"Good Politicians": Experimental Evidence on Motivations for Political Candidacy and Government Performance*

Saad Gulzar† Muhammad Yasir Khan‡

June 22, 2021

Abstract

How can we motivate 'good' politicians – those that will carry out policy that is responsive to citizens' preferences – to enter politics? In a field experiment in Pakistan, we vary how political office is portrayed to ordinary citizens. We find that emphasizing pro-social motives for holding political office instead of personal returns – 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. It also better aligns subsequent policies with citizens' preferences. The candidacy decisions are explained by social influence, and not information salience – we find that social versus personal messaging matters only when randomly delivered in a public setting but not in private. Results also show that changes in political supply, not citizen preferences or behavior, explain policy alignment. Taken together, the results demonstrate that non-financial motivations for political entry shape how politicians perform in office.

*We thank Graeme Blair, Robin Burgess, Leonardo Bursztyn, Michael Callen, Alex Coppock, Cesi Cruz, Ernesto Dal Bó, Livio Di Lonardo, Oeindrila Dube, James Fearon, Vicky Fouka, Guy Grossman, Miriam Golden, Michael Gilligan, Anna Grzymala-Busse, Rema Hanna, Maira Hayat, Saumitra Jha, Asim Khwaja, Horacio Larreguy, Sara Lowes, John Marshall, Sultan Mehmood, Umberto Mignozzetti, Laura Paler, Rohini Pande, Daniel Rubenson, Cyrus Samii, Raul Sanchez de la Sierra, Shanker Satyanath, Ken Scheve, Jacob Shapiro, Jeremy Springman, David Stasavage, Daniel Triesman, Pablo Querubin, Jeremy Weinstein, Lauren Young, Noam Yutchman, Austin Wright, and seminar participants at Berkeley, Brown, BREAD Stanford, Emory, Essex, ESOC, Georgetown, Harvard, Konstanz, LSE, MIT, MPSA, NEWEPS, NYU, Princeton, Stanford, UC Berkeley, UC Merced, UCLA, UCSD, UChicago Harris, Vanderbilt, World Bank, WESSI, and Yale for helpful comments and feedback. We also thank J-PAL Governance Initiative and the International Growth Center for research funding and support. Jawad Karim, Milad Khattak, and Mamoor Khan provided excellent research assistance. This experiment is pre-registered with AEA RCT Registry (0000685) and EGAP (20151102AA). IRB approval was obtained at NYU and UC Berkeley.

†Stanford University. Email: gulzar@stanford.edu

‡University of California, Berkeley. Email: yasir.khan@berkeley.edu

1 Introduction

While, scholars have spent considerable effort in examining how democracies may be improved by tailoring the incentives of politicians who have already been elected,[1] what is often missed, and no less important in improving democratic performance, is the *supply* of politicians. Who runs for political office affects policy, independent of, and prior to the rules under which politicians operate once elected. Understanding and improving political supply is perhaps of central importance in developing countries where "bad" politicians are said to 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? This requires us to answer two questions. First, who are good politicians? Are they motivated by ego-rents, like respect, status, and influence (Caselli and Morelli 2004; Gagliarducci and Nannicini 2013; Fisman et al. 2015), or by pro-social incentives (Besley and Ghatak 2005)? And second, how can we get these good politicians to run?[3] While, highlighting personal returns can crowd out candidacy among socially-minded people (Bénabou and Tirole 2006; Deci 1971; Frey 1997), it can also induce more competent, perhaps career-minded, people to seek political office (Schlesinger 1966; Ferraz and Finan 2011b). Conversely, highlighting pro-social motivations can encourage more socially-minded people to seek office (Deci 1972; Besley and Ghatak 2005), but they may also give public cover to more selfish people to become politicians.

We provide new evidence on these puzzles through a field experiment in Pakistan. We randomly-sample citizens and encourage them 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 (called *social villages*), while, in other villages, personal returns of political office, like enhancing one's respect and status, are made salient (called *personal villages*).[4] We study the impacts of these encouragements on candidacy decisions, voting decisions, and, perhaps most importantly, the

[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); may be motivated by private rents (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 may have criminal backgrounds (Vaishnav 2017; Blaydes 2010).

[3] What motivations might enable political entry of prospective candidates who benefit society at large is a key puzzle identified by recent review articles (Dal Bó and Finan 2018; Bandiera et al. 2019).

[4] 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.

alignment of subsequent policy outcomes with the preferences of the electorate.

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,[5] political entry decisions at the local level provide evidence on how one might broaden the 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.[6] 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.[7]

We begin by analyzing the political entry decisions of citizens. We find that in villages where politics is portrayed as enabling community-minded policy, relative to villages where it is portrayed as yielding personal benefits, people in our experimental sample are more likely to run for office. 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 the supply of politicians improve, 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 stand 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 to office. Taken together, this second result suggests that while the people mobilized into running for office are electable, we may not be seeing them in office in the status quo because they do not put themselves forward as candidates.

Finally, showing that social versus personal encouragements work to get people to run and for voters to elect is not enough for good government. We go a step further by examining if this matters for better aligning policy outcomes with citizens' preferences a year after politicians assumed office. To do so, we first measure citizen preferences for budgetary spending one year after their election, through a survey. This is the key decision made

[5]See Casey et al. (2021) for an experiment on political selection with political parties in Sierra Leone.

[6]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."

[7]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.

by local politicians. We show that there is considerable discrepancy in the decisions of the elected politicians and the preferences of the electorate in baseline villages. Next, by benchmarking actual policy decisions made by elected politicians against these preferences of the electorate, we ask if social versus personal messaging align or widen the gap between what policy is adopted and what citizens want? 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. This result on policy alignment provides direct evidence that social versus personal encouragements are yielding 'good' politicians to office.[8] Indeed, we also show that citizens in these villages are more satisfied with politicians' policy choices and exhibit more positive affect towards politics and the state.

In addition to the main results on candidacy, voting, and policy alignment, we provide further evidence that helps unpack mechanisms. We begin by considering two potential reasons for why candidacy behavior changed as a result of social vs personal messaging: candidacy behavior could have changed because encouragements to run for office made salient certain aspects of office (salience channel); and/or encouragements could have triggered social influence such as peer encouragement or changes in second order beliefs to influence an individuals decision calculus vis-a-vis political entry (social influence channel). To examine these first set of mechanisms, we make use of a further randomization in our experimental design: besides randomizing social versus personal encouragements at the village level, we also randomize at the village level 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. We find that social versus personal returns messaging increases candidacy, election, and policy alignment when encouragements are provided in public but not when they are provided in private. Our interpretation of these findings is that instead of improving the salience of certain aspects of office to prospective candidates, social versus personal encouragements likely operated through social influence where public signaling, common knowledge, and/or community coordination are important (Ashraf and Bandiera 2018; Tankard and Paluck 2016; Bursztyn and Jensen 2017; Harmon et al. 2019; DellaVigna et al. 2016). 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.

Besides mechanisms on the candidacy decision, we also examine mechanisms for why policy outcomes changed with treatments. We examine if the effects we observe are likely the result of changes in who runs for office (the selection channel) or whether they are

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

caused by different incentives politicians face once elected (the incentives channel), such as treatment-induced changes in citizens' policy preferences or accountability seeking behavior. We evaluate each channel in turn. First, we conduct sub-group analysis to show that people who are more pro-social on a pre-treatment survey measure are more likely to run for office and get elected when social versus personal benefits are highlighted in the village. Next, though conditioning on the post-treatment variable of election to the council, we show that social versus personal returns messaging made the pool of elected politicians from our sample more educated. Second, we examine the incentives channel and show that the policy alignment results do not arise because of changes in citizen preferences, which remain stable. Instead policy alignment is explained by changes in politician behavior. We further show that citizen behavior, as measured by the number of meetings they hold with elected politicians, also does not explain treatment effects. Taken together these results make a case for selection over the incentives channel in explaining policy alignment. That is, the likely mechanism for the policy alignment effects is a change in who ran for office and got elected rather than differences in citizens' behavior or preferences post-election.

