

# Diffusion in social networks: Experimental evidence on information sharing vs persuasion

Marcel Fafchamps<sup>1</sup>, Asad Islam<sup>2</sup>, Debayan Pakrashi<sup>3</sup>, and Denni Tommasi<sup>\*4</sup>

<sup>1</sup>Stanford University

<sup>2</sup>Monash University

<sup>3</sup>IIT Kanpur

<sup>4</sup>University of Bologna

June 2023

## Abstract

We conduct a clustered randomized control trial in 180 villages in Uttar Pradesh, India, to promote the adoption of a saving commitment product new to our study population. A random set of participants are targeted by our promotional campaign. The main purpose is to test whether the diffusion of the product among untargeted participants operates mostly via information sharing from the former to the latter, or whether it operates through persuasion from targeted participants incentivized to bring other participants to take-up the product. If take-up is higher in information villages than in persuasion villages, we will conclude that social learning is the main channel of diffusion. If, in contrast, take-up is higher in persuasion villages, we will conclude that persuasion is the main channel of diffusion. The outcome of the experiment has strong policy implications with broad applicability to developing and developed countries beyond the specific product that we provide in our setting.

**JEL Codes:** O16, D14, G21

**Keywords:** diffusion, social networks, savings, financial inclusion, information, persuasion.

**Study pre-registration:** AEA RCTR-0005801

---

\* Fafchamps: Freeman Spogli Institute for International Studies, Stanford University, USA. Email: [fafchamp@stanford.edu](mailto:fafchamp@stanford.edu). Islam: Centre for Development Economics and Sustainability (CDES), and Department of Economics, Monash University, Australia. E-mail: [asadul.islam@monash.edu](mailto:asadul.islam@monash.edu). Pakrashi: Department of Economic Sciences, Indian Institute of Technology, Kanpur, India. E-mail: [pakrashi@iitk.ac.in](mailto:pakrashi@iitk.ac.in). Tommasi: Department of Economics, University of Bologna, Italy. E-mail: [denni.tommasi@unibo.it](mailto:denni.tommasi@unibo.it).

# 1 Introduction

It is now well established that many human behaviors spread on social networks (Jackson et al., 2017). How to properly harness this process is important because, given access to limited resources, it is not always possible to directly provide information to everyone in a large target population. By targeting a subset of individuals with the right kind of intervention, governments, international aid agencies, and NGOs may find a more cost-effective way to diffuse new products and technologies.

Much of the economic literature on this topic tends to ascribe diffusion on networks to social learning, that is, to the peer-to-peer diffusion of information among individuals through contact – either in person (e.g., BenYishay and Mobarak, 2018; Beaman et al., 2021), over the phone (e.g., Cole and Fernando, 2020; Kelley et al., 2022), by letters (Hjort et al., 2021), by SMS (e.g., Batista et al., 2022), or on social media (e.g., Alatas et al., 2019). Transfer of information from one person to another is then assumed to induce recipients of this information to update their beliefs, which in turn leads them to revise their choices. This process of social learning is generally believed to be behind the adoption of not only new techniques and practices (e.g., Carter et al., 2021; Fafchamps et al., 2021), but also new products and services (e.g., Banerjee et al., 2013; Miller and Mobarak, 2015; Afzal et al., 2019; Fafchamps et al., 2022).

Yet we have known, since at least Bernays’ 1928 seminal book on advertising and propaganda (Bernays, 2004), that new behaviors can also spread through persuasion, without necessarily providing relevant or complete information about the material benefits of adoption. This is particularly true when those who can influence others have an incentive to do so, either because they benefit directly from others adopting, or because they are rewarded for bringing new customers. Persuasion can work in many different ways that include cajoling others, misdirecting them (Bernays, 2004), appealing to their self or social image (Bursztyn and Jensen, 2017), and referring them to role models or influential individuals (e.g., Porter and Serra, 2020; Fafchamps et al., 2020; Banerjee et al., 2021). Persuasion may also rely on implicit threats of ridicule or shame to those who do not comply with a supposed social norm to adopt (or not adopt) (e.g., Bursztyn et al., 2020a,b). Efforts to enforce compliance may even extend to ostracism, trolling, verbal abuse, physical attacks, and other forms of peer pressure.

Persuasion need not be nefarious, and there are multiple reasons why it may be benevolent (e.g., Esguerra et al., 2023). Persuasion may also help overcome obstacles to beneficial adoption due to ignorance, adherence to unequal gender norms, or failure of aspirations. Even if persuasion is benevolent, however, the adoption that it induced may not ultimately be in the interest of the adopter who was not necessarily provided accurate information about the costs and benefits of adoption.<sup>1</sup> This represents a major concern for policy makers who encourage the adoption of new products, behaviors, or technology, without necessarily knowing whether they truly benefit the target population (e.g., Berg et al., 2022). These concerns are particularly relevant when efforts to diffuse a product reward individuals who convince others to sign up. The reward may be financial or material – as in referral-by-client marketing models (e.g., Berg et al., 2019). It can also take the form of social validation granted by the promoter of the product or by the social group itself.

---

<sup>1</sup>Throughout this paper, we abstract from externality considerations by which an individual action benefits the group but is not beneficial to the individual – a situation that may, in some cases, justify forcing adoption but that is excluded from our experimental design.

In this paper we seek to disentangle adoption arising from the diffusion of information from adoption resulting from persuasion. To achieve this objective, we design a randomized controlled trial encouraging the adoption of a saving commitment product that is new to our target population. The behavior we promote is the take-up of this product over a specified time interval. We have reasons to believe that adopting this behavior is beneficial to many members of our target population – but probably not all (e.g., [Afzal et al., 2018, 2019](#)). This means that universal adoption need not be desirable. To test whether persuasion can induce unbeneficial adoption, we compare the take-up of our financial product under different diffusion treatments, some of which provide relevant information about the product, and some of which incentivize signing up other members of the community (e.g., [Shikuku et al., 2019](#); [BenYishay and Mobarak, 2019](#)). The experiment is designed to test whether persuasion without information can induce adoption. If it cannot, the risk of undesirable adoption is minimized. If it can, we compare the extent to which individual adopters benefit from the product under the different treatments.

The experiment takes place in rural India. We accompany the introduction of a savings commitment product introduced by a local partner with no permanent base of operation in any of the targeted villages. To encourage the adoption of its product, the NGO offers an unusually high return on early deposits (e.g., [Meriggi et al., 2021](#)). Because of the NGO's small footprint on the ground, this information is not widely known. Our promotional campaign visits a randomly selected subset of individuals in our study population whom, for the purpose of this study, we call primary participants.<sup>2</sup> In one treatment these individuals are provided with detailed information about the saving commitment product and a list of three secondary participants with whom they encouraged to share this information (e.g., [Banerjee et al., 2013](#); [Kondylis et al., 2017](#)). We call this the information treatment. In a second treatment, primary participants are not provided with relevant information but they are incentivized to induce one of three named secondary participants to sign up for the product (e.g., [Deserrano et al., 2022](#)). We call this second treatment the persuasion treatment since, in this case, the primary participant has no information to share about the product (apart from its existence) and therefore she has to rely on persuasion to get the reward. We also include a combined treatment in which the primary participants receive both treatments – the detailed information and the reward for signing someone up.

Treatment assignment is randomized across villages to avoid the diffusion of relevant information to those assigned to the pure persuasion treatment. In all three treatments, the assignment of secondary participants to primary participants is achieved by partitioning secondary participants in a village into non-overlapping groups of three, and then randomly assigning one member of each triad to be a primary participant. The random assignment of peers serves to eliminate bias due to homophily in peer self-selection (e.g., [Centola, 2010, 2011](#)) and, for this reason, it has been used in a growing number of experiments (e.g., [Cai and Szeidl, 2017](#); [Fafchamps and Quinn, 2018](#); [Fafchamps and Mo, 2018](#); [Fafchamps et al., 2021](#); [Corno et al., 2022](#); [Batista et al., 2023](#); [Caeyers and Fafchamps, 2023](#)).

Our main objective is to compare take-up of the product across treatments using administrative data on individual take-up to which we have access. We then combine this information with endline survey data on

---

<sup>2</sup>To minimize the role of network architecture in the diffusion process, we seed the network at multiple random points. [Akbarpour et al. \(2023\)](#) indeed shows that, for most social networks, multi-point random seeding yields as much diffusion as optimal seeding with full knowledge of the network structure.

savings, incomes, and consumption expenditures, and with a quiz of primary and secondary participants on their knowledge of the product. Our main control group is a randomly selected set of villages in which the product is available but no promotional campaign is conducted. We also have a set of inactive control villages in which the product is not available, so as to measure the treatment effect of the availability of the product itself, without any promotion.

Our testing strategy can be summarized as follows. Primary participants in the information-only treatment should adopt the product if, based on the extensive knowledge they acquired, they believe that adopting is in their best interest. Since primary participants are selected randomly among the population of interest, this gives us a benchmark level of informed adoption in our population of interest. In contrast, in the pure persuasion treatment, primary participants are only told of the existence of the product itself. This, in principle, gives us a second benchmark for uninformed adoption. Our main focus is to compare these two benchmarks to adoption by the secondary participants, i.e., those individuals who are not visited by our promotional campaign.

We first note that, in information-only villages, we expect secondary participants to adopt if they receive the product information from primary participants and, having considered this information, decide to adopt it because it is beneficial to them. It follows that, if this behavioral model is true, the difference in adoption level between the two sets of subjects tells us the extent to which relevant information about the product diffuses in the treated villages. For instance, if 30% of informed primary participants adopt while only 20% of secondary participants do, this indicates that information did not reach one third of the secondary participants.

Adding the sign-up reward to the information treatment incentivizes primary participants to invest additional effort in sharing the information with their assigned secondary participants. Based on the same behavioral model as above, the difference in the adoption level of secondary participants between information-only villages and combined-treatment villages will tell us whether incentivization was successful in disseminating the information more widely. If it raises the adoption level of secondary participants closer to that of primary participants in information-only villages, this will be seen as indicating a more widespread diffusion of information about the product. To verify that this is indeed the case, we will test whether secondary participants are capable of listing key features of the product at endline, and whether they do so as well as primary participants who received the information directly from our enumerators.

It is also possible that adoption by secondary participants in combined-treatment villages results from incentivized persuasion without information transmission. In this case, adoption will be observed by secondary subjects who have little or no knowledge of the costs and benefits of the product (apart from its existence). In this case, the adoption rate by secondary participants may even exceed that by primary participants in the information-only treatment, which constitutes our benchmark of optimal information-based adoption in a randomly selected sample of our target population.

While the above comparison provides some useful insights, it does not allow us to fully disentangle information diffusion from persuasion. This is the reason why we include persuasion-only treatment in which no product information is provided to anyone in the village. In this treatment, the product knowledge of both primary and secondary participants is expected to be much lower than that of primary participants in the

information-only treatment – something we will verify formally using the endline survey data. Given this, adoption by secondary participants can only be the result of persuasion. This treatment therefore gives us a lower bound on the distortion of adoption behavior induced by rewarding some villagers to sign up others. It is a lower bound because informed primary participants in the combined treatment may combine accurate information with misdirection to induce even more sign up.

Adoption due only to persuasion – that is, without (correct) information – is expected to be less beneficial since adopters could not have taken into account information about the true costs and benefits of take-up. To test this prediction, we examine the average effect of each of the treatments on the savings, income, and consumption expenditures of secondary adopters. If adoption is beneficial for adopters, we should observe an increase in engagement with the savings product, and possibly an increase in material welfare as measured by these indicators. If, on the other hand, take-up is unbeneficial for some adopters, engagement should be minimal and material welfare should not increase as much – and may even fall. To test this, we will use the average treatment effect on material welfare outcomes among primary participants in the information-only treatment as benchmark. We will then compare the average effect of the three treatment conditions on secondary participants. In the information-only treatment, the average welfare gain should scale down in proportion with the adoption rate. In the combined treatment, the welfare gain should be lower than in the information treatment if we observe excess adoption (i.e., an adoption rate higher than that of primary participants in the information-only treatment). In the persuasion-only treatment, material welfare gains should be the lowest for secondary participants – and may even be negative if those induced to adopt by persuasion are not those who benefit the most from the product.

There are, however, specific contexts in which persuasion may be better able to induce beneficial adoption than information diffusion. One such context is when women are dis-empowered and, as a result, unable to push for the adoption of products that would benefit them. Providing women with financial autonomy – e.g., through cash transfers (e.g., [Attanasio and Lechene, 2002, 2014](#); [Aker et al., 2016](#); [Tommasi, 2019](#); [Riley, 2020](#); [Field et al., 2021](#)) or microfinance (e.g., [Ashraf et al., 2010](#); [Dupas and Robinson, 2013b](#); [Afzal et al., 2019](#)) – has long been recognized as a way of empowering them. Our study area was chosen as one where such findings are likely to apply. It is therefore possible – and even likely – that adoption of our saving commitment product may appeal to women in our study area but that they may not be able to adopt on their own. In such context, the support of a female peer from the local community may help overcome the inertia or resistance of the family. Given that our saving commitment product is aimed at women, we must be open to the possibility that the persuasion treatment may foster beneficial adoption by removing this constraint to take-up.

If this hypothesis is true, not only should we observe increased take-up in the combined treatment, and some take-up in the persuasion-only treatment, but this take-up should be beneficial, on average, for the women who adopt and, possibly, for their household as well. We will test for this using endline data on female empowerment and on household material outcomes. Another tell-tale sign of this mechanism is coordinated adoption between primary participants and the secondary participants assigned to them: ‘safety-in-numbers’ may induce women to adopt jointly, as a way of fending off criticism. Using randomization inference, we will investigate this idea by testing whether correlation in adoption between primary participants and their assigned secondary participants is higher than what would arise by chance. It is indeed well known that

social networks commonly facilitate coordination of behaviour, especially in village economies ([Foster and Rosenzweig, 1995](#); [Cox and Fafchamps, 2007](#); [Conley and Udry, 2010](#)).

