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# Missing the target: Does increased capacity of the local government improve beneficiary selection?

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# Missing the target:
## Does increased capacity of the local government improve beneficiary selection?*

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**Abstract**

The implementation of social policies is often hampered by the fact that local decision-makers may be unwilling or unable to implement the policy as intended by the central government. In contrast to research that focuses on incentivizing and holding local decision makers accountable, we examine capacity constraints in the context of beneficiary selection. Using a large-scale randomized trial in Bangladesh, we find that training and data provision improved knowledge of selection criteria. However, evidence of better targeting was limited, except for easily observable indicators of vulnerability. Improvements in targeting were more pronounced in committees led by highly educated chairpersons.

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# 1 Introduction

Principal-agent problems are pervasive across all sectors of the economy. While non-aligned preferences and the inability to monitor agents have been studied extensively for many decades (e.g., Eisenhardt, 1989; Carlos, 1992; Vaubel, 2006; Rauchhaus, 2009; Gneezy, Meier and Rey-Biel, 2011), there is a paucity of research on the potential limitations of agents' capacity to perform tasks as expected by the principal. This is despite the fact that such limitations may be significant.

This phenomenon manifests in the context of the provision of public services, where national governments delegate the implementation of policies to local government representatives. These agents have discretion in their implementation of rules and policies but their interests are not always aligned with those of the national government. Consequently, moral hazard issues emerge, and resources are frequently utilized in ways that differ from the original intentions of the policy design (e.g., Bardhan and Mookherjee, 2006; Ferraz and Finan, 2011; Pepinsky, Pierskalla and Sacks, 2017). The performance of local governments also depends critically on the conditions in which they operate. They often face constraints in terms of training, information, financial resources and time. Such capacity constraints are particularly pronounced in developing countries (e.g., Besley and Persson, 2009). As a result, even local decision-makers who are honest and public-spirited may find it difficult to implement policies as intended.

The limited consideration of capacity constraints as causal determinants of policy implementation in the literature motivates our study. While capacity constraints have been documented in the literature (Besley and Persson, 2009, 2010), interventions aimed at mitigating them to improve targeting have received little attention.[1] Instead, research has focused primarily on interventions to improve the accountability of public officials or to assist citizens in claiming their rights. These include merit-based employment and pay schemes (Banerjee and Duflo, 2006; Bourdon, Froelich and Michaelowa, 2006, 2010; Duflo, Hanna and Ryan, 2012; Muralidharan and Sundararaman, 2011), the provision of information about entitlements and application assistance to the intended target group (Francken, Minten and Swinnen, 2009; Reinikka and Svensson, 2004, 2011; Gupta, 2017; Banerjee et al., 2018; Amirapu et al., 2024). Such systems include those designed to monitor and reward public officials and health workers (Banerjee et al., 2011; Deininger and Mpuga, 2005; Ashraf, Bandiera and Lee, 2014; Ashraf, Bandiera and Jack, 2014; Deserranno et al., 2024). However, in their meta-review on the impact of transparency on

[1] More evidence exists for the disbursement of social transfers to beneficiaries, where technology has been shown to play an important role in simultaneously addressing both capacity constraints and corruption (Muralidharan, Niehaus and Sukhtankar, 2016; Dodge et al., 2021)

governance, Kosack and Fung (2014) emphasize that in many cases the problem is not that local officials or other service providers are unwilling to cooperate, but that a variety of other reasons, including most importantly capacity constraints, affect their performance. In such situations, approaches that focus solely on monitoring and accountability may prove ineffective.

Our contribution is to shift the focus to specific and plausible capacity constraints in public service delivery. In particular, we investigate whether and how enhancing local capacity can improve the selection of beneficiaries with a specific focus on social transfers. In close collaboration with the Ministry of Social Welfare in Dhaka, we evaluate an intervention that aims to enhance local capacity by providing local-level selectors with training on selection rules and procedures, as well as readily accessible data on the target population. While our focus is on the selection of Old Age Allowance beneficiaries in Bangladesh, a rapidly aging developing country, our findings are applicable to other developing country contexts where local committees are responsible for selecting beneficiaries.

Our study thus complements work by Alatas et al. (2012) and Alatas et al. (2016), which examine the causal effects of major changes in selection procedures. Alatas et al. (2012) show that community-based targeting is less effective than targeting based on proxy means testing. Alatas et al. (2016) document that requiring individuals in the target group to apply improves targeting to poorer households in the Indonesian context. In contrast, in our study, we focus on local government capacity constraints and work directly with selectors by providing data and training.

The design of the field experiment is based on our research findings that social pensions are poorly targeted, that selectors lack knowledge of eligibility criteria, and that they face challenges in assessing applicants' eligibility (documented in Section 2 and, in more detail, in Asri et al., 2020). The intervention under evaluation demonstrably enhances local capacity; however, it does not yield a statistically significant improvement in the selection of beneficiaries in terms of the pre-registered measures of poverty and eligibility. Nevertheless, the interventions facilitate the selection of individuals who are visibly vulnerable according to multiple criteria, including a high poverty indicator and other more readily observable factors such as owning less land, living alone, and being unable to walk. The combined treatment of training and data provision also induces selectors to select more women as beneficiaries, who are otherwise disadvantaged in locally practiced selection procedures. The intervention improves pro-poor targeting in local government areas where committees are led by a highly educated chairperson, suggesting the need for longer-term capacity investments.

The rest of the paper is organized as follows. In Section 2 we provide the relevant background for the field experiment and insights from our descriptive research, Section 3 describes the intervention, Section 4 explains the empirical methodology and data, Section 5 presents the results, and Section 6 concludes.

# 2 Background

The population of Bangladesh is undergoing an accelerated aging process (United Nations, 2022). While in 2000, only 6% of the population were older than 60 years of age, this figure rose to 9% in 2020 and is projected to reach over 20% by 2050. In order to address the needs of an aging population, the Government of Bangladesh has implemented a contributory pension scheme for those who are able to save a portion of their income during their working years (Jahid, 2023).

For those currently employed and able to save, this policy is of significant importance. However, for the elderly poor who were unable to save during their working years and thus unable to satisfy their most basic needs, the Old Age Allowance provides a means of support. In order to mitigate the issue of old age poverty, the government has been providing a benefit of 500 Bangladeshi Taka (BDT; approximately 19 USD PPP) per month to selected beneficiaries since 1998. Since its inception, the number of beneficiaries has grown substantially, reaching over 5.8 million individuals and establishing it as one of the most expansive social safety nets in the country (Department of Social Services, 2020). Reaching the intended beneficiaries remains a major challenge given the increasing number of elderly poor (Moazzem and Shibly, 2023; Maxwell Stamp, 2017) and is expected to become even more important in the future (United Nations, 2022).

## 2.1 Guidelines for beneficiary selection

In the absence of up-to-date population registries that include income and wealth data, a local committee comprising 18 members is responsible for selecting a limited number of beneficiaries on an annual basis. The committee comprises representatives of the local government (referred to as the Union Parishad) and representatives of sets of two or three villages, also designated as wards. Each local government area is comprised of nine wards, with each ward represented by a single ward member. Additionally, the committee comprises three female representatives and the local social worker. The next level of administration is that of the subdistrict. Additionally, the elected subdistrict chairperson and the appointed subdistrict chief executive officer are represented in the selection committees through their designated representatives. Ultimately, the local member of parliament from the area is represented by one female and one male representative (Government of Bangladesh, 2013). All members of the selection committee, henceforth referred to as "selectors," are elected by the local population or appointed by another elected individual at a higher level, with the exception of the social worker, who is a civil servant, and the representative of the subdistrict chief executive officer, who is appointed by a civil servant.

Table 1: Administrative geography in rural Bangladesh

| **Average population** | **Administrative unit** | **Number** |
|---|---|---|
| 20 million | Division | 8 |
| 2.5 million | District | 64 |
| 250000 | Sub-district / Upazila | 492 |
| 27000 | Union - local government | 4573 |
| 3000 | Ward | 9 wards per union |

*Notes:* Population statistics are taken from United Nations (2022) and for further details on the local government system in Bangladesh see Commonwealth Local Government Forum (2018).

In regard to the selection of beneficiaries, the national government establishes the criteria, which include age, income, working status, physical condition (health), and social condition (household composition). Additionally, the government outlines the selection process to be followed. In accordance with the annual budgetary allotment for the social pension, the national government initially notifies the local governments at the union level of the number of supplementary pensions that will be available at the local level and requests their selection of new beneficiaries. Secondly, the selection committee is responsible for disseminating information to the local population regarding the selection process. This entails announcing the timing of the selection and the eligibility criteria. Subsequently, the selection committee identifies the most suitable candidates from the pool of applicants and presents the list of selected beneficiaries to the Old Age Allowance selection committee at the subdistrict level. The subdistrict committee is tasked with the responsibility of reviewing the list, making any necessary alterations, and subsequently approving it (Government of Bangladesh, 2013).

## 2.2 Targeting in practice

In practice, the selection of beneficiaries does not appear to align with the official guidelines. In our qualitative and quantitative research, we have identified two frequently used practices. First, selectors disseminate information to citizens, frequently those with whom they are personally acquainted, regarding the availability of new pensions. Alternatively, local residents may proactively seek to be considered as beneficiaries by contacting committee members. Secondly, the subdistrict government typically oversees the implementation of so-called "open-field selections." These involve the gathering of all elderly individuals from a specific local government area in front of the local government office. Committee members then proceed to conduct a selection process based on the aforementioned criteria and a set of selected questions.[2]

While in the first case, knowing someone from the selection committee appears to be crucial, in the second case the focus appears to shift towards age as the most important eligibility condition. Local representatives tend to ask about available family support and directly observe the physical condition of the elderly person (as long as the person is well enough to even appear at the place or has relatives to carry him or her to this place), but other criteria such as wealth appear to be neglected in this ad-hoc selection.[3] We observed that in some cases, the committees set the age cutoff higher than in the implementation guidelines supposedly to limit the number of elderly that they need to consider during the selection of beneficiaries.[4]

Our pilot survey, conducted in eight unions within the same region as the field experiment, indicates that beneficiaries are as eligible as non-beneficiaries.[5] Comparing the two groups in terms of their wealth (Figure 1, left panel) and more specifically in terms of their eligibility for the Old Age Allowance (Figure 1, right panel) shows that the two groups of beneficiaries (in green) and non-beneficiaries (in red) are hardly distinguishable, which means that targeting was not more effective than randomly selecting elderly into the program.

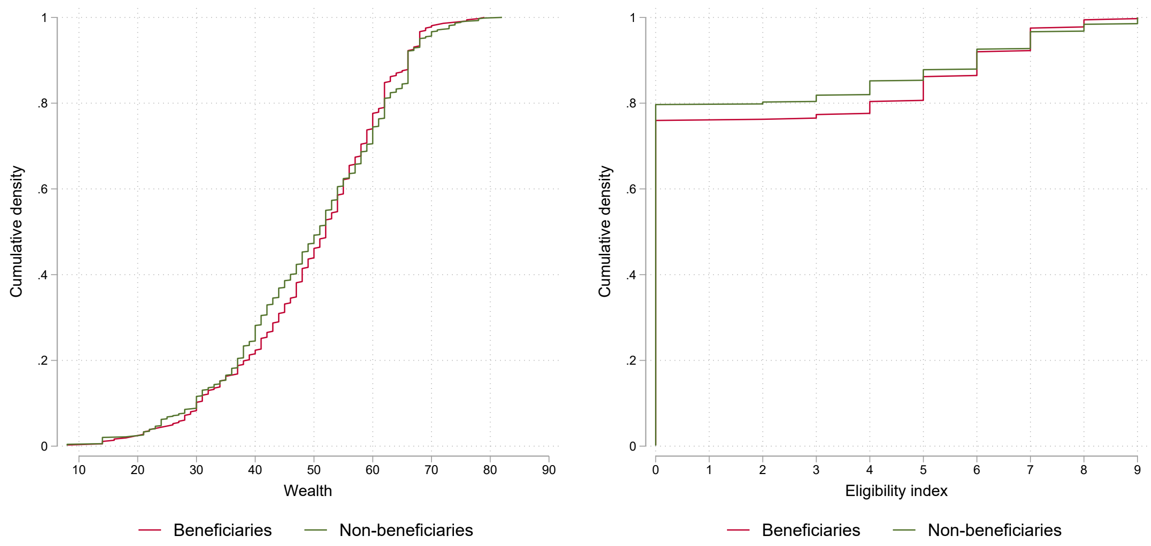
[2]As applications are seldom formalized, there is no systematic record of the number of applications received during each selection round. However, observations made during open field selections and the implementation of interventions for this study indicated that the number of individuals seeking to obtain the Old Age Allowance greatly exceeds the number of available pensions.

[3]The term "ad hoc" is used to describe the situation in which the selection process is conducted within a limited timeframe, such as a few hours, in which the members of a selection committee are required to evaluate hundreds of elderly individuals who are waiting in lines at a local government office.

[4]The respondents who participated in qualitative interviews and focus group discussions reported both scenarios. Moreover, one of our local co-authors, Kumar Biswas, attended open field selections with the objective of corroborating these insights derived from qualitative interviews and focus group discussions.

[5]In this phase of research, in May 2018, we collected survey data from three different groups: (i) a random sample of the elderly population (potential beneficiaries), $N = 1051$, (ii) a random sample of newly selected beneficiaries, $N = 363$, and (iii) the selectors who were in charge of the last beneficiary selection, $N = 77$.