This paper makes several contributions. Social scientists have spent considerable energy to build a body of knowledge on how to move democracies to be more responsive to citizens.[9] 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; Fisman et al. 2015) and bureaucratic selection (Ashraf et al. 2020) shows that pecuniary (Dal Bó et al. 2013; Deserranno 2019) and career incentives (Bertrand et al. 2020) matter in recruiting agents who are more competent, and thus may improve policy, our examination of pro-social motivation remains understudied.[10] This is identified as an open

[9]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).

[10]An exception is Barfort et al. (2019) who carry out a survey experiment in Denmark to show that pro-social instead of pecuniary returns are more likely to motivate honest individuals to enter public service.

question in a recent review of the political selection literature that says "while we have made progress in documenting some 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). For instance, aligning an individual's motivations with the organizational mission can compensate for low powered incentives (Besley and Ghatak 2005). Similarly, non-financial rewards have been shown to boost performance particularly on pro-social tasks (Ashraf et al. 2014). In the same spirit, this paper provides evidence that non-financial motivations, specifically pro-social aspects of the political job, can motivate candidacy and make representatives in poorer countries 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.

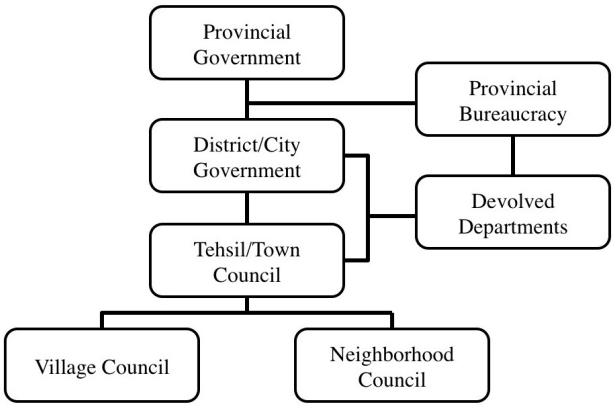


Figure 1: **Village Councils in Political Hierarchy**

2.2 Village Councils

We focus on village councils that represent about 6,500 voters on average (see Table A2 for descriptives).[11] Consistent with local elections in South Asia, voter turnout in 2015 was high

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

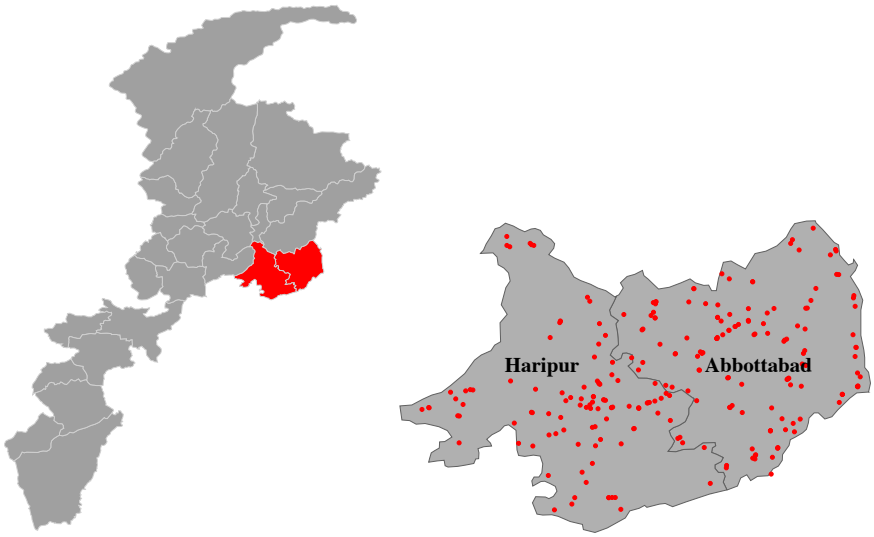


Figure 2: **Villages in Haripur and Abbottabad Districts (right) in Khyber Pakhtunkwa Province (left)**

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.

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



Figure 3: **Private One-on-One 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 selected via random walk in every village for a total of 9,310 people across 192 villages in the experimental sample.[12] Once a household is approached, enumerators conduct a short survey with a male respondent.[13] 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.[14]

[12]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.

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

[14]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

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.



Figure 4: **Public Meetings in Villages**

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 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

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 A16 shows that there is no evidence for differential selection into the public meeting by social versus personal treatments.

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. Unfortunately, a detailed baseline survey was not possible because of the short election timeline. 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 presents the overall design of the experiment across 192 villages. Villages are placed into 12 blocks by our field partner based on geographic proximity of and access constraints to villages. All treatments are block-randomized at the village level. 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.

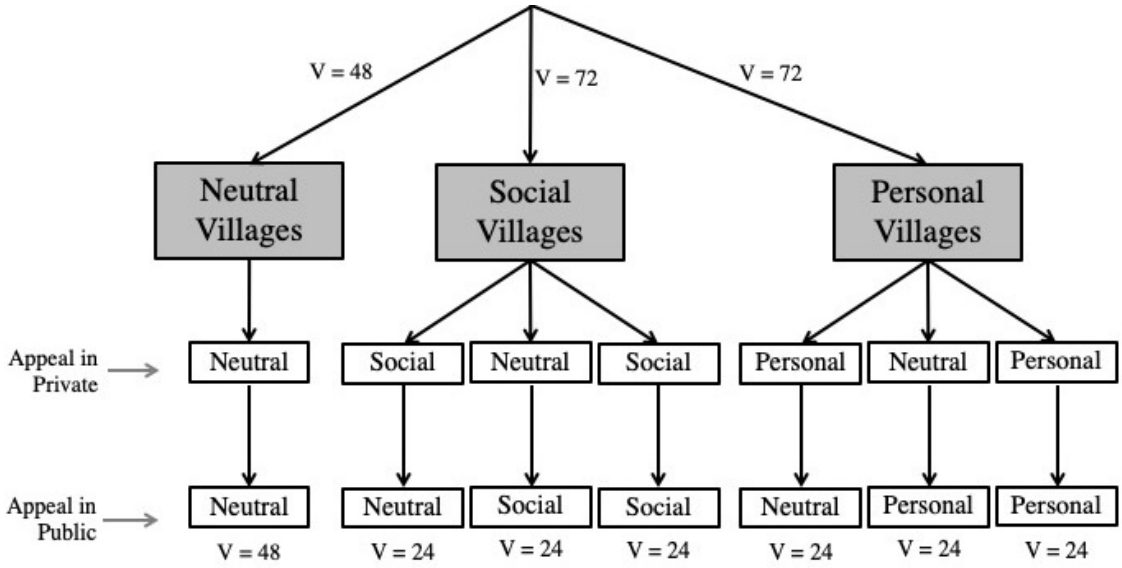


Figure 5: **Design of Field Experiment**

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 bottom two layers of the figure show the type of appeal made to a person to run for office. See text for details.

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).[15] 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 social and personal 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, and exploratory, analyses.

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

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 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.[16]

Table 2: **Effects on Candidacy and Election**

	Candidate=1 (1)	Elected=1 (2)
Social vs Personal	0.018***	0.012***
	(0.007)	(0.004)
	[0.007]	[0.007]
Social vs Neutral	0.010	0.005
	(0.008)	(0.005)
	[0.111]	[0.173]
Personal vs Neutral	-0.009	-0.007**
	(0.006)	(0.003)
	[0.153]	[0.096]
Neutral Mean	0.030	0.017
# Villages	192	192
# Observations	9310	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. "Elected" equals 1 if the individual won office and zero otherwise. 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.

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 Column 1 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 sample 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).

[16]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.

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. In this sense, we might expect that the new candidates that have put themselves forward have a negligible chance of getting elected to office.

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 Column 2 of Table 2 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).[17] 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.

4.4 Results on Policy Outcomes

Next, we evaluate if these changes affect policy. Evaluating changes in policy outcomes, while important, 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. Our design where we randomize 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 amount of resources are distributed within the community.

4.4.1 No policy change on the extensive margin

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.[18] To test this hypothesis, we return to

[17]Further, while no longer causally identified, we also find that the probability of election *conditional* on candidacy in social versus personal villages is 0.09 percentage point higher (exact-p = 0.14). Though statistically imprecise, this indicates that citizens carry a preference for the new politicians.

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

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.[19]

Examining the extensive margin, we confirm that our treatments do not explain any changes to the amount sanctioned to Village Councils (see Appendix Table A12). The data show that there is good adherence to rules on this margin.

4.4.2 Policy aligns with citizen preferences on the intensive margin

What remains is an examination of the intensive margin or how money is spent by the Village Councils. This decision is more under the control 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.[20] When we return to the field a year after elections we also survey a random sample of 1318 citizens in our sample villages to collect their spending preferences over budgets.[21] 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 Municipal Services, Infrastructure, Community, and a residual category that stores preferences that are officially not the primary responsibility of the village council.[22]

Figure 6 plots the distribution of these citizen preferences against how councils actually

[19] 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.

[20] 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).

[21] One might be concerned that citizen preferences themselves could be affected by treatment. We discuss in Section 5.2.2 below that this is not the case.

[22] *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.

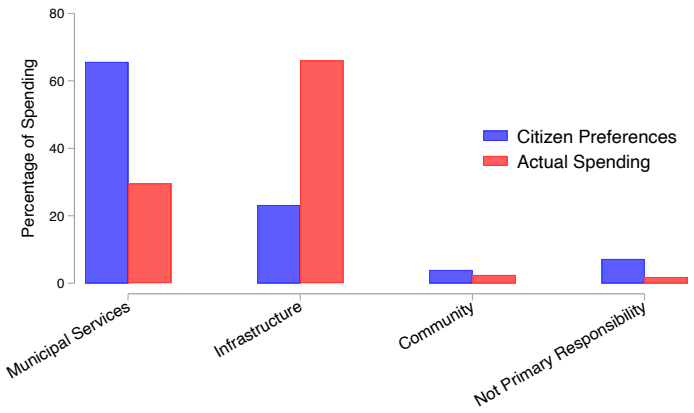


Figure 6: Citizen Preferences and Council Spending in Neutral Villages

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.

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 citizens and politicians are generally good at recognizing activities that are not the primary responsibility 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 and 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 preferences by summing over all four budget categories as follows: $\sqrt{\sum_J (B_{ji} - \overline{C}_{ji})^2}$.

Table 3 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.

Table 3: **Policy Effects: Distance between Citizen Preferences & 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.

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.[23]

4.4.3 Effects on Citizen Satisfaction and Trust

Next, we evaluate impacts of treatments on citizen satisfaction with policy decision in terms of projects selected for implementation, as well as broader trust in state institutions. After the councils decide on projects, and during our citizen survey, we ask citizens to rate their approval of the projects selected by the council. We find that citizens are 17.3 percent point (exact $p = 0.037$) more likely to say that they are satisfied with the specific projects chosen by their village council in social versus personal treatment villages (see Column 1 of Appendix Table A14). Citizens are also 12.1 percent point (exact $p = 0.094$) more likely to state that they trust the state in social versus personal treatment villages (Column 2). To code this index, we first ask citizens to tell us if they agree with the statement 'politics is a dirty

[23]Appendix Table A13 presents the results in tabular form.

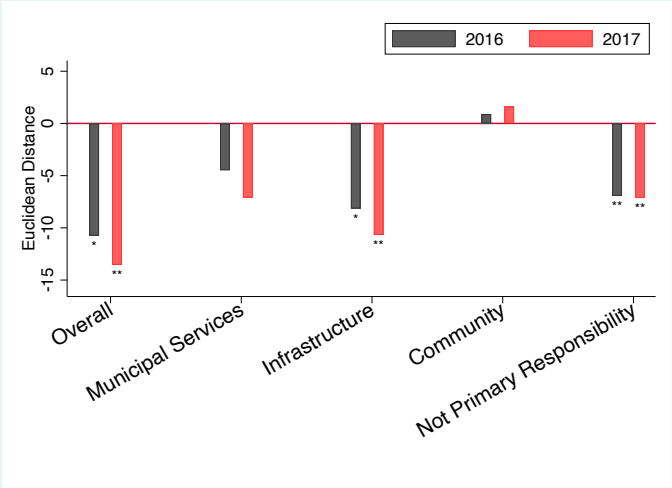


Figure 7: **Social versus Personal Policy Alignment Over Time (District Haripur only)**

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

word' to get their perception of overall politics. We convert the answers to a dichotomous variable that takes on a value of 1 to answers that did not agree with the statement and zero otherwise. We find the social versus personal villages increase disagreement with the statement by 12.2 percentage points (exact $p = 0.077$). Second, we also ask citizens their perceived likelihood of their lost wallet being returned by a public servant (police or some other functionary) if they found it. The answers are dichotomized on the median of the responses with 1 indicating higher trust in public servants and zero otherwise. The results on this variable are less precisely estimated: we observe a 0.096 increase in trust (exact $p = 0.130$).

Taken together, these results suggest that the projects chosen by councils have aligned better in social versus personal villages, and that citizens are more satisfied with these choices and exhibit more positive affect towards politics and the state (Acemoglu et al. 2020).

5 Mechanisms

In this section, we conduct exploratory analyses on potential mechanisms for the main experimental effects we report above. To summarize, we evaluate two sets of competing hypotheses regarding the likely mechanisms for our experimental results. First, focusing on why candidacy decisions changed, we show that treatment effects arise in public, but not private, meetings. This indicates that social influence is more important than information salience in our setting. These effects are causally identified. Second, focusing on why policy outcomes

changed, we conduct more exploratory analyses to show that treatment effects likely emerge from different (pro-social) types of people seeking office instead of changes in the incentives politicians faced in office as proxied by changes in citizen preferences and behavior.

5.1 Why Candidacy Decisions Changed? Salience and Social Influence

The results so far show that the pro-social portrayal of political office affects candidacy and election, and that this has a direct bearing on policy alignment with the preference of the electorate. Among others, we examine two potential explanations for why we may see these results on candidacy. First, it could be the case that encouragements to run for office that vary how political office is portrayed make salient aspects of political office for prospective candidates. Since, at the individual level, the decision to run depends on expected net benefits, including monetary and psychological, a prospective politician may receive from office, new information could sway the status-quo decision by strengthening the perceived association of political office with one type of benefit over the other. We label this first potential mechanism as 'salience'. It is perhaps reasonable to expect people to have limited information about what the specific new village government entails, though from our work in the field we note that people certainly have prior beliefs about what politics in general looks like.

Second, a large literature argues that social interactions play a key role in political behavior, in particular the decision to vote (Gerber et al. 2008; Bond et al. 2012; DellaVigna et al. 2016; Harmon et al. 2019). Theoretical work also suggests that these motivations should matter in prospective politicians' 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), potentially, impacted by people's opinions of the candidate, as well as a prospective candidate's evaluations of what others think.

It is likely that the direction of these two effects will vary by beliefs around the underlying reasons to seek office. Publicly highlighting the *social* benefits of office can increase the likelihood that people put themselves forward as candidates 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, publicly highlighting that political office is a vehicle for delivering societal benefits can help coordinate individuals to seek potential candidates who

fit the profile. It may also serve a community reminder to find and ask such people to run. In these ways, 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 can 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 prospective candidates. These explanations fall under the umbrella of 'social influence' as individual actions are influenced by the beliefs or actions of others in the community.

Estimation Using the randomization scheme of the experiment, we can study if the treatment effects are primarily arise from salience or social influence. The experiment randomizes whether benefits from office are randomly delivered in public versus private one-on-one meetings. We now decompose our main results to areas where treatments were either only delivered in public or only in private. We compare these villages to places where a neutral message was delivered in private and public. As before we focus on our subject pool of 9,310 individuals in 192 treatment villages and 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. We can therefore 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.[24]

Results Table 4 presents the results on candidacy, election, and overall policy alignment.[25] We see that the main treatments effects we described above are concentrated primarily in areas where messages were delivered in public meetings. When social benefits from office

[24] 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 A16).

[25] See appendix Table A15 for disaggregated policy effects for each budget category.

Table 4: **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: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Columns 1, 2, 4 and 5 use 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. Columns 3 and 6 use a village level dataset. "Policy Euclidean Distance" uses the euclidean distance in budget spending between policy decisions and citizens preferences as described in section 4.4. The first three columns report the effect of treatments delivered in public and the remaining columns 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.

are highlighted in public, relative to personal benefits, the probability that a person runs increases by 0.02 percentage points (exact p= 0.054); the probability that they win the election increases by 0.011 percentage points (exact p= 0.072); and subsequent policy alignment improves by 17.6 points (exact p= 0.015). We see no similar effects either in size or statistical significance for the case where messages are delivered only in private meetings. Though we have limited statistical ability to detect this, from the direction of change in point estimates in social versus neutral and personal versus neutral villages, we can see that the effects come from social messages increasing candidacy, election, and policy alignment, and personal messages decreasing them.

Taken together, our takeaway from these results is that information salience mattered less than social influence in determining who decided to enter the race, as well as subsequent election and policy outcomes. This is important because galvanizing candidates around the usual norm of candidacy to help others could be a particularly powerful tool.

5.2 Why Policy Outcomes Changed? Selection and Incentives

The results we present above can operate through at least two channels. It could be that different types of people decided to run for office as a result of treatments and as a consequence of this they chose different policy once elected (we label this the 'selection channel').

Alternatively, treatments perhaps did not change the types of people who ran, but they changed citizens' preferences or behavior in two potential ways that contributed to changes in policy outcomes. First, treatments could have directly affected citizens' policy preferences such that the bulk of the policy alignment effect we observe is explained by changes in citizen preferences instead of changes in politician behavior. This could be the case if, for instance, treatments informed citizens of the various tasks that politicians can perform and this information changed what citizens wanted the government to do. Second, it could be the case that the policy alignment effects we observe are explained by changes in the degree to which the electorate held the political class to account. We label these channels of change that run through citizens the 'incentives channel'.

5.2.1 Selection channel

We check for evidence related to the selection channel in two ways. First, we check if pro-social types in our experimental sample were more likely to respond to treatment. Second, we check if treatments affected the pool of candidates and elected councillors in our sample in terms of their education, income, and occupation. Though there are data limitations when we attempt to answer these questions, as we describe below, there are some interesting patterns in our data that help shed light on the selection channel.

Heterogeneous Effects by Pro-social types. First, 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 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.[26] The benefit of this strategy is that it maximizes our 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

[26]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'.

behavioral measure of pro-sociality.[27] As such, we treat this analysis as exploratory.

We take the average of responses across the four questions 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 that we report in 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). In addition, we also find that the increase in the probability of election is also larger for high versus low pro-social types: the difference is 1.4 percentage points (exact-p = 0.068).[28]

Examining the profile of the candidate and elected pools. Second, we examine if the profiles of people who run for office in our sample, and those that get elected, differ by treatment arms. This exercise is exploratory in nature because we condition the sample on people who have already decided to run for office as well as those that are already elected.

We present the results in Appendix Table A19. Broadly, we find that social versus personal villages exhibit no differences in the candidate pool as measured by years of schooling, income, and occupation. However, since the treatments increased the probability of candidacy in social villages and reduced it in personal villages, it could be that higher ability individuals entered the race in social villages, while low ability individuals exited the race in personal villages. There is some evidence consistent with this when we decompose treatment effects: candidates in social versus neutral villages report more income and are more

[27]The concern here is that instead of treatments motivating the most pro-social types to enter politics, they motivate those with the strongest social desirability concerns. It is unclear, however, why those with greater social desirability get elected by voters or implement better policy outcomes as we observe with our treatments.

[28]Since pro-social motivations are not randomized, as a robustness exercise, we check if the distribution of pro-sociality 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 A17. 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 A17. In the Appendix J.3, we re-generate Table A18 with the inclusion of this village level proportion of community-minded people as a control and find that the point estimates are robust and 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 villages.

Table 5: **Sub-group Effects by Pro-Social Type**

	Candidate=1			**Elected=1**		
Pro-social type:	*Low only*	*High only*	*High vs Low*	*Low only*	*High only*	*High vs Low*
	(1)	(2)	(3)	(4)	(5)	(6)
Social vs Personal	0.010	0.027***	0.019	0.006	0.019***	0.014
	(0.007)	(0.009)	(0.012)	(0.004)	(0.007)	(0.008)
	[0.083]	[0.011]	[0.069]	[0.111]	[0.014]	[0.068]
Social vs Neutral	0.010	0.010	0.002	0.004	0.006	0.003
	(0.008)	(0.012)	(0.014)	(0.005)	(0.009)	(0.011)
	[0.121]	[0.214]	[0.447]	[0.217]	[0.265]	[0.410]
Personal vs Neutral	-0.000	-0.017*	-0.017	-0.002	-0.013**	-0.011
	(0.005)	(0.009)	(0.010)	(0.004)	(0.006)	(0.008)
	[0.475]	[0.103]	[0.121]	[0.382]	[0.088]	[0.146]
# Villages	192	192	192	192	192	192
# Observations	5056	4254	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 variables are "Candidate" and "Elected" that take a value of one if the individual ran for office and got elected respectively, and zero otherwise. Columns 1 and 2 report sub-group analysis on Candidate as an outcome based on pro-social type of the individual. Column 1 reports the effects of treatments for "low" pro-social type and Column 2 reports the effects for "high" pro-social type. Column 3 reports the difference in effects between columns 2 and 3. Columns 4-6 report similar effects for Elected as an outcome. 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.

likely to be self-employed,[29] while candidates in personal versus neutral villages also report higher income and less likely to be students. Finally, examining the pool of people elected to village councils suggests that voters further screen candidates. People elected from our experimental sample in social versus personal villages have more education.

Taken together, the results on pro-sociality and the profile of people contesting elections provide suggestive evidence for the selection channel: treatments changed who decided to contest elections, and who voters chose from that list.

5.2.2 Incentives channel

We evaluate the incentives channel in two ways. First, we examine if policy alignment is explained by changes in citizen preferences or changes in politician behavior. Second, we examine if citizen behavior is directly affected by treatments.

Results are not explained by changes in citizen preferences Since there are two components of the Euclidean distance that measures policy alignment, citizen preferences

[29]Self-employed are usually people who are shopkeepers at corner stores.

and official budgets decided by elected politicians, 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, which is consistent with the idea that it is changes on the politician's side that drive improvements in policy. Alternatively, the treatments could have made 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 A20: 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.

Citizen behavior after elections does not explain results. An alternative approach to evaluating treatment effects is the idea that treatments had little to do with who contested elections and mostly operated through changes in citizen behavior. While it is unclear how this mechanism might also increase the rate of candidacy that we document in our experimental results, we nonetheless evaluate if it is indeed the case that citizens are exerting more effort towards holding their representatives to account or communicating their preferences (which are not different) to politicians.

