

# Digital Money, Physical Frictions: Payment Access and the Effectiveness of Public Programs\*

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## Abstract

Public programs in low-income countries are increasingly delivered through mobile money, yet most rural households still transact in cash. In rural Togo, the transport cost of reaching the nearest mobile-money agent to pay for electricity amounts to 28–43% of households' total electricity spending—a first-order component of the effective price they face. Exploiting the staggered rollout of a national solar subsidy, we find that the subsidy more than doubles adoption, with effects three times larger in villages where an agent is already within walking distance. A subsequent government-led expansion of agents into underserved villages closes most of this gap, isolating payment access from other correlates of remoteness. The same friction weakens Togo's emergency cash transfers, whose recipients must also reach an agent to convert digital payments into cash. Because private operators place agents where transaction volumes are highest, they underinvest in the remote areas where payment access matters most for public programs, creating a role for public investment in last-mile infrastructure.

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Governments in lower-income countries spend heavily on subsidies and cash transfers, yet why such programs succeed in some places and disappoint in others remains only partly understood (Vivalt, 2020; Meager, 2019; Duflo, 2017; Hanna and Olken, 2018). One reason, we argue, is that a program’s value to a household depends not only on what it offers but on what the household must spend to obtain it. As benefits shift from cash to digital delivery (Lindert et al., 2020), a large and overlooked part of that cost is the price of reaching the payment network. Most rural households still transact in cash (Demirgüç-Kunt et al., 2022), so a transfer paid into a mobile money account is worth less the farther a household must travel to an agent to turn it into spendable cash, and that agent network, built by private operators, is thinnest in the remotest areas, where public programs are needed most. We show that this cost can be first-order, shaping how well a program works. Studying two nationwide policies in Togo, a solar electrification subsidy and a government-led expansion of the mobile money agent network, we find that payment access is decisive: the subsidy’s effect on adoption is about three times larger where an agent is already within walking distance, and rolling agents out to underserved villages closes most of the remaining gap. A nationwide cash transfer program (Novissi) that we evaluate in the same setting shows the same pattern, with larger effects where agents are nearby.

Rural electrification is the central case in our analysis, and a natural one for this mechanism. Nearly 800 million people still lack electricity, overwhelmingly in rural South Asia and Sub-Saharan Africa, and off-grid solar has become the central margin of expansion (Burgess et al., 2023; Lang, 2025a): extending the grid to sparsely populated areas is prohibitively costly, and by 2040 some 70% of rural Africans gaining electricity are expected to do so off-grid (Africa Progress Panel, 2017). The product we study is a pay-as-you-go (PAYGO) solar home system, which relaxes liquidity constraints by letting households pay for electricity over time rather than all at once. Like any electricity service, it must be paid for on a recurring basis, so each payment requires a trip to an agent; because PAYGO is bought in small and frequent increments, those trips are especially numerous.

Despite heavy investment, take-up of rural electricity remains low at prevailing prices, and we offer a new explanation for why. The leading explanation in the literature is that rural households have low willingness to pay, which implies that subsidies must be very large to move adoption (Lee, Miguel and Wolfram, 2020; Burgess et al., 2023; Burlig and Preonas, 2022). We point to a complementary explanation: the posted price is not the price households actually face. In rural Togo the median village lies 6.7 km from the nearest agent, so the transport cost of making the repeated PAYGO payments absorbs 28% of total electricity spending for the median household, rising to 43% at the 75th percentile.

For poor, liquidity-constrained households these are first-order costs, and they depress take-up and distort measured price elasticities.

Costs like this have a long lineage in the economics of take-up, where paperwork, stigma, or a trip to a distant office can keep eligible households from collecting benefits they are owed ([Finkelstein and Notowidigdo, 2019](#)). The cost we study differs in one important respect: it is paid not once, at the point of claim, but at every transaction with the program, so it accumulates over a stream of payments and can become as large as the posted price of the service itself. The friction is new to mobile money but not to social policy: in the paper-voucher era of US food assistance, obtaining and redeeming benefits carried real time and travel costs, which the shift to electronic delivery later reduced ([Zhou et al., 2026](#); [Currie, 2006](#)).

We exploit two nationwide policies in Togo that lowered the cost of solar electricity through distinct channels. The first is the CIZO subsidy, rolled out in staggered fashion across districts between March and July 2019, which cut the cost of solar electricity by 18–44% depending on the kit and was among the first demand-side solar subsidies in Sub-Saharan Africa. The second is the government-led *Réseau Granulaire* expansion, which from September 2019 placed mobile money agents in remote villages that previously lacked nearby access; at baseline only 23% of our villages had an agent within 3 km, so the scope was large. By design, the first policy lowers the posted price and the second lowers the cost of paying it, and because the two were rolled out at different times we can identify each separately using interaction-weighted event studies ([Sun and Abraham, 2021](#)).

We combine four data sources. First, proprietary administrative records from Togo’s largest solar provider, covering more than 50,000 customers in over 2,000 rural villages, give us applications, installations, payments, and daily electricity use; uniquely, they let us observe the complete payment history of each customer.<sup>1</sup> Second, government data on the universe of mobile money agents, with GPS coordinates and opening dates, let us measure each village’s distance to the nearest agent and pin down the timing of agent arrival. Third, an original survey of rural moto-taxi fares lets us put a monetary value on the cost of reaching an agent. Fourth, anonymized data from the randomized evaluation of the Novissi emergency cash transfer program ([Aiken et al., 2022](#)), generously shared with us and merged with the agent data, let us test whether the same friction operates beyond electrification.

A simple framework organizes the analysis around two distinct barriers to adoption.

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<sup>1</sup>In 2016, these villages had very low electrification rates, with only about 7% of rural households accessing any source of electricity ([Government of Togo, 2016](#)).

A household adopts only if it is willing to pay the posted price and is able to complete the repeated mobile money payments the PAYGO contract requires, at a cost that rises with distance to the nearest agent; the subsidy relaxes the first barrier and agent expansion the second. Because the barriers are distinct, the two policies are structural complements: each enlarges the set of households for whom the other is the binding constraint, so the marginal effect of one is larger where the other is already in place. The same principle, that the cost of reaching the agent network is part of a program’s effective value, extends to any benefit delivered through mobile money, including cash transfers. A second channel works on the intensive margin: because PAYGO lets households choose how often to pay, a price cut lets them consolidate payments and save on trips, more so where a nearby agent makes payment schedules flexible. Each channel yields distinct, testable predictions.

Two results establish the role of payment access in adoption. First, the subsidy more than doubled solar adoption, consistent with contemporaneous evidence in the same setting ([Lang, 2025a](#)), but this average masks sharp heterogeneity: the effect was about three times larger in villages that already had an agent within 3 km (1.053 versus 0.347 additional applications per village-month), and the gap is significant at the 5% level and unchanged by controls for village wealth, population density, and market access. On its own this heterogeneity is only suggestive, since villages with and without agents may differ in unobserved ways that shape how subsidies translate into adoption. Our second result addresses this: studying the agent expansion, which was rolled out after the subsidy was already nationwide, we find that the arrival of an agent raised adoption by 0.498 applications per village-month, an effect close to the heterogeneity gap. Two designs that exploit different margins of the effective price at different times thus deliver consistent magnitudes.

Four further pieces of evidence tie this complementarity to transaction costs specifically. First, the subsidy–agent interaction is concentrated at the walking threshold: once the nearest agent is beyond 3 km, subsidy effectiveness drops sharply and then changes little with additional distance, matching the step-function structure of rural transport costs. If households also travelled to agents for unrelated reasons, that would only attenuate the near–far difference, so the sharp gap we estimate is a conservative lower bound; consistent with this, the subsidy effect remains positive even in the most remote villages rather than falling to zero. Second, the complementarity is largest among customers choosing the cheapest kit, those most likely to face binding liquidity and transaction-cost constraints, and is uniform across income source, age, and gender, as expected of a mechanism that operates at the village level. Third, the magnitudes line up: using observed

fares and payment frequency, the arrival of an agent cut the effective cost of electricity by about 34% for the median treated village, close to the 44% cut delivered by the subsidy itself, which is why the two produce adoption effects of similar size. Fourth, the complete payment histories let us study behaviour on the intensive margin: the subsidy cut payment frequency by 18% among existing customers without reducing their electricity use, as households bought in bulk to save on trips. Whether this response is larger or smaller near an agent is theoretically ambiguous: cheap trips weaken the incentive to consolidate, but flexible schedules give households more room to do so, and remote households are often already down to a single payment a month. The second force dominates: frequency falls more where agents are nearby.

The same friction operates well beyond electrification. We merge the randomized evaluation of Togo’s Novissi emergency cash transfer program ([Aiken et al., 2022, 2025](#)) with our agent data; because Novissi transfers were paid into mobile money accounts, beneficiaries still had to reach an agent to convert them into cash. A one-standard-deviation increase in canton-level agent access raises the treatment effect of Novissi by 0.049 standard deviations on financial health and 0.040 standard deviations on health care access, both outcomes that depend on spending power. The same pattern does not appear for placebo measures of remoteness: distance to the nearest financial institution, distance to the nearest market, and population density. This pattern is hard to reconcile with a generic remoteness story and points to payment access as a broad bottleneck for digitally delivered policy.

These findings reveal a specific market failure in the provision of payment infrastructure. A mobile money agent is typically a local shopkeeper or trader who holds both cash and a digital balance and earns a small commission on each deposit and withdrawal. Because agents handle their customers’ cash on the operator’s behalf, operators require them to post a large cash deposit to operate; an investment that pays off only where transaction volumes are high enough to cover the cost. Remote rural areas are exactly where volumes, and so private returns, are lowest, yet they are also where each additional agent does the most to amplify public policy. This gap between private returns and social value is what motivated the government’s *Réseau Granulaire* expansion, which we study. A back-of-the-envelope calculation suggests that extending walkable agent access to all villages in our sample would more than double the adoptions induced by the subsidy and move Togo materially closer to its 2030 universal-electrification target. Public investment in last-mile payment access therefore raises the social return on a broad portfolio of public programs, well beyond the direct gains to financial inclusion.

Our primary contribution is to the literature on financial inclusion and mobile money

(Burgess and Pande, 2005; Suri, 2011; Jack and Suri, 2014; Suri, 2017; Fink, Jack and Masiye, 2020; Breza and Kinnan, 2021; Lee et al., 2021; Fonseca and Matray, 2022; Berkouwer et al., 2023; Riley, 2024; Batista and Vicente, 2025), which documents the direct welfare effects of access to formal financial services through risk-sharing, consumption smoothing, labour supply, and investment. We identify an indirect channel: denser payment networks raise the effectiveness of *other* public programs by lowering the household-side cost of receiving their benefits. The closest precedent, Muralidharan, Niehaus and Sukhtankar (2016), shows that digitizing payments in India reduced leakage on the supply side, the state getting money to households; our mechanism runs the other way, governing the demand-side cost of households getting to the money, and we show it matters for two very different programs delivered through the same payment system. Our results are complementary to Aker et al. (2016), who find that paying a transfer through mobile money rather than in kind reduced recipients’ time costs in Niger: relative to manual or in-kind distribution, digital delivery saves time, but it does not eliminate the last-mile cost of reaching an agent to cash out, and it is that residual cost that we isolate.

The paper also relates to the take-up and ordeal literature, which asks why eligible households fail to access programmes that would benefit them (Nichols and Zeckhauser, 1982; Bertrand, Mullainathan and Shafir, 2006; Currie, 2006; Kleven and Kopczuk, 2011; Alatas et al., 2016; Deshpande and Li, 2019; Homonoff and Somerville, 2021; Finkelstein and Notowidigdo, 2019). In that framing the friction is a hassle cost capturing paperwork, stigma, or travel to an enrolment office—a one-time cost paid at the point of claim—and the evidence comes overwhelmingly from high-income settings, with Alatas et al. (2016) a notable exception in Indonesia. We add a structurally different friction: a cost paid not at enrolment but at every subsequent transaction with the programme. It scales with payment frequency, so its cumulative weight can rival the posted price of the service itself, and it is governed by the density of the payment network rather than by the design of the application process—placing its reduction in the domain of infrastructure policy. Finkelstein and Notowidigdo (2019) note that the shape of the cost distribution, not just its level, matters for the welfare consequences of take-up interventions; we document a sharp discontinuity at the walking threshold that follows directly from the structure of rural transport.

Finally, we contribute to work on electricity demand and PAYGO electrification in low-income settings (Lee, Miguel and Wolfram, 2020; Grimm et al., 2020; Burgess et al., 2023; Gertler, Green and Wolfram, 2024; Gertler et al., 2023; Lang, 2025a,b). Prior work documents high sensitivity of adoption to upfront connection prices, with low take-up even at heavily subsidized rates in both grid and off-grid markets. The PAYGO model—

now dominant in off-grid solar provision in sub-Saharan Africa—was designed to relax these upfront liquidity constraints by replacing a large one-time payment with a stream of smaller, recurring ones. We show that while PAYGO does mitigate upfront cost barriers, it introduces a new friction: households must make frequent payments through a payment network that is costly to reach physically. The payment-access cost therefore creates a wedge between posted prices and the effective cost of electricity, suppressing adoption and dampening measured price elasticities, particularly in remote areas. [Lang \(2025a\)](#), using the same subsidy policy, establishes that adoption is highly sensitive to per-unit electricity prices; we take this price sensitivity as our starting point and demonstrate that the payment-access cost is an equally important component of the effective price.

The remainder of the paper is organized as follows. Section 1 describes the setting and the two policies. Section 2 presents the conceptual framework. Section 3 describes the data and empirical strategy. Section 4 details the empirical specifications. Sections 5 and 6 present the main results on adoption and mechanisms driving the complementarity. Section 7 presents results from the Novissi cash transfers policy. Section 8 discusses policy implications and Section 9 concludes.