We have already conducted a pilot on 586 participants residing in 15 rural villages in Bihar, India. We find that adoption is higher in persuasion villages than in information villages, but that adoption is highest when both treatments are combined – suggesting a role for persuasion in the adoption of a saving commitment product by women. We are now in the process of scaling up the study to a much larger sample of 5,958 participants residing in 180 villages in Uttar Pradesh. A baseline survey has already collected information on all participants through an in-person survey conducted in July-August 2022.

Our paper contributes primarily to the literature studying how economic behaviours spread on social networks. Seminal works highlighting the importance of information diffusion include, among others, [Foster and Rosenzweig \(1995\)](#), [Bandiera and Rasul \(2006\)](#), [Conley and Udry \(2010\)](#), [Duflo et al. \(2011\)](#) and [Oster and Thornton \(2012\)](#). Those highlighting the diffusion of adoption include, among others, [Banerjee \(1992\)](#), [Centola \(2010\)](#) and [Cai et al. \(2015\)](#). Our main contribution is to design an experiment that allows us to disentangle the role of social learning from persuasion in the adoption process.

We also contribute to the literature on financial inclusion, which is an important aspect of the promotion of economic growth. Despite a significant increase in formal financial account ownership in recent years, a vast majority of adults in developing countries, particularly women, still do not use them regularly. According to the 2021 Global Findex Database ([Demirguc-Kunt et al., 2022](#)), adults and women residing in the poorest 40% of households are less likely to save formally. Several papers show that reducing the monetary and non-monetary costs associated with savings does not translate into regular use of formal bank accounts in most cases (e.g., [Dupas and Robinson, 2013a](#); [Prina, 2015](#); [Dupas et al., 2017, 2018](#)). Product design features that proved effective at incentivizing account usage and formal savings include: offering ATM cards or door-to-door collection services (e.g., [Nava et al., 2006](#); [Schaner, 2016](#); [Callen et al., 2019](#)); offering mobile-linked digital savings accounts (e.g., [Batista and Vicente, 2020](#); [De Mel et al., 2020](#)); and providing a higher, above market-level interest rate (e.g., [Schaner, 2015, 2018](#); [Karlan and Zinman, 2018](#)). We exploit a commitment savings device offering a higher return upon meeting a specific goal, and we contribute to the literature by studying which mechanism(s) – social learning, persuasion, or a combination of the two – can increase formal savings in local communities.

The outcome of our project is expected to have strong policy implications. The evidence thus far suggests that transferring funds to women's accounts gives them more control over the use of financial resources and improves economic empowerment (e.g., [Attanasio and Lechene, 2002, 2014](#); [Aker et al., 2016](#); [Tommasi, 2019](#); [Riley, 2020](#); [Field et al., 2021](#)). Moreover, savings accounts offering commitment devices increases women's power over household decisions (e.g., [Ashraf et al., 2010](#); [Dupas and Robinson, 2013b](#)). If financial inclusion is helped by persuasion (relative to pure information), we will be able to conclude that group mobilization (as a policy lever) can foster coordinated decisions to adopt formal financial tools, thereby promoting gender equality. If, on the other hand, information is what matters (as is often implicitly assumed), then a policymaker should focus on distributing accurate information as widely as possible, including via the media, IT, and social networks. The findings will have a direct impact not only on poor and marginalized households in India but

also on many other developing countries as well as on developed countries with peculiar societal imbalances. It will also provide governments and agencies with data, research findings, and policy implications from what will be the first rigorously designed and analyzed study on how persuasion is able (or not) to improve financial inclusion.

This pre-analysis plan is organized as follows. The experimental design is presented in Section 2. Implementation details regarding the timeline and the sample selection are provided in Section 3. The conceptual framework and testing strategy are discussed in Section 4, while the preliminary results from the pilot are summarized in Section 5. Sections 6 and 7 contain information about the limitations and challenges of the study, and administrative information about the project.

## 2 Experimental Design

Our experiment comprises a sample of women living in rural Uttar Pradesh, India. We will introduce a subsidized commitment savings product called "Save & Gain". Our local partner will offer this saving instrument to eligible women. The experiment is to see how we can foster the adoption of this product.

The sample is selected as follows:

- In each village  $v$  selected for the project, a relevant target population of size  $N_v$  is identified as potentially interested in the new saving instruments, with  $N_v = 30$  to 40 households.
- In all treatment villages, this population is then randomly divided into two groups: primary participants, denoted by the letter  $A$ , and secondary participants, denoted by the letter  $B$ , with  $A + B = N_v$ . All participants answer a baseline and an endline survey, but only primary participants receive the information or persuasion interventions described below.
- In each selected household, we survey one woman who satisfies the following selection criteria: aged between 18 and 50; already has a bank account; and agrees to show us her passbooks at endline.

To identify treatment effects, villages are randomized into five treatment groups, including one pure control group. These groups differ in the type of intervention they receive and which households receive them. The logic behind the experimental design was already explained in the introduction, but important details will become apparent below when we discuss the hypotheses it is designed to test.

### 2.1 Save & Gain (S&G)

Our subsidized commitment savings product will work in the following way (see Figure A1 in Appendix A.1 for a representative poster of our financial product). For each tranche of 100 INR (up to 500 INR) that is kept on the account for three months, we pay 20 INR (20% interest rate), and we do this once for the largest amount saved for three months over a predetermined window of six months.

- Example 1: a participant deposits 100 INR on day one of month one, another 200 on day one of month

two, and withdraws the lot on day one of month five; so, three months later. In this case, she receives an interest payment of  $3 \times 20 = 60$  INR at the end of the eligible period.

- Example 2: a participant deposits 100 INR on day one of month one, another 500 INR on day one of month two, and withdraws the lot on day one of month five; so, four and three months later, respectively. In this case, she receives  $5 \times 20 = 100$  INR (the amount of savings eligible is capped at 500; so, in this case, even if she saved 600 INR, we would only pay an interest on 500 INR).
- Example 3: a participant deposits 100 INR on day one of month four and withdraws the lot on day one of month seven; so, three months later. In this case, she receives  $1 \times 20 = 20$  INR (the first day of month four is 90 days away from the first day of month seven, which is the last day available of the six-month window, so she is still eligible).
- Example 4: a participant deposits 100 INR on day one of month five and withdraws the lot on day one of month eight; so three months later. In this case, she receives  $0 \times 20 = 0$  INR (the first day of month eight is outside the six-month window).

The interests in S&G are paid back in the following way. Six months after the beginning of the experiment, we will make two two-day visits to each village, two weeks apart. We take note of the passbooks and other evidence on the first day of each two-day visit. We return on the second day of the two-day visit to pay the interest.

## 2.2 Treatment Groups

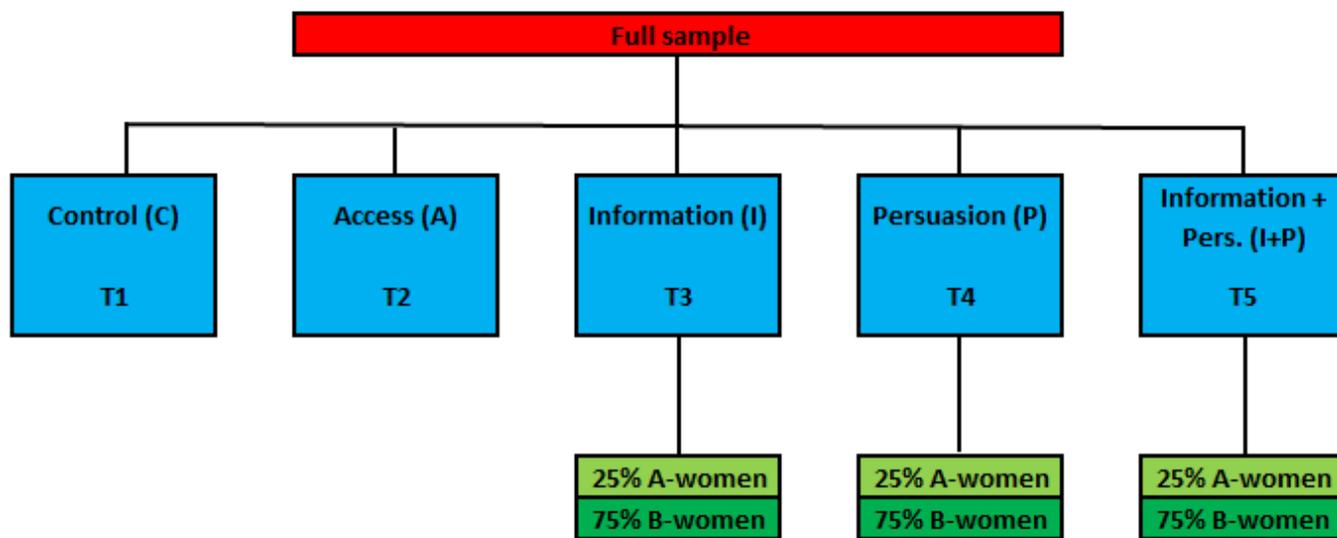
180 Villages are allocated equally to one of five groups, as summarized in Figure 1.

C (T1): This is the pure (inactive) control group. S&G is not offered in the village and no information is disseminated about it.

A (T2): This is the pure access treatment group. Participants in these villages receive no information or persuasion treatment, but S&G is available to all participants if they ask for it.

I (T3): This is the information sharing treatment group (see Figure A2 in Appendix A.1 for a representative poster summarizing the information treatment). S&G is available to all participants if they ask for it but the information about S&G is only given directly to the randomly selected primary participants. Specifically, in T3 villages, we have a team of marketer-enumerators who visit each primary at their home and spend 30 minutes or more explaining how S&G works, giving examples, explaining sign-up, and promoting the (expected) benefits of the product. This intervention also includes a training session and the distribution of written materials about how formal saving compares with existing traditional modes of saving. Each primary participant is then given the names of three secondary participants with whom she is encouraged to share the information she received. These secondary participants in T3 villages appear on a list held by the stall enumerators and can approach the stall to sign up and have their name ticked off a list. All village participants can sign up on their own if they wish to do so (e.g.,

**Figure 1: Experimental design**



if information circulates widely).

P (T4): This is the persuasion treatment group (see Figure A3 in Appendix A.1 for a representative poster summarizing the persuasion treatment). The group of primary participants is incentivized to induce secondary participants to adopt the new saving scheme. Only general information about the new saving scheme is provided to primary participants. Specifically, in T4 villages, we have a team of enumerators who visit primary participants in their homes and spend five minutes explaining that if they bring one of three listed secondary participants to sign up for S&G, they will receive a reward worth 50 INR. The recommender has to be present at the sign-up of the recommendee to get the reward. If the person they bring is not qualified (e.g., has not deposited at least 100 INR in the last month), the recommender gets no reward. The recommender is not obliged to sign up. Secondary participants can also sign up on their own (i.e., unaccompanied). In this case, no reward for their signing up is made since there is no evidence that signing up resulted from the effort of the primary participant.

I+P (T5): This is the information sharing + persuasion treatment. In these villages, primary participants receive the information treatment as in T3, plus the persuasion incentive as in T4. Specifically, we have a team of marketer-enumerators who visit primary participants at their homes, spend 30 minutes explaining S&G, as above, and an extra five minutes explaining the reward scheme. The rest is similar to the T4 group treatment described above.

### 2.3 Randomization

Our experiment is a clustered RCT with treatments randomized at the village level and interventions randomized at the participant level within villages. Randomization is carried out at three levels. First, at the village level, we assign one-fifth of the sample (35 villages, about 1,225 women) to the pure control treatment T1, one-fifth to the access treatment T2, one-fifth to the pure information treatment T3, one-fifth to the pure persuasion treatment T4, and one-fifth to the information + persuasion treatment T5.

Second, for treatment villages T3, T4 and T5, we randomly divide eligible women in each village into the

primary and secondary participants. More precisely, once we have our sample of  $N_j = A + B$  participants, for each village  $j$ , we randomly assign one-quarter of them to be primary participants and three-quarters to be secondary participants. Both randomizations are done after the baseline survey to ensure that the enumerators do not already know the identity of the primary participant at the time of the baseline survey.

Third, in  $T3$ ,  $T4$  and  $T5$ , we randomly partition secondary participants in each villages into non-overlapping groups of three, and we randomly assign each of these groups to one primary participants. Since primary participants form one quarter of the sample and secondary participants three quarters, this design ensures that each secondary participant is indirectly treated through the primary participant.

The randomization of villages follows a simple stratification rule based on the three variables that are the best predictors of future formal savings, based on  $R^2$  in our pilot: 1) formal savings in the past six months; 2) distance to the nearest bank in minutes; and 3) population size in the village. We calculate the average of these variables at the village level. Based on sample size, we then calculate the maximum number of strata that our dataset can support. In our case, each stratum must contain at least  $5 \times 2 = 10$  randomization units (Imbens, 2011). In order to satisfy this requirement, our dataset can support at most 12 strata, each composed of at least 10 villages. To achieve this, we consider three levels of village formal savings (low, medium, and high), two average levels of distance (above or below the sample median), and two average levels of population size (above or below the sample median). We generate 10,000 different allocations of treatment groups, and after conducting extensive balancing tests, we choose the best allocation in terms of balancedness.

## 2.4 Power Analysis

For the purpose of estimating average treatment effects on take-up, our main variable of interest, our key parameters are as follows:

- $\alpha = 0.05$ . This is standard type I error.
- $\kappa = 0.80$ . This is standard power.
- $J = 35$ . This is the number of clusters (villages) per treatment arm.
- $N_j = 24$ . This is the average number of  $B$  participants in each cluster (village)  $j$ .
- $ICC = 0.02$ . This is the intra-cluster correlation coefficient calculated using the baseline data.
- $R_1 = 0.10$ . This is the proportion of the variance in participants' take-up explained by participant covariates in our baseline data.
- $R_2 = 0.03$ . This is the proportion of the variance in participants' take-up explained by cluster (village) covariates in our baseline data.

With these parameters, our minimum detectable effect (MDE) is 0.15 standard deviation units. This number is smaller than the smallest effect size estimated in our pilot, which was carried out at the beginning of 2021 (for

more details, see section 5). During the pilot, eligible participants were contacted by the phone and offered S&G. Our effect sizes for  $B$  participants were 0.32 SD in the information treatment, 0.40 SD in the persuasion treatment, and 0.78 SD in the information + persuasion treatment. Although our intervention in the scaled-up experiment will require participants to keep their savings in the bank for longer than in the pilot (three months instead of one), it will also rely on in-person team visits to each village, not phone calls. This is likely to increase the effectiveness of the intervention. To be conservative about power, we need our intervention to have at least *half* of the smallest estimated treatment effect size in the pilot.

