



# Essays in Development Economics and Governance

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# Essays in Development Economics and Governance

A dissertation presented

by

Sarika Gupta

to

The Department of Public Policy

in partial fulfillment of the requirements

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in the subject of

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## **Essays in Development Economics and Governance**

### **Abstract**

Good governance plays an important role in facilitating economic development and promoting human welfare. This dissertation examines topics at the intersection of development economics and governance through a series of field experiments carried out in South Asia. In Chapter 1, I examine barriers to take-up of an unconditional cash transfer program provided by the government for poor widows in Delhi, India and measure the distributive implications of these barriers. To do this, I conduct a randomized controlled trial with eligible women, providing some with only information about the program and others with information plus varying degrees of application assistance. In Chapter 2, I follow-up with the women in the study from Chapter 1 to measure the impact of expanding access to this unconditional cash transfer on economic and psychological measures of well-being as well as levels of political engagement. In Chapter 3, my co-authors and I aim to understand whether information constraints of voters help explain the election of candidates facing criminal charges to public office in India. To this end, we run a large-scale, mobile-based voter information campaign around the Uttar Pradesh state assembly elections of 2017.

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To Jalpan and Sahana

# Introduction

Good governance plays an important role in facilitating economic development and promoting human welfare. This dissertation examines topics at the intersection of development economics and governance through a series of field experiments carried out in South Asia.

Governments worldwide administer targeted social programs to improve the well-being of vulnerable groups, yet many eligible citizens do not take up these programs. In Chapter 1, I examine take-up of an unconditional cash transfer program for poor widows and divorcees in Delhi, India. Despite the considerable benefits, only one-third of eligible citizens are enrolled, with lower enrollment among more vulnerable women. I conduct a field experiment with over 1,200 pension-eligible women to identify barriers to program take-up and their distributive implications.

One group of women is provided with only information about the program. Others receive information plus *mediation*: assistance with filling out the application form (basic mediation) or assistance engaging with political authorities (intensive mediation). I find that information alone raises application rates only among literate women. On the other hand, basic and intensive mediation increase average application rates by 41% and 70%, respectively.

Furthermore, providing mediation changes the applicant pool to include more vulnerable women: those who are illiterate, politically disconnected, or lack autonomy in their household. While conventional wisdom suggests that application ordeals ensure take-up by those with the highest marginal utility of enrollment, I show that ordeals can interact with capabilities of poor citizens to select *out* those with a high need for the program. Simpler

enrollment procedures and strengthened channels of bureaucratic mediation may facilitate more widespread and inclusive take-up.

In Chapter 2, I exploit the random variation in take-up of the widow pension scheme generated by the interventions employed in Chapter 1 to measure the impact of this unconditional cash transfer on economic and psychological measures of well-being. I find that two years after the interventions, treated women have increased their spending on vegetables and are less likely to be in debt or report food insecurity. They are more likely to have cash on hand for emergencies, more likely to report high levels of life satisfaction, and less likely to report anxiety about the future. These latter results hold for both the reduced form and the two-stage least squares analysis, which uses treatment assignment as an instrument for receiving the pensions. I also find that treated women are more likely to exhibit self-reliance in subsequent interactions with the government and believe that their elected official would help them if needed. Overall, these results suggest important improvements to well-being and empowerment from engaging the state and receiving this cash transfer from the government. This is an important finding given that unconditional cash transfer programs are becoming increasingly popular as a social protection strategy in developing countries.

In Chapter 3, my co-authors (Siddarth George and Yusuf Neggers) and I explore another important governance challenge in developing countries: adverse selection to political office. Specifically, criminality is a serious problem in Indian politics. Many major party candidates have charges for violent crimes like murder, rape, kidnapping and armed robbery. Moreover, criminal rule has been shown to hurt economic development, worsen poverty and increase crime, with costs estimated at 2-3% of GDP. One view is that criminals are over-represented in politics because they offer parties electoral advantages through their wealth, local networks, or ability to mobilize voters. We test an alternative theory that criminals get elected because voters lack information to evaluate and coordinate on good candidates, leading them to select on observable characteristics like ethnicity.

We partner with 3 Indian telecom providers and run a large-scale, mobile-based voter

information campaign around the Uttar Pradesh state assembly elections in 2017. Voters in randomly-selected villages receive a voice call and text message informing them about the criminal charges against all major party candidates standing in their constituency. Our campaign increases the votes received by non-criminal candidates by 2 percent and reduces the votes received by candidates charged with the most severe crimes by 13 percent. Effects are strongest for the “coordination” treatment arm, which provides a public signal that the same information has been sent to many other voters, and the “ethnicity” treatment arm, which urges voters to break default habits of voting along ethnic lines. Overall turnout is not affected by the interventions on average, reflecting the offsetting impacts on votes received for candidates of different types.

# Chapter 1

## Perils of the Paperwork: The Impact of Information and Application Assistance on Welfare Program Take-Up in India

### 1.1 Introduction

Low take-up of targeted social programs is a puzzling outcome seen in many contexts. Given that these programs often redistribute income to impoverished citizens, it is important to understand why this money is being left on the table. In developing countries, significant literature focuses on distortions in service provision due to leakage, elite capture, or other forms of corruption<sup>1</sup>, but less work considers constraints to program access when these issues are not at play.

Evidence from rich countries offers two primary explanations for low take-up of social

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<sup>1</sup>This work highlights intentional diversion of anti-poverty benefits from intended recipients (Banerjee et al., 2009; Bardhan and Mookherjee, 2006; Besley et al., 2011b; Niehaus and Atanassova, 2013; Reinikka and Svensson, 2004), targeting along partisan and ethnic lines (Chandra, 2007; Dunning and Nilekani, 2013; Habyarimana et al., 2007; Stokes et al., 2013), and unreliable service providers (Chaudhury et al., 2006; WB, 2003).

programs: lack of information and transaction costs of enrollment (Currie, 2004)<sup>2</sup>. I employ a field experiment to examine these two barriers to take-up, and their distributive implications, in the context of an unconditional cash transfer program in India. I find that in comparison to providing only information, reducing transaction costs is particularly important to increase take-up by more vulnerable individuals.

Like the majority of Indian states, the Delhi government provides a non-contributory, social pension for life to young widows and divorced women living in poverty. The transfer amount of Rs. 1,500 per month is substantial, equivalent to 70% of median per-capita consumption across urban areas of the state (NSS, 2012)<sup>3</sup>. Strict verification procedures and secure bank-to-bank payment systems leave little room for leakage. A recent survey by the World Bank (2014), however, finds that two-thirds of eligible women are *not* enrolled in this program. This resonates with a broader trend in India, where only 40% of citizens apply for the goods and services they report they need from the government (Demirguc-Kunt et al., 2017).

Enrollment in this pension program is dependent on undergoing an application process to prove one's eligibility, requiring citizens to self-select in to the program. It is possible that better information about program eligibility and benefits alone could boost enrollment, especially if more vulnerable citizens tend to be less informed about government programs. Providing information to citizens was shown to successfully increase take-up of food subsidies in Indonesia (Banerjee et al., 2015), while other studies examining the impact of information on community and voter empowerment efforts have found mixed results (Mansuri and Rao, 2013; Pande, 2011). If citizens do not take up social programs even when provided comprehensive information, it suggests a potentially important role of transaction costs in limiting take-up.

Of course, any time a program is targeted instead of universally provided, there will

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<sup>2</sup>Transaction costs are broadly defined to include the time, effort, monetary, and stigma-related costs associated with applying for or being enrolled in a given program.

<sup>3</sup>The transfer amount was raised to Rs. 2,500 per month in January 2017, making it equivalent to 118% of median per capita consumption in urban Delhi.

be some transaction costs involved due to the screening required to separate eligible from ineligible citizens, which will predictably lead to incomplete take-up (Kleven and Kopczuk, 2011). Common screening mechanisms include application forms and supporting documents used to prove eligibility. Still, standard economic theory would suggest that these screening procedures, or *ordeals*, promote self-selection by those with the highest marginal utility of enrollment, as they are most willing to undergo the hassle of applying (Nichols and Zeckhauser, 1982).

Yet in comparison to the rich, the poor may be particularly sensitive to small, short-run costs (Holla and Kremer, 2009; Mullainathan and Shafir, 2013) or face higher utility costs from the application process, leaving the effect of ordeals on targeting to be theoretically ambiguous (Alatas et al., 2016). Similarly, I argue that it is important to understand how administrative ordeals of welfare programs interact with personal capabilities of the poor to determine program take-up and the distribution of welfare recipients. In developing countries, poverty, especially among women, is associated with adult illiteracy, political dis-empowerment, and constraints on basic mobility outside of the household, so ordeals that work well in rich countries may have unintended consequences in such settings.

Therefore, in the context of the widow pension scheme in Delhi, I answer two important questions with my research. First, what is the role of information constraints and transaction costs of ordeals in explaining low take-up? Second, how does reducing either of these barriers change the distribution of applicants?

To answer these questions, I conduct a randomized controlled trial with over 1,200 pension-eligible women living across 75 Delhi slums. To apply for widow pensions, women must complete an application form, provide several supporting documents, and obtain the signature of their state legislator (MLA) before finally submitting the application to the district bureaucracy. One group of women in my study is provided only detailed information about the pension scheme, including benefits, eligibility requirements, and application procedures. The second intervention group, called the *basic mediation* group, receives identical information along with an application form and personal assistance with

filling it out. The third intervention group, or *intensive mediation* group, receives information, assistance filling out the form, and an opportunity to be accompanied to the MLA's office to obtain the required signature. A fourth group of women serves as the status quo or control group and receives neither information nor assistance.

I find that providing only information increases the number of women who begin the application but does not, on average, significantly increase the number who complete the entire process. Thus, in this setting, certain ordeals within the system are not easily overcome by better information alone. This is true despite the fact that providing information to women increased their knowledge of the program to the same extent as providing information plus mediation did. In the two mediation groups, however, more women complete all steps of the application, even the final steps with which we do not assist. In comparison to a 15% completion rate in the control group 4 months after the intervention, women who receive basic mediation are 41% (6pp) more likely to complete the application process, and women who receive intensive mediation are 70% (11pp) more likely to complete it.

Higher application rates do translate into higher enrollment: treated women, pooling across the two mediation groups, are 46% (7pp) more likely to be receiving pensions than those in the control group one year after the intervention. This effect is 2.5 times higher in magnitude than the impact of anti-corruption strategies that reduced leakages in similar social pension programs implemented in another Indian state (Muralidharan et al., 2016)<sup>4</sup>. In both cases, the focus is on ensuring eligible citizens receive an entitlement they are due from the government. The large impact of individual assistance found here highlights the importance of application ordeals in determining access to such entitlements, especially when common, corruption-related distortions are minimal.

Importantly, the basic and intensive mediation interventions assist citizens at two stages where application ordeals could potentially select out women with specific vulnerabilities. Having to complete an application form might exclude illiterate citizens, and getting the

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<sup>4</sup>This intervention was aimed at introducing a secure payment system for pensions using biometric identification in place of traditional cash distribution systems in villages.

politician's sign-off could exclude women with limited mobility outside of the household or those less connected to the MLA. My results show that the treatment impacts are conditioned by these characteristics of the women.

While 70% of the women in my sample are illiterate, the pure information treatment is only sufficient to help *literate* women complete all steps of the application on their own. Basic mediation at the stage of form filling, however, helps facilitate applications by illiterate women. Offering intensive mediation, or help visiting the legislator's office, has no additional impact on illiterate women beyond the impact of basic mediation. These findings suggest that for this group, obtaining and filling the application form is the more relevant barrier to take-up.

I also find that women who are less connected to the political gate-keeper of this program, the MLA, benefit more from mediation than those with stronger MLA connections. In addition, less connected women observe larger impacts from the two mediation treatments than from pure information. Surprisingly, less connected women benefit equally from basic and intensive mediation, suggesting that being connected with the MLA is more important for getting the form filled than getting the official signature, which may actually be given out impartially based on administrative criteria. These findings imply that the MLA is not necessarily using the signature requirement to provide biased access, but knowing the MLA provides a reliable source of mediation with the application form for some women.

Finally, the results show that women lacking autonomy in their household are dramatically underrepresented among applicants in the control group and benefit significantly from all three of the treatments offered. Notably, for these women, the intensive mediation, which helps them leave the house and obtain a politician signature, has a larger impact than either pure information or basic mediation. For these women, each trip outside of the home required for the application can be a challenge. Streamlined application procedures could improve take-up by such women, who may have the greatest need for an independent source of income from the state to improve their welfare.

Qualitative interviews and robustness checks support my interpretation of the findings

and help reject alternative explanations, as discussed in Section 5. Specifically, I rule out that the interventions primarily helped women overcome cognitive constraints or welfare stigma, and I show that results are not driven by women who would have eventually applied on their own .

This work contributes to existing literature aimed at understanding take-up of welfare benefits, which has been concentrated almost exclusively in rich countries (Bhargava and Manoli, 2015; Bettinger et al., 2012; Currie, 2004; Deshpande and Li, 2017)<sup>5</sup>. In developing countries, public service deficiencies are often attributed to a breakdown in accountability between citizens, politicians, and bureaucrats<sup>6</sup>. Less research in this context focuses on take-up of welfare programs that are relatively well-administered by the state. I help fill this gap by showing that lack of information and transaction costs associated with applying are important barriers to access in developing countries as well. In addition, reducing each of these barriers increases take-up by different types of individuals, highlighting the distributive implications of ordeals when they interact with personal vulnerabilities. In this context, citizens seem constrained in opting in to welfare programs *even when they want to and make efforts to* because they get lost along the way in a complex and taxing system. This calls for looking at take-up as more than an all-or-nothing decision, but instead one that reflects baseline empowerment and resources.

These results are consistent with behavioral theories that suggest small, situational barriers, such as “a testy bus ride, challenging hours, or the reluctance to face a contemptuous bank-teller” (Bertrand et al., 2004) may play a large role in preventing take-up of social programs by the poor. In such cases, minor changes that open up a “channel” of access, such as providing a map to the office or specifying a time to go, can help facilitate actionable

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<sup>5</sup>One notable exception is a paper from Indonesia that finds imposing an ordeal mechanism to induce self-targeting helps a social program reach poorer households than using automatic enrollment to target households (Alatas et al., 2016).

<sup>6</sup>Significant research documents varieties of this breakdown, and offers strategies such as decentralized government (Alderman, 2002; Alatas et al., 2012; Schneider and Sircar, 2014), political oversight of bureaucrats (Raffler, 2016), improved accountability between voters and politicians (Ferraz and Finan, 2011; Pande, 2011), secure payment infrastructures (Muralidharan et al., 2016), and community monitoring (Bjorkman and Svensson, 2009) as potential vehicles to improve outcomes.

behaviors (Bertrand et al., 2004). My findings support the idea that helping with initial steps or reducing hassles can substantially improve take-up. In addition, my heterogeneity analysis suggests that opening these access channels can be particularly important for those whose vulnerabilities cause them to face disproportionately high utility costs of engaging with application ordeals. These individuals may indefinitely opt out or procrastinate unless the process is made more user-friendly and less uncertain via simplification or offering help at challenging stages.

My findings also contribute to our understanding of citizen-state engagement in developing countries. In such contexts, citizens often rely on various intermediaries to access public services<sup>7</sup>. Some intermediaries require compensation, such as payment or political support (Stokes et al., 2013)<sup>8</sup>, while others come in the form of educated or well-connected friends, neighbors, or family members who can provide assistance (Bussell, 2011; Krishna, 2011). Despite these various avenues, I show that many citizens, especially the most vulnerable, actually face a *mediation deficit*, as providing them with basic assistance increases their take-up of state resources. This implies a dearth of reliable, unbiased mediation provided by state actors, who may lack the information, capacity, or political will to reach their most vulnerable constituents. Similarly, non-state actors, such as private or civil society groups, have not sufficiently filled this gap.

Finally, my work speaks to the growing body of literature focusing on political participation by women in developing countries (Bhalotra et al., 2013; Chattopadhyay and Duflo, 2004; Kruks-Wisner, 2011; Prillaman, 2016). Previous work finds that women who lack empowerment within the household are less likely to run for office (Chhibber, 2002), and I similarly find that these women are also less likely to access welfare schemes or engage with political intermediaries who can facilitate access. I corroborate the findings of Prillaman (2016) by showing that participation by women can be increased without changing their

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<sup>7</sup>I provide a useful categorization of mediation channels highlighted in the literature in the Appendix.

<sup>8</sup>Specifically in India, influential local leaders, both informal and elected, are often relied upon to serve as middlemen for gaining access to services, sometimes for a bribe (Bertrand et al., 2007; Bussell, 2014; Jha et al., 2007; Krishna, 2011; Peisakhin and Pinto, 2010; Kruks-Wisner, 2011).

position in the household when external factors are at play, in this case information and individual assistance to overcome ordeals.

From a policy perspective, this research highlights the importance of creating user-friendly, simple, and streamlined application processes in addition to raising awareness of social programs to facilitate greater and more inclusive take-up. Given potential vulnerabilities of targeted groups, strengthening mediation offered directly by the bureaucracy to assist eligible citizens overcome necessary screening ordeals can also improve take-up.

The remainder of this paper is organized as follows. Section 2 describes the study context, Section 3 explains the experimental design and data collection, Section 4 presents the results and robustness checks, and Section 5 concludes and discusses policy implications of this research.

## **1.2 Context of Study**

Female-headed households are more likely to fall into and remain trapped within, cycles of poverty (Krishna, 2012). Oftentimes, families facing such conditions are the first group to be targeted when governments start offering social protection. A classic example is seen in the United States, where the Mother's Pension program was the first welfare program offered by the government to assist families facing the loss or disability of the primary income earner (Aizer et al., 2016). In India, in addition to facing higher rates of poverty, widows traditionally represent one of the most vulnerable and discriminated against groups in the social hierarchy (UN, 2001). Consequently, in India, as in many other countries, social support is offered to women in such conditions.

### **1.2.1 Delhi Widow Pension Scheme**

The widow pension scheme in Delhi is a non-contributory, unconditional cash transfer for life. In addition to widows, women who are divorced, permanently separated, or abandoned by their husbands are covered by this program. To be eligible, women must fall between the ages of 18-59 (becoming eligible for the old-age pension scheme at age 60), have lived in

Delhi for at least 5 years, and have a household income that falls below a given threshold<sup>9</sup>. Once enrolled in the scheme, women receive Rs. 1,500 (approximately \$25) per month transferred directly into their single-user bank account, made in lump sum payments every three months<sup>10</sup>. Due to the automated and secure nature of the transfer, the program is not prone to payment leakages or substantial delays that are common to other social programs in India (WB, 2014; Muralidharan et al., 2016). Despite the clear benefits, a recent survey done by the World Bank (2014) indicates that 66% of eligible women in Delhi are not enrolled in this pension scheme.

The application process involves several steps, which are described below, followed by descriptive data from our surveys and the World Bank (2014) survey which highlights how these steps are completed in practice.

1. **Obtaining an application form:** Application forms are officially available at the state legislator's office (MLA office), the district office, or online.
2. **Filling out the application form:** The application form includes three pages with fields including the woman's name, her husband's name, her address, the date of her husband's death, her aadhar card number, her bank account number, her bank branch information, the information on her marital proof, her constituency number, her MLA's name, and her signature or thumb print.
3. **Gathering and attaching supporting documents:** Along with two passport-sized photographs, the following documents must be attached to the application form:
  - Proof of Marital Status: A death certificate of the husband or a legal divorce certificate is required for widows and divorcees, respectfully. For separated or abandoned women, a police report indicating that the husband has left, is

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<sup>9</sup>However, unlike other social schemes in India, this scheme is not limited to citizens who appear on the government's Below Poverty Line (BPL) list, which has been shown to contain both inclusion and exclusion errors (Niehaus and Atanassova, 2013).

<sup>10</sup>This amount was increased to Rs. 2,500 per month in January 2017, which was after all rounds of data collection used in this paper other than the qualitative interviews conducted in July 2017.

missing, or living separately is required.

- Aadhar Card: This unique identification card serves as identity proof and is linked to the applicant's bank account in order to process the payment.
- Proof of 5-Year Residency in Delhi: Many documents (e.g. electricity bill, ration card) can fulfill this requirement as long as it has the respondent's name and a Delhi address listed from at least 5 years ago.
- Bank Account Information: Applicants must provide a copy of their bank pass-book which has the bank branch information as well as the page with the last transaction which indicates the current balance<sup>11</sup>.

4. **Obtaining an MLA Signature**: Once the application form has been filled out and the necessary supporting documents have been attached, the applicant must take this completed package to the MLA's office to receive a signature on her application.

5. **Submitting Application to the District Office**: After getting the MLA signature, the application package can be submitted to the district office, which is the bureaucratic center where the application and pension payments are processed. At the district office, the application can either be accepted or rejected based on a review of the application and supporting documents by a government employee. If the application is accepted, the outcome is now out of the hands of the applicant, who will have to check her bank account after three months to see if the money is coming. There is no other way to confirm enrollment into the program other than to wait for the money to start coming to the bank account. If the applicant is rejected at the district office, she may be instructed to provide additional or corrected documents and come back or may be deemed ineligible (due to age, residence, or income), meaning she cannot get the pension.

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<sup>11</sup>If an applicant's bank balance is too high, she is deemed ineligible.

### 1.2.2 Completing the Application in Practice

Control group means from the end-line data collected in this study shed light on how citizens typically complete the application steps in practice. First, although multiple official avenues exist, 60% of the women who obtained an application form report getting it from the MLA office, and another 13% report getting it from a family member or neighbor (who likely got it from the MLA office). Only 6% of women in the control group say they got the form from the district office, and none report getting it online<sup>12</sup>. Of the women who filled out their forms, only 1% said they did it on their own, and only 5% report having to pay someone to fill it out for them<sup>13</sup>. On the other hand, 45% report a family member or neighbor filled it for them and 32% said they received help from the MLA office. Thus we see significant reliance on social and political networks to fulfill this step of the application process, and the use of non-state, paid middlemen is much less widespread than might be assumed.

Women must also travel to both the MLA and district offices in order to apply, which imposes time and financial costs. Table 1.1 highlights these costs for women in the control group who attempted the relevant application steps<sup>14</sup>. On average, a woman takes 2 trips to the MLA for application purposes, and each trip costs about Rs. 40 and takes 2 hours. Each trip to the district office, which tends to be much further than the local MLA office, requires a total of 3.7 hours and costs Rs. 140 round trip. In total, women spend on average 10.5 hours and Rs. 325 (about \$5.00) completing (or attempting) the MLA and district office steps of the application. While these are not negligible time and financial costs, they should not theoretically suffice to deter take-up of a scheme that provides a payout of Rs. 1,500 per month for life, unless women are severely credit constrained or risk averse. Baseline data shows that most women have at least one person they can borrow money from

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<sup>12</sup>see Appendix for breakdown of where women in each treatment group report obtaining their application forms.

<sup>13</sup>The average amount paid was about Rs. 850(\$13), with a minimum payment of Rs. 10 and a maximum payment of Rs. 4000.)

<sup>14</sup>see Appendix for breakdown of these costs across all treatment groups.

when necessary. This suggests other indirect costs of engaging with the state or barriers to engagement prevent certain women from completing the application process.

**Table 1.1:** *Time and Financial Costs for Control Group Applicants*

| <b>Measures</b>                                                                                                                                                                                                                                                                                                                                                                                                         |            |
|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|------------|
| Average Number of Visits to MLA                                                                                                                                                                                                                                                                                                                                                                                         | <b>2</b>   |
| Average Roundtrip Cost of Travel to MLA (Rupees)                                                                                                                                                                                                                                                                                                                                                                        | <b>41</b>  |
| Average Roundtrip Time to Travel to MLA (Hours)                                                                                                                                                                                                                                                                                                                                                                         | <b>1.5</b> |
| Average Time Spent at MLA Office (Hours)                                                                                                                                                                                                                                                                                                                                                                                | <b>.52</b> |
| Average Number of Visits to District Office                                                                                                                                                                                                                                                                                                                                                                             | <b>1.8</b> |
| Average Roundtrip Cost of Travel to District Office (Rupees)                                                                                                                                                                                                                                                                                                                                                            | <b>140</b> |
| Average Roundtrip Time to Travel to District Office (Hours)                                                                                                                                                                                                                                                                                                                                                             | <b>2.8</b> |
| Average Time Spent at District Office (Hours)                                                                                                                                                                                                                                                                                                                                                                           | <b>.87</b> |
| <b>Observations</b>                                                                                                                                                                                                                                                                                                                                                                                                     | <b>278</b> |
| Notes: This table presents the average amount of time and money women in the control group report that they spent in completing the MLA signature and district office submission requirements of the application process. The total at the bottom represents the number of women in this group that completed the End-Line I survey, but not all of these women completed the MLA and district office submission steps. |            |

### 1.2.3 Potential Barriers to Access

As is the case for many welfare entitlement programs, the application process for this scheme includes locating and interacting with both politician and bureaucratic offices as well as filling out a complex application form with several required supplemental materials. It is certainly possible that lack of information regarding the program or application procedures could limit take-up.

Even with information, however, citizens may still struggle to access the scheme. Getting the application form, obtaining the MLA signature, and submitting to the district office all involve a trip to the MLA or district office, which requires time, money, and oftentimes for this population, an available family member or friend to accompany. Filling out the

application form is another potential barrier to enrollment given the widespread illiteracy in the eligible population. In addition, women who rely exclusively on social, rather than political, networks may struggle to find someone to fill the form out *correctly*. Finally, the vast and stringent documentation requirements can be difficult to fulfill. Getting many of these documents requires a separate application process which comes with its own ordeals. Often the marital proof itself is the most difficult document to obtain<sup>15</sup>. In fact, in the survey data gathered by the World Bank, citizens indicate that obtaining and filling out application forms as well as fulfilling documentation requirements are important challenges in gaining access to social pensions (WB, 2014). Similarly, ex-post I find that less than 1% of the women surveyed in this experiment report filling the form on their own and many do not make it through the application because of document constraints.

There is also a politicized component to this application process in which citizens must receive the sign-off of their elected politician in order to successfully enroll in the scheme. This step can conceivably deter citizens lacking some degree of political connections with the MLA from undergoing the application process, especially women who have suddenly lost their spouse and are not accustomed to being the household member to make claims on the state (Prillaman, 2016). In addition, certain women might lack the freedom of mobility, the confidence, or the know-how to approach a politician's office and ask for a signature.

Taken together, the several layers of ordeals built into the application process suggests citizens may find it difficult to get through even once they have information. The field study described below aims to disentangle the need for better information alone from that of information with additional application assistance.

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<sup>15</sup>Our extensive fieldwork shows that obtaining a death certificate is very difficult if the cremation slip or burial documentation is lost. Obtaining a divorce certificate requires the divorce to be first legally passed in court, which is a lengthy process, especially for Muslim households that observe verbally sanctioned divorces. Providing proof of separation or abandonment requires cooperation from the police, which is difficult for an average citizen, let alone poor and marginalized women.

## 1.3 Research Design

### 1.3.1 Sample Selection and Randomization

To identify the study sample, I conducted a rapid screening exercise and census survey from July-August 2015 in 75 medium to large sized slum areas spanning 7 of the 10 administrative districts of Delhi. Every attempt was made to identify all widows, divorced, separated, or abandoned women who were not visibly much older than age 60. Once identified, women were rapidly screened for pension eligibility and then administered a census survey if eligible and not already enrolled in the pension scheme. A total of 4,347 women were screened, of which 82% were widows, 9% were abandoned, 5% were separated, and 4% were divorcees. Among the eligible women, 40% reported being enrolled in the pension scheme (comparable to the 34% enrollment rate found by the World Bank in 2014). Notably, this enrollment figure was actually 50% among widows and only 2.6% among non-widows who potentially did know the scheme applied to them.

Screening out women who were ineligible<sup>16</sup> or already receiving pensions, a short census survey was administered to the remaining 2,538 women. The data collected included basic demographic characteristics, address, household members' information, income, assets, reproductive history, and possession of government documents. Based on the project scope and purpose, we excluded women who did not have their marital proof or other difficult to obtain documents and did not seem likely to get them over the next 3-6 months. This left 1,420 women, of which 1,393 were selected for the study sample after dropping women whose self-reported income was too high.

Once the study sample was identified, women were randomly divided into 4 equal-sized groups, comprising of one control group and three treatment groups, detailed below. The randomization was done at the individual level, stratifying by slum.

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<sup>16</sup>This includes those whose length of stay in Delhi had not reached five years, those who are above the age of 60, and those who already receive income support from their husband's previous job, such as the wives of government employees.

### 1.3.2 Intervention

I employ three interventions designed to identify barriers that are driving low take-up of this program. Specifically, the aim is to separate the role played by an information deficit from that played by transaction costs in which citizens struggle to overcome application ordeals even with information. The ordeals targeted are those likely to impose high utility costs given the characteristics of eligible women, including obtaining and filling out the application form and going to get the MLA signature on the application <sup>17</sup>. To measure the impact of randomly reducing these informational and transactional barriers, different groups of women received the following interventions.

The first intervention group receives only information. This includes detailed information about the pension scheme, including the benefits, eligibility criteria, documentation requirements, how to open a bank account, and the locations/timings of the local MLA and district offices. This information is read out-loud to the respondent and also provided in written form for future reference.

In the second intervention group, called the *basic mediation* group, women receive the same information as the first group, but in addition, we provide them with an application form and fill it out for them. We do *not* assist with gathering or making photocopies of the required documents or with subsequent application steps.

The third group, or *intensive mediation* group, receives the same information and assistance given to the first two groups, and in addition, we offer to accompany the respondent to the MLA office for a signature. We make an appointment to take them to the MLA 3 days after the initial intervention of giving them information and filling out their form. We remind them to have all of their documents and photocopies on hand at the time of the appointment. In addition to making photocopies, this three day window gives women

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<sup>17</sup>The difficulty of providing all required documents for the application is an important barrier that is not explicitly addressed in the experimental design. It would not be possible to maintain the internal validity of the study by offering different types of documentation assistance on an individual basis by adjusting to the personal documentation constraints of different women. The information provided to women in all treatment groups, however, clearly explains which documents are required to apply and how to obtain each document, when applicable.

without a bank account (only 15% of the sample at baseline) an opportunity to open one before going to the MLA. After receiving the MLA signature, the women are on their own to submit the form at the district office.

Table 1.2 provides summary statistics and balance checks of census data across the treatment arms. The treatment groups are balanced across all tested indicators at the 5% significance level, and it is important to note the low literacy levels, low employment rates, low income levels, and large number of potentially dependent children among the sample of women. Women on average have lost a monthly income of Rs. 5,539 that used to come from their spouse, and the pension amount of Rs. 1,500 per month (now Rs. 2,500 per month), can make up for 25% (now 45%) of these lost wages. Some women did report applying to the pension scheme previously at the time of the census survey, but as we did not capture how far they got in the application process and whether their application was accepted at the district or not, we included these women in our sample as they could potentially still benefit from our interventions<sup>18</sup>.

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<sup>18</sup>We did not want to have an information effect of the census survey itself by asking detailed questions about the widow pension scheme and application procedures. Thus the questions asked about widow pensions were the same as those asked about all the other documents/services that were captured in the census survey.

**Table 1.2:** Balance Check for Census Sample

| Balance Variables                  | Control             | (T1)<br>Infor-<br>mation | (T2)<br>Basic<br>Mediation | (T3)<br>Intensive<br>Mediation | Joint<br>F-Test<br>p-value |
|------------------------------------|---------------------|--------------------------|----------------------------|--------------------------------|----------------------------|
| Age (yrs.)                         | 40.71<br>(0.55)     | 41.33<br>(0.52)          | 41.51<br>(0.57)            | 39.96<br>(0.54)                | 0.16                       |
| Literate (%)                       | 0.31<br>(0.03)      | 0.25<br>(0.02)           | 0.26<br>(0.02)             | 0.32<br>(0.02)                 | 0.08*                      |
| Muslim (%)                         | 0.27<br>(0.02)      | 0.29<br>(0.02)           | 0.27<br>(0.02)             | 0.32<br>(0.02)                 | 0.43                       |
| Non-Widows (%)                     | 0.22<br>(0.02)      | 0.18<br>(0.02)           | 0.21<br>(0.02)             | 0.22<br>(0.02)                 | 0.45                       |
| Household Size                     | 4.61<br>(0.13)      | 4.72<br>(0.12)           | 4.55<br>(0.12)             | 4.66<br>(0.13)                 | 0.79                       |
| Children Birthed                   | 4.02<br>(0.14)      | 4.01<br>(0.13)           | 4.01<br>(0.13)             | 3.62<br>(0.13)                 | 0.07*                      |
| Rent Home (%)                      | 0.10<br>(0.02)      | 0.10<br>(0.02)           | 0.13<br>(0.02)             | 0.12<br>(0.02)                 | 0.59                       |
| Years in Delhi                     | 28.09<br>(0.56)     | 27.15<br>(0.55)          | 28.73<br>(0.59)            | 27.76<br>(0.53)                | 0.24                       |
| No Personal Income (%)             | 0.50<br>(0.03)      | 0.50<br>(0.03)           | 0.50<br>(0.03)             | 0.51<br>(0.03)                 | 0.99                       |
| Monthly Income (Rs.)               | 1601.06<br>(111.40) | 1591.55<br>(112.99)      | 1591.89<br>(105.74)        | 1498.98<br>(99.94)             | 0.89                       |
| Spouse's Monthly Income (Rs.)      | 5360.96<br>(348.12) | 5558.66<br>(292.40)      | 5635.13<br>(354.19)        | 5594.69<br>(288.36)            | 0.94                       |
| Lives with Parents/In-Laws (%)     | 0.20<br>(0.02)      | 0.19<br>(0.02)           | 0.19<br>(0.02)             | 0.24<br>(0.02)                 | 0.19                       |
| Eligible > 1 Year (%)              | 0.36<br>(0.03)      | 0.39<br>(0.03)           | 0.36<br>(0.03)             | 0.36<br>(0.02)                 | 0.84                       |
| Previously Applied for Pension (%) | 0.22<br>(0.02)      | 0.22<br>(0.02)           | 0.20<br>(0.02)             | 0.23<br>(0.02)                 | 0.84                       |
| <b>Observations</b>                | <b>330</b>          | <b>355</b>               | <b>339</b>                 | <b>369</b>                     |                            |

Notes: This table compares average characteristics of the women in the sample across the control and treatment groups. Column 1 presents the overall mean for the entire sample whereas Columns 2, 3, 4, and 5 present means for the control group, treatment 1 group, treatment 2 group, and treatment 3 group, respectively. Column 6 presents the p-value for the joint F-test on whether any of the average values listed in columns 2-5 are statistically different from one another. \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

### 1.3.3 Data Collection

In addition to the census survey, the data used in this study came from one baseline survey, one intervention survey, two end-line surveys, one qualitative module, and administrative data on pension beneficiaries. Below are the details of each round of data collection,

including the time-line and number of women reached<sup>19</sup>. Table 1.3 summarizes the treatment design, data collection time-line, and sample size. Attrition between survey rounds was balanced across treatment groups, and reasons for attrition are provided in the Appendix. All survey data was collected on tablets, and only female surveyors conducted the baseline and qualitative questionnaire which includes potentially sensitive questions.

**Table 1.3: Research Design and Timeline**

| <b>Group</b>   | <b>Intervention</b>                      | <b>Census Sample</b> | <b>Baseline &amp; Intervention</b> | <b>End-line I</b> | <b>End-Line II</b> |
|----------------|------------------------------------------|----------------------|------------------------------------|-------------------|--------------------|
|                |                                          | July-Aug 2015        | Sept-Nov 2015                      | Feb-April 2016    | Aug-Oct 2016       |
| <b>Control</b> | None                                     | 330                  | 292                                | 278               | 256                |
| <b>T1</b>      | Information                              | 355                  | 322                                | 308               | 296                |
| <b>T2</b>      | Information + Filling Application Form   | 339                  | 308                                | 298               | 281                |
| <b>T3</b>      | Information + Filling Form + Take to MLA | 369                  | 335                                | 315               | 298                |
| <b>Total</b>   |                                          | <b>1393</b>          | <b>1257</b>                        | <b>1199</b>       | <b>1131</b>        |

- **Baseline & Intervention (September - November 2015, n = 1257)**

The baseline survey asks questions about family background; consumption patterns; borrowing/lending; previous experience with government programs; political profile; decision-making authority; personal autonomy; mental, social and physical health; and cognitive capacity. Immediately after the baseline survey is administered, the respondent receives an intervention based on her treatment assignment<sup>20</sup>. After the intervention, we administer a brief intervention survey to capture which components of the intervention the respondent complied with and which documents she had.

