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Barriers to Public Pension Program Participation in a Developing Country

Tomoaki Tanaka* Junichi Yamasaki† Yasuyuki Sawada‡ Khaliun Dovchinsuren§

Abstract

Increasing public pension participation rates in developing countries is an important policy objective. We study three possible constraints to such participation by using a randomized control trial and the administrative records covering about 40 percent of Mongolian subdistricts. We find that providing information about subsidiary monetary benefits (survivors' and disability pensions) does not increase participation significantly. However, providing information about the mobile phone payment of pension funds and dispatching experts to a pension administrative agency from a foreign aid agency both increase payments. These results imply that perceived transaction costs and trust affect demand for pension services. They also suggest that foreign aid can affect citizens' participation in public services by changing their perception of these services.

Keywords: Randomized Control Trial, Pension, Mobile Phones, Foreign Aid, Information Provision, Mongolia

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1 Introduction

Developing countries are aging rapidly. From 1960 to 1990, the proportion of the population aged 65 years and above increased from 3.79 percent to 4.86 percent in low- and middle-income countries, but by 2018 it had increased to 7.43 percent (World Bank, 2017). Such a rapid change, together with economic growth and changes in family culture, has increased demands for better pension systems, inducing developing countries to undertake reforms (World Bank, 2015). However, developing countries face fundamental challenges in expanding their pension systems.[1] Since many workers in developing countries are self-employed, governments cannot deduct their contributions from income at source. Therefore, the government should incentivize people to participate in the pension system. However, it is a challenge to mobilize sufficient budget resources to make pension systems more attractive. Moreover, people might not understand the benefit of pensions well owing to limited information or a lack of cognitive skills. People in rural areas might also live far from pension offices, thereby incurring substantial transaction costs for participating in the pension system and paying contributions by visiting pension offices. In addition, people may be distrustful of the government and pension offices, resulting in non-participation in the pension program.

In this study, we conduct a large-scale randomized control trial covering 40 percent of the subdistricts of Mongolia to measure the effects of different types of informational frictions on participation in a public pension program for self-employed workers in that country. The pension is not mandatory, and the government is trying to increase the currently low participation rate. The majority of self-employed people in rural areas are herders and they typically live far from the district (*soum*) center where the pension offices are located. Our study covers eight provinces (*aimag*) and 643 subdistricts (*bagh*), or about 40 percent of subdistricts in Mongolia.

Based on the previous literature on related financial products such as annuity products and finan-

[1] In low- and middle-income countries, the average proportion of active contributors to the labor force (working population) is 36 (24) percent (World Bank, 2014).

cial transactions (Beshears et al., 2013, 2014; Jack and Suri, 2014), we hypothesize three reasons for the low participation rate: first, people do not properly understand the benefits; second, they find it costly to visit district centers to pay their contributions; and, third, they do not trust the government. To overcome these constraints, we randomly distributed three types of leaflets in the studied subdistricts to notify people that (1) the pension comes with disability insurance (the disability treatment), (2) they can pay their contributions by mobile phone (the mobile treatment), and (3) an international aid agency supports the pension system by dispatching experts (the trust treatment). We provided a basic explanation of the pension system for the three treatment groups as well as for a control group. The information is similar to that the government used in its past campaigns.

This distribution took place at regular subdistrict meetings, where we also conducted surveys based on a structured questionnaire to obtain meeting participants' socioeconomic backgrounds. After six months, we matched the data with pension records provided by the Mongolian government to analyze the effect of different types of information provision.

The results show that the mobile and trust treatments affect payment. The disability treatment increases the contribution payment rate five months after the experiment by 0.63–0.70 percentage points (pp), although it is not statistically significant. On the other hand, we find that the mobile and trust treatments significantly increase the contribution payment rate by 1.48–1.50 pp and 1.52–1.56 pp, respectively. Because the unconditional mean of the payment rate in the control group was about 15 percent and declined by about 2 pp from one month before the experiment, this is a sizable effect. We decompose it into the effects on the existing participants (intensive margin) and on those who had never participated (extensive margin) and find that the effect is larger for existing participants. Therefore, our information provision treatments seem effective, mainly for encouraging existing participants to continue making contributions. We do not find strong evidence of a threat to the external validity from those who do not participate in subdistrict meetings or geographical spillover effects. We investigate other types of heterogeneous effects and find that the mobile treatment is effective essentially only for people in remote areas with less chance of visiting district centers; this is consistent with the

transaction cost channel. Finally, we analyze adverse selection by looking at the correlation between longevity risk proxies and contribution payment, finding mixed evidence that our treatments induce adverse selection.

Our study is linked to the literature on savings commitment devices such as pension or commitment saving accounts. Many factors can affect such saving, including financial literacy as well as understanding of compounds (Lusardi and Mitchelli, 2007; Song, 2015), time inconsistency (Thaler and Benartzi, 2004; Ashraf et al., 2006), reminders (Karlan et al., 2016), default (Madrian and Shea, 2001), simplification (Beshears et al., 2013), limited attention (Goda et al., 2014; Crossley et al., 2017), and peer effects (Beshears et al., 2015; Kast et al., 2018). Using a large-scale randomized control trial with administrative data, we find that transaction costs and trust can play a role in such saving, which are novel findings in the literature.[2][3][4]

Second, the study is connected to the literature on foreign aid. Most prior studies focus on the reduced-form impact of foreign aid on economic and social outcomes (Boone, 1996; Burnside and Dollar, 2000; Easterly et al., 2004; Nunn and Qian, 2014; Galiani et al., 2017). Instead, we focus on a particular mechanism to explain how foreign aid will work: it might affect a citizen's perception of a development project assisted by foreign aid and change his or her behavior. A few studies find that by knowing about the presence of foreign financing in a development project, citizens of developing countries might change their beliefs about the project's quality or their government's legitimacy

[2]Another related strand in the literature focuses on demand for annuity products. This literature strand investigates the reasons for low demand for annuity products (the annuity puzzle), such as adverse selection (Finkelstein and Poterba, 2004; Rothschild, 2009; Hosseini, 2015), yield differences (Friedman and Warshawsky, 1990), unexpected health shocks (Poterba et al., 2011), bequest motives (Inkmann et al., 2011; Lockwood, 2012), and other behavioral biases such as framing (Brown et al., 2008; Agnew et al., 2008; Chetty et al., 2014; Song, 2015; Schreiber and Weber, 2016). Most empirical studies in this literature strand use survey data on a developed country, whereas we use administrative data on a developing country, as in Song (2015). The state pension is a hybrid of saving and annuity products. Whereas the abovementioned studies analyze whether people annuitize their accumulated wealth at their retirement, our finding about the trust treatment is relevant for annuity products, which already involve long-term contracts until their death.

[3]In developing countries, such subjective beliefs play an important role in many decisions such as technology adoption (Conley and Udry, 2010), health behavior (Dupas, 2011; Delavande and Kohler, 2012), educational investment (Attanasio and Kaufmann, 2009; Jensen, 2010; Kaufmann, 2014), and financial products (Cole et al., 2013).

[4]Particularly in the case of Mongolia, low population density and distance from the administrative center will increase transaction costs. It will also hinder the spread of administrative power in general, as in Africa (Michalopoulos and Papaioannou, 2013; Herbst, 2014). This study shows that mobile phone payment can mitigate such a problem and include people who used to be outside the public service's coverage. This is somewhat similar to Fujiwara (2015), who shows that the introduction of electronic voting technology in Brazil works as de-facto enfranchisement.

(Sacks, 2012; Dietrich and Winters, 2015; Winters et al., 2017; Dietrich et al., 2017). Unlike these studies, we find that knowing about the presence of foreign experts changes actual economic behavior (i.e., the pension contribution).

The remainder of this paper is organized as follows. Section 2 explains the institutional background and our experiment. In Section 3, we present the main results and other results about heterogeneity and adverse selection. Section 4 concludes.

2 Background and Data

2.1 Mongolian Public Pension Scheme for Self-Employed Workers

For self-employed workers in Mongolia, public pension payments can be the primary income source after retirement. Yet, only 15 percent of our sample pay their contributions at the baseline.

Beshears et al. (2013) find that the multidimensional aspect of complicated financial products such as annuity products is an important factor in explaining demand for such products. Jack and Suri (2014) find that transaction costs discourage financial transactions to cope with risk in a developing country setting. Beshears et al. (2014) also find a negative correlation between the concern about the company not being able to pay in the future (counterparty risk) and people's annuity choice. Based on these studies, we hypothesize three reasons for the low participation rate. First, self-employed workers might not understand the full benefit of the public pension because of its complexity. Specifically, the public pension consists of retirement, disability, and survivors' pensions, which account for 75.8, 17.9, and 6.3 percent of total recipients, respectively. Because of these smaller numbers of recipients of the disability and survivors' pensions, people might not be aware of these potential immediate benefits.

Second, self-employed workers have to pay the contribution at banks for at least 15 years to obtain the regular benefit of the pension,[5] for which the transaction cost will not be small, particularly for

[5]The contribution is 10 percent of the reported monthly income or the minimum wage if their reported income is lower than that. Therefore, in 2017, they had to pay at least 240,000 MNT (monthly minimum wage) * 0.1 = 24,000 MNT (about 9–10 US dollars) every month. This is about the market value of two sheep and will be affordable for average households

herders, as most bank branches are located in district centers far from herders' houses. Moreover, 16.8 percent of our sample visit district centers less than once a month and 70.7 percent visit less than once a week. While they can pay their contributions by using the short message service through the mobile banking system, they might not know the system, and their perceived transaction cost would be higher than the actual transaction cost.[6]

Third, since participation in the public pension system has a long-term investment horizon for individuals, they might have concerns about the ability and trustworthiness of the pension administration. To investigate the role of trust, we exploit a unique setting wherein most Mongolian citizens regard corruption as a major problem.[7] In particular, corruption has occurred in the social insurance administration of Mongolia, including embezzling of social insurance funds by officials, causing citizens' trust in social insurance to deteriorate. However, it is an empirical question as to how much these concerns would affect the actual public pension participation rate. To improve administration capacity in general, the Mongolian government officially asked the Japanese government for technical cooperation to improve its social insurance administration in 2014. As part of the response, the Japanese government sent experts to the Mongolian pension office.