## 2.5 Balance

We require balance between each treatment group and the control, and between the different treatment groups. Since we will be testing some treatments against each other, we must set the bar high in terms of balance. Results for a large number of variables are presented in Tables A3-A4 of Appendix A.2. Balance is presented in terms of normalized differences between groups. Since participants are also randomized into primary and secondary, we conducted a balancedness analysis between these two groups. Results for the same set of variables are presented in Tables A5-A6 of Appendix A.2. Almost all the normalized differences are below 0.25, as recommended by Imbens and Rubin (2015).

## 3 Implementation

### 3.1 Timeline

This project is designed to be completed in approximately 20 months, with the workload distributed over three stages. The first stage, between April and September 2022, was dedicated to: hiring the trainers and enumerators; training the trainers and enumerators; completing the baseline survey; and randomizing treatments across the selected villages and the interventions across eligible women. The second stage, between October 2022 and April 2023, is when the experiment is implemented in the field. During this time, no survey data is collected but administrative data on sign-up is collected by our S&G partner. The third stage, between May and July 2023, will be dedicated to collecting the endline survey and paying back the interests of S&G. After the endline survey is complete, we will conduct the analysis and write the paper, which we hope to be ready by December 2023.

### 3.2 Sample and Outcomes

To address our research questions, we collect two types of data: survey data from participants; and administrative data from S&G about take-up and savings behavior.

We collected one baseline survey and will collect one endline survey. Figure 2 provides a map of the areas that are included in the study. In the surveys, we collect socio-economic information and detailed adoption and output data from all sampled women in the five sets of villages listed above. The period of six months from the baseline survey to the endline survey is chosen to allow for information diffusion and peer effects to operate after the interventions. During the endline survey which, like the baseline survey, will be conducted at

**Table 1: Milestones**

<b>Activity</b>	<b>Start</b>	<b>End</b>
<i>2022:</i>		
Hiring of trainers and enumerators	April	April
Selection of the villages	May	June
Training to trainers and enumerators	May	June
Baseline	July	August
Data cleaning and randomization	September	September
Program's implementation	October	December
<i>2023:</i>		
Program's implementation (cont'd)	January	April
Endline	June	July
Paying interests	July	July
Empirical analysis	August	October
Paper's write up	November	December

Notes: The table summarizes our timeline.

the home of the participants, we will collect detailed information about the respondents' saving and borrowing history over the previous six months.

Administratively collected take-up information will also be provided by S&G for all participants to the study, whether in control or treatment villages. This information will then be merged with the survey data using the name of the participants. We will also collect photographs of savings passbooks from participants in order to follow savings behavior over course of the study.

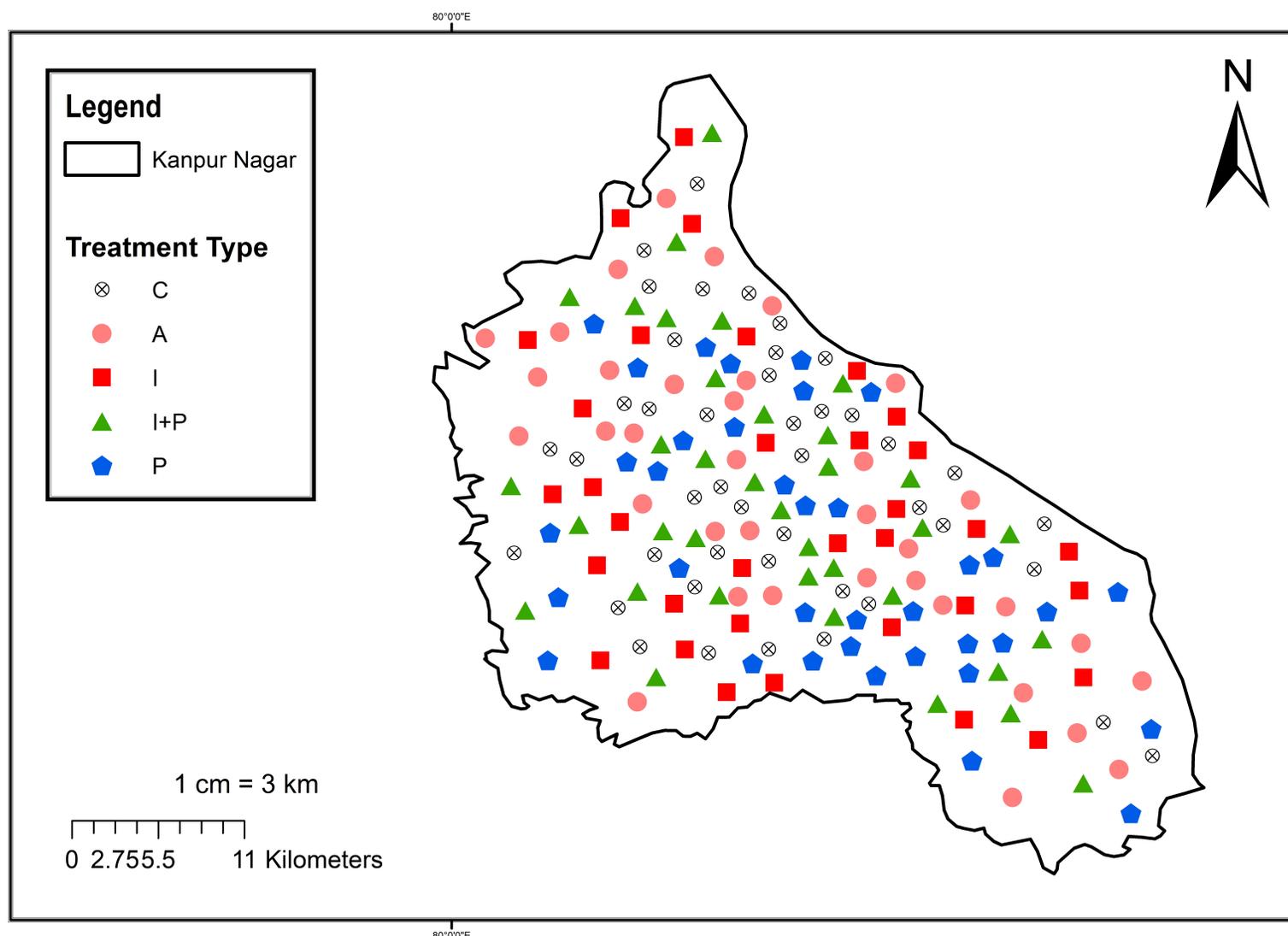
The survey data collection is conducted by female and male enumerators, trained by our local partner, on a one-on-one basis with participating women. Enumerators are Indian from Uttar Pradesh and are fluent in the Hindi language. The surveys include 12 modules covering: 1) location, identification, and household information; 2) wife and husband information; 3) assets; 4) wife income in rupees (last month); 5) household income in rupees (last month); 6) expenditure; 7) saving and borrowing behavior; 8) self-reported decision-making; 9) general health; 10) life satisfaction; 11) social desirability bias; and 12) risk-taking. The endline survey includes a quiz about the S&G savings products and collects photographs of S&G passbooks from willing participants.

We have two types of individual outcome variables: take-up; and economic outcomes. As explained above, take-up comes from administrative data provided by S&G. For the persuasion treatment, this also includes information about the presence of the primary participant at sign up to determine their reward.

The main individual outcome variables collected at baseline are:

1. Frequency of visits to banks and frequency of use of financial products. The main questions are: "How often do you save money?"; "How often you deposit money into the account?"; "Where did your household mostly save money in the last six months?"; and "Where do you prefer to save money?".
2. Individual and household income, assets, indebtedness, and expenditure. We have various standard

**Figure 2:** Villages in Uttar Pradesh included in the sample



questions about earnings, assets, and indebtedness. Regarding expenditure, we have a set of questions related to spending over the last week, one month, and six months to capture durable, semi-durable, and non-durable goods.

3. Subjective happiness/life satisfaction and sense of financial security. We have various questions related to self-reported decision-making, awareness, and mobility statements describing their financial situation or how worried they are about different aspects of their family. We also collect information on mental health indicators.

At endline, in addition to collecting data on the same individual outcome variables, we will also collect information on:

1. Average daily balance (in INR) on the participant's savings account over the duration of our intervention. This measures each participant's total engagement with the product during the treatment period and, for this reason, it is our main financial outcome of interest. The average daily balance will be calculated using passbooks from saving accounts and it is thus an objective, not self-reported, measure of savings behavior.<sup>3</sup> If we cannot collect information directly from the participants' passbook, we will substitute

<sup>3</sup>A passbook is a book in which the bank/financial institutions record the deposits and withdrawals made by a customer. In this book, the bank records each deposit made, each withdrawal, and the current balance, in a sequential manner. So, for instance, if there were a deposit made on January 1, it would be recorded. Then, if the next deposit were on February 11, that would appear as a second entry.

a coarser measure of engagement from answers to specifically designed questions.

2. Knowledge of S&G savings product. This will be administered in the form of a quiz.
3. Female empowerment indicators (e.g., involvement in household finances, executive and consultative agency regarding consumption choices, locus of control).

In Table 2, we report the summary statistics of the main variables of interest collected at baseline.<sup>4</sup> The average age of the women is 30, and 79% can read and write. They have been married for nine years on average, and their household comprises five individuals. Their husbands are four years older on average and have a similar level of education. In terms of economic status, most of these women report being housewives (82%), although some earn an income. The vast majority own their house (96%), and the majority also own a farm (64%). Almost all the women have visited a bank before (92%) and report having some formal savings, which is larger than their self-reported informal savings. They have also borrowed money both from formal institutions and from money lenders. They report depositing their money in the bank relatively infrequently, as 54% of them deposit only once every few months. On a scale of zero to 10, their risk attitude is below five, which imply that they are risk averse. Finally, 20% of surveyed women live in control villages (T1), 20% in the pure access treatment villages (T2), 20% in the information treatment villages (T3), 20% in the persuasion treatment villages (T4), and 20% in information + persuasion treatment villages (T5).

### 3.3 Attrition

It is possible that some participants might drop out in the middle of the intervention, or some might refuse to participate in the endline survey in June/July 2023. In that case, we will check if attrition is selective by comparing the baseline characteristics of the participants who dropped out of the study to those of participants who did not, and we will conduct this comparison both within and across treatment and control groups. In addition, we will also check for differences in the rate of attrition between the treatment and control groups (differential attrition). In the case of differential attrition, we will use Inverse Probability Weighting and Lee (2009) bounds to address attrition bias concerns.

## 4 Conceptual framework

As explained in the introduction, our objective is to understand how social learning and persuasion affect the take-up of a savings commitment product by rural women. The core of our conceptual framework can be summarized as three distinct causal mechanisms about individual adoption of the savings product by secondary participants.

Mechanism M1: A secondary participant is given information about a newly available product by a peer. This information changes her beliefs about the material costs and benefits of adoption. If the expected utility of adoption is positive, she adopts and engages with the product by making more deposits on her

---

<sup>4</sup>Whereas, in Table A1 and A2 of Appendix A.2, we report the same summary statistics separate for A and B participants, respectively.

**Table 2: Summary statistics: full sample**

	(1)	(2)	(3)	(4)	(5)
	Mean	Median	S.D.	Min	Max
<b>Demographics:</b>					
Age of respondent	29.65	29.00	6.52	18	56
Education of respondent	8.78	10.00	4.21	0	20
Can read and write [0,1]	0.79	1.00	0.41	0	1
Years of marriage	8.73	8.00	6.53	0	50
Household size	5.32	5.00	1.79	1	28
Group A [0,1]	0.24	0.00	0.43	0	1
Age of husband	34.21	34.00	7.17	18	65
Education of husband	8.99	10.00	4.49	0	30
<b>Income and assets:</b>					
Housewife [0,1]	0.82	1.00	0.38	0	1
Income from labor of respondent (INR)	582.39	0.00	2309.19	0	40000
Own home [0,1]	0.96	1.00	0.20	0	1
Own farm [0,1]	0.64	1.00	0.48	0	1
Amount of farm land (Bigha)	3.15	2.00	6.29	0	250
<b>Savings and borrowings:</b>					
Have you ever visited a bank? [0,1]	0.92	1.00	0.27	0	1
Self-reported formal savings, last 6 months (INR)	2049.23	500.00	5053.09	0	50000
Self-reported informal savings, last 6 months (INR)	503.73	0.00	1112.70	0	12000
Self-reported formal borrowings, last 6 months (INR)	852.59	0.00	16755.95	0	500000
Self-reported informal borrowings, last 6 months (INR)	204.93	0.00	6838.17	0	500000
Deposit at least once a week [0,1]	0.06	0.00	0.24	0	1
Deposit at least once a month [0,1]	0.25	0.00	0.43	0	1
Deposit once every few months [0,1]	0.54	1.00	0.50	0	1
Deposit once a year [0,1]	0.15	0.00	0.35	0	1
Risk attitude (0: low risk; 10: high risk)	4.52	4.00	2.10	0	9
<b>Treatment status:</b>					
C (T1)	0.20	0.00	0.40	0	1
A (T2)	0.20	0.00	0.40	0	1
I (T3)	0.20	0.00	0.40	0	1
P (T4)	0.20	0.00	0.40	0	1
I+P (T5)	0.20	0.00	0.40	0	1
Villages	180				
Observations	5958				

Notes: The Table reports the mean, standard deviation, minimum and maximum values of the main variables used in the paper. There is a total of 5,958 women in our sample, living in 180 villages in Uttar Pradesh, India.

savings account and keeping a higher average balance over the duration of our intervention (i.e., the time window over which the bonus can be earned).

Mechanism M2a: A secondary participant is persuaded by a peer who argues that take-up is the best course of action, e.g., because of self or social image concerns. If the secondary participant is persuaded, she adopts and subsequently learn the material costs and benefits of adoption. Some of the adopters realize their expected utility from engaging with the product is negative, they do not continue making savings deposits, and as a result hold a low average savings balance.

