

# Mobile Financial Services Use and Household Resilience in Rural Bangladesh after COVID-19: Mixed-Methods Observational Evidence

MD ASAD NOOR and NUSRAT JAHAN

Received: September 15, 2025 | Accepted: September 16, 2026

The present study examines the relationship between household resilience in rural Bangladesh and different categories of mobile financial services (MFS) use, as reported and categorized based on survey respondents' responses, following the COVID-19 shock. The study employed a convergent parallel mixed-methods design based on a survey of 300 households in Kurigram, Sunamganj, and Jhenaidah, 12 key informant interviews, and six focus group discussions. In the quantitative part of the study, users of MFS were split into regular users and occasional users, and then each group was compared with non-users. The specified 1:1 propensity-score matching algorithm failed to satisfy a balance criterion. Therefore, the main specification relied on propensity-score overlap weighting, covariate adjustment, and union-clustered standard errors. The use of MFS compared to non-use was associated with 17.42 percentage points lower decline of income, 2.51 points lower food-insecurity score, 14.06 percentage points lower probability of distress asset sale, and 3.88 fewer months to recovery among regular users. Only the decline of income was smaller among occasional users. No statistically significant gender- or income-based interactions were observed after correction for multiple testing. Exploratory decomposition did not support the hypothesized pathways through income smoothing and access to cash in emergencies. Fieldwork notes suggested same-day liquidity, shorter travel and waiting times, and challenges related to digital skills, internet stability, liquidity of agents, transaction fees, and trust. Results of the present study should be considered observational associations, rather than causal effects. Membership in the regular use category of MFS field-coded by field surveys was associated with better reported resilience outcomes, and it remains unclear whether MFS itself leads to such outcomes.

*JEL Classifications:* O16; D14; G21; I32; C21.

*Keywords:* Mobile Financial Services; Household Resilience; Financial Inclusion; Food Insecurity; Rural Bangladesh; COVID-19; Overlap Weighting.

## 1. INTRODUCTION

In many developing countries, MFS have become an alternative channel through which households receive remittances, payments, hold balances and gain short-term liquidity (Aker & Mbiti, 2010). The developmental impact of MFS becomes particularly evident in rural areas, where the spatial concentration of formal bank branches and the high costs of travel, transactions, and information can constrain access to conventional financial services. In particular, mobile money can facilitate remittance transfers, reduce transaction costs, strengthen risk-sharing mechanisms, and enhance households' capacity to manage and cope with shocks (Abiona & Koppensteiner, 2022; Jack & Suri, 2014; Munyegera & Matsumoto, 2016; Riley, 2018; Suri & Jack, 2016). Additionally, the period of COVID-19 pandemic has seen growth in usage of digital payments and their increasing importance to household financial resilience (Demirgüç-Kunt, et al. 2022).

Bangladesh represents an institutionally distinct setting for examining MFS and household resilience. Under the *Bangladesh Mobile Financial Services (MFS) Regulations*,

Md Asad Noor <asad@bus.green.edu.bd> is Assistant Professor Green Business School Green University of Bangladesh. Nusrat Jahan <nusrat@bus.green.edu.bd> is Assistant Professor Green Business School Green University of Bangladesh.

2022, Bangladesh Bank permits scheduled commercial banks, financial institutions, and government-entity-led providers to offer mobile money services (Bangladesh Bank, 2022). Bangladesh Bank's provider list includes Rocket, provided by Dutch-Bangla Bank PLC; bKash, provided by bKash Ltd.; and Nagad, provided by the Bangladesh Post Office under interim approval (Bangladesh Bank, n.d.). This institutional arrangement contrasts with the common mobile-network-operator-led model and includes regulated accounts linked to local cash-in and cash-out services, person-to-person transfers, merchant and bill payments, government transfers, and inward remittances. Access to liquidity through these services, however, depends on agent proximity and liquidity, transaction fees, network reliability, consumer protection, and users' ability to conduct transactions independently. Bangladesh-based research on bKash links MFS use to remittance flows, consumption and income smoothing, household welfare, and women's economic participation (Murshid, et al. 2020).

COVID-19 shock brought out the distinction between MFS and resilience more clearly. Households encountered shocks to employment, market functioning, mobility and remittances flows, while public and private transfers were increasingly conducted via digital means. MFS may potentially be associated with household resilience since it reduces time and distance of receiving funds, expands reach of family and social network and enables households to make payments or withdrawals immediately. On the other hand, households using MFS regularly may have different characteristics in terms of financial literacy, motivation, risk aversion, entrepreneurship, migration networks, social capital and pre-existing economic stability. These characteristics may influence both MFS adoption and recovery. Thus, cross-sectional comparison cannot distinguish whether MFS creates household resilience, whether more resilient households use MFS more frequently or whether there are other factors shaping both processes.

However, household resilience is also multidimensional. It does not refer only to income recovery after a shock, but to households' ability to absorb shock, keep minimum consumption level, preserve productive assets and recover livelihood functions (Barrett & Constanas, 2014). In this study, it is defined using four reported indicators: (1) the share of income decline relative to pre-pandemic income; (2) food insecurity; (3) distress asset sales; and (4) the time required for recovery. Consumption smoothing and access to emergency cash are considered as proposed mechanisms which may explain patterns in data.

This study aims to answer three research questions. First, what is the association of regular and occasional MFS use with income decline, food insecurity, distress asset sales and recovery time compared to non-use after matching on observed covariates? Second, is consumption smoothing and access to emergency cash statistically consistent with indirect associational pathways between regular MFS use and selected resilience outcomes? Third, does the adjusted association vary across female headship and baseline-income categories? Qualitative evidence provides contextual interpretation of these findings, including household descriptions of liquidity issues, transaction costs and MFS barriers.

The contribution is threefold. Empirically, the study provides multidimensional household-level evidence from rural districts of Bangladesh, an underexplored context in the existing literature compared with urban and national-level settings. Methodologically, the study distinguishes observed covariate balance from causal identification, uses overlap weightings when pre-specified matching did not result in acceptable balance and reports subgroup estimates and multiple testing adjustments. Substantively, the study distinguishes robust regular MFS results from occasional MFS results and proposes consumption smoothing and access to emergency cash pathways. The paper's claims are limited to adjusted observational associations.

## 2. LITERATURE REVIEW AND CONCEPTUAL FRAMING

### 2.1. Mobile Financial Services, Transaction Costs, and Risk Sharing

Transaction costs provide an economic rationale for using MFS in the context of shocks. For instance, during periods of shock, rural households may rely on geographically distant relatives, migrant workers, informal lenders, or other social contacts for financial support. By reducing these transaction costs and enabling payments across distances, MFS can facilitate the transfer of digital funds between geographically dispersed users through local cash-in and cash-out points.

A panel data analysis in Kenya indicates that an increase in mobile money adoption was associated with an improvement in the ability of households to smooth consumption in response to shocks through remittances and a greater variety of senders (Jack & Suri, 2014). Riley (2018) reaches a similar conclusion regarding the ability of mobile money adoption to facilitate risk sharing in case of village-level shocks. In Uganda, Munyegera and Matsumoto (2016) find mobile money adoption to have been linked to higher levels of household welfare and an increase in remittance frequency. There is evidence as well that in disaster settings, digital transfers and communication could be a more rapid solution than traditional channels (Blumenstock, et al. 2016). Taken together, these findings suggest that reduced transaction costs and network-based transfers may serve as important pathways linking digital financial services to households' ability to respond effectively to shocks.

Subsequent literature links financial inclusion to poverty alleviation and resilience directly. Abiona and Koppensteiner (2022) explore the impact of mobile money adoption in Tanzania on consumption smoothing, poverty status, and human capital investments in response to shocks. Yao, et al. (2023) study mobile money and household resilience in Kenya, and Suri and Jack (2016) investigate the longer-run poverty and gender effects of mobile money adoption. These studies suggest that digital finance matters for household well-being through liquidity, remittances, savings, risk sharing, and shifts in economic activity. At the same time, they do not indicate that all associations between MFS intensity and recovery from previous shocks are causal.