Our setting has the advantage of being able to analyze demand for pension products. Since there are no pension products in the private market in Mongolia, we can observe the demand and its selection with certainty by looking at the administrative data of the public pension. In addition, the distance

outside Ulaanbaatar, who are estimated to have about 125 sheep according to official statistics (`http://1212.mn/stat.aspx?LIST_ID=976_L10_1&type=tables`). For those born after 1960, which is the majority of our sample, the amount of monthly benefit is based on a notionally defined contribution formula—the amount of total contribution they have paid plus its interest divided by the remaining life expectancy—with a minimum guaranteed amount. The minimum guaranteed amount is 20 percent of the national average wage and 0.5 percent of the average wage for each additional year they have contributed beyond the minimum of 15 years. Those born before 1979 can also choose a defined benefit scheme under which the minimum guaranteed amount is 45 percent of their highest monthly average reported income for the five years they have contributed continuously. This also increases by 0.125 percent of the five-year-average reported income for each month they contribute in addition to the 20 years.

[6]They basically have to pay the contribution every month, but they can also choose a lump-sum payment scheme of two months, three months, six months, or one year. For example, if they choose the two-month type, they can pay the contribution for January and February in January. If they do not pay the two-month contribution by the end of January, they have to pay an additional charge (30 percent of the contribution) to pay the contribution for February.

[7]According to the Public Opinion Survey of Mongolia (International Republican Institute, 2016), 75 percent of the respondents answered that corruption was a major problem, while 13 percent ranked it as a minor problem. In addition, corruption was ranked third in response to an open question asking what the three most important problems facing Mongolia were, after unemployment and poverty.

between the district center and houses[8] provides a natural setting to determine how a government of a less populated country can expand its service in rural areas.

2.2 Experiment and Data

2.2.1 Randomization

Mongolia has four regions and a capital city with three administrative layers: 21 provinces, 331 districts, and 1,619 subdistricts. Figure 1 shows our randomization design graphically. Given the budget constraint, we target eight provinces from the four regions for our study. We selected at least one province from each region so that the total targeted population would be similar across regions, as shown in Figure 2. We stratify the subdistricts in these provinces based on the province and herder population ratio, a proxy for rurality, to obtain enough power to analyze the heterogeneous impacts based on remoteness. Within each province, we split the subdistricts into two groups—a high herder population ratio group and a low herder population ratio group—based on the median of the herder population ratio in each province. In total, we have 16 strata (8 provinces * 2 groups), and within each strata we randomly assign treatments to each subdistrict, rather than at the individual level, considering that there would be spillover by sharing information within the same subdistrict. We can also analyze cross-subdistrict spillover by exploiting an exogenous variation of the neighboring subdistricts' treatment status.

2.2.2 Treatment Overview and Implementation

In collaboration with the General Authority for Social Insurance of Mongolia, we prepared four sets of leaflets (Figure 3). The control leaflet provides only general information, and the other three treatments add pages to the leaflet for the control group to provide additional information. Therefore, all groups received the general information, and by comparing the treated and control groups, we can estimate the pure effect of providing the specific information the treated group received.

[8]Mongolia is one of the least populated countries. Its population density is just 1.8 people per square kilometer.

After we randomly allocated the control and treatment status to each subdistrict, we delivered the relevant types of leaflets to each subdistrict thorough the pension office to avoid any contamination risk. Moreover, we prepared an instruction guide and videos for the subdistrict governors, our implementors, to help them understand our study's purpose as well as the survey procedure they are supposed to perform. The questionnaire survey to collect basic characteristics and distribution of leaflets took place in the regular subdistrict meetings. We allowed them to vary the exact timing of the survey and leaflet distribution from March to August, depending on the timing of the upcoming meeting. We also allowed the subdistrict governors to encourage people's participation by announcing this survey as an important event to increase the sample size. We discuss whether this affected the external validity of our finding in Section 3.3.

It was impossible to monitor the entire survey procedure to check their compliance, but we visited a subdistrict meeting to confirm compliance as well as prepared a checklist to confirm they follow the procedure. Based on the 501 checklists we received, less than 1 percent of subdistrict governors did not follow the procedure (i.e., they were not provided with instructions by the pension office or could not distribute leaflets to the participants). We did not receive checklists for the remaining 189 subdistricts, meaning that the lower bound of our compliance rate is about 72 percent. Therefore, even if we interpret our treatment effect as intention-treatment effect, the difference from the effect of information provision would not be so large.

2.2.2.1 Control group: general information

Control groups are provided with general information such as the content of public pension and the targeted contribution amount, which is similar to the information the General Authority for Social Insurance has distributed in past campaigns. While "providing no leaflets" is another option for setting up the control group, we do not adopt it for finer comparison owing to a contamination risk; as the subdistrict governors and pension office inspectors are incentivized to promote the public pension to residents, if some of them do not receive any leaflets while others do, they might redistribute

leaflets to each other without our permission, leaving our intervention contaminated. To reduce such a contamination risk, we distribute leaflets to all subdistrict governors in the target provinces.

2.2.2.2 "Disability" treatment of survivors' and disability pensions: providing information on subsidiary monetary benefit

The first intervention material overviews the Mongolian social security system, including other insurances such as insurance against employment injury, and it explains the survivors' and disability pensions intensively as parts of the pension insurance. Most people will be aware of the retirement pension, as it is the most prominent part of the social insurance program in Mongolia. However, fewer people will be aware that the insurance program automatically includes survivors' and disability pensions, because the number of beneficiaries of these benefits is much smaller than that of the retirement pension. The additional information on these pensions may increase demand for the public pension by increasing the subjective expected return of the public pension. The intervention may be effective for alleviating adverse selection. As several preceding studies (e.g., Rothschild (2009), Hosseini (2015)) suggest, individuals with shorter life expectation tend to avoid the retirement pension, since they will receive fewer benefits if they die earlier. Notably, in contrast to the retirement pension, individuals or their families may receive the survivors' or disability pensions before their retirement. Consequently, this leaflet would be more appealing to individuals with shorter life expectations, alleviating the adverse selection problem of the pension program.

2.2.2.3 "Mobile" treatment of mobile banking service: reducing the transaction cost

The second leaflet adds an introduction to the mobile banking payment services. Herders, who make up 74 percent of individuals eligible to participate in the public pension in our sample, usually live in rural areas and migrate within each subdistrict. Since they live far from district centers, the transaction cost they incur in going to bank branches to pay their contributions is relatively high. While mobile

phone payment would decrease such transaction costs, people might not be aware of such services.[9] Through emphasis on information about the mobile payment service, we expect to lower the perceived transaction cost and increase the participation rate. Moreover, the intervention would be more effective for those who live in areas further away, since they would have higher transaction costs. [10]

2.2.2.4 "Trust" treatment of Japanese officials' assistance to the public pension administration: fostering trust in the pension system

The third material adds an outline of Japanese officials' development assistance to the social insurance administration. The Japanese government has implemented a technical cooperation project to strengthen the administration of social insurance by dispatching Japanese experts from the Japanese ministry and the agency for the social insurance. The project aims to build citizens' trust in Mongolian social insurance through the success of the project. It is commonly called the "*SINRAI* (trust) project." By distributing this material to citizens in this study, we aim to alleviate their perceived risk of default or increase the perceived return of the public pension. This intervention would be effective for those who have more trust in Japan, since they would have positive attitudes toward the impact brought by the Japanese cooperation.

2.3 Data

After the allocation of the treatments, we conducted a questionnaire survey in 643 subdistricts and distributed four types of leaflets at subdistrict meetings.[11] We conducted the survey and provided the treatments only for people aged below 60 years for males and 55 years for females, because, as of 2017, these are the ages when people start receiving their retirement pension in Mongolia. The questionnaire asked about basic household characteristics, frequency of visits to district centers, and

[9] In our sample, 96.96 percent of respondents have mobile phones and 92.92 percent of respondents have bank accounts in their households.

[10] Mobile phone network coverage is broad in Mongolia and covers rural areas as well; rural dwellers do not have to come to district centers to access the mobile phone network.

[11] There are 690 subdistricts in the target regions in total. For the other 47 subdistricts, we could not conduct the survey and distribution because of operational problems such as no regular subdistrict meetings during our survey period.

subjective questions about the trustworthiness of Japan.

As we mentioned before, we use the public pension administrative data about participation and contribution payment for those who answered the survey. We merge the survey data with the administrative data based on national identification numbers, which we collected during the survey. The administrative data cover information about how many months of contribution were paid in each year from 2006 to 2017 as well as the payment history for each month in 2017.[12]

We obtained 29,024 responses, of which 6,747 were dropped due to the respondents not being eligible for the public pension, such as elders and paid workers. We further dropped 7,526 responses from respondents whose written national identification numbers are not valid (not 10 digits) or inconsistent with their written birthday or gender, because the national identification number is determined by this information.[13] In addition, we dropped respondents whose national identification numbers are duplicates of those of other respondents.[14] We also dropped people who received the treated material after August to see the effect five months after the experiment in our data, which only covers the period to the end of 2017. This reduced the sample size by about 7 percent.[15] Finally, we use 13,618 observations in our analysis.