Mechanism M2b: A secondary participant has limited agency and a (pent-up) desire for more autonomy. She is subjected to the persuasion of a peer who offers moral support and argues that take-up is the best course of action for self-empowerment. If the secondary participant is persuaded, she adopts and engages with the product by keeping a higher average balance over the duration of our intervention.

Mechanism M3: A secondary participant is given information about a newly available product by a peer who argues that take-up is the best course of action and offers moral support. This combination of information and support changes the secondary participant's beliefs about the material and subjective costs and benefits of adoption. If the expected utility of adoption is positive, she adopts and engages with the product by keeping a higher average balance over the duration of our intervention.

## 4.1 Hypotheses

We test two types of analysis of hypotheses: reduced-form treatment effects; and mechanisms. In the first analysis, we estimate the effect of the different treatments on: knowledge of the savings product; take-up of the savings product; and household welfare. In the second, we seek to throw light on the mechanism behind the reduced-form treatment effects. Take-up is a dummy variable equal to 1 if the participants signs up for the savings commitment contract, and knowledge of the product is an index constructed as the sum of correct answers to a quiz about the savings product.

The first type of analysis is as follows. Regarding knowledge and take-up, our three main hypotheses are as follows:

H1: Knowledge and take-up of the savings product arise from the diffusion of information about the product and its anticipated benefits, i.e., from T3 and T5.

H2: Take-up of the savings product arises purely from the persuasion of peers, i.e., from T4 and T5, without increase in knowledge about the savings product.

H3: Take-up is highest if both mechanisms are at play, i.e., information diffusion and persuasion from peers, i.e., in T5.

These three hypotheses are tested by regressing knowledge and take-up on the three treatment dummies and testing differences in coefficients within regressions. As measure of knowledge, we will use the number of correct responses to the knowledge quiz.

Regarding economic outcomes, our primary outcome is engagement with the product as measured by the saving balances at the end of the intervention. Our main hypothesis is as follows:

H4: Informed take-up (*T3* and *T5*) of the savings product increases engagement with the product as measured by the savings balance at the end of the intervention (or a finer measure of engagement, such as the average balance during the intervention period, if participants share their passbooks with us). Uninformed take-up (*T4*) has an ambiguous effect a priori.

We also have a number of important secondary outcomes, with the following associated hypotheses:

H5: Informed take-up (*T3* and *T5*) of the savings product increases investment in lumpy expenditures, measured as total expenditures on durables, house repairs, livestock purchases, and business investment. Uninformed take-up (*T4*) has an ambiguous effect a priori.

H6: Informed take-up (*T3* and *T5*) of the savings product increases household income. Uninformed take-up (*T4*) has an ambiguous effect a priori.

H7: Take-up of the savings product resulting from persuasion (*T4* and *T5*) raises female empowerment, measured as the first factor from a principal component analysis of women's individual income, women's involvement in household finances, and indicators for life satisfaction, sense of financial security, and mental health.

To refine the interpretation – and verify the robustness – of our results regarding the welfare benefits of adoption, we will also examine the effect of the treatments on individual secondary outcomes. In particular, for H4, we will look at the frequency of deposits and the average balance kept over the duration of the study. For H5, we will separately look at expenditures on durables, house repairs, livestock purchases, and business investment. For H6, we will examine welfare indicators based on consumption and nutrition. For H7, we will test the effect of treatment on the different variables entering the principal component analysis.

The second type of analysis is as follows:

H8: In information treatments, *T3* and *T5*, knowledge of the savings product is correlated between the primary participant and the three secondary participants assigned to her.

H9: In all treatments, including *T4*, the take-up of the savings product is correlated between the primary participant and the three secondary participants assigned to her.

## 4.2 Empirical specification

This section details our estimation strategy and relates estimated parameters to hypotheses H1 to H7. The main equation that we estimate is the following ITT ANCOVA specification:<sup>5</sup>

$$y_{iv} = \beta_1 + \beta_2 T2_{iv} + \beta_3 T3_{iv} + \beta_4 T4_{iv} + \beta_5 T5_{iv} + \gamma \bar{y}_{iv} + \delta_s + \varepsilon_{iv} \quad (1)$$

where for participant  $i$ , in village  $v$ ,  $y_{iv}$  is a vector of outcome variables, which includes knowledge of the product, take-up of the product, and the economic outcomes listed in H4 to H7. The  $T1, \dots, T5$  variables are dummies for assignment to the four different treatments, keeping the pure control group T1 as the omitted category.  $T2_{iv}$  is equal to 1 if the participant is in the T2 treatment group (the pure access treatment group, A) and 0 otherwise.  $T3_{iv}$  is equal to 1 if the participant is in the T3 treatment group (the information treatment group, I) and 0 otherwise.  $T4_{iv}$  is equal to 1 if the participant is in the T4 treatment group (the persuasion treatment group, P) and 0 otherwise.  $T5_{iv}$  is equal to 1 if the participant is in the T5 treatment group (the information + persuasion treatment group, I+P) and 0 otherwise.

ANCOVA is preferred over a First Difference estimator or a Difference-in-Difference estimator when the autocorrelation of the outcome variable is low (McKenzie, 2012), making it particularly suited in our context. Controls therefore include  $\bar{y}_{iv}$ , the mean of the pre-treatment outcome for participant  $i$ . For information only collected at endline, such as the S&G knowledge quiz and the female empowerment indicators, the  $\bar{y}_{iv}$  regressor is omitted. Take-up is null at baseline by construction, which means that coefficient  $\gamma$  is subsumed in the intercept in that case. We also include a strata-specific fixed effect  $\delta_s$ , while  $\varepsilon_{iv}$  is an idiosyncratic error term.

Our main sample of interest is the secondary participants. We also estimate model (1) on primary participants to verify that our experiment reproduces commonly observed outcomes among participants who are directly targeted by the intervention. Regression model (1) will be estimated separately for primary participants and for secondary participants, clustering standard errors at the village level.<sup>6</sup> This is because these participants do not receive the same intervention.

When estimating treatment impacts, we will report  $p$ -values corrected for multiple hypothesis testing (MHT) using Westfall and Young (1993) for the primary outcome of H4 (savings balance). We will do MHT correction only across the coefficients on T3, T4, and T5 of that regression. All other outcomes and regression coefficients (in H1-H3, and H5-H9) will be treated as “secondary” outcomes and hypotheses.

Since the causal effect of the treatments on economic outcomes must, almost surely, be channelled through engagement with the product, we will also estimate a 2SLS regression of the form:

$$y_{iv} = \alpha_1 + \alpha_2 b_{iv}^* + \delta_s + \varepsilon_{iv} \quad (2)$$

in which the dependent variable  $y_{iv}$  is an outcome of interest and the main regressor  $b_{iv}^*$  is engagement with the product, e.g., average savings balance, instrumented with the different treatments using equation (1). This

<sup>5</sup>We also collect a large set of covariates  $X$  which can be included in the main specification to increase precision using ML methods (e.g., Chernozhukov et al., 2018). Based on the pilot, we have identified the following candidates for ML selection: farm land ownership, amount of farm land, income from labor at baseline, family size, age, age squared, education level, literacy, caste, years of marriage and risk attitude.

<sup>6</sup>As a robustness check, we will also calculate wild bootstrap t-statistics (e.g., Cameron et al., 2011).

will produce an estimate of the effect of savings on the outcome variables of interest.

For our second type of analysis (H8 and H9), we estimate, for each treatment separately, the sample correlation in knowledge and take-up between primary participants and the secondary participants assigned to them. We will then use randomization inference to compare these estimates to the simulated distribution of correlation coefficients that would arise with a counterfactual random assignment to peers.

### 4.3 Mechanisms behind take-up

Our experiment is designed to distinguish between mechanisms M1, M2a-b, or M3 by comparing treatment effects on take-up and knowledge of the product:

- Under Mechanism M1, average take-up and knowledge are such that  $T5 = T3 > T4 = T2 = 0$ .
- Under Mechanisms M2a or M2b, average take-up is such that  $T5 = T4 > T3 = T2 = 0$ . Knowledge about the product is low in all treatments.
- Under Mechanism M3, average take-up is such that  $T5 > T4 > T2 = 0$ ,  $T5 > T3 > T2 = 0$  but  $T4$  and  $T3$  are not ranked a priori. Knowledge is such that  $T5 = T3 > T4 = T2 = 0$ .

Our experiment is also designed to distinguish between mechanisms M1, M2a-b, or M3 indirectly from treatment effects on engagement with the product – as measured by the average savings balance – and, consequently, on household economic welfare and women’s empowerment:

- Under Mechanism M1, average savings balance and household welfare measures are such that  $T5 = T3 > T4 = T2 = 0$ . The effect on female empowerment is ambiguous.
- Under Mechanism M2a, average savings balance and household welfare measures are such that  $T5 = T3 < T4 = T2 = 0$  because of excess/misinformed adoption from persuasion. The effect on female empowerment is ambiguous, but possibly negative since the peer is not benevolent.
- Under Mechanism M2b, average savings balance and female empowerment are such that  $T5 = T3 > T4 = T2 = 0$ . The effect on average household welfare is ambiguous, but possibly positive if the interests of the female participant and the household are aligned.
- Under Mechanism M3, average household welfare is such that  $T5 \geq T3 \geq T2 = 0$  and  $T5 \geq T4 \geq T2 = 0$ , depending on the respective strengths of information and persuasion.  $T4$  and  $T3$  are not ranked a priori.

Additional evidence on mechanisms includes the following:

- Under Mechanisms M1 and M3, we should observe correlation in the knowledge level of the primary participants and each of their assigned secondary participants. This is because, in these two mechanisms, secondary participants are assumed to receive information directly from their primary participant.

- Under mechanisms M2a and M2b, we will test whether primary participants respond to incentivization by testing whether secondary participants who sign up for S&G do so in the presence of their assigned primary participant (which is required for receiving the reward).
- Under Mechanism M2b, correlation in adoption and in engagement between the primary participants and each of their assigned secondary participants may possibly be interpreted as a manifestation of support by the primary participant – and thus as evidence in favor of M2b.

These tests complement those discussed in the previous subsection as they serve to confirm the interpretation of the reduced-form results.

#### 4.4 Possible confounds

- Imitation: in treatments  $T3$ , reduced-form analysis does not allow to distinguish take-up by secondary participants that is due to information diffusion from adoption purely due to imitation. We can, however, test imitation indirectly by examining, in treatment  $T3$ , whether the adoption behavior of the primary participant is correlated with that of the three unseeded participants she is assigned to (as in, e.g., [Fafchamps et al., 2021](#)), and by comparing this to the correlation in product knowledge between them. If the primary participant is knowledgeable about the product while her three assigned secondary participants are not, but their take-up choices are correlated with that of the primary participant, this will indicate that imitation may be at play. In addition, imitators may lose momentum after the initial sign-up, and thus have a lower average savings balance than informed adopters. Both conjectures are testable from the data.
- Incentivization and effort: in  $T4$  and  $T5$ , the objective of incentivization is to induce the primary participant to put more effort in convincing their assigned secondary participants to sign up for the product. In treatment  $T5$ , the convincing work of the primary participant is facilitated by the detailed information she has been provided on the product. We therefore expect the primary participant to make use of this information when trying to convince the three secondary participants. But the primary participant may also try to convince others using non-informative techniques, such as persuasion, intimidation, or sharing the reward. In treatment  $T4$ , however, she can only use these non-informative techniques since no information is provided to participants other than the existence of the product. In this case, we expect incentivization to be less successful in  $T4$  than in  $T5$ , and thus sign-up to be lower. These predictions can all be tested from the data.
- Sharing the reward: if the sign-up reward is large enough, primary participants in  $T4$  and  $T5$  may induce sign-up by offering to share the reward with the secondary participant. While we cannot rule out that the reward may be shared, its magnitude is inferior to the financial effort that signing up represents: the primary participant receives a fixed payment of 50 INR for being present at sign-up only if the secondary participant is eligible, which requires having deposited a minimum of 100 INR on her account in the preceding month. This makes it unlikely that participants would sign up simply to share the reward. But if they did, we would expect no engagement with the product after sign-up by pairs primary and secondary participants. This is testable from the data.

- Signaling: secondary participants in *T4* and *T5* may interpret the reward as inducing the primary participant to misdirect them. Since this possibility is shared by both treatments, it does not affect our main testing strategy. But this signaling effect, if present, may reduce the ability of primary participants to convince others to adopt, as shown for instance by [Deserrano et al. \(2022\)](#). This will be reflected in the effectiveness of the persuasion treatment to induce adoption, especially in treatment *T4*. We see this as increasing the external validity of our findings to common situations when referral-by-clients is rewarded. If the dissuasive effect of signaling is strong enough, we expect less sign-up in *T5* than in *T3*, something we can test from the data.
- Informed primary participants in *T4*: In *T4*, primary participants are not provided with any information on the product – except that it exists. But anyone who adopts the product acquires some information about its costs and benefits. It is therefore conceivable that primary participants who have adopted the product may possess information about the costs and benefits of the product, and pass this information to secondary participants to induce adoption. We will test for this possibility in two ways: (1) we will compare adoption among the secondary participants of primary participants who have adopted to adoption among the secondary participants of primary participants who did not adopt; (2) we will compare the adoption of secondary participants as a function of the knowledge score of their assigned primary participant: if there is information transmission by the primary participant, we should observe higher adoption when the score of the primary participant is higher.

## 4.5 Treatment effect heterogeneity

There are three key dimensions of treatment effect heterogeneity that we will explore for our main outcome variables:

1. Initial level of saving behaviour. In the baseline questionnaire, we collected a set of questions related to saving behaviours and the value of saving, such as “What are your main saving goals?”, “How often do you save money?”, “Have you had a saving plan for old age?”, and “What is the least likely reason to cause savings to lose their value?”. We will examine treatment effect heterogeneity by different levels of baseline saving behaviours.
2. Decision making. In the baseline questionnaire, we collected several questions related to decision making within the household, which were self-reported by the eligible woman.<sup>7</sup> We will examine treatment effect heterogeneity by baseline median decision making level. We will also use the set of questions related to mobility as proxy for social network.
3. Social desirability. In the baseline questionnaire, we collected the 13 items of the Marlowe-Crowne scale related to social desirability ([Crowne and Marlowe, 1960](#)).<sup>8</sup> We will examine treatment effect heterogeneity by baseline median social desirability level. This analysis will be particularly useful to

<sup>7</sup>Specifically, we asked: “Who is the main decision maker on each of the following topic?” The questionnaire contains 12 standard topics (e.g., expenditure decisions on specific items, and decisions in general) where the answers can be either “You”, “Spouse”, “Both”, or “Others”.

