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

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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. Emphasizing prosocial 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. A year later, the treatment improves the alignment of policy with citizens' preferences. These effects emerge only when treatments are randomly delivered in a public setting. Taken together, the results demonstrate that how politics is perceived shapes who decides to run for office, who is elected, as well the policies that democracies deliver.

Key words: Political selection, Policy-making, State capacity

JEL codes: O12, D72, H75

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.

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), and Arias *et al.* (2019).

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Who runs for political office affects policy, independent of, and prior to the rules under which politicians operate once elected.

Running for political office is typically associated with enhancing one's status and influence. In 2022, Gallup Pakistan conducted a nationally representative survey with 1,529 respondents. 81% of them agreed with the statement that "people enter politics to gain influence and status."[2] Such perceptions can determine who self-selects into politics, perhaps leading to a preponderance of "bad" politicians across democracies.[3]

This paper asks if portraying political office as enabling prosocial behaviour can, instead, encourage "good" politicians—those that will carry out policy that is responsive to citizens' preferences (Lipset 1959; Dahl 1973; Besley and Coate 1997; Caselli and Morelli 2004)—to emerge. What motivations might enable entry of such good politicians is a key puzzle in the literature (Dal Bó and Finan 2018; Bandiera *et al.* 2019). The answer to this question is theoretically unclear. While, highlighting prosocial motivations can encourage more prosocial people to seek office (Deci 1972; Besley and Ghatak 2005), they may also give public cover to selfish people to become politicians or crowd-out personally driven, but perhaps, more competent individuals from seeking office (Joseph 1966; Ferraz and Finan 2011b; Gagliarducci and Nannicini 2013).

We provide new evidence on these puzzles through a field experiment in Pakistan. We randomly sample 9,310 citizens across 192 villages and encourage them to consider running for new village councils in private meetings, in public village meetings, or in both. Encouragements vary across villages in how political office is portrayed to prospective candidates: in some villages status-quo motivations of personal returns from political office, such as enhancing one's respect and status, are made salient (called *personal villages*, $n = 72$). In other villages, the ability to help the community through elected office is emphasized (called *social villages*, $n = 72$).[4] In addition to personal and social encouragements, in both arms citizens also receive basic information about contesting elections. A third arm of the experiment (called *neutral villages*, $n = 48$) carries out identical meetings that includes this basic information but does not include encouragements to run for office for social or personal gain. We study the impacts of social versus personal encouragements on who decides to run for office, who is voted into office, and, perhaps most importantly, whether policy outcomes align or diverge from the preferences of the electorate as a result. We also compare social and personal encouragements against the neutral condition to characterize the direction of effects.

The 2015 local government reform in Pakistan—the fifth most populous country—provides a good testing ground for this research. As opposed to state or national levels where other factors like parties and donors are perhaps more important, political entry decisions at the local level provide evidence on how one might broaden the composition and performance of the political class (Martinez-Bravo 2014; Martinez-Bravo *et al.* 2017), potentially nurturing leaders for

2. Gallup Pakistan provided us access to anonymized raw data for this analysis. In the data, only 28% of people agreed with the statement "people enter politics to serve their community." Besides Gallup, Pew Research Center (2020) finds that 64% of respondents across 34 countries do not believe that elected officials care about ordinary citizens.

3. Scholars have shown that how people perceive occupations matters for who self-selects into a profession and how that profession is run (Handy and Katz 1998; Linos 2017; Ashraf *et al.* 2020). In the case of politics, previous work has shown, for example, that politicians are not representative (Chattopadhyay and Duflo 2004; Querubin 2016; Cruz *et al.* 2017); are motivated by private rents (Reinikka and Svensson 2004; Eggers and Hainmueller 2009; Ferraz and Finan 2011b; Fisman *et al.* 2014; Folke *et al.* 2017; Prakash *et al.* 2019); and have criminal backgrounds (Blaydes (2011); Vaishnav 2017).

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 versus status-quo prosocial recruitment attracts talented individuals to apply for a new health care position in Zambia.

higher level politics at the very first step of the political career ladder.[5] Building a base of evidence on political entry at the local level is therefore an important precursor to understanding the political pipeline.[6] In addition, local governments are also an important site of study because they are responsible for the implementation of government programs and the provision of public goods.[7]

We begin by analysing 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 1.8 percentage points more likely to run for office (exact-$p = 0.007$). Given that candidacy occurs with low frequency, as 2.1 people out of 100 run for office in the personal benefits condition, this effect is about a 86% increase. Though we have limited statistical power, we also see that social and personal treatments move candidacy in opposite directions relative to the neutral condition: social treatment increases candidacy by 1 percentage points (exact-$p = 0.07$) while personal treatment reduces candidacy by 0.9 percentage points (exact-$p = 0.107$). Taken together, 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 policy-making 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 new politicians never stand a chance of getting elected by voters. To the contrary, we find that social versus personal treatments increase the probability that people from our experimental sample get elected by 1.2 percentage points higher (exact-$p = 0.006$), an effect of about 120% against the personal mean. As before, against the neutral condition, the probability of election increases with the social treatment (effect of 0.5 percentage point, exact-$p = 0.113$), and decreases with the personal treatment (effect of 0.7 percentage point, exact-$p = 0.057$). This second set of results suggest that while the people mobilized into running for office by prosocial encouragements are electable, we may not be seeing them in office in the status-quo because they do not put themselves forward as candidates.

Finally, while demonstrating the effectiveness of social versus personal encouragement in encouraging individuals to run and win office may be important, it alone is not sufficient to ensure good governance. We go a step further by examining if treatments improve the alignment of policy outcomes with citizens' preferences. To do so, we first measure citizen preferences for budgetary spending one year after their election through a survey. Preparing development budgets is the key decision made by these local politicians. We establish that there is considerable discrepancy in the decisions of the elected politicians and the preferences of the electorate in neutral villages. By benchmarking real policy decisions made by elected politicians against these

5. For instance, 22% of politicians elected in 2018 to the legislative assembly of the Punjab province had served previously in local governments.