As for randomization, we perform the balancing test using official statistics, such as population at the subdistrict level. Panels A and B in Table 1 show the results of the population-related variables and asset variables for herders; none of the variables shows systematic differences between the groups. In Panel C, we show the survey participation rate and total number of questionnaires we received, including the sample we dropped, as explained above, divided by the total population in each subdistrict.

[12]Our current data do not distinguish lump-sum payments from monthly payments (those who pay the contribution in both January and February and those who pay a two-month contribution in only January take the value of one in both January and February). This means that if some people already paid multiple-month contributions just before the experiment, their outcome variable automatically takes the value of one for several months after the experiment. We are requesting this multiple-month payment information, and, meanwhile, we set our main outcome variable five months after the experiment.

[13]If a man is born on January 2, 1987, his national identification number should be ##870102##), for example.

[14]These samples were dropped because of a wrong or duplicated ID are older by 1.01 years and less likely to have graduated from college (or higher) by 6.84 percent. We will discuss how this affects the generalizability of our main analysis later.

[15]About 2 percent of people answered that they were treated before March, but this is impossible because we started our experiment in March. We also dropped these people from our analysis.

We find that the survey participation rate was 5.66–6.38 percent per subdistrict and not significantly different between the groups. Note that, typically, only one household member attends the subdistrict meetings; thus, assuming there are three members in each household, about 16–19 percent of households per subdistrict attended the meetings. This is a plausible number because participation in subdistrict meetings is not mandatory and people may be far from subdistrict centers where the meetings are held.[16]

In Table 2, we perform the balancing test by regression analysis using our survey data.[17] Panel A compares the basic individual characteristics across the groups. We find that only the higher education dummy, which takes the value of one if the respondent completed at least secondary school, is significantly lower in the trust treatment group and jointly different; this might lead to bias in estimating the treatment effect. The past payment records in columns (5) and (6) do not show significant differences between the treatment groups and the control group. In the main analysis, we include the higher education dummy as a key control variable to remove potential selection bias and the past payment variables to obtain more precise point estimates.

In Panel B, we similarly regress other individual characteristics and other variables about how our treatment was implemented on the treatment dummies. Columns (1)–(5) show some significant differences at the 10 percent level in individual characteristics. The main specification does not control for these variables, because it drops a substantial amount of samples, but when we analyze the heterogeneous impacts, we can include controlling for these variables in the main specification. We allow the distribution and survey to be conducted at different timings depending on the subdistrict meeting schedule, but this might have affected the outcome directly through a seasonal income effect. Moreover, in some cases, subdistrict governors visited households to conduct the survey and distribution, which might have put physiological pressure on people to pay a contribution in response to such a

[16] We cannot include the payment information in this table because the administrative data do not contain address information. Therefore, we check the balancing test for the payment information only for the sample who participated in the subdistrict meeting, as shown in Table 2.

[17] See Table A.1 for detailed definitions of the variables.

visit. However, columns (6) and (7) show no differences in the timing or location of the survey across the groups. Overall, these results show that our randomization is performed successfully to estimate the average treatment effect at the national level.[18]

Although their low income will be a primary reason, information in general, transaction costs, and lack of trust also seem non-negligible reasons for their non-participation, which may be relatively easy causes for the government to tackle. We ask our sample members who do not join in the pension program for their reasons using multiple choice questions. We include five reasons as the multiple choices: lack of information, no need for the pension, the cost of traveling to pay the contribution, unaffordability, and other reasons, and they were chosen by 12.00, 6.14, 9.92, 56.13, and 8.16 percent of our non-joining sample, respectively. Moreover, we ask whether people think that the pension office will pay the benefit properly when they become eligible as recipients—11.63 percent of the non-participants answered "disagree a little" or "totally disagree."

3 Results

3.1 Main Result

First, we estimate the effect of each provision of leaflets according to the following specification:

$$\text{Outcome}_{ijkl} = \boldsymbol{D}_{jkl}\boldsymbol{\alpha} + \boldsymbol{X}_{ijkl}\boldsymbol{\beta} + \boldsymbol{\mu}_k + \boldsymbol{\nu}_l + \epsilon_{ijkl}, \tag{1}$$

where i is an individual, j is a subdistrict, k is a district, l is the strata, $Outcome_{ijkl}$ is the payment dummy five months after the experiment, $\boldsymbol{D}_{jkl}$ includes three sets of treatment variables, $\boldsymbol{\mu}_k$ indicates district fixed effects, and $\boldsymbol{\nu}_l$ indicates strata fixed effects. We take the payment dummy five months after the experiment as the treatment to observe the middle-term impacts of the distribution. In our

[18]We allow subdistrict leaders to distribute the leaflets at the beginning of the survey to avoid them forgetting this task. Although it is unlikely that participants read the leaflet before the survey, this might have allowed the treatment variables to affect several variables—the channel from the trust treatment to the Japan trust dummy, for example. However, Panel B of Table 2 shows that the risk of this occurring is minimal.

preferred specification, it includes a set of control variables ($\boldsymbol{X}_{ijkl}$) the higher education dummy and the payment record dummies in 2016 and one month before the experiment to remove the selection bias and obtain more precise point estimates, as in McKenzie (2012). We also include district fixed effects, because we ask the inspectors of district pension offices to deliver our material. ϵ_{ijkl} is clustered at the subdistrict level.[19]

Table 3 shows the results. In column (1), we use a plain vanilla specification only with strata fixed effects and treatment variables. We find positive but insignificant point estimates for the mobile and trust treatments. In column (2), we add payment variables before the experiment, thus reducing standard errors greatly, and we obtain significant results for the two treatments. By adding the district fixed effect, we similarly obtain more precise point estimates. Our preferred specification is column (4), where we add the higher education dummy that was not balanced in Table 2. In this specification, we again find significantly positive estimates of the mobile and trust treatments. The point estimates of these effects are 1.50 pp and 1.56 pp, respectively, and the coefficients and their significance are almost the same as those after controlling for other covariates in columns (5) and (6). Because the unconditional mean of the payment rate five months after the experiment is 0.15, the effect of the mobile (trust) treatment accounts for 10.00–10.40 percent of the mean. Moreover, the payment rate drops by 2.19 pp from one month before to five months after the experiment, implying that the mobile and trust treatments recover from such a drop by 68.49–71.23 percent.[20]

The result indicates that the improvement in respondents' subjective transaction cost and trust in the pension system changes their payment behavior. On the other hand, we do not find a significant effect of the disability treatment. This could be because our information provision does not change respondents' beliefs (they already knew of such an additional benefit), and the additional monetary

[19]When finding significant results, we also test the sharp null by randomized t-tests suggested by Young (2018) as a robustness check.

[20]When including the sample dropped because of their wrong or duplicated ID, the estimated effects fall by 0.3–0.5 pp as shown in A.2. This may be because the effects are heterogeneous and less educated people might have weaker effects, for example. However, the difference is attributed to the measurement error bias, because the dropped sample's payment record will be wrong. Since we use a binary variable as the outcome variable, ordinary least squares estimators have attenuation bias driven by such non-classical measurement error (Aigner, 1973).

benefit plays little role in the payment behavior.[21]

3.2 Extensive and Intensive Margins

As a further analysis, we separate the sample into two groups to observe the impact on the extensive and intensive margins. Although only about 17.3 percent of our sample paid the contribution in March 2017, about 55.2 percent paid at least once between 2006 and March 2017. This means that many people are not maintaining their contributions after participating in the public pension. Therefore, the natural question is how does the impact we find in Table 3 differ between the existing participants and those who had never participated.

Table 4 shows the results by splitting the sample into the existing participants and those who had never participated using the same specifications as in Table 3. It shows that the point estimates of the treatment effects are larger for the existing participants. For example, in columns (1)–(3), the effect of the disability, mobile, and trust treatments for the existing participants are 1.52 pp, 1.91 pp, and 2.54 pp, respectively. On the other hand, those for the people who had never participated are 0.06 pp, 1.03 pp, and 0.55 pp, respectively in columns (4)–(6), although the difference is statistically significant only for the trust treatment. Moreover, Table A.5 shows that the average effect is smaller for shorter terms, such as one month or three months after the experiment, using the same specification as in Table 3.[22] Overall, the treatments seem to increase the number of people who continuously pay contributions and who might stop paying without the treatment.[23]

[21] It is difficult to obtain their belief to separate these channels without changing their belief. For example, if we ask "Do you know that the public pension includes a disability pension benefit?" or "Do you think the public pension includes a disability pension benefit?" such questions would lead them to infer the existence of such benefit. Therefore, we do not ask these types of questions in our survey.

[22] This may be because some people have paid multiple-month contributions just before the experiment, and their payment variable automatically takes the value of one for several months after the experiment.

[23] We believe that these treatments are more cost-effective than increasing the monetary benefit from the pension to increase the contribution rate. It takes 0.22 US dollars per person to print and deliver the leaflets to provincial offices. Therefore, if we take the trust treatment as an example, it takes 0.22 US dollars to increase the probability of contribution by 1.52–1.56 pp. On the other hand, 0.22 US dollars is equal to just 0.27 percent of the monthly benefit of the retirement pension. Assuming that people will receive the retirement pension for five years, 0.22 US dollars is equal to 0.27/60 = 0.0045 percent. This is too small an amount for the additional monetary benefit to increase the contribution rate by 1.52–1.56 pp, even when we further include the delivery cost from provincial offices to subdistricts, which is estimated at about 0.06 US dollars based on the post office fee to deliver to district centers. Moreover, we can target existing participants who

The result implies that our treatments affect the intensive margin, and it has a clear policy implication. Because the government conducted a campaign in 2016 to increase the participation rate, the proportion of existing participants in our sample reached half. However, the perceived transaction cost and trust hinder participants from maintaining their contributions. Therefore, it would be better to improve these aspects to increase the number of active payers.