### 2.2. Evidence from Bangladesh

In case of Bangladesh, the MFS model features regulated service providers, agent delivery, and a wide array of allowable payments domestically. The 2022 regulations refer to the model of Bank/Financial Institution/Government Entity-led model and give permission for performing cash-in/cash-out, person-to-person transactions, business transactions, government transactions, e-commerce transactions, loan repayments, and inward-remittance payouts (Bangladesh Bank, 2022). This model could enable liquidity on demand, as customers may get payments on their accounts and convert the electronic value into cash locally. In turn, the usefulness of the possibility is conditional on the proximity of the agents, their liquidity, reliability, and network.

Experiments in Bangladesh provide very useful data. Lee, et al. (2021) provided poor rural households and migrants with access to mobile banking and found that the technology increased remittance flows and improved related outcomes, while also affecting the associated transaction costs. These findings provide evidence for the hypothesis that digitalisation can alter the functioning of migration and family-support

networks. However, the current research project involves the analysis of a wider variety of consequences after the shock experienced, as well as different degrees of MFS usage.

### 2.3. Household Resilience and Proposed Correlates

Resilience involves the ability of a household to cope with shocks while continuing to perform important functions (Barrett & Constanas, 2014). Stability of income measures the extent of economic disruption. Food insecurity represents whether households were able to sustain basic consumption. Asset distress sales involve potential harmful coping mechanisms that might hinder future earnings. Recovery time denotes how long the period of disruption lasted. These indicators are related and yet different dimensions; therefore, the evaluation based on multiple dimensions is better than the single one.

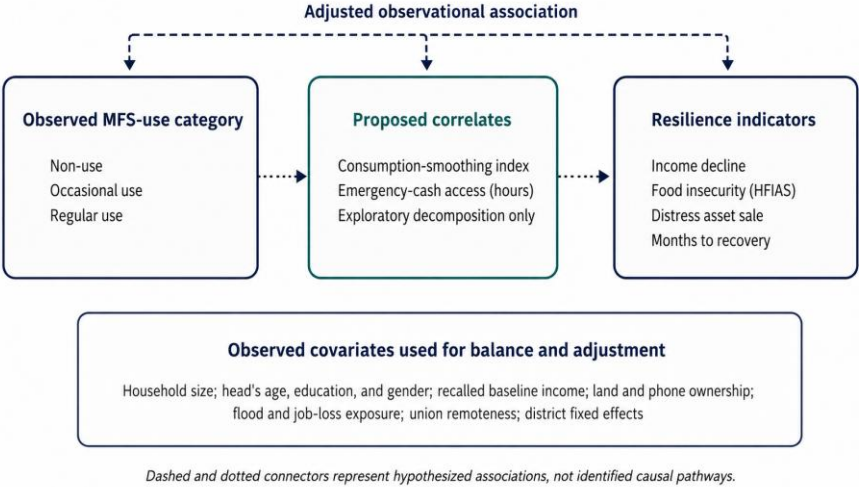
Consumption smoothing—keeping the level of necessary expenditure irrespective of income variation—is an integral part of risk management in households (Morduch, 1995). Fast access to emergency money is also a theoretically reasonable indicator of household resilience. Mobile money transfers can reduce the time and costs associated with accessing financial support, extend the geographic reach of risk-sharing networks, and help households maintain essential expenditures when their income is disrupted. There is research from Kenya and Tanzania suggesting the relation between mobile money and risk sharing, remittances receipt, consumption protection and coping with economic shocks (Abiona & Koppensteiner, 2022; Jack & Suri, 2014; Riley, 2018). The experimental research conducted in Bangladesh reveals that mobile banking increases the rural-urban remittance ties and influences household consumption, and Bangladesh-related research on bKash highlights income and consumption smoothing among households affected by shocks (Lee, et al. 2021; Murshid, et al. 2020). Relatively few Bangladesh-based studies explore the relationship between consumption smoothing and access to emergency money as potential correlates of multidimensional resilience outcomes through MFS use. This research motivates the present exploratory decomposition, however, does not provide the basis for causal mediation, since all three variables were measured at once in a cross-sectional survey.

### 2.4. Research Gap and Conceptual Framework

Three gaps motivate the analysis. First, Bangladesh-focused evidence has concentrated more heavily on access, adoption, remittances, and payment delivery than on a joint assessment of income, food security, asset protection, and recovery time. Second, the distinction between occasional and regular engagement is important because account ownership or one-off transfers may not represent the repeated use needed during a shock. Third, gender and income differences are frequently discussed in digital-inclusion policy, but modest samples can generate unstable subgroup findings unless interaction tests and statistical power are reported transparently. Figure 1 summarises the associational conceptual framework.

The framework distinguishes an observed exposure, proposed correlates, resilience indicators, and measured covariates. It is not a causal diagram. The empirical analysis estimates adjusted mean differences in the population with the greatest observed covariate overlap. The qualitative component is used to contextualise how participants experienced MFS, rather than to validate a causal mechanism.

**Fig. 1. Associational Conceptual Framework Linking MFS-use Category, Proposed Correlates, Resilience Indicators, and Observed Covariates.**



### 3. METHODOLOGY

#### 3.1. Research Design and Study Setting

This study employed a convergent parallel mixed-methods design. Household survey and key informant interview (KII) and focus group discussion (FGD) data were collected during the same fieldwork period and treated as separate evidence streams, which were integrated during interpretation by examining areas of convergence and divergence. The quantitative strand estimated adjusted associations between field-coded MFS-use categories and resilience indicators. The qualitative strand was intended to contextualise experiences of liquidity, transaction frictions, exclusion, and trust. Qualitative observations were not used to validate a causal mechanism or estimate the prevalence of particular experiences.

Fieldwork was conducted from December 2024 to March 2025 in Kurigram, Sunamganj, and Jhenaidah. The districts were selected purposively to represent northern, north-eastern, and south-western rural settings with differing exposure to economic and environmental risks and variation in infrastructure. Within districts, unions were classified as urban-adjacent or remote, and households were selected from local lists through a multistage procedure. The final survey contained 300 households: 120 non-users, 90 occasional users, and 90 regular users. The qualitative component comprised 12 KIIs with MFS agents, non-governmental organization (NGO) staff, and local leaders and six FGDs involving women, older residents, informal workers, and other rural users.

#### 3.2. Data Collection, Time References, and Ethics

The structured questionnaire recorded household characteristics, recalled pre-pandemic income, current income, shock exposure, MFS-use category, food insecurity, distress coping, recovery time, consumption smoothing, and emergency-cash access.

"Pre-pandemic income" referred to the 12-month period ending February 2020, whereas current income referred to the month of interview. Enumerators used locally salient events, including Eid and monsoon periods, to help respondents locate the reference period which may reduce but cannot eliminate error from a recall window approaching five years.

The KIIs and FGDs were conducted in Bangla. Participation was voluntary, informed consent was obtained, and identifying details were excluded from reporting. Source-level transcripts, coded extracts, and translation records were not retained. Consequently, no direct quotations are reported, and the available de-identified fieldwork summaries are used only for contextual interpretation.

### 3.3. Variables and Measurement

The field protocol defined regular use as MFS transactions on at least two days per week on average during the preceding three months, occasional use as no more than one transaction per month on average during the same period, and non-use as no MFS transaction during the preceding six months despite access to a mobile phone. The retained analytical file assigns all 300 households to one of these categories but does not preserve transaction counts, transaction days, dates, or a documented rule for households whose use fell between the occasional- and regular-use cut-offs. Consequently, the number and treatment of intermediate-frequency households were not reconstructed, and alternative cut-offs were not tested. The analysis therefore treated these variables as archived field-coded categories rather than a verified transaction-frequency measure or dose-response exposure.