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. 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. A 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 U.S.

preferences of the electorate, we ask if social versus personal messaging aligns or widens the gap between what policy is adopted and what citizens want?[8]

We find that in villages where people are encouraged to run to help their community instead of helping themselves, official budgetary spending is significantly more aligned with citizen preferences: the Euclidean distance between policy and citizen preferences decreases by 9.4 points (exact-$p = 0.04$). This is a 13.4% decrease relative to the personal condition policy gap. In monetary terms, the gap closes by Rs. 380,415 or about USD 3,800 on average. As before, the social treatment reduces the policy gap by 7.039 points (exact-$p = 0.08$) while personal treatment increases the gap by 2.361 points (exact-$p = 0.322$) against the neutral condition. Together, these results on policy alignment provide direct evidence that social versus personal encouragements are yielding "good" politicians to office. Indeed, citizens in these villages are more satisfied with politicians' policy choices and exhibit more positive affect towards politics and the state.

We proceed to examine mechanisms for candidacy decisions. We randomize each of the 72 social and 72 personal villages into three sub-treatments such that encouragements are delivered: (1) only in private one-on-one meetings to people in our experimental sample, (2) only in public meetings in the village, or (3) in both private and public meetings. We find that social versus personal treatments in private produce no discernible changes in candidacy decisions, suggesting that mechanisms, such as, individual encouragements, nudges, and information alone minimally impact the decision to run. However, unlike other political actions, such as donating to campaigns, the candidacy decision is likely shaped by a public channel: it could be the case that community members encourage a person to run for office if they are seen as prosocial (Bursztyn and Jensen 2017) or if they can commit to doing good policy when elected (Fujiwara and Wantchekon 2013); and/or it may be the case that a person might run even absent encouragement by others because they derive utility from being seen as running for prosocial reasons (DellaVigna *et al.* 2016). Consistent with these interpretations, we observe increases in candidacy and election, as well as improvements in policy alignment when social versus personal treatments are delivered in public meetings alone, or when combined with encouragements in private.[9]

We also study channels through which policy effects materialized. We explore if treatments improved the likelihood that a person from our experimental sample possessed the means to affect the policy-making process. We find that social versus personal treatments increased the probability that someone from the experimental sample was elected as a leader of the village council by 0.7 percentage points (exact-$p = 0.014$) or proposed a policy that was adopted by the council by 1.2 percentage points (exact-$p = 0.003$). These results show that how political office is portrayed affects not just candidacy decisions but also who comprises positions of leadership and influence in the political class. Examining alternative explanations, we show that changes in citizen's preferences or behaviour are less likely to explain the policy alignment results.

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.[10]

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

9. In the public only treatment, candidacy increases by 2.2 percentage points (exact-$p = 0.043$), election by 1.1 percentage points (exact-$p = 0.096$), and policy aligns by 18.219 points (exact-$p = 0.027$). Similarly, in the public and private treatment, candidacy and election increases by 3.8 and 2.3 percentage points (exact-$p = 0.002$ each), and policy alignment improves 18.763 points (exact-$p = 0.024$).

10. 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"

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 compliments recent studies that examine the link between policy outcomes and the selection of government personnel, such as, judges (Lim 2013; Mehmood 2022), bureaucrats (Dal Bó *et al.* 2013; Xu 2018; Ashraf *et al.* 2020; Colonnelli *et al.* 2020), and politicians (Chattopadhyay and Duflo 2004; Grossman 2014; Fujiwara 2015; Beath *et al.* 2016; Karpowitz *et al.* 2017; Casey *et al.* 2021; Cirone *et al.* 2021).

The political economy literature has long examined which incentives are likely to yield politicians that are better at aligning policy with citizen preferences (Caselli and Morelli 2004; Besley 2005; Besley and Ghatak 2005). While previous work focuses on pecuniary and career incentives (Ferraz and Finan 2011b; Gagliarducci and Nannicini 2013; Fisman *et al.* 2015), our examination of prosocial motivation remains understudied.[11] This is identified as an open question in a recent review of the political selection literature that says "while we have made progress in documenting some 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). As Bandiera *et al.* (2019) note, "finding (such) ways to leverage non-pecuniary incentives for politicians may be particularly important in poor countries," (p. 8) where budget constraints inhibit compensation as a primary recruitment strategy.

We also contribute to a field experimental literature on recruiting agents to public service jobs. Studies in this literature primarily examine recruitment to the bureaucracy. For instance, Dal Bó *et al.* (2013) randomize salary levels in Brazil and find that higher salaries recruit more competent bureaucrats. Ashraf *et al.* (2020) show that emphasizing career incentives without changing salary for public health jobs in Zambia similarly recruits agents who perform better at their job. While examining recruitment into a bureaucratic jobs is important because they implement policy, our contribution to this literature is to study recruitment into a *political* job which instead entails the formulation of policy.[12] Improvements in policies in the political arena requires that they be benchmarked against citizen preferences (Dahl 1973). Our results suggest that recruitment for political job works in different ways than bureaucratic jobs: prosocial motivations and public signalling can play an important role in deciding who enters politics.

More broadly, contrary to the folk theory that people are primarily selfish, this paper also relates to a large body of the literature spanning several disciplines that argues that intrinsic motivations such as prosociality and warm glow can shape civic and cooperative behaviour (Andreoni 1990, 1995a; Frey 1997; Bénabou and Tirole 2006; Broockman 2013). In this spirit, our research first extends prior work on how prosocial 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 prosocial features of political office can enhance coordination among voters around prosocial candidates, a question previously explored extensively in public goods games in laboratory studies (Andreoni 1995b; Ostrom 2000).

(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).

11. An exception is Barfort *et al.* (2019), who carry out a survey experiment in Denmark to show that prosocial instead of pecuniary returns are more likely to motivate honest individuals to enter public service.

12. Previous field experiments on politicians have focused on electoral rules (Beath *et al.* 2016) and organizations (Gulzar *et al.* 2020b; Casey *et al.* 2021).

The rest of the paper proceeds as follows: Section 2 describes the context of the study; Section 3 provides details of the experiment; Section 4 presents the results; Section 5 discusses mechanisms; and Section 6 concludes the paper.

2. CONTEXT

This section briefly reviews the history of devolution in Pakistan to help place the new reform in context. Next, it provides specifics of how village councils are formed, as well as information on the candidacy process. Finally, it provides some information on the area where we conduct the experiment, 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 neighbourhood levels are marked by the relative absence of formal party workers who can be called upon to run for offices of local government.