On the other hand, the effect for those who never participate (never participants, hereafter) is not relatively negligible. Indeed, if we compare the statistically significant effects of the mobile treatment in columns (1) and (4) divided by the unconditional mean of the outcome variables in the control group, column (4) shows a much larger effect (0.9115 = 0.0103/0.0113) than column (1) (0.0602 = 0.0191/0.3169). Therefore, in terms of the relative size, the mobile treatment has a large effect on never participants.

3.3 External Validity and Spillover

Our sample is self-selected in the sense that we only analyze people who participate in the subdistrict meeting. Because the governors may have encouraged people to join in the meetings by saying that they will make an important announcement about the pension system, the participants may be more interested in the pension system and more affected by information provision from the government. This could be a threat to the external validity of our analysis when extending the distribution to those people in the target provinces who do not participate in the meeting. If our concern is valid, the effect will be particularly high for people who do not participate in the meetings usually. We analyze this heterogeneous impact based on the attendance to the subdistrict meeting. In Table A.3, we do not find any statistically significant heterogeneity in the full sample or existing participants for each treatment in columns (1) and (2) or in our treatments overall in columns (3) and (4). Therefore, we will observe a similar amount of the effects for people who do not participate in the subdistrict meeting during the

have larger impacts to improve their cost effectiveness.

distribution. [24]

Another important question is whether our treatment effects spill over. To investigate such a spillover, we added the share of neighboring subdistricts allocated to each treatment group. Note that this is randomly allocated, because whether a subdistrict's neighboring subdistricts receive the disability treatment is randomly allocated as well. We employ two different definitions of neighbors—those within 20 km and within 50 km. Table A.3 shows the result using the full sample and existing participants. Overall, we do not find strong evidence of spillover in any specification, although the point estimate of the neighbor's trust and mobile treatments shows positive signs.

3.4 Heterogeneous Impacts

Table 5 shows regression results allowing heterogeneous impacts using the same specification of column (1) in Table 3. First, we interact a remoteness dummy variable that takes the value of one if the respondent visits district centers less than the average. On average, the people visit on 48.23 days a year, which is slightly less than every week. Because this variable has a substantial number of missing values,[25] we replicate the same regression analysis of column (1) in Table 3 by dropping samples whose remoteness dummy variable is missing. The result in column (1) is qualitatively similar to that in column (1) of Table 3, implying that we are looking at similar populations. In column (2), we interact the remoteness variable with the treatment variables. We find that only the effect of the mobile treatment is larger for people who are in remote areas, and the point estimates of the baseline effect show that all the effects of the mobile treatment come from these people. In columns (3) and (4), we use the existing participants only, and the qualitative pattern is quite similar to that in columns (1) and (2). This result is consistent with the hypothesis that the transaction cost channel would be stronger

[24] One concern of this analysis is that the attendance can indicate proximity to the government, and if people are closer to the government and trust the current pension system, the effect of informational provision may be stronger by believing in the information provided. This channel cancels out the selection problem above, and if this is true, the insignificant relationship between the attendance and treatment effects will not hold for people who did not participate in the meetings. However, the result does not change even after controlling for a trust measure of the pension system and its interaction term with the treatment variables. We omit the table for brevity.

[25] The reason for this low response rate will be that we had asked them about the frequency for each season, which was a lengthy question.

for people who have less opportunity to visit district centers.

In columns (5)–(8), we conduct the same type of analysis as in columns (1)–(4), using a dummy variable that takes the value of one if the respondent has higher subjective trust in Japan than the average. The coefficients for the interaction term between the trust treatment and the Japan trust dummy are positive (0.88 pp and 3.88 pp in columns (6) and (8), respectively), although they are not statistically significant for the full sample and marginally insignificant for the existing participants. In addition, the point estimates of the other interaction terms have similar magnitudes. These results imply that the trust measure is correlated with factors that increase the effect of other treatments, and thus it is difficult to identify the true heterogeneity of the trust indicator.[26][27]

Another finding is that restricted sample participants who can answer the question about trust are more affected by the treatments than those in the full sample are. If the results in columns (1) and (5) are compared with those in column (1) of Table 3, the point estimates are larger in most cases. In addition, if the sample is restricted to the existing participants in columns (3) and (7), the effect of the disability treatment becomes larger and statistically significant. This result may be because those who could answer those questions have higher cognitive skills and are more likely to respond to new information.

3.5 Adverse Selection

Adverse selection may be a key mechanism for the lack of annuity products in the private market: only people with longer life expectancy demand such products, and annuity products would not be profitable for private firms (Rothschild, 2009; Hosseini, 2015). Our treatments increase the contribution

[26]This might be also because people's perception about Japanese experts is different from their perception about Japanese people or Japan in general, which we asked in the questionnaire.

[27]We also analyze the heterogeneous effects of the trust treatment based on distrust of the pension office. If the information of foreign aid substitutes the distrust of the pension system, people who have such distrust will experience a bigger impact of the trust treatment. However, we do not find such heterogeneous impacts (results are available upon request). This may be because, for some people, the information of foreign aid has complementarity with trust in the pension system. For example, they might perceive foreign aid as an input to increase the return of pension funds, and that such increased return will benefit the recipients only if the pension system is politically stable or is not corrupt. Eliciting different aspects of trust and investigating the interaction among such aspects is left for future research.

payment but might attract higher risk people, which would affect the financial structure of the public pension system.

We use three measures as a proxy of longevity. The first measure is a dummy variable that takes the value of one if the respondent has shorter life expectancy than the gender-wise average, which is 66 years for males and 76 years for females. The second measure is the experience of negative health shock, such as hospitalization and accidents. The third measure is unhealthy behavior, such as drinking and smoking. For the latter two variables, we take an average of the z scores for each component respectively —experience of hospitalization and accidents and frequency of drinking and smoking—and normalize this again.

We estimate the following models:

$$\begin{aligned}
&\text{Contribution one month before the experiment}_{ijkl} \\
&= \alpha_0 \text{proxy of longevity}_{ijkl} + \boldsymbol{\mu}_k + \boldsymbol{\nu}_l + e_{ijkl}, \\
&\text{Contribution five months after the experiment}_{ijkl} - \text{Contribution one month before the experiment}_{ijkl} \\
&= \alpha_1 \text{proxy of longevity}_{ijkl} + \text{proxy of longevity}_{ijkl}\boldsymbol{D}_{jkl}\boldsymbol{\beta}_1 + \boldsymbol{D}_{jkl}\boldsymbol{\beta}_2 + u_{ijkl}.
\end{aligned}$$

α_0 indicates the adverse selection at the baseline, and if some of our treatments worsen the adverse selection, some of $\boldsymbol{\beta}_1$ becomes significant.

Table 6 shows the results. Columns (1) and (2) show how the correlation between shorter life expectancy and payment of contribution changes before and after the experiment across treatment groups. We observe the opposite pattern from adverse selection before the experiment in column (1). We also find the treatments improve the adverse selection in column (2). When we use negative health shock as a proxy for longevity in columns (3) and (4), we find no adverse selection at the baseline, but the mobile treatment significantly worsens the adverse selection (people who have negative health shock by one standard deviation are less likely to join the pension program by 2.79 pp). In column (5), we find adverse selection that people who smoke and/or drink do not demand public pension at the

baseline. In column (6), the coefficient becomes less steep in the control group, but it becomes steeper in the treatment groups (people who have unhealthy behavior by one standard deviation are less likely to join the pension program by 1.09 pp in the disability treatment), although these are not statistically significant. Considering the low contribution rate (about 15 percent), these results might suggest that non-negligible adverse selection occurs because of our treatments. Overall, we find mixed evidences for adverse selection.

4 Conclusion

To study the reasons behind low demand for public pension in the context of developing countries, we examine a Mongolian case using a large-scale randomized control trial. Based on the literature on the related financial products, we hypothesized that the reasons for low demand include people not being fully aware of the benefits of public pension, the high transaction costs involved in paying contributions, and a lack of trust in the pension system. To quantify these barriers, we randomly assign 643 subdistricts to three treatment groups and one control group. In addition to general information provided to the control group, we provide information on disability and survivors' pensions, mobile payments, and the dispatch of experts from foreign aid donors to the pension administrative agency for the three treatment groups.

By using administrative records, we find that in the mobile and trust treatments, people are more likely to pay the contribution five months after the experiment. The effects of these treatments are 10.00–10.40 percent of the unconditional mean of the outcome, or 68.49–71.23 percent of the drop in the payment rate from one month before to five months after the experiment, implying substantial effects. We do not find a significant effect from the disability treatment. The effect of the mobile and trust treatments are larger for existing customers, implying that the treatments affect the intensive margin of pension participation. In addition, people who are in remote areas experience larger effects from the mobile treatment, as the transaction cost channel predicts. We find a mixed evidence that our

treatments worsen adverse selection.

The results imply that trust and perceived transaction costs constrain demand for an annuity product, which is a novel finding in the literature. Such constraints would be more binding in developing countries because of the governance quality and level of financial development. Therefore, governments of developing countries experiencing population aging and trying to increase pension coverage will need to ease these constraints.

Moreover, our results shed light on the mechanism of the effect of foreign aid. We show that participation in public services by citizens in developing countries can be changed through knowledge about the presence of foreign assistance to the service. Such a channel has been under-investigated by the prior literature. The results also suggest a policy intervention regarding information provision about the donor to make foreign aid work if the assisted project needs citizens' participation. However, this effect will depend on how citizens feel about the donor or consider how the donor interacts with their government, which would demand a future study.

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Figure 1: Randomization Design

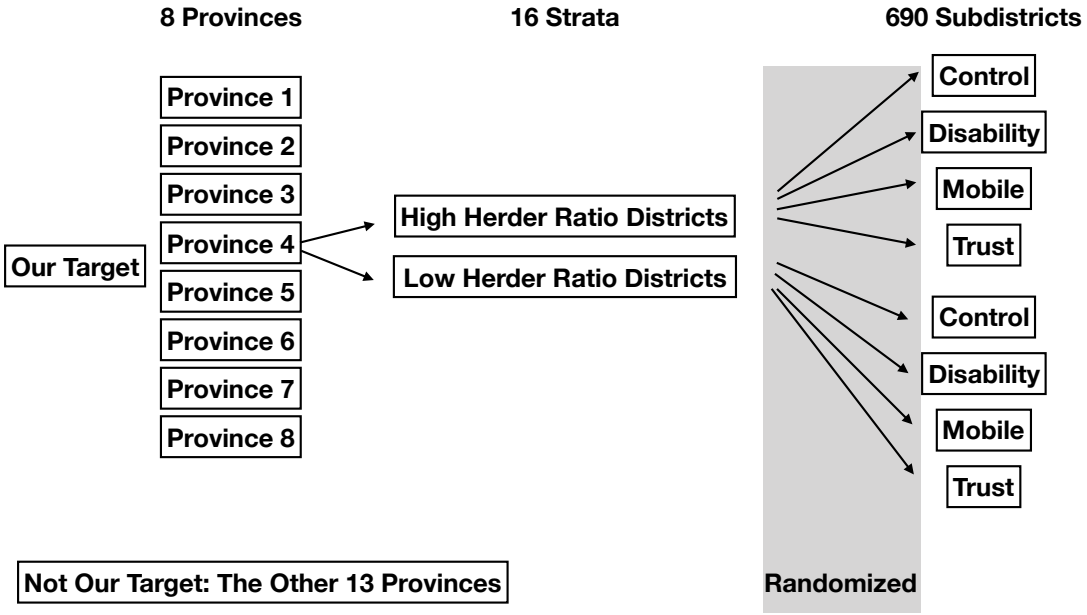
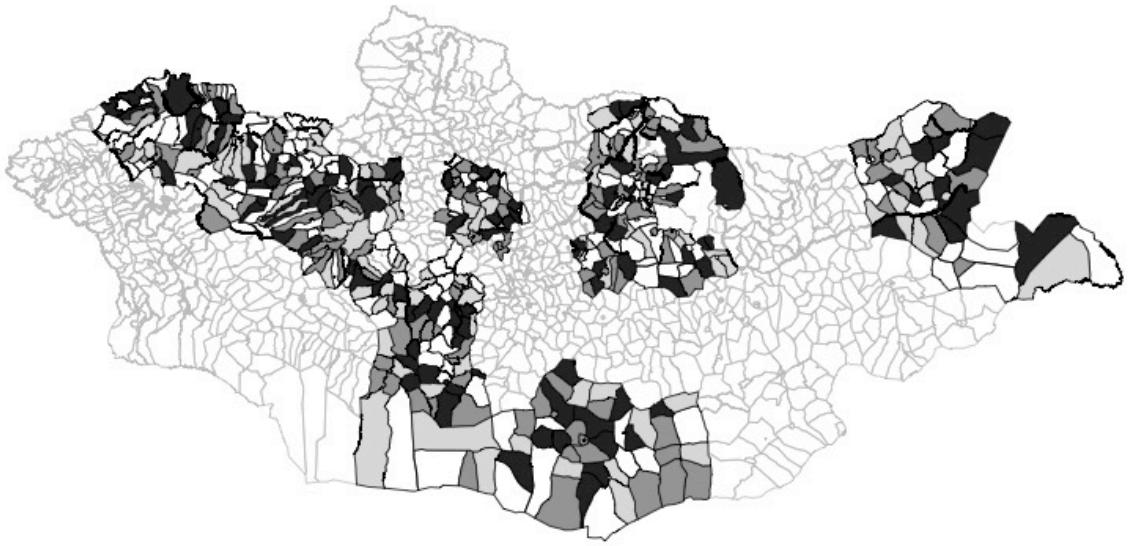


Figure 2: Map of Districts



Notes: Our sample include the subdistricts with black borders. White is control group, gray is the disability treatment group, darker gray is the mobile treatment group, and black is the trust treatment group. See subsection A.1 for details of the source and data construction.

Figure 3: Leaflets

(a) Control

(b) Disability

(c) Mobile

(d) Trust

Notes: We provide the leaflet in Panel (a) to the control group. For the treatment groups, we print Panel (b) for the disability treatment, (c) for the mobile treatment, and (d) for the trust treatment groups, respectively, on the back of the leaflet in Panel (a). See Figure A.1 for the English version.

Table 1: Balancing Test Using Official Statistics

Panel A: *Population*	Control	Disability	Mobile	Trust	p-value Joint F-test
Population	965.41	1,006.49	930.61	912.49	0.79
	(69.30)	(78.21)	(70.61)	(62.58)	
Number of Households	277.59	301.76	275.66	291.65	0.79
	(18.96)	(22.64)	(18.66)	(22.35)	
Herder Population	179.08	180.95	188.26	174.34	0.74
	(8.71)	(9.62)	(9.04)	(8.39)	
Herder Population Ratio	0.29	0.29	0.30	0.28	0.77
	(0.01)	(0.01)	(0.01)	(0.01)	
Herder Household Ratio	0.51	0.51	0.52	0.49	0.81
	(0.03)	(0.03)	(0.02)	(0.03)	
Panel B: *Assets of Herder Households (Number)*					
Horse	2,154.45	2,069.78	2,263.39	1,872.15	0.19
	(144.53)	(130.21)	(142.92)	(109.74)	
Cattle	2,542.35	2,527.42	2,689.75	2,421.53	0.78
	(169.97)	(184.72)	(203.93)	(172.65)	
Camel	238.09	283.10	250.78	383.02	0.45
	(53.15)	(77.41)	(49.45)	(93.07)	
Sheep	15,219.17	15,210.90	16,473.66	13,747.58	0.21
	(956.30)	(904.26)	(996.64)	(779.57)	
Goat	14,047.46	14,049.23	15,172.95	13,433.65	0.53
	(871.83)	(866.66)	(897.04)	(732.67)	
Television	78.98	81.71	84.44	74.58	0.32
	(3.99)	(4.14)	(3.91)	(3.64)	
Mobile Phones	104.58	105.13	110.24	96.13	0.27
	(5.00)	(5.58)	(5.09)	(4.88)	
Panel C: *Participation*					
Survey Participation Rate (percent)	5.66	5.88	6.38	5.85	0.47
	(0.36)	(0.31)	(0.33)	(0.32)	
Observations (N of Subdistricts)	173	168	179	170	

Source: The National Statistics Office of Mongolia (`www.1212.mn`).

Notes: *Survey Participation Rate (percent)* is the total number of our sample in the subdistrict divided by total population of the subdistrict, that is, *Total Number of Participants Including the Sample We Drop*/*Total Population* $*$ 100. Each column reports the mean of each variable at the subdistrict level. Robust standard errors are in parentheses. The last column reports the results of joint orthogonality tests on the treatment arms with the p-value. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

Table 2: Balancing Test Using Survey Data

Panel A	Age	Female	Higher Education	Herder	Any Payment in 2016	Payment 1M before Experiment
	(1)	(2)	(3)	(4)	(5)	(6)
Disability	0.0607	-0.0166	-0.0264	0.0180	0.0159	0.0012
	(0.3305)	(0.0154)	(0.0171)	(0.0235)	(0.0148)	(0.0114)
Mobile	0.0988	-0.0120	0.0024	0.0120	-0.0030	-0.0044
	(0.3165)	(0.0168)	(0.0181)	(0.0280)	(0.0159)	(0.0131)
Trust	0.0853	-0.0074	-0.0481***	0.0448*	0.0129	-0.0093
	(0.3413)	(0.0169)	(0.0185)	(0.0246)	(0.0164)	(0.0139)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,529	13,525	13,156	13,529	13,529	13,529
R^2	0.041	0.062	0.094	0.356	0.067	0.058
Join F-Test p-value	0.9911	0.7428	0.0255	0.3295	0.5432	0.8357

Panel B	Remoteness Dummy	Negative Health Shock	Unhealthy Behavior	Expected Longevity Dummy	Japan Trust Dummy	Treatment Month	Treatment at Home
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Disability	-0.0115	0.0127	0.0278	-0.0015	-0.0024	-0.0487	-0.0188
	(0.0261)	(0.0159)	(0.0257)	(0.0208)	(0.0216)	(0.0572)	(0.0270)
Mobile	-0.0115	-0.0086	0.0372	-0.0060	0.0187	-0.0201	-0.0435
	(0.0279)	(0.0163)	(0.0262)	(0.0205)	(0.0204)	(0.0512)	(0.0302)
Trust	-0.0553*	-0.0010	0.0054	-0.0232	0.0097	0.0039	-0.0316
	(0.0290)	(0.0165)	(0.0275)	(0.0225)	(0.0216)	(0.0552)	(0.0279)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,869	11,375	11,281	8,699	10,132	13,529	13,374
R^2	0.135	0.070	0.050	0.076	0.088	0.703	0.288
Joint F-Test P-Value	0.2869	0.6439	0.4422	0.7498	0.7136	0.8030	0.4955

Notes: Standard errors in parentheses are subdistrict-level cluster robust. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

Table 3: Main Result

	Payment Five Months After the Experiment					
	(1)	(2)	(3)	(4)	(5)	(6)
Disability	-0.0031	0.0034	0.0066	0.0063	0.0069	0.0070
	(0.0128)	(0.0055)	(0.0049)	(0.0050)	(0.0051)	(0.0051)
Mobile	0.0103	0.0125**	0.0147***	0.0150***	0.0149***	0.0148***
	(0.0133)	(0.0056)	(0.0052)	(0.0053)	(0.0055)	(0.0054)
Trust	0.0133	0.0108*	0.0163***	0.0156***	0.0154***	0.0152***
	(0.0148)	(0.0058)	(0.0054)	(0.0055)	(0.0055)	(0.0056)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	No	No	Yes	Yes	Yes	Yes
Payment One Month Before	No	Yes	Yes	Yes	Yes	Yes
Any Payment in 2016	No	Yes	Yes	Yes	Yes	Yes
Higher Education	No	No	No	Yes	Yes	Yes
Treatment Month	No	No	No	No	Yes	Yes
Treatment at Home	No	No	No	No	Yes	Yes
Female	No	No	No	No	No	Yes
Age	No	No	No	No	No	Yes
Occupation	No	No	No	No	No	Yes
Observations	12,712	12,712	12,712	12,365	12,246	12,243
R^2	0.011	0.690	0.697	0.697	0.697	0.697
Rand-t for Disability	0.6154	0.5185	0.2398	0.2607	0.2268	0.2178
Rand-t for Mobile	0.5155	0.0270	0.0110	0.0100	0.0110	0.0110
Rand-t for Trust	0.3666	0.0589	0.0130	0.0170	0.0190	0.0200

Notes: Standard errors in parentheses are subdistrict-level cluster robust. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level. The unconditional mean of the outcome variable in the control group is 0.1483. *Rand-t* indicates p-values obtained by randomization t-tests for each treatment as in Young (2018).

Table 4: Extensive Margin and Intensive Margin

	Payment Five Months After the Experiment						Testing Differences
	Existing Participants			Never Participants			Using (1) and (4)
	(1)	(2)	(3)	(4)	(5)	(6)	
Disability	0.0152	0.0159	0.0159	0.0006	0.0006	0.0005	$p = 0.185$
	(0.0102)	(0.0103)	(0.0103)	(0.0037)	(0.0038)	(0.0038)	
Mobile	0.0191*	0.0187*	0.0182*	0.0103***	0.0102**	0.0101**	$p = 0.439$
	(0.0106)	(0.0107)	(0.0108)	(0.0040)	(0.0041)	(0.0041)	
Trust	0.0254**	0.0242**	0.0242**	0.0055	0.0058	0.0057	$p = 0.058$
	(0.0099)	(0.0101)	(0.0102)	(0.0041)	(0.0042)	(0.0042)	
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Joint F-test: $p = 0.3657$
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	
Higher Education	Yes	Yes	Yes	Yes	Yes	Yes	
Contribution One Month Before	Yes	Yes	Yes	Yes	Yes	Yes	
Any Payment in 2016	Yes	Yes	Yes	Yes	Yes	Yes	
Treatment Month	No	Yes	Yes	No	Yes	Yes	
Treatment at Home	No	Yes	Yes	No	Yes	Yes	
Female	No	No	Yes	No	No	Yes	
Age	No	No	Yes	No	No	Yes	
Occupation	No	No	Yes	No	No	Yes	
Rand-t for Disability	0.2088	0.1978	0.1958	0.8981	0.8911	0.9201	
Rand-t for Mobile	0.1099	0.1219	0.1369	0.0240	0.0280	0.0270	
Rand-t for Trust	0.0290	0.0350	0.0400	0.2607	0.2468	0.2577	
R^2	0.669	0.670	0.670	0.139	0.142	0.142	
Observations	5,581	5,526	5,526	6,784	6,720	6,717	

Notes: Standard errors in parentheses are subdistrict-level cluster robust. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level. The unconditional mean of the outcome variable in the control group is 0.3169 for the existing participants and 0.0113 for the never participants. *Rand-t* indicates p-values obtained by randomization t-tests for each treatment as in Young (2018).

Table 5: Heterogeneous Impacts

	Payment Five Months After the Experiment							
	Full Sample		Existing Participants		Full Sample		Existing Participants	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Disability	0.0100	0.0023	0.0246*	0.0098	0.0131**	0.0105	0.0289**	0.0224
	(0.0068)	(0.0124)	(0.0139)	(0.0255)	(0.0057)	(0.0080)	(0.0120)	(0.0166)
Mobile	0.0216***	-0.0026	0.0421***	-0.0146	0.0127**	0.0105	0.0203	0.0090
	(0.0070)	(0.0143)	(0.0137)	(0.0268)	(0.0061)	(0.0081)	(0.0126)	(0.0158)
Trust	0.0186**	0.0153	0.0263**	-0.0025	0.0209***	0.0172*	0.0379***	0.0213
	(0.0074)	(0.0128)	(0.0130)	(0.0240)	(0.0065)	(0.0092)	(0.0118)	(0.0162)
Disability * Remoteness dummy		0.0101		0.0202				
		(0.0141)		(0.0297)				
Mobile * Remoteness dummy		0.0317*		0.0752**				
		(0.0166)		(0.0314)				
Trust * Remoteness dummy		0.0025		0.0385				
		(0.0149)		(0.0284)				
Remoteness dummy		-0.0158		-0.0355*				
		(0.0098)		(0.0214)				
Disability * Japan Trust dummy						0.0060		0.0146
						(0.0124)		(0.0237)
Mobile * Japan Trust dummy						0.0052		0.0275
						(0.0125)		(0.0244)
Trust * Japan Trust dummy						0.0088		0.0388
						(0.0140)		(0.0269)
Japan Trust dummy						-0.0012		-0.0044
						(0.0095)		(0.0179)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Higher Education	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contribution One Month Before	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Any Payment in 2016	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rand-t for X_i*Disability		0.5485		0.5145		0.6513		0.5564
Rand-t for X_i*Mobile		0.0460		0.0100		0.6883		0.2977
Rand-t for X_i*Trust		0.8781		0.2178		0.5065		0.1469
R^2	0.708	0.708	0.691	0.692	0.695	0.695	0.670	0.671
Observations	7,302	7,302	3,404	3,404	9,304	9,304	4,222	4,222

Notes: Standard errors in parentheses are subdistrict-level cluster robust. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level. The unconditional mean of the outcome variable in the control group is 0.1483 for the full sample and 0.3169 for the existing participants. *Rand-t* indicates p-values obtained by randomization t-tests for each interaction term as in Young (2018).

Table 6: Adverse Selection

	Contribution 1M Before Experiment	Change 5M After	Contribution 1M Before Experiment	Change 5M After	Contribution 1M Before Experiment	Change 5M After
	(1)	(2)	(3)	(4)	(5)	(6)
Shorter Life Expectancy	0.0169*	-0.0121				
	(0.0091)	(0.0094)				
Disability * Shorter Life Expectancy		0.0130				
		(0.0140)				
Mobile * Shorter Life Expectancy		0.0234*				
		(0.0135)				
Trust * Shorter Life Expectancy		0.0018				
		(0.0148)				
Disability		0.0135		0.0052		0.0095
		(0.0106)		(0.0067)		(0.0064)
Mobile		0.0253**		0.0129*		0.0173***
		(0.0111)		(0.0067)		(0.0066)
Trust		0.0140		0.0122*		0.0126*
		(0.0119)		(0.0068)		(0.0066)
Negative Health Shock			0.0089	0.0176		
			(0.0091)	(0.0128)		
Disability * Negative Health Shock				0.0037		
				(0.0158)		
Mobile * Negative Health Shock				-0.0279*		
				(0.0166)		
Trust * Negative Health Shock				-0.0206		
				(0.0177)		
Unhealthy Behavior					-0.0438***	0.0085*
					(0.0048)	(0.0050)
Disability * Unhealthy Behavior						-0.0113*
						(0.0067)
Mobile * Unhealthy Behavior						-0.0106
						(0.0070)
Trust * Unhealthy Behavior						-0.0046
						(0.0073)
Strata Fixed Effects	Yes	No	Yes	No	Yes	No
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8244	8244	10671	10671	10578	10578
R^2	0.067	0.001	0.061	0.001	0.070	0.001

Notes: Standard errors in parentheses are subdistrict-level cluster robust. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level. M = month. *Shorter Life Expectancy* takes the value of one if the respondent has shorter life expectancy than the gender-wise average, which is 66 years old for males and 76 years old for females. The second measure is the experience of a negative health shock such as hospitalization and accidents. The third measure is unhealthy behavior such as drinking and smoking. *Negative Health Shock* (*Unhealthy Behavior*) is an average of the z scores of the experience of hospitalization and accidents (frequency of drinking and smoking). We normalize these two measures.

A Appendix

A.1 Data Construction: Map

Because there was no publicly available subdistrict-level map, we obtained the map from the government implementing agency of Mongolia: the Agency for Land Administration and Management, Geodesy and Cartography. However, because there were spelling variations in the names of subdistricts as well as misrecording, we could not match 7 percent of the subdistricts in our dataset to the map. We keep these unmatched subdistricts throughout our analysis, except for the analysis of externalities.