<sup>8</sup>The scale, developed by psychologists, has been validated in various contexts (e.g., [Dhar et al., 2022](#)). It asks whether respondents have various too-good-to-be-true personality traits such as whether respondents are excellent listeners or never hurting anyone’s feelings on purpose to create a social desirability bias.

understand how actual saving behaviours of B secondary participants are affected by their “desire to fit in”. Whereas, this type of heterogeneity analysis will be less important for A primary participants.

## 5 Preliminary results from the pilot

The trial was piloted between September 2020 and January 2021 in 15 rural villages from the State of Bihar, India. Figure A4 provides a map of the areas that we covered in the pilot. We conducted the baseline in October 2020 with a randomly selected sample of 585 eligible women aged 18-40 years. Table A7 presents the summary statistics. After finalizing the intervention, we randomized the villages into one pure control (equivalent to T1) and three treatment groups I, P, I+P (equivalent to T3, T4, and T5, respectively). Table A8 shows that the groups were balanced. Finally, we ran the intervention during the first week of December 2020, and collected the endline in January 2021, one month after the intervention.

The pilot was designed exactly like the main trial, except that: (i) we randomized villages into four groups, not five; (ii) the implementation of the trial was conducted mostly on the phone, not in person, following the Covid-19 protocol that was in place at the time; and (iii) to receive the compensation of S&G (a higher interest rate compared to the market), eligible participants were encouraged to save via formal financial institutions for at least one month, not three months.<sup>9</sup> To estimate the effect of each treatment, we compare the outcomes – formal savings as stated on passbooks – of the three treatment groups with that of the control group. The pilot therefore essentially attempted to see how we could foster the adoption of our innovative financial product using information dissemination versus persuasion.

The main results of the pilot are twofold. First of all, each of the treatments was effective at increasing savings in formal institutions among secondary participants. Figure 3 above shows that mean savings reported on the passbooks at endline increased by 241 INR in T3 group and 308 INR in T4 group, against 159 INR in the pure control group T1. Table A9 in the Appendix reports that the effect sizes, calculated using equation (1), were large – 0.32 SD for T3 and 0.40 SD for T4.<sup>10</sup> These effects, though significantly larger compared to the control group, were not significantly different from each other. Second, information dissemination and persuasion were more effective when applied together compared to when these interventions were considered separately. Specifically, formal savings increased by 471 INR in T5, which corresponds to 0.78 SD at endline.<sup>11</sup>

## 6 Limitations and Challenges

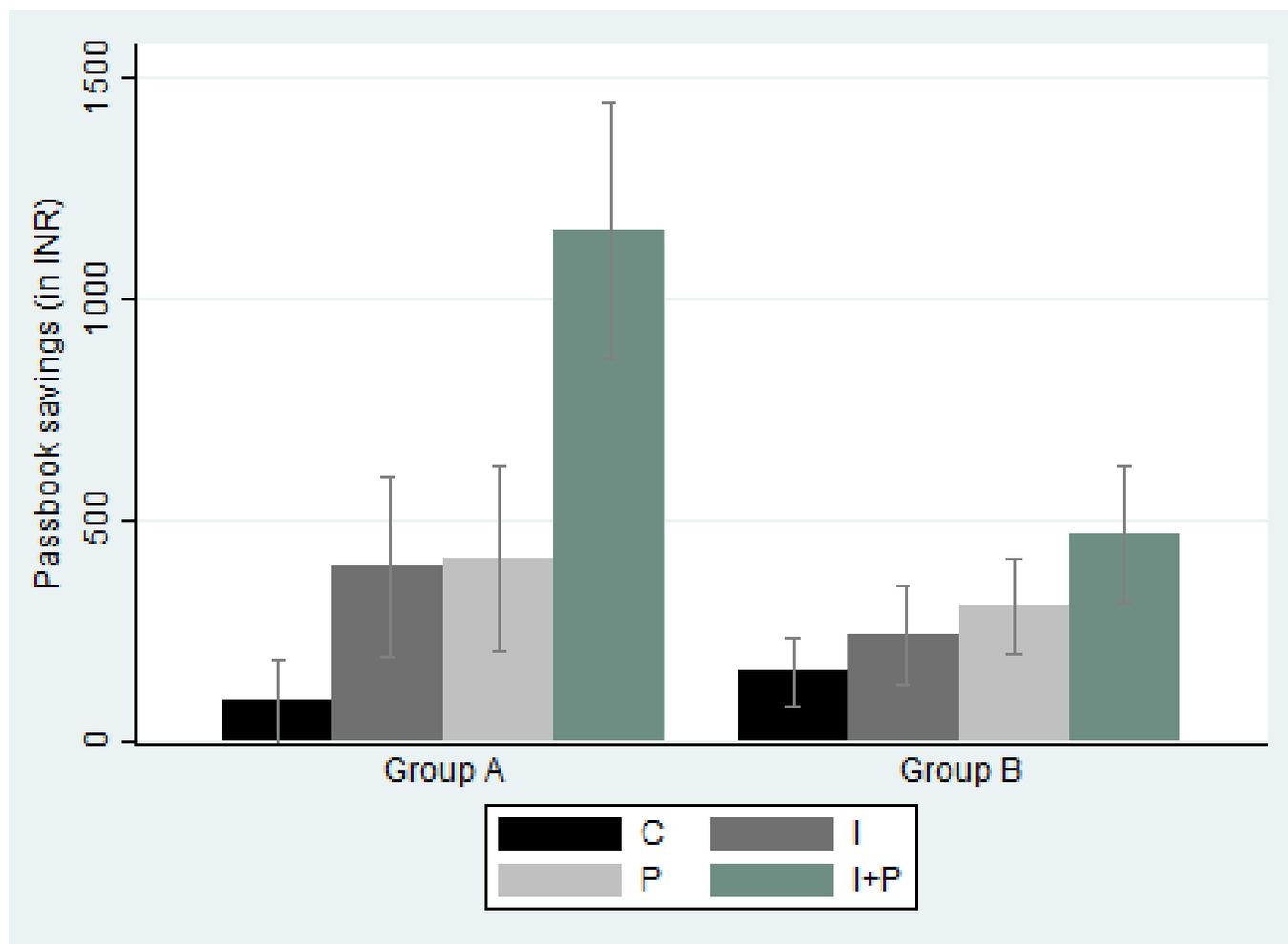
We will conduct our analysis using mainly ITT parameters. Nevertheless, the intervention we consider consists of offering an innovative financial service. An important first step in our analysis is to describe the take-up in the different treatment and control groups for this service. We will therefore examine in each treatment and control group the take-up for S&G.

<sup>9</sup>Differently from the presentation of the S&G financial product, and the different incentive schemes in I, P, and I+P groups, passbooks verification was conducted onsite by the enumerators at the endline. The team followed the Covid-19 protocol that was in place at the time.

<sup>10</sup>The specification includes dummies for information villages (I group), persuasion villages (P group) or information and persuasion villages (I+P group), as well as the following set of covariates: self-reported formal savings at baseline, farm land ownership, amount of farm land, income from labor at baseline, family size, age, age squared, education level, literacy, caste, years of marriage and risk attitude. Standard errors are clustered at village level.

<sup>11</sup>We observe some formal savings in the bank also by the control group which is not driven by our intervention. On average, primary participants save 92 INR and secondary participants save 159 INR. The difference is not statistically significant.

**Figure 3:** Mean savings in INR reported on passbooks at endline (last 1 month) by different groups: pilot sample



Notes: Mean savings in INR reported on passbooks at endline (last 1 month) by treatment group and type of participant.

There are other potential concerns that we considered and protected against in our design.

First, primary participants in *T3* have no information about who the secondary participants are, whereas, by design, primary participants in *T4* and *T5* know about three secondary participants. To deal with this potential asymmetry, in our design we let primary participants in *T3* villages know the names of three randomly selected secondary participants.

Second, having a pure access treatment *T2* is important for us. Indeed, suppose we do not have it and we find an effect for *T3*, *T4*, and *T5* compared to pure control *T1*. One could then argue that this effect arises just because we provided S&G. So, without *T2* control households, we would be unable to disentangle the existence of the product from the mechanisms we are interested in.

Third, at baseline, we did not inform survey respondents that a stall would be set up, and we did not tell them that they could approach the people at the stall to get information about S&G since this would defeat the information treatment.<sup>12</sup> We also instructed the people manning the stall not to answer questions or circulate information about the savings commitment product. The stall is there only to sign people up and verify their passbooks. In addition, those manning the stalls have been instructed to ask villagers' names first, to check whether the name is in the primary or secondary participant list, and only invite the person to sit down for sign-up if their name appears on the list.

<sup>12</sup>Furthermore, once villagers realize stall enumerators only spoke to people with a card, they would simply pass the card to each other.

Fourth, information on formal savings is collected at the endline on passbooks. If women in our sample have such passbooks, we will simply ask to see their passbooks. If they instead get monthly bank statements, we will ask to see the bank statements. Hence, there is no need for cooperation with the bank. Women in our sample must consent to show us their passbook or statement, which is required to participate in the experiment.

## **7 Administrative information**

Our project poses minimal risk to participants. Our project has been reviewed primarily by Indian Institute of Technology Kanpur, Institutional Review Board (IRB). The data collection involves no more risk than is typical for standard household survey questions on savings, employment, and gender role attitudes. Care has been taken to minimize COVID-19 risks, including use of masks and sanitizers by enumerators and efforts to undertake interviews outdoors whenever possible.

**Funding:** The project is directed jointly by the Monash Centre for Development Economics and Sustainability (CDES), the Monash South-Asia Research Network (SARN) and the Indian Institute of Technology (IIT) Kanpur. The experiment is supported by Monash Business School (Islam and Tommasi) and International Growth Centre (IGC) (Islam and Fafchamps).

**Institutional Review Board (ethics approval):** This study's protocols have been reviewed and approved by Institutional Ethic Committee (IRB) at the Indian Institute of Technology Kanpur (IRB Approval Number IITK/IEC/2019-20-II/17).

**Declaration of interest:** None.

## References

- AFZAL, U., G. D'ADDA, M. FAFCHAMPS, S. QUINN, AND F. SAID (2018): "Two Sides of the Same Rupee? Comparing Demand for Microcredit and Microsaving in a Framed Field Experiment in Rural Pakistan," *Economic Journal*, 128, 2161–2190.
- AFZAL, U., G. D'ADDA, M. FAFCHAMPS, S. R. QUINN, AND F. SAID (2019): "Implicit and Explicit Commitment in Credit and Saving Contracts: A Field Experiment," Working Paper 25802, National Bureau of Economic Research.
- AKBARPOUR, M., S. MALLADI, AND A. SABERI (2023): "Just a Few Seeds More: Value of Network Information for Diffusion," Graduate School of Business, Stanford University.
- AKER, J. C., R. BOUMNIJEL, A. MCCLELLAND, AND N. TIERNEY (2016): "Payment Mechanisms and Antipoverty Programs: Evidence from a Mobile Money Cash Transfer Experiment in Niger," *Economic Development and Cultural Change*, 65, 1–37.
- ALATAS, V., A. G. CHANDRASEKHAR, M. MOBIUS, B. A. OLKEN, AND C. PALADINES (2019): "When Celebrities Speak: A Nationwide Twitter Experiment Promoting Vaccination In Indonesia," Working Paper 25589, National Bureau of Economic Research.
- ASHRAF, N., D. KARLAN, AND W. YIN (2010): "Female Empowerment: Impact of a Commitment Savings Product in the Philippines," *World Development*, 38, 333–344.
- ATTANASIO, O. AND V. LECHENE (2002): "Tests of Income Pooling in Household Decisions," *Review of Economic Dynamics*, 5, 720–748.
- ATTANASIO, O. P. AND V. LECHENE (2014): "Efficient Responses to Targeted Cash Transfers," *Journal of Political Economy*, 122, 178–222.
- BANDIERA, O. AND I. RASUL (2006): "Social Networks and Technology Adoption in Northern Mozambique," *The Economic Journal*, 116, 869–902.
- BANERJEE, A., E. BREZA, A. G. CHANDRASEKHAR, AND M. MOBIUS (2021): "Naïve Learning with Uninformed Agents," *American Economic Review*, 111, 3540–74.
- BANERJEE, A., A. G. CHANDRASEKHAR, E. DUFLO, AND M. O. JACKSON (2013): "The Diffusion of Microfinance," *Science*, 341, 1236498.
- BANERJEE, A. V. (1992): "A Simple Model of Herd Behavior\*," *The Quarterly Journal of Economics*, 107, 797–817.
- BATISTA, C., M. FAFCHAMPS, AND P. VICENTE (2023): "A Field Experiment on Information Sharing in Social Networks," *World Bank Economic Review*.
- BATISTA, C., M. FAFCHAMPS, AND P. C. VICENTE (2022): "Keep It Simple: A Field Experiment on Information Sharing among Strangers," *World Bank Economic Review*, 36, 857–888.
- BATISTA, C. AND P. C. VICENTE (2020): "Improving access to savings through mobile money: Experimental evidence from African smallholder farmers," *World Development*, 129, 104905.
- BEAMAN, L., A. BENYISHAY, J. MAGRUDER, AND A. M. MOBARAK (2021): "Can Network Theory-Based Targeting Increase Technology Adoption?" *American Economic Review*, 111, 1918–43.

