Remitting against poverty: Eight-year evidence on digital remittances and rural poverty

Jean Lee, Mahreen Mahmud, Jonathan Morduch,
Saravana Ravindran, Abu Shonchoy, and Shashank Sreedharan*

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Abstract

Upward mobility for poor, rural workers often involves migrating to cities. To enable safer and more efficient remittance-sending, a randomized intervention introduced mobile banking to 815 "ultra-poor" urban migrant-household pairs in rural northwest Bangladesh. One year later, the intervention, which cost $PPP 32 on average, had increased urban-to-rural remittances, decreased rural poverty, and improved conditions in the lean season. Eight years later in 2023, treated households had 33 percent more productive assets by value. The treatment and control groups were using mobile money to comparable extents by then, and treatment effects on rural household consumption, income, poverty, and financial outcomes were no longer detectable.

*Lee: World Bank (jlee20@worldbank.org); Mahmud: University of Exeter (m.mahmud@exeter.ac.uk); Morduch: NYU (jonathan.morduch@nyu.edu); Ravindran: National University of Singapore (saravana@nus.edu.sg); Shonchoy: Florida International University (shonchoy@fiu.edu); Sreedharan: NYU (shashank.sreedharan@nyu.edu). We thank Seth Garz for motivating the study and the Gates Foundation for financial support. We are grateful for comments from presentations at Wharton, the World Bank ABCDE Conference, Oxford, Madrid, and KU Leuven. Ravindran received funding from the Lee Kuan Yew School of Public Policy, NUS. Preksha Jain and Tillmann Spindeldreier provided excellent research assistance, and Development Research Initiative (dRi) implemented the study in Bangladesh. The original study was registered in the AEA RCT Registry (AEARCTR-0003149), which has been supplemented with the pre-analysis plan for this long-run study. All views and any errors are our own.

1 Introduction

Migration has re-shaped low-income and middle-income economies as workers move to seek higher pay, most often moving from villages to cities. In Bangladesh, for example, the urban population has grown from 24 percent in 2000 to 41 percent in 2024 (World Bank, 2024).[1] Urban migrants have then sent remittances back to villages, spreading their gains. Like microfinance programs and "graduation" programs, the strategy of migrating and then remitting money home can bring large sums into poor rural areas.[2]

Sending money home through traditional means (like using a specialized remittance service or by traveling home with money directly) is not always cheap or safe, and remitting by using mobile money has gained popularity by offering reliability at a low cost (Suri and Jack, 2016). We present results from an 8-year follow up of an intervention that substantially expanded the use of mobile money among internal migrants and their families in Bangladesh (Lee et al., 2021).[3] The intervention targeted the poorest residents of one of the poorest regions of Bangladesh.

At baseline, the rural sample was classified as "ultrapoor," holding little land and earning income mainly through wage labor on other people's farms, similar to samples from northwest Bangladesh in Balboni et al. (2022), Bryan et al. (2014), and Bandiera et al. (2017). While the "graduation" interventions in Balboni et al. (2022) and

[1] International migration gets the most attention (see, e.g., McKenzie 2017 and Clemens 2011), and, globally, Bangladesh is a large source of international migrants (e.g., Mobarak et al. 2023). Migrants sent US$22.1 billion back to Bangladesh in 2022, the 7th highest level of international remittances in the world. Overall, however, there are many more internal migrants than international migrants.

[2] On microfinance in Bangladesh, see Armendáriz and Morduch (2010); Berg and Emran (2020), and Islam and Maitra (2012). On "graduation" programs, which transfer assets coupled with a range of training and other interventions, see Balboni et al. (2022); Bandiera et al. (2017); Banerjee et al. (2021) and Barker et al. (2024).

[3] Relative to traditional ways of remitting (e.g., returning to villages in person or paying for market-based remittance services), using mobile money can make sending remittances cheaper and more reliable (Stuart, 2020). Table 1 in Lee et al. (2021) shows that mobile money reduced the cost of sending $50 from Dhaka to northwest Bangladesh from about 20% of the remittance value and a lost day of work to 2% of the value with instant transmission.

Bandiera et al. (2017) are intensive (involving training and asset transfers) and thus relatively costly, the intervention in Lee et al. (2021) is light-touch and cost less than $12 per family ($PPP 32 in 2015 international dollars) because it could layer on top of existing migration practices and existing mobile money networks.

The intervention involved a one-time group-based training on how to use the bKash mobile banking service, the largest mobile money provider in Bangladesh. The intervention also provided translations of menus in the app and helped participants secure documents and other requirements to sign up for accounts. The intervention was implemented roughly simultaneously in two sites: a rural remittance-receiving sample in Gaibandha district and in a sample of migrants from the same households who had moved to Dhaka to find jobs. The dual-site study thus introduced mobile banking technology to both sides of the migration-remittance relationship, providing information and coordination.

The short-term results in Lee et al. (2021) show that the intervention led to a sharp rise in mobile money usage to 60 percent of the treatment group. Migrants who actively used mobile banking accounts increased the value of remittances by 26% of the control group baseline mean. This led to a 7.5% increase in daily per capita consumption in the rural sample and a decline in extreme poverty, reduction in borrowing, increase in saving, and improvements during the lean season. Self-employment and out-migration for work increased. Migrants, who were now sending higher levels of remittances, experienced an 11% decline in poverty and an increase in savings, but they reported worse physical and mental health compared to the control group of migrants.[4] These results build on related work in Kenya by Jack and Suri (2014) and Suri and Jack (2016) and in Tanzania by Riley (2018), together with evidence on the poverty-reducing impact of internal migration in Bangladesh (Bryan et al., 2014).

[4]Lee et al. (2022) find that the intervention also led to a narrowing of the gender gap in the use of mobile banking, with the female-to-male ratio of active users in the treatment group rising to 85% at endline relative to 35% in the control group. Narrowing the gender gap in usage did not, however, lead to similar patterns of sending digital remittances by gender. Male migrants increased remittances by 11 times as much as female migrants.

Eight years later, we followed up on the same sample of household-migrant pairs to evaluate the long-run impacts of early adoption of mobile money. The long-run follow-up survey was conducted between July and September 2023.[5] By 2023, the use of mobile banking had spread broadly in Bangladesh, and most of the control group had now adopted the technology. In 2023, 71 percent of the original control group was actively using mobile banking, an increase from 19 percent using bKash (the largest mobile banking service in Bangladesh) in 2014-15.[6]

Despite the spread of mobile banking, eight years later the original adopters in the treatment group had higher levels of total assets, driven by a 33% increase in the value of productive assets (predominantly concentrated in the value of agricultural land) relative to the control group and an increase of 66% over the baseline. The result contrasts with Suri and Jack (2016), who do not find that access to mobile money leads to long-run impacts on physical assets in Kenya.

Other impacts which were notable in the short-run treatment-control comparisons in Bangladesh are no longer detectable in the 2023 follow-up, consistent with the catch-up in mobile money use by the control group. We find null results on rural household consumption, income, poverty, and financial outcomes. We find a small decrease in the number and log value of remittances (consistent with migrants re-joining their families) and a decrease in wage labor (consistent with working more on one's own farm). Treatment effects on the health status of migrants found in the short-run evaluation (Lee et al., 2021) are no longer negative (impacts are now comparable between treatment and control groups).

[5]We were able to survey 82 percent of the rural households (665 of 813) and 77 percent of migrants (625 of 809) in the original study, a long-run follow-up survey rate similar to the 82.5 percent follow-up rate over approximately a decade in Baird et al. (2016) and the re-survey rate in Balboni et al. (2022) over a similar horizon. Attrition is overall balanced across treatment and control arms, although there are a few significant interactions with baseline (the relevant variables are included in the main ANCOVA specifications).

[6]As described below, the 19% usage rate in the control group reflects a 6-month reference period, and it only pertains to he use of bKash which was the dominant mobile banking provider in 2014-16. The usage data reported in Lee et al. (2021) covers the 13 month period from June 2015 - June 2016.

Given the extent of mobile banking in the control group by 2023, we cannot rule out that the positive impacts on consumption and poverty reduction, which were detectable in the original study, persist in 2023 but are now experienced in both the treatment and control groups, leading to small and not statistically significant treatment-control differences. This narrowing of the treatment-control comparison is similar to the long-run results of Barker et al. (2024) who find "fading effects" in intention-to-treat estimates (but not necessarily absolute reductions in impact for the treatment group) of a graduation program in Ethiopia attributable to catch-up by the control group.

When we consider just the impact on the value of productive assets after eight years, we calculate a benefit-cost ratio of 85. This is due to the large estimated long-run impact on land-holdings relative to the low added cost of the intervention (i.e., the intervention layers on top of existing investments to train migrants for factory work).

The result suggests the possibility that extending access to mobile money in low-income communities with migrants—where frictions keep the poorest and least educated residents from adopting the technology—can yield sizable long-run impacts relative to the size of the initial investment.

Section 2 discusses the context and sites, and describes the data and attrition. Section 3 describes the empirical strategy and interpretation. Section 4 describes the results, and Section 5 concludes.

2 Data and context

2.1 Context and intervention

The mobile money service bKash launched in 2011, and by 2014, the mobile money network had expanded sufficiently to make it a useful technology for rural adopters. Local agents operated in both Gaibandha District, the rural site for the study, and Dhaka, the urban site, making mobile money transfers possible from the urban to

rural northwest Bangladesh. At the baseline in 2014, 19 percent of the rural control group had actively used bKash in the past six months.[7] The period of the original study (2014-16) thus provided a promising window for studying the impact of mobile money, given the scope for increasing usage beyond 19%.

The initial intervention took place in April and May 2015, and consisted of a 30 to 45 minute training about how to sign up for and use the bKash service. This training was supplemented with basic assistance with enrollment in the bKash service. For example, if requested, the locally-recruited field staff assisted with gathering the necessary documentation for signing up for bKash and completing the application form.