- **End-Line I (February - April 2016, n = 1199)**

This survey captures detailed information about the respondent's application experience and steps completed. It also asks about treatment spillovers to neighbors, stigma with receiving welfare, usage of bank accounts, and final acceptance rates in to the

<sup>19</sup>All data collection and intervention activities were conducted by surveyors and field staff hired by J-PAL South Asia and trained by the senior research team, including the principal investigator.

<sup>20</sup>The surveyor did not know until the end of the baseline survey which treatment the respondent would be receiving, so biased data collection in relation to treatment assignment is not a concern.

pension program.

- **End-Line II (August - October 2016, n = 1131)**

This brief second end-line was conducted to recapture the final pension outcome<sup>21</sup> and also document what steps women completed in the application process in the longer run.

- **Administrative Pension Data**

To have an objective measure of whether or not the women in our sample are enrolled in the pension scheme, I scraped the Delhi government's widow pension website to obtain the official list of beneficiaries as of October 2016. Given the nature of the transfer, a lumpsum every three months, it is possible that newly enrolled women take some time to realize they are successfully enrolled due to low frequency of bank visits and the slightly arbitrary timing of the first transfer relative to these visits. Therefore, matching administrative data from October 2016, which aligns with the timing of the second end-line survey, with our sample data allowed for constructing a better measure of the actual pension outcome than what was captured by survey data alone.<sup>22</sup>.

- **Qualitative Data (June-July 2017, n=45)**

In order to get a more detailed and nuanced account of the application process, 45 women were randomly selected to participate in in-depth interviews, and the sample consisted of women who did not try to apply, those who tried but failed, and those who tried and were successful. The questions ask about personal attitudes towards and experiences with applying for government schemes, constraints with fulfilling application requirements including household level factors, perceptions of the assistance offered in our experiment, and benefits (perceived or actual) from receiving

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<sup>21</sup>At the time of the first end-line, many women who had applied for pensions post-intervention still did not know the outcome of their application.

<sup>22</sup>See Appendix for details of how our sample data was matched with administrative data and decisions taken in cases of ambiguity.

this particular pension.

Table 1.4 presents balance checks and descriptive statistics from the baseline survey. While almost all of the women voted in the last election, a majority report dissatisfaction with the government and believe that women cannot approach the government as easily as males. There are many women who report the MLA is the first person they turn to when they need assistance with public services, suggesting some have political connections; whereas there is also a minority of women who report never having been to a government office in their entire life. In terms of empowerment within the household, about a quarter of women are not involved in the majority of financial decisions of the household, and almost half lack some degree of autonomy, measured by requiring permission to do basic activities<sup>23</sup>. Finally, the financial hardships that could potentially be alleviated with a cash transfer are notable, as a majority of women face income and food insecurity, are unable to afford health care, and report anxiety about the future.

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<sup>23</sup>See Appendix for construction of financial decision making and autonomy variables.

**Table 1.4: Balance Check for Baseline Sample**

| <b>Balance Variables</b>              | <b>Control</b> | <b>T1</b>      | <b>T2</b>      | <b>T3</b>      | <b>Joint F-Test<br/>p-value</b> |
|---------------------------------------|----------------|----------------|----------------|----------------|---------------------------------|
| Voted (%)                             | 0.89<br>(0.02) | 0.87<br>(0.02) | 0.88<br>(0.02) | 0.89<br>(0.01) | 0.41                            |
| Satisfied with Govt. (%)              | 0.45<br>(0.03) | 0.45<br>(0.03) | 0.43<br>(0.03) | 0.44<br>(0.01) | 0.84                            |
| Turn to MLA for Services (%)          | 0.54<br>(0.03) | 0.55<br>(0.03) | 0.55<br>(0.03) | 0.54<br>(0.01) | 0.83                            |
| Women can Easily Approach Govt. (%)   | 0.49<br>(0.03) | 0.48<br>(0.03) | 0.45<br>(0.03) | 0.47<br>(0.01) | 0.76                            |
| Never Been to Govt. Office (%)        | 0.18<br>(0.02) | 0.19<br>(0.02) | 0.16<br>(0.02) | 0.17<br>(0.01) | 0.47                            |
| Make Fewer HH Financial Decisions (%) | 0.24<br>(0.02) | 0.25<br>(0.02) | 0.28<br>(0.02) | 0.26<br>(0.01) | 0.58                            |
| Lack Autonomy in HH (%)               | 0.46<br>(0.03) | 0.47<br>(0.03) | 0.48<br>(0.03) | 0.47<br>(0.01) | 0.95                            |
| No Meal Entire Day (%)                | 0.57<br>(0.03) | 0.56<br>(0.03) | 0.53<br>(0.03) | 0.55<br>(0.01) | 0.74                            |
| Can't Afford Healthcare (%)           | 0.74<br>(0.02) | 0.79<br>(0.02) | 0.74<br>(0.02) | 0.76<br>(0.01) | 0.38                            |
| Anxious about Future (%)              | 0.67<br>(0.03) | 0.70<br>(0.03) | 0.72<br>(0.02) | 0.70<br>(0.01) | 0.63                            |
| <b>Observations</b>                   | <b>292</b>     | <b>322</b>     | <b>308</b>     | <b>335</b>     |                                 |

Notes: This table compares average characteristics of the women who completed the baseline survey across the control and treatment groups. Column 1 presents the overall mean for the baseline sample whereas Columns 2, 3, 4, and 5 present means for the control group, treatment 1 group, treatment 2 group, and treatment 3 group, respectively. Column 6 presents the p-value for the joint F-test on whether any of the average values listed in columns 2-5 are statistically different from one another. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

### 1.3.4 Compliance

Compliance, or take-up of treatment, varied across the different treatment arms of the study. One notable barrier to compliance was that 20% of treated women, despite efforts to screen out such cases from inclusion in the sample, were already receiving pensions at the time of the baseline and intervention<sup>24</sup>. This does not change the interpretation of the results but

<sup>24</sup>Unfortunately, we do not have the same baseline level data on what percentage of women in the control group were getting pensions already as this was collected in the intervention survey. However, given balance across treatment and control groups on census level and baseline level characteristics (reported in Tables 2 and 4), there is no reason to believe the control group also had a significantly different rate of baseline pensioners.

simply reduces our power to detect treatment impacts.

For women in the pure information group, 90% complied by listening to the information we provided on the pension scheme and accepting the printed handout. Of those who did not comply with this treatment, 96% were already getting pensions at baseline and thus were uninterested. In the second treatment group, compliance with the information portion of the treatment was 93%, and compliance with allowing us to fill the application form was 75%. Of the women who declined our assistance with the application form, 42% were already getting pensions, 24% refused due to document constraints, and 16% already had their applications accepted at the district office and were awaiting their pension result.

In the third and most intensive treatment group, compliance with the information portion of the treatment was 91%, and compliance with the form assistance was 71%. Compliance with the final stage of the treatment, defined as actually coming with us to the MLA office to obtain a signature, was 28%. While this is relatively low compliance, it is understandable given the nature of this final stage. In order to go with us, the respondent would have to be prepared at the time of her appointment with all of her documents and willing to spend that specific afternoon going to the politician's office. Several women who were given appointments simply were not ready with their documents, photocopies, or bank account information at the time of the appointment. Others were busy and/or said they would go on their own as they had a neighbor or family member available to accompany them if needed. Of the women who did come with us for the MLA signature, 76% were successful in obtaining it immediately, whereas others were turned away for various reasons or asked to make modifications and return.

### **1.3.5 Estimation**

The results report intention-to-treat (ITT) estimates, which compares average outcomes in the treatment and control groups. All outcomes are estimated at the individual level. The estimation includes slum-fixed effects (randomization was stratified at the slum-level). Thus,

the basic equation I estimate is:

$$Y_{is} = B_0 + B_1T1_{is} + B_2T2_{is} + B_3T3_{is} + n_s + e_{is} \quad (1.1)$$

where  $Y_{is}$  is an outcome for individual  $i$  living in slum  $s$  and  $T_1, T_2, T_3$  are treatment dummies for the three different intervention groups.  $n_s$  represents slum fixed effects, and  $e_{is}$  is a robust error term. In the heterogeneity analysis, I estimate:

$$Y_{is} = B_0 + \sum_{n=1}^3 B_n Tn_{it} + B_4T1_{is} * X_{is} + B_5T2_{is} * X_{is} + B_6T3_{is} * X_{is} + B_7X_{is} + n_s + e_{is} \quad (1.2)$$

where  $X_{is}$  is the individual characteristic of interest for person  $i$  living in slum  $s$ .

## 1.4 Results

### 1.4.1 Effects on Knowledge Levels

As all of the treatment arms included an information component, a primary outcome of interest is whether or not women in the treatment groups could more accurately report details of the pension scheme and application process than women in the control group at the time of End-Line I (knowledge questions were not asked in the much shorter End-Line II questionnaire.). In addition, to make the case that application assistance matters over and above the role of information, it is important to show that information on its own increases knowledge levels to the same extent as the other interventions.

Whereas all but three women in the entire sample report knowledge of the scheme's existence, Table 1.5 presents the pooled and individual treatment impact on knowledge of specific program details. The results indicate that there is widespread knowledge among all women that the pension money is dispersed at the bank (column 4), but women in the pooled treatment group are 7 percentage points more likely to correctly report the pension amount awarded each month (column 1). In addition, belonging to a treatment group increases chances of knowing the MLA has to sign the application form by 8 percentage

points and that the form must be finally submitted at the district office by 11 percentage points. Importantly, information on its own significantly raises knowledge levels relative to the control group in all of these dimensions, and providing additional application assistance does not substantially increase knowledge levels of the program beyond what is accomplished from the pure information treatment.

**Table 1.5:** *Knowledge of Pension Scheme at End-Line*

| VARIABLES                | (1)<br>Pension<br>Amount | (2)<br>MLA Has<br>to Sign | (3)<br>Submit at<br>District Office | (4)<br>Collect<br>at Bank |
|--------------------------|--------------------------|---------------------------|-------------------------------------|---------------------------|
| Any Treatment            | 0.067**<br>(0.03)        | 0.077**<br>(0.03)         | 0.111***<br>(0.03)                  | 0.018<br>(0.03)           |
| Information (Info)       | 0.068*<br>(0.040)        | 0.060*<br>(0.035)         | 0.093**<br>(0.040)                  | 0.009<br>(0.030)          |
| Basic Mediation (BM)     | 0.041<br>(0.041)         | 0.076**<br>(0.036)        | 0.114***<br>(0.039)                 | 0.014<br>(0.031)          |
| Intensive Mediation (IM) | 0.091**<br>(0.041)       | 0.094***<br>(0.034)       | 0.126***<br>(0.039)                 | 0.031<br>(0.030)          |
| Observations             | 1,199                    | 1,199                     | 1,199                               | 1,199                     |
| Control Mean             | 0.507                    | 0.730                     | 0.576                               | 0.827                     |
| Info=BM                  | 0.498                    | 0.638                     | 0.577                               | 0.849                     |
| Info=IM                  | 0.550                    | 0.278                     | 0.374                               | 0.431                     |
| BM=IM                    | 0.211                    | 0.565                     | 0.747                               | 0.555                     |

Notes: This table presents pooled and individual impact on knowledge of pension scheme based on survey responses to questions asked in the End-Line I survey. *Information, Basic Mediation, and Intensive Mediation* are dummy variables that take a value of 1 if the respondent belongs to treatment group 1, treatment group 2, or treatment group 3, respectively. The dependent variables in Columns 1-4 are dummy variables capturing whether the respondent knows the amount of money pension beneficiaries receive each month from the government, that the MLA is the entity that must sign off on the application form, that the form must finally be submitted at the district office, and that the pension money is dispersed at the bank, respectively. All regressions include slum fixed-effects. Robust standard errors in brackets.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

These results show that women did have gaps in their knowledge which were filled by better information, suggesting that lack of specific information about the program benefits and application procedures, despite widespread general knowledge of its existence, could be a salient barrier to take-up. Whether or not providing this information is sufficient to increase enrollment, however, remains an important question which is addressed below.

### 1.4.2 Effects on Application Steps Completed

The main analysis focuses on the fraction of women in each treatment group that completed the given application steps *after* the intervention period given that a notable subset were already receiving pensions at baseline<sup>25</sup>. In addition, the preferred specification considers results at the time of End-Line I, as the End-Line II survey measured fewer outcomes, was completed by fewer women, and also captures a potential treatment impact of participating in the End-Line I survey itself. As the End-Line I survey asked very detailed questions about take-up of the pension program and completion of all application steps, it could have served as an indirect information treatment for the control group<sup>26</sup>. Thus, End-Line I results are better suited to capture the true impact of the interventions.

Table 1.6 displays both the pooled treatment effect and the individual treatment effect on application steps completed after the intervention period at the time of End-Line I. In comparison to women in the control group, women who received any treatment were more than twice as likely to obtain an application form and almost 50% more likely to complete all steps of the process by finally having their application accepted at the district office (Columns 1 and 6)<sup>27</sup>. Thus, as a first pass, the data suggests there is room to increase enrollment in targeted social programs in this context by improving access to information and reducing the burden of ordeals via application assistance.

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<sup>25</sup>Tables showing what fraction of women in each treatment group have *ever* completed the application steps are found in the Appendix. The results are similar but smaller in magnitude due to dilution of the sample with baseline pensioners.

<sup>26</sup>Tables showing results at the time of End-Line II are included in the Appendix. The results are similar, but the control means notably higher than at the time of End-Line I and women who received the information treatment are now less distinguishable from the control group.

<sup>27</sup>Once the form is accepted at the district office, it is out of the hands of the applicant to move the process forward, and it is usually just a matter of processing time before the pension money starts coming to the applicant's bank account. A very small percentage of applicants who have their forms accepted will not eventually get pensions due to some error in their application that was not noticed at the time of its submission.

**Table 1.6: Balance Check for Baseline Sample**

| VARIABLES                | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign  | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|--------------------------|-------------------------|-----------------------|-----------------------|---------------------|----------------------------|---------------------------|
| Any Treatment            | 0.237***<br>(0.031)     | 0.226***<br>(0.030)   | 0.165***<br>(0.029)   | 0.109***<br>(0.028) | 0.094***<br>(0.028)        | 0.070***<br>(0.026)       |
| Information (Info)       | 0.116***<br>(0.037)     | 0.106***<br>(0.037)   | 0.088**<br>(0.035)    | 0.057*<br>(0.034)   | 0.058*<br>(0.034)          | 0.040<br>(0.031)          |
| Basic Mediation (BM)     | 0.261***<br>(0.039)     | 0.254***<br>(0.039)   | 0.161***<br>(0.037)   | 0.097***<br>(0.036) | 0.077**<br>(0.035)         | 0.062*<br>(0.032)         |
| Intensive Mediation (IM) | 0.331***<br>(0.038)     | 0.316***<br>(0.038)   | 0.245***<br>(0.037)   | 0.171***<br>(0.036) | 0.145***<br>(0.035)        | 0.106***<br>(0.033)       |
| Observations             | 1,199                   | 1,199                 | 1,199                 | 1,199               | 1,199                      | 1,199                     |
| Control Mean             | 0.227                   | 0.223                 | 0.194                 | 0.187               | 0.176                      | 0.151                     |
| Info = BM                | 0.000                   | 0.000                 | 0.051                 | 0.255               | 0.583                      | 0.501                     |
| Info = IM                | 0.000                   | 0.000                 | 0.000                 | 0.002               | 0.014                      | 0.046                     |
| BM = IM                  | 0.081                   | 0.126                 | 0.035                 | 0.048               | 0.063                      | 0.197                     |

Notes: This table presents pooled and individual treatment impacts on steps completed in the application process at the time of End-Line I by those who applied *after* the intervention. *Information, Basic Mediation, and Intensive Mediation* are dummy variables that take a value of 1 if the respondent belongs to treatment group 1, treatment group 2, or treatment group 3, respectively. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectively. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment is statistically equal to the impact of another individual treatment. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 1.6 also shows the differential impacts of individual treatments. While information may be necessary to begin the application process, it is not sufficient on its own to significantly increase the rate of completed applications, measured by having forms accepted at the district office (Column 6). On the other hand, both basic and intensive mediation have a fairly large and significant impact on helping women make it all the way through the application process, suggesting both of these interventions are addressing salient barriers citizens face in enrollment beyond what better information can overcome. The basic mediation treatment leads to a 41% increase in completed applications, and the intensive mediation treatment leads to a 70% increase, representing a 6.2 and 10.6 percentage point increase from the control group mean, respectively. Because we excluded women from the

sample with obvious document constraints, these results can be considered a lower bound if we think women with document constraints would benefit even more from information and application assistance than women who at baseline had more of their documentation requirements already sorted.

The parameters at the bottom of Table 1.6 show how the treatment impacts compare against one other. At the stage of obtaining and filling out the application form, both of the mediation treatments have a larger and statistically distinguishable impact from that of information alone. While this is in some ways a mechanical result, it suggests that providing assistance to help citizens overcome these initial ordeals does increase the likelihood that these steps will be completed beyond the increase accomplished by providing information alone. Thus, citizens may be information constrained, but they are not *only* information constrained.

In terms of attempting the MLA signature, women who receive basic mediation, or information plus assistance with obtaining and filling out the form, do go on to complete this step at higher rates than women in the control group and women who receive only information. This behavior is line with predictions that opening channels of access by helping citizens start a process or get over initial barriers can facilitate completion of subsequent tasks. However, beyond this step, while the basic mediation treatment continues to significantly improve outcomes relative to the control group, it is no longer possible to distinguish between basic mediation and information alone. This could partially reflect constraints in statistical power, but likely also reflects the difficulty of overcoming subsequent application ordeals even once women have had their forms filled.

The impact of intensive mediation, on the other hand, is statistically distinguishable from that of only information at the 5% level for all steps of the application. This clearly shows that the ordeal of getting the politician signature limits take-up even once information on how and where to get this signature is provided. Furthermore, while many women who get their forms filled do not attempt or receive the MLA signature, once women get over the hump of receiving the signature, most do submit to district office without our assistance.

To further illustrate, the probability that a woman will go on to have her form accepted at the district office jumps from 52% once her form is filled to 76% once she obtains an MLA signature. But this probability only increases to 83% once she attempts a district office submission. This reflects that certain ordeals are more limiting than others, and the stage at which mediation is provided matters.

Why is the MLA signature step such an important bottleneck in the application process? At first glance, we may think this suggests a corruption story in which the MLA selectively gives a signature to political supporters or for a bribe. However, women surveyed in the end-line overwhelmingly report that the reason they failed to get the MLA signature was due to documentation issues, and there is no evidence of bribery or extortion. Because the MLA signature step is the first time citizens have their documents checked, this step selects out women with missing, erroneous, or incomplete documents. This suggests that the stringent document requirements, an ordeal not directly targeted by my interventions, play an important role in limiting take-up. Furthermore, given that those who do get the MLA signature go on to have their forms accepted by the bureaucracy adds credibility to the administrative screening function rather than giving out signatures only to supporters regardless of application quality.

Still, documentation constraints notwithstanding, we see a large impact of accompanying women to the MLA office on application completion rates, suggesting that this ordeal itself limits take-up to some extent. The heterogeneity analysis discussed in Section 4.4 helps shed light on what kind of women are particularly burdened by having to go the MLA office for sign-off.

Finally, if the primary reason for not obtaining an MLA signature or submitting to the district was that women face credit constraints in the face of travel costs of going to these offices, we should not see the results seen in Table 1.6. None of our treatments, including the intensive mediation treatment, changed or subsidized the travel costs, yet the interventions still mobilized many more women to go to the MLA and district offices to complete the application. This suggests that the price of travel does not deter them, but perhaps just

the thought of going alone or interacting with the government plays an important role. In addition, women may be more likely to use their savings or borrow money required for the fare when they feel more certainty of a positive outcome, which is likely facilitated by all of our treatments to some extent and increases with the level of support offered.

### 1.4.3 Effects on Getting Pensions

Women in the treatment groups not only succeeded in *applying* for pensions at higher rates than women in the control group, but eventually they were also *enrolled* in the pension scheme at higher rates. Table 1.7 shows the pension outcome results for the pooled and individual treatment groups (again showing outcomes for women who had applied post-intervention at the time of End-Line I<sup>28</sup>). In column 1, the pension outcome is measured using End-Line I data only. Due to low levels of reporting of getting pensions in both the End-Line I and End-Line II surveys despite high rates of successfully submitted applications, administrative data was relied upon to get more accurate measures of the pension outcome. Column 2 shows the results based purely on administrative data, and column 3 shows the results when the pension outcome is measured using a combination of administrative and End-Line II survey data, which is the preferred specification due to constraints of both data sources on their own<sup>29</sup>. This outcome essentially counts a woman as a pension recipient if she reported being so in the survey data *or* she matched in the pension beneficiary list provided in the administrative data online.

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<sup>28</sup>See Appendix for other specifications.

<sup>29</sup>The pure administrative data was not properly updated online for the East district, which led to under-reporting of the pension outcome for women in this area, about 20% of our sample. While this error would be balanced across treatment and control groups, it still lowers the true magnitude of the result. Furthermore, women who have moved recently are systematically less likely to match in the administrative data (see Appendix for details), which again weakens the magnitude of the result.

**Table 1.7:** *Pension Outcomes for Women Who Applied Between Intervention and End-Line I*

| VARIABLES                | (1)<br>End-Line I<br>Survey Data | (2)<br>Admin<br>Data | (3)<br>End-Line II Survey<br>+ Admin Data |
|--------------------------|----------------------------------|----------------------|-------------------------------------------|
| Any Treatment            | 0.025*<br>(0.014)                | 0.045**<br>(0.023)   | 0.062**<br>(0.025)                        |
| Information (Info)       | 0.039**<br>(0.019)               | 0.048*<br>(0.028)    | 0.048<br>(0.031)                          |
| Basic Mediation (BM)     | 0.011<br>(0.017)                 | 0.031<br>(0.028)     | 0.066**<br>(0.032)                        |
| Intensive Mediation (IM) | 0.025<br>(0.019)                 | 0.056**<br>(0.028)   | 0.070**<br>(0.032)                        |
| Observations             | 1,199                            | 1,199                | 1,199                                     |
| Control Mean             | 0.040                            | 0.115                | 0.147                                     |
| Info = BM                | 0.154                            | 0.552                | 0.567                                     |
| Info = IM                | 0.519                            | 0.782                | 0.479                                     |
| BM = IM                  | 0.448                            | 0.386                | 0.902                                     |

Notes: This table presents pooled and individual treatment impacts on final enrollment in the pension scheme by those who applied *after* the intervention and by End-Line I. *Information*, *Basic Mediation*, and *Intensive Mediation* are dummy variables that take a value of 1 if the respondent belongs to treatment group 1, treatment group 2, or treatment group 3, respectively. The dependent variable in Column 1 is dummy variable that takes a value of 1 if the respondent reported receiving pensions at End-Line I. The dependent variable in Column 2 is a dummy variable that takes a value of 1 if the respondent appeared in the official administrative list of pension beneficiaries published online in October 2016. The dependent variable in Column 3 is a dummy variable that takes a value of 1 if the respondent reported receiving pensions by the time of End-Line II survey or appeared in the official administrative list of pension beneficiaries published online in October 2016. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment is statistically equal to the impact of another individual treatment. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Pooling across all treatments, the interventions increased the number of pension beneficiaries by 6.2 percentage points, or 42% of the control mean (Column 3, Table 1.7). The magnitude of impact is almost identical for the women in T2 and T3, at 6.6 percentage points and 7.0 percentage points, respectively, and smaller and insignificant for the T1 group at 4.8 percentage points. These magnitudes are statistically indistinguishable across the three treatment groups, most likely due to being under-powered to detect small differences in outcomes. Still, the individual treatment impacts on the pension outcome closely match

the impact on application acceptance rates at the district office (Column 6, Table 1.6).

It is also important to acknowledge that even with extensive assistance, overall enrollment in the scheme remains relatively low. The pooled treatment mean for the pension outcome, regardless of application timing, is only 47.5% one year after the interventions (See Appendix). Survey data indicates that documentation constraints severely limit take-up, a stage where we did not offer mediation. Given the salience of this ordeal, providing assistance with documents may have led to much higher take-up rates, although many document problems facing the women in our sample are not easily solved. Thus, even though we don't see evidence of corruption in this program, there certainly is red tape that serves to limit and distort allocation rather than serve only as a dead-weight loss (Banerjee et al., 2013). At the same time, while the interventions had moderate average impact, some particular groups of women saw substantial magnitude shifts in their enrollment, suggesting the composition of beneficiaries itself changed in important ways with these treatments, as detailed in the next section.

#### **1.4.4 Heterogeneity of Impacts**

Average treatment effects highlight the importance of information barriers and application ordeals in limiting take-up of this pension scheme. Next, I test for heterogeneous results along the individual characteristics that were targeted by the design of the interventions. In particular, I look at differential treatment impacts based on literacy, political connections, and personal autonomy, as these characteristics may determine ability to utilize information and fulfill application requirements. These variables are minimally (sometimes negatively) correlated with one another, suggesting they are capturing different dimensions of vulnerability<sup>30</sup>.

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<sup>30</sup>The correlation between being illiterate and being less connected to the MLA is -0.06. The correlation between being illiterate and lacking autonomy in the household is -0.13. The correlation between being less connected to the MLA and lacking autonomy in the household is 0.05.

## Literacy

Seventy percent of the women in my sample, which is arguably representative of eligible women in Delhi, are illiterate. This could be an important factor that prevents citizens from completing the pension applications independently. Table 1.8 shows results from the interaction of literacy with the interventions. For women who only received information, the subset that was *literate* completed all steps of the application at substantially higher rates than illiterate women in this group, eventually having their forms accepted at the district office at 14 percentage point higher rate than illiterate women (column 6). In fact, *only* literate women benefited significantly from the pure information treatment, implying they are the only ones who could utilize the information without any other assistance. On one hand, it is reassuring to find that simply better information can help at least some subset of eligible citizens access social safety nets. On the other hand, given that the majority of eligible women are illiterate, this finding also suggests that better information is insufficient to help the majority of deserving citizens access entitlement programs in such a context without further assistance.

**Table 1.8:** Steps Completed Post-Intervention by End-Line I, Interaction with Illiteracy

| VARIABLES                | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign   | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|--------------------------|-------------------------|-----------------------|-----------------------|----------------------|----------------------------|---------------------------|
| Information (Info)       | 0.341***<br>(0.071)     | 0.324***<br>(0.071)   | 0.219***<br>(0.068)   | 0.215***<br>(0.067)  | 0.200***<br>(0.065)        | 0.140**<br>(0.061)        |
| Basic Mediation (BM)     | 0.332***<br>(0.070)     | 0.322***<br>(0.070)   | 0.197***<br>(0.067)   | 0.153**<br>(0.063)   | 0.134**<br>(0.061)         | 0.082<br>(0.057)          |
| Intensive Mediation (IM) | 0.521***<br>(0.060)     | 0.495***<br>(0.061)   | 0.387***<br>(0.063)   | 0.329***<br>(0.062)  | 0.279***<br>(0.061)        | 0.210***<br>(0.058)       |
| Illiterate               | 0.120**<br>(0.053)      | 0.115**<br>(0.052)    | 0.086*<br>(0.051)     | 0.109**<br>(0.050)   | 0.091*<br>(0.049)          | 0.064<br>(0.047)          |
| Info x Illiterate        | -0.310***<br>(0.084)    | -0.299***<br>(0.084)  | -0.182**<br>(0.081)   | -0.220***<br>(0.079) | -0.196**<br>(0.077)        | -0.138*<br>(0.072)        |
| BM x Illiterate          | -0.106<br>(0.086)       | -0.102<br>(0.085)     | -0.055<br>(0.081)     | -0.084<br>(0.078)    | -0.083<br>(0.076)          | -0.031<br>(0.070)         |
| IM x Illiterate          | -0.280***<br>(0.078)    | -0.264***<br>(0.078)  | -0.210***<br>(0.079)  | -0.233***<br>(0.078) | -0.197***<br>(0.076)       | -0.152**<br>(0.072)       |
| Observations             | 1,199                   | 1,199                 | 1,199                 | 1,199                | 1,199                      | 1,199                     |
| Control Mean             | 0.227                   | 0.223                 | 0.194                 | 0.187                | 0.176                      | 0.151                     |
| Impact on Illiterate:    |                         |                       |                       |                      |                            |                           |
| Info = BM                | 0.000                   | 0.000                 | 0.0150                | 0.069                | 0.244                      | 0.201                     |
| Info = IM                | 0.000                   | 0.000                 | 0.002                 | 0.015                | 0.057                      | 0.149                     |
| BM = IM                  | 0.765                   | 0.841                 | 0.458                 | 0.535                | 0.470                      | 0.866                     |

Notes: This table presents individual treatment impacts specifically for illiterate women on steps completed in the application process at the time of End-Line I by those who applied *after* the intervention. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment on illiterate women is statistically equal to the impact of another individual treatment on illiterate women by comparing the additive impact of the treatment dummy plus the interaction dummy. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Unlike the pure information treatment, the two mediation treatments are able to help both literate and illiterate women, although a higher magnitude impact is still felt by literate women. This suggests that individual assistance is certainly important for illiterate women, but all else equal, being literate is advantageous to make the most of the mediation that is available<sup>31</sup>.

<sup>31</sup>Table 1.8 shows a positive and significant coefficient on the *Illiterate dummy* for most application steps,

Importantly, there is no stage in the application process at which the impact of intensive mediation on illiterate women is distinguishable from the impact of basic mediation. As can be expected, for illiterate women, getting the form and having it filled seems to be a more relevant barrier to enrollment than attempting the politician's signature, and there is no added value of providing them with mediation at the signature stage. In addition, at the final stage of district submission, the impact of either mediation treatment on illiterate women is no longer distinguishable from the negligible impact of information, suggesting that this may be another stage of the process which is particularly difficult for illiterate women without assistance. This may be because the district office is on average further from the average woman's home than the MLA office and more difficult to find. Qualitative interviews suggest that navigating the journey to the government offices itself can be particularly taxing for illiterate women:

*"For illiterate people it is very difficult. Literate people will know what bus goes where and can read the office address. The uneducated people have to keep asking around."*

### **Political Connections**

Citizens may find it difficult to fulfill application requirements or access mediation when they are generally less plugged in to political networks and lack connections to the political figure involved with administering a given program. In this case, the MLA and his/her office staff play a central role in both informally helping citizens with their forms and officially providing the signoff required to move forward with the application. I create a dummy variable capturing whether a women turns first to her MLA when she needs

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meaning that illiterate women in the control group are more likely to complete the application than their literate counterparts. There are several potential explanations for this, but in particular, it is important to note that literate women in the sample are more likely to also be non-widows, and non-widows in the control group are much less likely to apply for this scheme given their probable lack of awareness that the scheme extends beyond widows to include women who are separated, divorced, or abandoned. As seen in the Appendix tables, non-widows are significantly underrepresented among control group applicants, and when I run the literacy heterogeneity test on the sample omitting non-widows, the coefficient on the illiterate dummy remains positive but smaller in magnitude and no longer significant.

assistance with public services as a proxy for being politically connected with the MLA. In this case, political connectedness does not necessarily reflect partisan affiliation but instead confidence and experience engaging with this political figure. Table 1.9 shows results from the interaction of political connectedness with the interventions.

**Table 1.9:** *Steps Completed Post-Intervention by End-Line I, Interaction with Connections to MLA*

| VARIABLES                    | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign  | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|------------------------------|-------------------------|-----------------------|-----------------------|---------------------|----------------------------|---------------------------|
| Information (Info)           | 0.126**<br>(0.052)      | 0.123**<br>(0.051)    | 0.103**<br>(0.049)    | 0.061<br>(0.048)    | 0.064<br>(0.047)           | 0.031<br>(0.044)          |
| Basic Mediation (BM)         | 0.190***<br>(0.054)     | 0.178***<br>(0.053)   | 0.119**<br>(0.051)    | 0.043<br>(0.048)    | 0.022<br>(0.047)           | -0.011<br>(0.043)         |
| Intensive Mediation (IM)     | 0.285***<br>(0.053)     | 0.288***<br>(0.052)   | 0.215***<br>(0.051)   | 0.132***<br>(0.050) | 0.115**<br>(0.048)         | 0.090*<br>(0.046)         |
| Less Connected to MLA        | -0.016<br>(0.053)       | -0.004<br>(0.052)     | -0.007<br>(0.050)     | -0.031<br>(0.049)   | -0.022<br>(0.048)          | -0.027<br>(0.044)         |
| Info x Less Connected to MLA | -0.023<br>(0.076)       | -0.039<br>(0.076)     | -0.035<br>(0.072)     | -0.013<br>(0.070)   | -0.014<br>(0.069)          | 0.018<br>(0.064)          |
| BM x Less Connected to MLA   | 0.157**<br>(0.080)      | 0.167**<br>(0.079)    | 0.094<br>(0.076)      | 0.119<br>(0.073)    | 0.121*<br>(0.071)          | 0.161**<br>(0.067)        |
| IM x Less Connected to MLA   | 0.101<br>(0.078)        | 0.061<br>(0.078)      | 0.065<br>(0.076)      | 0.085<br>(0.074)    | 0.066<br>(0.072)           | 0.036<br>(0.067)          |
| Observations                 | 1,199                   | 1,199                 | 1,199                 | 1,199               | 1,199                      | 1,199                     |
| Control Mean                 | 0.227                   | 0.223                 | 0.194                 | 0.187               | 0.176                      | 0.151                     |
| Impact on Less Connected:    |                         |                       |                       |                     |                            |                           |
| Info = BM                    | 0.000                   | 0.000                 | 0.011                 | 0.039               | 0.082                      | 0.053                     |
| Info = IM                    | 0.000                   | 0.000                 | 0.000                 | 0.002               | 0.014                      | 0.126                     |
| BM = IM                      | 0.504                   | 0.957                 | 0.257                 | 0.335               | 0.513                      | 0.653                     |

Notes: This table presents individual treatment impacts specifically for less politically connected women on steps completed in the application process at the time of End-Line I by those who applied *after* the intervention. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment on less politically connected women is statistically equal to the impact of another individual treatment on less politically connected women by comparing the additive impact of the treatment dummy plus the interaction dummy. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

First, we see that the impact of providing only information is not conditioned by having political connections, suggesting provision of information is a neutral intervention in this regard. On the other hand, basic mediation has a statistically larger positive impact on less

politically connected women than it does on women with political connections, as seen by the positive coefficients on the interaction term in each stage of the application. Ultimately, women less connected with the MLA in the basic mediation group witness a 15 percentage point increase in their acceptance rates at the district office in comparison to these same type of women in the control group. This evidence suggests that political connections are useful for obtaining application forms and getting them filled, which many women do at the MLA office. Women without these connections are underrepresented in the applicant pool relative to what is possible when apolitical avenues for completing these steps are readily available.

In comparing treatment impacts with one another, the parameters at the bottom of Table 1.9 show that the impact of both mediation treatments on less politically connected women is positive and statistically distinguishable from the impact of just pure information on these women across all application steps. Thus, information is less useful to woman lacking political connections than providing them with mediated assistance with the application. In addition, as was the case with illiterate women, the impact of the two mediation treatments on less politically connected women is not distinguishable from one another, meaning there is no added value of providing intensive mediation to these women beyond help with filling the form. Perhaps counter-intuitively, it seems that having political connections may be more important in providing access to the intermediary role played by the MLA's office in helping women fill out their forms than in helping them get the official signature, which may actually be given out on the basis of fulfilling administrative criteria.

These findings suggest that MLAs have inserted themselves within the application process by being the signing off authority not necessarily to provide biased access to their supporters, but to provide local screening and possibly to ensure citizens must rely on them to access government resources. This likely has substantial payoff during election time. Whether we consider this patronage or constituency service, it is evident from the results that their informal assistance does not reach everyone and contributes to a mediation deficit faced by citizens who are less inclined to turn to the MLA for assistance in the first place. While citizens may find other intermediaries to fill the form for them, such as neighbors

and friends, because the MLA is the signing off authority, the form must be filled according to their strict guidelines, making it difficult for those who are not utilizing MLA staff to do it correctly. Due to thorough piloting done before carrying out our interventions, we learned the exact details of how to fill out the form and thus provided a *reliable* substitute to the MLA staff when providing our mediation treatments. Notably, providing this reliable substitute at the stage of form filling allowed less connected women to receive the sign-off from the MLA at the same rate as less connected women who were specifically helped with the sign-off via intensive mediation. This likely reflects that less connected women hesitate to approach politicians' offices to obtain forms or ask for assistance, but once these initial ordeals are overcome, they are more likely to take the step of going to get the MLA signature even without assistance.

