal formula. However, it is conceivable that varying the pool of politicians affects whether more resources from the provincial government arrive in the village even in the presence of such rules.[16] To test this hypothesis, we return to villages one year after the elections between June and July of 2016. We collect information from the first budget documents prepared by each Village Council at the end of the fiscal year. These include information on the total amounts sanctioned by the provincial Finance Department, as well as information on how Village Councils actually decide to spend these allocations.[17]

We first probe the official data and confirm that our treatments do not explain any changes to the amount sanctioned to Village Councils (see Appendix Table A11). The data show that there is good adherence to rules on the extensive margin.

What remains is an examination of the intensive margin or how money is spent by the Village Councils. This decision is controlled more by the effort of elected Village Councilors. It may be the case, for example, that people motivated by social benefits are actually not better at their job than status quo politicians because they might have less human capital and would therefore deviate more from what citizens want. In contrast, it could be the case that these people are in fact better at their job because they are motivated to make government work for the community by ascertaining the needs of their constituents.

Our measure of policy efficacy on the intensive margin therefore compares the spending decisions of elected councils with how citizens would like the money to be spent.[18] When we return to the field a year after elections we also survey a random sample of 1318 citizens in each village to collect their spending preferences over budgets. We ask citizens to divide a hypothetical Rs. 100 village development budget over a set of spending priorities. Citizen responses are collapsed into four broad categories based on the nature of the spending item. These categories are

[16]Indeed previous research, for instance on politicians salary caps, suggests there remain considerable variation around officially designated rules (Ferraz and Finan 2011b).

[17]This information is available with the village Secretary. We were able to collect it from all villages except three that were facing a gridlock over spending decisions. In appendix I.2 we show that missing data is not correlated with treatments, and that our results are robust to extreme value (Manksi) bounds.

[18]This also links well with theoretic work on citizen candidates that measure the distance between the preferences of the citizenry with those who run for office (Besley and Coate 1997).

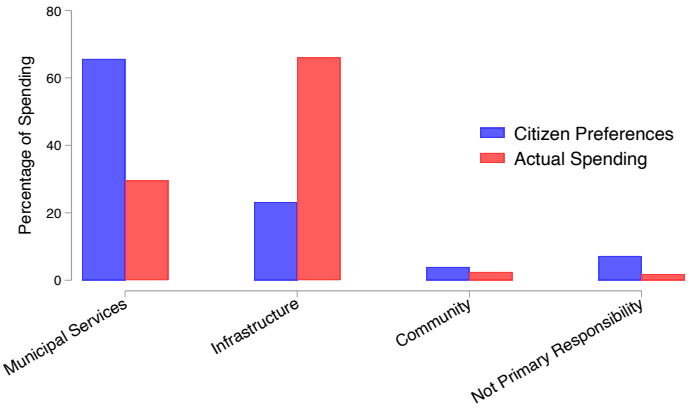

Figure 6: Citizen Preferences and Council Spending in Neutral Villages

Notes: This figure shows the proportion of budget allocated by citizens to different categories in a hypothetical exercise against the allocations by elected councils in the actual annual budget of 2016 in Neutral Villages.

Municipal Services, Infrastructure, Community, and a residual category that stores preferences that are officially not the primary responsibility of the village council.[19]

Figure 6 plots the distribution of these citizen preferences against how councils actually chose to spend the money through their official budgets in Neutral message villages. While, it is evident that citizens prefer that a majority of the budget be spent on municipal services, councils actually spend mostly on infrastructure projects. Community projects are not preferred by either group. Similarly, both groups are generally good at recognizing activities that are not the primary responsibilities of the village councils.

The large dichotomy in how councils are spending the money allocated to them versus constituent preferences motivate the investigation of whether treatments widened or closed this gap. To do this, we measure the Euclidean distance between spending citizen preferences. We calculate the distance for each budget category $j \in J$ by using the formula $\sqrt{(B_{ji} - \overline{C}_{ji})^2}$, where B_{ji} refers to the percentage of the budget spent on j in village i and $\overline{C}_{ji}$ 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

[19] *Municipal Services* include allocations 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.

preferences by summing over all four budget categories as follows: $\sqrt{\sum_J (B_{ji} - \overline{C}_{ji})^2}$.

Table 4: **Distance between Citizen Preferences and Council Budgets**

	Euclidean Distance (1)	Municipal Services (2)	Infrastructure (3)	Community (4)	Not Primary Responsibility (5)
Social vs Personal	-10.224**	-6.011	-8.113**	0.168	-2.980
	(4.876)	(3.768)	(3.879)	(1.186)	(2.145)
	[0.018]	[0.058]	[0.020]	[0.450]	[0.059]
Social vs Neutral	-6.901	-5.815	-5.626	1.106	0.620
	(5.845)	(4.540)	(4.589)	(1.293)	(2.025)
	[0.105]	[0.089]	[0.103]	[0.238]	[0.406]
Personal vs Neutral	3.323	0.196	2.488	0.937	3.601*
	(5.513)	(4.317)	(4.320)	(1.274)	(2.177)
	[0.267]	[0.465]	[0.285]	[0.264]	[0.047]
Neutral Mean	67.425	42.500	48.448	4.797	7.218
# Villages	189	189	189	189	189

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This 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 citizen preferences and actual spending by the council. The distance for each category is calculated using the formula $\sqrt{(B_{ji} - \overline{C}_{ji})^2}$, where B_{ji} refers to the percentage of the budget spent on category j in village i and $\overline{C}_{ji}$ 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: $\sqrt{\sum_J (B_{ji} - \overline{C}_{ji})^2}$. Each regression uses block fixed effects. Robust standard errors are reported in parenthesis. Exact p-values are in square brackets.

