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Essays on the Incentives and Motivations of State Personnel

by

Muhammad Yasir Khan

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requirements for the degree of

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of the

University of California, Berkeley

Committee in charge:

Professor Ernesto Dal Bó, Chair
Professor Edward Miguel
Professor Frederico Finan

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Abstract

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This dissertation comprises three studies on the incentives and motivations of public sector personnel, including bureaucrats and politicians. Chapter 1 tries to understand how public organizations can strengthen the mission motivation of public employees. Chapters 2 and 3 explore how the supply of politicians can be improved by either appealing to the intrinsic motivation of potential candidates or removing the cost and information barriers that prevent them from becoming politicians.

In chapter 1, I investigate if public organizations can motivate workers by emphasizing the organizational mission to them. I implement a field experiment in partnership with the Department of Health in Pakistan, where I randomly emphasize the public health mission to community health workers, provide performance-linked financial incentives, or do both. The mission treatment improves worker performance across incentivized (home visits) *and* non-incentivized tasks, while financial incentives improve performance only on the incentivized task. Financial incentives also become less effective at increasing home visits when combined with the mission treatment. Finally, the mission treatment improves downstream child health outcomes—there is a lower prevalence of diarrhea and higher vaccination rates. These results highlight that promoting an organization's mission can be a powerful motivator for public workers, especially in weakly institutionalized environments.

Chapter 2 asks the question: how can we motivate 'good' politicians – those that will carry out a policy that is responsive to citizens' preferences – to enter politics? In a field experiment in Pakistan, I vary how political office is portrayed to ordinary citizens. I find that emphasizing pro-social motives for holding political office instead of personal rewards – 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. Emphasizing pro-social motives also better aligns subsequent policies with citizens' preferences. The candidacy decisions are explained by social influence, and not information salience – I 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 also shape how politicians perform in office.

In chapter 3, I focus on understanding what barriers may prevent citizens from becoming politicians. Local governments are said to be susceptible to elite capture in the developing world. Reforms that aim to improve political competition by encouraging non-elite candidates may help reduce elite capture. This chapter reports findings from a randomized control trial that encouraged non-elite candidacy by relaxing three barriers to entry: the cost of running for office, information on benefits from the office, and information on their chances to win the elections. The experiment was conducted before elections for village councils in rural Pakistan and has three primary results. First, it is possible to identify prospective non-elite politicians through the involvement of community members. Second, subsidizing the cost of candidacy, through the provision of services of a lawyer, has a positive effect on the decision of non-elite candidates to enter politics. Third, treatment effects are stronger among candidates who hold higher beliefs about their chance of winning and who expect higher benefits from office.

This dissertation is dedicated to my parents.

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Chapter 1

Mission Motivation and Public Sector Performance: Experimental Evidence from Pakistan

1.1 Introduction

Motivating employees is a central challenge for all organizations. For many employees, especially in non-profit and public-sector organizations, two key sources of motivation tend to drive performance: financial incentives and intrinsic motivation from an organizational mission. In this paper, I study how each of these individual motives, as well as how their combination, affects performance. This remains an open question for several reasons. For example, highlighting mission-motives may not matter to individuals who have already selected to be part of a public-service organization. Additionally, emphasizing the mission may stimulate performance improvements on some dimensions at the expense of others—comparable to the multitasking problem of performance-linked monetary incentives (Holmstrom and Milgrom, 1991). Furthermore, if the mission intrinsically motivates workers, adding financial incentives may lead to the *crowding-out* of motivations (Deci et al., 1999, Gneezy et al., 2011, Cassar, 2018). Such potentially conflicting considerations necessitate understanding whether and how these two motivations affect workers' performance.

The economic literature has not addressed the effect that emphasizing a mission can have on worker performance.[1] While a few studies have provided insights into the mission as a signal to match workers with employers—i.e., theoretically (Besley and Ghatak, 2005, Prendergast, 2008, Cassar and Armouti-Hansen, 2019) and in laboratory settings (Banuri and Keefer, 2016, Carpenter and Gong, 2016)—none have been able to quantify the impact emphasizing an organization's mission has on workers' effort when workers have already selected to work for an organization. This leaves an important gap in the literature since

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[1] To date, scholars have studied how to get workers to exert effort using pay-for-performance (Lazear, 1996, Prendergast, 1999, Holmstrom, 2017, Khan et al., 2016, Muralidharan and Sundararaman, 2011, Glewwe et al., 2010, Lazear, 2000), non-financial rewards (Ashraf et al., 2014,a, Neckermann et al., 2014, Kolstad, 2013, Delfgaauw et al., 2013, Ager et al., 2016, Gubler et al., 2016), career concerns (Holmstrom, 1999, Dewatripont et al., 1999, Khan et al., 2019), and social incentives (Ashraf and Bandiera, 2018, Exley, 2018).

many organizations regularly communicate and emphasize the mission to people already working there to prevent it from fading into the background. [2]

In this paper, I experimentally test whether emphasizing an organization's mission motivates workers to exert effort in their job. To accomplish this objective, I partner with the District Health Officer (DHO) in Haripur, Pakistan, to implement a mission-training program for existing community health workers. Under the treatment, workers watch a video of the DHO describing and emphasizing the mission and then participate in *reflection* sessions with a facilitator to discuss the mission. Workers are encouraged to share thoughts about the mission as well as their experiences relating to the mission. Notably, the discussion of mission is not a one-off event; instead, the treatment is designed as a repeated engagement in the form of three monthly sessions.[3] The treatment's delivery is bundled with a skills-refresher training for the workers, which enables using a placebo treatment in the experiment—i.e., only including a skills refresher in the training without any discussion about the mission.

I also test the effect of a performance-linked financial incentive. Workers in the financial incentives treatment group can earn a bonus of up to 2.9% of their monthly salaries based on the number of households they serve. As in many settings, this financial incentive is tied to just one dimension of what in fact is a multitasking job.

I unpack the theoretical ambiguity about how the mission and financial incentives may interact as motivators by including a group of workers who receive both treatments. As theory predicts that there can either be a crowding-in—where the two motivations are additive (Bowles and Polania-Reyes, 2012)—or a crowding-out—where the additional motivators diminish intrinsic motivations for the job (Gneezy et al., 2011, Deci et al., 1999, Benabou and Tirole, 2003, Cassar, 2018)—this third treatment enables me to isolate the impact of the combined motivators. Finally, a set of workers continue to operate under the status-quo regime, which forms the pure control group for comparison.

The community health workers of the Department of Health provide several desirable organizational features, making the experimental study of mission-driven motivations and financial incentives possible. These workers are permanent government employees, functioning in non-overlapping communities. Their job is fundamentally mission oriented, yet the department does not emphasize the mission during routine operations, which can diminish workers' intrinsic, mission-based motivations. The community health workers are responsible for outreach activities focused on basic and preventive health services. Consequently, they are required to visit each household in their community, making monthly visits a key measurable metric of performance in what is essentially a multitasking setting. Activities and tasks carried out during the visits provide measures of multitasking in this setting. However, neither the visits nor associated tasks are monitored by the managers, which potentially

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[2]For example, Teach for America emphasizes its mission of educational equity to motivate its staff—who have already been selected to work for them—through summer training programs (Diamond, 2010). This approach is even true for corporations. According to a survey reported in Harvard Business Review (2015), nearly half of surveyed organizations invest in such activities as emphasizing organizational mission in order to motivate workers.

[3]By design, the treatment is delivered over three months following the example of organizations who frequently use their mission to motivate workers. For instance, Nike constantly emphasizes the mission to its workers by encouraging executives to "work the corporate mission statement into regular conversation" (Lashinsky, 2015).

leads workers to shirk their responsibilities. The combination of these features makes this organization a good setting for the study.

Using home visits and associated multiple tasks as measures, I examine the efficacy of the mission, financial incentive, and combined treatments on the performance of workers. To measure worker behavior, I conduct monthly surveys of ten random households in the community of each worker and ask whether the households were visited during the previous calendar month. In cases where households were visited, I further collect information on the activities performed during the visit, such as examination of pregnant women and children, discussion on disease prevention, and screening for tuberculosis. These additional data quantify treatment impact and the corresponding quality of the workers' effort toward the organization's goals.

Based on the household survey data, the treatment emphasizing the mission brings a 16.2% improvement in household visits by the community health workers over the status quo. These data show that this treatment increases the probability of a household visit by 5.7 percentage points over the baseline of 35.3 %, observed in the pure control group. I am able to directly attribute this change to the mission treatment, as the placebo training treatment does not achieve any significant improvement in household visits. I also find that the workers in the mission treatment group improve performance not only against the home-visits metric but across multiple other tasks. The mission treatment brings an improvement of 0.17 standard deviations on a multitasking index comprised of improving antenatal and child checks, imparting health literacy for disease prevention, screening households for tuberculosis, and organizing vaccination camps. In comparison to the mission treatment, the financial incentives improve household-visit performance by 27.5 percent, increasing the probability of a household visit by 9.7 percentage points above the control probability of 35.3%. However, workers receiving the financial incentive treatment do not change behavior on multiple-tasks performance—there is no discernible difference between the multitasking index of the pure control and the financial incentive treatment groups.

I do not find evidence for crowding-out of motivations when the mission treatment is combined with the financial incentives, but the data show financial incentives do lose their effectiveness. The workers in the combined treatment improve performance in terms of household visits compared to the pure control group, but the improvement is not as high as the group that received only the financial incentive. Namely, the combined-treatment group improves performance by 6.7 percentage points as opposed to an improvement of 9.7 percentage points achieved with the similar, but stand-alone, financial incentive. The difference between the two effects is statistically different from zero, which indicates that the financial incentives become less effective rather than the intrinsic motivations getting crowded-out.[4]

The difference in household visits between the financial incentives and the combined treatment appears to be driven by multitasking. Similar to the stand-alone mission treatment, the combined (mission plus financial incentive) treatment improves the performance of workers on multiple tasks, as reflected by an increase of 0.143 standard deviations on the

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[4]See Kamenica (2012), Gneezy et al. (2011), Frey and Jegen (2001), Bowles and Polania-Reyes (2012), De Wit and Bekkers (2016), Deci et al. (1999), Desmidt (2016) for review of crowding-out literature from the viewpoint of different fields.

multitasking index compared to the pure control group. In comparison, the financial incentive treatment does not improve productivity on any of these tasks. This difference in the allocation of effort results in the financial incentives becoming less effective when combined with the mission treatment in terms of increasing household visits.

To explore the mechanism behind all these results, I survey workers and find those in the mission and combined treatments become more intrinsically motivated in two ways. First, I find workers in the mission and combined treatments believe their department cares about the mission and is more aligned with their preferences, which in turn drives these workers to feel more attached to their job. I interpret these beliefs as evidence of intrinsic motivation due to the alignment of the mission with workers' preferences. Second, as I detail later in this paper, one year after the experiment, I find that workers in the mission and combined treatments are more altruistic, which I discern using an incentivized willingness-to-work task. These two pieces of evidence confirm that the mission treatment intrinsically motivates workers to perform better.

The mission treatment motivates workers to improve effort in terms of the number of household visits and in performing multiple tasks. However, these are inputs in the process of improving the health of the community—the ultimate mission of the organization. Therefore, I also collect data through household surveys and from administrative registers to trace the effect of the mission treatment on the health of children. Mission-emphasizing and combined treatments result in better health outcomes among children compared to the control group, as measured through an index of outcomes. The mission and combined treatments reduce prevalence of diarrhea and increase the proportion of children vaccinated. The financial incentive treatment also improves health; however, the effects of this treatment are smaller in magnitude than the mission and combined treatments – the financial incentives treatment reduces the prevalence of diarrhea but does not influence the vaccination rates.

I rule out three alternative explanations for how the mission treatment works. First, if the mission treatment provided new information about the tasks that the workers need to perform, then we should find that the placebo group of workers would improve on performance measures related to mother and child health, which was the focus of the skills-refresher training. However, we do not find any evidence of effectiveness of the placebo treatment. Second, if the mission treatment made workers concerned about being monitored—and thereby resulted in higher effort—then their perception of being monitored would be different from other workers. However, we find the mission-emphasizing and combined treatment workers do not have different beliefs about being monitored compared to the workers in other treatments and control groups. Third, in addition to being intrinsically motivated by the mission treatment, it is conceivable that workers may be influenced by their peers to perform. I randomize workers receiving the mission training into either a group setting or into a private, one-on-one session with the facilitator. I find no difference in the performance of the two sub-treatments, and therefore reject the possibility that peer influence may add to the individual motivation due the mission.

This paper provides, to my knowledge, the first empirical evidence via a field experiment that emphasizing the mission can motivate workers to perform. Theoretical literature has argued that mission motivation works on the selection margin (Besley and Ghatak, 2005, Prendergast, 2007, Cassar and Armouti-Hansen, 2019)—i.e., organizations invest in mission to attract workers who have similar preferences—and that it helps economize on incentives

(Wilson, 1989). This paper, however, provides evidence that mission also generates an effect beyond the selection margin. When organizations emphasize their mission, they motivate workers who are already part of the organization to exert more effort. With this result, this paper also contributes to the literature of personnel economics encompassing financial rewards (Lazear, 2000, Prendergast, 1999, Gibbons, 1998) and social incentives (Ashraf and Bandiera, 2018, Ellingsen and Johannesonn, 2008, Rotemberg, 1994). Further, this study extends the literature that workers may get sentimental utility from their organization (Akerlof and Kranton, 2005) and empirically establishes that managers can "exploit" such sentimental utility by emphasizing the mission.

The findings in this paper contribute to, and link, existing literature regarding the problems of multitasking (Holmstrom and Milgrom, 1991, Baker, 1992, Hart et al., 1997) and crowding-out (Gneezy et al., 2011, Deci et al., 1999, Frey and Jegen, 2001). While the paper does not find crowding-out of intrinsic motivations, it is the first paper to report that financial incentives can lose effectiveness due to the addition of intrinsic motivations. It also provides evidence that this loss of effectiveness of financial incentives is linked to multitasking. The paper shows that emphasizing the mission motivates agents to be better workers overall, which helps ward against the tendency to direct effort only to the contractible tasks due to financial incentives.[5] However, this equitable allocation to multiple tasks can reduce the efficacy of performance-linked financial incentives for the incentivized task when the two are combined.

This paper also contributes to the literature on improving public services in countries with weak institutions who struggle to enforce contracts. In such an environment, emphasizing the mission motivates workers to work harder without changing the terms of the contract. Existing literature has focused the debate on either selecting better workers to join the public sector (Dal Bó et al., 2013, Deserranno, 2019, Ashraf et al., 2018) or designing performance-contingent incentives to address under-performance.[6] This paper takes the literature beyond the debate between performance-contingent incentives and selection, and instead argues that the public sector in places with weak institutions can use the mission to activate intrinsic motivations of already contracted agents, making them perform better without changing the incentives.

Lastly, this paper highlights the importance of clear communication from managers as an important component of managerial practice. By providing clear communication about the mission, managers set expectations about organizational values. This process in turn motivates workers to contribute more to the organization. In this sense, the paper relates to the literature on management practices in public organizations (Rasul and Rogger, 2016, Bloom et al., 2015, Janke et al., 2019, Fenizia, 2019) and firms (Bloom and Van Reenen, 2010, Bloom et al., 2013) by proving a causal link between managerial communication and worker performance.

In the rest of the paper, I first describe the context and subject population of this experiment and then detail the experiment's methodologies. Thereafter, I highlight results relating to household visits, multitasking, and health outcomes. I discuss intrinsic preferences

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[5]See Dewatripont et al. (2000) for a review of other ways to address the multi-tasking problem.

[6]Performance-contingent incentives studied in the literature are either financial (Khan et al., 2016, Muralidharan and Sundararaman, 2011, De Ree et al., 2018, Duflo et al., 2012, Glewwe et al., 2010, Banerjee and Duflo, 2006) or non-financial (Ashraf et al., 2014,a, Khan et al., 2019).

as a possible mechanism before wrapping up the discussion in the conclusion section.

1.2   Context

1.2.1   Community Health Workers

Community health workers play a key role in delivering preventive and basic health care in many countries around the world, including in developed countries, such as the United States of America. Researchers have estimated that about five million such workers operate within the global healthcare system (Perry et al., 2014). These workers' role has received special attention in low- and middle-income countries since the 1970s, which faced extreme an shortage of trained health professionals to promote preventive health care aimed at achieving sustainable development goals (Scott et al., 2018).

In Pakistan, community health workers are considered the backbone of the preventive and primary healthcare system, especially in rural areas. These workers function as a separate division of the Department of Health that is called the Lady Health Workers (LHW) program. This division was established as a special program in 1993, with a total of 96,000 workers across the country (Jalal, 2011). Since 2014, they are considered full-time public-sector employees, with a defined service structure and job protections equivalent to those afforded other members of the state bureaucracy.

Community health workers in Pakistan are all women.[7] They are hired by the Department of Health to work in specific communities in each district. They are affiliated with a health clinic for reporting purposes, but their work involves providing services outside of the facility to a clearly defined community. They do not overlap with other community health workers in their geographical sphere of responsibility. Since they work in non-overlapping communities, they also do not have any systematic interaction with other health workers in their routine jobs. This feature of the organization helps the current study by limiting the scope for spillovers, and it also makes feasible a clean measurement of performance.

Community health workers are primarily outreach workers. Their core duty is to provide preventive and basic health care to citizens at the citizens' doorstep. Thus, providing any kind of service hinges on workers making visits to the households. Such visits are important for workers to stay up-to-date on the health status of the community, and to educate the household members about disease prevention. During these visits, community health workers advise women on birth control, provide antenatal checks to monitor the health of expectant mothers, and follow up after the birth to advise on disease prevention and nutrition. Performing these duties requires community health workers to visit households regularly in order to keep track of marriages, pregnancies, and births. Notably, these tasks are assigned to the workers by their division in the Department of Health, meaning that these activities are considered core duties.

Additionally, these workers perform tasks which are not considered core duties but that have been added to their roster of tasks. In this research, I focus on two of these additional tasks, as these activities have a significant impact on the health of the community. Firstly,

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[7]According to the World Health Organization, 70% of workers in the health sector in 104 countries are women (Boniol et al., 2019).

workers have been asked to help the department fight the spread of tuberculosis in rural communities. To support this goal, they are supposed to ask the household if anyone has been coughing in the family for more than a week. If families respond affirmatively, the workers ask further questions about the nature of the cough and whether the person is present in the house, and they may refer the potential patient to visit the nearest clinic for consultation with a doctor.

Secondly, to support vaccinations, community health workers organize community immunization camps. Normally, trained technicians based in health facilities provide vaccinations, and parents can take children to health clinics for routine vaccinations. However, to make access less costly for families, the technician can also organize camps in communities to bring the service closer to households. The successful organization of these camps requires effort from the health worker within the community, who teams up with the technician. Though community health workers are not directly responsible for vaccinations, they use their interactions with families to encourage mothers to get their children vaccinated.

Workers receive a fixed monthly salary that is not dependent on their performance. The salary of a community health worker is about Rs. 17,500 per month, which is on par with the minimum wage set by the Government of Pakistan and higher than salaries in the informal sector for a person with a similar skill profile. There are limited alternative employment opportunities in the rural areas, though the skills gained from being a community health worker can be utilized to act as an informal private healthcare provider. Community health workers do not have a direct path for career progression—theoretically, they can apply for the job of a supervisor (if there is an opening), but those positions are few and open to competition from the outside.

Even though there are no prospects for moving up the career ladder, the job of a community health worker comes with the same protections afforded to any other full-time employee of the state. Anecdotally, no one leaves the job and no one gets fired from it. There is no objective system of monitoring other than a register of information the workers keep, which can be checked by a supervisor, if needed. This lack of incentives and difficulty in measuring performance creates conditions for potential moral hazard.

1.2.2 Haripur District

Haripur lies in the Khyber Pakhtunkhwa province of Pakistan and has a population of 1.003 million people. The district is considered one of the better areas in Pakistan in terms of economic development: It is ranked at 18th out of 114 districts in the country in terms of the Human Development Index, which makes it comparable to Lebanon in the overall score. According to the most recently available statistics, the female literacy rate in the district is 60% and the male literacy rate is 82%.

The Health Department in Haripur operates one district hospital and 40 rural clinics. Each rural clinic employs a doctor, a nurse, a pharmacist, and a vaccination technician. These staff work inside the facility. The department also employs 710 community health workers to serve local communities. Despite a wide public-health network, about 58% of households rely on private health care when a child gets sick.

1.3   Details of the Experiment

This section details the experiment that was designed in partnership with the District Health Officer (DHO) to motivate the community workers. I first describe the research activities that took place between the end of 2018 and mid-2020, including treatments and data collection. Thereafter, I describe my tests of randomization balance.

1.3.1   Treatments

Organizational Mission

This treatment entails what was pitched as a training session between worker(s) and a facilitator. Before the start of this experiment, I worked with the District Health Officer (DHO) to record a short video of the officer describing and emphasizing the organizational mission of the LHW program (the division that employs the workers). In the video, the DHO gives the following message (translated from Urdu):

Today, I want to give LHWs a message about the LHW Program's mission and purpose. You are the Department of Health's vanguard for mother and child health. It is our resolve that I will extend health services to every household through this program so that no mother or child becomes a victim of any disease. The mission of this program is to ensure no mother or child is left without basic health services. And neither should a mother be left without knowledge about her own health and that of her child. I pay my tribute to your services. And I believe you will continue with your good work.

Representatives of the DHO office contacted the workers to invite them to the training sessions. I randomized how the treatment was delivered to the workers to decipher whether the peers-influence channel for the mission treatment affected behavior: In the **Mission Private** treatment, the worker and facilitator met one-on-one in a private setting, whereas in the **Mission Public** treatment, they met in a group setting with other workers. The group sizes were between 20 to 30 workers, depending on the logistics of the area.

In the session with a facilitator, the worker(s) were first asked to write on a piece of paper what they thought the organizational mission was. Thereafter, they watched the video. The facilitator then guided the workers through discussions of this mission statement, whether it aligned with their view, how it would influence their work, to what extent it was important, etc. The treatment was delivered in a participatory manner such that the facilitators did not "teach" but rather asked questions to direct the discussions and to invite workers to participate by sharing their views. The facilitators maintained a similar lines of questions in the private and the public sessions. In the public sessions, they made sure that every worker had the opportunity to voice their opinion and participate in the discussion. Such efforts were intended to help the workers internalize the mission statement and feel as though they had a stake in the process.

This discussion was followed by refresher training on the basic skills required for preventive and basic healthcare provisions. It used case studies on care for pregnant women and for children. The inclusion of the skills-refresher materials helped make the discussion about the mission appear more organic to the session and also provided a baseline for the placebo treatment in order to rule out some alternative explanations for the mission-driven

motivation. Each session lasted two-to-four hours and was repeated monthly for a period of three months. In the subsequent sessions, the mission discussion focused more on sharing experiences from the field and how the workers connected with the organizational mission.

In the original randomization, the public treatment group was split into two sub-treatments. In the **Mission Public, Not Observable**, the workers were told the purpose of these sessions was not to discuss their performance; inversely, in the **Mission Public, Observable**, the workers were told that the group would discuss the performance of workers in the third session. I introduced this variation to mediate any workplace-norms mechanisms that may be driven by concerns for social image among peers. For the analysis in this paper, I pool these variations into one main mission treatment.

Performance-based Financial Incentives

Workers in this group were informed by the Health Department at the start of the project that they had been selected for a program where they could earn a financial reward based on the number of households they visited every month. Namely, they could earn Rs. 25 for every additional household visited over and above their routine (baseline) visits—for up to 20 additional households. I used the month of November 2018 as a baseline. Through this incentive, workers could earn a maximum incentive of Rs. 500 ($ 3.5) if they visited all 20 additional households in the month or visited all households assigned to them (i.e., if they ran out of additional households in their assigned area). The maximum incentive therefore totalled to 2.9% of their monthly salary. This incentive was provided for three months, though the workers did not know the term limit before the end of the third month. Mathematically, this treatment can be written as:

$$w_{ij} = \begin{cases} 25 * x_{ij} & x_{ij} < 20 \\ 500 & x_{ij} \geq 20 \\ 500 & x_{ij} + h_i = H_i \end{cases}$$

where w_{ij} is the amount earned by worker i in month j when she visits x_{ij} households over and above the number of households visited in baseline h_i or when she runs out of total assigned households H_i.

The baseline benchmark and the subsequent incentive payment was based on the data collected in the independent survey, described in section 1.3.4. The first incentive payment was made during the second month of the experiment, after the first round of surveys collecting information about visits during the previous calendar month was completed.

Combined Treatment of Financial Incentive and Mission

For this treatment, I paired the Mission Public sessions with the financial incentive offered to the workers. Workers were informed they had been selected for a financial incentive program through a phone call, and they were invited to the Mission Public sessions, described above. The reward amount earned by each worker was privately disclosed, and the training sessions did not include any discussion of the financial incentive, which kept the financial rewards portion of the treatment comparable to the standalone financial incentive treatment.

Placebos and Control

In order to rule out alternative explanations for any results found during this experiment, I included placebo treatments as well as a pure control group in the experiment.

Placebo: During the placebo, a group of community health workers met in a public setting to receive a refresher training on the basic services the workers were expected to provide to their communities. The refresher training contents were the same as those delivered during the latter half of the mission-treatments sessions. I also divided this treatment into sub-groups based on whether an announcement about performance would be made or not— following the methods of the mission-emphasizing treatments, in one group, I explicitly that announced there would be no discussion of workers' performance related to the refresher training, and in a second group, I informed workers that the group would discuss their performance in the third session. For the analysis in this paper, I pool these sub-treatments in one placebo group.

Control: The pure control workers neither participated in training sessions nor received any financial incentives. In this way, this group continued under the status-quo condition.

1.3.2 Sample and Design

I randomized the 710 Lady Health Workers into treatment groups, as shown in Figure 1.1. The randomization was done at the individual level but block-stratified at the clinic level. Each treatment condition had 89 workers except the "Placebo training, observable" treatment group, which had 88 workers. For the main analysis, I combine all the sub-groups of the mission and placebo treatments into their respective groups.

1.3.3 Timeline

As shown in Figure B.4, the project began in December 2018 with a baseline survey of households, followed by a worker survey in January 2019. The Department of Health sent invitations for their first respective training treatments to the selected workers during the last week of January. At the same time, workers undergoing the financial incentive treatment were informed about the opportunity to earn a "bonus" based on performance. The first training sessions were held at the beginning of February, repeating monthly until April 2019. Post-surveys of the households were launched on the 1st of March 2019 and continued until June. I collected administrative data and conducted individual phone interviews with each of the workers in April 2020.

1.3.4 Data Sources

I use data from household surveys, worker surveys, and administrative reports to trace the effects of treatments on performance.

Household Surveys   I surveyed ten randomly selected households in the target community of each worker during five rounds of surveys—one baseline survey, three post–treatment-session surveys (administered during the month following the training sessions), and one post-experiment survey administered a month after the completion of the experiment. Since the workers' communities were not all the same size, each community's households had a different probability of selection for a survey. Consequently, I used inverse probability weights in the regressions when using these data.

The households were selected through randomization carried out in the field, and the surveys were administered to female respondents by female enumerators to account for any cultural sensitivities that respondents may have.

The baseline survey was conducted in December 2018, at which time each responding household was asked if the health worker visited in the previous calendar month (i.e., November 2018). The post-treatment surveys were administered every month from March to June 2019, beginning at the first of every month. In each survey, the households were asked information about the previous completed calendar month—for example, the survey starting March 1st collected information from households about worker activities in February. Households were then resampled after the first post-treatment survey. The experiment ended by the end of April 2019, so the survey in May was the last round to collect information relevant to the duration of the experiment. I administered an additional round of surveys in June 2019 to collect information regarding visits a month after the completion of the experiment.

In addition to asking about whether health workers visited a home, I also collected information on the health of children, their vaccination status, and other activities performed by the workers. However, due to financial constraints and the need to complete a large number of surveys in a limited amount of time, I did not include all questions in all rounds of surveys.

Worker Surveys: I administered a baseline survey to the workers in January 2019. This survey collected information on worker tenure; motivation for public service, using (Perry, 1996); and IQ, using Raven's matrices. An end-line survey of workers was later administered in June 2019. This survey collected information on the beliefs of workers regarding the mission, its importance, and their identification with the organization. Finally, a post–end-line survey was administered a year after completion of the project. This survey collected further information on the beliefs of workers as well as allowing me to administer a lab in the field experiment for studying the persistence of the treatment effects.

Administrative Reports: To trace the effect of treatments on the health outcomes of the communities, I collected data on the mortality rates of mothers and children within the assigned communities of each worker. For each worker, I also collected body weight data from the administrative reports—generated by the health workers—for five random children. I collected this information one year after the treatments were administered.

1.3.5 Randomization Balance

Table 1.1 uses the baseline household data to test for randomization across the workers assigned to the different treatments. The table reports a joint orthogonality test between the treatments and confirms treatment assignment does not predict performance or community characteristics at the baseline. I also test for differences between each treatment condition and the pure control condition and report the p-value from the Wald test of the null hypothesis—i.e., that there is no difference between the treatment and control. In this table, I pool the mission and placebo sub-treatments in their respective groups. I also report the balance of the original randomization in Table 1.2. Both tables show the treatments are orthogonal to the distribution of community characteristics.

Table 1.3 provides summary statistics about the community workers and households in the experiment. The average worker is responsible for serving 156 households, and on average, they have been working in the same position within the department for fifteen years. Additionally, on average, these workers have completed ten years of schooling, which is higher than the average 3.8 years of schooling for women in Pakistan. About 38% of them also have a healthcare-related certification. Table 1.4 reports the balance between the treatments on individual characteristics of workers. Data on these characteristics were collected before the start of the experiment but only became available after randomization was complete. The treatments are balanced on all variables except for the tenure of workers.

1.4 Main Results

In this section I report my analysis of the data, with a focus on questions of whether emphasizing the organizational mission improves performance. I first describe my estimation strategy for studying these questions and then move to the results section.

The data were collected through a survey of households in the respective communities of the 710 community health workers, as described in 1.3.4. I run the following regression to estimate the effects:

$$
\begin{aligned}
V_{ijmb} = \beta_0 + \beta_1 * Mission_{jb} + \\
\beta_2 * FinancialIncentive_{jb} + \beta_3 * Mission\&FinancialIncentive_{jb} + \\
\beta_4 * Placebo_{jb} + B_b + M_m + z_{jb} + \epsilon_{ijmb}
\end{aligned}
\tag{1.1}
$$

Equation 3.1 presents the main estimation used to analyze household-level data. V_{ijmb} is the outcome reported by household i from the community of worker j in survey round m. $Mission_{jb}$, $FinancialIncentive_{jb}$, and $Mission\&FinancialIncentive_{jb}$ represent treatment dummies for each worker indicated by j in block b. $Placebo_{jb}$ takes a value of one for the placebo treatments and zero otherwise. z_{jb} controls for the baseline performance of worker j; however, this term is only included when the outcome variable is a visit. To absorb block- and survey-month specific variation in the data, B_b is a vector of the randomization-block controls, and M_m captures survey-month. ϵ_{ijmb} is an idiosyncratic error term. When a variable is only reported in one round of surveys, I omit the vector of month dummies. In this estimation, I pool all sub-treatments of the mission into one treatment and also pool the two placebo sub-treatments into one.

For analyses using worker-level data, I estimate Equation 1.2. V_{jb} is the dependent variable in the worker-level estimates reported by (or for) the worker j. B is a vector of the randomization-block controls and ϵ_{jb} is an idiosyncratic error term.

$$
\begin{aligned}
V_{jb} = \beta_0 + \beta_1 * Mission_{jb}+ \\
\beta_2 * FinancialIncentive_{jb} + \beta_3 * Mission\&FinancialIncentive_{jb}+ \\
\beta_4 * Placebo_{jb} + B_b + \epsilon_{jb}
\end{aligned}
\tag{1.2}
$$

1.4.1   The Effect of Mission on Visits

I study whether emphasizing the mission gets workers to improve their performance in terms of visiting more households. I also study how it interacts with performance-linked payments in this same environment.

Table 1.5 presents the main results of Equation 3.1. Each column in the first panel presents results from regressions using household data pooled across the three waves of household surveys conducted during the experiment. Each regression uses randomization-block and survey-wave fixed effects and clusters standard errors at the worker level. I have data on ten households per community in each wave of the post–treatment-session survey, but as the communities are different sizes, I weight each point with the inverse probability of being selected for the survey in order to make the data representative. Further, to achieve higher precision, I include the baseline performance level of workers in the regression, reported in Column 2—here, the baseline performance is defined as the probability that a household was visited by the worker before the start of the experiment. Column 1 reports the results of Equation 3.1 without controlling for the baseline performance of workers.

As shown in the first row of Column 1, if I do not control for the baseline performance, the mission treatment improves the probability of a household visit by 5.1 percentage points. This effect changes only marginally when I add the baseline controls to the regression, as shown in Column 2. When I include these baseline controls, workers improve visits by 5.7 percentage points over a control mean of 35.3 percent. This change is a 16.14 % increase in the performance of workers achieved via the mission treatment, suggesting that emphasizing the organization's mission does work as an incentive to existing workers. The observed extra effort translates into eight additional visits in a given month, on average. the fourth row of the table shows that the placebo treatment does not achieve a significant improvement in performance. This helps rule out alternative explanations, such as the act of socialization in meetings as the main driver of the effect (see appendix section A.1 for more discussion).

I also study how traditional financial incentives perform in this same environment. The second row of Table 1.5 reports the effect of performance-based financial incentives on the probability of household visits. The probability of a household visit increases by 10.1 percentage points for this group when not controlling for baseline performance (Column 1) and by 9.7 percentage points when I control for the baseline performance (Column 2), compared to the status-quo condition. Such results indicate that financial incentives improve the performance of community health workers by 27.4 percent. This improvement translates into 15.1 additional household visits by the workers in a month.

1.4.2 The Combined Effect of Mission and Financial Incentive

In the preceding analysis, I establish that the mission treatment gets workers to improve effort on home visits. In this section, I study how the mission treatment interacts with financial incentives. Many organizations use the mission alongside financial incentives, expecting the two to additively complement each other. Theoretically, the literature argues that the two can complement each other if they provide some "good news" about the intentions of the principal (Bowles and Polania-Reyes, 2012). However, if the mission treatment and financial incentives send opposing signals, then the two treatments may cancel the effect of each other (Benabou and Tirole, 2006) or result in crowding-out of intrinsic motivations (Frey and Jegen, 2001). [8]

To study the combined effect, I include a group of workers in the experiment who receive both the mission-emphasizing and financial incentive treatments. The third row in Table 1.5 shows the effect of this combined treatment on the probability of a household visit. The effect of combining the two treatments is large and statistically different from the pure control group. These workers improve by 6.8 percentage points above the control condition (Column 2), which is an improvement of 19.2 percent in performance. However, despite this treatment motivating workers to work harder, the effect is not additive because combining the mission and financial incentives does not lead to an even higher improvement in performance. On the contrary, the effect of the combined treatment is smaller than the group that received just the financial incentive treatment but slightly higher than the mission treatment.

I test the differences between coefficients on the treatment dummies in the second part of Table 1.5 in order to see if the effects are indeed different from each other. This part of the table reports the linear combinations of coefficients and tests them against the null hypothesis that the difference between them is zero. I report the $p-values$ of these tests in square brackets. In the second row of the second half of Table 1.5, I find that the effect of the mission is smaller in magnitude than the effect of the combined treatment (mission and financial incentive) but is not statistically distinguishable. This result suggests that the intrinsic motivation does not get crowded out when the two treatments are combined.

The third row of the second half of Table 1.5 reports the combined treatment is smaller than that of the financial incentive treatment. Receiving the two treatments together diminishes the effect of financial incentives by almost 3 percentage points. This difference is also statistically different from zero. While combining the two treatments does not crowd-out the intrinsic motivations, the combination does appear to reduce the effectiveness of the financial incentives substantially.

While I will discuss possible channels for this effect in the next section, the current evidence indicates that it is at least not driven by a ceiling effect in the financial incentives treatment. If the ceiling effect were in play, workers in the group receiving both the mission-emphasizing and financial incentive would have at least improved by as much as the financial incentive–only group. Instead, their performance is lower than the financial incentive–only group, rejecting the possibility of a ceiling effect.

The preceding analysis shows organizations can use their mission to address the moral hazard arising out of incomplete contracts. Not surprisingly, offering financial rewards has

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[8]Crowding-out of motivations means that any effort due to the intrinsic motivation of workers will be eliminated, resulting in an effect that will at the very least be less than the effect of the mission treatment.

the bigger effect on the effort of workers. However, a puzzle emerges: when the two treatments are combined, the financial incentives appear to be less effective in motivating effort—workers in the combined treatment leave money on the table even though intrinsic motivation does not get crowded-out. I explore a possible reason behind this puzzling phenomenon in the next section.

1.5 Why Do the Financial Incentives Become Less Effective?

In this section, I argue the puzzle of the diminishing effectiveness of financial incentives is due to the mission treatment stimulating workers to allocate effort to multiple tasks, whereas the financial treatment does not. To support my argument, I first provide evidence that there is no difference between the mission and the financial treatments in terms of the overall effort, proxied by the length of the workday. Second, I provide evidence that workers in the mission-emphasizing and combined treatments allocate effort to performing multiple tasks whereas those in the financial treatment do not. The latter focus mostly on visiting more households, which is the incentivized task.

1.5.1 Time Spent on the Job

In the workers' endline survey, I collect information regarding the length of their typical workday. In Appendix Table A3, Column 1, I find that even though the treated workers increase the amount of time they spend on their job in a given day relative to the pure control, there are no differences between the mission, combined, and financial incentives treatments. In the status-quo case, the workers self-report that they spend about 318.4 minutes (or five-and-a-half hours) on their job everyday. The mission treatment increases this time duration by 16.9 minutes, and the financial incentive and combined treatments increase the reported duration by about fifteen minutes. I conclude that all treated workers exert similar levels of effort on their jobs. But since there are differences in how many households they visit, as reported in Table 1.5, based on their treatment groups, the workers may allocate their effort to multiple tasks differently. I check this notion in the next section.

1.5.2 Multitasking

As the financial incentives treatment increases the workers' monetary utility only if they improve performance on the incentivized task, theory suggests workers only exert effort for that task at the expense of non-contractable tasks. In contrast, I hypothesize that the mission treatment motivates workers to improve performance without directing effort to any one task, resulting in better performance overall. If that is indeed the case, it helps resolve the puzzle of financial incentives becoming less effective for the incentivized task when a financial incentive is added to the mission treatment. To this end, I examine the breakdown in workers' multitasking activities and show that the financial incentives, indeed, do not improve effort on non-incentivized tasks suggesting that improvement in home visits came at the cost of multitasking.

Workers perform multiple tasks that can be largely divided into core and non-core tasks. Core tasks are the activities they are expected to perform during a visit—such as antenatal checks, child health exams, and discussions about disease prevention. Non-core tasks include activities outside the direct responsibilities of workers. I track two such non-core tasks—screening for tuberculosis and helping the department's immunization technicians organize immunization camps in their communities. Table 1.6 presents the analysis of these core and non-core tasks. The first four columns of the table use data from the household surveys and the fifth column relies on data from worker surveys.

Table 1.6, Columns 1 – 3 report workers' core activities, with the first two columns using data from the household surveys during the experiment and Column 3 using responses to the post-experiment survey. Column 1 of Table 1.6 shows the effect of this study's treatments on whether the workers perform antenatal checks on pregnant women during their visit. The mission and combined treatments increase the probability of an antenatal check by 5.2 and 4.6 percentage points, respectively, over the control mean of 35.9%. The financial incentive treatment does not have any effect on this task. Column 2 reports the effects on children examined. Workers in the mission and combined treatments are 3.2 and 2.7 percentage points more likely to examine children, respectively. However, only the effect of the mission treatment is significant. Workers in the financial incentive treatment improve on this task by 2.4 percentage points, but the effect is not significant. Column 3 reports that when asked whether workers discussed general disease prevention, household responses differ across the study's treatments. Workers who received the mission and combined treatments are 5 and 5.8 percentage points, respectively, more likely to discuss disease prevention with the household, whereas the financial incentives treatment sees a 2.4 percentage points increase, though this effect is statistically not significant.

As a non-core activity for the home visits, workers have been asked to screen households for symptoms of TB and refer suspected patients to doctors for diagnosis.[9] Column 4 of the table reports the effect of this study's treatments on the probability of a household being screened for TB, based on data collected in two rounds of surveys. The mission treatment appears to motivate workers to improve their performance on this task: Workers in the mission and combined treatments are 4.7 and 4.4 percentage points, respectively, more likely to screen households for TB. However, workers in the financial incentives group do not improve their performance on this task at all.

The last task I analyze is workers' participation in co-organizing immunization camps, a non-core activity. As discussed earlier, community health workers encourage parents to get their children vaccinated as part of their core duties, but these workers are not directly responsible for providing vaccination services. Instead, children are taken to a health facility, where trained technicians vaccinate them. To improve coverage rates, technicians may also organize community camps to bring their services closer to families, making it less costly for families to have their children vaccinated. In organizing these camps, the community health workers help manage logistics, advertise the camp, and help mothers bring children to the location. Workers do not get paid extra for this activity and can easily shirk some of

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[9]There is a separate division within the health department that is focused exclusively on addressing the spread of tuberculosis. This division has its own staff and is integrated in the health facilities. However, the department has asked community health workers to help refer suspected cases of tuberculosis to doctors for proper diagnosis. From there, these patients are then traced by the separate division.

their responsibilities unless they are motivated by the mission to help improve the health of mothers and children in their communities.

In the endline survey, I asked workers how many camps they helped organize during the three-months period. Column 5 reports the effects of treatments on their responses. Workers on average, report organizing 5.7 camps over three months in the control group. Workers in the mission and combined treatments, on average, organized nearly an additional half camp during this same period. In comparison, workers in the financial incentive treatment increased their effort by 0.17 extra camps.

The preceding five results prove that the mission-treated workers perform better overall and financial incentives treated workers do not. Interestingly, the combined-treatment group showed comparable increases to the mission-alone treatment across all metrics.

I also combine the information from the five tasks into a multitasking index. First, I collapse the household data into a worker-level data set by calculating the mean performance for each worker on each task. If the performance was measured in multiple waves of surveys, I collapse the data at the worker–survey-wave level. This step confirms that all the data are at the same level of aggregation, as the information on immunization camps is available only at the worker level. The collapsed data for each variable at the worker–survey-wave level is standardized using the mean and standard deviation of the pure control. Missing data are imputed with the mean of the respective treatment groups. I weight each component by the inverse of the variance-covariance matrix before combining it in one mean index, as prescribed by Anderson (2008). For robustness, Appendix Table A4 uses the index constructed without weighting the data with the variance-covariance matrix, wherein each component of the index is assigned equal weight, similar to Kling et al. (2007).

The analysis using the multitasking index is reported in Column 6 of Table 1.6. The results present a clear picture that the mission treatment improves the performance of workers on multiple tasks by 0.175 standard deviation. Similarly, the combined treatment has an effect of 0.143 standard deviation. Both effects are statistically different from zero. In contrast, financial incentives alone have no effect on the multitasking index.

In the second half of the table, I test whether the coefficients on the treatment dummies are similar to each other. The effect of the financial incentive treatment is 0.18 and 0.15 standard deviations smaller than the mission and the combined treatments, respectively. These results show the difference in allocation of effort between the financial and mission treatments. Further, the combined treatment's effect cannot be statistically distinguished from the effect of the mission treatment, indicating that the workers in both groups are motivated in the same manner.

These results prove that the higher effort on home visits by the workers receiving financial incentives was the result of them ignoring the multiple tasks they are required to perform. By contrast, those that receive the mission treatment, alone and combined with the financial, allocate effort to all tasks irrespective of whether they are contractible or not. Given that all the groups of workers exert similar levels of overall effort, as proxied by the time spent working in a day, it follows that workers' differences in task allocation explain why the financial incentive becomes relatively less effective in the combined treatment.

1.6   Health Outcomes

While community health workers are considered a key link in improving maternal and child health in developing countries, the improvements in task performance that I have discussed thus far do not inherently equate to changes in health outcomes within the communities these workers serve. Consequently, in this section, I study whether this study's treatments help translate into improved health of mothers and children.

To trace the effects of this study's treatments on health outcomes, I use two sources of data. First, I rely on reports from households, as recorded in their surveys. Within these surveys, I collect information about the prevalence of diarrhea and the vaccination status of the household's children under the age of two years. Second, I use administrative reports prepared by the workers as part of their routine job, I collect information on child and maternal mortality.[10] I combine these outcomes in an index of health outcomes using the methods outlined in Anderson (2008). I also collect child-weight data from administrative registers to supplement the analysis.

Diarrhea is the most basic preventable disease whose prevalence the community health workers can influence via teaching about both prevention—e.g., the importance of sanitation and clean drinking water—and treatment—e.g., how to make and use re-hydration solutions. Diarrhea is also the second most common reason for childhood deaths globally.[11] In the post-experiment survey, I asked households if any child had diarrhea during the previous four months. I use this information to construct a dichotomous variable of diarrhea prevalence.[12]

Column 1 in Table 1.7 reports the effects of this study's treatments on the prevalence of diarrhea in households that have at least one child. Nearly 29% of the households in the control group report children getting diarrhea in the four-month time period being studied. However, my three treatments—mission, financial incentive, and combined treatments—lead to a substantial reduction in diarrhea, indicating that workers' performance improved on this basic dimension through all treatments. Interestingly, as discussed later, the placebo group—who received training about health concerns but not the mission—did not see a change in health outcomes. Importantly, the effects of the treatments are comparable to results achieved by public health interventions exclusively focused on diarrhea. Figure A2 in the appendix, plots the relative risk ratios of diarrhea in the treatment groups and results from a meta-analysis of public health interventions reported in Fewtrell et al. (2005), showing a similar range of improvement.

Next, I track if the workers' efforts translate into increased vaccination rates. Though workers can influence vaccination camps, household vaccinations are not a direct output of the workers because vaccinations are the result of demand from parents and supply of vaccination services from the health department. Community health workers *can* influence vaccination rates by making sure parents are educated about the need for vaccinations and

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[10]The plan to collect this information through an independent survey of households did not materialize due to the emergence of Covid-19. To minimize contact, I adapted the study to collect administrative records.

[11]According to the CDC fact sheet on Diarrhea: https://www.cdc.gov/healthywater/pdf/global/programs/globaldiarrhea508c.pdf, accessed on 09/03/2020.

[12]It is important to note that the definition to track diarrhea is different from the World Health Organization prescribed definition of "3 or more loose or liquid stools per day". I directly ask the households about diarrhea instead of asking about 3 or more loose bowel movements.

informing them of any immunization camps should they wish to vaccinate their children. I collected information about vaccination in two waves of surveys: one wave, during the survey conducted in March 2019, and the second wave, after the experiment in June 2019. During these surveys, enumerators asked households about the vaccination status of each child along with their age. Then, using guidelines from the CDC, enumerators calculated whether the child had received timely vaccinations and noted the number of children who were indeed fully vaccinated as per the prescribed schedule.

I use the proportion of children vaccinated in each household (for households with at least one child) as the main outcome in the analysis reported in Column 2 of Table 1.7. It appears that only the mission-emphasizing and combined treatments have a substantial effect on the proportion of children vaccinated in a timely manner. Children in these treatment groups were nearly 3 percentage points more likely to be vaccinated, whereas workers receiving just the financial incentives have a smaller effect—namely, 1.2 percentage points. These treatment effects are directly linked to workers' multitasking performance, discussed in Section 1.5.

From the administrative registers, I extract the number of children born alive in 2019 and how many of them did not survive during the year. I also extract the number of mothers who died while giving birth or due to birth-related complications during the year. The effects of this study's treatments on child mortality appear in Column 3 of Table 1.7, and on maternal mortality appear in Column 4. Given that both events are rare, I do not have enough statistical power to make conclusive claims about the effects. However, the treatments appear to have a negative effect on the mortality rates over the year.

To mitigate the concern that I am not picking up on chance effects and to better understand the relationship between the treatments and the health outcomes, I combine the four measures into one summary health index. Since the data are only available for 703 of the 710 workers, I assign the average value of the treatment group to the missing data and use Anderson (2008)'s method to weight each component by the inverse of the variance-covariance matrix to create a summary index of health outcomes. For robustness, Appendix Table A4 uses the index constructed without weighting the data with the variance-covariance matrix— with each component of the index assigned equal weight, similar to Kling et al. (2007). The robustness check validates this weighted approach.

The results reported in Column 5 of Table 1.7 indicate that the mission treatment has a positive effect on the health of the community. Workers in the mission treatment generate a 0.205 standard deviations improvement in the composite health index. This effect is followed by the positive effect of 0.164 standard deviations achieved by the combined treatments. The financial incentive treatment also achieved an improvement of 0.12 standard deviations. However, this effect is 0.084 standard deviations smaller ($p-value = 0.127$) than the improvement brought about by the workers motivated by the organizational mission. Based on these results, I can argue that this study's treatments led to meaningful improvements in the health of communities served by workers, especially those workers motivated by the mission of the organization.

Though the body weight of children (collected from administrative registers) represents another valuable metric of the treatments' effects on health outcomes, I omitted these data from the above analysis because this information was only available for 543 workers—the remaining workers did not have functional scales to measure children's weight. Though availability of these data are balanced across treatments, the data show some differences in

terms of gender and age across the groups (see Table A5). Therefore, I do not include these data in the main analysis but present my findings as additional supporting evidence in the appendix.

Table A6 reports effects on the health outcomes presented in the main Table 1.7 but add the children's weight data as Column 5. This column reports the effects of the treatments on the weight of children, as measured in kilograms and after controlling for the age and gender of children. The important takeaway from this analysis is that the coefficients on the three treatments are all positive, though only the combined treatment has a statistically significant effect on children's weight. In Column 6, I combine the weight data in the health index. As with the main result in this section, both the mission-emphasizing and combined treatments appear to have a much stronger effect on the health of children compared to the financial incentive treatment. In particular, the mission treatment improves health by 0.096 standard deviations ($p-value = 0.075$) more than the financial incentive, and the combined treatment improves health by 0.068 standard deviations ($p-value = 0.29$) over the financial incentive.

1.7 How Does the Mission Treatment Work?

In this section, I argue that the mission-emphasizing treatment intrinsically motivates workers to perform. I use two pieces of evidence to make this argument. First, workers may have preferences to work for a mission-driven organization. The mission treatment therefore activates such preferences by signaling an alignment between the preferences of workers and the organization. Second, the treatment stimulates altruistic preferences, making the worker more pro-social.

1.7.1 Alignment of Preferences

In the end-line survey, I ask workers whether they agree with statements acknowledging the mission to be central to the operations of their organization. I specifically ask them to rate on a scale of 1 to 7 (with 7 communicating "Very Strongly Agree") how much they agree with the following statements:

1. Mission Importance: I like the LHW program more than other departments because of the importance it places on the mission.

2. Mission Alignment: I believe the LHW program's mission is very similar to my thinking since the beginning of 2019.

3. Mission Dependent Attachment: If the LHW program's mission was something else, I would not have been as attached to the program.

Workers' responses to these statements help me to assess whether workers believe that their preferences align with the organization's mission.

Column 1 in Table 1.8 reports effects of the treatments on the index of beliefs. Workers in the mission-emphasizing and the combined-treatment groups are 0.201 and 0.23 standard deviations more likely to believe their preferences are aligned with the organization. The

second part of the table compares the coefficients on treatments. The effects on workers' beliefs of the mission-emphasizing treatment and the combined treatment are different from the effect of the financial incentive treatment by 0.23 and 0.26 standard deviations, respectively. Importantly, emphasizing the mission has a similar effect on beliefs in the mission alone and combined treatments, with a difference of 0.036 standard deviation. Appendix Table A7 reports the components of this index. The mission and combined treatments have positive and large effects on all beliefs. Workers in these groups are more likely to believe their organization considers the mission to be important, to believe the mission is aligned with their own thinking, and to feel more attached to their work because of the mission. These effects do not exist for the financial incentive and placebo groups.

These results provide evidence that the workers' preferences for a mission-driven organization is one of the main channels for the mission treatment's influence over the performance of workers.

1.7.2 Altruistic Preferences

I also test if the treatment activates altruistic preferences in the workers. If so, the worker becomes more pro-social and receives utility from performing costly activities without a financial compensation.

A year after the experiment, I find workers in the mission-emphasizing and combined treatments are pro-social in their behavior towards their job. In April 2020, I administered an incentivized activity to elicit the willingness of workers to perform an activity for different rates of compensation, following the Becker-DeGroot-Marschak mechanism. Though the activity was designed to be performed in person, I had to modify the experiment to a phone-based activity due to the Covid-19 pandemic.

During this follow-on activity, my team called workers on the phone and introduced themselves as part of the respective training and/or financial incentives program—or the survey program (for the pure control group)— that the workers had participated in a year ago. The workers were asked whether they would be willing to make a list of households with pregnant women and/or children in return for some to-be-determined remuneration. Then, after confirming that the workers' responses would be kept confidential, the enumerators read out the list of incentive rates one-by-one and asked the workers to inform the research team about whether they would accept such an offer or not. To make their answers incentive-compatible, the enumerator made clear that the actual offer would be selected randomly from their decisions. Though it was made clear to the workers that implementation depended on how the Covid-19 situation evolved,[13] the enumerators impressed upon the workers that their compensation decision would likely be implemented. In the menu of compensation offers, the first was Rs. 0—asking them if they would do the work for free. Responses to this offer helped us understand if the treated workers were motivated to perform the job without any monetary compensation.

Column 2 of Table 1.8 reports the effects of the treatments on workers' willingness to work without a payment. Workers who received the mission treatment are 10.5 percentage points more willing than the control group to perform the extra work without being paid.

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[13]Unfortunately, the pandemic prevented this final implementation.

Compared to this, the workers who received exclusively the financial incentive treatment are 5.8 percentage points *less likely* than the control group to accept the job without a compensating payment, though the effect is statistically not different from the control group. The second part of the table reports that the effects of the mission and combined treatments are different from the financial incentive treatment, though they are similar to each other. These results reveal that the mission treatment makes the workers more intrinsically motivated.

1.8 Alternative Explanations for How the Mission Treatment Works

In this section, I study three alternative mechanisms for the effect of the mission treatment on worker performance. First, I explore if peer influence adds to the individual motivation of workers to perform. Second, I examine if the mission treatment works purely through conveying information about the type of tasks a worker should perform. Third, I evaluate whether it is possible that the mission provides workers with information about being monitored, thereby prompting them to work harder.

1.8.1 The Role of Peers

I explore whether the mission treatment influences the behavior of workers through their peers, in addition to individually motivating them. Such a channel can work in two ways. First, workers' beliefs may change regarding what their peers care about, which in turn may change workers' expectations about their own effort. If workers do not want to appear to be behaving any differently from their peers, they may change their own behavior. Second, workers may not care about deviating from the expected effort level per-se, but they may learn from their peers what is important during the performance of their job. This learning may also stimulate effort.

The design of the experiment helps us untangle the additional effect of mission treatment on workers through their peers. As discussed in Section 1.3.1, the mission treatment was delivered in two different ways. In the first, workers received the treatment individually through one-on-one interactions with a facilitator. Under this individual treatment, I restricted the worker's knowledge about others receiving the same treatment.

Under the second treatment, workers received the treatment in a group setting, where the treatment sessions implied that the organizational mission is common knowledge. Thus, I assumed the effect of the treatment on this group would be through a combination of intrinsic preferences and of the additional effect due to peers. Differencing the effect of individual treatment from public treatment would thus reveal any additional behavioral changes due to changes in expectations about peers' effort. I estimate the effect of the two modes of treatment by estimating the following equation on the full sample.

$$V_{ijmb} = \beta_0 + \beta_1 * MissionPublic_{jb} + \beta_2 * MissionPrivate_{jb}$$
$$+ \beta_3 * FinancialIncentive_{jb} + \beta_4 * Mission + FinancialIncentive_{jb} + \beta_5 * Placebo_{jb} \quad (1.3)$$
$$+ B_{jb} + z_{jb} + M_m + \epsilon_{ijmb}$$

In Column 1 of Table 1.9, I show that the workers in the public and private groups have higher reported motivation for the mission, indicating that their intrinsic preferences are activated in both groups. However, Column 2 shows workers in the private group do not believe their co-workers to be additionally motivated by the mission relative to the control.[14] Though the difference between the private and public group in Column 2 is not statistically different, the magnitude is large. This outcome suggests that, with a bigger sample, the difference could have been statistically significant.

Column 3 of Table 1.9 reports that both the public and private treatments lead to very similar effects on the performance of workers. The second part of the table reports the result of testing $\beta_1 - \beta_2 = 0$. I cannot reject the null hypothesis that the coefficients of private treatment (pure preference channel) and public treatment (a combination of preference and norms channel) are the same. This result suggests the mission treatment may not stimulate an additional effect via expectations about peers. While preferences can be endogenous, the assignment to public treatments was random so I can confidently rule out peer influence as a mechanism of mission treatment.

1.8.2 Mission as Information

The second alternative explanation I test is whether the mission treatment acts as an instrument of learning and information transmission for the workers. It is possible that the workers optimize their efforts on certain tasks in the status-quo based on the information they have. However, conceivably, the mission treatment alters the set of information available to the worker by highlighting duties such antenatal care and child health. Workers following this new information may re-optimize from other tasks to the performance metrics they received via the treatment.

I test for this mechanism by including a placebo treatment within the experiment. The placebo group receives the refresher training, just like the public mission treatment, but does not discuss the mission during the training. If the mission treatment works by channeling information to workers, I should see the placebo also training workers to improve their effort. Additionally, if the mission works through conveying specific topics to the workers, then the workers undergoing the placebo treatment should exert more effort on tasks related to the topics discussed in their refresher training.

I do not find evidence to support this explanation. The placebo treatment does not increase household visits, as reported in Table A1, and also has no effect on the specific tasks related to mother and child health, as reported in Table 1.6. These results suggest that providing information is not the main channel through which the mission treatment works.

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[14]I measure intrinsic preferences and beliefs about others through survey statements. *Mission Importance, Self* is captured by the agreement of workers with the statement "Mission-driven motivation is important for me." *Mission Importance, Others* is captured by the agreement of workers with the statement "Mission-driven motivation is important for my co-workers."

1.8.3 Monitoring

The third potential channel explaining the mission treatment's changes in worker behavior relates to activated concerns about being monitored. Emphasizing the mission may make workers realize the manager considers their job to be important for the mission and will thus be monitoring them more to make sure everyone is performing well. If this channel is activated, workers in the mission-treatment group should believe they are being monitored more than the control group. During the endline survey, I ask all workers to communicate their perception of being monitored during the last few months. I plot the mean response and confidence intervals of the responses for all treatment groups in Appendix Figure A3. There is no visible difference in the perception of workers about being monitored across treatments. Thus, I can rule out monitoring as the main channel for influencing workers undergoing the mission treatment.

1.9 Conclusion

Many organizations use the mission to motivate their workers. However, despite the ubiquitousness of such mission statements and the substantial theoretical interest in this question, no known empirical literature has demonstrated whether organizational missions motivate workers and if this translates into better performance. This paper provides empirical evidence from the field that communication about an organizational mission indeed motivates workers. Such improved motivation yields increased productivity not only within core duties but also across multiple tasks and translates into better health outcomes for children. This finding is especially relevant to settings where performance is not easily observable—such as within public health settings—and/or is not easily enforced through contracts.

The paper also highlights the tension between using a mission to intrinsically motivate versus using financial incentives. Based on the evidence, if policy goals are measurable and do not require multitasking, managers may opt for high powered financial incentives. However, if the goals require workers to perform multiple tasks that cannot be contracted, as is the case with many public services, emphasizing the mission to motivate workers is a powerful tool in getting them to perform better.

A significant number of people living in developing countries rely on the state to provide such basic services as health, education, and sanitation. This reality makes these service providers one of the most important links in the development chain; yet, countries have been spending significant resources on improving outcomes without similar returns on investment. Especially in the context of health service delivery in Pakistan—where this project was implemented—improvements have been slow. Based on the results in this paper, policymakers should consider investing in motivating workers through better organizational designs that keep the mission central to the operational strategy.

While the experiment benefits from the unique organizational features of community health workers who experience non-overlapping areas of responsibility, the findings here are generalizable to many settings. Firstly, many countries employ community health workers to provide outreach services. Consequently, the findings are relevant to many countries even if I limit the generalizability to only those organizations performing the same tasks as

those in my setting. Secondly, these results also speak to the broader question of getting bureaucracies to perform better. Foundations of modern bureaucratic organizations, as outlined by Weber (1922), have no space for emotions, with clearly laid-out rules governing the behavior of service providers. However, the nature of public service still holds more appeals for people who care about serving others. This study provides an example of how bureaucratic organizations can harness the intrinsic motivations of people in order to improve service. However, this requires the job to have a natural orientation towards a mission. How will employees respond if the organization does not have a natural mission to serve a larger purpose, is a question for future research to explore.

The study also opens pathways for future empirical research on the intrinsic motivations of public sector workers. One immediate question to explore is how motivated agents work in teams. While incentives in teamwork have received considerable attention in the literature, one area that has eluded researchers is the area of teamwork in the public sector. With the evidence that mission-motivated workers improve performance holistically in their jobs, the next question to examine is whether such motivations also translate to working in teams with colleagues who may or may not be as motivated.

The mission treatment meaningfully changes the behavior of the workers and even impacts health outcomes. An important question, therefore, is why the health department is not already taking advantage of this clear opportunity to achieve improvement. While we lack the data to answer this question comprehensively, discussions with policy partners reveal that managers in the department do informally adopt the strategy. However, there are several potential explanations for why this is not institutionalized in public sector organizations. For example, the incentives of managers are not aligned with the mission emphasizing events becoming the norm in the organization. It requires costly arrangements that do not directly benefit the managers as their performance is not evaluated based on how workers perform. Understanding why this easy-to-address inefficiency persists represents an important avenue for future work, both because it is practically relevant, and possibly because it may reveal deeper causes of institutional failure.

1.10   Figures

Figure 1.1: Design of the Experiment
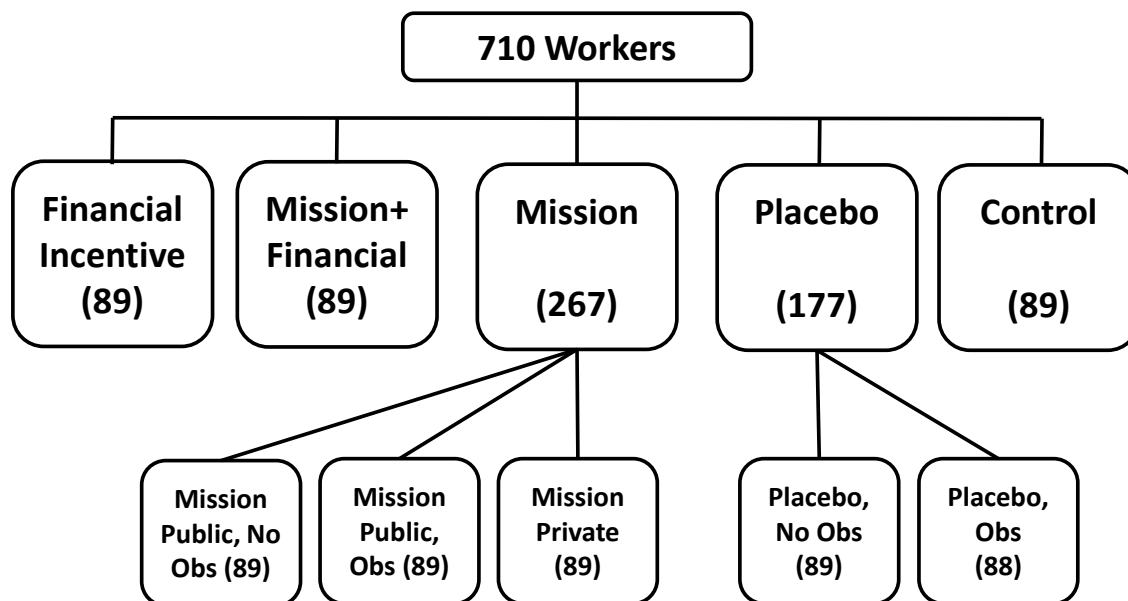
Figure 1.2: Timeline
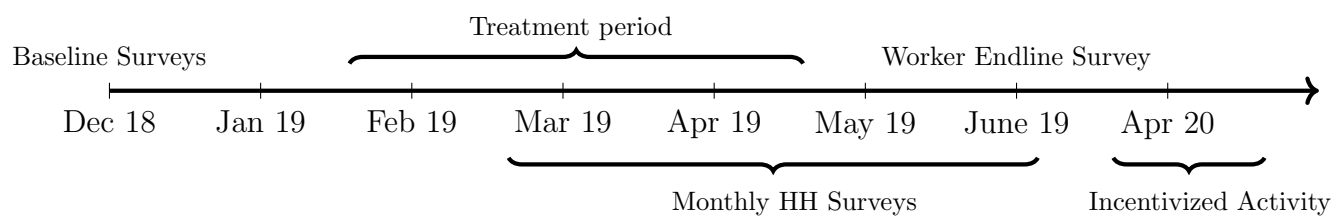
Figure 1.3: Training Activities

Figure 1.4: Survey Activities

Figure 1.5: Effects of Treatments on Household Visits
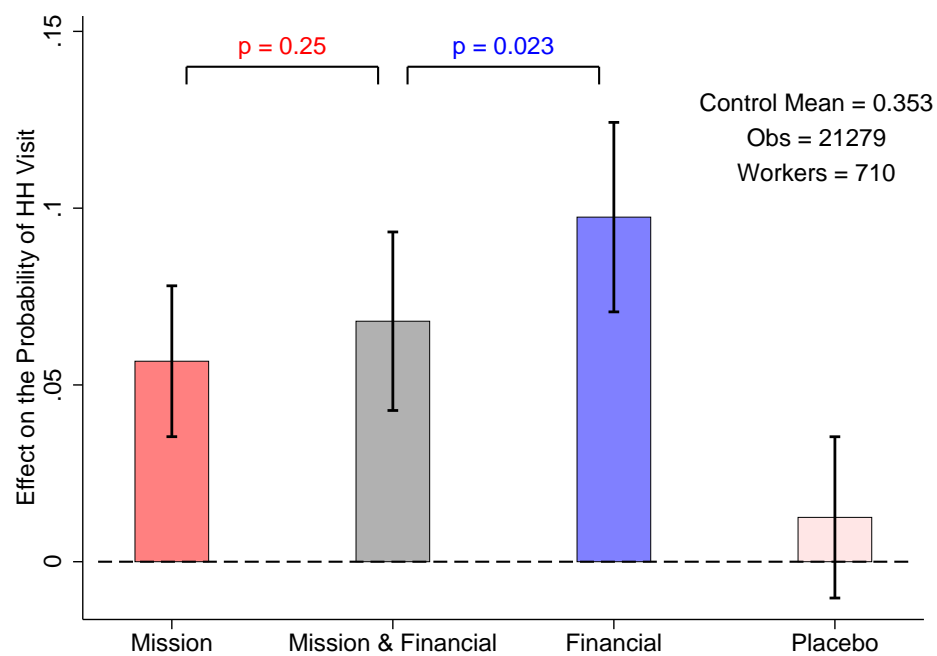
1.11 Tables

Table 1.1: Balance Table: Pooled Treatments

	Total HH Assigned	No. of Preg. Women per HH	No. of Child. Under two per HH	LHW Visit	Distance in mins
A. Control	155.625	0.276	0.516	0.385	15.963
	(3.833)	(0.020)	(0.031)	(0.023)	(0.611)
B. Mission	156.936	0.275	0.484	0.353	16.306
	(2.097)	(0.011)	(0.019)	(0.013)	(0.400)
C. Financial Incentive	156.213	0.284	0.565	0.391	16.691
	(3.716)	(0.020)	(0.039)	(0.022)	(0.599)
D. Mission+Financial Incentive	155.438	0.299	0.508	0.382	16.002
	(3.832)	(0.019)	(0.035)	(0.024)	(0.543)
E. Placebo	154.819	0.288	0.513	0.374	16.268
	(2.605)	(0.015)	(0.024)	(0.015)	(0.411)
Hypothesis tests Joint orthogonality p-value	0.98	0.84	0.44	0.48	0.91
A-B =0	0.76	0.96	0.38	0.22	0.64
A-C=0	0.91	0.77	0.32	0.86	0.39
A-D=0	0.97	0.41	0.87	0.93	0.96
A-E=0	0.86	0.63	0.94	0.69	0.68
# of Households	7099	7099	7099	7099	7099
# of Workers	710	710	710	710	710

Note: Standard Errors clustered at the worker level.

Table 1.2: Balance Table: Disaggregated Treatments

	Total HH Assigned	No. of Preg. Women per HH	No. of Child. Under two per HH	LHW Visit	Distance in mins
A. Control	155.625	0.276	0.516	0.385	15.963
	(3.833)	(0.020)	(0.031)	(0.023)	(0.611)
B. Group Mission	154.326	0.281	0.493	0.361	16.275
	(3.559)	(0.018)	(0.032)	(0.022)	(0.459)
C. Group Mission + Observability	157.966	0.280	0.484	0.354	16.269
	(3.697)	(0.022)	(0.032)	(0.021)	(0.828)
D. Private Mission	158.517	0.264	0.474	0.344	16.373
	(3.624)	(0.019)	(0.036)	(0.023)	(0.740)
E. Group Mission + Financial Incentive	155.438	0.299	0.508	0.382	16.002
	(3.833)	(0.019)	(0.035)	(0.024)	(0.543)
F. Financial Incentive	156.213	0.284	0.565	0.391	16.691
	(3.716)	(0.020)	(0.039)	(0.022)	(0.599)
G. Socialization	153.303	0.298	0.492	0.394	16.416
	(3.707)	(0.021)	(0.032)	(0.023)	(0.616)
H. Socialization + Observability	156.352	0.278	0.534	0.353	16.119
	(3.656)	(0.020)	(0.036)	(0.018)	(0.544)
Hypothesis tests Joint orthogonality p-value	0.98	0.94	0.71	0.59	0.99
A-B =0	0.80	0.86	0.61	0.45	0.68
A-C=0	0.66	0.90	0.48	0.32	0.77
A-D=0	0.58	0.66	0.37	0.21	0.67
A-E=0	0.97	0.41	0.87	0.93	0.96
A-F=0	0.91	0.77	0.32	0.86	0.39
A-G=0	0.66	0.46	0.59	0.78	0.60
A-H=0	0.89	0.94	0.70	0.29	0.85
# of Households	7099	7099	7099	7099	7099
# of Workers	710	710	710	710	710

Note: Standard Errors clustered at the worker level.

Table 1.3: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
# of Households in Community	155.97	34.913	68	232	710
Years of Schooling	10.034	2.405	5	18	707
Healthcare Certificate	0.38	0.486	0	1	707
Tenure in Years	15.299	5.458	1	27	575
Proportion of HHs visited	0.371	0.21	0	1	710
Proportion of HHs with Pregnant Women	0.26	0.17	0	0.9	710
Proportion of HHs with Children	0.397	0.221	0	0.9	710

Table 1.4: Balance Table: Individual Characteristics

	Years of Schooling	Health Diploma	Tenure in Years	PSM Score	IQ Score
A. Control	10.253	0.352	16.000	3.664	0.602
	(0.246)	(0.051)	(0.664)	(0.068)	(0.021)
B. Mission	10.007	0.376	15.624	3.659	0.575
	(0.146)	(0.030)	(0.359)	(0.036)	(0.014)
C. Financial Incentive	10.273	0.466	13.746	3.595	0.579
	(0.267)	(0.053)	(0.689)	(0.068)	(0.021)
D. Mission+Financial Incentive	9.795	0.398	15.870	3.631	0.546
	(0.222)	(0.052)	(0.639)	(0.067)	(0.024)
E. Placebo	9.966	0.350	14.966	3.563	0.548
	(0.194)	(0.036)	(0.449)	(0.049)	(0.016)
Hypothesis tests					
Joint orthogonality p-value	0.58	0.45	0.08	0.56	0.25
A-B =0	0.39	0.69	0.62	0.94	0.29
A-C=0	0.96	0.12	0.02	0.47	0.46
A-D=0	0.17	0.53	0.89	0.73	0.08
A-E=0	0.36	0.97	0.20	0.23	0.04
# of Households	707	707	575	709	710
# of Workers	707	707	575	709	710

Note: Standard Errors clustered at the worker level.

Table 1.5: Effects on the Probability of Household Visit

| | *Dep Var: Household Visit = 1* | |
	(1)	(2)
Mission	0.051***	0.057***
	(0.012)	(0.011)
Financial Incentive	0.101***	0.097***
	(0.015)	(0.014)
Mission and Financial Incentive	0.069***	0.068***
	(0.014)	(0.013)
Placebo	0.013	0.013
	(0.012)	(0.012)
Control Mean	0.353	0.353
# of Observations	21279	21279
# of Workers	710	710
Block & Wave Fixed Effects	✓	✓
Baseline Controls	-	✓
	Linear Combinations of Coefficients	
Mission − Financial Incentive	-0.050***	-0.041***
	[0.000]	[0.000]
Mission − Mission and Financial	-0.018	-0.011
	[0.126]	[0.254]
Financial Incentive − Mission and Financial	0.033**	0.029**
	[0.031]	[0.023]

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This table reports the effects of treatments on the probability of household visits using a linear probability model. It uses household-level data collected from three rounds of surveys. The first part of the table reports the coefficients on each treatment dummy. Standard errors clustered at the worker level are reported in parentheses. Results in Column (1) do not control for the baseline performance whereas Column (2) does include baseline performance as a control. Each regression uses randomization-block and survey-wave fixed effects. The second part of the table reports linear combinations of coefficients and tests them against a null of zero difference. $p-values$ of the tests are reported in square brackets.

Table 1.6: Effects on Multitasking

	Antenatal Check = 1	Children Examined = 1	Discussed Prevention = 1	Tuberculosis Screening	# of Vaccination Camps	Multitasking Index
	(1)	(2)	(3)	(4)	(5)	(6)
Mission	0.052**	0.032**	0.050*	0.047**	0.468*	0.176***
	(0.022)	(0.015)	(0.029)	(0.021)	(0.269)	(0.043)
Financial Incentive	-0.004	0.024	0.024	0.005	0.167	-0.009
	(0.028)	(0.017)	(0.036)	(0.023)	(0.326)	(0.052)
Mission and Financial Incentive	0.046*	0.027	0.058*	0.044**	0.476	0.143***
	(0.025)	(0.017)	(0.034)	(0.022)	(0.345)	(0.051)
Placebo	-0.039	0.009	0.016	0.018	-0.290	-0.022
	(0.026)	(0.016)	(0.031)	(0.022)	(0.292)	(0.047)
Control Mean	0.359	0.457	0.477	0.360	5.716	0.000
# of Observations	1915	3347	7100	8588	702	710
# of Workers	646	689	710	710	702	710
Condition	Pregnant	Children	-	Visit	-	-
Data Source	HH Survey	HH Survey	HH Survey	HH Survey	Worker Survey	-
Linear Combinations of Coefficients						
Mission − Financial Incentive	0.057***	0.008	0.026	0.042***	0.301	0.185***
	[0.000]	[0.491]	[0.349]	[0.006]	[0.244]	[0.000]
Mission − Mission and Financial	0.007	0.004	-0.008	0.003	-0.008	0.033
	[0.613]	[0.708]	[0.767]	[0.838]	[0.979]	[0.347]
Financial Incentive − Mission and Financial	-0.050**	-0.003	-0.034	-0.039**	-0.309	-0.152***
	[0.014]	[0.820]	[0.309]	[0.021]	[0.357]	[0.001]

Note: This table reports the effects of treatments on multitasking. The first four columns report the effects of treatment on the probability workers perform specific tasks using household-level data collected through surveys, and Columns 5 reports the effects of treatments on the number of vaccination camps using worker-level data. Column 6 combines the data used in the first five columns into a summary index of multitasking. Each regression controls for randomization-block fixed effects. Analysis using data from multiple rounds of surveys (Columns 1, 2 and 4) also control for survey-wave fixed effects. Standard errors are clustered at the worker level and reported in parentheses. The second half of the table reports linear combinations of coefficients on the treatments and tests them against a null of zero difference. $p-values$ of the tests are reported in square brackets.

Table 1.7: Effects of Treatments on Health Outcomes

	Prevalence of Diarrhea	Proportion Timely Vaccinated	Mortality Rate: Children	Mortality Rate: Mother	Health Index
	(1)	(2)	(3)	(4)	(5)
Mission	-0.071**	0.031***	-0.003	-0.001	0.205***
	(0.035)	(0.011)	(0.002)	(0.001)	(0.051)
Financial Incentive	-0.098**	0.012	-0.001	0.000	0.121*
	(0.039)	(0.013)	(0.003)	(0.002)	(0.068)
Mission and Financial Incentive	-0.076*	0.029**	-0.001	-0.000	0.164***
	(0.039)	(0.012)	(0.003)	(0.001)	(0.059)
Placebo	-0.002	0.007	-0.001	-0.001	0.048
	(0.036)	(0.011)	(0.002)	(0.001)	(0.054)
Control Mean	0.287	0.888	0.008	0.002	-0.000
# of Observations	2292	5136	703	703	710
# of Workers	686	710	703	703	710
Data Source	HH Survey	HH Survey	Admin	Admin	–
	Linear Combinations of Coefficients				
Mission − Financial Incentive	0.027	0.019***	-0.002	-0.001	0.084
	[0.364]	[0.002]	[0.508]	[0.343]	[0.127]
Mission − Mission and Financial	0.005	0.002	-0.002	-0.001	0.041
	[0.855]	[0.841]	[0.416]	[0.503]	[0.347]
Financial Incentive − Mission and Financial	-0.021	-0.018	0.000	0.001	-0.043
	[0.530]	[0.102]	[0.989]	[0.699]	[0.489]

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This table reports the effects of treatments on the health outcomes mentioned in the column headers, using household and administrative data. Each regression controls for randomization-block fixed effects and Column 2 also uses survey-round fixed effects as the information was collected in multiple rounds. Standard errors are clustered at the worker level and reported in parentheses. The second half of the table reports linear combinations of coefficients on the treatments and tests them against a null hypothesis of zero difference. $p-values$ of the tests are reported in square brackets.

Table 1.8: Intrinsic Motivation of Mission-Treated Workers

	Index of Mission Motivation (1)	Willingness to Work for Rs. 0=1 (2)
Mission	0.201***	0.105*
	(0.071)	(0.059)
Financial Incentive	-0.031	-0.058
	(0.090)	(0.076)
Mission and Financial Incentive	0.238***	0.135*
	(0.079)	(0.070)
Placebo	-0.146*	0.012
	(0.081)	(0.065)
Control Mean	0.000	0.614
# of Observations	705	707
# of Workers	705	707

	Linear Combinations of Coefficients	
Mission − Financial Incentive	0.232***	0.163***
	[0.001]	[0.007]
Mission − Mission and Financial	-0.036	-0.029
	[0.515]	[0.578]
Financial Incentive − Mission and Financial	-0.269***	-0.193***
	[0.001]	[0.007]

Note: This table reports the effect of treatments on two measures of intrinsic motivations. Column 1 reports the effect of treatment on a mission-motivation index that combines workers' responses to three statements: (1) Importance: "I like the LHW program more than other departments because of the importance it places on the mission." (2) Alignment: "I believe the LHW program's mission is very similar to my thinking since the beginning of 2019." (3) Attachment: "If the LHW program's mission was something else, I would not have been as attached to the program." Column 2 depicts workers' willingness to work for Rs. 0, using the BDM method in a lab-in-the-field activity. All regressions control for randomization-block fixed effects, and standard errors are clustered at the worker level. The second panel reports differences between coefficients and tests them against a null hypothesis of zero. $p-values$ of the tests are reported in square brackets.

Table 1.9: Peer Influence

| | Mission Importance: | | Household |
	Self	Others	Visit = 1
	(1)	(2)	(3)
Individual Treatment	0.324**	0.144	0.054***
	(0.130)	(0.131)	(0.013)
Group Treatment	0.258**	0.215*	0.058***
	(0.120)	(0.125)	(0.011)
Control Mean	0.000	-0.000	0.353
# of Households	701	700	21279
# of Clusters	701	700	710
Data Source	Worker Survey	Worker Survey	HH Survey
Baseline Control	-	-	✓
	Linear Combination of Coefficients		
Group - Individual Treatment	-0.065	0.071	0.004
	[0.469]	[0.497]	[0.702]

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This table reports the effects of mission treatment's sub-treatments. Columns 1 and 2 use data from the endline survey of workers to test whether workers' stated beliefs about the importance of the mission to themselves and their beliefs about their co-workers are affected by the treatment's mode of delivery. *Mission Importance, Self* is captured by whether the workers agree with the statement "Mission-driven motivation is important to me." *Mission Importance, Others* is captured by whether workers agree with the statement "Mission-driven motivation is important to my co-workers." Column 3 uses household-survey data to test whether public delivery of the mission training had any positive effect on worker performance beyond the effect of the workers' intrinsic preferences, captured by the privately delivered treatment. The first half of the table reports selected coefficients from a full regression, as per Equation 3.2. The regressions control for randomization-block fixed effects. Column 3 also controls for the survey-wave fixed effects and baseline performance. Standard errors clustered at the worker level are reported in parentheses. The second half of the table reports the linear combination of the coefficients and tests them against a null hypothesis of zero difference. The $p-value$ of the tests are reported in square brackets.

Chapter 2

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

2.1   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, 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 (Benabou and Tirole, 2006, Deci, 1971, Frey, 1997), it can also induce more competent, perhaps career-minded, people to seek political office (Ferraz and Finan, 2011). Conversely, highlighting pro-social motivations can encourage more socially-minded people to seek office (Deci, 1972, Besley, 2005), but they may also give public cover to more selfish people to

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[1]See, for example, Ferraz and Finan (2011), Pande (2011), 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, 2004a, Querubin et al., 2016); may be motivated by private rents (Fisman et al., 2014, Ferraz and Finan, 2011, 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).

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 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

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[4]Our formulation of treatments in the political sphere are inspired by Ashraf et al. (2018) who examine bureaucratic recruitment and demonstrate that career benefits attract talented individuals to apply for a new health care position in Zambia.

[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.

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 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 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.

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

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, 2004a).

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). While previous work on political and bureaucratic selection shows that pecuniary and career incentives matter in recruiting agents who are more competent, and thus may improve policy, our examination of pro-social motivation remains understudied (Dal Bó et al., 2013, Gagliarducci and Nannicini, 2013, Fisman et al., 2015, Ashraf et al., 2018, Deserranno, 2019).[10] 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).

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, 2005). Similarly, non-financial rewards have been shown to boost performance particularly on pro-social tasks (Ashraf et al., 2014b). 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

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[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.

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, Benabou 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, 1995, Ostrom, 2000).

2.2 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.

2.2.1 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, Khan, and Myerson, Cheema et al.). 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 2.1, 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 2.2. These districts have slightly better health, education, and public service outcomes compared to the provincial averages (P&DD, 2008). As we describe below, these districts were chosen once we identified a local partner.

2.2.2 Village Councils

We focus on village councils that represent about 6,500 voters on average (see Table B1 for descriptives).[11] Consistent with local elections in South Asia, voter turnout in 2015 was high at around 76% signaling their importance to local communities. This is about 21 percentage points higher than the national turnout in the general elections of 2013.

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 B.1, 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.

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

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, 2004a, 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.

2.3 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 B.4 presents a timeline of the project.

2.3.1 Public and Private Meetings

Activities on the ground proceed as follows. First, the **private meetings**. A pair of enumerators from our partner NGO Sangum canvass on average 48 households 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 2.3.2 for description). Finally, subjects are invited to a public meeting in the village, and the time and location details for these are shared.[14]

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[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.2 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 2.3 shows examples of these interactions between enumerators and citizens.

Second, the **public meetings**. Enumerators then proceed to prepare for the public meeting. All the public sessions are organized within the same village to make them accessible for citizens. During the session, the field staff follows the guidelines discussed in section 2.3.2. 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 2.4 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.

2.3.2 Treatments on how Political Office is Portrayed

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

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

There are some additional aspects of the treatment that are important. First, we focus on keeping the interaction between enumerators and citizens natural. A consequence of this is that we kept pre-treatment surveys short. 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 B.3), 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

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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 B12 shows that there is no evidence for differential selection into the public meeting by social versus personal treatments.

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.

2.3.3   Randomization

Figure 2.5 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 2.1.

2.3.4   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 B3. Overall, the tests suggest that the randomization was successful. Appendix Table B1 presents the summary statistics for the subject pool.

2.3.5   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 B.5, 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 2.4.2 and 2.4.3, are registered as the primary outcomes of interest in the pre-analysis plan. Second, the performance outcomes reported in Section 2.4.4 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 2.4.2, 2.4.3 and 2.4.4. As such, we treat these comparisons as the primary hypotheses of interest. We also decompose the main

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

treatment effects in various ways to analyze how the main effects came to be. These are secondary, and exploratory, analyses.

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

2.4.1 Estimation

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

$$Y_{iv} = \beta_1 Neutral_v + \beta_2 Social_v + \beta_3 Personal_v + \gamma_v + \varepsilon_{iv} \tag{2.1}$$

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 2.1 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 2.1; 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 2.1. 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]

2.4.2 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.

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[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.

Table 2.2 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 Table B4).

2.4.3   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 2.2 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.

2.4.4   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.

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[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.

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 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 B8). The data show that there is good adherence to rules on this margin.

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]

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

[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 Table B6 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 2.5.2 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 2.6 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 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 2.3 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 2.6. As before, the effects move in opposite directions when social or personal benefits are directly compared against the neutral condition.

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 2.7 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]

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 B10). 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 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

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[23]Appendix Table B9 presents the results in tabular form.

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).

2.5   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.

2.5.1   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:

$$Y_{iv} = \beta_{nn}N_v + \beta_{sn}A_v + \beta_{ns}B_v + \beta_{ss}C_v + \beta_{pn}D_v + \beta_{np}E_v + \beta_{pp}F_v + \gamma_v + \varepsilon_{iv} \quad (2.2)$$

where A-E are indicator variables that correspond to each labeled cell in Table 2.1, 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 2.4 presents the results on candidacy, election, and overall policy alignment.[25] We see that the main treatments effects we described above are concentrated pri-

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[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 B12).

[25]See appendix Table B11 for disaggregated policy effects for each budget category.

marily in areas where messages were delivered in public meetings. When social benefits from office 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.

2.5.2 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'.

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 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 2.2. 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., 2018). 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 B15. 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 treat-

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[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'.

[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 B13. 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 B13. In the Appendix, we re-generate Table B14 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.

ment effects: candidates in social versus neutral villages report more income and are more 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.

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 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 B16: 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 B17 that while the baseline rate of political engagement is fairly high at

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

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]

2.6 Conclusion

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

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

Our experiment reveals that non-pecuniary 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.

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

[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.

2.7 Figures

Figure 2.1: Village Councils in Political Hierarchy
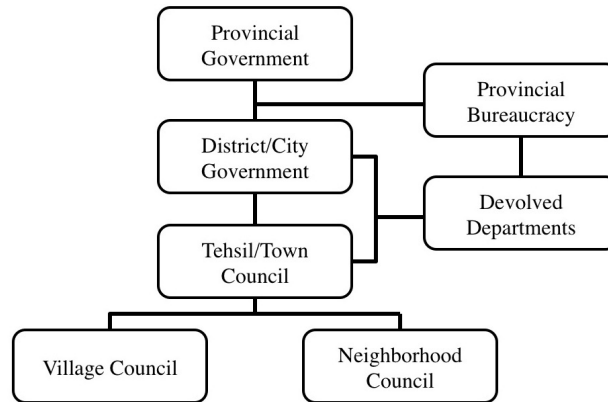
Figure 2.2: Villages in Haripur and Abbottabad Districts (right) in Khyber Pakhtunkwa Province (left)
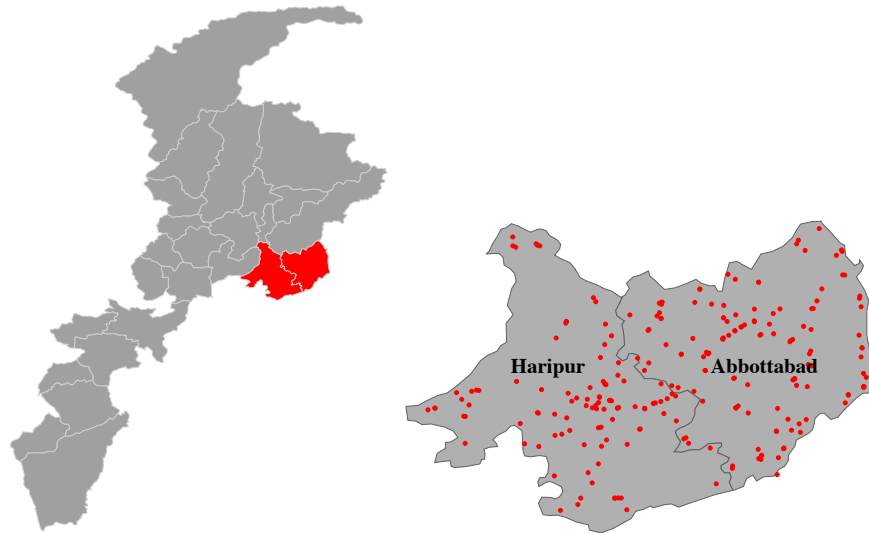
Figure 2.3: Private One-on-One Meetings

Figure 2.4: Public Meetings in Villages

Figure 2.5: Design of Field Experiment
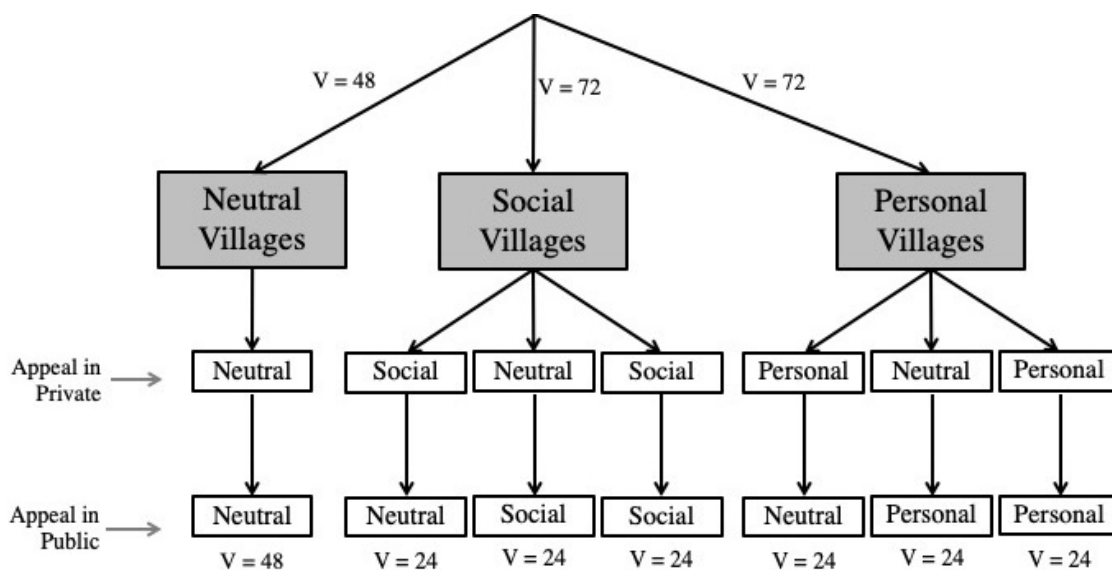
Notes: This figure shows the randomization scheme. All treatment randomizations are at the village level. *V* refers to the number of villages in a treatment category. The bottom two layers of the figure show the type of appeal made to a person to run for office. See text for details.

Figure 2.6: Citizen Preferences and Council Spending in Neutral Villages
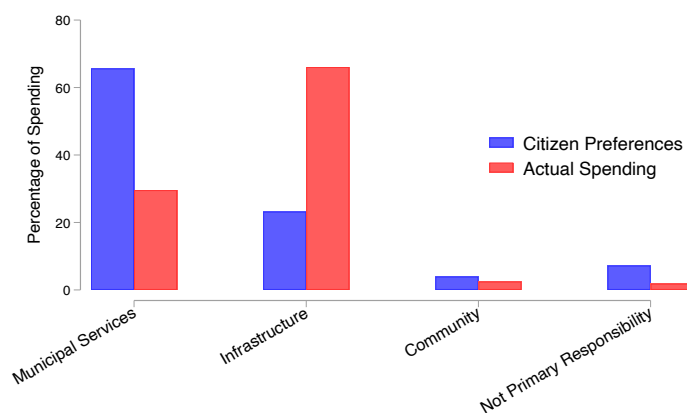
Notes: This figure shows the proportion of budget allocated by citizens to different categories in a hypothetical exercise against the allocations by elected councils in the actual annual budget of 2016 in Neutral Villages.

Figure 2.7: Social versus Personal Policy Alignment Over Time (District Haripur only)
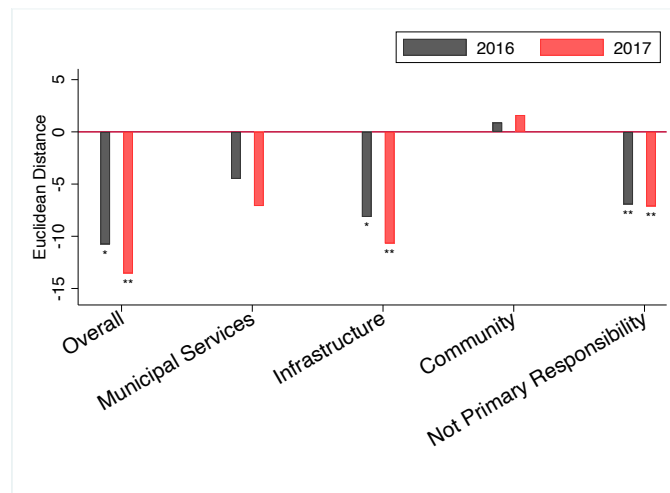
Notes: * Exact $p < 0.1$, ** Exact $p < 0.05$, *** Exact $p < 0.01$.

2.8 Tables

Table 2.1: Village Level Treatment Allocation

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

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

Table 2.2: 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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. 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.

Table 2.3: 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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This table uses a village level dataset that is constructed based on official budget data from the councils and the preferences of citizens regarding the budget. The dependent variable in each column is defined as the quadratic distance between citizen preferences and actual spending by the council. The distance for each category is calculated using the formula $\sqrt{(B_{ji} - \overline{C}_{ji})^2}$, where B_{ji} refers to the percentage of the budget spent on category j in village i and $\overline{C}_{ji}$ is the average of citizen preferences for spending on that category in village i. The overall difference in column (1) is calculated using the formula: $\sqrt{\sum_J (B_{ji} - \overline{C}_{ji})^2}$. Each regression uses block fixed effects. Robust standard errors are reported in parenthesis. Exact p-values are in square brackets.

Table 2.4: 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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. 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 2.4.4. 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.

Table 2.5: Sub-group Effects by Pro-Social Type

Pro-social type:	Candidate=1			Elected=1		
	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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. 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.

Chapter 3

Relaxing Barriers to Political Entry of Non-Elites: Experimental Evidence from Local Government Elections in Pakistan

3.1 Introduction

Elite capture of institutions, especially decentralized institutions, is well established (Bardhan and Mookherjee, 2000a). Elected offices that are meant to serve the interests of voters are routinely contested by candidates who belong to elite families (Broockman, 2014)) and political dynasties (Dal Bó et al. (2009), Cheema et al. (2013). In contrast, the non-elites are often missing from politics. In this paper, we propose three possible reasons that may deter potential candidates from entering politics. We design and implement a randomized control trial that addresses the reasons behind a lack of non-elite participation in local government elections, and studies its effect on the actual candidacy decision.

Decentralization of the elected institutions has been one of the central themes of reform across the developing world. Two related beliefs have primarily driven the reform process. One, bringing government closer to the people will make it more responsive to the needs of local public goods by being able to aggregate their preferences better (Oates, 1999); and second, citizens will be able to hold their leaders accountable (Bardhan and Mookherjee, 2000a). This second belief rides on the underlying assumption that the elected officials in a distant government come from more powerful backgrounds and hence the populace cannot hold them accountable. In contrast, politicians at a closer level, elected to decentralized offices, are easy to monitor and will thus be more accountable to the citizens. Rich literature from the last three decades compellingly shows that decentralization has failed to deliver (Wibbels, 2006) mainly because of elite capture of local institutions (Bardhan and Mookherjee, 2000b, Mattingly, 2016, Fritzen, 2006). This begs the question: why do non-elites not enter politics to ensure a better distribution of public goods?

Several potential reasons may constrain an individual from entering politics [1]. The

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[1]The decision of citizens to run or not for public office is important because it can have implications for

cost-benefit analysis of a potential candidate is affected by the cost of making the entry, and the expected benefit one can have from office. This analysis requires information about the possibility of winning elections and also the knowledge of expected benefits from office. Existing research suggests that people often lack information that can shape their political behavior (Casey, 2015). Thus providing information can potentially induce citizens to enter politics. Prospective candidates may not join politics if the decision is net costly for them, either because of a high cost of entry or limited financial benefit from it (Messner and Polborn, 2004). Empirical evidence on the financial benefits has found that selection into the public sector is sensitive to the incentives associated with it (Dal Bó et al., 2013). Such financial rewards also motivate politicians once in office (Ferraz and Finan (2009), Gagliarducci and Nannicini (2013)). There is, to our knowledge, no study on the cost side of the equation that affects the decision to run for office.

We design and implement an experiment before the first ever Village Council elections held in rural Pakistan in 2015. We divide 240 villages in 196 treatment and 44 pure control villages. In each of the treatment villages, we survey about 48 individuals selected at random and ask them to nominate potential candidates who are suitable for public office but may not run for it. We get on average ten nominated politicians per village with whom we run an experiment to unpack the constraints that restrict political entry. We seek out those citizens who are on the margins of politics but may not enter the race for office and provide them with one of the three treatments. The first treatment tests whether the subsidizing the cost of declaring candidacy drives political entry; the second examines if information about the chances of winning the election affects entry, and the third treatment focuses on information about benefits from office.

To study if cost is a barrier, we provide a subsidy to selected individuals in the form of services of a lawyer. Free services of a lawyer substantially reduce the cost of formally declaring candidacy for the citizen. The lawyer prepares official declaration papers for the candidate and presents them for scrutiny to the Returning Officer with judicial power to approve the application. The Returning Officer cross-examines the potential candidates, assisted by the lawyer, to ensure they meet the minimum eligibility requirements.

To examine if information frictions are also restricting entry in politics we run two information campaigns. In the first, we inform the potential candidates that they have been nominated to run for office by others in the village. We interpret this treatment as information about electability. In the second information treatment, we inform th prospective candidates about the benefits they can have from office. The benefits information is further divided into social and personal benefits as explained below. Table 3.1 shows the sample size and cross randomization of each treatment.

This experiment was conducted before the elections for Village Councils in 2015 in two districts of Khyber Pakhtunkhwa province of Pakistan. After the elections, we acquire administrative records from the Election Commission of Pakistan that includes lists of candidates on the ballot and the number of votes they received in the election. The administrative data is matched with the list of potential candidates to identify individuals who formally ran for office and also those who won the elections.

The analysis using the matched survey and administrative data suggests three lessons

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policy (Besley, 2005, Ashworth, 2012) and provision of public services (Chattopadhyay and Duflo, 2004a)

about non-elites' entry into politics. First, the cost of running for office is a significant barrier that prevents non-elite citizens from entering public life. Subsidizing the candidacy process through the provision of a lawyer increases the probability of a citizen to run for office. This result is robust to the use of intention to treat design and an instrumental variable approach.

Second, information on the chance of winning the election (electability) and the benefits from office do not appear to have a major effect on non-elite candidacy. They do not seem to be taken into account when citizens decide to run for office. However, there is evidence that for specific populations electability and benefits matter. We collect prior beliefs of citizens about their chances to win an election and the benefits they can get from office. Using prior beliefs to study the heterogeneous response of citizens, we find that citizens who have higher expectation of benefits from office are more likely to respond to the cost subsidy treatment in the form of a lawyer. Breaking down the prior beliefs and analyzing response at different levels of beliefs highlight the importance of strong priors, almost all treatments primarily affect individuals who are very confident of their chances to win in the elections.

The role of costs and benefits in the political entry decision have been studied extensively in the theoretical literature (Caselli and Morelli (2004)). There appears to be a trade-off between the competence of elected leaders and their representativeness (Besley and Coate, 1997). However empirical research suggests that financial incentives attract and retain competent politicians (Ferraz and Finan (2011)). However, financial incentives do not tell the full story. Keeping the financial incentives constant, special interest groups' information and mobilization efforts shape the set of people who consider entering politics (Broockman (2014), Preece et al. (2016)). This results in a situation where underrepresented groups do not believe the information coming from "party establishments" (Butler and Preece, 2016) and in the absence of another source, information may become a hard barrier preventing them from entering politics. In this paper, we overcome these barriers by undertaking a non-partisan drive and rely on randomly selected individuals to identify potential candidates instead of relying on party establishments.

This study makes four main contributions to the literature. First, to our knowledge, this is the first paper to study the decision process of non-elite citizens to become politicians experimentally. Second, the paper has been able to identify the role of cost as a barrier to politically entry and provides conclusive evidence that, keeping financial incentives constant, equally competent candidates may sit out the elections due to costs associated with it. Third, it makes use of prior beliefs to identify how different types of citizens respond to the three types of barriers. Potential non-elite candidates, who are confident of their chances or have high expectations of benefits, may need a little nudge to encourage political entry. Fourth, it validates a methodology of identifying candidates that are equally competent and more representative of the general population. This method may be adopted by democracy promotion programs in weak and new democracies.

The main contribution of this paper is to take a holistic view of the decision equation of the potential politicians. The candidacy literature on non-elite representation is still in infancy and has mostly focused on gender (Butler and Preece, 2016) and working class (Carnes and Lupu, 2016) candidacy in American context. This is one of the first papers to study the decision process of non-elites in a developing country context that have implications for real election. Our work is closest to a working paper by Cruz and Davidson (2017)

regarding the methodology identifying potential candidates[2]. Their interest is limited to increasing competition, through identification and conveying the electability information. We take a more holistic look at the calculus of potential individuals regarding political entry. Additionally, they focus on village level competition whereas we are focused on individual decisions to run for office. Hence we are better suited to identify the factors that constrain political participation and may contribute to the continued capture of local institutions by elites.

In rest of the paper, we first provide a brief description of the candidacy decision, then describe the experiment and sampling methodology. We explain the characteristics of our sample and the data collection during the study. We then lay out the primary and heterogeneous results. We also discuss alternative explanation and provide a short assessment of the election results.

3.2   Non-Elite Candidacy

Political representation of citizens is one of the fundamental tenants of democracies (Pennock and Chapman, 1968). In an ideal society, voters have certain preferences, interests and principals. They rally around candidates with similar interest and those who can act as their representatives. However, in reality elected representation has been limited to elites in several countries as a result of capture and dynastic politics (Dal Bó et al., 2009, Cheema et al., 2013) [3]

The dearth of candidates from non-elite class is not because of legal bars on running for office - usually in democracies anyone allowed to vote can also become a candidate. Rather, the lack of average citizens in politics is driven by other factors that have been rarely investigated. These factors include the cost of running in election, an assessment of whether the persons will be able to get votes that informs their chances of winning, and finally, an expectation of the benefits from office, either social or personal.

When citizens decide on whether to become politicians they must undertake a cost benefit analysis at the individual level, but also encompassing their community or the group that they belong too. The first element of such an analysis is how much can they spend on running for office. While campaign finance issues have been studied extensively in developed countries Jacobson (1978), the literature on developing countries is sparse. Campaign financing requirements pose a significant burden on citizens of all types interested in running for office, but its adverse effects are more pronounced on minority and under-represented groups such as the poor or females Wolak (2015). Reduction in the costs associated with elections will most certainly increase participation in politics.

The benefit side of the candidacy equation is more complex then cost side. First, for any benefits to accrue the potential candidates should have a sense of their chances to win the election. This requires expanding resources on collecting information from the electorate through polls. Since non-elites are less likely to have such resources to spend on gathering

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[2]Another paper that is closer in taste to this study is Ravanilla (2016). However, the focus of that work is on training good quality politicians whereas we are focused on alleviating barriers to entry

[3]Several countries have tried to correct the situation by enacting quotas (Chattopadhyay and Duflo, 2004b).

the information about their *electability*, they are less likely to enter the race even if the voters are willing to support them.

Once the candidates understand their chances, they have to evaluate the benefits from office in case of winning the election. The benefits can be of two types, one monetary and other non-monetary. While the monetary compensation is fixed for a given office (assuming no corruption), the non-monetary part varies and depends on the person. There are two main components to the non-monetary part of the benefits from public office. One is personally benefiting from office in the form of prestige, recognition and status, and second is to gain satisfaction from pro-social aspects of the job such as helping the community (Benabou and Tirole, 2003). These benefits from the job may seem obvious to some people, but for the non-elites in rural areas they may not have enough information on what a public office entails. It is also possible that they expect the benefits to accrue to only those candidates who are part of the traditional ruling elites, which may further reduce the expected benefit from office for them.

Taken together these factors help the citizens decide whether to enter politics or not. Assuming, all individual undertake the calculus, many will be far off from the net positive benefit, such as those not interested in politics at all. It is only those that are close to get a net positive value from running in elections, are the possible contenders for the office. Among the non-elites the possible contenders for political office may end up not running as they are just marginally short of getting a net positive benefit from elections. Those candidates, marginally short of the decision, are the ones who may have high expectation of benefits but are short on the cost side or those who can cover the cost, are also aware of the benefits but do not have the resources to poll the voters to form an expectation about their chances to win. In short, the prospective candidates will believe the elections are costly for them, may believe there are not many benefits from office or may not know their chances to wig.

The simple exposition of the decision to run for office as a cost benefit analysis helps us to design an experiment that can test if these barriers matter for the decision of the non-elites to not run for office. Acknowledging that not every citizen is interested in politics or can be a viable candidate, and identifying viable candidates is key to understanding the cost benefit analysis of non-elites and to increase their representation in politics, we can draw two sets of hypothesis from this discussion. The first set is that cost of running a campaign or filing papers, and the lack of information about the chances to win and the benefits from office are the driving force behind the lack of non-elites candidates. second, it is the people close to getting a net positive benefit from office that are most affected by the cost and information barriers to office. Thus the closer an individual is to get a net benefit, in expectation, from running for office the more likely they are to respond to any incentives. Individuals who have a higher perception of affordability of the election, better knowledge about the benefits from office, or have a strong belief in their chances to win are most likely to be closer to entering the electoral race.

To test the ideas discussed in this section, we implement a study before the first ever Local Government Elections in Pakistan. Using a random survey of the households we first identify potential non-elite candidates who are on the border of becoming politicians. This non-elite sample of prospective candidates is randomly allocated to three different main treatments that relaxes the cost constraints and removes information barriers about the

candidates' chances to win the election and the benefits they can have from office.

3.3   Experiment

We conduct an experiment in 240 villages spread across Haripur and Abbottabad districts in Khyber Pakhtunkhwa Pakistan. In each village, we conduct a survey of citizens to identify potential candidates that are on the boundary of deciding whether to run for office or not. These individuals are randomly assigned to three sets of treatments including subsidy through the provision of a lawyer to file candidacy papers, information on their electability and information on non-monetary benefits from office.

3.3.1   Context

Before 2015, the lowest tier of elected government in Khyber Pakhtunkhwa (KP) Pakistan was the provincial legislature. A previous law introduced in 2000 that formed the elected government structure below the provincial assembly had been suspended in 2008. The old law stipulated the lowest elected tier of the government to be the Union Council (UC). [4]In 2015, the Government introduced Village Councils (VCs) as a new tier of local governments for the very first time and reinstated District and Town councils that were in suspension since 2008. As a result of reform, the 986 union councils are replaced by 3493 village and neighborhood councils, increasing the number of representatives in the province to around forty thousand from a few hundred. First elections under the new law took place in May 2015 on a non-party basis for the VCs.

Each Village Council comprises seven to 15 elected members depending on the population of the village. Out of these, up to seven council seats are open to everyone in the village, called the general seats. Whereas the remaining seats are reserved for youth, peasant, and women representatives. The elections take place on the basis of a single ward system for each category, where the village serves as the ward. A candidate winning the most number of votes in the general category is declared Nazim (head) of the council while the one securing the second most number of votes is declared Naib Nazim (deputy head) of the council.

A village council is responsible for governing the village as an administrative unit. Each VC is responsible for monitoring of public services provided in the village, registration of births and deaths and setting up an alternative dispute resolution mechanism. Each year the VCs are allocated funds for development of the village through an Annual Development Plan prepared by the council.

In the new system, the establishment of the Village Councils creates an opportunity for the emergence of a new crop of politicians from the citizens who had not been formally part of governance in the village. We use this unique setting to study the decision of the citizens to become politicians by officially declaring candidacy in the elections.

To study the candidacy process we conducted two experiments. The main experiment reported in Gulzar and Khan (2021) focuses on the social dimensions of candidacy. We provided information on personal and social benefits from office to citizens in selected villages.

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[4]A UC is a collection of several villages or urban wards with an average population around 25000. KP has 986 union councils.

We vary whether this information was provided in private and/or in a public setting. In the second experiment, the focus of this paper, we study the person-specific factors driving the decision to become a politician by subsiding services of a lawyer to help file papers, information on electability and information on benefits from office.

3.3.2 Sample Selection

We randomly select 240 VCs from the population of villages in the two districts and divide them into 196 treatment and 44 pure control villages. Table C1 in Appendix provides summary statistics of the villages and individuals in our sample. Most of the villages have good mobile phone connectivity and have literacy rates that are higher than the national average. The Village Councils have an average size of 9.8 members and an average of 6500 registered voters. The elections in 2015 were hotly contested, for an average of 6 general seats, there were 9 candidates. Voter turn out was also on the higher side, nearly 76% of voters turned out to vote in 2015 compared to the national average of 55% in 2013 parliamentary elections.

In each treatment village, we survey randomly selected fifty households. In the absence of housing lists, we use random walk sampling method to select respondents to administer a short survey. At the end of the survey, respondents are asked to identify up to three individuals from the village who should run for office but may not end up entering politics. There were two reasons to use this method. First, it will help us get at increasing participation from non-elites who have a shot at winning the elections since actual voters nominate them. Second, it enables us to work with individuals on the border of becoming a politician. These include people who have the time and inclination to join politics if there was no elite capture of institutions. After identifying the experimental sample, we sort them alphabetically according to their last names and transfer the sorted list to a pre-printed randomization sheet. The randomization sheet decides the treatment to be administered to each nominated person. This sheet is filled in the field due to time and logistical constraints however treatment assignments for each row were decided before the start of the fieldwork. In Table 3.2 we show this randomization was reasonably balanced on a number of individual characteristics. The sampled individuals have, on average, nine years of schooling. Very few of the sampled individuals have previous experiences of being on committees formed informally in the village. Once the assignment is complete, the survey team approaches the individuals and administers the treatments.

Table 3.1 reports the sample size in each treatment condition. Since all the treatments were cross randomized, there are eight treatment combinations. In the table, columns represent types of benefit messages delivered and rows represent a combination of whether electability information was provided, and within given electability information whether services of a lawyer were provided or not. During the analysis, we combine the social and personal benefits to create a pooled sample of benefits. Each cell in the table represents the number of citizens in a treatment condition. The very first cell is a combination of neutral message, no lawyer, and no electability information. It represents the control condition in this experiment.

Table 3.2 reports the balance across treatment groups. We report results from joint orthogonality test of the null hypothesis that treatments predict characteristics of the potential

candidates. Information on prior beliefs was collected before the treatment during the short survey we conduct as part of the sample selection. We ask the respondents to tell us, on a scale of one to five, how likely are they to win the election if they run for office. Similarly, we ask how affordable is running for office, one being least affordable and five being very affordable. After the administration of treatments, we return to administer a long survey with the respondents in which we collect information on family income, years of schooling and prior political experience. We use prior political experience and self-reported connections with politicians and bureaucrats to construct an index of political eliteness. We also construct a second measure of eliteness which we call the traditional elite. We use enumerator observation about whether a person can be regarded as elite in the village and if they own separate quarters to entertain guests. Even though all this information was collected post-treatment, the nature of these variables is such that it is unlikely our treatment could affect them in a short time.

3.3.3 Treatments

The study comprises three primary treatments: subsidy in the form of services of a lawyer, information on electability and information on benefits from office. After introducing themselves to the citizens the enumerators ask them a few simple questions as part of the survey about beliefs on affordability of election and their chances to get elected if they were to run for office. The survey is followed by a script read out by the enumerator informing subjects about upcoming elections. This script is read to everyone in the sample, including the control individuals.

You might be aware that for the first time the elections on May 30th will elect a council at the village level, comprising 10 to 15 members. People who are 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 with two references.

In the **Lawyer treatment** group citizens are provided services of a lawyer who helps them in filing candidacy papers. Filing for candidacy is a non trivial cost associated with becoming politician. The interested citizens are required to collect candidacy papers from the office Returning Officers usually situated in District and Tehsil Courts. These papers are filled and submitted within three days. The prospective candidates have to submit certain certificates along with the papers that certify their tax non-defaulter and non-criminal status. These papers cannot be prepared without the services of a lawyer. In this treatment the subjects are informed that a lawyer can help them prepare candidacy papers for free if they wish to run for office. Following is the precise script of the message.

If you would like to contest the village council elections, we can help you with the process of filing papers by providing a lawyer. This lawyer, available in the local courts, will help our listed candidates in filing their papers and provide advice on related legal matters. If you would like to utilize this facility, then you can contact the lawyer at this number: We will forward your name to him by tomorrow

The **benefits treatment** is further split in two types of messages. The personal benefit message focuses on personal benefits a politician can have. The script of this message focuses on non-monetary benefits that can accrue to a politician.

People who are elected to the village election will be provided with a golden opportunity to

move forward in politics, as well as improve their 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 progression in politics. Besides this, council members will be also improve their influence. They will be known as leaders in their neighborhoods, that will increase their recognition. Their children will also be able to build a network in the area, which will make their entry into politics easier.

The **social benefits** message focuses on social benefits like serving one's community and the poor.

People who are elected to the village election will be provided a golden 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 welfare and securing the rights of the poor. Working together with the district governments, they will improve the village school and health facilities. An elected councillor will have a unique opportunity to address the problems of his neighborhood, which will make him the standard bearer of social development for the village.

Since the sample for this experiment is identified by asking a random sample of citizens about who would they like to see run for office, this gives us some information about the preferences of some voters. In the **Electability** treatment group, subjects are inform that they have been nominated by fellow residents to run for office in a poll that has been conducted by the research team.

"We have talked to a lot people in the village, and in the survey, many people have nominated you for the village council elections. Based on the opinions of people in this survey you should really consider contesting the elections because there are good chances of your success."

3.3.4 Data

This experiment uses three sources of data, a short pre-treatment survey, administrative records from Election Commission of Pakistan and a long survey conducted post treatment. The short survey is administered to the sample of 2039 citizens before administration of treatment. This survey collects basic information and prior beliefs of the respondents about affordability of elections, chances to win office, and the benefits from public office. The second source of data is administrative records of Election Commission of Pakistan. Electoral data is collected for all candidates who run for office in sample villages. These administrative records are then matched with the sample of potential candidates. The third source of data is a long survey that is administered after declaration of candidacy. This survey collects detailed information on factors that went into the decision, the political experience of the subjects and their future political plans. The survey also collect information on psychometric and behavioral variables that are adopted from Callen et al. (2018) and Ashraf et al. (2014).

The main outcome of interest in this study is whether citizens declare candidacy and enter politics. This outcome is based on matched administrative records from Election Commission of Pakistan (ECP). The decision to enter politics is recorded as "filer". It takes the value 1 if the citizen officially files candidacy papers and zero otherwise. A secondary outcome is whether someone wins election or not. This information is also collected from the administrative data of ECP.

3.3.5 Comparison with Status-quo Politicians

The first challenge in relaxing barriers to political entry is to identify a sample of non-elite citizens who can be potential candidates in the election. This sample of prospective politicians should be on the margin, such that the barriers constitute binding constraints on their entry to politics. Only then relaxation of the barriers may affect their behavior. This is important for two reasons. One, not everyone has interest in politics. A person not interested in politics may decide to stay out of it even if subsidized heavily. Second, identifying persons that are too far out from the constraints may never have a realistic shot at the political office. In the absence of any credible information about the competence of candidates, a person too different from political class may result in non-serious candidacy. We overcome this challenge by relying on a survey of citizens to help us identify potential candidates [5]. This method rely on community's ability to provide information that is not readily accessible to the researcher, helping overcoming the challenge of identifying a pool of candidates that are viable alternative but also non-elite.

In order to confirm that the sample is indeed non-elite, this section undertakes a comparison of the potential candidates' pool with status-quo politicians. Ideally, the status-quo politicians should be from the same villages but after treatments were delivered the comparison pool may have been affected. This problem is solved by comparing the potential candidates' pool with status-quo politicians from the control villages. Table 3.3 reports the results of the comparison along six dimensions. These dimensions are selected such that they are informative and also not easily manipulatable. Ideally the comparison should be along the measures of competence, elite status and motivations. Our sample appears to be similar to status-quo politicians on the number of years of schooling (column 2), pro-social motivation index [6] (column 3), and almost similar on family income (column 1). However the nominated politicians have a higher personal motivation [7], and they score lower on measures of eliteness.

This comparison reveals a few points that need to be emphasized. First, if we take years of schooling as proxies for competence of individuals [8] the sampled prospective politicians are as competent as the status-quo politicians. This indicates that the sample is not very unique in terms of how voters may see their competence. Second, the prospective politicians are indeed less elite when compared on the measures of traditional and political eliteness.

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[5]The method has also been used by Cruz et al. (2017) in identifying potential candidates.

[6]To construct this index, we combine five psychometric indices based on Ashraf et al. (2014) (desire for pro-social impact, sees self as pro-social, affective commitment to beneficiaries, pleasure based pro-social motivation, and pressure based prosocial motivation), contribution to a local NGO in a hypothetical dictator game, as well as a binary measure of whether the candidate has ever volunteered in the community.

[7]We measure the degree to which candidates are motivated by personal benefits from political office. We have measures for two aspects of personal benefits. First, we examine the degree to which 'political ambition' drives subjects. For this, we combine three psychometric indices (again based on Ashraf et al. (2014)): career orientation, calling, and status-seeking. Second, we examine whether subjects are motivated by the ability to extract money from a position of power. For this, we use the number of statements respondents agree with in a list experiment where the treatment list includes the following statement "village council membership is a good way to make money".

[8]Ideally we would have liked direct measure of competence but in the absence of such a measure we use years of schooling as proxy. Years of schooling has been shown to correlate with later life success (Card, 2001).

This is reassuring as the whole premise of the paper rests on identifying less elite prospective politicians. Identifying a less elite sample means there are potential politicians who can join the elected offices but they may not be able to do so due to barriers to entry that we try to examine in this paper. Lastly, if citizens draw inference about the quality of the candidates from their current income and will be less likely to support low income candidates, this may in turn mean the non-elites with low incomes will not run for office. Analysis in table 3.3 reveal that the prospective candidates pool is almost similar to status-quo politicians on this measure too.

3.4   Analysis and Results

3.4.1   Estimation

Our main results use the estimation based on following linear probability equation:

$$y_i = \alpha_0 + \alpha_1 * t_{li} + \alpha_2 * t_{ei} + \alpha_3 * t_{bi} + \psi_v + \epsilon \tag{3.1}$$

We administer treatments at the individual level and study the effects on an outcome that is represented by y_i in equation 3.1 which takes a value of one if citizen files papers and zero otherwise. We compare the coefficients of three treatment groups with control condition where individuals receive no treatment information. t_{li} in 3.1 stands for the lawyer treatment, it takes value of 1 if a lawyer was provided and zero otherwise. t_{ei} represents the electability treatment information to individual i, similarly t_{bi} represents the benefits treatment group. Here we have pooled the two types of benefits. We undertake the same estimation for the case when benefits are disaggregated as represented by equation 3.2.

$$y_i = \alpha_0 + \alpha_1 * t_{li} + \alpha_2 * t_{ei} + \alpha_3 * t_{pi} + \alpha_4 * t_{si} + \psi + \epsilon \tag{3.2}$$

In equation 3.2 t_{pi} stands for the personal benefits treatment and t_{si} stands for social benefits treatment provided to individual i.

In the above estimations we include village fixed effects by including dummies psi_v for each village v. It is important to include the village fixed effects in the estimation as the randomization and treatment is at the individual level within a village but there may be certain unobservable characteristics of the village that can drive results in one direction or the other. Further since this experiment shares the village sample with another village level experiment, inclusion of the village fixed effects will control for any spillovers that may affect the outcome. Such spillovers will be common for both treatment and control individuals within a village, so including village dummies should effectively remove the effects of other experiment from our estimation.

Lastly, the provision of lawyer in equations 3.1 and 3.2 estimate an intention to treat effect which assumes all those offered lawyers end up using them. However that is not the case since the usage of a lawyer may be driven by some unobservable characteristic that may also be driving the decision to enter politics. In order to assess the effect of lawyer used on the decision to file candidacy we use the treatment assignment as instrumental variable for lawyer use. Since treatment assignment was done randomly it fulfills the exclusion restriction

that the only effect it has on filing of papers is through the use of lawyer's services. We use a Two stage least square estimation reported in equations 3.3 and 3.4:

$$t_{ui} = \beta_0 + \beta_1 * t_{li} \tag{3.3}$$

$$y_i = \alpha_0 + \alpha_1 * \hat{t_{ui}} + \alpha_2 * t_{ei} + \alpha_3 * t_{bi} + \psi_v + \epsilon \tag{3.4}$$

Where t_{ui} represents if person i used the services of a lawyer offered to him, this is instrumented with t_{li}. And $\hat{t_{ui}}$ is the predicted value of lawyer use based on the instrumental variable.

3.4.2   Effects on Candidacy

In this section we examine the effects of experimental treatments on the decision of a citizen to enter politics. Information on the candidacy is collected from the administrative data of Election Commission of Pakistan. It is recorded as one if the citizen has declared candidacy and zero other wise. We call this variable "Filer".

   Table 3.4 reports the results of the main analysis. First thing to note is that the mean proportion of citizens running for office in the control group is fairly high. This high rate of candidacy is unsurprising since the sample is unique and not representative of the citizens. While identifying the sample, we had asked citizens to nominate persons who they think should run for office but the nominees *may* not run. The intention was to get at people who really are at the verge of becoming politicians but may not do so due to the cost or information frictions. Additionally, the control group also receives information about the election to ensure that all experimental conditions have common information level about the election. This in itself may have caused the prospective candidates to file papers [9].

   Each column reports the result of a regression that estimates the effect of a treatment on the probability of filing candidacy officially. In the regressions, each individual is treated as single data point and their decision to run for office or not is coded as one or zero, respectively, as the dependent variable. Column 1 estimates equation 3.1, column 2 estimates equation 3.2, and columns 3 and 4 report the results of estimating equation 3.3 but with appropriate treatments. Since treatment assignment was done within the village, it is possible that some villages are more politically active than others or some may be poorer than others. Such differences, particularly those that make some barriers more or less salient, may affect the results. In order to control for variation that is explained by the village characteristics, each column includes village fixed effects in the regressions.

   Column 1 in Table 3.4 presents the main findings. Here we have pooled the benefits treatments into one group and focus on the three main treatments. "Filer" is the dependent variable in each regression. The results suggest that the cost of running for office is the main barrier that matters for the non-elites' decision to enter politics. The point estimate on lawyer treatment is 0.048, which means that subsidizing the cost through a lawyer leads to a 4.8 percentage point increase in the probability of running for office. In control group the probability of running for office is 30.7% so subsidizing the cost results in an increase of 15% in the probability of running for office.

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[9]In the companion paper we find the effect of just talking to citizens is huge: it leads to a 2.6 percentage points increase in the probably of entering politics over a baseline of 0.5 %. However that alone cannot explain the mean candidacy in control group in this experiment which is 30%.

This result makes sense in the light of information we collected from the individuals before the treatment was administered.Only 24 percent of respondents considered the cost of running for election as affordable for them, indicating that many non-elites may not run for office due to the cost barriers. Thus, subsidizing the cost through provision of lawyer

Electability information and highlighting benefits from office does not have a statistically significant effect. However, the coefficients are positive and not small, which indicates they may also be increasing candidacy but we can not make that claim statistically due to relatively large standard errors.

It is possible that the benefits treatment may have opposing effects for personal and social benefits. Since the cost to run for office has to be borne by person it is likely that the citizens may only be interested if the benefits from office are also accrued to the person only, whereas the social benefits would make the public office less appealing for those interested in cost benefit analysis that involve personal benefits only. If that is the case then pooling the benefits treatment together may cancel the opposing effects. To investigate if that is indeed the case, column (2) reports the result when we disaggregate the benefits into social and personal groups. First thing to note is that both coefficients are in the positive direction, suggesting that highlighting personal and social benefits may not have opposing reaction. Second, highlighting personal benefits has a large co-efficient, almost as large as the lawyer treatment. But it stays insignificant due to large standard errors. Though the effects are not significant, the results are suggestive that information on benefits may get more non-elites to run for office.

Columns (1) and (2) attest to the importance of cost in the decision to enter politics. This suggests that non-elite candidates, who may be as competent as elite candidates, are not entering politics due to the cost associated with running for office. However, the analysis in these columns use an intention to treat design for studying the effect of lawyer. Here, we are assuming that everyone assigned to the lawyer treatment actually used their services and filed the papers. However, in our analysis we find that it is not the case. We provide services of lawyers to 1025 citizens from the sample. Out of these, 764 individuals end up using the services to prepare candidacy papers. However not all 764 officially file the papers to run for office, since they have a choice to withdraw at any stage of the process or get the papers prepared but not submit at all. Many do exercise this choice and only 278 formally appear on the ballot paper by officially declaring candidacy.

The decision to use the lawyer may not be random and correlated with some unobservables that may also be driving the decision to file candidacy. Therefore to get a cleaner effect of the lawyer treatment on those who actually use the services, we use the treatment assignment as an instrumental variable to get an unbiased estimate of the lawyer use on filing papers. The effect of lawyer use on filing papers is reported in column (3) and (4) Table 3.4. In this analysis we first use treatment assignment to the "lawyer" group as instrumental variable for whether the citizen used the lawyer or not. Column (3) reports the results from a specification that pools the benefits and column (4) uses disaggregated benefits treatments. The first stage of the analysis is very strong with an F-statistic of 4979. The instrumented lawyer treatment comes out to be significant too, strengthening our earlier finding that cost is the main barrier to political entry. The point estimate is 0.065 which is larger than the point estimate in columns (1) and (2) as expected. If citizens use services of the lawyer they are more likely to enter politics.

Next we turn our attention to study if combination of different factors could be influencing the decision to run for office. Our design allows us to study different combination of treatment groups. We use our design to study the pure effect of each treatment group and report the results in appendix Table C4. However we do not find any evidence on any of the treatment combinations. This is because we lose statistical power by cutting the sample in 12 different groups which leads to the standard errors becoming very large.

Finally, one may be concerned that the standard errors in the village maybe correlated. Though the inclusion of village fixed effects should help control for any village specific relationship that we do not observe, we do undertake a robustness check by explicitly clustering standard errors at the village level. The findings from this analysis are reported in appendix Table C3. The first two columns report the intention to treat effect of the provision of a lawyer whereas the last two columns use treatment assignment as instrumental variable for the use of lawyer. The results in this table prove that the analysis in this section is robust to the type of standard errors used.

The above discussion establishes the importance of cost associated with running for office as the main barrier to political entry. We do not find statistically significant effect of information about electability or benefits on the decision to enter politics. But the effects are positive and relatively large. In the electoral process at the village level one of the main costs associated with the decision to enter politics is finding a suitable lawyer and paying them a fee to help submit the candidacy papers. However this can be generalized to other settings where cost of entering politics includes making an effort to get on a party ticket or incurring monetary costs associated with campaigning. In this sense the results are generalizable to other settings. At the very least, the results speak to the importance of making political entry less costly for citizens in poor and developing countries. Many new democracies adopt devolution of governance to give power to the people in an effort to improve public service delivery. However, such efforts may bore little fruit if the elected officials are representative of the elite only. The main finding in this section suggests that such initiatives must take into account the cost of officially declaring candidacy.

3.4.3   Who becomes a candidate?

This section explores the relationship between characteristics of potential candidates and their responses to the treatments. As discussed earlier, the sampled individuals responded to a short survey before the treatment conditions were administered. This survey collected information on prior beliefs of the candidates. The survey included beliefs of the respondents about the affordability of the elections [10], their self-perceived chances to win the election if they were to run for office [11], and the social and personal benefits from public office [12].

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[10]Subjects were asked to rank on a scale of 1 to 5 if they believed the cost associated with running for office to be affordable for them or not, 1 meant least affordable and 5 meant very affordable

[11]Subjects were asked to rank their chances of winning the election if they were to run. The ranking were on a scale of one to five, with one being least likely and five stood for most likely.

[12]Subjects were presented with four statements that identified social benefits and four statements that identified personal benefits of office. They were asked whether they agreed with statements on a scale of one to five, one mean "absolutely do not agree" and five meant "absolutely agree"/ them according to importance.

In the subsequent analysis, we cut the data for each prior on the median and generate a dummy that takes value of one if the prior value is the same or higher than the median, and zero otherwise. For analysis, we include an interaction term between the treatment and the dummy for high prior belief as per equation 3.5.

$$y_i = \alpha_0 + \alpha_1 * t_{li} + \alpha_2 * t_{ei} + \alpha_3 * t_{pi} + \alpha_4 * t_{si} + \alpha_5 * prior_i * treatment_i + \alpha_6 * prior_i + \psi + \epsilon \quad (3.5)$$

Table 3.5 reports the results of estimating equation 3.5 for prior beliefs regarding electability of the respondents and affordability of the costs associated with running for office. The interaction terms between the priors and the treatments are relevant for understanding if people with a high prior belief respond differently to the treatments. Panel A reports the responses of individuals to treatments based on their perceived electability. The results suggest that those with high prior belief about their ability to win the elections are more likely to run for office when their priors were confirmed by the treatment. The interaction with lawyer and benefits treatments are insignificant. Panel B reports the results of analyzing effects on the candidacy decisions based on the belief of individuals regarding the cost of elections. Higher sense of affordability did not result in a differentially higher or lower response to the treatments. The response to provision of lawyer and information on benefits is positive but that to the electability treatment is negative. Though these interactions are statistically insignificant.

The response to electability treatment by individuals with high prior is consistent with at least two explanations. First, it is possible that the treatment served as an affirmation of the information that the individual already believed. We did not collect information on how confident were the individuals about their beliefs to study whether that is the case. The second possible explanation is that the citizens, in addition to getting affirmed, also thought that the their beliefs have been supported because the research team or the enumerators believe them to be different from other potential candidates. This second explanation is less likely because the control group is also *selected* for receiving information about the election which means the effect of being selected should be the same across both groups.

The lack of heterogeneous effect of lawyer on the perception of affordability is unsurprising. If individuals perceives the election to be very affordable, they will run for office without any subsidy, however if the election is very unaffordable for some citizens the provision of a small subsidy in the form of a lawyer may not be enough to change the cost side of their decision equation.

Table 3.6 reports the result of estimating equation 3.5 with prior beliefs about social benefits and personal benefits from office. As already mentioned, these beliefs have been cut at the median creating a dummy variable of "high prior" that takes value of 1 if the value is same as or above the median, and zero if it is below. Panel A of the table reports heterogeneous response of individuals who have a high prior on social benefits from office. Column 1 of the table reports interaction of high prior, and the lawyer treatment leads to differentially large effect on the probability of filing papers. Providing services of a lawyer to a person with higher prior on social benefits from office leads to a 7.8 percentage points increase in the likelihood of filing candidacy and entering politics formally. Column 2 reports interaction with electability information which is positive but insignificant, and column 3 reports the response to social benefits treatment which is negative and insignificant. Interestingly,

the response of individuals with high prior on social benefits from office to the information treatment on personal benefits from office is significant and large, adding 9 percentage points to the probability of running for office.

Panel B of table 3.6 reports interaction of the treatments with high prior belief above personal benefits from public office. In this panel, only the interaction of high prior with lawyer treatment is positive and significant as reported in column 5. The individuals with high prior belief about personal benefits from office are 7 percentage more likely to run for office as a result of receiving a lawyer treatment. The remaining interactions are all positive but insignificant indicating, but not conclusively, that those who have a high prior are more like to respond to the treatments as we expected.

The analysis presented thus far indicates the importance of highly motivated individuals for improving non-elite candidacy. Individuals who have high expectations about their chances to win are more likely to respond to the treatment, similarly individuals who have a high prior on benefits from office also respond more to the treatments. For all such individuals, it is the cost subsidy in the form of services of a lawyer that gets them to run for the office. This indicates the importance of designing policies that lower the cost of running for office, otherwise highly motivated but non-elite candidates may sit out the elections.

The above analysis may be masking considerable heterogeneity in responses. Specifically, the expectations and beliefs were solicited through categorical responses. Thus there may be opposing response of prospective politicians which will be masked by focusing on a dummy variable. Observing some relations graphically will help in providing a more holistic picture of how individuals at certain levels of expectations respond to the treatments compared to the control. Since all the prior beliefs were collected at a five point scale now turns to exploration of responses to treatments at each value of the prior beliefs. The analysis is exploratory and comes with a caveat that not all levels of prior have enough sampling units.

We run regressions at each level of reported priors for each treatment separately, by restricting the sample to only treatment and control politicians who have reported that level of belief, and control for village fixed effects. The coefficients of each regression is plotted along with the confidence intervals. Appendix C.1 reports the graphs plotted for each prior belief and each of the treatment groups.

There appears to be a positive relationship between the beliefs and response of the prospective politician in all treatments. In figure C1, the individuals who report that there is absolutely no chance of them winning the elections, respond negatively to all treatments whereas individuals who are absolutely sure of their chances respond positively to all treatments. These responses are statistically different from zero for all treatments except the personal benefits information. Similarly, figure C2 shows that those in the least affordable category, respond positively to almost all treatments. But more importantly, those who consider the cost of running election to be just affordable (by selecting the middle category of 3) are most likely to respond to the treatments, indicating that treatments help change their cost-benefit consideration of running for office. Lastly, the response to treatments at different levels of expectation about benefits from office are reported in figures C3 and C4. In line with the analysis so far, individuals with high value of belief about the expected benefits, both social and personal, have positive response to all treatments.

The analysis highlights that individuals who have truly strong priors are more likely to respond to the treatments. Individuals who responded to have high prior-beliefs about

winning and benefits from office are the ones who respond positively to the treatments. These are probably the persons who are on the margin of entering politics but just need a nudge either in the form of cost subsidy or information on the process and prospects of their candidacy.

3.4.4 Effects on Elections

The main focus of this paper is candidacy decision and the factors that constitute barriers to political entry. However, once citizens decide to enter politics, by running for public office, it is natural to ask how do they fare in elections. In this section we explore the electoral performance of the candidates who appear on the ballot paper. Table 3.7 reports the unconditional analysis on results of election. In this table the dependent variable is called *elected*. It takes the value one if the person wins the election and zero otherwise. First thing to note is that subsidizing the cost to let more people run for office does not translate into higher election rate. This is not unexpected since running for office is necessary but not sufficient condition to win an election. Electoral success requires running an effective campaign that revolves around forming a winning coalition or appealing to largest group of voters. Since the experiment induced first time and non-elite politicians to run for office, they may still lack the knowledge to run a campaign that wins elections.

Second notable result from the table is that the probability to win an election is 16% in the control group which is fairly high. Thus it is not that our experimental sample cannot win elections but rather the extra candidates induced due to treatments could not win more seats. Finally, we find that the group receiving information about benefits from office do get elected more often. There is a possibility that the information motivates them to exert extra effort in running a campaign that can win an office. It is not possible to test if that is indeed the case in the absence of information on the campaigns themselves. But breaking down the sample on the kind of benefits that were mentioned during the treatment reveals the personal benefits treatment driving this result, as reported in column 2 and 4 of Table 3.7. This suggests that information on personal benefits may drive up the motivation of candidates.

3.5 Conclusion

Equal right to political participation is one of the bedrock principles of democratic societies. It is then important to understand what prevents citizens from participating in politics by running for public office. This paper sheds light on the decision of citizens to enter politics by relaxing three types of barriers: Cost of running for office, Information about electability and information about benefits that holding a public office provides.

Cost of running for office is the main barrier preventing citizens from entering politics. Subsiding the cost through provision of services of a lawyer who helps file papers significantly increases the probability that a citizen will enter politics. In the absence of any support, the probability of a citizen running for office in our sample is 30%. The provision of lawyer increases this probability by nearly four percentage points which is an improvement of 15% over the baseline.

The lack of information about electability and benefits from office are not driving the citizens' decisions. Providing the citizens this information does not effect their probability to run for office. However this result is for the average effect. Breaking down the anaylsis by prior beliefs of population result in a more nuanced analysis. Probability of running for office as a result of treatments increase for citizens who have a high expectation of benefits from office, both personal and social.

This study speaks to the possibility of establishing inclusive elected institutions at the local level that are not captured by elites. Every year a significant amount of funds are spent by multilateral and bilateral donor agencies and non profit organizations to promote democracy in fragile countries. This study provides evidence that such programs can be effective if we understand what prevents citizens from participating in democratic institutions.

Further, the paper extends the debate about the idea of human resource allocation to the realm of politics and contributes to the literature of who becomes a candidate? (Dal Bó et al., 2017), and how to encourage under represented sections of society such women (Butler and Preece, 2016) and non-elites (Pande, 2003).

3.6   Tables

Table 3.1: Treatment Allocation

		Neutral Message	Personal Message	Social Message
No Electability	No Lawyer	259	136	118
	Lawyer	262	126	122
Electability	No Lawyer	256	125	121
	Lawyer	253	121	131

Note: Each cell represents the number of citizens in a treatment.

Table 3.2: Balance Table

	Income	Schooling	Committee Exp	Cost Prior	Electability Prior	Personal Benefits Prior	Social Benefits Prior
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lawyer	416.356	0.376	-0.003	-0.079	-0.028	0.046	0.039
	(682.491)	(0.167)	(0.008)	(0.045)	(0.043)	(0.043)	(0.043)
Electability	164.652	0.093	0.002	0.016	0.034	-0.014	-0.062
	(671.143)	(0.164)	(0.008)	(0.045)	(0.044)	(0.042)	(0.042)
Social Benefits	-1050.849	-0.198	0.014	-0.053	-0.031	0.101	-0.025
	(862.351)	(0.221)	(0.009)	(0.061)	(0.061)	(0.057)	(0.059)
Personal Benefits	-616.069	-0.032	-0.000	-0.093	0.048	0.056	0.034
	(988.046)	(0.224)	(0.010)	(0.058)	(0.057)	(0.059)	(0.059)
Control	-170.983	0.191	0.015	-0.102	-0.079	0.058	-0.066
	(1186.171)	(0.296)	(0.014)	(0.081)	(0.079)	(0.075)	(0.080)
Mean of Variable	23760.30	9.48	0.02	-0.00	-0.00	-0.00	0.00
# Observations	1985	2021	2039	2039	2039	2039	2039
Joint orthogonality p-value	0.61	0.20	0.40	0.33	0.37	0.45	0.29

Note: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Each column is a regression with the independent variable mentioned in column name, which is regressed on treatment dummies. The regressions include village fixed effects. The joint orthogonality tests the null hypothesis that treatments predict characteristics of the sampled individual.

Table 3.3: Comparison with Pure Control Politicians

	Income (in Rs.) (1)	Years of Schooling (2)	Pro-Social Index (3)	Personal Index (4)	Political Elite Index (5)	Trad. Elite Elite (6)
A: Nominee Politicians (N=2045)	23765.54	9.48	3.60	3.27	0.09	0.14
	(358.256)	(0.077)	(0.006)	(0.009)	(0.004)	(0.008)
B: Status-quo Politicians (N=1031)	24830.82	9.45	3.62	3.23	0.14	0.25
	(552.543)	(0.119)	(0.009)	(0.013)	(0.006)	(0.013)
A-B: Difference	-1065.28	0.03	-0.01	0.04	-0.05	-0.11
p-Value of Difference	0.106	0.809	0.169	0.013	0.000	0.000
# Observations	2828	2861	2854	2866	2866	2867

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This table reports the means of politician characteristics in pure control groups and the sampled potential candidates. The characteristic of interest is mentioned in the column. Difference between the means of the two groups is reported as "A-B:Difference" and p-values tests if the two groups are similar.

Table 3.4: Effects on Candidacy

	Dependent Variable: Filer			
	(1)	(2)	(3) IV	(4) IV
Lawyer	0.048**	0.048**	0.065***	0.065***
	(0.019)	(0.019)	(0.024)	(0.024)
Electability	0.011	0.011	0.011	0.011
	(0.019)	(0.019)	(0.018)	(0.018)
Benefits	0.029		0.029	
	(0.019)		(0.018)	
Social Benefits		0.019		0.019
		(0.027)		(0.026)
Personal Benefits		0.038		0.039
		(0.028)		(0.026)
Constant	0.307***	0.307***	0.367**	0.362**
	(0.019)	(0.019)	(0.143)	(0.143)
F-Stat			4979	4974
N	2039	2039	2039	2039
Standard Errors	Robust	Robust	Robust	Robust

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are reported in parenthesis. All regressions include village fixed effects. column (1) and (2) use an intention to treat design with respect to the provision of lawyer, whereas column (3) and (4) use the provision of lawyer as an instrumental variable for the use of lawyer, which is then used to estimate the local average treatment effect of lawyer on filing papers. The dependent variable is "filer" in all columns.

Table 3.5: Prior on Electability and Affordability

	Dependent Variable: Filer					
	Panel A: Electability Prior			Panel B: Affordability Prior		
	(1)	(2)	(3)	(4)	(5)	(6)
Lawyer	0.059	0.049**	0.048**	0.015	0.048**	0.048**
	(0.041)	(0.019)	(0.019)	(0.032)	(0.019)	(0.019)
Electability	0.010	-0.058	0.010	0.012	0.029	0.012
	(0.019)	(0.041)	(0.019)	(0.019)	(0.032)	(0.019)
Benefits	0.030	0.030	0.017	0.030	0.030	0.018
	(0.019)	(0.019)	(0.042)	(0.019)	(0.019)	(0.032)
Electability Prior	0.092***	0.042	0.076**			
	(0.034)	(0.036)	(0.036)			
Affordability Prior				0.013	0.051*	0.029
				(0.030)	(0.030)	(0.030)
High Prior x Lawyer	-0.013			0.051		
	(0.047)			(0.041)		
High Prior x Electability		0.086*			-0.026	
		(0.047)			(0.041)	
High Prior x Benefits			0.017			0.019
			(0.048)			(0.041)
Constant	0.235***	0.273***	0.247***	0.298***	0.273***	0.288***
	(0.033)	(0.034)	(0.034)	(0.028)	(0.028)	(0.028)
N	2039	2039	2039	2039	2039	2039
Standard Errors	Robust	Robust	Robust	Robust	Robust	Robust

Note: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Robust standard Errors are reported in parenthesis. All regressions include village fixed effects. The priors have been cut at the median of prior beliefs. "High Prior" is a dummy that takes value of 1 if the prior is equal to or higher than the median value. Each column reports the result of a regression as per specification 3.5. Panel A, reports interactions of the high prior on electability with the treatments and panel B reports the interaction of high prior on affordability of elections with the treatments.

Table 3.6: Prior on Social and Personal Benefits from Office

Dependent Variable: Filer	Panel A: Social Benefits Prior				Panel B: Personal Benefits Prior			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lawyer	0.007	0.046**	0.046**	0.047**	0.010	0.048**	0.049**	0.049***
	(0.026)	(0.019)	(0.019)	(0.019)	(0.030)	(0.019)	(0.019)	(0.019)
Electability	0.013	-0.018	0.013	0.014	0.010	-0.019	0.011	0.011
	(0.019)	(0.026)	(0.019)	(0.019)	(0.019)	(0.030)	(0.019)	(0.019)
Social Benefits	0.019	0.017	0.037	0.018	0.019	0.019	0.010	0.021
	(0.027)	(0.027)	(0.036)	(0.027)	(0.027)	(0.027)	(0.039)	(0.027)
Personal Benefits	0.039	0.040	0.038	-0.007	0.039	0.038	0.038	0.014
	(0.028)	(0.028)	(0.028)	(0.035)	(0.028)	(0.028)	(0.028)	(0.038)
Social Benefits Prior	0.015	0.024	0.063**	0.031				
	(0.030)	(0.029)	(0.026)	(0.026)				
Personal Benefits Prior					-0.056*	-0.047	-0.025	-0.032
					(0.029)	(0.029)	(0.025)	(0.025)
High Prior x Lawyer	0.078**				0.070*			
	(0.039)				(0.040)			
High Prior x Electability		0.062				0.053		
		(0.039)				(0.040)		
High Prior x Social Benefits			-0.036				0.017	
			(0.048)				(0.047)	
High Prior x Personal Benefits				0.090*				0.042
				(0.049)				(0.048)
Constant	0.298***	0.295***	0.276***	0.290***	0.338***	0.334***	0.321***	0.325***
	(0.023)	(0.024)	(0.023)	(0.022)	(0.025)	(0.025)	(0.024)	(0.024)
N	2039	2039	2039	2039	2039	2039	2039	2039
Standard Errors	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust

Note: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Robust standard Errors are reported in parenthesis. All regressions include village fixed effects. The priors have been cut at the median–"High Prior" is a dummy that takes value of 1 if the prior is equal to or higher than the median value. Each column reports the result of a regression as per specification 3.5 interacting treatments with the prior mentioned in the panel heading.

Table 3.7: Results on Election

	Dependent Variable: Elected			
	(1)	(2)	(3) IV	(4) IV
Lawyer	0.007	0.007	0.009	0.009
	(0.016)	(0.016)	(0.020)	(0.020)
Electability	0.001	0.001	0.001	0.001
	(0.016)	(0.016)	(0.015)	(0.015)
Benefits	0.035**		0.035**	
	(0.016)		(0.016)	
Social Benefits		0.026		0.026
		(0.023)		(0.022)
Personal Benefits		0.043*		0.043**
		(0.023)		(0.022)
Constant	0.163***	0.163***	0.147	0.143
	(0.016)	(0.016)	(0.108)	(0.108)
F-Stat			4979	4974
N	2039	2039	2039	2039
Standard Errors	Robust	Robust	Robust	Robust

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors are reported in parenthesis. All regressions include village fixed effects. column (1) and (2) use an intention to treat design with respect to the provision of lawyer, whereas column (3) and (4) use the provision of lawyer as an instrumental variable for the use of lawyer, which is then used to estimate the local average treatment effect of lawyer on filing papers. The dependent variable is "elected".

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Appendices

Appendix A

Mission Motivation and Public Sector Performance: Experimental Evidence from Pakistan

A.1 Alternative Explanations for the Effect of Mission Treatment

I have established that introducing a pro-social mission to workers of a public sector organization motivates them to improve their performance. However, it is possible that the reason for improved performance is not the mission itself but something else that also changed for the treatment workers.

The main alternative explanation for why mission may work, may relate to the way the main treatment was delivered. The treatment brings workers together in groups, and the workers also interact consistently over three months with a facilitator. The group setting may result in more social interaction between workers (Feigenberg et al., 2013) and interaction with a facilitator may create goodwill towards the organization. Thus workers can become more inclined towards their duties by virtue of having more goodwill towards the organization and its people.

I test for this alternative explanations by including in the design of the experiment a placebo treatment. As discussed in section 1.3.1, the study includes a treatment group that receives the refresher training just like the public mission treatment but does not discuss the mission or shows the video of DHO. This treatment group is similar to the public mission group in terms of receiving refresher training and socializing with other workers. If these alternate reasons were behind the change in performance of workers I should see no difference between the placebo treatment and the mission treatment.

The second row in Table A1 reports coefficients of placebo treatments in the regressions. It is clear that the observed effects of the mission are not driven by these alternate explanations, otherwise I would have seen similar magnitudes between the mission and the placebo treatments. I formally test for the difference between the mission and placebo treatments in second part of the table. I can comfortably reject the null hypothesis that the effect of the mission is driven entirely but these alternative explanations.

A.2   Decay of the Mission Motivation

In this section, I show that the effect of the mission treatment does not disappear immediately after the experiment has ended – it decays at a much slower rate compared to the effect of the financial incentives. To study the decay, the health department announces to workers at the end of the three months that the project is ending; the workers will not be meeting again to discuss the organizational mission, and that they will also not receive any financial incentive based on their performance. However, I still tracked their performance through a survey of households to see if they continued their improved effort. This can be considered a strong test of sustainability because the treatment was not stopped quietly, instead it was done with clear announcements.

Table A2 shows the effect of the treatments on the probability of a household visit after the experiment ended. Workers who received the mission continued to serve their communities with a higher effort post experiment. Column 1 reports the probability of a household visit in the mission treatment group was 3.7 percentage points higher than the pure control group. The effect of financial incentive, not surprisingly, went down significantly from 9.8 percentage points (as reported in Table 1.5) to 1.2 percentage points, which can not be statistically distinguished from zero. The effect of combined treatment is also not statistically distinguishable from control but the point estimate is bigger than the financial incentives.

In order to better understand the difference in persistence of the treatment effects, I report difference-in-difference estimates in Column 2, using the experimental period as the baseline. This helps in directly comparing the rates of decay across treatments. The effect of mission treatment decays at a slow rate of 1.3 percentage points but the effect of financial incentives decays at a rate of 9.2 percentage points. When combined the rate is 3.9, which is smaller than the financial incentives rate but larger than the effect of just the mission treatment. The results confirm that the mission treatment led to a change in the worker performance that is more lasting than the effect of just the financial incentives.

A.3   Time Spent by Workers

One aspect of multitasking that I see in the data is that the improvement in performance on the household visits does not come at the expense of other tasks that are measured. It is possible that there is a cost in terms of some other tasks that I are not measuring. To investigate this I collect data on the time spent in each visit from the households, and ask the workers in the endline survey what time they start and end their day in the community to measure the total effort proxied by the time spent on the job. Appendix Table A3 shows the results from analysis of time spent on the job.

In column 1, I find there is no negative effect on the time spent in each household, which is reassuring that the improvement in performance as measured by more visits does not come at the expense of quality of the visit proxied by time spent in each visit.

A.4 Who Responds to the Treatments?

In this section, I explore heterogeneous responses to treatments based on time-invariant characteristics of workers recorded before the launch of this experiment. The main question explored in this section is how high ability workers and those with higher public service motivation respond differently to the mission and financial treatments. I use baseline performance and Raven's IQ to proxy for ability of workers, and use Perry (1996) to measure their motivation for public service. The discussion in this section is suggestive due limited statistical power.

Ability: Workers can have different levels of abilities related to their jobs. I do not have a single good measure to help discern high ability workers from the low ability ones. But I do know their baseline performance in terms of household visits that can help us identify which workers perform better than others in a status-quo environment. Further, I have a proxy of their inherent "intelligence" measured through Raven's IQ test.[1] Using these two measures, I study how workers respond differentially to the mission and financial treatments given their baseline abilities.

I create a dummy variable that takes value of one if the baseline performance of a worker is higher than the median value of performance in the control group before the experiment. Column 1 in table A9 reports the heterogeneous response of such high performing workers to the three main treatments. High performers do not appear to be differentially responding to the mission treatments, whether delivered alone or in combination with the financial incentive. It is important to note that, the coefficients on these two treatments are almost zero. However, they do respond to standalone financial incentive very strongly. If the baseline performance measures ability, then higher ability workers appear to care more for financial incentives compared to low ability ones. Column 2 uses score on Raven's IQ test to explore the response of high IQ workers to the treatments. Again, I decide on high IQ individuals as those with score higher than the median worker in the control group. The effects are statistically insignificant but the signs on coefficients support the result in column 1. The coefficient on financial incentive is positive and the magnitude is different from zero, through insignificant due to lower power.

On the selection margin several studies have explicitly identified that high ability workers are more likely to be attracted to public sector jobs if there are stronger financial incentives directly (or indirectly through career progression) linked to the job (Dal Bó et al., 2013, Ashraf et al., 2018). The result on ability in this section show that it may also be true for the performance of workers who have already selected to work for the government.

Public Service Motivation: I measure public service motivation of workers using an abridged version of Perry (1996) based on Callen et al. (2018) in the baseline survey. Using median of the control group, I construct a categorical variable taking value 1 if the PSM score of a worker is above the median. Column 3 in table A9 reports the heterogeneous effects of treatments based on high PSM on the probability of a household visit. I cannot definitely discern if there are differential effects of treatments based on high PSM because

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[1] I acknowledge the limitations of this test given that it was not developed for a Pakistani context.

none of the effects are statistically significant. The coefficients are not small in magnitude and with a bigger sample, I would be powered to statistically distinguish them from zero. But to the extent the signs on the coefficients are indicative of the behavior, it appears that workers with high public service motivation react negatively to the introduction of financial incentive. This directional effect appears to be driven purely by the signaling value of financial incentives as it does not appear for the workers receiving both mission and financial incentive treatment. Again the suggestive direction of result here is in line with the effects observed in selection studies that show that financial incentive may serve as a negative signal for motivated workers to join a public service organization (Deserranno, 2019).

A.5 Appendix Figures and Tables

Table A1: Alternative Explanations for Mission

	Dep Var: Household Visit = 1	
	(1)	(2)
Mission	0.051***	0.057***
	(0.012)	(0.011)
Placebo	0.013	0.013
	(0.012)	(0.012)
Control Mean	0.353	0.353
# of Observations	21279	21279
# of Workers	710	710
Block & Wave Fixed Effects	✓	✓
Baseline Controls	-	✓
Linear Combinations of Coefficients		
Mission − Placebo	0.038	0.044
	[0.000]	[0.000]

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

Notes: This figure reports the effects of treatments on Relative Risk Ratio of diarrhea and compares them with the effects of public health interventions using a meta-analysis of literature by Fewtrell et al. (2005).

Table A2: Persistence and Decay in the Effects

Dep. Variable	Post-Experiment: Household Visit =1	
	Persistence of Effects (1)	Rate of Decay (2)
Mission	0.034* (0.020)	-0.013 (0.022)
Financial Incentive	0.011 (0.026)	-0.092*** (0.028)
Mission and Financial Incentive	0.027 (0.025)	-0.039 (0.028)
Control Mean	0.299	0.299
# of Observations	710	1420
# of Workers	710	710
Baseline Controls	✓	-
Model	AR(1)	DiD w. Exp Period
Linear Combinations of Coefficients [p-Value]		
Mission − Financial Incentive	0.024 [0.258]	0.079*** [0.001]
Financial Incentive − Mission and Financial	-0.016 [0.545]	-0.053* [0.062]

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This table uses household data to study the persistence of effects and the rate of decay. Column 1 reports the effects of treatments on the probability of household visits post-experiment. Column 2 reports the rate of decay in the effect of household visits post-experiment using experimental period as the baseline in a difference-in-difference estimation. Standard errors clustered at the worker level are reported in parentheses. Second part of the table reports linear combinations of coefficients and test them against a null of zero difference. $p-values$ of the tests are reported in square brackets.

Table A3: Effects on Effort Proxied by Time

| | Minutes Spent: | |
| | Total Work-Day | On a Visit |
	(1)	(2)
Mission	16.857***	0.185
	(5.870)	(0.549)
Financial Incentive	15.241*	0.661
	(8.000)	(0.656)
Mission and Financial Incentive	15.097**	0.296
	(7.526)	(0.693)
Placebo	4.145	0.423
	(6.276)	(0.573)
Control Mean	318.409	18.398
# of Observations	705	5612
# of Workers	705	703
Data Source	Worker Survey	HH Survey
	Linear Combinations of Coefficients	
Mission − Financial Incentive	1.616	-0.476
	[0.824]	[0.364]
Mission − Mission and Financial	1.760	-0.111
	[0.793]	[0.847]
Financial Incentive − Mission and Financial	0.144	0.366
	[0.987]	[0.588]

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This table reports the effects of treatments on time spent in a household during a visit (column 1) and overall time spent on the job during a given day (column 2). Each regression uses block fixed effects and standard errors are reported in parentheses. Second part of the table reports linear combinations of co-efficients and test them against a null of zero difference. $p-values$ of the tests are reported in brackets.

Table A4: Health and Multitasking Indices Using Equal Weights

	Multitasking Index (1)	Health Index (2)
Mission	0.183***	0.201***
	(0.043)	(0.052)
Financial Incentive	-0.036	0.112
	(0.053)	(0.072)
Mission and Financial Incentive	0.141***	0.155**
	(0.050)	(0.060)
Placebo	-0.039	0.055
	(0.047)	(0.054)
Control Mean	-0.000	-0.000
# of Observations	710	710
# of Workers	710	710
	Linear Combinations of Coefficients	
Mission − Financial Incentive	0.219	0.089
	[0.000]	[0.133]
Mission − Mission and Financial	0.042	0.046
	[0.224]	[0.316]
Financial Incentive − Mission and Financial	-0.177	-0.043
	[0.000]	[0.515]

Note: The table reports indices of multitasking and health outcomes that are created using equally weighted data following Kling et al. (2007). All regressions control for randomization block fixed effects and standard errors are clustered at the worker level. Second half of the table reports differences between coefficients and tests them against a null hypothesis of no difference. $p-values$ of the tests are reported in square brackets.

Table A5: Balance of Children Weight Data

	Weight Data Not Available = 1 (1)	Gender Boy = 1 (2)	Age in Months (3)
Mission	0.228	0.451	16.093
	(0.026)	(0.015)	(0.396)
Financial Incentive	0.236	0.475	15.000
	(0.045)	(0.025)	(0.644)
Mission and Financial Incentive	0.270	0.438	15.414
	(0.047)	(0.026)	(0.574)
Placebo	0.209	0.400	15.329
	(0.031)	(0.018)	(0.422)
Pure Control	0.239	0.439	15.776
	(0.046)	(0.023)	(0.513)
	p-value of hypotheses		
Joint orthogonality p-value	0.873	0.120	0.551
Mission − Control = 0	0.846	0.673	0.625
Financial − Control = 0	0.967	0.302	0.347
Mission and Financial − Control = 0	0.637	0.974	0.638
Placebo − Control = 0	0.590	0.175	0.502
# of Observations	710	2708	2708
# of Workers	710	542	542

Notes: This table reports the balance on availability, age and gender of the child weight data.

Table A6: Effects of Treatments on Health Outcomes

	Prevalence of Diarrhea (1)	Proportion Timely Vaccinated (2)	Mortality Rate: Children (3)	Mortality Rate: Mother (4)	Weight of Children (Kg) (5)	Health Index (6)
Mission	-0.071**	0.031***	-0.003	-0.001	0.116	0.187***
	(0.035)	(0.011)	(0.002)	(0.001)	(0.136)	(0.048)
Financial Incentive	-0.098**	0.012	-0.001	0.000	0.188	0.089
	(0.039)	(0.013)	(0.003)	(0.002)	(0.151)	(0.069)
Mission and Financial Incentive	-0.076*	0.029**	-0.001	-0.000	0.306*	0.157***
	(0.039)	(0.012)	(0.003)	(0.001)	(0.164)	(0.058)
Placebo	-0.002	0.007	-0.001	-0.001	-0.026	0.053
	(0.036)	(0.011)	(0.002)	(0.001)	(0.144)	(0.052)
Control Mean	0.287	0.888	0.008	0.002	10.648	-0.000
# of Observations	2292	5136	703	703	2711	542
# of Workers	686	710	703	703	543	542
Data Source	HH Survey	HH Survey	Admin	Admin	Admin	–
Linear Combinations of Coefficients						
Mission − Financial Incentive	0.027	0.019***	-0.002	-0.001	-0.073	0.098*
	[0.364]	[0.002]	[0.508]	[0.343]	[0.529]	[0.075]
Mission − Mission and Financial	0.005	0.002	-0.002	-0.001	-0.190	0.030
	[0.855]	[0.841]	[0.416]	[0.503]	[0.152]	[0.489]
Financial Incentive − Mission and Financial	-0.021	-0.018	0.000	0.001	-0.117	-0.068
	[0.530]	[0.102]	[0.989]	[0.699]	[0.431]	[0.294]

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This table reports the effects of treatments on health outcomes mentioned in the column headers with addition of the analysis on child weight, using household and administrative data. $p-values$ of the tests are reported in square brackets. Each regression controls for randomization block fixed effects and column 2 also uses survey round fixed effects as the information was collected in multiple rounds. The analysis in column 5 also controls for age and gender of the children for whom the weight data is reported. Standard errors are clustered at the worker level and reported in parentheses.

Table A7: Beliefs About the Role of Mission in the Organization

	Index of Beliefs	Importance	Mission Alignment	Attachment
	(1)	(2)	(3)	(4)
Mission	0.201***	0.216*	0.174*	0.215*
	(0.071)	(0.115)	(0.104)	(0.110)
Financial Incentive	-0.031	0.045	-0.160	0.024
	(0.090)	(0.139)	(0.143)	(0.141)
Mission and Financial Incentive	0.238***	0.252**	0.218*	0.244**
	(0.079)	(0.127)	(0.119)	(0.118)
Placebo	-0.146*	-0.092	-0.302**	-0.043
	(0.081)	(0.129)	(0.126)	(0.123)
Control Mean	0.000	0.000	0.000	0.000
# of Observations	705	705	705	705
# of Workers	705	705	705	705
	Linear Combinations of Coefficients			
Mission − Financial Incentive	0.232***	0.170*	0.334***	0.191*
	[0.001]	[0.099]	[0.004]	[0.083]
Mission − Mission and Financial	-0.036	-0.036	-0.044	-0.028
	[0.515]	[0.674]	[0.584]	[0.714]
Financial Incentive − Mission and Financial	-0.269***	-0.207*	-0.378***	-0.220*
	[0.001]	[0.078]	[0.003]	[0.063]

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This table reports the effects of treatments on standardized stated beliefs regarding organizational mission. Index of beliefs is a composite index of workers' agreement with three statements on a scale of 1 to 7. (1) Importance: I like the LHW program more than other departments because of the importance it places on the mission. (2) Alignment: I believe the LHW program mission is very similar to my thinking since the beginning of 2019. (3) Attachment: If the LHW program mission was something else, I would not have been as attached to the program. Regressions controls for randomization blocks. First half the table reports the coefficients on each treatment. The regressions control for randomization block fixed effects. Standard errors clustered at the worker level are reported in parentheses. Second part of the table reports linear combinations of coefficients and test them against a null of zero difference. $p - values$ of the tests are reported in square brackets.

Table A8: Effects of Treatment on Acceptance of Offers

| | *Accept to Work = 1* | | | |
| | *Rs. 0* | *Rs. 50* | *Rs. 100* | *Rs. 200* |
	(1)	(2)	(3)	(4)
Mission	0.105*	0.008	0.007	-0.001
	(0.059)	(0.061)	(0.062)	(0.063)
Financial Incentive	-0.058	-0.018	0.020	0.010
	(0.076)	(0.075)	(0.076)	(0.076)
Mission and Financial Incentive	0.135*	0.001	0.010	0.019
	(0.070)	(0.074)	(0.076)	(0.077)
Placebo	0.012	-0.015	0.010	0.028
	(0.065)	(0.066)	(0.067)	(0.067)
Control Mean	0.614	0.466	0.545	0.557
# of Observations	707	707	707	707
# of Workers	707	707	707	707
Block Fixed Effects	✓	✓	✓	✓
	Linear Combinations of Coefficients			
Mission − Financial Incentive	0.163	0.026	-0.013	-0.011
	[0.007]	[0.678]	[0.834]	[0.856]
Mission − Mission and Financial	-0.029	0.007	-0.003	-0.020
	[0.578]	[0.911]	[0.959]	[0.741]
Financial Incentive − Mission and Financial	-0.193	-0.019	0.010	-0.009
	[0.007]	[0.799]	[0.898]	[0.901]

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This table reports the effects of treatments on accepting to work for various offers on an activity one year after the experiment. The dependent variable in each regression takes the value 1 if the worker accepts the offer mentioned in the column heading. Each regression uses block fixed effects and standard errors are reported in parentheses. Second part of the table reports linear combinations of coefficients and test them against a null of zero difference. *$p-values$* of the tests are reported in brackets.

Table A9: Who Responds to the Treatments

	Dependent Var: Household Visit = 1		
	(1)	(2)	(3)
Mission	0.058***	0.042***	0.061***
	(0.013)	(0.015)	(0.014)
Financial Incentive	0.083***	0.115***	0.090***
	(0.016)	(0.020)	(0.020)
Mission and Financial Incentive	0.068***	0.055***	0.067***
	(0.016)	(0.020)	(0.019)
High Baseline Performance	0.086***		
	(0.020)		
Mission X High Baseline Performance	0.004		
	(0.024)		
Financial Incentive X High Baseline Performance	0.055**		
	(0.028)		
Mission and Financial Incentive X High Baseline Performance	0.007		
	(0.028)		
Public Service Motivation (PSM)		0.003	
		(0.021)	
Mission X PSM		0.020	
		(0.024)	
Financial Incentive X PSM		-0.033	
		(0.031)	
Mission and Financial Incentive X PSM		0.027	
		(0.030)	
IQ			-0.005
			(0.022)
Mission X IQ			-0.027
			(0.025)
Financial Incentive X IQ			0.027
			(0.032)
Mission and Financial Incentive X IQ			0.004
			(0.029)
Control Mean			
# of Observations	21279	21279	21279
# of Workers	710	710	710
Block & Wave Fixed Effects	✓	✓	✓

Note: This table reports the heterogeneous effect to treatments based on their characteristics. Regressions control for randomization block and survey wave fixed effects and standard errors are clustered at the worker level.

Figure A1: Effects of Treatments on Multitasking Index
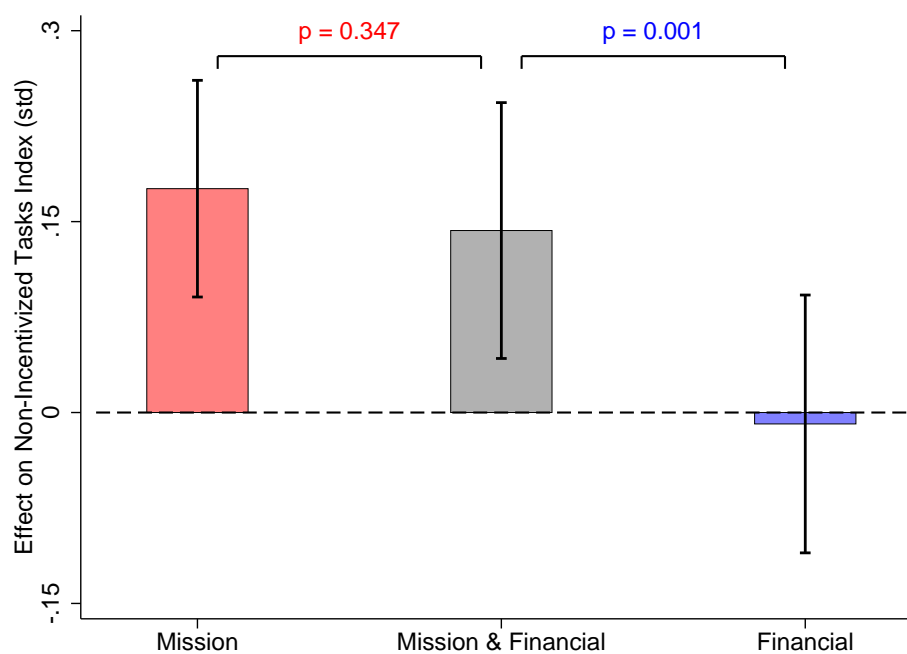
Figure A2: Comparison of Health Effect on Diarrhea with other Interventions
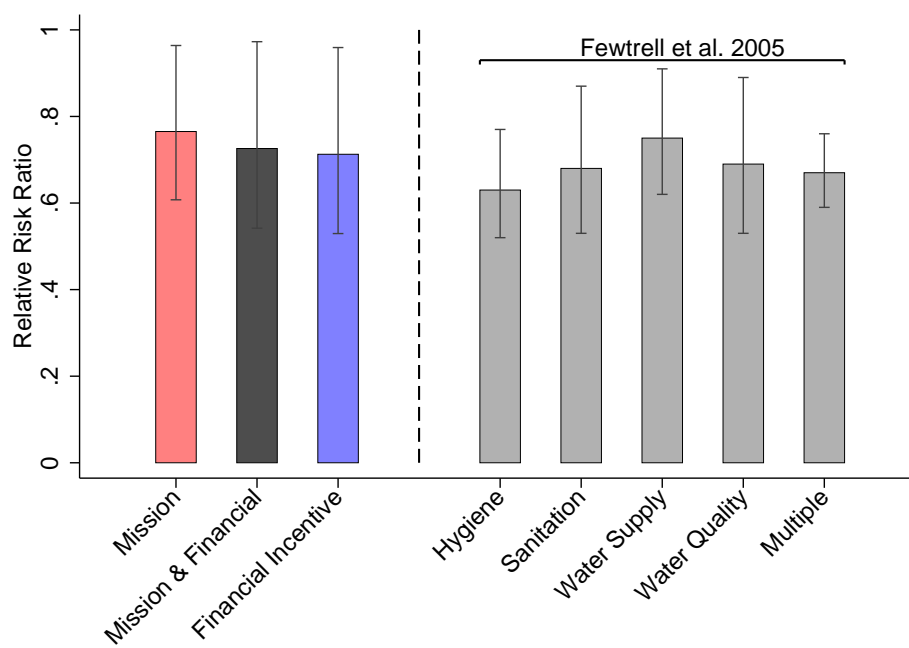
Figure A3: Perception of Workers About Being Monitored
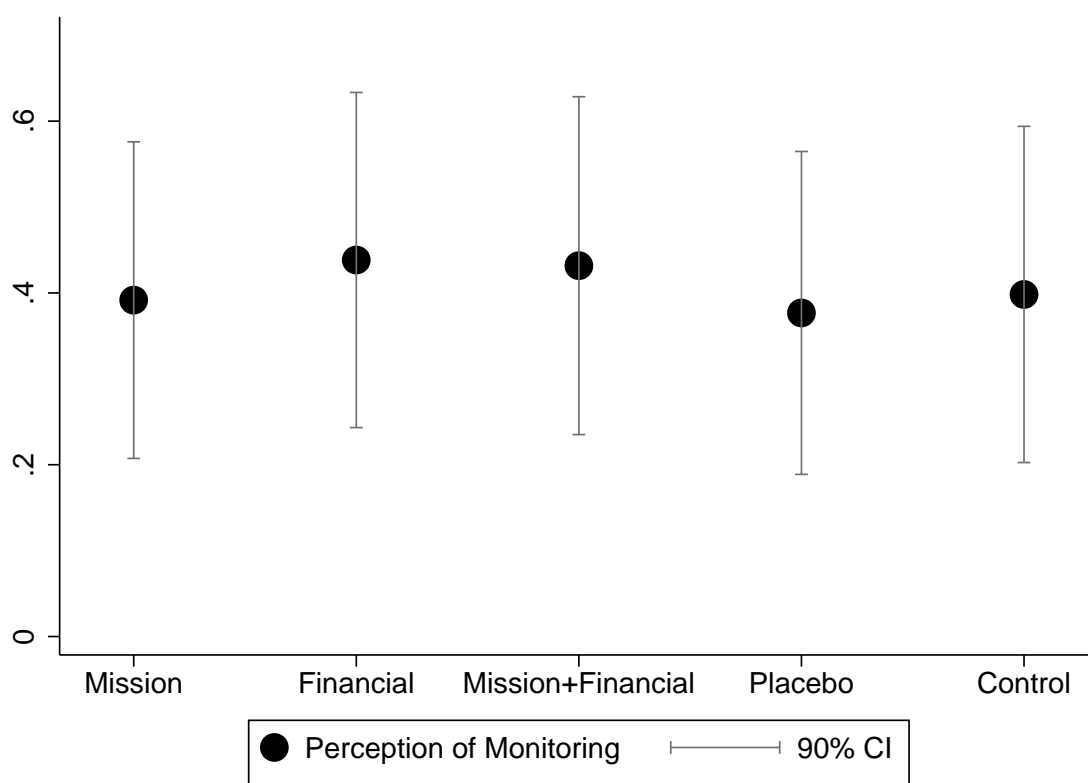
Notes: This figure plots the mean perception of being monitored reported by workers in different treatment groups using data from worker survey.

Appendix B

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

B.1   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.2   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, some studies show 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.

B.3   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

B.4   Timeline

Below, we provide a condensed timeline for the project.

1. March 3-14, 2015: Pilot for treatment design in Haripur District, KP

2. Last week of March - April 13, 2015: Administering Treatments

3. April 13-17, 2015: Candidates file their papers

4. May 3-28, 2015: Survey of all candidates

5. May 30, 2015: Election Day

6. June 23 - July 31, 2016: Performance surveys of council members and citizens

7. June 25, 2015: Initial notification of results

8. August 30, 2015: Oaths of office begin

B.5   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

As noted in the PAP, we 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

Overall, the results reported in Section 2.4 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 2.4.4 are not pre-registered but correspond to Analysis 2.5 in the PAP, that relates to calculating village level effects. The analysis in section 2.5.1 was pre-registered as Analysis 2.1 in the plan. Lastly, some variables used in the analysis in section 2.5.2 were not pre-registered.

Table B3: Overall Balance for Experiment

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

Note: This table shows randomization balance by treatment arm. The bottom part reports p-values comparing indicated coefficients. The joint orthogonality test checks if all coefficients are equal. All regressions include block fixed effects. Robust standard errors are reported in parentheses.

Table B4: 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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. 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.

Table B5: 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

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

Table B6: 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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. This table checks for balance in missingness of data used in Table 2.3. 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 B7: Policy Effects - Manski Bounds

	Effects on Euclidean Dist in Table 2.3 (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

Note: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. 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.

Table B8: Extensive Margin of Performance - Total Resources

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

Note: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. 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 B9: 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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. 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.

Table B10: 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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. 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.

Table B11: Policy Effects of Public and Private Treatments

	Total Distance	Municipality	Infrastructure	Community	Not Primary Responsibility
	(1)	(2)	(3)	(4)	(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

Note: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. This table reports the effect of treatments based on whether they are delivered in private or public on the performance of elected councils. The table uses a village level dataset that is constructed based on official budget data from the councils and the preferences of citizens regarding the budget. Each column is a dependent variable that is defined as quadratic distance between the preferences of citizens regarding the category of budget mentioned in the column header and the actual spending by the council. The distance is calculated using the formula $j \in J$ by $\sqrt{(B_j - \overline{C}_j)^2}$, where B_j refers to the percentage of the budget spent on j and $\overline{C}_j$ is the average of citizen preferences for spending on that category. The overall difference in column (1) is calculated using the formula: $\sqrt{\sum_J (B_j - \overline{C}_j)^2}$. Panel A reports the comparison for treatments delivered in public, and panel B reports the comparisons when treatments are delivered in private. Each regression uses block fixed effects. Standard errors are clustered at the village level and reported in parenthesis.

Table B12: 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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. 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.

Table B13: 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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. 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 B14: 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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. 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 2.2. 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.

Table B15: 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		

Note: 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.

Table B16: 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

Note: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. 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.

Table B17: 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

Note: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. 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.

Appendix C

Relaxing Barriers to Political Entry of Non-Elites: Experimental Evidence from Local Government Elections in Pakistan

C.1   Appendix Figures and Tables

Table C1: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Village Characteristics					
Num Settlements	2.23	1.58	1	11	191
Cell Reception	0.91	0.29	0	1	191
Distance to main road	8.14	16.98	0.5	100	191
Distance to District HQ	26.76	19.52	2	165	191
Distance to County HQ	22.92	17.61	1	110	191
Literacy Rate	50.41	12.29	17.6	74.10	175
Council size	9.81	1.27	7	14	191
Average # of candidates on general seat	9.02	2.57	5	23	191
Average # of general seats	6.08	0.96	5	10	191
Total Votes	6552.73	3060.1	1385	17345	191
Polled Votes	5486.92	2494.9	902	14498	185
Rejected Votes	398.81	320.97	0	1675	185
Voter turnout	0.76	0.11	0.46	0.99	185
Individual Characteristics					
Family Income (Rs.)	23760.3	14695.86	1000	350000	1985
Years of Schooling	9.48	3.42	0	18	2021
Village Committee Member	0.02	0.15	0	1	2039
Prior on Winning	3.97	0.87	1	5	2039
Prior on Cost to Run	3.23	0.84	1	5	2039

Table C2: Balance Table for Indicies

	Pro-Social Index (1)	Personal Index (2)	Political Elites (3)	Traditional Elites (4)
Lawyer	0.010	0.021	0.003	0.024
	(0.012)	(0.018)	(0.007)	(0.016)
Electability	-0.000	0.018	-0.001	0.029
	(0.012)	(0.018)	(0.007)	(0.016)
Social Benefits	-0.025	-0.012	-0.015	-0.014
	(0.017)	(0.025)	(0.010)	(0.021)
Personal Benefits	-0.006	-0.020	0.006	0.082
	(0.016)	(0.023)	(0.009)	(0.023)
Control	0.006	0.048	-0.004	0.065
	(0.021)	(0.032)	(0.013)	(0.030)
Mean of Variable	4	3	0	0
# Observations	2012	2020	2021	2021
Joint orthogonality p-value	0.429	0.215	0.510	0.010

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

Table C3: Main Results-Clustered SE

	Dependent Variable: Filer			
	(1)	(2)	(3)	(4)
Lawyer	0.049**	0.049**	0.065**	0.065**
	(0.028)	(0.028)	(0.025)	(0.026)
Electability	0.017	0.017	0.017	0.017
	(0.379)	(0.381)	(0.376)	(0.378)
Benefits	0.023		0.024	
	(0.213)		(0.198)	
Social Benefits		0.024		0.026
		(0.363)		(0.325)
Personal Benefits		0.022		0.022
		(0.437)		(0.451)
Constant	0.307***	0.307***	0.306***	0.307***
	(0.000)	(0.000)	(0.000)	(0.000)
F-Stat			2.447	1.849
N	2039	2039	2039	2039
	Dependent Variable: Elect			
	(1)	(2)	(3)	(4)
Lawyer	0.008	0.008	0.010	0.010
	(0.651)	(0.649)	(0.650)	(0.647)
Electability	0.003	0.003	0.003	0.003
	(0.867)	(0.865)	(0.866)	(0.864)
Benefits	0.034**		0.035**	
	(0.021)		(0.020)	
Social Benefits		0.032		0.032
		(0.115)		(0.109)
Personal Benefits		0.037		0.037
		(0.110)		(0.108)
Constant	0.162***	0.162***	0.162***	0.162***
	(0.000)	(0.000)	(0.000)	(0.000)
F-Stat			1.922	1.446
N	2039	2039	2039	2039

Notes: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Standard Errors are reported in parenthesis and clustered at village level.

Table C4: Effects Controlling for all Treatment Groups

	Dependent Variable: Filer	
	(1)	(2)
Lawyer	0.045	0.046
	(0.218)	(0.217)
Electability	0.005	0.005
	(0.896)	(0.894)
Benefits	0.016	
	(0.672)	
Social Benefits		0.030
		(0.507)
Personal Benefits		0.003
		(0.949)
Lawyer+Electability	-0.004	-0.004
	(0.938)	(0.936)
Lawyer+Personal Benefits		0.021
		(0.746)
Electability + Personal Benefits		0.082
		(0.209)
Lawyer + Electability + Personal Benefits		-0.063
		(0.510)
Electability + Social Benefits		-0.053
		(0.409)
Lawyer + Electability + Social Benefits		0.065
		(0.489)
Lawyer+Social Benefits		-0.003
		(0.959)
Constant	0.313***	0.313***
	(0.000)	(0.000)
N	2039	2039
Standard Errors	Robust	Robust

Notes: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard Errors are reported in parenthesis. All regressions include village fixed effects.

Table C5: Effects Controlling for all Treatment Groups - Clustered SE

	Dependent Variable: Filer	
	(1)	(2)
Lawyer	0.049**	0.047
	(0.028)	(0.250)
Electability	0.017	0.015
	(0.381)	(0.698)
Social Benefits	0.024	0.036
	(0.363)	(0.500)
Personal Benefits	0.022	0.002
	(0.437)	(0.963)
Lawyer+Electability		-0.003
		(0.963)
Lawyer+Personal Benefits		0.014
		(0.822)
Electability + Personal Benefits		0.056
		(0.412)
Lawyer + Electability + Personal Benefits		-0.059
		(0.506)
Electability + Social Benefits		-0.048
		(0.481)
Lawyer + Electability + Social Benefits		0.061
		(0.563)
Lawyer+Social Benefits		-0.005
		(0.942)
Constant	0.307***	0.309***
	(0.000)	(0.000)
# Village	192	192
N	2039	2039
Standard Errors	Clustered	Clustered

*Notes*: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. p-values are reported in parentheses. Standard errors are clustered at the village level.
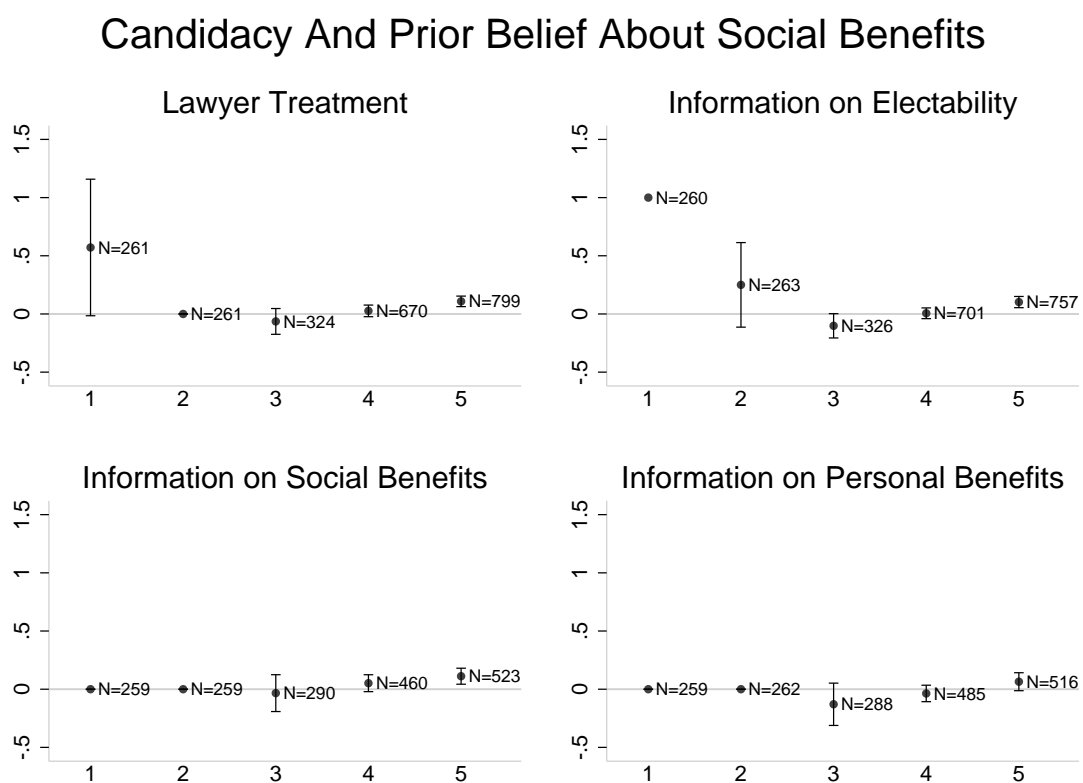
Figure C1: Response to treatments based on prior belief about winning the election

Candidacy And Prior Belief About Winning
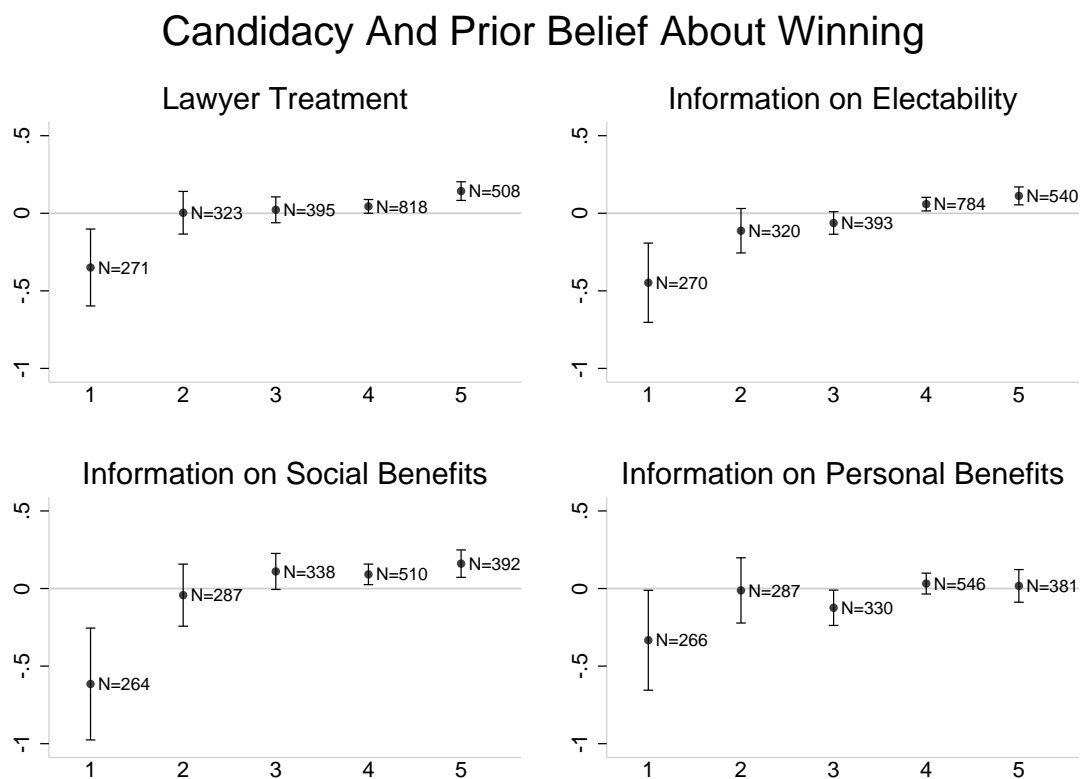
Figure C2: Response to treatments based on prior belief about affordability of the election

Candidacy And Prior Belief About Affordability
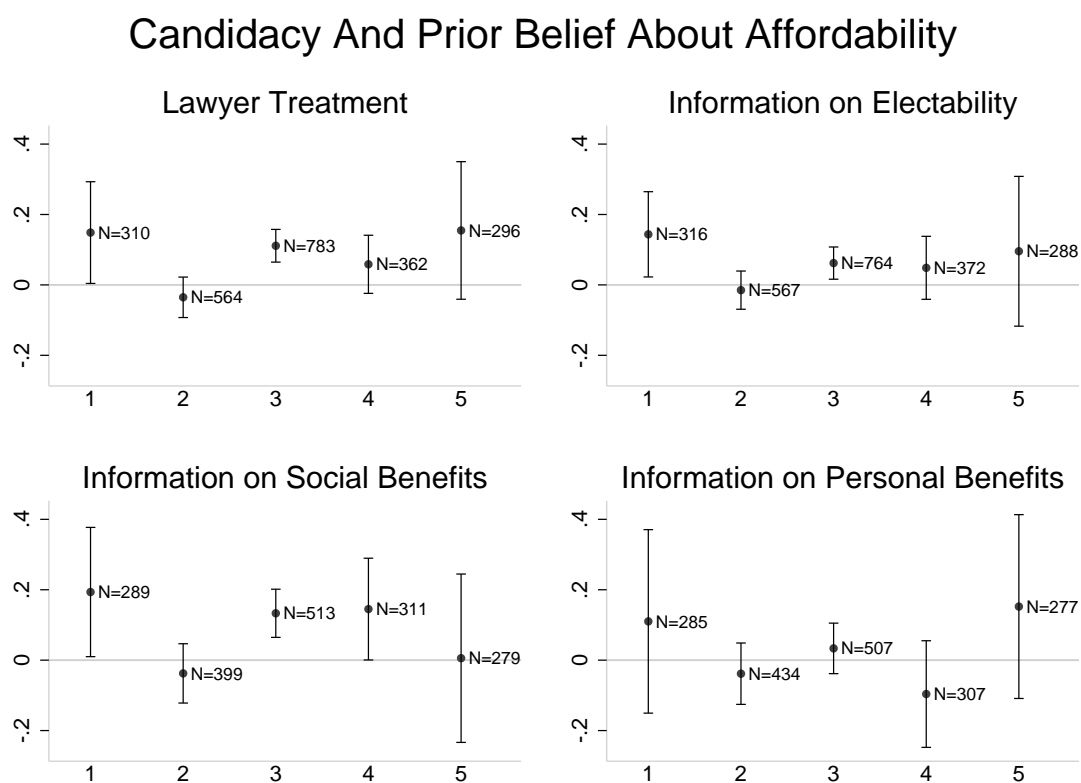
Figure C3: Response to treatments based on prior belief about social benefits

Candidacy And Prior Belief About Social Benefits

Figure C4: Response to treatments based on prior belief about personal benefits

Candidacy And Prior Belief About Personal Benefits