Figure A.1: Leaflets in English

Types of Social Insurance Services Provided for the Insured

Pension Insurance
- ✓ Retirement pension
- ✓ Disability pension
- ✓ Survivors pension

Benefits Insurance
- ✓ Sickness benefit
- ✓ Maternity benefit
- ✓ Funeral grant

Insurance Against Employment Injury and Occupational Diseases
- ✓ Disability pension
- ✓ Dependent's pension
- ✓ Temporary disability benefit
- ✓ Rehabilitation payments
- ✓ Pension insurance contributions
- ✓ Expenses for preventive measures
- ✓ Expenses for sanatorium care

What are necessary for social insurance contract:
- ✓ Determined monthly income to pay social insurance contributions
- ✓ Payment of social insurance contributions applicable periods
- ✓ Social insurance handbook

Voluntary Social Insurance

Except for workers who have signed labor contracts, employers and other people can participate in any form of social insurance. For example:

- People who are self-employed
- Herders
- Farmers
- Miners in small mines
- Freelance artists
- Unemployed persons
- Citizens living in a foreign country
- Students
- etc.

The Rates of Social Insurance Contributions are as follows:
- Pension insurance – 10.0%
- Benefits insurance – 1.0%
- Insurance against employment and occupational diseases – 1.0%

The base income for calculating social insurance contributions cannot be less than the minimum wage.

The phone number of the Social Insurance General Office: 7777-1289 www.ndaatgal.mn fb/sigomongolia

The leaflet number: 5 (6,7, or 8)

Social Insurance General Office of Mongolia

Are you concerned about your future living costs when you cannot work due to old age, injury or illness?

By participating in the voluntary social insurance, the individuals who felt concerned about the living costs after their retirement, as you do, now have confidence in the future.

We recommend the voluntary social insurance since, if you have properly paid your contributions, you can receive a pension from the Mongolian government in the future in the case of being unable to work. Taking part in the social insurance will help you to avoid future financial problems such as running out of money or being unable to provide for your family.

Yet, you may still be uncertain about whether you will receive the pension, even if you pay all your contributions properly.

(Please turn over.)

(a) Control

Figure A.1: Leaflets in English (cont.)

The leaflet number: 6

Do you think that you can only access the pension only after you have retired?

The National Social Security Fund provides not only retirement pensions but also invalidity pensions and survivor's pensions. If you become disabled for a long time due to illness or injury, the invalidity pension can support you and your family until your recovery. If participants, unfortunately, passed away leaving their family incapable of working, the survivors could be able to receive the survivor pension.

- Accidents such as falling off horses or bikes or contracting an unexpected disease can happen to anyone at any time, causing disabilities or even death.
- In the case of disability, you could be eligible to receive the invalidity pension, and in the case of death, your family could be eligible to receive the survivor's pension.
- While the retirement pension is only for people of retirement age, people of almost any age are eligible to receive the invalidity and survivor's pensions.

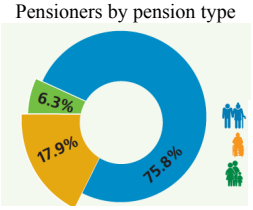
Example of receiving a pension

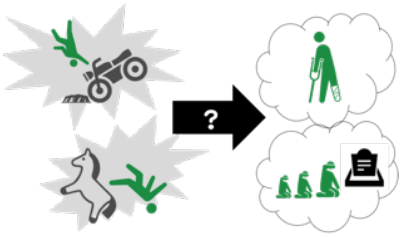
A harder called Mr. A injured his leg falling from a horse during his work. Although Mr. A had to earn income for his wife and children, he was unable to work as a herder for a while. At that time, a social insurance inspector kindly told him that participants in the social insurance could receive a disability pension. Thanks to the fact that he was a participant and he'd been paying his contributions every month, the public pension revenue prevented his family from becoming poor.

Can I really receive the pension?

Although only people over retirement age are eligible to receive the retirement pension, people below this age can receive the disability pension or survivor's pension, if certain conditions are met.

Pensioners by pension type





Invalidity Pension

An insured person who has lost not less than 50 percent of his/her capacity for work permanently or for a long duration due to a non-occupational disease or accident shall be eligible for the invalidity pension as long as they have met the following conditions:

- Have paid pension insurance contributions for not less than 20 years
- Have paid contributions for a period of three years out of five, immediately preceding the date of commencement of invalidity

Survivor's Pension

In the event that an insured person dies due to a non-occupational disease or accident, the dependent family members shall be eligible for the survivor's pension as long as they have met the following conditions:

- Have paid pension insurance contributions for not less than 20 years
- Have paid for a period of three years out of five, immediately preceding the date of passing away

To inquire about participation in the pension please use the phone number (7777-1289) or ask one of your local social insurance inspectors.

(b) Disability

Figure A.1: Leaflets in English (cont.)

The leaflet number: 8

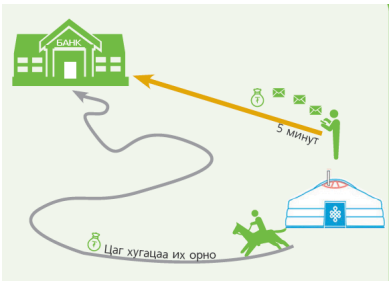
Are you concerned about having to go to the bank every month to pay your social insurance contributions?

We offer a service which allows you to pay your contributions using your mobile phone eliminating the need to go to the bank

- To make it easy for people living remote areas to pay their contributions, we began the mobile banking payment service.
- You can skip a trip to a bank and spend this time for family chores, such as managing livestock.
- It is very easy to open a mobile bank account and transferring money.

What is “Mobile banking”?

- Mobile banking is a service which allows you to transfer money and manage your account with your mobile phone.
- You can use it in banks which offer mobile banking.
- You can use the service with either mobile phones or smart phones.
- If you already use this service, you can start paying your contributions immediately.



How to open a mobile bank account and transfer money

<Procedure>

- Ask a social insurance inspector for the details of the account to pay your contributions to via mobile banking.
- Go to your local bank and fill in an application form to apply for mobile banking.
- Go to your mobile network provider’s shop, fill in an application form to apply for mobile banking.
- Follow the instructions to activate the mobile banking service, and then you can use your phone to pay your contributions to the bank.

To inquire about participation in the pension please use the phone number (7777-1289) or ask one of your local social insurance inspectors.

(c) Mobile

Figure A.1: Leaflets in English (cont.)

The leaflet number: 7

Introduction of the SINRAI Project

To improve the reliability of social insurance administration, the Japan International Cooperation Agency (JICA) launched a "Project on Strengthening the Capacity for Social Insurance Operation" (the SINRAI Project).

- JICA, the Ministry of Labour and Social Protection, and the Health and Social Insurance General Office are jointly conducting this project.
- The goal is to improve the administration of Mongolian social insurance, including such activities as providing information, collecting contributions, and paying pensions.
- Well-experienced Japanese experts from the Health, Labour and Welfare Ministry and the Japan Pension Service have been actively working since the middle of 2016.

Introduction from a Japanese expert:

Everyone reaches an advanced age. Also, we are at risk of severe illness and injury in our life. If we lose the breadwinner in our family, how does the surviving family maintain their living standards?

Social insurance is the system in which citizens help each other and hedge future risks, not in a single family but in a whole country. The ultimate purpose of our project is, through the cooperation of Mongolia and Japan, to ensure that Mongolian people become able to live free from anxiety by protecting various risks in their lives.

JICA Chief Advisor Mamoru Yamashita

'SINRAI' means 'Itgeltsel' (trust) in Mongolian. It conveys our hope to build citizens' trust in the Mongolian social insurance, the Mongolian government and Japan through the success of the project.

About the SINRAI Project:

The project has various activities, such as holding seminars for Mongolian social insurance officers, training them in Japan, dispatching Japanese short-term experts in pension records, pension actuary and pension services, and making a curriculum and textbooks for Mongolian social insurance officers' training.

Project period: June 2016 - May 2020

Japanese experts:

- Mr. Mamoru Yamashita (Chief Advisor/ Social Insurance Policy) *dispatched from the Health, Labour and Welfare Ministry
- Mr. Akihiro Takanashi (Social Insurance Operation) *dispatched from the Japan Pension Service
- Ms. Erika Kikuchi (Project Coordinator)

To inquire about participation in the pension please use the phone number (7777-1289) or ask one of your local social insurance inspectors.

(d) Trust

Notes: We provide the leaflet in Panel (a) to the control group. For the treatment groups, we print Panel (b) for the disability treatment, (c) for the mobile treatment, and (d) for the trust treatment groups, respectively, on the back of the leaflet in Panel (a). See Figure 3 for the original version.

Table A.1: Definition of Variables

Variable	Original Questions	Construction
Remoteness Dummy	You or your household members, excluding children under 18 years of age, visit district or province centers nearby about XXX days a month in (Spring/Summer/Autumn/Winter).	After averaging these season-specific variables, we construct a dummy variable that takes the value of one if this value is greater than its unconditional mean.
Expected Longevity Dummy	The average expectancy is 66.0 years for Mongolian males and 75.8 for Mongolian females. How long do you think you will you live? (1. Less than 54 years, 2. 55–59 years, 3. 60–64 years, 4. 65–69 years, 5. 70–74 years, 6. 75–79 years, 7. 80–84 years, 8. 85–89 years, 9. 90 years or more)	We construct a dummy variable that takes the value of one if the answer is greater than or equal to 4 for males and 6 for females, which corresponds to their average life expectancy.
Negative Health Shock	Have you been hospitalized in the last 12 months? (1. Yes, 2. No) How many times have you had horse or bike accidents in the past three years? (1. None, 2. Once, 3. Twice, 4. More than twice)	After normalizing these two variables, we sum them (after taking negative for the hospitalization question).
Unhealthy Behavior	How many days have you smoked this month? (1. None, 2. 1–9 days, 3. 10 days–less than every day, 4. Every day) How many days have you drunk at least one glass of alcohol this month? We define one glass of alcohol as one can of beer, one glass of wine, or one shot of cognac, vodka, whiskey, or rum. (1. None, 2. 1–9 days, 3. 10 days–less than every day, 4. Every day)	After normalizing these two variables, we sum them.
Japan Trust Dummy	Do you think Japan is a trustworthy country? (1. Very much 2. Somewhat 3. Neutral 4. Not so much 5. Not at all)	We construct a dummy variable that takes the value of one if the respondent chooses 1 for this answer (the median is 3).

Table A.2: Including the Sample Dropped from the Main Result Because of an ID Mismatch

	Payment Five Months After the Experiment					
	(1)	(2)	(3)	(4)	(5)	(6)
Disability	-0.0022	0.0007	0.0037	0.0032	0.0038	0.0069*
	(0.0107)	(0.0042)	(0.0036)	(0.0037)	(0.0037)	(0.0040)
Mobile	0.0022	0.0078*	0.0092**	0.0097**	0.0095**	0.0125***
	(0.0107)	(0.0041)	(0.0038)	(0.0039)	(0.0039)	(0.0044)
Trust	0.0010	0.0067	0.0118***	0.0116***	0.0116***	0.0124***
	(0.0116)	(0.0043)	(0.0039)	(0.0040)	(0.0040)	(0.0044)
Strata Fixed Effects	Yes	No	Yes	Yes	Yes	Yes
Payment One Month Before	No	Yes	Yes	Yes	Yes	Yes
Any Payment in 2016	No	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Higher Education	No	No	No	Yes	Yes	Yes
Treatment Month	No	No	No	No	Yes	Yes
Treatment at Home	No	No	No	No	Yes	Yes
Female	No	No	No	No	No	Yes
Age	No	No	No	No	No	Yes
Job	No	No	No	No	No	Yes
Observations	19493	19493	19493	18764	18532	16052
R^2	0.010	0.700	0.704	0.704	0.705	0.707
Rand-t for Disability	0.7243	0.8771	0.3566	0.4346	0.4216	0.1299
Rand-t for Mobile	0.8172	0.0509	0.0260	0.0240	0.0280	0.0120
Rand-t for Trust	0.8601	0.1159	0.0070	0.0100	0.0140	0.0190

Notes: Standard errors in parentheses are subdistrict-level cluster robust. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level. The unconditional mean of the outcome variable in the control group is 0.1483. *Rand-t* indicates p-values obtained by the randomization t-tests for each treatment as in Young (2018). This includes the sample that we drop from the main analysis because of an ID mismatch. See the main text for the details.

Table A.3: External Validity

	Payment Five Months After the Experiment			
	Full	Existing Participants	Full	Existing Participants
	(1)	(2)	(3)	(4)
Disability	0.0084	0.0190*		
	(0.0051)	(0.0105)		
Disability * Attendance in 2016	-0.0069	-0.0104		
	(0.0055)	(0.0112)		
Mobile	0.0156***	0.0196*		
	(0.0053)	(0.0109)		
Mobile * Attendance in 2016	-0.0028	-0.0027		
	(0.0054)	(0.0114)		
Trust	0.0160***	0.0234**		
	(0.0054)	(0.0098)		
Trust * Attendance in 2016	-0.0056	-0.0101		
	(0.0058)	(0.0108)		
Attendance in 2016 (standardized)	0.0043	0.0083	0.0043	0.0083
	(0.0040)	(0.0082)	(0.0040)	(0.0082)
Treated			0.0132***	0.0208***
			(0.0042)	(0.0079)
Treated * Attendance in 2016			-0.0050	-0.0077
			(0.0046)	(0.0092)
Strata Fixed Effects	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes
Payment One Month Before	Yes	Yes	Yes	Yes
Any Payment in 2016	Yes	Yes	Yes	Yes
Observations	11,956	5,411	11,956	5,411
R^2	0.700	0.671	0.700	0.671
Rand-t for Disability * Attendance in 2016	0.2348	0.3756		
Rand-t for Mobile * Attendance in 2016	0.6144	0.7942		
Rand-t for Trust * Attendance in 2016	0.3407	0.3636		
Rand-t for Treated * Attendance in 2016			0.2977	0.4236

Notes: Standard errors in parentheses are subdistrict-level cluster robust. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level. The unconditional mean of the outcome variable in the control group is 0.1483. *Rand-t* indicates p-values obtained by randomization t-tests for each treatment as in Young (2018). *Attendance in 2016* indicates the attendance of subdistrict meetings in 2016, top-coded at four times and standardized. *Treated* takes the value of one if an individual is allocated in one of the treated groups.

Table A.4: Spillover

	Payment Five Months After the Experiment							
	Full Sample				Existing Participants			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Disability	0.0063	0.0054	0.0061	0.0075	0.0152	0.0153	0.0146	0.0160
	(0.0050)	(0.0052)	(0.0054)	(0.0053)	(0.0102)	(0.0111)	(0.0113)	(0.0110)
Mobile	0.0150***	0.0147***	0.0149***	0.0143**	0.0191*	0.0190*	0.0216*	0.0204*
	(0.0053)	(0.0056)	(0.0057)	(0.0057)	(0.0106)	(0.0109)	(0.0113)	(0.0114)
Trust	0.0156***	0.0164***	0.0175***	0.0157***	0.0254**	0.0281***	0.0307***	0.0276**
	(0.0055)	(0.0057)	(0.0061)	(0.0060)	(0.0099)	(0.0105)	(0.0113)	(0.0110)
Disability within 20 km		-0.0095	-0.0110			-0.0105	-0.0123	
		(0.0084)	(0.0084)			(0.0178)	(0.0177)	
Trust within 20 km		0.0084	0.0072			0.0183	0.0161	
		(0.0086)	(0.0086)			(0.0174)	(0.0175)	
Mobile within 20 km		0.0053	0.0039			0.0093	0.0069	
		(0.0086)	(0.0088)			(0.0168)	(0.0170)	
Disability within 50 km			0.0195	0.0194			0.0054	0.0057
			(0.0195)	(0.0197)			(0.0350)	(0.0352)
Trust within 50 km			0.0248	0.0254			0.0415	0.0445
			(0.0202)	(0.0203)			(0.0360)	(0.0360)
Mobile within 50 km			0.0188	0.0183			0.0414	0.0412
			(0.0163)	(0.0163)			(0.0316)	(0.0316)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Payment One Month Before	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Any Payment in 2016	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Higher Education	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,365	11,920	11,920	11,920	5,581	5,382	5,382	5,382
R^2	0.697	0.697	0.697	0.697	0.669	0.670	0.670	0.670
Rand-t for Disability within 20 km		0.3516	0.2897			0.6294	0.5674	
Rand-t for Mobile within 20 km		0.3776	0.4575			0.3686	0.4326	
Rand-t for Trust within 20 km		0.5804	0.7073			0.6364	0.7483	
Rand-t for Disability within 50 km			0.4096	0.4176			0.9101	0.8791
Rand-t for Mobile within 50 km			0.2867	0.2847			0.3317	0.2737
Rand-t for Trust within 50 km			0.3457	0.3177			0.2817	0.2577

Notes: Standard errors in parentheses are subdistrict-level cluster robust. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level. The unconditional mean of the outcome variable in the control group is 0.1483. *Rand-t* indicates p-values obtained by randomization t-tests for each treatment as in Young (2018). *Disability within 20 km* is the proportion of subdistricts allocated as the disability treatment group within 20 km. Other variables are similarly defined.

Table A.5: Short-Term Effects

	Payment after the Experiment					
	One Month After			Three Months After		
	(1)	(2)	(3)	(4)	(5)	(6)
Disability	-0.0021	-0.0022	-0.0022	0.0024	0.0026	0.0027
	(0.0041)	(0.0041)	(0.0041)	(0.0048)	(0.0048)	(0.0048)
Mobile	0.0005	0.0004	0.0003	0.0089*	0.0089*	0.0088
	(0.0047)	(0.0047)	(0.0047)	(0.0053)	(0.0054)	(0.0054)
Trust	-0.0055	-0.0058	-0.0059	0.0100*	0.0098*	0.0097*
	(0.0046)	(0.0046)	(0.0046)	(0.0053)	(0.0053)	(0.0053)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Higher Education	Yes	Yes	Yes	Yes	Yes	Yes
Contribution One Month Before	Yes	Yes	Yes	Yes	Yes	Yes
Any Payment in 2016	Yes	Yes	Yes	Yes	Yes	Yes
Treatment Month	No	Yes	Yes	No	Yes	Yes
Treatment at Home	No	Yes	Yes	No	Yes	Yes
Female	No	No	Yes	No	No	Yes
Age	No	No	Yes	No	No	Yes
Occupation	No	No	Yes	No	No	Yes
Observations	12,364	12,245	12,242	12,364	12,245	12,242

Notes: Standard errors in parentheses are subdistrict-level cluster robust. * Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level. The unconditional mean of the payment after one (three) month(s) after the experiment dummy is 16.73 (15.66).

Abstract (in Japanese)

要約

途上国において、公的年金加入率の向上は重要な政策目標である。我々は、人々の年金加入を妨げると考えられる 3 つの制約を検証すべく、モンゴルにおける約 40%の村落を対象にランダム化比較試験(RCT)を実施し、行政データを用いて分析を行った。結果、年金の副次的な便益（障害年金及び遺族年金）に関する情報を提供しても、年金支払いを促進するという証拠は得られなかった。しかし、携帯電話による保険料支払い方法に関する情報や、国際援助機関から年金行政機関への専門家派遣に関する情報を提供すると、年金支払いを促進することが明らかになった。これらの分析結果は、主観的な取引コストや信頼が公的年金への需要と関連していることを示している。また、国際援助が人々の公的サービスへの認識を変化させることで、人々の公的サービスへの参加を促進しうることも示唆している。

キーワード: ランダム化比較試験、年金、携帯電話、国際援助、情報提供、モンゴル

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