Figure 1: Wealth and eligibility of beneficiaries and non-beneficiaries

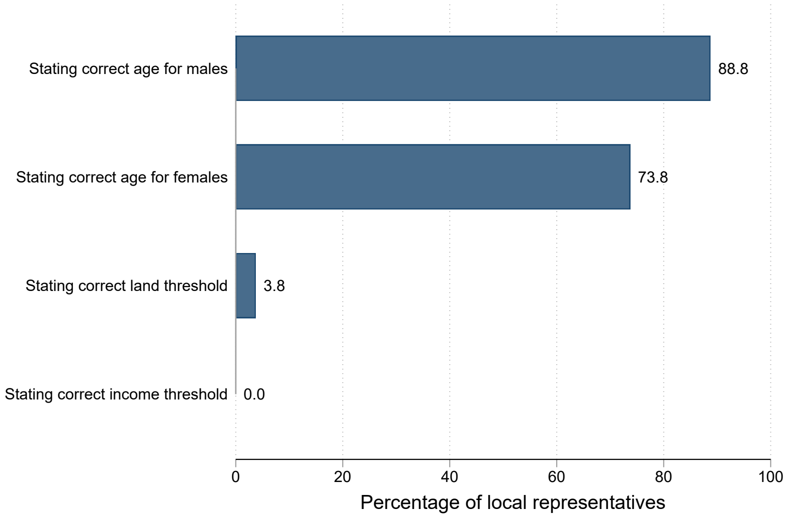



*Notes:* The panel on the left displays the cumulative density functions (cdfs) of wealth, as measured by the inverted probability of poverty index (Schreiner, 2013; Kshirsagar et al., 2017), the right panel displays the cdfs of an eligibility index based on the official eligibility rules (see Appendix C.3). Source: Beneficiary and elderly survey 2018.

## Underlying reasons

There are a number of potential explanations for why the Old Age Allowance program is not targeted towards the poor. Selectors in charge may struggle to follow the guidelines in practice. The results of our pilot survey indicate that selectors possess only a limited understanding of the eligibility criteria. While most of the selectors know the correct age threshold for males (88.8%) and the correct age threshold for females (73.8%), only very few know the threshold for land ownership (3.8%) and for income (0.0%).

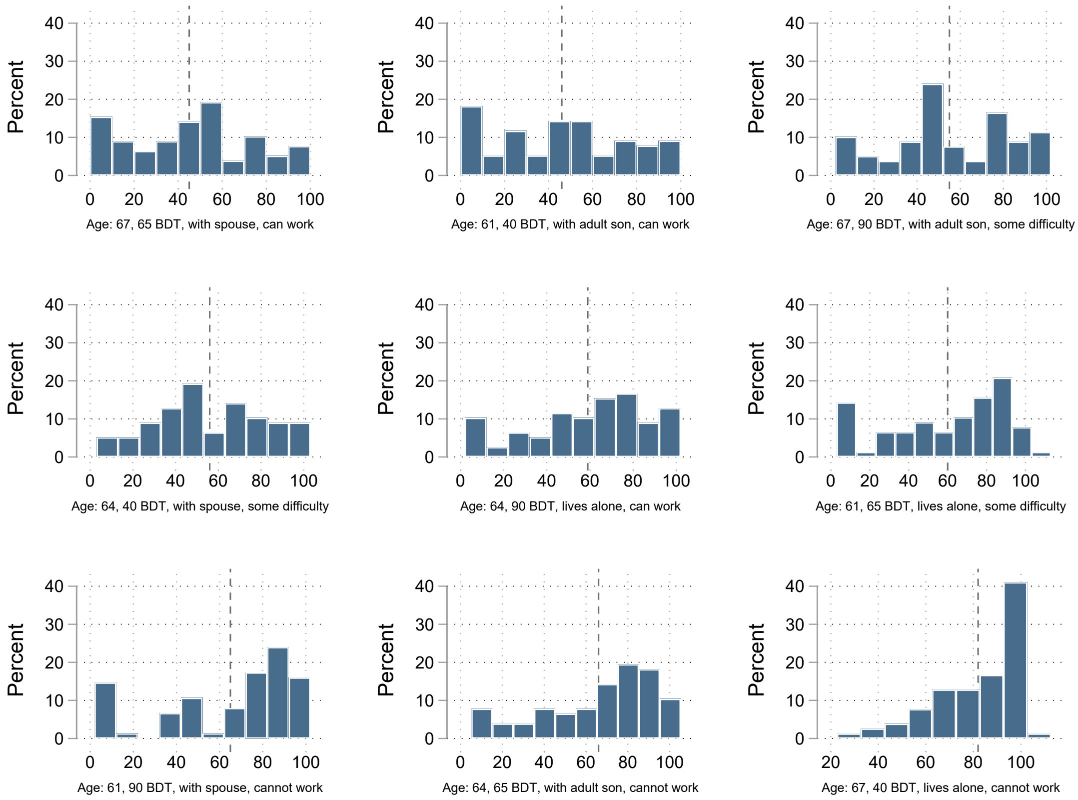
Figure 2: Knowledge of eligibility criteria




*Notes:* Knowledge of individual eligibility criteria as a measure of selectors' capacity.
Source: Pilot survey of selectors, 2018.

It appears that selectors encounter difficulties when attempting to assess the eligibility of individuals (Figure 3). We asked the selectors to rate 18 profiles of fictional applicants, which were varied according to the following criteria: gender, age, amount of cash available per day, living situation, and whether the applicant has physical difficulties with work (as indicated in the figure labels). A total of 16 out of the 18 profiles received ratings from 0 to 100 and the remaining two ratings from 20 to 100. It is noteworthy that the explained variation in ratings, as measured with the R-squared triples once we account for selector fixed effects indicating a lot of variation in eligibility ratings between the selectors. For instance the R-squared increases from 12% to 31% for female profiles when we include selector fixed effects and the selector fixed effects are jointly significant based on an F-Test (Asri et al., 2020).

Figure 3: Eligibility ratings - female profiles




The vertical dashed line indicates the mean value of all ratings.

*Notes:* The nine panels represent one profile each. A profile consists of a characterization of a potential female or male Old Age Allowance applicant with differences in age, income, coresidence status (living alone, with spouse or with adult son) and ability to work. Source: Pilot survey of selectors 2018.

When surveyors ask selectors whether they need support for the eligibility assessment, 60% of the respondents report that they very much need support and being asked for the type of support, 46% indicate that they need support in terms of staff and 37% indicate that they needed support in terms of data, while only 9.5% and 8.1% indicate that they most urgently need more funding and better guidelines, respectively. The considerable demand for additional data appears to be a genuine requirement. This reflects the understanding that data, in the form of information about the applicants, is crucial for a comprehensive selection process.[6]

In light of these descriptive results and the dearth of evidence in the literature concerning the potential benefits of enhancing local-level capacity for beneficiary selection in the context of social transfer targeting, our intervention design is oriented towards addressing these capacity constraints.

[6]The corresponding survey question described data as information on the people in the target group.

## 3 Description of intervention

The intervention design builds directly on the aforementioned insights on the mistargeting of the Old Age Allowance in Bangladesh (see Section 2). Its primary objective is to address the prevailing capacity constraints. The underlying theory of change posits that an intervention that enhances the knowledge of eligibility criteria and provides information on the target group to selectors can improve the selection of beneficiaries. Accordingly, an intervention comprising two components was designed. Training and data on the target group are provided to selectors in order to facilitate a more systematic and eligibility-focused allocation of social pension benefits. The intervention was conducted by a nonprofit organization on behalf of the Department of Social Services.

The intervention was implemented at the local government level. In each treatment area, the training component was provided to all selectors. However, the target-group data collection and transfer was implemented only in three out of nine randomly selected wards in each treatment union.

**Component 1: Training selectors on the beneficiary selection criteria**

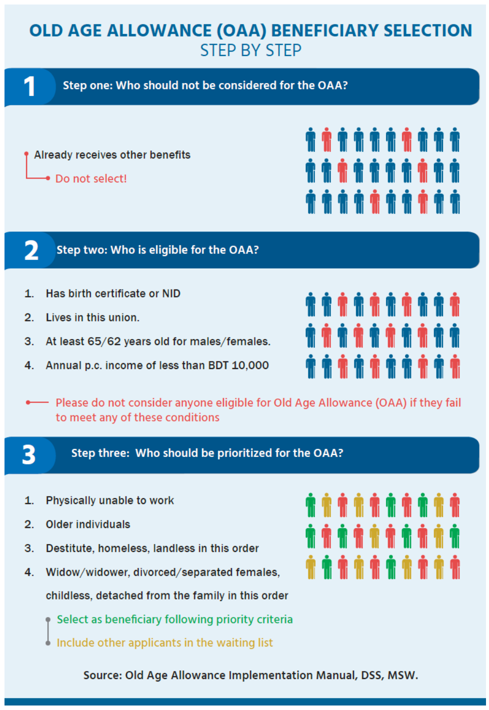
The training program on the selection criteria for the Old Age Allowance and on an information tool, designated as the "Eligibility Information Card" (EIC), was developed in collaboration with the Ministry of Social Welfare. The EIC is a tool that can be utilized to efficiently gather pertinent data regarding an applicant's eligibility, facilitating the organization of applicants based on their fulfillment of eligibility and priority criteria. One-to-one training sessions were designed in which the trainer would present videos to the trainee and engage in a structured discussion of the content. The one-to-one format enabled the trainer to interact with local representatives from a range of educational backgrounds, including those with no formal education and university graduates. The videos ensured that all trainees received the same information without any alterations or interpretations by the trainers. Moreover, the videos permitted the content to be divided into segments, thereby facilitating the assurance that each trainee had a comprehensive grasp of the material presented after each video. The training sessions were conducted in private, typically at the residence of the selection committee member or in a government office.

The training program was conducted in accordance with a structured protocol, which included the presentation of educational videos, guided verbal interactions with the trainee, a brief practical exercise, and a final examination. The practice session included the sorting of hypothetical profiles in accordance with the national guidelines. In the event that a trainee failed to comprehend or misinterpreted the material presented, the trainer provided a reiteration of the explanations and addressed any remaining queries. The duration of each training session was between 45 and 90 minutes. The animated videos, which were created specifically for this intervention, provide information about the policy objectives of the Old Age Allowance and illustrate how a systematic selection of beneficiaries can be carried out. Figure 4 illustrates the content of the videos, which follow the plot. Upon completion of the training, the trainer provided the trainee with a foldable poster that summarized the three steps for beneficiary selection (Figure 5).

Figure 4: Training videos

*Notes:* Screenshots from the video scenes used for training purposes. The videos were used to demonstrate the official selection criteria and to show how the selection committee could select beneficiaries following the criteria and using the eligibility information card.

Figure 5: Handover




*Notes:* The poster summarizes the steps for beneficiary selection and the Bangla version of this poster was handed over to each trainee at the end of the training.

In a similar manner to the development of the training program for local representatives, the training of trainers was also designed and carried out in collaboration with representatives from the National Academy of Social Services and the Department of Social Services. The training of trainers was centered on the protocol and content for imparting the training to local government representatives, as well as familiarizing the trainers with the requisite background knowledge on the scheme and the eligibility criteria.

**Component 2: Providing data on individuals in the target group**

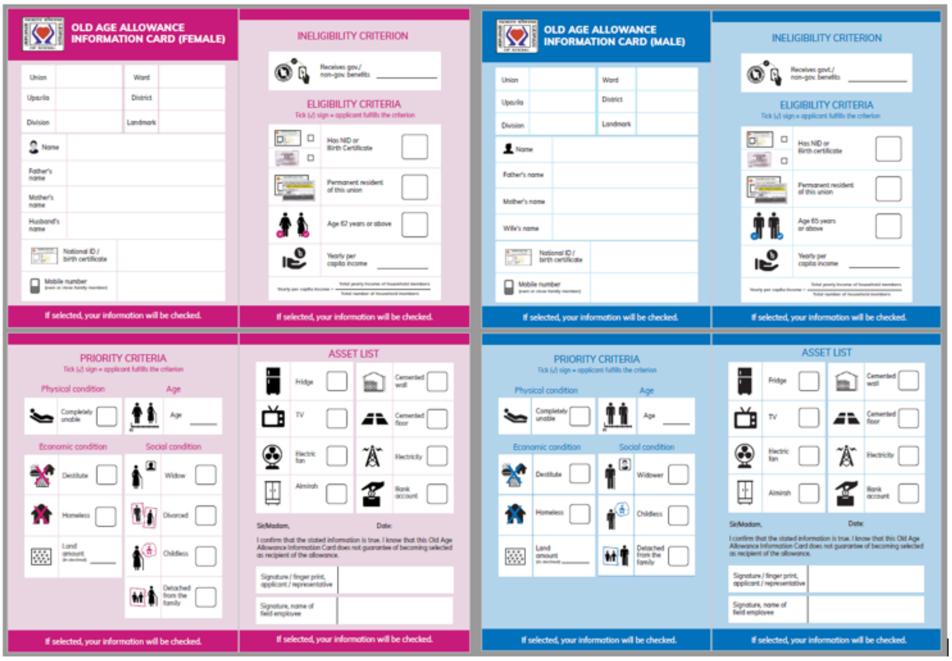
We designed the EIC as shown in Figure 6 in collaboration with the Department of Social Services, under the Ministry of Social Welfare. Following the government manual, and as mentioned above, the EIC provides all relevant information on the elderly person in an easily accessible format. This includes identifying information (page 1), receipt of other benefits, fulfillment of eligibility criteria including age, permanent residency, and income (page 2), and fulfillment of priority criteria including physical ability to work, age and economic and social living conditions (page 3). On the final page, the field officer from our intervention team enters supplementary economic data regarding the household, including information on durable assets, ownership of a bank account, and access to electricity. To facilitate comprehension for individuals with diverse educational backgrounds, pictograms are utilized for each criterion, with a tick or cross indicating compliance, except for income and land amount. Both, the field officer and the elderly person signed the EIC.[7] The field officers completed two identical data collection forms. The initial card was submitted directly to the union selection committee with the elderly individual's consent. The second card was provided to the elderly individual, who could utilize it to furnish all pertinent information to the selectors for the purpose of applying for the Old Age Allowance. Furthermore, the elderly person could utilize this card to remind the local selectors of all pertinent information, in the event that the local selectors did not give sufficient attention to the provided EICs. Once the EICs had been completed in three of the nine wards, the teams of field officers produced copies of the EICs for the project records and submitted the completed forms to the secretary of the local government.