This paper focuses on Khyber Pakhtunkhwa, a province of 30 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 30 May 2015. These elections were held on non-party basis, that is, candidates could not use party symbols and affiliations. The absence of deep political networks in villages meant that parties did not play a very organized role in political selection or campaigns.

As shown in Figure 1, Village Councils (together with Neighbourhood Councils for urban areas), constitute the lowest tier of local government. We conduct our experiment in Haripur and Abbottabad districts as shown in Figure 2. These districts have slightly better health, education, and public service outcomes compared to the provincial averages (MICS 2008). As we describe below, these districts were chosen once we identified a local partner.

2.2. *Village councils*

The village councils established under the 2015 reform represent about 6,500 voters (see Table A2 for descriptives). Before this, the lowest tier of elections were at the union council level with latest elections at that level in 2007.

Responsibilities. Much like the rest of the developing world, these local village 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

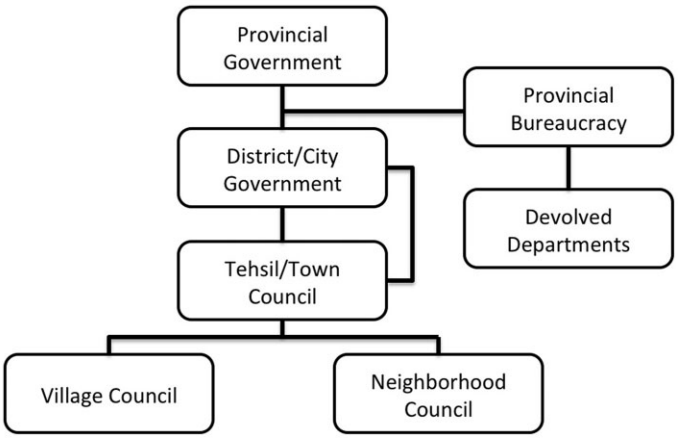

FIGURE 1
Village councils in political hierarchy

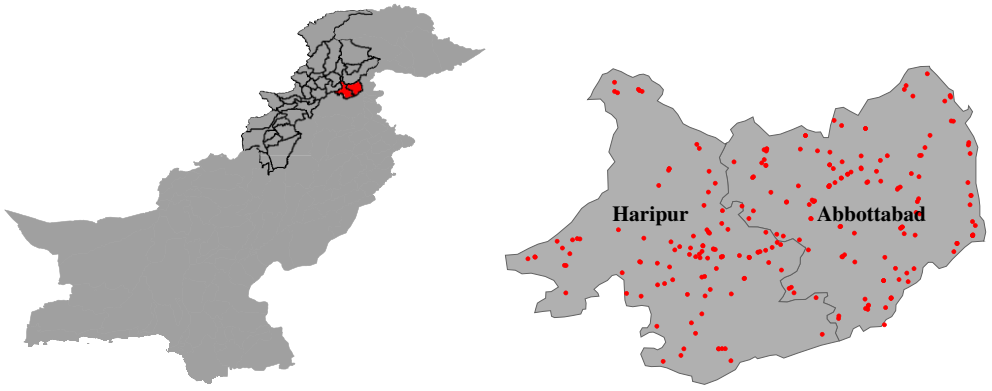

FIGURE 2
Villages in Haripur and Abbottabad districts (right) in Khyber Pakhtunkwa Province (districts outlined in left panel)

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/open seat candidate, one for peasant/worker, one for youth, and two for women seats. The number of general/open seats in the village council are determined by how large the village is terms of population. If, for example, there are six open seats then the top six candidates that receive the most votes are elected to the open 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 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, bringing 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.* 2020a). 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 or a neutral message, and whether the portrayal is varied in private or public. In this section, we (1) explain how field activities unfolded (2) describe the treatments, and (3) present details of the randomization. Appendix Section D presents a timeline of the project. Finally, in Appendix Section E, we discuss ethical considerations with respect to the experiment.

For the experiment, we partnered with a local NGO headquartered in the city of Haripur called Sangum Development Organization. Sangum was chosen with the help of a network of community organizations who identified it as an able local NGO with a history of implementing programs on the rule of law, access to justice, community mobilization for village development, as well economic development programs related to skills training and livelihoods. Working with a local community organization like Sangum is important because they have invested in trust-based relationships with communities they serve. These relations are established because all personnel in the organization belong to local communities and have been working in these areas for a long period of time.

3.1. *Selection of the sample*

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

FIGURE 3
Private one-on-one meetings

experimental sample. In Appendix B, we provide further details regarding sampling, the challenges around working at the household level, and effectively yielding a male sample. Further, Section G shows that the sample is balanced across treatment arms on observable characteristics both at the individual and the village level.

3.2. *Public and private meetings*

Next we describe meetings with the 9,310 selected individuals who comprise the experimental sample. First, enumerators have a **private meeting** with subjects. Once a household is approached, enumerators conduct a short survey with a male respondent. A detailed baseline survey was not possible because of the short election timeline. After the survey, enumerators deliver a social or personal encouragement, or a neutral message in this private one-on-one meeting with the subject (see Section 3.3 for details). Finally, subjects are invited to a public meeting in the village, and the time and location details for this meeting are shared.[13] The private meetings are usually held at the respondent's dwelling and last between 10 and 15 minutes. Figure 3 shows examples of these interactions between enumerators and citizens.

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. 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 in Section 3.3). Figure 4 shows examples of these sessions in three villages. On average, a public session lasts 30–40 minutes in the village.

3.3. *Treatments on how political office is portrayed*

During the private and public meetings, we vary experimentally how political office is portrayed in conversation with citizens. These treatments were developed after detailed piloting with focus groups before fieldwork commenced. There are three types of conversations: neutral, social, and personal encouragement.

13. It is made clear that the public meeting is open to others who may be interested in finding out more about the upcoming elections. We decided to not make public meetings exclusive to those we invited for two reasons. First, since the treatments involve encouraging people to run for office, we wanted to ensure that at the village level, people had the opportunity to receive information on how to contest if they were interested. Second, logistically, it is difficult and unpleasant to deny permission to people who are interested in finding out more about the elections. Table A33 shows that there is no evidence for differential selection into the public meeting by social versus personal treatments.