Table 1  
*Variables and Measurement*

| Variable              | Role               | Operational measure                                                | Interpretation                                                         |
|-----------------------|--------------------|--------------------------------------------------------------------|------------------------------------------------------------------------|
| MFS use               | Exposure           | Field-coded non-use, occasional use, or regular use                | Two pairwise contrasts: regular vs. non-use and occasional vs. non-use |
| Income decline        | Outcome            | Percentage difference between recalled baseline and current income | Lower values indicate a more favorable outcome                         |
| Food insecurity       | Outcome            | Continuous HFIAS-derived score in the supplied dataset             | Higher values indicate worse food insecurity                           |
| Distress asset sale   | Outcome            | 1 if the household reported selling productive assets to cope      | Lower probability indicates a more favorable outcome                   |
| Recovery time         | Outcome            | Reported months to return to pre-pandemic income                   | Fewer months indicate faster recovery                                  |
| Consumption smoothing | Proposed correlate | Continuous index supplied in the dataset                           | Higher values indicate greater reported smoothing capacity             |
| Emergency-cash access | Proposed correlate | Hours required to obtain emergency cash                            | Fewer hours indicate faster access                                     |
| Covariates            | Adjustment set     | Demographic, economic, shock, remoteness, and district variables   | Used for propensity estimation, balance, and outcome adjustment        |

Four outcomes represented household resilience. Income decline was the percentage difference between recalled pre-pandemic and current monthly income; negative values indicate current income above the recalled baseline. Food insecurity was represented by a score described in the analytical file as derived from the Household Food Insecurity Access Scale (HFIAS). The observed score ranged from 0.9 to 22.6, with higher values indicating greater food insecurity. The archived file did not contain the nine item-level responses or the transformation formula that produced the one-decimal values. Consequently, the variable is not described as the standard raw HFIAS total, and internal-consistency reliability is not reported. The inability to reconstruct its scoring procedure is treated as a measurement limitation.

The propensity model included household size; age, education, and gender of the household head; recalled baseline income; land ownership; phone ownership; flood exposure; job-loss exposure; union remoteness; and district fixed effects. Table 1 summarizes the measures and interpretation.

### 3.4. Data Quality and Missing Values

The dataset contained 300 unique household identifiers and no duplicate records. Nine cells were missing: three education values, three HFIAS-derived scores, and three consumption-smoothing values. Primary analyses used complete cases, and k-nearest-neighbor imputation was examined as a sensitivity analysis. Pairwise primary analytic samples ranged from 205 to 208 because the non-user group was compared separately with each MFS-use group. All supplied sampling weights equaled one.

Nine households had negative income-decline values, indicating that their reported current income was slightly higher than recalled pre-pandemic income, and one household had a zero-month recovery value. The source questionnaires were not available in the retained analytical archive for re-verification. These observations were retained in the primary analysis because the values are conceptually possible. Outcome-specific exclusion analyses were subsequently conducted to determine whether they materially influenced the findings. Because item-level HFIAS and consumption-smoothing responses were unavailable, the reliability of these measures could not be assessed.

### 3.5. Statistical Analysis

The field-coded MFS-use category was treated as an observational exposure, with regular use and occasional use compared separately with non-use. A corresponding model was estimated for occasional use versus non-use. The prespecified analysis used 1:1 nearest-neighbor propensity-score matching (PSM) with replacement and an absolute propensity-score caliper of 0.05 (Austin, 2011). Reanalysis showed that this matcher did not reduce every absolute standardized mean difference (SMD) below 0.10. The maximum post-match absolute SMD was 0.139 for regular versus non-use and 0.207 for occasional versus non-use. The matched estimator was therefore retained only as a sensitivity analysis.

The primary specification used propensity-score overlap weights (Li, et al. 2018). For each pairwise comparison, MFS users received weight  $1 - e(X)$ , and non-users received weight  $e(X)$ , where  $e(X)$  is the estimated probability of belonging to the relevant MFS-use group conditional on observed covariates. Overlap weighting emphasises households with the greatest observed comparability and avoids extreme inverse-probability weights. Weighted linear outcome models were also re-adjusted for the propensity covariates. Standard errors (SEs) were clustered by union (21 clusters), and two-sided 95 percent confidence intervals

(CIs) were reported. The resulting estimand is an adjusted mean difference in the overlap population, not an average causal treatment effect.

The analysis included several additional checks. Because the original transaction-frequency field was unavailable, we also conducted an exploratory category-label perturbation analysis. In 500 repetitions at each of three levels, 5 percent, 10 percent, or 20 percent of the regular- and occasional-use labels were randomly exchanged, after which the propensity-score and outcome models were re-estimated. This stress test examines sensitivity to non-differential classification noise between the two active-use categories. It is not an alternative-threshold analysis and cannot identify how genuinely intermediate-frequency households were classified. First, full-sample covariate-adjusted regressions and the original matching specification assessed sensitivity to the estimator. Second, *k*-nearest-neighbor imputation assessed the limited item nonresponse. Third, female-headship and median-baseline-income interaction models addressed the heterogeneity question. The Benjamini-Hochberg procedure controlled the false-discovery rate across 16 interaction tests (Benjamini & Hochberg, 1995). Cluster-adjusted minimum detectable effects (MDEs) were calculated to describe subgroup precision. Fourth, product-of-coefficients models examined the proposed consumption-smoothing and emergency-cash pathways with 5,000 union-cluster bootstrap repetitions conditional on the estimated weights. These models are labelled exploratory associational decompositions, not causal mediation analyses.

Rosenbaum matched-pair bounds were calculated for distress asset sales using no-replacement pairs because conventional bounds require independent pairs (Rosenbaum, 2002). Approximate Oster coefficient-stability calculations were also examined for continuous outcomes (Oster, 2019). These diagnostics test sensitivity under specific assumptions; they do not eliminate omitted-variable bias or establish temporal ordering.

Exploratory heterogeneity was assessed using separate full-sample covariate-adjusted interaction models for regular versus non-use and occasional versus non-use. The modifiers were female headship and recalled baseline-income status. At-or-below-median income was defined as baseline monthly household income not exceeding the sample median of 11,617 Bangladeshi taka (BDT); this is a sample-based classification rather than an official poverty threshold. Each model included the MFS-use indicator, the modifier, their interaction, and the same demographic, economic, shock, remoteness, and district covariates used in the primary models. Standard errors were clustered at the union level. Four outcomes were examined for each of two use-category contrasts and two modifiers, producing 16 interaction tests. Nominal *p* values and Benjamini-Hochberg false-discovery-rate-adjusted *q* values are reported. Because the subgroup cells were modest, the interaction results are interpreted as exploratory.

### 3.6. Qualitative Analysis and Integration

The fieldwork included 12 key informant interviews and six focus group discussions conducted in Bangla. Source-level transcripts, detailed field notes, coded extracts, source identifiers, and translation records were not retained and therefore could not be used to audit individual statements. Accordingly, no direct quotations are reported, and the study does not claim independently verified coding, thematic saturation, intercoder agreement, or systematic qualitative subgroup differences. The available de-identified fieldwork summaries are used only as contextual notes concerning access to funds, transaction frictions, digital capability, network conditions, agent liquidity, and trust. They are not used as estimates of prevalence, evidence of causality, or statistical confirmation of gender- or income-based heterogeneity.

## 4. RESULTS

### 4.1. Descriptive Statistics

Table 2 presents unadjusted group characteristics computed from the analytical dataset. Mean recalled baseline income was BDT 11,317 among non-users, BDT 11,585 among occasional users, and BDT 11,893 among regular users. Female-headed households represented 29.2 percent of non-users, 17.8 percent of occasional users, and 23.3 percent of regular users. COVID-related income loss was common in all three groups.

The raw outcome differences were largest for regular use. Mean reported income decline was 25.31 percent among non-users, 20.99 percent among occasional users, and 8.09 percent among regular users. The corresponding mean HFIAS-derived scores were 11.72, 11.06, and 9.34, respectively. Distress asset sales were reported by 25.0 percent of non-users, 17.8 percent of occasional users, and 12.2 percent of regular users. Mean recovery time was 11.83, 10.96, and 7.96 months, respectively. These comparisons are descriptive and do not adjust for observed differences.

