Pre-analysis plan

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1 Trial Title

If you build it, will they come? Incentivizing the Adoption of Digital Financial Services in Niger

2 Country

Niger

3 Status

Ongoing

4 Keywords

Mobile money, financial incentive, technology adoption, remittances, randomized controlled trial, Niger

5 Abstract

Remittances and transfers are crucial for households in rural Niger, serving as tools for consumption smoothing and risk management in the absence of formal financial infrastructure. Mobile money, a non-bank digital payment platform, has the potential to lower transaction costs and improve the speed, security, and reliability of transfers. Yet, despite strong demand for affordable money transfer mechanisms, mobile money adoption in Niger remains among the lowest in Sub-Saharan Africa. We conduct a randomized controlled trial with 978 households across 61 villages, testing the impact of a simple financial incentive, paired with information and reminders, on mobile money adoption and use. We complement this with detailed data on agent networks to explore how local supply-side constraints impact adoption. We hypothesize that incentivized adoption will lead to sustained usage, greater remittance inflows, and ultimately improve household resilience and welfare.

6 Trial Start Date

February, 2024

7 Intervention Start Date

May 2025

8 Intervention End Date

July 2025

9 Trial End Date

January 2026

10 Outcomes

This pre-analysis plan was finalized prior to the availability of any outcome data related to our main financial incentive intervention. The implementation of the alternative incentive schemes as a randomized controlled trial (RCT) enables the identification of causal effects of these incentives on uptake and usage, and ultimately, their impact on household welfare. For dispersed outcomes, we will standardize and/or winsorize them at 1% level. The analysis will include the following outcomes:

10.1 Household outcomes

10.1.1 Mobile money adoption (extensive margin)

- Has the respondent heard of mobile money?
- Does the respondent or any household member have a mobile money account?
- Knowledge about mobile money
  - Transfer costs (for 10,000 CFA transfer)
  - Delivery of transfer
  - Functionalities of mobile money

10.1.2 Mobile money usage (intensive margin)

- Did the respondent use mobile money at least once in their life?
- Did the respondent use mobile money in the last three months?
- How many mobile money transfers has the respondent received in the last three months?
- How many mobile money transfers has the respondent sent in the last three months?
- Which functionalities of mobile money has the respondent used?
- Did the respondent use the mobile money account to save money?
- How much money does the respondent normally save in the mobile money account?
- How was the respondent's experience with mobile money?
- How was the respondent's experience with mobile money agent(s)?

10.1.3 Migration and transfers

- Has any household members migrated (inter-)nationally in the last twelve months?
- What were the main destinations of the migrating household members?
- Did the household receive transfers from someone outside of the village in the last twelve months?
- How many transfers has the household received in the last twelve months? (categorical)
- What was the main purpose of the last transfer received?
- Which transfer channels was used for the last transfer received?
- What were the problems with using the respective transfer channel?
- Did the household send any transfer to someone outside of the village in the last twelve months?
- How many transfers has the household sent in the last twelve months? (categorical)
- Which transfer channel was used for the last transfer received?
- Which is the preferred transfer channel?

10.1.4 Household activities, consumption and well-being

- What are the main income sources for the household?
- How much income did the household generate in the last week?
- Household assets
  - Ownership
  - Quantity
- Household expenditures in the last week
  - Food
  - Education
  - Health
  - Ceremonies/celebrations
  - Phone/airtime/battery charging
- Subjective well-being of own household (measured on a Likert scale ranging from 1 (poor) to 5 (rich))
- Subjective well-being of neighbors (measured on a Likert scale ranging from 1 (poor) to 5 (rich))
- Food insecurity
  - In the last twelve months, were there any months when the household did not have enough food to meet the household's needs?
  - In which months did the household not have enough food to meet the household's needs?

10.1.5 Phone usage

- Did the respondent use a mobile phone/smartphone in the last twelve months?
- How many phones does the household own in total?
- Which mobile network operator does the respondent or any household member use?

10.1.6 Shocks and health expenditures

- Which types of shocks did the household face in the last twelve months?
- What were the household's strategies to cope with the shock(s)?
- In the last twelve months, has any member of the household needed medical treatment, but was unable to get it because of lack of money?
- In case of emergency, how many days would it take to get help from relatives/friends?

11 Experimental Design

11.1 Sampling and data collection

Our study takes place in the Dosso region in Niger. Our sample of households is drawn from 61 villages located in nine communes in the departments of Dogon-Doutchi and Tibiri. The villages have been randomly chosen, but we had to avoid insecure villages while constituting the sample. The household baseline survey was conducted in February 2024 with 978 households. We recorded GIS locations of villages and households. We also implemented a phone survey in September 2024 interviewing the same households to assess the impact of a small randomized information treatment embedded in the baseline survey (see below for further details). The follow-up survey will take place in August 2025.

We also collect data on agents and agent networks to explore how local supply-side constraints impact adoption of mobile money. Our sample of agents was obtained through a listing of mobile money and airtime agents in 38 markets in the departments of Dogon-Doutchi and Tibiri. The markets were identified as the key markets associated with the villages from our household sample. The agent baseline survey was carried out in August 2024 and it was conducted with 190 agents from 36 markets. Out of the 190 agents, 92 are mobile money as well as airtime agents and 98 are only working as airtime agents. We will do an additional follow-up survey with the agents.