The majority of responses to inquiries posed during the EIC process could be readily discerned and verified by local representatives, including matters pertaining to land ownership, physical capability for work, homelessness, or social living arrangements. To deter misrepresentation in general and, in particular, with respect to the few questions that cannot be readily observed (e.g., income), it was explicitly stated on the EIC that the information provided would be verified if the elderly individual was identified as an Old Age Allowance beneficiary. Given the disparate rules pertaining to age and social condition for males and females, two versions of the EIC were designed: one for female

[7] If the elderly person could not sign, the person put a thumbprint.

potential beneficiaries and one for male potential beneficiaries, differing in terms of age, social condition, and, for practical reasons, color (as shown in Figure 6).

Figure 6: Eligibility Information Card (EIC) for female and male applicants




*Notes:* After extensive piloting, the EIC was developed in collaboration with the Department of Social Services and designed by a professional graphic designer. We present the English version here, but in the field, we only used the Bangla version.

### Implementation of both components

In consideration of the intrinsic characteristics of the two intervention components, they were implemented by two distinct groups of field personnel. The initial cohort of trainers is typically comprised of individuals who have completed Social Science Master's programs, which has enabled them to articulate the eligibility criteria with clarity and to communicate effectively with the selectors. Secondly, field officers are experienced enumerators who are capable of communicating with elderly individuals in a patient and clear manner, as well as interacting with local representatives in an appropriate and effective way.[8]

The trainers worked in the unions before the field officers arrived. They typically fixed training appointments with local representatives a few days before reaching the union and carried out the training sessions. Trainers further completed preparatory arrange-

[8]In light of the necessity for rigorous security measures and the demand for frequent and extensive travel, it was deemed appropriate to appoint only male trainers and field officers.

ments for the filling of EICs. They met the subdistrict Social Service Officer, informed the local government chairperson and ward representatives of the three selected wards, selected the venue where the EICs could be filled for the elderly, and organized the public announcements with a megaphone on a vehicle two days and one day before the event. The venue had to be a public and central place easily reachable for everyone living in the ward considering only places where everyone in the target group would feel comfortable.[9]

It was fortunate that the implementation of the intervention was completed prior to the advent of the global pandemic in Bangladesh. However, the selection of beneficiaries occurred during the spring and summer of 2020, a period during which the global pandemic of coronavirus disease 2019 (Covid-19) was ongoing and subject to locally implemented anti-Covid-19 measures. In the endline survey, the overwhelming majority of selectors indicated that the advent of the pandemic did not affect the selection of beneficiaries. The percentages are statistically indistinguishable, with 94.9% of the selectors in the treatment areas and 93.5% of the selectors in the control areas reporting that the beneficiary selection was not impacted by the pandemic. This finding aligns with the reports received from the Department of Social Services.

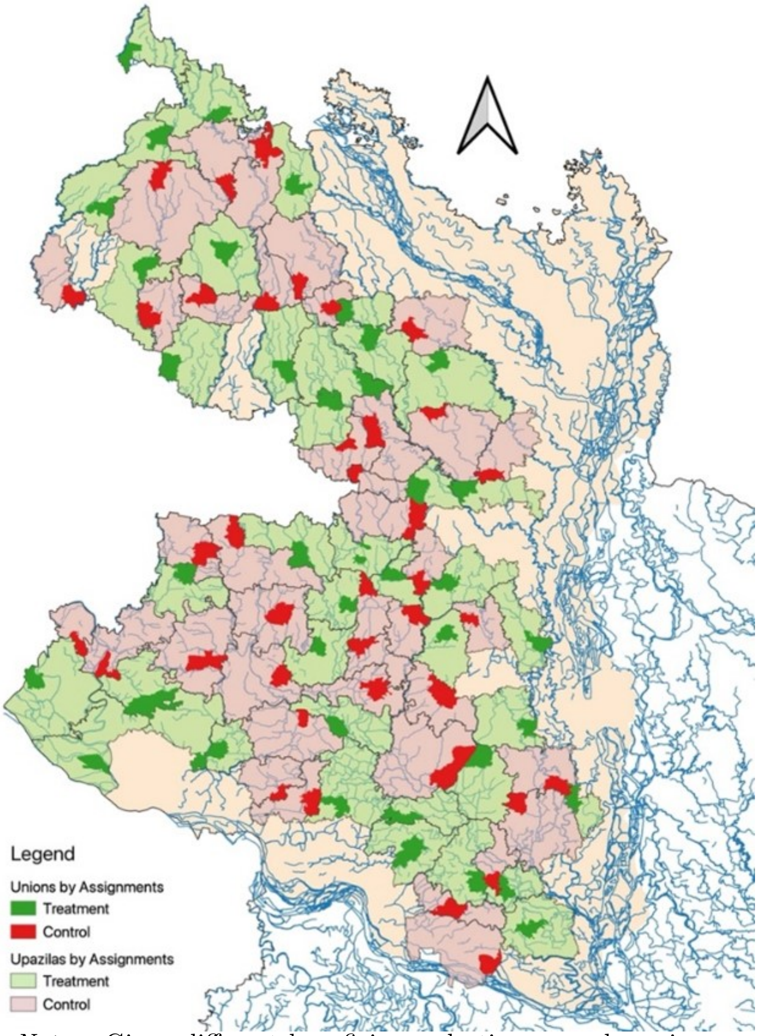
# 4 Empirical methodology and data

To investigate the causal impact of the implemented interventions, a cluster randomized controlled trial was conducted. The trial comprised a treatment group comprising two sub-groups and a control group. It was conducted from fall 2019 until spring 2021.[10] The randomized controlled trial was carried out in 80 rural unions located in 80 sub-districts as shown in the map in Figure 7. The randomization into treatment and control group was stratified by district ensuring that in each of 14 districts approximately the same number of unions was assigned either to treatment or control.

[9]For instance, private property of locally influential people or religious places such as mosques or temples could not be used.

[10]Baseline data collection in September-October 2019 and intervention in January-February 2020 were carried out before the onset of the COVID-19 pandemic in Bangladesh. The endline data collection took place during the pandemic in February-March 2021 but during a time period with very low incidence rates while following precautionary measures in terms of social distancing and mask-wearing.

Figure 7: Map of treatment and control unions in Rajshahi and Rangpur




*Notes:* Given different beneficiary selection procedures in rural and urban areas, we focused on rural sub-districts. We worked in one union per upazila as highlighted in the map. The complete and partial treatment wards are located within the dark green areas and the control group wards are located in the dark-red areas. We excluded flood-prone areas (in light orange) to ensure the feasibility of data collection.

In local government areas assigned to the treatment group, the two components of the intervention were implemented as follows: All 18 committee members responsible for selecting beneficiaries from all nine wards received the training but the data on the target group were only provided for three out of nine wards. The endline data collection was conducted in six wards, encompassing three wards where the target group data were made available to the selection committee and three wards where such data were not provided.

In order to evaluate the effect on targeting efficacy, a comparison is made between the poverty status and eligibility of beneficiaries in treatment areas with that of beneficiaries in control areas. Our experimental design permits the distinction between the impact of providing training and data (complete treatment) and the impact of providing only training (partial treatment). This is achieved by comparing new beneficiaries in control wards with those in complete treatment wards on the one hand, and with those in partial treatment wards on the other.

## 4.1 Hypotheses and outcome measures as pre-registered

We pre-registered our hypotheses and main outcome variables in a pre-analysis plan. While our primary focus is on examining the impact of training and data provision on the selection of beneficiaries, to gain insight into the underlying causal mechanisms, it is essential to first assess whether the intervention enhances the capacity of the local selection committees:[11]

**Hypothesis 1**: The intervention increases the knowledge of eligibility rules among the local representatives in the treatment group compared to the local representatives in the control group.

We examined our first hypothesis using data from the endline survey of selectors. This data counted the number of correct answers given to questions on eligibility, priority, and selection procedures. See Appendix C.1 for details. The unit of analysis for this hypothesis test is the selection committee member.

To assess the impact on targeting, we focus on the main objective of such cash transfer programs which is to reduce poverty. The Probability of Poverty Index (PPI) developed by Innovations for Poverty Action is a general poverty measure that can be used to calculate how likely it is that a household is living below a poverty line (Schreiner, 2013; Kshirsagar et al., 2017). We compare the probability of poverty of newly selected beneficiaries in treatment unions to the probability of poverty of newly selected beneficiaries in control unions.

The recently updated PPI for Bangladesh includes a series of questions pertaining to the location of residence, the size and composition of the household, the highest grade completed by any member of the household, the ownership of durable assets, the wall material, the availability of an electricity connection, and the type of toilet used. The advantage of this approach is that it relies on only 10 simple survey questions, which can be easily verified. In order to evaluate the impact of the intervention, we utilise

[11] For the purpose of presenting our results, we rearranged the order of our hypotheses compared to the pre-analysis plan.

the PPI constructed for the subset of households in rural areas that corresponds to the geographical scope of our study. Please refer to Appendix C.2 for a list of the survey questions used for the PPI, as well as a description of the index.[12]

Our main expectation is that the intervention providing practical support to local decision makers will improve the targeting of social pensions towards the elderly poor. Hence, we expect that newly selected beneficiaries in the treatment unions will be, on average, more likely to be poor than newly selected beneficiaries in control unions.

As previously stated, our design permits the differentiation between the impact of the complete treatment (training and data provision) vs. the impact of the partial treatment. Both impacts are measured in comparison to beneficiaries in control areas where no intervention took place. The difference in the impact between the complete treatment and the partial treatment (if any) will indicate the impact of providing data, an important capacity constraint which has been neglected in previous research. If providing data on the elderly in the target group is relevant for the selection of beneficiaries, the effect size for the complete treatment should be larger than the effect size for the partial treatment.

While the probability of poverty represents our primary outcome of interest, we will also examine the impact on the eligibility index. This is a weighted score that we have constructed to indicate directly whether and to what extent newly selected beneficiaries fulfill the eligibility and priority criteria as stated in the Old Age Allowance implementation manual. Further details can be found in Appendix C.3. As local representatives are trained to adhere to the selection criteria set forth in this manual, we anticipate observing an enhancement in the eligibility index.[13]

In light of the aforementioned considerations, we have formulated the following hypotheses regarding the targeting performance, with the unit of analysis being the recently selected Old Age Allowance beneficiary.

**Hypothesis 2**: The joint provision of training and data on the target group increases the mean probability of poverty of newly selected Old Age Allowance beneficiaries in the treatment wards compared to newly selected beneficiaries in the control group (complete treatment).

[12]While the probability of poverty obtained from the PPI has been our primary choice for measuring potential improvements in the targeting performance because it uses simple survey questions that are easily verifiable, it also has some disadvantages such as time- and region-dependencies. To address these limitations, we complemented our data collection with additional variables indicating poverty and eligibility, including land and asset ownership.

[13]As the government does not use an eligibility index, we had to use our own and necessarily somewhat arbitrary weights. To address this issue, we also examine the individual components of the eligibility index.

**Hypothesis 3**: The provision of training increases the mean probability of poverty of newly selected Old Age Allowance beneficiaries in the treatment wards compared to newly selected beneficiaries in the control group (partial treatment)

**Hypothesis 4** for the complete treatment and **Hypothesis 5** for the partial treatment state the same expectations with the eligibility index as outcome variable.

As stated in our pre-analysis plan, if the provision of data about the elderly in the target group is relevant, the effect size for the complete treatment should be larger than the effect size for the partial treatment.

In addition, we evaluate the potential indirect impact on the beneficiary selection of another social benefits program, namely the Widow Allowance. This serves as an illustrative example of analogous programs that could potentially benefit from the selectors' exposure to training in a more comprehensive manner. The Widow Allowance is subject to analogous regulations and procedures, and its selection of beneficiaries occurs concurrently. The composition of the Old Age Allowance selection committee largely overlaps with that of the Widow Allowance selection committee. Having acquired a systematic approach to selecting beneficiaries for the Old Age Allowance and observed a systematic data collection methodology, committee members may also be able to enhance the selection of Widow Allowance beneficiaries.

In order to ascertain the potential indirect impact on the targeting of the Widow Allowance, we propose the following hypothesis, with the unit of analysis being the newly selected beneficiary of the Widow Allowance.

**Hypothesis 6**: The intervention increases the mean probability of poverty of newly selected Widow Allowance beneficiaries in the treatment group compared to newly selected Widow Allowance beneficiaries in the control group.[14]

## 4.2 Data

### Baseline data

Our baseline data collection was conducted as a phone survey. The sample consists of all 18 selectors from all 83 local governments in our study area. Assuming that every position is filled in all committees this would amount to 1494 selectors in total. Our team

[14]As the EIC was only available to individuals of an advanced age, and not to widows of a younger age, the Widow Allowance beneficiaries were only surveyed in areas where the complete treatment had been implemented. We therefore do not distinguish here between complete and partial treatment.

of enumerators managed to interview 92% of them (N=1378). The remaining 8% were either vacant positions, not reachable, postponed the call multiple times because they were busy or stated being unwilling to participate. We dropped three unions as the UP Chairmen did not participate in the survey. These unions also had the least number of selectors participating in the survey. The final baseline sample consists of 1317 selectors out of 1440 (92%) assuming that all positions are filled.