Table 2  
*Household Characteristics and Outcomes by MFS-use Category*

| Variable                             | Non-users<br>(n = 120) | Occasional<br>users (n = 90) | Regular users<br>(n = 90) | Total<br>(N = 300) |
|--------------------------------------|------------------------|------------------------------|---------------------------|--------------------|
| Household size                       | 5.63 (2.34)            | 5.47 (2.51)                  | 5.53 (2.23)               | 5.55 (2.35)        |
| Age of household head (years)        | 45.33 (11.09)          | 46.22 (8.13)                 | 45.26 (9.54)              | 45.58 (9.79)       |
| Education of household head (years)  | 5.81 (2.57)            | 5.41 (2.54)                  | 5.46 (2.74)               | 5.59 (2.61)        |
| Recalled baseline income (BDT/month) | 11,317 (3,969)         | 11,585 (4,128)               | 11,893 (4,022)            | 11,570 (4,026)     |
| Female-headed household (%)          | 29.2                   | 17.8                         | 23.3                      | 24.0               |
| Land ownership (%)                   | 65.8                   | 62.2                         | 66.7                      | 65.0               |
| Phone ownership (%)                  | 90.0                   | 81.1                         | 88.9                      | 87.0               |
| COVID-related income loss (%)        | 77.5                   | 75.6                         | 78.9                      | 77.3               |
| Income decline (%)                   | 25.31 (5.61)           | 20.99 (6.05)                 | 8.09 (5.63)               | 18.85 (9.26)       |
| HFIAS-derived score                  | 11.72 (3.89)           | 11.06 (3.70)                 | 9.34 (4.12)               | 10.81 (4.02)       |
| Distress asset sale (%)              | 25.0                   | 17.8                         | 12.2                      | 19.0               |
| Recovery time (months)               | 11.83 (3.34)           | 10.96 (3.30)                 | 7.96 (3.26)               | 10.41 (3.68)       |

Note. Values are means (standard deviations) unless percentages are shown. Valid N is 297 for education and the HFIAS-derived score because of item nonresponse.

### 4.2. Covariate Balance and Overlap

Table 3 reports the propensity-score balance and overlap diagnostics. The area under the receiver operating characteristic curve (AUC) was 0.574 for regular versus non-use and 0.634 for occasional versus non-use, indicating modest discrimination rather than near-deterministic exposure assignment. Before overlap weighting, the maximum absolute SMD was 0.162 and 0.263, respectively. Overlap weighting reduced these values to 0.0005 and

0.0006. The effective sample sizes were 87.8 regular users and 114.3 non-users in the regular-use comparison and 86.1 occasional users and 108.4 non-users in the occasional-use comparison.

Table 3

*Propensity-score Balance Diagnostics*

| Contrast               | Eligible MFS users | Eligible non-users | AUC   | Maximum Absolute SMD Before Overlap Weighting | Maximum Absolute SMD after Overlap Weighting | Effective N (MFS/non) |
|------------------------|--------------------|--------------------|-------|-----------------------------------------------|----------------------------------------------|-----------------------|
| Regular vs. non-use    | 89                 | 118                | 0.574 | 0.162                                         | 0.0005                                       | 87.8 / 114.3          |
| Occasional vs. non-use | 90                 | 118                | 0.634 | 0.263                                         | 0.0006                                       | 86.1 / 108.4          |

*Note.* Effective sample sizes are weight-based diagnostics, not household counts. The original 1:1 matching specification had maximum post-match absolute SMDs of 0.139 and 0.207 and is therefore reported as a sensitivity analysis.

#### 4.3. Main Adjusted Associations

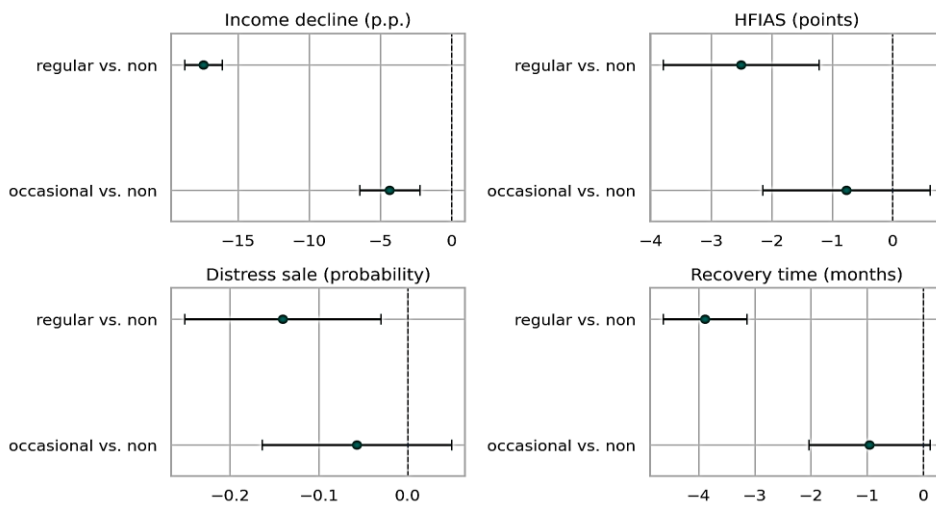
Table 4 reports the overlap-weighted, covariate-adjusted estimates. Relative to non-use, regular MFS use was associated with a 17.42 percentage-point smaller income decline (95 percent CI -18.73 to -16.11), a 2.51-point lower HFIAS-derived score (95 percent CI -3.79 to -1.22), a 14.06 percentage-point lower probability of distress asset sale (95 percent CI -25.09 to -3.04), and 3.88 fewer months to recovery (95 percent CI -4.63 to -3.14). Each confidence interval excluded zero.

Table 4

*Overlap-weighted Adjusted Associations Between MFS use and Resilience Outcomes*

| Contrast               | Outcome                      | Estimate | SE   | 95% CI           | p     | Analytic N |
|------------------------|------------------------------|----------|------|------------------|-------|------------|
| Regular vs. non-use    | Income decline (p.p.)        | -17.42   | 0.63 | [-18.73, -16.11] | <.001 | 207        |
| Regular vs. non-use    | HFIAS-derived score (points) | -2.51    | 0.62 | [-3.79, -1.22]   | <.001 | 205        |
| Regular vs. non-use    | Distress asset sale (p.p.)   | -14.06   | 5.28 | [-25.09, -3.04]  | .015  | 207        |
| Regular vs. non-use    | Recovery time (months)       | -3.88    | 0.36 | [-4.63, -3.14]   | <.001 | 207        |
| Occasional vs. non-use | Income decline (p.p.)        | -4.34    | 1.01 | [-6.44, -2.24]   | <.001 | 208        |
| Occasional vs. non-use | HFIAS-derived score (points) | -0.77    | 0.66 | [-2.15, 0.62]    | .262  | 206        |
| Occasional vs. non-use | Distress asset sale (p.p.)   | -5.71    | 5.11 | [-16.37, 4.94]   | .276  | 208        |
| Occasional vs. non-use | Recovery time (months)       | -0.96    | 0.52 | [-2.04, 0.12]    | .079  | 208        |

*Note.* Pairwise overlap-weighted linear models re-adjusted for the propensity covariates and used standard errors clustered by union (21 clusters). Negative estimates denote more favorable outcomes for the MFS-use group. These estimates are adjusted observational associations, not causal effects.

**Fig. 2. Displays the Adjusted Estimates and their 95 Percent Confidence intervals.**

For occasional use, the income-decline association was smaller but clearly different from zero (-4.34 percentage points; 95 percent CI -6.44 to -2.24). Associations with the HFIAS-derived score (-0.77 points; 95 percent CI -2.15 to 0.62), distress asset sales (-5.71 percentage points; 95 percent CI -16.37 to 4.94), and recovery time (-0.96 months; 95 percent CI -2.04 to 0.12) were imprecise and did not meet the 5 percent threshold. Thus, the dataset supports a broad regular-use pattern, whereas the occasional-use pattern is limited mainly to the reported income outcome.

**Fig. 2. Overlap-weighted Adjusted Associations with 95 Percent Confidence Intervals.**

#### 4.4. Gender and Income Heterogeneity

None of the 16 female-headship or baseline-income interaction tests remained statistically significant after Benjamini–Hochberg adjustment. For regular versus non-use, the adjusted association with income decline was similar across male- and female-headed households (interaction = -0.06 percentage points, 95 percent CI [-4.13, 4.01],  $p = .976$ ,  $q = .976$ ). The female-headship interaction for recovery time was larger but imprecise (2.91 months, 95 percent CI [-0.02, 5.83],  $p = .052$ ,  $q = .276$ ). The baseline-income interactions for regular use were also imprecise across all four outcomes (all  $q \geq .685$ ). These results did not provide robust evidence that the regular-use associations differed by female headship or the sample-based income classification.