11.2 Interventions

As part of our baseline survey, we conducted an information experiment. The intervention involved a two-page flyer that explains mobile money, detailing how cash-in, cash-out, and transfer processes are executed. In addition to the flyer, treated respondents received a verbal explanation of mobile money and its services. To vary the treatment intensity, we randomly selected either 50% or 100% of respondents within a village to receive both the flyer and the verbal explanation.

To encourage the adoption and usage of mobile money, we implemented a financial incentive intervention. Households first received information about mobile money (as during the baseline survey), and were offered an incentive to use the service. The household was offered an incentive of 2,000 CFA (via mobile money), if they received a transfer via mobile money within one month from any person (i.e., a sender). The sender received a financial incentive of 1,000 CFA for making the mobile money transfer to the household. Transfer reception was verified through

the network operator. The control group received no financial incentive.

We added reminder phone calls to test whether reminding households about the offer has an effect on their take-up of the financial incentive offer. We called half of the treated households to thank them for their participation in the last survey and to remind them that they only had two weeks left to receive a mobile money transfer from any person, in order to get the financial incentive. To test whether the continuous engagement with the households regarding our mobile money study had any effect, we also called half of the control households to thank them for their participation in the last survey and to remind them that we would be re-interviewing them shortly. Together, these three components allow us to assess both the independent and combined effects of addressing different barriers to mobile money adoption.

11.3 Randomization procedure

First, we randomly assigned the information treatment for the baseline survey. For this, we grouped communes into three regions: North, Middle, and South. Next, we created strata based on commune group and participation in a previous study on an adult literacy program. Villages were then randomly assigned to receive no information (1/3) or information (2/3). The villages that received information, the treatment intensity was either 50% or 100%. In the 50% treatment intensity villages, the random assignment of the information was stratified by respondents' gender.

Second, we offered the financial incentive in the same 40 villages that received the baseline information treatment (pooling villages that received the 50% and 100% information treatments). The financial incentive was offered to all respondents in the 40 villages. We tested the treatment design in a pilot with a small urban sample beforehand. Taking the baseline information experiment into account, we have villages where all respondents received the information at baseline and during the intervention and the financial incentive (*Full*) and villages where only half of the respondents received the information at baseline, but then all respondents received the information during the intervention and the financial incentive (*Half*).

Third, we randomly assigned households into the phone call component. This household-level randomization was stratified by village and respondents' gender. Taking the reminder into account, we end up with these four groups: (1) a pure control group, (2) a control group that received a phone call, (3) a treatment group that received the information and the incentive, and (4) a treatment group that received the information, incentive and phone call reminder (see also Figure 1 in the Appendix).

11.4 Spillovers

Treated households may share the information or the flyer that is handed out as part of the intervention with other households in the same village or other villages. Information sharing within the same village is not problematic for our identification, because we randomize the treatment at the village level. Since we will estimate intent-to-treat effects, information sharing across treated and control villages may only underestimate the effect of the information. However, we will monitor the extent of information sharing. Since from a policy perspective information sharing is a desired action which should increase the cost-effectiveness of the intervention, we will ask households whether they shared the information about mobile money with other households.

Given the personalized nature of our financial incentive and phone call and the in-person monitoring done by our implementing partner, we do not expect significant spillovers from our financial incentive treatment on control villages. The treatment and control villages are geo-

graphically dispersed, which minimizes the likelihood of interaction between treated and control units. These factors together suggest that any unintended influence of the treatment on control groups is likely to be small.

11.5 Model specification

The primary objective of this analysis is to estimate the impact of the intervention on the outcome variables measured during the follow-up surveys. First, will estimate intention-to-treat (ITT) effects with the following regression:

$$Y_{ivt} = \beta_0 + \beta_1 Treat_v + \theta_s + \epsilon_{ivt} \tag{1}$$

where Y_{ivt} is the post-treatment outcome for individual i in village v measured in survey wave t. $Treat_v$ is the treatment indicator equal to 1 if village v received any treatment (i.e., information about mobile money and a financial incentive to use it). β_1 is the effect of the treatment relative to the control group. θ_s are strata fixed-effects and ϵ_{ivt} is the error term. The treatment assignment took place on the village level, so standard errors are clustered at the village level.

We also estimate the following equation:

$$Y_{ivt} = \beta_0 + \beta_1 Full_v + \beta_2 Half_v + \theta_s + \epsilon_{ivt} \tag{2}$$

where Y_{ivt} is the post-treatment outcome for individual i in village v measured in survey wave t. $Full_v$ equals 1 if the entire village v received information about mobile money at baseline, and $Half_v$ equals 1 if only half the respondents in village v received it. Both *Full* and *Half* villages later received the financial incentive with the information again. β_1 and β_2 are the effects of either the *Full* or *Half* treatment relative to the control group. θ_s are strata fixed-effects and ϵ_{ivt} is the error term. The treatment assignment took place on the village level, so standard errors are again clustered at the village level.

Robustness checks: As a robustness check, we will include respondent- and household-level baseline covariates X_{iv0} (age, gender, whether respondent is household head, education, and whether household has a migrant). The baseline covariates are imbalanced, given that the same respondent could not always be interviewed for consecutive survey waves. We will also estimate an ANCOVA model, where we have the outcome at baseline (Y_{iv0}) for some outcomes related to mobile money adoption and use. Furthermore, we will correct our standard errors due to a small number of clusters in the control group (i.e., 20 clusters).

Further heterogeneity analysis: We will estimate heterogeneous effects based on whether the household has any migrant, and by whether the respondent had heard of mobile money at baseline, had previously used mobile money at baseline, and had a misperception about mobile money transfer fees at baseline. Further, we will explore heterogeneous effects across other dimensions, including whether the household has experienced a recent shock, whether it faces supply side barriers regarding mobile money adoption and usage. While these analyses may be underpowered, we will conduct an ex-post power analysis to assess this. We will also estimate the heterogeneous effects of receiving a reminder call, examining whether it had an additional impact on take-up of the financial incentive and our outcomes of interest.

11.6 Multiple hypothesis testing

Since we estimate a large number of outcomes, there is an increased risk of falsely rejecting the null hypothesis due to multiple comparisons. Therefore, we will control for the False Discovery Rate (FDR) by Benjamini and Hochberg (1995).

12 Was the treatment clustered?

Yes

13 Planned Number of Clusters

61

14 Planned Number of Observations

3,314 observations across three household survey rounds and two agent survey rounds. For the baseline survey 978 respondents from households and 190 mobile money and airtime agents were interviewed. For the households, with one baseline, phone and follow-up survey, the number of observations for a small number of outcomes is 2,934. It is 1,956 for the remaining outcomes. For the agents alone, with one follow-up the number of observations for some outcomes is 380, and 190 for the remaining outcomes.

We will test for differential attrition in the phone and in-person follow-up surveys. If attrition is less than 10 percent and uncorrelated with treatment, we will proceed without making corrections. If attrition rates are greater than 10% and we find evidence of differential attrition by treatment status, we will estimate pairwise Lee bounds for our treatment effects.

14.1 Power analysis

We use our baseline data to perform power calculations as it provides information on the means, standard deviations and intra-cluster correlation of key outcome variables. Our power calculations are for intention-to-treat effects. The MDEs are based on a 95% confidence interval and a power of 80%. The baseline data shows a mean of households using mobile money of 0.02, with a standard deviation of 0.16 and an intra-cluster correlation (ICC) of 0.125. Given our average cluster size of 16 households per cluster and 61 clusters in total, we are powered to detect a minimum effect of 0.06.

15 Was IRB approval obtained?

IRB Name: Social, Behavioral & Educational Research IRB, Tufts University
IRB Approval Date: April 25, 2024
IRB Approval Number: STUDY00004897

Appendix

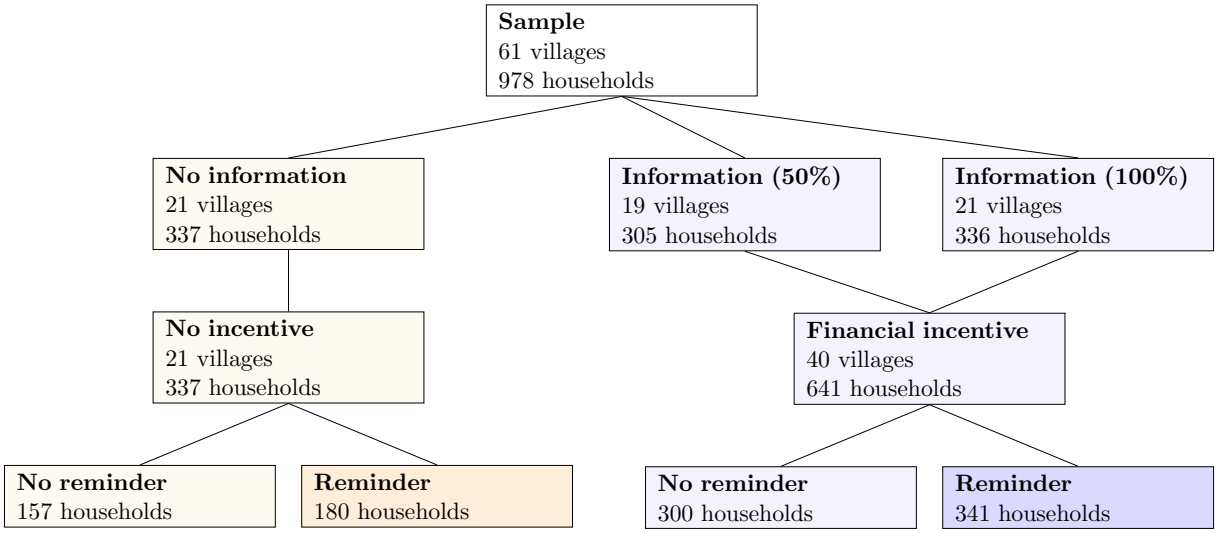

Figure 1: Experimental design