Table 4 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). We find that 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 10.2 points (exact-p = 0.018). Importantly, the primary contributors to this decrease in distance, as shown in columns (2) to (5), are spending on municipal and infrastructure categories which were the main non-aligned categories in Figure 6. As before, the effects move in opposite directions when social or personal benefits are directly compared against the neutral condition.

Since there are two component of the Euclidean distance, the changes we observe

above could arise because of movement in either component. For example, treatments can influence how elected politicians behave while making the budgets which should be reflected in how they spend the money. Second, the treatments can make the citizens change their expectations of the elected councils leading to changes in their preferences. We distinguish between these two explanations by decomposing the euclidean distance effects into its component parts in Appendix Table A14: budgetary spending (panel A), and citizen preferences (panel B). We find compelling evidence that the reduction in the euclidean distance between politician behavior and citizen preferences arises primarily from changes in the former term, which changes along infrastructure and municipal services dimensions, and not citizen preferences, which do not change substantively or statistically. This finding is significant because it reaffirms that the policy effects we observe emerge primarily through politician changes instead of shifts in citizen preferences.

Finally, for one of the two districts in our sample (Haripur), we were also able to retrieve budget data for an additional fiscal year (2017). Figure 7 shows that policy alignments for social versus personal villages in this sample can be seen in our data up to two years after the elections. Appendix tables A13 presents the results in tabular form.

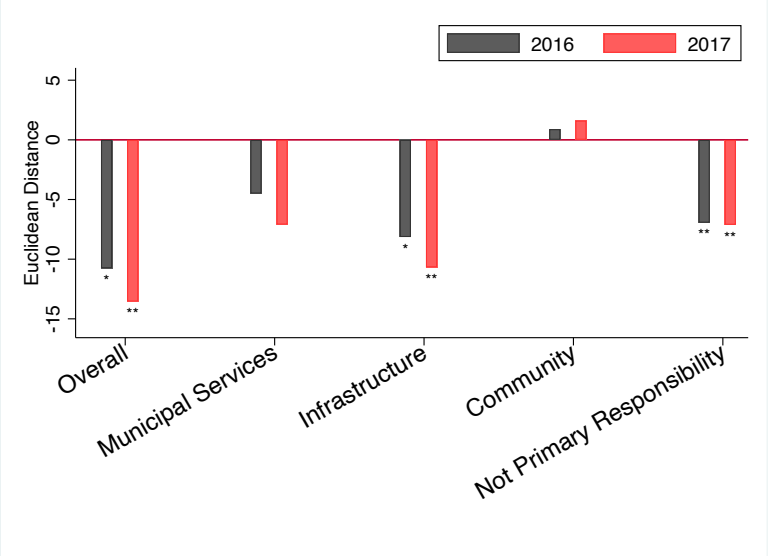

Figure 7: **Social versus Personal Policy Alignment Over Time (District Haripur only)**

Notes: * Exact $p < 0.1$, ** Exact $p < 0.05$, *** Exact $p < 0.01$.

Overall, the results above show that how political office was portrayed before the process of candidacy has a large effect on reducing the disparity between policies undertaken by elected village councils and citizen preferences. This suggests that indeed who runs for office has a potential to have a direct bearing on how democracies perform.

5 Mechanism: The Public Calculus of Candidacy

Motivation The results so far show that how political office is portrayed affects candidacy and election to office, and that this has a direct bearing on policy alignment with the preference of the electorate. There are two potential explanations for why we may see the results on candidacy. First, it could be the case that encouragements to run for office that vary how political office is portrayed carry new information for prospective candidates that was otherwise not available. Since, at the individual level, the decision to run depends on how the expected net benefit, including monetary and psychological, a prospective politician can expect from office, new information could sway the status-quo decision by strengthening the perceived association of political office *more* with one type of benefit over the other.

While it is possible that the information channel alone is sufficient to change the candidacy calculus of prospective candidates, we further postulate that a the public calculus of candidacy may also be important. A large literature argues that social interactions play a key role in political behavior, particularly the decision to vote (Gerber et al. 2008; Bond et al. 2012; DellaVigna et al. 2016) but also in their decision to run for office (Caselli and Morelli 2004). Unlike the decision to vote, where the secret ballot helps to protect from shame induced by not voting, the candidacy decision is public, carries externalities for the entire community (Ashraf and Bandiera 2018) and, potentially, impacted by people's opinions of the candidate, as well as a prospective candidate's evaluations of what others think.