For occasional versus non-use, two interactions had nominal  $p$  values below .05: female headship for income decline (5.77 percentage points, 95 percent CI [1.50, 10.03],  $p = .011$ ) and at-or-below-median income for recovery time (2.41 months, 95 percent CI [0.42, 4.40],  $p = .020$ ). Both adjusted to  $q = .161$  and therefore did not remain statistically significant after correction for 16 tests. The remaining occasional-use interactions were also not statistically distinguishable from zero after multiplicity adjustment. These

nominal patterns are treated as exploratory signals rather than evidence of systematic subgroup differences.

Before outcome-specific missingness, female-headed households comprised 35 non-users, 16 occasional users, and 21 regular users. Households at or below the median recalled baseline income comprised 60 non-users, 45 occasional users, and 45 regular users. The HFIAS-derived models had slightly smaller analytic samples because of three missing HFIAS-derived values and three missing education values. The modest subgroup cells limit precision, particularly for female-headed households. Cluster-adjusted minimum-detectable-effect calculations indicated that female-headed subgroup comparisons required standardized differences of approximately  $d = 0.81\text{--}0.99$ , whereas at-or-below-median-income comparisons required approximately  $d = 0.58\text{--}0.68$ . Non-detection should therefore not be interpreted as evidence that the associations are identical across groups. Detailed interaction estimates are reported in Appendix Table A2.

#### 4.5. Exploratory Associational Decompositions

The exploratory decomposition did not support a positive indirect pathway through consumption smoothing. For the HFIAS-derived score, the estimated indirect association through consumption smoothing was  $-0.28$  points (95 percent bootstrap CI  $-0.99$  to  $0.79$ ;  $p = .616$ ), while the direct component was  $-2.23$  points. The indirect component was small and not statistically distinguishable from zero, so a positive mediated share is not supported.

For distress asset sales, the emergency-cash decomposition produced an indirect component of  $+26.92$  percentage points (95 percent bootstrap CI  $9.35$  to  $43.24$ ) and a direct component of  $-40.98$  percentage points, yielding the total of  $-14.06$  percentage points. The opposite signs indicate inconsistent mediation or suppression, not a stable positive pathway. Reporting a percentage mediated would therefore be misleading. Table 5 emphasises the indirect components and their interpretation; the complete decomposition is provided in Appendix Table A3.

Table 5

##### *Exploratory Indirect Associations for Regular MFS use Versus non-use*

| Outcome and Proposed correlate                         | Indirect Estimate | 95 percent Bootstrap CI | Bootstrap p | Interpretation                                                    |
|--------------------------------------------------------|-------------------|-------------------------|-------------|-------------------------------------------------------------------|
| HFIAS-derived score via consumption Smoothing (points) | -0.28             | [-0.99, 0.79]           | .616        | No distinguishable indirect Association                           |
| Distress asset sale via Emergency-cash hours (p.p.)    | 26.92             | [9.35, 43.24]           | .005        | Opposite in sign to the total; Suppression/inconsistent mediation |

*Note.* Product-of-coefficients decompositions used overlap weights and 5,000 union-cluster bootstrap repetitions conditional on the estimated weights. They are not estimates of the average causal mediation effect (ACME) or average direct effect (ADE).

#### 4.6. Sensitivity Analyses

The full-sample covariate-adjusted estimates were close to the primary overlap-weighted estimates. For regular versus non-use, they were  $-17.32$  percentage points for income decline,  $-2.48$  points for the HFIAS-derived score,  $-13.45$  percentage points for distress asset

sale, and -3.84 months for recovery. K-nearest-neighbor imputation of the nine missing cells also produced similar estimates: -17.29, -2.45, -12.62 percentage points, and -3.83 months, respectively.

The 1:1 PSM sensitivity analysis showed the same direction for most regular-use outcomes but did not satisfy the prespecified balance criterion. Its regular-use distress-sale estimate was also imprecise. These results reinforce the decision not to use the matched model as the primary numerical basis. Appendix Table A1 reports the estimator comparison.

For the distress-sale sensitivity check, the regular-use matched-pair association had a one-sided  $p = .008$  at  $\Gamma = 1$  and became non-significant at approximately  $\Gamma = 1.33$ , indicating sensitivity to modest hidden bias. The occasional-use contrast was already non-significant at  $\Gamma = 1$ . For regular-use recovery time, the approximate Oster delta required to move the estimate to zero was 43.77; for some other outcomes, coefficient movement after adjustment made a conventional positive delta uninterpretable. These diagnostics do not overcome the possibility of omitted-variable bias. Appendix Table A4 summarizes the hidden-bias and precision diagnostics.

As a data-quality sensitivity check, the nine negative income-decline observations were excluded from the income model and the single zero-month observation was excluded from the recovery model. The regular-use association remained -16.34 percentage points for income decline (95 percent CI [-17.45, -15.24]) and -3.79 months for recovery time (95 percent CI [-4.49, -3.10]). The occasional-use estimates were -4.34 percentage points (95 percent CI [-6.44, -2.24]) and -0.96 months (95 percent CI [-2.04, 0.12]), respectively. The substantive pattern therefore does not depend on the flagged observations, although this sensitivity analysis does not substitute for verification against the original questionnaires. Appendix Figures A1 and A2 present post-match covariate balance for the regular-use and occasional-use contrasts.

The category-label perturbation analysis indicated that the direction of the regular-use results was not dependent on moderate random exchange between the regular- and occasional-use groups. All four median regular-use estimates remained favorable at the 5 percent, 10 percent, and 20 percent exchange levels. Under the most extensive 20 percent exchange, the median estimates were -14.93 percentage points for income decline, -2.22 points for the HFIAS-derived score, -12.80 percentage points for distress asset sale, and -3.31 months for recovery. Across the simulations, the regular-use association remained statistically distinguishable from zero in every repetition for income decline, the HFIAS-derived score, and recovery time, and in 75.8 percent of repetitions for distress asset sale. This stress test supports directional robustness to simulated category-label noise. However, it does not reproduce alternative transaction-frequency thresholds and should not be interpreted as evidence of threshold or dose-response robustness. Detailed simulation results are reported in Appendix Table A5.

#### 4.7. Qualitative Findings

The archived fieldwork summaries identified possible practical conditions surrounding MFS use. These included same-day access to transferred funds, reduced travel and waiting time when a functioning agent was available, and continuing difficulties involving mobile-network reliability, agent liquidity, digital capability, transaction charges, and trust. Because the source-level qualitative records were unavailable for re-audit, these observations are presented only as contextual fieldwork notes and not as systematically verified themes or evidence that MFS caused the quantitative outcomes.

With respect to gender and economic position, the summaries suggested that household-head gender may not fully represent who controls the handset, MFS account, credentials, transfers, or spending decisions. They also pointed to possible resource-related barriers involving phone access, transaction costs, small or irregular balances, and dependence on relatives or agents. However, the available de-identified fieldwork summaries did not contain a traceable income classification for the qualitative participants. These observations therefore contextualize limitations in the quantitative gender and income measures; they are not treated as qualitative proof of subgroup differences. Research Question 3 is answered primarily through the interaction analysis reported above.

## 5. DISCUSSION

### 5.1. Main Findings and Contribution

The central finding is a consistent adjusted association between regular MFS use and all four resilience indicators. After balancing observed covariates through overlap weighting and re-adjusting the outcome models, regular users reported smaller income declines, lower HFIAS-derived scores, fewer distress asset sales, and shorter recovery times than comparable non-users.

This result contributes Bangladesh-specific evidence to a literature dominated by East African settings. It is consistent with studies linking mobile money to remittance receipt, risk sharing, consumption protection, and household resilience (Abiona & Koppensteiner, 2022; Jack & Suri, 2014; Munyegera & Matsumoto, 2016; Riley, 2018; Yao, et al. 2023). It is also consistent with experimental evidence that mobile banking can change rural-urban remittance relationships in Bangladesh (Lee, et al. 2021). The present study adds breadth across four resilience dimensions and distinguishes regular from occasional use, but its design is weaker for causal identification than the panel, quasi-experimental, or experimental studies in that literature.

The scale of the income-decline association is large and deserves careful interpretation. The current-income variable is arithmetically derived from recalled baseline income and the reported decline measure, and the survey was conducted nearly five years after the baseline reference period. Regular users may recall income differently, may have different livelihood trajectories, or may have adopted MFS in response to recovery. The large estimate should therefore be understood as a conditional difference in reported income trajectories, not as the amount of income decline prevented by MFS.