The baseline survey, which lasted approximately 20 to 25 minutes, was designed to ascertain whether and to what extent selectors were aware of the eligibility criteria for the Old Age Allowance. Additionally, data were gathered on selectors' need for support in selecting beneficiaries and their willingness to engage in deceptive behavior for private gain. This was accomplished through the use of a dice game as a mind game, which was specifically adapted for use in the phone survey. In the dice game, the enumerator rolls a die 15 times, and the respondent silently counts how many times the number on the die reported by the enumerator matches the number in her mind, selecting a number from 1 to 6 for each die roll.[15] For each self-reported match, the respondent is remunerated with BDT 20. The aforementioned dice game was employed to obtain an individual-level measure of honesty, which was subsequently used for our analysis of heterogeneous impacts.[16]

As will be discussed in greater detail below, the impact of the intervention may depend on the willingness to apply the selection rules learned in the training and to use the data from the EIC, which might be linked to the measure of (dis)honesty. A very similar measure has been shown to predict corrupt behavior and support for rule-breaking by public sector employees in India (Hanna and Wang, 2017). It is therefore possible that this also relates to corrupt targeting practices. The baseline questionnaire additionally encompassed socio-economic variables, including education, literacy, land ownership, and income, in addition to occupational experience as a local government representative and political party affiliation. In addition to the phone survey of local representatives, we utilize subdistrict development statistics as potentially relevant covariates.

The balance checks presented in Table 2 utilize data from the baseline survey and administrative data from the subdistrict level. Our control and treatment samples are balanced in terms of the baseline data and in terms of the subdistrict level development indicators. Only reading ability is slightly higher among the representatives in the control group

[15] It should be noted that in Bangladesh, the vast majority of the population is able to count with one hand until at least 16. Prior to the commencement of the survey, the methodology was extensively piloted to ensure that respondents would not experience any difficulties in counting and would not require the use of pen and paper during the course of the survey.

[16] In Appendix D, we present the distribution of the number of reported matches.

than in the treatment group (significant at the 5% level). The null hypothesis of joint orthogonality cannot be rejected.

Table 2: Balance checks

| | (1) Control | (2) Treatment | (3) P-value of difference |
|---|---|---|---|
| ***Panel A: Baseline survey data*** | | | |
| Female | 0.25 | 0.25 | 0.957 |
| Age | 45.33 | 45.87 | 0.375 |
| Education | 9.77 | 9.60 | 0.384 |
| Can read | 0.97 | 0.95 | 0.033 |
| Can write | 0.96 | 0.94 | 0.096 |
| Land | 291.93 | 260.83 | 0.230 |
| Monthly household income (in thousands) | 42.30 | 48.10 | 0.380 |
| First time representative | 0.72 | 0.74 | 0.509 |
| Years in current position | 4.73 | 5.06 | 0.395 |
| Knowledge index OAA | 1.65 | 1.66 | 0.799 |
| Knowledge index WA | 1.10 | 1.12 | 0.556 |
| Matches dice game | 5.19 | 4.96 | 0.294 |
| Observations | 670 | 647 | 1,317 |
| ***Panel B: Upazila statistics*** | | | |
| Total population (in thousands) | 267.54 | 263.29 | 0.890 |
| Number of households (in thousands) | 267.54 | 263.29 | 0.890 |
| Rural population (%) | 85.83 | 88.18 | 0.333 |
| Poverty HCR (%) | 29.19 | 29.51 | 0.890 |
| Extreme poverty HCR (%) | 15.38 | 15.55 | 0.918 |
| Employment agriculture (%) | 69.06 | 70.22 | 0.701 |
| Employment industry (%) | 6.68 | 6.43 | 0.825 |
| Employment services (%) | 24.26 | 23.34 | 0.693 |
| Electrified (%) | 44.07 | 42.54 | 0.642 |
| Has flush toilet (%) | 24.32 | 24.78 | 0.860 |
| Literate adult population (%) | 45.79 | 44.39 | 0.297 |
| Less than primary school (%) | 54.45 | 55.91 | 0.253 |
| School attendance 6-10 years (%) | 79.91 | 79.45 | 0.513 |
| Underweight children (%) | 33.51 | 33.93 | 0.414 |
| Population 65 plus (%) | 4.73 | 4.89 | 0.209 |
| Observations | 40 | 40 | 80 |

*Notes:* In Panel A, using the baseline data, we compare the mean values of the selectors in the control group with the selectors in the treatment group. In Panel B, using subdistrict development statistics, we compare the mean values of subdistricts in the control group with subdistricts in the treatment group.

**Endline data**

The endline surveys of selectors focused on testing whether the intervention improved the selectors' knowledge of eligibility and priority criteria. We surveyed all the selectors again on their knowledge of the Old Age Allowance and Widow Allowance selection criteria. This time the team of surveyors managed to survey 1335 selectors out of 1440 (93%).

The endline surveys of new beneficiaries was designed to test whether the intervention improved the targeting of the benefits. From the newly selected beneficiaries, we collected data required to calculate the PPI and the eligibility index as well as other socio-economic variables such as education and their knowledge of the Old Age Allowance and Widow Allowance selection criteria.

We collected data from six wards in the treatment unions and three wards in the control unions. We cover six wards in the treatment unions so that our endline-data consists of data from three wards where representatives were trained and received target-group data and from another three wards where representatives were trained but did not receive target-group data. In each ward, the sampling plan was to interview 5 randomly selected beneficiaries of Old Age Allowance and 5 randomly selected beneficiaries of the Widow Allowance, both selected in the 2020 selection after the intervention phase. Since beneficiary lists had very different lengths across wards and unions, these targets could not always be fulfilled. While all sampled beneficiaries per ward were listed and surveyed in a random order, the survey teams ended up interviewing fewer beneficiaries in some wards and more beneficiaries in other wards. Overall, the endline sample includes 1810 Old Age Allowance beneficiaries (compared to 1800 observations targeted), and 1166 Widow Allowance beneficiaries (compared to 1200 targeted). The samples are split approximately equally between the complete/partial treatment groups and the control group.

## 4.3 Analysis

### 4.3.1 Main analysis

According to our pre-registered empirical analysis, we focus on measuring the impact of the intervention on the knowledge index (H1), on the probability of poverty of newly selected Old Age Allowance beneficiaries (H2 and H3), on the eligibility index (H4 and H5), and on the probability of poverty of newly selected Widow Allowance beneficiaries (H6).

First, for the effect on selectors' knowledge of the selection criteria, we estimate the following regression model to assess the intention-to-treat (ITT) effect of the intervention, as a few selectors missed the training:

$$Y_{ij} = \gamma_1 + \gamma_2 T_j + \delta X_{ij} + \epsilon_{ij} \tag{1}$$

where $Y_{ij}$ is the knowledge index of selector $i$ in union $j$, $T_j$ is the binary indicator of treatment status of union $j$, $X_i$ is a vector of baseline variables of local representative i and $e_{ij}$ is the standard error clustered at the union level. We include as covariates individual-level baseline values of local representative's age, reading ability, years of education, knowledge index of Old Age Allowance rules, and strata dummies (for each district).

As part of our manipulation test of whether the intervention components actually increased local capacity, we can explore the extent to which the provision of data about the target group through the completion of EICs and their submission to the committee was taken up at the local level. Our data allow us to (a) link endline beneficiary data to intervention EIC data to check how many beneficiaries had an EIC filled out, and (b) compare within complete treatment areas beneficiaries for whom we have a linked EIC and beneficiaries for whom we do not have a linked EIC to understand how they differ in terms of targeting criteria.

Second, for the impact on the targeting performance, we measure the probability of poverty and the eligibility index for the new beneficiaries and estimate the following regression model to assess the ITT effect of the intervention:

$$Y_{ij} = \alpha_1 + \alpha_2 T_j + \beta X_j + \epsilon_{ij} \tag{2}$$

where $Y_{ij}$ is the probability of poverty for beneficiary $i$ in union $j$, $T_j$ is a binary indicator of treatment status of union $j$, $X_j$ is a vector of baseline characteristics of union $j$ and $\epsilon_{ij}$ is the standard error clustered at the union level.

As covariates in regression model (1), we include baseline values of local representatives' average knowledge index of the Old Age Allowance rules, their average honesty score, their reading ability, strata dummies (for each district), and relevant subdistrict development statistics (namely, total population, percentage of literate population, extreme poverty headcount ratio, and population aged 65 and over). These variables are chosen because they are expected to be good predictors of the endline outcome variable. As preregistered, we present all regression results with and without the preregistered baseline covariates.

The training component was implemented as planned in all 40 unions, but three treatment unions did not allow our team members to fill out the EICs for the elderly stating different reasons such as having already started the selection process or not wanting external people to interact with the people in the target group.

### 4.3.2 Heterogeneous treatment effects

Selectors may respond differently to the intervention depending on their preferences and backgrounds. For the stratified intervention to work effectively, our theory of change rests on two key assumptions. First, selectors generally want to improve the targeting of social transfers to the elderly poor. Second, given the information and data provided with the intervention, selectors have the capacity to improve the selection of beneficiaries. As suggested by Ravallion (2020), we make these two assumptions explicit in our RCT design so that we can test these assumptions beyond the average causal impact of the stratified intervention.

As pre-registered, we test whether the impact of the intervention is stronger in areas where selectors are, on average, more honest. Our expectation is that selectors who choose to report the number of matches honestly, rather than lie for private gain, should be more open to learning from the training and more likely to use the data provided about the target group. We test this hypothesis by defining a union-level variable that indicates whether a selection committee is relatively more honest or not. The binary variable equals one if the union-level average number reported in the dice game is lower than the average number reported by the median union selection committee. We then additionally include in our regression model 2 an interaction term of the honesty variable and the treatment indicator to assess the impact of the intervention on relatively more honest unions. Similarly, we test whether the impact varies with the honesty of the chairperson and define a union-level variable indicating whether the chairperson is relatively more honest or not.

Directly related to the ability of the selection committee or chairperson to apply the learning from the training and to use the data provided with the EICs, we explore whether the treatment effects are different in areas where the selection committee or chairperson is educated. We also include an interaction term between the presence of an educated selection committee (chairperson) and the treatment indicator in our regression model 2.

For both honesty and education, we use median splits to categorize committees or chairpersons as more or less honest or educated. Accordingly, a committee or chairperson is considered “more honest” if the average number of reported matches is lower than the median and “highly educated” if the average number of years of completed education is higher than the median of committees or chairpersons.

# 5 Results

In the following, we present our findings from testing the hypotheses described above. We first examine the impact of the intervention on selectors' knowledge of the criteria, and then move on to the impact of the intervention on targeting in terms of the pre-registered probability of poverty and the eligibility index, as well as more exploratorily observable indicators such as land ownership, assets, social and physical living conditions, and gender. Where indices are used, for the knowledge, eligibility, and asset indices, we present results in standard deviations with z-scores using control group means and standard deviations.

## 5.1 Impact on knowledge and take-up of EICs

We commence by examining the extent to which the intervention enhances the selectors' comprehension of the eligibility criteria. This enhanced understanding can be regarded as a prerequisite for the intervention's efficacy, though it is not a guarantee of success. Only if the intervention effectively cultivates the selectors' grasp of the eligibility criteria can it facilitate an optimal selection of beneficiaries. Overall, about one year after the intervention, in Table 3, we find that the intervention significantly improves selectors' knowledge index by about 0.33-0.34 standard deviations ($p < 0.01$).[17]

Table 3: ITT - Selectors' knowledge of eligibility criteria

| | (1)<br>Knowledge | (2)<br>Knowledge |
|---|---|---|
| Treated | 0.332 | 0.343 |
| | (0.065) | (0.071) |
| N | 1334 | 1335 |
| | | |
| Control group mean | 0.000 | 0.000 |
| Covariates | Yes | No |

*Notes:* The dependent variable is z-standardized based on five questions about OAA eligibility rules. All specifications include district fixed effects to account for stratification. Pre-registered union-level baseline covariates are included as indicated. Standard errors are clustered at the union level and shown in parentheses.

Among the 18 selectors in a union, the nine ward members who represent each ward can be identified as having received the complete, partial, or no treatment. It is thus

[17]The results presented here stem from all the selectors who participated in the endline but we obtain the same results when we focus on all selectors who were the same in the baseline and in the endline as shown in Appendix E.

possible to undertake an exploratory examination of this subgroup to ascertain whether exposure to the complete treatment resulted in greater knowledge improvements than exposure to the partial treatment. We do so by comparing ward members in complete or partial treatment areas with ward members in control areas. While all coefficients go in the expected direction, for this subgroup of selectors, in Table 4 we only find lasting knowledge improvements when they were exposed to the complete treatment.

Table 4: ITT - Ward members' knowledge of eligibility criteria (SD)

| | (1)<br>Knowledge | (2)<br>Knowledge |
|---|---|---|
| ***Panel A: Any treatment vs. control*** | | |
| Treated | 0.165 | 0.199 |
| | (0.098) | (0.101) |
| N | 660 | 660 |
| ***Panel B: Complete treatment (T1) vs. control*** | | |
| Training and EIC | 0.233 | 0.277 |
| | (0.098) | (0.101) |
| N | 558 | 558 |
| ***Panel C: Partial treatment (T2) vs. control*** | | |
| Only training | 0.098 | 0.127 |
| | (0.139) | (0.140) |
| N | 560 | 560 |
| Control group mean | 0.000 | 0.000 |
| P-value (T1-T2) | 0.275 | 0.248 |
| Covariates | Yes | No |

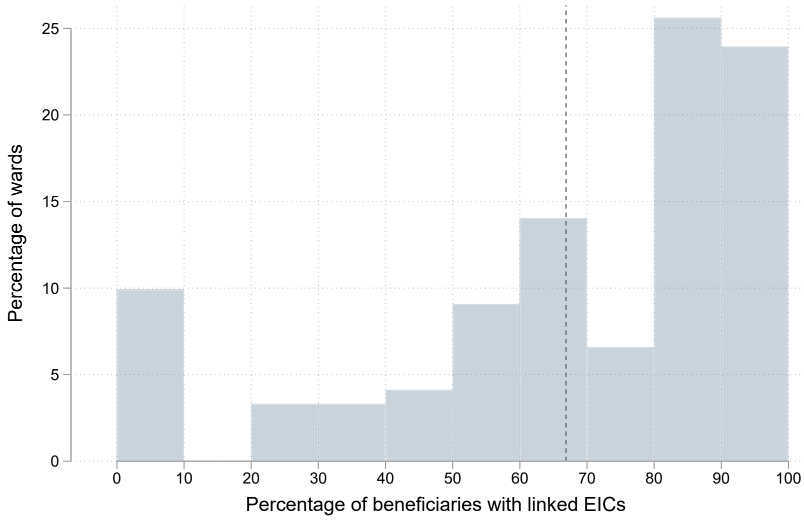
*Notes:* The dependent variable is z-standardized based on five questions about OAA eligibility rules. Panel A uses data from 458 UP Members from control wards and 202 UP Members from either complete or partial treatment wards. Panel B and Panel C use the same sample of UP Members from control wards but 100 UP Members from complete treatment wards in Panel B and 102 UP Members from partial treatment wards in Panel C. All specifications include district fixed effects to account for stratification. Pre-registered baseline covariates are included as indicated. Standard errors are clustered at the union level and shown in parentheses.

It seems that training alone did not have a long-term impact on their knowledge and that primarily the repeated exposure to the EICs enabled the members to remember more selection rules until the time of the endline survey more than one year after the intervention. However, it is crucial to note here that ward representatives have lower education levels than other committee members and therefore the repeated exposure to

EICs may have been more crucial for them than for other committee members from the same union who according to their different roles in the committee were not repeatedly exposed to the EICs.[18]

The extent to which ward members were exposed to the EICs is contingent upon two factors: (1) the extent to which the committee utilized the EICs that were submitted to the local government office, and (2) the degree to which individuals in possession of EICs engaged with local selectors during the selection process. Although our data do not permit direct observation of this phenomenon, it is important to note that 68% of beneficiaries in fully treated areas had filled EICs during the course of our intervention. This indicates a high rate of uptake among beneficiaries, as illustrated in Figure 8. At the ward level, we observe that in nearly 50% of the wards, more than 80% of the newly selected beneficiaries had an EIC filled.

Figure 8: Distribution of the percentage of beneficiaries with linked EICs at the ward level




*Notes:* This histogram indicates for the wards assigned to the complete treatment ($n = 120$) the percentage of wards having 0-10, 10-20, 20-30 etc. percent of beneficiaries with linked EICs. As such we can see that close to 50% of the wards have between 80-100% of beneficiaries with linked EICs. The mean value of linked EICs at the ward level is indicated with the red vertical line at 68%.

As part of our assessment of the impact of the intervention components on local capacity, we examine the characteristics of beneficiaries who filled an EIC compared to those who did not. In Table 5, a comparison is made between beneficiaries within complete

[18]In our sample of selectors, UP Women members have on avg. 8.2 years of education (n=236), UP Members 8.74 years (n=689), female representative of member of parliament 9.32 years (n=59), representative subdistrict chairperson 11.25 years (n=54), male representative of member of parliament 12.19 years (n=70), UP Chairman 12.51 years (n=76), Union Social Worker 13.12 years (n=75) and representative of the subdistrict Chief Executive Officer 14.42 years (n=57).

treatment areas for which a linked EIC is available and those for whom no linked EIC is available. We find that beneficiaries with a linked EIC own on average fewer assets and have a lower per capita annual income ($p < 0.05$). Further, their probability of being poor in terms of the PPI is slightly higher and their land ownership is lower but these differences are only significant at the 10% level. These results hold independently of whether or not covariates are included.[19]

Table 5: Comparison of new beneficiaries with and without linked EIC

| | (1) Prob. of poverty | (2) Asset index (SD) | (3) Land | (4) P.c. income annual |
|---|---|---|---|---|
| With EIC | 0.017 | -0.177 | -10.391 | -3273.265 |
| | (0.010) | (0.082) | (5.182) | (1390.990) |
| N | 619 | 619 | 619 | 619 |
| Mean without linked EIC | 0.196 | 0.000 | 39.888 | 21953.319 |
| Covariates | Yes | Yes | Yes | Yes |

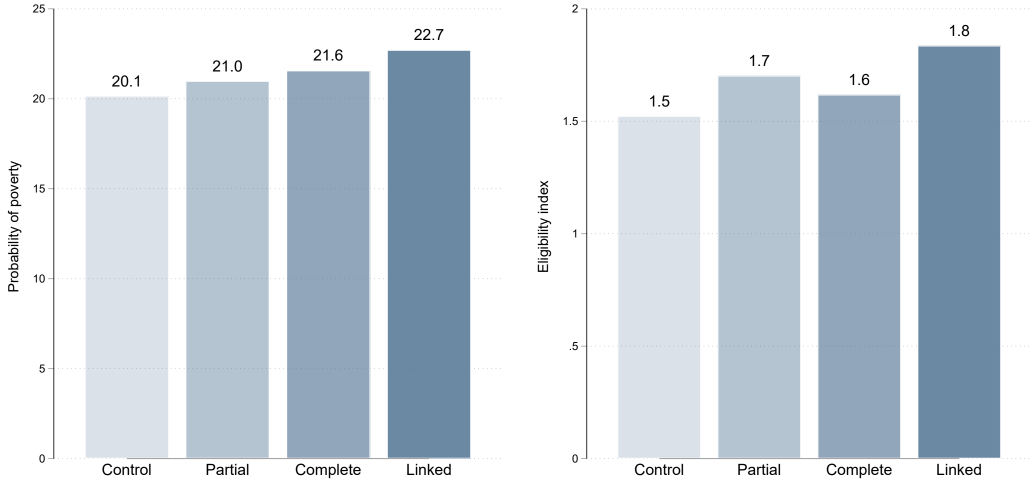
*Notes:* The asset index is z-standardized. Covariates include baseline variables aggregated at the union level, upazila statistics and district fixed effects. Standard errors are clustered at the union level and shown in parentheses.

Moreover, our data permit a descriptive comparison of the average probability of poverty and eligibility index among beneficiaries from a union, with the averages computed separately for beneficiaries in the control group, the partial treatment group, the complete treatment group, and beneficiaries with a linked EIC. As anticipated, the probability of poverty is positively correlated with the intensity of treatment exposure (as shown in Figure 9). However, this increase is relatively modest for the probability of poverty (from 20.1% to 22.7%) and for the eligibility index (from 1.5 to 1.8 out of 12).

The data demonstrate that the intervention, comprising two components, enhanced capacity in two meaningful ways. It augmented the selectors' understanding of the selection criteria and furnished data on individuals within the target demographic at the local level through the distribution of eligibility information cards. The following section will examine whether an increase in capacity through the complete and partial intervention resulted in improved targeting.

[19] Asset ownership reported on the EICs is very close to what the beneficiaries report in the endline data collection when the surveyors visits their house. The average difference between the asset index computed with the EIC data and that computed with the endline data (using the exact same questions) is 0.04 assets and statistically insignificant. This suggests that systematic misreporting of one's own wealth is not a problem in the target-group data provision component of the intervention.

Figure 9: Average probability of poverty and eligibility index by treatment dosage




## 5.2 Impact on targeting performance

We examine the impact on the targeting performance according to our hypotheses. Did the selectors in treatment areas select poorer or more eligible individuals than in treatment areas? We present our results for the PPI and for the eligibility index in Table 6. For both outcome variables, we show first the impact of any treatment, then the impact of the complete treatment and finally the impact of the partial treatment from separate regressions.

Our main results show positive but statistically insignificant coefficients indicating that we cannot reject the null-hypothesis. In terms of their probability of poverty and their eligibility index, beneficiaries in treatment and control areas are statistically inistinguishable ($p > 0.2$).

A potential concern is that even though the intervention improved the local capacity of selectors, selecting poor elderly and eligible elderly remains a major challenge and difficult task. Even though selectors know more about the selection criteria, as shown in Table 3, they do not seem to have complete knowledge of selection criteria and sorting or categorizing applicants could be still difficult especially when it requires taking into account many criteria as requested by the national government.

Upon examination of the raw data pertaining to the probability of poverty and eligibility index (Figure 10), it becomes evident that selection committees in treatment areas do not generally select poorer individuals. However, it is observed that the selectors tend to select a few more of the elderly who are very likely to be poor with a probability of poverty index of more than 0.5 (in our sample only 5% of the beneficiaries in treatment and control areas) which is also confirmed in corresponding regressions in Table 7. The complete treatment increases the probability of selecting those who are likely to be poor by approx. 3 percentage points ($p < 0.01$) and similarly the partial treatment increases the probability of selecting those who are likely to be poor by approx. 2 percentage points ($p < 0.05$ when including covariates and $p < 0.06$ when only including district fixed effects). For the eligibility index, we do not see a similar result as presented in Appendix G.[20]

Table 6: ITT - Probability of poverty and eligibility index (SD)

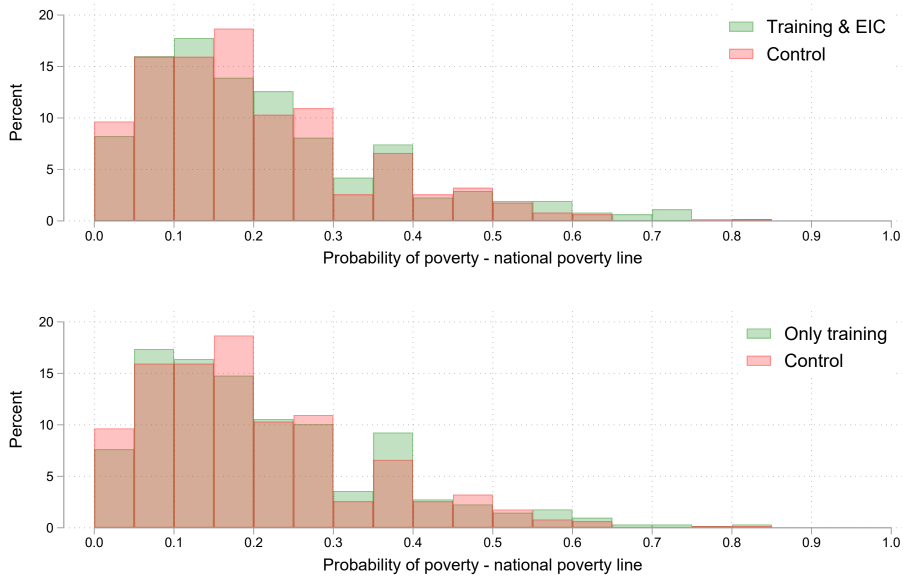
| | (1) Prob. of poverty | (2) Prob. of poverty | (3) Eligibility index | (4) Eligibility index |
|---|---|---|---|---|
| ***Panel A: Any treatment vs. control*** | | | | |
| Treated | 0.008 | 0.008 | 0.038 | 0.042 |
| | (0.007) | (0.007) | (0.070) | (0.063) |
| N | 1856 | 1856 | 1856 | 1856 |
| ***Panel B: Complete treatment (T1) vs. control*** | | | | |
| Training and EIC | 0.008 | 0.010 | 0.023 | 0.023 |
| | (0.008) | (0.008) | (0.065) | (0.062) |
| N | 1240 | 1240 | 1240 | 1240 |
| ***Panel C: Partial treatment (T2) vs. control*** | | | | |
| Only training | 0.005 | 0.005 | 0.051 | 0.067 |
| | (0.007) | (0.008) | (0.084) | (0.076) |
| N | 1237 | 1237 | 1237 | 1237 |
| Control group mean | 0.200 | 0.200 | 0.000 | 0.000 |
| P-value (T1-T2) | 0.675 | 0.507 | 0.658 | 0.428 |
| Covariates | Yes | No | Yes | No |

*Notes:* The eligibility index is z-standardized. All specifications include district fixed effects to account for stratification. Pre-registered baseline covariates are included as indicated. Standard errors are clustered at the union level and shown in parentheses.

These findings suggest that selectors may prioritize indicators of vulnerability that are more readily observable, which could be associated with elevated probabilities of living below the poverty line, as indicated by PPI data. Going beyond pre-registered outcomes in Table 9, we find suggestive evidence that selectors in treatment areas are more likely to select beneficiaries who own less land, who live alone and who cannot walk. However,

[20] As we constructed the eligibility index closely following the selection guidelines issued by the government, it is worth noting that about 80% of the beneficiaries do not fulfill the eligibility condition of low income which has been set extremely low by the government.

Figure 10: Probability of poverty by treatment group




while the increased likelihood of selecting individuals who own less land and live alone is driven by the complete treatment ($p < 0.1$), the impact on selecting those who cannot walk is driven by the partial treatment ($p < 0.05$). Our results are robust to several alternative choices (see Appendix H).

Ultimately, our data enable us to investigate, in an exploratory manner, whether there was a change in the gender composition of beneficiaries as a result of the intervention. This is motivated by the fact that in most committees, only three out of 18 members are female (given the reserved slots for female representatives in the local government). Therefore, the selection of beneficiaries is predominantly a male-dominated task. In accordance with local gender roles and gender norms, male selectors typically have personal knowledge of men in their area and are aware of women only through other men. While the informal selection of beneficiaries through interactions between citizens and selectors is more likely to be relevant for men, the training and data provision may encourage selectors to select more women as beneficiaries. The training encourages selectors to consider various types of vulnerability. In this regard, EICs may serve as a valuable tool for individuals in the target group, facilitating the sharing of information on their eligibility, which is particularly beneficial for women.

To investigate this issue, we can utilize both administrative data, including beneficiary lists that were employed for sampling purposes, and our survey data. The former is more advantageous than the latter, as it encompasses data on all beneficiaries within the designated study areas and can be integrated with the same covariates at the local

Table 7: ITT - Probability of poverty according to PPI

| | (1) Prob. > 0.3 | (2) Prob. > 0.3 | (3) Prob. > 0.4 | (4) Prob. >0.4 | (5) Prob. > 0.5 | (6) Prob. > 0.5 |
|---|---|---|---|---|---|---|
| ***Panel A: Any treatment vs. control*** | | | | | | |
| Treated | 0.025 | 0.034 | 0.014 | 0.013 | 0.025 | 0.024 |
| | (0.017) | (0.019) | (0.013) | (0.015) | (0.008) | (0.008) |
| N | 1856 | 1856 | 1856 | 1856 | 1856 | 1856 |
| ***Panel B: Complete treatment (T1) vs. control*** | | | | | | |
| Training and EIC | 0.022 | 0.032 | 0.017 | 0.019 | 0.028 | 0.030 |
| | (0.023) | (0.025) | (0.017) | (0.018) | (0.010) | (0.011) |
| N | 1240 | 1240 | 1240 | 1240 | 1240 | 1240 |
| ***Panel C: Partial treatment (T2) vs. control*** | | | | | | |
| Only training | 0.024 | 0.031 | 0.009 | 0.006 | 0.021 | 0.019 |
| | (0.018) | (0.020) | (0.014) | (0.016) | (0.010) | (0.010) |
| N | 1237 | 1237 | 1237 | 1237 | 1237 | 1237 |
| Control group mean | 0.185 | 0.185 | 0.093 | 0.093 | 0.035 | 0.035 |
| P-value (T1-T2) | 0.941 | 0.969 | 0.599 | 0.401 | 0.579 | 0.396 |
| Covariates | Yes | No | Yes | No | Yes | No |

*Notes:* The dependent variable is equal to 1 if the beneficiary's probability to be poor is greater than 0.3, greater than 0.4 or greater than 0.5, and 0 otherwise. All specifications include district fixed effects to account for stratification. Pre-registered baseline covariates are included as indicated. Standard errors are clustered at the union level and shown in parentheses.

government level. The results obtained using the administrative data are presented in Table 8, and the same results are obtained when the survey data on gender are used. A regression of female on the pooled, complete, and partial treatment indicators reveals that selectors in complete treatment areas are 5-8 percentage points more likely to select female beneficiaries than selectors in control areas ($p < 0.01$ when including covariates and $p < 0.05$ when only including district fixed effects). Given a control group mean of 0.45, the effect size is approximately 10–17.5% of the mean value, which is a notable magnitude. The estimates for the partial treatment are positive but statistically insignificant, indicating that the training alone did not result in an increase in the representation of women among the beneficiaries ($p > 0.1$). It can therefore be concluded that the provision of EICs encouraged selectors to consider a greater number of females during the beneficiary selection process, providing selectors with more information about women seeking the social pension receipt.

As a logical consequence of the moderate impacts on targeting of the Old Age Allowance, we do not find any significant effects for the targeting of the widow allowance as shown in Appendix I. Therefore, the hypothesis that providing training and data to selectors of the Old Age Allowance could have positive spillover effects on the targeting of the Widow Allowance is not supported by the evidence.

Table 8: ITT - Selection of women as beneficiaries - administrative beneficiary lists

| | (1) Female | (2) Female |
|---|---|---|
| ***Panel A: Any treatment vs. control*** | | |
| Treated | 0.060 | 0.031 |
| | (0.020) | (0.021) |
| N | 3051 | 3051 |
| ***Panel B: Complete treatment (T1) vs. control*** | | |
| Training and EIC | 0.079 | 0.050 |
| | (0.024) | (0.025) |
| N | 2037 | 2037 |
| ***Panel C: Partial treatment (T2) vs. control*** | | |
| Only training | 0.033 | 0.012 |
| | (0.025) | (0.024) |
| N | 1964 | 1964 |
| Control group mean | 0.454 | 0.454 |
| P-value (T1-T2) | 0.102 | 0.150 |
| Covariates | Yes | No |

*Notes:* The dependent variable is equal to 1 if the beneficiary is female. All specifications include district fixed effects to account for stratification. Pre-registered baseline covariates are included as indicated. Standard errors are clustered at the union level and shown in parentheses.

Table 9: ITT - Other easily observable indicators of vulnerability

| | (1) Land | (2) Land | (3) Assets | (4) Assets | (5) Lives alone | (6) Lives alone | (7) Cannot walk | (8) Cannot walk |
|---|---|---|---|---|---|---|---|---|
| ***Panel A: Any treatment vs. control*** | | | | | | | | |
| Treated | -9.355 | -8.840 | -0.068 | -0.067 | 0.020 | 0.019 | 0.034 | 0.026 |
| | (6.869) | (6.509) | (0.064) | (0.066) | (0.017) | (0.015) | (0.016) | (0.015) |
| N | 1856 | 1856 | 1856 | 1856 | 1856 | 1856 | 1856 | 1856 |
| ***Panel B: Complete treatment (T1) vs. control*** | | | | | | | | |
| Training and EIC | -14.993 | -12.676 | -0.093 | -0.089 | 0.041 | 0.035 | 0.021 | 0.016 |
| | (7.113) | (6.613) | (0.075) | (0.074) | (0.022) | (0.019) | (0.017) | (0.016) |
| N | 1240 | 1240 | 1240 | 1240 | 1240 | 1240 | 1240 | 1240 |
| ***Panel C: Partial treatment (T2) vs. control*** | | | | | | | | |
| Only training | -7.668 | -5.892 | -0.027 | -0.034 | 0.002 | 0.003 | 0.046 | 0.037 |
| | (7.696) | (6.955) | (0.063) | (0.068) | (0.017) | (0.017) | (0.019) | (0.018) |
| N | 1237 | 1237 | 1237 | 1237 | 1237 | 1237 | 1237 | 1237 |
| Control group mean | 46.527 | 46.527 | 0.000 | 0.000 | 0.092 | 0.092 | 0.066 | 0.066 |
| P-value (T1-T2) | 0.081 | 0.076 | 0.242 | 0.339 | 0.051 | 0.103 | 0.135 | 0.180 |
| Covariates | Yes | No | Yes | No | Yes | No | Yes | No |

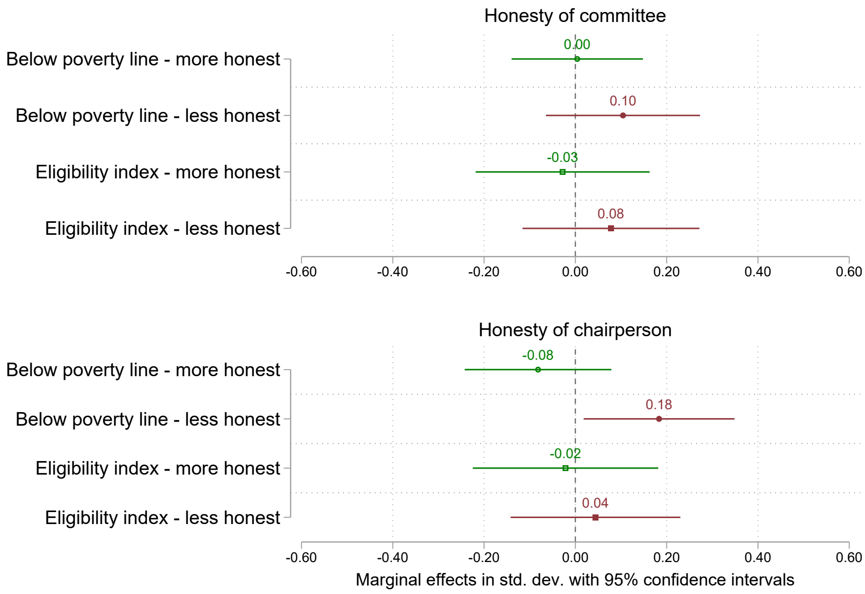
*Notes:* The asset index is z-standardized. All specifications include district fixed effects to account for stratification. Pre-registered baseline covariates are included as indicated. Standard errors are clustered at the union level and shown in parentheses.

## 5.3 Heterogeneity

One potential source of heterogeneity was anticipated, namely, selectors' willingness to enhance the selection of beneficiaries by targeting poorer and more eligible individuals. It was hypothesized that this willingness to enhance and to select a greater number of individuals according to the established guidelines would be inversely correlated with selectors' inclination to circumvent the established rules for the sake of personal gain. Consequently, we inquired about selectors' inclination to engage in deceptive practices for personal benefit in our initial survey of selectors. As preregistered, we anticipated observing larger effects in areas where committees are relatively more honest or, in our interpretation, more willing to learn from the training and to utilize those learnings in conjunction with the data from the EIC. Figure 11, however, demonstrates that the incremental effects of the comprehensive intervention are not statistically significant for committees or chairpersons with higher levels of honesty.

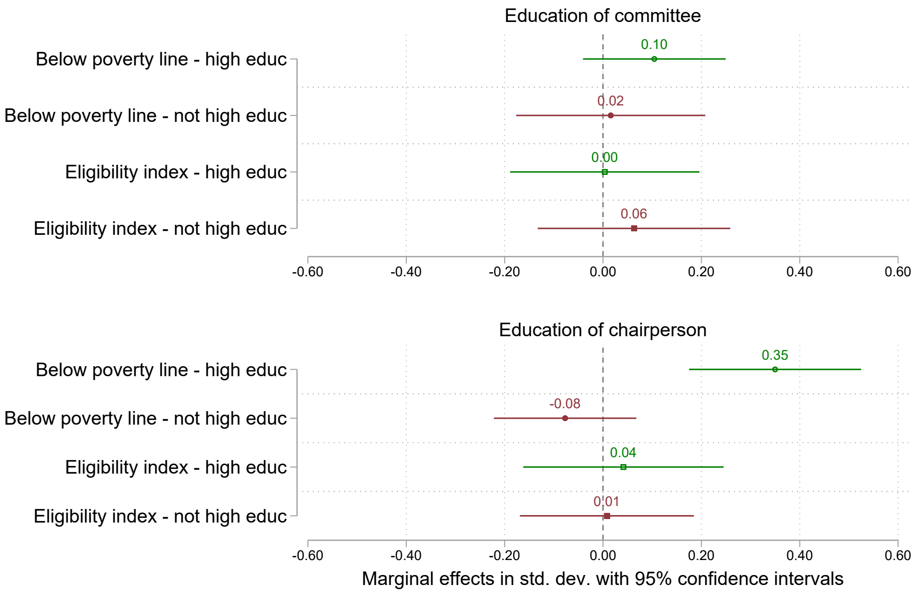
As previously stated, another plausible but not pre-registered source of heterogeneity, is the education of the selectors in charge as it may be immediately linked to the ability to use the learning from the training and the EICs for the selection. We test therefore whether the impact of the intervention varies with the committee's education or with the chairperson's education. We do not find evidence for heterogeneity in treatment effects depending on the committee's education but we do find the treatment effects vary with the chairperson's education (Figure 12). In complete treatment areas, committees with a highly educated chairperson select individuals who are 0.29 standard deviations more likely to be poor compared to the control group. In contrast, committees in complete treatment areas without a highly educated chairperson select individuals who are as likely to be poor as the beneficiaries selected by committees in control areas. The treatment effects for committees with highly educated chairpersons vs. the treatment effects for committees without highly educated chairperson are statistically different from each other as visualized by the non-overlapping confidence intervals ($p < 0.01$). The corresponding regression tables are shown in Appendix J.

Figure 11: Honesty of selectors and marginal effects of the complete treatment




*Notes:* We compute the marginal effect of the treatment separately for each subgroup from the regression coefficients in Appendix J. We use the number of reported matches in the dice mind game as a measure of dishonesty. We refer to a more honest committee if the average number of reported matches of all committee members is lower than the median of the average number of matches reported by all committees. We refer to a more honest chairperson if the chairperson reports fewer matches than the median number of matches reported by all chairpersons.

Figure 12: Education of selectors and marginal effects of the complete treatment




*Notes:* We compute the marginal effect of the treatment separately for each subgroup from the regression coefficients in Appendix J. We use the number of years of completed education to categorize committees and chairpersons as "highly educated" and "not highly educated". A committee is highly educated if the committee has an average education greater than the median of the average education of the committees. A chairperson is highly educated if a chairperson has an education level grater than the median of the education of the chairpersons.

# 6 Conclusion

The objective of this study was to investigate whether augmenting local capacity can improve the beneficiary selection process for a social transfer. Our work can be framed within a typical principal-agent framework, where the national government (principal) designs the program, and the local government (agent) selects beneficiaries. We focus on the capacity constraints faced by local-level agents, which we have identified and documented in our study.

Our results show that the intervention, which included training and information provision, successfully improved selectors' comprehension of eligibility and priority criteria. While this did not lead to overall improvements in targeting based on our pre-registered poverty and eligibility indices, we observed enhancements in specific, easily observable dimensions. Notably, the intervention improved selection of individuals who were highly likely to be poor, owned less land, lived alone, and had lower physical mobility. The improvements in poverty likelihood and land ownership appear to stem from both the comprehensive training and the provision of Eligibility Information Card (EIC) data, whereas the better targeting of individuals living alone and those with reduced mobility seems to be a result of the training alone.

Despite our intervention, significant complexities and capacity constraints persist, leading selectors to prioritize a few easily observable indicators of vulnerability. Notably, the full treatment package, which includes both training and EICs, improved the selection of women as beneficiaries. This is particularly significant as women were previously disadvantaged in the selection process. The EICs provide women with a tangible tool to communicate their eligibility information, thus addressing a pre-existing bias. It is important to highlight that this improvement in gender equity was only observed with the complete intervention, not with partial implementation.

Our findings indicate that committees led by more educated chairpersons achieved better poverty-based targeting in response to our intervention. This result underscores the importance of addressing broader structural capacity constraints within local governments, beyond just providing training and information. It suggests that the effectiveness of interventions may be influenced by the existing human capital and leadership qualities at the local level, pointing to the need for a more comprehensive approach to enhancing local government capacity for improved beneficiary selection. Enhancing the general education level of the existing LG leaders or selecting more educated candidates can potentially also improve social safety net targeting.

Future research should explore multiple dimensions of capacity constraints while also examining the intrinsic motivation of decision-makers. Such comprehensive studies could significantly improve the effectiveness of social transfer programs like Bangladesh's Old Age Allowance in combating extreme poverty. By addressing both structural limitations and individual motivations, we can develop more targeted interventions that enhance the impact of these crucial poverty alleviation efforts.

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# Appendix for online publication

## A Timeline

<table>
<tr><td>2018-2019</td><td colspan="2">Pilot and RCT design jointly with<br>Dept. of Social Services</td></tr>
<tr><td>Summer 2019</td><td colspan="2">Pre-registration at AEA and pre-analysis plan</td></tr>
<tr><td>Fall 2019</td><td colspan="2">Baseline survey of selectors from 80 rural local<br>governments in 80 subdistricts (n=1261)</td></tr>
<tr><td>Jan' -<br>Feb' 2020</td><td>40 local governments:<br>Training for all selectors<br>EIC in 3 out of 9 wards</td><td>40 local governments:<br>status quo</td></tr>
<tr><td>April -<br>July 2020</td><td colspan="2">Nation-wide selection of new beneficiaries</td></tr>
<tr><td>Feb' -<br>Mar' 2021</td><td colspan="2">Endline surveys:<br>Selectors (n=1335) - knowledge<br>New Old Age Allowance beneficiaries (n=1856) - eligibility and poverty<br>New Widow Allowance beneficiaries (n=1202) - eligibility and poverty</td></tr>
</table>

## B Summary statistics

Below we present the summary statistics from the selectors surveyed in the baseline and from the Old Age Allowance beneficiaries and Widow Allowance beneficiaries surveyed in the endline.

Table B1: Summary statistics Old Age Allowance beneficiaries (endline)

| | . | | | | |
|---|---|---|---|---|---|
| | mean | p50 | sd | min | max |
| Prob. poor national poverty line | 0.21 | 0.17 | 0.15 | 0.01 | 0.84 |
| Eligibility index | 1.61 | 0.00 | 3.28 | 0.00 | 12.00 |
| Ind. monthly income | 1771.69 | 625.00 | 2190.00 | 0.00 | 22542.00 |
| Total land | 40.87 | 12.00 | 95.19 | 0.00 | 3034.50 |
| Asset count | 3.22 | 3.00 | 1.67 | 0.00 | 8.00 |
| Asset count quintile | 2.80 | 3.00 | 1.58 | 1.00 | 5.00 |
| Asset quintile PCA | 2.85 | 3.00 | 1.60 | 1.00 | 5.00 |
| Knowledge index | 0.76 | 1.00 | 0.85 | 0.00 | 3.00 |
| Female | 0.45 | 0.00 | 0.50 | 0.00 | 1.00 |
| Age | 71.57 | 70.00 | 6.97 | 53.00 | 108.00 |
| Rajshahi | 0.39 | 0.00 | 0.49 | 0.00 | 1.00 |
| Rangpur | 0.61 | 1.00 | 0.49 | 0.00 | 1.00 |
| Observations | 1856 | | | | |

Table B2: Summary statistics Widow Allowance beneficiaries (endline)

| | . mean | p50 | sd | min | max |
|---|---|---|---|---|---|
| Prob. poor national poverty line | 0.21 | 0.16 | 0.15 | 0.01 | 0.81 |
| Ind. monthly income | 1370.33 | 875.00 | 1459.56 | 0.00 | 21458.00 |
| Total land | 18.34 | 4.50 | 41.44 | 0.00 | 706.00 |
| Asset count | 2.96 | 3.00 | 1.61 | 0.00 | 8.00 |
| Asset count quintile | 2.58 | 3.00 | 1.56 | 1.00 | 5.00 |
| Asset quintile PCA | 2.62 | 3.00 | 1.59 | 1.00 | 5.00 |
| Female | 0.99 | 1.00 | 0.08 | 0.00 | 1.00 |
| Age | 52.96 | 53.00 | 8.93 | 21.00 | 83.00 |
| Rajshahi | 0.40 | 0.00 | 0.49 | 0.00 | 1.00 |
| Rangpur | 0.60 | 1.00 | 0.49 | 0.00 | 1.00 |
| Observations | 1202 | | | | |

# C Description of indices

## C.1 Knowledge index - Selection committee members

During endline-data collection, the selection committee members were asked questions on the eligibility and priority criteria for the Old Age Allowance. Based on correct/incorrect responses, we count the number of correct responses indicating the local representative's knowledge of eligibility and priority criteria. The following questions were used for the calculation of the knowledge index corresponding to a count of correctly stating the eligibility/priority rules:

1. Female age cutoff
2. Male age cutoff
3. Landless cutoff
4. Income cutoff
5. Eligible if receiving government pension?

## C.2 Probability of poverty index

As described in the main text, the PPI developed by Innovations for Poverty Action weighs responses to a small set of survey questions to compute a PPI score, which then indicates the likelihood of a household living in poverty. A lower score indicates a higher likelihood of living in poverty. Different poverty lines can be applied including absolute and relative poverty lines as well as national and international poverty lines. "This PPI is based on data from Bangladesh's 2016 Household Income and Expenditure Survey (HIES) 2016 produced by Bangladesh Bureau of Statistics and was released in July 2020. The PPI includes the following questions:

1. In which division does the household live?
2. How many household members are there in the household?
3. How many household members are between 0-9 years of age?

4. What was the highest grade completed by anyone in the household?
5. Does your household own a refrigerator?
6. Does your household own a fan?
7. What is the construction material of the walls of the main room?
8. Does the household have an electricity connection?
9. What kind of toilet facility do members of your household usually use?

## C.3 Eligibility index

According to the implementation manual 2013, there are ineligibility, eligibility and priority criteria to select beneficiaries for the Old Age Allowance. A person is ineligible for Old Age Allowance if she receives any other government or non-government benefit regularly such as other social safety nets, government pension or formal sector pension. To be eligible for Old Age Allowance, an individual needs to fulfill all four eligibility criteria:

1. Has to be a permanent resident.
2. Has to have National Identity Card or birth certificate
3. Has to be 62 years of age or more for females and 65 years or more for males.
4. Annual per capita income (i.e. annual household income divided by the number of household members) has to be less than BDT 10,000.

The eligibility index is 0 if the person either fulfills the ineligibility criterion or does not meet one of the required eligibility conditions. To select only few among the eligible elderly for Old Age Allowance, the government prescribes the use of priority criteria. However, these criteria are hard to implement on the ground as government guidelines tend to lack clear instructions. Such as according to the economic condition, priority should be given in the order of destitute, homeless and landless, but there is no clear instruction on how to measure destitution. To simplify these different conditions for our analysis, four conditions are prioritized to create the eligibility index. These are age, ownership of land, living with adult child or alone, and physical ability to work.

**Age:** An elderly receives either 1, 2 or 3 based on the number of years an elderly is older than the cutoff. Below, we show the scoring method:

| **For male elderly** | |
|---|---|
| **Rule** | **Score** |
| $65 \leq \text{age} \leq 69$ | 1 |
| $70 \leq \text{age} \leq 75$ | 2 |
| $\text{age} \geq 76$ | 3 |

| **For female elderly** | |
|---|---|
| **Rule** | **Score** |
| $62 \leq \text{age} \leq 66$ | 1 |
| $67 \leq \text{age} \leq 72$ | 2 |
| $\text{age} \geq 73$ | 3 |

**Land ownership:** Elderly receive 1, 2 or 3 depending on how much agricultural land their household owns. Below, we show the rules for the scores.

| Rule | Score |
|---|---|
| Land ownership > 100 decimals | 1 |
| 50 decimals ≤ land ownership ≤ 100 decimals | 2 |
| Land ownership < 50 decimals | 3 |

According to the manual, if an elderly lives in a household that owns less than 50 decimals of land excluding the dwelling house, the elderly will be considered as landless.

**Social condition:** Depending on whom the elderly are living with, they receive a score ranging from 1 to 3 for the social condition:

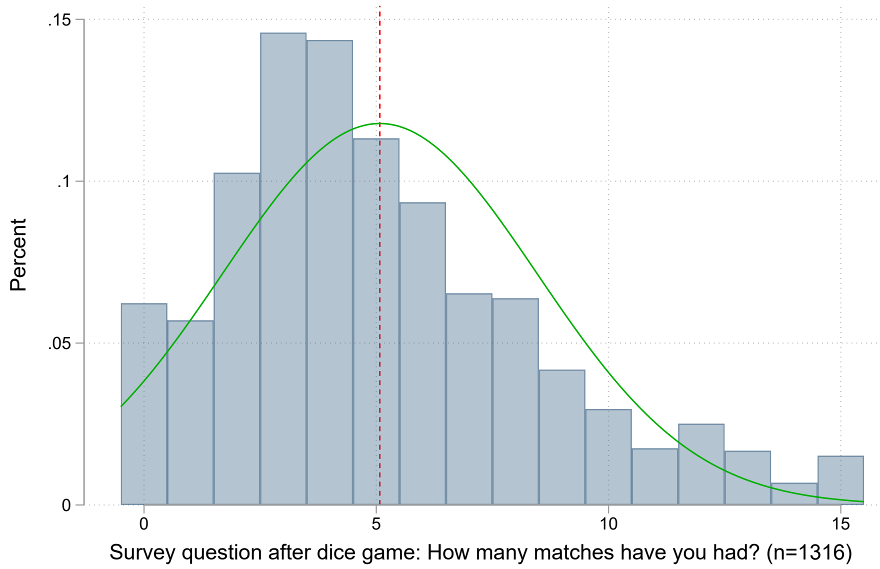
| Rule | Score |
|---|---|
| Lives with adult son/daughter | 1 |
| Lives with other adult family member except son/daughter | 2 |
| Lives alone | 3 |

**Physical condition:** We use the ability to walk as a proxy for ability to work following the scoring rules below.

| Rule | Score |
|---|---|
| Able to walk without difficulty | 1 |
| Able to walk with some difficulty | 2 |
| Able to walk with severe difficulty or unable to walk | 3 |

# D Dishonesty measure

Figure D1: Number of matches reported in dice game - baseline



# E Impact on knowledge of selectors - matched

Table E1: ITT - Knowlege of selectors - matched in baseline and endline

| | (1) Know index | (2) Know index | (3) Know index |
|---|---|---|---|
| Treated | 0.289 | 0.344 | 0.306 |
| | (0.069) | (0.071) | (0.073) |
| N | 1192 | 1334 | 1192 |
| | | | |
| Control group mean | 0.017 | 0.017 | 0.017 |
| Covariates | Yes | No | Lasso |

*Notes:* The dependent variable is z-standardized based on five questions about OAA eligibility rules. All specifications include district fixed effects to account for stratification. Pre-registered union-level baseline covariates are included as indicated. Standard errors are clustered at the union level and shown in parentheses.

# F How do beneficiaries with and without EIC differ?

Table F1: Comparison of new beneficiaries with and without EIC

| | (1) Prob. of poverty | (2) Asset index (SD) | (3) Land | (4) P.c. income annual |
|---|---|---|---|---|
| With EIC | 0.018 | -0.164 | -8.377 | -3217.113 |
| | (0.009) | (0.084) | (5.242) | (1453.231) |
| N | 619 | 619 | 619 | 619 |
| Covariates | No | No | No | No |
| Mean without linked EIC | 0.194 | 0.000 | 39.888 | 21953.319 |

*Notes:* The asset index is z-standardized. Specificiation without covariates. Standard errors are clustered at the union level and shown in parentheses.