- BENYISHAY, A. AND A. M. MOBARAK (2018): “Social Learning and Incentives for Experimentation and Communication,” *The Review of Economic Studies*, 86, 976–1009.
- (2019): “Social Learning and Incentives for Experimentation and Communication,” *Review of Economic Studies*, 86, 976–1009.
- BERG, E., M. GHATAK, R. MANJULA, D. RAJASEKHAR, AND S. ROY (2019): “Motivating knowledge agents: Can incentive pay overcome social distance?” *Economic Journal*, 129, 110–142.
- BERG, E., R. MANJULA, AND D. RAJASEKHAR (2022): “Pushing welfare: Encouraging awareness and uptake of social benefits in South India,” *Economic Development and Cultural Change*, 70, 901–939.
- BERNAYS, E. (2004): *Propaganda*, Brooklyn, NY: Ig Publishing, first published in 1928.
- BURSZTYN, L., G. EGOROV, AND S. FIORIN (2020a): “From Extreme to Mainstream: The Erosion of Social Norms,” *American Economic Review*, 110, 3522–48.
- BURSZTYN, L., A. L. GONZÁLEZ, AND D. YANAGIZAWA-DROTT (2020b): “Misperceived Social Norms: Women Working Outside the Home in Saudi Arabia,” *American Economic Review*, 110, 2997–3029.
- BURSZTYN, L. AND R. JENSEN (2017): “Social Image and Economic Behavior in the Field: Identifying, Understanding, and Shaping Social Pressure,” *Annual Review of Economics*, 9, 131–153.
- CAEYERS, B. AND M. FAFCHAMPS (2023): “Exclusion Bias and the Estimation of Peer Effects,” Working paper.
- CAI, J., A. DE JANVRY, AND E. SADOULET (2015): “Social Networks and the Decision to Insure,” *American Economic Journal: Applied Economics*, 7, 81–108.
- CAI, J. AND A. SZEIDL (2017): “Interfirm Relationships and Business Performance\*,” *The Quarterly Journal of Economics*, 133, 1229–1282.
- CALLEN, M., S. DE MEL, C. MCINTOSH, AND C. WOODRUFF (2019): “What Are the Headwaters of Formal Savings? Experimental Evidence from Sri Lanka,” *The Review of Economic Studies*, 86, 2491–2529.
- CAMERON, A. C., J. B. GELBACH, AND D. L. MILLER (2011): “Robust Inference With Multiway Clustering,” *Journal of Business Economic Statistics*, 29, 238–249.
- CARTER, M., R. LAAJAJ, AND D. YANG (2021): “Subsidies and the African Green Revolution: Direct Effects and Social Network Spillovers of Randomized Input Subsidies in Mozambique,” *American Economic Journal: Applied Economics*, 13, 206–29.
- CENTOLA, D. (2010): “The Spread of Behavior in an Online Social Network Experiment,” *Science*, 329, 1194–1197.
- (2011): “An Experimental Study of Homophily in the Adoption of Health Behavior,” *Science*, 334, 1269–1272.
- CHERNOZHUKOV, V., D. CHETVERIKOV, M. DEMIRER, E. DUFLO, C. HANSEN, W. NEWEY, AND J. ROBINS (2018): “Double/debiased machine learning for treatment and structural parameters,” *The Econometrics Journal*, 21, C1–C68.

- COLE, S. A. AND A. N. FERNANDO (2020): “‘Mobile’izing Agricultural Advice Technology Adoption Diffusion and Sustainability,” *The Economic Journal*, 131, 192–219.
- CONLEY, T. G. AND C. R. UDRY (2010): “Learning about a New Technology: Pineapple in Ghana,” *American Economic Review*, 100, 35–69.
- CORNO, L., E. L. FERRARA, AND J. BURNS (2022): “Interaction, Stereotypes, and Performance: Evidence from South Africa,” *American Economic Review*, 112, 3848–3875.
- COX, D. AND M. FAFCHAMPS (2007): “Chapter 58 Extended Family and Kinship Networks: Economic Insights and Evolutionary Directions,” Elsevier, vol. 4 of *Handbook of Development Economics*, 3711–3784.
- CROWNE, D. AND D. MARLOWE (1960): “A New Scale of Social Desirability Independent of Psychopathology,” *Journal of consulting psychology*, 24, 349–54.
- DE MEL, S., C. MCINTOSH, K. SHETH, AND C. WOODRUFF (2020): “Can Mobile-Linked Bank Accounts Bolster Savings? Evidence from a Randomized Controlled Trial in Sri Lanka,” *The Review of Economics and Statistics*.
- DEMIRGUC-KUNT, A., L. KLAPPER, D. SINGER, AND S. ANSAR (2022): “The Global Findex Database 2021: Financial Inclusion, Digital Payments, and Resilience in the Age of COVID-19,” *Washington, DC: World Bank*.
- DESERRANO, E., G. LEON, AND F. WITOELAR (2022): “When Transparency Fails: Financial Incentives for Local Banking Agents in Indonesia,” Working paper.
- DHAR, D., T. JAIN, AND S. JAYACHANDRAN (2022): “Reshaping Adolescents’ Gender Attitudes: Evidence from a School-Based Experiment in India,” *American Economic Review*, 112, 899–927.
- DUFLO, E., M. KREMER, AND J. ROBINSON (2011): “Nudging Farmers to Use Fertilizer: Theory and Experimental Evidence from Kenya,” *American Economic Review*, 101, 2350–90.
- DUPAS, P., D. KARLAN, J. ROBINSON, AND D. UBFAI (2018): “Banking the Unbanked? Evidence from Three Countries,” *American Economic Journal: Applied Economics*, 10, 257–97.
- DUPAS, P., A. KEATS, AND J. ROBINSON (2017): “The Effect of Savings Accounts on Interpersonal Financial Relationships: Evidence from a Field Experiment in Rural Kenya,” *The Economic Journal*.
- DUPAS, P. AND J. ROBINSON (2013a): “Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya,” *American Economic Journal: Applied Economics*, 5, 163–92.
- (2013b): “Why Don’t the Poor Save More? Evidence from Health Savings Experiments,” *American Economic Review*, 103, 1138–71.
- ESGUERRA, E., L. VOLLMER, AND J. WIMMER (2023): “Influence Motives in Social Signaling: Evidence from COVID-19 Vaccinations in Germany,” *AER: Insights*, 5, 275–291.
- FAFCHAMPS, M., A. ISLAM, A. MALEK, AND D. PAKRASHI (2021): “Mobilizing P2P Diffusion for New Agricultural Practices: Experimental Evidence from Bangladesh,” *World Bank Economic Review*, 35, 1076–1101.
- FAFCHAMPS, M., A. ISLAM, M. A. MALEK, AND D. PAKRASHI (2020): “Can referral improve targeting? Evidence from an agricultural training experiment,” *Journal of Development Economics*, 144, 102436.

- FAFCHAMPS, M. AND D. MO (2018): “Peer Effect Heterogeneity in Computer Assisted Learning: Evidence from a Randomized Experiment,” *Experimental Economics*, 21, 355–382.
- FAFCHAMPS, M. AND S. QUINN (2018): “Networks and Manufacturing Firms in Africa: Results from a Randomized Field Experiment,” *World Bank Economic Review*, 32, 656–675.
- FAFCHAMPS, M., M. SÖDERBOM, AND M. VAN DEN BOOGART (2022): “Adoption with Social Learning and Network Externalities\*,” *Oxford Bulletin of Economics and Statistics*, 84, 1259–1282.
- FIELD, E., R. PANDE, N. RIGOL, S. SCHANER, AND C. TROYER MOORE (2021): “On Her Own Account: How Strengthening Women’s Financial Control Impacts Labor Supply and Gender Norms,” *American Economic Review*, 111, 2342–75.
- FOSTER, A. D. AND M. R. ROSENZWEIG (1995): “Learning by Doing and Learning from Others: Human Capital and Technical Change in Agriculture,” *Journal of Political Economy*, 103, 1176–1209.
- HJORT, J., D. MOREIRA, G. RAO, AND J. F. SANTINI (2021): “How Research Affects Policy: Experimental Evidence from 2,150 Brazilian Municipalities,” *American Economic Review*, 111, 1442–80.
- IMBENS, G. W. (2011): “Experimental design for unit and cluster randomid trials,” International initiative for impact evaluation paper.
- IMBENS, G. W. AND D. B. RUBIN (2015): *Causal inference in statistics, social, and biomedical sciences*, Cambridge University Press.
- JACKSON, M. O., B. W. ROGERS, AND Y. ZENOU (2017): “The Economic Consequences of Social-Network Structure,” *Journal of Economic Literature*, 55, 49–95.
- KARLAN, D. AND J. ZINMAN (2018): “Price and control elasticities of demand for savings,” *Journal of Development Economics*, 130, 145–159.
- KELLEY, E., C. KSOLL, AND J. MAGRUDER (2022): “How do Online Job Portals affect Employment and Job Search? Evidence from India,” Tech. rep.
- KONDYLIS, F., V. MUELLER, AND J. ZHU (2017): “Seeing is believing? Evidence from an extension network experiment,” *Journal of Development Economics*, 125, 1–20.
- LEE, D. S. (2009): “Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects,” *The Review of Economic Studies*, 76, 1071–1102.
- MCKENZIE, D. (2012): “Beyond baseline and follow-up: The case for more T in experiments,” *Journal of Development Economics*, 99, 210–221.
- MERIGGI, N. F., E. BULTE, AND A. M. MOBARAK (2021): “Subsidies for technology Adoption: Experimental evidence from rural Cameroon,” *Journal of Development Economics*.
- MILLER, G. AND A. M. MOBARAK (2015): “Learning About New Technologies Through Social Networks: Experimental Evidence on Nontraditional Stoves in Bangladesh,” *Marketing Science*, 34, 480–499.
- NAVA, A., D. KARLAN, AND W. YIN (2006): “Deposit Collectors,” *The B.E. Journal of Economic Analysis & Policy*, 5, 1–24.
- OSTER, E. AND R. THORNTON (2012): “Determinants of Technology Adoption: Peer Effects in Menstrual Cup Take-Up,” *Journal of the European Economic Association*, 10, 1263–1293.

- PORTER, C. AND D. SERRA (2020): “Gender Differences in the Choice of Major: The Importance of Female Role Models,” *American Economic Journal: Applied Economics*, 12, 226–54.
- PRINA, S. (2015): “Banking the poor via savings accounts: Evidence from a field experiment,” *Journal of Development Economics*, 115, 16–31.
- RILEY, E. (2020): “Resisting Social Pressure in the Household Using Mobile Money: Experimental Evidence on Microenterprise Investment in Uganda,” .
- SCHANER, S. (2015): “Do Opposites Detract? Intrahousehold Preference Heterogeneity and Inefficient Strategic Savings,” *American Economic Journal: Applied Economics*, 7, 135–74.
- (2016): “The Cost of Convenience? Transaction Costs, Bargaining Power, and Savings Account Use in Kenya,” *Journal of Human Resources*.
- (2018): “The Persistent Power of Behavioral Change: Long-Run Impacts of Temporary Savings Subsidies for the Poor,” *American Economic Journal: Applied Economics*, 10, 67–100.
- SHIKUKU, K., J. PIETERS, E. BULTE, AND P. LÄDERACH (2019): “Incentives and the Diffusion of Agricultural Knowledge: Experimental Evidence from Northern Uganda,” *American Journal of Agricultural Economics*, 101, 1164–1180.
- TOMMASI, D. (2019): “Control of resources, bargaining power and the demand of food: Evidence from PROGRESA,” *Journal of Economic Behavior Organization*, 161, 265–286.
- WESTFALL, P. AND S. S. YOUNG (1993): *Resampling-Based Multiple Testing: Examples and Methods for p-Value Adjustment*, Wiley.

# A Appendix

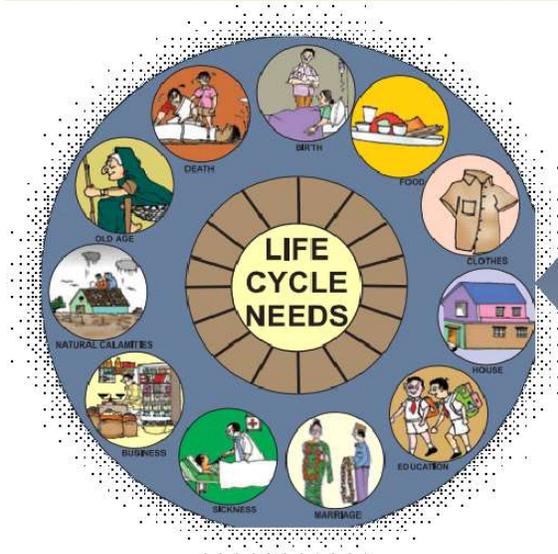
## A.1 Intervention posters

Figure A1: S&G poster

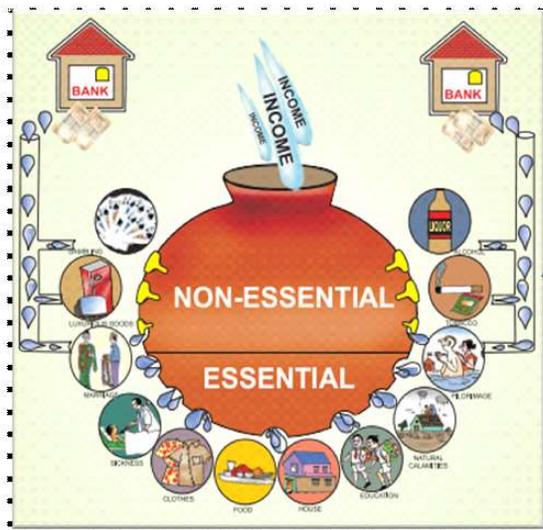


Figure A2: Awareness poster

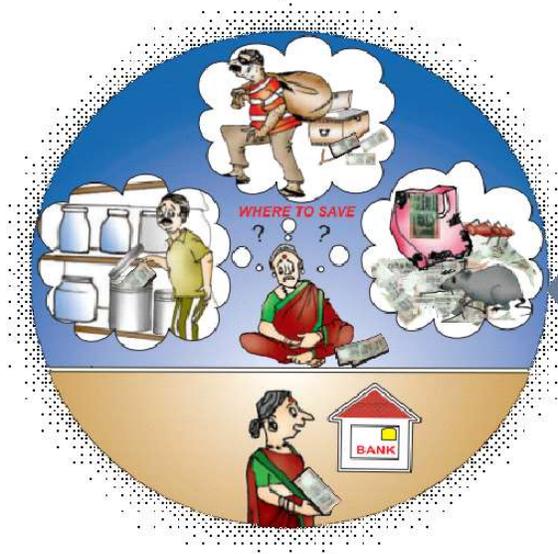
## Save in a bank for a secured future



**Save money for all your life cycle needs.**  
Saved money can be used during old age, future emergencies and to cover education, food, health, business, and marriage expenses of family members, etc.