The archived regular-use category shows a stronger adjusted pattern than the archived occasional-use category. However, because the analytical file does not retain transaction-frequency records, this contrast cannot be interpreted as evidence that increasing transaction frequency produces progressively stronger resilience outcomes. It represents a comparison among field-coded categories rather than a dose-response relationship.

### 5.2. Proposed Pathways

The exploratory decompositions did not support the proposed beneficial pathways. The indirect association through consumption smoothing was small and imprecise, while the emergency-cash analysis produced opposite-sign indirect and direct components consistent with suppression. These findings show that theoretical plausibility and contextual fieldwork notes are insufficient to establish a statistical or causal mechanism; accordingly, no positive mediated proportion is reported.

Several explanations are possible. The aggregate indices may measure the proposed constructs imperfectly. The timing of consumption smoothing and emergency-cash access may overlap with the outcomes. Social networks, migration, wealth, or financial capability may jointly determine MFS use, the proposed correlates, and resilience. The qualitative evidence can still identify experiences of liquidity and lower transaction frictions, but it should be presented as contextual evidence rather than validation of a causal mediation model.

### **5.3. Gender and Income Differences**

The exploratory interaction analysis did not identify a gender- or baseline-income difference that remained statistically significant after correction for multiple testing. This finding represents non-detection rather than equivalence. Female headship does not measure individual ownership or control of a handset, MFS account, credentials, transfers, or spending, while a sample-median income split compresses substantial variation in economic position. The female-headed subgroup cells were also too small to detect many moderate interactions. The contextual fieldwork summaries point to possible issues involving account control, phone access, digital capability, transaction costs, and dependence on agents or relatives, but the absence of source-level records prevents these summaries from being treated as independently auditable subgroup findings. Future research should measure individual account control, digital capability, remittance-network strength, and multidimensional economic status directly and use a larger prospective sample designed for subgroup inference.

### **5.4. Institutional Interpretation and Policy Implications**

Bangladesh's regulated, agent-based MFS architecture helps connect electronic accounts to local liquidity. Permitted person-to-person, government-to-person, merchant, bill-payment, and remittance-payout functions provide multiple ways for households to move funds without visiting a bank branch (Bangladesh Bank, 2022). The qualitative evidence suggests that agent proximity and same-day access can reduce practical transaction frictions. However, the system's resilience value depends on network quality, agent liquidity, transparent charges, consumer protection, and users' independent ability to complete transactions.

Four cautious policy implications follow. First, regulators and providers may prioritize agent reliability and liquidity in remote unions, not merely agent counts. Second, government and development agencies may pilot MFS-enabled emergency and social-protection payments with explicit evaluation of receipt, cash-out access, fees, exclusion, and grievance resolution. Third, digital and financial capability initiatives may focus on safe, independent use among women, older adults, and less educated users while avoiding the assumption that training alone resolves infrastructure or trust barriers. Fourth, research and policy partnerships may facilitate privacy-protecting links between surveys and transaction records so that usage intensity and timing can be measured rather than recalled.

These are policy directions suggested by the observed patterns and participant accounts; they are not interventions whose effects were tested in this study. Any expansion may be paired with prospective evaluation.

### **5.5. Limitations**

The study has six major limitations. First, the cross-sectional design cannot establish temporal ordering. Current regular users may have used MFS before the shock, adopted it

during recovery, or increased its usage because they were already more economically connected. Reverse causality and simultaneity remain plausible.

Second, propensity-score methods address only observed covariates. Financial literacy, entrepreneurial aptitude, risk preferences, migration and remittance networks, social capital, local agent density, and pre-existing resilience were not measured comprehensively. Overlap weighting achieved excellent balance on included covariate means, but it cannot balance variables absent from the dataset.

Third, the survey relies on retrospective reports over a long interval. Recall error in baseline income, shock experience, and recovery time may attenuate, inflate, or otherwise distort associations. If recall differs by current MFS status, the direction of bias is not predictable. The same-time measurement of exposure, proposed correlates, and outcomes also invalidates a causal interpretation of the decomposition.

Fourth, the analytical file lacks raw transaction-frequency data. Although every household was assigned to a mutually exclusive field-coded use category, the treatment of borderline or intermediate-frequency households cannot be reconstructed, and alternative thresholds cannot be tested. The supplementary label-perturbation analysis shows that the direction of the regular-use results is not dependent on moderate random exchange between the regular- and occasional-use categories, but it does not establish threshold robustness. The estimates therefore compare archived categories and do not demonstrate a dose-response relationship.

Fifth, only aggregate HFIAS-derived and consumption-smoothing scores were available. The item-level responses, scoring formula, reference-period documentation, and internal-consistency reliability could not be independently reconstructed. The HFIAS-derived values include one-decimal scores and should not be interpreted as a verified standard nine-item HFIAS total. In addition, nine negative income-decline values and one zero-month recovery value could not be checked against the source questionnaires. Exclusion analyses indicate that these observations do not drive the main results, but source verification remains unavailable.

Sixth, the total sample is modest for three districts, three exposure groups, and 16 subgroup interaction tests. The study had limited power to detect moderate gender- and income-based differences, particularly among female-headed households. Furthermore, source-level KII and FGD records were unavailable for independent audit. The retained qualitative summaries are therefore used only as contextual fieldwork notes and cannot establish prevalence, subgroup differences, mediation, or causality.

## 6. CONCLUSION

Regular MFS use was consistently associated with more favorable reported post-COVID resilience outcomes in the supplied rural-household dataset. After overlap weighting and covariate adjustment, households in the archived regular-use category reported smaller income declines, lower HFIAS-derived scores, fewer distress asset sales, and shorter recovery times than non-users. Households in the occasional-use category differed clearly from non-users only in reported income decline. These findings distinguish the archived use categories, but they do not establish that greater transaction frequency causes stronger resilience.

The exploratory analysis did not support the proposed mechanism or subgroup differences. Consumption smoothing did not exhibit a statistically distinguishable indirect association with the HFIAS-derived outcome, and emergency-cash hours produced a suppression pattern rather than a stable positive mediated share. Gender and baseline-income interactions did not remain significant after multiplicity adjustment, although subgroup

precision was limited. Contextual fieldwork notes point to possible benefits involving rapid access to transferred funds and possible barriers involving phone control, digital capability, network reliability, agent liquidity, and trust; these notes are not treated as independently verified qualitative findings.

The appropriate conclusion is therefore cautious. Membership in the archived regular-use category accompanies stronger reported recovery outcomes, but the observational design cannot determine whether MFS produced those outcomes, whether more resilient households were more likely to use MFS regularly, or whether both were shaped by unmeasured financial and social capabilities. Stronger causal evidence requires longitudinal measurement, objective transaction histories, clearer temporal ordering, preserved qualitative source records, and experimental or defensible quasi-experimental variation.

## 7. DATA AVAILABILITY

The anonymised household-level dataset and reproducible statistical code may be made available subject to consent, confidentiality, and journal requirements. The retained qualitative archive contains only de-identified fieldwork summaries; source-level transcripts, coded extracts, and translation records are unavailable for sharing or re-audit.

## APPENDIX

### A. STATISTICAL DIAGNOSTICS AND SENSITIVITY RESULTS

Table A1

*Primary and Sensitivity Estimates with 95 percent Confidence Intervals*

*Panel A. Regular MFS use versus non-use*

| Outcome                                 | Overlap-weighted           | Full-sample Adjusted       | 1:1 PSM Sensitivity        | kNN-imputed Overlap-weighted |
|-----------------------------------------|----------------------------|----------------------------|----------------------------|------------------------------|
| Income decline (percentage points)      | -17.42<br>[-18.73, -16.11] | -17.32<br>[-18.72, -15.93] | -18.19<br>[-19.81, -16.56] | -17.29 [-18.51, -16.07]      |
| HFIAS-derived score (points)            | -2.51 [-3.79, -1.22]       | -2.48 [-3.76, -1.20]       | -2.57 [-3.59, -1.54]       | -2.45 [-3.70, -1.20]         |
| Distress asset sale (percentage points) | -14.06<br>[-25.09, -3.04]  | -13.45<br>[-24.30, -2.60]  | -10.13<br>[-25.34, 5.09]   | -12.62 [-22.88, -2.36]       |
| Recovery time (months)                  | -3.88 [-4.63, -3.14]       | -3.84 [-4.62, -3.05]       | -4.32 [-5.50, -3.15]       | -3.83 [-4.55, -3.10]         |

