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

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Abstract

To improve targeting of social policies, research has focused on incentives and accountability of local decision makers. This paper identifies the capacity of the local government as another key constraint which has received little attention. We examine whether and how a locally implemented capacity building intervention including training and data provision for the national Old Age Allowance program in Bangladesh can improve the selection of beneficiaries. The results of a large-scale clustered randomized controlled trial in 80 rural municipalities show that the intervention enhanced the knowledge of eligibility criteria but did not improve the targeting performance. We further document that improvements in the targeting performance depend on selection committee's willingness to improve beneficiary selection and that the illegitimate collection of fees is prevalent.

Keywords: social policy, targeting, local governance, randomized controlled trial, Bangladesh

JEL Codes: D91, I38, H55, H75

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1 Introduction

The COVID-19 pandemic has highlighted the importance of social protection schemes enabling households and individuals to satisfy their most basic needs during economically difficult phases. While social protection schemes received a lot of attention during the pandemic, throughout the last decades, governments in developing countries have been expanding social welfare programs with the objective of alleviating poverty (Barrientos and Hulme, 2010; Fiszbein et al., 2014). Given the limited availability of public resources for social protection, especially in developing countries, most schemes target specific groups in the population who need public support the most. However, many policies that specifically target the poor, are poorly targeted and often fail to reach the intended beneficiaries (Alatas et al., 2012; White, 2017; Asri, 2019).

Motivated by better knowledge of the local context, national governments often delegate the implementation of social policies to local-government representatives and officials, who tend to work under severe constraints, not just with respect to financial and physical resources, but also with respect to access to relevant information and tools to process this information (Alderman, 2002; UNCDF and UNDP, 2012; UNDP, 2016; World Bank, 2004, 2017).

Previous literature discusses two main reasons for discrepancies in the way policies are nationally designed (often with international assistance) and how they are locally implemented (Lipsky, 1980; Niehaus et al., 2013; Pressman and Wildavsky, 1984; Steiner, 2000). First, local-government representatives have discretionary power with respect to how they implement certain rules and guidelines, and they have interests which might not be well aligned with those of the national government, potentially due to bribing and other forms of special-interest influence. Hence, moral hazard problems arise and resources are often channeled to other recipients than the original target group of the policy. Second, the local administration's performance depends on the conditions under which they are working; they face certain capacity constraints in terms of training, information, financial resources, and time. As a consequence, even honest and public-spirited local decision-makers might not be able to implement the policies as intended.

It is the second reason and its almost complete neglect in the empirical literature that motivates our study. While capacity constraints have been documented in the literature, interventions that aim at alleviating them have hardly received any attention. Instead, development research has focused primarily on measures to improve accountability of public officials or on supporting citizens to claim their entitlements. These include performance-linked employment and salary schemes (Banerjee and Duflo, 2006; Bourdon et al., 2006,

2010; Duflo et al., 2012; Muralidharan and Sundararaman, 2011), information provision about entitlements to the intended target group (Francken et al., 2009; Reinikka and Svensson, 2004, 2011), or other monitoring and reward systems designed to incentivize public officials (Banerjee et al., 2011; Deininger and Mpuga, 2005). However, in their meta review on the effect of transparency on governance, Kosack and Fung (2014) emphasize that in many cases the problem is not that local officials or other service providers do not want to collaborate but a variety of other reasons, such as capacity constraints, affect their performance. In such situations, approaches focusing exclusively on monitoring and accountability might be ineffective. Further, while monitoring might, of course, be an effective method for a single program in some settings (e.g., Muralidharan et al., 2018), the administrative and financial burden created by such efforts could quickly grow out of bounds when hundreds of policies need to be monitored in a country. Also for this reason, it is worthwhile to explore other channels, such as capacity building, to improve the implementation of public programs.

It is hard to imagine how targeting of social policies can be effectively improved without relaxing state-capacity constraints. Without reliable data on the poverty of the local population it is almost impossible to correctly select those individuals who need the financial support the most. Similarly, even with the best of intentions, without appropriate training on eligibility rules and implementation guidelines, it will be impossible for a local-government representative or official to carry out a selection of the most eligible beneficiaries according to the national guidelines. Both problems, the lack of income data and not knowing the government guidelines appear to be particularly severe in developing countries where resources for training are scarce and most people work in the informal economy. These issues may even become worse if instead of local-government officials elected local-government representatives are in charge who work on public-sector tasks next to their regular full-time job. In Bangladesh, while the officials typically have university education, are competitively selected for their work as civil servants and trained for their specific tasks, the elected local representatives have very heterogeneous educational and professional backgrounds and lack formal preparation or training for the numerous responsibilities that they need to fulfill for a small honorarium.

Combining insights from previous literature with our own formative research on the local implementation of the national Old Age Allowance (OAA) program in Bangladesh, we, first, examine the underlying reasons for mistargeting of a national social pension program for the elderly poor. Second, we analyze how the targeting of social transfers can be improved. For this purpose we evaluate an intervention, developed in close collaboration with the Ministry of Social Welfare in Dhaka, that provides training on selection rules

and procedures as well as data on the target group to the Old Age Allowance beneficiary selection committee members. We further test for potential spillover effects to another program, the Widow Allowance scheme, which uses similar poverty-focused targeting rules and selection procedures.

The results from the first part of our analysis show that the targeting performance of the Old Age Allowance program is poor and that lacking state-capacity is a very likely reason for this. However, the intervention that we evaluate in the second part does not improve the targeting performance despite the fact that the intervention improves the knowledge of eligibility criteria among the local government representatives and the beneficiaries. Suggesting a plausible reason for the null effect on targeting performance, we find evidence of the relevance of bribe payments in the context of beneficiary selection. Moreover, we find that the intervention improved targeting in treatment areas with more honest selection committees, that is with average honesty measures above the median score among all municipalities. Hence, our findings suggest that both capacity constraints and corruption need to be addressed at the same time in future interventions.

The remainder of the paper is structured as follows. In Section 2, we provide background information on the Old Age Allowance in Bangladesh. We describe the selection criteria and processes as well as the the prevailing shortcomings of the current implementation. In Section 3, we describe the intervention. In Section 4, we explain our study design and the data. Section 5 presents the results and Section 6 concludes.

2 Background

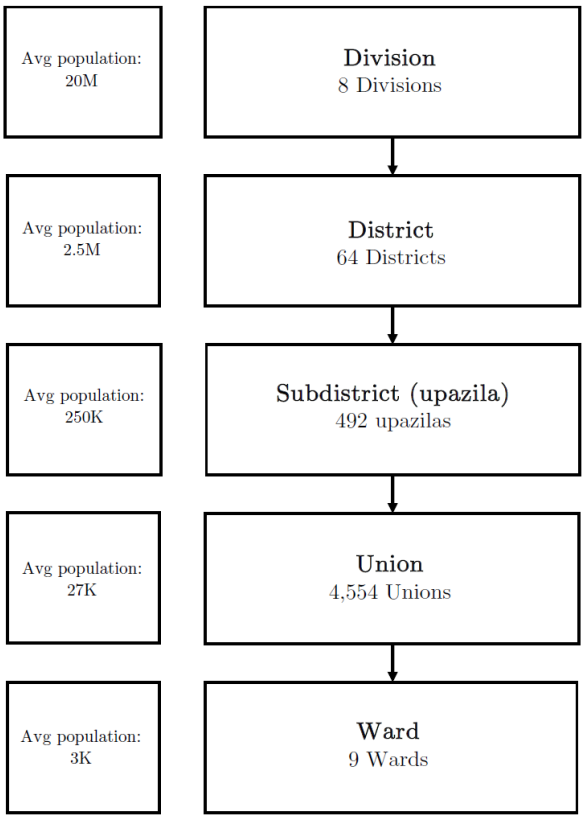
In 1998, the national government introduced the Old Age Allowance as a social pension scheme for elderly poor. The primary objective of the scheme is to mitigate old age poverty and the government provides a benefit of currently 500 Bangladeshi Taka (BDT; around 6 USD) per month to selected beneficiaries. The number of beneficiaries increased continuously since its introduction and it is now with more than 5.7 million beneficiaries one of the largest social safety nets in the country (Department of Social Services, 2020). Given the accelerating demographic change in Bangladesh, it is expected to become even more important in the future (United Nations, 2019). Reaching the intended beneficiaries remains a major challenge (Maxwell Stamp, 2017).

2.1 Guidelines for beneficiary selection

For the selection of beneficiaries the national government provides the criteria: age, income, working status, physical condition (health) and social condition (household com-

position). At the lowest level of the local government, also called Union Parishad (UP), an Old Age Allowance selection committee is in charge of selecting beneficiaries.[1] This committee includes representatives of the municipality, called union, as well as representatives of sets of two or three villages, also called wards. Each union consists of nine wards and each ward is represented by one representative, the UP Member. The administrative level above the union, the subdistrict, called upazila, is also represented in the union selection committee. The 18 member selection committee includes the UP Chairman, nine UP Members, the Union Social Worker, three women representatives, called UP Women Members, each of them representing three wards, the Representative of the Upazila Chairman, the Representative of the Upazila Nirbahi Officer (i.e. of the chief executive officer of an upazila) and one female and one male Representative of the Local Member of Parliament at the union level (Government of Bangladesh, 2013).

Figure 1: Administrative structure of Bangladesh



Source: Government of Bangladesh (2022) and United Nations (2019).

In terms of implementation, the national government describes the process as follows: Based on the annual budget allocation for the social pension, the national government first informs the local governments (OAA selection committee) at the union level about

[1]Figure 1 provides an overview of Bangladesh's administrative structure.

the number of additional pensions that will be available locally, and requests them to select new beneficiaries. Second, the selection committee informs the local population about the selection process by announcing the timing of the selection and the eligibility criteria. Third, the selection committee selects beneficiaries among the applicants and submits the list of selected beneficiaries to the Old Age Allowance selection committee at the upazila level. The upazila committee has the responsibility to review the list, make changes if required and approve it (Government of Bangladesh, 2013).

2.2 Targeting in practice

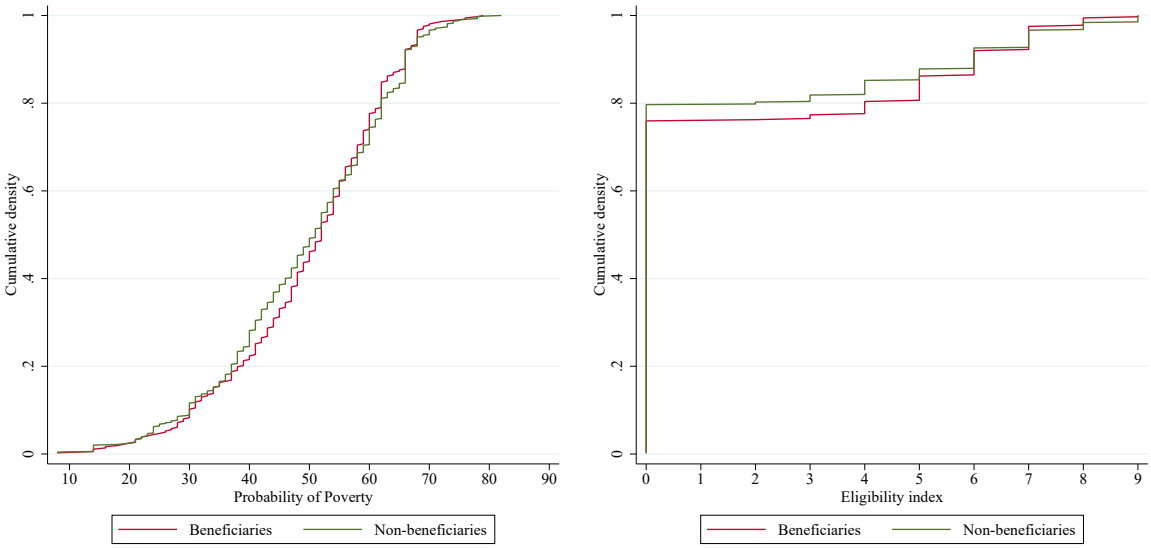
In practice, the selection of beneficiaries often does not seem to follow the official guidelines. In our qualitative and quantitative field research from Spring 2018, we observe two frequently used practices. First, individual selection committee members inform some citizens (of their choosing) about the availability of new pensions, arrange their documents and include them on the list. Second, typically organized and monitored by the upazila level, so-called "open-field selections" are organized in which all the elderly from a union gather in front of the Union Parishad office on one day and the representatives go through the lines of men and women to make a selection following few of the above described selection criteria. While in the former case, knowing someone from the selection committee appears to be crucial, in the latter case the focus appears to shift towards the age as binding condition, local representatives ask about available family support and directly observe the physical condition of the elderly person. Other criteria such as household income or land ownership appear to be neglected in this ad-hoc selection.[2]

A pilot survey of beneficiaries and non-beneficiaries that we conducted in spring 2018 in eight unions (from the same region where we later implemented the randomized controlled trial) documents that beneficiaries are as eligible as non-beneficiaries.[3] Comparing the two groups in terms of their poverty (Figure 2, left panel) and more specifically in terms of their eligibility for the Old Age Allowance (Figure 2, right panel) shows that the two groups of beneficiaries (in green) and non-beneficiaries (in red) are hardly distinguishable, which means that targeting was as effective as randomly selecting old people into the program.

[2]Respondents in qualitative interviews and focus group discussions reported both scenarios. Further, one local Co-PI attended open field selections to confirm these insights from qualitative interviews and focus group discussions.

[3]In this phase of formative 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 local government representatives (LGs), who were in charge of the last round of selections, $N = 80$.

Figure 2: Poverty and eligibility of beneficiaries and non-beneficiaries



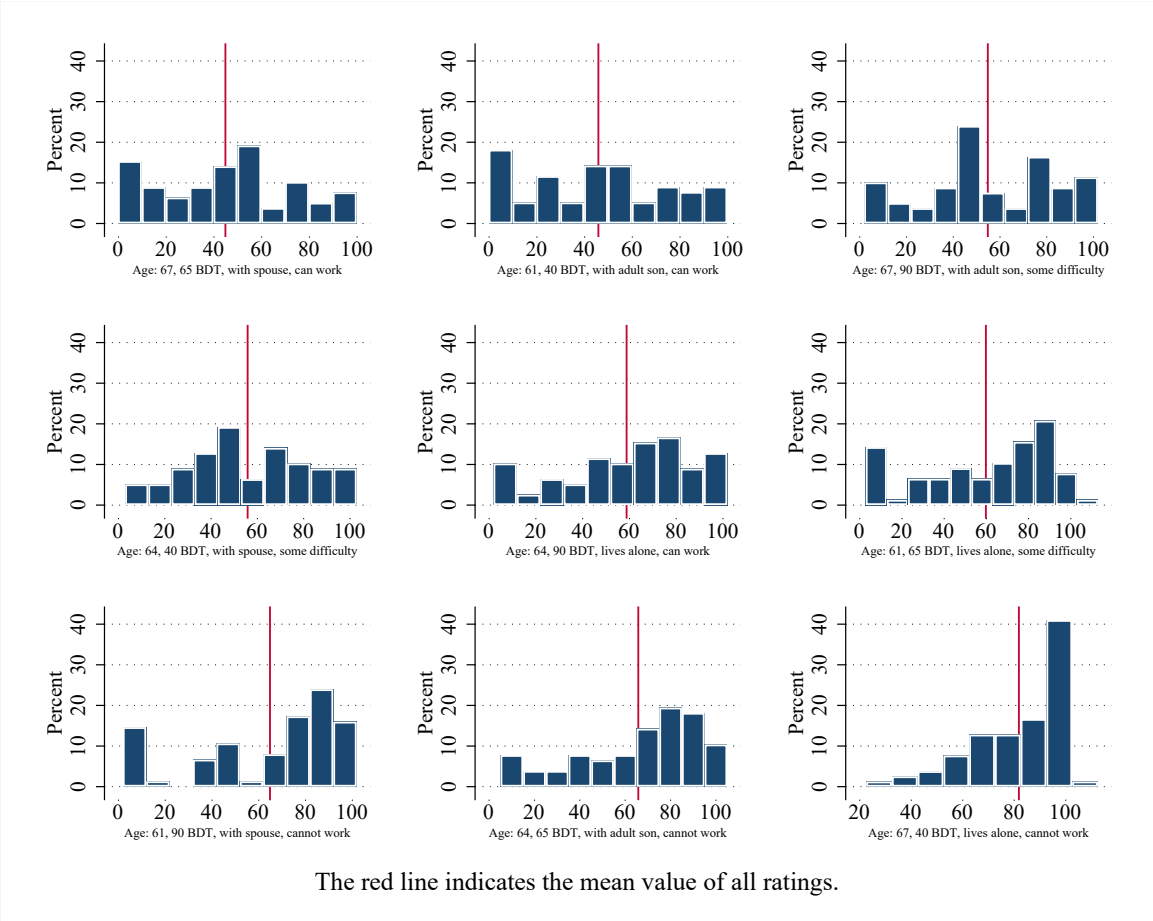
Notes: The panel on the left displays the cumulative density functions (cdfs) of wealth, as measured by the probability of poverty index, PPI (Schreiner, 2013; Kshirsagar et al., 2017), the right panel displays the cdfs of an eligibility index based on the official eligibility rules (see Appendix B).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

2.3 Underlying reasons

Various reasons may explain why the Old Age Allowance program is not targeted towards the poor. On the one hand, selectors in charge may struggle to follow the guidelines in practice. Our pilot survey of local government representatives demonstrates that those who are in charge of selecting beneficiaries have only very partial knowledge 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%).

Also, selectors seem to struggle with assessing the eligibility of individuals (Figure 3). Being confronted with 18 fictional profiles (9 male and 9 female) with varying age, taka available per day for basic needs, coresidence and physical difficulty to work (see figure labels), the selectors gave eligibility ratings ranging from 0 to 100 for 16 out of 18 profiles, and while 20 percent of the variation in the eligibility ratings can be explained by selector dummies the attributes of the fictitious applicants explain only 12-14 percent of it (see regression tables in Appendix C).

Figure 3: Eligibility ratings - female profiles



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

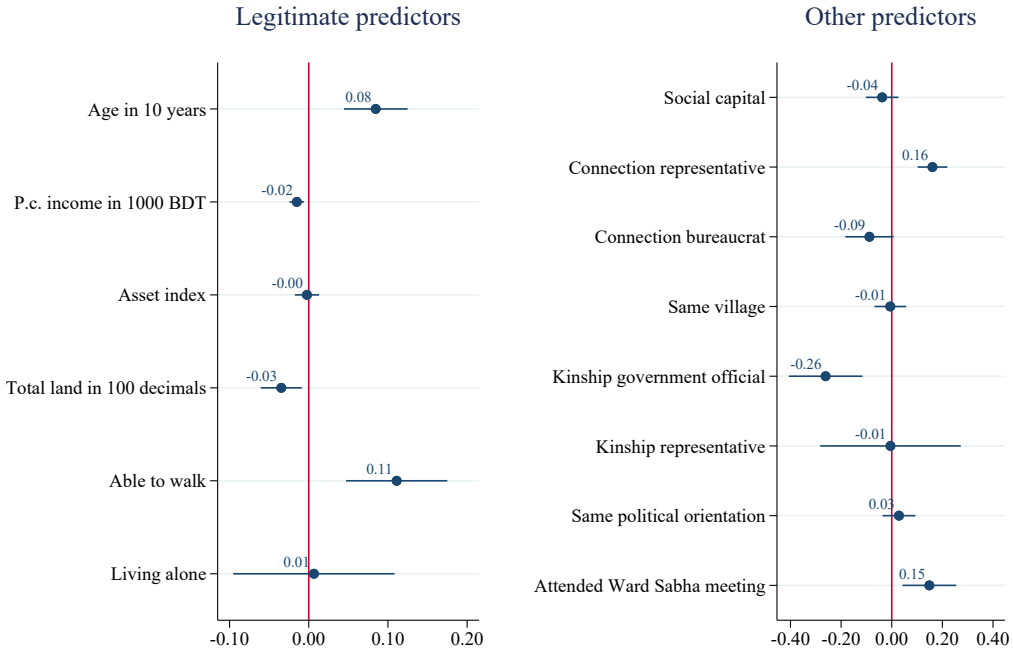
When surveyors asked selectors whether they need support for the eligibility assessment, 60% of the respondents reported that they very much need support and being asked for the type of support, 46% indicated that they need support in terms of staff and 37% indicated that they needed support in terms of data, while only 9.5% and 8.1% indicate that they most urgently need more funding and better guidelines, respectively. The strong request for more data seems to reflect a genuine need. It reflects the understanding that data (in the form of information about the applicants) is important for a proper selection.[4]

Finally, we observe that individuals who know the selectors personally have a significantly higher chance of getting selected (Figure 4, coefficient of *Connection representative*). At first sight, this appears to be closely linked to corruption and selectors violating the guidelines for private gains. However, given the need for support described above, it could also signal that selectors simply rely on the local information that they have and they know

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

better about people that they know than those that they do not know.[5]

Figure 4: Legitimate predictors and other predictors of beneficiary selection

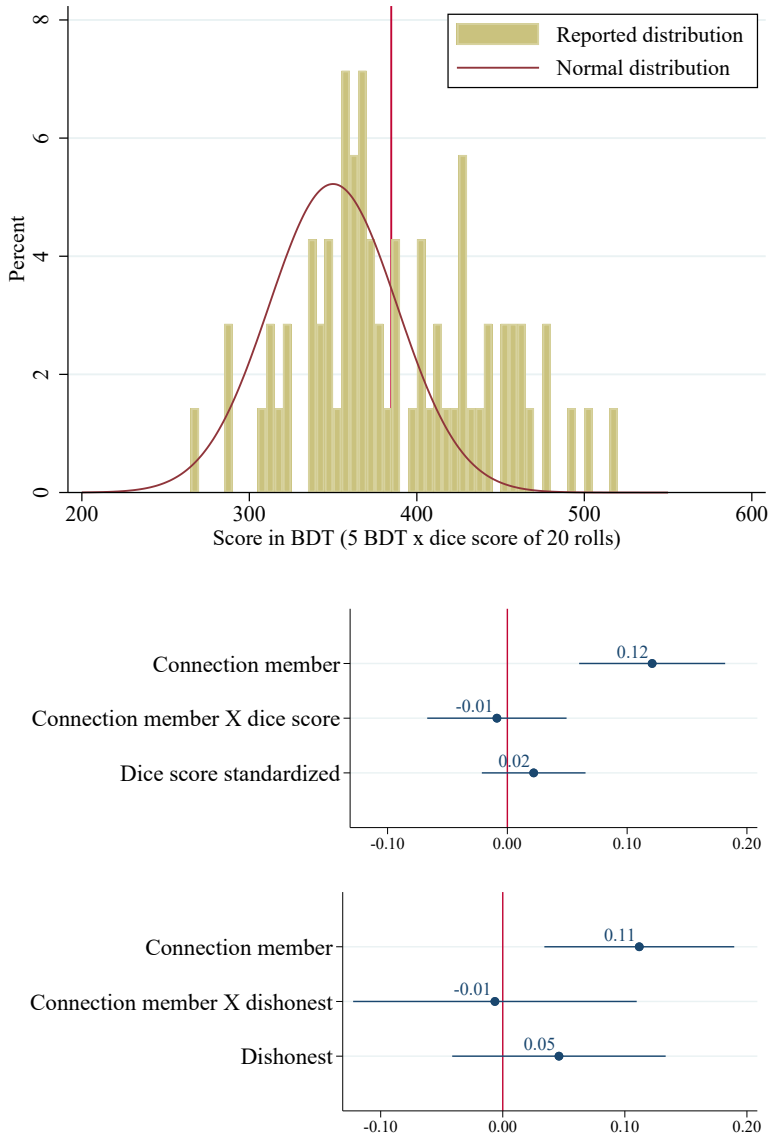


Notes: The coefficients in both panels stem from regressing pension receipt (i.e. being a beneficiary or not) on all these characteristics of beneficiaries and non-beneficiaries. The regression tables are shown in Appendix C. The results presented here are from the most comprehensive specification including gender and religion as covariates as well as union fixed effects.

In the intervention, we focus on the capacity constraints, as the literature on whether and how reducing capacity constraints can improve the targeting of social transfers is very scarce. Moreover, an empirical analysis of whether more dishonest selectors are more likely to rely on personal connections, cannot be confirmed in the pilot data (Figure 5, lower panel, insignificant interaction effects). We use the dice game (Fischbacher and Föllmi-Heusi, 2013) as adapted by Hanna and Wang (2014) to illicit the preference for dishonesty.

[5]The negative and significant coefficient for being a kinship of a government officials can be explained by two factors. First, family members of government officials benefit from a stable and secure income. Second, government officials locally perform primarily administrative tasks but do not have the power to decide about the selection of beneficiaries.

Figure 5: Dishonesty and the relevance of personal connections



Notes: The upper panel shows the empirical (histogram) and the theoretically expected (red normal) distribution of dice rolls. The vertical line displays the mean of the empirical distribution. The lower panel shows results from regressions of the beneficiary status on having a connection and the dice score (and alternatively an indicator of having reported a very high number) and the interaction term. Regression tables are provided in Appendix C.

Subjects report in private the observed number of repeated rolls of a die and thereby face an incentive to lie (the upper panel of Figure 5 shows the empirical and the theoretical "honest" distribution). The insignificant coefficient of the interaction term in combination with the previously presented results suggests that corruption might not be the most important problem but rather missing knowledge and data.

3 Description of intervention

The intervention design builds directly on these insights on the mistargeting of the Old Age Allowance in Bangladesh as presented in the previous Section 2 with a primary focus on addressing the prevailing capacity constraints. The underlying theory of change is that an intervention that improves the knowledge of eligibility criteria and provides information on the target group to those who are in charge of selecting beneficiaries, can improve the selection of beneficiaries. Given this theory of change, we designed an intervention with two components. We provide training to local government representatives and data on the target group to facilitate a more systematic and eligibility focused allocation of the social pension benefits. The intervention was carried out by an not-for-profit organization on behalf of the Department of Social Services.

The intervention was implemented at the union level. In each treatment union, the training component was provided to all selection committee members who are responsible for the selection of beneficiaries from all nine wards, but the target-group data collection and transfer was implemented only in three out of nine wards in each treatment union.

Component 1: Training Old Age Allowance selection committee members on the beneficiary selection criteria

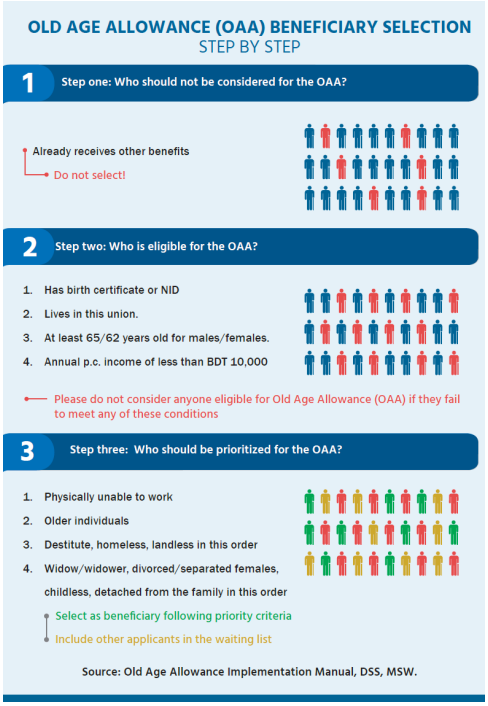
The training on the selection criteria for the OAA and on an information tool that we call the "Eligibility Information Card" (EIC) was developed in collaboration with the Ministry of Social Welfare. We designed one-to-one training sessions in which the trainer would show videos to the trainee and have a structured discussion of the content. The one-to-one approach allowed the trainer to engage more effectively with local representatives of different educational backgrounds including non-literate individuals and university graduates. The videos ensured that the same information reaches every trainee without being altered or interpreted differently by each trainer. The training took place in private, in most cases at the home of the selection committee member or in a government office.

The trainers followed a training protocol consisting of showing videos, having structured

Figure 6: Training videos

Notes: Screenshots from the video scenes used for training purposes.

Figure 7: 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.

verbal interactions with the trainee, conducting a short practice session, and ending with a quiz. The practice session included sorting hypothetical profiles following the national guidelines. In case someone missed or misunderstood content, the trainer repeated the explanations and answered any remaining questions. Each training session took between 45 and 90 minutes. The animated videos specifically produced for this intervention inform about the policy objectives of the Old Age Allowance and illustrate how a systematic selection of beneficiaries can be carried out. Figure 6 shows screenshots from the videos following the plot. At the end of the training, the trainer handed out a foldable poster to the trainee that summarized the three steps for beneficiary selection (Figure 7).

Similar to the development of the training program for local representatives, we also designed and carried out the training of trainers together with representatives from the National Academy of Social Services and the Department of Social Services. The training of trainers focused on the protocol and content for giving the training to the local-

government representatives and familiarized the trainers with the required background knowledge on the scheme and the eligibility criteria.

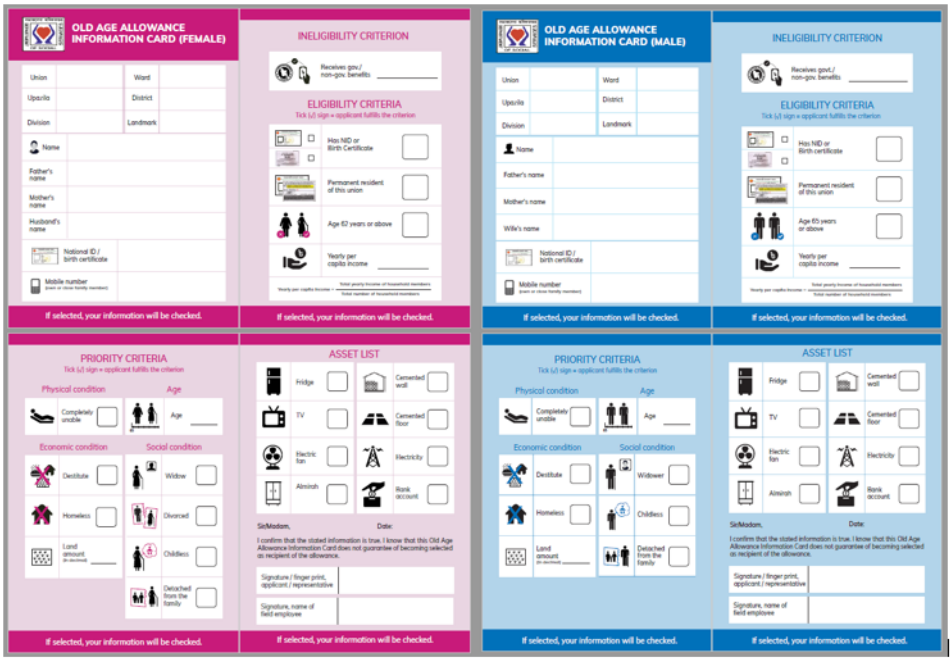
Component 2: Providing data on the Old Age Allowance target-group using Eligibility Information Cards

We designed the EIC as shown in Figure 8 in collaboration with the Department of Social Services, under the Ministry of Social Welfare. Following the government manual, the EIC can be used to collect 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 last page, the field officer enters complementary economic information on the household including information on durable assets, having a bank account and electricity. To make the information easily understandable for people with very different educational backgrounds we used pictograms for each criterion and each criterion is marked with a tick or a cross except for income and land amount. Both, field officer and elderly person signed the EIC.[6] The field officers filled two cards with the identical information. The first card was provided directly to the union selection committee with consent from the elderly. The second card was given to the elderly person who could use it to provide all relevant information to the selection committee members to apply for OAA. The elderly person could use this card to remind the local selection committee member of all her relevant information (in case the local selection committee member is not given attention to the provided EICs). After filling the EICs in the three different wards (every union has nine wards, see Figure 1), the teams of field officers, made copies of the EICs for the project records and submitted the filled EICs to the Union Secretary. Most answers to questions asked during EIC filling, are easily observable locally (e.g. land ownership, physical ability to work, homelessness or social living situation). Nevertheless, to discourage misreporting for the few questions that cannot be easily observed (e.g. income), it was announced and clearly stated on the EIC that provided information will be checked if the elderly person is selected as OAA beneficiary. Since rules with respect to age and social condition differ for females and males; and local representatives are requested to select a certain number of new beneficiaries among female elderly and male elderly separately every year, we designed two EICs — one for female potential beneficiaries and one for male potential beneficiaries that differ in the age and social condition (as well as, for practical reasons, in their color)

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

as shown in Figure 8.

Figure 8: 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 here the English version but in the field, we only used the Bangla version.

Implementation of both components

Due to their nature, the two intervention components were implemented by two different groups of field staff. First trainers, typically graduates of Social Science Master programs with the ability to explain the eligibility rules clearly and to communicate effectively with local representatives. Second, field officers, experienced enumerators who patiently and politely dealt with elderly people and knew how to interact with local representatives.[7]

The trainers worked in the municipalities before the field officers did. They typically fixed training appointments with local representatives a few days before reaching the union and carried out the training either at a local government office or at the local representative's home. Trainers further completed preparatory arrangements for the filling of EICs. They met the Upazila Social Service Officer, informed the UP Chairman and Members 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 before,

[7]Due to security concerns and the requirement of frequent and extensive travel, all trainers and field officers were male.

and again one day before the event. The venue had to be a public and central place easily reachable for everyone living in the ward.

Fortunately, the implementation of the intervention was completed prior to the COVID-19 pandemic in Bangladesh. However, the selection of beneficiaries done by the local government representatives took place in Spring-Summer 2020 during the COVID-19 pandemic and subject to locally implemented measures.

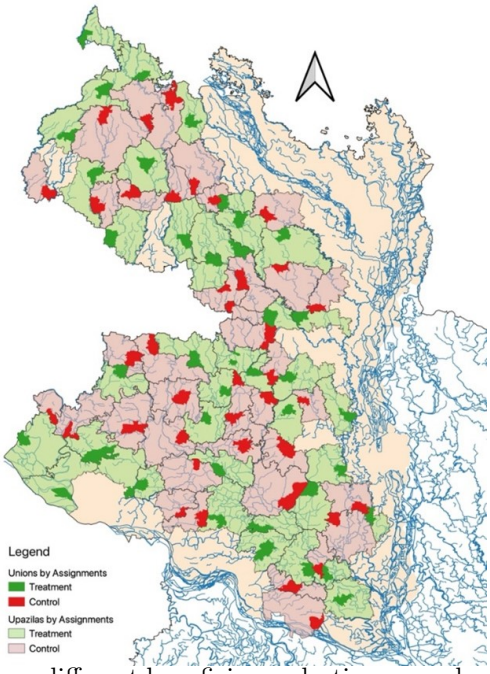
4 Empirical methodology and data

For this study, we implemented a cluster-randomized controlled trial with one treatment group (with two sub-groups as explained below) and one control group from Fall 2019 until Spring 2021.[8] The randomized controlled trial was carried out in 80 rural unions located in 80 sub-districts as shown in the map in Figure 9. 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.

In unions assigned to the treatment group, the two components of the intervention were implemented as follows: the training was provided to all selection committee members and the data was provided in three out of nine wards. Hence, we start assessing the impact of the state-capacity intervention by comparing beneficiaries from treatment unions to beneficiaries in control unions (treatment vs. control) and then proceed to comparing treatment areas that received training and data to control areas that did not receive either of the two (training and EIC vs. control) and treatment areas where the representatives only received the training but no data to control areas that did not receive either of the two (only training vs. control).

[8] We provide a timeline in the Appendix A. 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 but during a time period with very low incidence rates while following precautionary measures in terms of social distancing and mask-wearing.

Figure 9: 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 and further excluded flood-prone areas to ensure feasibility of data collection.

4.1 Hypotheses and outcome measures

Our primary hypotheses focus on the direct impact of the intervention on the targeting performance. With the secondary hypotheses, we examine the channels behind the impacts as well as the potential indirect impact of the intervention on another social transfer, the Widow Allowance. As primary outcome measure, we focus on the main objective of such social transfer programs, which is to reduce poverty. We use the Probability of Poverty Index (PPI) developed by Innovations for Poverty Action to compare the poverty status of newly selected beneficiaries in treatment unions with the poverty status of newly selected beneficiaries in control unions. The PPI is a general poverty measure that indicates how likely it is that a household is poor (Schreiner, 2013; Kshirsagar et al., 2017). The recently updated PPI for Bangladesh includes questions on location of residence, household size, household composition, highest grade completed by anyone in the household, ownership of durable assets, wall material, electricity connection and type of toilet used. The advantage is that it relies only on 10 simple survey questions which are easily verifiable. For the impact evaluation, we use the PPI constructed for the subset of households in rural areas corresponding to our study area which only inlcudes rural areas. Appendix B provides the list of survey questions used for the PPI and a

more detailed description of the index.[9]

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, poorer than newly selected beneficiaries in control unions. Going beyond this general expectation, our design also allows us to distinguish two types of impact assessments focused on the targeting of the social pension — the impact of the complete treatment (training and data provision) vs. the impact of the partial treatment (only training). In a treatment union, all 18 committee members responsible for selecting beneficiaries from all nine wards receive the training but only three out of nine wards (every union has nine wards) receive the data on the target group. The endline-data collection took place in six wards covering three wards where target-group data was provided, and three wards where target-group data was not provided to the selection committee. When comparing to the control group, this set up hence allows us to evaluate the impact of receiving the complete treatment, i.e. training and target-group data; and the impact of receiving only the training. These two impacts are measured in comparison to unions in the control group 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 PPI score is our main outcome of interest, we will also examine the impact on the eligibility index, which is a weighted score indicating whether and to what extent newly selected beneficiaries fulfill the eligibility and priority criteria as stated in the implementation manual, also described in detail in Appendix C. As local representatives are trained to follow the selection criteria as per Old Age Allowance Manual, we expect to observe an improvement in the eligibility index.

These are our primary hypotheses:[10]

Hypothesis 1: The joint provision of training and data on the target group increases the **mean PPI** of newly selected Old Age Allowance beneficiaries in the treatment wards compared to newly selected beneficiaries in the control group (**complete treatment**).

[9] While 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. We therefore complemented our data collection with additional variables indicating poverty and eligibility including land, asset ownership and income.

[10] We registered our six hypotheses in our pre-analysis plan at the AEA RCT registry (AEARCTR-0004510).

Hypothesis 2: The provision of training increases the **mean PPI** of newly selected Old Age Allowance beneficiaries in the treatment wards compared to newly selected beneficiaries in the control group (**partial treatment**)

Hypothesis 3: The joint provision of training and data on the target group increases the **mean eligibility index** of newly selected Old Age Allowance beneficiaries in the treatment wards compared to newly selected beneficiaries in the control group (**complete treatment**).

Hypothesis 4: The provision of training increases the **mean eligibility index** of newly selected Old Age Allowance beneficiaries in the treatment wards compared to newly selected beneficiaries in the control group (**partial treatment**)

Our secondary research hypotheses, focus on the expected channel behind the impact and the potential indirect impact on the beneficiary selection of another social benefits program: the Widow Allowance, which follows similar rules and procedures and its selection of beneficiaries takes place at the same time. The group of people in the OAA selection-committee largely overlaps that of the Widow Allowance selection-committee and by having learnt a systematic way of selecting beneficiaries for the Old Age Allowance and having observed a systematic data collection approach, committee members might also be able to improve the selection of Widow Allowance beneficiaries.

Hypothesis 5: The intervention increases on average the **knowledge of eligibility rules** among the local representatives in the treatment group compared to the local representatives in the control group.

We test this hypothesis using a knowledge index which counts the number of correct answers to questions on eligibility criteria, priority criteria and selection procedures, explained in more detail in Appendix B.

Hypothesis 6: The intervention increases the **mean PPI** of newly selected Widow Allowance beneficiaries in the treatment group compared to newly selected Widow Allowance beneficiaries in the control group.

To examine the impact on the targeting performance of the OAA in Hypothesis 1-2 and 3 and 4, the units of analysis are the newly selected OAA beneficiaries. For Hypothesis 5, the units of analysis are the local representatives and for Hypothesis 6, the units of

analysis are the newly selected Widow Allowance beneficiaries.

4.2 Data

Baseline data

Our baseline data collection was conducted as a phone survey. The sample consists of all 18 selection committee members from all 83 unions in our study area. Assuming that every position is filled in all committees this would amount to in total 1494 selection committee members. Our team 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 3 unions as the UP Chairmen did not participate in the survey. These unions also had the least number of selection committee members participating in the survey. The baseline surveys lasted between 25 and 30 minutes.

Baseline data-collection was focused on capturing whether and to what extent union selection committee members know the eligibility rules for the Old Age Allowance. Apart from these knowledge questions, we also collected data on their need for support for selecting beneficiaries and their willingness to lie for private gain using a dice game adapted specifically for the phone survey. In the dice game, the enumerator rolls a die 15 times and the respondent thinks for each die roll of a number between 1 and 6 and silently counts how many times the number on the die reported by the enumerator is matching with the number in her mind.[11] For each match, the respondent receives BDT 20. With this dice game, we obtained a measure of (dis)honesty at the individual level for our exploratory analyses of potential heterogeneous impacts.[12]

Described in more detail below, the impact of the intervention may depend on the willingness to apply the selection rules learnt 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, which we also used in our formative research (see Section 2), has been shown to predict corrupt behavior and support for rule-breaking by public sector employees in India (Hanna and Wang, 2014). So, it might be the case that it also relates to corrupt targeting practice. The baseline questionnaire further covered socio-economic variables such as education, literacy, land ownership and income, as well as working experience as local-government

[11] In Bangladesh, literally everyone knows how to count with one hand until at least 16. We piloted this extensively to ensure that respondents would not struggle to count and would not need pen and paper while taking part in the survey.

[12] We present in the appendix first the histogram of the number of reported matches and second OLS regressions examining whether any observable characteristics vary with the number of matches reported.

representative and party affiliation. In addition to the phone survey of local representatives, we use upazila statistics to check whether our samples are balanced.

The balance checks presented in Table 1 use data from the baseline survey and administrative data from the upazila level. Our control and treatment samples are balanced in terms of the baseline data and in terms of the upazila level development indicators. Only reading ability is slightly higher among the representatives in the control group than in the treatment group (significant at the 5% level). The null hypothesis of joint orthogonality cannot be rejected.

Endline data

The endline-data collection focused on testing whether the intervention improved the targeting of the benefits and the knowledge of eligibility and priority criteria. We collected data from newly selected OAA beneficiaries, from union selection committee members and from newly selected Widow Allowance beneficiaries. From the newly selected beneficiaries, we collected data on socio-economic variables (such as: education, land ownership, income), and variables required to calculate the Poverty Probability Index (PPI), and the knowledge indices of the OAA and Widow Allowance selection criteria. Moreover, we collected data on personal connections to local representatives and officials now and two years ago. From the selection-committee members, we collected data on their knowledge of the OAA and Widow Allowance selection criteria as well as data on socio-economic variables such as education, literacy, land ownership and income along with working experience as local-government representative and party affiliation.

We collected data from six wards in the treatment unions and three wards in the control unions. As mentioned earlier, 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 OAA 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 beneficiaries were randomly ranked, 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), 1335 local government representatives (compared to 1440 targeted) and 1166 Widow Allowance beneficiaries (compared to 1200 targeted). The samples are split approximately equally between treatment and control.

Table 1: Balance checks

	(1) Control	(2) Treatment	(3) P-value of difference
Panel A: Baseline survey data			
Female	0.25	0.25	0.983
Age	45.33	45.87	0.330
Education	9.77	9.60	0.382
Can read	0.97	0.95	0.028
Can write	0.96	0.94	0.104
Land	292	261	0.181
Monthly household income	42,300	48,097	0.327
First time representative	0.72	0.74	0.511
Years in current position	4.73	5.06	0.194
Knowledge index OAA	1.65	1.66	0.706
Knowledge index WA	1.10	1.12	0.520
Matches dice game	5.19	4.96	0.203
Observations	670	647	1,317
Panel B: Upazila statistics			
Total population	267,536	263,293	0.890
Number of households	65,985	63,240	0.701
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
Has tap water (%)	2.70	2.79	0.927
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 mean values of the selectors in the treatment group. In Panel B, using upazila development statistics, we compare the mean values of upazila statistics of upazilas in the control group and upazilas in the treatment group.

4.3 Analysis

4.3.1 Main analysis

In the pre-registered empirical analysis, we focus on measuring the impact of the intervention on the PPI of newly selected OAA beneficiaries (H1 and H2), on the eligibility index (H3 and H4), on the knowledge index (H5), and on the PPI of newly selected Widow Allowance beneficiaries (H6). First for our primary outcome of interest, in each union, we measure the PPI for the surveyed newly selected beneficiaries, so that we have several measurement points. We estimate the below regression model to assess the intention-to-treat (ITT)[13] effect of the intervention:

$$Y_{ij} = \alpha_1 + \alpha_2 T_j + \beta X_j + \epsilon_{ij} \tag{1}$$

where Y_{ij} is the measurement of the outcome variable PPI 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 ϵ_{ij} is the standard error clustered at the union level. As a robustness check, we also estimate the outcomes without baseline covariates.

As covariates in regression model (1), we include baseline values of local representatives' average knowledge index of OAA rules, their average honesty score, their reading ability, strata dummies (for each district) and relevant upazila level development statistics (namely total population, percentage of literate population, extreme poverty head count ratio and population 65 and above). These variables are chosen because they are expected to be good predictors of the outcome variable in the endline. We proceed analogously for testing the hypotheses on the eligibility index (H3 and H4) and the PPI of newly selected Widow Allowance beneficiaries (H6).

Below, we present our regression results first for the pooled treatment (either complete or partial) as specified in Equation 1 and then examine the relevance of the treatment dose regressing our outcome variables on an indicator variable for the complete treatment and an indicator variable for the partial treatment. The left-out-category is hence the pure control group and our estimates show the impact of the intervention type — complete or partial — always in comparison to the pure control group. In the appendix, we further present the main results for an alternative specification with two separate regressions in which we regress the outcome variables either on the complete or the partial indicator as independent variable of interest.

[13] As two unions in the treatment group had already completed their selection of new beneficiaries, we have a non-compliance rate of 5%.

When testing the hypothesis on the impact of the intervention on the knowledge index (H5), we adapt the regression model as follows:

$$Y_{ij} = \gamma_1 + \gamma_2 T_j + \delta X_{ij} + \epsilon_{ij} \tag{2}$$

where Y_{ij} is the knowledge index of local representative i in union j, T_j is again 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 OAA rules, and strata dummies (for each district). As a robustness check, we also estimate the regression model without baseline covariates.

Besides the knowledge gaps being directly addressed by the intervention, as specified in our pre-analysis plan, we also examine exploratorily whether and to what extent the complete and partial treatment may improve the beneficiaries' knowledge of specific eligibility and priority criteria.

4.3.2 Use of EICs for selection

Regarding the provision of "data on the target group" by filling out EICs and transferring them to the committee, an important question is whether these EICs were used in the selection of beneficiaries. To answer this question exploratorily, our data allows us to (a) link the beneficiary data from the endline with the EIC data from the intervention and (b) to compare within complete treatment areas beneficiaries for which we have a linked EIC and beneficiaries for whom we do not have a linked EIC.

4.3.3 Heterogeneous effects

As pre-registered in our pre-analysis plan, we examine whether the impact of the intervention is stronger in areas where the selection committee members are on average more honest. Our expectation is that selection committee members who choose to report the number of matches honestly instead of lying for private gain could be more open to learning from the training and more keen to use the provided data on the target group. We test this hypothesis by defining a variable at the union level that indicates whether a selection committee is relatively more honest or not. The binary variable is equal to 1 if the 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 1, an interaction term of the honesty variable and treatment indicator to assess

the impact of the intervention for relatively more honest unions.

Directly linked to the selection committee's ability to apply the learning from the training and to use the data provided with the EICs, we examine exploratorily whether treatment effects are different in areas where the selection committee is led by a highly educated individual. Given that the UP Chairman is also the chairman of the selection committee, we define a selection committee as being led by a highly educated person if the UP Chairman has an undergraduate or postgraduate degree and also include an interaction term of having an educated chairman and the treatment indicator in our regression model 1.

4.3.4 Prevalence of "fee" payments

To assess the prevalence of corrupt selection procedures and especially the collection of "fees" for the inclusion of individuals in the beneficiary list, we include a list experiment in the endline data collection (following Gilens et al. (1998) and Blair and Imai (2012)). All respondents were shown a pictogram and asked about the number of activities they completed when they tried to get selected as beneficiaries. Randomly selected respondents were shown either six activities including the fee payment (also called "veiled list") or five activities excluding the fee payment ("unveiled list"). By comparing the reported average number of activities from the group that saw the veiled list with the group that saw the unveiled list, we can measure the percentage of individuals having paid a bribe in our sample.

5 Results

In the following, we will present our findings when testing the previously described hypotheses. We first examine the intervention's impact on targeting and then assess the impact on selectors' knowledge of eligibility criteria as well as beneficiaries' knowledge of eligibility criteria. To unpack our main results, we then examine whether and to what extent EICs were used in practice, whether treatment effects are different for more honest selection committees and for selection committees headed by a highly educated Union Parishad Chairman. Lastly, we present the findings from the list experiment to measure the prevalence of fee payments influencing the beneficiary selection.

The analysis for the results presented below was pre-registered in our pre-analysis plan unless we mention explicitly that these are exploratory findings.

5.1 Impact on targeting

We examine the impact on the targeting performance according to our hypotheses for the probability of poverty index and the eligibility index. We start by presenting the results in Table 2 for our main specification in Equation 1 regressing the outcome variables indicating eligibility, probability to be below the national poverty line and the eligibility index, on the treatment variable indicating whether a union was assigned to the treatment or not. Next, to test our hypotheses on the impact of complete and partial treatment, in Table 3, we regress the primary outcome variables on the variables indicating complete treatment (training and EIC) and partial treatment (only training). All specifications include district fixed effects as the randomization was stratified by district. Covariates from the baseline aggregated at the union level and upazila statistics are included in the second and fourth specification. All specifications indicate that the treatment did not impact the average eligibility of the newly selected beneficiaries in treatment areas compared to control areas.[14]

Table 2: Impact on targeting overall - eligibility of new beneficiaries

	(1) Prob. poor (PPI)	(2) Prob. poor (PPI)	(3) Eligibility index	(4) Eligibility index
Treated	0.008	0.008	0.118	0.133
	(0.251)	(0.249)	(0.594)	(0.510)
N	1856	1856	1856	1856
Covariates	Yes	No	Yes	No
Control group mean	0.200	0.200	1.514	1.514

Notes: Covariates include baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at the union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

[14]We obtain the same result when we use separate regressions comparing the complete treatment to pure control and comparing the partial treatment to pure control.

Table 3: Impact on targeting by treatment dose - eligibility of new beneficiaries

	(1) Prob. poor (PPI)	(2) Prob. poor (PPI)	(3) Eligibility index	(4) Eligibility index
Training and EIC	0.010	0.010	0.046	0.061
	(0.217)	(0.217)	(0.832)	(0.765)
Only training	0.005	0.006	0.192	0.206
	(0.475)	(0.463)	(0.470)	(0.395)
N	1856	1856	1856	1856
Covariates	Yes	No	Yes	No
Control group mean	0.200	0.200	1.514	1.514

Notes: Covariates include baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at the ward level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Going beyond our pre-specified hypotheses and main outcome variables, we examine the impact on targeting in terms of alternative indicators of eligibility. Starting with individual income and land ownership, we observe, similar to before, that beneficiaries in unions assigned to the treatment do not have less income or own less land. All point estimates are as expected negative but insignificant. When we examine the relevance of the dose (complete vs. partial treatment), we find suggestive evidence ($p < 0.1$) that beneficiaries in complete treatment wards own 12 decimals less land than beneficiaries in control areas.

Table 4: Impact on targeting by treatment dose - income and land

	(1) P.c. income annual	(2) P.c. income annual	(3) Total land (decimal)	(4) Total land (decimal)
Training and EIC	-798.094	-464.518	-12.974*	-12.396*
	(0.345)	(0.616)	(0.068)	(0.066)
Only training	-334.970	-12.906	-5.679	-5.212
	(0.755)	(0.990)	(0.443)	(0.459)
N	1856	1856	1856	1856
Covariates	Yes	No	Yes	No
Control group mean	20508.911	20508.911	46.527	46.527

Notes: Covariates include baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at the union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Focusing now on the treatment dose with two treatment indicators for complete and

partial treatment, in Table 5 we explore the possibility, that the intervention encouraged selectors to consider relatively easily observable variables such as durable assets. As before point estimates are all negative as expected but insignificant.

Table 5: Impact on targeting - income and land

	(1) Assets count	(2) Assets count	(3) Assets index	(4) Assets index
Training and EIC	-0.191	-0.187	-0.132	-0.131
	(0.145)	(0.160)	(0.267)	(0.276)
Only training	-0.115	-0.112	-0.072	-0.073
	(0.332)	(0.367)	(0.488)	(0.504)
N	1856	1856	1855	1855
Covariates	Yes	No	Yes	No
Control group mean	3.343	3.343	0.187	0.187

Notes: Covariates include baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at the union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Other easily observable variables that signal the beneficiary's need to receive the allowance include being older, being physically weak captured here by being unable to walk, physical frailty and lack of social support. In Table 6, we find that the treatment did not impact the age of selected beneficiaries, however, in terms of needing support due to physical frailty or living alone, we find that beneficiaries in partial treatment areas are 4.4 percentage points more likely to be unable to walk ($p < 0.05$) and in complete treatment areas 3.7 percentage points more likely to live alone ($p < 0.1$).

Table 6: Impact on targeting - easy to observe

	(1) Age	(2) Age	(3) Cannot walk	(4) Cannot walk	(5) Lives alone	(6) Lives alone
Training and EIC	-0.168	-0.221	0.024	0.016	0.037*	0.035*
	(0.773)	(0.708)	(0.161)	(0.312)	(0.094)	(0.077)
Only training	0.115	0.077	0.044**	0.037**	0.004	0.003
	(0.870)	(0.909)	(0.023)	(0.043)	(0.843)	(0.875)
N	1856	1856	1856	1856	1856	1856
Covariates	Yes	No	Yes	No	Yes	No
Control group mean	71.541	71.541	0.066	0.066	0.092	0.092

Notes: Covariates include baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at the union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

As pre-specified, we further examine targeting of the Widow Allowance with the general idea that learning how to assess the eligibility of applicants for the Old Age Allowance could also help selectors to assess the eligibility for the Widow Allowance. In Table 7, we cannot reject the corresponding null-hypothesis. Our results suggest that the intervention did not impact the targeting of the Widow Allowance.

Table 7: Widow allowance targeting impacts

	(1) Prob. poor (PPI)	(2) Total land	(3) Ind. income	(4) Assets
Training and EIC	0.004	-1.636	-123.661	-0.043
	(0.641)	(0.532)	(0.221)	(0.720)
Control group mean	0.20	19.07	1416.45	2.61
N	1202	1202	1202	1202

Notes: Covariates include baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at the union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

From here, we proceed to examining how the intervention was received by the selection committee members. To understand this overall very muted impact, we turn first to the hypothesized channel — improvement in knowledge — and then examine whether EICs were used, potential heterogeneous effects in terms of the selection committee's preference for honesty and the selection committee's chairman's level of education and finally the prevalence of corrupt practices in the selection of beneficiaries.

5.2 Impact on knowledge of eligibility criteria

In the following, we examine whether and how the intervention improves the knowledge of eligibility criteria among the selectors. In a sense, this knowledge improvement can be seen as a first step for the intervention to be effective. Only if the intervention improved the knowledge of the eligibility criteria among the selection committee members, it can enable an improvement of the beneficiary selection.

Overall, about one year after the intervention, in Table 8, we find that for selectors surveyed in the baseline and endline (Panel A), the intervention significantly improves the knowledge index by 0.227 on a scale from 0 to 5. Given the mean of the control group of 2.82, the average knowledge increased by 8 percent. The improvement in the knowledge index is primarily driven by an improved knowledge of the income threshold which increased by 15.1 percentage points for the selectors in the treatment group. The results are similar, when we regress the knowledge indicators on the treatment for all selectors surveyed in the endline (Panel B) and then control for baseline variables aggregated at the

union level, upazila statistics and district fixed effects. We do not observe any knowledge improvements regarding land and age cutoffs[15]

In Table 9, we examine whether and how the intervention impacts the knowledge of eligibility criteria among the beneficiaries. Compared to the selectors, beneficiaries know much less about the eligibility criteria. On average in the control group, beneficiaries know only 0.56 out of 4 eligibility rules and none of the respondents knows all four eligibility rules that we asked them about. The intervention increases this knowledge index by 0.392 in complete treatment areas and by 0.312 in partial treatment areas. Beneficiaries seem to learn primarily about the age cutoffs in both types of treatment areas and about the land cutoff in partial treatment areas. Overall, the impact on knowledge of eligibility criteria is similar in areas that received training and EIC and areas that only received training.

Table 8: Impact on selectors' knowledge of eligibility criteria

	(1)	(2)	(3)	(4)	(5)
	Know index	Income	Land	Female age	Male age
Panel A: Matched selectors (baseline and endline)					
Treated	0.227***	0.151***	-0.000	0.044	0.017
	(0.000)	(0.000)	(0.993)	(0.185)	(0.124)
Control group mean	2.82	0.16	0.04	0.75	0.94
N	1192	1192	1192	1192	1192
Panel B: All selectors (only endline)					
Treated	0.262***	0.146***	0.007	0.059*	0.021*
	(0.000)	(0.000)	(0.724)	(0.073)	(0.096)
Control group mean	2.82	0.16	0.04	0.74	0.94
N	1245	1245	1245	1245	1245

Notes: Panel A includes all local government representatives that participated in baseline and endline. Covariates include individual-level baseline values of local representative's age, reading ability, years of education, knowledge index of OAA rules, and strata dummies (for each district). Panel B includes all local government representatives that participated in the endline. Covariates include baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

[15]In Bangladesh, selectors in charge have given traditionally a lot of attention to land ownership. Further, the land criterion is a priority criterion and not an eligibility condition and therefore the income threshold might have been remembered better from the training.

Table 9: Beneficiaries: Knowledge of cutoffs

	(1) Know index	(2) Male age	(3) Female age	(4) Land	(5) Income
Training and EIC	0.392***	0.164***	0.156***	0.009	0.062
	(0.000)	(0.000)	(0.000)	(0.273)	(0.120)
Only training	0.312***	0.136***	0.107***	0.013**	0.055
	(0.000)	(0.000)	(0.000)	(0.020)	(0.161)
Control group mean	0.56	0.32	0.07	0.01	0.16
N	1856	1856	1856	1856	1856

Notes: Dependent variable is knowledge index (0-4), stating correct cutoff for age (male/female), for land and income. We control for baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.3 Use of EICs

Turning now to the question of whether EICs were used for the selection of beneficiaries, we compare within complete treatment areas beneficiaries for which we have a linked EIC with beneficiaries for whom we do not have a linked EIC.

Indeed, we find that around 68% of the beneficiaries that we randomly selected from the beneficiary lists for our endline had filled in and submitted an EIC to the selection committee. Comparing new beneficiaries with EIC with new beneficiaries without, we find that beneficiaries with a linked EIC have a slightly higher probability to be poor according to PPI (1.7 percentage points significant at the 10 percent level), have a 0.297 units lower asset index, own 10.4 decimals less land (significant at 10 percent level) and have a 3273 BDT lower per capita annual income (significant at the 5 percent level). This suggests that the EICs were in fact used by the selection committees for many selections of beneficiaries but not for all.[16]

[16] Asset ownership reported on the EICs is very close to what the beneficiaries report in the endline data collection when the enumerator 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.

Table 10: Comparison of new beneficiaries with and without EIC

	(1) Prob. poor (PPI)	(2) Asset index DHS	(3) Land owned	(4) P.c. income annual
With EIC	0.017*	-0.297**	-10.391*	-3273.265**
	(0.093)	(0.032)	(0.052)	(0.024)
N	619	618	619	619
Covariates	Yes	Yes	Yes	Yes
Mean without merged EIC	0.194	0.197	39.888	21953.319

Notes: Covariates include baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at the union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.4 Heterogeneous treatment effects

5.4.1 Honesty of selectors

To examine whether treatment effects are different for beneficiaries living in areas with relatively more honest selection committees, as pre-registered, we include the interaction term for the honesty indicator variable and the treatment variable in our regressions. The marginal effects for the impact in more honest unions is presented in Table 13. The estimates remain insignificant for the probability to be poor according to the national poverty line and for the eligibility index.

As described above, it might be harder to observe improvements in targeting with respect to complex indices such as PPI or eligibility index compared to relatively easier observable indicators such as ownership of durable assets. Going beyond our pre-registered analysis, we examine heterogeneous treatment effects also for durable assets and show in the table below that corresponding to our expectation the complete treatment was more effective in areas with more honest selection committee members. In treatment areas with honest selection committee members, new beneficiaries own on average 0.378 fewer assets (control group mean is 3.343) than new beneficiaries in control areas when we use a simple count of assets and a 0.344 standard deviation lower asset index when we use principal component analysis (control group mean is 0.187). This result also matches the descriptive result above on the usage of eligibility information cards, in which apparently information on asset ownership was considered as useful.

Overall, the measure of the preference for dishonesty obtained from the dice game is likely to be a lower bound approximation of the preference for willingness to violate rules for private gains as corrupt behaviors respond to many other non-financial incentives such as political motives such as clientelism, reciprocating to real life interactions or other types

of local network motivations.

Table 11: Impact in more honest unions - eligibility

	(1) Prob. poor (PPI)	(2) Prob. poor (PPI)	(3) Eligibility index	(4) Eligibility index
Training and EIC=1	0.012	0.012	-0.133	-0.125
	(0.312)	(0.249)	(0.690)	(0.707)
Covariates	Yes	No	Yes	No
Control group mean	0.200	0.200	1.514	1.514
N	1240	1240	1240	1240

Notes: Marginal effects for more honest treatment unions compared to less honest treatment unions using interaction of honesty indicator with treatment variable. We control for baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Impact in more honest unions - asset ownership

	(1) Asset count	(2) Asset count	(3) Asset index	(4) Asset index
Training and EIC=1	-0.389**	-0.378**	-0.357**	-0.344**
	(0.039)	(0.027)	(0.045)	(0.034)
Covariates	Yes	No	Yes	No
Control group mean	3.343	3.343	0.187	0.187
N	1240	1240	1239	1239

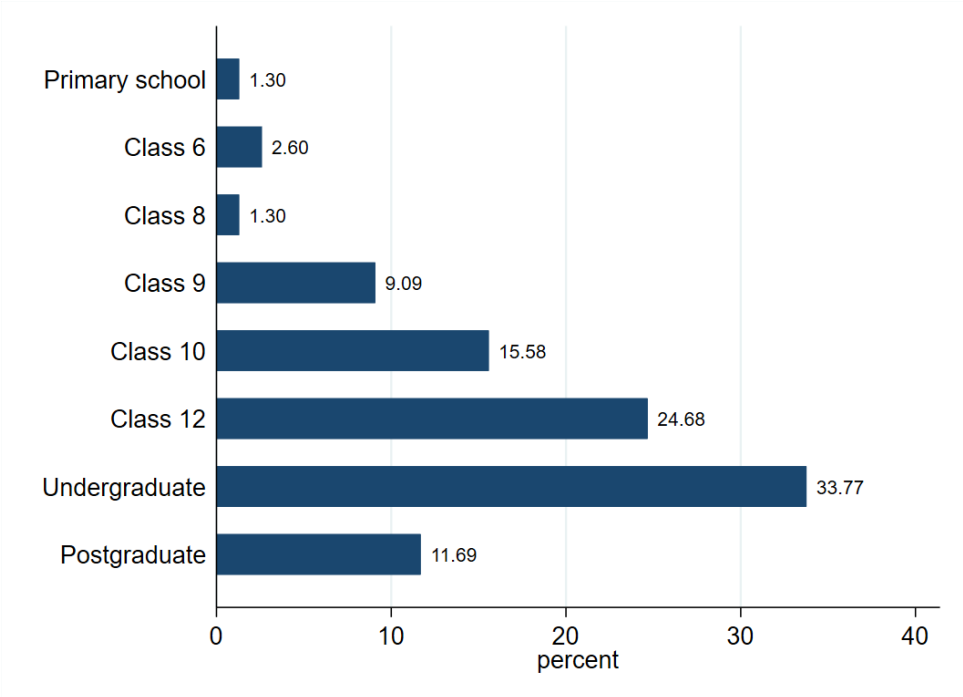
Notes: Marginal effects for more honest treatment unions compared to less honest treatment unions using interaction of honesty indicator with treatment variable. We control for baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.4.2 Education level of the chairman

Another plausible, though not pre-registered, source of heterogeneity in the treatment effect is that Union Parishad chairmen who head the selection committees have widely differing levels of education as shown in the graph below. Here the idea is that it is not only easier for highly educated individuals to absorb the learning from the training, it is also easier for them to plan how the committee can use the data from the EICs and organize a more systematic selection of beneficiaries in-line with the selection criteria.

As shown descriptively below, about 45% of the chairmen have either an undergraduate or a postgraduate degree and the remaining 55% have lower levels of education. We

Figure 10: Education levels of Union Parishad Chairmen



Notes: Level of education as reported by the Union Parishad Chairman during the baseline data collection. Sample size=77

categorize these chairmen as "highly educated" and use interaction effects to examine whether the treatment effect is different in unions with a highly educated UP Chairman.

Indeed, the results demonstrate that the treatment effect is stronger and positive in areas with a highly educated UP Chairman. As such, in treatment areas, beneficiaries are 4.2 percentage points more likely to be poor than beneficiaries in control areas and similar to the finding for honest selection committees, beneficiaries in treatment areas with selection committees headed by a highly educated UP Chairman, own on average 0.355 fewer assets if we consider a simple count (significant at the 5% level) and have a 0.272 standard deviations lower asset index (significant at the 10% level).

Table 13: Impact in unions with an educated chairman - eligibility

	(1) Below national poverty line	(2) Below national poverty line	(3) Eligibility index	(4) Eligibility index
Training and EIC=1	0.042***	0.038***	0.364	0.156
	(0.001)	(0.001)	(0.307)	(0.606)
Covariates	Yes	No	Yes	No
Control group mean	0.200	0.200	1.514	1.514
N	1180	1180	1180	1180

Notes: Marginal effects for treatment unions with a highly educated chairman compared to unions with less educated chairman using interaction of indicator variable for chairman having at least a Bachelor completed. Covariates and district fixed effects are interacted with a binary variable indicating whether the union selection committee is more honest than the median union. We control for baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Impact in unions with an educated chairman - asset ownership

	(1) Asset count	(2) Asset count	(3) Asset index	(4) Asset index
Training and EIC=1	-0.486**	-0.355**	-0.410*	-0.272*
	(0.029)	(0.024)	(0.051)	(0.067)
Covariates	Yes	No	Yes	No
Control group mean	3.343	3.343	0.187	0.187
N	1180	1180	1179	1179

Notes: Marginal effects for treatment unions with highly educated UP Chairman compared to less honest treatment unions using interaction of honesty indicator with treatment variable. Covariates and district fixed effects are interacted with a binary variable indicating whether the union selection committee is more honest than the median union. We control for baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.5 Prevalence of "application fees"

Following the suspicion that the prevalence of corrupt practices could play a role, we use a list experiment to measure descriptively the prevalence of bribe payments (as described in Section 4).

Table 15 shows that on average, about 20 percent of the Old Age Allowance beneficiaries and 17 percent of the Widow Allowance beneficiaries reported having paid a bribe to be selected as beneficiaries suggesting that corruption does play a role for the targeting of social pension beneficiaries in Bangladesh.

Table 15: Payment of application fee

	(1) N Activities OAA	(2) N Activities OAA	(3) N Activities WA	(4) N Activities WA
Veiled list	0.199***	0.192***	0.169***	0.164***
	(0.000)	(0.000)	(0.002)	(0.003)
Covariates	Yes	No	Yes	No
Control group mean	3.53	3.53	3.50	3.50
N	1856	1856	1202	1202

Notes: Dependent variable is number of activities completed when applying for allowance. We control for baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

6 Conclusion

Our results demonstrate that the state-capacity building intervention — complete and partial — does, on average, not improve the targeting of social pensions. This is despite a couple of additional findings, which show that the intervention does influence knowledge and selection practices. One year after the intervention, we find a significant impact on the knowledge of the eligibility criteria among both selectors and beneficiaries. We also observe that most of the newly selected beneficiaries filled in an EIC and that the newly selected beneficiaries with EIC are poorer than those without, when poverty is measured using the assets listed on the EIC. This suggests that many selectors do use the EICs but not for all selections.

While our study does not allow us to directly identify why the targeting performance does not improve, we find suggestive evidence. We document that even after having received our training and the data through the eligibility information cards, major capacity constraints remain, and corruption appears to play a role, too. The targeting improvements in areas with more honest selection committees suggests an important link between honesty and the willingness to improve targeting of social transfers.

Further, we uncover that the complete intervention improved targeting in areas with selection committees headed by a highly educated UP Chairman. This suggests that the education of people in leadership roles is essential to reap benefits from training and other capacity-building interventions. At the same time this points at a specific type of capacity constraints, namely the education levels of local government representatives, that the here evaluated intervention could not address.

Taking together our results, future research would benefit from accounting for capacity constraints in various dimensions and multiple levels while also addressing the intrinsic motivation of individuals in charge. Further studies in this direction could help enormously to improve the effectiveness of social transfers such as Bangladesh's Old Age Allowance to eradicate extreme poverty in the future.

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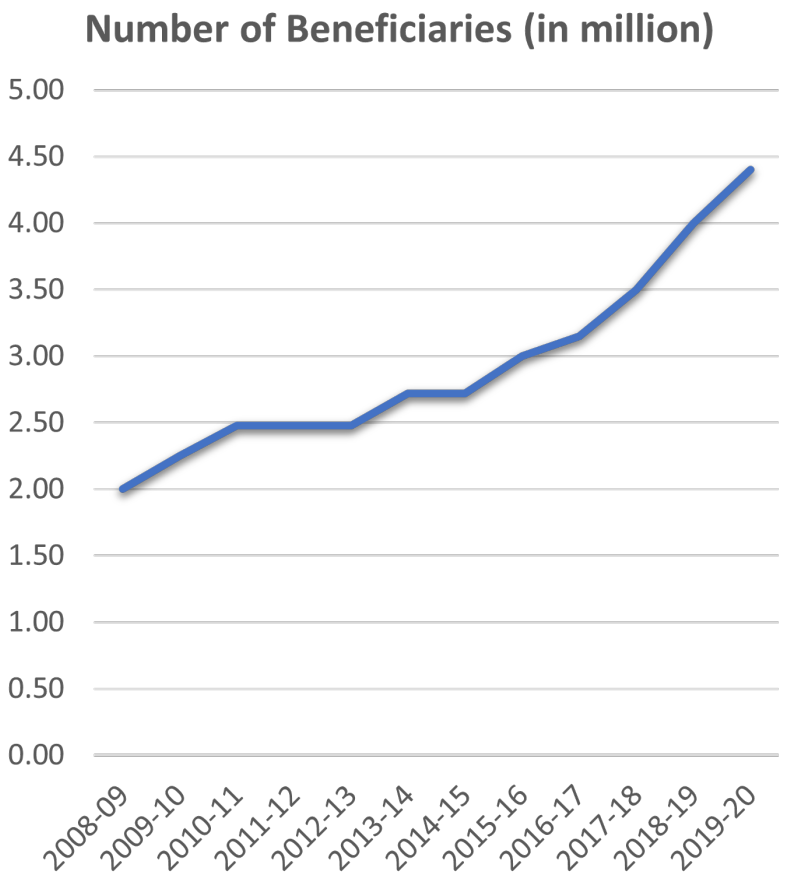
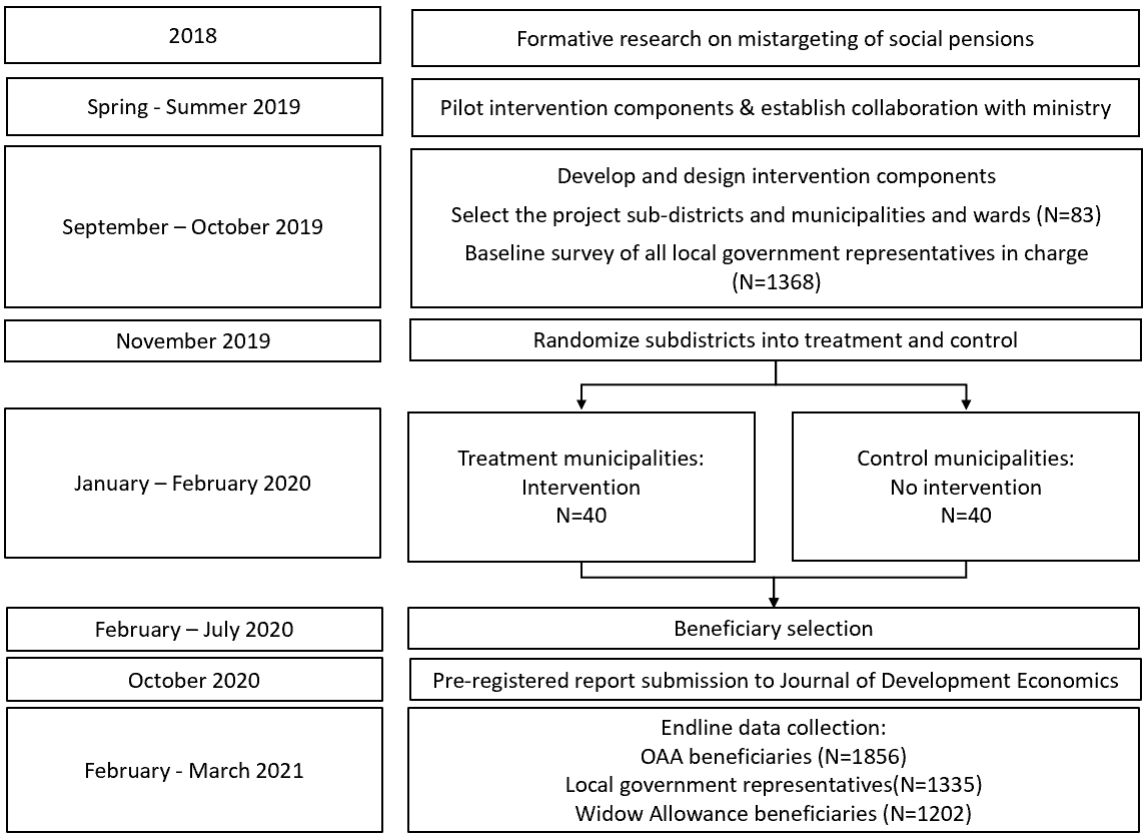
A Background information - Old Age Allowance

Figure A1: Old Age Allowance number of beneficiaries



Source: Department of Social Services (2020).

B Timeline


2018
Formative research on mistargeting of social pensions
Spring - Summer 2019
Pilot intervention components & establish collaboration with ministry
September – October 2019
Develop and design intervention components
Select the project sub-districts and municipalities and wards (N=83)
Baseline survey of all local government representatives in charge (N=1368)
November 2019
Randomize subdistricts into treatment and control
January – February 2020
Treatment municipalities: Intervention N=40
Control municipalities: No intervention N=40
February – July 2020
Beneficiary selection
October 2020
Pre-registered report submission to Journal of Development Economics
February - March 2021
Endline data collection:
OAA beneficiaries (N=1856)
Local government representatives(N=1335)
Widow Allowance beneficiaries (N=1202)

C Detailed intervention description

Table C.1: Detailed description of both intervention components

Item No.	Component 1: Training	Component 2: Data provision
1. Brief Name		
1.1 Write the name	Training Old Age Allowance selection committee members on the beneficiary selection criteria and procedures	Filling Eligibility Information Cards for the elderly and transferring the same cards to the Old Age Allowance selection committees.
2. Why?		
2.1 Describe rationale	As many local representatives in charge of selecting new beneficiaries for the Old Age Allowance are not informed about the national guidelines for eligibility, ARCED Foundation in collaboration with the Department of Social Services provided a training program. The objective was that in the future, local representatives can apply the guidelines learnt in the training and will select the most vulnerable among the elderly.	To make a systematic selection of beneficiaries, union selection committee members lack data on the people in the target group. Filling out Eligibility Information Cards and transferring these EICs to the union selection committee has the primary purpose of informing selection committee members about the elderly in the target group of the Old Age Allowance. The Eligibility Information Card contains all the relevant information on an elderly person that needs to be considered to make a systematic selection of beneficiaries in the area.

Table C.1: Detailed description of both intervention components

Item No.	Component 1: Training	Component 2: Data provision
3. What?		
3.1 Materials	Tablets with videos stored on them, training protocol to review content, quiz, boxes, example eligibility information cards, handout **Description of video plot**: The video starts by explaining the objective of the Old Age Allowance. Afterwards, it shows a UP Chairman sitting together with the other Old Age Allowance beneficiary selection committee members. As the chairperson of the committee, he reads aloud a letter from the Ministry of Social Welfare. The committee is requested to select new beneficiaries for the Old Age Allowance. The UP Chairman is shown thinking about the challenge of selecting a few elderly among many poor elderly and is then shown using the newly introduced Eligibility Information Card and three colored boxes in green, yellow and red to make a systematic selection of beneficiaries jointly with the other members of the Old Age Allowance Committee. The selection is then approved by the upazila level committee and the video shows newly selected beneficiaries collecting their pension benefits.	Old Age Allowance Information Cards, calculator for land and income calculation, tablet with app for age calculation, waiting numbers for managing the crowd.

Table C.1: Detailed description of both intervention components

Item No.	Component 1: Training	Component 2: Data provision
3.2 Procedures	Steps as presented in the protocol for trainers: **Step 1:** Meet Upazila Social Service Officer, Union Social Worker and UP Chairman **Step 2:** Meet the UP Members of the wards where EIC filling will take place. Identify a suitable venue for EIC filling. **Step 3:** Make appointments with the selection committee members for training. **Step 4:** To prepare EIC filling, communicate with the miking vendor **Step 5:** To prepare EIC filling, monitor miking before the field officers for EIC filling arrive. **Step 6:** Provide training to the selection committee members **Step 7:** Handover all relevant information to the EIC filling team	Steps as presented in the protocol for filling EICs: **Step 1:** Confirm preparations with trainer over the phone **Step 2:** Receive detailed information from trainer **Step 3:** Meet UP Chairman and UP Secretary **Step 4:** Meet the three UP Members from selected wards **Step 5:** Monitor miking **Step 6:** Confirm table and chair arrangement **Step 7:** Visit locations for EIC filling and ensure local support **Step 8:** Set up the booth **Step 9:** Welcome all attendees and manage waiting people **Step 10:** Fill ICs *Page 1:* Identifying information *Page 2:* Other benefits from government or NGOs (ineligibility criterion), eligibility criteria incl. having national ID, birth certificate, being a permanent resident of the union, being at least as old as the eligibility age (62 years for females and 65 years for males) and household's annual per capita income of less than 10'000 BDT. *Page 3:* Priority criteria namely physical condition, age of the elderly, economic and social condition *Page 4:* Household ownership of durable assets, housing materials, electricity, bank account, signatures by field officer and elderly person. **Step 11:** Make photocopies for Dhaka office **Step 12:** Hand-over ICs to the Union Secretary

Table C.1: Detailed description of both intervention components

Item No.	Component 1: Training	Component 2: Data provision
4. Who provided?		
4.1 Intervention provider	Trainers - Master graduates primarily from Social Sciences - Competitive selection process, with interview and mock-test - Final selection was done after observing performance during field practice - Important skills: able to communicate clearly, able to explain rules, very good manners when dealing with local representatives, previous field experience - All male trainers due to the requirement of travelling alone and frequently and also after sunset at times. - Received 5 days long training including two field practice days. - During field practice days, trainers trained local representatives and were observed by the monitoring team for feedback and selection of supervisors.	Field officers for EIC filling - BA/MA degrees - Long working experience in data collection - Patient with elderly respondents - All male trainers due to the requirement of travelling frequently and also after sunset at times. - Received 3 days long training including one day field practice. - During field practice, filled EICs for elderly, transferred EICs to UP Secretary and were observed by the monitoring team for feedback and selection of supervisors.

Table C.1: Detailed description of both intervention components

Item No.	Component 1: Training	Component 2: Data provision
5. How?		
5.1 Mode of delivery	One-on-one training using 10 videos that explain the guidelines to select the beneficiaries for the Old Age Allowance following the protocol (see the video plot below). Training also includes using two sets of pre-filled Information Cards as samples. The first set was used during training in order to ensure that the trainee understands the concepts properly. The second set was used as a practical exercise in the end of the training.	Field officers fill out the Eligibility Information Cards for the elderly. They give the original to the elderly person, fill out one copy and transfer it to the selection committee to provide data on the elderly in the target group to the selection committee members.
6. Location		
6.1 Location	40 unions in rural areas of Rangpur and Rajshahi divisions in the Northwest of Bangladesh. Each union is located in a different upazila. The training typically took place either at the Union Parishad Office or at the home of the local representatives. As per protocol, trainers were not allowed to give the training in the open and had to send curious observers away to mitigate any distraction from the training.	40 unions in rural areas of Rangpur and Rajshahi divisions in the Northwest of Bangladesh. Each union is located in a different upazila. Filling of EICs took place in 120 wards (3 per union). Field officers were requested to select easily accessible public places in the randomly selected wards at which people of any gender and religion would feel comfortable. Commonly selected places were primary schools.

Table C.1: Detailed description of both intervention components

Item No.	Component 1: Training	Component 2: Data provision
7. When and how much?		
7.1 Number of sessions, schedule, duration, intensity, dose	Each selection committee member received the same training individually. Each training lasted between 40 and 90 minutes but most trainings took 60 minutes. At the end of the training the local representatives received a hand-out summarizing the training content	Filling of EICs took place once on a prescheduled date from 8 am in the morning until 6 pm in the evening.
8. Tailoring		
8.1 Was the intervention personalized or adapted?	The content of the training was the same for every training participant, but trainers offered to show a video again and gave more time to people who needed more time to understand or had more questions.	Field officers always requested the same information from the elderly but they were trained to collect all the information in a rather conversational mode and to give more time to elderly who struggled to speak or hear. In other cases, for instance when adult children provided all relevant information on the elderly parent, they were able to proceed faster but also sticked to the conversational mode.
9. Modifications		
9.1 Was the intervention modified?	No modifications.	No modifications.
10. How well?		
10.1 Delivery of intervention	The intervention was delivered as planned.	The intervention was delivered as planned.

D Description of indices

D.1 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?