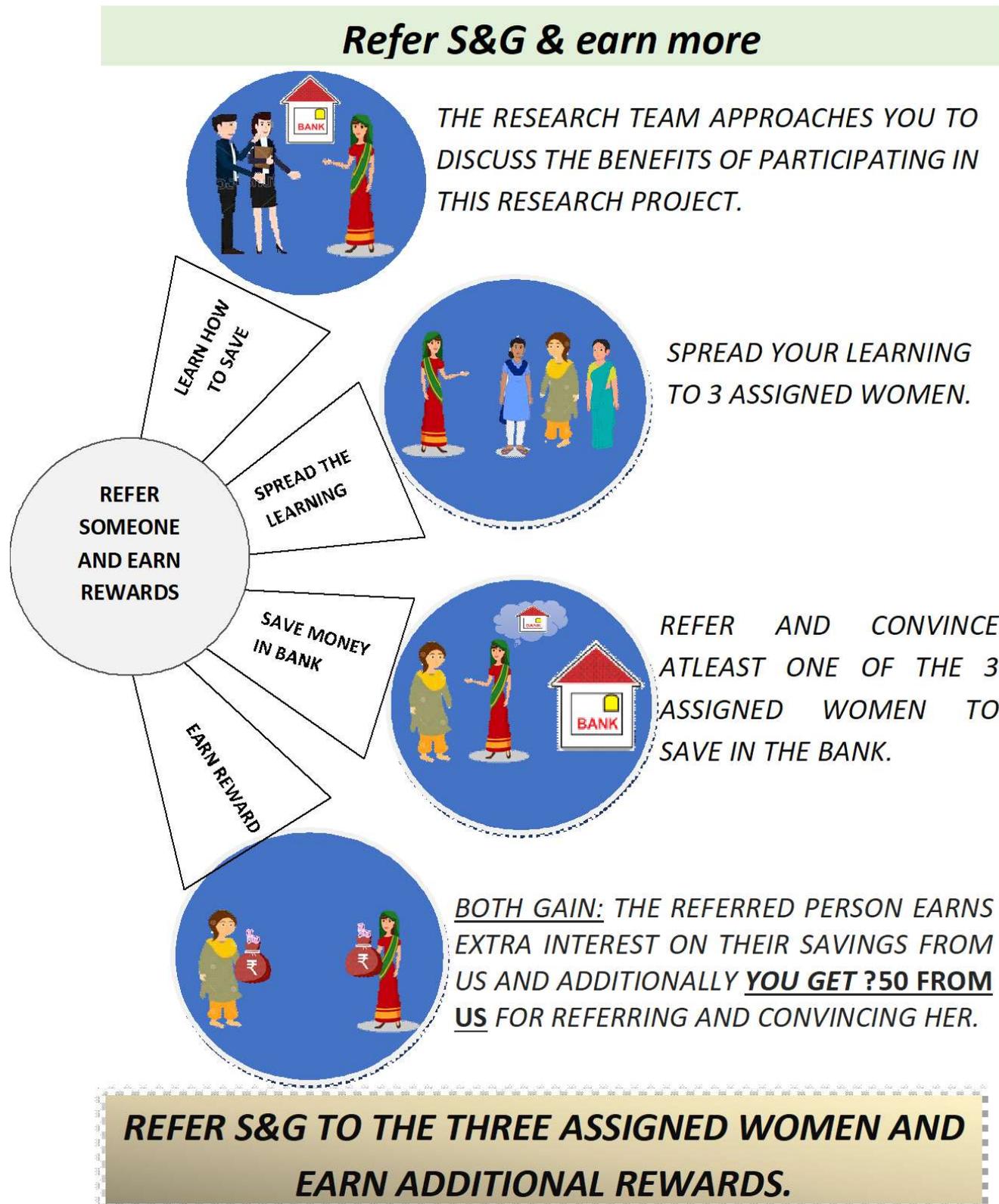
**Prioritize your expenses.**  
Stop spending your hard-earned money on non-essential items like alcohol, smoking, gambling, and luxurious goods and save this amount in a bank to earn interest and further increase consumption of essential items.



**Always save your money in a bank.**  
Saving your money in your bank account protects it from thieves and robbers, rats and pests, and other unforeseen circumstances like demonetization, etc.



Figure A3: Peer referral poster



## A.2 Additional tables from baseline data

**Table A1:** Summary statistics: A participants

	(1)	(2)	(3)	(4)	(5)
	Mean	Median	S.D.	Min	Max
<b>Demographics:</b>					
Age of respondent	29.50	29.00	6.42	18	55
Education of respondent	8.99	10.00	4.02	0	20
Can read and write [0,1]	0.80	1.00	0.40	0	1
Years of marriage	8.61	8.00	6.48	0	40
Household size	5.37	5.00	1.93	2	27
Age of husband	34.21	34.00	7.21	21	65
Education of husband	9.04	10.00	4.44	0	17
<b>Income and assets:</b>					
Housewife [0,1]	0.82	1.00	0.39	0	1
Income from labor of respondent (INR)	596.71	0.00	2396.49	0	20000
Own home [0,1]	0.96	1.00	0.21	0	1
Own farm [0,1]	0.62	1.00	0.49	0	1
Amount of farm land (Bigha)	2.83	2.00	3.96	0	50
<b>Savings and borrowings:</b>					
Self-reported formal savings, last 6 months (INR)	1989.51	500.00	4954.90	0	50000
Self-reported informal savings, last 6 months (INR)	481.58	0.00	1103.90	0	10000
Self-reported formal borrowings, last 6 months (INR)	1356.54	0.00	23787.86	0	500000
Self-reported informal borrowings, last 6 months (INR)	113.10	0.00	1931.90	0	50000
Deposit at least once a week [0,1]	0.05	0.00	0.21	0	1
Deposit at least once a month [0,1]	0.24	0.00	0.43	0	1
Deposit once every few months [0,1]	0.55	1.00	0.50	0	1
Deposit once a year [0,1]	0.15	0.00	0.36	0	1
Risk attitude (0: low risk; 10: high risk)	4.50	4.00	2.09	0	9
<b>Treatment status:</b>					
C [0,1]	0.20	0.00	0.40	0	1
A [0,1]	0.20	0.00	0.40	0	1
I [0,1]	0.20	0.00	0.40	0	1
P [0,1]	0.20	0.00	0.40	0	1
I+P [0,1]	0.20	0.00	0.40	0	1
Villages	180				
Observations	1415				

*Notes:* The Table reports the mean, standard deviation, minimum and maximum values of the main variables used in the paper. There is a total of 1,415 A primary participants in our sample, living in 180 villages in Uttar Pradesh, India.

**Table A2: Summary statistics: B participants**

	(1)	(2)	(3)	(4)	(5)
	Mean	Median	S.D.	Min	Max
<b>Demographics:</b>					
Age of respondent	29.70	29.00	6.55	18	56
Education of respondent	8.72	10.00	4.26	0	20
Can read and write [0,1]	0.78	1.00	0.41	0	1
Years of marriage	8.77	8.00	6.54	0	50
Household size	5.30	5.00	1.74	1	28
Age of husband	34.21	34.00	7.16	18	65
Education of husband	8.97	10.00	4.51	0	30
<b>Income and assets:</b>					
Housewife [0,1]	0.83	1.00	0.38	0	1
Income from labor of respondent (INR)	577.93	0.00	2281.56	0	40000
Own home [0,1]	0.96	1.00	0.20	0	1
Own farm [0,1]	0.65	1.00	0.48	0	1
Amount of farm land (Bigha)	3.25	2.00	6.85	0	250
<b>Savings and borrowings:</b>					
Self-reported formal savings, last 6 months (INR)	2067.84	500.00	5083.68	0	50000
Self-reported informal savings, last 6 months (INR)	510.63	0.00	1115.46	0	12000
Self-reported formal borrowings, last 6 months (INR)	695.62	0.00	13855.09	0	500000
Self-reported informal borrowings, last 6 months (INR)	233.54	0.00	7756.47	0	500000
Deposit at least once a week [0,1]	0.06	0.00	0.25	0	1
Deposit at least once a month [0,1]	0.25	0.00	0.44	0	1
Deposit once every few months [0,1]	0.54	1.00	0.50	0	1
Deposit once a year [0,1]	0.14	0.00	0.35	0	1
Risk attitude (0: low risk; 10: high risk)	4.53	4.00	2.10	0	9
<b>Treatment status:</b>					
C [0,1]	0.20	0.00	0.40	0	1
A [0,1]	0.20	0.00	0.40	0	1
I [0,1]	0.20	0.00	0.40	0	1
P [0,1]	0.20	0.00	0.40	0	1
I+P [0,1]	0.20	0.00	0.40	0	1
Villages	180				
Observations	4543				

Notes: The Table reports the mean, standard deviation, minimum and maximum values of the main variables used in the paper. There is a total of 4,543 B secondary participants in our sample, living in 180 villages in Uttar Pradesh, India.

**Table A3: Balancing tests between treatments (1)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Formal saving	Informal saving	Borrowing bank	Borrowing other	Distance bank (min.)	Distance bank (km)	Pop. village	HH village	Wheat cultivated	Own farm	Farm amount
<b>A (T2)</b>	77.45 (111.34)	-25.69 (45.58)	-694.94 (606.33)	72.14 (73.04)	-0.31 (0.99)	0.54 (0.92)	-41.30 (256.66)	-211.12 (235.52)	-0.06 (0.09)	-0.00 (0.02)	0.53* (0.29)
<b>I (T3)</b>	-22.86 (111.79)	9.06 (45.60)	-392.60 (645.33)	90.10 (92.80)	-0.52 (0.89)	0.26 (0.81)	-117.82 (261.40)	-218.57 (247.86)	-0.05 (0.10)	-0.02 (0.02)	-0.10 (0.16)
<b>P (T4)</b>	-8.96 (112.89)	14.30 (45.79)	-331.99 (720.43)	462.95 (402.48)	-0.47 (1.09)	-0.29 (0.86)	-102.98 (272.29)	-195.55 (233.70)	0.08 (0.09)	-0.00 (0.02)	0.06 (0.18)
<b>I+P (T5)</b>	-23.87 (100.46)	-85.68* (44.64)	862.19 (925.96)	181.67* (107.21)	-0.77 (0.93)	0.12 (0.88)	-100.86 (246.91)	-224.52 (233.20)	0.03 (0.10)	0.01 (0.02)	0.26 (0.21)
<b>Strata FE</b>	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean (Dep. Var.):	2049.23	503.73	852.59	204.93	22.48	3.97	976.59	229.28	0.72	0.64	3.15
SD (Dep. Var.):	5053.09	1112.70	16755.95	6838.17	14.79	3.94	1008.32	679.09	0.45	0.48	6.29
Clusters:	180	180	180	180	180	180	180	180	180	180	180
Observations:	5958	5958	5958	5958	5958	5958	5958	5958	5958	5958	5958
Adjusted R <sup>2</sup> :	0.03	0.01	0.00	-0.00	0.17	0.20	0.32	0.10	0.16	0.00	0.00
<i>Normalized difference between groups:</i>											
T2-T1:	0.02	-0.02	-0.06	0.04	0.02	0.13	-0.02	-0.20	-0.12	0.00	0.07
T3-T1:	-0.00	0.01	-0.03	0.05	0.01	0.08	-0.05	-0.19	-0.12	-0.04	-0.01
T4-T1:	-0.00	0.01	-0.02	0.04	-0.04	-0.08	-0.07	-0.18	0.20	-0.00	0.01
T5-T1:	-0.01	-0.08	0.04	0.07	-0.02	0.05	-0.10	-0.22	0.06	0.02	0.05
T2-T3:	0.02	-0.03	-0.05	-0.01	0.01	0.07	0.05	-0.05	-0.00	0.05	0.08
T2-T4:	0.02	-0.04	-0.04	-0.04	0.06	0.22	0.08	-0.07	-0.32	0.00	0.07
T2-T5:	0.03	0.06	-0.08	-0.04	0.04	0.09	0.14	0.13	-0.19	-0.02	0.03
T3-T4:	0.00	-0.01	-0.01	-0.04	0.04	0.19	0.03	-0.02	-0.32	-0.04	-0.03
T3-T5:	0.01	0.09	-0.06	-0.03	0.03	0.03	0.09	0.20	-0.18	-0.07	-0.06
T4-T5:	0.01	0.10	-0.05	0.03	-0.01	-0.14	0.04	0.20	0.14	-0.02	-0.04