# G Impact on targeting - eligibility index

Figure G1: Eligibility index by treatment group

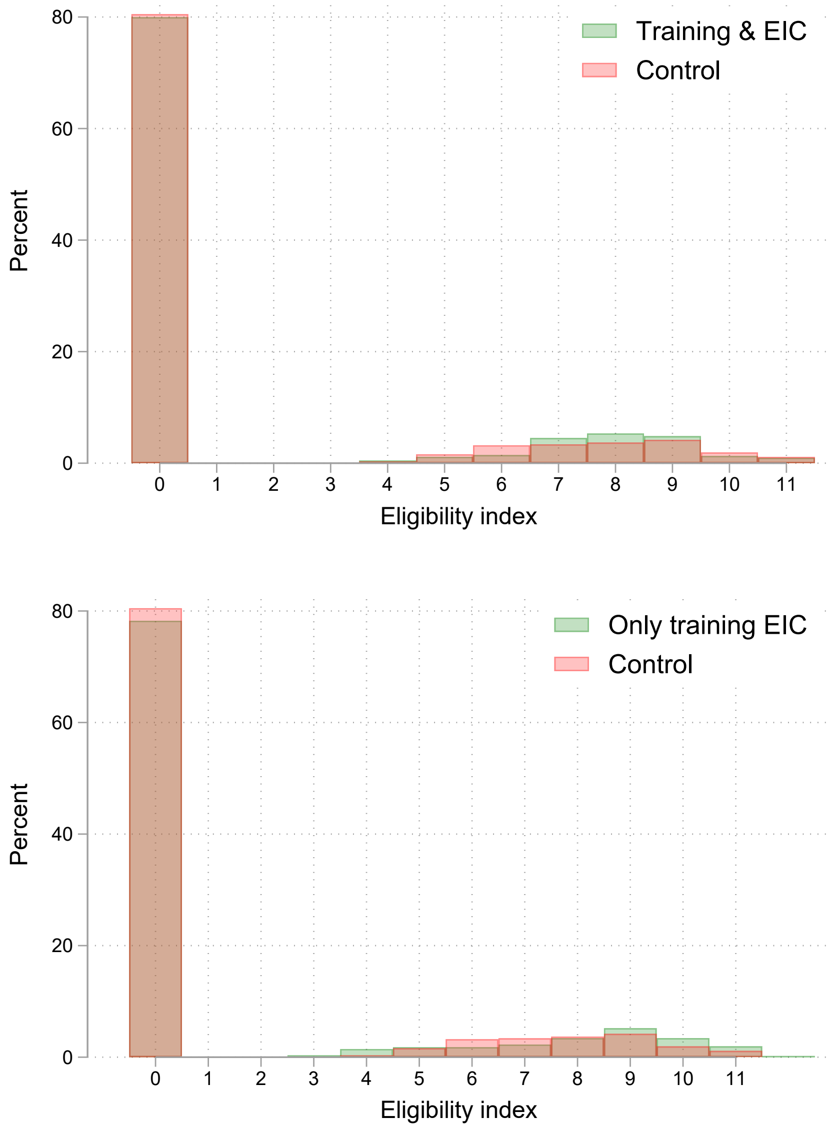


Table G1: ITT - Likely eligible - high eligibility index (EI) with income

| | (1)<br>EI: 90pct | (2)<br>EI: 90pct | (3)<br>EI: 95pct | (4)<br>EI: 95pct | (5)<br>EI: 99pct | (6)<br>EI: 99pct |
|---|---|---|---|---|---|---|
| ***Panel A: Any treatment vs. control*** | | | | | | |
| Treated | 0.022 | 0.023 | 0.014 | 0.016 | 0.005 | 0.003 |
| | (0.021) | (0.018) | (0.016) | (0.014) | (0.005) | (0.005) |
| N | 1856 | 1856 | 1856 | 1856 | 1856 | 1856 |
| ***Panel B: Complete treatment vs. control*** | | | | | | |
| Training and EIC | 0.017 | 0.015 | -0.000 | -0.001 | -0.001 | -0.002 |
| | (0.018) | (0.017) | (0.015) | (0.014) | (0.006) | (0.005) |
| N | 1240 | 1240 | 1240 | 1240 | 1240 | 1240 |
| ***Panel C: Partial treatment vs. control*** | | | | | | |
| Only training | 0.026 | 0.032 | 0.026 | 0.034 | 0.008 | 0.008 |
| | (0.025) | (0.022) | (0.018) | (0.017) | (0.007) | (0.007) |
| N | 1237 | 1237 | 1237 | 1237 | 1237 | 1237 |
| Control group mean | 0.110 | 0.110 | 0.072 | 0.072 | 0.011 | 0.011 |
| P-value (T1-T2) | 0.618 | 0.302 | 0.059 | 0.007 | 0.180 | 0.114 |
| Covariates | Yes | No | Yes | No | Yes | No |

*Notes:* The dependent variable is equal to 1 if the beneficiary's eligibility index is as high as the 90th, 95th and 99th percentile and 0 otherwise. All specifications include district fixed effects to account for stratification. Pre-registered baseline covariates are included as indicated. Standard errors are clustered at the union level and shown in parentheses.

# H Robustness checks

Our main results show that increases in selectors' knowledge did not seem to translate into improved poverty-based targeting when we focus on our pre-registered outcomes probability of poverty and the eligibility index except for the subgroup of committees with highly educated chairpersons who improve poverty-based targeting of social pensions. We further exploratorily document that the complete intervention increases the selection of individuals who are very likely to be poor according to a high PPI, who own less land and, suggestively, who live alone. Finally, we document that the complete intervention increased the selection of women as beneficiaries. To test the robustness of our results, we focus on three potential issues: Non-compliance, selection of covariates and deviations from the sampling protocol. Our main results are robust to several alternative choices:

**Accounting for non-compliance**

Three local governments did not allow our teams to complete the data provision component of our intervention. While there is a compliance rate of 93%, it is worth checking how our results change when we account for non-compliance. Instead of the pre-registered ordinary least squares estimates, we therefore also examine local average treatment effects from two-stage least squares estimation using the treatment assignment as an exogenous and relevant instrument for complete treatment implementation (F-stat= 175.5). Table H1 shows that the results are very similar in magnitude and significance.

**Data driven selection of covariates**

While we pre-registered to present regressions with and without pre-registered baseline covariates, we lacked prior knowledge on which variables could be predictive of our outcome variables and improve our statistical power and precision of estimates. We can therefore use lasso post-double selection of covariates as a data-driven approach for the selection of covariates. We find that with post-double lasso selection, apart from the strata fixed effects for the districts that we use in every specification, only the local poverty rate is being selected by the model and our results with lasso-selected covariates are very similar in magnitude and significance compared to the previously presented regressions with pre-registered covariates and without covariates.

**Accounting for deviations from the sampling protocol**

As discussed above, due to different numbers of beneficiaries in each ward and each union, the surveyors sometimes deviated from the sampling protocol during the endline. We first collected the beneficiary lists from every subdistrict office and prepared ward-specific lists with randomly ranked beneficiaries that we handed out to our surveyors. In every ward, the surveyors were supposed to survey five beneficiaries in the sequence of the provided ward-level beneficiary list but they were not always able to do so as either there were fewer than five beneficiaries in a ward or they were unable to locate some of the listed beneficiaries. Whenever fewer than expected beneficiaries were surveyed, surveyors were encouraged to compensate for it by surveying more beneficiaries in other wards from the same union. As the ranks of the surveyed beneficiaries go in 5% of the observations beyond rank 7, we check whether our main results hold when we focus on the observations up to rank 7. Table H3 shows that the results are very similar in magnitude and significance.

Table H1: LATE results accounting for non-compliance

| | (1) Prob. of poverty | (2) Eligibility index (SD) | (3) High prob. of poverty | (4) Land | (5) Asset index (SD) | (6) Lives alone | (7) Cannot walk | (8) Female |
|---|---|---|---|---|---|---|---|---|
| Completed treatment | 0.009 | 0.025 | 0.029 | -16.006 | -0.099 | 0.044 | 0.023 | 0.073 |
| | (0.009) | (0.065) | (0.011) | (7.407) | (0.078) | (0.023) | (0.018) | (0.029) |
| N | 1240 | 1240 | 1240 | 1240 | 1240 | 1240 | 1240 | 1240 |
| Control group mean | 0.20 | 0.00 | 0.04 | 46.53 | 0.00 | 0.09 | 0.07 | 0.44 |

*Notes:* The eligibility index and the asset index are z-standardized. All specifications include district fixed effects to account for stratification and pre-registered baseline covariates. Standard errors are clustered at the union level and shown in parentheses.

Table H2: ITT results with lasso-selected covariates

| | (1) Prob. of poverty | (2) Eligibility index | (3) High prob. of poverty | (4) Total land | (5) Asset index | (6) Lives alone | (7) Cannot walk | (8) Female |
|---|---|---|---|---|---|---|---|---|
| Training and EIC | 0.010 | 0.023 | 0.030 | -12.676 | -0.089 | 0.035 | 0.016 | 0.050 |
| | (0.008) | (0.062) | (0.011) | (6.613) | (0.074) | (0.019) | (0.016) | (0.025) |
| N | 1240 | 1240 | 1240 | 1240 | 1240 | 1240 | 1240 | 2037 |
| Control group mean | 0.20 | 0.00 | 0.04 | 46.53 | 0.00 | 0.09 | 0.07 | 0.45 |

*Notes:* The eligibility index and the asset index are z-standardized. Pre-registered covariates are selected with post double lasso. All specifications include district fixed effects to account for stratification. Standard errors are clustered at the union level and shown in parentheses.

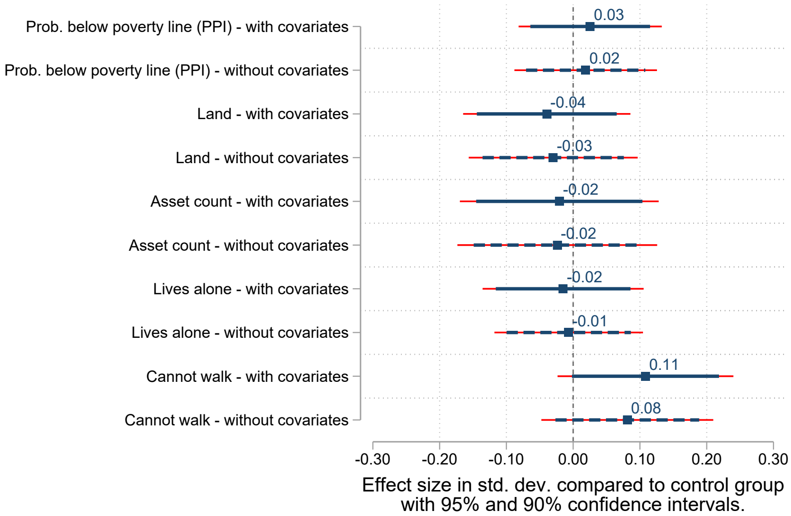
Table H3: ITT results up to rank 7 (95% of the sample)

| | (1) Prob. of poverty | (2) Eligibility index | (3) High prob. of poverty | (4) Total land | (5) Assets | (6) Lives alone | (7) Cannot walk | (8) Female selected |
|---|---|---|---|---|---|---|---|---|
| Training and EIC | 0.011 | 0.017 | 0.028 | -16.122 | -0.097 | 0.039 | 0.029 | 0.065 |
| | (0.008) | (0.065) | (0.011) | (7.218) | (0.077) | (0.022) | (0.017) | (0.027) |
| N | 1174 | 1174 | 1174 | 1174 | 1174 | 1174 | 1174 | 1174 |
| Control group mean | 0.20 | 0.00 | 0.04 | 47.38 | 0.00 | 0.09 | 0.07 | 0.45 |

*Notes:* The eligibility index and the asset index are z-standardized. All specifications include district fixed effects to account for stratification and pre-registered covariates. Standard errors are clustered at the union level and shown in parentheses.

# I Impact on targeting of widow allowance

Figure I1: ITT - Targeting of widow allowance



# J Heterogeneity - regression tables

Table J1: Heterogeneity by honesty of the committee

| | (1)<br>Prob. of poverty | (2)<br>Eligibility index |
|---|---|---|
| Training and EIC | 0.104 | 0.078 |
| | (0.085) | (0.097) |
| More honest committee | 0.038 | 0.014 |
| | (0.073) | (0.094) |
| Training and EIC X more honest committee | -0.100 | -0.106 |
| | (0.111) | (0.143) |
| N | 1240 | 1240 |

*Notes:* The dependent variables are z-standardized. We refer to a more honest committee if the average number of reported matches of all committee members is lower than the median of the average number of matches reported by all committees. All specifications include pre-registered union-level covariates and district fixed effects to account for stratification. Standard errors are clustered at the union level and shown in parentheses.

Table J2: Heterogeneity by honesty of the chairperson

| | (1)<br>Prob. of poverty | (2)<br>Eligibility index |
|---|---|---|
| Training and EIC | 0.183 | 0.044 |
| | (0.083) | (0.093) |
| More honest chairperson | 0.229 | -0.026 |
| | (0.081) | (0.111) |
| Training and EIC X more honest chairperson | -0.265 | -0.066 |
| | (0.120) | (0.142) |
| N | 1153 | 1153 |

*Notes:* The dependent variables are z-standardized. We refer to a more honest chairperson if the chairperson reports fewer matches than the median number of matches reported by all chairpersons. The number of matches is missing for six chairpersons. All specifications include pre-registered union-level covariates and district fixed effects to account for stratification. Standard errors are clustered at the union level and shown in parentheses.

Table J3: Heterogeneity by education of the committee

| | (1) Prob. of poverty | (2) Eligibility index |
|---|---|---|
| Training and EIC | 0.016 | 0.063 |
| | (0.097) | (0.098) |
| High education | -0.104 | -0.043 |
| | (0.072) | (0.103) |
| Training and EIC X high education | 0.089 | -0.059 |
| | (0.125) | (0.144) |
| N | 1240 | 1240 |

*Notes:* The dependent variables are z-standardized. A committee is highly educated if the committee has an average education greater than the median of the average education of the committees. All specifications include pre-registered union-level covariates and district fixed effects to account for stratification. Standard errors are clustered at the union level and shown in parentheses.

Table J4: Heterogeneity by education of the chairperson

| | (1) Prob. of poverty | (2) Eligibility index |
|---|---|---|
| Training and EIC | -0.077 | 0.008 |
| | (0.073) | (0.089) |
| High education | -0.163 | -0.022 |
| | (0.075) | (0.126) |
| Training and EIC X high education | 0.427 | 0.034 |
| | (0.128) | (0.139) |
| N | 1182 | 1182 |

*Notes:* The dependent variables are z-standardized. A chairperson is highly educated if a chairperson has an education level grater than the median of the education of the chairpersons. The education level is missing for four chairpersons. All specifications include pre-registered union-level covariates and district fixed effects to account for stratification. Standard errors are clustered at the union level and shown in parentheses.

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