We hypothesize that candidacy will be affected by social phenomenon, but the direction of effects will vary by beliefs around the underlying reasons to seek office. If the citizen's belief about reason associated with seeking office is different from the benefits that gives politicians ego-rents, they will stay out of politics (Caselli and Morelli 2004). We further assume that it is more socially acceptable to run for community-minded reasons than personal gain. Of course, theoretically, it is possible

that the two motivations are socially seen to be complementary: that is, a politician who is career-minded is also a politician who is community-minded. However, our assumption might be reasonable based on seminal work on candidacy that argues that career concerns are usually at odds with perceived public-spiritedness (Schlesinger 1966). This assumption is also consistent with substantive knowledge of the research site that we discuss in the context section.

Under this assumption, a public signal on the *social* benefits of office will change the candidacy calculus. This is for two potential reasons. First, prospective candidates might increasingly expect others to believe that those running for office are doing so to help their community (rather than themselves). Previous work suggests that such concerns about societal beliefs influence individual's actions (see Bursztyn and Jensen (2017) for a review). Second, a public signal may serve as a coordination device for the community to encourage community-minded individuals to run (Blair et al. 2019). Conversely, a public signal on the *personal* benefits of office will reduce candidacy for two similar reasons: first, people may expect others to believe that those running are driven by a desire to help themselves instead of the community; and/or second, a public signal may serve as a coordination device for the community to not support personally motivated people from the set of candidates.

Estimation Using the randomization scheme of the experiment, we can study if the treatment effects are stronger when benefits from office are delivered in public versus private one-on-one meetings. As before we focus on our subject pool of 9,310 individuals in 192 treatment villages. We run the following regressions:

$$Y_{iv} = \beta_{nn}N_v + \beta_{sn}A_v + \beta_{ns}B_v + \beta_{ss}C_v + \beta_{pn}D_v + \beta_{np}E_v + \beta_{pp}F_v + \gamma_v + \varepsilon_{iv} \quad (2)$$

where A-E are indicator variables that correspond to each labeled cell in Table 1, while N_v is an indicator variable for villages that receive a neutral message in the private as well as the public meetings. The subscripts 'p' and 'n' on the coefficient of A refer to a social message in private, but a neutral message in public. The rest of indicators are similarly labelled. As we estimate the model without an intercept, the β coefficients denote the means for outcomes for each group. As before, we can impose linear restrictions to calculate the treatment effect of public versus private conversations. For example, $\beta_{ns} - \beta_{np}$ gives the effect of social vs personal messages

in public only, while $\beta_{sn} - \beta_{pn}$ gives the effect in private only. We can similarly compare social and personal benefits to the neutral condition in public and private conversations.[20]

Table 5: **Candidacy, Election, and Policy Effects of Public and Private Treatments**

	Public Only Treatments			Private Only Treatments		
	Filed Papers (1)	Elected to Council (2)	Policy Euclidean Distance (3)	Filed Papers (4)	Elected to Council (5)	Policy Euclidean Distance (6)
Social vs Personal	0.022*	0.011*	-17.629**	-0.004	0.002	8.811
	(0.011)	(0.006)	(7.157)	(0.009)	(0.007)	(9.366)
	[0.054]	[0.072]	[0.015]	[0.655]	[0.780]	[0.348]
Social vs Neutral	0.013	0.004	-9.481	-0.009	-0.002	-0.351
	(0.012)	(0.006)	(7.375)	(0.009)	(0.006)	(8.475)
	[0.280]	[0.532]	[0.200]	[0.306]	[0.705]	[0.967]
Personal vs Neutral	-0.009	-0.007*	8.148	-0.005	-0.004	-9.162
	(0.006)	(0.003)	(6.260)	(0.007)	(0.005)	(7.568)
	[0.141]	[0.055]	[0.195]	[0.480]	[0.360]	[0.228]
Neutral Private, Neutral Public Mean	0.030	0.017	67.425	0.030	0.017	67.425
# Observations	9310	9310	189	9310	9310	189
# Villages	192	192	189	192	192	189

Notes: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. The table uses a dataset of randomly selected individuals to report the effect of treatments based on whether the treatment was delivered in private or in public. "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 and election and zero otherwise. "Policy Euclidean Distance" uses the distance in budget spending between policy decisions and citizens preferences as described in section 4.4. First three columns report the effect of treatments delivered in public and the remaining report the comparisons when treatments are delivered in private. Each regression uses block fixed effects. Standard errors are clustered at the village level and reported in parenthesis. Exact p-values are in square brackets.

Results Table 5 presents the results on candidacy, election, and overall policy outcomes.[21] We see that the main treatments effects we described above are concentrated primarily in public meetings instead of private meetings. When social benefits from office are made salient in public, relative to personal benefits, the probability that a person runs and wins increases, and the gap between citizen preferences and implemented policies shrinks.