# 1 Context and Policies

## 1.1 Rural Electrification in Togo

Togo, with a population of 8.5 million and a GDP per capita of \$2,200 (PPP, 2017), relies heavily on agriculture: the sector employs the majority of the labour force and contributes around 29% of GDP. In 2015, over half of the population lived below the international poverty line of \$1.90 per day, and poverty is disproportionately concentrated in rural areas.

While global electricity access has risen to around 90%, access in Sub-Saharan Africa remains low, and Togo is no exception. Urban electrification has improved, with roughly 45% of households connected to the grid, but rural areas remain starkly underserved, with just 7% of rural households electrified ([Government of Togo, 2016](#)). This leaves almost one million Togolese households without electricity, the vast majority of them in rural areas.

In response, the Government of Togo adopted a national electrification strategy known as *CIZO* (“light up” in Mina) in 2017, which sets the objective of achieving universal access to electricity by 2030. The strategy combines grid extension in more densely populated areas with decentralised solutions. Extending the grid to sparsely populated rural areas is costly, and low average consumption limits the financial viability of traditional

grid-based expansion. As a result, the policy anticipates that at least half of newly electrified households will be served by off-grid solutions, primarily mini-grids and solar home systems, especially in rural and low-density areas. A central pillar of the off-grid programme is the deployment of solar home systems to approximately 550,000 mostly rural households. These off-grid efforts, and in particular the solar home system component, form the core policy environment for our study.

## 1.2 Solar Home Systems

The recent rise of cost-effective off-grid solutions, particularly solar panels, has changed the landscape of rural electrification. The cost of photovoltaic modules has fallen from well over \$70 per watt in the late 1970s to around \$2 per watt in 2010 and roughly \$0.19 per watt in 2020, a reduction of more than 99%. These dramatic cost declines have created new opportunities to electrify remote areas where grid extension is prohibitively expensive and have made stand-alone solar home systems (SHS) a viable technology for rural households in low-income countries. Within Togo's electrification strategy, SHS have therefore emerged as a central off-grid technology for reaching dispersed rural households.

Off-grid solar solutions differ from grid connections along several dimensions. (1) *Flexibility*. Off-grid systems can be deployed quickly at the household or village level, without large investments in transmission and distribution infrastructure. (2) *Usage*. SHS typically support low- to medium-power appliances—such as general lighting, phone charging, radios, fans, and small televisions—whereas grid connections or larger solar installations are needed for high-power appliances and productive or industrial use. (3) *Reliability*. Urban areas in Togo suffer frequent grid outages ([Kpemoua, 2016](#)), so well-sized solar systems can offer more reliable power, although shortages may still occur in periods of consecutive heavy rainfall, especially for medium-power appliances.<sup>2</sup> While off-grid solar presents a flexible and potentially more reliable alternative, its affordability remains a challenge for poor rural households, who typically rely on subsistence agriculture and lack access to formal credit.

These constraints have spurred the development of *pay-as-you-go* (PAYGO) SHS offered by private companies across Sub-Saharan Africa, which allow households to acquire solar systems through a rent-to-own contract rather than a large one-off purchase. Poor rural households in Togo typically rely on subsistence agriculture, face substantial income risk, and have limited access to formal credit. Under the PAYGO model, customers first

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<sup>2</sup>Shortages may still happen with solar solutions in areas experiencing consecutive heavy rainy days, particularly when households use medium-power appliances.

select a bundle and make a modest upfront downpayment, which secures the installation of the solar panel, battery, and chosen appliances in their home as well as the first month of electricity. After installation, customers make recurring payments over time, renting access to the system while gradually building ownership. The total stream of payments is structured so that, after roughly three years of regular payments and continuous usage, the customer owns the system outright. This study uses data from the largest SHS provider in Togo. The “basic” kit bundle consists of a small solar panel, a battery, three light bulbs, and a phone charger. The downpayment, which covers installation and the first 30 days of electricity, is 8,960 CFA (approximately \$15), followed by a daily rate of 160 CFA (about \$0.27), equivalent to roughly \$8 per month (using an exchange rate of 600 CFA per US dollar). The SHS provider also offers “plus” and “premium” kits with additional appliances: the plus kit includes a radio, while the premium kit includes both a radio and a TV.<sup>3</sup>

Unlike most grid electricity payment systems, the PAYGO rent-to-own model allows customers flexibility in when and how much they pay over time. Customers purchase electricity in daily units, gaining unlimited access to their SHS for the number of days purchased. If access time expires, the solar company can remotely lock the SHS, preventing further use until additional days are bought—a form of “digital collateral” (Gertler, Green and Wolfram, 2024). Customers who do not make payments for over sixty consecutive days are considered in default, and the company may repossess the SHS after 120 days of inactivity although in practice this is not common. The combination of remote lockout and the credible threat of repossession makes PAYGO contracts enforceable even in settings with weak institutions, as observed in similar systems in Rwanda (Lang, 2025b).

### 1.3 Financial Inclusion, Mobile Money, and Digital Payments

Overall financial inclusion in Togo remains low. In 2017, only 45% of adults held an account at a formal financial institution or with a mobile money provider, and account ownership was concentrated in urban areas (World Bank Group, 2017). At the same time, the country’s digital landscape changed rapidly over the 2010s under an explicit government strategy to promote digital transformation. The mobile penetration rate increased from 40% in 2011 to 78% in 2021, internet penetration rose from less than 5% to 75%, and the share of adults with a mobile money account increased from essentially zero in 2011 to about 58% in 2021. Between 2014 and 2017 alone, the share of adults with a mobile money

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<sup>3</sup>The downpayment for the plus kit is 12,320 CFA (approximately \$20.50) and the implied monthly payment is about \$11. The downpayment for the premium kit is 21,000 CFA (around \$35), and the implied monthly payment is about \$19.

account grew from 1% to 21%, making mobile money the main driver of rising financial inclusion over this period. The government has also used digital payments at scale for social protection—for example, the Novissi programme distributed digital cash transfers to informal workers during the COVID-19 pandemic—further familiarising households with mobile money and strengthening the underlying infrastructure.

PAYGO SHS contracts in Togo are built around this expanding digital payments infrastructure. Customers pre-pay for electricity access using their mobile phones via mobile money: they initiate a payment from their mobile money wallet, which immediately credits their SHS account and adds days of electricity access. Mobile operators such as Togocel and Moov in Togo, and their respective mobile money services (TMoney and Flooz), are therefore critical partners for SHS providers. Digital payments are particularly attractive in rural areas, where households are geographically dispersed and cash collection by the firm would otherwise be extremely costly.

However, most rural Togolese households still earn and transact primarily in cash, so mobile money agents serve as the key physical payment infrastructure that enables digital transfers. To fund their mobile money wallets, they must visit a mobile money agent, deposit cash, and have the agent credit their account. As a result, accessing digital payments still requires physical travel. In rural Togo, mobile money agents are typically located in larger villages or small towns, so many households face non-trivial distances to reach the nearest agent.<sup>4</sup>

We document that these distances are large in practice. Figure A1 plots the distribution of village-level distances to the nearest mobile money agent at the start of our study period and shows that this distance is often substantial, with a median of 6.7 km. For households that both live far away from agents and must make frequent payments due to liquidity constraints, this generates a sizeable transaction cost in the electricity payment process.

## 1.4 Policy 1: Nationwide Solar Home System Subsidy

In 2019, the Government of Togo launched a nationwide demand-side subsidy for solar home systems called *CIZO Cheque* as part of the CIZO programme to achieve universal access to electricity by 2030. As described in Section 1.1, the national electrification strategy foresees roughly half of new connections coming from off-grid technologies, with SHS playing a central role in sparsely populated rural areas. This was the first government subsidy in Africa explicitly designed to support solar energy payments. The first

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<sup>4</sup>In Rwanda, Lang (2025b) find that most PAYGO customers visit a mobile money agent for every solar payment, rather than saving money in their mobile wallets. We find a similar pattern from qualitative interviews and field visits in rural Togo, a comparable context.

off-grid SHS company only began operating in Togo in 2017, so the government introduced a large public subsidy to accelerate SHS adoption and make monthly payments more affordable for low-income households.

The Togolese programme is distinctive in that it offers an untargeted end-user subsidy, rather than a pure supply-side results-based financing scheme. The subsidy takes the form of a uniform price reduction of \$0.12 (CFA 70) per day of electricity purchased (or \$3.50 per month), regardless of the SHS bundle chosen. For the basic kit described in Section 1.2, this corresponds to a reduction in the effective monthly cost from about \$8 to \$4.50, a price cut of roughly 44%. For the plus kit, the subsidy reduces the monthly cost from around \$11 to about \$8 (a 30% reduction), and for the premium kit from roughly \$20 to about \$16 (an 18% reduction). Given that monthly GDP per capita in Togo was around \$57 in 2019, the subsidy represents a sizeable transfer for rural households.

Operationally, the subsidy leaves the initial downpayment and installation unchanged. Customers still pay the full upfront cost, including the first month of electricity. For subsequent months, the government matches customers' electricity purchases up to a ceiling of roughly \$3.50 per month: customers pay their usual PAYGO tariff, and the government tops up the remaining amount so that a full month of access is unlocked. Customers who do not make payments, or who default on their contracts, do not receive any subsidy. The subsidy is implemented digitally, via a national platform linking SHS providers, mobile operators, and La Poste, consistent with Togo's broader digitalisation strategy.

A key feature of this policy for our purposes is its staggered rollout. Between March and July 2019, the subsidy was introduced in three successive cohorts of districts: Phase 1 in March, Phase 2 in May, and Phase 3 in July (see Panel A of Figure 1). Each cohort became eligible for the subsidy on its phase-specific start date, while the rest of the country continued to face pre-subsidy prices. This scattered rollout across space and time provides quasi-experimental variation in exposure to the subsidy, which we exploit to identify its causal effects using an event-study design described in Section 4.1.

In principle, all rural households using PAYGO SHS were eligible to benefit from the subsidy, conditional on making payments. In practice, not all customers actually received the subsidy on every payment. To operationalise the programme and promote digital financial inclusion, the government required that SHS payments be made through a *personal* mobile money account that was linked to the customer's solar account. This design facilitated automated top-ups and digital monitoring, but it also created frictions in the early months of the programme. Administrative and technical issues in reconciling beneficiary lists between SHS companies, mobile operators, and the government agency

meant that some customers were not correctly flagged as eligible when they made payments, and therefore did not receive the top-up. As a result, only a fraction of payments were effectively subsidised, especially at the start of the rollout (see Figure 2).

## 1.5 Policy 2: Mobile Money Agent Network Expansion Policy

The second policy we study is a large-scale, supply-side expansion of the mobile money agent network, designed primarily to expand access to digital financial services in rural areas and, in doing so, to lower the costs of using mobile money. In late 2019, shortly after the SHS subsidy rollout, the Togolese government launched the financial inclusion programme *Réseau Granulaire* (Granular Network). Under this programme, the national postal operator, La Poste, recruited, trained, and hired its own agents to provide mobile money services, deploying them across more than 200 villages nationwide. The programme explicitly targeted rural localities that previously lacked nearby access to mobile money agents. Importantly, the subsidy was already in place nationwide when the agent expansion began, so we identify only the effects of mobile money agent arrival, conditional on the presence of the subsidy.

The expansion was implemented in a decentralised and staggered manner. Regional supervisors at La Poste were given discretion to identify villages with poor access to agents, subject to broad programme guidelines, and new agents were then rolled out progressively across villages from September 2019 onwards. This process created substantial variation in the timing of the first local agent arrival across villages. In total, 101 villages in our data received their first mobile money agent through this programme between September 2019 and December 2021 (see Panel B of Figure 1). We define a village as gaining access when an agent first opens within 3 km of the village centre, a distance within which customers can typically walk without incurring monetary travel costs.

The timing of the agent expansion exhibits two distinct phases due to the COVID-19 pandemic (see Figure 3). From September 2019 to March 2020, there was a continuous expansion of the mobile money agent network, with new agents opening every couple of months in different villages. With the onset of COVID in April 2020, the programme was abruptly paused, and no new agents were added in rural areas for the remainder of 2020. The expansion then resumed in early 2021, with a second wave of deployments. We exploit this staggered rollout in our empirical design. In particular, we use the discrete timing of the first agent arrival in each village to study how improved access to mobile money agents affects SHS adoption.

## 2 Theoretical Framework

This section develops a simple framework linking the three policy settings studied in the paper: the solar subsidy, the expansion of mobile money agents, and the Novissi cash transfer programme. The central idea is that when a policy is delivered or paid for through mobile money, its effective value depends not only on the nominal price or transfer amount, but also on the cost of reaching the payment network.

### 2.1 Setup

Let  $d$  denote the distance from a household’s village to the nearest mobile money agent. Each time a household transacts through mobile money—whether making an electricity payment or cashing out a government transfer—it incurs a cost  $c(d)$ , encompassing transport expenses, travel time, and forgone earnings. We assume  $c(0) = 0$  and  $c'(d) \geq 0$ : costs are increasing in distance. Households within walking distance of an agent (roughly 0–3 km) incur negligible monetary cost; beyond this threshold, reaching an agent typically requires hiring a moto-taxi, generating a discrete jump in costs.<sup>5</sup>

A household’s decision to adopt a solar home system depends on two independent conditions: (i) willingness to pay for electricity exceeds the price, and (ii) the household can complete the required mobile money transactions. Household  $i$  has willingness to pay  $\theta_i \sim F$  (with density  $f$  and complementary cumulative distribution function  $\bar{F}$ ) and transactional capacity  $\kappa_i \sim G$  (with density  $g$  and complementary cumulative distribution function  $\bar{G}$ ) that must exceed the transaction cost  $c(d)$ . We interpret  $\kappa_i$  as the household’s capacity to absorb per-transaction costs—reflecting phone and mobile money account ownership, the time available for travel, and familiarity with the payment system—expressed in the same monetary units as  $c(d)$ . Aggregate demand is:

$$D(p, d) = \underbrace{\bar{F}(p)}_{\text{affordability}} \times \underbrace{\bar{G}(c(d))}_{\text{accessibility}}. \quad (1)$$

The multiplicative structure captures the key feature of our setting: affordability and accessibility are independent barriers. A household that is willing to pay the price but cannot reach an agent does not adopt; nor does one that can easily reach an agent but finds the price too high.