### **Household-Level Empowerment**

Increasing work documents the various constraints women face in articulating their rights and responsibilities as citizens in comparison to men, motivating the importance of considering household level factors that might limit women's access to government schemes. To capture household-level empowerment, our baseline survey measured a woman's involvement in financial decision-making of the household and her autonomy to partake in activities within and outside of the household without permission. Women who are left out of decision-making or need permission for simple actions such as going shopping or taking a nap might also find it difficult to take actions to promote their welfare and access state resources. Since these two measures are highly correlated, the analysis focuses on lack of autonomy in the household, which likely has more direct implications for applying to government programs.

Table 1.10 shows the results from the interaction of the treatments with a dummy variable representing women who lack autonomy in the household. One striking finding is that less autonomous women in the control group are 76% less likely to complete the application process than more empowered women in the control group. In addition, all three treatments

have a larger magnitude impact on less autonomous women than more autonomous women. Moreover, for less autonomous women, information and basic mediation have statistically identical impact beyond the first two application steps which are mechanically facilitated by basic mediation. However, the large, positive impact of intensive mediation on less autonomous women remains distinguishable from the impact of pure information and from the impact of basic mediation for all steps of the application process (beyond the first two steps). Less autonomous women who receive intensive mediation see a 17 percentage point increase in their application acceptance rates at the district office relative to their control group counterparts (column 6).

**Table 1.10:** *Steps Completed Post-Intervention by End-Line I, Interaction with Autonomy in the Household*

| VARIABLES                        | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign  | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|----------------------------------|-------------------------|-----------------------|-----------------------|---------------------|----------------------------|---------------------------|
| Information (Info)               | 0.060<br>(0.053)        | 0.066<br>(0.053)      | 0.043<br>(0.051)      | 0.025<br>(0.050)    | 0.031<br>(0.049)           | 0.026<br>(0.045)          |
| Basic Mediation (BM)             | 0.189***<br>(0.056)     | 0.187***<br>(0.055)   | 0.120**<br>(0.053)    | 0.050<br>(0.051)    | 0.047<br>(0.050)           | 0.056<br>(0.047)          |
| Intensive Mediation (IM)         | 0.250***<br>(0.054)     | 0.225***<br>(0.054)   | 0.163***<br>(0.053)   | 0.092*<br>(0.051)   | 0.077<br>(0.050)           | 0.055<br>(0.046)          |
| Lack of Autonomy                 | -0.123**<br>(0.053)     | -0.118**<br>(0.052)   | -0.112**<br>(0.049)   | -0.110**<br>(0.049) | -0.105**<br>(0.047)        | -0.068<br>(0.045)         |
| Info x Lack of Autonomy          | 0.121<br>(0.077)        | 0.086<br>(0.076)      | 0.099<br>(0.072)      | 0.069<br>(0.070)    | 0.059<br>(0.069)           | 0.031<br>(0.065)          |
| BM x Lack of Autonomy            | 0.156**<br>(0.079)      | 0.144*<br>(0.079)     | 0.090<br>(0.075)      | 0.102<br>(0.072)    | 0.067<br>(0.070)           | 0.014<br>(0.066)          |
| IM x Lack of Autonomy            | 0.175**<br>(0.077)      | 0.195**<br>(0.077)    | 0.176**<br>(0.075)    | 0.169**<br>(0.073)  | 0.148**<br>(0.071)         | 0.111*<br>(0.066)         |
| Observations                     | 1,199                   | 1,199                 | 1,199                 | 1,199               | 1,199                      | 1,199                     |
| Control Mean                     | 0.227                   | 0.223                 | 0.194                 | 0.187               | 0.176                      | 0.151                     |
| Impact on Less Autonomous Women: |                         |                       |                       |                     |                            |                           |
| Info = BM                        | 0.005                   | 0.002                 | 0.215                 | 0.263               | 0.634                      | 0.774                     |
| Info = IM                        | 0.000                   | 0.000                 | 0.000                 | 0.002               | 0.009                      | 0.025                     |
| BM = IM                          | 0.173                   | 0.133                 | 0.026                 | 0.049               | 0.038                      | 0.056                     |

Notes: This table presents individual treatment impacts specifically for women that lack autonomy in the household on steps completed in the application process at the time of End-Line I by those who applied *after* the intervention. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment on less autonomous women is statistically equal to the impact of another individual treatment on less autonomous women by comparing the additive impact of the treatment dummy plus the interaction dummy. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Therefore, while any level of support helps less autonomous women complete the application in greater numbers, the additional support offered by the most intensive treatment in which these women are accompanied to a local politician's office does have a larger impact. This suggests that women lacking autonomy benefit more from assistance that mobilizes them and helps them leave the house to interact with outside entities. Thus application procedures that require multiple trips outside of the home may disadvantage such women

who need household permission to engage with these ordeals. One woman in my sample explains the challenge of negotiating with family members:

*“I don’t get any support from my family in applying for these schemes. They tell me if something happens to me on the way, it is not their responsibility. That is when I get more reluctant about going to get the work done.”*

Given that this program provides women with a cash transfer directly into their personal bank accounts, the results in Table 1.10 suggest that women who might benefit most from access to their own source of income find it particularly difficult to access this government resource. This can perpetuate the dis-empowerment of these women relative to their more autonomous counterparts and limit their ability to improve their welfare through government assistance.

#### **1.4.5 Robustness and Alternative Explanations**

Did we simply speed up a process that would have eventually occurred organically anyway? If so, this would imply that take-up happens more quickly for some than others but information barriers and ordeals do not select out more vulnerable women in the long run. Although even short-term loss of income for these women is of significance, it is important to understand whether or not long-term outcomes actually shifted due to the interventions.

As shown in Table 1.11, applicants in the control group are more likely to be women who have *recently* become eligible, measured by having been widowed, divorced, separated, or abandoned for less than a year at the time of the census. This suggests that the normal rate of take-up that occurs is mostly among women who are newly eligible and have the resources and connections to overcome ordeals in the application process. With our randomly assigned information and mediation, however, a new group of women is brought into the fold, including women who had been eligible for over a year, sometimes for several years. It is unlikely that these women would have gotten through the application on their own without the interventions given these same women are less likely to be applying from

the control group where assistance was not provided<sup>32</sup>.

**Table 1.11:** *Steps Completed Post-Intervention by End-Line I, Interaction with Eligibility Timing*

| VARIABLES                 | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign  | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|---------------------------|-------------------------|-----------------------|-----------------------|---------------------|----------------------------|---------------------------|
| Any Treatment             | 0.264***<br>(0.037)     | 0.250***<br>(0.036)   | 0.182***<br>(0.034)   | 0.115***<br>(0.033) | 0.113***<br>(0.032)        | 0.071**<br>(0.029)        |
| Eligible < 1yr at Census  | 0.119**<br>(0.056)      | 0.122**<br>(0.056)    | 0.146***<br>(0.054)   | 0.135**<br>(0.053)  | 0.162***<br>(0.052)        | 0.138***<br>(0.049)       |
| Any Treat x Eligible <1yr | -0.077<br>(0.066)       | -0.070<br>(0.066)     | -0.049<br>(0.063)     | -0.019<br>(0.062)   | -0.054<br>(0.061)          | -0.005<br>(0.058)         |
| Observations              | 1,199                   | 1,199                 | 1,199                 | 1,199               | 1,199                      | 1,199                     |
| Control Mean              | 0.227                   | 0.223                 | 0.194                 | 0.187               | 0.176                      | 0.151                     |

Notes: This table presents pooled treatment impact specifically for women that have been eligible for less than 1 year at the time of the census on steps completed in the application process at the time of End-Line I by those who applied *after* the intervention. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Another interpretation of the findings may be that the interventions simply helped women prioritize pensions as 'top of mind' given that the deprivations associated with poverty taxes their cognitive bandwidth (Mullainathan and Shafir, 2013). In other words, it isn't the ordeals themselves that limit take-up by some women, but the fact that they have multiple demands on their attention, and are thus unable to focus on the pension application. I capture cognitive capacity of women at baseline by asking them to memorize 3, 4, and 5-digit numbers and lists of 3, 4, and 5 words. As shown in Table 1.12, the interventions did not have a larger impact on women with lower cognitive capacity.

<sup>32</sup>The same results found in Table 1.11 are presented from End-Line II in the Appendix. Notably, control group applicants are no longer more likely to be newly eligible to the scheme, suggesting some unintended impact from participating in the End-Line I survey. Otherwise, there would be no reason for the composition of the control group to change between the two end-line surveys, lending support to the decision to use End-Line I results as the preferred specification for measuring the isolated impact of the treatments.

**Table 1.12:** *Steps Completed Post-Intervention by End-Line I, Interaction with Cognitive Capacity*

| VARIABLES              | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign  | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|------------------------|-------------------------|-----------------------|-----------------------|---------------------|----------------------------|---------------------------|
| Any Treatment          | 0.237***<br>(0.041)     | 0.231***<br>(0.041)   | 0.177***<br>(0.038)   | 0.113***<br>(0.037) | 0.102***<br>(0.036)        | 0.087***<br>(0.034)       |
| Memorized Four Words   | 0.027<br>(0.053)        | 0.032<br>(0.053)      | 0.030<br>(0.050)      | 0.024<br>(0.050)    | 0.013<br>(0.048)           | 0.024<br>(0.045)          |
| Any Treat x Four Words | -0.001<br>(0.063)       | -0.012<br>(0.062)     | -0.026<br>(0.060)     | -0.008<br>(0.058)   | -0.018<br>(0.056)          | -0.039<br>(0.053)         |
| Observations           | 1,199                   | 1,199                 | 1,199                 | 1,199               | 1,199                      | 1,199                     |
| Control Mean           | 0.227                   | 0.223                 | 0.194                 | 0.187               | 0.176                      | 0.151                     |

Notes: This table presents pooled treatment impact specifically for women that were able to memorize a list of 4 words on steps completed in the application process at the time of End-Line I by those who applied *after* the intervention. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Similarly, my qualitative interviews indicate that that women are deeply concerned with and attentive to the process they need to undergo to access these pensions. Even those who have not successfully applied give accounts of many failed attempts at trying to decipher or navigate the system. Those who have not tried express disillusionment with the state due to previous negative experiences or exasperation with trying to gather the required documents before they can apply. None, however, show apathy towards the application process or incredulity regarding the many ways receiving these pensions could benefit their life. An account from one woman in my sample further illustrates that ordeals rather than inattention seem to be an important barrier to take-up:

*“There was never a time when the work gets complete at one visit or in one attempt. For everything, they ask us to come many times. They will say it is because you do not have the necessary documents or because it is not the correct time or correct office to get it done, etc. The work is never completed easily. [...] The procedure is so rigid/fixed, they never have an alternate way of doing something.”*

Even if women do dedicate mental bandwidth to the pension applications, it is possible that the interventions worked by helping women get over procrastination. This interpretation, however, is not inconsistent with the finding that ordeals matter for take-up decisions. By helping women overcome information barriers and application ordeals, we may have reduced the utility costs of undergoing the application process to the point that women were no longer indefinitely procrastinating. It follows that reducing the burden of ordeals disproportionately helped women whose vulnerabilities may cause them to face the highest utility costs of the ordeals, or be more likely to procrastinate. Ultimately, whether more vulnerable women are actively opting out or simply procrastinating due to utility costs is not of huge consequence to the main interpretation of the findings that ordeals can limit take-up by such women.

It is also important to recognize, however, that none of our treatments helped women get all the way through the application process, as even our most intensive treatment stopped at the point of the MLA signature, leaving women on their own to complete the most time-consuming and costly step (see Table 1.1) of submitting the form at the district office on their own. Yet several women completed steps beyond where our treatments came in, with a significant number submitting their forms at the district office, which is the main application step we consider when discussing the effectiveness of the interventions. This suggests that even if their baseline inaction is due to procrastination, the women driving the main outcome of district acceptance are willing to put in the time, effort, and even money it takes to complete the application. Many simply need assistance and some assurances in order to do so.

Finally, public economics literature from the United States has found that stigma can limit take-up of certain welfare programs among eligible citizens. From the piloting exercises carried out before designing this experiment, it did not appear that receiving pensions from the government was stigmatized among the eligible women living in the slums we worked in. Still, to provide some clarity regarding whether or not stigma is a salient access barrier

in this context, we asked women in our End-Line I survey if they thought the government should provide cash assistance to the poor and whether they personally felt any hesitance in receiving cash assistance from the government. Women in all groups responded 99% yes to the first question and 98% no to the second question, respectively. Therefore, it is unlikely that the results reflect women feeling less welfare stigma because of our interventions and taking up pensions in response.

## 1.5 Conclusion and Policy Implications

There is widespread evidence that cash transfer programs can improve the well-being of recipients and their families, especially when targeted at women (Bastagli et al., 2016). Oftentimes this evidence comes from experimental settings in which the selection of correct beneficiaries is ensured by the research design<sup>33</sup>. In real-world implementation, however, the reach and effectiveness of anti-poverty programs depends in part on the process by which eligible citizens are expected to enroll and whether on average, they have the capacity to do so or not.

Using a field experiment in Delhi with women eligible for the widow pension scheme, I show that simply assisting with required paperwork can boost enrollment, especially by more vulnerable women. Providing information without offering assistance, however, only helps more affluent women complete the application in higher numbers. While we may assume citizens are simply uninformed about social programs and require better information, my findings suggest information alone may not be the silver bullet to help the poor unless complementary efforts are made to ensure they can make use of the information. In addition, I find that stringent documentation requirements significantly limit take-up of this program even with these interventions. Therefore, although there is little evidence of corruption, the process is certainly marked with notable red tape that serves to limit and distort allocation.

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<sup>33</sup>Exceptions include (Aizer et al., 2016; Duflo, 2003)

These findings come with clear policy implications. First, when the target population of a program is largely poor, illiterate, and dis-empowered, simple and straightforward application procedures with flexibility in documentation criteria can go a long way in facilitating inclusive take-up. In other words, ordeals should be designed keeping both Type I and Type II errors in mind so that the majority of *eligible* citizens are not deterred from enrolling. Secondly, investing in mediation offered by a neutral and accessible bureaucracy can decrease dependency on ad-hoc political and social networks to facilitate take-up. Finally, relying on self-selection can potentially leave out particularly vulnerable populations, suggesting a need for proactive outreach in local communities via information campaigns, distribution of application forms, and enrollment camps. There are also potential benefits of linking up with agencies that keep lists of widows, divorcees, and poor citizens and having an automatic enrollment option.

Ultimately, facilitating maximum take-up is as relevant to policy design as the decision regarding the type and amount of payout being offered<sup>34</sup>. An alternative to targeted welfare programs is providing all citizens with a universal basic income (UBI). Many countries, including India, are considering this option as it can provide necessary social protection while eliminating the need for multiple programs and a complex welfare administration. Yet proper implementation of UBI, assuming budgetary approval and political buy-in, will require the government to first provide unique identification and expand financial inclusion to all citizens. Therefore, ensuring equal access even to UBI requires addressing the non-trivial barriers to initial enrollment highlighted here. Otherwise, citizens who stand to benefit most from such an income maintenance program might not receive it.

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<sup>34</sup>For example, in January 2017, the Delhi government substantially raised the monthly pension amount given to women enrolled in the widow pension scheme, suggesting that funding for the scheme is plentiful. Yet no changes were made to the actual administration of the scheme to improve its reach and accessibility, potentially limiting the true welfare gains made by this policy change.

## Chapter 2

# The Impact of Increasing Access to an Unconditional Cash Transfer Program on Women's Well-Being in India

### 2.1 Introduction

Governments invest substantial resources in social welfare programs to ensure poor or otherwise vulnerable citizens can overcome income shocks and adverse life events. Unconditional cash transfer (UCT) programs are becoming an increasingly common method of getting these resources in the hands of citizens in developing countries. At least one non-contributory UCT is offered in 130 low- and middle-income countries, including 40 of the 48 countries in sub-Saharan Africa<sup>1</sup> (Bastagli et al., 2016). UCTs often require less administrative capacity and overhead than in-kind or conditional transfers. In addition, proponents of unconditional cash transfers emphasize the added dignity and autonomy that comes with allowing beneficiaries to make their own spending decisions (Blattman and Niehaus, 2014; Gentilini, 2016; Handa et al., 2017).

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<sup>1</sup>This number is up from 20 sub-Saharan countries with UCT programs as recently as 2010 (Bastagli et al., 2016).

Several research studies have debunked common concerns that unconditional cash transfers will be wasted on temptation goods like tobacco or alcohol (Evans and Popova, 2017) or crowd out participation in income-earning activities (Banerjee et al., 2017). Instead, there is a growing body of work suggesting that UCTs can improve well-being to the same extent as in-kind or conditional cash transfers (Baird et al., 2011; Blattman et al., 2013; Cunha, 2014; Haushofer and Shapiro, 2016).

Much of this evidence, however, comes from short-term transfers provided in experimental settings in which the administration of the transfer and selection of beneficiaries is done by well-trained research teams and NGOs. In practice, imperfect government bureaucracies administer such programs at a large scale to beneficiaries who often have self-selected into the program, making systematic evaluation of existing programs prone to selection bias, among other challenges.

In this paper, I provide evidence on the impact of an unconditional cash transfer program in a real-world setting by exploiting random variation in take-up of a prominent social pension program for poor widows in India. I measure the impact of receiving this cash transfer on finances, consumption, and mental health of vulnerable women. I also examine whether the process of engaging the state to claim this entitlement alters women's views of government or future levels of political participation.

In a previous study, I studied the enrollment barriers to this type of social pension program by conducting a randomized controlled trial with women who are eligible for the widow pension scheme in Delhi<sup>2</sup>. This program provides women with a payment of Rs. 2,500 (\$40) per month for the rest of their lives, paid in 3-month installments to their single-user bank accounts. Yet, evidence suggests that a majority of eligible women do not benefit from this program, with only 34% enrolled in 2014 (WB, 2014). To better understand barriers to take-up and their distributive consequences, I selected over 1,200 eligible women for the original study and provided them with the interventions described below. Women

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<sup>2</sup>This pension program, while labeled as a widow pension scheme, is actually also for women who are no longer with their spouse due to divorce, separation, or abandonment.

were divided into four groups consisting of one control group and three treatment groups, stratifying by which slum neighborhood they lived in.

One intervention group received only detailed information about the pension program and the application process. The second group received information and assistance with filling out the application form, and the third group received this same information and form assistance along with an offer to be accompanied to the local politician's office where the pension application is authorized. These interventions increased the rate of successful application submissions to the pension scheme by 25% - 70%, depending on the intensity of the treatment offered.

As a result of these interventions, women in the study experienced random variation in eventual access to this pension scheme, which provides them with a substantial unconditional cash transfer for life. I use this first stage variation to measure the impact of the interventions on various measures of well-being two years after the initial intervention. The main analysis relies on reduced form estimates that directly capture the impact of the pooled treatment indicator on these outcomes, under the assumption that many of the outcomes considered are likely changing as a result of the increased income from the pensions. The Appendix also includes two-stage least squares (2SLS) estimates, using assignment to treatment as an instrument for getting the pensions to estimate the causal impact of getting pensions on the measures of well-being considered.

I find that household spending on vegetables, a staple of food item for the poor, increases by 10% among women who received an intervention. In addition, treated women are less likely to be in debt, more likely to have cash on hand for emergencies, and less likely to report food insecurity. These impacts on consumption and finance outcomes are seen both in the reduced form analysis and in the 2SLS analysis, though only the increase in emergency cash retains statistical significance using the 2SLS method due to power constraints. Still, as it is likely that the reduced form results reflect improvements that come from access to the pension program, this is suggestive evidence of important improvements to economic well-being of recipients of this unconditional cash transfer.

I also consider psychological impacts of the interventions, as recent work on cash transfer programs has emphasized the importance of such programs in improving mental health in addition to material well-being (Baird et al., 2013; Lund et al., 2011). This may be particularly important to consider in this context given that the women targeted by this cash transfer often face social discrimination due to their marital status in addition to struggling with poverty (UN, 2001). By providing a guaranteed source of income to these women and helping them achieve financial independence, this transfer could improve their mental health and life satisfaction.

My results show that women who receive an intervention are 64% more likely to report being very satisfied with life than women in the control group. Treated women also see notable improvements to their mental health score, driven by a reduction in anxiety about the future, which is significant at the 1% level. These improvements to life satisfaction and anxiety levels hold in the 2SLS analysis as well, suggesting that they represent the causal impact of accessing the pension money from the government. In light of evidence that poverty and poor mental health can feed in to one another (Dercon and Krishnan, 2009; Lund et al., 2011; Patel and Kleinman, 2003), these results indicate that this income maintenance program may play an important role in potentially breaking this vicious cycle.

While the improvements to mental health are promising, women in the treatment groups do not see a corresponding improvement in physical health or household level empowerment. Therefore, while this cash transfer program does seem to alleviate some of the mental strain facing women living in insecure circumstances, to alter health outcomes or underlying power dynamics in the household may require much more time than what is captured by this relatively short-term follow-up study.

Finally, given the nature of the original interventions employed, which empower women with better information regarding their entitlements and assist them in engaging with political and bureaucratic state systems, I also measure the impact of treatments on certain political outcomes. These outcomes are only considered using reduced form analysis, as it is not likely that they are only influenced by pension status but could also reflect updating

of views and behaviors due to the interventions themselves.

First, I look at whether women who received the interventions have different views of the government two years after the intervention in comparison to women in the control group. I find that while their overall view of the government is only marginally better, treated women are more likely to report that their Member of Legislative Assembly (MLA) would help them if approached for assistance. Given that the MLA is the political figure that is involved in sanctioning pension applications, this suggests positive updating from exposure to and engagement with this political figure.

I also find that treated women are more self-reliant in dealing with the government as a result of the experiment. Specifically, women in the pooled treatment group are 77% more likely to report they first turn to themselves when they need to get something done from the government than women in the control group, who are more likely to say they turn first to a politician for help. This suggests some degree of political empowerment as a result of the interventions. At the same time, women do not appear to be more politically active due to the interventions and in fact are less likely to have made a political complaint or visited their politicians' offices in the past year. While this may reflect political disengagement, it also could simply be capturing the lower need for these women to engage with political figures once they have enrolled in the pension scheme successfully.

For all of the above-mentioned economic, psychological, and political outcomes, I also conduct heterogeneity analysis to understand whether the interventions affected women with no personal income at baseline differently than those who did already have some form of income. I find that women with no baseline income see a larger increase in vegetable consumption and having emergency cash on hand than women who had a baseline income. In addition, those without a baseline income see a greater reduction in anxiety levels and increase in political self-reliance. Therefore, it appears that mobilizing more women to engage with the government and eventually access this cash transfer program does have notable impact on the poorest of the poor, making this an effective and important social protection program.

These results contribute to the small but growing body of work aimed at understanding the impact of unconditional cash transfer programs that come in the form of non-contributory social pensions from the government. Other work in this domain has found that the old-age pension offered in South Africa improved the anthropometric status of girl children when the transfer was given to elderly women, but there were no benefits to child health when the transfer was given to elderly men (Duflo, 2003). An evaluation of the Mother's Pension program in the United States, targeted at poor families who had suffered the loss of the main income earner, found long-term impacts of the program on children's health status, educational attainment, adult income, and longevity (Aizer et al., 2016). I build upon these results which emphasize impacts on child health by measuring the impact of a social pension on the immediate recipients themselves. Oftentimes such programs are designed with the primary objective of providing social support to the individuals targeted, yet we lack sufficient evidence on the impacts. Here, I find that unconditional cash transfers targeted at particularly vulnerable women, often with dependent children and no source of personal income, can improve finances, consumption, and mental health substantially.

With this analysis, I also contribute to recent work done by political scientists that emphasizes the role "administrative burdens" play in how individuals experience and view their state (Moynihan et al., 2014). Women who received our interventions, providing them with information and application assistance with a prominent welfare program, were more likely to report self-reliance in subsequent interactions with the government. This suggests that policies that reduce or streamline administrative burdens and thus result in a more positive experience of governance among citizens may fundamentally change how citizens engage with the state. This also speaks to work that highlights the potential role of policies in feeding back in to politics (Mettler and Soss, 2004; Pierson, 1993). Additionally, my findings show that treated women weakly update on their general views of the government but strongly update on their belief that the politician responsible for social welfare would assist them if asked. This is in line with work emphasizing the role of welfare in eliciting political support (Manacorda et al., 2011).

From a policy perspective, these results show that social protection programs in the form of unconditional cash transfers can be a useful tool to improve the well-being of vulnerable citizens, especially destitute women. It follows that it is essential to ensure the last mile problem of eligible citizens successfully enrolling in and benefiting from such programs is addressed. In addition, the nature of the transfer evaluated in this research, monthly payments for life, actually resembles the basic income model more closely than many cash transfer programs which provide lump-sums or incremental payments but for a limited duration of time. Thus, while not an explicit test of the basic income model, these results are suggestive of important benefits of this type of regular income-maintenance program, as discussed in more detail in the conclusion.

The rest of this paper is organized as follows. Section 2 provides an overview of the context of the study, and Section 3 provides details of the research design, including sample selection, interventions, data collection, and analysis plan. Section 4 presents and discusses the results, and Section 5 concludes and offers policy implications.

## **2.2 Context of Study**

Female-headed households are becoming more common in India. In 1993, 9.2% of households had a female head, and by 2014, that number had risen to 14.4% (WB, 2018). Recognizing the unique economic challenges of such households as well as the social discrimination often faced by Indian women who have lost or have been separated from their husbands, the Government of India offers a non-contributory social pension to poor women who are widowed, divorced, abandoned, or separated. In Delhi, this pension scheme provides a transfer amount of Rs. 2500 (\$40) per month to beneficiaries, paid in three-month installments for life. This amount is slightly greater than median per capita consumption across all of urban Delhi (NSS, 2012), making it a substantial amount of money in this context.

However, evidence suggests that this important social protection program does not reach many eligible women due to challenges associated with the application process (WB, 2014). To enroll into this welfare program, women must obtain an application form, fill

out the application form, attach several supporting documents, have the form authorized by a local politician's office, and then deposit the form to their district bureaucracy. Each of these steps poses different challenges to women with different characteristics. Recent evidence finds that ultimately women who are illiterate, less politically connected, and less autonomous within their households benefit most when provided with assistance to complete the application process rather than trying to independently navigate the system (Gupta, 2017). These women are systematically underrepresented among beneficiaries when application support is not available.

Thus, there is reason to believe that empowering more women to access this pension program can alter their views of the government and their political engagement as citizens. In addition, receiving this unconditional cash transfer from the government can impact the health, happiness, and overall well-being of beneficiaries. In this paper, I examine these various outcomes for women who were brought into the pension scheme after an experiment which boosted enrollment by offering women various degrees of assistance with the application process.

## **2.3 Research Design**

### **2.3.1 Sample Selection and Randomization**

The first stage of this study was a randomized controlled trial aimed at understanding the extent to which lack of information and/or application support limits enrollment in this pension program among eligible women living in Delhi. In order to determine the salience of these potential take-up barriers as well as understand their distributional consequences, I conducted an experiment with over 1,200 women living across 75 slums of Delhi who are eligible for the pension scheme.

To identify the sample of women to include in the study, I conducted a census exercise in the summer of 2015. This was done across 75 slums spanning 7 of the 10 administrative districts of Delhi. From this exercise, over 4,000 women were identified who were widowed,

divorced, separated, or abandoned, 40% of which were already receiving pensions. After dropping those already enrolled in the program and those who did not meet the eligibility criteria or certain documentation requirements, about 1,400 were selected for participation in the study. These women were randomly divided into 4 equal-sized groups, stratifying by slum. One group served as the control group, and the remaining 3 groups received interventions as explained below.

### **2.3.2 Intervention**

The interventions were designed to increase take-up of the widow pension program with two overarching goals: 1. To understand barriers to take-up and their distributional consequences 2. To measure the impact of engaging with the state and receiving an unconditional cash transfer on political empowerment and economic well-being.

In the first intervention group, women were provided with only detailed information about the pension program, including the benefits, eligibility criteria, documentation requirements, and application procedures. In the second intervention group, women were provided with this same information but also given personal assistance with filling out an application form for this program. In the final and most intensive intervention group, women were provided with the same information and form assistance provided to the second group, but in addition, they were offered accompaniment to go to the local politician's office to have their application form authorized.

In the second and third interventions, ordeals that could be particularly challenging for poor women with limited literacy and potentially low levels of political connectedness and personal autonomy were targeted by the treatment design.

Appendix Table B.1 provides summary statistics from the census data, broken down by the different treatment groups. The last column in this table shows that the groups are balanced at the 5% significance level for all variables at the time of the census. In addition, we see from this table that the women in the study sample face many hardships, including low literacy levels, low employment rates, low income levels, and large number of

potentially dependent children. On average, women report lost wages from their spouse's income of Rs. 5,539 per month. By providing a cash transfer of Rs. 2,500 per month, the pension program can make up for 45% of these lost wages.

### **2.3.3 Data Collection**

This analysis relies on several rounds of data collection. After finalizing the sample of women from the census survey, a lengthy baseline survey was conducted with 1,257 women from September - November of 2015. This survey captured information on family members, demographics, political participation, decision-making and autonomy, mental and physical health, consumption, and finances. Immediately after completing the baseline survey, women were administered the intervention to which they were assigned, with women in the control group receiving no intervention. For all women who received any of the three above-mentioned interventions, a separate intervention survey was completed to capture respondent compliance with each step of the intervention and to capture which documents the respondent was able to show.

Approximately 4 months after the baseline and intervention period, the first end-line was administered to all women in the sample from February - April, 2016. Of the 1,257 women who completed the baseline and intervention, 1,199 (or 95%) completed this end-line survey. Attrition was balanced across all groups. This end-line survey was designed to capture how far each respondent had gotten in the application process for the pension scheme, whether or not they are receiving pensions, spillovers to neighbors, and bank account usage.

At the time of the first end-line survey, many women had completed the application process but had not started receiving their pension payments yet. To get a better measure of this final outcome, we conducted a short, rapid end-line survey (End-Line II) with our sample from August-October of 2016 and were able to reach 1,131 women from our original sample.

Almost two years after the original intervention, we followed up with women to capture the impact of engaging with the government and potentially receiving this cash transfer on

their political, social, and economic well-being. This round of data collection, referred to here after as the *Welfare End-Line*, occurred from June - August of 2017, and covered 1,005 women from the original sample. Table 2.1 shows that attrition does not seem to be selective as almost all census variables are still balanced at the time of the Welfare End-Line when comparing each treatment group with the control group<sup>3</sup>. Most of the variables shown in Table 2.1 are also included as controls in the main specifications for the results presented in this paper.

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<sup>3</sup>Three exceptions are that literacy is lower in the T1 group than the control, the average number of children birthed is slightly lower in the T3 group than control, and the percentage of renters is slightly higher in T3 than in control.

**Table 2.1:** Balance Check for Welfare End-Line Sample

|                                | (1)                 | (2)                   | (3)                 | (4)                 | (5)                   | (6)                 | (7)                 |
|--------------------------------|---------------------|-----------------------|---------------------|---------------------|-----------------------|---------------------|---------------------|
|                                | Control             | T1                    | T2                  | T3                  | Control<br>vs. T1     | Control<br>vs. T2   | Control<br>vs. T3   |
| Age (years)                    | 41.64<br>(0.66)     | 41.74<br>(0.58)       | 42.00<br>(0.62)     | 40.21<br>(0.60)     | -0.08<br>(0.89)       | -0.26<br>(0.92)     | 1.61*<br>(0.91)     |
| Literate %                     | 0.30<br>(0.03)      | 0.22<br>(0.03)        | 0.26<br>(0.03)      | 0.31<br>(0.03)      | 0.09**<br>(0.04)      | 0.03<br>(0.04)      | -0.02<br>(0.04)     |
| Muslim %                       | 0.25<br>(0.03)      | 0.29<br>(0.03)        | 0.26<br>(0.03)      | 0.33<br>(0.03)      | -0.03<br>(0.03)       | 0.00<br>(0.03)      | -0.06*<br>(0.03)    |
| Non-Widows (%)                 | 0.18<br>(0.03)      | 0.17<br>(0.02)        | 0.20<br>(0.03)      | 0.20<br>(0.02)      | 0.00<br>(0.03)        | -0.03<br>(0.04)     | -0.04<br>(0.04)     |
| Household Size                 | 4.59<br>(0.15)      | 4.88<br>(0.14)        | 4.56<br>(0.14)      | 4.57<br>(0.15)      | -0.32<br>(0.21)       | 0.07<br>(0.20)      | 0.00<br>(0.21)      |
| Children Birthed               | 4.18<br>(0.16)      | 4.13<br>(0.15)        | 4.16<br>(0.15)      | 3.64<br>(0.15)      | 0.07<br>(0.22)        | 0.08<br>(0.21)      | 0.56***<br>(0.22)   |
| Rent Home (%)                  | 0.06<br>(0.02)      | 0.09<br>(0.02)        | 0.11<br>(0.02)      | 0.12<br>(0.02)      | -0.03<br>(0.02)       | -0.05**<br>(0.03)   | -0.07**<br>(0.03)   |
| Years in Delhi                 | 28.71<br>(0.67)     | 27.85<br>(0.66)       | 28.86<br>(0.67)     | 28.24<br>(0.61)     | 0.78<br>(0.94)        | -0.21<br>(0.94)     | 0.40<br>(0.91)      |
| No Personal Income (%)         | 0.50<br>(0.03)      | 0.52<br>(0.03)        | 0.46<br>(0.03)      | 0.46<br>(0.03)      | -0.03<br>(0.05)       | 0.04<br>(0.05)      | 0.04<br>(0.05)      |
| Monthly Income (Rs.)           | 1618.94<br>(134.87) | 1510.67<br>(131.33)   | 1724.80<br>(125.47) | 1654.21<br>(120.72) | 96.27<br>(189.57)     | -105.37<br>(182.29) | -100.87<br>(177.47) |
| Spouse's Monthly Income (Rs.)  | 5702.32<br>(459.61) | 11243.05<br>(4214.49) | 5558.06<br>(427.24) | 5498.66<br>(353.70) | -5476.73<br>(4629.41) | 210.14<br>(628.61)  | 312.67<br>(583.64)  |
| Lives with Parents/In-Laws (%) | 0.19<br>(0.03)      | 0.17<br>(0.02)        | 0.15<br>(0.02)      | 0.21<br>(0.03)      | 0.01<br>(0.03)        | 0.02<br>(0.03)      | -0.03<br>(0.04)     |
| Eligible < 1 Year (%)          | 0.37<br>(0.03)      | 0.37<br>(0.03)        | 0.34<br>(0.03)      | 0.38<br>(0.03)      | 0.00<br>(0.04)        | 0.03<br>(0.05)      | 0.00<br>(0.04)      |
| Previously Applied for Pension | 0.23<br>(0.03)      | 0.23<br>(0.03)        | 0.21<br>(0.03)      | 0.24<br>(0.03)      | 0.01<br>(0.04)        | 0.02<br>(0.04)      | 0.00<br>(0.04)      |
| <b>Observations</b>            | <b>227</b>          | <b>267</b>            | <b>250</b>          | <b>261</b>          | <b>494</b>            | <b>477</b>          | <b>488</b>          |

Notes: This table compares average characteristics of the women in the sample across the control and individual treatment groups. Columns 1, 2, 3, and 4 present means for the control group, treatment 1 group, treatment 2 group, and treatment 3 group, respectively. Columns 5, 6, and 7 show the difference in means between each treatment group and the control group, controlling for slum fixed effects. Stars signify whether the difference in means is statistically different from zero. \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

The Welfare End-Line was also accompanied by a Qualitative Module aimed at better understanding women's frustrations with the application process, their limitations in successfully engaging with the state, and their impression of how the pension could or has benefited them. This qualitative module was carried out with 45 women.

One final data source used in this project is administrative data from the Delhi government that is published on their widow pension scheme website. I scraped a list of current beneficiaries from this website in October 2016 (right after the End-Line II survey) in order to have a more objective measure of the pension outcome.

### **2.3.4 Potential for Increasing Empowerment and Well-Being**

The interventions from this research study successfully increased take-up of the widow pension scheme by 25% - 70% depending on the intensity of the treatment. In addition, the interventions that included an assistance component were useful for bringing more women in to the program who are illiterate, politically disconnected, and less autonomous in their households. Given that more women were able to successfully engage the state to avail an important entitlement and that many are now receiving a substantial cash transfer for life, this experiment provides an opportunity to further measure the impact of these changes on the lives of the women. Namely, women in the treatment groups may have updated on their views of the state and the modes of political participation they engage in. In addition, they may see important improvements to their economic, psychological, and physical well-being as a result of enrolling in this targeted social protection program and receiving a regular income from the government.

Column 1 of Table 2.2 and Table 2.3 display control group means for all outcomes of interest in this study, captured in the Welfare End-Line survey. Columns 2 and 3 show the control group means separately for women who did have a source of personal income at baseline and those who did not, respectively, as this will be the focus of the heterogeneity analysis that follows.

From looking at these tables, it is clear that there are many dimensions of well-being that can be improved for women. A majority of women are in debt and do not believe they can pay off their debts in the next year. Inability to meet food and health care needs is prevalent while life satisfaction is very low. Women with no personal source of income are more likely to report that they are not involved in the majority of financial decisions of the household and that they lack some degree of autonomy, measured by requiring permission to do basic activities. The widow pension has the potential to alleviate many of these hardships by providing women with a stable source of income, which can be spent on immediate needs and also provide security and financial independence in the future.

**Table 2.2:** *Economic Outcomes of the Control Group, by Income Status*

|                                    | (1)<br>Overall    | (2)<br>Personal<br>Income | (3)<br>No Personal<br>Income | (4)<br>Income vs.<br>No Income |
|------------------------------------|-------------------|---------------------------|------------------------------|--------------------------------|
| Spending on Vegetables             | 436.71<br>(19.60) | 446.93<br>(23.63)         | 426.22<br>(31.54)            | -12.94<br>(45.58)              |
| Spending on Fruit                  | 115.38<br>(16.18) | 100.88<br>(15.24)         | 130.41<br>(28.94)            | -61.82*<br>(36.24)             |
| Spending on Meat                   | 139.24<br>(19.05) | 134.86<br>(21.57)         | 143.65<br>(31.57)            | -45.96<br>(43.18)              |
| Spending on Beverages              | 52.11<br>(7.51)   | 41.37<br>(8.45)           | 63.04<br>(12.42)             | -36.20**<br>(17.25)            |
| Spending on Alcohol                | 53.99<br>(17.97)  | 56.23<br>(23.12)          | 51.65<br>(27.79)             | -5.50<br>(42.74)               |
| In Debt                            | 0.64<br>(0.03)    | 0.69<br>(0.04)            | 0.59<br>(0.05)               | 0.07<br>(0.07)                 |
| Can Pay-Off Debt in 1 Year         | 0.30<br>(0.04)    | 0.33<br>(0.05)            | 0.27<br>(0.05)               | -0.02<br>(0.09)                |
| Has Emergency Cash on Hand         | 0.33<br>(0.03)    | 0.39<br>(0.05)            | 0.27<br>(0.04)               | 0.16**<br>(0.07)               |
| Food Insecurity Index              | 0.44<br>(0.02)    | 0.49<br>(0.03)            | 0.39<br>(0.03)               | 0.07<br>(0.05)                 |
| Healthcare Insecurity Index        | 0.53<br>(0.02)    | 0.57<br>(0.02)            | 0.49<br>(0.02)               | 0.08<br>(0.04)                 |
| Very Satisfied with Life           | 0.07<br>(0.02)    | 0.04<br>(0.02)            | 0.10<br>(0.03)               | -0.04<br>(0.04)                |
| Poor Mental Health Index           | 2.25<br>(0.06)    | 2.34<br>(0.08)            | 2.15<br>(0.10)               | 0.19<br>(0.15)                 |
| Anxious About Future               | 0.58<br>(0.03)    | 0.56<br>(0.05)            | 0.59<br>(0.05)               | -0.01<br>(0.08)                |
| Poor Health in General             | 0.63<br>(0.03)    | 0.67<br>(0.04)            | 0.59<br>(0.05)               | 0.05<br>(0.07)                 |
| Illness or Injury in Past 6 Months | 0.21<br>(0.03)    | 0.21<br>(0.04)            | 0.21<br>(0.04)               | 0.01<br>(0.06)                 |
| Makes Fewer Financial Decisions    | 0.19<br>(0.03)    | 0.08<br>(0.03)            | 0.30<br>(0.04)               | -0.22***<br>(0.06)             |
| Lacks Autonomy in Household        | 0.56<br>(0.03)    | 0.45<br>(0.05)            | 0.66<br>(0.04)               | -0.23***<br>(0.08)             |
| <b>Observations</b>                | <b>227</b>        | <b>114</b>                | <b>113</b>                   |                                |

Notes: Column 1 of this table displays the control mean for all outcomes related to consumption, finances, and subjective well-being used in this analysis captured in the Welfare End-Line survey. Columns 2 and 3 show control means for women who already had a source of personal income at the time of the baseline survey and those who did not, respectively. Column 4 displays the difference in means between Columns 2 and 3, with stars indicating significance levels on the test of whether these differences are statistically different from zero.

\*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

**Table 2.3:** *Political Outcomes of the Control Group, by Income Status*

|                                                   | (1)<br>Overall | (2)<br>Personal<br>Income | (3)<br>No Personal<br>Income | (4)<br>Income<br>vs. No<br>Income |
|---------------------------------------------------|----------------|---------------------------|------------------------------|-----------------------------------|
| Views of State Index                              | 0.43<br>(0.02) | 0.44<br>(0.02)            | 0.42<br>(0.02)               | 0.01<br>(0.04)                    |
| MLA Would Help Me if Asked                        | 0.52<br>(0.03) | 0.49<br>(0.05)            | 0.55<br>(0.05)               | -0.03<br>(0.07)                   |
| Need to Know MLA to Access Resources              | 0.50<br>(0.03) | 0.51<br>(0.05)            | 0.50<br>(0.05)               | 0.02<br>(0.08)                    |
| MCD Councilor Would Help Me if Asked              | 0.51<br>(0.03) | 0.54<br>(0.05)            | 0.47<br>(0.05)               | 0.09<br>(0.08)                    |
| Need to Know MCD Councilor to Access Resources    | 0.55<br>(0.03) | 0.58<br>(0.05)            | 0.51<br>(0.05)               | 0.07<br>(0.08)                    |
| Turns to Self to Access State Resources           | 0.04<br>(0.01) | 0.05<br>(0.02)            | 0.04<br>(0.02)               | 0.02<br>(0.03)                    |
| Turns to Family/Friends to Access State Resources | 0.04<br>(0.01) | 0.02<br>(0.01)            | 0.07<br>(0.02)               | -0.04<br>(0.03)                   |
| Turns to Govt Employee to Access State Resources  | 0.02<br>(0.01) | 0.01<br>(0.01)            | 0.03<br>(0.02)               | -0.02<br>(0.02)                   |
| Turns to Pradhan to Access State Resources        | 0.04<br>(0.01) | 0.06<br>(0.02)            | 0.03<br>(0.02)               | 0.02<br>(0.03)                    |
| Turns to MLA to Access State Resources            | 0.66<br>(0.03) | 0.67<br>(0.04)            | 0.65<br>(0.04)               | 0.03<br>(0.07)                    |
| Turns to MCD Councilor to Access State Resources  | 0.16<br>(0.02) | 0.18<br>(0.04)            | 0.14<br>(0.03)               | 0.05<br>(0.05)                    |
| Complained about Services to Politician Past Year | 0.30<br>(0.03) | 0.32<br>(0.04)            | 0.27<br>(0.04)               | 0.06<br>(0.07)                    |
| Helped Others Apply for Govt Program in Past Year | 0.26<br>(0.03) | 0.25<br>(0.04)            | 0.27<br>(0.04)               | -0.03<br>(0.07)                   |
| Voted MCD Elections                               | 0.83<br>(0.03) | 0.86<br>(0.03)            | 0.80<br>(0.04)               | 0.03<br>(0.06)                    |
| Went to MLA Office in Past Year                   | 0.44<br>(0.03) | 0.45<br>(0.05)            | 0.44<br>(0.05)               | 0.05<br>(0.08)                    |
| Went to MCD Councilor Office in Past Year         | 0.26<br>(0.03) | 0.26<br>(0.04)            | 0.25<br>(0.04)               | -0.02<br>(0.06)                   |
| Applied for Another Program in Past Year          | 0.39<br>(0.03) | 0.44<br>(0.05)            | 0.35<br>(0.04)               | 0.16**<br>(0.08)                  |
| <b>Observations</b>                               | <b>227</b>     | <b>114</b>                | <b>113</b>                   |                                   |

Notes: Column 1 of this table displays the control mean for all outcomes related to views of the state and political engagement used in this analysis captured in the Welfare End-Line survey. Columns 2 and 3 show control means for women who already had a source of personal income at the time of the baseline survey and those who did not, respectively. Column 4 displays the difference in means between Columns 2 and 3, with stars indicating significance levels on the test of whether these differences are statistically different from zero. \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

Promisingly, women who are receiving the pension report almost unanimously (99%) that they have control over how their pension money is spent, suggesting we may expect to

see improvements to their well-being from this transfer. In terms of top spending categories, a majority of women, 62%, list food as their number one expense covered by the pension money. Another 15% list personal medical expenses as their top expense and 10% list education expenses for children or grandchildren. The fact that such immediate, non-luxury needs are being met with this pension payment further suggests that this transfer has the potential to have a notable and important impact on the well-being of its recipients. As one pension recipient noted in the qualitative survey:

*“It feels great to get the pension. We don’t have to worry as much - we are able to pay for the children’s books and we can pay for our food and household expenses. I am happy that I am receiving the money...I am able to pay for some things I was not able to pay for earlier.”*

### 2.3.5 Estimation

The main results report intention-to-treat (ITT) reduced form estimates of the impact of treatment assignment on the above-mentioned political and economic outcomes. The preferred specification for the main analysis estimates the following equation:

$$Y_{is} = B_0 + B_1 Treatment_{is} + \delta X_i + n_s + e_{is} \quad (2.1)$$

where  $Y_{is}$  is an outcome for individual  $i$  living in slum  $s$  and  $Treatment_{is}$  is a dummy for being assigned to any one of the three treatment groups.  $\delta X_i$  is a vector of individual level controls<sup>4</sup>,  $n_s$  represents slum fixed effects, and  $e_{is}$  is a robust error term.

For some outcomes, I look at the second-stage impact of getting pensions by utilizing the treatment assignment as an instrument. For this analysis, I estimate:

$$Pension_{is} = B_0 + B_1 Treatment_{is} + \delta X_i + n_s + e_{is} \quad (2.2)$$

$$Y_{is} = B_0 + B_1 Pension_{is} + \delta X_i + n_s + e_{is} \quad (2.3)$$

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<sup>4</sup>Controls include respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index at baseline.

where  $Pension_{is}$  is a dummy variable capturing whether or not the individual started receiving the pension having applied after the intervention period.

## 2.4 Results

### 2.4.1 Effects on Consumption and Finances

A primary outcome of interest in the evaluation of any cash transfer program is how the transfer affects consumption patterns of recipients. As discussed above, it is clear that the bulk of this transfer amount is used by women to purchase food items. Therefore, one might expect to see movement in spending on various food categories as a result of getting this transfer.

Table 2.4 shows the reduced form impact of receiving any intervention in this study on household expenditures (in Indian rupees) on various food items. Column 1 in Table 2.4 shows that spending on vegetables, which are a major spending item for these households as evidenced by the control means, increases by almost 10% for women in the pooled treatment group. Spending on what are considered to be more luxury items such as fruit, meat, and beverages (not including water), however, remains largely unchanged, as seen in Columns 2-4 in Table 2.4. Finally, spending on alcohol does go down by one-third, but this result is not statistically significant. Still this null result helps dispel the concern that unconditional cash will be spent on temptation goods, either directly by the recipient or via capture by another family member.

**Table 2.4:** *Impact of Pooled Treatment on Consumption Levels*

| VARIABLES          | (1)<br>Spending on<br>Vegetables | (2)<br>Spending<br>on Fruit | (3)<br>Spending<br>on Meat | (4)<br>Spending on<br>Beverages | (5)<br>Spending on<br>Alcohol |
|--------------------|----------------------------------|-----------------------------|----------------------------|---------------------------------|-------------------------------|
| Received Treatment | 39.017*<br>(21.296)              | -0.724<br>(16.412)          | 0.955<br>(21.826)          | 3.942<br>(8.379)                | -18.597<br>(19.130)           |
| Observations       | 982                              | 990                         | 991                        | 988                             | 966                           |
| Control Mean       | 436.7                            | 115.4                       | 139.2                      | 52.11                           | 53.99                         |

Notes: This table presents the pooled treatment impact on household spending levels for consumption goods as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variables in Columns 1- 5 measure household spending in INR over the past week on the following items (1) vegetables (2) fruits (3) meat, poultry, and eggs (4) beverages (not including water) (5) alcohol, respectively. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, asset index and baseline spending on the given dependent variable. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

As the interventions themselves should not have directly affected spending on food items other than by increasing enrollment in the pension scheme, the results presented in Table 2.4 suggest this cash transfer facilitated increased consumption of essentials like vegetables. To provide more direct evidence of this chain of causality, I also present results using the two-stage least squares method in Table B.3 of the Appendix. I use the pooled treatment dummy as an instrument for receiving pensions post-intervention to measure the impact of receiving these pensions on spending levels across the various food items. While none of the coefficients in Table B.3 are significant, they all go in the same direction as the reduced form results.

It is also important to note whether changes in consumption varies by baseline income levels. Table 2.5 presents results from heterogeneity analysis looking at how women who had no income at the time of the baseline compare to those who did. Looking at Columns 1 and 5, we see that women with no baseline income see a greater increase in vegetable consumption and a greater decrease in alcohol consumption than those who did have an income, respectively. These differences are not statistically significant but suggest that a higher-powered study of this type of cash transfer program may find positive impacts

concentrated among the poorest women.

**Table 2.5:** *Impact of Pooled Treatment on Consumption Levels, Interaction with Baseline Income Status*

| VARIABLES             | (1)<br>Spending on<br>Vegetables | (2)<br>Spending<br>on Fruit | (3)<br>Spending<br>on Meat | (4)<br>Spending on<br>Beverages | (5)<br>Spending<br>on Alcohol |
|-----------------------|----------------------------------|-----------------------------|----------------------------|---------------------------------|-------------------------------|
| Received Treatment    | 32.431<br>(26.839)               | 7.304<br>(18.827)           | 36.881<br>(31.536)         | 16.590<br>(10.974)              | -17.601<br>(26.917)           |
| No Personal Income    | -25.677<br>(39.589)              | 35.323<br>(33.503)          | 41.646<br>(40.518)         | 24.431<br>(16.025)              | -8.362<br>(36.770)            |
| Treatment x No Income | 12.963<br>(45.028)               | -16.201<br>(38.390)         | -73.050<br>(46.928)        | -25.593<br>(17.591)             | -2.210<br>(44.044)            |
| Observations          | 982                              | 990                         | 991                        | 988                             | 966                           |
| Control Mean          | 436.7                            | 115.4                       | 139.2                      | 52.11                           | 53.99                         |

Notes: This table presents the pooled treatment impact on household spending levels for consumption goods depending on the baseline income status of the respondent. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. *No Personal Income* is a dummy variable that takes on a value of 1 if the respondent did not have any source of personal income at the time of the baseline study. The dependent variables in Columns 1-5 measure household spending in INR over the past week on the following items (1) vegetables (2) fruits (3) meat, poultry, and eggs (4) beverages (not including water) (5) alcohol, respectively. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, asset index, and baseline spending on the given dependent variable. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

In addition to movement in actual spending levels, this cash transfer can also be expected to have an impact on the general financial status of the women who benefit from it. In Table 2.6, I show the reduced form impact of the pooled treatment on household indebtedness, ability to pay off debts, possession of emergency cash, and indexes of food and health care insecurity. Women who receive any of the three interventions are 6 percentage points less likely to be in debt, and those that are in debt are 8 percentage points more likely to report they will be able to pay off their debt in the next year. In addition, women who are treated are 8 percentage points more likely to report having emergency cash on hand two years after the intervention. They also witness a decrease in their level of food insecurity but no change in health care insecurity<sup>5</sup>.

<sup>5</sup>components of these two indexes are individually analyzed in Appendix Table B.6.

**Table 2.6: Impact of Pooled Treatment on Finances**

| VARIABLES          | (1)<br>In<br>Debt  | (2)<br>Can Pay Debt<br>in 1 Year | (3)<br>Emergency<br>Cash on Hand | (4)<br>Food Insecurity<br>Index | (5)<br>Healthcare<br>Insecurity Index |
|--------------------|--------------------|----------------------------------|----------------------------------|---------------------------------|---------------------------------------|
| Received Treatment | -0.061*<br>(0.036) | 0.081*<br>(0.044)                | 0.082**<br>(0.036)               | -0.043*<br>(0.026)              | -0.025<br>(0.020)                     |
| Observations       | 1,005              | 594                              | 1,005                            | 1,005                           | 1,005                                 |
| Control Mean       | 0.643              | 0.301                            | 0.326                            | 0.443                           | 0.532                                 |

Notes: This table presents the pooled treatment impact on household finances as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variables in Columns 1-3 are dummy variables that take a value of 1 if the respondent (1) has any outstanding loans, (2) believes she can pay off current debt within 1 year, (3) keeps emergency cash on hand. The dependent variables in Column 4 and Column 5 measure food and healthcare insecurity, respectively (higher values signify greater insecurity). All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

The results from Table 2.6 provide evidence that the financial situation of women has notably improved as a result of these interventions. To more directly test the role getting pensions plays in this relationship, Appendix Table B.5 shows the 2SLS results using the treatment assignment as an instrument for getting pensions. All variables continue to move in the same direction as seen in Table 2.6, and being more likely to have emergency cash on hand remains significant. This is an important indicator of economic self-sufficiency, as having cash on hand allows women the ability to deal with unexpected shocks or immediate family needs. There are some concerns that the instrumental variable approach in this situation may violate the exclusion restriction, as perhaps our interventions caused women to pay more attention to their finances even if they did not eventually get the pension money, so the main analysis relies on the reduced form results.

As with consumption, I also examine how women who had no income at baseline are differentially impacted by the treatments in comparison to those who did have an income in regards to their financial situation. Table 2.7 shows that while treated women with no baseline income have similar outcomes as treated women that did have an income across most of the indicators, one important exception is possession of emergency cash. The

increased likelihood of having emergency cash on hand is completely concentrated among women who had no baseline income. As having cash on hand for emergencies can play a vital role in a household's ability to deal with unexpected shocks, especially health shocks, these results suggest that this social protection program is providing an importance source of financial stability to women who without it have no other means of personal income or insurance.

**Table 2.7:** *Impact of Pooled Treatment on Finances, Interaction with Baseline Income Status*

| VARIABLES             | (1)<br>In<br>Debt | (2)<br>Can Pay Debt<br>in 1 Year | (3)<br>Emergency<br>Cash on Hand | (4)<br>Food Insecurity<br>Index | (5)<br>Healthcare<br>Insecurity Index |
|-----------------------|-------------------|----------------------------------|----------------------------------|---------------------------------|---------------------------------------|
| Received Treatment    | -0.066<br>(0.051) | 0.071<br>(0.063)                 | 0.011<br>(0.051)                 | -0.067*<br>(0.038)              | -0.047<br>(0.034)                     |
| No Personal Income    | -0.034<br>(0.066) | 0.025<br>(0.079)                 | -0.098<br>(0.065)                | -0.101**<br>(0.047)             | -0.090**<br>(0.045)                   |
| Treatment x No Income | 0.011<br>(0.074)  | 0.022<br>(0.092)                 | 0.143**<br>(0.073)               | 0.047<br>(0.053)                | 0.045<br>(0.050)                      |
| Observations          | 1,005             | 594                              | 1,005                            | 1,005                           | 1,005                                 |
| Control Mean          | 0.643             | 0.301                            | 0.326                            | 0.443                           | 0.532                                 |

Notes: This table presents the pooled treatment impact on household finances depending on the baseline income status of the respondent. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. *No Personal Income* is a dummy variable that takes on a value of 1 if the respondent did not have any source of personal income at the time of the baseline study. The dependent variables in Columns 1-3 are dummy variables that take a value of 1 if the respondent (1) has any outstanding loans, (2) believes she can pay off current debt within 1 year, (3) keeps emergency cash on hand. The dependent variables in Column 4 and Column 5 measure food and healthcare insecurity, respectively (higher values signify greater insecurity). All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

## 2.4.2 Effects on Subjective Well-Being

Recent literature evaluating the impacts of cash transfers and other programs designed to alleviate poverty has emphasized the importance of considering subjective well-being outcomes, as these can be as important to overall quality of life as the consumption and financial outcomes discussed above.

Table 2.8 shows how women in the pooled treatment group compare to those in the control group on various dimensions of subjective well-being measured two years after the

intervention. Women who received a treatment are 64% more likely to report being very satisfied with life (Column 1, Table 2.8), a notable jump from the control mean of 7%. In addition, they have better mental health outcomes, driven by reduced anxiety levels as seen in Column 3. Given that enrollment in the pension program provides a guaranteed source of income for life for these women, anxiety is an area of mental health we would expect to improve more dramatically than some of the other indicators included in the index<sup>6</sup>. Notably, as seen in Appendix Table B.8, which presents results from the 2SLS analysis, there is evidence that the improvement in life satisfaction and reduction in anxiety levels is being driven via the channel of getting pensions from the government. So in addition to the financial benefits of the program, it also appears to improve the lives of women along these other important dimensions.

**Table 2.8: Impact of Pooled Treatment on Subjective Well-Being**

| VARIABLES          | (1)<br>Very Satisfied | (2)<br>Poor Mental Health Index | (3)<br>Anxious       | (4)<br>Poor Health | (5)<br>Sick in Past 6 Months | (6)<br>Less Financial Decision-Making | (7)<br>Lacks Autonomy |
|--------------------|-----------------------|---------------------------------|----------------------|--------------------|------------------------------|---------------------------------------|-----------------------|
| Received Treatment | 0.045**<br>(0.020)    | -0.065<br>(0.070)               | -0.102***<br>(0.037) | -0.035<br>(0.035)  | -0.011<br>(0.030)            | -0.012<br>(0.023)                     | 0.032<br>(0.037)      |
| Observations       | 1,005                 | 992                             | 1,005                | 1,005              | 1,005                        | 1,005                                 | 1,005                 |
| Control Mean       | 0.0705                | 2.248                           | 0.577                | 0.630              | 0.211                        | 0.189                                 | 0.555                 |

Notes: This table presents the pooled treatment impact on various measures of subjective well-being as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variable in Column 1 is a dummy variable that takes a value of 1 if the respondent reports being very satisfied with life. The dependent variable in Column 2 is a mental health index which represents the average value for 7 indicators measuring how often the respondent has exhibited signs of poor mental health in the past week. Each indicator takes a value from 0-3, with 3 being the most frequent. In Column 3, the dependent variable is a dummy representing whether the respondent often or always feels anxious about the future. Columns 4 and 5 are dummy variables that take a value of 1 if the respondent self-reports as being in generally poor health or having been sick in the past 6 months, respectively. Columns 6 and 7 are dummy variables that take a value of 1 if the respondent is not involved in many financial decisions in the household and if the respondent lacks autonomy to engage in various activities in the household without permission, respectively. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

While life satisfaction and mental health improve with treatment assignment, there is little movement on physical health indicators or household bargaining outcomes (Columns

<sup>6</sup>See Appendix Table 9 for the analysis on each component of the mental health index.

4-7), which we may expect are much stickier over time. So while women do seem better off in some dimensions of well-being, it is hard to conclude from this analysis that the cash transfer program changed their status in the household, at least in the short-term time horizons we are studying. It is possible, however, that these outcomes will shift slowly over time as women bring a regular source of income to the household, especially given the positive results on female empowerment found in other evaluations of cash transfer programs (Hagen-Zanker et al., 2017). In addition, qualitative surveys indicate the women value the financial independence that comes with receiving pensions and the feeling that they are not a burden on others:

*“Earlier, I was completely dependent on my mother. I would always have to ask my parents for money. There is no longer a burden on me or my parents.”*

Table 2.9 shows the impact of the treatments on these subjective well-being outcomes based on baseline income of the respondent. In Columns 6 and 7, we see that generally it is the women with no source of personal income in the control group that are less involved with household financial decisions and lack autonomy in the household. As having no income seems predictive of these outcomes, while we do not see movement in these measures at the time of our Welfare End-Line, it is possible that they will improve over the longer time horizon.

Table 2.9 also suggests that the improvements to life satisfaction and overall mental health are larger for women who already had some source of personal income at baseline, but the improvements to anxiety levels are greater for women who did not previously have any source of income. None of these differences, however, are statistically significant.

**Table 2.9: Impact of Pooled Treatment on Subjective Well-Being, Interaction with Baseline Income Status**

| VARIABLES             | (1)<br>Very<br>Satisfied | (2)<br>Poor Mental<br>Health Index | (3)<br>Anxiety<br>Level | (4)<br>Poor<br>Health | (5)<br>Sick in Past<br>6 Months | (6)<br>Less Financial<br>Decision-Making | (7)<br>Lacks<br>Autonomy |
|-----------------------|--------------------------|------------------------------------|-------------------------|-----------------------|---------------------------------|------------------------------------------|--------------------------|
| Received Treatment    | 0.075***<br>(0.026)      | -0.160*<br>(0.093)                 | -0.095*<br>(0.054)      | -0.072<br>(0.051)     | 0.011<br>(0.045)                | -0.005<br>(0.027)                        | 0.046<br>(0.051)         |
| No Personal Income    | 0.058*<br>(0.034)        | -0.263**<br>(0.125)                | -0.008<br>(0.069)       | -0.048<br>(0.063)     | 0.005<br>(0.056)                | 0.130***<br>(0.042)                      | 0.225***<br>(0.065)      |
| Treatment x No Income | -0.060<br>(0.041)        | 0.189<br>(0.142)                   | -0.014<br>(0.077)       | 0.075<br>(0.072)      | -0.045<br>(0.062)               | -0.013<br>(0.046)                        | -0.024<br>(0.073)        |
| Observations          | 1,005                    | 992                                | 1,005                   | 1,005                 | 1,005                           | 1,005                                    | 1,005                    |
| Control Mean          | 0.0705                   | 2.248                              | 0.577                   | 0.630                 | 0.211                           | 0.189                                    | 0.555                    |

Notes: This table presents the pooled treatment impact on various measures of subjective well-being, depending on the baseline income status of the respondent. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. *No Personal Income* is a dummy variable that takes on a value of 1 if the respondent did not have any source of personal income at the time of the baseline study. The dependent variable in Column 1 is a dummy variable that takes a value of 1 if the respondent reports being very satisfied with life. The dependent variable in Column 2 is a mental health index which represents the average value for 7 indicators measuring how often the respondent has exhibited signs of poor mental health in the past week. Each indicator takes a value from 0-3, with 3 being the most frequent. In Column 3, the dependent variable is a dummy representing whether the respondent often or always feels anxious about the future. Columns 4 and 5 are dummy variables that take a value of 1 if the respondent self-reports as being in generally poor health or having been sick in the past 6 months, respectively. Columns 6 and 7 are dummy variables that take a value of 1 if the respondent is not involved in many financial decisions in the household and if the respondent lacks autonomy to engage in various activities without permission, respectively. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

### 2.4.3 Effects on Views of the State

As the interventions employed in this study not only helped women eventually access a cash transfer but also helped them engage with the government and effectively navigate the bureaucracy, it is useful to understand whether the interventions changed the way citizens view the state or how they engage the state. This analysis can help build our understanding of citizen-state relations and potential channels for boosting citizen empowerment.

**Table 2.10: Impact of Pooled Treatment on Views of the State**

| VARIABLES          | (1)<br>View of State<br>Index | (2)<br>MLA Would<br>Help Me | (3)<br>Need to Know<br>MLA | (4)<br>MCD Would<br>Help Me | (5)<br>Need to<br>Know MCD |
|--------------------|-------------------------------|-----------------------------|----------------------------|-----------------------------|----------------------------|
| Received Treatment | 0.031<br>(0.020)              | 0.081**<br>(0.038)          | 0.038<br>(0.038)           | 0.051<br>(0.038)            | 0.012<br>(0.038)           |
| Observations       | 1,005                         | 1,005                       | 1,005                      | 1,005                       | 1,005                      |
| Control Mean       | 0.429                         | 0.520                       | 0.502                      | 0.507                       | 0.546                      |

Notes: This table presents the pooled treatment impact on views of the state as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variable in Column 1 is an index measuring views of the state, with higher values representing a more positive view. The dependent variables in Columns 2-5 are dummy variables that take a value of 1 if the respondent (2) believes her MLA would assist her if she asked (3) believes it is necessary to know the MLA to access government resources (4) believes her MCD Councilor would help her if she asked and (5) believes it is necessary to know the MCD Councilor to access government resources, respectively. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

Table 2.10 shows that while their overall perception of the state only slightly improves (Column 1), women who receive the interventions are 8 percentage points more likely to report that their MLA would help them if asked for personal assistance. Treated women are also more likely to report that their MCD Councilor (a more local political than the MLA) would help them, but the magnitude is smaller and insignificant (Column 4). Given that the MLA is the political figure that is responsible for authorizing pension applications, the fact that we see more positive updating on the MLA's helpfulness than the MCD Councilor's suggests that perceptions of politician's can be altered through direct engagement, which was facilitated by our interventions.

Table 2.10 also captures whether treated women are more likely to report that one must know the MLA to access government resources (Column 3) or that one must know the MCD Councilor to access government resources (Column 5). Both outcomes are positive but statistically insignificant, with the larger magnitude result seen in relation to needing to know the MLA. Therefore, while this is a weak result, it is suggestive that mobilizing more women to undergo a process that requires engagement with the MLA also increased their general perception that political connections to this type of figure play a role in resource

allocation. In other words, it may have increased the belief that allocation is not completely fair or objective.

**Table 2.11:** *Impact of Pooled Treatment on Views of the State, Interaction with Baseline Income Status*

| VARIABLES             | (1)<br>View of State<br>Index | (2)<br>MLA Would<br>Help Me | (3)<br>Need to Know<br>MLA | (4)<br>MCD Would<br>Help Me | (5)<br>Need to Know<br>MCD |
|-----------------------|-------------------------------|-----------------------------|----------------------------|-----------------------------|----------------------------|
| Received Treatment    | 0.028<br>(0.029)              | 0.109**<br>(0.055)          | 0.014<br>(0.053)           | -0.001<br>(0.053)           | -0.039<br>(0.054)          |
| No Personal Income    | -0.036<br>(0.036)             | 0.029<br>(0.070)            | -0.015<br>(0.069)          | -0.085<br>(0.069)           | -0.084<br>(0.069)          |
| Treatment x No Income | 0.005<br>(0.040)              | -0.058<br>(0.078)           | 0.050<br>(0.077)           | 0.105<br>(0.077)            | 0.102<br>(0.078)           |
| Observations          | 1,005                         | 1,005                       | 1,005                      | 1,005                       | 1,005                      |
| Control Mean          | 0.429                         | 0.520                       | 0.502                      | 0.507                       | 0.546                      |

Notes: This table presents the pooled treatment impact on views of the state as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. *No Personal Income* is a dummy variable that takes on a value of 1 if the respondent did not have any source of personal income at the time of the baseline study. The dependent variable in Column 1 is an index measuring views of the state, with higher values representing a more positive view. The dependent variables in Columns 2-5 are dummy variables that take a value of 1 if the respondent (2) believes her MLA would assist her if she asked (3) believes it is necessary to know the MLA to access government resources (4) believes her MCD Councilor would help her if she asked and (5) believes it is necessary to know the MCD Councilor to access government resources, respectively. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

To understand whether these outcomes vary for women based on baseline income levels, Table 2.11 shows results from the heterogeneity analysis. Interestingly, women who had no personal source of income at baseline are less likely to report that the MLA would help them and are more likely to report that knowing the MLA and MCD Councilor is necessary to access government resources. This suggests that perhaps more vulnerable women do not update positively from government engagement but instead it reinforces a view that the system is stacked against them. This may or may not reflect an actual difference in how poorer women are treated by politicians and government officials. The qualitative surveys, however, do indicate some level of frustration among more vulnerable women, at least on a literacy dimension, regarding how they are treated by officials:

*“They are rude with those who are illiterate. [...] Just because someone is illiterate does not mean they are stupid. They act like they do not have the time to explain the process/procedure*

to us. [...] We cannot do anything about it; we just have to accept their behavior and whatever they say.”

#### 2.4.4 Effects on Political Engagement

By helping women engage the state to access this important transfer program, it is possible that we also empowered them to be more independent and politically active in the future.

Table 2.12 shows who women report they turn to first when they need to obtain a public service. Two years after our interventions, women in the treatment group are 77% more likely to report that they turn first to themselves. This indicates a notable shift in self-reliance and confidence in navigating government systems. There is also a decline in the number of women who report turning first to the MLA or the MCD Councilor but no change in the number who turn to family and friends, government employees, or pradhans (slum leaders). Thus, it appears that the increase in self-reliance is being driven by a decrease on reliance on political channels of access, but citizens still do not seem to think of government employees, representing bureaucratic channels of engagement, as a viable channel for getting work done. This suggests an area of weak state capacity or visibility that could be strengthened.

**Table 2.12:** *Impact of Pooled Treatment on Who One Relies on to Access the State*

| VARIABLES          | (1)<br>Turn to<br>MLA | (2)<br>Turn to<br>MCD | (3)<br>Turn to<br>Self | (4)<br>Turn to<br>Fam/Friends | (5)<br>Turn to<br>Pradhan | (6)<br>Turn to Gov't<br>Employee |
|--------------------|-----------------------|-----------------------|------------------------|-------------------------------|---------------------------|----------------------------------|
| Received Treatment | -0.024<br>(0.035)     | -0.037<br>(0.027)     | 0.034*<br>(0.018)      | -0.007<br>(0.015)             | 0.014<br>(0.015)          | 0.005<br>(0.011)                 |
| Observations       | 1,005                 | 1,005                 | 1,005                  | 1,005                         | 1,005                     | 1,005                            |
| Control Mean       | 0.661                 | 0.163                 | 0.0441                 | 0.0441                        | 0.0441                    | 0.0176                           |

Notes: This table presents the pooled treatment impact on who respondents turn to first when they need to get something done from the government, as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variables in Column 1 – 6 represent dummy variables which take a value of 1 when the respondent turns first to (1) her MLA, (2) her MCD Councilor, (3) herself, (4) her family and friends, (5) her Pradhan (slum leader), and (6) a government employee, respectively. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, asset index, and the baseline measure for each given dependent variable. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

The heterogeneity analysis in Table 2.13 shows whether women who had no personal income at baseline have different outcomes than those who did. Interestingly, it appears that women with no baseline income see a larger increase in self-reliance than those who already had an income, suggesting that the interventions helped empower very poor women to engage with the government more independently.

