

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

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Abstract

Despite the potential impacts of digital financial services (DFS) on household welfare, adoption rates remain low in some of the world's poorest countries. A key question is whether households face behavioral or information frictions that marginal incentives can overcome, and how these interact with uneven supply. To investigate this, we implemented a randomized controlled trial in Niger to estimate the causal effect of information provision and financial incentives on DFS adoption and subsequent welfare outcomes. We further integrate spatial data on agent networks to assess the role of supply-side constraints as barriers to market penetration. The results indicate that relaxing information asymmetries increases awareness but does not shift adoption behavior, consistent with models in which information is a necessary but not sufficient condition for technology diffusion. By contrast, a modest financial transfer acts as a salient incentive, generating a statistically significant increase in both adoption and usage, and allows households to smooth consumption in the face of shocks. This suggests that both liquidity constraints and present-biased preferences may be binding impediments to diffusion.

Keywords: mobile money, technology adoption, remittances, information, incentives, Niger.

JEL Codes: D14, E42, G21, G51, G53, O16.

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1 Introduction

Digital financial services (DFS) have emerged as a promising tool to expand access to financial services and smooth consumption in low-income settings (Apeti, 2023; Blumenstock et al., 2016; Jack and Suri, 2014; Munyegera and Matsumoto, 2016; Riley, 2018; Suri and Jack, 2016). By enabling users to store and transfer funds via basic mobile phones, DFS can offer a low-cost, secure, and convenient alternative to traditional banking, particularly in areas where formal financial infrastructure is limited or absent (Ahmad et al., 2020; Apeti, 2023; Aron, 2018; Pazarbasioglu et al., 2020; Suri et al., 2023). Yet despite this promise, uptake in the Global South remains uneven, especially in rural areas. This can be due to demand-side frictions, such as information and liquidity constraints, or supply-side factors, such as uneven access to DFS agents.

A growing number of studies have attempted to address these constraints to encourage adoption, either by providing information (Aggarwal et al., 2020; Lee et al., 2021), building the agent infrastructure (Wieser et al., 2019), or both (Batista and Vicente, 2025). In this study, we examine how a light-touch, low-cost, demand-side intervention could affect the adoption of an existing DFS – mobile money – and assess how adoption could influence welfare outcomes. Specifically, we test whether the provision of simple information, as compared to information plus a one-time financial incentive, is sufficient for households to experiment with the technology and use it. The financial incentive is conditional on *receiving* a mobile money transfer, but is unique in that it provides incentives to both parties involved in the transaction. In this manner, we specifically recognize the role of the social network in mobile money adoption.

We study this question in the context of a randomized controlled trial (RCT) with agricultural households in rural Niger. The setting is well-suited in that over 50% of households have at least one seasonal migrant, and hence are accustomed to receiving remittances several times per year. Our study area has modest access to a mobile money agent infrastructure, with an average distance to the nearest agent of four km, ranging between less than one to eleven km.

To encourage adoption, we randomly assigned 61 villages to a treatment or control group. The first intervention was a simple information intervention, wherein households received a flyer and simple instructions on how to use mobile money at the end of a household survey. The second intervention, a financial incentive, provided 4 USD to recipient households who received any money transfer via mobile money within a one-month period, as well as 2 USD to the senders of the transaction. While the information and financial incentive interventions were conducted in the same villages, we staggered their timing, thereby allowing us to measure the impact of information alone and to compare it with information plus a financial incentive. We use rich household survey data before, during and after our interventions to measure the dynamic effects of the interventions over time. We combine these data with transfer data from the mobile money service provider, which enables us to have an observable measure of transactions. Finally, we collected detailed data on mobile money agents, to understand how supply-side factors support or constrain adoption.

We have three main findings. First, despite the fact that mobile money has been present in Niger

for over 16 years and that there is demand for money transfers, adoption and usage remains low. While 35% of households had heard of mobile money, on average, fewer than 3% had used mobile money at baseline. Households who had received money transfers at baseline, primarily used local “hawalas”, which are present in major cities. Second, while a simple and low-cost information intervention was successful in increasing households’ knowledge about DFSs, it was insufficient to spur adoption. Yet when adding on a small financial incentive, this dramatically increased adoption and usage. Treated households were more likely to have a mobile money account by 32 percentage points, representing an increase by 632% compared to the control group. The likelihood of receiving a mobile money transfer rose by 14 percentage points, representing a 195% increase compared to the control group. This also resulted in improvements in households’ ability to smooth expenditures and health spending in the face of shocks. Third, while the density of mobile money agents remains uneven, it has increased substantially. In prior work, Aker et al. (2020) found that access to mobile money agents was a binding constraint in rural areas. During our census and survey, we find that agents were present in 84% of markets in our sample. Furthermore, we do not find heterogeneous impacts of our information and financial incentive interventions based on the distance to agents or agent density.

Our study contributes to two strands of the literature. First, we add to the growing body of evidence on the economic impacts of mobile money, particularly in understudied contexts such as Niger. Existing studies find that mobile money increases the likelihood of migration and remittances (Batista and Vicente, 2025; Lee et al., 2021; Munyegera and Matsumoto, 2016) and enhances households’ ability to share risks, cope with shocks and smooth consumption (Batista and Vicente, 2025; Blumenstock et al., 2016; Jack and Suri, 2014; Riley, 2018; Wieser et al., 2019), and increases savings, consumption, and overall welfare (Aggarwal et al., 2020; Munyegera and Matsumoto, 2016; Suri and Jack, 2016). It has also been linked to improved business performance (Bastian et al., 2018), with particularly strong effects for women who face pressure to share business earnings with their household (Riley, 2024).

Second, our work directly contributes to literature that seeks to expand access to and adoption of DFSs. Many of those studies either provide training or waivers to potential users (Aggarwal et al., 2020; Lee et al., 2021), increase access to mobile money agent infrastructure (Wieser et al., 2019), or address both supply and demand-side factors (Batista and Vicente, 2025). In contrast, our approach uses a light-touch low-cost information intervention, along with a simple incentive for usage, that could be easily scaled. In addition, we focus on whether small financial incentives can motivate individuals to experiment with mobile money, consistent with prior evidence showing that subsidies and incentives can effectively overcome reluctance to try new technologies (Bryan et al., 2014; Cole et al., 2011; Dalton et al., 2024; Meriggi et al., 2021; Riley et al., 2025). Our study is thus closest to that of Riley et al. (2025), who studied the impact of incentives with microfinance clients. By contrast, our work simply targets rural households, and thus may have broader external validity. Furthermore, our study is unique in that we leverage a detailed dataset of mobile money agents. We use these data to estimate heterogeneity with respect to local agent access (e.g., density and distance), to study how supply-side infrastructure constraints or supports adoption.

The remainder of the paper proceeds as follows. Section 2 describes the context of our intervention, and outlines the study design, including data and estimation approach. Section 3 presents our empirical findings, and section 4 concludes.

2 Research Setting and Design

2.1 Research Setting

Niger is the most financially excluded country in the world with only 15% of the adult population using formal financial services (Klapper et al., 2025). At the same time, money transfers are common in Niger: about 18% of the adult population received a domestic remittance and 23.5% received an international remittance in 2024 (Klapper et al., 2025). The official remittances, accounting for 3–4% of GDP, likely understate total volumes given the prevalence of informal mechanisms (World Bank, 2020). Although mobile money has been available since 2009, adoption remains limited. In Niger, only 6% of the adult population has a mobile money account, compared to 44% and 48% account ownership rates in neighboring Burkina Faso and Mali (Klapper et al., 2025). Many rural households rely on private transfer agencies for person-to-person transfers, but these providers concentrate cash-in/cash-out points in semi-urban areas and do not offer account-based services.

Prior research suggests that mobile money remains “underadopted” due to a variety of supply- and demand-side constraints (Aker et al., 2020; Diallo et al., 2023). The first is *information* asymmetries: most households are unaware of mobile money and have never used it, and do not know (or understand) the fee structure or where to find agents. This is further constrained by network effects. Given the low rates of adoption by others, it remains difficult for potential users to engage with the technology. The second barrier is *liquidity* constraints: given high rates of poverty and intra-annual variation in incomes, households report having insufficient “cash on hand” to send money transfers (Appendix Table A11). Finally, the distribution and density of agents remains low despite improvements; households must travel four km on average to reach the nearest agent with distances varying between less than one and eleven km. This is driven, in part, by agents’ own liquidity constraints, weak network coverage, and uncertain demand (Appendix Table A12).

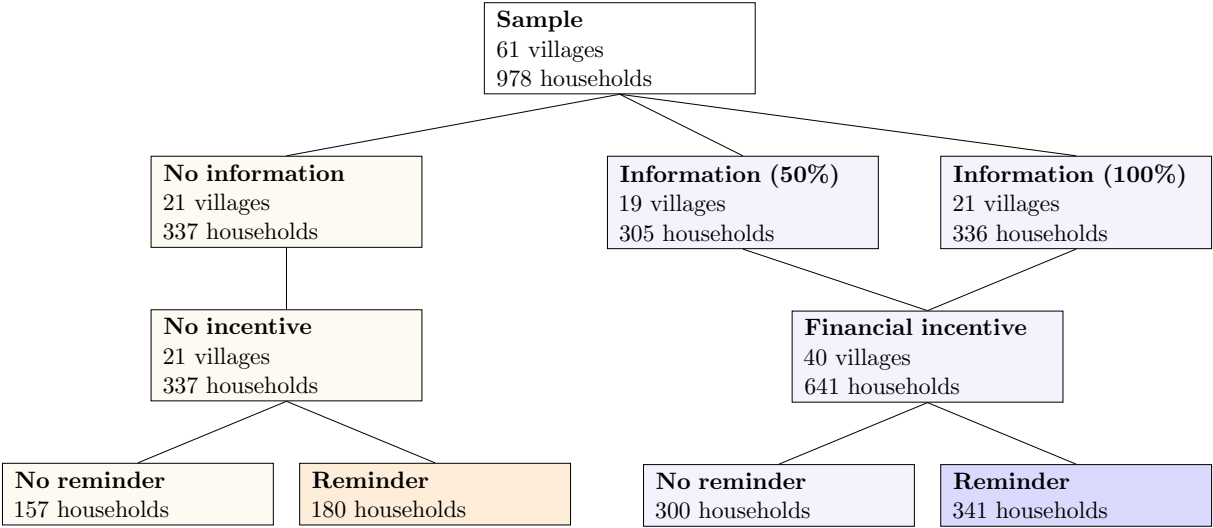
2.2 Interventions

To address the barriers to adoption outlined above, we worked with a research firm, Sahel Consulting Group, to implement two interventions with rural, agricultural households across 61 villages. The first intervention was an information treatment. This treatment involved providing respondents with a flyer and a short explanation of mobile money (e.g., what it is and how it works) at the end of our baseline survey in February 2024 (Appendix A2 and A3).

While knowing about mobile money is a necessary precondition for adoption, mobile money is an experiential network good that requires the participation of (at least) two parties. As a result, a key question is how to encourage households to use the technology for the first time, with the aim of encouraging sustained adoption. Therefore, the second intervention was a one

time, small-scale financial incentive, provided approximately 15 months after the information intervention. After providing the same information again, households were offered an incentive of 2,000 FCFA (4 USD), if they received a money transfer from any person via mobile money during a one-month period. In an effort to incentivize cooperation between the receiver and sender, the intervention also provided the sender with a financial incentive of 1,000 FCFA (2 USD). If the household did not have a mobile phone, and hence could not open a mobile money account, they could list the phone number of a trusted friend or relative to whom money could be sent. We paid out the incentives via mobile money (Appendix A4). Figure 1 visualizes the interventions and experimental design. We also conducted reminder phone calls, and randomly assigned households to a reminder call regarding the financial incentive offer (treatment) or a simple phone call thanking them for their participation (control).

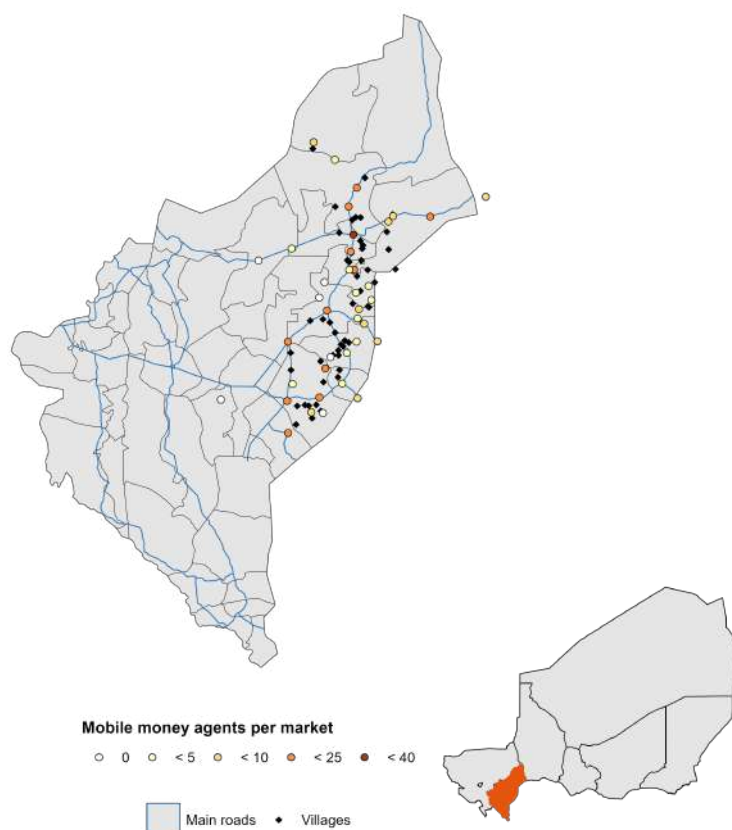
Figure 1. Experimental design



2.3 Experimental Design

Prior to the introduction of the interventions, we identified 61 villages and 38 markets across nine communes in one region (Dosso) of Niger. Among these villages, we first stratified by commune size, gender, and participation in a prior study before randomly assigning them to either treatment (40 villages) or control (21 villages). Within treatment villages we varied the intensity of treatment, so that either 50% or 100% of respondents received the initial information intervention. In the tables, we refer to these as the *Half* or *Full* treatments. In the first stage (i.e., February 2024), only the information intervention was provided in treatment villages. In the second stage (May 2025), both the information and financial incentive were provided to all households within treatment villages. As a result, households in *Full* villages received the flyer twice, while households in *Half* villages received it once or twice, depending on their baseline assignment.

Figure 2. Study villages and markets in Dosso



2.4 Why Should Information and Financial Incentives Make a Difference?

Our experimental design targets key barriers to mobile money adoption and use in rural areas – specifically, limited information, low perceived benefits, and the two-sided nature of money transfers, which require both a sender and a receiver to engage with the system.

The provision of information and a financial incentive could affect our outcomes of interest in several ways. First, the information intervention (alone) could enhance understanding of how the service works and reduce information frictions. This should, in theory, increase households' awareness of the financial service and potentially generate interest in opening a mobile money account. If information is the binding constraint, this will also increase adoption and usage.

If information is insufficient to generate adoption, then the financial incentive could affect adoption in one of three ways. First, it could serve as a simple income transfer, offsetting transaction costs associated with mobile money or liquidity constraints. Second, it could incentivize households to test the technology and engage their social network. Third, the financial incentive could make the information provided more salient, thereby boosting adoption.

Beyond the effects on adoption, the interventions could affect downstream outcomes. By lowering transfer costs and improving speed and safety, mobile money should strengthen informal risk sharing and consumption smoothing, yielding short-run welfare gains for households, especially when exposed to shocks (Ahmed and Cowan, 2021; Batista and Vicente, 2025; Jack and Suri,

2014; Munyegera and Matsumoto, 2016; Riley, 2018; Suri et al., 2012). All of these effects may be moderated by agent networks (Aker et al., 2020; Riley, 2018).

2.5 Data and Estimation Strategy

The data from this paper come from three primary sources. First, we conducted several rounds of household surveys, either in-person or via phone. Second, we did a census and a detailed survey of mobile money and mobile phone agents in markets located near our sample villages. Third, we obtained data on the transfers received by households in our sample. Figure 3 displays the timing of the surveys together with the interventions.

2.5.1 Household Survey Data

We collected four rounds of household surveys over a two-year period. In February 2024, we conducted the baseline survey, measuring key demographic, socio-economic, and income data, as well as patterns of mobile money adoption and usage. During this baseline, we also implemented the information treatment in targeted villages. In order to measure the short-term impacts of the information intervention, we conducted a household phone survey in September 2024, approximately seven months after the baseline survey, primarily measuring impacts on mobile money adoption and usage (Midline survey I). In May 2025, we conducted our third household survey, at the end of which we implemented our financial incentive intervention. Prior to making the financial incentive offer, we measured a variety of mobile money-related outcomes. We then contacted a random subset of households via phone in June 2025, primarily to remind treated households of the offer.¹ Finally, the Endline survey was conducted in August 2025. The survey was predominantly in person, but four villages were surveyed via phone due to security concerns. While attrition differed by survey round, it was not differential by treatment status, nor conclusively different by baseline covariates (Appendix Table A1).

Figure 3. Timeline of surveys and interventions

	Data Collection	Interventions
February 2024	Baseline survey + Agent census	Information
August 2024	Agent Baseline survey	
September 2024	Midline survey I (by phone)	
May 2025	Midline survey II	Financial Incentive (+ Information)
June 2025		Reminder (by phone)
July 2025	Endline Airtel tranfer records	
August 2025	Endline survey (partially by phone)	

Notes: For the endline survey, households from 4 villages were surveyed via phone due to security concerns.

¹The reminder phone calls were cross-randomized by treatment and control. Households in treated villages received a reminder about the offer, whereby households in control villages received a simple phone call to ask if they had any questions.

2.5.2 Agent Data

In February 2024, we identified approximately 38 markets in the same area as the 61 villages, and conducted a census of both mobile money and airtime agents. Six months later in August 2025, we conducted a survey of agents to provide market-level measures and agent-level characteristics. We surveyed airtime agents, who provide SIM cards, airtime recharge and related services, and mobile money agents. All mobile money agents are also simultaneously airtime agents.

2.5.3 Mobile Money Transaction Data

As part of our intervention, Airtel (the main mobile operator) granted us access to a platform that allowed us to verify whether respondents had received any mobile money transactions over a three-month period. Through this platform, we were able to confirm whether a transaction had occurred and retrieve the date of the last transaction. This information was used to identify participants who had followed through with our incentive mechanism, enabling us to process payouts to the correct mobile money accounts. We then contacted the qualifying respondents to collect details about the person who sent them the transfer, so that incentive payments could be issued to the sender.

2.5.4 Summary Statistics and Pre-intervention Balance

Table 1 reports baseline balance in household covariates by treatment status. Column (1) lists control-group means (standard deviations in parentheses). To test for balance across groups, columns 2–4 present the coefficient estimates and standard errors of the difference between the baseline mean in the *Any treatment* (i.e., both *Half* or *Full* treatment villages), *Full*, and *Half* treatment villages, respectively. All specifications include strata fixed effects, and standard errors are clustered at the village level.

Panel A summarizes baseline sociodemographics. On average, 60% of respondents are female, the mean age is 46, and about half report any formal education. Households size is on average 9.3, of whom 5.3 are adults. Households report less than four income sources on average. A majority experienced food shortages in the past year and reported at least one shock in the last twelve months. Panel B examines households' migration and money transfer patterns. At baseline, 64% of households reported having a temporary migrant in the household, with an average of 1.5 migrants per household. Receiving transfers is more common than sending: 70% received any transfer in the past year, as compared to 27% who sent a transfer. Households primarily received transfers via local agencies (e.g., BNIF Afuwa, Al Izza, NITA), with smaller shares via bus, friends or relatives, and mobile money. Phone ownership is high (92%), but awareness and adoption of mobile money are low: 35% have heard of mobile money, only 3% have ever used it, and just 1% report any mobile money account in the household. Overall, differences across treatments are small, indicating that randomization successfully balanced on observable characteristics. As a robustness check, we control for variables that are statistically significant at baseline, namely age and income sources.

Table 1. Balance: Treatment Groups vs. Control Group

	Control Mean (SD) (1)	Any – Control Coeff. (SE) (2)	Full – Control Coeff. (SE) (3)	Half – Control Coeff. (SE) (4)
<i>Panel A: Sociodemographic characteristics</i>				
Female	0.60 (0.49)	-0.06** (0.03)	0.00 (0.03)	-0.07*** (0.02)
Age	45.77 (14.86)	1.46 (1.31)	-1.62 (1.26)	3.25** (1.45)
Any formal education	0.55 (0.50)	0.08 (0.08)	-0.01 (0.08)	0.10 (0.07)
Household size	9.35 (4.35)	0.02 (0.37)	-0.27 (0.35)	0.32 (0.43)
Number of adults	5.25 (2.69)	0.07 (0.22)	-0.08 (0.19)	0.16 (0.25)
Number of income sources	3.60 (1.11)	-0.09 (0.09)	-0.18** (0.09)	0.09 (0.09)
Had lack of food	0.88 (0.32)	-0.00 (0.02)	0.03 (0.02)	-0.03 (0.02)
Experienced shock	0.99 (0.11)	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)
<i>Panel B: Migrants and transfers</i>				
Short-term migrant	0.64 (0.48)	0.02 (0.03)	-0.01 (0.04)	0.04 (0.04)
Number of migrants	1.15 (1.25)	0.07 (0.09)	0.03 (0.09)	0.04 (0.08)
Household sent transfer	0.27 (0.44)	0.03 (0.03)	0.02 (0.03)	0.01 (0.03)
Household received transfer	0.70 (0.46)	0.02 (0.04)	-0.02 (0.04)	0.04 (0.04)
... via BNIF Afuwa, Al Izza	0.55 (0.50)	-0.03 (0.05)	-0.00 (0.04)	-0.02 (0.04)
... via bus	0.02 (0.15)	0.01 (0.01)	0.00 (0.01)	0.01 (0.01)
... via friend/family member	0.23 (0.42)	0.03 (0.03)	0.01 (0.03)	0.02 (0.03)
... via m-money	0.02 (0.13)	-0.00 (0.01)	-0.01 (0.01)	0.01 (0.01)

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Balance: Treatment Groups vs. Control Group (continued)

	Control Mean (SD) (1)	Any – Control Coeff. (SE) (2)	Full – Control Coeff. (SE) (3)	Half – Control Coeff. (SE) (4)
<i>Panel C: Mobile money usage</i>				
Household owns phone	0.92 (0.27)	0.00 (0.02)	-0.02 (0.02)	0.02 (0.02)
Heared of m-money	0.35 (0.48)	0.02 (0.03)	-0.04 (0.04)	0.07* (0.04)
Used m-money	0.03 (0.16)	0.00 (0.01)	-0.01 (0.01)	0.01 (0.01)
Has m-money account	0.01 (0.08)	0.00 (0.00)	0.00 (0.00)	-0.00 (0.01)

Notes: Column (1) reports control-group means (standard deviations in parentheses). Columns (2)–(4) report treatment–control differences from separate OLS regressions of each variable on an indicator for: (2) *Any treatment* (*Half* or *Full*), (3) *Full*, and (4) *Half*. All specifications include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and shown in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 shows the baseline characteristics of the agents in our sample. Mobile money and airtime agents are relatively young, with an average age of around 36 years, 73% have a formal education, and nearly nine years of work experience as an agent. Mobile money agents have, on average, 3.5 years of experience specifically in mobile money services. Mobile money agents tend to be slightly younger than airtime agents and are more likely to operate in multiple markets (3.78 vs. 2.45 markets on average). Only 9% of all agents have access to a bank account, with mobile money agents slightly more likely to have one than airtime agents (12% vs. 6%). Most agents rely on simple setups such as kiosks, tables, or umbrellas, with mobile money agents far more likely to use such branding (91%). The majority of agents report having chosen their business location themselves.

Table 2. Agent Characteristics

	All agents		Airtime agents		M-money agents	
	Mean	(SD)	Mean	(SD)	Mean	(SD)
	(1)	(2)	(3)	(4)	(5)	(6)
Age (years)	35.78	(9.88)	37.87	(10.56)	33.47	(8.55)
Formal education	0.73	(0.44)	0.61	(0.49)	0.87	(0.34)
Experience agent (years)	8.66	(5.74)	8.24	(6.06)	9.12	(5.36)
Experience m-money agent (years)	3.49	(2.74)	1.50	(2.12)	3.53	(2.74)
Has bank account	0.09	(0.29)	0.06	(0.24)	0.12	(0.33)
Has workers in m-money business	0.54	(0.50)	0.50	(0.50)	0.58	(0.50)
Markets covered	3.08	(2.13)	2.45	(1.78)	3.78	(2.28)
Has kiosk/booth/umbrella	0.73	(0.45)	0.56	(0.50)	0.91	(0.29)
Owns kiosk	0.46	(0.50)	0.47	(0.50)	0.46	(0.50)
Chose kiosk location	0.88	(0.32)	0.91	(0.29)	0.87	(0.34)

Notes: N = 190 agents (100 airtime, 90 mobile money). Two agents among the airtime agents are also mobile money agents, but were inactive at the time of the survey because they never get clients.

2.6 Estimation Strategy

To estimate the impact of our interventions on mobile money adoption and usage, we use several specifications. First, we estimate the impact of the information treatment on households' behavior in the short and medium-term, and test how this varies with treatment intensity. Next, we estimate the effect of the bundled financial incentive and information treatment, and also whether the initial treatment intensity assignment persists. The regression specifications appear below.

Information Treatment (I)

$$Y_{ivt} = \beta_0 + \beta_1 Treat_v^I + \theta_s + \epsilon_{ivt} , \quad (1)$$

where Y_{ivt} is outcome for individual i in village v , measured in survey wave t . $Treat_v^I$ is the treatment indicator equal to 1 if village v was assigned to the information treatment in February 2024. θ_s are strata fixed-effects, ϵ_{ivt} is the error term. Since the treatment assignment took place at the village level, standard errors are clustered at the village level. t indexes the survey wave, either the first or second midline conducted in September 2024 or May 2025, respectively. Our key coefficient of interest is β_1 , which measures the intent-to-treat (ITT) effect of the information intervention relative to the control. To improve precision, we control for baseline outcomes, when available.

To test whether treatment effect varied by information intensity, we estimate the following specification:

$$Y_{ivt} = \beta_0 + \beta_1 Treat_v^{I,Full} + \beta_2 Treat_v^{I,Half} + \beta_3 Treat_v^{I,Half} \times I_{iv} + \theta_s + \epsilon_{ivt} , \quad (2)$$

where $Treat_v^{I,Full}$ is the treatment indicator equal to 1 if all households in a given village v were assigned to the information treatment, and $Treat_v^{I,Half}$ is equal to one if only half of the

respondents in a given village were assigned to the information treatment. I_{iv} is equal to one if a given individual in a *Half* village was assigned to receive the information, so that $Treat_v^{I,Half} \times I_{iv}$ is the interaction between the two. Thus, β_2 measures spillover effects within *Half* villages on control households.

Financial Incentive and Information Treatment (F)

$$Y_{ivt} = \delta_0 + \delta_1 Treat_v^F + \theta_s + \nu_{ivt} , \quad (3)$$

where $Treat_v^F$ is the treatment indicator equal to 1 if village v received the financial incentive and information treatment. δ_1 is the effect of the treatment relative to the control group. ν_{ivt} is the error term. t indexes alternatively the Endline Airtel transfer records (2 months post treatment) and the Endline survey (3 months post treatment). As the reminder call could have also affected our results, we control for the reminder call and its interaction. We provide power calculations in Appendix section C.

3 Results

3.1 Effects of the Information (Only) Treatment

3.1.1 Did Households Receive the Information Treatment?

Table 3 tests whether the initial information treatment was implemented as intended. Column (1) shows that households in treated villages were 57 percentage points more like to report receiving a flyer (Column 1) than those in the control, and more likely to keep it (Column 4). The non-zero means in control villages are likely the result of measurement error, rather than spillovers across villages. Overall, 39% of households in treated villages shared the information that they received (Column 7).

Comparing intensities, *Full* villages had consistently higher compliance rates than *Half* villages (Columns 2, 5, and 8), which is by design. Households who were not assigned to receive the information in *Half* villages were also significantly more likely to have received or kept the flyer and share the information relative to the control. The effects are small, as expected, but still large in magnitude, representing 80% of the control mean. Taken together, the results point to high rates of compliance when information is provided and substantial spillovers. Since we analyze intent-to-treat effects, information sharing across treated and control villages would underestimate treatment effects.

3.1.2 Did the Information Treatment Induce Mobile Money Adoption?

Table 4 reports the intent-to-treat effects of the information intervention on three outcomes across two survey rounds. In the short term, households in any treatment village increased their awareness of mobile money by 13 percentage points (Panel A, Column 1), with larger effects under *Full* treatment as compared with *Half* treatment (Column 2). The effects on behavior are large yet imprecise: while treated households were more likely to open an account (Panel

Table 3. Compliance with mobile money information: Midline survey I

	Information								
	Received flyer			Kept flyer			Shared info		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any treat	0.57*** (0.05)			0.57*** (0.04)			0.35*** (0.03)		
Full		0.72*** (0.05)	0.72*** (0.05)		0.73*** (0.05)	0.73*** (0.05)		0.41*** (0.04)	0.41*** (0.04)
Half		0.41*** (0.04)	0.11** (0.05)		0.40*** (0.04)	0.10** (0.04)		0.27*** (0.04)	0.10** (0.04)
Half × Info			0.58*** (0.05)			0.60*** (0.05)			0.34*** (0.06)
Observations	941	941	941	941	941	941	941	941	941
Adjusted R ²	0.30	0.36	0.46	0.30	0.36	0.47	0.15	0.17	0.21
Control mean	0.14	0.14	0.14	0.13	0.13	0.13	0.04	0.04	0.04
Half = Full		0.00			0.00			0.00	

Notes: ITT estimates. Outcomes are dummies: received flyer (Columns 1–3), kept flyer (Columns 4–6), and shared information (Columns 7–9). Villages were randomly assigned to *Full* where all sampled households received the flyer, *Half* where 50% of sampled households randomly received the flyer, or *Control*. *Any treatment* combines *Full* and *Half* villages. The “spillover” specification contains indicators for *Full* villages, *Half* villages without information assignment (captures spillovers to non-assigned households), and *Half × Info* for households with information assignment in *Half* villages. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. *Half = Full* reports the p -value of equality of coefficients. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A, Column 4) and use mobile money since baseline (Panel A, Column 7), none of these effects are statistically significant at conventional levels. Households in *Full* villages were 6 percentage points more likely to open a mobile money account, with a statistically significant impact at the 10 percent level. This suggests that there may be some non-monotonic effects of account opening.

Panel B measures the impact of the information intervention 15 months later, in May 2025. While the effect on awareness is larger (Column 1), the earlier impacts on adoption and usage dissipate: while the control mean hovers at 3 percent, the coefficients on the treatment indicators are not distinguishable from zero (Columns 4 and 7, respectively).