[20] A concern here is that there might be differential selection into attending the public meetings by what treatments were delivered in private. We find no evidence for this in the data. (see Appendix Table A15).

[21] See appendix Table A12 for policy effects for each budget category.

These results suggest that the *information* content of the conversations alone mattered less than their public aspects. This is important because the results show that candidacy is particularly affected by what people think perceive *others* perceive as their reasons to run for office. Galvanizing candidates around the already prevalent norm of pro-social candidacy could be a particularly powerful tool.

6 Conclusion

This paper presents new evidence on an important channel of improving representative democracy: the supply of politicians. It shows that the way in which politics is portrayed to ordinary citizens affects who decides to enter politics, who gets elected, as well as policy outcomes.

We study candidate entry in the shadow of a large policy reform in democratization in Pakistan. The reform represents a potential watershed moment in Pakistan's democratic consolidation – the number of directly elected representatives in the province we study rise from 125 in 2013 to more than 48,000 in 2017. Locally elected government holds the promise of feeding a stream of talent that will eventually rise up the political ranks. Understanding how the decisions to run for these offices are shaped and how the local talent pool can be improved is therefore important not just in Pakistan, but in a variety of contexts where local governments are the grassroots of democracy.

Our experiment reveals that non-pecuniary incentives can be particularly powerful in mobilizing a political class that delivers responsive policy to the electorate. When political office is presented in terms of its pro-social versus personal benefits, particularly in public settings, people who would not have otherwise run for office become political candidates. Presenting themselves as candidates has the knock on effect of them getting elected because they are now presented to voters on the ballot. Finally, the encouragements also align downstream policy outcomes more closely with the preferences of citizens suggesting that who runs for office has a direct bearing on the policy outcomes we observe.

As politics continues to be viewed with greater skepticism in many developing countries,[22] this first result outlines that it is perhaps possible to improve the supply

[22]According to the World Values survey, 69.1 percent and 66.4 percent of respondents in Pakistan report little to no confidence in the parliament and political parties respectively (Inglehart et al.

of politicians in developing countries if we focus on the determinants of their initial decision to run. There exist people who are responsive to citizen preferences but are not contesting elections and therefore giving citizens a chance to elect them.

2014).

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ONLINE APPENDIX

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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 Sampling procedure

Random Walk As 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. As with most research in this context, contact was made with more than 95 percent of households approached. Our analysis shows that we are not elite biased in our sampling: people approached earlier in the fieldwork (closer to the center) versus those approached later (farther from the center) look the same on observables. These results are available upon request. Importantly, the sampling strategy is the same across all treatment arms so any measurement error should be uncorrelated to treatment assignment.

Sample Selection Sampling at the household level effectively translates into male respondents in our context. 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 women's 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 fieldwork whatsoever. Finally, research has shown that the expression of womens' political preferences in Pakistan tend to align with male members of the household (Bari 2005). However, Khan (2017) shows that even when actual preferences are different across men and women within a household, expressed preferences might be the same. In this sense, boosting women's political participation directly might require a deeper transformation of norms that we are now examining in current work in Pakistan.

C Scripts for Conversations

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."*

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."*

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."

Outline of Public Meetings

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 (neutral message)
5. Discussion on **Personal** *or* **Social** Benefits to councilors
6. Questions and Discussion
7. End

D 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

E Ethical considerations

This experiment is focused on improving the supply of politicians in an actual election. This merits a discussion of ethical considerations. The project has received approval from Institutional Review Boards (IRB) of our universities. Nonetheless, there are additional concerns that go beyond the question of IRB which we want to address in this note. We take guidance from the categorization of ethical concerns in Humphreys (2015). This relates to questions of audience, agency, and consent in field experiments.

Audience: Our local field partners, the NGO called Sangam Development Organization (SDO) and the state of Pakistan can be considered as the main audience of this project. There could be two ethical concerns that emerge from our interaction with SDO and operating in Pakistan. First, SDO may have been led to undertake activities which are not in their best interest. The project may have been detrimental to the state and society of Pakistan. Second, there is a conflict of interest between the research team and SDO, especially due to financial linkages.

The main idea regarding the first concern is that researchers based in developed world may have undue influence on the design of the project that asks the local partners to undertake activities that are detrimental to them or the Pakistani state. We believe this concern is ameliorated by the fact that both researchers on this project are from Pakistan and one of us is a native of the province where this project was implemented. In fact, at the time of implementation of this project, one member of the research team was not even based at a western institution and worked locally in Pakistan. Furthermore, the project was implemented with due permissions from government authorities in the area. They gave permission after evaluating the project objectives, design, and activities. In addition, these permissions were regularly reviewed by state officials to ensure that the project continued to not be detrimental to the society or the state.

Additionally, SDO is part of a network of organizations that helps it run independent local operations but as part of a bigger platform. This arrangement potentially protects it from undue influence by any one partner, for example, the research team. This also addresses the second point of ethical concern: SDO is not completely reliant on the research team for financial support as it has access to a broader set of resources by virtue of being part of a network. This limits the ability of any one of their part-

ners to use financial support as a means to influence their work. The research team worked with SDO as equal partners, relying on their expertise of fieldwork in the area.