D.2 Eligibility index

According to the implementation manual 2013, there are ineligibility, eligibility and priority criteria to select beneficiaries for the Old Age Allowance (OAA). A person is ineligible for OAA 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 OAA, 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 OAA, 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**
$65 \leq$ age ≤ 69	1
$70 \leq$ age ≤ 75	2
age ≥ 76	3

For female elderly	
Rule	**Score**
$62 \leq$ age ≤ 66	1
$67 \leq$ age ≤ 72	2
age ≥ 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 $\leq$ land ownership $\leq$ 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.3 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?

D.4 Knowledge index - Beneficiaries

During endline-data collection, the beneficiaries 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 beneficiary'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

E Background: Regression results

Table E1: Rating of profiles

	(1) All profiles	(2) All profiles	(3) Male profiles	(4) Male profiles	(5) Female profiles	(6) Female profiles
2 years older than cutoff	9.82***	9.76***	9.97**	9.59***	3.54	3.61
	(0.000)	(0.000)	(0.001)	(0.000)	(0.216)	(0.113)
5 years older than cutoff	10.06***	10.00***	12.18***	11.89***	3.99	3.96
	(0.001)	(0.000)	(0.000)	(0.000)	(0.222)	(0.085)
65 BDT per day	-1.14	-1.09	0.16	0.18	-2.37	-2.34
	(0.414)	(0.494)	(0.932)	(0.933)	(0.169)	(0.311)
40 BDT per day	2.29	2.15	3.06	2.73	1.58	1.55
	(0.193)	(0.174)	(0.135)	(0.216)	(0.456)	(0.496)
Lives with spouse	2.21	2.05	4.71*	4.58*	-0.22	-0.37
	(0.183)	(0.190)	(0.029)	(0.032)	(0.909)	(0.868)
Lives alone	8.22***	8.24***	4.93**	5.03*	11.44***	11.42***
	(0.000)	(0.000)	(0.007)	(0.019)	(0.000)	(0.000)
Can work with difficulties	9.27***	9.02***	11.48***	11.21***	7.12**	7.08**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)	(0.002)
Cannot work	22.03***	21.89***	23.38***	23.36***	20.72***	20.65***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Avg. prediction of Y	60.7	60.7	62.0	62.0	59.4	59.3
Adj. R-squared	0.13	0.33	0.14	0.36	0.12	0.31
F-stat	13.39	16.77	14.78	11.59	14.44	9.73
P-value of F-stat	0.00	0.00	0.00	0.00	0.00	0.00
SE clustered by respondent	Yes	No	Yes	No	Yes	No
Respondent fixed effects	No	Yes	No	Yes	No	Yes
Controlling for sequence	Yes	Yes	No	No	No	No
Observations	1384	1384	687	687	697	697

Notes: Dependent variable is the eligibility rating ranging from 0 to 100. We estimate a linear model using ordinary least squares. Number of observations is equal to number of local representatives that participated in the rating task (n=77) times the number of profiles being rated (n=18). Each local representative rated 18 profiles. Attrition was relatively low, only 6 percent for the profile that received the lowest number of ratings. This regression analysis was described in our pre-analysis plan. Source: UP Chairmen and UP Members from the local government survey (n=77) * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table E2: Does the relevance of personal connections change with the UP Member's dishonesty?

	(1)	(2)
Connection member	0.121***	0.112**
	(0.031)	(0.040)
Connection member X dice score	-0.009	
	(0.030)	
Dice score standardized	0.022	
	(0.022)	
Connection member X dishonest		-0.006
		(0.059)
Dishonest		0.046
		(0.045)
Avg. prediction of Y	0.344	0.344
Adj. R-squared	0.065	0.064
Union fixed effects	Yes	Yes
Observations	1029	1051

Notes: Dependent variable is social pension receipt. We control for legitimate and other predictors of social pension receipt. Source: Beneficiary survey (N=362) and non-beneficiaries from general elderly survey (N=689) P-values are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table E3: Legitimate and other predictors of social pension receipt

	(1)	(2)	(3)	(4)	(5)
Age in 10 years	0.076***	0.075***	0.085***	0.085***	0.085***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
P.c. income in 1000 BDT	-0.015***	-0.015***	-0.015**	-0.015**	-0.015**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Asset index	-0.005	-0.004	-0.003	-0.003	-0.002
	(0.548)	(0.592)	(0.694)	(0.663)	(0.771)
Total land in 100 decimals	-0.032*	-0.032*	-0.035**	-0.034**	-0.035**
	(0.012)	(0.010)	(0.006)	(0.006)	(0.009)
Living alone	-0.025	-0.024	-0.028	-0.026	0.006
	(0.608)	(0.621)	(0.567)	(0.589)	(0.900)
Able to walk		0.103**		0.117***	0.111***
		(0.002)		(0.000)	(0.001)
Social capital			-0.028	-0.038	-0.038
			(0.394)	(0.246)	(0.250)
Connection representative			0.158***	0.162***	0.161***
			(0.000)	(0.000)	(0.000)
Same village			-0.001	-0.007	-0.006
			(0.974)	(0.835)	(0.858)
Kinship government official			-0.251***	-0.253***	-0.261***
			(0.000)	(0.001)	(0.000)
Kinship representative			0.057	0.001	-0.005
			(0.690)	(0.997)	(0.971)
Same political orientation			0.040	0.031	0.029
			(0.213)	(0.336)	(0.386)
Attended Ward Sabha meeting				0.152**	0.149**
				(0.005)	(0.006)
Avg. prediction of Y	0.344	0.344	0.344	0.344	0.344
Adj. R-squared	0.029	0.032	0.054	0.066	0.066
Union fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1051	1051	1051	1051	1051

Notes: Dependent variable is pension receipt. (1) includes all legitimate predictors as in pre-analysis plan. (2) replaces health index by individual indicators able to talk and able to see near not displayed. (3) includes legitimate and other predictors as described in pre-analysis plan. (4) replaces health index by individual indicators and adds attending ward sabha meetings as additional variable of interest. (5) includes control variables female and muslim. Source: Beneficiaries from beneficiary sample (N=362) and non-beneficiaries from general elderly sample (N=689). P-values are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

F Summary statistics

Below we present the summary statistics from the endline data collection - first of Old Age Allowance beneficiaries and second of Widow Allowance beneficiaries.

Table F1: 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 F2: 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				

G Dishonesty measure and its correlates

Figure G1: Number of matches reported in dice game - baseline

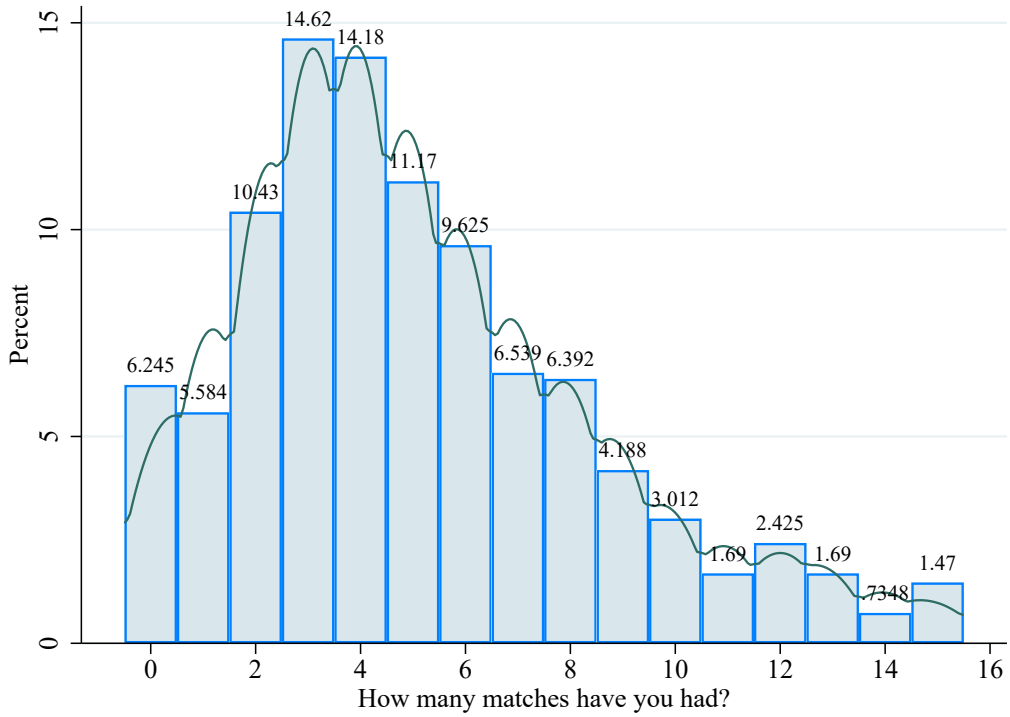


Table G1: Correlates of reported matches dice game

	(1) Matches reported	(2) Matches reported
Female	1.325***	1.321***
	(0.000)	(0.001)
Age (years)	0.014	0.014
	(0.196)	(0.194)
Highest level of education	-0.021	-0.026
	(0.502)	(0.421)
Household size	-0.069*	-0.074**
	(0.066)	(0.048)
Avg. monthly household income	-0.000	-0.000
	(0.473)	(0.580)
Total cultivable agricultural land owned (in decimal)	0.000	-0.000
	(0.934)	(0.907)
First time in office	0.397	0.422
	(0.153)	(0.126)
Total experience of respondent	0.012	0.013
	(0.214)	(0.178)
Ruling party	0.285	0.288
	(0.202)	(0.247)
UP Chairman	-0.255	-0.224
	(0.527)	(0.580)
UP Member	-0.068	-0.067
	(0.799)	(0.796)
UP Women Member	0.201	0.188
	(0.660)	(0.684)
Upazila extreme poverty HCR	0.014	0.076
	(0.442)	(0.184)
Observations	1361	1361
Avg. prediction of Y	5.074	5.074
Adj. R-squared	0.043	0.040
District fixed-Effects	No	Yes
F-stat	7.376	5.592
P-value of F-stat	0.000	0.000

Notes: We test for covariates that may correlate with the number of matches reported of the dice game. We control for baseline variables aggregated at the union level, upazila statistics in (1). We further include district fixed effects in (2). P-values are shown in parentheses. Standard errors are clustered at the union level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

H Further analysis

H.1 Impact on targeting: Alternative specification

Table H1: Impact on targeting - eligibility of new beneficiaries

	(1) Below national poverty line	(2) Below national poverty line	(3) Eligibility Index	(4) Eligibility Index
Panel A: Complete treatment vs. control				
Training and EIC	0.008	0.010	0.073	0.073
	(0.319)	(0.220)	(0.721)	(0.711)
N	1240	1240	1240	1240
Panel B: Partial treatment vs. control				
Only training	0.005	0.005	0.160	0.214
	(0.441)	(0.525)	(0.551)	(0.375)
N	1237	1237	1237	1237
Panel C: Any treatment vs. control				
Treated	0.008	0.008	0.118	0.133
	(0.251)	(0.249)	(0.594)	(0.510)
N	1856	1856	1856	1856
Covariates	Yes	No	Yes	No
Control group mean	0.200	0.200	1.514	1.514

Notes: Covariates include baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at the union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table H2: Impact on targeting in terms of income and land

	(1) Ind. income	(2) Ind. income	(3) Total land	(4) Total land
Panel A: Complete treatment vs. control				
Training and EIC	-155.684	-162.441	-15.465**	-12.676*
	(0.301)	(0.258)	(0.037)	(0.059)
N	1240	1240	1240	1240
Panel B: Partial treatment vs. control				
Only training	-102.643	-82.944	-8.519	-5.892
	(0.480)	(0.558)	(0.284)	(0.399)
N	1237	1237	1237	1237
Panel C: Any treatment vs. control				
Treated	-121.991	-120.317	-10.555	-8.840
	(0.345)	(0.331)	(0.142)	(0.178)
Covariates	Yes	No	Yes	No
Control group mean	1830.4	1830.4	46.5	46.5
N	1856	1856	1856	1856

Notes: Covariates include baseline variables aggregated at the union level, upazila statistics and district fixed effects. P-values are shown in parentheses. Standard errors are clustered at the union level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$