During the citizen survey that was conducted a year after councils were elected, we asked citizens if they had met with anyone from the village council in the previous month. We sum the total number of meetings reported in the village in our sample and show in Appendix Table A21 that while the baseline rate of political engagement is fairly high at over 80 meetings per neutral village, there is no difference in meetings held in social versus personal villages, suggesting that changes in citizen contact are is not the likely mechanism for the effects we discuss above.[30]

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

[30]If anything, both social and personal villages exhibit slightly fewer meetings with councillors compared to neutral villages.

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 pro-social 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,[31] this first result outlines that it is perhaps possible to improve the supply 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.

[31] 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. 2014).

References

Acemoglu, Daron, Ali Cheema, Asim I Khwaja, and James A Robinson, "Trust in state and nonstate actors: Evidence from dispute resolution in Pakistan," *Journal of Political Economy*, 2020, *128* (8), 3090–3147.

Andreoni, James, "Impure Altruism and Donations to Public Goods: A Theory of Warm-Glow Giving," *The Economic Journal*, 1990, *100* (401), 464–477.

_ , "Cooperation in Public-Goods Experiments: Kindness or Confusion?," *The American Economic Review*, 1995, *85* (4), 891–904.

_ , "Warm-Glow versus Cold-Prickle: The Effects of Positive and Negative Framing on Cooperation in Experiments*," *The Quarterly Journal of Economics*, 02 1995, *110* (1), 1–21.

Arias, Eric, Pablo Balán, Horacio Larreguy, John Marshall, and Pablo Querubín, "Information provision, voter coordination, and electoral accountability: Evidence from Mexican social networks," *American Political Science Review*, 2019, *113* (2), 475–498.

Ashraf, Nava and Oriana Bandiera, "Social incentives in organizations," *Annual Review of Economics*, 2018, *10*, 439–463.

_ , _ , and Kelsey Jack, "No Margin, No Mission? A Field Experiment on Incentives for Public Service Delivery," *Journal of Public Economics*, 2014, pp. 1–46.

_ , _ , Edward Davenport, and Scott S Lee, "Losing Prosociality in the Quest for Talent? Sorting, Selection, and Productivity in the Delivery of Public Services," *American Economic Review*, 2020.

Bandiera, Oriana, Michael Callen, Katherine Casey, Eliana La, Camille Landais Ferrara, and Matthieu Teachout, "State effectiveness," *IGC Working Paper*, 2019.

Barfort, Sebastian, Nikolaj A Harmon, Frederik Hjorth, and Asmus Leth Olsen, "Sustaining Honesty in Public Service: The Role of Selection," *American Economic Journal: Economic Policy*, November 2019, *11* (4), 96–123.

Benabou, Roland and Jean Tirole, "Intrinsic and extrinsic motivation," *The review of economic studies*, 2003, *70* (3), 489–520.

Bénabou, Roland and Jean Tirole, "Incentives and prosocial behavior," *American economic review*, 2006, *96* (5), 1652–1678.

Bertrand, Marianne, Robin Burgess, Arunish Chawla, and Guo Xu, "The glittering prizes: Career incentives and bureaucrat performance," *The Review of Economic Studies*, 2020, *87* (2), 626–655.

Besley, Timothy, "Political selection," *Journal of Economic perspectives*, 2005, *19* (3), 43–60.

_ , *Principled agents?: The political economy of good government*, Oxford University Press on Demand, 2006.

_ **and Maitreesh Ghatak**, "Competition and incentives with motivated agents," *American Economic Review*, 2005, *95* (3), 616–636.

_ **and Stephen Coate**, "An economic model of representative democracy," *The Quarterly Journal of Economics*, 1997, (February).

Blair, Graeme, Rebecca Littman, and Elizabeth Levy Paluck, "Motivating the adoption of new community-minded behaviors: An empirical test in Nigeria," *Science advances*, 2019, *5* (3), eaau5175.

Blaydes, Lisa, *Elections and distributive politics in Mubaraks Egypt*, Cambridge University Press, 2010.

Bond, Robert M, Christopher J Fariss, Jason J Jones, Adam D I Kramer, Cameron Marlow, Jaime E Settle, and James H Fowler, "A 61-million-person experiment in social influence and political mobilization," *Nature*, sep 2012, *489* (7415), 295–298.

Broockman, David E, "Black politicians are more intrinsically motivated to advance blacks? interests: A field experiment manipulating political incentives," *American Journal of Political Science*, 2013, *57* (3), 521–536.

Burgess, Robin, Remi Jedwab, Edward Miguel, Ameet Morjaria, and Gerard Padró i Miquel, "The value of democracy: evidence from road building in Kenya," *American Economic Review*, 2015, *105* (6), 1817–51.

Bursztyn, Leonardo and Robert Jensen, "Social image and economic behavior in the field: Identifying, understanding, and shaping social pressure," *Annual Review of Economics*, 2017, *9*, 131–153.

Caselli, Francesco and Massimo Morelli, "Bad politicians," *Journal of Public Economics*, 2004, *88* (3-4), 759–782.

Casey, Katherine, Abou Bakarr Kamara, and Niccoló Meriggi, "An experiment in candidate selection," Technical Report, American Economic Review 2021.

Chattopadhyay, R and E Duflo, "Women as policy makers: Evidence from a randomized policy experiment in India," *Econometrica*, 2004, *72* (5), 1409–1443.

Cheema, Ali, Adnan Khan, and Roger Myerson, " Breaking the Counter-cyclical Pattern of Local Democracy in Pakistan," 2010, (May).

Cruz, Cesi, Julien Labonne, and Pablo Querubin, "Politician Family Networks and Electoral Outcomes: Evidence from the Philippines," *American Economic Review*, 2017.

Dahl, Robert Alan, *Polyarchy: Participation and opposition*, Yale University Press, 1973.

Dal Bó, Ernesto and Frederico Finan, "Progress and perspectives in the study of political selection," *Annual Review of Economics*, 2018, *10*, 541–575.

Dal Bó, Ernesto, Frederico Finan, and Martín a Rossi, "Srengthening State Capabilities: The Role of Financial Incentives in the Call to Public Service," *Quarterly Journal of Economics*, 2013.

Deci, Edward L, "Effects of externally mediated rewards on intrinsic motivation.," *Journal of personality and Social Psychology*, 1971, *18* (1), 105.

_ , "Intrinsic motivation, extrinsic reinforcement, and inequity.," *Journal of personality and social psychology*, 1972, *22* (1), 113.

DellaVigna, Stefano, John A List, Ulrike Malmendier, and Gautam Rao, "Voting to tell others," *The Review of Economic Studies*, 2016, *84* (1), 143–181.

Deserranno, Erika, "Financial incentives as signals: experimental evidence from the recruitment of village promoters in Uganda," *American Economic Journal: Applied Economics*, 2019, *11* (1), 277–317.

Dunning, Thad, Guy Grossman, Macartan Humphreys, Susan D Hyde, Craig McIntosh, and Gareth Nellis, *Information, Accountability, and Cumulative Learning: Lessons from Metaketa I*, Cambridge University Press, 2019.

Eggers, Andrew C and Jens Hainmueller, "MPs for sale? Returns to office in postwar British politics," *American Political Science Review*, 2009, *103* (4), 513–533.

Ferraz, Claudio and Frederico Finan, "Electoral accountability and corruption: Evidence from the audits of local governments," *American Economic Review*, 2011, *101* (June), 1274–1311.

_ **and** _ , "Motivating politicians: The impacts of monetary incentives on quality and performance," *Working Paper*, 2011.

Fisman, Raymond, Florian Schulz, and Vikrant Vig, "The private returns to public office," *Journal of Political Economy*, 2014, *122* (4), 806–862.

_ **, Nikolaj A Harmon, Emir Kamenica, and Inger Munk**, "Labor supply of politicians," *Journal of the European Economic Association*, 2015, *13* (5), 871–905.