Agency: The main concern with respect to agency is the autonomy of all stakeholders to make independent decisions. Two sets of stakeholders that are particularly important from the stand point of ethical considerations are the potential candidates and voters.

Potential candidates are the set of randomly selected citizens who were treated by the field teams with information about elections and benefits from office. We believe the potential candidates had agency to make independent and autonomous decisions because of three reasons. First, the research team did not have any power over any of the subjects to not listen to the treatment. In the spirit of standard consent protocol, all subjects were asked for explicit consent before our enumerators could inform them about anything and they could discontinue the interaction at any time. Second, after the treatment the subjects were free to disregard the information we gave them and did not need to act on anything. Third, they did not draw any material benefit from participating in the study. Even the information provided during the treatments was accessible to the subjects at other forums. Voters also retained complete agency in terms of selecting their representatives. The research did not try to influence the voters' decisions. Together these suggest that stakeholders had autonomy to act as they desired and the research team did not have undue influence over their decisions.

Consent: We sought consent from all subjects of the study before proceeding with the survey or treatment.

F 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 scope.

Data collection

All data on candidacy and election are available to the authors after the PAP is registered. The analysis on candidacy and election is pre-registered, while the analysis on performance effects is not pre-registered. However, the performance results makes use of administrative data and a citizen survey on budgets and the collection of those data commenced before authors had the final administrative data on elections in hand. That is, we were not aware of the effects on candidacy and election by the time we started collecting data on performance.

Variables

Guided by Olken (2015), we consider effects on 'primary outcomes' of interest. 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, various seminar 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 and election with variable 4 and 5 (page 16 of PAP), which is not self-reported and is retrieved directly from the election commission. Using the administrative measure of candidacy and election 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 changes to the labels of a treatment and a variable. The Personal Benefits treatment was labeled as Career Benefits and pro-social type (used for heterogeneous effects) was labeled as "prior on pro-sociality" in the PAP. We consider the new labels

are better reflections of what the variables measure.

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 policy outcomes in Section 4.4 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 in Main Analyses	**Type**	**Tables in Paper**	**Registered in PAP?**	**PAP Section 5 variable #**
Filed papers	Outcome	Table 2	Yes	4
Elected to council	Outcome	Table 3	Yes	5
Budget and Preferences	Outcome	Table 4	No	
Pro-Social Type	Moderator	Table 2	Yes	19
		Table 3	Yes	19

G 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
Pro-social Low Type	0.543	0.498	0	1	9310
Pro-social High Type	0.457	0.498	0	1	9310
Village Characteristics					
Village Population (1998)	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
Registered Votes (2015)	6531.344	3066.435	1385	17345	192
Turnout	0.76	0.107	0.457	0.992	186

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 4.4. Three Village Councils did not prepare a budget due to gridlock. Table A9 provides evidence that treatments do not predict missing data.

Table A4: **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 Private, Neutral Public	4188.384	2.363	1.738	20.863	12.617	34.055	73.167	6.487
	(730.981)	(0.376)	(2.302)	(3.432)	(3.838)	(0.018)	(0.023)	(0.398)
B. Personal Private, Neutral Public	3970.954	2.332	-0.094	20.005	13.560	34.047	73.193	6.343
	(689.699)	(0.439)	(2.472)	(3.304)	(4.151)	(0.022)	(0.024)	(0.378)
C. Neutral Private, Personal Public	4290.217	2.316	4.545	23.819	13.530	34.033	73.167	6.360
	(670.607)	(0.422)	(3.945)	(3.387)	(4.363)	(0.019)	(0.023)	(0.370)
D. Personal Private, Personal Public	4187.863	2.611	1.862	17.463	8.431	34.033	73.187	6.378
	(703.264)	(0.438)	(4.021)	(3.288)	(3.883)	(0.021)	(0.024)	(0.376)
E. Social Private, Neutral Public	4947.825	2.431	3.303	15.294	6.982	34.036	73.183	6.795
	(662.900)	(0.358)	(2.331)	(3.294)	(3.674)	(0.021)	(0.024)	(0.360)
F. Neutral Private, Social Public	3723.231	2.022	2.528	16.009	8.753	34.038	73.181	6.171
	(647.360)	(0.397)	(2.666)	(3.515)	(4.061)	(0.020)	(0.028)	(0.361)
G. Social Private, Social Public	3811.318	2.132	8.078	13.391	6.528	34.060	73.172	6.184
	(705.307)	(0.424)	(5.398)	(3.590)	(4.291)	(0.022)	(0.026)	(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. All regressions include block fixed effects. Robust standard errors are reported in parantheses.