The intervention aimed to reduce the main barriers to adoption of mobile banking, including lack of information and language constraints. At the time, mobile banking services in Bangladesh used Unstructured Supplementary Service Data (USSD) menus which allowed mobile banking services to be used on basic feature phones (i.e., phones that are not smartphones). The menus, however, were in English, creating a large hurdle for poorer villagers in Gaibandha with only basic levels of numeracy and literacy even in Bangla (Bengali). The intervention taught the basic steps and protocols of bKash use, and provided participants with practical, hands-on experience sending transfers to establish proficiency with the service.[8]

[7] In Lee et al. (2021), active account use was defined as 1 if the household performed any type of bKash transaction over the 13 month period from June 2015 - June 2016 (including deposits, withdrawals, remittances, and airtime top-ups) or 0 otherwise, constructed using administrative data from bKash. In the 2023 long-run follow-up, however, the question was asked with a reference period of 6 months. Active use in Lee et al. (2021) was reported as 22 percent in control and 70 percent in treatment. The figures here are lower because we have re-calculated the earlier data to conform to the 6-month reference period in the 2023 survey. The new numbers show that in 2016 the migrant control group used mobile banking at a rate of 19 percent, and the migrant treatment group usage rate was 54 percent (data are from the appendix to Lee et al. 2021).

[8] Within the treatment group, we also cross-randomized: (1) whether migrants were approached before or after their sending households (whether they were first or second movers) and (2) whether migrant-household pairs received a pro-social marketing message that emphasized the benefits of the technology for their family as well as for themselves as individuals. While recruitment was coordinated so that pairs were visited within a short time of each other, they were not primed in any way to transact with each other. See Lee et al. (2022). At the midline we also added a survey that measured willingness-to-pay for household items after asking respondents to think in terms of

The intervention increased usage in the treatment group which allowed estimation of the impact of bKash adoption. At the endline in 2016, Lee et al. (2021) estimate that 60 percent of the rural treatment group actively used bKash in the preceding six months, an increase of 41 percentage points due to the intervention.

The spread of mobile banking then continued throughout the country, and the range of providers increased. Bangladesh, as a whole, also experienced steady economic improvements during this period. Between 2014 and 2022, Bangladesh's GDP per capita grew from 116,618 taka to 177,301 taka, adjusted for inflation, an increase of 52 percent or roughly 4.8% per year on average.[9] The question here is whether the impacts on early adopters of mobile money persisted to be detectable eight years later

2.2 Data

2.2.1 Original sample

The analysis focuses on paired rural-urban samples where the urban sample includes migrants identified in the initial study (in 2014-16) as living and working in Dhaka. The rural sample is comprised of the migrants' families in Gaibandha District in northwest Bangladesh, about 8-12 hours from by bus from Dhaka. Gaibandha is one of the poorest regions of Bangladesh, and has been historically vulnerable to seasonal food insecurity during the *monga* lean season. The per capita consumption of the sample in Gaibandha is similar to that of the "ultrapoor" target groups in graduation programs (Bandiera et al., 2017) and the temporary migrants in Bryan et al. (2014).

Sample recruitment in the original study took place between September 2014 and February 2015, focusing on households in Gaibandha District with family members who had migrated to Dhaka. Recruitment was initially from a roster of migrant-sending

bKash or cash (Lee et al., 2023).

[9]Data are from World Bank Indicators. `https://data.worldbank.org/indicator/NY.GDP.PCAP.KN?end=2022&locations=BD&start=2014`.

households who had participated in a training program (SHIREE) through the non-governmental organization Gana Unnayan Kendra. The program prepared young adults to work in factories making ready-made garments. This sample was supplemented by snowball sampling to reach a final sample of 815 household-migrant pairs. The snowball sample was recruited by contacting households in the rural SHIREE sample and asking them to suggest households that were similar to them in two dimensions: (1) having a household member that had migrated to Dhaka for work, and (2) that were also poor.

At the start of the study, in 2014-15, approximately three quarters of the rural households were poor at baseline as measured by the local poverty line, while 51 percent were poor according the global line for extreme poverty ($1.90 per day, measured at 2011 PPP exchange rates and converted to 2014 taka with the Bangladesh CPI). At baseline, rural households in the sample were functionally landless, owning on average 0.1 acre (10 decimals) of land, just enough for their homesteads, and primarily worked as wage laborers. Wage labor (primarily on farms owned by others) accounted for 65 percent of household income, while remittances accounted for 21 percent. Self-employment accounted for another 6 percent of income, and farming contributed just 5 percent.

2.2.2 Long-run follow-up sample

The eight-year follow-up survey was conducted in July through September 2023. The surveys include a broad set of questions that capture economic activity, household consumption, assets, the flow of remittances, and the use of mobile money services.

In May 2024, we conducted a short, 10-question telephone resurvey in the rural sample, focused on housing conditions (housing quality, toilet quality, number of rooms in the house) and assets (livestock ownership and value). The survey team was able to resurvey 542 of 662 rural households (82 percent) from long-run follow-up sample.

The original sample started with 815 rural-urban household pairs. Eight observations

could not be used, leading to a final sample of 813 rural households and 809 urban households. Overall, we were able to re-survey 82 percent of the baseline rural sample and 77 percent of the baseline migrant sample. The final sample is thus 665 households and 625 migrants, with a subsample of 593 matched rural-urban household pairs.[10]

These rates are similar to follow-up rates in other long-run studies, such as the long-run follow up of the deworming intervention in Kenya conducted by Baird et al. (2016) and the graduation intervention studied by Balboni et al. (2022). Similar to Baird et al. (2016), who had an 84 percent tracking rate over a comparable period, we follow a highly mobile population as we are studying migrants and families with migrants.

3 Empirical Strategy

3.1 Estimation

The research design involves two sites, Dhaka and Gaibandha, with paired migrants and family members. We follow a pre-analysis plan and analyze the urban and rural samples separately.

The pre-registered primary outcomes of interest for rural households are the use of mobile money, remittances received, consumption, squared poverty gap and assets. Secondary outcomes include borrowing and saving, financial inclusion, migration and household size, income sources/self-employment and wage labor, education, child labor, health, food security and consumption smoothing, productive and non-productive assets and poverty headcount.

The primary outcomes of interest for urban migrants include the use of mobile money, remittances sent, poverty and expenditures. The secondary outcomes for urban

[10]Of the 413 rural treatment households, 87 (21%) could not be reached and of the 402 control households, 66 (16.4%) could not be reached in the rural areas. In the urban site, 102 (24.7%) households in the treatment and 93 (30.1%) in the control group could not be reached.

migrants include borrowing and saving, employment and time use and health.[11]

We report intention-to-treat (ITT) effects using an Analysis of Covariance (ANCOVA) specification:

$$Y_{i,t+1} = \beta_0 + \beta_1 Treatment_i + \beta_2 Y_{i,t} + X_i + \epsilon_i \quad (1)$$

where i indexes either the rural household or the urban migrant and Y_i denotes the outcome of interest. $Treatment_i$ is an indicator for the household-migrant pair being randomised to receive the mobile banking training. Periods t and $t+1$ refer to the baseline and long-run follow-up survey, respectively. X_i is a vector of baseline controls selected using PDSLASSO as specified in the registered pre-analysis plan. Variables that are imbalanced at baseline are also included in controls.

We conduct the analysis separately for the rural household and urban migrant samples. Since randomization occurred at the household level, we do not cluster standard errors. We focus primarily on outcomes in levels (vs. log or inverse hyperbolic sine transformations) following Chen and Roth (2023) and Mullahy and Norton (2022).

For remittances, we collect monthly data for the previous six months. To exploit the temporal variation in this, we will also estimate the following equation on the stacked baseline and follow-up migrant-month level data:

$$Y_{i,t} = \beta_1 Followup_t + \beta_2 Treatment_i * Followup_t + \sum_{t=1}^{6} \beta_{3,t} Month_t + \beta_{4,i} + \epsilon_{i,t} \quad (2)$$

Here, $\beta_{3,t}$ captures month fixed effects and $\beta_{4,i}$ refers to migrant fixed effects. $Followup_t$ is an indicator for a followup observation. The coefficient of interest is β_2, the

[11] We did not measure asset-holdings by migrants and focus only on assets held by rural households. The pre-analysis plan mistakenly identified assets as a primary outcome for urban migrants.

coefficient on the interaction between $Treatment_i$ and $Followup_t$. For regressions using equation (2), standard errors are clustered at the migrant level.

We correct for testing multiple hypotheses across primary and secondary outcomes and within a family of outcomes. We show result as indices of standardized variables and then use Anderson's q-values to adjust for the false discovery rate (Anderson, 2008). We use Romano-Wolf corrections within families of outcomes.

For poverty analyses, we use data in 2015 taka and, since the monetary variables are deflated to 2015 levels, we define the poverty headcount using the 2015 rural poverty line of 74.21 taka per day per person and urban poverty line of 98.59 taka per day per person, used in Lee et al. (2021).

For monetary values, the deflated data are converted to 2015 international dollars using data from World Bank Indicators ($1 = 27.05 taka). Asset and consumption data are winsorized at 1% and 99%.

3.2 Attrition

There is no signficant difference in attrition in the urban sample (2.6 percentage points, p = 0.389; Table A2, Panel A). In the rural sample, however, the treatment group had 4.6 percentage points greater attrition (p = 0.089; Table A1, Panel A). Total asset value is a significant negative predictor of attrition overall but not differentially for the treatment group. Differential attrition increases with rural daily per capita expenditures and whether the household head is female in the rural sample (shown in Panels B and C of Tables A1 and A2).

To assess whether the main results are robust to selective attrition, we implement Lee (2009) bounds for all primary outcomes with significant treatment effects. The Lee bounds approach trims the treatment group to equalize retention rates with the control group, calculating treatment effects under alternative assumptions about the outcomes of those who dropped out. We report both lower bounds (assuming attriters had the best outcomes) and upper bounds (assuming attriters had the worst outcomes). For remittances, the bounds are calculated on 7-month totals rather than

monthly averages due to data structure requirements, which limit comparability.