We maintain that households face binding liquidity constraints. In rural Togo, where most households earn small and irregular incomes from subsistence agriculture, they can-

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<sup>5</sup>Our transport cost survey documents one-way moto-taxi fares with a fixed component of roughly 350 CFA plus 50 CFA per kilometre—about 100 CFA per kilometre all-in at typical distances—generating a step-function cost structure at the walking threshold. See Section 3.5.

not pay the full price of a solar home system in a single period. Instead, they must make repeated payments via mobile money, each of which incurs the cost  $c(d)$ .<sup>6</sup>

## 2.2 Complementarity between Subsidies and Agent Access

A subsidy  $s$  reduces the effective price from  $p$  to  $p - s$ . The marginal effect of the subsidy on adoption is:

$$\frac{\partial D}{\partial s} = f(p - s) \cdot \bar{G}(c(d)), \quad (2)$$

which is strictly larger in villages closer to agents (lower  $d$ , higher  $\bar{G}$ ). Conversely, reducing distance through agent expansion increases adoption by:

$$\frac{\partial D}{\partial(-d)} = \bar{F}(p - s) \cdot g(c(d)) \cdot c'(d), \quad (3)$$

which is larger when the price is lower (higher  $\bar{F}(p - s)$  in subsidised areas). We evaluate at the effective price  $p - s$  because, in our setting, the agent expansion occurred with the subsidy already in place nationwide. The cross-partial derivative captures structural complementarity:

$$\frac{\partial^2 D}{\partial s \partial(-d)} = f(p - s) \cdot g(c(d)) \cdot c'(d) > 0. \quad (4)$$

This is positive wherever the densities  $f$  and  $g$  are positive and  $c'(d) > 0$ : subsidies and agent proximity are mutually reinforcing because they address independent barriers to adoption.<sup>7</sup> Intuitively, adoption requires clearing two independent barriers—affordability and accessibility—so relaxing one has larger marginal returns when the other is already relaxed. The subsidy brings households above the price threshold, but only those that can also reach an agent will adopt; conversely, an agent unlocks transactional access, but marginally more for households that can also afford the price. Each instrument amplifies the other because it expands the set of households for whom the other instrument is the binding constraint.

Because rural transport costs are closer to a step function than to a smooth gradient (Section 3.5), it is useful to restate the result in discrete form, which requires no differentiability. Let  $c_{\text{near}} < c_{\text{far}}$  denote transaction costs with and without a nearby agent. The

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<sup>6</sup>In a comparable PAYGO setting in Rwanda, [Lang \(2025b\)](#) find that most customers visit a mobile money agent for every solar payment rather than maintaining balances in their wallets. We observe a similar pattern in qualitative interviews and field visits in rural Togo.

<sup>7</sup>Independence of  $\theta_i$  and  $\kappa_i$  is needed only for the product decomposition in Equation 1, not for the sign of the complementarity. For an arbitrary joint distribution,  $D = \Pr(\theta_i > p - s, \kappa_i > c(d))$  and  $\partial^2 D / \partial s \partial(-d) = f_\theta(p - s) \cdot f_{\kappa|\theta}(c(d) | p - s) \cdot c'(d) \geq 0$ , where  $f_{\kappa|\theta}$  is the conditional density. Positive correlation between willingness to pay and transactional capacity—for example, because both rise with wealth—therefore does not overturn the complementarity.

difference-in-differences of adoption with respect to the subsidy and agent proximity is:

$$[\bar{F}(p - s) - \bar{F}(p)] \times [\bar{G}(c_{\text{near}}) - \bar{G}(c_{\text{far}})] > 0, \quad (5)$$

which is positive for any marginal distributions  $F$  and  $G$ : demand is supermodular in the subsidy and agent proximity. This discrete statement is the object our event-study designs estimate.

**Complementarity versus non-linearity.** We now distinguish this structural complementarity from a similar phenomenon. Consider an alternative model where price and transaction costs enter the adoption threshold additively: household  $i$  adopts if  $y_i > p + c(d)$ , where income  $y_i \sim H$  with density  $h$ , so that  $D = \bar{H}(p + c(d))$ . The cross-partial with respect to  $s$  and  $(-d)$  is then  $-h'(p + c(d)) \cdot c'(d)$ , which is positive only if  $h'(p + c) < 0$ —that is, only on the right tail of a unimodal income distribution. In this model, the apparent interaction is an artefact of distributional curvature: reducing  $c$  shifts the threshold to a higher-density region where price reductions mechanically affect more households. The effect can reverse sign if the threshold falls on the increasing portion of the density.

The distinction matters for policy. Under structural complementarity (the multiplicative model), deploying agents and subsidies together generates gains above and beyond what either achieves alone, regardless of context. Under non-linearity alone (the additive model), the apparent interaction is contingent on where in the income distribution the adoption threshold falls.

The two models are distinguished by how the subsidy’s effect scales with access. Under the multiplicative model, the subsidy’s *proportional* effect on adoption,  $[\bar{F}(p - s) - \bar{F}(p)]/\bar{F}(p)$ , is invariant to agent access for any distributions  $F$  and  $G$ : access scales the *level* of the response through  $\bar{G}(c(d))$  but leaves its ratio to baseline adoption unchanged. Under the additive model, the proportional effect varies with  $c(d)$  except in the knife-edge case of an exponential (constant-hazard) distribution. We implement this ratio test in Section 5.2, and use the distance-band analysis in Section 6.1.2 to test the step-function cost structure itself (Prediction P3).

## 2.3 Payment Frequency and the Intensive Margin

The complementarity above operates on the extensive margin—whether a household adopts. A second channel operates through how existing customers pay. Define the total monthly cost of electricity as:

$$P(p, \tau) \equiv p + n(p, \tau) \cdot \tau, \quad (6)$$

where  $n$  is the number of payments per month and  $\tau = c(d)$  is the per-trip transaction cost. If payment frequency were fixed, changes in  $p$  and  $\tau$  would affect  $P$  additively and there would be no complementarity. But under the PAYGO model, customers choose how often to pay and so  $n$  is a function of  $p$  and  $\tau$ . The cross-partial of  $P$  is:

$$\frac{\partial^2 P}{\partial p \partial \tau} = n_p + \tau n_{p\tau}, \quad (7)$$

where  $n_p \equiv \partial n / \partial p$  and  $n_{p\tau} \equiv \partial^2 n / (\partial p \partial \tau)$ . The first term,  $n_p$ , captures how a price reduction changes payment frequency, holding transaction costs fixed. When  $p$  falls, households can afford the same electricity with fewer, larger payments, economising on trips:  $n_p > 0$ . This makes the two instruments substitutes in levels, since fewer payments reduce the importance of  $\tau$ .

The second term,  $\tau n_{p\tau}$ , captures how transaction costs shape the price-responsiveness of payment frequency. When  $\tau$  is high (no nearby agent), payment schedules are rigid and households have little scope to adjust frequency in response to price changes ( $n_p$  is small). When  $\tau$  is low (agent nearby), payment flexibility is greater and  $n$  responds more strongly to price reductions. This implies  $n_{p\tau} < 0$ , so  $\tau n_{p\tau} < 0$ , pushing the cross-partial toward complementarity.<sup>8</sup> Whether the flexibility effect dominates the level effect is theoretically ambiguous. In the canonical inventory model of payment timing (Baumol, 1952; Tobin, 1956), the level effect dominates exactly ( $\tau n_{p\tau} = -n_p/2$ ), so the two instruments are substitutes in  $P$ ; more generally, when  $n$  is chosen to minimise the full cost of paying for electricity, the envelope theorem implies that the cross-partial of the minimised cost equals  $n_p > 0$ . Super-additive reductions in total cost therefore require a departure from a smooth payment-timing trade-off; the frequency floor of footnote 8 is one such departure.

The robust, testable content of this section therefore concerns payment frequency itself. The framework generates three intensive-margin predictions: the subsidy should reduce payment frequency ( $n_p > 0$ ); the reduction should be *larger* in villages with lower transaction costs ( $n_{p\tau} < 0$ ); and, symmetrically, the arrival of an agent should *increase* payment frequency among existing customers ( $n_\tau < 0$ ).

## 2.4 Cash Transfer Effectiveness

The transaction-cost principle extends to any policy delivering benefits through mobile money, though it operates differently for transfers than for PAYGO adoption. For adop-

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<sup>8</sup>The sign follows from the floor on payment frequency: at baseline, half of customers average just one payment per month, so households facing high  $\tau$  are already at or near the minimum feasible frequency and cannot consolidate further when the price falls, while households near an agent can. The same floor implies  $n_\tau < 0$ : when an agent arrives, customers move off the floor toward more frequent payments.

tion, agent access is a *gate*: a household that cannot transact does not adopt at all, which is what generates the multiplicative structure of Equation 1. For a transfer already received, access is a *tax* on value rather than a precondition for it. The intensive-margin channel of Section 2.3 likewise has only a partial analogue here—beneficiaries choose how often to cash out rather than how often to pay—which we note below but do not test, as we do not observe beneficiaries’ cash-out behaviour.

A beneficiary at distance  $d$  who receives a digital transfer  $S$  must visit an agent to cash out, incurring  $c(d)$ . The effective transfer is  $S - c(d)$ , and the treatment effect on spending-dependent outcomes is:

$$\text{TE}(d) = \beta \cdot (S - c(d)) \quad \text{for } S > c(d). \quad (8)$$

Treatment effects are decreasing in distance: beneficiaries farther from agents retain less of the transfer after cash-out costs. For  $S \leq c(d)$ , a transfer cannot profitably be cashed out in the month it arrives. In practice, beneficiaries could accumulate several monthly transfers before making a single trip, in which case the relevant cost is  $c(d)/k$  per transfer for a beneficiary cashing out every  $k$  months. This is the cash-transfer analogue of the payment-frequency margin in Section 2.3: cash-out frequency falls with distance, trading transaction-cost savings against delayed consumption. Either way, the effective value of the transfer declines with  $c(d)$ . Because accumulation partially offsets distance costs, the access gradient we estimate is net of this coping margin and therefore understates the gross per-trip friction.

For outcomes that do not require converting the transfer into cash—such as mental health, which may respond to the psychological reassurance of receiving support—the treatment effect may not directly depend on  $c(d)$ :

$$\text{TE}_{\text{non-monetary}}(d) = \gamma \cdot S. \quad (9)$$

For such outcomes the framework predicts a weaker interaction with mobile money access, or none at all.

## 2.5 Predictions

The framework generates five testable predictions:

**P1. Subsidy  $\rightarrow$  adoption.** The SHS subsidy increases adoption. The effect is larger in villages closer to mobile money agents (Equation 2).

**P2. Agents  $\rightarrow$  adoption.** The deployment of mobile money agents directly increases

adoption by lowering  $c(d)$ . The effect is larger in subsidised areas (structural complementarity, Equation 4).

**P3. Walking threshold.** The subsidy effect drops discretely once the nearest agent is beyond walking distance ( $\sim 3$  km), and changes little with additional distance thereafter, reflecting the step-function cost structure of rural transport.

**P4. Payment frequency.** The subsidy reduces payment frequency as households buy electricity in bulk. This reduction is larger in villages with a nearby agent, where payment schedules are more flexible; conversely, agent arrival increases payment frequency among existing customers (Section 2.3).

**P5. Cash transfers  $\times$  access.** The treatment effect of Novissi on spending-dependent outcomes (financial health, health care access) is larger in cantons with better mobile money access. No interaction is predicted for non-monetary outcomes (mental health).

### 3 Data and Summary Statistics

In this section, we describe our data sources and present summary statistics.

#### 3.1 Solar Home System Data

We obtain household-level administrative data from the largest provider of off-grid solar home systems (SHS) in Togo. The data include information on customer applications and installation dates, as well as household-level registration details such as basic characteristics and previous energy sources. In addition, we observe transaction-level records on customer payments and SHS usage, which allow us to track repayment behaviour and patterns of electricity consumption over time.

The company data covers 49,046 households across 2,212 villages from November 2017 up to April 2021. During the study period, there were two major off-grid solar companies in Togo, which eventually expanded to five in 2020. Given the similar products and pricing structures, we believe the data is reasonably representative of the SHS population in Togo.

Summary statistics here in Table 1. **TO UPDATE PROPERLY** Table A3 presents descriptive statistics for households in February 2019 (before the subsidy launch) and May 2020 (about a year after the launch). At baseline, the average household head was 41 years old; 78% worked in agriculture, 10% owned a business, and 11% were employees.

Before adopting SHS, 53% of households used kerosene, lanterns, or had no access to energy, while 44% relied on flashlights. Fewer than 2% had access to electricity through the grid, a generator, or solar energy.

From the transaction-level data, we constructed several variables to track electricity usage over time. Table A3 details the bundle choices customers made at registration. They chose between three options: (i) the basic kit (a small solar panel with three light bulbs and a phone charger), (ii) the intermediary kit (basic kit plus a radio), and (iii) the premium kit (intermediary kit plus a TV). Conceptually, we consider that households first decide whether or not to adopt the solar panel at the proposed basic kit's price. After making this decision, they add additional items at their convenience like a radio or TV, increasing the overall price. The share of households selecting the basic kit increased from 32% at baseline to 46% in May 2020.