H Additional Results on Candidacy

H.1 Village level Effects on Candidate Pool Size

Table A5: **Effects on Number of Candidates at Village Level**

	# Total Candidates (1)	# Candidates Open Seats (2)	# Candidates Reserved Seats
Social vs Personal	0.961	0.483	0.477
	(0.824)	(0.407)	(0.571)
	[0.142]	[0.133]	[0.228]
Social vs Neutral	0.920	0.416	0.504
	(0.981)	(0.462)	(0.671)
	[0.185]	[0.199]	[0.243]
Personal vs Neutral	-0.041	-0.068	0.027
	(0.849)	(0.411)	(0.592)
	[0.485]	[0.443]	[0.489]
Neutral Mean	19.083	8.917	10.167
# Observations	192	192	192
# Villages	192	192	192

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. The table uses administrative data from Election Commission of Pakistan. Dependent variable in column 1 is total number of candidates that appeared on ballot. Column 2 uses the total number of candidates who ran on open seats and column 3 uses number of candidates that ran on reserved seats as dependent variables. Each regression uses block fixed effects. Robust Standard errors are reported in parentheses. Exact p-values are reported in square brackets.

H.2 Robustness Checks for Sub-group Analysis by Pro-Social Type

Table A6 reports that distribution of high pro-social types is slightly skewed across the treatment conditions. This could mean that the response of high types to the treatments could be driven by the high number of such types in certain villages. In order to test if that is indeed the case we explicitly control for the proportion of high types in a village. This section reports all the tables that use the social type in the analysis while controlling for the proportion of high types in each village. Assuringly, our results do not change substantially when we control for the proportion of high pro-social types in each village.

Table A6: **Distribution of High Pro-Social Type**

Dependent variable:	High=1 (1)	High=1 (2)
Social Vs Personal	0.016	0.000
	(0.034)	(0.000)
Social Vs Neutral	-0.047	-0.000
	(0.040)	(0.000)
Personal Vs Neutral	-0.063*	-0.000
	(0.036)	(0.000)
Neutral Mean	0.491	0.491
Controls	No	Yes
# Observations	9310	9310
# Villages	192	192

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. This table uses data of random individuals to test the relationship between treatments and distribution of pro-social "high" types in a village. The dependent variable takes a value 1 if the individual is "high" pro-social type and 0 otherwise. In column 1 the table reports the relationship without controlling for the proportion of high types and column 2 reports the relationship after controlling for the proportion of high types in a village. Each regression uses block fixed effects. Standard errors are clustered at the village level and reported in parentheses.

Table A7: **Heterogeneous Response by type (with control)**

Pro-social Type:	Low only (1)	High only (2)	Diff: (2)-(1) (3)
	Dep Var: Filed Papers		
Social vs Personal	0.011	0.026***	0.017
	(0.007)	(0.009)	(0.011)
	[0.245]	[0.175]	[0.021]
Social vs Neutral	0.011	0.011	0.000
	(0.008)	(0.012)	(0.014)
	[0.020]	[0.129]	[0.234]
Personal vs Neutral	0.000	-0.015*	-0.016
	(0.006)	(0.009)	(0.010)
	[0.105]	[0.480]	[0.078]
# Villages	192	192	192
# Observations	5056	4254	9310
	Dep Var: Elected		
Social vs Personal	0.006	0.018***	0.012
	(0.004)	(0.007)	(0.008)
	[0.079]	[0.229]	[0.028]
Social vs Neutral	0.004	0.007	0.001
	(0.005)	(0.009)	(0.010)
	[0.009]	[0.165]	[0.357]
Personal vs Neutral	-0.002	-0.011*	-0.011
	(0.004)	(0.006)	(0.008)
	[0.106]	[0.445]	[0.172]
# Villages	192	192	192
# Observations	5056	4254	9310

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This table reports heterogeneous effects by pro-social types while controlling for the proportion of high pro-social types in each village as a robustness check for results reported in Tables 2 and 3. The dependent variable in first panel takes a value of 1 if the person's name appears on ballot paper and 0 otherwise. The dependent variable second panel takes a value of 1 if a person is declared elected by the Election Commission of Pakistan and 0 otherwise. Column 1 uses data only of individuals that are "low" pro-social type and Column 2 restricts sample to individuals that are "high" pro-social type. Column 3 reports the difference between column 1 and column 2. Each regression uses block fixed effects. Standard errors are clustered at the village level and reported in parentheses. Exact p-values are reported in square brackets.

I Additional Results on Performance of Councils

I.1 Summary Statistics on Preferences and Budgets

Table A8: **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.

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 4.4. Table A8 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, Shapiro, and Eynde

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.

I.2 Missing Data Analysis

Table A9: **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 4. 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 reported in parentheses. All regressions include block fixed effects.