*Panel B. Occasional MFS use versus non-use*

| Outcome                            | Overlap-weighted     | Full-sample Adjusted | 1:1 PSM Sensitivity  | kNN-imputed Overlap-weighted |
|------------------------------------|----------------------|----------------------|----------------------|------------------------------|
| Income decline (percentage points) | -4.34 [-6.44, -2.24] | -4.37 [-6.53, -2.22] | -6.00 [-7.91, -4.09] | -4.30 [-6.40, -2.20]         |
| HFIAS-derived score (points)       | -0.77 [-2.15, 0.62]  | -0.87 [-2.27, 0.52]  | -0.77 [-2.26, 0.72]  | -0.74 [-2.10, 0.62]          |

|                                               |                         |                         |                        |                         |
|-----------------------------------------------|-------------------------|-------------------------|------------------------|-------------------------|
| Distress asset sale<br>(percentage<br>points) | -5.71 [-16.37,<br>4.94] | -6.65<br>[-16.89, 3.59] | 2.57 [-9.55,<br>14.69] | -5.43 [-16.07,<br>5.20] |
| Recovery time<br>(months)                     | -0.96 [-2.04, 0.12]     | -0.81 [-1.83,<br>0.20]  | -1.03 [-2.32,<br>0.25] | -0.97 [-2.05,<br>0.12]  |

*Note:* Entries are point estimates with 95 percent confidence intervals in brackets. Negative estimates indicate more favorable outcomes for the MFS-use group relative to non-users. Income-decline and distress-asset-sale estimates are reported in percentage points; the remaining outcomes use the units shown. The overlap-weighted, full-sample adjusted, and kNN-imputed estimates use union-clustered standard errors. The PSM specification is presented as a sensitivity analysis because maximum post-match absolute standardized mean differences exceeded 0.10. PSM used 1:1 nearest-neighbor matching with replacement, an absolute propensity-score caliper of 0.05, regression adjustment, and union-clustered standard errors. kNN denotes k-nearest-neighbor imputation.

Table A2  
*Gender and Income Interaction tests*

| Contrast              | Modifier               | Outcome                     | Interact<br>-ion | 95% CI        | p    | BH q |
|-----------------------|------------------------|-----------------------------|------------------|---------------|------|------|
| Regular vs non-use    | Female headship        | Income decline (p.p.)       | -0.06            | [-4.13, 4.01] | .976 | .976 |
| Regular vs non-use    | Female headship        | HFIAS-derived score         | -1.66            | [-4.47, 1.16] | .234 | .612 |
| Regular vs non-use    | Female headship        | Distress sale (probability) | 0.09             | [-0.24, 0.41] | .592 | .789 |
| Regular vs non-use    | Female headship        | Recovery time (months)      | 2.91             | [-0.02, 5.83] | .052 | .276 |
| Regular vs non-use    | At/below-median income | Income decline (p.p.)       | 1.41             | [-2.51, 5.33] | .462 | .720 |
| Regular vs non-use    | At/below-median income | HFIAS-derived score         | 0.45             | [-2.35, 3.24] | .743 | .825 |
| Regular vs non-use    | At/below-median income | Distress sale (probability) | 0.10             | [-0.14, 0.35] | .385 | .685 |
| Regular vs non-use    | At/below-median income | Recovery time (months)      | 0.65             | [-1.29, 2.59] | .495 | .720 |
| Occasional vs non-use | Female headship        | Income decline (p.p.)       | 5.77             | [1.50, 10.03] | .011 | .161 |
| Occasional vs non-use | Female headship        | HFIAS-derived score         | -0.42            | [-3.17, 2.33] | .754 | .825 |
| Occasional vs non-use | Female headship        | Distress sale (probability) | 0.21             | [-0.22, 0.63] | .318 | .636 |
| Occasional vs non-use | Female headship        | Recovery time (months)      | 1.30             | [-1.08, 3.68] | .268 | .612 |
| Occasional vs non-use | At/below-median income | Income decline (p.p.)       | -1.19            | [-2.59, 0.21] | .091 | .363 |
| Occasional vs non-use | At/below-median income | HFIAS-derived score         | 0.45             | [-2.78, 3.69] | .773 | .825 |
| Occasional vs non-use | At/below-median income | Distress sale (probability) | 0.19             | [-0.07, 0.45] | .149 | .475 |
| Occasional vs non-use | At/below-median income | Recovery time (months)      | 2.41             | [0.42, 4.40]  | .020 | .161 |

*Note.* Interaction confidence intervals use union-clustered standard errors with 21 clusters. Distress-sale interactions are reported in probability units; other interactions use the units shown. Before outcome-specific missingness, the female-headed subgroup contained 35 non-users, 16 occasional users, and 21 regular users. The at-or-below-median-income subgroup contained 60 non-users, 45 occasional users, and 45 regular users. No interaction remained statistically significant after Benjamini–Hochberg adjustment across 16 tests.

Table A3  
*Complete Exploratory Decomposition for Regular use versus non-use*

| Outcome and Proposed Correlate                | Component | Estimate     | 95% bootstrap CI | Bootstrap p |
|-----------------------------------------------|-----------|--------------|------------------|-------------|
| HFIAS-derived score via consumption smoothing | Indirect  | -0.28 points | [-0.99, 0.79]    | .616        |
| HFIAS-derived score via consumption smoothing | Direct    | -2.23 points | [-4.14, -0.51]   | .012        |
| HFIAS-derived score via consumption smoothing | Total     | -2.51 points | [-3.64, -1.23]   | <.001       |
| Distress asset sale via emergency-cash hours  | Indirect  | 26.92 p.p.   | [9.35, 43.24]    | .005        |
| Distress asset sale via emergency-cash hours  | Direct    | -40.98 p.p.  | [-61.19, -19.62] | <.001       |
| Distress asset sale via emergency-cash hours  | Total     | -14.06 p.p.  | [-23.42, -3.28]  | .012        |

*Note.* Opposite-sign direct and indirect distress-sale components indicate suppression/inconsistent mediation. No percentage mediated is reported. The models are exploratory and associational.

Table A4  
*Hidden-bias and Precision Diagnostics*

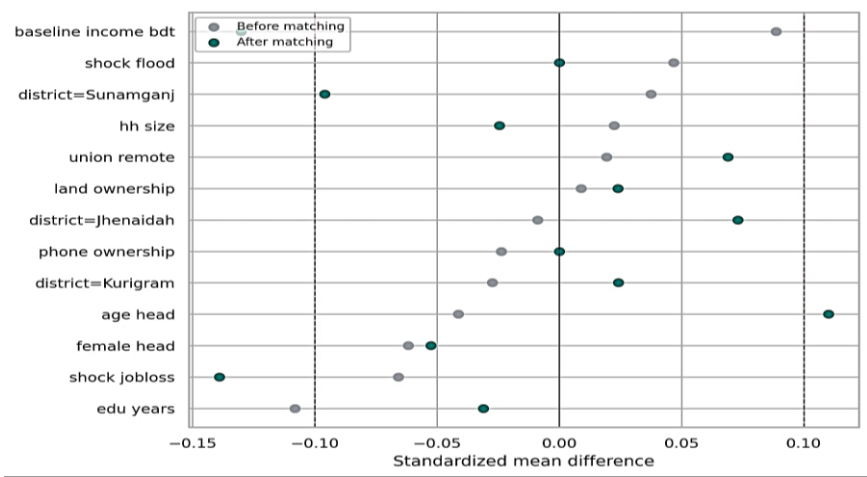
| Diagnostic           | Contrast/outcome                      | Result                | Interpretation                                 |
|----------------------|---------------------------------------|-----------------------|------------------------------------------------|
| Rosenbaum bound      | Regular vs. non-use; distress sale    | Critical Gamma = 1.33 | Sensitive to modest hidden bias                |
| Rosenbaum bound      | Occasional vs. non-use; distress sale | P = .419 at Gamma = 1 | No matched-pair association before hidden bias |
| Oster approximation  | Regular vs. non-use; recovery time    | Delta to zero = 43.77 | Coefficient-stability diagnostic only          |
| Cluster-adjusted MDE | Full pairwise comparisons             | d = 0.41–0.52         | Moderate effects detectable                    |
| Cluster-adjusted MDE | Female-headed subgroups               | d = 0.81–0.99         | Limited power for moderate interactions        |
| Cluster-adjusted MDE | At/below-median-income subgroups      | d = 0.58–0.68         | Limited power for small subgroup differences   |