4 Results

4.1 First Stage: Mobile Money Use and Impact on Remittances

Figure 1 presents first stage results. The figures show averages of outcomes for treatment and control groups at year 0 (baseline); year 1 (the short-run analyzed in Lee et al. 2021), and year 8 (the long-run follow up). Intention-to-treat estimates and 95% confidence intervals are shown for years 1 and 8.[12]

Panels A (rural households) and B (urban migrants) show the rapid increases in mobile money usage for the treatment group in year 1 described by Lee et al. (2021). While the intervention focused on the bKash service, the outcomes are not restricted to bKash. In both rural and urban samples, mobile money usage increased by 49 percentage points ($p < 0.001$) after the intervention.

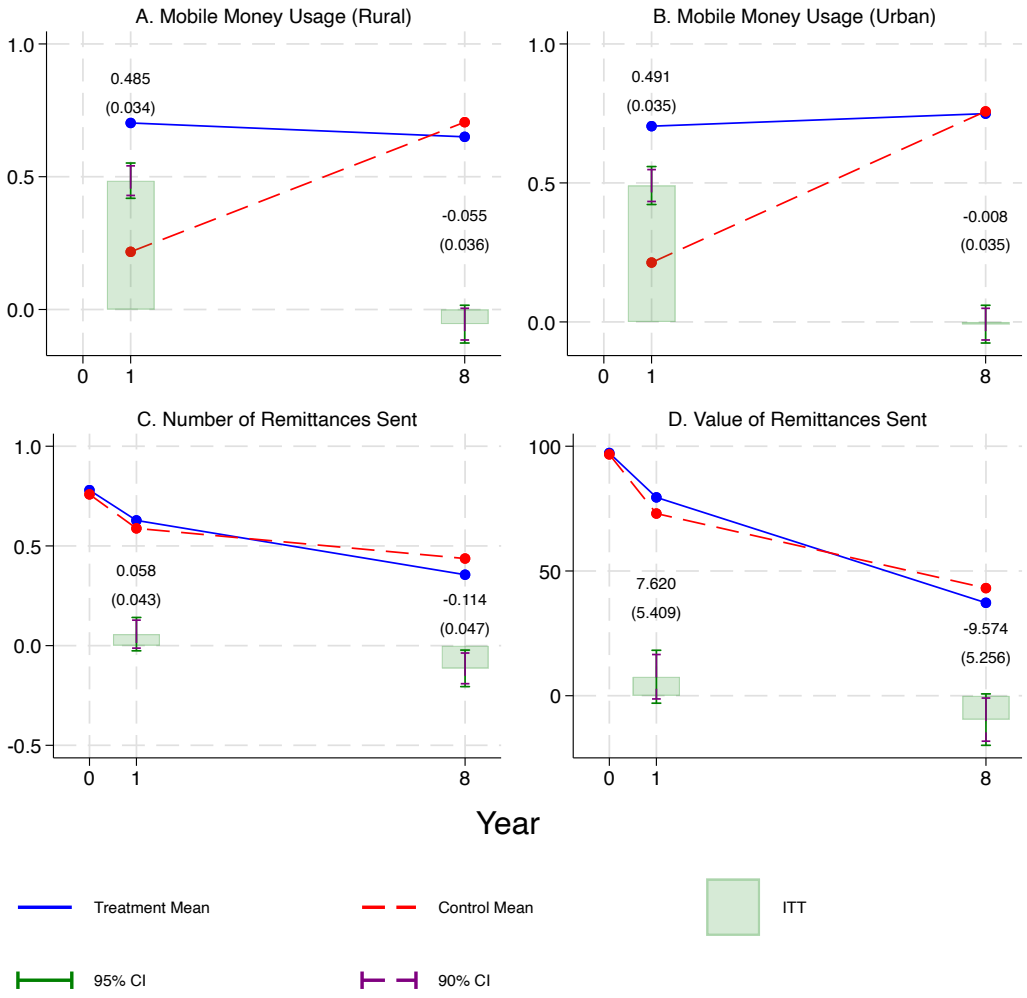
Panels A and B also show the catch-up by the control group by year 8 (2023). Mobile money usage is now very similar in the treatment and control groups, and the estimated treatment effect is small, negative, and not statistically significant. Similarly, we find no notable difference when just focusing on bKash usage. We find very high rates of engagement with mobile money in the rural control group: 82% have accounts and 71% actively use them.[13] For urban migrants in the control group, the corresponding figures are five percentage points higher for account ownership (87%) and use (76%). These control group averages from the 2023 follow-up survey are large relative to the use of mobile money services in the short-run follow-up survey in 2016. The rates of use over the past six months among the control group in 2016 were just 19% for the rural households and urban migrants.[14]

[12] Corresponding results are in Appendix Table A8.

[13] In 2024, we re-surveyed by phone two-thirds of the long-run sample: 229 members of the control group and 211 in the treatment group. In this subsample, the control households reported adopting mobile money on average five years after baseline—approximately four years after the intervention induced early adoption in the treatment subsample.

[14] The pre-analysis plan specifies that mobile money usage is the main variable of interest. We

Figure 1: First Stage and Remittances



The lines show unadjusted means for treatment and control groups at baseline (year 0), endline (year 1), and long-run follow-up (year 8). The bars show ITT estimates from ANCOVA specifications with PDSLASSO-selected controls; error bars report 95% and 90% confidence intervals. Mobile money usage (Panels A, B) is an indicator for any mobile money transaction in the past six months. Number of remittances (Panel C) is the average monthly count of remittances sent over the past six months. Value of remittances (Panel D) is the average monthly value in 2015 PPP USD. Year 0 is omitted from Panels A and B because the baseline sample was restricted to households without active mobile money accounts. Standard errors are heteroskedasticity-robust.

Panels B and C show corresponding results for remittances. The results are based on remittances sent by migrants to their rural families, using data on total remittances sent (from all sources) in the past six months. In the original, larger sample of Lee et al. (2021), an intention-to-treat estimate showing a 12% increase in the value of remittances was estimated at year 1. In the smaller sample here, Panels C and D show that the year 1 impacts on the number and value of remittances were positive but somewhat smaller (a roughly 8% increase in remittance value) and not statistically different from 0 or 12%.[15]

By year 8, migrants in the treatment group sent 0.11 fewer remittances in total relative to migrants in the control group (on a base of 0.44 remittances per month across the past six months). Correspondingly, the value of remittances sent by the treatment group in year 8 was -$9.57 (s.e.=5.26) lower than the control group.[16]

Panel D of Figure 1 shows that the decline in remittances for the treatment group is part of a larger overall decline in the value of remittances sent in the control group too. Appendix Table A7 shows that by year 1, the average monthly value of remittances sent by the treatment group was $80 in 2015 PPP-adjusted international dollars. In the 8-year follow-up, the average remittance value had dropped to $36. For the control group, the value of remittances fell from $73 to $42.

The summary statistics in Appendix Table A6 shows a possible explanation: by the time of the long-run follow-up, 23% of the migrants in the treatment group had merged again with their originating family, mainly by returning to Gaibandha (20%). This is true also for 26% of the control group (21% back in Gaibandha). For roughly

also find similar, not statistically significant, levels of account ownership.

[15]To check for the possible effects of sample selection, in Table A5 we show Lee bounds for Year 1 outcomes on the 2023 sample. The table shows that the effect on the value of remittances in year 1—when adjusted for the difference between monthly and 7-month specifications— falls within the bounds. Since the remittance estimate of 10.15 USD is monthly, multiplying by 7 yields 71.05 USD, which falls within the 7-month bounds of [35.82, 82.64].

[16]In Table A3 we show Lee bounds for Year 8 outcomes. The table shows that the treatment effect on the number of remittances (-0.114, significant at 5%) falls outside the Lee bounds which imply a larger negative value for the impact on the number of remittances. Similarly, the estimate of the value of remittances (-9.574, significant at 10%), when scaled up for the 7 months, falls outside the Lee bounds [-38.4, -6.8].

a quarter of the sample then, remittances would no longer have been necessary.

Panel A of Appendix Table A9 shows that these merges help explain the overall drop in remittances. The regression shows a modest and not significant overall drop of -$7.95 (s.e.=6.05) between year 1 and 8, but a large and significant drop of -$35.60 (s.e.=8.47, $p < .001$) for households that had merged.

Panel B of Appendix Table A9 investigates whether the decline in the value of remittances can be explained by additional migrants leaving for cities—which could explain why the migrants in our sample, who would then be under less pressure to send money home, were sending less in year 8. The additional migrants do not explain the broad drop in remittances in our sample, nor does it have a differential impact on the treatment group.

4.2 Broad Outcomes

4.2.1 Treatment effects on aggregate indices

By the time of the long-run follow up, the economic status of households in the sample had improved meaningfully. Daily per capita consumption in the rural sample increased from $PPP 2.26 at baseline to $PPP 3.16 in the long-term follow up.

Among migrants, monthly expenditure more than doubled from $PPP 195.37 at baseline to $PPP 446.11 in the long-run follow-up control group, while daily per capita expenditure increased from $PPP 4.52 to $PPP 6.11.

Section 4.1 showed that by year 8 the control group had caught up to the treatment group in mobile money usage. Figure 2 aggregates impacts on a broad range of socio-economic outcomes, showing that treatment and control groups persist in being similar on these outcomes. The year 1 results already show that, with the exception of urban health, there was no notable impact on these aggregated outcomes. That continues at year 8.