**Table 2.13:** *Impact of Pooled Treatment on Who One Relies on to Access the State, Interaction with Baseline Income Status*

| VARIABLES             | (1)<br>Turn to<br>MLA | (2)<br>Turn to<br>MCD | (3)<br>Turn to<br>Self | (4)<br>Turn to<br>Fam/Friends | (5)<br>Turn to<br>Pradhan | (6)<br>Turn to Gov't<br>Employee |
|-----------------------|-----------------------|-----------------------|------------------------|-------------------------------|---------------------------|----------------------------------|
| Received Treatment    | -0.041<br>(0.049)     | -0.046<br>(0.039)     | 0.015<br>(0.024)       | 0.014<br>(0.016)              | 0.018<br>(0.023)          | 0.009<br>(0.011)                 |
| No Personal Income    | -0.022<br>(0.063)     | -0.047<br>(0.048)     | -0.024<br>(0.029)      | 0.044<br>(0.027)              | -0.009<br>(0.028)         | 0.022<br>(0.020)                 |
| Treatment x No Income | 0.034<br>(0.071)      | 0.018<br>(0.054)      | 0.040<br>(0.036)       | -0.043<br>(0.030)             | -0.009<br>(0.031)         | -0.008<br>(0.022)                |
| Observations          | 1,005                 | 1,005                 | 1,005                  | 1,005                         | 1,005                     | 1,005                            |
| Control Mean          | 0.661                 | 0.163                 | 0.0441                 | 0.0441                        | 0.0441                    | 0.0176                           |

Notes: This table presents the pooled treatment impact on who respondents turn to first when they need to get something done from the government, depending on the baseline income status of the respondent. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. *No Personal Income* is a dummy variable that takes on a value of 1 if the respondent did not have any source of personal income at the time of the baseline study. The dependent variables in Column 1 – 6 represent dummy variables which take a value of 1 when the respondent turns first to (1) her MLA, (2) her MCD Councilor, (3) herself, (4) her family and friends, (5) her Pradhan (slum leader), and (6) a government employee, respectively. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, asset index, and the baseline measure for each given dependent variable. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

Finally, I consider whether the interventions led women to be more politically active and engaged by increasing their confidence or comfort with state engagement. In Table 2.14, the impact of the treatments on partaking in various political activities are shown. Column 1 shows that women are 8 percentage points less likely to have made a political complaint in the past year if they are in any of the three treatment groups. While this may on one hand be interpreted as women being less politically active, it is also possible that because we helped them access one of the most important government programs available to their demographic, treated women simply had less reason to complain to politicians than women

in the control group. After all, the most common complaint women launched to politicians was in regards to the pension scheme, with 61% of women who made a complaint stating it was about widow pensions. The second highest category of complaints was in regards to water and not nearly as common, with only 20% of women making this complaint.

In looking at the remaining outcomes in Table 2.14, however, we see a similar trend in which the women in the treatment groups are actually less likely to partake in activities such as helping others apply for government programs, going to the MLA office, going to the MCD Councilor office, or applying for other schemes. While again, this may be because our interventions helped them obtain an important entitlement from the government so they had less need to interact with certain political figures or apply for other schemes, it also suggests the possibility of complacency and even apathy towards the state once certain resources are obtained. Fortunately, treated women are not less likely to vote (Column 3), suggesting that this avenue of political participation remains constant even after accessing welfare entitlements.

**Table 2.14:** *Impact of Pooled Treatment on Political Participation*

| VARIABLES          | (1)<br>Complained<br>to Politician | (2)<br>Helped<br>Others Apply | (3)<br>Voted MCD<br>Election | (4)<br>Went to<br>MLA | (5)<br>Went to<br>MCD | (6)<br>Applied for<br>Programs |
|--------------------|------------------------------------|-------------------------------|------------------------------|-----------------------|-----------------------|--------------------------------|
| Received Treatment | -0.079**<br>(0.034)                | -0.037<br>(0.032)             | 0.029<br>(0.027)             | -0.057<br>(0.038)     | -0.041<br>(0.031)     | -0.056<br>(0.037)              |
| Observations       | 1,005                              | 1,005                         | 1,005                        | 1,005                 | 1,005                 | 1,005                          |
| Control Mean       | 0.295                              | 0.260                         | 0.828                        | 0.445                 | 0.256                 | 0.392                          |

Notes: This table presents the pooled treatment impact on various dimensions of political participation, as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variables in Column 1 – 6 represent dummy variables which take a value of 1 if the respondent partook in the following activities in the past year (1) complained to an elected official about difficulties accessing public services or welfare programs, (2) helped other people in her community apply for government programs (3) voted in the MCD Councilor election (4) went to her MLA's office for any reason (5) went to her MCD Councilor's office for any reason (6) applied for government program(s) other than the widow pension scheme, respectively. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

Table 2.15 suggests that some dimensions of political participation by women who had no income at baseline is affected differently by the treatments than their average impact or

the impact on women who did have a baseline income. Treated women with no baseline income are more likely to vote, more likely to have gone to their MLA and MCD Councilor offices in the past year, and more likely to have applied for other schemes in the past year in comparison to treated women who already had a source of personal income at baseline. These differences, however, are only statistically significant for the outcome of applying for other schemes (Column 6). Still, the coefficients suggest that the decline in political engagement seen in Table 2.14 is driven by women who were already better off at baseline. Those who were worse off do not see substantial changes in levels of political engagement with politicians or application systems when considering the treatment impacts specifically on this subset. So for at least some women, political engagement remains important even after receiving assistance with accessing this pension scheme.

**Table 2.15:** *Impact of Pooled Treatment on Political Participation, Interaction with Baseline Income Status*

| VARIABLES             | (1)<br>Complained<br>to Politician | (2)<br>Helped<br>Others Apply | (3)<br>Voted MCD<br>Election | (4)<br>Went to<br>MLA | (5)<br>Went to<br>MCD | (6)<br>Applied for<br>Programs |
|-----------------------|------------------------------------|-------------------------------|------------------------------|-----------------------|-----------------------|--------------------------------|
| Received Treatment    | -0.069<br>(0.050)                  | -0.013<br>(0.048)             | -0.006<br>(0.037)            | -0.114**<br>(0.053)   | -0.053<br>(0.043)     | -0.123**<br>(0.050)            |
| No Personal Income    | -0.051<br>(0.063)                  | -0.010<br>(0.059)             | -0.067<br>(0.050)            | -0.031<br>(0.069)     | -0.003<br>(0.057)     | -0.115*<br>(0.067)             |
| Treatment x No Income | -0.022<br>(0.069)                  | -0.048<br>(0.067)             | 0.072<br>(0.055)             | 0.115<br>(0.076)      | 0.024<br>(0.064)      | 0.133*<br>(0.075)              |
| Observations          | 1,005                              | 1,005                         | 1,005                        | 1,005                 | 1,005                 | 1,005                          |
| Control Mean          | 0.295                              | 0.260                         | 0.828                        | 0.445                 | 0.256                 | 0.392                          |

Notes: This table presents the pooled treatment impact on various dimensions of political participation, depending on the baseline income status of the respondent. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. *No Personal Income* is a dummy variable that takes on a value of 1 if the respondent did not have any source of personal income at the time of the baseline study. The dependent variables in Column 1 – 6 represent dummy variables which take a value of 1 if the respondent partook in the following activities in the past year (1) complained to an elected official about difficulties accessing public services or welfare programs, (2) helped other people in her community apply for government programs (3) voted in the MCD Councilor election (4) went to her MLA's office for any reason (5) went to her MCD Councilor's office for any reason (6) applied for government program(s) other than the widow pension scheme, respectively. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

## 2.5 Conclusion and Policy Implications

Social safety nets provide essential resources to citizens to improve their circumstances and break out of poverty. As has been the case in many middle-income countries, India's booming economic growth has also coincided with increasing inequality, making government spending in the area of social protection vital to facilitate shared prosperity. With this research, I examine whether one of the primary social welfare programs in India, social pensions for targeted groups, is improving the well-being of recipients along several important dimensions. The implications of this research, however, are relevant much more broadly, given the spread of non-contributory unconditional cash transfer programs worldwide (Bastagli et al., 2016).

Here, I focus on social pensions offered to poor widows and other destitute women living in Delhi. This vital source of income can alleviate consumption constraints and mental strain facing women in these conditions. Yet, it is difficult to capture the impacts of such programs that are already operating at scale due to selection bias, as different types of women may be more or less likely to successfully navigate state systems and enroll in this transfer program. With the interventions employed in the first stage of this research, I randomly offered some women information and others information plus varying degrees of application assistance to enroll in this program. Therefore, I generated random variation in take-up of this UCT, allowing me to measure the impact of these interventions on economic, psychological, and political outcomes as evidence of the impact of receiving this government transfer.

I find that women in the pooled treatment group, those whose enrollment was in some way or another facilitated by our interventions, see notable improvements to their financial situation and mental health status two years after the intervention. I also find that these women are now more self-reliant in their interactions with government and have positively updated on the helpfulness of their MLA. Many of these results are larger for women who had no source of personal income at baseline, i.e. those who stood to benefit the most from having a reliable, guaranteed source of income for life. At the same time, I do not see

notable changes in physical health or household level empowerment, suggesting that these outcomes may be more sticky over time or require a larger or more prolonged income shock to exhibit notable shifts.

Overall, these results show an important promise of social protection programs in the form of unconditional cash payments for improving the well-being of vulnerable individuals, especially women. In terms of policy implications, this reiterates the importance of ensuring that such programs are effectively reaching all eligible individuals and that those who are in the greatest need of support are not systematically being left behind. In addition, the nature of this cash transfer, which provides regular, monthly payments directly in to recipients' bank accounts for life, resembles the universal basic income (UBI) model in some ways more than a traditional cash transfer program which provides only a lump sum transfer or incremental transfers for only a fixed duration of time. Therefore, these findings may be relevant for the debate on the effectiveness of UBI as a type of safety net for the poor.

While the implementation of UBI is different from targeted transfers and comes with different budgetary implications (Hanna and Olken, 2018), my findings show the importance of getting cash to individuals who are eligible for government support and are not necessarily able to access it given the stringent bureaucratic screenings that must be put in place for targeted programs. Therefore, it is possible that a country like India would be better off providing a single UBI to all of its citizens rather than having dozens of targeted programs which imperfectly reach intended beneficiaries and come with large overhead and administrative costs. In fact, estimates by India's Chief Economic Advisor suggest that a UBI could reduce poverty in the country by two-thirds (Sandefur, 2017), but would only be affordable if other programs are scaled back.

Of course there are many political and financial considerations to make in the implementation of UBI, including the targeting issue which is not automatically solved by making a benefit universal (Currie, 2004). In any case, these findings support the view that regular and unconditional cash payments from the government are making a difference in the lives of women who are able to receive them.

## Chapter 3

# Texting Criminals Out of Office: Experimental Evidence from India<sup>1</sup>

### 3.1 Introduction

Elections provide citizens with an essential platform to exercise political choice, hold politicians accountable, and elect leaders who can effectively represent their needs. Yet, adverse selection to political office continues to be a salient concern in low- and middle-income democracies. Given the growing evidence that politician identity matters for economic and policy outcomes (Besley et al., 2011a; Chattopadhyay and Duflo, 2004; Pande, 2003; Prakash et al., 2015), it is important to understand what sustains this low-quality equilibrium. Notably, adverse selection may be particularly consequential for governance and development in weakly institutionalized settings, where formal institutions impose fewer constraints on the in-office behavior of politicians.

In this paper, we aim to understand why Indian voters continue to elect criminals in to public office and measure whether mobile-based information campaigns can improve political selection. Criminality is a serious concern in Indian politics—34% of the Members of Parliament (MPs) in 2014, for example, were facing criminal charges, including many for

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<sup>1</sup>Co-authored with Siddarth George and Yusuf Neggers

violent crimes such as murder, rape, kidnapping, and armed robbery (Prakash et al., 2015). We carry out a large-scale field experiment to measure the role of information and voter coordination in determining voting behavior and election outcomes in this setting.

While there are many explanations for why voters may elect low-quality candidates to office, especially in contexts with pervasive ethnic voting (Banerjee and Pande, 2009) and vote-buying (Bratton, 2008; Vicente and Wantchekon, 2009), recent work has emphasized that information constraints also have an important role to play (Pande, 2011). Specifically, poorly informed voters may lack the ability to effectively screen candidates, and in the absence of good information about quality, voters may rely on noisy signals about a candidate's distributive and policy preferences conveyed by her ethnicity or religion (Banerjee et al., 2012). In addition, coordination failure amongst voters has also been discussed as one reason for poor political selection (Myerson, 1993, 1999), and low information levels may arguably exacerbate coordination problems.

Our experiment is designed to test whether better information can change political selection and the extent to which this information improves outcomes by helping voters break patterns of ethnic voting or coordinate their response to low quality candidates, i.e. criminals. We partner with 3 major telecom companies in India, with a combined average census-village-level market share of 40%, to run a mobile-based information campaign in the run-up to the 2017 state assembly elections of Uttar Pradesh. We blast about 865,000 mobile service subscribers living across 2,600 villages with various text and voice messages, randomizing at the village level. Each recipient receives a voice message and text message two days before the election, and for some interventions, individuals receive the same voice message again one day before the election.

In the 2,780 control villages, citizens receive no messages. In the treated villages, individuals receive one of 5 different intervention messages depending on which group their village is randomly assigned to. The first and most basic intervention provides citizens with a *placebo message*, which simply urges recipients to get to know their candidates and think carefully before casting their vote. Individuals in a second group of treatment

villages receive the *women's mobilization message*, which includes the placebo message plus a line encouraging female turnout. In a third group of treatment villages, individuals are given the *basic information message*, which includes the placebo message and in addition provides recipients with information on the criminal charges, if any, facing all of their major party candidates. A fourth intervention group receives the *information plus coordination message*, which includes the basic information message plus a public signal informing voters that many other citizens living in their area have received this same message. Finally, a fifth treatment group receives an *information plus ethnic-voting message* in which voters are provided criminality information and additionally urged to break the habit of voting along caste lines.

The criminality information that is shared with voters is taken from a publicly available database of candidate affidavits that each candidate must file by law in order to contest in an Indian election. Our analysis also relies on polling station level electoral data, which closely aligns with village boundaries, gathered from Uttar Pradesh Chief Election Office.

We find that, pooling across the three treatments which provide information on criminality, voters do reward clean candidates and punish criminals. Candidates facing no criminal charges receive 2 percent more votes due to the information interventions. Candidates with murder charges see a 13 percent reduction in votes and a resulting 3 percentage point decline in their vote shares.

In order to understand the mechanism behind this result, we compare the outcomes of the basic information treatment with that of the information plus coordination and the information plus ethnic-voting treatments. While the coefficients move in the same direction in each of the information treatments, the decline in vote share for candidates with murder charges is larger in magnitude and significant in the information treatments that include the coordination message or the ethnic-voting message. In other words, voters who received information about criminality and also a public signal that others also received this message were more likely to decisively punish criminals at the ballot box. This suggests that information on its own may not be sufficient to improve political selection in settings where

voters also face barriers to coordination. In addition, voters who received information about criminality and were also reminded not to vote along ethnic lines saw a larger shift away from criminal candidates than those only provided with information. Here again, we see that information on its own is not as effective without clear actionable implications, in this case encouraging breaking the habit of default, ethnic-based voting decisions.

Our findings contribute to a growing experimental literature that has shown moderately positive effects of voter information campaigns on electoral accountability and political selection (Pande, 2011). We add to this literature by providing evidence of the importance of voter coordination and dismantling default decision-making as mechanisms through which information can affect these outcomes. These results contrast with an explanation for criminal politicians which argues that individuals may actually prefer criminals as they are better at providing private benefits and selective goods (Vaishnav, 2012). Here, with better information, voters in aggregate switch away from criminals—reflecting some combination of changes in choice of candidate among voters and in who chooses to vote in the first place—in line with previous work from India which suggests that voters generally show an aversion to criminality or corruption (Banerjee et al., 2014).

Our work also builds upon existing work on voter information by evaluating an intervention that is easily scalable. It is difficult to scale insights from a majority of previous studies in developing countries because most experiments have been intensive, small-scale campaigns, often door-to-door or in village meetings (Banerjee et al., 2011; Arias et al., 2017). Scale is arguably best achieved via mobile-based interventions, given the high density of mobile phones in the developing world. While there is a growing literature on using mobile technology to encourage voter turnout before elections in rich countries (Dale and Strauss, 2009; Malhotra et al., 2011), we are only aware of two previous studies that have examined the effects of text messaging in the electoral setting in developing countries, both of which take place in sub-Saharan Africa (Aker et al., 2015; Marx et al., 2015). In addition, both of these studies utilized mobile technology to help “get out the vote” whereas our experiment is unique in that we provided candidate-specific information with this technology aimed at

affecting voter choice. By running our experiment in partnership with 3 large Indian telcos, we are able to reach over 50% of households in our treatment villages and can therefore better estimate the impact and cost-effectiveness of voter information campaigns at scale.

The remainder of this paper is structured as follows. Section 2 provides a background on elections and politics in Uttar Pradesh, the setting for this experiment. Section 3 details the research design, data, and analysis plan, and Section 4 discusses the results of the experiment. Section 5 concludes and offers remarks regarding the cost-effectiveness and policy implications of this work.

## **3.2 Background**

### **3.2.1 Indian Elections**

India became the world's largest democracy on August 15, 1947. Unlike many developing countries have that transitioned in and out of democratic rule over the past century, India has maintained its democratic tradition with free and fair elections for the past 70 years. Yet this has not come without its challenges, as Indian politics is mired with ethnic fractionalization (Chandra, 2007), corruption (Bussell, 2012), and criminality (Prakash et al., 2015). India also has over 200 political parties, with 7 parties officially recognized as National parties and the remaining categorized as state or regional parties. A unique feature of Indian politics is that all registered parties must have an associated symbol, and these symbols can be at times more widely recognized than the name of the party itself.

India is a federal parliamentary republic, with both national and state legislatures elected every five years on a rotating basis. Elected representatives in state assemblies are referred to as Members of the Legislative Assembly (MLAs), and they oversee single member constituencies after winning first-past-the-post elections. MLAs have many constitutionally sanctioned responsibilities, enjoy control over bureaucratic promotions and transfers (Iyer and Mani, 2012), and play a significant role in resource allocation within their state and constituency. Thus it is feasible that the quality of candidates elected to this position should

matter for economic and development outcomes, and empirical evidence shows that Indian MLAs facing criminal charges generate 22-percentage point lower economic activity than their non-criminal counterparts (Prakash et al., 2015).

### **3.2.2 Uttar Pradesh**

Our study focuses on the state assembly elections of Uttar Pradesh (UP), a state in Northern India. With a population of over 200 million, UP is the most populous state of India and the largest subnational unit in the world. It is also among the poorest states of India, with low human development indicators and a largely rural population. Still, following the trend across the developing world, India has seen an increase in mobile usage over the past several years, making mobile technology a feasible way to spread information widely in this context, even in rural areas (GSMA, 2016).

UP has historically been rife with ethnic politics and political corruption. Nearly 22% of MLAs elected to office in 2012 faced serious criminal charges like murder, rape, kidnapping and extortion (Association for Democratic Reform, 2012). Politician quality is lowest in ethnically lop-sided constituencies, suggesting that voters may be trading off ethnic preferences for candidate quality (Banerjee and Pande, 2009). Electoral participation of marginalized groups (e.g. women), who are arguably disproportionately affected by criminal politicians, is also low.

Given this political, economic, and social backdrop, UP provides an ideal setting for testing a mobile-based information campaign aimed at increasing voter awareness of candidate criminality, allowing voters to better coordinate on their preferences, and breaking default habits of ethnic voting.

### **3.2.3 Uttar Pradesh State Assembly Elections 2017**

Given the sheer size of the electorate in UP, elections take place in phases. In 2017, there were 7 election phases, one every 4 days between February 11, 2017 – March 8, 2017. Each

phase of the election covered 40-75 of the total 403 constituencies in UP<sup>2</sup>. There were four major parties vying for power in the elections: the Bharatiya Janata Party (BJP), the Bahujan Samaj Party (BSP), the Indian National Congress (Congress), and Samajwadi Party (SP). These parties are represented by the following party symbols: a lotus flower, an elephant, an open hand, and a cycle, respectively. This election was a landslide victory for the BJP, who won 325 seats, representing over a three-fourths majority.

As recent trends from UP would have predicted, there were many candidates facing criminal charges standing for office during the 2017 elections. To give a sense of how common criminality is in this context, Table 3.1 provides a snapshot of the criminal charges facing the major party candidates standing for election in Phase 4 in the 38 constituencies targeted with our interventions. As seen in the table, of the 119 candidates captured in our dataset, 35% face at least one criminal charge, and over 40% of these individuals face charges for violent crimes.

**Table 3.1:** *Criminal Charges of Major Party Candidates, Uttar Pradesh State Elections 2017, Phase 4*

| <b>Measures</b>                                            |              |
|------------------------------------------------------------|--------------|
| Candidates with Any Criminal Charge                        | <b>35.3%</b> |
| Candidates with only Non-Violent Charges                   | <b>20.2%</b> |
| Candidates with Violent Charges, No Murder-Related Charges | <b>6.7%</b>  |
| Candidates with at least one Attempt to Murder Charge      | <b>5%</b>    |
| Candidates with at least one Murder Charge                 | <b>3.4%</b>  |
| <b>Observations</b>                                        | <b>119</b>   |

Notes: This table presents the average number of criminal charges facing 119 of the major party candidates standing for election to the state legislative assembly in Uttar Pradesh in 2017. The averages shown above represent charges against candidates in the 38 assembly constituencies covered by our study in Phase 4 of the election. Row 1 shows the percentage of candidates with any criminal charge, whereas rows 2-5 break down the total average in Row 1 by type of charge. Data on criminal charges comes from publically available candidate affidavits filed with the UP State Election Commission before the election.

<sup>2</sup>See Appendix for a constituency-level map of Uttar Pradesh.

Figure 3.1 and Figure 3.2 show the breakdown of criminality by political party, and we see that parties do vary to some extent in the nature of criminality facing their candidates. The BJP stands out as having both the highest percentage of criminals and the highest percentage of violent criminals. At the other extreme, while the Congress party has a moderate number of criminal candidates, non of them face violent criminal charges. To ensure that these differences across parties are not driving our results, we include specifications with party fixed-effects in each of our main regression tables.

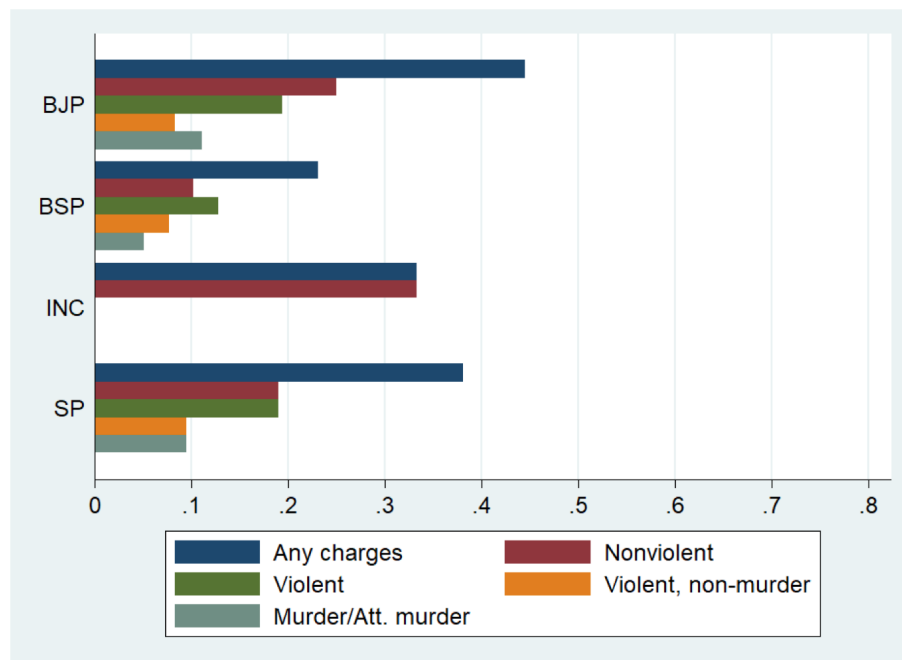


**Figure 3.1:** *Criminality by Political Party, Types of Charges*

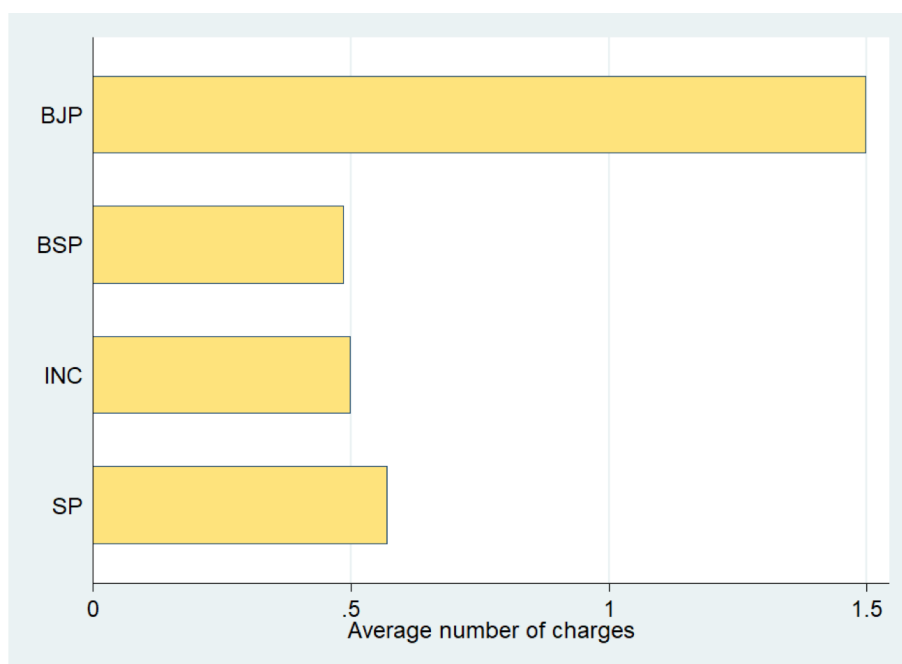


**Figure 3.2:** *Criminality by Political Party, Number of Charges*

What does voting for criminals look like in UP in the absence of our interventions? Table 3.2 shows the result of running fixed effects regressions of vote share on candidate criminality in the control villages in our sample. As seen in Column 1, criminal candidates on average receive a 6 percentage point higher vote share than non-criminal candidates, but this general result does not hold when we include party fixed effects in Column 2. In Columns 3 and 4, we compare candidates with non-violent criminal charges to those with violent criminal charges. Here, we see that even when accounting for party fixed effects, violent criminals are rewarded at the ballot box. Column 4 in Table 3.2 shows that violent criminals on average receive vote shares that are almost 5 percentage points higher than non-criminal candidates. Thus, at status quo, it does not appear that citizens are punishing criminals and may even reward them at the polls. With our study, we aim to understand the role of information, or lack thereof, regarding the criminal charges facing candidates in explaining this relationship.

**Table 3.2:** Electoral Advantage for Criminals? Voting Patterns in the Control Group

|                 | (1)<br>Vote Share    | (2)<br>Vote Share  | (3)<br>Vote Share   | (4)<br>Vote Share   |
|-----------------|----------------------|--------------------|---------------------|---------------------|
| Criminal        | 0.0637**<br>(0.0279) | 0.0250<br>(0.0183) |                     |                     |
| Non-Violent     |                      |                    | 0.0297<br>(0.0325)  | 0.00723<br>(0.0208) |
| Violent         |                      |                    | 0.102**<br>(0.0398) | 0.0468*<br>(0.0242) |
| Adjusted $R^2$  | 0.058                | 0.214              | 0.067               | 0.216               |
| Constituency FE | Yes                  | Yes                | Yes                 | Yes                 |
| Party FE        | No                   | Yes                | No                  | Yes                 |
| Observations    | 8711                 | 8711               | 8711                | 8711                |

Notes: This table shows results from polling-station-by-candidate-level regressions of vote share on criminality in the control group from Phase 4 of the UP State Assembly Elections of 2017. In Columns 1 and 2, the independent variable *Criminal* is a dummy that takes the value of 1 if the candidate faces any criminal charges, regardless of type. In Columns 3 and 4, the independent variable *Non-Violent* is a dummy variable that takes a value of 1 if the candidate faces charges that are all non-violent, and *Violent* is dummy variable that takes a value of 1 if the candidate faces at least 1 violent criminal charge. All regressions include controls for total population, share female, share SC/ST, share literate, number of registered voters, and constituency fixed-effects. Columns 2 and 4 also include party fixed-effects. Standard errors, clustered at the village and candidate level, are shown in brackets. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

### 3.3 Research Design

#### 3.3.1 Intervention Structure

##### Message Content and Execution

In order to carry out our intervention, we partnered with 3 major telecom companies in India: Idea, Airtel, and Vodafone. On average, the combined coverage rates (in terms of subscribers/village population) of these three companies in the villages in our sample is 44%. For each treatment village, we blasted all mobile users with numbers registered to the given village jurisdiction under these telecom companies. The script of the voice message is longer and more detailed than that of the text message, but both convey similar information.<sup>3</sup> Voice messages were recorded in Hindi, and text messages were sent in the

<sup>3</sup>See Appendix for English script of the voice and text messages used in each treatment.

Hindi language and Devanagari script.

It is common for citizens to receive voice and text messages in the days leading up to an election from different parties and civil society groups. But all parties must strictly adhere to a freeze on campaigning that goes in to effect 1 day before the election. To ensure that our messages were perceived differently from party-driven campaigning or propaganda, our messages were sent on behalf of our implementation partner, the Center for Governance and Development (CGD) in India, a non-profit, non-partisan political watchdog organization.

Villages that were assigned to the control group did not receive any voice or text message blasts. The remaining villages were randomly assigned to receive one of the following interventions:

**1. Placebo Message:** This message does not include any criminality information but instead simply urges citizens to learn about their candidates and make well-formed decisions on election day. This message is included in the study to determine whether voters respond to any prompt that focuses their attention on learning about candidate quality. If voters shift support away from criminal candidates in response to the placebo message to the same extent as they do in response to a message providing detailed criminality information, it suggests that voters simply need a reminder to learn about their candidates but are not necessarily information constrained about criminality.

The voice blast used in this treatment is below, and the text message script is an abridged version of the same message:

*"This message is from an unbiased, non-political NGO, Centre for Governance and Development. Get to know your candidates correctly, and on Election Day, give your vote only after thinking carefully!"*

**2. Women's Mobilization Message:** This message contains the same content as the placebo message and adds a line encouraging females to turn out to vote. The purpose of this intervention is two-fold. One, given the traditionally low turnout of female voters in UP even in comparison to other Indian states, we are interested in understanding if this kind of mass media campaign can facilitate higher turnout. In addition, it is possible that

women have different preferences regarding politician characteristics than men, especially as it pertains to candidate criminality, so increasing female turnout may also indirectly decrease vote share of criminals. The script used for the voice message in this intervention is as follows:

*“This message is from an unbiased, non-political NGO, Center for Governance and Development. Women across the country are voting in record numbers. When all household members vote, your family’s power increases. So definitely encourage all women to vote! Get to know your candidates correctly, and on Election Day, give your vote only after thinking carefully!”*

**3. Basic Information Message:** This message contains the same content as the placebo message, but in addition provides detailed information regarding the criminal charges facing the major party candidates in the recipient’s constituency. Below is an example of the content of this message for a constituency in which the major party candidates range from facing no criminal charges to facing serious violent charges:

*“This message is from an unbiased, non-political NGO, Center for Governance and Development. Get to know your candidates correctly, and on Election Day, give your vote only after thinking carefully! In your area: 1. Madhusudan Kushwaha from BSP (elephant party) has 1 criminal case, with attempt to murder charges. 2. Prakash Dwivedi from BJP (lotus party) has no criminal cases. 3. Vivek Kumar Singh from Congress (hand party) has 3 criminal cases, but has no violent charges.”*

As seen in the above example, the criminality information includes the name of the candidate, the name of the party he or she represents, the symbol associated with the given party, and the nature of pending criminal charges against the candidate, if any. When a candidate has any criminal charges, the number of charges is stated, followed by the specific charges for any violent crimes. If a candidate faces only non-violent criminal charges, after the number of charges, the message informs listeners that none of the charges are violent. If the candidate faces no criminal charges, the message informs listeners that the given candidate faces no criminal charges.

By structuring the messages in this way, we allow citizens to not only learn about the magnitude of criminal charges facing a given candidate, but also differentiate between

violent and non-violent charges, as presumably these may weigh differently in their voting decisions. Following the categorization standards of the legal code in India, the following crimes are included in the violent category in our messages: murder, attempt to murder, culpable homicide, attempt to commit culpable homicide, rape, attempt to commit rape, kidnapping and abduction, dacoity, preparation and assembly for committing dacoity, robbery, and arson. For all messages containing criminality information, the order of the candidates was randomized to avoid any biases in rank order affecting voter response and thus interpretation of the outcome.

**4. Information plus Coordination Message:** This message has the same content as the basic information message but also emphasizes to listeners that the message has been shared widely to other individuals living in their area. This intervention is designed to test whether voters respond more strongly to information when they receive a public signal that allows them to coordinate their response with other voters. This message uses the following format:

*“This message is from an unbiased non-political NGO, Center for Governance and Development, and many people in your area have already received it. Get to know your candidates correctly, and on Election Day, give your vote only after thinking carefully! In your area: <<Insert Criminality Information of Candidates>>. Now you can elect the right candidate with the people in your area.”*

**5. Information plus Ethnic Voting Message:** This message also builds upon the basic information message, but instead of adding the coordination element, it urges citizens to break the habit of ethnic voting. This intervention is designed to test whether criminality information is used more seriously to update voting behavior when it is coupled with a message that draws one’s attention to flaws in default decision-making which are likely to undermine such information. Given the history of ethnic politics and voting in this context, this is an important channel to test or rule out when trying to understand how voters respond to information about candidate quality. This message contains the following:

*“This message is from an unbiased, non-political NGO, Center for Governance and Development. Don’t follow your old habits and vote only on the basis of caste or religion. Get to know your*

*candidates correctly, and on Election Day, give your vote only after thinking carefully! In your area: <<Insert Criminality Information of Candidates>>.”*

Due to different contractual agreements with different telecom companies, the above-mentioned interventions occurred in different Phases of the election and some followed slightly different blast protocols than others. The placebo message was only sent to voters in Phase 7 of the elections, which was held on March 8, 2017, and individuals received one voice and one text message two days before the election and the same voice message again the day before the election. The women’s mobilization message was sent both in Phase 4 and in Phase 7 of the election, and in both phases, the blasting protocol was the same as the placebo message. In other words, individuals who received women’s mobilization message received one voice and one text message two days before the election, and the same voice message again the day before the election.

All three of the interventions that include information on candidate criminality (interventions 3, 4, and 5 as described above) were only carried out in Phase 4 of the election, held on February 23, 2017. In villages that were selected to receive any of these three interventions, individuals were sent one voice message and one text message with the corresponding information two days before the election. Unlike the women’s mobilization and placebo interventions, however, they were not sent another voice message on the day before the election.

In total, the placebo message was sent to registered mobile users of the Airtel company across 450 villages, and the women’s mobilization message was sent to Airtel customers across 785 villages. Each of the three information interventions was sent to Vodafone and Idea customers across about 460 villages. The next section provided details on the sampling and randomization.

### **Sample Selection and Randomization**

Phase 4 of the elections covered 11 districts containing 53 assembly constituencies (ACs). Due to the unavailability of reliable polling station location data, the 2 districts of Allahabad

and Chitrakoot were excluded from our experiment, leaving an initial set of 39 ACs and 9,627 census villages. To avoid urban areas, where cross-contamination of treatment and control areas was a greater concern, and to improve our statistical power (by reducing the number of observations per treatment unit), we further restricted our sample to villages with between 1 and 2 polling stations. In addition, we excluded extremely large or small census villages from our sample, dropping villages in the top and bottom percentiles of the district-level distribution in terms of total population (corresponding roughly to populations below 150 and greater than 5,150). Finally, given that the delivery of our treatments relied on mobile phone possession, we also excluded villages in the bottom percentile in terms of village-level population share covered as Vodafone+Idea subscribers (corresponding to roughly a 10.6% coverage rate or lower). Together, these yield an experimental sample of 4,131 villages across 38 ACs.

We aimed to reach approximately 500,000 individuals with our messaging campaigns in Phase 4. 80 percent of this total was allocated equally across the 3 candidate-information arms (basic information, information plus ethnic-voting, and information plus coordination) and 20 percent to the women's mobilization arm, with the total number of treated individuals allocated across ACs in proportion to their populations. Villages were then randomly assigned across the 4 treatment arms and control, stratifying by: AC; number of polling stations contained within; above/below AC-level median Vodafone+Idea subscriber share of population; and above/below AC-level median Vodafone+Idea number of subscribers.

Phase 7 of the elections covered 7 districts containing 40 ACs. We were able to run messaging campaigns in 3 districts covering 13 ACs and 5,081 villages. After excluding 2 ACs due to polling-station-location data quality concerns, we then followed the same procedure as in Phase 4 to define our experimental sample, yielding 1,276 villages across 11 ACs. For Phase 7, we aimed to reach approximately 250,000 individuals, split evenly between our women's mobilization and placebo arms and again allocating numbers of treated individuals in proportion to total AC-level population. Using the same strata as in Phase 4, villages were then randomly assigned across the 2 treatment arms and control.

To alleviate potential concerns regarding the validity of the randomization before proceeding with analysis of our experiment, we follow the standard approach of testing for balance in a set of observable pre-election characteristics across our treatment and control villages. In particular, we consider in Table 3.3 the following village-level variables: total population, female population share, SC/ST population share, literacy rate, total number of registered voters, number of polling stations. We see that the average village in our sample has roughly 1,500 inhabitants. Roughly 48 percent of these individuals are female, 30 percent are SC/ST, and 56 percent are literate. In addition, there are roughly 1,200 total registered voters and 1.3 polling stations per village on average. Examining the coefficients from a regression of each village-level characteristic on a set of treatment dummies and the set of randomization strata reveals no systematic significant differences. In particular, the equality of only 4 out of 30 comparisons can be rejected at the 10 percent level or below, approximately the rate that would be expected by chance.

**Table 3.3:** *Balance Check Across Treatment and Control Groups*

|                  | Control            | Women              | Basic<br>Info      | Info +<br>Coord    | Info +<br>Ethnic   | Placebo            | (1) vs.<br>(2)   | (1) vs.<br>(3)      | (1) vs.<br>(4)    | (1) vs.<br>(5)    | (1) vs.<br>(6)   |
|------------------|--------------------|--------------------|--------------------|--------------------|--------------------|--------------------|------------------|---------------------|-------------------|-------------------|------------------|
|                  | (1)                | (2)                | (3)                | (4)                | (5)                | (6)                | (7)              | (8)                 | (9)               | (10)              | (11)             |
| Population       | 1514.08<br>(17.78) | 1351.27<br>(32.41) | 1565.51<br>(46.16) | 1550.65<br>(44.06) | 1527.69<br>(44.42) | 1249.97<br>(41.43) | 21.60<br>(33.23) | -20.11<br>(34.82)   | -11.24<br>(34.71) | -14.69<br>(34.95) | 35.35<br>(46.70) |
| Share Female     | 0.48<br>(0.00)     | 0.48<br>(0.00)     | 0.48<br>(0.00)     | 0.48<br>(0.00)     | 0.48<br>(0.00)     | 0.48<br>(0.00)     | 0.00<br>(0.00)   | 0.00<br>(0.00)      | 0.00<br>(0.00)    | 0.00<br>(0.00)    | 0.00<br>(0.00)   |
| Share SC/ST      | 0.29<br>(0.00)     | 0.34<br>(0.01)     | 0.30<br>(0.01)     | 0.29<br>(0.01)     | 0.29<br>(0.01)     | 0.35<br>(0.01)     | -0.01<br>(0.01)  | -0.01*<br>(0.01)    | 0.00<br>(0.01)    | -0.01<br>(0.01)   | 0.02*<br>(0.01)  |
| Share Literate   | 0.57<br>(0.00)     | 0.56<br>(0.00)     | 0.57<br>(0.00)     | 0.57<br>(0.00)     | 0.57<br>(0.00)     | 0.55<br>(0.00)     | 0.00<br>(0.00)   | 0.00<br>(0.00)      | 0.00<br>(0.00)    | 0.00<br>(0.00)    | 0.00<br>(0.01)   |
| Reg. Voters      | 1229.05<br>(11.23) | 1223.07<br>(21.75) | 1225.77<br>(27.58) | 1181.43<br>(26.42) | 1223.17<br>(25.97) | 1240.87<br>(30.47) | 14.13<br>(18.14) | -44.01**<br>(19.09) | 10.15<br>(19.30)  | -23.77<br>(19.13) | -4.11<br>(26.69) |
| Polling Stations | 1.34<br>(0.01)     | 1.30<br>(0.02)     | 1.32<br>(0.02)     | 1.31<br>(0.02)     | 1.33<br>(0.02)     | 1.28<br>(0.02)     | 0.00<br>(0.00)   | -0.01<br>(0.00)     | 0.01***<br>(0.01) | 0.00<br>(0.00)    | 0.00<br>(0.01)   |
| N                | 2641               | 759                | 441                | 436                | 435                | 439                | 3400             | 3082                | 3077              | 3076              | 3080             |

Notes: This table compares average characteristics of all of the villages in our sample across the control and treatment groups. Columns 1 – 6 show the means for the following: (1) the control group (2) the women's mobilization message group (3) the basic information message group (4) the information plus coordination message group (5) the information plus ethnic voting message group and (6) the placebo group, respectively. Columns 7-11 show the difference in means between each treatment group and the control group. Stars are used to indicate whether the differences in means are statistically different from one another. \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

### 3.3.2 Data

This analysis relies on several sources of data. The candidate criminality information that is inserted in the voice and text messages and used in the subsequent analysis comes from publicly available candidate affidavits filed with the Election Commission of India. In 2003, the Indian Supreme Court made it mandatory for all candidates to the national or state legislative bodies of India to file sworn affidavits which provide details of their education, assets, liabilities, and criminal convictions or charges. Given strict penalties for lying and strong incentives for opposition parties or media to uncover such lies, misreporting in such affidavits is considered to be minimal (Prakash et al., 2015).

As these affidavits are only filed during the nomination period (roughly 15-19 days before the election), we had a very short window of time to turn the data around to share with our telecom partners in order to blast the information to citizens before the elections. Therefore, we did not wait for the information to be summarized online and instead manually gleaned the information from each individual affidavit for all of the major party candidates standing for election in the constituencies selected for this study.

For demographic information about the villages in our sample, we used data from the 2011 Indian National Census. This dataset provided us with population numbers, caste composition, literacy rates, gender composition, land area, and land usage patterns for each village. In addition, we used 2011 census village shapefiles acquired from ML InfoMap (2011) together with polling station GPS coordinates from the dataset of Susewind (2014), which allowed us to spatially match polling stations to census villages in order to conduct the polling-station- and polling-station-by-candidate-level analyses of our results.

We obtained polling station level electoral data from the Uttar Pradesh Office of the Chief Electoral Officer (CEO). This includes the total turnout in each polling station as well as turnout by gender. It also shows the total number of registered voters (referred to as “electors”) in each polling station and the number of votes given to each candidate.

Finally, our telecom partners provided us with data on their mobile coverage rates in the villages covered in this study as well as pick-up rates and duration of listening rates for

the voice messages that we sent. This data will be used to calculate various measures of treatment intensity in future analysis.

### 3.3.3 Empirical Strategy

Given the randomized assignment of treatment status, our core empirical strategy is straightforward. We first examine the electoral impacts of the information campaigns on candidates of different types, estimating the following baseline polling-station-by-candidate-level regressions:

$$Y_{cpva} = \alpha_a + \beta Info_{va} + \theta Crim_{ca} + \phi (Info_{va} * Crim_{ca}) + \mathbf{X}'_{pva} \lambda + \epsilon_{cpva}. \quad (3.1)$$

$Y_{cpva}$  is a voting outcome (vote share or log votes) for candidate  $c$  at polling station  $p$  in census village  $v$  in assembly constituency  $a$ ,  $Crim_{ca}$  is an indicator taking value 1 if a candidate has any criminal charges and 0 otherwise, and  $\alpha_a$  are assembly constituency fixed effects.  $Info_{va}$  is an indicator for whether subscribers in a given village received any of the information treatments (basic information, information plus ethnic-voting, information plus coordination). The set of additional controls,  $\mathbf{X}$ , includes village-level characteristics (log population, share female, share SC/ST, share literate), the log number of registered voters at the polling station, and fixed effects for the remaining randomization strata (number of polling stations contained within; above/below AC-level median Vodafone+Idea subscriber share of population; and above/below AC-level median Vodafone+Idea number of subscribers). Standard errors are clustered by both village and candidate. We exclude the sample of villages receiving the placebo or women's mobilization treatments from this analysis. As a robustness check, we also consider versions of this specification which additionally include party fixed effects, or replace the assembly constituency fixed effects with candidate fixed effects (which are equivalent to assembly-constituency-by-party fixed effects).

In the above regression,  $\beta$  gives the average effect of receiving any of the information treatments for candidates with no criminal charges, and  $\phi$  gives the marginal effect of

treatment for criminally-charged candidates. We also consider a specification that allows for heterogeneity in impacts by the different information treatments, where we replace the combined candidate-information-campaign indicator in equation 3.1 with three separate dummies corresponding to inclusion in each arm of the information intervention. Further, we allow for heterogeneity in treatment effects by severity of criminal charges, replacing the criminality dummy with a set of indicator variables capturing into which of a set of mutually exclusive categories of most-severe charge types each candidate falls: non-violent, violent unrelated to murder, attempted murder, and murder.

When considering the potential effects of the information messaging campaigns on overall turnout at polling stations, we consider specifications of the form:

$$Y_{pva} = \alpha_a + \beta Info_{va} + \phi (Info_{va} * AnyCrim_a) + \mathbf{X}'_{pva} \lambda + \epsilon_{pva}. \quad (3.2)$$

Here, the outcome of interest is the log votes cast in total (or separately by gender) at polling station  $p$  in village  $v$ .  $AnyCrim_a$  is an indicator variable for whether one or more of the 3 candidates from whom criminality information was provided in the assembly constituency had criminal charges. The assembly-constituency fixed effects absorb the main effect, while the interaction of  $AnyCrim_a$  with  $Info_{va}$  allows for the turnout effects of the information treatments to vary with the nature of the candidate information revealed. As the pattern of criminal charges facing the three main party candidates included in the information treatments may be correlated with other outcomes that influence voting, the interpretation of the coefficient on the interaction term is necessarily suggestive. We also consider versions of the above specification which include an additional interaction of the information treatment indicator with an indicator for whether any of the candidates for whom information was provided had murder charges; or replaces the  $AnyCrim_a$  interaction with interactions with dummies for whether 1, 2, or 3 of those candidates had criminal charges. Standard errors are clustered by census village.

## 3.4 Results

### 3.4.1 Impact of Information on Electoral Outcomes

A primary outcome of interest in this analysis is whether or not the information-based interventions changed the voting decisions of citizens. Table 3.4 shows how the polling station level vote shares of candidates with different criminal backgrounds changed as a result of the information interventions, pooling across all three interventions that provided criminality information. Columns 1-3 show the change in vote shares for those without and those with any criminal charge, regardless of the nature of the charge. In Column 2, the preferred specification which includes both constituency and party fixed-effects, we see that candidates without a criminal charge saw a small but significant increase in their vote share, and those that do have criminal charges saw an overall decrease in their vote shares. However, these magnitudes are quite small in both cases.

**Table 3.4:** *Impact of Information on Vote Shares of Criminal Candidates*

|                            | (1)<br>Vote Share     | (2)<br>Vote Share      | (3)<br>Vote Share     | (4)<br>Vote Share       | (5)<br>Vote Share       | (6)<br>Vote Share       |
|----------------------------|-----------------------|------------------------|-----------------------|-------------------------|-------------------------|-------------------------|
| Info                       | 0.00309*<br>(0.00179) | 0.00306**<br>(0.00146) | 0.00327<br>(0.00213)  | 0.00288*<br>(0.00170)   | 0.00292*<br>(0.00148)   | 0.00327<br>(0.00291)    |
| Info x Criminal            | -0.00525<br>(0.00734) | -0.00517<br>(0.00693)  | -0.00577<br>(0.00718) |                         |                         |                         |
| Info x Non-Violent         |                       |                        |                       | -0.00307<br>(0.00932)   | 0.00177<br>(0.00885)    | 0.00229<br>(0.00922)    |
| Info x Violent, Non-Murder |                       |                        |                       | 0.00898<br>(0.0163)     | -0.000480<br>(0.0156)   | -0.00506<br>(0.0150)    |
| Info x Murder Attempt      |                       |                        |                       | -0.0157<br>(0.0197)     | -0.0163<br>(0.0178)     | -0.0160<br>(0.0180)     |
| Info x Murder              |                       |                        |                       | -0.0262***<br>(0.00868) | -0.0318***<br>(0.00711) | -0.0348***<br>(0.00623) |
| Adjusted $R^2$             | 0.057                 | 0.215                  | 0.383                 | 0.076                   | 0.225                   | 0.384                   |
| Constituency FE            | Yes                   | Yes                    | No                    | Yes                     | Yes                     | No                      |
| Party FE                   | No                    | Yes                    | No                    | No                      | Yes                     | No                      |
| Candidate FE               | No                    | No                     | Yes                   | No                      | No                      | Yes                     |
| Observations               | 13686                 | 13686                  | 13683                 | 13686                   | 13686                   | 13683                   |

Notes: This table shows results from polling-station-by-candidate-level regressions of vote share on the pooled information treatment interacted with different types of criminality. All data represented in this table comes from Phase 4 of the UP State Assembly Elections of 2017. In Row 1, the independent variable *Info* is a dummy that takes the value of 1 if the candidate is standing in a polling station that was given any one of the three information-based interventions. In Rows 2-6, the independent variables are interactions of the *Info* variable with: *Criminal*, a dummy that takes the value of 1 if the candidate faces any criminal charges; *Non-Violent*, a dummy that takes a value of 1 if the candidate faces charges that are all non-violent; *Violent, Non-Murder*, a dummy that takes a value of 1 if the candidate faces at least one violent charge but no charges related to murder; *Murder Attempt*, a dummy that takes a value of 1 if the candidate faces at least one attempt to murder charge but no murder charges; and *Murder*, a dummy that takes the value of 1 if the candidate faces at least one murder charge. Regressions include controls for total population, share female, share SC/ST, share literate, number of registered voters, and constituency fixed-effects, party fixed-effects, or candidate fixed-effects, as indicated by “Yes” or “No” in the bottom panel. Standard errors, clustered at the village and candidate level, are shown in brackets. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

In Columns 4-6 of Table 3.4, we examine more closely how voters respond to different types of criminal charges facing their major party candidates. In this case, it is clear that voters punish candidates more strongly based on the severity of their crimes, as there is a large and significant overall decrease in vote share for potential murderers by 3 percentage points. Those with attempt to murder charges see a smaller decline in overall vote share, and those with non-violent charges see essentially no difference in vote share in comparison to clean candidates.

While vote share of candidates is a useful and practical measure to consider in terms of

what determines final election results, it also has limitations in its interpretation. After all, changes in vote shares may be driven by an increase in votes for Candidate A or decrease in votes for Candidate B, and it is not possible to tease apart the two potential mechanisms. Therefore, in Table 3.5, we present the same analysis as seen in Table 3.4 but use log of total votes received for each candidate at the polling station level as the outcome of interest rather than candidate vote share.

**Table 3.5: Impact of Information on Total Votes for Criminal Candidates**

|                            | (1)<br>Log Votes     | (2)<br>Log Votes     | (3)<br>Log Votes    | (4)<br>Log Votes     | (5)<br>Log Votes     | (6)<br>Log Votes     |
|----------------------------|----------------------|----------------------|---------------------|----------------------|----------------------|----------------------|
| Info                       | 0.0220**<br>(0.0102) | 0.0216**<br>(0.0101) | 0.0239*<br>(0.0128) | 0.0212**<br>(0.0103) | 0.0211**<br>(0.0105) | 0.0239*<br>(0.0128)  |
| Info x Criminal            | -0.0347<br>(0.0354)  | -0.0344<br>(0.0326)  | -0.0388<br>(0.0327) |                      |                      |                      |
| Info x Non-Violent         |                      |                      |                     | -0.0427<br>(0.0450)  | -0.0247<br>(0.0427)  | -0.0261<br>(0.0442)  |
| Info x Violent, Non-Murder |                      |                      |                     | 0.0601<br>(0.0589)   | 0.0231<br>(0.0538)   | 0.00610<br>(0.0501)  |
| Info x Murder Attempt      |                      |                      |                     | -0.0586<br>(0.0667)  | -0.0595<br>(0.0584)  | -0.0663<br>(0.0560)  |
| Info x Murder              |                      |                      |                     | -0.132*<br>(0.0717)  | -0.152**<br>(0.0751) | -0.159**<br>(0.0712) |
| Adjusted R <sup>2</sup>    | 0.213                | 0.314                | 0.445               | 0.225                | 0.320                | 0.445                |
| Constituency FE            | Yes                  | Yes                  | No                  | Yes                  | Yes                  | No                   |
| Party FE                   | No                   | Yes                  | No                  | No                   | Yes                  | No                   |
| Candidate FE               | No                   | No                   | Yes                 | No                   | No                   | Yes                  |
| Observations               | 13524                | 13524                | 13521               | 13524                | 13524                | 13521                |

Notes: This table shows results from polling-station-by-candidate-level regressions of log votes on the pooled information treatment interacted with different types of criminality. All data represented in this table comes from Phase 4 of the UP State Assembly Elections of 2017. In Row 1, the independent variable *Info* is a dummy that takes the value of 1 if the candidate is standing in a polling station that was given any one of the three information-based interventions. In Rows 2-6, the independent variables are interactions of the *Info* variable with: *Criminal*, a dummy that takes the value of 1 if the candidate faces any criminal charges; *Non-Violent*, a dummy that takes a value of 1 if the candidate faces charges that are all non-violent; *Violent, Non-Murder*, a dummy that takes a value of 1 if the candidate faces at least one violent charge but no charges related to murder; *Murder Attempt*, a dummy that takes a value of 1 if the candidate faces at least one attempt to murder charge but no murder charges; and *Murder*, a dummy that takes the value of 1 if the candidate faces at least one murder charge. Regressions include controls for total population, share female, share SC/ST, share literate, number of registered voters, and constituency fixed-effects, party fixed-effects, or candidate fixed-effects, as indicated by “Yes” or “No” in the bottom panel. Standard errors, clustered at the village and candidate level, are shown in brackets. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Looking at Column 2 of Table 3.5, we see the same pattern as observed from Table 3.4 in which the number of votes for clean candidates increases by a small order of magnitude

and the number of votes for criminal candidates, pooling across all levels of criminality, decreases by a small order of magnitude. Just from looking at Column 2, it is not clear which effect is driving the main result. When we turn to Columns 4-6, however, it becomes apparent that the change in vote share for murderers seen in Table 3.4 is being driven by a overall decrease in the total number of votes cast for criminals with murder charges. Column 5 of Table 3.5 shows that candidates with murder charges receive 13% fewer votes than clean candidates.

Overall, the results presented in Table 3.4 and Table 3.5 suggest that the criminality information provided to voters in our interventions was useful to them and influenced their voting decisions. This supports the notion that baseline knowledge levels of candidate quality may be low, in line with previous research conducted in India which suggests voters do update and make sophisticated voting decisions when more information on candidates is made available (Banerjee et al., 2011). We contribute to this literature by showing that even one-time, easily scalable, mobile-based interventions can achieve substantial impact by moving votes away from those with the most severe criminal charges. In the analysis thus far, however, we have pooled across all three of the information interventions, which has not provided a clear mechanism behind this result. Next we consider whether there are differential impacts from the different types of information messages provided.

### **3.4.2 Mechanisms Behind the Impact of Information**

Our interventions included three information-based treatments. One provided only basic information on candidate criminality, while the other two combined this basic information with other messaging aimed at influencing the extent to which individuals utilize the candidate criminality data when making their voting decisions. Table 3.6 shows the impact of each information treatment on polling station level vote shares of major party candidates. In Column 2 of Table 3.6, we see that the information plus coordination message and the information plus ethnic-voting message both increase vote shares for clean candidates by 0.5 percentage points and 0.6 percentage points, respectively. Additionally, in both of these

interventions, the vote share for criminal candidates, pooling across all types of criminality, goes down, although the change is small in magnitude and statistically insignificant.

**Table 3.6:** *Impact of Different Information Messages on Vote Shares of Criminal Candidates*

|                        | (1)<br>Vote Share       | (2)<br>Vote Share      | (3)<br>Vote Share      | (4)<br>Vote Share       | (5)<br>Vote Share       | (6)<br>Vote Share       |
|------------------------|-------------------------|------------------------|------------------------|-------------------------|-------------------------|-------------------------|
| Basic Info             | -0.00279<br>(0.00296)   | -0.00115<br>(0.00262)  | -0.000964<br>(0.00371) | -0.00320<br>(0.00341)   | -0.00146<br>(0.00311)   | -0.000970<br>(0.00448)  |
| Basic Info x Criminal  | 0.0101<br>(0.00970)     | 0.00548<br>(0.00953)   | 0.00495<br>(0.00995)   |                         |                         |                         |
| Info+Ethnic            | 0.00606<br>(0.00385)    | 0.00555*<br>(0.00326)  | 0.00586<br>(0.00405)   | 0.00564<br>(0.00391)    | 0.00531<br>(0.00367)    | 0.00586<br>(0.00446)    |
| Info+Ethnic x Criminal | -0.00920<br>(0.00978)   | -0.00771<br>(0.00940)  | -0.00860<br>(0.00949)  |                         |                         |                         |
| Info+Coord             | 0.00601***<br>(0.00215) | 0.00478**<br>(0.00201) | 0.00491<br>(0.00311)   | 0.00621**<br>(0.00262)  | 0.00489**<br>(0.00205)  | 0.00490<br>(0.00410)    |
| Info+Coord x Criminal  | -0.0172<br>(0.0108)     | -0.0137<br>(0.0107)    | -0.0140<br>(0.0106)    |                         |                         |                         |
| Info Basic x Murder    |                         |                        |                        | -0.000814<br>(0.0220)   | -0.0114<br>(0.0250)     | -0.0143<br>(0.0244)     |
| Info+Ethnic x Murder   |                         |                        |                        | -0.0440***<br>(0.00981) | -0.0506***<br>(0.00777) | -0.0538***<br>(0.00853) |
| Info+Coord x Murder    |                         |                        |                        | -0.0342<br>(0.0236)     | -0.0339<br>(0.0216)     | -0.0367*<br>(0.0217)    |
| Adjusted $R^2$         | 0.057                   | 0.215                  | 0.383                  | 0.076                   | 0.225                   | 0.384                   |
| Constituency FE        | Yes                     | Yes                    | No                     | Yes                     | Yes                     | No                      |
| Party FE               | No                      | Yes                    | No                     | No                      | Yes                     | No                      |
| Candidate FE           | No                      | No                     | Yes                    | No                      | No                      | Yes                     |
| Observations           | 13686                   | 13686                  | 13683                  | 13686                   | 13686                   | 13683                   |

Notes: This table shows results from polling-station-by-candidate-level regressions of vote share on the individual information treatments interacted with different types of criminality. All data represented in this table comes from Phase 4 of the UP State Assembly Elections of 2017. In Rows 1, 3, 5, the independent variables *Basic Info*, *Info+Coord*, and *Info+Ethnic* are dummy variables that take a value of 1 if the candidate is standing in a polling station that received the basic information, information plus coordination, or information plus ethnic voting treatments, respectively. In Rows 2, 4, and 6, these treatment dummies are interacted with another dummy variable, *Criminal*, which takes the value of 1 if the candidate faces any criminal charges, regardless of type. In Rows 7-9, the three treatment dummies are interacted with *Murder*, a dummy variable that takes the value of 1 if the candidate faces at least one murder charge. Regressions include controls for total population, share female, share SC/ST, share literate, number of registered voters, and constituency fixed-effects, party fixed-effects, or candidate fixed-effects, as indicated by “Yes” or “No” in the bottom panel. Standard errors, clustered at the village and candidate level, are shown in brackets. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

In Columns 4-6, we look more carefully at the types of criminal charges that elicit a negative response from voters and whether the coordination or ethnic-voting messages

facilitate this response to a greater extent than the basic information message. For this analysis, we consider how voters across the three different informational interventions respond to candidates facing murder charges. Column 5 shows that the vote share for murderers in polling stations that received the basic information treatment declines by 1.3 percentage points. In comparison, the vote share for candidates facing a murder charge in polling stations that received the coordination or ethnic-voting message declines by 2.9 percentage points and 4.8 percentage points, respectively. For the ethnic-voting intervention, the difference in vote share between clean candidates and murderers is statistically significant at the 1% level.

The results presented in Table 3.6 indicate that voters respond much more strongly to criminality information when they either receive a public signal telling them that others have also received the information or when the information is coupled with a statement urging them not to employ default voting behavior that might undermine this information. The larger of the two results comes from adding the anti-ethnic-voting signal to the message, which compliments previous research that has found ethnic preferences to be most salient when voters lack objective dimensions along which to compare candidates (Banerjee et al., 2012).

In Table 3.7, results for the same analysis is shown using the log of the number of votes for each candidate as the outcome instead of vote share. In Column 5, we see that the basic information treatment causes candidates with murder charges to experience a 15.9% decline in their total votes. In the polling station areas that received the information plus coordination or information plus ethnic voting treatments, total votes for murderers declines by 7.1% and 16.5%, respectively. Thus, when we consider the total votes cast for each candidate, the impact of the additional coordination or ethnic-voting components of the messages is less salient.

**Table 3.7:** *Impact of Different Information Messages on Total Votes for Criminal Candidates*

|                        | (1)                  | (2)                  | (3)                 | (4)                  | (5)                  | (6)                   |
|------------------------|----------------------|----------------------|---------------------|----------------------|----------------------|-----------------------|
|                        | Log Votes            | Log Votes            | Log Votes           | Log Votes            | Log Votes            | Log Votes             |
| Basic Info             | 0.0137<br>(0.0155)   | 0.0203<br>(0.0144)   | 0.0208<br>(0.0192)  | 0.0122<br>(0.0173)   | 0.0192<br>(0.0158)   | 0.0207<br>(0.0196)    |
| Basic Info x Criminal  | -0.00777<br>(0.0432) | -0.0261<br>(0.0415)  | -0.0271<br>(0.0425) |                      |                      |                       |
| Info+Ethnic            | 0.0230<br>(0.0162)   | 0.0203<br>(0.0164)   | 0.0239<br>(0.0188)  | 0.0211<br>(0.0170)   | 0.0191<br>(0.0172)   | 0.0239<br>(0.0196)    |
| Info+Ethnic x Criminal | -0.0137<br>(0.0473)  | -0.00775<br>(0.0452) | -0.0141<br>(0.0454) |                      |                      |                       |
| Info+Coord             | 0.0295*<br>(0.0166)  | 0.0244*<br>(0.0138)  | 0.0270<br>(0.0184)  | 0.0304*<br>(0.0173)  | 0.0249<br>(0.0161)   | 0.0270<br>(0.0185)    |
| Info+Coord x Criminal  | -0.0839<br>(0.0507)  | -0.0700<br>(0.0476)  | -0.0760<br>(0.0464) |                      |                      |                       |
| Basic Info x Murder    |                      |                      |                     | -0.138<br>(0.152)    | -0.178<br>(0.163)    | -0.180<br>(0.158)     |
| Info+Ethnic x Murder   |                      |                      |                     | -0.160**<br>(0.0684) | -0.184**<br>(0.0749) | -0.192***<br>(0.0716) |
| Info+Coord x Murder    |                      |                      |                     | -0.0991*<br>(0.0588) | -0.0960*<br>(0.0485) | -0.108**<br>(0.0523)  |
| Adjusted $R^2$         | 0.213                | 0.314                | 0.445               | 0.225                | 0.320                | 0.445                 |
| Constituency FE        | Yes                  | Yes                  | No                  | Yes                  | Yes                  | No                    |
| Party FE               | No                   | Yes                  | No                  | No                   | Yes                  | No                    |
| Candidate FE           | No                   | No                   | Yes                 | No                   | No                   | Yes                   |
| Observations           | 13524                | 13524                | 13521               | 13524                | 13524                | 13521                 |

Notes: This table shows results from polling-station-by-candidate-level regressions of log votes on the individual information treatments interacted with different types of criminality. All data represented in this table comes from Phase 4 of the UP State Assembly Elections of 2017. In Rows 1, 3, 5, the independent variables *Basic Info*, *Info+Coord*, and *Info+Ethnic* are dummy variables that take a value of 1 if the candidate is standing in a polling station that received the basic information, information plus coordination, or information plus ethnic voting treatments, respectively. In Rows 2, 4, and 6, these treatment dummies are interacted with another dummy variable, *Criminal*, which takes the value of 1 if the candidate faces any criminal charges, regardless of type. In Rows 7-9, the three treatment dummies are interacted with *Murder*, a dummy variable that takes the value of 1 if the candidate faces at least one murder charge. Regressions include controls for total population, share female, share SC/ST, share literate, and number of registered voters, and constituency fixed-effects, party fixed-effects, or candidate fixed-effects, as indicated by “Yes” or “No” in the bottom panel. Standard errors, clustered at the village and candidate level, are shown in brackets. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

In the analysis shown in both Table 3.6 and Table 3.7, we are not able to reject the equality of the coefficients for the impacts of each of the information-based treatments on vote shares or total votes for criminals, murderers or otherwise. Still, the results suggest that voters do care about electing high quality candidates to office and will update their

voting decisions accordingly when provided useful information. In addition, information on its own may not be as effective in contexts where voters feel pressure to follow the majority or have long-standing traditions of ethnic voting. In such cases, information may be a more powerful tool to help improve political selection in to office when it is coupled with context-specific messaging that helps voters overcome the likely hurdles they may face in actualizing the information in to action.

### **3.4.3 Impact of Interventions on Voter Turnout**

Thus far, we have provided evidence that criminal candidates were punished by voters as a result of our information-based treatments, with those facing the most serious crimes experiencing the largest penalty at the ballot box. An important consideration, however, is whether this decline in votes for criminals represents citizens switching votes between good and bad candidates or a change in the total number of citizens voting. In Table 3.8, we examine the impact of the pooled information treatment on polling station level voter turnout. Overall, we do not observe any significant changes in voter turnout in the positive or negative direction as a result of the information-based interventions.

**Table 3.8:** *Impact of Information on Polling Station Level Turnout, by Criminality of Candidates*

|                 | (1)<br>All<br>Votes | (2)<br>Female<br>Votes | (3)<br>Male<br>Votes | (4)<br>All<br>Votes  | (5)<br>Female<br>Votes | (6)<br>Male<br>Votes | (7)<br>All<br>Votes  | (8)<br>Female<br>Votes | (9)<br>Male<br>Votes |
|-----------------|---------------------|------------------------|----------------------|----------------------|------------------------|----------------------|----------------------|------------------------|----------------------|
| Info            | 0.00590<br>(0.0047) | 0.00391<br>(0.0049)    | 0.00601<br>(0.0052)  | 0.00662<br>(0.0086)  | 0.00918<br>(0.0091)    | 0.00353<br>(0.011)   | 0.00662<br>(0.0086)  | 0.00919<br>(0.0091)    | 0.00353<br>(0.011)   |
| Info x Criminal |                     |                        |                      | -0.000212<br>(0.010) | -0.00709<br>(0.011)    | 0.00421<br>(0.012)   |                      |                        |                      |
| Info x Murder   |                     |                        |                      | -0.00427<br>(0.0222) | 0.00112<br>(0.0221)    | -0.00511<br>(0.0241) |                      |                        |                      |
| Info x 1 Crim   |                     |                        |                      |                      |                        |                      | 0.00478<br>(0.0104)  | -0.00381<br>(0.0109)   | 0.0103<br>(0.0123)   |
| Info x 2 Crim   |                     |                        |                      |                      |                        |                      | -0.0174<br>(0.0180)  | -0.0188<br>(0.0183)    | -0.0145<br>(0.0198)  |
| Info x 3 Crim   |                     |                        |                      |                      |                        |                      | 0.000186<br>(0.0184) | 0.00344<br>(0.0206)    | -0.00271<br>(0.0206) |
| Adjusted $R^2$  | 0.826               | 0.809                  | 0.832                | 0.825                | 0.809                  | 0.826                | 0.826                | 0.809                  | 0.826                |
| Observations    | 4619                | 4619                   | 5543                 | 4619                 | 4619                   | 4618                 | 4619                 | 4619                   | 4618                 |

Notes: This tables shows regressions of log votes cast in total (or separately by gender) at the polling station level on the pooled information treatment interacted with different characteristics of candidates running at the polling station. All data represented in this table comes from Phase 4 of the UP State Assembly Elections of 2017. In Row 1, the independent variable *Info* is a dummy that takes the value of 1 if the polling station was targeted by any one of the three information-based interventions. In Rows 2-6, the independent variables are interactions of the *Info* variable with: *Criminal*, a dummy that takes the value of 1 if any major party candidate on the ticket for the given polling station faces any criminal charges; *Murder*, a dummy variable that takes the value of 1 if any major party candidate on the ticket for the given polling station faces at least one murder charge; *1 Crim*, a dummy variable that takes the value of 1 if there is only 1 candidate facing criminal charges among major party candidates on the ticket for the given polling station; *2 Crim*, a dummy variable that takes the value of 1 if there are exactly 2 candidates facing criminal charges among major party candidates on the ticket for that given polling stations; and *3 Crim*, a dummy variable that takes a value of 1 if there are exactly 3 candidates facing criminal charges among major party candidates for the given polling station. All regressions include controls for total population, share female, share SC/ST, share literate, number of registered voters, and constituency fixed-effects. Standard errors, clustered at the village level, are shown in brackets. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

In Columns 1-3 of Table 3.8, we capture the impact the treatments on total number of votes cast, number of female votes, and number of male votes, respectively. The point estimates are all positive but represent a less than 1% shift in the number of votes and in all cases are not statistically significant. In Columns 4-6, we consider whether the information treatments are more likely to affect turnout, again total and by gender, in polling stations that have at least one criminal candidate or specifically at least one candidate with a murder charge. Again, we see no notable impacts on turnout when testing this dimension of polling station level heterogeneity. Finally, in Columns 7-9, we test whether the information treatments are more likely to affect turnout depending on the number of criminal candidates

standing for election at the polling station. We do not find any significant shifts in turnout along this dimension.

Based on the evidence presented in Table 3.8, it seems likely that the decline in vote shares and total number of votes presented in Tables 4-7 represent voters changing their votes from criminal candidates to clean candidates as a result of the information treatments. Importantly, we do not observe a pattern in which we depress turnout and cause disillusionment by providing this information, as was the case in Mexico when corruption information was publicized a week before the 2009 municipal elections (Chong et al., 2015). At the same time, however, we do not seem to mobilize more citizens to come out and vote in order to act on the information either. Of course there is the possibility that certain subgroups increased votes while others decreased votes, leading to no overall impact on turnout. Our analysis shows that this is not true along the gender dimension, but it may be happening along other sub-group dimensions not observable in our data.

### **3.5 Conclusion**

Improving political selection has the potential to have long-lasting impacts on economic development and social values. Yet, many democratic countries continue to struggle with electing high-quality candidates in to office. India, the world's largest democracy, has a rich democratic history but also faces a growing problem of criminals in politics. In this study, we aim to understand what constraints Indian citizens face, if any, in electing non-criminal candidates to office and the extent to which lack of information is a salient barrier. We partner with 3 large telecom companies and blast voters with voice and text messages containing information about the criminal charges facing their major party candidates just days before the Uttar Pradesh state assembly elections of 2017.

Our results suggest that one reason for the pervasive election of criminals in this context does seem to be that voters face information constraints, as we observe a change in voting patterns once criminality information is provided. This does not seem to be driven simply by the fact that voters are being encouraged to think about who they vote for, as the content

of the information determines the magnitude of the response. Specifically, while we do not see any significant change in vote shares for non-violent criminals, those facing murder charges experience a 13 percent decline in the number of votes they receive, leading to a 3 percentage point decline in their vote share. This is a non-trivial magnitude when we consider the winning margins of the election we targeted with this campaign. Of the 403 races held across UP in 2017, one in five were determined by a margin of 3 percentage points or less.

From a policy perspective, these results provide evidence that low-cost, easily-scalable, mobile-based information campaigns can be a powerful tool to empower citizens in developing countries, especially those living in rural areas with low literacy levels, to make more informed decisions at the ballot box. In addition, the fact that voters responded most strongly to the most serious charges also helps alleviate concerns that spreading criminality information will lead to good candidates losing elections due to petty or spurious charges.

This paper summarizes very preliminary analysis of the data from this election and this study. In the future, we plan to add several layers of analysis to check the robustness of our results and understand the mechanisms driving the patterns we observe. We plan to use market share data shared from our telco partners as well as pick-up and listening rates of our voice messages to calculate different measures of treatment intensity that can be used for heterogeneity analysis. In addition, we will examine potential spillover effects, conduct cost-effectiveness calculations, and examine potential impacts on the identity of who actually wins assembly elections.

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# Appendix A

## Appendix to Chapter 1

### A.1 Supplementary Tables and Figures

**Table A.1:** *Where do Women Obtain Pension Application Forms?*

|                              | <b>Control</b> | <b>T1</b>  | <b>T2</b>  | <b>T3</b>  |
|------------------------------|----------------|------------|------------|------------|
| MLA Office                   | 60%            | 57%        | 30%        | 35%        |
| J-PAL Research Team          | 2%             | 12%        | 45%        | 52%        |
| Family Member or Neighbor    | 13%            | 8%         | 7%         | 6%         |
| District Office              | 7%             | 7%         | 8%         | 3%         |
| Pradhan or Agent             | 2%             | 5%         | 2%         | 1%         |
| NGO or School                | 2%             | 2%         | 2%         | 0%         |
| Online                       | 0%             | 3%         | 0%         | 0%         |
| Municipal Counselor's Office | 5%             | 2%         | 0%         | 1%         |
| Other or Unknown Source      | 8%             | 4%         | 6%         | 1%         |
| <b>Total</b>                 | <b>165</b>     | <b>211</b> | <b>254</b> | <b>269</b> |

Notes: This table presents a summary of where women in each treatment group reported they obtained their application forms from. The totals at the bottom of each column represent the number of women in each group that obtained an application form. The percentages in the table represent what percentage of these total women got their form from each of the listed sources on the left-hand side of the table.

**Table A.2:** *Time and Financial Costs for MLA Signature and District Office Submission*

|                                                | <b>Control</b> | <b>T1</b>  | <b>T2</b>  | <b>T3</b>  |
|------------------------------------------------|----------------|------------|------------|------------|
| Number of Visits to MLA                        | 2.1            | 1.8        | 1.9        | 1.8        |
| Round-Trip Cost of Travel to MLA (Rupees)      | 41             | 40         | 51         | 33         |
| Round-Trip Time to Travel to MLA (Hours)       | 1.5            | 1.5        | 1.4        | 1.4        |
| Time Spent at MLA Office (Hours)               | 0.5            | 0.5        | 0.4        | 0.4        |
| Number of Visits to District Office            | 1.8            | 1.9        | 1.8        | 1.7        |
| Round-Trip Cost of Travel to District (Rupees) | 140            | 139        | 132        | 145        |
| Round-Trip Time to Travel to District (Hours)  | 2.8            | 2.7        | 2.6        | 2.7        |
| Time Spent at District Office (Hours)          | 0.9            | 0.9        | 0.8        | 0.8        |
| <b>Observations</b>                            | <b>165</b>     | <b>211</b> | <b>254</b> | <b>269</b> |

Notes: This table presents the average amount of time and money women in each treatment group report that they spent in completing the MLA signature and district office submission requirements of the application process. The totals at the bottom of each column represent the number of women in each group that completed the End-Line I survey, but not all of these women completed the MLA and district office submission steps.

**Table A.3:** *Reasons for Attrition after Census Survey*

|                 | <b>Baseline</b> | <b>End-Line I</b> | <b>End-Line II</b> |
|-----------------|-----------------|-------------------|--------------------|
| In Village      | 18%             | 17%               | 13%                |
| Busy            | 16%             | 2%                | 0%                 |
| Refused         | 15%             | 31%               | 7%                 |
| Has shifted     | 13%             | 24%               | 32%                |
| Not Found       | 7%              | 0%                | 1%                 |
| With Husband    | 6%              | 5%                | 12%                |
| Remarried       | 5%              | 7%                | 21%                |
| Age 60+         | 4%              | 0%                | 0%                 |
| Has Passed Away | 1%              | 12%               | 13%                |
| Other           | 14%             | 2%                | 0%                 |
| <b>Total</b>    | <b>136</b>      | <b>58</b>         | <b>68</b>          |

Notes: This table presents reasons why some respondents were not able to complete each subsequent round of surveying after the census survey from which the original sample was identified. The totals at the bottom of each column represent the number of women that did not complete the given survey, and the percentages represent what fraction of this total is represented by the reasons stated on the left-hand side of the table.

**Table A.4:** *Application Steps Completed by End-Line I, Pre- and Post-Intervention*

| VARIABLES                | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign  | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|--------------------------|-------------------------|-----------------------|-----------------------|---------------------|----------------------------|---------------------------|
| Any Treatment            | 0.195***<br>(0.033)     | 0.187***<br>(0.033)   | 0.142***<br>(0.034)   | 0.107***<br>(0.035) | 0.080**<br>(0.035)         | 0.056<br>(0.034)          |
| Information (Info)       | 0.080**<br>(0.040)      | 0.071*<br>(0.040)     | 0.077*<br>(0.041)     | 0.063<br>(0.041)    | 0.059<br>(0.041)           | 0.036<br>(0.041)          |
| Basic Mediation (BM)     | 0.258***<br>(0.036)     | 0.254***<br>(0.037)   | 0.157***<br>(0.040)   | 0.112***<br>(0.041) | 0.079*<br>(0.042)          | 0.066<br>(0.042)          |
| Intensive Mediation (IM) | 0.248***<br>(0.036)     | 0.236***<br>(0.036)   | 0.192***<br>(0.039)   | 0.146***<br>(0.041) | 0.102**<br>(0.041)         | 0.065<br>(0.041)          |
| Observations             | 1,199                   | 1,199                 | 1,199                 | 1,199               | 1,199                      | 1,199                     |
| Control Mean             | 0.597                   | 0.590                 | 0.522                 | 0.475               | 0.457                      | 0.406                     |
| Info = BM                | 0.000                   | 0.000                 | 0.036                 | 0.213               | 0.607                      | 0.460                     |
| Info = IM                | 0.000                   | 0.000                 | 0.002                 | 0.034               | 0.272                      | 0.462                     |
| BM = IM                  | 0.730                   | 0.545                 | 0.332                 | 0.392               | 0.571                      | 0.986                     |

Notes: This table presents pooled and individual treatment impacts on steps completed in the application process at the time of End-Line I regardless of whether these steps were taken before or after the intervention. *Information, Basic Mediation, and Intensive Mediation* are dummy variables that take a value of 1 if the respondent belongs to treatment group 1, treatment group 2, or treatment group 3, respectfully. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment is statistically equal to the impact of another individual treatment. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.5: Pension Outcomes for Women in End-Line I Sample**

| VARIABLES                | (1)<br>End-Line I<br>Survey Data | (2)<br>Admin<br>Data | (3)<br>End-Line II Survey<br>+ Admin Data |
|--------------------------|----------------------------------|----------------------|-------------------------------------------|
| Any Treatment            | 0.025<br>(0.029)                 | 0.022<br>(0.032)     | 0.026<br>(0.034)                          |
| Information (Info)       | 0.036<br>(0.035)                 | 0.037<br>(0.039)     | 0.034<br>(0.041)                          |
| Basic Mediation (BM)     | 0.042<br>(0.036)                 | 0.005<br>(0.038)     | 0.032<br>(0.041)                          |
| Intensive Mediation (IM) | -0.003<br>(0.035)                | 0.024<br>(0.038)     | 0.014<br>(0.041)                          |
| Observations             | 1,199                            | 1,199                | 1,199                                     |
| Control Mean             | 0.223                            | 0.331                | 0.432                                     |
| Info = BM                | 0.858                            | 0.401                | 0.950                                     |
| Info = IM                | 0.251                            | 0.734                | 0.612                                     |
| BM = IM                  | 0.195                            | 0.609                | 0.661                                     |

Notes: This table presents pooled and individual treatment impacts on final enrollment in the pension scheme by anyone who completed the End-Line I survey, regardless of the timing of the application. *Information*, *Basic Mediation*, and *Intensive Mediation* are dummy variables that take a value of 1 if the respondent belongs to treatment group 1, treatment group 2, or treatment group 3, respectfully. The dependent variable in Columns 1 is dummy variable that takes a value of 1 if the respondent reported receiving pensions at End-Line I. The dependent variable in Column 2 is a dummy variable that takes a value of 1 if the respondent appeared in the official administrative list of pension beneficiaries published online in October 2016. The dependent variable in Column 3 is a dummy variable that takes a value of 1 if the respondent reported receiving pensions by the time of the End-Line II survey or appeared in the official administrative list of pension beneficiaries published online in October 2016. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment is statistically equal to the impact of another individual treatment.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.6:** *Application Steps Completed Post-Intervention by End-Line II*

| VARIABLES                | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign  | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|--------------------------|-------------------------|-----------------------|-----------------------|---------------------|----------------------------|---------------------------|
| Any Treatment            | 0.231***<br>(0.035)     | 0.197***<br>(0.035)   | 0.092***<br>(0.035)   | 0.087**<br>(0.034)  | 0.087***<br>(0.033)        | 0.056*<br>(0.032)         |
| Information (Info)       | 0.107**<br>(0.042)      | 0.065<br>(0.042)      | 0.024<br>(0.042)      | 0.030<br>(0.041)    | 0.033<br>(0.040)           | 0.009<br>(0.038)          |
| Basic Mediation (BM)     | 0.279***<br>(0.042)     | 0.251***<br>(0.042)   | 0.086**<br>(0.042)    | 0.077*<br>(0.041)   | 0.078*<br>(0.041)          | 0.068*<br>(0.040)         |
| Intensive Mediation (IM) | 0.309***<br>(0.041)     | 0.278***<br>(0.041)   | 0.166***<br>(0.042)   | 0.153***<br>(0.041) | 0.149***<br>(0.041)        | 0.092**<br>(0.039)        |
| Observations             | 1,131                   | 1,131                 | 1,131                 | 1,131               | 1,131                      | 1,131                     |
| Control Mean             | 0.387                   | 0.371                 | 0.398                 | 0.320               | 0.301                      | 0.266                     |
| Info = BM                | 0.000                   | 0.000                 | 0.129                 | 0.240               | 0.255                      | 0.124                     |
| Info = IM                | 0.000                   | 0.000                 | 0.000                 | 0.002               | 0.004                      | 0.031                     |
| BM = IM                  | 0.442                   | 0.508                 | 0.054                 | 0.065               | 0.084                      | 0.555                     |

Notes: This table presents pooled and individual treatment impacts on steps completed in the application process at the time of End-Line II by those who applied *after* the intervention. *Information*, *Basic Mediation*, and *Intensive Mediation* are dummy variables that take a value of 1 if the respondent belongs to treatment group 1, treatment group 2, or treatment group 3, respectfully. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment is statistically equal to the impact of another individual treatment. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.7:** Pension Outcomes for Women who Applied between Intervention and End-Line II

| VARIABLES                | (1)<br>End-Line II Survey<br>Data | (2)<br>Admin<br>Data | (3)<br>End-Line II Survey<br>+ Admin Data |
|--------------------------|-----------------------------------|----------------------|-------------------------------------------|
| Any Treatment            | 0.024<br>(0.026)                  | 0.036<br>(0.026)     | 0.048*<br>(0.029)                         |
| Information (Info)       | 0.010<br>(0.031)                  | 0.042<br>(0.032)     | 0.037<br>(0.035)                          |
| Basic Mediation (BM)     | 0.026<br>(0.032)                  | 0.010<br>(0.032)     | 0.038<br>(0.036)                          |
| Intensive Mediation (IM) | 0.035<br>(0.032)                  | 0.055*<br>(0.032)    | 0.069*<br>(0.036)                         |
| Observations             | 1,131                             | 1,131                | 1,131                                     |
| Control Mean             | 0.148                             | 0.160                | 0.199                                     |
| Info = BM                | 0.599                             | 0.311                | 0.982                                     |
| Info = IM                | 0.409                             | 0.694                | 0.370                                     |
| BM = IM                  | 0.777                             | 0.160                | 0.392                                     |

Notes: This table presents pooled and individual treatment impacts on final enrollment in the pension scheme by those who applied *after* the intervention and by End-Line II. *Information*, *Basic Mediation*, and *Intensive Mediation* are dummy variables that take a value of 1 if the respondent belongs to treatment group 1, treatment group 2, or treatment group 3, respectfully. The dependent variable in Columns 1 is dummy variable that takes a value of 1 if the respondent reported receiving pensions at End-Line II. The dependent variable in Column 2 is a dummy variable that takes a value of 1 if the respondent appeared in the official administrative list of pension beneficiaries published online in October 2016. The dependent variable in Column 3 is a dummy variable that takes a value of 1 if the respondent reported receiving pensions at End-Line II or appeared in the official administrative list of pension beneficiaries published online in October 2016. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment is statistically equal to the impact of another individual treatment. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.8:** *Application Steps Completed by End-Line II, Pre- and Post-Intervention*

| VARIABLES                | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|--------------------------|-------------------------|-----------------------|-----------------------|--------------------|----------------------------|---------------------------|
| Any Treatment            | 0.183***<br>(0.030)     | 0.148***<br>(0.032)   | 0.055*<br>(0.032)     | 0.064*<br>(0.035)  | 0.060*<br>(0.035)          | 0.037<br>(0.036)          |
| Information (Info)       | 0.095***<br>(0.037)     | 0.053<br>(0.039)      | 0.022<br>(0.039)      | 0.032<br>(0.042)   | 0.030<br>(0.042)           | 0.012<br>(0.043)          |
| Basic Mediation (BM)     | 0.235***<br>(0.032)     | 0.203***<br>(0.034)   | 0.054<br>(0.038)      | 0.066<br>(0.042)   | 0.063<br>(0.042)           | 0.057<br>(0.043)          |
| Intensive Mediation (IM) | 0.221***<br>(0.032)     | 0.189***<br>(0.035)   | 0.087**<br>(0.037)    | 0.095**<br>(0.041) | 0.087**<br>(0.041)         | 0.042<br>(0.043)          |
| Observations             | 1,131                   | 1,131                 | 1,131                 | 1,131              | 1,131                      | 1,131                     |
| Control Mean             | 0.703                   | 0.688                 | 0.695                 | 0.594              | 0.574                      | 0.523                     |
| Info = BM                | 0.000                   | 0.000                 | 0.386                 | 0.385              | 0.415                      | 0.267                     |
| Info = IM                | 0.000                   | 0.000                 | 0.068                 | 0.103              | 0.149                      | 0.458                     |
| BM = IM                  | 0.512                   | 0.597                 | 0.348                 | 0.459              | 0.547                      | 0.705                     |

Notes: This table presents pooled and individual treatment impacts on steps completed in the application process at the time of End-Line II regardless of whether these steps were taken before or after the intervention. *Information*, *Basic Mediation*, and *Intensive Mediation* are dummy variables that take a value of 1 if the respondent belongs to treatment group 1, treatment group 2, or treatment group 3, respectfully. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment is statistically equal to the impact of another individual treatment. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.9: Pension Outcomes for Women in End-Line II Sample**

| VARIABLES                | (1)<br>End-Line II<br>Survey Data | (2)<br>Admin<br>Data | (3)<br>End-Line II Survey<br>+ Admin Data |
|--------------------------|-----------------------------------|----------------------|-------------------------------------------|
| Any Treatment            | -0.001<br>(0.035)                 | 0.008<br>(0.033)     | 0.014<br>(0.036)                          |
| Information (Info)       | 0.005<br>(0.042)                  | 0.024<br>(0.040)     | 0.023<br>(0.043)                          |
| Basic Mediation (BM)     | 0.012<br>(0.043)                  | -0.012<br>(0.040)    | 0.012<br>(0.043)                          |
| Intensive Mediation (IM) | -0.020<br>(0.042)                 | 0.012<br>(0.040)     | 0.006<br>(0.043)                          |
| Observations             | 1,131                             | 1,131                | 1,131                                     |
| Control Mean             | 0.398                             | 0.355                | 0.461                                     |
| Info = BM                | 0.862                             | 0.366                | 0.780                                     |
| Info = IM                | 0.538                             | 0.760                | 0.662                                     |
| BM = IM                  | 0.440                             | 0.545                | 0.880                                     |

Notes: This table presents pooled and individual treatment impacts on final enrollment in the pension scheme by anyone who completed the End-Line II survey, regardless of the timing of the application. *Information*, *Basic Mediation*, and *Intensive Mediation* are dummy variables that take a value of 1 if the respondent belongs to treatment group 1, treatment group 2, or treatment group 3, respectfully. The dependent variable in Columns 1 is dummy variable that takes a value of 1 if the respondent reported receiving pensions at End-Line II. The dependent variable in Column 2 is a dummy variable that takes a value of 1 if the respondent appeared in the official administrative list of pension beneficiaries published online in October 2016. The dependent variable in Column 3 is a dummy variable that takes a value of 1 if the respondent reported receiving pensions at End-Line II or appeared in the official administrative list of pension beneficiaries published online in October 2016. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment is statistically equal to the impact of another individual treatment. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.10:** Steps Completed Post-Intervention by End-Line I, Interaction with Non-Widow Status

| VARIABLES                | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign   | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|--------------------------|-------------------------|-----------------------|-----------------------|----------------------|----------------------------|---------------------------|
| Information (Info)       | 0.044<br>(0.043)        | 0.045<br>(0.043)      | 0.040<br>(0.041)      | 0.016<br>(0.040)     | 0.023<br>(0.039)           | 0.027<br>(0.037)          |
| Basic Mediation (BM)     | 0.147***<br>(0.045)     | 0.143***<br>(0.045)   | 0.073*<br>(0.043)     | 0.036<br>(0.041)     | 0.020<br>(0.040)           | 0.026<br>(0.038)          |
| Intensive Mediation (IM) | 0.237***<br>(0.045)     | 0.232***<br>(0.045)   | 0.176***<br>(0.044)   | 0.132***<br>(0.043)  | 0.105**<br>(0.042)         | 0.090**<br>(0.040)        |
| Non-Widow                | -0.264***<br>(0.043)    | -0.256***<br>(0.043)  | -0.250***<br>(0.040)  | -0.235***<br>(0.039) | -0.228***<br>(0.038)       | -0.191***<br>(0.036)      |
| Info x Non-Widow         | 0.340***<br>(0.085)     | 0.279***<br>(0.083)   | 0.216***<br>(0.076)   | 0.176**<br>(0.071)   | 0.153**<br>(0.069)         | 0.039<br>(0.054)          |
| BM x Non-Widow           | 0.533***<br>(0.079)     | 0.519***<br>(0.080)   | 0.408***<br>(0.081)   | 0.279***<br>(0.077)  | 0.261***<br>(0.074)        | 0.164**<br>(0.068)        |
| IM x Non-Widow           | 0.422***<br>(0.076)     | 0.372***<br>(0.079)   | 0.310***<br>(0.077)   | 0.184**<br>(0.073)   | 0.186***<br>(0.071)        | 0.083<br>(0.064)          |
| Observations             | 1,199                   | 1,199                 | 1,199                 | 1,199                | 1,199                      | 1,199                     |
| Control Mean             | 0.227                   | 0.223                 | 0.194                 | 0.187                | 0.176                      | 0.151                     |
| Impact on Non-Widows:    |                         |                       |                       |                      |                            |                           |
| Info = BM                | 0.001                   | 0.000                 | 0.010                 | 0.123                | 0.168                      | 0.045                     |
| Info = IM                | 0.001                   | 0.001                 | 0.005                 | 0.100                | 0.111                      | 0.052                     |
| BM = IM                  | 0.796                   | 0.493                 | 0.956                 | 1.000                | 0.907                      | 0.795                     |

Notes: This table presents individual treatment impacts specifically for non-widows on steps completed in the application process at the time of End-Line I by those who applied *after* the intervention. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment on non-widows is statistically equal to the impact of another individual treatment on non-widows by comparing the additive impact of the treatment dummy plus the interaction dummy.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.11:** Steps Completed Post-Intervention by End-Line I, Interaction with Illiteracy for Widows only

| VARIABLES                   | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign   | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|-----------------------------|-------------------------|-----------------------|-----------------------|----------------------|----------------------------|---------------------------|
| Information (Info)          | 0.266***<br>(0.100)     | 0.265***<br>(0.100)   | 0.199**<br>(0.098)    | 0.226**<br>(0.096)   | 0.221**<br>(0.095)         | 0.210**<br>(0.093)        |
| Basic Mediation (BM)        | 0.146<br>(0.093)        | 0.150<br>(0.093)      | 0.076<br>(0.089)      | 0.016<br>(0.082)     | 0.013<br>(0.081)           | 0.025<br>(0.079)          |
| Intensive Mediation (IM)    | 0.428***<br>(0.086)     | 0.413***<br>(0.086)   | 0.382***<br>(0.088)   | 0.364***<br>(0.088)  | 0.294***<br>(0.087)        | 0.227***<br>(0.084)       |
| Illiterate                  | 0.070<br>(0.070)        | 0.067<br>(0.069)      | 0.043<br>(0.068)      | 0.074<br>(0.067)     | 0.054<br>(0.065)           | 0.031<br>(0.062)          |
| Info x Illiterate           | -0.272**<br>(0.112)     | -0.269**<br>(0.111)   | -0.193*<br>(0.109)    | -0.254**<br>(0.107)  | -0.238**<br>(0.105)        | -0.221**<br>(0.103)       |
| BM x Illiterate             | 0.001<br>(0.108)        | -0.009<br>(0.107)     | -0.001<br>(0.102)     | 0.021<br>(0.096)     | 0.008<br>(0.094)           | -0.003<br>(0.091)         |
| IM x Illiterate             | -0.248**<br>(0.102)     | -0.235**<br>(0.102)   | -0.269***<br>(0.102)  | -0.302***<br>(0.102) | -0.246**<br>(0.100)        | -0.180*<br>(0.096)        |
| Observations                | 946                     | 946                   | 946                   | 946                  | 946                        | 946                       |
| Control Mean                | 0.227                   | 0.223                 | 0.194                 | 0.187                | 0.176                      | 0.151                     |
| Impact on Illiterate Women: |                         |                       |                       |                      |                            |                           |
| Info = BM                   | 0.001                   | 0.002                 | 0.127                 | 0.131                | 0.373                      | 0.420                     |
| Info = IM                   | 0.000                   | 0.000                 | 0.022                 | 0.047                | 0.129                      | 0.173                     |
| BM = IM                     | 0.524                   | 0.462                 | 0.443                 | 0.616                | 0.540                      | 0.578                     |

Notes: This table restricts the sample to only widows and presents individual treatment impacts specifically for illiterate women on steps completed in the application process at the time of End-Line I by those who applied *after* the intervention. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment illiterate women is statistically equal to the impact of another individual treatment on illiterate women by comparing the additive impact of the treatment dummy plus the interaction dummy. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.12: Steps Completed Post-Intervention by End-Line II, Interaction with Illiteracy**

| VARIABLES                   | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign   | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|-----------------------------|-------------------------|-----------------------|-----------------------|----------------------|----------------------------|---------------------------|
| Information (Info)          | 0.338***<br>(0.080)     | 0.311***<br>(0.080)   | 0.226***<br>(0.082)   | 0.246***<br>(0.078)  | 0.230***<br>(0.078)        | 0.160**<br>(0.074)        |
| Basic Mediation (BM)        | 0.460***<br>(0.075)     | 0.416***<br>(0.076)   | 0.195**<br>(0.080)    | 0.197***<br>(0.074)  | 0.166**<br>(0.073)         | 0.112<br>(0.071)          |
| Intensive Mediation (IM)    | 0.573***<br>(0.065)     | 0.530***<br>(0.067)   | 0.358***<br>(0.073)   | 0.341***<br>(0.070)  | 0.300***<br>(0.070)        | 0.224***<br>(0.069)       |
| Illiterate                  | 0.199***<br>(0.065)     | 0.218***<br>(0.064)   | 0.172***<br>(0.066)   | 0.197***<br>(0.062)  | 0.168***<br>(0.061)        | 0.129**<br>(0.060)        |
| Info x Illiterate           | -0.320***<br>(0.095)    | -0.341***<br>(0.095)  | -0.279***<br>(0.096)  | -0.299***<br>(0.093) | -0.272***<br>(0.092)       | -0.207**<br>(0.088)       |
| BM x Illiterate             | -0.257***<br>(0.092)    | -0.235**<br>(0.093)   | -0.157<br>(0.096)     | -0.172*<br>(0.091)   | -0.127<br>(0.089)          | -0.065<br>(0.086)         |
| IM x Illiterate             | -0.384***<br>(0.084)    | -0.367***<br>(0.085)  | -0.277***<br>(0.090)  | -0.271***<br>(0.087) | -0.218**<br>(0.087)        | -0.191**<br>(0.085)       |
| Observations                | 1,131                   | 1,131                 | 1,131                 | 1,131                | 1,131                      | 1,131                     |
| Control Mean                | 0.387                   | 0.371                 | 0.398                 | 0.320                | 0.301                      | 0.266                     |
| Impact on Illiterate Women: |                         |                       |                       |                      |                            |                           |
| Info = BM                   | 0.000                   | 0.000                 | 0.053                 | 0.093                | 0.080                      | 0.033                     |
| Info = IM                   | 0.000                   | 0.000                 | 0.006                 | 0.009                | 0.008                      | 0.071                     |
| BM = IM                     | 0.780                   | 0.730                 | 0.396                 | 0.360                | 0.376                      | 0.768                     |

Notes: This table presents individual treatment impacts specifically for illiterate women on steps completed in the application process at the time of End-Line II by those who applied *after* the intervention. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment illiterate women is statistically equal to the impact of another individual treatment on illiterate women by comparing the additive impact of the treatment dummy plus the interaction dummy. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.13:** Steps Completed Post-Intervention by End-Line II, Interaction with Connections to MLA

| VARIABLES                    | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|------------------------------|-------------------------|-----------------------|-----------------------|--------------------|----------------------------|---------------------------|
| Information (Info)           | 0.097*<br>(0.058)       | 0.077<br>(0.059)      | 0.098*<br>(0.058)     | 0.078<br>(0.057)   | 0.079<br>(0.056)           | 0.054<br>(0.053)          |
| Basic Mediation (BM)         | 0.205***<br>(0.059)     | 0.154***<br>(0.059)   | 0.035<br>(0.059)      | 0.028<br>(0.058)   | 0.031<br>(0.056)           | 0.031<br>(0.054)          |
| Intensive Mediation (IM)     | 0.214***<br>(0.058)     | 0.214***<br>(0.058)   | 0.132**<br>(0.058)    | 0.124**<br>(0.057) | 0.132**<br>(0.056)         | 0.111**<br>(0.055)        |
| Less Connected to MLA        | -0.078<br>(0.063)       | -0.090<br>(0.062)     | -0.049<br>(0.063)     | -0.050<br>(0.061)  | -0.037<br>(0.059)          | -0.006<br>(0.057)         |
| Info x Less Connected to MLA | 0.017<br>(0.086)        | -0.031<br>(0.085)     | -0.164*<br>(0.084)    | -0.107<br>(0.082)  | -0.102<br>(0.080)          | -0.097<br>(0.077)         |
| BM x Less Connected to MLA   | 0.156*<br>(0.086)       | 0.205**<br>(0.086)    | 0.108<br>(0.088)      | 0.103<br>(0.085)   | 0.101<br>(0.084)           | 0.080<br>(0.081)          |
| IM x Less Connected to MLA   | 0.204**<br>(0.083)      | 0.135<br>(0.085)      | 0.071<br>(0.087)      | 0.060<br>(0.085)   | 0.033<br>(0.084)           | -0.043<br>(0.081)         |
| Observations                 | 1,131                   | 1,131                 | 1,131                 | 1,131              | 1,131                      | 1,131                     |
| Control Mean                 | 0.387                   | 0.371                 | 0.398                 | 0.320              | 0.301                      | 0.266                     |
| Impact on Less Connected:    |                         |                       |                       |                    |                            |                           |
| Info = BM                    | 0.000                   | 0.000                 | 0.001                 | 0.007              | 0.008                      | 0.007                     |
| Info = IM                    | 0.000                   | 0.000                 | 0.000                 | 0.000              | 0.001                      | 0.047                     |
| BM = IM                      | 0.303                   | 0.857                 | 0.332                 | 0.390              | 0.580                      | 0.466                     |

Notes: This table presents individual treatment impacts specifically for less politically connected women on steps completed in the application process at the time of End-Line II by those who applied *after* the intervention. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment on less politically connected women is statistically equal to the impact of another individual treatment on less politically connected women by comparing the additive impact of the treatment dummy plus the interaction dummy. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.14:** Steps Completed Post-Intervention by End-Line II, Interaction with Financial Decision-Making

| VARIABLES                                         | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign  | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|---------------------------------------------------|-------------------------|-----------------------|-----------------------|---------------------|----------------------------|---------------------------|
| Information (Info)                                | 0.066<br>(0.049)        | 0.043<br>(0.049)      | 0.007<br>(0.048)      | 0.008<br>(0.047)    | 0.005<br>(0.046)           | -0.020<br>(0.044)         |
| Basic Mediation (BM)                              | 0.209***<br>(0.049)     | 0.197***<br>(0.049)   | 0.062<br>(0.049)      | 0.047<br>(0.048)    | 0.034<br>(0.048)           | 0.035<br>(0.046)          |
| Intensive Mediation (IM)                          | 0.274***<br>(0.048)     | 0.250***<br>(0.048)   | 0.140***<br>(0.048)   | 0.127***<br>(0.048) | 0.121**<br>(0.047)         | 0.075<br>(0.046)          |
| Fewer Financial Decisions                         | -0.119*<br>(0.071)      | -0.107<br>(0.071)     | -0.121*<br>(0.072)    | -0.166**<br>(0.066) | -0.190***<br>(0.062)       | -0.163***<br>(0.060)      |
| Info x Fewer Financial Decisions                  | 0.171*<br>(0.101)       | 0.088<br>(0.100)      | 0.066<br>(0.100)      | 0.089<br>(0.093)    | 0.113<br>(0.089)           | 0.120<br>(0.085)          |
| BM x Fewer Financial Decisions                    | 0.289***<br>(0.094)     | 0.224**<br>(0.096)    | 0.099<br>(0.099)      | 0.122<br>(0.093)    | 0.183**<br>(0.091)         | 0.138<br>(0.088)          |
| IM x Fewer Financial Decisions                    | 0.143<br>(0.094)        | 0.112<br>(0.095)      | 0.111<br>(0.099)      | 0.111<br>(0.095)    | 0.120<br>(0.091)           | 0.077<br>(0.087)          |
| Observations                                      | 1,131                   | 1,131                 | 1,131                 | 1,131               | 1,131                      | 1,131                     |
| Control Mean                                      | 0.387                   | 0.371                 | 0.398                 | 0.320               | 0.301                      | 0.266                     |
| Impact on Women Making Fewer Financial Decisions: |                         |                       |                       |                     |                            |                           |
| Info = BM                                         | 0.001                   | 0.000                 | 0.302                 | 0.371               | 0.227                      | 0.352                     |
| Info = IM                                         | 0.029                   | 0.005                 | 0.037                 | 0.086               | 0.125                      | 0.493                     |
| BM = IM                                           | 0.277                   | 0.463                 | 0.292                 | 0.407               | 0.763                      | 0.794                     |

Notes: This table presents individual treatment impacts specifically for women who make fewer financial decisions in the household on steps completed in the application process at the time of End-Line II by those who applied *after* the intervention. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment on women who make fewer financial decisions is statistically equal to the impact of another individual treatment on women who make fewer financial decisions by comparing the additive impact of the treatment dummy plus the interaction dummy. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.15:** Steps Completed Post-Intervention by End-Line II, Interaction with Autonomy in Household

| VARIABLES                        | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|----------------------------------|-------------------------|-----------------------|-----------------------|--------------------|----------------------------|---------------------------|
| Information (Info)               | 0.021<br>(0.058)        | 0.013<br>(0.058)      | -0.014<br>(0.057)     | -0.019<br>(0.056)  | -0.023<br>(0.055)          | -0.036<br>(0.053)         |
| Basic Mediation (BM)             | 0.162***<br>(0.059)     | 0.132**<br>(0.059)    | 0.028<br>(0.058)      | -0.005<br>(0.057)  | -0.007<br>(0.056)          | 0.003<br>(0.055)          |
| Intensive Mediation (IM)         | 0.258***<br>(0.057)     | 0.220***<br>(0.057)   | 0.131**<br>(0.058)    | 0.093<br>(0.057)   | 0.092*<br>(0.056)          | 0.044<br>(0.054)          |
| Lack of Autonomy                 | -0.064<br>(0.063)       | -0.082<br>(0.063)     | -0.058<br>(0.063)     | -0.107*<br>(0.061) | -0.127**<br>(0.058)        | -0.112**<br>(0.057)       |
| Info x Lack of Autonomy          | 0.189**<br>(0.086)      | 0.114<br>(0.086)      | 0.084<br>(0.086)      | 0.109<br>(0.083)   | 0.124<br>(0.081)           | 0.102<br>(0.077)          |
| BM x Lack of Autonomy            | 0.252***<br>(0.085)     | 0.257***<br>(0.085)   | 0.127<br>(0.087)      | 0.179**<br>(0.084) | 0.188**<br>(0.083)         | 0.145*<br>(0.080)         |
| IM x Lack of Autonomy            | 0.113<br>(0.084)        | 0.127<br>(0.085)      | 0.078<br>(0.086)      | 0.133<br>(0.084)   | 0.127<br>(0.082)           | 0.108<br>(0.079)          |
| Observations                     | 1,131                   | 1,131                 | 1,131                 | 1,131              | 1,131                      | 1,131                     |
| Control Mean                     | 0.387                   | 0.371                 | 0.398                 | 0.320              | 0.301                      | 0.266                     |
| Impact on Less Autonomous Women: |                         |                       |                       |                    |                            |                           |
| Info = BM                        | 0.000                   | 0.000                 | 0.163                 | 0.160              | 0.183                      | 0.151                     |
| Info = IM                        | 0.006                   | 0.000                 | 0.0210                | 0.023              | 0.047                      | 0.127                     |
| BM = IM                          | 0.430                   | 0.459                 | 0.372                 | 0.396              | 0.530                      | 0.940                     |

Notes: This table presents individual treatment impacts specifically for women that lack autonomy in the household on steps completed in the application process at the time of End-Line I by those who applied *after* the intervention. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. The bottom panel shows p-values when testing whether the impact of an individual treatment on less autonomous women is statistically equal to the impact of another individual treatment on less autonomous women by comparing the additive impact of the treatment dummy plus the interaction dummy. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.16:** Steps Completed Post-Intervention by End-Line II, Interaction with Eligibility Timing

| VARIABLES                 | (1)<br>Obtained<br>Form | (2)<br>Form<br>Filled | (3)<br>MLA<br>Attempt | (4)<br>MLA<br>Sign | (5)<br>District<br>Attempt | (6)<br>District<br>Accept |
|---------------------------|-------------------------|-----------------------|-----------------------|--------------------|----------------------------|---------------------------|
| Any Treatment             | 0.238***<br>(0.044)     | 0.199***<br>(0.044)   | 0.079*<br>(0.044)     | 0.073*<br>(0.042)  | 0.081**<br>(0.041)         | 0.045<br>(0.038)          |
| Eligible < 1yr at Census  | 0.041<br>(0.065)        | 0.044<br>(0.066)      | 0.014<br>(0.064)      | 0.066<br>(0.063)   | 0.080<br>(0.062)           | 0.103*<br>(0.060)         |
| Any Treat x Eligible <1yr | -0.020<br>(0.074)       | -0.006<br>(0.075)     | 0.036<br>(0.074)      | 0.036<br>(0.072)   | 0.012<br>(0.071)           | 0.029<br>(0.069)          |
| Observations              | 1,131                   | 1,131                 | 1,131                 | 1,131              | 1,131                      | 1,131                     |
| Control Mean              | 0.387                   | 0.371                 | 0.398                 | 0.320              | 0.301                      | 0.266                     |

Notes: This table presents pooled treatment impact specifically for women that have been eligible for less than 1 year at the time of the census on steps completed in the application process at the time of End-Line II by those who applied *after* the intervention. The dependent variables in Columns 1-6 are dummy variables that take a value of 1 if (1) the respondent obtained an application form, (2) the respondent's form has been filled, (3) the respondent went to the MLA's office to attempt getting a signature, (4) the respondent was successful in obtaining the signature (5) the respondent went to the district office to attempt submitting her application, and (6) the respondent's form was accepted at the district office, respectfully. All regressions include slum fixed-effects (the stratification variable), and robust standard errors are presented in brackets. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table A.17:** Mediation Channels

|                         | Legal                                                                                                                                                                                                                                                                                           | Illegal                                                                                                                                                                                  |
|-------------------------|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
|                         | Assistance provided does not include illicit fees or subvert official inclusion criteria.                                                                                                                                                                                                       | Assistance provided includes illicit fees or subverts official inclusion criteria.                                                                                                       |
| <b>State Actors</b>     | <ul style="list-style-type: none"> <li>• Bureaucrats</li> <li>• Politicians</li> <li>• Government employees</li> </ul>                                                                                                                                                                          | <ul style="list-style-type: none"> <li>• Bureaucrats</li> <li>• Politicians</li> <li>• Government Employees</li> </ul>                                                                   |
| <b>Non-State Actors</b> | <ul style="list-style-type: none"> <li>• Registered NGOs or private enterprises.<br/>→ Standardized and publicized fees</li> <li>• Unelected local leaders without fees<br/>→ Ex. <i>pradhans</i>, <i>fixers</i>, <i>party-workers</i></li> <li>• Friends, family, or acquaintances.</li> </ul> | <ul style="list-style-type: none"> <li>• Unregistered individuals or groups<br/>→ Unreported and arbitrary payments<br/>→ Ex. <i>agents</i>, <i>brokers</i>, <i>middlemen</i></li> </ul> |

## A.2 Data

### 1. Matching Administrative Data with Survey Data:

Administrative data was scraped from the Delhi widow pension website, which was updated through October 2016 at the time. This data contains a list of roughly 154,000 approved widow pension beneficiaries with just three variables: woman's name, husband's name, and address.