We measured the number of payments made each month, the average payment size, and how these payments translated into electricity consumption, as indicated by the utilization rate. A 100% utilization rate means the customer purchased electricity for every day of the month. While there is some variation across customers, the average utilization rate was 72% at baseline and 74% at endline. Most customers in our estimating sample had electricity for most of the month, with an average of 27 days and a notable mode of 30 days. In the four months before the subsidy rollout, households made on average 1.63 payments per month, with significant variance. Notably, 51% of the sample made one payment per month on average, suggesting they bought in bulk, while the rest made two or more payments per month.

### 3.2 Subsidy Data

We obtain administrative data on the SHS subsidy from La Poste, which operates the national CIZO platform on behalf of the Government of Togo. The dataset contains transaction-level records for all subsidy disbursements (top-ups) processed during our study period, allowing us to observe, for each SHS payment, whether a subsidy was actually credited. On average, around 80% of SHS payments made by customers received the intended top-up, indicating that the CIZO system successfully channelled subsidies for the vast majority of transactions. At the same time, this implies that roughly 20% of payments were not subsidised in practice; as a result, effective subsidy outreach was somewhat more limited (see Figure 2).

### 3.3 Mobile Money Agent Network Data

We obtain administrative data from the Ministry of Public Service Efficiency and Digital Transformation, which cover the full universe of approximately 13,000 mobile money agents in Togo that are part of the Togocel and MOOV networks. The dataset reports the longitude and latitude of every registered agent, together with information on their presence over time, from 2010 to 2024 at monthly or yearly frequencies. These data allow us to construct time-varying village-level measures of access to mobile money agents.

Throughout the paper, we say that a village has a mobile money agent—or easy access to one—if at least one agent is located within a 3 km radius of the village centre. We use 3 km because it is a plausibly walkable distance and is conservative, given that we lack village boundary data and households can live quite far from the village centre. Moreover, villages are sometimes located close to one another, so this radius-based measure performs well in practice.<sup>9</sup> In January 2019—prior to the subsidy rollout—only 516 villages (23% of our sample) had a mobile money agent, while the remaining 1,696 villages had limited access. Panel A of Figure 1 shows the spatial distribution of villages by agent access and reveals substantial variation across the country.

### 3.4 Mobile Money Agent Policy Expansion

We obtain administrative data on the large-scale mobile money agent expansion programme implemented by La Poste and the Togolese government (Section 1.5). These data cover all La Poste mobile money agents recruited under the *Réseau Granulaire* initiative and report the geographic coordinates and an approximate date on which the agents first became operational. We use this information to identify the villages that gained access to a La Poste agent through the programme and to construct village-level timelines of agent arrival for our empirical analysis. Consistent with our definition of a village having a mobile money agent, we say that the policy introduces an agent to a village when a new agent is posted within 3 km of the village centre.

### 3.5 Transport Costs Data

We collect original data on transport costs from moto-taxi drivers in 34 rural villages across Togo’s five regions. For each village, we record the price, distance, and travel time to multiple nearby villages, yielding 126 origin–destination observations. Rural residents, including SHS customers, frequently rely on motorbike taxis to reach neighbouring villages when walking is too time-consuming, so these data provide direct evidence on the cost of travelling to a mobile money agent. We use them to quantify the monetary sav-

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<sup>9</sup>In our analysis, we also show that the results are robust to alternative distance thresholds.

ings from consolidating electricity payments into fewer transactions, thereby reducing the number of trips required. Since the survey was conducted in 2023, we deflate reported prices to 2019, the period of analysis. See Table [A15](#).

Using these data, we estimate that travel costs to mobile money agents account for approximately 28% of the total cost of electricity in rural Togo, rising to around 43% in the most remote regions (75th percentile). This estimate only includes transportation costs, and therefore likely understates the full transaction cost burden.<sup>10</sup>

### 3.6 Control Variables

We include three main controls. First, we measure village-level wealth across Togo by using data from Meta’s Relative Wealth Index, a global geospatial database developed by [Chi et al. \(2022\)](#). This index combines household survey data with non-traditional sources like satellite imagery, cellular network data, topographic maps, and privacy-protected Facebook connectivity data. Machine learning algorithms predict relative wealth at a fine resolution of 2.4 km<sup>2</sup>. Importantly, this data captures *relative* wealth within each country, enabling us to compare wealth across villages in Togo. Since not every village directly corresponds to a wealth cell, we calculate the wealth score for each village using the nearest cell. Second, we measure village-level population density—defined using a 3km radius around the village coordinates—using gridded data from WorldPop.<sup>11</sup> Third, we measure the village-level distance to the nearest market using data on the universe of markets for Togo in order to capture market proximity and access.

## 4 Empirical Specifications

In this section, we lay out the core empirical specifications and identification strategy. Our primary analysis is conducted at the village level, which is the smallest geographic unit available. For both policies, we use an event-study design that exploits the staggered rollout of the interventions over time and across locations. We estimate dynamic effects using the interaction-weighted (IW) estimator of [Sun and Abraham \(2021\)](#), which

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<sup>10</sup>To obtain these estimates, we first calculate the total distance households travel each month to make payments. We multiply the median distance to the nearest mobile money agent prior to the study period by the average number of payments customers make each month, and then by two to account for round trips. We then multiply this total travel distance by the median price per kilometre for hiring a moto taxi, which we measure in our survey of transport costs in rural villages.

<sup>11</sup>We use WorldPop population density estimates for Togo, which provide the number of people per square kilometre at 30 arc-second resolution (approximately 1 km at the equator). The data are produced in WGS84 using a Random Forest-based dasymetric redistribution approach and are adjusted to match official United Nations population estimates from the 2019 Revision of the World Population Prospects (DOI: 10.5258/SOTON/WP00675).

is designed for settings with staggered treatment timing and is robust to treatment effect heterogeneity across cohorts and cross-lag contamination. We adopt this approach in place of a standard two-way fixed effects (TWFE) model, which can produce misleading estimates under staggered rollouts (Borusyak, Jaravel and Spiess, 2021; De Chaisemartin and d’Haultfoeuille, 2020; Goodman-Bacon, 2021). Throughout we use the “last treated” group as the control group, as recommended by Sun and Abraham (2021).

Our identification strategy for both policies relies on two key assumptions. First, the parallel trends assumption, which requires that, in the absence of the policy, the difference in outcomes between treated and control villages would have evolved similarly over time (Angrist and Pischke, 2008). Second, we assume that there was no anticipatory behaviour. We provide a partial test of these assumptions by evaluating pre-trends, using the same number of pre-treatment periods as post-treatment periods.

#### 4.1 Event-Study Analysis of the Subsidy Policy

The subsidy policy was announced in February 2019 and rolled out over six months, reaching different groups of districts at three different times (see Figure 1). This staggered rollout provides natural variation to evaluate the initial impact of the policy. The timeline for the phased rollout is as follows:

- *Phase 1:* March 2019: Subsidy begins in 11 districts (826 villages).
- *Phase 2:* May 2019: Subsidy begins in an additional 13 districts (677 villages).
- *Phase 3:* July 2019: Subsidy begins in the final 12 districts (709 villages).

The Government of Togo first launched the policy in districts with the lowest electrification rates (0–10%), then extended it to districts with 11–20%, and finally to those with 21–40%, based on 2016 data. Table A4 shows increasing baseline electrification rates across the three district groups according to the 2016 government figures.<sup>12</sup> By 2019, however, the 2016 data were outdated, and more arbitrary considerations influenced the timing of rollout. In particular, the national coordinator sometimes delayed the subsidy in districts that were thought to have better grid access by 2019.

Although the timing of the district-level rollout was not random—since districts were grouped by baseline electrification rates—these differences in overall access need not translate into systematic differences among off-grid rural households, which are relatively homogeneous across Togo, a small country. To assess whether the staggered

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<sup>12</sup>Since the data were provided in ranges, we use the midpoint of each range to calculate district averages. All districts in Group 1 are in the 0–10% range, hence there is a 5% mean with no standard deviation.

district-level rollout does in fact generate compositional differences in the rural households served by the SHS provider, we examine baseline balance in household characteristics and pre-subsidy electricity behaviour across the three groups (Table A1). Overall, the three district groups show limited baseline differences in the characteristics of pre-subsidy customers. There are modest differences in income sources—customers in Group 3 are less likely to work in agriculture (67%) and more likely to own a business (16%) compared to Groups 1 and 2—but there are no major differences in previous energy sources, with low electricity access common across all groups. Pre-subsidy electricity behaviour is also similar, although Group 3 customers are somewhat more likely to choose the premium kit and make a slightly higher number of payments. Taken together, this suggests that, despite differences in average district electrification rates, the rural communities in which SHS are adopted are relatively similar across rollout phases.

**We should re-do this balance table and not do district level, but customer or even village level**

We estimate the following village-level regression:

$$Y_{vdt} = \lambda_t + \theta_v + \sum_{e \in \{1,2\}} \sum_{l=-3, \neq -1}^3 \beta_e^l \mathbb{1}\{Sub_{vd} = e\} \cdot D_{vdt}^l + \epsilon_{vdt}, \quad (10)$$

where  $Y_{vdt}$  denotes an outcome for village  $v$  in district  $d$  during month  $t$ . In our main specification,  $Y_{vdt}$  is the number of applications to purchase a SHS in village  $v$  in month  $t$ . The terms  $\lambda_t$  and  $\theta_v$  are month and village fixed effects, respectively. The indicators  $\mathbb{1}\{Sub_{vd} = e\}$  are dummies for whether village  $v$  in district  $d$  belongs to subsidy rollout cohort  $e \in \{1, 2\}$ , corresponding to districts that became eligible in March and May, respectively. Following [Sun and Abraham \(2021\)](#), villages in the last set of districts to become eligible (Group 3, July rollout) serve as the “last treated” control group. The variables  $D_{vdt}^l = \mathbb{1}\{t - T_{vd}^{\text{sub}} = l\}$  are relative-time indicators that equal one if month  $t$  is  $l$  periods away from the month  $T_{vd}^{\text{sub}}$  in which village  $v$  first became eligible for the subsidy. We exclude the pre-period  $l = -1$  as is standard practice. We include three post-treatment periods, since all districts were eligible by July 2019 as well as three pre-treatment periods. We cluster standard errors at the village level.

The parameters of interest are the interaction-weighted dynamic effects  $\hat{\beta}^l = \sum_{e \in \{1,2\}} \omega_e^l \hat{\beta}_e^l$ , where the weights  $\omega_e^l$  reflect the sample share of cohort  $e$  in relative period  $l$ . For  $l \geq 0$ ,  $\hat{\beta}^l$  captures the impact of the subsidy  $l$  months after implementation as a weighted average of cohort-specific treatment effects. To obtain a “static” average treatment effect and its associated standard error, we follow [Sun and Abraham \(2021\)](#) and average the post-treatment dynamic effects:

$$\hat{\beta}^{avg} = \frac{\sum_{l=0}^3 \hat{\beta}^l}{4}. \quad (11)$$

## 4.2 Event Study Analysis of the Mobile Money Agent Policy

We leverage the government-led expansion of mobile money agents, described in Section 1.5, to estimate the causal effect of improved agent access using an event-study design. As shown in Figure 3, 99 villages with no prior mobile money agent gained access to their first agent at different times between September 2019 and May 2021 under this policy. In total, 290 villages lie within 3 km of one of the newly deployed agents, but 191 of these already had a mobile money agent nearby before the programme and therefore do not experience a first arrival.<sup>13</sup> The expansion proceeded continuously from September 2019 until it was abruptly halted in March 2020 with the onset of COVID, and then resumed in January 2021.

Our event-study design exploits both the staggered timing of agent arrivals during the first phase (September 2019–March 2020) and the COVID-induced pause, with villages that eventually received an agent in January 2021–May 2021 serving as the eventually-treated control group. We focus on the period from September 2019 to December 2020 for this analysis and combine the villages in the second phase to increase the number of villages in our control group. As additional analysis, we also estimate a specification that uses only villages receiving an agent in January 2021 as the control group. Since the recorded agent arrival dates are approximate rather than exact, we aggregate the data into 2-month periods and estimate the event-study at this bimonthly frequency. The staggered expansion thus generates six treatment cohorts defined by the two-month period in which the agent arrives, with the number of villages in parentheses: September 2019 (43), November 2019 (4), January 2020 (10), March 2020 (27), January 2021 (5) and May 2021 (10).

We estimate the following village-level regression:

$$Y_{vt} = \delta_t + \phi_v + \sum_{e \in \{1,2,3,4\}} \sum_{l=-7, \neq -1}^7 \tau_e^l \mathbb{1}\{Agent_v = e\} \cdot A_{vt}^l + \varepsilon_{vt}, \quad (12)$$

where  $Y_{vt}$  denotes an outcome for village  $v$  in bimonth  $t$ . In our main specification,  $Y_{vt}$  is the number of monthly SHS applications in village  $v$  in bimonth  $t$ , where we take an average across both months so that our outcomes are comparable to the monthly

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<sup>13</sup>We use annual data on the universe of mobile money agents that were not part of the government-led expansion and require that no agent is observed within 3 km of the village in any year up to 2021 in order to classify it as having no prior nearby agent.

analysis. The terms  $\delta_t$  and  $\phi_v$  are time and village fixed effects, respectively. The indicators  $\mathbb{1}\{Agent_v = e\}$  are dummies for whether village  $v$  belongs to treatment cohort  $e \in \{1, 2, 3, 4\}$ , where a cohort is defined by the bimonth in which the village first received a mobile money agent. The variables  $A_{vt}^l = \mathbb{1}\{t - T_v^{\text{agent}} = l\}$  are relative-time indicators that equal one if bimonth  $t$  is  $l$  periods away from the bimonth  $T_v^{\text{agent}}$  in which village  $v$  first became treated. As before, we omit the pre-period  $l = -1$  and include seven pre-treatment and seven post-treatment periods in the event window. We cluster standard errors at the village level.

The parameters of interest are the interaction-weighted dynamic effects  $\hat{\tau}^l = \sum_{e=1}^4 \omega_e^l \hat{\tau}_e^l$ , where the weights  $\omega_e^l$  denote the sample share of cohort  $e$  observed in relative period  $l$ . For  $l \geq 0$ ,  $\hat{\tau}^l$  captures the impact of gaining access to a mobile money agent  $l$  bimonths after arrival as a weighted average of cohort-specific treatment effects. To obtain a “static” average treatment effect, we average the post-treatment dynamic effects:

$$\hat{\tau}^{avg} = \frac{\sum_{l=0}^7 \hat{\tau}^l}{8}. \quad (13)$$

### 4.3 Customer-Level Event Study Analysis

We also evaluate the effects of the policies on customer-level outcomes for pre-existing customers. We implement the same regression specifications (Equations 10 and 12) but rather than using village fixed effects, we use customer fixed effects. We restrict the sample in two ways. First, we include only customers who had already adopted a solar home system in the period immediately before the policy change (February 2019 for the subsidy policy and July 2019 for the mobile money agent expansion). Therefore, while the sample is unbalanced for some of the pre-policy periods, it is balanced in the post-policy periods. Second, we focus on relatively active customers, defined as those who used their system for at least half of the days in that pre-policy period. This restriction ensures that our results are not driven by customers who had effectively disengaged from the product prior to the policy change, and who are thus unlikely to be affected by the policy.

## 5 Adoption Effects and the Subsidy–Agent Complementarity

We proceed in three steps. First, we document the large and positive impact of the subsidy on SHS adoption. Second, we examine heterogeneity by pre-subsidy access to mobile money agents and show that the subsidy’s effect is substantially larger in villages with an agent. This heterogeneity is, however, only suggestive, as it may reflect other unob-

served differences across villages. Third, we turn to the mobile money agent expansion policy and estimate the causal impact of the arrival of an agent, showing that improved access to mobile money services can causally account for much of the gap observed in the heterogeneous subsidy responses.

## 5.1 Subsidy Effects on Adoption

We exploit the phased rollout of the subsidy across districts to estimate its causal impact on solar home system adoption, measured using the number of new monthly applications. We begin by plotting the evolution of solar home system adoption separately for each of the three district groups in Figure A2, covering the two-year period from September 2018 to August 2020. Four features stand out. First, adoption increases sharply and immediately when the subsidy is introduced in a given group. Second, there is no clear evidence that households in any of the groups anticipated the subsidy. Third, outside the rollout windows, the three groups move closely together, displaying similar peaks and troughs over time. Fourth, there is a clear level shift in adoption when comparing the pre-subsidy period, when adoption is relatively flat, to the post-subsidy period, when all districts are treated and average adoption is considerably higher. Taken together, these patterns provide suggestive evidence that the identifying assumptions underlying our event-study analysis are plausible.

We then plot the event-study coefficients and show a sharp and immediate increase in applications following the introduction of the subsidy, with the effect continuing to rise over the next two months before stabilising (Figure 4 and Equation 10). There is no evidence of differential pre-trends prior to the policy change. We report the average effects in Panel A of Table 2. The estimates show that the subsidy increases adoption by 0.473 new applications per village-month on average, more than doubling adoption relative to the pre-policy control mean of 0.397.<sup>14</sup>

## 5.2 Heterogeneous Subsidy Effects by Access to Mobile Money Agents

To provide initial evidence that transaction costs shape both adoption and policy effectiveness, we examine whether the effect of the subsidy on adoption differs by pre-subsidy access to mobile money agents. As described in Section 3, households must pay for electricity through a mobile money account, while rural customers still transact primarily in cash. Mobile money agents therefore play a key role in converting cash into digital balances and facilitating electricity payments. We classify a village as having a mobile

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<sup>14</sup>These findings are in line with contemporaneous work of [Lang \(2025a\)](#), which uses similar data, but focuses on the district level results.

money agent if there is an agent located within 3 km of the village centre in December 2018, three months before the subsidy rollout.

Figure 5 plots the event-study coefficients separately for villages with and without a mobile money agent. The subsidy effect is substantially larger in villages with an agent. Two months after the subsidy is introduced, adoption rises by nearly 1.5 additional applications per village-month in villages with a mobile money agent, compared with only around 0.3 to 0.5 additional applications in villages without one.<sup>15</sup> Reassuringly, for both groups, there is no evidence of differential pre-trends prior to the subsidy rollout.

Table 2 reports the corresponding average effects separately by baseline access to mobile money agents. In Panel B, for villages with a mobile money agent, the subsidy increases adoption by 1.053 applications per village-month on average. In Panel C, for villages without a mobile money agent, the corresponding effect is only 0.347. The average treatment effect is therefore nearly three times larger in villages with an agent, indicating that the subsidy is substantially more effective where households have easier access to mobile money services. The difference across the two groups of 0.706 (+189% vs. +115%) is also highly statistically significant: we reject equality of the effects at the 5% level.

The framework also pins down how these effects should scale. Under the multiplicative model of Section 2.2, the subsidy’s proportional effect is invariant to agent access, while its level effect scales with  $\bar{G}(c(d))$ . The estimates are consistent with this pattern: relative to pre-policy means, the subsidy raises adoption by 189% in villages with an agent (1.053/0.558) and by 115% in villages without (0.347/0.301), and we cannot reject equality of the proportional effects, even as the level effects differ sharply.

Our results are robust to the distance threshold used to define mobile money access. The main specification uses 3 km, which we consider conservative. Appendix Table A2 shows that varying the cutoff—down to 1 km or 2 km, or up to 4 km and 5 km—yields qualitatively unchanged heterogeneous subsidy effects that remain statistically significant at all thresholds. Point estimates are in fact larger under narrower definitions of proximity (see also Appendix Figure A4).

Access to a mobile money agent could reflect a range of other potential confounders. In particular, agents may be more likely to operate in villages that are wealthier, more densely populated, or closer to markets, each of which could independently affect SHS adoption. Wealthier villages may contain more households able to afford a system, denser villages may facilitate information diffusion or make marketing more effective, and villages closer to markets may face lower search and transaction costs more generally. In

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<sup>15</sup>The standard errors are larger for villages with a mobile money agent because only around 25% of villages in our sample had an agent.

columns (2) to (4) of Table 2, we therefore augment our main empirical specification by interacting time-invariant measures of village wealth, population density, and distance to markets with time fixed effects. The estimated average effect of the subsidy is virtually unchanged, and, importantly, the heterogeneous effects in Panels B and C are not attenuated.

While access to a mobile money agent may be correlated with other village characteristics, the heterogeneous subsidy effects remain virtually unchanged after flexibly controlling for baseline wealth, population density, and market access. This suggests that these observable differences are unlikely to explain the substantially larger subsidy response in villages with an agent. At the same time, this heterogeneity remains descriptive rather than causal, as villages with and without an agent may still differ along other unobserved dimensions. For this reason, we next turn to the mobile money agent expansion policy to estimate the causal effect of improved agent access.

### 5.3 Mobile Money Agent Effects on Adoption

To move beyond descriptive heterogeneity, we next exploit the mobile money agent expansion policy to estimate the causal effect of improved access to a mobile money agent on SHS adoption. By the time this policy was launched, the SHS subsidy had already been rolled out nationwide, so the estimates isolate how the arrival of a mobile money agent affects adoption conditional on the subsidy being in place. If the heterogeneous subsidy effects documented above reflect a causal role for mobile money access, then the arrival of an agent should raise adoption in previously underserved villages and reduce the adoption gap relative to villages that already had an agent.

We exploit the staggered rollout of the policy across villages to estimate these effects using the number of new monthly SHS applications. Our event-study design draws on both the staggered arrival of agents during the first phase of the rollout (September 2019–March 2020) and the COVID-induced pause, with villages that were only treated later, between January and May 2021, serving as the eventually treated control group. We plot the event-study coefficients in Figure 6. Although the estimates are relatively imprecise, given the much smaller sample of treated villages in this analysis (99, compared with 2,212 in the subsidy analysis), the figure shows a positive effect following the arrival of a mobile money agent.<sup>16</sup> There is also no evidence of differential pre-trends prior to agent arrival.

In Panel A of Table 3, we find that the arrival of a mobile money agent increases SHS

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<sup>16</sup>Our analysis is conducted at the two-month level, but we divide the adoption outcome by two so that it is expressed in village-month applications and is therefore directly comparable to the subsidy effects.

adoption by 0.498 applications per village-month on average, with the estimate statistically significant at the 5% level. Because our last treated group combines villages that received an agent in January 2021 with those treated only in May 2021, this specification pools cohorts that are separated by four months. In Panel B, we therefore use only the January 2021 cohort as the eventually treated control group and exclude the May 2021 cohort. Under this alternative specification, the estimated effect rises to 0.719 applications per village-month and remains significant at the 5% level. Both estimates indicate that households respond to the arrival of a mobile money agent by adopting a solar home system. Moreover, these magnitudes are of the same order as the gap in subsidy effects between villages with and without a mobile money agent (0.706 applications per village-month), lending further support to a causal interpretation of the heterogeneity documented above.

We perform a similar set of robustness checks by flexibly controlling for baseline village wealth, population density, and distance to markets. In columns (2) to (4) of Table 3, we augment our main specification by interacting these time-invariant village characteristics with time fixed effects. The estimated effects remain virtually unchanged, with no meaningful attenuation in the point estimates. The estimates are, however, somewhat less precise and are statistically significant at the 10% level.

## 5.4 Additional Robustness and Potential Confounders

Throughout the results, we have controlled for the possibility that mobile money agent proximity proxies for underlying village characteristics—namely wealth, population density, and market access—and shown that our estimates are robust to these controls. In this section, we implement additional robustness checks to rule out potential confounders.

### 5.4.1 Supply-Side Constraints

We address the possibility that supply-side factors may have differentially influenced adoption during the subsidy period—for example, that increased adoption could reflect greater attention from the solar company toward villages eligible for the subsidy compared to the control group. We present three arguments against this hypothesis. First, the solar company relies on local shops (approximately one shop per district), which are responsible for supplying their respective areas and operate independently. We observe no reduction in supply in control districts during the subsidy rollout, suggesting no reallocation of resources. Second, discussions with the solar company indicate that the observed impact is driven by increased demand, not by increased sales attention to the subsidy villages. Third, our adoption measure is based on the number of applicants, not connected customers. While supply issues might affect the number of connected customers,

leading to installation delays, they are less likely to influence the number of applicants. Our results are consistent across both measures, and the lack of significant discrepancies between them suggests that supply-side effects are minimal (Appendix Table A5).

#### **5.4.2 Information Effects**

We address concerns that the increase in adoption might be driven by information channels and greater awareness of the SHS product rather than the price reduction itself. Information barriers are particularly relevant when assessing policy impacts, especially in rural areas of low-income countries where knowledge of new technologies is often limited. To investigate this, we collected detailed marketing campaign data from the solar company over time (bi-monthly) at the shop level. We found no significant increase in information campaigns during the subsidy launch; if anything, the number of campaigns decreased everywhere during the subsidy launch. We further discuss this issue in Appendix Section A2 where we explore the role of marketing campaigns and show that they do not appear to drive our results. However, this is one type of information campaign, and we cannot fully rule out the information channel potentially coming from public officials, which might have targeted treated areas first.

#### **5.4.3 Representativeness of the Mobile Money Agent Villages**

We address the concern that villages selected for the mobile money agent expansion may differ systematically from other villages without an agent. The event-study sample comprises only 99 villages—around 5% of villages without a mobile money agent at the time of the subsidy launch—raising the question of whether the effects of agent arrival in this selected subsample generalise to the broader population of unconnected villages. To assess this, we exploit the fact that the subsidy rollout provides independent variation even within this subsample, and estimate the effect of the subsidy on adoption using these 99 villages alone. Table A6 shows an average effect of 0.384 applications per village-month—very close to the estimate of 0.347 for all villages without mobile money access (Panel C of Table 2)—and stable across specifications that flexibly control for wealth, population density, and market proximity. That expansion villages exhibit subsidy responses that are virtually identical to those of the broader population of villages without an agent suggests that they are not systematically different along dimensions relevant to subsidy adoption. This, in turn, strengthens the interpretation that the heterogeneous subsidy effects documented in Section 5.2 reflect the causal role of mobile money infrastructure, rather than pre-existing differences across village types.

#### 5.4.4 COVID Effects

We address the concern that the COVID-19 pandemic may confound our estimates of the mobile money agent expansion. This concern does not apply to our subsidy results, since the subsidy rollout was completed in March 2019—more than a year before the onset of the pandemic. For the agent expansion, however, agents may have become differentially more valuable during the pandemic: as households avoided travel and mobile money served as a channel for government cash transfers, proximity to an agent could have increased solar adoption through a COVID-specific mechanism unrelated to the payment-infrastructure channel we study. One example is the GiveDirectly-Novissi cash transfer programme, jointly implemented by GiveDirectly and the Togolese Ministry of Digital Transformation, which we discuss in more detail in Section 7. However, because this programme was launched only in November 2020, it is unlikely to have materially affected our estimates.

Figure A2 shows that aggregate adoption fell modestly in mid-2020, although it remained well above pre-subsidy levels and recovered quickly, suggesting that the pandemic did not substantially disrupt SHS adoption. To assess whether our estimates are driven by the pandemic period, Table A8 presents event-study results for the mobile money agent expansion using two shortened samples. Panel A restricts the analysis to data through May 2020—the earliest feasible endpoint that preserves at least one post-treatment observation for villages receiving an agent in March 2020, before the COVID-related pause—reducing the sample by approximately one-fifth. The estimated adoption effect is 0.483 applications per village-month, virtually unchanged relative to our main estimate of 0.498, and remains statistically significant at the 5% level. Panel B imposes a stricter cutoff of March 2020, coinciding with the onset of the pandemic, which further compresses the post-treatment window and leaves fewer post-treatment periods for identification. The point estimate falls modestly to 0.376; as expected given the sharper loss of statistical power, precision deteriorates and the estimate is no longer statistically significant at conventional levels. Across both panels, specifications that flexibly control for wealth, population density, and market proximity yield similarly patterned, though somewhat more attenuated, estimates, consistent with our main results. Taken together, these results suggest that although the post-2020 data contribute meaningfully to the precision of our main estimates, our results are robust to restricting the sample to the pre-COVID period. We therefore find no evidence that the pandemic biases our estimates of the adoption effects of mobile money agent expansion.