Folke, Olle, Torsten Persson, and Johanna Rickne, "Dynastic political rents? Economic benefits to relatives of top politicians," *Economic Journal*, 2017.

Frey, Bruno S, "Not just for the money," 1997.

Fujiwara, Thomas, "Voting Technology, Political Responsiveness, and Infant Health: Evidence from Brazil," *Econometrica*, 2015.

Gagliarducci, Stefano and Tommaso Nannicini, "Do better paid politicians perform better? Disentangling incentives from selection," *Journal of the European Economic Association*, 2013, *11* (2), 369–398.

Gerber, Alan S and Donald P Green, *Field experiments: Design, analysis, and interpretation*, WW Norton, 2012.

Gerber, Alan S., Donald P. Green, and Christopher W. Larimer, "Social Pressure and Voter Turnout: Evidence from a Large-Scale Field Experiment," *American Political Science Review*, 2008, *102* (01), 33–48.

Grossman, Guy and Kristin Michelitch, "Information dissemination, competitive pressure, and politician performance between elections: A field experiment in Uganda," *American Political Science Review*, 2018, *112* (2), 280–301.

Gulzar, Saad, Nicholas Haas, and Benjamin Pasquale, "Does Political Affirmative Action Work, and For Whom? Theory and Evidence on India's Scheduled Areas," *American Political Science Review*, 2020.

Harmon, Nikolaj, Raymond Fisman, and Emir Kamenica, "Peer effects in legislative voting," *American Economic Journal: Applied Economics*, 2019, *11* (4), 156–80.

Humphreys, Macartan and Jeremy Weinstein, "Policing politicians: Citizen empowerment and political accountability in Uganda," *Unpublished manuscript*, 2012.

Inglehart, R., C. Haerpfer, A. Moreno, C. Welzel, K. Kizilova, J. Diez-Medrano, M. Lagos, P. Norris, E. Ponarin, and B. Puranen, "World Values Survey: Round Six - Country-Pooled Datafile," Technical Report 2014.

Joseph, A Schlesinger, "Ambition and politics: political careers in the United States," *Chicago: Rand NcNally*, 1966.

Landmann, Andreas and Bjorn Vollan, "Pro-sociality of democratic leaders: Evidence from village elections in the Philippines," *Working Paper*, 2020.

Lipset, Seymour Martin, "Political man: The social bases of politics," 1959.

Malik, Rabia, "(A) Political Constituency Development Funds: Evidence from Pakistan," *British Journal of Political Science*, 2019, pp. 1–18.

Martinez-Bravo, Monica, "The Role of Local Officials in New Democracies: Evidence From Indonesia," *American Economic Review*, 2014, pp. 1–49.

_ , Priya Mukherjee, and Andreas Stegmann, "The Non-Democratic Roots of Elite Capture: Evidence From Soeharto Mayors in Indonesia," *Econometrica*, 2017, *85* (6), 1991–2010.

MICS, "Report on Multiple Indicator Cluster Survey," 2008.

Myerson, Roger, "Local foundations for strong democracy in Pakistan," *Working Paper*, 2009.

Ostrom, Elinor, "Collective action and the evolution of social norms," *Journal of economic perspectives*, 2000, *14* (3), 137–158.

Pande, Rohini, "Can informed voters enforce better governance? Experiments in low-income democracies," *Annual Review of Economics*, 2011.

Prakash, Nishith, Marc Rockmore, and Yogesh Uppal, "Do criminally accused politicians affect economic outcomes? Evidence from India," *Journal of Development Economics*, 2019, *141*, 102370.

Querubin, Pablo et al., "Family and politics: Dynastic persistence in the Philippines," *Quarterly Journal of Political Science*, 2016, *11* (2), 151–181.

Ravanilla, Nico, "Attracting Good People into Public Service: Evidence from a Field Experiment in the Philippines," *Working Paper*, 2016.

Reinikka, Ritva and Jakob Svensson, "Local capture: evidence from a central government transfer program in Uganda," *The quarterly journal of economics*, 2004, *119* (2), 679–705.

Salim, Ahmed, "Political Parties in Pakistan," 2005, *12* (1).

Tankard, Margaret E and Elizabeth Levy Paluck, "Norm perception as a vehicle for social change," *Social Issues and Policy Review*, 2016, *10* (1), 181–211.

Vaishnav, Milan, *When crime pays: Money and muscle in Indian politics*, Yale University Press, 2017.

Young, Alwyn, "Channelling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results," *The Quarterly Journal of Economics*, 2019.

ONLINE APPENDIX

Contents

A Details of How Candidacy is Declared **2**

B Sampling procedure **2**

C Scripts for Conversations **3**

D Timeline **4**

E Ethical considerations **4**

F Linkages and Deviations from Pre-Analysis Plan **6**

G Summary Statistics and Balance Tables **9**

H Additional Results on Candidacy **14**

H.1 Village level Effects on Candidate Pool Size 14

I Additional Results on Performance of Councils **15**

I.1 Summary Statistics on Preferences and Budgets 15

I.2 Missing Data Analysis 16

I.3 Policy Effects on Extensive Margin 18

I.4 Treatments improve citizens satisfaction with chosen policy 20

J Additional Results on Mechanisms **21**

J.1 Policy Effects of Public and Private Treatments 21

J.2 Selection into Public Meeting 22

J.3 Robustness Checks for Sub-group Analysis by Pro-Social Type 22

J.4 Profile of Candidate and Elected Pools 25

J.5 Decomposing Policy Effects 26

J.6 Citizen's Behavior 27

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 (**?**). However, **?** 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 scope of IRB which we want to address in this note. We take guidance from the framework of ethical concerns presented in **?**

Policy Equipoise: Theoretically it is unclear whether people attracted with pro-social messaging will become better politicians or those driven by the possibility of ego-rents. As explained in the paper, while pro-social messaging may attract more community minded persons it is unclear if such people are good at delivering public goods. Similarly, ego-rents may attract higher ability people who have the potential to be good policy makers/politicians. Due to these competing predictions, we believe the treatments in this paper are in policy equipoise.

Role of researchers with respect to implementation: The researchers actively participated in the project. This fact was conveyed to the subjects through verbal consent approved by NYU IRB via protocol number 15-10593. However, there may be a concern that western researchers may ask the subjects 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 and the other one has spent significant time in that province. 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.

Potential harms to participants or nonparticipants from the interventions or policies: Ex-ante there was no potential of harm to participants. The decision to run for office or not is a right of every citizen above the age of 18 years. One additional concern may be that the project provided valuable information to a select few citizens in the village and hence gave them an undue advantage in terms of candidacy. However, this concern is ameliorated because of two reasons. One, the people selected for to be a part of our experimental sample were drawn at random from the village and every potential male citizen had an equal chance of being selected. Second, our treatments always included community meetings that were open to everyone in the village. This was intentionally designed as such to gave an additional opportunity to people not selected for the in-person arm to receive the same information in the public meeting. There is, however, a concern left about women being excluded from the process. This is a limitation that arose from our desire to adhere to local norms. Local norms prohibit interaction between outsider men and local women. Since, our team was made up of male enumerators we could not reach women citizens while respecting the local norms. Further, due to similar concerns for respecting local norms, as well as limitation of our resources, we could not hire women enumerators and bus them to locations to approach the women citizens. We are circumventing some of these problems in our subsequent work as enabling women's political participation is an important question in the context of Pakistan.