The figure focuses on 11 indices made up of standardized components. For the rural sample, these include: rural consumption and poverty, rural assets, rural employment

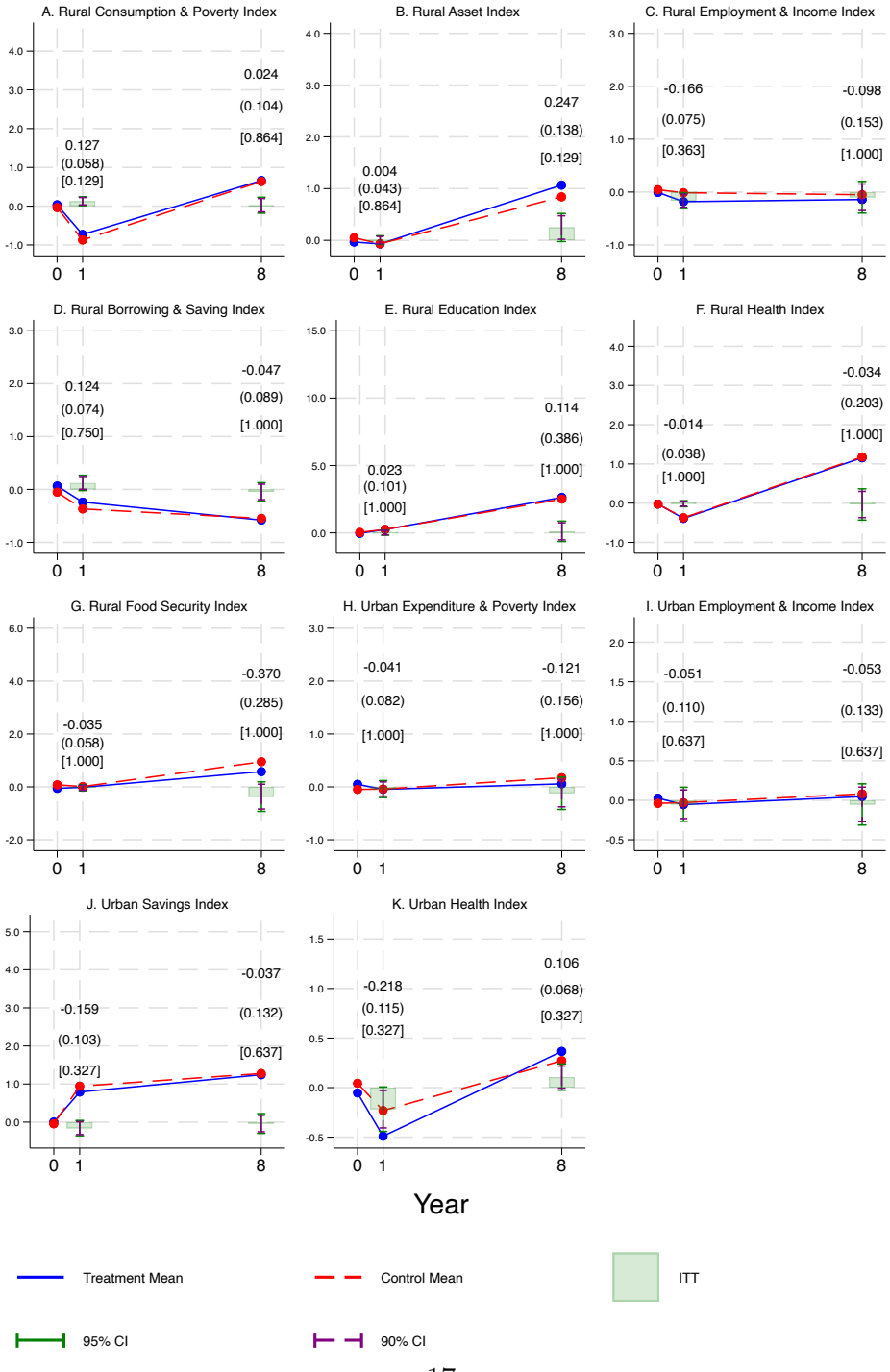
and income, rural borrowing and saving, rural education, rural health, and rural food security. For the urban sample, these include: urban expenditure and poverty, urban employment and income, urban savings, and urban health.

Panel A of the figure shows that rural households faced challenges in year 1 versus year 0 which were mitigated by the treatment. Conditions improved considerably by year 8. Apart from rural assets, which are considered more fully in Section 4.3, there are no meaningful, significant differences in the indices of treatment and control groups at year 8.

Lee et al. (2021) found that the intervention had negative short-run impacts on the health of migrants. This was seen as a potentially important negative consequence of spreading access to mobile technology. The result was consistent with mental and physical stress caused by working harder to send more mobile remittances. By year 8, those negative impacts are no longer detectable across a series of measures from the widely-used Short-Form 8 (SF-8) Health Survey. All variables are self-reported and ordered on a scale of 1–5 with a reference frame of the past four weeks. We transform the ordered responses into indicator variables and run linear probability models where all variables are defined such that positive treatment effects indicate better outcomes. Panel K shows the results when aggregating the eight health variables into a health index following (Kling et al., 2007). Outcome variables are transformed into z-scores and then aggregated to form an index (the standardized average across each outcome). constructed with equal weight on each of the eight health variables. Between year 1 and year 8, the treatment group improved its conditions such that there is no longer a meaningful or significant difference between the treatment and control groups.

The lack of a difference on the urban health index is consistent with Panel I which shows no difference in earnings and employment. It is also consistent with panel J which shows no difference in saving and debt for migrants.

Figure 2: Treatment Effects on Standardized Indices



Notes: The lines are equal to (unadjusted) means for treatment and control households; the bars are ANCOVA estimates with PDSLASSO. The error bars report 95% and 90% confidence intervals. Standard errors are heteroskedasticity-robust. Coefficients, standard errors (in parentheses), and q-values [in brackets] are shown.

4.2.2 Rural consumption and poverty

For rural households, pre-specified primary outcomes included household expenditure and poverty (measured using the poverty headcount and the squared poverty gap of Foster et al. 1984).

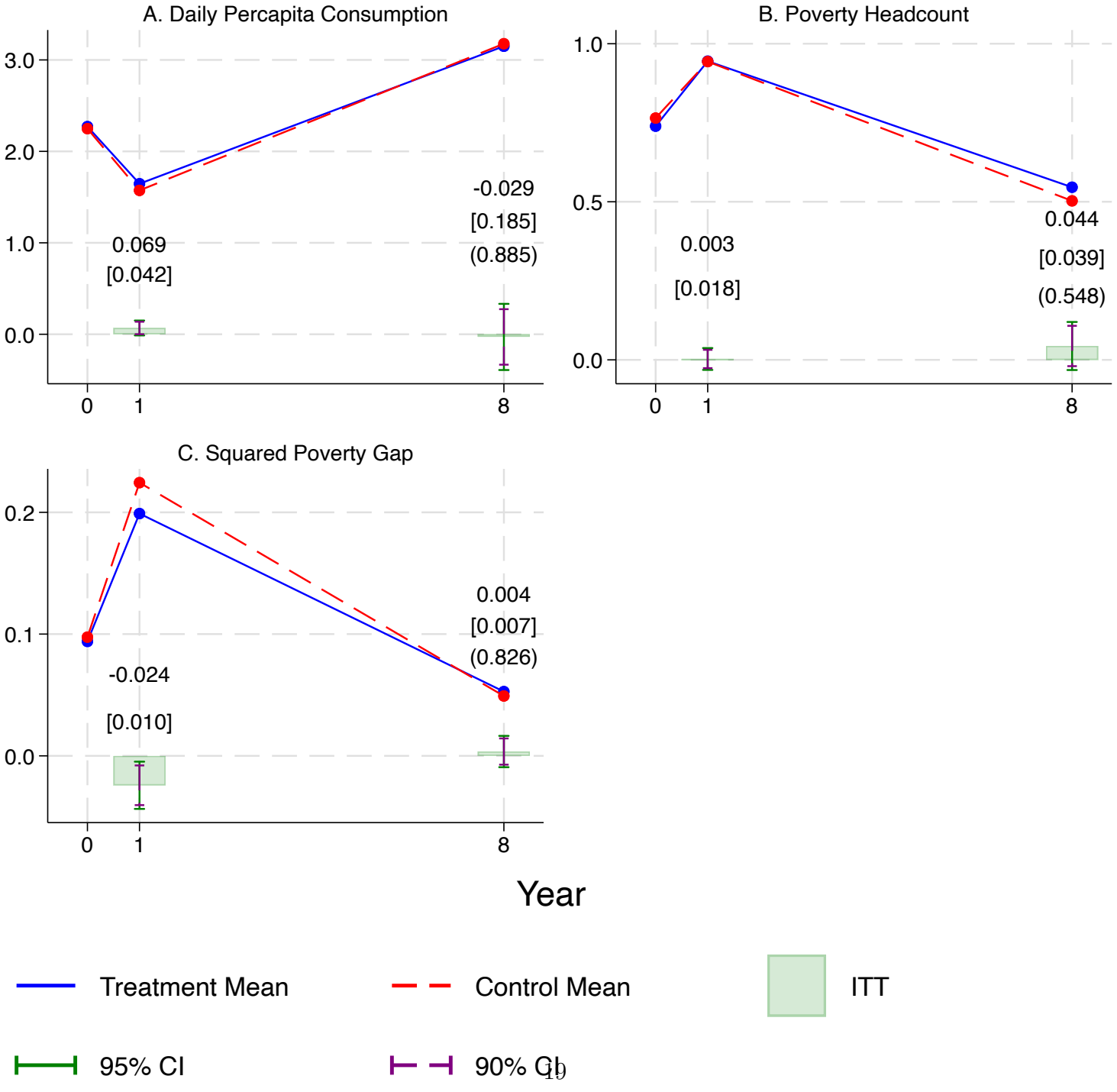
Consistent with general improvements in conditions in Gaibandha, and in Bangladesh as a whole, daily per capita consumption increased strongly between years 0 and 1 and year 8. Panel A of Figure 3 shows no differential impact: the treatment-control impact is effectively zero. The treatment effect on total household consumption per month is very small and not statistically significant.

In line with this null result, panels B and C find little impact on poverty by the time of the long-run follow-up. Panel B presents the impact on the poverty headcount using the World Bank global poverty line for extreme poverty.[17] The treatment effect is small and not statistically significant: 4.4 percentage points (s.e. = 3.9 percentage points) relative to a baseline control group mean of 76%.

While Lee et al. (2021) find a significant drop in extreme poverty for the treatment group in year 1, when measured with a distribution-sensitive index, we fail to detect a difference at year 8. To investigate impacts on the distribution of poverty, we use the squared-gap index of Foster et al. (1984) in panel C. The index places greatest weight on the deprivations of the poorest households, and we estimate a small and not statistically significant treatment effect.

[17] We use the international poverty line of $2.15 per person per day from 2016 updated to 2023. The line was 60 taka per person per day in 2016 (in 2017 international dollars). Updated to 2023 using the Bangladesh price deflator, the line is 93.32 taka per person per day.

Figure 3: Rural Household Sample: Consumption and Poverty



Notes: The lines are equal to (unadjusted) means for treatment and control households; the bars are ANCOVA estimates with PDSLASSO. The error bars report 95% and 90% confidence intervals. Standard errors are heteroskedasticity-robust. Coefficients, standard errors (in parentheses), and q-values [in brackets] are shown.

4.2.3 Urban consumption and poverty

For urban migrants, we pre-specified expenditure and poverty as the primary outcomes in the pre-analysis plan. In the short-run, migrants were working more and increasing their incomes, leading to a drop in measured poverty (Lee et al., 2021). Figure 4 shows that these impacts are no longer detectable in treatment-control comparisons. Expenditures have increased on average (Panel A), but Panels B and C show that poverty has risen, indicating that the rise in average expenditures masks heterogeneity.

While poverty increased overall between years 1 and 8, we do not detect statistically significant differential treatment-control impacts on the poverty headcount (Panel B) or squared poverty gap measures (Panel C).

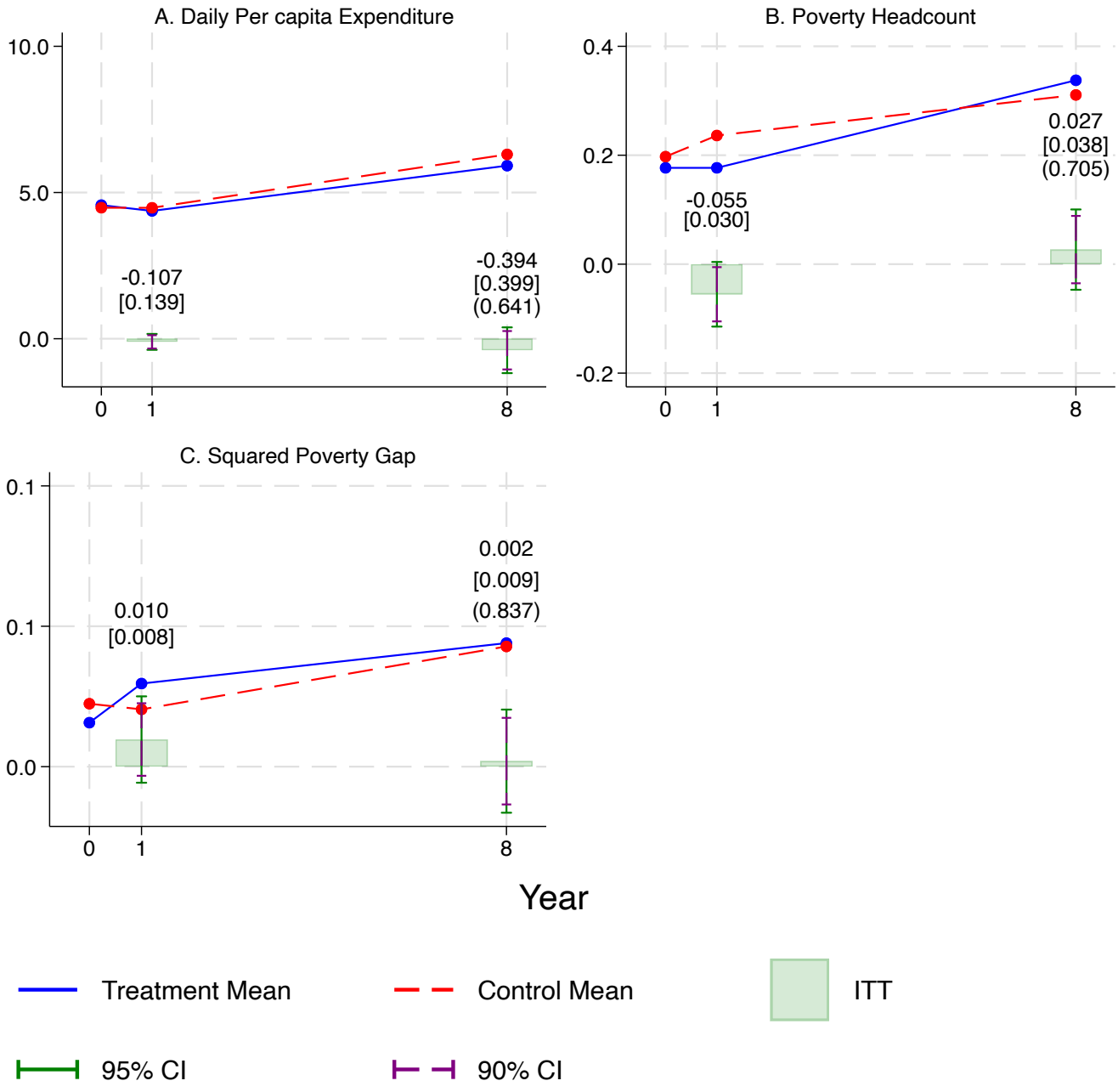
4.3 Rural Assets

The increase in assets is the clearest long-run impact of the intervention. Panel B of Figure 2 shows an increase in the index of rural assets, with a 0.25 standard deviation impact (q-value = 0.129). This is a key long-run primary outcome in the pre-analysis plan, and we disaggregate the result by non-productive and productive assets in Table 1. The results show that the increase in assets is driven by productive investments, which are driven by an increase in the value of agricultural landholdings. Suri and Jack (2016), in contrast, do not find that access to mobile money leads to long-run impacts on physical assets in Kenya, although they find increases in financial saving.

In Gaibandha, both treatment and control groups increased their asset holdings over the 8-year period. The control group mean value of total assets was 813,604 taka by 2023, compared to just 238,363 taka at baseline in 2014/5. The average value of agricultural land held by the control group had increased from 124,935 taka at baseline to 351,167 at the time of the long-run follow-up. The analyses show, however, that the treatment group had even larger gains.

Column 1 of Table 1 shows impacts on the total value of non-productive assets. These include VCRs, bicycles, cabinets, cassettes, clocks, electric fans, jewelry, motorcycles,

Figure 4: Urban Migrant Sample: Expenditure and Poverty



Notes: The lines are equal to (unadjusted) means for treatment and control households; the bars are ANCOVA estimates with PDSLASSO. The error bars report 95% and 90% confidence intervals. Standard errors are heteroskedasticity-robust. Coefficients, standard errors (in parentheses), and q-values [in brackets] are shown.

Table 1: Rural Assets

Panel A: Asset Values

	Non-Productive Asset Value	Productive Asset Value
bKash Treatment	592	2,794**
	(865)	(1,299)
Control Mean	9,639	8,444
Treatment Mean	10,719	9,767
Baseline Control Mean	3,859	5,354
Baseline Treatment Mean	4,039	2,981
Romano-Wolf p-value	0.499	0.095*

Panel B: Land Variables

	Land Value	Land Decimals	Agricultural Land Value	Agricultural Land Decimals
bKash Treatment	3,142*	3.4	2,689**	2.9
	(1,738)	(2.6)	(1,226)	(2.3)
Control Mean	16,972	21.9	7,700	13.7
Treatment Mean	19,120	24.7	9,019	15.9
Baseline Control Mean	8,965	16.8	5,291	11.3
Baseline Treatment Mean	6,775	14.2	2,865	8.6
Romano-Wolf p-value	0.173	0.350	0.088*	0.357

Notes: Standard errors in parentheses. Data are in 2015 PPP USD. Specifications include controls selected by PDSLASSO, estimated with heteroskedasticity-robust errors. Data are Winsorized at 1% and 99%. N = 662 for most columns; N = 660 for Productive Asset Value.

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

phones, radios, residential land, sewing machines, stoves, tube wells, vans, watches, refrigerators, and tablets. The net impact is $PPP 592 relative to a baseline mean of $PPP 3,948, an increase of 15% with a relatively large standard error (s.e.=865).

The impact on productive assets shows the importance of investment. Column 2 of Table 1 shows impacts on the total value of productive assets. These include tractors, threshers, and other agricultural machinery; farmed land; fallow land; vehicles used in production; and tools. The net impact is $PPP 2,794 relative to a baseline mean of $PPP 4,187 (s.e. = 1,299; $p < 0.05$; Romano-Wolf corrected $p = 0.089$), an increase of 66% over the baseline. Relative to the control group year 8 mean of $PPP 8,444, this is a 33% increase.

Table 3 of Lee et al. (2021) shows that the intervention cost $11.36. Converted to $PPP in 2015 yields a cost of $PPP 32.75. Taking the estimated treatment effect for productive assets from Table 1 ($PPP 2,794 in 2015 values) and dividing by $PPP 32.75 yields a simple benefit-cost ratio of 85.3. Since the marginal cost of the intervention was so inexpensive because it was added in the context of existing migration relationships, the benefit-cost ratio is consequently very large.

Column 3 combines all land (residential, farmed, and fallow), yielding an impact of $PPP 3,142 (s.e. = 1,738; $p < 0.10$; Romano-Wolf corrected $p = 0.155$), consistent with land accumulation explaining the asset increases. Precision is reduced due to including residential land in the aggregate. Relative to a baseline mean of $PPP 7,887, this is an increase of 40%. Relative to the control group year 8 mean of $PPP 16,972, this is a 19% increase.

The increase in land value is reflected in the increase in land size, measured in decimals (1/100 acre) in column 4. The treatment effect is not significant but is large, showing a 20% increase over baseline (treatment effect = 3 decimals, s.e.=3 decimals, baseline mean = 15 decimals).

The specifications in the table follow the pre-specified analysis in equation 1. At baseline, assets are not balanced across treatment and control groups, and ANCOVA helps to correct baseline imbalance by including the baseline outcome on the right

hand side. Panel B of Table A6 shows that while there is balance across all other variables, the control group started with significantly more land. The total rural assets of the treatment group were $PPP 7,277 (s.d. = 13,572) versus $PPP 10,472 (s.d. = 24,292) for the control group, a significant difference with $p = 0.038$. For productive assets, the treatment group held $PPP 3,028 (s.d. = 9,750) versus $PPP 6,383 (s.d. = 21,893) for the control group, significant with $p = 0.012$.

To explore robustness, we consider other indicators of increased assets. In May 2024, the research team resurveyed 82 percent of the rural sample, focusing on short questions on investment. Relevant results are reported in Table A12. Column 1 shows increases in housing quality, with treated households being 7.8 percent more likely ($p < 0.1$) to live in a brick house at year 8, relative to control households. Improvements in toilet quality and livestock ownership are positive (livestock increase by 9 percent relative to the control mean, consistent with increased land) but not statistically significant.

In Table A13, we consider wage labor to see whether changes in wage work are consistent with increases in land ownership. Columns 1 and 2 of Table A13 indicate decreases in wage labor (working for others), with a 12 percent drop in wage income ($p < 0.05$) and a 10 percent drop in the number of wage laborers, consistent with shifts of labor toward households' own land.

4.4 Heterogeneity

4.4.1 Heterogeneity by Gender

In Kenya, Suri and Jack (2016) found that the long-run impacts of mobile money on poverty were concentrated among female-headed households. In Bangladesh, Lee et al. (2022) turned to senders of remittances, and found that female migrants were as likely as males to adopt mobile money but remitted much less. Table A15 examines whether treatment effects differ by the gender of the household head receiving digital remittances (rural sample) or migrant sending them (urban sample).

In contrast to the Kenya results, we find no significant heterogeneity by gender across any outcome for recipients of digital money. For rural households, the treatment effect on assets for female-headed households (0.421, s.e.=0.216) is larger than for male-headed households (0.225, s.e.=0.154), but we cannot reject the null hypothesis that the impacts are the same (p=0.462 for the test of differential effects). Similarly, we find no differential effects on consumption, employment, or financial outcomes by the gender of households in the rural sample.

For urban migrants, as in Lee et al. (2022) we find similar impacts on long-run usage of mobile money for males and females. In contrast, to the year 1 results of Lee et al. (2022), we do not detect a difference by migrant gender in remittance-sending behavior in year 8. Male and female migrants show comparable treatment effects on their expenditure, employment, savings, and health outcomes.

The absence of gender heterogeneity in our context may reflect differences in the samples and settings in Kenya and Bangladesh. Our sample consists of migrant-sending households where remittances are explicitly part of economic relationships for all households, whether male-headed or female-headed, whereas the Kenya setting involved broader mobile money adoption without an explicit focus on migration-remittance relationships.

4.4.2 Heterogeneity by Recruitment Source

In the original sample, the households are drawn from two sources. One is a factory-readiness training program in Gaibandha for prospective migrants (called SHIREE), which accounted for 43% of the treatment group. The other 57% came from a snowball sample of migrants who recruited based on similarity in poverty status and having migrants in Dhaka. In the long-run follow-up sample in 2023, the division is similar: 41% from SHIREE and 59% from the snowball sample. Table A16 examines whether treatment effects differ between the two groups.

We find significant heterogeneity in remittance outcomes. The reduction in remittances at year 8 is concentrated among SHIREE households. While non-SHIREE households

show a marginally significant reduction in remittance value (-$5.75, s.e.=7.17), SHIREE households experience an additional -$9.32 reduction (s.e.=10.35), where we reject the null of equality of coefficients on remittance value (p=0.044). This pattern is consistent with non-SHIREE migrants having stronger connections in Dhaka, having migrated on their own without participation in the SHIREE factory-readiness program; we then see them continuing to send remittances at high levels.

Consistent with the higher values of remittances, the impact on productive assets for non-SHIREE households (0.342, s.e=0.206) is larger than for SHIREE households, although we cannot reject the equality of treatment effects (p=0.505). This suggests that the positive impact on rural assets does not depend on the migrants having first participated in the SHIREE program.

5 Conclusion

This study shows that access to mobile money built long-run wealth for the poorest rural residents in one of the poorest parts of Bangladesh. Eight years after rural residents started using mobile money as a way to receive remittances from relatives who had migrated to Dhaka, the value of their productive assets was a third higher for the treatment group relative to the control group and two-thirds higher than their holdings at baseline. Mobile money made it easier for migrants, who had left their families to work in Dhaka, to send money home, which was then spent and invested by their rural-based families.

An experimentally-induced 49 percentage point increase in mobile money usage in 2015 allowed estimation of the short-term impacts of mobile money, making it possible to see how using mobile money increased rural consumption and decreased extreme rural poverty in the first year of adoption. By the long-term follow-up survey eight years later, the usage gap had closed, and treatment-control impacts were no longer detectable on the short-term measures, including consumption and poverty.

The fading short-term treatment effects reflects the fact that the control group was able to catch up on their own over the next eight years.[18] Even without the training and logistical assistance offered to the treatment group, most of the control group had successfully overcome the main barriers to signing up and using mobile money. Those barriers included lack of experience with the technology; low levels of literacy, especially in English; and difficulties obtaining the documents required for signing up. Their efforts to adopt mobile money on their own reinforce the initial proposition that the technology could be valuable even for very poor households.

Alternative programs have shown impacts in similar populations in the same region of Bangladesh. Bryan et al. (2014), for example, show the positive impacts of subsidizing temporary migration during lean seasons. Balboni et al. (2022) show the power of big-push interventions that aim to transform the livelihoods of "ultra-poor" residents—interventions which are often described as "graduation" programs. Graduation programs have shown persistent impacts over ten years in West Bengal, India (Banerjee et al., 2021) and eleven years in Northwest Bangladesh (Balboni et al., 2022), reinforcing the idea that the interventions are helping to overcome poverty traps.[19]

In a parallel to our study, Banerjee et al. (2021) find that the long-term gains to the graduation program in West Bengal largely came from improved labor market opportunities, especially via migrating to work in the capital, Kolkata. While the intervention studied here is not a graduation program and lacks the advantages of the big-push approach, it is considerably less expensive by directly facilitating remittance-sending by migrants. Our results suggest that this approach is another way to bring relatively large sums of money to poor communities, especially where budgets for intensive interventions are limited.[20] The approach does this by amplifying economic strategies that are already being pursued by the households themselves.

[18] We cannot rule out that the impacts of mobile money persist for the treatment group even though are no longer detectable due to the control group's catch-up.

[19] Evidence of catch-up by the control group in a graduation program in Ethiopia suggests that the poverty trap mechanism is not univeral (Barker et al., 2024).

[20] Replications of the original intervention are ongoing in six new sites in India, Bangladesh and Pakistan, following a methodology for site selection developed in Gechter et al. (2024).

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Appendix

Table A1: Differential Attrition: Rural Household Sample

Panel A: Simple Differential Attrition Test	
bKash Treatment	0.046*
	(0.027)
Observations	814
R-squared	0.003

Panel B: Main Effects		**Panel C: Interaction Effects**	
bKash Treatment	0.011		
	(0.361)		
Has mobile	-0.259	bKash × Has mobile	-0.243
	(0.182)		(0.291)
Household size	-0.016	bKash × Household size	0.029
	(0.027)		(0.036)
Total HH income	0.000	bKash × HH income	-0.000
	(0.000)		(0.000)
No. of children	0.006	bKash × No. children	-0.004
	(0.028)		(0.040)
Agri. land value	0.000	bKash × Agri. land	0.001
	(0.001)		(0.001)
Total asset value	-0.000**	bKash × Asset value	0.000
	(0.000)		(0.000)
No. of rooms	-0.007	bKash × No. rooms	-0.031
	(0.028)		(0.042)
Own dwelling	-0.143	bKash × Own dwelling	0.179
	(0.096)		(0.125)
Daily consumption	-0.001	bKash × Consumption	0.003**
	(0.001)		(0.001)
Upper poverty line	0.016	bKash × Upper poverty	0.073
	(0.066)		(0.093)
Intl. poverty line	0.024	bKash × Intl. poverty	-0.017
	(0.052)		(0.076)
Other upazila	0.060	bKash × Other upazila	-0.036
	(0.039)		(0.056)
HH head age	-0.001	bKash × HH head age	-0.003
	(0.001)		(0.002)
HH head female	-0.061	bKash × HH head female	0.205**
	(0.055)		(0.095)
HH head primary	-0.036	bKash × HH head primary	0.005
	(0.050)		(0.073)
Observations: 814			
R-squared: 0.062			

Notes: Standard errors in parentheses. Dependent variable is 1 if a household left the sample between baseline and long-run follow-up. Panel A shows simple treatment effect on attrition. Panels B and C include baseline characteristics and their interactions with treatment.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Differential Attrition: Urban Migrant Sample

Panel A: Simple Differential Attrition Test	
bKash Treatment	0.026
	(0.030)
Observations	815
R-squared	0.001

Panel B: Main Effects		**Panel C: Interaction Effects**	
bKash Treatment	0.214		
	(0.215)		
Has bank account	-0.029	bKash × Has bank account	0.119
	(0.069)		(0.100)
Formal employment	0.016	bKash × Formal employment	-0.045
	(0.063)		(0.104)
Annual earnings	0.000	bKash × Annual earnings	-0.000
	(0.000)		(0.000)
Tenure in Dhaka	-0.011	bKash × Tenure in Dhaka	0.018
	(0.013)		(0.019)
Value of remittances sent	0.000	bKash × Remittances sent	-0.000
	(0.000)		(0.000)
Daily per capita expenditure	0.000	bKash × Daily expenditure	-0.001
	(0.001)		(0.001)
Female migrant	0.107*	bKash × Female migrant	-0.058
	(0.056)		(0.079)
Migrant age	-0.004	bKash × Migrant age	0.002
	(0.005)		(0.007)
Migrant completed primary	-0.030	bKash × Completed primary	0.036
	(0.043)		(0.061)
Observations: 815			
R-squared: 0.029			

Notes: Standard errors in parentheses. Dependent variable is 1 if migrant household left the sample between baseline and long-run follow-up. Panel A shows simple treatment effect on attrition. Panels B and C include baseline characteristics and their interactions with treatment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Lee Bounds Analysis: Robustness to Differential Attrition

Outcome	Original Estimate	Lower Bound Coef.	Lower Bound p-value	Upper Bound Coef.	Upper Bound p-value	Trim Prop.
Panel A: Rural Household Outcomes						
Productive Assets (Taka)	2,794**	1,068	0.561	5,844***	0.005	5.500%
Agricultural Land (Taka)	2,689**	1,099	0.536	5,600***	0.007	5.600%
Wage Income (Taka)	-400**	-478**	0.021	48	0.845	5.600%
No. Wage Laborers	-0.113*	-0.146**	0.033	-0.012	0.877	5.600%
Brick House (=1)	0.078*	0.029	0.572	0.109**	0.017	7.500%
Panel B: Remittances (monthly averages)						
No. of Remittances	-0.114**	-0.086*	0.051	-0.036	0.658	2.030%
Value of Remittances (USD)	-9.574*	-6.40	0.200	-1.28	0.871	2.030%

Notes: This table reports Lee (2009) bounds that address differential attrition between treatment and control groups. The trimming proportion shows the percentage of treatment observations removed to equalize retention rates. Lower bounds assume households who attrited had the best outcomes; upper bounds assume they had the worst outcomes. Original estimates that fall within the bounds are robust to selective attrition. Panel B reports bounds for monthly average remittances. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Lee Bounds Analysis: Standardized Indices

Index	Original Estimate	Lower Bound Coef.	Lower Bound p-value	Upper Bound Coef.	Upper Bound p-value	Trim Prop.
Panel A: Rural Household Indices						
Consumption & Poverty	0.024	-0.086	0.485	0.184	0.122	5.8%
Assets	0.247	0.135	0.470	0.601	0.005	5.8%
Employment & Income	-0.098	-0.159	0.346	0.183	0.165	5.8%
Borrowing & Saving	-0.047	-0.156	0.174	0.142	0.260	5.8%
Education	0.114	0.127	0.790	0.700	0.142	5.8%
Health	-0.034	-0.123	0.576	0.483	0.054	5.8%
Food Security	-0.370	-0.827	0.041	-0.268	0.374	5.8%
Panel B: Urban Migrant Indices						
Expenditure & Poverty	-0.121	-0.247	0.263	0.007	0.976	2.4%
Employment & Income	-0.053	-0.126	0.526	0.036	0.823	2.4%
Savings	-0.037	-0.083	0.587	0.012	0.942	2.4%
Migrant Health	0.106	0.057	0.652	0.134	0.065	2.4%

Notes: This table reports Lee (2009) bounds for standardized index outcomes at Year 8. Indices are constructed as equally-weighted averages of z-scored component variables within each domain. The trimming proportion shows the percentage of treatment observations removed to equalize retention rates between the baseline sample and LRFU-matched sample. Lower bounds assume households who attrited had the best outcomes; upper bounds assume they had the worst outcomes. Original estimates that fall within the bounds are robust to selective attrition. Original estimates use ANCOVA with PDSLASSO-selected controls. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Lee Bounds Analysis: Year 1 Effects in LRFU-Matched Sample

Outcome	Original Estimate	Lower Bound Coef.	Lower Bound p-value	Upper Bound Coef.	Upper Bound p-value	Trim Prop.
Panel A: Rural Household Outcomes						
Productive Assets (Taka)	3,866	3,051	0.366	4,670	0.171	5.500%
Agricultural Land (Taka)	3,777	3,036	0.362	4,675	0.165	5.600%
Wage Income (Taka)	-523**	-664**	0.012	-31	0.920	5.600%
No. Wage Laborers	-0.154**	-0.178**	0.029	0.009	0.899	5.600%
Panel B: Remittances (monthly averages)						
No. of Remittances	0.054	0.028	0.567	0.089*	0.052	2.030%
Value of Remittances (USD)	10.15**	4.97	0.436	11.10	0.184	2.030%

Notes: This table reports Lee (2009) bounds for Year 1 (endline) treatment effects in the sample of households tracked to the Year 8 long-run follow-up. The column shows estimates in the matched sample: ANCOVA with PDSLASSO-selected controls for Panel A; household fixed effects panel regression for Panel B. The trimming proportion shows the percentage of treatment observations removed to equalize retention rates between the full baseline sample and the LRFU-matched sample. Lower bounds assume households not tracked to LRFU had the best Year 1 outcomes; upper bounds assume they had the worst. Current estimates that fall within the bounds are consistent with effects being driven by reduced power rather than sample selection.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Summary Statistics by Treatment Assignment

	Treatment			Control			Treat-control
	Mean	SD	Obs.	Mean	SD	Obs.	p-value
Panel A: Long-Run Follow-up (2023)							
Mobile money account ownership (rural)	0.78	0.42	326	0.82	0.39	336	0.209
Mobile money usage (rural)	0.65	0.48	326	0.71	0.46	336	0.130
Mobile money account ownership (urban)	0.90	0.30	311	0.87	0.34	309	0.200
Mobile money usage (urban)	0.75	0.43	311	0.76	0.43	309	0.816
Number of remittances sent	0.36	0.48	311	0.44	0.51	313	0.043**
Value of remittances sent	37	57	311	43	58	313	0.204
Total consumption (rural)	4,163	2,670	326	4,312	2,858	336	0.489
Daily per capita expenditure (rural)	11.40	7.32	326	11.81	7.83	336	0.489
Poverty headcount (rural)	0.02	0.15	326	0.01	0.08	336	0.085*
Squared poverty gap (rural)	0.0002	0.002	326	0.0001	0.002	336	0.653
Total asset value (rural)	21,178	33,539	326	18,536	29,463	336	0.282
Total productive asset value (rural)	10,249	24,416	326	8,674	20,902	336	0.373
Household size (rural)	3.99	1.93	326	4.02	1.81	336	0.869
Monthly expenditure (urban)	473	462	311	446	323	309	0.405
Daily per capita expenditure (urban)	5.92	4.79	311	6.30	5.26	309	0.341
Poverty headcount (urban)	0.34	0.47	311	0.31	0.46	309	0.47
Squared poverty gap (urban)	0.04	0.10	311	0.04	0.13	309	0.898
Merged households (all)	0.23	0.42	290	0.26	0.44	297	0.547
Merged households (in Gaibandha)	0.20	0.40	290	0.21	0.41	297	0.641
Merged households (in Dhaka)	0.02	0.14	290	0.01	0.10	297	0.297
Merged household (other)	0.02	0.13	290	0.03	0.18	297	0.208
p-value of F-test for joint orthogonality = 0.544							
Panel B: Baseline (2014-15)							
Number of remittances sent	0.78	0.51	311	0.76	0.47	313	0.588
Value of remittances sent	97	164	311	97	67	313	0.916
Total consumption (rural)	2,792	1,087	326	2,767	987	336	0.748
Daily per capita expenditure (rural)	7.65	2.98	326	7.58	2.70	336	0.808
Poverty headcount (rural)	0.74	0.44	326	0.76	0.42	336	0.446
Squared poverty gap (rural)	0.0001	0.002	326	0.00003	0.0006	336	0.509
Total asset value (rural)	7,277	13,572	326	10,472	24,292	336	0.038**
Total productive asset value (rural)	3,028	9,750	325	6,383	21,893	336	0.012**
Household size (rural)	3.82	1.63	326	3.85	1.62	336	0.817
Monthly expenditure (urban)	203	104	311	195	102	309	0.364
Daily per capita expenditure (urban)	4.57	1.67	311	4.48	1.51	309	0.485
Poverty headcount (urban)	0.18	0.38	311	0.20	0.40	309	0.512
Squared poverty gap (urban)	0.02	0.06	311	0.02	0.10	309	0.306

Notes: Mobile money account ownership and usage statistics are not available at baseline as the sample was restricted to households without mobile money accounts. Household remigration outcomes are not reported at baseline as no sampled households had merged at the time. All values are in 2015 PPP USD (27.05 BDT = 1 international dollar)

Table A7: Comparison of Year 1 and Year 8 Outcomes by Treatment Status

	Treatment			Control		
	Year 8 (2023)	Year 1	p-value	Year 8 (2023)	Year 1	p-value
Mobile money usage (rural)	0.65	0.99	0.000***	0.71	0.99	0.000***
Mobile money usage (urban)	0.75	0.70	0.178	0.76	0.21	0.000***
Number of remittances sent	0.35	0.63	0.000***	0.41	0.59	0.000***
Value of remittances sent	36	80	0.000***	42	73	0.000***
Total consumption (rural)	4,163	2,217	0.000***	4,312	2,214	0.000***
Daily per capita expenditure (rural)	11.40	6.07	0.000***	11.81	6.07	0.000***
Poverty headcount (rural)	0.02	0.94	0.000***	0.01	0.94	0.000***
Squared poverty gap (rural)	0.0002	0.001	0.170	0.0001	0.001	0.373
Total asset value (rural)	21,178	9,385	0.002***	18,536	6,257	0.000***
Total productive asset value (rural)	10,249	4,970	0.130	8,674	1,814	0.000***
Monthly expenditure (urban)	473	209	0.000***	446	203	0.000***
Daily per capita expenditure (urban)	5.92	4.37	0.000***	6.30	4.48	0.000***
Poverty headcount (urban)	0.34	0.18	0.000***	0.31	0.24	0.036**
Squared poverty gap (urban)	0.04	0.03	0.094*	0.04	0.02	0.004***

Notes: This table compares outcomes between Year 8 (2023) and Year 1 surveys for treatment and control groups separately. P-values are from t-tests comparing the means between the two survey rounds within each treatment arm. Stars indicate statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: First Stage and Remittances

	Panel A: Mobile Money Usage		Panel B: Remittances	
	(1) Rural	(2) Urban	(3) Number	(4) Value
bKash treatment	-0.055	-0.008		
	(0.036)	(0.035)		
bKash treatment × Long-run			-0.114**	-9.602*
			(0.047)	(5.258)
Control Mean	0.705	0.757	0.437	43.13
Treatment Mean	0.650	0.749	0.356	37.25
Baseline Mean	-	-	0.769	97.01

Standard errors in parentheses

Long-run = long run follow up (year 8).

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

Table A9: Determinants of Remittances at Year 8 (Long run follow up)

	Panel A: Remittances & Remigration	
	(1)	(2)
	Number	Value
Treatment × Long-run	-0.105*	-7.945
	(0.0569)	(6.047)
Long-run × Merged HH	-0.324***	-35.60***
	(0.0701)	(8.470)
Treatment × Long-run × Merged	0.0150	-0.0845
	(0.0965)	(11.74)
R-squared	0.349	0.276
Observations	11,673	11,673
	Panel B: Multiple Migrants	
	(3)	(4)
	Number	Value
Treatment × Long-run	-0.127**	-7.063
	(0.0626)	(7.074)
Long-run × Multiple migrants	-0.0695	0.868
	(0.0674)	(7.278)
Treatment × Long-run × multiple	0.0943	-0.409
	(0.0953)	(10.43)
R-squared	0.339	0.279
Observations	12,749	12,749

Notes: Estimates from remittance-month panel data covering all remittances sent by migrants to their households in the previous 7 months. All regressions include household and month fixed effects. Panel A includes controls for Treatment, Long-run, and Merged HH as main effects; Panel B includes controls for Treatment, Long-run, and Multiple migrants as main effects. Only relevant interaction terms are reported for brevity. Long-run = long run follow up (year 8). Standard errors clustered at household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: Treatment Effects on Standardized Indices

	Panel A: Rural Outcomes					
	Consumption & Poverty	Asset	Employment & Income	Borrowing & Saving	Education	Health
bKash Treatment	-0.033	0.247*	-0.098	-0.125	0.114	-0.034
	(0.110)	(0.138)	(0.153)	(0.252)	(0.386)	(0.203)
Control Mean	-0.781	0.840	-0.050	1.556	2.505	1.179
Treatment Mean	-0.821	1.067	-0.141	1.427	2.631	1.157
Baseline Mean	-0.007	0.009	0.019	0.003	0.003	-0.024
Anderson q-value	1.000	0.495	1.000	1.000	1.000	1.000

	Panel B: Urban Outcomes			
	Expenditure & Poverty	Employment & Income	Savings	Urban Health
bKash Treatment	0.048	-0.053	-1.316	0.106
	(0.201)	(0.133)	(1.569)	(0.068)
Control Mean	0.868	0.081	8.531	0.272
Treatment Mean	0.927	0.047	7.161	0.366
Baseline Mean	-0.006	-0.006	-0.010	-0.005
Anderson q-value	1.000	1.000	1.000	0.594

Standard errors in parentheses

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

Table A11: Rural Assets (Without Baseline Controls)

	Non-Productive Asset Value	Productive Asset Value	Land Value	Land Decimals
bKash Treatment	736	774	634	1
	(920)	(1,513)	(2,004)	(3)
Control Mean	9,639	8,444	16,972	22
Treatment Mean	10,719	9,767	19,120	25
Baseline Mean	3,948	4,187	7,887	15

Notes: Standard errors in parentheses. Data are in 2015 PPP USD. Specifications include controls selected by PDSLASSO, estimated with heteroskedasticity-robust errors. Data are Winsorized at 1% and 99% of the distribution. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: Housing Condition & Livestock

Panel A: Housing Conditions			
	Brick House	Pukka Toilet	No. of Rooms
bKash Treatment	0.0783*	0.0112	0.00766
	(0.0406)	(0.0419)	(0.0852)
Control Mean	0.62	0.59	2.22
Treatment Mean	0.70	0.60	2.22
Observations	542	542	542
Panel B: Livestock			
	Livestock Ownership	No. of Livestock	Value of Livestock
bKash Treatment	0.0614	0.561	-2435.8
	(0.0397)	(0.500)	(7244.7)
Control Mean	0.66	1.44	23672.13
Treatment Mean	0.72	2.00	22244.74
Observations	542	542	373

Standard errors in parentheses

Data from telephone resurvey conducted in May 2024 with 542 of 662 rural households (82 percent) from long-run follow-up sample.

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

Table A13: Treatment Effects on Rural Wage Employment

	(1) Total Wage Income	(2) No. of Wage Laborers
bKash Treatment	-400.2**	-0.113*
	(191.1)	(0.0609)
R^2		
Control Mean	1,676	0.70
Treatment Mean	1,296	0.60
Baseline Mean	3,366	1.10
Observations	662	662

Standard errors in parentheses

Wage income is total annual earnings from wage labor activities.

Number of wage laborers is count of household members engaged in wage work.

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

Table A14: Treatment Effects on Consumption and Poverty

Panel A: Rural			
	(1)	(2)	(3)
	Total Consumption	Poverty Headcount	Squared Poverty Gap
bKash Treatment	-4,265	0.03	0.01
	(5,173)	(0.04)	(0.01)
Control Mean	115,068	0.67	0.10
Treatment Mean	111,887	0.70	0.11
Baseline Mean	75,575	0.75	0.09
Romano-Wolf p-value	0.63	0.63	0.51

Panel B: Urban			
	(4)	(5)	(6)
	Monthly Expenditure	Poverty Headcount	Squared Poverty Gap
bKash Treatment	639	0.03	0.00
	(851)	(0.04)	(0.01)
Control Mean	12,067	0.31	0.04
Treatment Mean	12,789	0.34	0.04
Baseline Mean	5,386	0.19	0.02
Romano-Wolf p-value	0.82	0.82	0.84

Standard errors in parentheses

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

Table A15: Heterogeneous Treatment Effects by Gender

Panel A (Rural): Heterogeneity by Female Head of Household			
Outcome	Treatment (Male-headed HH)	Treatment × Female HH Head	p-value (Heterogeneity)
Mobile Money Usage	-0.0626	0.0644	0.556
	(0.0388)	(0.109)	
Consumption & Poverty	0.0743	-0.425	0.136
	(0.113)	(0.285)	
Assets Index	0.225	0.197	0.462
	(0.154)	(0.267)	
Employment & Income	-0.0822	-0.132	0.598
	(0.172)	(0.251)	
Borrowing & Savings	-0.0120	-0.342	0.188
	(0.0950)	(0.260)	
Education Index	0.209	-0.721	0.516
	(0.410)	(1.110)	
Health Index	0.0193	-0.280	0.518
	(0.225)	(0.433)	

Panel B (Urban): Heterogeneity by Female Migrant			
Outcome	Treatment (Male Migrant)	Treatment × Female Migrant	p-value (Heterogeneity)
Mobile Money Usage	0.00571	-0.126	0.241
	(0.0370)	(0.108)	
Expenditure & Poverty	-0.140	0.0676	0.898
	(0.164)	(0.526)	
Employment & Income	-0.0370	-0.180	0.707
	(0.137)	(0.479)	
Savings Index	-0.00487	-0.279	0.512
	(0.140)	(0.425)	
Health Index	0.0994	0.105	0.618
	(0.0719)	(0.209)	
Remittances (member–month panel with HH + month FE)			
Number of Remittances	-0.0977*	-0.118	0.369
	(0.0506)	(0.132)	
Value of Remittances	-9.623*	3.919	0.798
	(5.634)	(15.31)	

Notes: Column 1 reports the treatment effect for male-headed households (Panel A) and male migrants (Panel B) respectively. Column 2 reports the interaction with an indicator variable for female. Column 3 reports heterogeneity p-values from tests of the interaction coefficient equal to zero. Rural and urban outcomes use ANCOVA with PDSLASSO-selected controls; remittances use household and month fixed effects with standard errors clustered at the household level. Sample sizes vary by outcome: Panel A uses $N = 662$ for mobile money usage and $N = 586$ for indices; Panel B uses $N = 620$ for mobile money usage, $N = 586$ for indices, and $N = 12{,}348$ for remittances.
*
$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Heterogeneous Treatment Effects by SHIREE Status

Outcome	Treatment (Non-SHIREE)	Treatment × SHIREE	p-value (Equality)
Panel A: Mobile Money Usage			
Rural Household	-0.029	-0.071	0.075*
	(0.048)	(0.073)	
Urban Migrant	-0.012	0.008	0.945
	(0.043)	(0.071)	
Panel B: Remittances			
Number of Remittances	-0.094	-0.050	0.037**
	(0.063)	(0.093)	
Value of Remittances	-5.745	-9.318	0.044**
	(7.170)	(10.35)	
Panel C: Rural Household Outcomes			
Consumption & Poverty	0.131	-0.274	0.369
	(0.139)	(0.209)	
Assets	0.342*	-0.231	0.505
	(0.206)	(0.268)	
Employment & Income	-0.194	0.239	0.710
	(0.245)	(0.275)	
Borrowing & Saving	-0.101	0.126	0.857
	(0.117)	(0.180)	
Education	-0.051	0.415	0.538
	(0.509)	(0.780)	
Health	-0.138	0.243	0.726
	(0.274)	(0.405)	
Panel D: Urban Migrant Outcomes			
Expenditure & Poverty	0.031	-0.375	0.198
	(0.189)	(0.328)	
Employment & Income	0.035	-0.212	0.401
	(0.168)	(0.270)	
Savings	-0.222	0.447*	0.246
	(0.177)	(0.262)	
Health	0.123	-0.038	0.439
	(0.086)	(0.140)	

Notes: This table examines whether treatment effects differ by whether households were recruited from the SHIREE program roster versus snowball sampling. Column 1 shows treatment effects for non-SHIREE households. Column 2 shows the interaction coefficient, indicating differential effects for SHIREE households (i.e., the additional effect for SHIREE households beyond the baseline treatment effect). Column 3 reports p-values from tests of equality of treatment effects across groups (testing whether $\beta_1 + \beta_3 = 0$). All regressions use ANCOVA specifications with PDSLASSO variable selection and heteroskedasticity-robust standard errors.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.