Taken together, the results in this section establish a clear pattern: the subsidy and mobile money agents are complements, not substitutes. The subsidy raises adoption by

1.053 applications per village-month where an agent is present, but only 0.347 where one is not; the causal arrival of an agent closes most of this gap. We now turn to the mechanisms underlying this complementarity.

## 6 Mechanisms Driving the Complementarity

Having documented the adoption effects of the subsidy and the mobile money agent expansion, we now examine the *mechanisms* behind their complementarity. Our central argument is that transaction costs are the key friction: the subsidy relaxes the budget constraint on solar home system adoption, while mobile money agents relax the payment-infrastructure constraint. This section presents four pieces of evidence. First, observed travel costs are large enough to account for the magnitude of the adoption effects. Second, the subsidy–access interaction is concentrated at the walking threshold, matching the step-function structure of rural transport costs. Third, the complementarity is strongest among the customers most likely to face binding constraints. Fourth, the subsidy shifts payment behaviour in the direction implied by households economising on trips to the agent, and does so more strongly where agents are nearby. We then turn to alternatives and show that the results are difficult to reconcile with other channels.

### 6.1 Evidence on the Transaction-Cost Channel

Rural households in Togo transact primarily in cash, yet PAYGO electricity payments must be made via mobile money, requiring a trip to a mobile money agent to convert cash into a digital balance. The median household in our sample is located over 6 km from the nearest agent, making each payment round trip costly. Using the transport cost data we collected across 126 origin-destination village pairs in rural Togo (see Section 3.5), we investigate the role of transaction costs as a mechanism for the complementarity documented above. We proceed in three steps. First, we benchmark the magnitude of transaction costs against the price of electricity to assess whether they are large enough to plausibly account for the adoption effects we estimate. Second, we examine whether the cost structure is linear or non-linear in distance—a distinction with direct implications for whether the subsidy’s effectiveness should vary smoothly with distance or exhibit a discrete threshold. Third, we ask how transaction costs shape households’ payment frequency responses to the subsidy, and whether the interaction between payment flexibility and transaction costs can rationalise the observed complementarity.

### 6.1.1 The Magnitude of Transaction Costs

Using our data on transportation costs, we can approximately calculate the transaction costs incurred by customers. Within walking distance of a mobile money agent—roughly 0–3 km—households incur no meaningful transport cost to make a payment. Beyond this threshold, reaching an agent typically requires hiring a moto-taxi, which involves a fixed cost of approximately \$0.70 (CFA 350) plus a modest per-kilometre fare of \$0.10 (CFA 50) (Appendix Figure A3). Our fare data contain very few prices for distances less than 3 km, and virtually none for distances less than 2 km, consistent with the walking threshold documented in Section 6.1.2.

To benchmark the magnitude of these frictions, we directly compare the total monthly cost of paying for electricity before and after the mobile money agent expansion. In the pre-expansion period, customers in villages that would later receive a nearby agent made an average of 2.2 payments per month. The median distance to the nearest mobile money agent for these villages was approximately 6.5 km before the expansion and fell to within 3 km afterward. At a round-trip moto-taxi cost of roughly \$2.70 for 6.5 km, the cost of 2.2 round trips would amount to approximately \$6 per month—exceeding the \$4.50 post-subsidy monthly electricity price. The arrival of a nearby agent would have eliminated these moto-taxi costs entirely, resulting in considerable savings.

These estimates are necessarily approximate, and there are reasons to think they both overstate and understate the true burden. On the one hand, our calculation may overestimate transaction costs if customers combine payment trips with other errands, so the marginal cost of reaching an agent is lower than the full fare. Some households may also have bicycles or walk longer distances, reducing or eliminating the moto-taxi cost. On the other hand, several factors suggest the estimates are conservative. First, we measure distance from the village centre, while rural households often live on dispersed farms further out. Second, the fare captures only the monetary cost and excludes the opportunity cost of travel time, which could otherwise be devoted to farming or household production. Third, it does not account for repeat trips required when agents are absent or mobile signal fails—both common in rural settings. Fourth, it abstracts from the cost of arranging transport itself: in remote villages, moto-taxis are not always available, and locating a driver takes time.

### 6.1.2 Non-Linear Effects of Distance

The structure of transaction costs provides a sharp test of Prediction P3: whether the relevant friction is a discrete infrastructure constraint or a smooth cost gradient. Crucially, because moto-taxi rates per kilometre are not steep in rural Togo, total transport costs do

not rise sharply with additional distance once a moto-taxi is taken. This implies a non-linear, step-function cost structure: a discrete jump at the walking threshold, followed by a relatively flat cost schedule beyond it. To test this prediction, Figure 7 reports heterogeneous subsidy effects separately for four distance bands: villages with a mobile money agent within 0–3 km, and, among villages without an agent, bands of 3–6 km, 6–9 km, and 9+ km. The subsidy effect falls sharply from the 0–3 km band to the 3–6 km band, but is remarkably stable across the 3–6 km, 6–9 km, and 9+ km bands—consistent with the step-function cost structure described above. Appendix Figure A4 shows similar patterns using even more granular 1 km distance bands.

This non-linearity is the signature of a discrete infrastructure constraint rather than a smooth cost gradient: a model in which transaction costs rise continuously with distance would predict a monotonically falling profile, whereas nearly all of the interaction between the subsidy and mobile money access is concentrated at the walking-distance margin.<sup>17</sup> We note that this pattern identifies the *shape of the cost function* rather than the structure of demand: an additive model with the same step-function costs would generate the same band profile. The structural distinction between the multiplicative and additive models rests instead on the proportional-effects test of Section 5.2.

### 6.1.3 Adoption Effects by Customer Types

If the complementarity operates through transaction costs, heterogeneity should concentrate where constraints bind hardest. Kit type provides a natural test: basic-kit customers are poorer and more liquidity-constrained than those choosing the plus or premium kits. Table 4 confirms this. The subsidy–agent gap is 0.344 applications per village-month for basic kits, compared with 0.128 and 0.234 for plus and premium kits respectively. Agent arrival (Panel C) closes nearly the entire adoption gap for basic kits (0.349,  $p < 0.10$ ) but has smaller, insignificant effects on the higher-tier kits. The concentration of the complementarity in the most constrained segment is consistent with the transaction-cost channel.

Customer-level characteristics at registration—income source, age, and gender—are not good proxies for transaction costs, which are governed by village geography. Accordingly, Appendix Table A9 shows positive and significant subsidy effects in both mobile-money and non-mobile-money villages across all six subgroups, with no systematic con-

<sup>17</sup>Strictly, fares are not flat beyond the threshold: the per-kilometre component implies round-trip costs from roughly CFA 1,000 just beyond the walking threshold to CFA 2,000 or more in the farthest band (Section 6.1.1). Since the subsidy effect in each band is proportional to  $\bar{G}(c(d))$ , a flat profile despite rising fares implies that few households are on the margin of transactional capacity over this range of costs: once a moto-taxi is required at all, the additional fare screens out almost no additional households. The flat profile thus reflects the fixed cost of motorised transport together with this local flatness of  $\bar{G}$ , not literal flatness of  $c(d)$ . Coping through delegated payments (Section 6.2.2), which caps the effective cost of a payment at the price of compensating a traveller, would generate the same pattern.

centration. This uniformity is what we would expect from a village-level mechanism.

#### 6.1.4 Payment Behaviour and Bulk Purchasing

Section 2.3 derived a prediction on the intensive margin: if the subsidy lowers the price of electricity, liquidity-constrained households should make fewer, larger payments—buying in bulk to economise on costly trips to the agent. This adjustment should be *larger* where a nearby agent makes payment schedules more flexible. Table 5 tests both parts.

**The subsidy reduces payment frequency.** Panel A reports the response across all villages. The subsidy has no effect on the probability of making any payment (column 1)—the relevant margin is not whether customers pay but how often. The number of payments per month falls by 0.255 from a mean of 1.318 (column 2,  $p < 0.01$ ), and the probability of multiple payments falls by 3.0 percentage points (column 3). Results are unchanged conditioning on at least one payment (columns 4–5). When the price falls, households consolidate payments, consistent with bulk purchasing.

**The reduction is larger where agents are nearby.** Panels B and C split the sample by baseline agent access. With an agent within 3 km, the number of payments falls by 0.319; without one, by 0.185 (difference significant at 10%). The probability of multiple payments falls by 5.0 percentage points with an agent versus 1.2 without. This heterogeneity maps directly onto the cross-partial derived in Section 2.3: the “level” effect ( $n_p > 0$ ) is present everywhere, while the “flexibility” effect ( $n_{p\tau} < 0$ ) shows up in the gap between panels. That the gap is concentrated at the walking-distance threshold—rather than varying smoothly with distance—matches the step-function cost structure in Section 6.1.2.

**Agent arrival increases payment flexibility.** Panel D examines the reverse: how agent arrival affects payment behaviour among existing customers. All frequency point estimates are positive, with the probability of multiple payments rising by 6.8 percentage points (column 5,  $p < 0.05$ ). The remaining estimates are directionally consistent but imprecise given the smaller sample. When an agent arrives, existing customers shift toward more flexible, higher-frequency schedules—the  $n_\tau < 0$  implication of Section 2.3.

**Interpretation.** The subsidy and mobile money agents interact on the intensive margin: the subsidy makes bulk purchasing affordable, while nearby agents make flexible payment schedules feasible. Without an agent, households cannot exploit the flexibility PAYGO offers and are locked into infrequent payments dictated by the cost of each trip. With an agent, the same price reduction translates into a larger adjustment in payment behaviour—the  $n_{p\tau} < 0$  signature predicted in Section 2.3. Each avoided trip saves the most where agents are far, but households near an agent can consolidate further, while remote households, many already making a single payment per month, cannot.

## 6.2 Ruling Out Alternative Explanations

We consider three complementary checks to help rule out alternative explanations for the observed complementarity between the subsidy and mobile money agents. First, we examine whether the complementarity is driven by differential demand for or usage of solar home systems. Second, we assess whether payment coordination frictions account for the results. Third, we test whether mobile money agents raise subsidy receipt rates. In each case, the evidence is inconsistent with the alternative explanation.

### 6.2.1 Demand and Usage Effects

A phone and a solar home system are complementary goods: households need a phone to make payments for their SHS, and electricity is in turn needed to keep the phone charged. While phones could be charged elsewhere, doing so at home is most convenient. By extension, a mobile money agent in the village could increase the value of owning a phone by expanding access to digital services, thereby shifting demand for a solar home system upward—providing an alternative explanation for the complementarity between the subsidy and mobile money agents.

We explore this potential mechanism by examining electricity usage of the solar home system among existing customers in Appendix Table A10. The flexible payment structure allows households to miss payments and thus be locked out of their system, preventing the solar panel from charging the battery and, in turn, from powering lamps or charging devices. Among our sample of active customers, pre-policy usage is high at around 95%, which is only partly mechanical: while our inclusion criterion requires that customers have electricity for at least half the month in the pre-policy period, the pre-policy mean covers all pre-periods. We find that the subsidy has no effect on usage in both villages with and without access to a mobile money agent and across all three outcomes capturing the extensive and intensive margins of use. The estimates are precisely estimated and are not statistically different between villages with and without an agent. Similarly, the arrival of a mobile money agent has no effect on usage. If the presence of an agent increased the value of owning a phone, for which electricity is a complementary good, we would expect usage to rise. Instead, we again find a precisely estimated null effect.

Second, we also observe hourly wattage use, which allows us to confirm these patterns more directly. On average, households consume about 76 Wh per day, roughly consistent with a small solar panel powering a few LED lamps and a phone. Usage is extremely stable across days (median coefficient of variation  $\approx 0.30$ ), and consumption is heavily concentrated in the evening and night hours, which together account for about 62% of total use, consistent with lighting as the primary application. Patterns are broadly similar

across households, with only a small minority exhibiting irregular or daytime-intensive use suggestive of business activity. Although customers in areas with a mobile money agent consume about 6% more watt-hours on average, this difference is quantitatively modest. These patterns help to rule out the possibility that the complementarity between the subsidy and the mobile money agent is driven by differential demand for electricity, which would be reflected in usage of the solar home system.

### 6.2.2 Coordination Frictions

In the presence of transaction costs, customers could, in theory, coordinate to mitigate these costs by pooling resources and delegating payments to a subset of individuals. For example, customers might ask one person in the village—e.g., someone who already travels to a market with a mobile money agent—to take their cash and complete the cash-in on their behalf. Such arrangements would generate substantial savings for other customers but require coordination. If widespread, this type of coordination would effectively neutralise transaction costs as a barrier to adoption.

We leverage the fact that all payments are made by phone, so we can observe both the phone number used for the payment and the solar home system account into which the payment was made. We then match the payment phone number to the set of phone numbers recorded for each customer (each customer has at least one phone number on record, and up to three). This allows us to identify cases in which a customer paid for their SHS using a third party's phone, and thus to measure the extent of delegated payments and coordination.<sup>18</sup>

Payment coordination is, if anything, a force that should attenuate our main mechanism rather than explain it. If households in remote villages can routinely delegate trips to a subset of travellers, then the absence of a nearby agent should matter less for both adoption and payment behaviour. We do find baseline evidence of delegated payments. In villages with access to a mobile money agent, roughly one-third of customers regularly rely on third-party payments, and a similar share of payments are made through third parties, indicating that payment frictions are reduced but not eliminated. The corresponding averages are noticeably higher—closer to one-half—in villages without access to a mobile money agent.

The subsidy appears to lower measured coordination because it reduces payment fre-

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<sup>18</sup>There are two situations in which this measure will fail to capture payment coordination. First, payments may be made regularly by someone outside the village (for example, a migrant household member who routinely pays for electricity). In such cases, the payment pattern reflects household arrangements rather than within-village coordination to overcome transaction costs. Second, a customer may use a phone number that is not registered with the company; these payments will not be flagged as third-party transactions in our data, even if they involve coordination.

quency, and therefore the number of occasions on which delegation is useful (see Appendix Table [A11](#), columns 1 and 2). This can occur both with and without a nearby agent. Importantly, however, the reduction in coordination is not meaningfully larger in villages without agent access.

In addition, coordination does not respond when a local agent arrives: Appendix Table [A11](#) shows near-zero effects of agent arrival on the share of paying customers using third parties and on the share of payments made by third parties. We therefore interpret coordination as an imperfect coping device rather than as the mechanism behind our main results. Households do try to economize on trips, but it does not eliminate the payment-infrastructure barrier that drives the larger subsidy response in high-access villages. Taken together, these findings suggest that the complementarity between the subsidy and the mobile money agent is unlikely to operate primarily through a reduction in coordination frictions.

### **6.2.3 Subsidy Disbursement Effects**

A mobile money agent in the village could, in principle, complement the subsidy by helping customers actually receive it, especially given the low levels of financial literacy in these settings. We assess this mechanism by examining the impact of the mobile money agent policy on subsidy disbursement in Appendix Table [A12](#). At the village level, we find that neither the share of paying customers nor the share of payments that receive the subsidy—and are therefore matched by La Poste and disbursed to the SHS company—is affected by the arrival of a mobile money agent. These aggregates, however, combine both existing and newly adopting customers. To test whether new adopters are more likely to take up the SHS because they can more easily receive the subsidy, we examine whether the arrival of a mobile money agent increases the share of new customers whose first payment receives the subsidy. The estimated effect is essentially zero and very precisely estimated. Taken together, these results provide no evidence that the presence of a mobile money agent raises subsidy disbursement rates for either existing or new customers, making this an unlikely driver of the observed complementarity.

## **7 External Validity: Cash Transfers Through the Same Payment Infrastructure**

The main analysis shows that payment infrastructure shapes the effectiveness of the solar subsidy because households must use mobile money to pay for electricity. We now ask whether the same logic extends to another policy delivered through mobile money: Novissi, Togo’s emergency cash transfer programme during COVID-19.

Novissi targeted poor cantons and disbursed monthly transfers directly into beneficiaries' mobile money accounts—12,250 CFA for women and 10,500 CFA for men. Because beneficiaries needed to reach a mobile money agent to cash out, the effective value of the transfer should be lower in places with worse agent access. This yields a sharp prediction: treatment effects should be larger where mobile money access is better for outcomes that depend on purchasing power, but not necessarily for outcomes such as mental health that may respond to the reassurance of support itself. We combine the randomized evaluation data from Aiken et al. with our administrative data on the location of mobile money agents. After matching canton names, we link 9,284 individuals in 236 cantons to a canton-level measure of mobile money access: the share of villages in the canton with an agent within 3 km, standardized to mean zero and unit variance. Access varies substantially across the Novissi sample. On average, 42.4% of villages in a canton are within 3 km of an agent; 20.7% of cantons have no village within 3 km, while only 13.0% have full coverage. We estimate a simple interaction specification that regresses each standardized outcome on treatment, mobile money access, and their interaction, controlling for strata, enumerator, and survey-week fixed effects and using the original sampling weights. Table 6 reports the results. First, in the matched sample, the main treatment effects closely reproduce the original experiment: Novissi improves food security by 0.073 standard deviations, mental health by 0.073 standard deviations, and the combined seven-outcome index by 0.064 standard deviations.

Second, and more importantly for our purposes, these effects are larger in cantons with better mobile money access for the outcomes most directly tied to spending power. A one-standard-deviation increase in canton-level access raises the treatment effect on financial health by 0.049 standard deviations and on health care access by 0.040 standard deviations. The interaction for the combined index is positive at 0.026 standard deviations, though imprecisely estimated. By contrast, mental health shows no positive interaction (-0.028 standard deviations), and financial inclusion is essentially zero (-0.004). This pattern is consistent with the transaction-cost mechanism: access matters when beneficiaries must convert digital transfers into usable purchasing power.<sup>19</sup>

The magnitudes are economically plausible. Using our transport-cost survey, a round trip to an agent costs roughly 700 CFA plus 100 CFA per kilometre. At the median canton-level distance in the Novissi sample, 3.9 km, cashing out once costs roughly 1,100 CFA, or about 10% of the average transfer. At the 75th percentile distance, 6.4 km, that rises to

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<sup>19</sup>In Appendix A3, we provide additional robustness: we do not find the same heterogeneity by alternative measure of infrastructure or urbanization, reassuring that the effect is driven by mobile money agents themselves.

about 1,340 CFA, or 12% of the transfer. In the 20.7% of cantons where no village is within 3 km of an agent, the median distance is 10.3 km, implying a cash-out cost of about 1,730 CFA—roughly 15% of the transfer—before accounting for time. These are substantial losses in effective transfer value.

We view this evidence as suggestive rather than definitive, since mobile money access is not cross-randomized in the Novissi setting (Appendix A3 provides further discussion and robustness about the heterogeneous effects—reproducing the same analysis by proximity to the nearest financial institution, proximity to the nearest market, and population density, where we do not find such effects—and also testing for non-linearity). Still, the same payment infrastructure that amplifies the solar subsidy also appears to amplify the value of digital cash transfers. In both contexts, policy benefits are delivered digitally, yet their realized value depends on the cost of reaching the last-mile cash-in or cash-out point.

## 8 Policy Implications

The preceding sections show that a digitally delivered subsidy is not a uniform policy when households remain cash-based. The posted subsidy lowers the price of electricity everywhere, but the *effective* price reduction depends on the cost of reaching the payment network. This has two implications. First, it changes how much the 2019–2022 subsidy could plausibly contribute toward Togo’s 2030 electrification target. Second, it implies a broader role for public investment in payment infrastructure.

### 8.1 Benchmarking the Estimates Against the 2030 Target

Togo’s target of 550,000 solar home system households by 2030 provides a useful benchmark for the magnitude of our estimates. On the government’s timetable, this corresponds to roughly 150,000 additional households every three years. We use our estimates to conduct an illustrative accounting exercise. We do not interpret these calculations as exact forecasts: our data cover one provider, our primary outcome is applications rather than completed installations, and the arithmetic abstracts from equilibrium and implementation constraints. The exercise is nevertheless informative about orders of magnitude.

In our sample of 2,212 villages, the pre-policy control mean of 0.397 new applications per village-month implies about 32,000 applications over 36 months in the absence of the subsidy. A steady-state extrapolation that applies the average treatment effect of 0.473 applications per village-month over the same horizon implies roughly 38,000 additional applications, bringing the total to about 69,000 within the provider we study. Even allow-

ing for activity by other firms, this comparison suggests that subsidy alone is unlikely to close the remaining electrification gap.

How much more could be achieved if payment access were better? We consider two illustrative benchmarks. The first is an upper-bound exercise that asks what would happen if all villages responded to the subsidy as today's high-access villages do. Using the estimated subsidy effect of 1.053 applications per village-month in villages with an agent within 3 km implies roughly 84,000 subsidy-induced applications over 36 months, or about 116,000 applications in total once baseline demand is included. Even this optimistic benchmark remains below the roughly 150,000 additional households needed over a three-year period to stay on pace.

The second benchmark is more conservative and more tightly tied to the causal access estimates. Applying the agent-arrival estimate of 0.498 applications per village-month to the villages that initially lacked an agent within 3 km implies roughly 30,000 additional applications over 36 months. This would raise the total from about 69,000 to about 99,000 applications within the provider we study. Taken together, these calculations bracket the likely order of magnitude of the gains from expanding walkable payment access. Either way, reducing transaction costs could materially increase the contribution of subsidy policy toward the 2030 target.

This interpretation is consistent with the threshold pattern in Figure 7. The key margin is whether the payment network is within walking distance, not whether already-connected villages become marginally closer to an agent. For policy design, this suggests that moving villages from “no nearby payment point” to “some nearby payment point” is likely to generate larger returns than further densifying the network in places that are already served.

## **8.2 Payment Infrastructure as Complementary Public Capital**

Why might private deployment fall short of the social optimum? Mobile money agents are placed where transaction volume is sufficient to cover operating costs. In remote rural areas, low population density and limited mobile money usage weaken private incentives to enter. But these are precisely the places where poor payment access most reduces the effectiveness of digitally delivered public programmes.

Our evidence shows that this wedge is not specific to solar. In the PAYGO market, better agent access amplifies the effect of a price subsidy by lowering the cost of making repeated payments. In the Novissi programme, the same logic appears in reverse: digital cash transfers have larger effects on outcomes that depend on converting digital balances into spending power. More broadly, any policy that relies on digital channels—whether

to deliver benefits or collect payments—will be less effective where households face high costs of cashing in or cashing out.

The social return to rural payment infrastructure therefore exceeds the private commission revenue earned by the operator. An agent in an underserved village can raise the take-up of subsidized electricity, increase the realized value of digital transfers, and improve the performance of other digitally delivered public services. Payment infrastructure is thus a form of complementary public capital, akin to roads or telecommunications networks: its value comes partly from the portfolio of downstream activities it enables.

This does not imply that payment infrastructure should replace price subsidies. Rather, the two should be designed jointly. Where payment access is already good, subsidy dollars have greater traction. Where households remain far from an agent, improving walkable access can substantially raise the return on existing subsidy policy. Togo's *Réseau Granulaire* illustrates this logic. Our estimates suggest that bringing an agent within walking distance can generate adoption gains of the same order of magnitude as the subsidy itself. Whether further public deployment is optimal depends on operating costs and on the broader set of programmes that use digital channels. But the central implication is robust: in cash-based rural economies, payment infrastructure is part of policy delivery itself.

## 9 Conclusion

Governments in low-income countries increasingly deliver benefits and collect payments through digital systems, yet households still transact mainly in cash. This paper shows that the cost of bridging this gap—reaching the physical network of mobile money agents that converts cash into digital balances—is a quantitatively important bottleneck for policy effectiveness. In rural Togo, transport costs to access mobile money agents account for 28% of total electricity expenditure for the median household, rising to 43% at the 75th percentile. These are not incidental frictions. They depress take-up, distort measured price elasticities, and weaken the effectiveness of policies delivered through digital payment channels.

We exploit two nationwide policies in Togo—a staggered solar home system subsidy and a government-led expansion of mobile money agents—to establish three main results. First, the subsidy more than doubles solar adoption, but its effect is three times larger in villages with a nearby mobile money agent: 1.053 additional applications per village-month where an agent is present within 3 km, compared with just 0.347 where one is not. This gap of 0.706 applications is significant at the 5% level and survives flexible controls for village wealth, population density, and market access. Second, the causal

arrival of a mobile money agent raises adoption by 0.498 applications per village-month—a magnitude of the same order as the heterogeneous subsidy gap and equivalent to a 34% reduction in the effective price of electricity. Third, the subsidy enables existing customers to consolidate payments and buy electricity in bulk, but this adjustment is concentrated in villages with nearby agents, where payment schedules are most flexible. Together, these results establish that the subsidy and agent access are complements, not substitutes: one relaxes the household budget constraint, the other the payment-infrastructure constraint, and each amplifies the returns to the other.

The mechanism is not specific to electricity. Combining the randomised evaluation data from Togo’s Novissi emergency cash transfer with our mobile money agent data, we find that a one-standard-deviation increase in canton-level mobile money access raises the treatment effect on financial health by 0.049 standard deviations and on health care access by 0.040 standard deviations. This pattern is consistent with the transaction-cost channel: what matters is the cost of converting digital balances into usable cash.

These findings have a direct implication for the role of the state. Mobile money agent networks are built by private operators whose expansion decisions are governed by profitability. Remote, sparsely populated rural areas—precisely the places where complementary public programs are most needed—are where private incentives to build agent coverage are weakest. But those are also the places where the public returns to agent coverage are largest, because agents amplify the effectiveness of every policy that relies on digital payment channels. The case for public investment in last-mile payment infrastructure is therefore essential to raise the social return on a much broader portfolio of public programs. As governments across Sub-Saharan Africa accelerate the shift toward digital service delivery, the returns to last-mile payment infrastructure are likely to be large and growing. Understanding where and how to invest in that infrastructure—and how to coordinate it with the broader portfolio of public programmes it supports—is an important direction for future research.