FIGURE 4
Public meetings in villages

The encouragements carry language that comes from, and is directly relevant to, the population where we conduct the experiment. The treatments were delivered in a conversational manner by the enumerators 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. Below we provide the scripts that were used to train the enumerators on the key talking points for each treatment. 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.

Now, we describe the three ways in which encouragements to run for office were delivered. The first condition which we label neutral message, provides basic information about when elections are going to be held as well as the eligibility criteria for candidacy. The enumerators use the following script as a guide to deliver this treatment in private and public meetings.

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 is not 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."

Importantly, all meetings across the 192 villages in our sample include this neutral message as a baseline from which further encouragements may be provided. In this sense, the neutral condition can be thought of as a premise for having a conversation with people.

On top of a neutral message, some conversations, labelled 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. This is reflected in the following script.

Social Benefits Script: Neutral Script and "People who are elected to the village election will be given an 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 neighbourhood, and this will make him the standard-bearer of social development for the village."

Similarly, building on the neutral message, some conversations that we label personal messages highlight how political office can boost one's respect, status and influence.

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 neighbourhoods, 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."

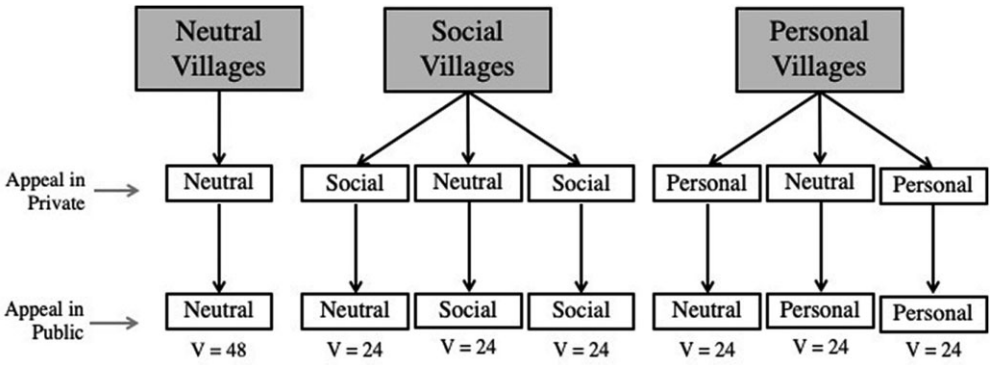

FIGURE 5
Design of field experiment

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.

3.4. *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. These blocks serve as randomization strata. All treatments are randomized at the village level within each block. We randomly divide villages into three types of treatments. 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. Finally, social and personal benefits treatments are not cross randomized: that is, a village can only receive one type of encouragement but not both.

3.5. *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. Using these data, as well as the short survey with the experimental sample, we test for balance of our randomization at the village and individual level in Appendix Tables A4, A5, A6, and A7. Individually, we find only seven instances of imbalance across 150 hypothesis tests, which is less than five percent of the total tests conducted. Only distance to district headquarter is significant in a joint orthogonality test. Below we show that our results are robust to controlling for imbalanced and all covariates. Appendix Table A2 presents the summary statistics for the subject pool.

3.6. *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). In Appendix F, we describe how the analysis in this paper relates to the PAP and lists changes. There are two main points to note. First, the analysis on candidacy and election to the village council, the main outcomes in Sections 4.2 and 4.3, are registered as the

primary outcomes of interest. 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.

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 and run regressions of the following form:

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

where Y_{iv} is an outcome, such as candidacy, for individual i in village v. $Social_v$ is an indicator variable for villages where a social message was delivered in either public or private; and $Personal_v$ is an indicator variable for villages where personal benefits were made salient in either public or private. β_1 and β_2 give the effect of treatments against neutral villages, which is the omitted category. γ_b 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. 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.[14]

With this setup, we impose the linear restriction, $\beta_1 - \beta_2 = 0$, to compute the effect of making social versus personal benefits of office salient and report the result at the end of each table.

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 (2015) as well as those elected to village councils.[15]

Table 1 column 1 shows the results.[16] Relative to personal benefits, social benefits increase the probability of candidacy by 1.8 percentage points (exact-$p = 0.007$), an increase of 86%. Though we have limited statistical power, we also see that the effects are the result of social and personal benefits changing behaviour in opposite directions: highlighting social benefits increases candidacy by 1 percentage point (exact-$p = 0.07$) while highlighting personal benefits reduces the probability of candidacy by 0.9 percentage points (exact-$p = 0.107$).

We probe the robustness of our results in four ways. First, to ensure that our effects are not driven by a few villages, we drop one village at a time from our sample (see Appendix Figure A2). Second, we progressively add pre-treatment controls to the regression, both balanced and imbalanced (see Table A19). Third, we add fixed effects for the order in which individuals were approached in a village (see Table A21). Fourth, we also add enumerator fixed effects to

14. 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.

15. We collected lists of candidates for each council from the Election Commission and matched people by name and father name to the experimental sample of 9,310. Rarely, if there was any confusion, our field partners called the village council secretary (a local bureaucrat) to confirm the match.

16. The low baseline rate of candidacy in the general population means that smaller absolute movements on candidacy behaviour translates into large relative effects.

TABLE 1
Effects on candidacy and election

	Candidate=1 (1)	Elected=1 (2)
Social Treatment	0.010	0.005
	(0.008)	(0.005)
	[0.070]	[0.113]
Personal Treatment	−0.009	−0.007
	(0.006)	(0.003)
	[0.107]	[0.057]
Neutral Mean	0.030	0.017
# Villages	192	192
# Observations	9310	9310
Linear Restrictions		
Social versus Personal	0.018	0.012
	(0.007)	(0.004)
	[0.007]	[0.006]

Notes: The table uses a dataset of randomly selected individuals. The dependent variable "Candidate" takes a value of one if the individual appears on the 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 parentheses. Exact p-values are in square brackets.

our analysis (see Table A22). In all cases, we find that our results remain robust both statistically and substantively.

Beyond these main effects, we also examine how changes in the experimental sample affect candidacy at the village level (see Appendix Table A9). First, we find that the main effects carry through to the size of the candidate pool at the village level where we document that about 0.96 additional persons run in social versus personal villages, though this is not precisely estimated (exact-$p = 0.143$). Second, we decompose this overall change in the candidate pool across open and reserved races and find that the effects are evenly split across the two types of races. Third, we also decompose the overall effect on the size of the candidate pool across three samples of interest: the experimental sample we randomly selected in villages; people not in the experimental sample who nevertheless attended the public meeting; and the rest of the village. Examining the first shows that the village level change primarily arises from changes in our experimental sample where social versus personal treatments increase candidacy by 0.861 individuals (exact-$p = 0.008$). On the second, there is about a 0.3 person decrease for people who self-selected into the public meeting (exact-$p = 0.094$). In the rest of the village, we do not detect changes in candidacy behaviour, though, against the neutral condition, social treatment and personal treatments increases the candidate pool size a little. Finally, an examination of the candidate pool size in our experiment with other villages in our two districts where we did no field research (that is, pure control villages) shows that the neutral condition increases the candidate pool size by 0.409 (exact-$p = 0.52$) but that the main difference against pure controls emerges in the social treatment that adds about 1.3 people to the candidate pool (exact-$p = 0.102$) (see Appendix Table A10). Conditioning on the neutral treatment helps partial out the effect of being approached, focusing on the effect of how political office is portrayed.

4.3. *Results on voting*

Next, we analyse 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 was elected to political office. We find in Column 2 of Table 1 that when social benefits are made salient versus personal benefits, the unconditional probability of people from our experimental sample getting elected to office is 1.2 percentage points higher (exact-$p = 0.006$). This can be decomposed into a 0.5 percentage point (exact-$p = 0.113$) increase in the probability of getting elected when social benefits are made salient and a 0.7 percentage point (exact-$p = 0.057$) decrease when personal benefits are highlighted.

As before, results remain robust to dropping one village at a time (Figure A2), adding controls (Table A19), and adding order of approach (Table A21) and enumerator fixed effects (Table A22). We also check the electoral performance conditional on candidacy in Appendix Table A8 which should be interpreted with caution as they are no longer causally identified and there is limited statistical power. We find that the probability of election among candidates in our experimental sample in social versus personal villages is 9.6 percentage points higher (exact-$p = 0.103$). We also check the effect of social versus personal messaging on vote share and rank among candidates and find a decrease in the former by 3.6 percentage points (exact-$p = 0.221$) and an increase in the later by 0.7 (exact-$p = 0.113$). One interpretation of this suggestive analysis is that candidates in our sample, while having a higher likelihood of election, face races that are more competitive.

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 cannot analyse individual-level performance of our experimental sample as 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. 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.[17] To test this hypothesis, we return to villages one year after the elections between June and July of 2016. We collect information from the first budget documents prepared by each Village Council at the end of the fiscal year. These include information on the total amounts sanctioned by the provincial Finance Department, as well as information on how Village Councils actually decide to spend these allocations.[18]

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

18. 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 are not correlated with treatments, and that our results are robust to extreme value (Manksi) bounds.

Examining the extensive margin, we confirm that our treatments do not explain any changes to the amount sanctioned to Village Councils (see Appendix Table A14). 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.[19] When we return to the field a year after elections we also survey a random sample of 1,318 citizens in our sample villages to collect their spending preferences over budgets.[20] We ask citizens to divide a hypothetical Rs. 100 village development budget over their spending priorities in an open ended question. Citizen responses are collapsed into four categories depending on the nature of the spending item. These categories are based on the functions of village councils specified in the Local Government Act of 2013. They are Municipal Services, Infrastructure, and Community. We also create a residual category that stores preferences that are officially not the primary responsibility of the village council.[21]

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

The 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 2 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

19. 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).

20. One might be concerned that citizen preferences themselves could be affected by treatment. We evaluate this in Section 5.3. Additionally, citizen preferences were collected before the councils spent any money in the villages.

21. *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 centre. *Not Primary Responsibility* includes provision of electricity, transport service, security, skills development, and a residual other category.

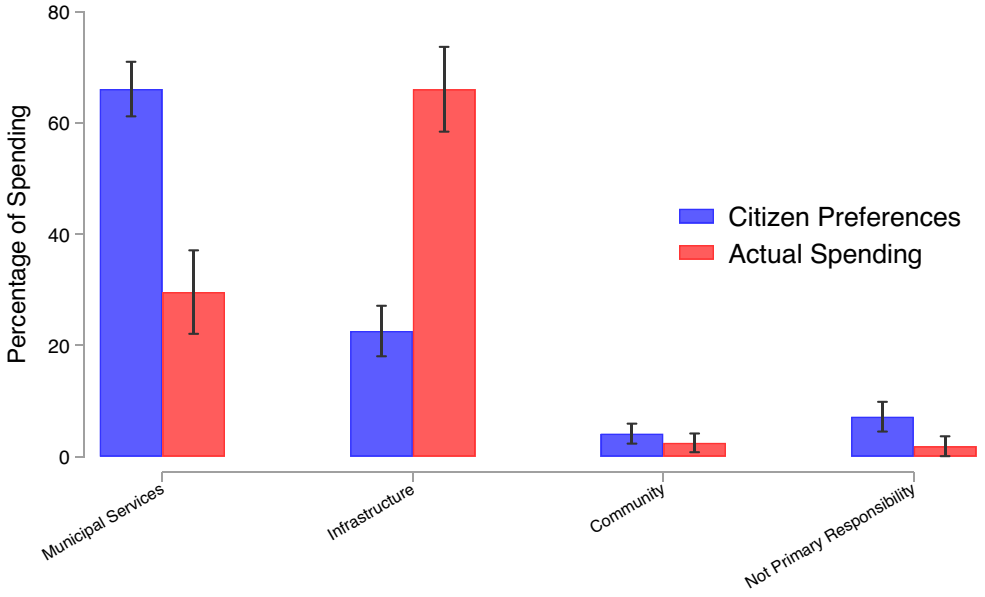

FIGURE 6
Citizen preferences and council spending in neutral villages

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. Error bars report the 95% confidence interval. Citizen preferences are statistically significantly different from budgetary spending for *Municipal*, *Infrastructure*, and *Not Primary Responsibility* with $p < 0.01$. For *Community*, the p-value of the difference is 0.2. *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 centre. *Not Primary Responsibility* includes provision of electricity, transport service, security, skills development, and a residual other category.

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 9.4 points (exact-$p = 0.04$). 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 relative to the neutral condition though the statistical power here is limited: social treatments reduce the policy gap by 7.039 points (exact p-value= 0.08) while personal treatments increase the gap by 2.361 (exact p-value= 0.322). These results are robust to dropping one village at a time (Figure A2), adding controls (Table A20), and adding enumerator fixed effects (Table A22).

We also check effects on the linear difference in the rupee value of projects chosen as a part of the official budget and those that citizens wanted (see Appendix Table A15). The monetary value for official projects is available in official budget data. For citizens, we divide the total budget allocated to the village across policy domains in proportion to citizen preferences. We find that social versus personal messaging closes the policy gap by Rs. 380,415 (exact $p = 0.088$)—about USD 3,800 at the time—which, compared to the personal village gap of Rs. 2,603,333 is about a 14.6% effect.

For one of the two districts in our sample (Haripur), we were also able to retrieve budget data for an additional fiscal year (2017). Though the power is a little limited as the sample is cut in less than half, there is evidence that the effects persist for up to two years after the elections (see Appendix Table A16). We also check if these policy effects correlate with the degree to which citizen preferences align with each other in the village. We show in Appendix Table A17 that in

TABLE 2
Policy effects: distance between citizen preferences and council budgets

	Euclidean Distance (1)	Municipal Services (2)	Infrastructure (3)	Community (4)	Not Primary Responsibility (5)
Social Treatment	−7.039	−5.900	−5.157	0.785	0.344
	(5.615)	(4.318)	(4.456)	(1.328)	(1.996)
	[0.080]	[0.063]	[0.104]	[0.261]	[0.427]
Personal Treatment	2.361	−0.175	1.948	0.259	3.231
	(5.212)	(4.019)	(4.192)	(1.289)	(2.103)
	[0.322]	[0.480]	[0.313]	[0.413]	[0.062]
Neutral Mean	67.877	42.995	48.555	5.001	7.520
No. Observations	189	189	189	189	189
Linear Restrictions					
Social versus Personal	−9.400	−5.725	−7.105	0.526	−2.887
	(4.810)	(3.662)	(3.864)	(1.161)	(2.050)
	[0.040]	[0.083]	[0.049]	[0.338]	[0.097]

Notes: 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 parentheses. Exact p-values are in square brackets.

villages where citizen preferences are less dispersed, policy effects are larger with social versus personal messaging but the result is not statistically significant. Whereas in places with more dispersed preferences this effect is relatively smaller. However, the difference between the two types of villages is not statistically significant.

4.4.3. Effects on citizen satisfaction and trust. Next, we evaluate the impacts of treatments on citizen satisfaction with policy decisions 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 17.1% point (exact $p = 0.036$) 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 Panel A, Appendix Table A18). Citizens are also 7.6% point (exact $p = 0.109$) more likely to state that they trust the state in social versus personal treatment villages (Column 2).[22] Panel B probes the robustness of this result with village averages instead of dichotomizing the outcomes. This reduces the statistical power for the analysis, but we find that the citizen satisfaction result remains robust. 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 (Acemoglu *et al.* 2020).

22. This mean index comprises two variables. First, we first ask citizens to tell us if they agree with the statement "politics is a dirty 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. 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.

5. MECHANISMS

This section examines potential mechanisms for the main experimental effects on candidacy, election, and policy, and evaluates alternative explanations of results.

5.1. *How candidacy decisions emerged?*

To understand candidacy decisions more precisely, we use a feature of the experiment that randomizes whether social versus personal encouragements are delivered in public meetings, in private one-on-one meetings, or in both. We then discuss the results and potential interpretations in light of a stylized framework.

Empirical Strategy. As before our focus is on the subject pool of 9,310 individuals in 192 treatment villages. As a reminder, meetings always occurred with subjects and what varied was whether a social, personal, or neutral message as delivered in private, public, or both. We estimate the effects with the following regression:

$$\begin{aligned} Y_{iv} = {} & \beta_1 Social\ Private_v + \beta_2 Social\ Public_v + \beta_3 Social\ Private\ \&\ Public_v \\ & + \beta_4 Personal\ Private_v + \beta_5 Personal\ Public_v + \beta_6 Personal\ Private\ \&\ Public_v \\ & + \gamma_b + \varepsilon_{iv} \end{aligned}$$

The omitted category corresponds to villages that receive a neutral message in the private as well as the public meeting. We can impose linear restrictions to calculate further effects of interest as before. For example, $\beta_2 - \beta_5$ gives the effect of social versus personal messages in public only, while $\beta_1 - \beta_4$ gives the effect in private only. Finally, $\beta_3 - \beta_6$ gives the effect of a social versus a personal message in both the public meeting as well as the private meeting.[23]

Results. Table 3 presents the results on candidacy, election, and overall policy alignment. The first part of the table reports results from the regression above, while the bottom part shows results from linear restrictions on this regression. We find three results. First, we do not see any effects of highlighting social versus personal benefits from office in private meetings only. Second, highlighting social versus personal benefits of office in public meetings alone increases candidacy (effect of 2.2 percentage points, exact $p = 0.043$), election (effect of 1.1 percentage points, exact $p = 0.096$), and improves policy alignment (effect of 18.219 points, exact $p = 0.027$). Finally, when social versus personal benefits are highlighted in both private and public meetings we observe the largest treatment effects on candidacy (effect of 3.8 percentage points, exact $p = 0.002$), election (effect of 2.3 percentage points, exact $p = 0.002$), as well as policy alignment (effect of 18.763 points, exact $p = 0.024$).

Discussion and Additional Results. We now describe a framework to guide the interpretation of these results on mechanisms for candidacy.[24] Consider a citizens' decision to enter the race via two stages, a policy stage and a candidacy stage. In the policy stage, elected representatives decide on policy actions, where prosocial actions close the gap between policy and preferences of the community. In the candidacy stage, citizens decide whether to enter the political race or not. In the candidacy stage, citizens conduct a calculus where they weigh the likelihood of accruing prosocial and personal returns from office against the cost of running. In addition to benefits like warm glow from serving others and status returns, this utility is also shaped by how others perceive one's reasons for candidacy. The status-quo returns reflect the fact that candidacy is

23. 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 Section L.5).

24. See Appendix K for a more formalized argument.

TABLE 3
Candidacy, election, and policy effects of public and private treatments

	Candidate (1)	Elected to Council (2)	Policy Euclidean Distance (3)
Social Treatment, Private	−0.009	−0.002	0.097
	(0.009)	(0.006)	(8.279)
	[0.195]	[0.419]	[0.496]
Social Treatment, Public	0.013	0.004	−10.520
	(0.012)	(0.006)	(6.966)
	[0.100]	[0.252]	[0.073]
Personal Treatment, Private	−0.005	−0.004	−9.349
	(0.007)	(0.005)	(7.203)
	[0.331]	[0.271]	[0.102]
Personal Treatment, Public	−0.009	−0.007	7.700
	(0.006)	(0.003)	(6.106)
	[0.205]	[0.162]	[0.164]
Social Treatment, Private and Public	0.026	0.013	−10.586
	(0.013)	(0.009)	(7.741)
	[0.008]	[0.024]	[0.077]
Personal Treatment, Private and Public	−0.012	−0.010	8.177
	(0.008)	(0.004)	(6.528)
	[0.121]	[0.055]	[0.136]
Neutral Mean	0.030	0.017	67.877
No. Villages	192	192	189
No. Observations	9310	9310	189
Linear Restrictions			
Social Private versus Personal Private	−0.004	0.003	9.446
	(0.009)	(0.007)	(9.176)
	[0.392]	[0.378]	[0.160]
Social Public versus Personal Public	0.022	0.011	−18.219
	(0.011)	(0.006)	(6.989)
	[0.043]	[0.096]	[0.027]
Social Public & Private versus Personal Public and Private	0.038	0.023	−18.763
	(0.013)	(0.009)	(8.125)
	[0.002]	[0.002]	[0.024]

Notes: Columns 1 and 2 use a dataset of randomly selected individuals (the experimental sample) to report the effect of treatments based on whether the treatment was delivered in private only, in public only, or in both. "Candidate" takes a value of one if the individual appears on the ballot and zero otherwise. "Elected to Council" takes a value one if the individual wins the election and zero otherwise. Column 3 use a village level dataset. "Policy Euclidean Distance" uses the euclidean distance between budget spending and citizens preferences as described in Section 4.4. Each regression uses block fixed effects. Standard errors are clustered at the village level and reported in parentheses. Exact *p*-values are in square brackets.

considered a domain of the elite, where those with a preference for personal benefits from office run and carry out policy actions that lead to personal enrichment over the benefit of society (see introduction for survey evidence from Gallup Pakistan). The experiment then seeks to study the effects of highlighting social returns from office relative to these baseline beliefs.

When enumerators highlight that political office can be a means for helping one's community in one-on-one conversations, a prospective candidate's beliefs about these returns from office can update sufficiently to induce them to run. However, the results in Table 3 suggest that conversations in private produced no discernible changes in candidacy decisions, suggesting

that mechanisms, such as, individual encouragements, nudges, and information alone minimally impact the decision to run.

Publicly highlighting the social benefits from office, on the other hand, can affect a person's calculus of running through additional channels, for instance, via encouragement by others to run or by being perceived to run for prosocial reasons, irrespective of their actual preferences. The prediction here is that candidacy will increase relative to the status-quo, though, both prosocial and non-prosocial types may enter which makes the likely policy impacts ambiguous. The logic for public messaging combined with private messaging is similar though effects might be larger as the perceived returns from office are now shaped by a combination of private and public calculi. Indeed, the results in Table 3 suggest that the presence of public treatments either by themselves or combined with private treatments matter in moving candidacy and election outcomes, with larger effects when combined with private encouragement. This suggests that public encouragements have an important role to play in shaping candidacy decisions.

The framework highlights that treatment effects can emerge through changing the types of people who run, and also through how the community is likely to respond. We conduct two exploratory analyses to shed light on these channels. We first examine treatment heterogeneity by a measure of prosociality.[25] We find that social versus personal messaging increases the likelihood that a prosocial person from our sample becomes a candidate by 2.6 percentage points (exact-$p = 0.035$), and is elected to the village council by 1.6 percentage points (exact $p = 0.0578$)—see Appendix L.1.1 for details. These differences are driven by villages where social versus personal treatments are delivered in both private and public. Second, we explore if treatments mobilize the community to nominate individuals to run. We find that social versus personal messaging increases the likelihood that candidates report that they were nominated by their community to run by 1.8 percentage points (exact $p = 0.007$)—see Appendix L.1.2 for details. We also find that this treatment effect primarily emerges when social versus personal messaging is done in the public and private and public conditions, but not in the private condition alone.

5.2. *Unpacking policy mechanisms*

How did treatments affect the policy-making process? In this section, we show that people elected from the experimental sample had the means to affect policy change and did so during policy deliberations.

5.2.1. Effects on leadership positions and project proposals. We study if people from our experimental sample gained influence over the budget-making process. The budget is a document prepared by committees that cannot be approved if the Chairperson (or Deputy Chairperson in lieu of the Chairperson) of the village council does not give their final sign-off. Therefore, while the budget-making process itself is collaborative, it gives village council members who are assigned to specific committees as well as those who are elected to leadership positions more influence in the final policy decisions. In addition to holding positions of influence, we also study if people elected from the experimental sample contributed to policy deliberations by proposing projects.

25. Note that prosociality is measured imperfectly through a pre-treatment survey with the experimental sample where respondents provide stated agreement with four survey questions: "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."

TABLE 4
Effects on leadership positions and project proposals

	Elected as Leader or Committee Member (1)	Elected and Project Proposer (2)
Social Treatment	0.001	0.010
	(0.004)	(0.004)
	[0.309]	[0.010]
Personal Treatment	−0.006	−0.002
	(0.003)	(0.003)
	[0.026]	[0.297]
Neutral Mean	0.012	0.009
No. Villages	192	192
No. Observations	9310	9310
Linear Restrictions		
Social versus Personal	0.007	0.012
	(0.003)	(0.004)
	[0.014]	[0.003]

Notes: The table uses a dataset of randomly selected individuals. The dependent variable in column 1, is equal to 1 if the person held the chair or vice chair position on the village council, or was a member of a working committee on the council. The dependent variable in column 2 equals 1 if the person proposed a project that was selected in the final budget for the village council after the first year. Each regression uses block fixed effects. Standard errors are clustered at the village level and reported in parentheses. Exact *p*-values are in square brackets.

To study this, we first code a variable as one if, when elected to the council, a person from our experimental sample was the chairperson, the vice chairperson, or a member of a village council committee. Next, we create a variable that equals one when someone from our experimental sample is elected to the village council and proposed a project that was adopted by the council. This variable proxies for agenda setting on the village council. Both variables equal zero when the person did not fulfill the criteria or if they were not elected.

Table 4 presents the results. We find that social versus personal messaging increases the likelihood that a person from our sample: is a leader or committee member by 0.7 percentage points in column 1 (exact $p = 0.014$); and is someone who proposed a project that was adopted by the village council by 1.2 percentage points in column 2 (exact $p = 0.003$). In Appendix Table A29, we decompose these results for private, public, and private and public sub-treatments and show that leadership and proposer effects are largest when social versus personal messaging is done in both private and public as before.

5.2.2. Instrumental variables results. We also conduct an exploratory analysis where we examine if the number of people elected to the village council from our experimental sample contributes to reducing the Euclidean distance between citizen preferences and budgetary decisions. Focusing on the social versus personal villages, we use the social treatment indicator as an instrument for the number of people elected to the council. We use Anderson–Rubin confidence intervals for inference (Andrews *et al.* 2019; Lal *et al.* 2021). Results are reported in Appendix Table A30.

We find that policy outcomes improve as the total number of people elected from the experimental sample increases. We can further decompose this along two dimensions. Exploring selection, we show that policy outcomes improve when the number of prosocial types or the number of those who are encouraged by the community to run increase in the village council. Examining policy influence, we find that policy improves when the number of those elected to

leadership positions from this sample increase, as well as if the number of project proposers elected from this sample increase.

The benefit of this approach is that it helps identify the effect of both the election of individuals from our experimental sample to the village council as well as a withdrawal of some of these individuals as a result of treatments. The drawback is that the key assumption of this instrumental variables analysis is that of exclusion restriction, which in this context, means that treatments affect policy outcomes only through the channel of people getting elected (or not) to the village council. It is, of course, conceivable that treatments can influence policy outcomes via other channels, such as, changing the beliefs or actions of citizens at large. While the evidence in Section 5.3 suggests that this may not be the case, we nevertheless recommend that the IV results should be interpreted with caution.

5.3. *Alternative explanations*

5.3.1. Changes in citizen preferences. Since there are two components of the Euclidean distance that measures policy alignment, citizen preferences and official budgetary spending decided by elected politicians, the changes we observe in policy alignment could arise because of movement in either component. For example, if treatments influence how elected politicians behave while making budgets, this should be reflected in how they spend the money. Alternatively, the treatments could have directly affected citizens' preferences which change the incentives environment under which politicians operate. We distinguish between these two explanations by decomposing the euclidean distance effects into its component parts in budgetary spending and citizen preferences (see Appendix Table A31). We find that the reduction in the euclidean distance between politician behaviour and citizen preferences arises primarily from changes in the former term. That is, politicians' budgetary spending changes along infrastructure and municipal services dimensions. In addition, the size of change in budgets is much larger than changes in citizen preferences: for example, the budget allocation to infrastructure in social villages is 7.303 percentage points (exact p-value=0.050) less than in the personal villages, while citizen preferences are different by only 0.268 percentage points (exact p-value=0.466). This finding is significant because it shows that the likely main driver of change in policy alignment is shifts in politician behaviour instead of citizen policy preferences.

5.3.2. Citizen behaviour after elections. In addition to direct changes in citizens' policy preferences, it could also be the case that treatments affect how citizens hold politicians to account. There are at least two aspects of this: first it could be the case that citizens increase the rate at which they hold politicians to account by meeting them more regularly and expressing their demands. Second, it is possible that citizens meet politicians at the same rate, but that they increase the intensity of demands in those meetings or make the criteria of evaluating the performance of politicians more stringent.

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 A32 that while the baseline rate of political engagement is fairly high at over 148 meetings per neutral village, there is no difference in meetings held in social versus personal villages.[26]

26. If anything, both social and personal villages exhibit fewer meetings with councilors compared to neutral villages.

This provides evidence that at least the first channel of citizen behaviour is not affected by treatments. Unfortunately, in the absence of data on the contents of citizen meetings with politicians, we are unable to directly test for the second channel whose existence we cannot rule out.

5.3.3. Enumerator effort. It is possible that the social versus personal treatment effects are explained by differential effort by the enumeration team of the NGO. While this is not explicitly testable, a few pieces of evidence allay this concern: First, we find that social versus personal messaging in private had no effect on the probability that individuals attended the public meetings, an immediate outcome to measure differences in enumerator effort (see Appendix Section L.5). Second, we find that there are no effects of social versus personal messaging in private on candidacy for office (see Table 3). Third, our balance analysis shows that there is also no evidence that enumerator effort in recruiting individuals in social versus personal villages yields citizens that differed on observables (see Appendix Table A7). Finally, we find that our results are also robust to the inclusion of enumerate fixed effects which block concerns that the results are driven by differences in effort by some enumerators (see Appendix Table A22).

6. CONCLUSION

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

We study candidate entry in the shadow of a large policy reform in democratization in Pakistan, where 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 can 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 in a variety of contexts where local governments are the grassroots of democracy.

Our experiment reveals that non-pecuniary prosocial incentives can be particularly powerful in mobilizing a political class that delivers more responsive policy to the electorate. When political office is presented in terms of its prosocial 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 prosocial encouragements at the candidacy stage also result is better alignment of downstream policy outcomes 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 great skepticism around the world,[27] our results outline that it is perhaps possible to improve the supply of politicians 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.

27. According to the World Values survey, 69.1% and 66.4% of respondents in Pakistan report little to no confidence in the parliament and political parties, respectively (Inglehart *et al.* 2014).

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Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

Data Availability Statement

The data and code underlying this research is available on Zenodo at https://doi.org/10.5281/zenodo.10086097.

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