Notes: Dependent variables reported at the top of the table. Standard errors are clustered at village level. Observations are at the women level. In each specification, we control for strata fixed effects (FE). At the bottom of the table, we report balancing results in terms of normalized differences between groups. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A4: Balancing tests between treatments (2)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Age	Education	HH size	N. of sons	N. of daughters	Risk	Women mobility	Poverty	Near market (km)	Near grocery (km)	District city (km)
<b>A (T2)</b>	-0.52 (0.39)	0.15 (0.20)	0.09 (0.07)	-0.05 (0.04)	-0.03 (0.04)	-0.09 (0.11)	0.06 (0.09)	0.09 (4.54)	0.16 (0.73)	0.04 (0.74)	-1.60 (3.64)
<b>I (T3)</b>	-0.49 (0.46)	0.17 (0.22)	-0.03 (0.07)	-0.06 (0.04)	-0.02 (0.04)	-0.04 (0.10)	0.22** (0.10)	3.87 (5.04)	0.70 (0.71)	0.46 (0.70)	-3.39 (3.13)
<b>P (T4)</b>	-0.24 (0.44)	0.08 (0.24)	-0.05 (0.07)	0.01 (0.04)	0.01 (0.04)	0.11 (0.09)	0.01 (0.09)	0.97 (5.07)	0.04 (0.60)	-0.20 (0.60)	-3.16 (3.71)
<b>I+P (T5)</b>	-0.50 (0.43)	0.22 (0.20)	-0.01 (0.07)	-0.05 (0.04)	-0.04 (0.04)	-0.01 (0.10)	0.05 (0.09)	-2.48 (4.86)	0.64 (0.63)	0.58 (0.67)	-3.45 (3.31)
<b>Strata FE</b>	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean (Dep. Var.):	29.65	8.78	5.32	0.96	0.83	4.52	1.26	19.62	4.69	4.37	32.64
SD (Dep. Var.):	6.52	4.21	1.79	0.81	0.83	2.10	0.44	22.16	3.68	3.65	16.24
Clusters:	180	180	180	180	180	180	180	180	180	180	180
Observations:	5958	5958	5958	5958	5958	5958	5958	5958	5958	5958	5958
Adjusted R <sup>2</sup> :	0.01	0.00	0.00	0.00	0.00	0.01	0.15	0.17	0.35	0.33	0.24
<i>Normalized difference between groups:</i>											
T2-T1:	-0.08	0.03	0.06	-0.06	-0.04	-0.04	0.15	-0.05	0.09	0.05	-0.08
T3-T1:	-0.08	0.04	-0.01	-0.08	-0.03	-0.01	0.50	0.12	0.25	0.17	-0.20
T4-T1:	-0.04	0.02	-0.03	0.01	0.01	0.05	0.01	0.05	0.00	-0.08	-0.18
T5-T1:	-0.08	0.05	-0.00	-0.07	-0.05	-0.00	0.13	-0.16	0.23	0.21	-0.23
T2-T3:	-0.00	-0.00	0.06	0.02	-0.01	-0.03	-0.35	-0.18	-0.13	-0.10	0.11
T2-T4:	-0.04	0.01	0.08	-0.07	-0.05	-0.09	0.14	-0.10	0.10	0.11	0.10
T2-T5:	0.00	-0.02	0.06	0.01	0.01	-0.03	0.02	0.11	-0.11	-0.13	0.14
T3-T4:	-0.04	0.02	0.02	-0.09	-0.04	-0.06	0.49	0.08	0.25	0.24	-0.00
T3-T5:	0.00	-0.01	-0.00	-0.01	0.02	-0.01	0.37	0.28	0.03	-0.04	0.04
T4-T5:	0.05	-0.03	-0.02	0.07	0.06	0.06	-0.12	0.20	-0.24	-0.29	0.04

Notes: Dependent variables reported at the top of the table. Standard errors are clustered at village level. Observations are at the women level. In each specification, we control for strata fixed effects (FE). At the bottom of the table, we report balancing results in terms of normalized differences between groups. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A5: Balancing tests between A vs B participants (1)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Formal saving	Informal saving	Borrowing bank	Borrowing other	Distance bank (min.)	Distance bank (km)	Pop. village	HH village	Wheat cultivated	Own farm	Farm amount
<b>A (T2)</b>	-75.60 (385.36)	-60.36 (106.34)	-1310.95 (1566.48)	-119.32 (78.92)	1.64 (1.36)	0.02 (0.04)	-7.00 (8.61)	-6.38 (7.34)	-0.00 (0.00)	-0.02 (0.04)	-0.32 (0.57)
<b>I (T3)</b>	457.63 (518.65)	-151.71 (114.66)	-543.96 (1734.94)	123.88 (220.55)	0.58 (1.17)	-0.01 (0.04)	-4.69 (10.18)	-9.12 (9.34)	-0.01** (0.00)	-0.04 (0.05)	0.52 (0.38)
<b>P (T4)</b>	387.37 (461.90)	-96.17 (115.34)	919.46 (2376.76)	-336.51 (584.80)	-0.32 (1.40)	0.01 (0.03)	0.25 (10.15)	-6.29 (6.82)	-0.01* (0.00)	-0.05 (0.05)	0.46 (0.42)
<b>I+P (T5)</b>	89.83 (433.36)	-88.91 (104.20)	-1123.39 (2531.57)	-267.87** (134.35)	0.28 (1.10)	-0.00 (0.04)	-7.68 (8.47)	-7.18 (7.97)	0.00 (0.00)	-0.00 (0.05)	0.17 (0.42)
<b>Strata FE</b>	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean (Dep. Var.):	2049.23	503.73	852.59	204.93	22.48	3.97	976.59	229.28	0.72	0.64	3.15
SD (Dep. Var.):	5053.09	1112.70	16755.95	6838.17	14.79	3.94	1008.32	679.09	0.45	0.48	6.29
Clusters:	180	180	180	180	180	180	180	180	180	180	180
Observations:	5958	5958	5958	5958	5958	5958	5958	5958	5958	5958	5958
Adjusted R <sup>2</sup> :	0.03	0.01	0.00	-0.00	0.17	0.20	0.32	0.10	0.16	0.00	0.00
<i>Normalized difference between groups:</i>											
T2-T1:	-0.04	-0.03	-0.08	-0.11	0.07	0.14	-0.02	-0.20	-0.12	-0.03	-0.03
T3-T1:	0.03	-0.06	-0.00	0.08	0.01	0.09	-0.05	-0.19	-0.13	-0.12	-0.02
T4-T1:	0.02	-0.02	0.05	0.10	-0.09	-0.08	-0.07	-0.18	0.20	-0.08	-0.01
T5-T1:	-0.04	-0.11	0.03	-0.09	-0.04	0.05	-0.10	-0.22	0.07	0.01	-0.01
T2-T3:	-0.06	0.03	-0.10	-0.09	0.07	0.07	0.04	-0.05	0.01	0.09	-0.01
T2-T4:	-0.05	-0.01	-0.10	-0.11	0.16	0.22	0.07	-0.07	-0.32	0.05	-0.02
T2-T5:	-0.00	0.08	-0.08	-0.03	0.11	0.09	0.13	0.12	-0.19	-0.05	-0.02
T3-T4:	0.01	-0.05	-0.05	-0.01	0.09	0.19	0.03	-0.02	-0.33	-0.04	-0.02
T3-T5:	0.06	0.05	-0.04	0.09	0.04	0.03	0.09	0.19	-0.20	-0.14	-0.02
T4-T5:	0.05	0.10	0.01	0.11	-0.05	-0.14	0.04	0.20	0.13	-0.09	0.00

Notes: Dependent variables reported at the top of the table. Standard errors are clustered at village level. Observations are at the women level. In each specification, we control for strata fixed effects (FE). We also control for participant-in-group-A dummy variable, and for each treatment dummy A, I, P, and I+P. What is reported in the table is the coefficient attached to the interaction of each treatment dummy and the participant-in-group-A dummy. At the bottom of the table, we report balancing results in terms of normalized differences between groups.

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

**Table A6:** Balancing tests between A vs B participants (2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Age	Education	HH size	N. of sons	N. of daughters	Risk	Women mobility	Poverty	Near market (km)	Near grocery (km)	District city (km)
<b>A (T2)</b>	-0.21 (0.54)	-0.60 (0.40)	-0.03 (0.15)	0.01 (0.08)	-0.03 (0.08)	-0.02 (0.20)	0.00 (0.00)	0.36* (0.21)	-0.03 (0.02)	-0.02 (0.02)	0.04 (0.13)
<b>I (T3)</b>	-0.18 (0.63)	-0.24 (0.42)	-0.19 (0.15)	-0.01 (0.07)	-0.12 (0.07)	-0.01 (0.19)	-0.00 (0.00)	-0.04 (0.26)	-0.03 (0.03)	-0.02 (0.03)	0.18 (0.14)
<b>P (T4)</b>	0.05 (0.56)	0.00 (0.33)	-0.05 (0.17)	0.00 (0.07)	-0.03 (0.08)	0.32 (0.21)	0.00 (0.00)	0.05 (0.20)	-0.03 (0.02)	-0.00 (0.02)	-0.03 (0.12)
<b>I+P (T5)</b>	-0.26 (0.55)	0.32 (0.39)	-0.21 (0.13)	-0.02 (0.07)	-0.07 (0.07)	-0.05 (0.21)	0.00 (0.00)	0.14 (0.21)	-0.02 (0.02)	-0.01 (0.02)	-0.02 (0.14)
<b>Strata FE</b>	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean (Dep. Var.):	29.65	8.78	5.32	0.96	0.83	4.52	1.26	19.62	4.69	4.37	32.64
SD (Dep. Var.):	6.52	4.21	1.79	0.81	0.83	2.10	0.44	22.16	3.68	3.65	16.24
Clusters:	180	180	180	180	180	180	180	180	180	180	180
Observations:	5958	5958	5958	5958	5958	5958	5958	5958	5958	5958	5958
Adjusted R <sup>2</sup> :	0.01	0.00	0.00	0.00	0.00	0.01	0.14	0.17	0.35	0.33	0.24
<i>Normalized difference between groups:</i>											
T2-T1:	-0.11	-0.01	0.11	-0.09	-0.03	-0.07	0.15	-0.05	0.09	0.05	-0.08
T3-T1:	-0.11	0.06	-0.02	-0.13	-0.11	-0.04	0.50	0.12	0.25	0.17	-0.20
T4-T1:	-0.04	0.09	0.02	-0.03	0.02	0.14	0.01	0.04	-0.00	-0.08	-0.18
T5-T1:	-0.12	0.18	-0.02	-0.12	-0.08	-0.05	0.13	-0.16	0.24	0.21	-0.23
T2-T3:	-0.01	-0.07	0.12	0.04	0.07	-0.03	-0.34	-0.17	-0.13	-0.10	0.11
T2-T4:	-0.08	-0.10	0.08	-0.06	-0.05	-0.21	0.14	-0.09	0.10	0.11	0.10
T2-T5:	0.01	-0.19	0.12	0.03	0.05	-0.02	0.02	0.12	-0.11	-0.13	0.14
T3-T4:	-0.07	-0.03	-0.04	-0.10	-0.12	-0.19	0.48	0.07	0.25	0.24	0.00
T3-T5:	0.01	-0.12	0.00	0.00	-0.02	0.00	0.36	0.28	0.03	-0.04	0.05
T4-T5:	0.09	-0.10	0.04	0.10	0.10	0.19	-0.12	0.20	-0.24	-0.29	0.04

Notes: Dependent variables reported at the top of the table. Standard errors are clustered at village level. Observations are at the women level. In each specification, we control for strata fixed effects (FE). We also control for participant-in-group-A dummy variable, and for each treatment dummy A, I, P, and I+P. What is reported in the table is the coefficient attached to the interaction of each treatment dummy and the participant-in-group-A dummy. At the bottom of the table, we report balancing results in terms of normalized differences between groups. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .



**Table A8: Balancing tests between treatments: pilot sample**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Age	Education	Literacy	Marriage	Household size	Caste	Own farm	Farm land	Income
<b>I (T1)</b>	0.07 (0.70)	0.53 (1.71)	0.17 (0.21)	-0.02 (0.93)	0.71 (0.73)	-0.25 (0.27)	0.42 (0.24)	-43.53 (61.45)	0.20* (0.10)
<b>P (T2)</b>	-0.28 (0.87)	0.78 (1.68)	0.15 (0.14)	-0.57 (1.02)	0.89 (0.66)	-0.32 (0.26)	0.25 (0.30)	13.05 (91.83)	-0.04 (0.09)
<b>I+P (T3)</b>	-2.19** (0.90)	0.51 (1.39)	0.26 (0.16)	-1.86* (0.94)	-0.00 (0.89)	-0.11 (0.26)	-0.01 (0.32)	-99.64* (47.53)	0.32** (0.12)
<b>Baseline savings</b>	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean (Dep. Var.):	31.43	8.48	2.43	8.20	7.46	3.04	3.59	282.54	5.76
SD (Dep. Var.):	5.67	4.75	0.76	5.26	3.34	1.22	5.66	381.45	1.25
Clusters:	15	15	15	15	15	15	15	15	15
Observations:	586	586	586	586	586	586	586	586	586
Adjusted R <sup>2</sup> :	0.02	0.16	0.20	0.03	0.22	0.04	0.89	0.30	0.13
<i>Normalized difference between groups:</i>									
T1-T0:	0.03	-0.00	0.09	0.04	0.34	-0.26	0.23	-0.00	0.71
T2-T0:	-0.05	0.17	0.21	-0.12	0.27	-0.24	0.10	-0.02	0.26
T3-T0:	-0.35	-0.01	0.20	-0.30	0.13	-0.15	0.17	-0.34	0.26
T2-T1:	-0.08	0.19	0.13	-0.17	-0.06	0.02	-0.12	-0.01	-0.45
T2-T3:	0.30	0.20	0.01	0.18	0.14	-0.09	-0.06	0.30	-0.00
T1-T3:	0.39	0.01	-0.12	0.35	0.20	-0.11	0.06	0.29	0.45

Notes: Dependent variables reported at the top of the table. Standard errors are clustered at village level. Observations are at the women level. In each specification, we control for baseline self-reported savings. At the bottom of the table, we report balancing results in terms of normalized differences between groups. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A9:** Effect sizes on savings reported on passbooks at endline (last 1 month): pilot sample

	(1)	(2)	(3)
	<b>A participants</b>	<b>B participants</b>	<b>All participants</b>
<b>I (T1)</b>	0.22 (0.21)	0.32* (0.17)	0.27* (0.14)
<b>P (T2)</b>	0.43 (0.25)	0.40* (0.19)	0.41** (0.16)
<b>I+P (T3)</b>	2.43*** (0.52)	0.78*** (0.19)	1.18*** (0.14)
<b>Covariates</b>	✓	✓	✓
<b>Baseline savings</b>	✓	✓	✓
Clusters:	15	15	15
Observations:	144	441	585
Adjusted $R^2$ :	0.325	0.099	0.137

Notes: Dependent variable: Savings as collected from passbooks in standardized mean difference (z-scores). Key variables: dummies for information villages (I group), persuasion villages (P group) or information and persuasion villages (I+P group). Covariates: self-reported formal savings at baseline, farm land ownership, amount of farm land, income from labor at baseline, family size, age, age squared, education level, literacy, caste, years of marriage and risk attitude. Standard errors are clustered at village level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .