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**Conference Paper**

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Beiträge zur Jahrestagung des Vereins für Socialpolitik 2024: Upcoming Labor Market Challenges

**Provided in Cooperation with:**

Verein für Socialpolitik / German Economic Association

*Suggested Citation:* Kaiser, Tim; Hamdan, Jana; Menkhoff, Lukas; Xu, Yuanwei (2024) : Scaling up financial education: Evidence from a randomized saturation experiment, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2024: Upcoming Labor Market Challenges, ZBW - Leibniz Information Centre for Economics, Kiel, Hamburg

This Version is available at:

<https://hdl.handle.net/10419/302424>

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# Scaling financial education among micro-entrepreneurs: A randomized saturation experiment

Jana S. Hamdan, Tim Kaiser, Lukas Menkhoff, and Yuanwei Xu

**Abstract.** We study direct, spillover and saturation effects of a financial education program on micro-entrepreneurs in Uganda. We randomize the program at the cluster-level, and then randomize the share of treated individuals within treated clusters. 15 months later, the treated show expected effects, such as increased use and amount of mobile money savings or higher investments. Spillovers on untreated peers tend to be insignificant. However, when the share of treated in treated clusters (i.e., saturation) increases, the beneficial effects on the treated wear off, so that the net effect of the program may turn insignificant if operated at scale.

**JEL classification:** C93, D14, G53, O12

**Keywords:** Financial Literacy, Micro-Entrepreneurs, Mobile Money, Spillover Effects

February 22, 2024

PRELIMINARY DRAFT

**Acknowledgements:** We thank Mountains of the Moon University in Fort Portal as well as Filder Aryemo (Gaplink Uganda) and Helke Seitz (DIW Berlin) for support in field work. This research was funded by the German Research Foundation through the Research Training Group (RTG) 1723 “Globalization and Development.” We thank participants of the American Economic Association Meetings 2022, the Southern Economic Association Annual Conference 2022, and the Bank of Finland Financial Literacy Conference 2023 for comments. An earlier version of this research circulated as “Financial education and spillover effects: Experimental evidence from Uganda.” The field experiment was pre-registered under AEARCTR-0006407. Declarations of interest: none.

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## 1 Introduction

Financial education for small scale entrepreneurs, often as part of broader business education, is a common type of development assistance. Financial education programs cause intended directional changes in financial behaviors in a wide variety of settings (see, e.g., Miller et al., 2015; Kaiser et al., 2022). This literature has largely focused on direct causal effects of financial education programs on the beneficiaries with limited attention into potential externalities of treatment. It seems well possible that an effective training can be scaled up to very large groups or may have positive spillover effects on untreated (e.g., Frisancho, 2023). Still, there is hardly any evidence whether this really materializes for financial education among micro-entrepreneurs. Depending on the local context, entrepreneurs compete with each other to varying degrees so that crowding out effects may dominate (see, e.g., Drexler et al., 2014; McKenzie and Puerto, 2021). The question whether positive or negative spillovers are present is crucial to assess the net effect of a policy intervention such as financial education.

We study this question in an RCT with about 2,000 micro-entrepreneurs in rural Uganda. The design allows to distinguish direct intention to treat (ITT) effects, spillover effects on untreated, and saturation effects from increasing the share of treated. We find several direct effects as intended by the training, in particular on mobile money savings and investments which is largely in line with the literature. We do not find any positive spillover effects, and we find rather negative saturation effects. Overall, this indicates that scaling such a training does not necessarily keep the level of treatment effectiveness.

We conduct a large-scale randomized saturation experiment with 108 clusters of 1,975 micro-entrepreneurs in Western Uganda to estimate treatment effects for several important estimands in the context of spillover effects ignored in the extant literature on financial education interventions (see Baird et al., 2018). For this research we collect information about the location and size of all permanent trading centers in the Kabarole district of Western Uganda with about 350,000 inhabitants. We identify 108 trading centers with almost 5,500 micro-enterprises and randomize clusters to one of four assigned treatment saturations, i.e., where 0% (pure control clusters), 50%, 75% and 100% of the sampled individuals within clusters are then randomly invited to a financial education program previously shown to generate large shifts in financial behavior among the directly treated in an earlier field experiment (Kaiser and Menkhoff, 2022). Thus, we relax the

assumption of no interference and set up the experiment to identify direct and spillover effects in the context of partial interference.

For expositional purposes we first report results with the typical assumption of no interference between units (i.e., the stable unit treatment value assumption), where within-cluster control individuals are used as a counterfactual. Under this assumption, we estimate large effects of the program on the directly treated, such as a large increase mobile money savings and other savings at both the extensive and intensive margins.

We then move to a discussion of pooled intention to treat and spillover effects and a granular analysis of the size of direct and spillover effects in a linear model of treatment saturation. The results appear heterogeneous by outcome type: For most outcomes related to savings and the use of mobile money as a savings technology, the direct effects on those assigned to treatment in treated clusters are similarly large but the indirect effects of the treatment on the untreated individuals in treated clusters are mostly estimated with a negative sign, albeit none of them being statistically significant at conventional levels except a reduction in transfers via mobile money at the extensive and intensive margins. In an affine model of spillover effects, we instrument the *true treatment saturation* (i.e., the product of the relative frequency of sampled households relative to the clusters size and the assigned treatment saturation) with the assigned saturation and study the slope effects of increasing the share of treated individuals. For most outcomes related to mobile money use and savings, the *treatment effect on the uniquely treated* (i.e., the intention to treat effect on a sole individual offered treatment within a cluster) is estimated to be large and statistically significant.

Yet, we find evidence that the beneficial effects of the treatment on mobile money use and savings decline with increasing saturation while the magnitude of the spillover on the non-treated does not appear to change with increasing saturation. Turning to the total causal effect of the program (i.e., the average causal effect of all individuals in treated clusters relative to pure control clusters) suggest that the net effects at the cluster-level are likely to be zero, on average and at the average realized saturations. Thus, our finding is two-sided: while financial education seems to benefit those directly exposed, these beneficial effects wear off as a larger share of peers are treated, so the net effects are questionable in our setting and if this program is operated at a larger scale. This finding motivates to also assess potential spillovers in future trainings and to think about how possible negative spillovers might be reduced or avoided.

**Literature.** Our study complements three strands of literature, i.e., the analysis of (1) financial- and business education programs, (2) mobile money adoption, and (3) spillover effects of educational interventions. Financial education has been recognized as an important element of education for the whole population as evidenced by respective programs supported by the OECD worldwide (OECD, 2020). Micro-entrepreneurs in developing countries are a particular target of these efforts as improved financial behavior may not only contribute to their family welfare but may also stimulate economic growth. While the use of improved financial behavior is undisputed, there was some discussion whether financial trainings are able to realize this ambition (Fernandes et al., 2014). However, most recent evidence clearly shows that financial education, even when evaluated via randomized controlled trials (RCTs) and when corrected for publication bias, has a positive causal effect on financial knowledge and downstream financial behaviors (Miller et al., 2015; Kaiser et al., 2022). However, the challenge remains to make best use of resources by increasing effectiveness of financial education. There are several ways that have been suggested, such as using (very costly) individual counseling (Carpena et al., 2019), to rely on goal setting (Carpena et al., 2019), to use an entertainment environment (Berg and Zia, 2017), to time education at a “teachable moment” (Doi et al., 2014; Kaiser and Menkhoff, 2017) or to rely on “active learning” as a teaching method (Freeman et al., 2014; Kaiser and Menkhoff, 2022). The intervention studied here relies on the two latter elements, i.e., the training is designed to meet the purposes of these rural micro-entrepreneurs and the way of delivery follows the concept of “active learning.” The treatment effect on the uniquely treated suggests relatively large effects on treated individuals absent any spillover effects. Thus, this paper shows that this treatment is – in principle – able to change individual financial behavior which is in line with evidence from recent meta-analyses (Kaiser et al., 2022) and an earlier experiment in the same setting (Kaiser and Menkhoff, 2022).

This study also adds directly to the research on mobile money adoption. The intervention provides information about mobile money as a savings-technology and, in particular, about mobile money transfers. While there is lot of evidence showing the impact of mobile money on risk sharing and household welfare (Jack and Suri, 2014; Munyegera and Matsumoto, 2016; Riley, 2018) and financial inclusion (Hamdan et al., 2022), there is little explicit evidence on the success of addressing mobile money in a financial education training. Chiwaula et al. (2020) combine a

financial literacy and mobile money training with reminders among community savers in Malawi, finding a positive effect on mobile money savings four months after the intervention. However, they cannot differentiate the impact of the training from the reminders and focus on a selected group whose members already save. Our results show that financial education interventions can stimulate mobile money use as savings technology both at the extensive and intensive margins but that the spillover dynamics need to be considered when rolling out education programs.

Finally, this study contributes to literature on spillover effects of financial literacy and education and human capital programs more broadly. In the context of general financial literacy and education, Haliassos et al. (2020) find positive spillovers of financially knowledgeable neighbors on stock market participation. They use a natural experiment, exploiting the variation of average financial literacy in Swedish neighborhoods, to study the long-term impact on refugees, who were randomly allocated to these neighborhoods. Frischno (2023a) and Bruhn et al. (2016) find some evidence of intergenerational spillovers to parents when children are exposed to financial education in high school.

In the context of business and financial education programs for entrepreneurs, Drexler et al. (2014) conduct an ex post analysis of potential spillovers from a financial- and business education program randomized at the individual-level by relying on geographic proximity of treated peers. They find suggestive evidence that treatment effects on sales decline when more businesses of similar kind are treated in proximity, indicating that some degree of crowding-out of other businesses in the same markets may occur.

Closer to our design is McKenzie and Puerto (2021) who do not find spillovers from business education in their study on small female-led businesses in Kenya. The positive effects of the assignment to a training do not come along with significant spillover effects on the untreated businesses in the same markets, so that markets appear to grow as a result. Similarly, Calderon et al. (2020) find no evidence of spillovers in a small-scale experiment in Mexico studying the effects of a business and financial-literacy program. Thus, our study is among the first to identify declining treatment effects on business and household financial outcomes when the share of treated individuals increases. This result has important implications for the design and evaluation of financial education programs operated at scale.

The paper is organized as follows. In the next Section 2, we describe our study sample, the

research design, the financial education program, the empirical strategy, summary statistics, balance, program take-up and attrition. Results are discussed in Section 3 and we conclude in Section 4.

## **2 Sample and experimental design**

### **2.1 Study setting, sample, and timeline**

Uganda is similar to many developing countries in its stage of financial development and financial inclusion: mobile money services have boosted basic access to financial accounts significantly in recent years, however, the access to traditional formal financial institutions (banks) remains low and a gender gap persists ([Demirgüç-Kunt et al., 2020](#)). The 46-million population grows at an annual rate of 3.3 percent, 46 percent of Ugandans are below the age of 15 and 75 percent live in rural areas ([WDI, 2020](#)). Most Ugandans work for themselves or for their families in low-quality jobs, and less than 10 percent are in formal employment ([Merotto, 2019](#)). Thus, policy efforts to support the growth of small businesses are needed to create jobs and improve their quality – “whether as entrepreneurs or as employees, rural youth need access to finance, technology, skills, and assets” ([Merotto, 2019](#)). Meanwhile, the Ugandan National Financial Inclusion Strategy (NFIS) 2017-2022 unites efforts to improve financial security via financial inclusion. Its goals include increasing women’s use of financial services, individual emergency savings and, more generally, consumer protection ([Bank of Uganda, 2017](#)).

In this context, we implement our study in the rural Kabarole district in the Western region of Uganda. We take this district as it was used to be known to people during baseline and still continues in several government statistics; officially, the southern part of this district, i.e., the former county of “Bunyangabu,” has become a new independent district in 2017. The population of Kabarole, as we still use it, in 2020 is estimated to be about 528,000 ([UBOS, 2021](#)). The majority of the population reside outside the main town, Fort Portal.

Our sample is composed of small business owners from this area. They typically run their family enterprises, usually in the form of small shops, in so-called trading centers. These are shopping streets usually lined along the main road of villages and small towns. As there is no official documentation of the existing trading centers in the district, we mapped the 113 trading

centers within Kabarole with the support of local government officials and university staff in November 2018, prior to the baseline survey. The baseline survey is implemented from February to April 2019 in all the mapped villages, and a total of 108 trading centers are included in the study. Five mapped trading centers are not included as no open business was found during the baseline visit. Figure 1 displays the locations of the 108 trading centers on a map.

< Figure 1 >

During the baseline survey, we conduct face-to-face interviews with a total of 2,223 small business owners, with an average of about 20 respondents in each trading center.<sup>1</sup> About 62 percent of interviewed small business owners have small retail or wholesale shops, and 28 percent run a service business, mostly hair dressing salons and restaurants. The remaining 10 percent are in manufacturing, mostly in furniture making or metal goods production. We estimate that this sample covers roughly 40 percent of all small businesses in the rural areas of Kabarole, as we count 5,478 shop units that seem to be active in some way. All those with an open small business are, in principal, eligible to participate in the study.

Our sampling procedure is designed to interview a sufficiently large share of small business owners in each trading center. As smaller trading centers are typically less busy, many business owners also work in subsistence farming in proximity to their shop, and close their business for part of the day. In order to interview enough business owners in smaller trading centers, we approach every open business in trading centers with up to 100 businesses and invite them to participate in our survey (which they basically always accept). This strategy results in covering 46 percent of shops in these areas, implying that the majority of shops (54 percent) were not open during our visiting time.

In the nine largest trading centers, i.e., those with more than 100 small businesses, we randomly approach every third open business. As here about 75 percent of shops are open, we cover about a quarter of all these shops. In total, this brings us to the outcome of interviewing 36 percent of all small business owners. Thus, in a big (small) trading center, we under-(over-)sample.

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<sup>1</sup> Prior to the baseline survey, we piloted the interviews in Rugombe, a community in the adjacent Kyenjojo district. There, we also piloted the intervention, the financial education training, in August 2019.



Moreover, the sampling rate ranges from 20 to 100 percent across the trading centers, with an overall average of 43.6 percent (SD of 12.5 percent). In order to control for these varying sampling probabilities by trading center size, we will incorporate inverse sampling weights in our later analysis.

On average, a baseline survey takes about 45 minutes and is compensated with 4,900 UGX (equivalent to 1.40 USD). It collects extensive information on the financial situation and type of the businesses and the financial decision-making of the owners, particularly their saving, borrowing, transfer behaviors and the use of mobile money. Two survey experiments are incorporated into the baseline survey as well, one on the willingness to pay of mobile money for making transfers, the other measuring the risk preferences; both are not further reported in this paper.

We re-visit the 54 treatment trading centers in August and September 2019 and implement our financial education intervention, thus four to six months after the baseline.<sup>2</sup> Treated baseline participants are invited in advance via phone calls. On the day of the intervention, those participants are called again prior to the beginning of the training. In addition, they are informed of and encouraged to take part in the training by the local council, elected officials who run villages (the lowest political administrative units in Uganda), on the day of the training as well. During the training, other participants who are not invited show up in the training and request to participate. We allow them to attend the training, and give them the cash compensation at the end. In the ITT analysis, these participants belong to the spillover group because they are not invited. This may tentatively increase positive spillovers.

The follow-up survey is conducted in two stages, about 13-19 months after the intervention. The first round of collection is done using phone surveys from October to December 2020, reaching 72.9 percent of the baseline sample. Afterwards, in April 2021, we follow up with hitherto unreachable participants via face-to-face interviews. On average, the follow-up survey takes on average about 30 minutes and is compensated with 6,400 UGX (equivalent to 1.80 USD). Overall, we reach 90.7 percent ( $N = 1,975$ ) of baseline participants.

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<sup>2</sup> During this period, we updated the contact details of those business owners who participated in the training.

In the baseline and the intervention phases, the study is implemented in cooperation with our mentioned partner, the MMU. The follow-up data collection is implemented by Gaplink Uganda, a Kampala-based independent research company. Their team is kept unaware of the experimental design, and is provided with only the contact details of the participants and local councils. All surveys are conducted in the local language Rutooro or optional in the official language English. The responses are recorded on tablets using SurveyCTO.

## **2.2 Financial education program**

We evaluate the effectiveness of a five-hour financial education program. It includes a total of six sessions – one introductory session and five further sessions with the following topics: (1) budgeting and record keeping, (2) saving, (3) debt management, (4) investment and (5) money transfer. Participants always start with the introductory session, and then immediately move from session (1) until session (5), spending up to one hour per station.

In all training sessions, the trainers employ an active learning method ([Kaiser and Menkhoff, 2022](#)), where numerous interactive activities are included in the curriculum to engage the participants in the learning process and to encourage discussions. They include, for example, picture stories, real-life case studies as exercises, small-group discussions and the evaluation of individual situations. These features promote paying attention and learning from peers. The goals of the training include, for example, learning how to budget and financially plan for themselves and their business, being able to distinguish needs from wants, promoting savings and knowing the different ways of saving, selecting and managing debt, making an investment plan and increasing business investments, and understanding the risks and costs associated with several ways to transfer money, in particular how to use mobile money.

The program is implemented in function halls, school classrooms or suitable outside areas in or near the center of the trading centers (or villages) in our study region. On average, the training lasts five hours, with a group consisting of 17 participants, on average (with a range between four and 46). The training is delivered by one licensed financial literacy trainer, called the master trainer, as well as five local undergraduate students from the public Mountain of the Moon University (MMU), who act as trainers, being responsible for one session each. All trainers take part in a three-days training, where they become familiar with the curriculum, the learning goals, and the

active learning method. All trainers' teaching qualifications are verified by holding mock training sessions and a pilot training. The entire team that partly alternated during the intervention includes two female and two male master trainers. It further consists of 16 female trainers, 11 male trainers, five female research assistants, and one male research assistant.

Participants of the training receive a cash payout of 10,000 UGX (equivalent to 2.80 USD) after completion of all training sessions. According to the baseline data, the average monthly income from the respondent's business and other sources is about 447,000 UGX. Thus, they earn an average of 17,880 UGX per day when assuming 25 working days per month. Therefore, the opportunity cost of attending a training is likely compensated for with the disbursed expense allowance.

### **2.3 Two-stage randomization**

The randomization is conducted in two stages. In the first stage, 54 out of a total of 108 clusters (trading centers) are randomly allocated to the treatment group and the other 54 are allocated to the control group. The randomization is conducted via a two-stage stratification strategy based on the mobile money account ownership rates and financial literacy levels at baseline.

We first create three strata based on the cluster-level average share of mobile money account owners, splitting the 108 clusters into low, middle and high mobile money ownership rates, each with 36 clusters. Afterwards, within each stratum, we further create six substrata depending on the average financial literacy score.<sup>3</sup> We obtain a total of 18 strata, with six clusters within each stratum. We perform the randomization into control and treatment group in a 1:1 ratio, using a random seed in Stata.

In the second step, among the 54 treated clusters, we further randomize the cluster-level treatment intensity with probability 1/3. In 18 clusters we invite 100 percent of the baseline respondents to participate in the financial education program, in another 18 clusters we invite 75

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<sup>3</sup> The two variables are chosen because participants with different baseline financial literacy levels and mobile money adoption may be significantly differently impacted by the financial education program. In order to understand possible heterogeneity effects, it is, for instance, necessary to have sufficient low financial literacy respondents in every group.

percent, and in the remaining 18 clusters we invite 50 percent. Overall, 879 respondents are directly targeted to the take part in the training, thus 39.5 percent of the baseline sample. The treatment intensity is varied to assess the magnitude of spillovers.

We register our trial and pre-analysis plan in the American Economic Association’s registry for randomized controlled trials (ID [AEARCTR-0006407](#)). Blinding of participants and the trainers was not feasible. However, the endline survey was conducted by a firm being new to this study and we did not inform them about the allocation of survey participants to the different groups, so that there should be no distortion from this side.

## 2.4 Empirical models

As described in Section 2.3, our randomized saturation experiment allows for partial interference between units, i.e., interference of individuals within clusters but stable unit treatment values between clusters (see Baird et al., 2018). This section describes the main estimands discussed in this paper. We focus on reduced form results (Intention to Treat Effects), i.e., the effect of being assigned to treatment and spillover groups and linear spillover analyses. We report Local Average Treatment Effects allowing for two-sided non-compliance within clusters (i.e., the effect of attending the program) based on IV-estimation in the appendix which come with an additional identifying assumption of no direct effects of the invitation alone (i.e., the exclusion restriction). We estimate all models with weighted least squared using saturation and sampling weights: Observations in the pure control clusters are weighted by the inverse of the sampling probability for each cluster ( $w_{i,c,t} = p_c^{-1}$ ). Treated individuals in treated clusters are weighted by the inverse of the product of the assigned treatment saturation ( $\pi$ ) and the sampling probability  $w_{i,c,t} = (\pi_c p_c)^{-1}$ . Untreated individuals in treated clusters are weighted by  $w_{i,c,t} = ((1 - \pi_c) p_c)^{-1}$ .

### 2.4.1 Assuming no interference within clusters

As a first estimand, we report the intention to treat effects of being assigned to the program while assuming stable unit treatment values (SUTVA) both within and between clusters, i.e., imposing the strong assumption of no interference. Thus, we estimate:

$$y_{i,c(t)} = \alpha_0 + \beta_1 T_{i,c} + \delta_1 y_{ic(t-1)} + \delta_2 m_{ic(t-1)} + \lambda X_{s,i,c} + \varepsilon_{ict} \quad (1)$$

Here,  $y_{i,c(t)}$  is the outcome  $y$  of individual  $i$  in cluster  $c$  at the time of follow-up  $t$ .  $\alpha_0$  denotes the intercept.  $\hat{\beta}_1$  captures the intention to treat effect of being assigned to treatment  $T$  (i.e., being personally invited to attend the training within a treated cluster). Thus, the omitted category consists of both individuals in pure control clusters as well as the within-cluster control group (i.e., the spillover group). We control for the lagged outcome at baseline  $y_{ic(t-1)}$ , an indicator for missing outcome values at baseline  $m_{ic(t-1)}$  as well as randomization strata dummies, enumerator dummies and a field interview dummy contained in the vector  $X_{s,i,c}$ .  $\varepsilon_{ict}$  denotes the error term. Standard errors are clustered at the market-level, i.e., the highest level of random assignment.

#### 2.4.2 Pooled intention to treat and spillover effects

Next, we relax the assumption of no interference to an assumption allowing partial interference, i.e., assuming spillovers on the treated and non-treated within clusters but not between treatment and pure control clusters. We define the following equation with the saturation weights defined above to estimate the pooled intention to treat and spillover effects, i.e., the average ITT and spillover on the non-treated effects across all saturations.

$$y_{i,c(t)} = \alpha_0 + \beta_1 T_{i,c} + \beta_2 S_{i,c} + \delta_1 y_{ic(t-1)} + \delta_2 m_{ic(t-1)} + \lambda X_{s,i,c} + \varepsilon_{ict} \quad (2)$$

In this case,  $\hat{\beta}_1$  captures the pooled intention to treat effect of being assigned to treatment  $T$  and  $\hat{\beta}_2$  captures the pooled spillover on the non-treated within treated clusters.

#### 2.4.3 Linear spillover analysis

Because the ITT and spillover effects can be heterogenous at any given saturation and the experiment was explicitly designed to estimate slope rather than pooled effects (see Baird et al. 2018 for a discussion of this trade-off in experimental design), we next move to a discussion of linear spillover effects. Thus, we impose the functional form:

$$y_{i,c(t)} = \alpha_0 + \beta_1 T_{i,c} + \beta_2 S_{i,c} + \beta_3 (T_{i,c} \times \Pi_c) + \beta_4 (S_{i,c} \times \Pi_c) + \delta_1 y_{ic(t-1)} + \delta_2 m_{ic(t-1)} + \lambda X_{s,i,c} + \varepsilon_{ict} \quad (2)$$

Here,  $\hat{\beta}_1$  captures the intercept of the treatment group or the *treatment effect on the uniquely treated*, i.e., the intention to treat effect on a hypothetical sole individual invited to treatment within a cluster.  $\hat{\beta}_3$  is the slope effect capturing how the spillover on the treated changes at a given true saturation ( $\Pi_c$ ) (i.e., combining the estimates of  $\hat{\beta}_1$  and  $\hat{\beta}_3$  yields the intention to treat at a given saturation which is the sum of the treatment effect on the uniquely treated and the spillover effect on the treated).  $\hat{\beta}_2$  captures the spillover effect at saturation zero (which should be zero if spillovers have a linear relationship with the saturation)<sup>4</sup>.  $\hat{\beta}_4$  is the slope effect capturing how spillovers on the non-treated within treated clusters change with increasing saturation. Due to our experimental design, only individuals sampled at baseline can be invited to participate in the program, i.e., random assignment is conditional on being sampled at baseline. Since the sampling probabilities vary with cluster size in our setting, this creates a ‘gateway to treatment’. Because spillovers can occur in the larger population within the cluster, we instrument the true treatment saturation ( $\Pi_c$ , i.e., the actual share of treated individuals within the cluster which is partly endogenous) with the assigned treatment saturation ( $\pi_c$ , the share of treated individuals in the sample) (see Baird et al., 2018).

#### 2.4.4 Total causal effects

Finally, one may be interested in the total causal effect, i.e., the overall cluster-level difference between treated and pure control clusters. This is defined as the weighted average of the pooled ITT and spillover effects across all saturations. Thus, one may estimate equation (2) with the saturation weights defined above and estimate the linear combination of  $\hat{\beta}_1 + \hat{\beta}_2$ .

### 2.5 Summary statistics, attrition, and randomization balance

Overall, we evaluate the impact of our intervention on a total of 21 variables. Table 1 displays summary statistics of these 21 outcome variables as well as selected socio-demographic characteristics to verify the orthogonality of randomization at baseline. Table 1 reveals that 64% of our sampled micro-entrepreneurs are female. Moreover, the respondents are on average 34 years old and have some secondary education.

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<sup>4</sup> Testing  $\hat{\beta}_2 = 0$  serves as a test for the linearity of the spillover effect (Baird et al. 2018).

< Table 1 >

As can be seen from Table 1, for most variables there are no baseline difference across the three different treatment arms. Specifically, there are no significant differences in the areas of household characteristics, and there are no significant differences between targeted respondents compared to the control group across the observed characteristics. However, some variables differ between the spillover and the control group as the spillover individuals report on average lower savings, loans, and investments.

The compliance rate upon invitation to the training among our baseline sample was 67.02%. Overall, few variables correlate with take-up of the training. Within the treated clusters, participation rates are lower on average among the more educated compared to those with fewer years of education. This may reflect that they think there is less to learn. Among those who are untargeted and thus serve as the spillover group, we find that wealth proxied by number of assets correlates negatively with training participation. This shows a tendency that those less well-off are more interested to participate in a financial training. Similar to McKenzie and Puerto (2021), we find a positive coefficient of previous training participation for take-up, however, our coefficient is not statistically significant.

The endline survey tracks 90.7% of the baseline sample, making attrition relatively low between the two waves, apart by about 15 months on average. However, respondents in the treatment clusters are 3.34% less likely to attrite than respondents in the control clusters (92.2% tracking rate in the treatment clusters against 88.87% in the control clusters). To understand why differential attrition occurs, we exploit the different reasons for attrition that have been documented in the endline survey, and we find suggestions that the differential attrition is due to the differences in business survival. There are five reasons for attrition: relocation, death, decline, sickness and imprisonment, and cases where the reason is unknown. Of these, relocation causes about 71% of the attrition cases, and only ten participants declined to participate in the endline survey. Those participants who relocated closed down the original business as interviewed in the baseline, and migrated to another region. In addition, about half of them indicated directly that their business collapsed and they had to shut down the business and leave. According to the baseline data, those

who relocated had lower business performance initially, compared to those participated in the endline survey.

When we regress the experimental design indicators on the attrition status and control for strata fixed effects, we find that participants in the control clusters are 3.8 percentage points more likely to attrite than those in the treatment clusters. Moreover, there is no difference in coefficients between being directly targeted or in the spillover group. Thus, the differential attrition is not driven by treatment intensity, but appears to be occurring on the trading center level. Rather, the difference in attrition is almost completely driven by the difference in relocation (or business closure) by treatment status of the trading centers. Those in the treated clusters were 3.0 percentage points less likely to close down their business and relocate between baseline and endline, compared to those in the control clusters. There are only minor differences in the probability of declining to participate in the endline survey across different arms, or other reasons. Due to the overall small differences in attrition, we do not further consider this issue in the following analysis.

### **3 Results**

In line with the training curriculum, we present outcomes in the five areas of the treatment, however, we arrange results here according to the focus of the training on the use of mobile money. Section 3.1 covers the general use of mobile money, Section 3.2 presents results on the specific behavior regarding money transfers which is the favorite application of mobile money, and Section 3.3 reports further results on record keeping, saving behavior, loan take-up, and investments. Some discussion is provided in Section 3.4.

The tables presenting results in these three sections are always organized in the same four panels. The first panel, i.e. Panel A, shows results under the assumption, that those invited to the treatment are compared to those in the control groups plus those in treatment clusters who were not invited, and thus can be also regarded as a control group; we label the result a “broad ITT effect.” Panel B informs about treatment effects in treated clusters relative to control clusters, and splits covered entrepreneurs in treated clusters into those being invited to the training (the “narrow ITT effect”) and those not being invited, i.e. the potential spillover group that we are particularly interested in. Panel C zooms further in, as it considers the saturation of treatments, here covered



linearly as a broad band from 1% saturation to 100%. This effect has to be added to the narrow ITT effect, so that it is analyzed whether and which direction saturation has an effect, separated from the treatment and the spillover group. Finally, Panel D shows the total causal effect for all individuals covered in treated clusters (i.e. invited and participating, invited and not participating, and not invited) vs. those in control clusters.

### 3.1 Effects on mobile money use

We measure the effects of the financial education training on the use of mobile money here in three groups, i.e., general account usage, use of mobile money savings, and use of mobile money loans. The expected effect from the treatment is an intensified use of mobile money and more mobile money savings, while an impact on mobile money loan take-up is open.

**Reading example.** Before presenting results of interest, we would like to give a reading example which shows exemplarily how tables can be understood. Therefore, we focus on Table 2 column (3), i.e. potential treatment effects on the incidence of MM savings. The first two lines provide results of the ITT effect. In Panel A the ASUTV-assumption is made, i.e. no spillovers. This coefficient is 0.067. Relative to the use of MM savings in the control group of 19.2%, the treatment increases the incidence of MM savings by 34.9%. Interestingly, however, we observe slightly negative spillovers as can be seen in line 3 for the spillover group, so that the narrow ITT effect is smaller than the broad ITT effect, i.e. 0.057. It is also statistically highly significant and implies an increase by 29.7%. Then we turn to Panel C, considering an effect from true saturation which is on average roughly 30%. Thus, the coefficient on “assigned to training” of 0.191 in combination with an average saturation effect of about 0.12 (interaction coefficient of  $0.004 \times 30$ ) leads to the same order of magnitude as the effect discussed above, being about 0.06. The same kind of calculation indicates that the average effect on the spillover group is negative, as seen before. Overall, we get our main result, here for the incidence of MM savings, that the financial education treatment can have high ITT effects, statistically and economically, but may generate also negative spillovers; these can be also seen in Panel D. These spillovers depend on the degree of saturation. Given the numbers of this case, a saturation of 50%, i.e. treating half of the population in a given cluster, appears to erase the otherwise positive treatment effects. If saturation is below 30%, ITT effects can be even higher than indicated here. We do not claim to take these

numbers literally, but this simple linear analysis shows a negative effect from increasing saturation.

< Table 2 >

**General MM account use.** Regarding general account usage, mobile money use is very widespread as more than 91% of the control group have an own account at endline and the range of activities is 1.87, relative to a maximum of 4. Thus, the point estimates of the training are positive but not statistically significant according to Panel B, with increases of 2 percentage points and 0.06 more activities, respectively (see columns 1 and 2 in Table 2). These effects present increases by more than 2% and 3%. Still, the interaction coefficients with saturation are significantly negative.

**MM savings.** The treatment impact is much stronger when it comes to MM savings behavior. We had discussed the impact on the incidence of MM savings above already (column 3). Regarding MM savings amount, some money is held in mobile money accounts in the control group, and this amount is higher by 24% higher in the narrow ITT group (column 4). In line with these results, the share of MM savings to total savings increases, too (column 5). Here, the spillover effect is positive, but due to decreasing general savings amounts (relative to control) as we will see later. In all three measures regarding MM savings, we get strong positive ITT effects but also significantly negative saturation effects.

**MM loans.** The use of MM loans is 9.1% for the control group, and there is no significant impact from the training (column 6). This does not indicate a failure because the training emphasizes a prudent use of loans so that it would be necessary to capture what had been done with the money and not whether a loan has been taken. Loan use, however, is not trivial to capture in a short survey.

**Interim summary.** The training has the expected effect on MM use and these effects can be economically large. However, there are hardly positive spillovers, rather coefficients signs tend to be negative. The saturation analysis indicates a possible explanation as it shows a negative effect from increasing saturation. This can be interpreted as a dominating effect from competition advantages over market increase: those treated create an advantage over the untreated, but if too many get treated there is not advantage anymore. The treatment impact remains with the treated and thus does not generate positive spillovers.

### **3.2 Effects on the use of mobile money transfers**

Transfers via mobile money to individuals in other places have been the historical starting point of mobile money and are the most often used MM-product. The training discusses the costs and safety of the four main forms of transfers, i.e., via mobile money, informal transfers (friends, busses etc.), semiformal (via semiformal financial institutions) and formal transfers via banks. An aspect of costs is, if possible, the avoidance of transferring very small amounts where minimum fees can amount to 20% of the transfer sum.

Comparing endline with baseline reveals an enormous increase in the use of MM-transfers. 78.6% of the control group use this service at endline, while just 30.5% did so at baseline. Moreover, MM-transfers have become the favorite form of transfer with 46%, ahead of informal transfers with 40% and the two other forms mentioned above, which are relatively negligible. This big push in using MM-transfers is due to the Corona-pandemic with its contact restrictions, favoring the use of MM, but it is not due to the treatment as Table 3 shows. Looking at the conventional ITT-effects documented in Panel B, there is no coefficient coming close to statistical significance, indicating that the training does not change behavior. Surprisingly, the MM-transfers even decline in the spillover group, relative to control, but this is mainly due to their lower baseline numbers, so that the same relative change looks smaller in absolute terms (see columns 1 and 2). This baseline effect also contributes to the decline of the share of MM-transfers (column 3) and of total fee amount paid for transfers (column 4), so that we can neglect that the coefficient on fees paid in Panel A (column 4) turns marginally significant. Overall, these results indicate that the training does not succeed in current form for the target group.

< Table 3 >

### **3.3 Effects on further outcomes**

While our financial education training has a quite novel focus on mobile money, it still covers more conventional topics. These topics are grouped into the five areas mentioned above. In general, the treatment has many effects in the intended direction measured by regression signs and several of these coefficients are statistically significant. While we had discussed the area (v) on financial

transfers above as it is dominated by mobile money, the four remaining areas are now discussed in the order as they are covered by the financial education treatment.

Regarding (i) personal financial management, we focus on record keeping and separating personal from business finances. Results in Table 4, Panel A, columns (1) and (2) show, that the point estimates are positive, but small and far away from statistical significance. Probably this change needs repetition, and not just a one-hour training, to exercise the new behavior so that a repeated training might be able to create sustained change. An undesired result is the significant coefficient in Panel C column (2) on the spillover group: actually, these coefficients should be close to zero and insignificant, because there are no spillovers when saturation is close to zero. The case here indicates that the linear specification is inappropriate and there may be non-linear effects (which we cannot identify with the limited number of observations).

< Table 4 >

The general training outcome seems different in area (ii), the savings domain. There are slight increases in having any savings, see column (3), and in the amount of savings (column 4). The increase is much stronger for bank savings only, both at the intensive and extensive margin, i.e., an increase of 29% at the extensive margin and about 27% at the intensive margin relative to control (columns 5 and 6). Panel C shows a negative effect from saturation as in Table 3 above. The net effect on savings is mainly a shift away from savings with semi-formal financial institutions which decreased in amount by 6.5 percentage points to a share of 60%. Still, such a shift towards formal savings, including an increase of MM savings, is intended by the treatment.

Results on the two remaining treatment areas are shown in Table 5. Regarding debt, i.e., area (iii), the focus is mainly on avoiding unproductive and too expensive debt. Thus, the intention is neither a general increase or decrease of debt, but a qualitative change, such as a shift towards cheaper formal loans. Coefficients in columns (1) to (3) point indeed into this direction, as total loans (incidence and amounts) slightly decrease but formal loans increase, with even higher point estimates for the spillover group; however, again the effects are too small to be likely precisely estimated in our sample with limited power. Finally, in line with an emphasis on stimulating longer-term economic success, treatment area (iv) is about the productive role of increased investments.

The treatment indeed causes increased incidence and amounts of investments. The effects of the treatment are statistically significant and economically substantial, as incidence increases by 6% and amount by 5%, respectively, at a time of economic crisis.

< Table 5 >

Overall, independent from the novel focus on mobile money, this treatment causes effects on standard areas of financial education as other trainings before. In short, effects are largest and significant regarding an increase in formal savings and investments.

### 3.4 Discussion

We structure this discussion according to the three effects of the financial education training being distinguished so far.

**Direct effects.** The direct effects of this training can be compared to the literature. It is well-known that some training areas of financial education generate better results than others. In particular, the saving education works well, while training on borrowing behavior provides mixed results (Kaiser and Menkhoff, 2017). This is largely, what we find, too. Moreover, the training curriculum here is very similar to the one of Kaiser and Menkhoff (2022), and the pattern of outcome effects is also very similar. Still, the degree of effectiveness may be somewhat smaller, possibly because the economic environment was so volatile due to Covid-19 and related long business and school closures, which may overlay treatment effects and increase heterogeneity (standard errors). New is the focus on mobile money, where training effects on use and saving of MM work well, while the area of transfer behavior does not create any effects.

**Spillover effects.** Coming to the second group of effects, i.e. spillovers in treated clusters from the invited to the non-invited, results are largely neutral. This seems also to be in line with the literature. The positive spillovers being documented seem to apply to studies covering the private domain of individuals. For example, individuals buy stocks because they learn this from neighbors, or parents learn from their children receiving financial education at school. However, if the business domain is covered in studies, there may be even negative spillover effects as the success of one small enterprise may come at the expense of a competing entrepreneur who does

not receive a training. Our study clearly falls in the business domain.

**Saturation effects.** Finally, the analysis of saturation effects appears to be new to the literature on financial education. Here we find tentatively negative effects which may be explained analogously to the spillover discussion. If a single entrepreneur is financially educated this creates a competitive advantage, but if all are educated, this increases the level of competition for all but does not create by itself an expansion of the market or of productivity. As an indication of such a mechanism, we repeat Table 2 but do not consider overall saturation but only the saturation within three distinguished industry sectors. Results in Table 6 cautiously indicate that saturation effects become stronger when the share of enterprises from one sector is higher, which is consistent with competition at work.

< Table 6 >

This result is not in contradiction to Angelucci and De Giorgi (2009) or Egger et al. (2022) examining general equilibrium effects of large cash transfers. These transfers are large enough to increase demand, and as a consequence to increase jobs. In our case, however, the increase is in knowledge and it is not easy to transform this into real output.

#### 4 Conclusion

The lack of knowledge and skills is an obvious bottleneck in the development of micro-enterprises. Consequently, policy invests into trainings to (partially) overcome this bottleneck, and financial education is typically part of these efforts. The literature on financial education has focused on the direct effects of these treatments and finds significant positive effects in general (Kaiser et al., 2022).

These positive effects are typically ITT-effects, i.e. considering also that part of the target group that does not follow the invitation to trainings. However, policy is not only interested in effects on the treated (and invited non-participants) but also on potential effects if the training is scaled up. Thus, our research studies two potential effects which may occur in such a situation: first, spillovers may occur from treated to non-invited in treated areas which would ease the scaling

in case of positive spillovers. Second, there may be saturation effects, i.e. the dependence of treatment effectiveness on the share of treated to the total population in the treatment area, and scaling up would be eased if such effects are either positive or neutral. Thus, we analyze in total three effects, i.e. the conventional direct effect of financial education, a spillover effect on the non-invited in treated areas, and a saturation effect estimating whether an increasing share of the treated impacts measured effect size. We find that the direct effects are often positive, the spillover effects seem to be largely neutral, and the saturation effects are negative.

This extends the literature in several ways. The basic RCT provides for another case that even a relatively short financial training can change behavior, also covering the use of mobile money. This is the basis to show that scaling this treatment – and keeping the degree of effectiveness – is not an easy task. While there is hardly any spillover in the general setting, this changes when we consider different degrees of saturation. We find that higher saturation has a negative effect on behavior change.

Regarding policy implications of these results, we first emphasize that the evidence comes from one RCT, and thus would obviously profit from further investigations. Moreover, we analyze impacts among micro-entrepreneurs who are competing against each other to some extent, so that positive spillovers reported in the literature when treating individuals in their private domain are no contradiction to the results here. The saturation result seems to fit into the scarce literature, that any positive ITT-effect of an RCT cannot be assumed to be fully scalable. This motivates to consider treatment effects beyond the direct ITT-effects. Regarding entrepreneurs it could be important to focus on trainings which care about increasing markets and not focusing only the single entrepreneur.

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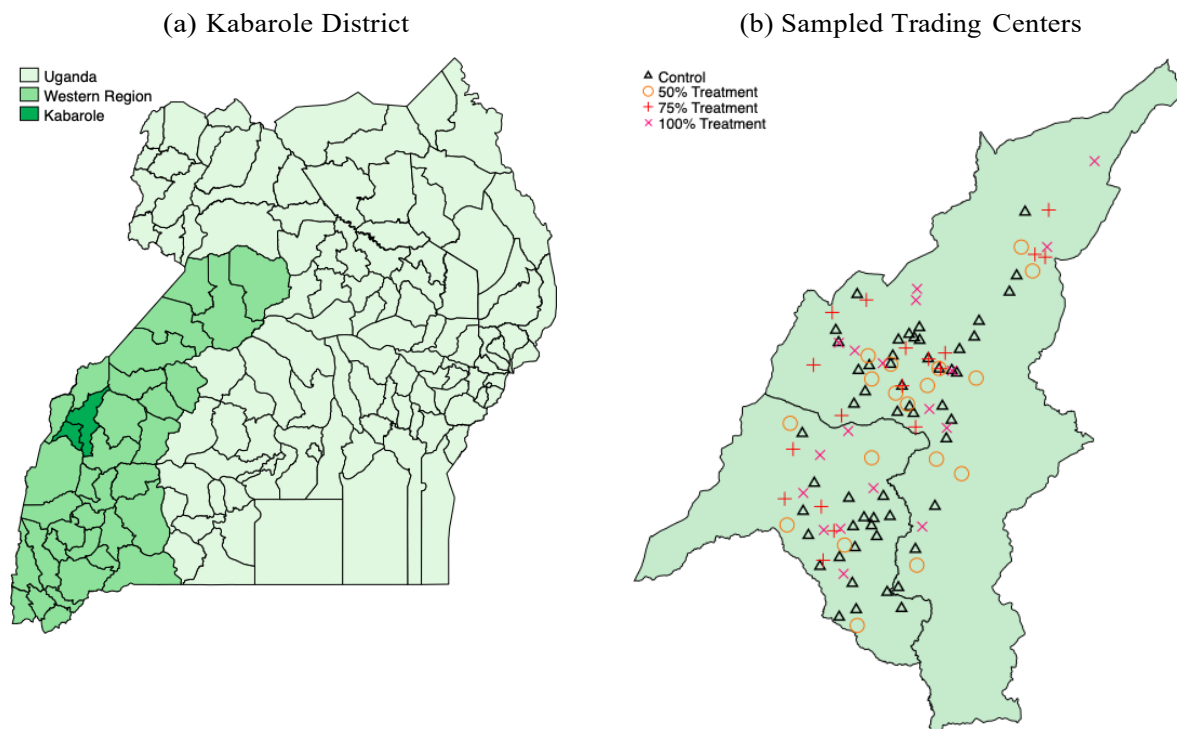
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**Figure 1: Study Setting in Western Uganda**



*Note:* The figure shows in (a) the location of the Kabarole district in Western Uganda, and in (b) the distribution of the 108 sampled trading centers in this district by treatment status.

**Table 1: Balance at Baseline in the Endline Sample (n=1,975)**

|  | <i>Means (Std. Dev.)</i>      |                            |                                 |  |
|--|-------------------------------|----------------------------|---------------------------------|--|
|  | Pure Control Group<br>(n=862) | Spillover group<br>(n=320) | Assigned to training<br>(n=793) | <i>Equality of<br/>means<br/>(p-value)</i> |
| Female (0/1)                                 | 0.638<br>(0.481)              | 0.661<br>(0.474)           | 0.615<br>(0.487)                | 0.348                                      |
| Age  | 33.681<br>(11.390)            | 34.886<br>(11.507)         | 34.554<br>(11.803)              | 0.202                                      |
| Married (0/1)                                | 0.504<br>(0.500)              | 0.498<br>(0.501)           | 0.515<br>(0.500)                | 0.755                                      |
| Years of Education                           | 8.957<br>(4.551)              | 9.132<br>(4.650)           | 8.826<br>(4.613)                | 0.130                                      |
| Financial Literacy (0-7)                     | 3.707<br>(1.637)              | 3.568<br>(1.633)           | 3.726<br>(1.641)                | 0.571                                      |
| Risk Tolerance (0-10)                        | 5.341<br>(2.708)              | 5.383<br>(2.809)           | 5.252<br>(2.694)                | 0.274                                      |
| Household Size                               | 3.992<br>(2.424)              | 4.065<br>(2.355)           | 4.104<br>(2.479)                | 0.496                                      |
| # Assets                                     | 37.660<br>(18.213)            | 37.685<br>(17.179)         | 38.058<br>(17.558)              | 0.782                                      |
| Household Consumption (UGX)                  | 506,097<br>(352,976)          | 517,887<br>(342,138)       | 505,608<br>(333,849)            | 0.561                                      |
| Any Mobile Money (MM) activity (0/1)         | 0.508<br>(0.500)              | 0.306<br>(0.461)           | 0.518<br>(0.500)                | 0.720                                      |
| # MM Activities (0-4)                        | 0.705<br>(0.807)              | 0.433<br>(0.731)           | 0.696<br>(0.782)                | 0.943                                      |
| Any MM Savings (0/1)                         | 0.100<br>(0.300)              | 0.063<br>(0.243)           | 0.073<br>(0.261)                | 0.228                                      |
| MM Savings Amount (IHST)                     | 1.188<br>(3.607)              | 0.713<br>(2.835)           | 0.905<br>(3.243)                | 0.308                                      |
| Share of MM Savings in Total Savings (%)     | 4.783<br>(18.994)             | 3.721<br>(17.727)          | 3.755<br>(17.370)               | 0.406                                      |
| Any Loan via MM (0/1)                        | 0.001<br>(0.036)              | 0.000<br>(0.000)           | 0.004<br>(0.067)                | 0.201                                      |
| Any MM Transfers (0/1)                       | 0.305<br>(0.461)              | 0.177<br>(0.382)           | 0.292<br>(0.455)                | 0.718                                      |
| MM Transfers Amount (IHST)                   | 3.716<br>(5.674)              | 2.102<br>(4.570)           | 3.474<br>(5.465)                | 0.562                                      |
| Share MM Transfers in Total Transfers (%)    | 29.297<br>(44.799)            | 16.349<br>(36.093)         | 27.507<br>(43.837)              | 0.611                                      |
| Total Fee Amt. Paid for Transfers (IHST)     | 2.350<br>(3.909)              | 1.289<br>(3.120)           | 2.207<br>(3.782)                | 0.544                                      |
| Keeps records (0/1)                          | 0.233<br>(0.423)              | 0.140<br>(0.347)           | 0.211<br>(0.409)                | 0.792                                      |
| Separate records business and personal (0/1) | 0.248<br>(0.432)              | 0.138<br>(0.345)           | 0.233<br>(0.423)                | 0.624                                      |
| Any Savings (0/1)                            | 0.802<br>(0.399)              | 0.456<br>(0.498)           | 0.783<br>(0.412)                | 0.534                                      |
| Savings Amount (IHST)                        | 10.688<br>(5.507)             | 6.078<br>(6.733)           | 10.498<br>(5.686)               | 0.548                                      |
| Any Bank Savings (0/1)                       | 0.154<br>(0.361)              | 0.108<br>(0.311)           | 0.176<br>(0.381)                | 0.081                                      |
| Bank Savings Amount (IHST)                   | 2.152<br>(5.131)              | 1.561<br>(4.512)           | 2.479<br>(5.403)                | 0.091                                      |
| Any Loan (0/1)                               | 0.356<br>(0.479)              | 0.196<br>(0.397)           | 0.343<br>(0.475)                | 0.674                                      |
| Total Loan Amount (IHST)                     | 10.669<br>(5.529)             | 10.017<br>(5.843)          | 10.175<br>(5.824)               | 0.203                                      |
| Any Formal Loan (0/1)                        | 0.054<br>(0.225)              | 0.037<br>(0.188)           | 0.041<br>(0.197)                | 0.493                                      |
| Any Investment (0/1)                         | 0.873<br>(0.333)              | 0.521<br>(0.500)           | 0.869<br>(0.338)                | 0.806                                      |
| Investment Amount (IHST)                     | 12.372<br>(4.890)             | 7.407<br>(7.174)           | 12.354<br>(4.963)               | 0.802                                      |
| Joint orthogonality (p-value)                |                               |                            |                                 | 0.410                                      |

Note: The table displays the summary statistics for the control group and by individual treatment status for the endline estimation sample (N=1,975) at baseline in 2019. In addition, p-values for the equality of means are shown. These are estimated with inverse sampling probability weights, enumerator fixed effects, field interview dummy, strata fixed effects and clustering standard errors at the trading center level. p-values are unadjusted for multiple hypothesis testing. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 2: Treatment effects on mobile money use**

|   | (1)<br>Any MM<br>activity<br>(savings,<br>transfer,<br>loan,<br>payments)<br>(0/1) | (2)<br>Number of<br>financial<br>activities in<br>which MM<br>is used<br>(max=4) | (3)<br>Any MM<br>Savings<br>(0/1) | (4)<br>MM<br>Savings<br>Amount<br>(IHST) | (5)<br>Share of<br>MM<br>Savings in<br>Total<br>Savings<br>(Percent) | (6)<br>Any loan<br>via Mobile<br>Money (0/1) |
|---|--|--|-----------------------------------|--|--|--|
| <i>Panel A: Assuming Stable Unit Treatment Value</i>            |  |  |                                   |  |  |  |
| Assigned to Training  | 0.027<br>(0.017)   | 0.130**<br>(0.057)   | 0.067***<br>(0.019)               | 0.657***<br>(0.232)                      | 1.034<br>(0.916)   | -0.005<br>(0.018)                            |
| <i>Panel B: Pooled Intention to Treat and Spillover Effects</i> |  |  |                                   |  |  |  |
| Assigned to Training  | 0.020<br>(0.018)   | 0.063<br>(0.058)   | 0.057***<br>(0.020)               | 0.541**<br>(0.248)                       | 2.452***<br>(0.896)  | -0.018<br>(0.015)                            |
| Spillover Group   | -0.012<br>(0.020)  | -0.118*<br>(0.064)   | -0.017<br>(0.026)                 | -0.204<br>(0.316)                        | 2.489*<br>(1.415)  | -0.023<br>(0.020)                            |
| T = S ( <i>p</i> – value)                                       | 0.116  | 0.008  | 0.003                             | 0.014                                    | 0.978  | 0.839  |
| <i>Panel C: Linear Saturation Analysis</i>                      |  |  |                                   |  |  |  |
| Assigned to Training  | 0.129**<br>(0.062)   | 0.452***<br>(0.169)  | 0.191***<br>(0.055)               | 1.923***<br>(0.712)                      | 7.045***<br>(2.248)  | 0.006<br>(0.043)                             |
| Spillover Group   | 0.111<br>(0.107)   | 0.376<br>(0.277)   | 0.132<br>(0.090)                  | 1.482<br>(1.091)                         | 2.075<br>(4.242)   | 0.012<br>(0.078)                             |
| Assigned to Training ×<br>True Saturation (%)                   | -0.003**<br>(0.002)  | -0.012**<br>(0.005)  | -0.004***<br>(0.002)              | -0.044**<br>(0.020)                      | -0.154**<br>(0.066)  | -0.001<br>(0.001)                            |
| Spillover Group ×<br>True Saturation (%)                        | -0.005<br>(0.004)  | -0.019<br>(0.011)  | -0.006<br>(0.003)                 | -0.063<br>(0.041)                        | 0.034<br>(0.181)   | -0.001<br>(0.003)                            |
| <i>Panel D: Total Causal Effect</i>                             |  |  |                                   |  |  |  |
| Treated Cluster   | 0.007<br>(0.016)   | -0.010<br>(0.051)  | 0.027<br>(0.020)                  | 0.243<br>(0.237)                         | 2.467***<br>(0.920)  | -0.020<br>(0.012)                            |
| Pure Control Mean   | 0.917  | 1.866  | 0.192                             | 2.243                                    | 4.558  | 0.091  |
| Pure Control Std. Dev.  |  | 0.884  |                                   | 4.705                                    | 16.454   |  |

**Table 3: Treatment effects on transfers**

|   | (1)<br>Any MM<br>transfers (0/1) | (2)<br>MM Transfers<br>Amount (IHST) | (3)<br>Share of MM<br>Transfers in<br>Total Transfers | (4)<br>Total fee amount<br>paid for transfers<br>(IHST) |
|---|----------------------------------|--------------------------------------|---|---|
| <i>Panel A: Assuming no Interference</i>                        |                                  |                                      |   |   |
| Assigned to Training  | 0.026<br>(0.029)                 | 0.318<br>(0.377)                     | -0.165<br>(2.374)                                     | 0.401*<br>(0.204)                                       |
| <i>Panel B: Pooled Intention to Treat and Spillover Effects</i> |                                  |                                      |   |   |
| Assigned to Training  | -0.025<br>(0.030)                | -0.294<br>(0.397)                    | -0.550<br>(2.628)                                     | 0.137<br>(0.227)  |
| Spillover Group   | -0.090**<br>(0.035)              | -1.073**<br>(0.474)                  | -0.676<br>(3.071)                                     | -0.464<br>(0.283)                                       |
| T = S ( <i>p</i> – value)                                       | 0.058                            | 0.087                                | 0.966   | 0.0188  |
| <i>Panel C: Linear Saturation Analysis</i>                      |                                  |                                      |   |   |
| Assigned to Training  | 0.056<br>(0.079)                 | 0.746<br>(1.016)                     | 0.371<br>(7.910)                                      | -0.142<br>(0.672)                                       |
| Spillover Group   | -0.061<br>(0.129)                | -1.205<br>(1.756)                    | -8.508<br>(11.657)                                    | -1.240<br>(1.176)                                       |
| Assigned to Training ×<br>True Saturation (%)                   | -0.003<br>(0.002)                | -0.035<br>(0.029)                    | -0.039<br>(0.222)                                     | 0.008<br>(0.020)  |
| Spillover Group ×<br>True Saturation (%)                        | -0.001<br>(0.006)                | 0.009<br>(0.077)                     | 0.321<br>(0.494)                                      | 0.030<br>(0.049)  |
| <i>Panel B: Total Causal Effect</i>                             |                                  |                                      |   |   |
| Treated Cluster   | -0.051*<br>(0.028)               | -0.606<br>(0.371)                    | -0.601<br>(2.417)                                     | -0.104<br>(0.221)                                       |
| Pure Control Mean   | 0.786                            | 10.031                               | 46.541  | 8.276   |
| Pure Control Std. Dev.  |                                  | 5.501                                | 38.752  | 3.850   |

**Table 4: Treatment effects on record keeping and savings outside mobile money**

|  | (1)<br>Keeps<br>records<br>(0/1) | (2)<br>Keeps<br>money<br>separate for<br>business<br>and<br>personal<br>finances<br>(0/1) | (3)<br>Any<br>Savings<br>(0/1) | (4)<br>Savings<br>Amount<br>(IHST) | (5)<br>Any<br>Bank<br>Savings<br>(0/1) | (6)<br>Bank<br>Savings<br>Amount<br>(IHST) |
|--|----------------------------------|---|--------------------------------|------------------------------------|--|--|
| <i>Panel A: Assuming no Interference</i>                 |                                  |   |                                |                                    |  |  |
| Assigned to Training                                     | 0.008<br>(0.025)                 | 0.040*<br>(0.023)   | 0.028<br>(0.018)               | 0.452*<br>(0.272)                  | 0.049**<br>(0.023)                     | 0.567*<br>(0.310)                          |
| <i>Panel B: Intention to Treat and Spillover Effects</i> |                                  |   |                                |                                    |  |  |
| Assigned to Training                                     | 0.008<br>(0.031)                 | 0.022<br>(0.028)  | 0.020<br>(0.020)               | 0.207<br>(0.291)                   | 0.053**<br>(0.022)                     | 0.627**<br>(0.298)                         |
| Spillover Group  | 0.001<br>(0.037)                 | -0.031<br>(0.027)   | -0.013<br>(0.022)              | -0.431<br>(0.335)                  | 0.007<br>(0.024)                       | 0.105<br>(0.329)                           |
| T = S ( <i>p</i> – value)                                |                                  |   |                                |                                    |  |  |
| <i>Panel C: Linear Saturation Analysis</i>               |                                  |   |                                |                                    |  |  |
| Assigned to Training                                     | 0.006<br>(0.068)                 | 0.053<br>(0.081)  | 0.040<br>(0.056)               | 0.219<br>(0.731)                   | 0.160***<br>(0.054)                    | 1.995***<br>(0.739)                        |
| Spillover Group  | 0.119<br>(0.143)                 | 0.207**<br>(0.100)  | 0.011<br>(0.097)               | 0.552<br>(1.520)                   | 0.012<br>(0.092)                       | 0.622<br>(1.300)                           |
| Assigned to Training ×<br>True Saturation (%)            | 0.000<br>(0.002)                 | -0.001<br>(0.002)   | -0.001<br>(0.002)              | 0.001<br>(0.021)                   | -0.004**<br>(0.002)                    | -0.045**<br>(0.021)                        |
| Spillover Group ×<br>True Saturation (%)                 | -0.005<br>(0.006)                | -0.010**<br>(0.004)   | -0.001<br>(0.004)              | -0.040<br>(0.062)                  | 0.000<br>(0.004)                       | -0.016<br>(0.057)                          |
| <i>Panel D: Total Causal Effect</i>                      |                                  |   |                                |                                    |  |  |
| Treated Cluster  | 0.005<br>(0.030)                 | 0.001<br>(0.025)  | 0.007<br>(0.018)               | -0.049<br>(0.267)                  | 0.034*<br>(0.018)                      | 0.418*<br>(0.248)                          |
| Control Mean   | 0.485                            | 0.562   | 0.853                          | 11.526                             | 0.181                                  | 2.332                                      |
| Control Std. Dev.  |                                  |   |                                | 5.247                              |  | 5.222                                      |

**Table 5: Loans, business investments and profits**

|  | (1)<br>Any loan<br>(0/1) | (2)<br>Total Loan<br>Amount<br>(IHST) | (3)<br>Any formal<br>loan<br>(0/1) | (4)<br>Any<br>investment<br>(0/1) | (5)<br>Investment<br>Amount<br>(IHST) |
|--|--------------------------|---------------------------------------|------------------------------------|-----------------------------------|---------------------------------------|
| <i>Panel A: Assuming no Interference</i>                 |                          |                                       |                                    |                                   |                                       |
| Assigned to Training                                     | -0.001<br>(0.019)        | 0.025<br>(0.218)                      | 0.003<br>(0.016)                   | 0.060***<br>(0.020)               | 0.811***<br>(0.276)                   |
| <i>Panel B: Intention to Treat and Spillover Effects</i> |                          |                                       |                                    |                                   |                                       |
| Assigned to Training                                     | -0.020<br>(0.021)        | -0.364<br>(0.273)                     | 0.016<br>(0.015)                   | 0.044**<br>(0.022)                | 0.574*<br>(0.309)                     |
| Spillover Group  | -0.033<br>(0.023)        | -0.683**<br>(0.299)                   | 0.022<br>(0.019)                   | -0.027<br>(0.029)                 | -0.415<br>(0.399)                     |
| T = S ( <i>p</i> – value)                                |                          |                                       |                                    |                                   |                                       |
| <i>Panel C: Linear Saturation Analysis</i>               |                          |                                       |                                    |                                   |                                       |
| Assigned to Training                                     | -0.036<br>(0.058)        | -0.199<br>(0.643)                     | 0.008<br>(0.047)                   | 0.082<br>(0.059)                  | 1.143<br>(0.873)                      |
| Spillover Group  | 0.121<br>(0.087)         | 1.296<br>(1.070)                      | -0.207**<br>(0.089)                | 0.284***<br>(0.097)               | 3.907***<br>(1.455)                   |
| Assigned to Training ×<br>True Saturation (%)            | 0.001<br>(0.002)         | -0.003<br>(0.019)                     | 0.000<br>(0.001)                   | -0.001<br>(0.002)                 | -0.014<br>(0.027)                     |
| Spillover Group ×<br>True Saturation (%)                 | -0.006*<br>(0.004)       | -0.080*<br>(0.044)                    | 0.009**<br>(0.004)                 | -0.012***<br>(0.004)              | -0.173***<br>(0.060)                  |
| <i>Panel D: Total Causal Effect</i>                      |                          |                                       |                                    |                                   |                                       |
| Treated Cluster  | -0.025<br>(0.019)        | -0.492*<br>(0.256)                    | 0.018<br>(0.014)                   | 0.016<br>(0.021)                  | 0.178<br>(0.303)                      |
| Control Mean   | 0.792                    | 10.669                                | 0.090                              | 0.763                             | 10.698                                |
| Control Std. Dev.  |                          | 5.529                                 |                                    |                                   | (6.068)                               |



**Table 6: Linear saturation analysis within sector**

|  | (1)<br>Any MM<br>activity<br>(savings,<br>transfer, loan,<br>payments)<br>(0/1) | (2)<br>Number of<br>financial<br>activities in<br>which MM<br>is used<br>(max=4) | (3)<br>Any MM<br>Savings<br>(0/1) | (4)<br>MM Savings<br>Amount<br>(IHST) | (5)<br>Share of<br>MM<br>Savings in<br>Total<br>Savings<br>(Percent) |
|--|---|--|-----------------------------------|---------------------------------------|--|
| Assigned to Training                                   | 0.099<br>(0.072)  | 0.842***<br>(0.217)  | 0.421***<br>(0.108)               | 3.995***<br>(1.270)                   | 5.471<br>(3.409)   |
| Spillover Group  | 0.400***<br>(0.151)   | 1.282764**<br>(0.510)  | 0.477**<br>(0.210)                | 5.669**<br>(2.492)                    | 9.780<br>(10.054)  |
| Assigned to Training ×<br>Within sector saturation (%) | -0.001<br>(0.002)   | -0.0217***<br>(0.006)  | -0.010***<br>(0.003)              | -0.010***<br>(0.036)                  | -0.172<br>(0.112)  |
| Spillover Group ×<br>Within sector saturation (%)      | -0.015<br>(0.006)   | -0.049**<br>(0.0208)   | -0.015*<br>(0.008)                | -0.187*<br>(0.097)                    | -0.148<br>(0.457)  |
| Pure Control Mean                                      | 0.917   | 1.866  | 0.192                             | 2.243                                 | 4.558  |
| Pure Control Std. Dev.                                 |   | 0.884  |                                   | 4.705                                 | 16.454   |