Table A5  
*Sensitivity of Regular-Use Estimates to Random Perturbation of MFS-Use Category Labels*

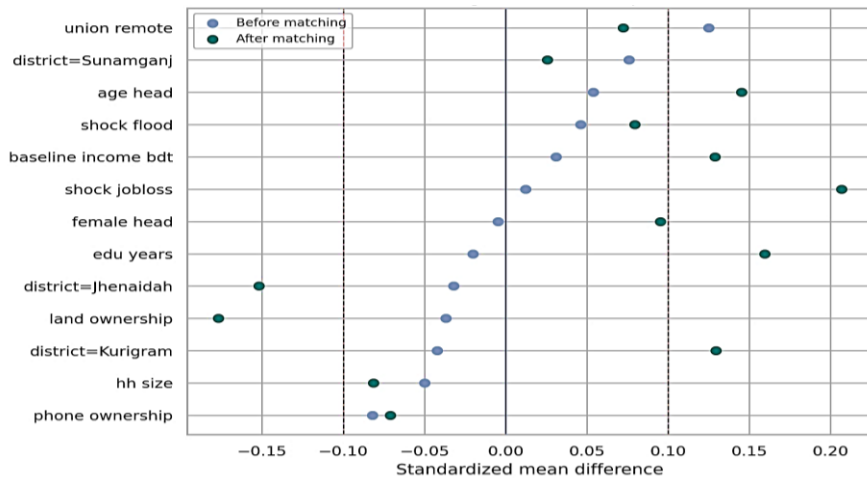
| Labels Exchanged in Each Direction | Income Decline, p.p.    | HFIAS-Derived Score  | Distress Sale, p.p.     | Recovery Time, Months |
|------------------------------------|-------------------------|----------------------|-------------------------|-----------------------|
| 5% (4 per group)                   | -16.87 [-17.19, -16.59] | -2.44 [-2.64, -2.24] | -13.87 [-15.62, -11.99] | -3.77 [-3.91, -3.60]  |
| 10% (9 per group)                  | -16.19 [-16.60, -15.71] | -2.35 [-2.70, -2.07] | -13.39 [-15.90, -10.56] | -3.60 [-3.86, -3.35]  |
| 20% (18 per group)                 | -14.93 [-15.55, -14.34] | -2.22 [-2.62, -1.81] | -12.80 [-16.15, -9.20]  | -3.31 [-3.66, -2.97]  |

*Note:* Values are medians [5<sup>th</sup> – 95<sup>th</sup> simulation percentiles] across 500 repetitions. The percentiles describe variation across random label exchanges and are not sampling confidence intervals. Propensity scores and outcome models were re-estimated in each repetition. This analysis assesses category-label perturbation and does not constitute an alternative-threshold test.

**Figure A1.**  
**Covariate Balance under 1:1 PSM with Replacement: Regular use Versus non-use.**



**Figure A2.**  
**Covariate Balance under 1:1 PSM with Replacement: Occasional use versus non-use.**



**Funding** The authors affirm that the study was completed without financial sponsorships that might compromise, or appear to compromise, the impartiality of the research reported in this article.

**Declarations**

**Conflict of Interest** The authors state that they have no financial, professional, or personal affiliations or relationships that might compromise, or appear to compromise, the impartiality of the research reported in this article.

## REFERENCES

- Abiona, O., & Koppensteiner, M. F. (2022). Financial inclusion, shocks, and poverty: Evidence from the expansion of mobile money in Tanzania. *Journal of Human Resources*, 57(2), 435–464. <https://doi.org/10.3368/jhr.57.2.1018-9796R1>.
- Aker, J. C., & Mbiti, I. M. (2010). Mobile phones and economic development in Africa. *Journal of Economic Perspectives*, 24(3), 207–232. <https://doi.org/10.1257/jep.24.3.207>.
- Austin, P. C. (2011). An introduction to propensity score methods for reducing the effects of confounding in observational studies. *Multivariate Behavioral Research*, 46(3), 399–424. <https://doi.org/10.1080/00273171.2011.568786>.
- Bangladesh Bank. (n.d.). Payment systems. Retrieved August 10, 2026, from <https://www.bb.org.bd/en/index.php/financialsystems/paysystems>.
- Bangladesh Bank. (2022). *Bangladesh mobile financial services (MFS) regulations, 2022*. <https://www.bb.org.bd/mediaroom/circulars/psd/feb152022psd04e.pdf>.
- Barrett, C. B., & Constan, M. A. (2014). Toward a theory of resilience for international development applications. *Proceedings of the National Academy of Sciences*, 111(40), 14625–14630. <https://doi.org/10.1073/pnas.1320880111>.
- Benjamini, Y., & Hochberg, Y. (1995). Controlling the false discovery rate: A practical and powerful approach to multiple testing. *Journal of the Royal Statistical Society: Series B (Methodological)*, 57(1), 289–300. <https://doi.org/10.1111/j.2517-6161.1995.tb02031.x>.
- Blumenstock, J. E., Eagle, N., & Fafchamps, M. (2016). Airtime transfers and mobile communications: Evidence in the aftermath of natural disasters. *Journal of Development Economics*, 120, 157–181. <https://doi.org/10.1016/j.jdeveco.2016.01.003>.
- Demirgüç-Kunt, A., Klapper, L., Singer, D., & Ansar, S. (2022). *The Global Findex Database 2021: Financial inclusion, digital payments, and resilience in the age of COVID-19*. World Bank. <https://doi.org/10.1596/978-1-4648-1897-4>.
- Jack, W., & Suri, T. (2014). Risk sharing and transactions costs: Evidence from Kenya's mobile money revolution. *American Economic Review*, 104(1), 183–223. <https://doi.org/10.1257/aer.104.1.183>.
- Lee, J. N., Morduch, J., Ravindran, S., Shonchoy, A., & Zaman, H. (2021). Poverty and migration in the digital age: Experimental evidence on mobile banking in Bangladesh. *American Economic Journal: Applied Economics*, 13(1), 38–71. <https://doi.org/10.1257/app.20190067>.
- Li, F., Morgan, K. L., & Zaslavsky, A. M. (2018). Balancing covariates via propensity score weighting. *Journal of the American Statistical Association*, 113(521), 390–400. <https://doi.org/10.1080/01621459.2016.1260466>.
- Morduch, J. (1995). Income smoothing and consumption smoothing. *Journal of Economic Perspectives*, 9(3), 103–114. <https://doi.org/10.1257/jep.9.3.103>.
- Munyegera, G. K., & Matsumoto, T. (2016). Mobile money, remittances, and household welfare: Panel evidence from rural Uganda. *World Development*, 79, 127–137. <https://doi.org/10.1016/j.worlddev.2015.11.006>.
- Murshid, K. A. S., Khandker, S., Ali, K. S., Samad, H., & Hossain, M. (2020). *Impact of mobile financial services in Bangladesh: The case of bKash*. Bangladesh Institute of Development Studies. <https://www.bids.org.bd/page/researches?rid=200>.
- Oster, E. (2019). Unobservable selection and coefficient stability: Theory and evidence. *Journal of Business & Economic Statistics*, 37(2), 187–204. <https://doi.org/10.1080/07350015.2016.1227711>.

- Riley, E. (2018). Mobile money and risk sharing against village shocks. *Journal of Development Economics*, 135, 43–58. <https://doi.org/10.1016/j.jdeveco.2018.06.015>
- Rosenbaum, P. R. (2002). *Observational studies* (2nd ed.). Springer. <https://doi.org/10.1007/978-1-4757-3692-2>.
- Suri, T., & Jack, W. (2016). The long-run poverty and gender impacts of mobile money. *Science*, 354(6317), 1288–1292. <https://doi.org/10.1126/science.aah5309>.
- Yao, B., Shanoyan, A., Schwab, B., & Amanor-Boadu, V. (2023). The role of mobile money in household resilience: Evidence from Kenya. *World Development*, 165, 106198. <https://doi.org/10.1016/j.worlddev.2023.106198>.