Table 4. Effects of the information on mobile money adoption: Midline surveys I and II

	Mobile Money								
	Heard of			Household account			Used since last survey		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Panel A: Midline Survey I</i>									
Any treat	0.13*			0.05			0.05		
	(0.07)			(0.03)			(0.04)		
Full		0.18**	0.18**		0.06*	0.06*		0.05	0.05
		(0.08)	(0.08)		(0.04)	(0.04)		(0.04)	(0.04)
Half		0.07	0.02		0.04	0.04		0.06	0.06
		(0.08)	(0.08)		(0.04)	(0.04)		(0.05)	(0.05)
Half × Info			0.11**			0.00			-0.00
			(0.04)			(0.02)			(0.02)
Observations	941	941	941	941	941	941	941	941	941
Adjusted R ²	0.13	0.13	0.14	0.05	0.05	0.05	0.04	0.04	0.04
Control mean	0.53	0.53	0.53	0.04	0.04	0.04	0.06	0.06	0.06
Half = Full		0.11			0.68			0.85	
<i>Panel B: Midline Survey II</i>									
Any treat	0.20***			0.00			0.00		
	(0.04)			(0.01)			(0.01)		
Full		0.22***	0.22***		-0.01	-0.01		0.00	0.00
		(0.04)	(0.04)		(0.01)	(0.01)		(0.01)	(0.01)
Half		0.18***	0.19***		0.01	-0.01		0.00	0.01
		(0.05)	(0.07)		(0.02)	(0.01)		(0.02)	(0.02)
Half × Info			-0.02			0.02			-0.02
			(0.06)			(0.02)			(0.01)
Observations	966	966	966	966	966	966	966	966	966
Adjusted R ²	0.06	0.06	0.06	0.00	0.00	0.00	0.01	0.01	0.01
Control mean	0.52	0.52	0.52	0.03	0.03	0.03	0.03	0.03	0.03
Half = Full		0.42			0.42			0.92	

Notes: ITT estimates using data from Midline I (7 months after the information intervention; Panel A) and Midline II (15 months after the intervention; Panel B). Outcomes are dummies: heard of mobile money (Columns 1–3), household has a mobile money account (Columns 4–6), and has used mobile money since the last survey (Columns 7–9). Villages were randomly assigned to *Full* where all sampled households received the flyer, *Half* where 50% of sampled households randomly received the flyer, or *Control*. *Any treatment* combines *Full* and *Half* villages. The “spillover” specification contains indicators for *Full* villages, *Half* villages without information assignment (captures spillovers to non-assigned households), and *Half* × *Info* for households with information assignment in *Half* villages. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. *Half* = *Full* reports the *p*-value of equality of coefficients. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.2 Effects of the Additional Financial Incentive

3.2.1 Did Households Receive the Information and Incentive?

Similar to the first intervention, endline compliance with the information intervention was essentially universal. Compliance in the treatment villages was 95% (Column 1, with only 2% of households in the control group reporting that they had received the flyer). 94% of treated households kept the flyer (Column 4), and 83% shared the information with others (Column 7). As expected, since all households in treated villages received the information, *Full* and *Half* villages are nearly identical. Moreover, the *Half* $\times$ *Info* terms are close to zero and insignificant (Columns 3, 6, 9), indicating no persistent differential effect from baseline information treatment assignment. For the financial incentive offer, only 28% of treated households recalled having received the offer as compared with 0% in the control (Columns 10–12). Since we find no evidence for spillover effects, we pool the *Full* and *Half* villages and only report estimates for the outcomes measured after the second intervention using *Any treatment*.

Table 5. Compliance with the second intervention: Endline survey

	Information									Incentive		
	Received flyer			Kept flyer			Shared info			Received incentive		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Any treat	0.93*** (0.01)			0.93*** (0.01)			0.82*** (0.02)			0.28*** (0.04)		
Full		0.93*** (0.02)	0.93*** (0.02)		0.93*** (0.02)	0.93*** (0.02)		0.82*** (0.03)	0.82*** (0.03)		0.32*** (0.05)	0.32*** (0.05)
Half		0.93*** (0.02)	0.94*** (0.02)		0.94*** (0.02)	0.94*** (0.02)		0.83*** (0.02)	0.85*** (0.03)		0.24*** (0.04)	0.25*** (0.05)
Half $\times$ Info			-0.00 (0.03)			-0.00 (0.03)			-0.04 (0.04)			-0.02 (0.04)
Observations	944	944	944	944	944	944	944	944	944	944	944	944
Adjusted R ²	0.84	0.84	0.84	0.84	0.84	0.84	0.62	0.62	0.62	0.18	0.18	0.18
Control mean	0.02	0.02	0.02	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.00	0.00
Half = Full		0.85			0.86			0.78			0.21	

Notes: ITT estimates using endline survey data. Outcomes are dummies: received flyer (Columns 1–3), kept flyer (Columns 4–6), shared information (Columns 7–9), and received the financial incentive (Columns 10–12). Villages were randomly assigned to *Full* where all sampled households received the flyer, *Half* where 50% of sampled households randomly received the flyer, or *Control*. *Any treatment* combines *Full* and *Half* villages. The “spillover” specification contains indicators for *Full* villages, *Half* villages without information assignment at baseline in 2024 (but received information later at the second intervention in 2025), and *Half* $\times$ *Info* for households with information assignment in *Half* villages in both interventions. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. *Half* = *Full* reports the *p*-value of equality of coefficients. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.2.2 Did the Additional Financial Incentive Induce Mobile Money Adoption?

Table 6 shows the impacts of the financial incentive intervention bundled with information on household transfers, using Airtel records, whereas Table 7 shows the impacts on adoption and usage using survey data.

Table 6. Effects of the financial incentive on mobile money adoption: Endline transfer records

	Mobile Money					
	Any transfer		Transfer since offer		Transfer since reminder	
	(1)	(2)	(3)	(4)	(5)	(6)
Any treat	0.10** (0.04)	0.10* (0.06)	0.14*** (0.03)	0.13*** (0.04)	0.10*** (0.02)	0.07** (0.03)
Reminder		0.02 (0.05)		0.01 (0.02)		-0.00 (0.01)
Any treat $\times$ Reminder		0.00 (0.06)		0.02 (0.03)		0.04 (0.03)
Observations	966	966	966	966	966	966
Adjusted R ²	0.05	0.05	0.06	0.06	0.03	0.03
Control mean	0.15	0.14	0.04	0.04	0.02	0.02

Notes: ITT estimates using transfer records provided by Airtel. Outcomes are indicator variables: any mobile money transfer (Columns 1-2), any mobile money transfer made after the financial incentive was offered (Columns 3-4), and any mobile money transfer made after the reminder call (Columns 5-6). *Any treatment* combines *Full* and *Half* villages. We include an indicator for receiving the *Reminder* call and the interaction *Any treatment* $\times$ *Reminder*. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The Airtel transfer records include the date of the last transfer received by our respondents and the name and number of the sender. We used these records to verify eligibility and process incentive payouts to the corresponding accounts. With this generated data set, we estimate treatment effects on mobile money usage. Table 6 shows that the intervention increased transactions. The probability of receiving any transfer increased by 10 percentage points (or around 66-70%; Columns 1–2) and of making a transfer after the incentive was offered in mid-May by 13–14 percentage points, a more than three-fold increase compared to the control mean (Columns 3–4). Most transfers happened shortly after the rollout of the intervention, fewer after the reminder calls. The reminder did not have any additional effect, neither in general nor on the treatment group.

Table 7. Effects of the financial incentive on mobile money adoption: Endline survey

	Mobile Money							
	Heard of		Household account		Ever used		Used since offer	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any treat	0.15*** (0.03)	0.16*** (0.03)	0.32*** (0.04)	0.30*** (0.05)	0.14*** (0.04)	0.18*** (0.04)	0.16*** (0.03)	0.18*** (0.04)
Reminder		-0.01 (0.05)		-0.01 (0.03)		0.05 (0.03)		0.03 (0.03)
Any treat × Reminder		-0.01 (0.05)		0.03 (0.05)		-0.07 (0.05)		-0.04 (0.04)
Observations	944	944	944	944	944	944	944	944
Adjusted R ²	0.06	0.06	0.13	0.13	0.03	0.03	0.05	0.05
Control mean	0.80	0.80	0.05	0.05	0.12	0.09	0.08	0.07

Notes: ITT estimates using endline survey data. Outcomes are dummies: heard of mobile money (Columns 1–2), household has a mobile money account (Columns 3–4), has ever used mobile money (Columns 5–6), and has used mobile money since the financial incentive was offered, so after mid-May 2025 (Columns 7–8). *Any treatment* combines *Full* and *Half* villages. We include an indicator for receiving the *Reminder* call and the interaction *Any treatment* × *Reminder*. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

At endline, ITT estimates also show sizable self-reported adoption rates (Table 7). Assignment to *Any treatment* raised awareness by 15–16 percentage points from the control mean of 80% (Columns 1–2), increased household account ownership by 30–32 percentage points from a control mean of 5% (Columns 3–4), and increased usage by 14–18 percentage points from a control mean of about 10% (Columns 5–6). Usage, specifically since the incentive was offered in mid-May 2025, also rose by 16–18 percentage points (Columns 7–8). The effects for the reminder call and its interaction with treatment are small and statistically insignificant across outcomes. Overall, and in contrast to the information treatment at baseline, the information and financial incentive intervention increased mobile money account ownership and usage substantially.

Our results are robust to using an ANCOVA specification that controls for the baseline value of each outcome (Appendix Table A2). Because baseline adoption of mobile money was very low and exhibited little variation, the baseline measure adds minimal predictive power for follow-up outcomes. Accordingly, our estimates and standard errors are virtually unchanged, and significance levels remain the same.

Next, we look at mobile money use by transaction type: we check whether households received or sent transfers, and if so, whether those were made via mobile money. We also asked respondents to report the average balance kept in their mobile money account (Appendix Table A8). Treated households were more likely to receive a transfer via mobile money by 2–3 percentage points, and slightly more likely to send via mobile money by 1 percentage point. Both transactions had a nearly zero prevalence in the control group. By contrast, there was no detectable change in the probability of receiving or sending any transfer, but treated households reported on average a higher balance on their mobile money account (FCFA 175.42). Again, we find no difference between households that received a reminder call from those that did not.

3.3 Did the Information Increase Knowledge about Mobile Money?

The baseline survey identified information frictions as a key barrier to mobile money use. Accordingly, clarifying what the service does and how to use it should reduce these frictions and improve knowledge about the service.

Seven months after the information intervention (Midline I), the flyer substantially improved knowledge about mobile money functions (Appendix Table A9). Treated households listed 0.32 additional functions relative to a control mean of 0.93 out of 4 functions, and were 15 percentage points more likely to name at least one function. Effects on naming any function are significantly larger in villages where all respondents received the information (*Full* villages). In *Half* villages, average effects are smaller and not significant at conventional levels, but among households assigned to the information treatment within *Half* villages, knowledge increased significantly. Impacts are concentrated on direct recipients, with no statistically significant spillovers. While Table 3 indicates frequent information-sharing, these exchanges did not translate into higher knowledge among untreated households. Three months after the financial incentive and a second exposure to the same information (Endline; Appendix Table A10), treated respondents still named more functions and were 8 percentage points more likely to list at least one, though the control mean was already high at 0.88. We do not find any differential effects for households that received the information twice in the *Half* villages.

3.4 Heterogeneous Effects

Treatment effects may be heterogeneous across household migration status, gender, education, mobile-phone ownership, and baseline beliefs about the costs of transfer modes. It is reasonable to assume that household adoption and usage would vary by these covariates. However, for the information intervention Appendix Tables A3 and A4 show this is not the case. The information intervention had no differential effects along most margins, except for baseline beliefs about costs.

We also assess effect heterogeneity of the financial incentive using both the Airtel transfer records and the endline survey. Whether we look at the occurrence of any transfer or at having received or sent a transfer separately, we find no robust evidence for heterogeneous effects. None of the interaction terms are statistically different from zero. The details of this analysis are shown in the Appendix (Table A5). Repeating the analysis using the endline survey shows a more nuanced picture. Although treatment interaction terms with migrant status and phone ownership are again statistically insignificant across the three outcomes, the effects on account ownership are larger for female respondents, and both account ownership and mobile money use are higher among respondents with any formal education (Table 8). Yet, overall, given the absence of substantial differences in the effect size across groups, we conclude that our intervention seems to have worked for all. This is quite a remarkable result and not what we had expected.

Table 8. Heterogeneous effects of the financial incentive on mobile money adoption: Endline survey

	Mobile Money																	
	Heard of						Household account						Ever used					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Any treat	0.17*** (0.04)	0.13*** (0.04)	0.15*** (0.05)	0.21** (0.09)	0.14*** (0.03)	0.15*** (0.03)	0.33*** (0.05)	0.26*** (0.05)	0.23*** (0.05)	0.23*** (0.08)	0.30*** (0.04)	0.33*** (0.04)	0.12* (0.06)	0.12** (0.05)	0.05 (0.06)	0.22*** (0.06)	0.15*** (0.04)	0.16*** (0.04)
Migrant	0.04 (0.04)						-0.04* (0.02)						-0.06 (0.05)					
Any treat × Migrant	-0.03 (0.05)						-0.01 (0.04)						0.03 (0.06)					
Female		0.02 (0.06)						-0.06 (0.04)						-0.05 (0.05)				
Any treat × Female		0.04 (0.05)						0.10** (0.05)						0.05 (0.05)				
Education			-0.00 (0.05)						-0.05 (0.03)						-0.12** (0.05)			
Any treat × Education			0.01 (0.06)						0.14** (0.06)						0.16** (0.07)			
Phone				0.07 (0.09)						-0.01 (0.05)						0.11*** (0.03)		
Any treat × Phone				-0.06 (0.10)						0.09 (0.09)						-0.08 (0.06)		
More costly vs. agency					-0.03 (0.06)						0.00 (0.03)						-0.01 (0.03)	
Any treat × More costly vs. agency					0.03 (0.06)						0.03 (0.04)						-0.01 (0.04)	
More costly vs. bus						-0.03 (0.06)						0.04 (0.04)						0.12 (0.08)
Any treat × More costly vs. bus						0.04 (0.07)						-0.06 (0.06)						-0.14 (0.09)
Observations	944	944	944	944	944	944	944	944	944	944	944	944	944	944	944	944	944	944
Adjusted R ²	0.06	0.06	0.06	0.06	0.06	0.06	0.13	0.13	0.14	0.13	0.13	0.13	0.03	0.03	0.04	0.03	0.03	0.04
Control × (X = 0)	0.78	0.80	0.80	0.73	0.81	0.80	0.08	0.08	0.06	0.05	0.05	0.05	0.15	0.12	0.17	0.00	0.12	0.10

Notes: ITT estimates using endline survey data. Outcomes are dummies: heard of mobile money (Columns 1–4), household has a mobile money account (Columns 5–8), and has ever used mobile money (Columns 9–12). *Any treatment* combines *Full* and *Half* villages. We interact *Any treatment* with baseline characteristics: household having a migrant, respondent being female, having attained any education, and owning a mobile phone. Control × (X = 0) refers to the mean for control households, for which the covariate used for the heterogeneity analysis equals zero. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.4.1 The Role of Supply-side Constraints

On the supply side, the literature and our survey data identify access to a reliable agent network as an important determinant of mobile money adoption. Therefore, we test whether treatment effects were moderated by supply-side factors. We would expect a positive relationship between the number of agents and awareness, adoption, and use of mobile money, but a negative relationship between distance to the nearest agent and these outcomes. Easier access to agents could facilitate mobile money adoption after receiving the information and following through with the financial incentive.

Appendix Table A6 shows that seven months after the information intervention (Midline I) the relationship between agent density (i.e., the number of agents in the nearest market, or in the three nearest markets together) and mobile money awareness is positive but very small. Contrary to our expectations, distance to markets, where mobile money agents could be located, and mobile money awareness are positively correlated. Yet, distance to market mattered little once households received the information treatment. At endline, these access measures no longer correlate with awareness or adoption, nor do we observe differential effects of our financial incentive (Appendix Table A7).

3.5 Welfare Effects

In this section, we test whether our intervention strengthens households' ability to cope with shocks. Previous studies suggest that mobile money facilitates remittances and smooths consumption in response to shocks (e.g., Jack and Suri, 2014; Munyegera and Matsumoto, 2016; Riley, 2018; Suri et al., 2012). We also examine some general welfare measures, even if we do not expect to see much in the short term.

We find some suggestive evidence of improved shock coping (Table 9). Shocks reduced the likelihood of reporting any expenditure in the last week by 3 percentage points (Column 4); however, the treatment significantly increased the likelihood of any expenditure by 10 percentage points. Yet treated households spent less in the first place, maybe because they saved more. Treated households also had a lower likelihood of experiencing unmet medical needs by 7 percentage points (Column 7). Treated households that experienced a shock, were 14 percentage points less likely to have experienced an unmet need. Again, suggesting that mobile money enabled households to better cope with shocks. We do not see any effects in terms of food security and relative well-being. The effects are imprecisely estimated and may need more time to materialize.

Table 9. Effect of the financial incentive on welfare indicators: Endline survey

	Lack of food		Any expenditure		Rel. well-being		Unmet medical need	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any treat	-0.03 (0.03)	-0.06 (0.08)	-0.00 (0.01)	-0.09** (0.04)	0.03 (0.07)	0.20 (0.23)	-0.07** (0.03)	0.06 (0.07)
Shock		0.03 (0.06)		-0.03*** (0.01)		0.29 (0.23)		0.21*** (0.06)
Any treat $\times$ Shock		0.04 (0.08)		0.10*** (0.04)		-0.19 (0.25)		-0.14* (0.08)
Observations	944	944	944	944	944	944	944	944
Adjusted R ²	0.02	0.02	0.00	0.01	0.01	0.01	0.01	0.01
Control mean	0.89	0.89	0.98	0.98	-0.12	-0.12	0.30	0.30

Notes: ITT estimates using endline survey data. Outcomes: Any lack of food in the past 12 months is binary (Columns 1–2); any expenditure in the past 7 days (Columns 3–4) is binary; well-being gap between perceived self and neighbors is measured on a 5-point scale (1 = poor, 5 = rich) (Columns 5–6); and any unmet need for medical care due to lack of money in the past 12 months is binary (Columns 7–9). *Any treatment* combines *Full* and *Half* villages. Columns with interactions include *Shock*, an indicator for experiencing any recent household shock, and the interaction *Any treatment* $\times$ *Shock*. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4 Conclusion

We explored how to boost mobile money adoption in a poor rural context where households largely rely on remittances to cope with shocks and daily expenditures, but use unsafe, expensive, and hence inefficient transfer means. We compared two low-cost interventions: providing information using a simple flyer presented to households, and offering a small financial incentive to encourage both the recipient and the sender to open a mobile money account and make a transfer.

Our findings show that although information alone increased mobile money awareness, it did not encourage adoption and use. While we find strong evidence of information diffusion, only treated households improve their knowledge about mobile money, hence, spillovers increased awareness but not knowledge. We find that households that overestimated transfer costs at baseline and updated those beliefs were more likely to open accounts and to use mobile money.

Knowledge about mobile money is necessary but not sufficient for adoption; a modest financial incentive was successful to encourage first-time use. Account ownership increased by 32 percentage points (i.e., six times the control mean of 5%) and usage by 14 percentage points (i.e., a more than two-fold increase). These are, by any standard, large effects. We find stronger effects for female and educated respondents but otherwise find no evidence of heterogeneity across observables, suggesting the incentive was broadly effective across groups.

Given the light-touch design and short post-intervention window, we did not expect large downstream welfare gains. Even so, we see suggestive improvements in shock coping, including increased spending and lower likelihood of unmet medical needs. However, assessing sustained use and welfare impacts will require later rounds of follow-up.

An innovative feature of our study is the detailed data on the mobile agent network and agent characteristics, allowing us to model potential supply-side constraints. Surprisingly, particularly given the resource-constrained setting, we do not find strong evidence that supply-side constraints shaped the effectiveness of our intervention.

The light-touch nature of this intervention suggests that it is cost-effective, as compared with similar interventions, and easily scalable. In total, our intervention cost approximately 9 USD per respondent, comparable with costs in Aggarwal et al. (2020) and Riley et al. (2025), yet lower than those in Batista and Vicente (2025) and Wieser et al. (2019). However, a key question is whether these effects are lasting. Future work should also investigate whether increased adoption triggers changes on the supply side.

We believe our findings are most relevant for low-adoption, remittance-reliant rural settings where mobile money is available but awareness and know-how are limited. In such environments, targeted information that also corrects price misperceptions, coupled with a low-cost first-use incentive, can raise awareness and trigger uptake of digital financial services.

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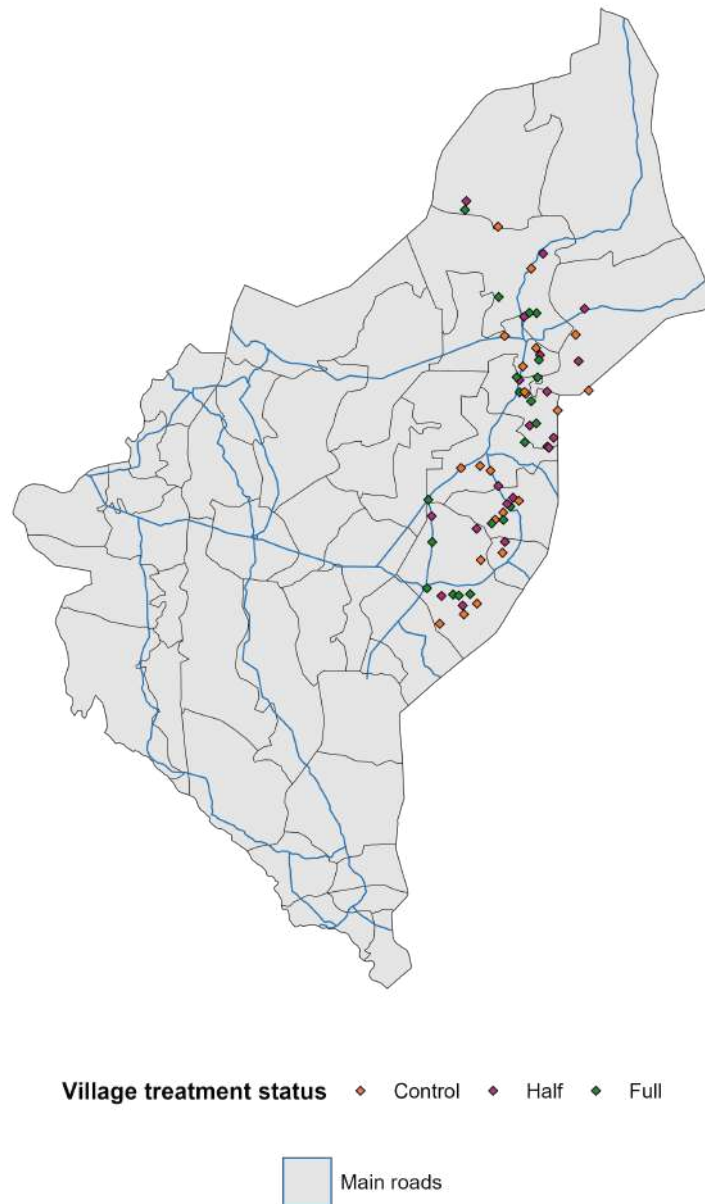
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Appendix

A Treatment status of villages

Figure A1. Information treatment status of study villages



B Attrition

Table A1. Predictors of Attrition

	Attrition			
	Midline I 2024	Midline II 2025	Reminder 2025	Endline 2025
	(1)	(2)	(3)	(4)
Any treat	-0.01 (0.02)	0.01 (0.01)	0.06 (0.04)	-0.02 (0.02)
Age	-0.00 (0.00)	-0.00 (0.00)	-0.00** (0.00)	-0.00*** (0.00)
Female	0.05 (0.03)	0.01 (0.02)	-0.07 (0.05)	0.01 (0.02)
Any formal education	-0.03* (0.02)	-0.02** (0.01)	0.04 (0.03)	-0.01 (0.01)
Own phone	-0.03 (0.02)	-0.02 (0.02)	0.00 (0.05)	-0.02 (0.04)
Any migrants	0.01 (0.01)	0.01 (0.01)	0.03 (0.03)	0.01 (0.01)
Receive transfer last year	0.00 (0.01)	0.01 (0.01)	-0.02 (0.03)	-0.00 (0.01)
Control mean	0.05	0.01	0.08	0.05
Full sample attrition rate	0.04	0.01	0.12	0.03
Observations	978	978	521	978
R-squared	0.03	0.01	0.12	0.03

Notes: Simple difference estimates for *Any treatment* and baseline covariates including geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses.

C Power analysis

Ex-ante power calculations at baseline survey Using our baseline survey data we ran ex-ante power calculations for our clustered design with 21 control and 40 treatment villages and on average respondents per cluster. Assuming a significance level of 5% and 80%. Ex-ante, we were powered to detect a minimum detectable effect (MDE) of 10 percentage points for whether the respondent has heard of mobile money (control mean = 35%, ICC = 0.02). MDEs were smaller for household account ownership with 3 percentage points (control mean = 1%, ICC = 0) and 4 percentage points for having used mobile money in the year prior to the baseline survey (control mean = 3%, ICC = 0).

Ex-post power at endline survey Using our endline survey data, we ran ex-post power calculations to determine what the minimum detectable effects sizes are assuming a gain a significance level of 5% and 80%. The MDE for having heard of mobile money is 10 percentage points (control mean = 80%, ICC = 0.09), 14 percentage points for household account ownership (control mean = 5%, ICC = 0.26), and 12 percentage points for ever having used mobile money (control mean = 12%, ICC = 0.11). In short, by endline the design remained sensitive to about

10–14 percentage point treatment effects across outcomes, with larger MDEs where intra-cluster correlation was higher.

D ANCOVA estimation

Table A2. Effects of the financial incentive on mobile money adoption: Endline survey

	Mobile Money		
	Heard of	Household account	Ever used
	(1)	(2)	(3)
Any treat	0.15*** (0.03)	0.32*** (0.04)	0.14*** (0.04)
Observations	944	908	944
Adjusted R ²	0.06	0.13	0.03
Control mean	0.80	0.05	0.12

Notes: ITT estimates using endline survey data estimating an ANCOVA specification, including the baseline values of the outcome. Outcomes are dummies: heard of mobile money (Column 1), household has a mobile money account (Column 2), has used mobile money (Column 3). *Any treatment* combines *Full* and *Half* villages. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E Heterogeneity analysis

Table A3. Heterogeneous effects of the information on mobile money adoption: Midline survey I

	Mobile Money																	
	Heard of						Household account						Used since last survey					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Any treat	0.18** (0.07)	0.09 (0.07)	0.16* (0.09)	0.21 (0.13)	0.15** (0.07)	0.12* (0.07)	0.07** (0.03)	0.06* (0.03)	0.03 (0.04)	0.06 (0.04)	0.04 (0.04)	0.04 (0.03)	0.06** (0.03)	0.06 (0.04)	0.06 (0.05)	0.11** (0.05)	0.04 (0.04)	0.04 (0.04)
Migrant	0.05 (0.05)						0.02 (0.02)						0.05* (0.03)					
Any treat × Migrant	-0.08 (0.06)						-0.02 (0.03)						-0.02 (0.04)					
Female		-0.15** (0.07)						0.02 (0.02)						0.01 (0.03)				
Any treat × Female		0.06 (0.07)						-0.01 (0.02)						-0.01 (0.03)				
Education			-0.04 (0.09)						0.03 (0.03)						0.06 (0.04)			
Any treat × Education			-0.04 (0.10)						0.03 (0.04)						-0.02 (0.05)			
Phone				0.25** (0.10)						0.07*** (0.02)						0.09*** (0.03)		
Any treat × Phone				-0.09 (0.13)						-0.01 (0.04)						-0.06 (0.05)		
More costly vs. agency					0.06 (0.06)						-0.05** (0.02)						-0.03 (0.02)	
Any treat × More costly vs. agency					-0.07 (0.07)						0.04 (0.03)						0.04 (0.03)	
More costly vs. bus						-0.09 (0.08)						-0.06** (0.02)						-0.08** (0.03)
Any treat × More costly vs. bus						0.02 (0.10)						0.06* (0.03)						0.09** (0.04)
Observations	941	941	941	941	941	941	941	941	941	941	941	941	941	941	941	941	941	941
Adjusted R ²	0.13	0.13	0.13	0.14	0.13	0.13	0.05	0.05	0.06	0.06	0.06	0.05	0.05	0.04	0.05	0.05	0.04	0.05
Control × (X = 0)	0.50	0.66	0.56	0.35	0.51	0.53	0.03	0.04	0.04	0.00	0.06	0.04	0.03	0.07	0.04	0.00	0.07	0.07

Notes: ITT estimates using Midline survey I data. Outcomes are dummies: heard of mobile money (Columns 1–6), household has a mobile money account (Columns 7–12), and has used mobile money since the baseline survey (Columns 13–18). *Any treatment* combines *Full* and *Half* villages. We interact *Any treatment* with baseline characteristics: household has migrant, respondent is female, respondent has attained any education, and household owns a mobile phone, as well as baseline beliefs about transfer costs. *More costly vs. agency* = 1 if the respondent reported a higher fee for sending 10,000 FCFA via mobile money than via a transfer agency (e.g., BNIF, NITA) at baseline, 0 otherwise. *More costly vs. bus* = 1 if the reported fee via mobile money exceeds the fee via bus. Control × (X = 0) refers to the mean for control households, for which the covariate used for the heterogeneity analysis equals zero. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4. Heterogeneous effects of the information on mobile money adoption: Midline survey II

	Mobile Money																	
	Heard of						Household account						Used since last survey					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Any treat	0.26*** (0.06)	0.19*** (0.07)	0.16** (0.07)	0.05 (0.12)	0.21*** (0.06)	0.21*** (0.05)	-0.01 (0.02)	-0.00 (0.02)	0.01 (0.02)	0.00 (0.01)	-0.02 (0.01)	-0.01 (0.01)	-0.00 (0.02)	0.01 (0.02)	0.01 (0.02)	0.02 (0.02)	0.01 (0.01)	0.00 (0.02)
Migrant	0.04 (0.05)						0.00 (0.02)						-0.02 (0.02)					
Any treat × Migrant	-0.09 (0.06)						0.01 (0.03)						0.01 (0.03)					
Female		-0.08 (0.07)						-0.03 (0.02)						-0.02 (0.01)				
Any treat × Female		0.01 (0.08)						-0.00 (0.02)						-0.01 (0.02)				
Education			-0.09 (0.06)						0.00 (0.02)						0.00 (0.02)			
Any treat × Education			0.07 (0.07)						-0.02 (0.02)						-0.01 (0.02)			
Phone				0.00 (0.09)						0.03*** (0.01)						0.04** (0.02)		
Any treat × Phone				0.16 (0.12)						-0.00 (0.01)						-0.02 (0.02)		
More costly vs. agency					0.02 (0.06)						-0.01 (0.02)						0.01 (0.02)	
Any treat × More costly vs. agency					-0.01 (0.06)						0.05** (0.02)						-0.00 (0.02)	
More costly vs. bus						0.03 (0.08)						-0.03** (0.01)						-0.01 (0.03)
Any treat × More costly vs. bus						-0.02 (0.09)						0.04 (0.03)						0.01 (0.04)
Observations	966	966	966	966	966	966	966	966	966	966	966	966	966	966	966	966	966	966
Adjusted R ²	0.06	0.07	0.07	0.07	0.06	0.06	0.00	0.00	0.00	0.00	0.01	0.00	0.01	0.01	0.01	0.01	0.01	0.01
Control × (X = 0)	0.50	0.60	0.58	0.54	0.51	0.52	0.03	0.04	0.03	0.00	0.03	0.03	0.04	0.04	0.03	0.00	0.03	0.03

Notes: ITT estimates using Midline II survey data. Outcomes are dummies: heard of mobile money (Columns 1–4), household has a mobile money account (Columns 5–8), and has used mobile money since the Midline survey I (Columns 9–12). *Any treatment* combines *Full* and *Half* villages. We interact *Any treatment* with baseline characteristics: household has migrant, respondent is female, respondent has attained any education, and household owns a mobile phone. Control × (X = 0) refers to the mean for control households, for which the covariate used for the heterogeneity analysis equals zero. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5. Heterogeneous effects of the financial incentive on mobile money adoption: Endline transfer records

	Mobile Money																	
	Any transfer						Transfer since offer							Transfer since reminder				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Any treat	0.26*** (0.06)	0.19*** (0.07)	0.16** (0.07)	0.05 (0.12)	0.21*** (0.06)	0.21*** (0.05)	-0.01 (0.02)	-0.00 (0.02)	0.01 (0.02)	0.00 (0.01)	-0.02 (0.01)	-0.01 (0.01)	-0.00 (0.02)	0.01 (0.02)	0.01 (0.02)	0.02 (0.02)	0.01 (0.01)	0.00 (0.02)
Migrant	0.04 (0.05)						0.00 (0.02)						-0.02 (0.02)					
Any treat × Migrant	-0.09 (0.06)						0.01 (0.03)						0.01 (0.03)					
Female		-0.08 (0.07)						-0.03 (0.02)						-0.02 (0.01)				
Any treat × Female		0.01 (0.08)						-0.00 (0.02)						-0.01 (0.02)				
Education			-0.09 (0.06)						0.00 (0.02)						0.00 (0.02)			
Any treat × Education			0.07 (0.07)						-0.02 (0.02)						-0.01 (0.02)			
Phone				0.00 (0.09)						0.03*** (0.01)						0.04** (0.02)		
Any treat × Phone				0.16 (0.12)						-0.00 (0.01)						-0.02 (0.02)		
More costly vs. agency					0.02 (0.06)						-0.01 (0.02)						0.01 (0.02)	
Any treat × More costly vs. agency					-0.01 (0.06)						0.05** (0.02)						-0.00 (0.02)	
More costly vs. bus						0.03 (0.08)						-0.03** (0.01)						-0.01 (0.03)
Any treat × More costly vs. bus						-0.02 (0.09)						0.04 (0.03)						0.01 (0.04)
Observations	966	966	966	966	966	966	966	966	966	966	966	966	966	966	966	966	966	966
Adjusted R-squared	0.0645	0.0650	0.0652	0.0680	0.0625	0.0626	0.000524	0.00285	0.00121	0.00219	0.00784	0.00138	0.00875	0.00875	0.00732	0.00872	0.00745	0.00733
Control × (X = 0)	0.500	0.602	0.582	0.538	0.513	0.516	0.0250	0.0376	0.0274	0	0.0302	0.0316	0.0417	0.0376	0.0342	0	0.0251	0.0316

Notes: ITT estimates using transfer records provided by Airtel. Outcomes are dummies: any mobile money transfer (Columns 1-3), any mobile money transfer made after the financial incentive was offered (Columns 4-6), and any mobile money transfer made after the reminder call (Columns 7-9). *Any treatment* combines *Full* and *Half* villages. We interact *Any treatment* with baseline characteristics: household having a migrant, respondent being female, having attained any education, and owning a mobile phone. Control × (X = 0) refers to the mean for control households, for which the covariate used for the heterogeneity analysis equals zero. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6. Heterogeneous effects of the information on mobile money adoption: Midline survey I and Agent survey

	Mobile Money								
	Heard of			Household account			Used since last survey		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any treat	0.21** (0.10)	0.26** (0.12)	0.40*** (0.09)	0.09* (0.05)	0.09* (0.05)	0.16*** (0.06)	0.12** (0.05)	0.06 (0.06)	0.13** (0.07)
Agents: nearest market	0.01** (0.01)			0.00 (0.00)			0.00 (0.00)		
Any treat × Agents: nearest market	-0.01 (0.01)			-0.00* (0.00)			-0.01** (0.00)		
Agents: 3 nearest markets		0.01** (0.00)			0.00 (0.00)			-0.00 (0.00)	
Any treat × Agents: 3 nearest markets		-0.00 (0.00)			-0.00 (0.00)			-0.00 (0.00)	
Distance to market (km)			0.07*** (0.02)			0.01* (0.01)			0.01 (0.01)
Any treat × Distance to market (km)			-0.05*** (0.02)			-0.03** (0.01)			-0.02 (0.01)
Observations	941	941	941	941	941	941	941	941	941
Adjusted R ²	0.15	0.15	0.17	0.06	0.06	0.07	0.06	0.04	0.05
Control mean	0.53	0.53	0.53	0.04	0.04	0.04	0.06	0.06	0.06

Notes: ITT estimates using Midline survey I data and interacting it with data from the Agent survey and Household Baseline survey. Outcomes are dummies: heard of mobile money (Columns 1–3), household has a mobile money account (Columns 4–6), and has used mobile money since the baseline survey (Columns 7–9). *Any treatment* combines *Full* and *Half* villages. We interact *Any treatment* with market level data on the agent count in the nearest market, the total agent count in the three nearest markets, and the distance to the nearest market (in km). All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7. Heterogeneous effects of the financial incentive on mobile money adoption: Endline survey and Agent survey

	Mobile Money								
	Heard of			Household account			Ever used		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any treat	0.20*** (0.04)	0.26*** (0.07)	0.15*** (0.05)	0.41*** (0.06)	0.42*** (0.10)	0.32*** (0.08)	0.14** (0.06)	0.09 (0.09)	0.19*** (0.07)
Agents: nearest market	0.00 (0.00)			-0.00** (0.00)			-0.01*** (0.00)		
Any treat × Agents: nearest market	-0.00 (0.00)			-0.01** (0.00)			0.00 (0.00)		
Agents: 3 nearest markets		0.00 (0.00)			-0.00 (0.00)			-0.00* (0.00)	
Any treat × Agents: 3 nearest markets		-0.00 (0.00)			-0.00 (0.00)			0.00 (0.00)	
Distance to market (km)			-0.00 (0.01)			-0.01 (0.01)			-0.01 (0.01)
Any treat × Distance to market (km)			0.00 (0.01)			-0.00 (0.02)			-0.01 (0.01)
Observations	944	944	944	944	944	944	944	944	944
Adjusted R ²	0.06	0.07	0.06	0.17	0.16	0.14	0.05	0.04	0.04
Control mean	0.80	0.80	0.80	0.05	0.05	0.05	0.12	0.12	0.12

Notes: ITT estimates using Endline survey data and interacting it with data from the agent survey and Household Baseline data. Outcomes are dummies: heard of mobile money (Columns 1–3), household has a mobile money account (Columns 4–6), and has ever used mobile money (Columns 7–9). *Any treatment* combines *Full* and *Half* villages. We interact *Any treatment* with market level data on the number of agents in the nearest market, the number of agents in the three nearest markets, and the distance to the nearest market (km). All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

F Additional Outcomes

Table A8. Effects of the information on mobile money usage: Endline survey

	Received transfer		Received m-money		Sent transfer		Sent m-money		Kept m-money	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Any treat	0.04 (0.03)	0.05 (0.04)	0.02** (0.01)	0.03** (0.01)	0.01 (0.03)	0.00 (0.05)	0.01* (0.00)	0.01 (0.00)	175.42* (91.16)	235.35 (186.89)
Reminder		0.05 (0.05)		0.01 (0.01)		0.00 (0.06)		-0.00 (0.00)		-58.46 (66.75)
Any treat × Reminder		-0.02 (0.06)		-0.02 (0.02)		0.01 (0.07)		-0.00 (0.01)		-115.25 (191.32)
Observations	944	944	944	944	944	944	944	944	944	944
Adjusted R ²	0.01	0.01	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.00
Control mean	0.72	0.70	0.01	0.01	0.29	0.29	0.00	0.00	37.61	68.21

Notes: ITT estimates using Endline survey data. Outcomes are indicators for having received any transfer in the past year (Columns 1–2), having received this transfer via mobile money (Columns 3–4), having sent any transfer (Columns 5–6), having sent this transfer via mobile money (Columns 7–8), and the amount usually kept in mobile money account in FCFA (Columns 9–10). *Any treatment* combines *Full* and *Half* villages. We include an indicator for receiving the *Reminder* call and the interaction *Any treatment* × *Reminder*. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9. Effects of the financial incentive on mobile money knowledge: Midline survey I

	Count of functions mentioned			Any function mentioned		
	(1)	(2)	(3)	(4)	(5)	(6)
Any treat	0.32** (0.14)			0.15** (0.07)		
Full		0.42** (0.16)	0.42** (0.16)		0.21** (0.08)	0.21** (0.08)
Half		0.21 (0.16)	0.07 (0.17)		0.09 (0.08)	0.03 (0.09)
Half × Info			0.27** (0.11)			0.11** (0.05)
Observations	941	941	941	941	941	941
Adjusted R ²	0.14	0.14	0.15	0.13	0.14	0.14
Control mean	0.93	0.93	0.93	0.46	0.46	0.46
Half = Full		0.17			0.09	

Notes: ITT estimates using Midline survey data. Outcomes: Count of functions mentioned is how many functions the respondent could name out of opening an account, withdrawing cash, depositing into their own account, depositing to another number/account, and paying bills (range 0–4) (Columns 1-3). Any function mentioned = 1 if at least one of these functions was named (count ≥ 1), 0 otherwise (Columns 4-6). Villages were randomly assigned to *Full* where all sampled households received the flyer, *Half* where 50% of sampled households randomly received the flyer, or *Control*. *Any treatment* combines *Full* and *Half* villages. The “spillover” specification contains indicators for *Full* villages, *Half* villages without information assignment at baseline in 2024 (but received information later at the second intervention in 2025), and *Half* × *Info* for households with information assignment in *Half* villages in both interventions. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10. Effects of the financial incentive on mobile money knowledge: Endline survey

	Count of functions mentioned			Any function mentioned		
	(1)	(2)	(3)	(4)	(5)	(6)
Any treat	0.47*** (0.09)			0.08*** (0.02)		
Full		0.47*** (0.10)	0.47*** (0.10)		0.09*** (0.02)	0.09*** (0.02)
Half		0.47*** (0.11)	0.43*** (0.11)		0.07*** (0.02)	0.08*** (0.03)
Half $\times$ Info			0.08 (0.13)			-0.02 (0.03)
Observations	944	944	944	944	944	944
Adjusted R ²	0.04	0.04	0.04	0.03	0.03	0.03
Control mean	2.10	2.10	2.10	0.88	0.88	0.88
Half = Full		0.977			0.416	

Notes: ITT estimates using Endline survey data. Outcomes: Count of functions mentioned is how many functions the respondent could name out of opening an account, withdrawing cash, depositing into their own account, depositing to another number/account, and paying bills (range 0–4) (Columns 1-2). Any function mentioned = 1 if at least one of these functions was named (count ≥ 1), 0 otherwise (Columns 3-4). Villages were randomly assigned to *Full* where all sampled households received the flyer, *Half* where 50% of sampled households randomly received the flyer, or *Control*. *Any treatment* combines *Full* and *Half* villages. The “spillover” specification contains indicators for *Full* villages, *Half* villages without information assignment at baseline in 2024 (but received information later at the second intervention in 2025), and *Half* $\times$ *Info* for households with information assignment in *Half* villages in both interventions. All regressions include geographic- and gender-strata fixed effects; standard errors are clustered at the village level and reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

G Descriptive Statistics

Table A11. Household constraints to mobile money: Baseline survey

	Households	
	Mean (1)	(SD) (2)
Lack of information	0.88	(0.33)
No phone	0.18	(0.38)
Agent liquidity issues	0.07	(0.25)
Agent too far	0.15	(0.36)
Bad experience	0.02	(0.15)
Too expensive	0.04	(0.19)
Too complicated	0.52	(0.50)
Fear of losing money/trust	0.04	(0.18)
No ID	0.09	(0.28)
No SIM	0.12	(0.33)
No money	0.35	(0.48)

Notes: Column (1) displays the share of respondents that mentioned each respective constraint to using mobile money during the Baseline survey. Respondents could mention multiple constraints.

Table A12. Mobile money agent constraints to mobile money: Baseline survey

	Mobile money agents	
	Mean (1)	(SD) (2)
Liquidity issues	0.66	(0.48)
Low customer access	0.59	(0.49)
Small profits	0.48	(0.50)
Competition	0.10	(0.30)
Agent's m-money fee knowledge	0.00	(0.00)
Client knowledge	0.23	(0.43)
Network problems	0.73	(0.44)

Notes: Column (1) displays the share of mobile money agents that mentioned each respective constraint with respect to their mobile money business. Agents could mention multiple constraints.

Table A13. Household descriptives on mobile money usage: Endline survey

	Mean	SD
<i>Panel A: Used mobile money for (users only, N=198)</i>		
Pay subscriptions, bills, etc.	0.24	0.43
Pay for communication	0.42	0.49
Save money	0.59	0.49
Make or receive payments for salary, school fees, government	0.21	0.41
Send or receive money to/from a relative	0.58	0.50
<i>Panel B: Reasons for not using mobile money (non-users, N=746)</i>		
Don't know it	0.25	0.43
Don't have a mobile phone	0.21	0.40
No cash available at the agent (agent lacks liquidity)	0.33	0.47
Agent is too far	0.17	0.37
Had a bad experience	0.02	0.14
Too expensive	0.05	0.21
Don't understand how it works (too complicated)	0.51	0.50
Afraid of losing money	0.03	0.18
No ID	0.09	0.29
No registered SIM card	0.07	0.25
<i>Panel C: Reasons for not taking up the offer (N=12)</i>		
Did not find a sender	0.08	0.29
Sender forgot	0.00	0.00
Sender did not want to use m-money	0.00	0.00
Did not find m-money agent	0.00	0.00
Compensation too weak	0.08	0.29
Timeframe too short	0.25	0.45
No time/forgot	0.25	0.45
No interest	0.08	0.29
Too complicated	0.08	0.29
No reason	0.33	0.49
Don't know	0.00	0.00
I did receive a m-money transfer	0.00	0.00

Notes: Panel A displays the functions that mobile money users (N = 198) used mobile money for, with multiple answers being possible., Panel B displays the reasons why non-users (N = 746) did not use mobile money, with multiple answers being possible. Panel C displays the reasons why treated respondents did not take up the financial incentive offer, with multiple answers being possible.

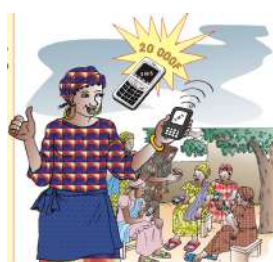
H Intervention Flyer

Figure A2. Mobile money information flyer page 1



HAUSA

M'kudi/Orange Money/Flooz Money sarushi ne da ke ba ku damar aika kudi ta wayar salula, kamar chap-chap, amma zaka iya amfani da kudin don kowane dalili.



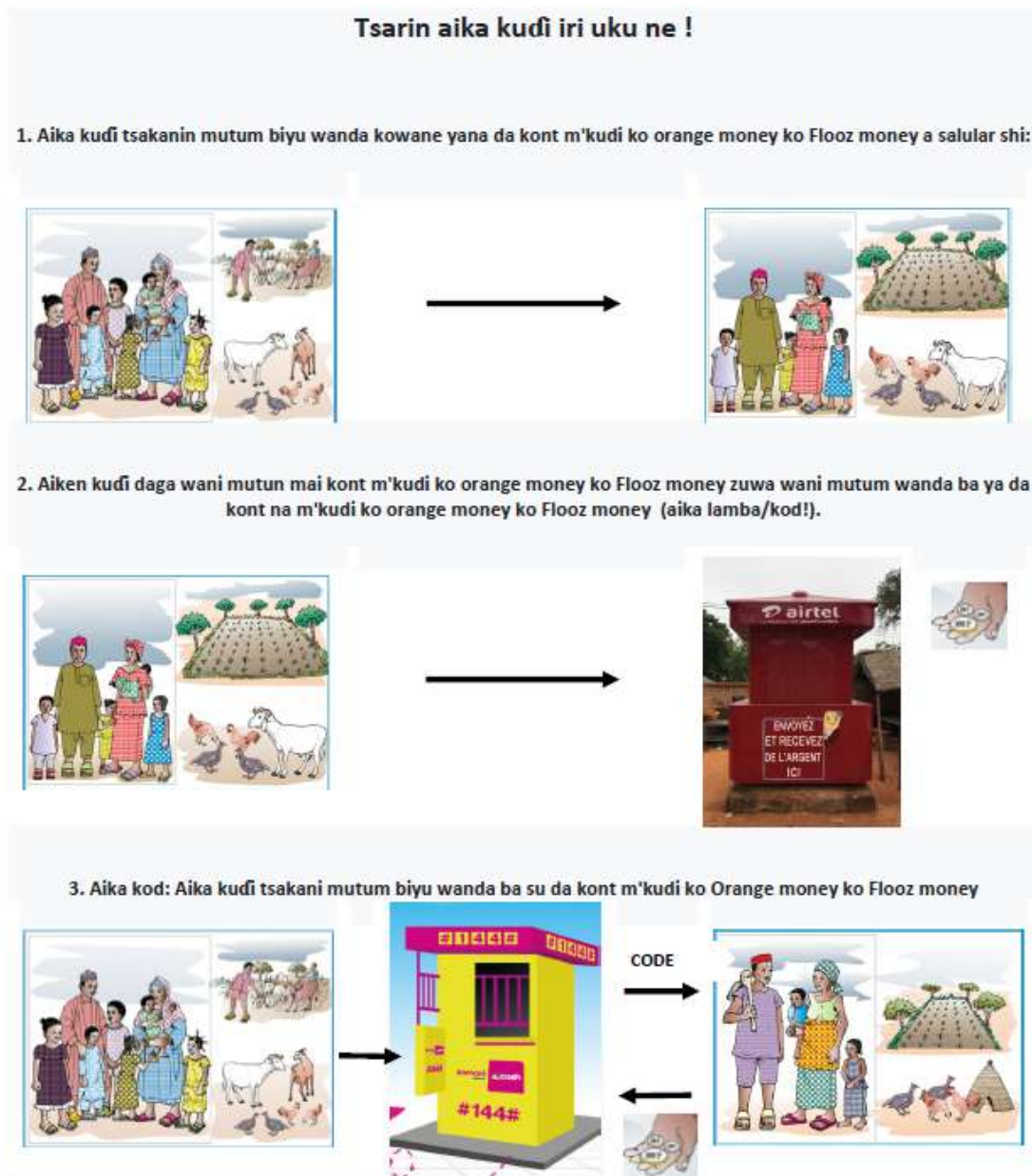
Idan wani yana son aika muku kudi, ya na iya zuwa wurin ma'aikacin m'kudi ko Orange Money ko Flooz Money, ya sa kudi a wayarshi kuma ku ba shi lambar wayar mutumen da za'a aikama kudi. Idan an aika kudin nan take kudin zai shiga cikin salular, sai ku ji 'kara' da safo tare da jimilan kudin, misali 20,000 CFA



Don cire kudin ku, dole ne ku je wurin ma'aikacin m'kudi ko Orange Money ko Flooz Money wakili - ba a'aikaci mai saida kiredi kadai ba. A yawancin lokaci akwai abin da za'a biya don cire kudin ku, don haka za a sami bambanci tsakanin kudin da aka aiko da abin da zaku karɓa. Kuma wannan kudin ya ta'allaka da tsarin ko wane kampani. Misali, idan wani ya aiko muku 10,000 CFA, za ku sami tsakanin 9,600 da 9,750 CFA don kudin aike.

Notes: Translation: “M’kudi/Orange Money/Flooz Money is a service that lets you send money by mobile phone, like “chap-chap,” but you can use the money for any purpose. When someone wants to send you money, they can go to a M’kudi/Orange Money/Flooz Money agent, deposit cash to their phone account, and give your phone number. The transfer is automatic and immediate. When the money is sent, you’ll hear a “beep” and receive a message showing the amount, for example 20,000 FCFA. To withdraw your money, you must go to a M’kudi/Orange Money/Flooz Money agent—not just a general mobile phone (airtime) agent. There is often a fee to cash out, so there will be a difference between the amount sent and what you receive. It depends on the mobile operator and the amount sent. For example, if someone sends you 10,000 FCFA, you will receive between 9,600 and 9,750 FCFA because of the fee.”

Figure A3. Mobile money information flyer page 2



Notes: Translation: “There are three ways to transfer money: 1. Transfer between two people who both have a M’kudi/Orange Money/Flooz Money account. 2. Transfer from someone with a M’kudi/Orange Money/Flooz Money account to someone without an account (send-code). 3. Send-code: transfer between two people who do not have an account.”

Figure A4. Mobile money financial incentive flyer

Wannan tukuicin an ke bance shi musamman dan ku. Ba'a iya aika shi ga wani mutum.



Idan wani ya aika muku kudi ta hanyar mobile money akalla sau ɗaya a cikin makonni biyu (2) masu zuwa, zamu baka jika biyu 2000 na FCF ta hanyar mobile money



Wanda ya fara aika muku kudi ta hanyar mobile money a cikin makonni biyu (2) masu zuwa zai samu jika ɗaya 1000 na FCFA daga gare mu ta hanyar mobile money.



Ana iya baka wannan tukuicin daga yau har zuwa tsawon wata ɗaya. Za a biya tukuicin sau ɗaya kuma ba za a samu karin biyan kudi ba a cikin wannan tsarin.

Notes: Translation: “This incentive is tailored specifically for you. It cannot be transferred to another person. If someone sends you money via mobile money at least once in the next two (2) weeks, we will give you a 2,000 FCFA bonus via mobile money. The first person to send you money via mobile money in the next two (2) weeks will receive a 1,000 FCFA bonus from us via mobile money. You can receive this incentive from today for up to one month. It will be paid once only, and there will be no additional payments under this program.”

“Sur quoi la fondera-t-il l’économie du monde qu’il veut gouverner ? Sera-ce sur le caprice de chaque particulier ? Quelle confusion ! Sera-ce sur la justice ? Il l’ignore.”

Pascal



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