Table A10: **Policy Effects - Manski Bounds**

	Effects on Euclidean Dist in Table 4 (1)	Best Case Scenario (2)	Worst Case Scenario (3)
Social vs Personal	-10.224**	-11.093**	-9.438*
	(4.876)	(4.952)	(4.914)
	[0.035]	[0.027]	[0.052]
Social vs Neutral	-6.901	-10.374*	-4.608
	(5.845)	(6.158)	(5.897)
	[0.129]	[0.227]	[0.053]
Personal vs Neutral	3.323	6.570	-1.020
	(5.513)	(5.611)	(5.906)
	[0.301]	[0.151]	[0.440]
Neutral Mean	67.425	64.720	70.508
# Villages	189	192	192

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. This table uses Manski bounds to assess the best and worst case scenarios for missing budget data. 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. Each column is a dependent variable that is defined as 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 is calculated using the formula $j \in J$ by $\sqrt{(B_j - \overline{C}_j)^2}$, where B_j refers to the percentage of the budget spent on j and $\overline{C}_j$ is the average of citizen preferences for spending on that category. The overall difference in column (1) is calculated using the formula: $\sqrt{\sum_J (B_j - \overline{C}_j)^2}$. Bounds for Social vs Personal comparison: For the calculation of the best case scenario in column 2, we replace the overall distance for any Social message villages that are missing the budget information with the minimum observed value of the overall distance. For Personal villages, we replace missing information with the maximum value. For the worst case scenario, we replace the former with the maximum and the latter with the minimum value. Bounds for Social vs Neutral comparison: these bounds are calculated similar to the above procedure. Bounds for Personal vs Neutral comparison: the procedure for these bounds is reversed as the observed coefficient is position. For the best case scenario, we replace missing personal villages data with the maximum observed value of the overall distance, while replacing the neutral villages with the minimum observed value of the overall distance. For the worst case scenario, the procedure is reversed. Each regression uses block fixed effects. Standard errors are clustered at the village level and reported in parenthesis.

I.3 Policy Effects on Extensive Margin

Table A11: **Extensive Margin of Performance - Total Resources**

	Total Resources (in Rs.) (1)	Log(Total Resources) (2)
Social vs Personal	-9763	.00684
	(145412)	(.0647)
	[0.200]	[0.900]
Social vs Neutral	99384	.0505
	(172275)	(.0785)
	[0.300]	[0.300]
Personal vs Neutral	109147	.0436
	(160448)	(.0725)
	[.3]	[.3]
Neutral Mean	2284106	15
# Villages	189	189

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. This table presents the effect of treatments on extensive margin of total resources available to Village Councils. Dependent variable in column 1 is total amount of budget allocated to the village council in Pakistani Ruppees, and the dependent variable is log of Pakistani ruppees column 2. Each regression uses block fixed effects, robust standard errors reported in parentheses and the exact p-value are reported in brackets.

I.4 Policy Effects of Public and Private Treatments

Table A12: **Policy Effects of Public and Private Treatments**

	Total Distance (1)	Municipality (2)	Infrastructure (3)	Community (4)	Not Primary Responsibility (5)
			Panel A: Public Only Treatment		
Social vs Personal	-17.629**	-16.781***	-12.281**	1.646	0.512
	(7.157)	(6.074)	(5.975)	(2.367)	(3.517)
	[0.015]	[0.006]	[0.041]	[0.488]	[0.885]
Social vs Neutral	-9.481	-10.685*	-7.234	2.445	1.940
	(7.375)	(5.925)	(5.884)	(2.030)	(3.064)
	[0.200]	[0.073]	[0.220]	[0.230]	[0.527]
Personal vs Neutral	8.148	6.096	5.047	0.799	1.428
	(6.260)	(5.278)	(5.180)	(1.824)	(2.742)
	[0.195]	[0.250]	[0.331]	[0.662]	[0.603]
Neutral Private, Neutral Public Mean	67.425	42.500	48.448	4.797	7.218
# Observations	189	189	189	189	189
			Panel B: Private Only Treatment		
Social vs Personal	8.811	8.653	4.463	1.112	0.757
	(9.366)	(6.944)	(6.985)	(1.889)	(2.813)
	[0.348]	[0.214]	[0.524]	[0.557]	[0.788]
Social vs Neutral	-0.351	0.896	-1.030	0.813	0.369
	(8.475)	(6.152)	(6.525)	(1.801)	(2.631)
	[0.967]	[0.884]	[0.875]	[0.652]	[0.889]
Personal vs Neutral	-9.162	-7.757	-5.494	-0.299	-0.388
	(7.568)	(6.009)	(5.579)	(1.541)	(2.266)
	[0.228]	[0.198]	[0.326]	[0.846]	[0.864]
Neutral Private, Neutral Public 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 based on whether they are delivered in private or public on the performance of elected councils. 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. Each column is a dependent variable that is defined as 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 is calculated using the formula $j \in J$ by $\sqrt{(B_j - \overline{C}_j)^2}$, where B_j refers to the percentage of the budget spent on j and $\overline{C}_j$ is the average of citizen preferences for spending on that category. The overall difference in column (1) is calculated using the formula: $\sqrt{\sum_J (B_j - \overline{C}_j)^2}$. Panel A reports the comparison for treatments delivered in public, and panel B reports the comparisons when treatments are delivered in private. Each regression uses block fixed effects. Standard errors are clustered at the village level and reported in parenthesis.