Potential harms to research participants or research staff from data collection: The IRB for data protections were approved by several Human Subject Review boards. We did not find any potential of harm to research participants or staff during the implementation of the study. One concern that might be important here relates to the results on policy outcomes where we see that in some villages the gap between budgetary spending and citizen preferences increases, that is policy outcomes become worse. Ex-ante, as we discuss at length

in the paper, and in the policy equipoise discussion above, it was unclear if 1) our relatively light touch treatments would impact at all people's decisions to run for office, 2) if voters would even elect the people who decided to run because of the treatments, and 3) if the politicians would behave differently once elected. There are compelling theoretical reasons to believe that policy outcomes would not change even if who was running and who got elected changed. We see our paper as a first data point in connecting these motivations for candidacy with policy outcomes, and because of the importance of these questions, hope that we have more research on this question so that we can have a better evidence base for what motivations yield a political class that performs well in office, and how we might mobilize such politicians.

Financial and reputational conflicts of interest: The researcher had no conflict of interest.

Intellectual freedom: The researchers have complete intellectual freedom to report the results of this study.

Feedback to participants or communities: We did not have resources to provide feedback to each of the 9310 participants after the project.

Foreseeable misuse of research results: We do not foresee any potential of misuse of the research results.

Other Ethics Issues to Discuss: None to our knowledge.

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 **?**, 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. The analysis in section 5.1 was pre-registered as Analysis 2.1 in the plan. Lastly, some variables

used in the analysis in section 5.2 were not pre-registered as indicated in table A1.

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

Variable	**Var Type**	**Tables in Paper**	**Registered in PAP?**	**PAP Section 5 variable #**
Primary Outcomes:				
Filed papers	Outcome	Table 2	Yes	4
Elected to council	Outcome	Table 2	Yes	5
Budget and Preferences	Outcome	Table 3	No	
Mechanisms:				
Satisfaction with Projects Chosen	Outcome	Table A14	No	
Trust in State Index	Outcome	Table A14	No	
Doesn't Hold Negative View of Politics	Outcome	Table A14	No	
Trusts Public Servants	Outcome	Table A14	No	
Pro-Social Type	Moderator	Table 5	Yes	19
Family Income	Moderator	Table A19	No	
Years of Schooling	Moderator	Table A19	Yes	16
Occupation	Moderator	Table A19	No	
Number of Meetings with Councilors	Outcome	Table A21	No	

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 A10 provides evidence that treatments do not predict missing data.

Table A4: **Village Characteristics Balance for Sub-Treatments**

	Village Pop (1)	Number Settlements (2)	Dist Road (3)	Dist HQ (4)	Dist Teh HQ (5)	Long (6)	Lat (7)	Num Gen Seats (8)	Influential Families (9)
A. Neutral Private, Neutral Public	4188.384	2.363	1.738	20.863	12.617	34.055	73.167	6.487	7.186
	(730.981)	(0.376)	(2.302)	(3.432)	(3.838)	(0.018)	(0.023)	(0.398)	(2.459)
B. Personal Private, Neutral Public	3970.954	2.332	-0.094	20.005	13.560	34.047	73.193	6.343	8.112
	(689.699)	(0.439)	(2.472)	(3.304)	(4.151)	(0.022)	(0.024)	(0.378)	(2.427)
C. Neutral Private, Personal Public	4290.217	2.316	4.545	23.819	13.530	34.033	73.167	6.360	7.118
	(670.607)	(0.422)	(3.945)	(3.387)	(4.363)	(0.019)	(0.023)	(0.370)	(2.794)
D. Personal Private, Personal Public	4187.863	2.611	1.862	17.463	8.431	34.033	73.187	6.378	7.673
	(703.264)	(0.438)	(4.021)	(3.288)	(3.883)	(0.021)	(0.024)	(0.376)	(2.601)
E. Social Private, Neutral Public	4947.825	2.431	3.303	15.294	6.982	34.036	73.183	6.795	7.108
	(662.900)	(0.358)	(2.331)	(3.294)	(3.674)	(0.021)	(0.024)	(0.360)	(2.672)
F. Neutral Private, Social Public	3723.231	2.022	2.528	16.009	8.753	34.038	73.181	6.171	7.099
	(647.360)	(0.397)	(2.666)	(3.515)	(4.061)	(0.020)	(0.028)	(0.361)	(2.476)
G. Social Private, Social Public	3811.318	2.132	8.078	13.391	6.528	34.060	73.172	6.184	6.412
	(705.307)	(0.424)	(5.398)	(3.590)	(4.291)	(0.022)	(0.026)	(0.399)	(2.475)
Hypothesis tests p-values									
Joint orthogonality p-value	0.241	0.848	0.811	0.258	0.422	0.823	0.699	0.298	0.496
A-B =0	0.623	0.939	0.517	0.850	0.829	0.708	0.185	0.549	0.206
A-C=0	0.799	0.891	0.542	0.525	0.840	0.244	0.994	0.572	0.938
A-D=0	0.999	0.540	0.979	0.452	0.298	0.324	0.320	0.645	0.663
A-E=0	0.126	0.833	0.601	0.262	0.181	0.425	0.383	0.238	0.923
A-F=0	0.194	0.333	0.802	0.314	0.359	0.395	0.589	0.135	0.899
A-G=0	0.398	0.537	0.306	0.127	0.132	0.827	0.834	0.225	0.289
# Villages	192	192	192	192	192	192	192	192	192

Notes: This table shows randomization balance by sub-treatment arms. 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.

Table A5: **Village Characteristics Balance for Main Treatments**

	Village Pop (1)	Number Settlements (2)	Dist Road (3)	Dist HQ (4)	Dist Teh HQ (5)	Long (6)	Lat (7)	Num Gen Seats (8)	Influential Families (9)
Social	3396.552	1.635	7.037	21.135	16.307	34.180	73.300	5.714	3.008
	(420.112)	(0.283)	(1.932)	(3.564)	(3.639)	(0.013)	(0.020)	(0.237)	(0.481)
Personal	3388.282	1.861	4.504	26.665	20.720	34.173	73.304	5.692	3.769
	(461.083)	(0.318)	(2.385)	(3.779)	(3.877)	(0.016)	(0.019)	(0.255)	(0.603)
Neutral	3426.920	1.804	4.134	27.098	21.498	34.190	73.289	5.819	3.322
	(499.068)	(0.347)	(2.600)	(4.665)	(4.574)	(0.017)	(0.022)	(0.272)	(0.646)
Hypothesis tests p-values									
Joint orthogonality p-value	0.993	0.613	0.586	0.078	0.129	0.564	0.633	0.768	0.376
Social - Personal = 0	0.977	0.342	0.397	0.032	0.076	0.592	0.741	0.890	0.165
Social - Neutral = 0	0.928	0.540	0.341	0.141	0.125	0.541	0.494	0.573	0.589
Personal - Neutral = 0	0.905	0.842	0.899	0.911	0.820	0.290	0.339	0.478	0.504
# Villages	192	192	192	192	192	192	192	192	192

Notes: This table shows randomization balance by main treatments. 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.

Table A6: **Individual Characteristics Balance for Sub Treatments**

	Pro-Social Type High (1)	Perceived Electability (2)	Perceived Affordability (3)	Leadership Experience (4)	Main Castes (5)
A. Neutral Private, Neutral Public	0.647	0.299	0.342	0.006	0.772
	(0.061)	(0.023)	(0.061)	(0.005)	(0.082)
B. Personal Private, Neutral Public	0.575	0.275	0.347	0.005	0.810
	(0.065)	(0.023)	(0.061)	(0.005)	(0.086)
C. Neutral Private, Personal Public	0.604	0.301	0.386	0.008	0.795
	(0.058)	(0.027)	(0.066)	(0.005)	(0.085)
D. Personal Private, Personal Public	0.571	0.295	0.400	0.016	0.760
	(0.067)	(0.024)	(0.063)	(0.006)	(0.086)
E. Social Private, Neutral Public	0.534	0.290	0.386	0.012	0.764
	(0.063)	(0.024)	(0.064)	(0.006)	(0.083)
F. Neutral Private, Social Public	0.644	0.285	0.392	0.009	0.644
	(0.067)	(0.026)	(0.063)	(0.005)	(0.098)
G. Social Private, Social Public	0.621	0.294	0.398	0.008	0.793
	(0.068)	(0.024)	(0.069)	(0.005)	(0.088)
Hypothesis tests p-values					
Joint orthogonality p-value	0.273	0.399	0.736	0.266	0.208
A-B =0	0.147	0.026	0.905	0.943	0.486
A-C=0	0.301	0.846	0.387	0.465	0.653
A-D=0	0.138	0.781	0.169	0.036	0.818
A-E=0	0.020	0.474	0.359	0.155	0.879
A-F=0	0.965	0.379	0.271	0.227	0.025
A-G=0	0.660	0.711	0.243	0.431	0.719
# Observations	9310	9310	9310	9303	9310

Notes: This table shows randomization balance by sub treatment arms. 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.