The match was attempted with all 1199 women in the sample who completed the End-Line I survey. The first round of matching was done using a fuzzy matching code in Stata with a manual check and a second round was done checking all remaining women in the sample against the administrative data manually.

After this matching exercise, 384 women were classified as exact matches, meaning the wife's name, the husband's name, and the addresses were essentially identical in the administrative data and our survey data. Another 739 women were classified as non-matches, meaning there were no entries in the admin data in which the variables match well.

The remaining 76 women seemed to match with an entry in the administrative data but had some mismatch in one of the variables. They were handled as follows. Women whose name and husband's name matched exactly with the administrative data and whose address matched except for the specific house number, meaning the block letter, slum name, and area name matched exactly, were counted as matches. Women whose name and address matched exactly but the husband's name was slightly different were also counted as matches. In both cases, these women were counted as matches due to high probability of being a match given that surveyors could make small errors in capturing addresses and women sometimes reported their husbands' nicknames or informal names instead of official names when completing the survey. The remaining women were not counted as matches even if two of the three variables were exact matches.

## **2. Creating a Dummy Measure for Financial Decision-Making Power in Household:**

All women who completed the baseline survey were asked who has the final say in spending decisions in their household (multiple selections allowed) on the following items: a. Food Items; b. Respondent's Clothing; c. Daily Household Items (matches, soap); d. Large Household Items (bike, TV); e. Entertainment f. Healthcare for Respondent or Family; g. Education for Respondent's Children or Grandchildren; h. Savings.

The heterogeneity analysis in this paper relies on a dummy variable that takes a value of 1 if women are involved in only 0-3 of these 8 financial decisions in their household. Such women are categorized as having "less financial decision-making authority" in their household than the women who report being involved in 4 or more of these decisions.

## **3. Creating a Dummy Measure for Lack of Autonomy in the Household:**

All women who completed the baseline survey were asked whether they need permission from anyone in their household to partake in the following activities: a. Visit Friends; b. Visit Natal Relatives; c. Go Shopping for Personal Items; d. Seek Healthcare for Herself or Her Children; e. Enroll her Children in School; f. Watch TV or Listen to Music; g. Sleep an Extra Hour in the Morning.

The heterogeneity analysis in this paper relies on a dummy variable that takes a value of 1 if women report needing permission for participating in any 1 or more of these activities. Such women are categorized as "lacking autonomy" in their household in comparison to the women who report not needing permission for participating in any of these activities.

## **Appendix B**

# **Appendix to Chapter 2**

### **B.1 Supplementary Tables and Figures**

**Table B.1:** *Balance Check from Census Sample*

| Balance Variables                  | Control             | (T1)<br>Infor-<br>mation | (T2)<br>Basic<br>Mediation | (T3)<br>Intensive<br>Mediation | Joint F-<br>Test<br>p-value |
|------------------------------------|---------------------|--------------------------|----------------------------|--------------------------------|-----------------------------|
| Age (yrs.)                         | 40.71<br>(0.55)     | 41.33<br>(0.52)          | 41.51<br>(0.57)            | 39.96<br>(0.54)                | 0.16                        |
| Literate (%)                       | 0.31<br>(0.03)      | 0.25<br>(0.02)           | 0.26<br>(0.02)             | 0.32<br>(0.02)                 | 0.08*                       |
| Muslim (%)                         | 0.27<br>(0.02)      | 0.29<br>(0.02)           | 0.27<br>(0.02)             | 0.32<br>(0.02)                 | 0.43                        |
| Non-Widows (%)                     | 0.22<br>(0.02)      | 0.18<br>(0.02)           | 0.21<br>(0.02)             | 0.22<br>(0.02)                 | 0.45                        |
| Household Size                     | 4.61<br>(0.13)      | 4.72<br>(0.12)           | 4.55<br>(0.12)             | 4.66<br>(0.13)                 | 0.79                        |
| Children Birthed                   | 4.02<br>(0.14)      | 4.01<br>(0.13)           | 4.01<br>(0.13)             | 3.62<br>(0.13)                 | 0.07*                       |
| Rent Home (%)                      | 0.10<br>(0.02)      | 0.10<br>(0.02)           | 0.13<br>(0.02)             | 0.12<br>(0.02)                 | 0.59                        |
| Years in Delhi                     | 28.09<br>(0.56)     | 27.15<br>(0.55)          | 28.73<br>(0.59)            | 27.76<br>(0.53)                | 0.24                        |
| No Personal Income (%)             | 0.50<br>(0.03)      | 0.50<br>(0.03)           | 0.50<br>(0.03)             | 0.51<br>(0.03)                 | 0.99                        |
| Monthly Income (Rs.)               | 1601.06<br>(111.40) | 1591.55<br>(112.99)      | 1591.89<br>(105.74)        | 1498.98<br>(99.94)             | 0.89                        |
| Spouse's Monthly Income (Rs.)      | 5360.96<br>(348.12) | 5558.66<br>(292.40)      | 5635.13<br>(354.19)        | 5594.69<br>(288.36)            | 0.94                        |
| Lives with Parents/In-Laws (%)     | 0.20<br>(0.02)      | 0.19<br>(0.02)           | 0.19<br>(0.02)             | 0.24<br>(0.02)                 | 0.19                        |
| Eligible > 1 Year (%)              | 0.36<br>(0.03)      | 0.39<br>(0.03)           | 0.36<br>(0.03)             | 0.36<br>(0.02)                 | 0.84                        |
| Previously Applied for Pension (%) | 0.22<br>(0.02)      | 0.22<br>(0.02)           | 0.20<br>(0.02)             | 0.23<br>(0.02)                 | 0.84                        |
| <b>Observations</b>                | <b>330</b>          | <b>355</b>               | <b>339</b>                 | <b>369</b>                     |                             |

Notes: This table compares average characteristics of the women in the sample across the control and treatment groups at the time of the original census survey in 2015. Column 1 presents the overall mean for the entire sample whereas Columns 2, 3, 4, and 5 present means for the control group, treatment 1 group, treatment 2 group, and treatment 3 group, respectively. Column 6 presents the p-value for the joint F-test on whether any of the average values listed in columns 2-5 are statistically different from one another. \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

**Table B.2:** *Impact of Pooled Treatment on Consumption Levels, No Controls*

| VARIABLES          | (1)<br>Spending on<br>Vegetables | (2)<br>Spending<br>on Fruit | (3)<br>Spending<br>on Meat | (4)<br>Spending on<br>Beverages | (5)<br>Spending on<br>Alcohol |
|--------------------|----------------------------------|-----------------------------|----------------------------|---------------------------------|-------------------------------|
| Received Treatment | 40.650*<br>(22.373)              | 3.877<br>(17.736)           | 8.956<br>(22.871)          | 6.266<br>(8.622)                | -17.746<br>(18.989)           |
| Observations       | 991                              | 998                         | 998                        | 992                             | 983                           |
| Control Mean       | 436.7                            | 115.4                       | 139.2                      | 52.11                           | 53.99                         |

Notes: This table presents the pooled treatment impact on household spending levels for consumption goods as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variables in Columns 1- 5 measure household spending in INR over the past week on the following items (1) vegetables (2) fruits (3) meat, poultry, and eggs (4) beverages (not including water) (5) alcohol, respectively. All regressions include slum fixed-effects, and robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

**Table B.3:** *Impact of Getting Pensions on Consumption Levels, 2SLS*

| VARIABLES       | (1)<br>Spending on<br>Vegetables | (2)<br>Spending<br>on Fruit | (3)<br>Spending<br>on Meat | (4)<br>Spending on<br>Beverages | (5)<br>Spending<br>on Alcohol |
|-----------------|----------------------------------|-----------------------------|----------------------------|---------------------------------|-------------------------------|
| Receive Pension | 456.458<br>(298.926)             | -8.574<br>(194.472)         | 11.049<br>(252.738)        | 46.748<br>(99.194)              | -200.477<br>(216.998)         |
| Observations    | 982                              | 990                         | 991                        | 988                             | 966                           |
| Control Mean    | 460.5                            | 109.9                       | 146.6                      | 54.87                           | 37.36                         |

Notes: This table presents the impact of receiving the widow pension on household spending levels for consumption goods as measured at the time of the Welfare End-Line survey. *Received Pension* is a dummy variable that takes on a value of 1 if the respondent started receiving the pension having applied after the intervention period for this study, instrumented by belonging to any of the three intervention groups. The dependent variables in Columns 1- 5 measure household spending in INR over the past week on the following items (1) vegetables (2) fruits (3) meat, poultry, and eggs (4) beverages (not including water) (5) alcohol, respectively. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, asset index and baseline spending on the given dependent variable. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.0

**Table B.4: Impact of Pooled Treatments on Finances, No Controls**

| VARIABLES          | (1)<br>In<br>Debt  | (2)<br>Can Pay Debt<br>in 1 Year | (3)<br>Emergency<br>Cash on Hand | (4)<br>Food Insecurity<br>Index | (5)<br>Healthcare<br>Insecurity Index |
|--------------------|--------------------|----------------------------------|----------------------------------|---------------------------------|---------------------------------------|
| Received Treatment | -0.067*<br>(0.036) | 0.080*<br>(0.044)                | 0.073**<br>(0.035)               | -0.032<br>(0.026)               | -0.020<br>(0.025)                     |
| Observations       | 1,005              | 594                              | 1,005                            | 1,005                           | 1,005                                 |
| Control Mean       | 0.643              | 0.301                            | 0.326                            | 0.443                           | 0.532                                 |

Notes: This table presents the pooled treatment impact on household finances as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variables in Columns 1-3 are dummy variables that take a value of 1 if the respondent (1) has any outstanding loans, (2) believes she can pay off current debt within 1 year, (3) keeps emergency cash on hand. The dependent variables in Column 4 and Column 5 measure food and healthcare insecurity, respectively (higher values signify greater insecurity). All regressions include slum fixed-effects, and robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

**Table B.5: Impact of Getting Pensions on Finances, 2SLS**

| VARIABLES       | (1)<br>In<br>Debt | (2)<br>Can Pay Debt<br>in 1 Year | (3)<br>Emergency<br>Cash on Hand | (4)<br>Food Insecurity<br>Index | (5)<br>Healthcare<br>Insecurity Index |
|-----------------|-------------------|----------------------------------|----------------------------------|---------------------------------|---------------------------------------|
| Receive Pension | -0.692<br>(0.474) | 0.879<br>(0.636)                 | 0.938*<br>(0.510)                | -0.490<br>(0.331)               | -0.281<br>(0.302)                     |
| Observations    | 1,005             | 594                              | 1,005                            | 1,005                           | 1,005                                 |
| Control Mean    | 0.590             | 0.353                            | 0.358                            | 0.439                           | 0.527                                 |

Notes: This table presents the impact of receiving the widow pension on household finances as measured at the time of the Welfare End-Line survey. *Received Pension* is a dummy variable that takes on a value of 1 if the respondent started receiving the pension having applied after the intervention period for this study, instrumented by belonging to any of the three intervention groups. The dependent variables in Columns 1-3 are dummy variables that take a value of 1 if the respondent (1) has any outstanding loans, (2) believes she can pay off current debt within 1 year, (3) keeps emergency cash on hand. The dependent variables in Column 4 and Column 5 measure food and healthcare insecurity, respectively (higher values signify greater insecurity). All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01

**Table B.6:** *Impact of Pooled Treatment on Individual Components of Food and Healthcare Insecurity Indices*

| VARIABLES             | (1)<br>Income<br>Insufficient<br>for Food | (2)<br>Went<br>Hungry,<br>Self | (3)<br>Went<br>Hungry,<br>Family | (4)<br>Borrowed<br>for Health<br>Expenses | (5)<br>Income<br>Insufficient<br>for Healthcare | (6)<br>Went W/out<br>Healthcare,<br>Self | (7)<br>Went W/out<br>Healthcare,<br>Family |
|-----------------------|-------------------------------------------|--------------------------------|----------------------------------|-------------------------------------------|-------------------------------------------------|------------------------------------------|--------------------------------------------|
| Received<br>Treatment | -0.050<br>(0.036)                         | -0.062*<br>(0.036)             | -0.016<br>(0.032)                | 0.072**<br>(0.036)                        | 0.012<br>(0.034)                                | -0.032<br>(0.037)                        | -0.007<br>(0.036)                          |
| Observations          | 1,005                                     | 1,005                          | 1,005                            | 1,005                                     | 1,005                                           | 1,005                                    | 1,005                                      |
| Control Mean          | 0.670                                     | 0.423                          | 0.238                            | 0.366                                     | 0.740                                           | 0.626                                    | 0.396                                      |

Notes: This table presents the pooled treatment impact on the individual components of the food insecurity index (Columns 1-3) and health care insecurity index (Columns 4-7) as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variables in Columns 1-7 are dummy variables that take a value of 1 if the respondent (1) reports HH income is insufficient to meet food needs (2) went a day without food in the past year (3) reported another family member went a day without food in the past 6 months (4) usually has to borrow money for health expense (5) reports HH income is insufficient to meet medical needs (6) went without health care when it was needed in the past year (7) reports another family member went without health care when it was needed in the past 6 months. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

**Table B.7:** *Impact of Pooled Treatment on Subjective Well-Being, No Controls*

| VARIABLES             | (1)<br>Very<br>Satisfied | (2)<br>Poor Mental<br>Health Index | (3)<br>Anxious      | (4)<br>Poor<br>Health | (5)<br>Sick in Past<br>6 Months | (6)<br>Less Financial<br>Decision-Making | (7)<br>Lacks<br>Autonomy |
|-----------------------|--------------------------|------------------------------------|---------------------|-----------------------|---------------------------------|------------------------------------------|--------------------------|
| Received<br>Treatment | 0.042**<br>(0.020)       | -0.102<br>(0.074)                  | -0.095**<br>(0.038) | -0.041<br>(0.036)     | -0.009<br>(0.030)               | -0.003<br>(0.029)                        | 0.035<br>(0.037)         |
| Observations          | 1,005                    | 992                                | 1,005               | 1,005                 | 1,005                           | 1,005                                    | 1,005                    |
| Control Mean          | 0.0705                   | 2.248                              | 0.577               | 0.630                 | 0.211                           | 0.189                                    | 0.555                    |

Notes: This table presents the pooled treatment impact on various measures of subjective well-being as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variable in Column 1 is a dummy variable that takes a value of 1 if the respondent reports being very satisfied with life. The dependent variable in Column 2 is a mental health index which represents the average value for 7 indicators measuring how often the respondent has exhibited signs of poor mental health in the past week. Each indicator takes a value from 0-3, with 3 being the most frequent. In Column 3, the dependent variable is a dummy representing whether the respondent often or always feels anxious about the future. Columns 4 and 5 are dummy variables that take a value of 1 if the respondent self-reports as being in generally poor health or having been sick in the past 6 months, respectively. Columns 6 and 7 are dummy variables that take a value of 1 if the respondent is not involved in many financial decisions in the household and if the respondent lacks autonomy to engage in various activities in the household without permission, respectively. All regressions include slum fixed-effects, and robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

**Table B.8: Impact of Getting Pensions on Subjective Well-Being, 2SLS**

| VARIABLES       | (1)<br>Very<br>Satisfied | (2)<br>Poor Mental<br>Health Index | (3)<br>Anxious     | (4)<br>Poor<br>Health | (5)<br>Sick in Past<br>6 Months | (6)<br>Less Financial<br>Decision-Making | (7)<br>Lacks<br>Autonomy |
|-----------------|--------------------------|------------------------------------|--------------------|-----------------------|---------------------------------|------------------------------------------|--------------------------|
| Receive Pension | 0.523*<br>(0.296)        | -0.752<br>(0.875)                  | -1.168*<br>(0.602) | -0.387<br>(0.419)     | -0.126<br>(0.351)               | -0.137<br>(0.264)                        | 0.367<br>(0.449)         |
| Observations    | 1,005                    | 992                                | 1,005              | 1,005                 | 1,005                           | 1,005                                    | 1,005                    |
| Control Mean    | 0.0996                   | 2.170                              | 0.507              | 0.609                 | 0.211                           | 0.194                                    | 0.611                    |

Notes: This table presents the impact of receiving the widow pension on various measures of subjective well-being as measured at the time of the Welfare End-Line survey. *Received Pension* is a dummy variable that takes on a value of 1 if the respondent started receiving the pension having applied after the intervention period for this study, instrumented by belonging to any of the three intervention groups. The dependent variable in Column 1 is a dummy variable that takes a value of 1 if the respondent reports being very satisfied with life. The dependent variable in Column 2 is a mental health index which represents the average value for 7 indicators measuring how often the respondent has exhibited signs of poor mental health in the past week. Each indicator takes a value from 0-3, with 3 being the most frequent. In Column 3, the dependent variable is a dummy representing whether the respondent often or always feels anxious about the future. Columns 4 and 5 are dummy variables that take a value of 1 if the respondent self-reports as being in generally poor health or having been sick in the past 6 months, respectively. Columns 6 and 7 are dummy variables that take a value of 1 if the respondent is not involved in many financial decisions in the household and if the respondent lacks autonomy to engage in various activities in the household without permission, respectively. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

**Table B.9: Impact of Pooled Treatment on Individual Components of Mental Health Index**

| VARIABLES          | (1)<br>Unhappy   | (2)<br>Trouble<br>Concentrating | (3)<br>Depressed  | (4)<br>Anxious      | (5)<br>Restless<br>Sleep | (6)<br>Talked<br>Less | (7)<br>Crying<br>Spells |
|--------------------|------------------|---------------------------------|-------------------|---------------------|--------------------------|-----------------------|-------------------------|
| Received Treatment | 0.017<br>(0.125) | -0.134<br>(0.106)               | -0.125<br>(0.104) | -0.223**<br>(0.098) | -0.095<br>(0.104)        | 0.012<br>(0.098)      | -0.154<br>(0.109)       |
| Observations       | 1,003            | 1,003                           | 1,002             | 1,003               | 1,001                    | 997                   | 1,000                   |
| Control Mean       | 2.048            | 2.195                           | 2.504             | 2.802               | 2.242                    | 1.730                 | 2.172                   |

Notes: This table presents the pooled treatment impact on the individual components of the poor mental health index as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variables in Columns 1-7 are indicators for how often the respondent has exhibited the given feeling or behavior in the past week on a scale from 0-3, with 3 representing often and 0 representing never. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

**Table B.10: Impact of Treatments on Views of the State, No Controls**

| VARIABLES          | (1)<br>View of State<br>Index | (2)<br>MLA Would<br>Help Me | (3)<br>Need to Know<br>MLA | (4)<br>MCD Would<br>Help Me | (5)<br>Need to Know<br>MCD |
|--------------------|-------------------------------|-----------------------------|----------------------------|-----------------------------|----------------------------|
| Received Treatment | 0.029<br>(0.020)              | 0.075**<br>(0.038)          | 0.042<br>(0.038)           | 0.045<br>(0.038)            | 0.016<br>(0.038)           |
| Observations       | 1,005                         | 1,005                       | 1,005                      | 1,005                       | 1,005                      |
| Control Mean       | 0.429                         | 0.520                       | 0.502                      | 0.507                       | 0.546                      |

Notes: This table presents the pooled treatment impact on views of the state as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variable in Column 1 is an index measuring views of the state, with higher values representing a more positive view. The dependent variables in Columns 2-5 are dummy variables that take a value of 1 if the respondent (2) believes her MLA would assist her if she asked (3) believes it is necessary to know the MLA to access government resources (4) believes her MCD Councilor would help her if she asked and (5) believes it is necessary to know the MCD Councilor to access government resources, respectively. All regressions include slum fixed-effects, and robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

**Table B.11: Impact of Pooled Treatment on Individual Components of Views of the State Index**

| VARIABLES          | (1)<br>Gov't Helps<br>Poor | (2)<br>Satisfied<br>with Gov't | (3)<br>Comfortable Going<br>Alone, Gov't Office | (4)<br>Women Can<br>Approach Gov't | (5)<br>Applications<br>Are Easy |
|--------------------|----------------------------|--------------------------------|-------------------------------------------------|------------------------------------|---------------------------------|
| Received Treatment | 0.057<br>(0.038)           | 0.011<br>(0.037)               | 0.031<br>(0.037)                                | 0.019<br>(0.037)                   | 0.039<br>(0.026)                |
| Observations       | 1,005                      | 1,005                          | 1,005                                           | 1,005                              | 1,005                           |
| Control Mean       | 0.524                      | 0.639                          | 0.423                                           | 0.432                              | 0.128                           |

Notes: This table presents the pooled treatment impact on the individual components of the views of state index as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variables in Columns 1-5 are dummy variables that take a value of 1 if the respondent (1) believes the government does enough to help the poor (2) reports satisfaction with the government (3) reports comfort going along to government offices (4) reports that women can approach the government as easily as men, and (5) reports that she personally finds it easy to complete government applications. All regressions include slum fixed-effects and control for respondent age, marital status, years living in Delhi, literacy, religion, number of children, household income, household size, and asset index. Robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

**Table B.12: Impact of Pooled Treatment on Who One Relies on to Access the State, No Controls**

| VARIABLES             | (1)<br>Turn to<br>MLA | (2)<br>Turn to<br>MCD | (3)<br>Turn to<br>Self | (4)<br>Turn to<br>Fam/Friends | (5)<br>Turn to<br>Pradhan | (6)<br>Turn to Gov't<br>Employee |
|-----------------------|-----------------------|-----------------------|------------------------|-------------------------------|---------------------------|----------------------------------|
| Received<br>Treatment | -0.027<br>(0.035)     | -0.038<br>(0.027)     | 0.036**<br>(0.017)     | -0.007<br>(0.015)             | 0.013<br>(0.015)          | 0.007<br>(0.011)                 |
| Observations          | 1,005                 | 1,005                 | 1,005                  | 1,005                         | 1,005                     | 1,005                            |
| Control Mean          | 0.661                 | 0.163                 | 0.0441                 | 0.0441                        | 0.0441                    | 0.0176                           |

Notes: This table presents the pooled treatment impact on who respondents turn to first when they need to get something done from the government, as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variables in Column 1 – 6 represent dummy variables which take a value of 1 when the respondent turns first to (1) her MLA, (2) her MCD Councilor, (3) herself, (4) her family and friends, (5) her Pradhan (slum leader), and (6) a government employee, respectively. All regressions include slum fixed-effects, and robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

**Table B.13: Impact of Pooled Treatment on Political Participation**

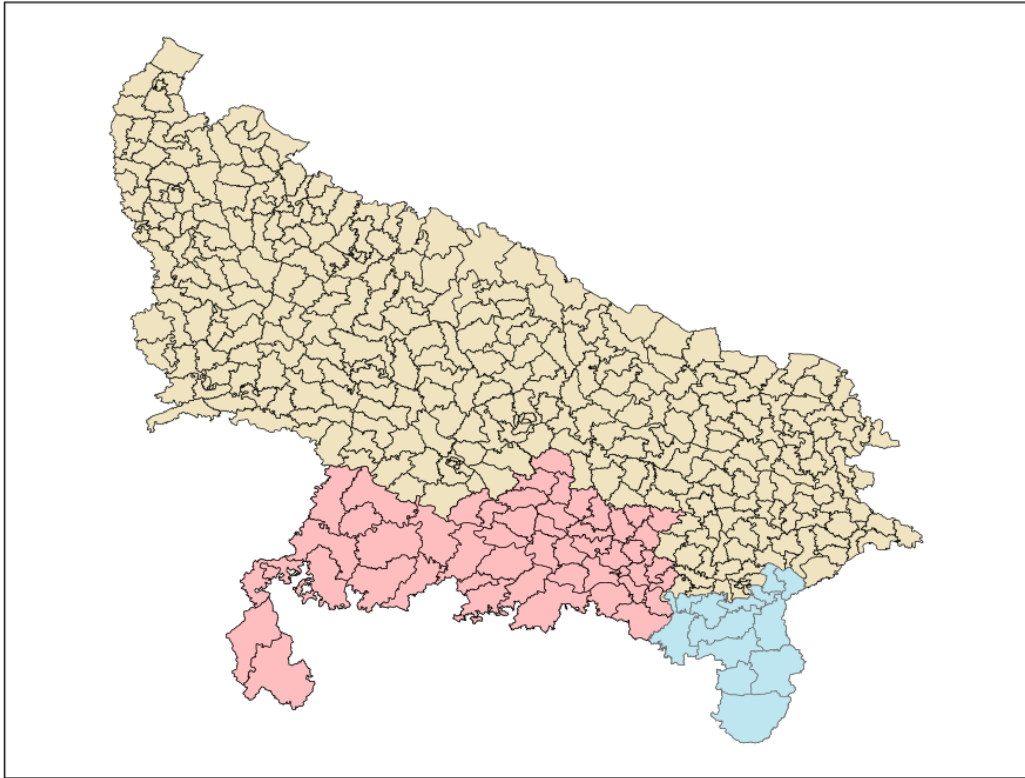
| VARIABLES          | (1)<br>Complained<br>to Politician | (2)<br>Helped Others<br>Apply | (3)<br>Voted MCD<br>Election | (4)<br>Went to<br>MLA | (5)<br>Went to<br>MCD | (6)<br>Applied for<br>Programs |
|--------------------|------------------------------------|-------------------------------|------------------------------|-----------------------|-----------------------|--------------------------------|
| Received Treatment | -0.076**<br>(0.034)                | -0.041<br>(0.032)             | 0.026<br>(0.027)             | -0.055<br>(0.038)     | -0.039<br>(0.031)     | -0.059<br>(0.037)              |
| Observations       | 1,005                              | 1,005                         | 1,005                        | 1,005                 | 1,005                 | 1,005                          |
| Control Mean       | 0.295                              | 0.260                         | 0.828                        | 0.445                 | 0.256                 | 0.392                          |

Notes: This table presents the pooled treatment impact on various dimensions of political participation, as measured at the time of the Welfare End-Line survey. *Received Treatment* is a dummy variable that takes on a value of 1 if the respondent belongs to any of the three intervention groups in this study and a value of 0 if the respondent belongs to the control group. The dependent variables in Column 1 – 6 represent dummy variables which take a value of 1 if the respondent partook in the following activities in the past year (1) complained to an elected official about difficulties accessing public services or welfare programs, (2) helped other people in her community apply for government programs (3) voted in the MCD Councilor election (4) went to her MLA's office for any reason (5) went to her MCD Councilor's office for any reason (6) applied for government program(s) other than the widow pension scheme, respectively. All regressions include slum fixed-effects, and robust standard errors are presented in brackets. Statistical significance is denoted as: \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01.

## **Appendix C**

# **Appendix to Chapter 3**

### **C.1 Supplementary Tables and Figures**



Notes: The map above shows a constituency-level map of Uttar Pradesh, India. The constituencies in pink held elections in Phase 4 of the 2017 State Assembly Elections, and the constituencies highlighted in blue held elections in Phase 7 of the elections.

**Figure C.1:** *Map of Uttar Pradesh State Assembly Constituencies, 2017 Election*

**Table C.1:** *Impact of Women's Mobilization Message on Vote Shares of Criminal Candidates*

|                             | (1)<br>Vote<br>Share | (2)<br>Vote<br>Share | (3)<br>Vote<br>Share | (4)<br>Vote<br>Share  | (5)<br>Vote<br>Share  | (6)<br>Vote<br>Share |
|-----------------------------|----------------------|----------------------|----------------------|-----------------------|-----------------------|----------------------|
| Women                       | 0.0110<br>(0.00838)  | 0.0117<br>(0.00814)  | 0.0126<br>(0.00926)  | 0.0102<br>(0.00868)   | 0.0112<br>(0.00823)   | 0.0126<br>(0.00944)  |
| Women x Criminal            | -0.0130<br>(0.0134)  | -0.0149<br>(0.0123)  | -0.0176<br>(0.0140)  |                       |                       |                      |
| Women x Non-Violent         |                      |                      |                      | -0.00361<br>(0.0169)  | -0.0142<br>(0.0144)   | -0.0184<br>(0.0164)  |
| Women x Violent, Non-Murder |                      |                      |                      | 0.00516<br>(0.0230)   | -0.0111<br>(0.0201)   | -0.0215<br>(0.0235)  |
| Women x Murder Attempt      |                      |                      |                      | -0.0398<br>(0.0347)   | -0.0246<br>(0.0355)   | -0.0176<br>(0.0387)  |
| Women x Murder              |                      |                      |                      | -0.0415**<br>(0.0198) | -0.000904<br>(0.0188) | -0.00705<br>(0.0143) |
| Adjusted R <sup>2</sup>     | 0.054                | 0.198                | 0.353                | 0.071                 | 0.206                 | 0.353                |
| Constituency FE             | Yes                  | Yes                  | No                   | Yes                   | Yes                   | No                   |
| Party FE                    | No                   | Yes                  | No                   | No                    | Yes                   | No                   |
| Candidate FE                | No                   | No                   | Yes                  | No                    | No                    | Yes                  |
| Observations                | 9943                 | 9943                 | 9943                 | 9943                  | 9943                  | 9943                 |

Notes: This table shows results from polling-station-by-candidate-level regressions of vote share on the women's mobilization treatment interacted with different types of criminality. All data represented in this table comes from Phase 4 of the UP State Assembly Elections of 2017. In Row 1, the independent variable *Women* is a dummy that takes the value of 1 if the candidate is standing in a polling station that was given any one of the three information-based interventions. In Rows 2-6, the independent variables are interactions of the *Women* variable with: *Criminal*, a dummy that takes the value of 1 if the candidate faces any criminal charges; *Non-Violent*, a dummy that takes a value of 1 if the candidate faces charges that are all non-violent; *Violent, Non-Murder*, a dummy that takes a value of 1 if the candidate faces at least one violent charge but no charges related to murder; *Murder Attempt*, a dummy that takes a value of 1 if the candidate faces at least one attempt to murder charge but no murder charges; and *Murder*, a dummy that takes the value of 1 if the candidate faces at least one murder charge. Regressions include controls for total population, share female, share SC/ST, share literate, number of registered voters, and constituency fixed-effects, party fixed-effects, or candidate fixed-effects, as indicated by "Yes" or "No" in the bottom panel. Standard errors, clustered at the village and candidate level, are shown in brackets. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table C.2:** *Impact of Women's Mobilization Message on Polling Station Turnout, by Criminality of Candidates*

|                | (1)<br>All Votes     | (2)<br>Female<br>Votes | (3)<br>Male<br>Votes | (4)<br>All<br>Votes  | (5)<br>Female<br>Votes | (6)<br>Male<br>Votes  | (7)<br>All<br>Votes   | (8)<br>Female<br>Votes | (9)<br>Male<br>Votes |
|----------------|----------------------|------------------------|----------------------|----------------------|------------------------|-----------------------|-----------------------|------------------------|----------------------|
| Women          | 0.0124*<br>(0.00732) | 0.00896<br>(0.00780)   | 0.00598<br>(0.00653) | 0.00501<br>(0.0149)  | 0.00346<br>(0.0145)    | 0.00410<br>(0.0185)   | 0.00508<br>(0.0149)   | 0.00352<br>(0.0145)    | 0.00418<br>(0.0185)  |
| Women x Crim   |                      |                        |                      | -0.00320<br>(0.0167) | -0.00545<br>(0.0168)   | -0.000398<br>(0.0207) |                       |                        |                      |
| Women x Murder |                      |                        |                      | 0.0751**<br>(0.0314) | 0.0729**<br>(0.0340)   | 0.0807**<br>(0.0335)  |                       |                        |                      |
| Women x 1 Crim |                      |                        |                      |                      |                        |                       | -0.000649<br>(0.0173) | -0.00403<br>(0.0173)   | 0.00353<br>(0.0216)  |
| Women x 2 Crim |                      |                        |                      |                      |                        |                       | 0.0370<br>(0.0255)    | 0.0375<br>(0.0271)     | 0.0391<br>(0.0286)   |
| Women x 3 Crim |                      |                        |                      |                      |                        |                       | 0.00431<br>(0.0286)   | -0.00374<br>(0.0379)   | 0.0108<br>(0.0352)   |
| Adjusted $R^2$ | 0.817                | 0.801                  | 0.825                | 0.810                | 0.796                  | 0.818                 | 0.810                 | 0.796                  | 0.818                |
| Observations   | 3813                 | 3813                   | 4266                 | 3358                 | 3358                   | 3357                  | 3358                  | 3358                   | 3357                 |

Notes: This tables shows regressions of log votes cast in total (or separately by gender) at the polling station level on the women's mobilization treatment interacted with different characteristics of candidates running at the polling station. All data represented in this table comes from Phase 4 of the UP State Assembly Elections of 2017. In Row 1, the independent variable *Women* is a dummy that takes the value of 1 if the polling station was targeted by the women's mobilization message. In Rows 2-6, the independent variables are interactions of the *Women* variable with: *Crim*, a dummy that takes the value of 1 if any major party candidate on the ticket for the given polling station faces any criminal charges; *Murder*, a dummy variable that takes the value of 1 if any major party candidate on the ticket for the given polling station faces at least one murder charge; *1 Crim*, a dummy variable that takes the value of 1 if there is only 1 candidate facing criminal charges among major party candidates on the ticket for the given polling station; *2 Crim*, a dummy variable that takes the value of 1 if there are exactly 2 candidates facing criminal charges among major party candidates on the ticket for that given polling stations; and *3 Crim*, a dummy variable that takes a value of 1 if there are exactly 3 candidates facing criminal charges among major party candidates for the given polling station. All regressions include controls for total population, share female, share SC/ST, share literate, number of registered voters, and constituency fixed-effects. Standard errors, clustered at the village level, are shown in brackets. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## C.2 Treatment Scripts

### 1. Placebo Message

- Text Message: *"Vote only after getting to know your candidates!"*
- Voice Message: *"This message is from an unbiased, non-political NGO, Centre for Governance and Development. Get to know your candidates correctly, and on Election Day, give your vote only after thinking carefully!"*

### 2. Women's Mobilization Message

- Text Message: *"Women are voting in record numbers. Encourage all women to vote! Vote only after getting to know your candidates!"*
- Voice Message: *"This message is from an unbiased, non-political NGO, Center for Governance and Development. Women across the country are voting in record numbers. When all household members vote, your family's power increases. So definitely encourage all women to vote! Get to know your candidates correctly, and on Election Day, give your vote only after thinking carefully!"*

### 3. Basic Information Message

- Text Message: *"Vote only after getting to know your candidates! In your area: <Insert Criminality Information of Candidates>"*
- Voice Message: *"This message is from an unbiased, non-political NGO, Center for Governance and Development. Get to know your candidates correctly, and on Election Day, give your vote only after thinking carefully! In your area: <Insert Criminality Information of Candidates>"*

### 4. Information plus Coordination Message

- Text Message: *"Vote only after getting to know your candidates! Many people in your area have already received this message. In your area: <Insert Criminality Information of Candidates>"*

- Voice Message: *"This message is from an unbiased non-political NGO, Center for Governance and Development, and many people in your area have already received it. Get to know your candidates correctly, and on Election Day, give your vote only after thinking carefully! In your area: «Insert Criminality Information of Candidates». Now you can elect the right candidate with the people in your area."*

## 5. Information plus Ethnic Voting Message

- Text Message: *"Don't vote only on the basis of caste or religion. Vote only after getting to know your candidates! In your area: <Insert Criminality Information of Candidates>."*
- Voice Message: *"This message is from an unbiased, non-political NGO, Center for Governance and Development. Don't follow your old habits and vote only on the basis of caste or religion. Get to know your candidates correctly, and on Election Day, give your vote only after thinking carefully! In your area: «Insert Criminality Information of Candidates>."*