Table A13: **Overtime Policy Alignment in District Haripur**

	Total Distance (1)	Municipal (2)	Infrastructure (3)	Community (4)	Not Primary Responsibility (5)
Panel A: 2016 Budget for Haripur District					
Social vs Personal	-10.758	-4.501	-8.146	0.900	-6.945**
	(7.060)	(5.606)	(5.691)	(1.701)	(3.434)
	[0.067]	[0.215]	[0.071]	[0.302]	[0.012]
Social vs Neutral	-12.040	-9.666	-9.932	2.082	1.669
	(9.206)	(7.203)	(7.098)	(1.764)	(2.755)
	[0.072]	[0.067]	[0.065]	[0.138]	[0.334]
Personal vs Neutral	-1.282	-5.166	-1.786	1.182	8.614**
	(8.916)	(7.117)	(6.728)	(1.497)	(3.508)
	[0.439]	[0.212]	[0.391]	[0.260]	[0.003]
Neutral Mean	67.425	42.500	48.448	4.797	7.218
# Villages	82	82	82	82	82
Panel B: 2017 Budget for Haripur District					
Social vs Personal	-13.548*	-7.103	-10.683*	1.616	-7.121*
	(7.141)	(5.840)	(5.856)	(1.934)	(3.957)
	[0.029]	[0.108]	[0.033]	[0.221]	[0.019]
Social vs Neutral	-12.794	-6.641	-13.743*	0.403	2.313
	(8.999)	(6.905)	(7.061)	(2.413)	(3.217)
	[0.066]	[0.160]	[0.022]	[0.445]	[0.309]
Personal vs Neutral	0.755	0.462	-3.059	-1.213	9.434**
	(8.892)	(7.043)	(6.867)	(2.285)	(3.988)
	[0.455]	[0.474]	[0.346]	[0.288]	[0.005]
Neutral Mean	63.780	38.702	45.225	6.092	8.056
# Villages	79	79	79	79	79

Notes: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. This table uses a village level dataset that is constructed based on official budget data of 2016 and 2017 from the councils and the preferences of citizens regarding the budget in the Haripur district. The dependent variable in each column is defined as the quadratic distance between citizen preferences and actual spending by the council. The distance for each category is calculated using the formula $\sqrt{(B_{ji} - \overline{C}_{ji})^2}$, where B_{ji} refers to the percentage of the budget spent on category j in village i and $\overline{C}_{ji}$ 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: $\sqrt{\sum_J (B_{ji} - \overline{C}_{ji})^2}$. Each regression uses block fixed effects. Robust standard errors are reported in parentheses and exact p-values are reported in square brackets.

I.5 Decomposing Policy Effects

Table A14: **Decomposing Policy Effects Across Budget and Citizen Preferences**

	Municipal (1)	Infrastructure (2)	Community (3)	Not Primary Responsibility (4)
Panel A: Budget Spending				
Social Vs Personal	8.345**	-7.303*	1.180	-2.222
	(3.872)	(3.827)	(0.912)	(1.568)
	[0.020]	[0.000]	[0.120]	[0.100]
Social Vs Neutral	4.884	-4.397	0.193	-0.680
	(4.806)	(4.816)	(1.138)	(1.185)
	[0.040]	[0.060]	[0.320]	[0.300]
Personal Vs Neutral	-3.462	2.906	-0.987	1.542
	(4.552)	(4.670)	(0.984)	(1.785)
	[0.300]	[0.220]	[0.260]	[0.320]
Neutral Mean	29.590	66.069	2.476	1.865
# Villages	189	189	189	189
Panel B: Citizen Preferences				
Social Vs Personal	-0.418	1.987	0.335	-1.904
	(2.936)	(2.236)	(1.186)	(1.970)
	[0.300]	[0.160]	[0.420]	[0.220]
Social Vs Neutral	-0.182	-1.308	0.713	0.776
	(3.530)	(3.023)	(1.295)	(2.013)
	[0.520]	[0.220]	[0.280]	[0.280]
Personal Vs Neutral	0.237	-3.294	0.378	2.680
	(3.460)	(2.978)	(1.263)	(1.941)
	[0.340]	[0.100]	[0.500]	[0.120]
Neutral Mean	65.528	23.044	4.178	7.250
# Villages	192	192	192	192

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This 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. Dependent variables in Panel A are the proportion of official budget allocated to each category mentioned in the column headers. Dependent variables in Panel B are the preferences of citizens regarding the proportion of budget allocated to each category mentioned in the column header. Each regression uses block fixed effects. Robust standard errors are reported in parentheses and exact p-values are reported in square brackets.

J Selection into Public Meeting

Table A15: **Effects by Randomization Cell**

	Attended Pub Meet (1)
Social Private Vs Personal Private	0.004
	(0.028)
	[0.837]
Social Private Vs Neutral	0.029
	(0.023)
	[0.444]
Personal Private Vs Neutral	0.024
	(0.022)
	[0.097]
Neutral Mean	0.802
# Villages	192
# Observations	9310

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. This table uses data from random individuals. Dependent variable takes value of 1 if the individual attended the public meeting held in the village and zero otherwise. Standard errors are clustered at the village level and reported in parentheses. All regressions include block fixed effects.

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