Table A7: **Individual Characteristics Balance for Main Treatments**

	Pro-Social Type High (1)	Perceived Electability (2)	Perceived Affordability (3)	Leadership Experience (4)	Main Castes (5)
Social	0.417	0.240	0.391	0.010	0.676
	(0.076)	(0.025)	(0.061)	(0.005)	(0.076)
Personal	0.401	0.241	0.377	0.010	0.730
	(0.074)	(0.023)	(0.062)	(0.005)	(0.073)
Neutral	0.464	0.249	0.342	0.006	0.713
	(0.076)	(0.023)	(0.066)	(0.006)	(0.076)
Hypothesis tests p-values					
Joint orthogonality p-value	0.221	0.532	0.380	0.206	0.322
Social - Personal = 0	0.634	0.920	0.626	0.996	0.137
Social - Neutral = 0	0.239	0.327	0.167	0.114	0.367
Personal - Neutral = 0	0.084	0.339	0.308	0.130	0.686
# Observations	9310	9310	9310	9303	9310

Notes: This table shows randomization balance by main treatments. 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 A8: **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.

I Additional Results on Performance of Councils

I.1 Summary Statistics on Preferences and Budgets

Table A9: **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 A9 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 (**?**). 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 A10: **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 3. 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 A11: **Policy Effects - Manski Bounds**

	Effects on Euclidean Dist in Table 3 (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 A12: **Extensive Margin of Performance - Total Resources**

	Total Resources (in Rs.)	Log(Total Resources)
	(1)	(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 in column 2 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.

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.4 Treatments improve citizens satisfaction with chosen policy

Table A14: **Citizen Satisfaction and Trust**

	Satisfaction with Projects Chosen by Politicians (1)	Trust in State Index (2)	Doesn't Hold Negative View of Politics (3)	Trusts Public Servants (4)
Social vs Personal	0.173**	0.121	0.122	0.096
	(0.083)	(0.082)	(0.082)	(0.078)
	[0.037]	[0.094]	[0.077]	[0.130]
Social vs Neutral	0.070	0.105	0.074	0.070
	(0.094)	(0.098)	(0.092)	(0.085)
	[0.266]	[0.162]	[0.234]	[0.251]
Personal vs Neutral	-0.103	-0.016	-0.048	-0.026
	(0.093)	(0.091)	(0.088)	(0.087)
	[0.167]	[0.434]	[0.305]	[0.410]
Neutral Mean	0.500	1.652	0.354	0.604
# of Villages	192	192	192	192

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Each regression uses block fixed effects. This table uses data from citizen surveys collated at the village council level. Dependent variable in column 1 is above the median citizen approval of development projects. Citizens were asked to rank each project on a five point Lickert scale. This information was used to first calculate village level approval ratings then create a dichotomized variable by using the median approval rating across village councils. Column 2 uses a mean index of columns 3 and 4 as a dependent variable. Column 3 uses disagreement of citizens with a negative statement about politics, as a dependent variable. Citizen respond on five points Lickert scale to the statement 'Politics is a dirty word'. The responses are first averaged at the village level. A dichotomous variable is constructed that takes a value of one if citizens disagree with the statement and zero otherwise. Column 4 uses responses of citizens to an imaginary scenario about losing their wallet. They are asked separately to rank on a five point Lickert scale a policeman and a government servant's likelihood to return the wallet. These responses are averaged at the village level, then dichotomized on median. A dichotomous variable of trust variable is created if the average response is above median on either of the two statements. Robust standard errors are reported in parenthesis. Exact p-values are in square brackets.

J Additional Results on Mechanisms

J.1 Policy Effects of Public and Private Treatments

Table A15: **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.

J.2 Selection into Public Meeting

Table A16: **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.

J.3 Robustness Checks for Sub-group Analysis by Pro-Social Type

Table A17 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 A17: **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 A18: **Heterogeneous Response by type (controlling for proportion pro-social)**

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. 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.

J.4 Profile of Candidate and Elected Pools

Table A19: **Profile of Candidates and Elected Individuals**

	Social vs Personal		Social vs Neutral		Personal vs Neutral		Neutral
	Coef	Exact p	Coef	Exact p	Coef	Exact p	Mean
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Candidate Pool							
Years of Schooling	0.081	0.406	0.202	0.266	0.121	0.344	8.841
Log(Family Income)	-0.045	0.448	0.129	0.091	0.174	0.064	9.716
Occupation:							
Private Employee	0.010	0.398	-0.008	0.403	-0.018	0.321	0.043
Self Employed	0.022	0.283	0.105	0.065	0.082	0.188	0.174
Daily Wage Laborer	-0.016	0.426	-0.023	0.279	-0.007	0.353	0.058
Farmer	-0.042	0.232	-0.061	0.204	-0.019	0.437	0.275
Businessman	0.036	0.255	0.036	0.269	0.001	0.507	0.217
Retired	-0.003	0.362	-0.032	0.232	-0.030	0.347	0.145
Student	0.015	0.276	-0.030	0.193	-0.045	0.094	0.058
NGO worker	-0.029	0.137	0.006	0.444	0.035	0.135	0.029
N Candidates	220		212		148		
Panel B: Elected Pool							
Years of Schooling	0.876	0.097	0.967	0.133	0.091	0.505	8.462
Log(Family Income)	0.035	0.228	0.131	0.103	0.096	0.279	9.775
Occupation:							
Private Employee	0.052	0.236	-0.025	0.217	-0.077	0.078	0.077
Self Employed	0.011	0.582	0.067	0.265	0.056	0.191	0.128
Daily Wage Laborer	-0.041	0.197	-0.013	0.322	0.028	0.455	0.051
Farmer	0.036	0.261	-0.060	0.448	-0.096	0.261	0.359
Businessman	0.004	0.482	0.112	0.106	0.109	0.123	0.128
Retired	-0.005	0.355	-0.053	0.135	-0.048	0.235	0.179
Student	0.013	0.220	-0.039	0.126	-0.051	0.069	0.051
NGO worker	-0.080	0.044	-0.000	0.620	0.080	0.034	0.026
N Elected	117		119		78		

Notes: This table compares the candidate and elected pools across the treatment conditions. The table uses a dataset of randomly selected individuals. Inference relies on clustering at the village level. We do not include block fixed effects in this model because of limited sample.

J.5 Decomposing Policy Effects

Table A20: **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.6 Citizen's Behavior

Table A21: **Effect on the Number of Meetings**

	Meeting Councilors (1)
Social vs Personal	-1.840
	(3.739)
	[0.328]
Social vs Neutral	-6.868
	(4.196)
	[0.070]
Personal vs Neutral	-5.028
	(4.062)
	[0.148]
Neutral Mean	83.708
# of Villages	192

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. This table uses data from citizens. The dependent variable is the total number of meetings all the citizens reported to have held with their council members in the month before the survey. Robust standard errors are reported in parentheses and exact p-values are reported in square brackets. All regressions include block fixed effects.