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## Tables and Figures

Table 1: Summary Statistics

|                                           | Mean   | Std. Dev. | Percentiles      |                  |                  |                  |                  | Obs.      |
|-------------------------------------------|--------|-----------|------------------|------------------|------------------|------------------|------------------|-----------|
|                                           |        |           | 10 <sup>th</sup> | 25 <sup>th</sup> | 50 <sup>th</sup> | 75 <sup>th</sup> | 90 <sup>th</sup> |           |
|                                           | (1)    | (2)       | (3)              | (4)              | (5)              | (6)              | (7)              | (8)       |
| Panel A. Household Characteristics        |        |           |                  |                  |                  |                  |                  |           |
| Age at adoption                           | 38.8   | 11.7      | 25.0             | 30.0             | 37.0             | 46.0             | 55.0             | 52,847    |
| Female-headed                             | 0.031  | 0.173     |                  |                  |                  |                  |                  | 52,878    |
| Income source:                            |        |           |                  |                  |                  |                  |                  |           |
| Agriculture                               | 0.708  | 0.455     |                  |                  |                  |                  |                  | 52,878    |
| Own business                              | 0.164  | 0.370     |                  |                  |                  |                  |                  | 52,878    |
| Employee                                  | 0.089  | 0.284     |                  |                  |                  |                  |                  | 52,878    |
| Other                                     | 0.040  | 0.196     |                  |                  |                  |                  |                  | 52,878    |
| Previous energy source:                   |        |           |                  |                  |                  |                  |                  |           |
| Grid                                      | 0.002  | 0.048     |                  |                  |                  |                  |                  | 52,878    |
| Generator                                 | 0.010  | 0.098     |                  |                  |                  |                  |                  | 52,878    |
| Solar                                     | 0.007  | 0.080     |                  |                  |                  |                  |                  | 52,878    |
| Lantern                                   | 0.440  | 0.496     |                  |                  |                  |                  |                  | 52,878    |
| Flashlight                                | 0.164  | 0.370     |                  |                  |                  |                  |                  | 52,878    |
| Kerosene/candles                          | 0.317  | 0.465     |                  |                  |                  |                  |                  | 52,878    |
| None                                      | 0.035  | 0.185     |                  |                  |                  |                  |                  | 52,878    |
| Kit type:                                 |        |           |                  |                  |                  |                  |                  |           |
| Basic                                     | 0.490  | 0.500     |                  |                  |                  |                  |                  | 52,878    |
| Plus                                      | 0.196  | 0.397     |                  |                  |                  |                  |                  | 52,878    |
| Premium                                   | 0.309  | 0.462     |                  |                  |                  |                  |                  | 52,878    |
| Panel B. SHS Payment Behaviour (Monthly)  |        |           |                  |                  |                  |                  |                  |           |
| Any payment                               | 0.401  | 0.490     |                  |                  |                  |                  |                  | 1,243,891 |
| Number of payments                        | 0.750  | 1.420     | 0.000            | 0.000            | 0.000            | 1.000            | 2.000            | 1,243,891 |
| Multiple payments                         | 0.156  | 0.362     |                  |                  |                  |                  |                  | 1,243,891 |
| Any usage                                 | 0.611  | 0.488     |                  |                  |                  |                  |                  | 1,243,891 |
| Utilization rate                          | 0.526  | 0.462     | 0.000            | 0.000            | 0.679            | 1.000            | 1.000            | 1,243,867 |
| Panel C. Village-Level Adoption (Monthly) |        |           |                  |                  |                  |                  |                  |           |
| Monthly applications                      | 0.440  | 2.025     | 0.000            | 0.000            | 0.000            | 0.000            | 1.000            | 2,212     |
| Monthly applications (female)             | 0.008  | 0.090     | 0.000            | 0.000            | 0.000            | 0.000            | 0.000            | 2,212     |
| Monthly applications (male)               | 0.432  | 2.007     | 0.000            | 0.000            | 0.000            | 0.000            | 1.000            | 2,212     |
| Panel D. Village Characteristics          |        |           |                  |                  |                  |                  |                  |           |
| MoMo agent distance, 2019 (km)            | 12.3   | 7.9       | 3.1              | 6.2              | 10.9             | 17.2             | 23.5             | 2,212     |
| MoMo agent distance, 2020 (km)            | 10.4   | 7.4       | 2.3              | 4.9              | 9.0              | 14.3             | 20.8             | 2,212     |
| MoMo agent distance, 2021 (km)            | 9.2    | 6.8       | 1.8              | 4.3              | 7.7              | 12.4             | 18.4             | 2,212     |
| Nearby MoMo agent, 2019                   | 0.096  | 0.294     |                  |                  |                  |                  |                  | 2,212     |
| Nearby MoMo agent, 2020                   | 0.135  | 0.342     |                  |                  |                  |                  |                  | 2,212     |
| Nearby MoMo agent, 2021                   | 0.165  | 0.371     |                  |                  |                  |                  |                  | 2,212     |
| Relative Wealth Index                     | -0.309 | 0.218     | -0.515           | -0.438           | -0.351           | -0.235           | -0.054           | 2,204     |
| Population density (per km <sup>2</sup> ) | 216.9  | 533.0     | 29.7             | 48.6             | 95.3             | 200.7            | 410.2            | 2,212     |
| Distance to market (km)                   | 3.8    | 3.4       | 0.3              | 1.0              | 3.1              | 5.7              | 8.2              | 2,212     |

Notes: .

Table 2: Subsidy Effects on Adoption

|                                                                | Monthly Applications for Solar Home Systems |                           |                     |                     |
|----------------------------------------------------------------|---------------------------------------------|---------------------------|---------------------|---------------------|
|                                                                | Main Specification                          | Robustness Specifications |                     |                     |
|                                                                | (1)                                         | (2)                       | (3)                 | (4)                 |
| <i>Panel A. All Villages</i>                                   |                                             |                           |                     |                     |
| Subsidy                                                        | 0.473***<br>(0.067)                         | 0.500***<br>(0.071)       | 0.497***<br>(0.070) | 0.513***<br>(0.087) |
| Pre-policy control mean                                        | 0.397                                       | 0.397                     | 0.397               | 0.397               |
| <i>p</i> -value: joint test of pre-trends                      | [0.481]                                     | [0.609]                   | [0.607]             | [0.590]             |
| Villages                                                       | 2,212                                       | 2,212                     | 2,212               | 2,212               |
| Observations                                                   | 15,484                                      | 15,484                    | 15,484              | 15,484              |
| <i>Panel B. Villages with Access to Mobile Money Agents</i>    |                                             |                           |                     |                     |
| Subsidy                                                        | 1.053***<br>(0.299)                         | 1.110***<br>(0.320)       | 1.108***<br>(0.320) | 1.113***<br>(0.314) |
| Pre-policy control mean                                        | 0.558                                       | 0.558                     | 0.558               | 0.558               |
| <i>p</i> -value: joint test of pre-trends                      | [0.869]                                     | [0.774]                   | [0.776]             | [0.793]             |
| Villages                                                       | 516                                         | 516                       | 516                 | 516                 |
| Observations                                                   | 3,612                                       | 3,612                     | 3,612               | 3,612               |
| <i>Panel C. Villages without Access to Mobile Money Agents</i> |                                             |                           |                     |                     |
| Subsidy                                                        | 0.347***<br>(0.085)                         | 0.350***<br>(0.084)       | 0.339***<br>(0.081) | 0.349***<br>(0.090) |
| Pre-policy control mean                                        | 0.301                                       | 0.301                     | 0.301               | 0.301               |
| <i>p</i> -value: joint test of pre-trends                      | [0.397]                                     | [0.404]                   | [0.439]             | [0.413]             |
| Villages                                                       | 1,696                                       | 1,696                     | 1,696               | 1,696               |
| Observations                                                   | 11,872                                      | 11,872                    | 11,872              | 11,872              |
| <i>p</i> -value: test of equality (B = C)                      | [0.033]                                     | [0.030]                   | [0.029]             | [0.024]             |
| Village fixed effects                                          | Yes                                         | Yes                       | Yes                 | Yes                 |
| Time fixed effects                                             | Yes                                         | Yes                       | Yes                 | Yes                 |
| Wealth controls                                                | No                                          | Yes                       | Yes                 | Yes                 |
| Population density controls                                    | No                                          | No                        | Yes                 | Yes                 |
| Distance to market controls                                    | No                                          | No                        | No                  | Yes                 |

*Notes:* This table reports the average effect of subsidy eligibility on monthly village-level applications for solar home systems, estimated using the interaction-weighted estimator of [Sun and Abraham \(2021\)](#) applied to Equation (11). The sample covers 2,212 villages observed monthly from December 2018 to October 2019. Treatment is defined by the staggered rollout of subsidy eligibility across three groups of districts (March, May, and July 2019), with the last group serving as the control. Panel A includes all villages. Panel B restricts to villages within 3 km of a mobile money agent at baseline; Panel C restricts to villages without such access. The *p*-value for the test of equality (B = C) tests whether the subsidy effect differs across the two subsamples. Column (1) is the main specification with village and time fixed effects. Columns (2)–(4) add controls for village-level wealth (Relative Wealth Index), population density, and distance to the nearest market. Standard errors clustered at the village level are in parentheses. The *p*-value for the joint test of pre-trends tests the null that all pre-treatment coefficients are jointly zero. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 3: Mobile Money Agent Effects on Adoption

|                                                                                         | Monthly Applications for Solar Home Systems |                           |                   |                   |
|-----------------------------------------------------------------------------------------|---------------------------------------------|---------------------------|-------------------|-------------------|
|                                                                                         | Main Specification                          | Robustness Specifications |                   |                   |
|                                                                                         | (1)                                         | (2)                       | (3)               | (4)               |
| <i>Panel A. All Mobile Money Agent Expansion Villages</i>                               |                                             |                           |                   |                   |
| Mobile Money Agent Arrival                                                              | 0.498**<br>(0.235)                          | 0.483*<br>(0.249)         | 0.472*<br>(0.246) | 0.456*<br>(0.257) |
| Pre-policy control mean                                                                 | 0.381                                       | 0.381                     | 0.381             | 0.381             |
| <i>p</i> -value: joint test of pre-trends                                               | [0.142]                                     | [0.102]                   | [0.087]           | [0.143]           |
| Villages                                                                                | 99                                          | 99                        | 99                | 99                |
| Observations                                                                            | 1,484                                       | 1,484                     | 1,484             | 1,484             |
| <i>Panel B. All Mobile Money Agent Expansion Villages Excluding the May 2021 Cohort</i> |                                             |                           |                   |                   |
| Mobile Money Agent Arrival                                                              | 0.719**<br>(0.362)                          | 0.742**<br>(0.352)        | 0.696*<br>(0.372) | 0.695*<br>(0.396) |
| Pre-policy control mean                                                                 | 0.700                                       | 0.700                     | 0.700             | 0.700             |
| <i>p</i> -value: joint test of pre-trends                                               | [0.331]                                     | [0.319]                   | [0.410]           | [0.462]           |
| Villages                                                                                | 89                                          | 89                        | 89                | 89                |
| Observations                                                                            | 1,334                                       | 1,334                     | 1,334             | 1,334             |
| Village fixed effects                                                                   | Yes                                         | Yes                       | Yes               | Yes               |
| Time fixed effects                                                                      | Yes                                         | Yes                       | Yes               | Yes               |
| Wealth controls                                                                         | No                                          | Yes                       | Yes               | Yes               |
| Population density controls                                                             | No                                          | No                        | Yes               | Yes               |
| Distance to market controls                                                             | No                                          | No                        | No                | Yes               |

*Notes:* This table reports the average effect of mobile money agent arrival on monthly village-level applications for solar home systems, estimated using the interaction-weighted estimator of [Sun and Abraham \(2021\)](#) applied to Equation (13). The sample consists of 99 villages that received their first mobile money agent within 3 km through the government-led expansion programme between September 2019 and May 2021. The analysis is conducted at the bimonthly frequency; outcomes are averaged across both months within each bimonth to maintain comparability with the monthly subsidy analysis. Villages receiving an agent in January–May 2021 serve as the control group. Panel A includes all 99 expansion villages. Panel B excludes the 10 villages in the May 2021 cohort, restricting the control group to the 5 villages treated in January 2021. Column (1) is the main specification with village and time fixed effects. Columns (2)–(4) add controls for village-level wealth (Relative Wealth Index), population density, and distance to the nearest market. Standard errors clustered at the village level are in parentheses. The *p*-value for the joint test of pre-trends tests the null that all pre-treatment coefficients are jointly zero. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 4: Mobile Money Agent Effects on Adoption by Kit

|                                                                | Monthly Applications for Solar Home Systems |                     |                     |
|----------------------------------------------------------------|---------------------------------------------|---------------------|---------------------|
|                                                                | Basic<br>(1)                                | Plus<br>(2)         | Premium<br>(3)      |
| <i>Panel A. Villages with Access to Mobile Money Agents</i>    |                                             |                     |                     |
| Subsidy                                                        | 0.512**<br>(0.199)                          | 0.193**<br>(0.096)  | 0.350***<br>(0.078) |
| Pre-policy control mean                                        | 0.110                                       | 0.092               | 0.359               |
| <i>p</i> -value: joint test of pre-trends                      | [0.912]                                     | [0.880]             | [0.658]             |
| Villages                                                       | 516                                         | 516                 | 516                 |
| Observations                                                   | 3,612                                       | 3,612               | 3,612               |
| <i>Panel B. Villages without Access to Mobile Money Agents</i> |                                             |                     |                     |
| Subsidy                                                        | 0.168**<br>(0.066)                          | 0.065***<br>(0.016) | 0.116***<br>(0.032) |
| Pre-policy control mean                                        | 0.073                                       | 0.043               | 0.186               |
| <i>p</i> -value: joint test of pre-trends                      | [0.093]                                     | [0.537]             | [0.397]             |
| Villages                                                       | 1,696                                       | 1,696               | 1,696               |
| Observations                                                   | 11,872                                      | 11,872              | 11,872              |
| <i>Panel C. All Mobile Money Agent Expansion Villages</i>      |                                             |                     |                     |
| Mobile Money Agent Arrival                                     | 0.349*<br>(0.183)                           | 0.031<br>(0.067)    | 0.119<br>(0.112)    |
| Pre-policy control mean                                        | 0.143                                       | 0.038               | 0.200               |
| <i>p</i> -value: joint test of pre-trends                      | [0.399]                                     | [0.408]             | [0.239]             |
| Villages                                                       | 99                                          | 99                  | 99                  |
| Observations                                                   | 1,484                                       | 1,484               | 1,484               |
| <i>p</i> -value: test of equality (A = B)                      | [0.140]                                     | [0.175]             | [0.001]             |

*Notes:* This table reports the average effect of mobile money agent arrival on monthly village-level applications for solar home systems, estimated using the interaction-weighted estimator of [Sun and Abraham \(2021\)](#) applied to Equation (13). The sample consists of 99 villages that received their first mobile money agent within 3 km through the government-led expansion programme between September 2019 and May 2021. The analysis is conducted at the bimonthly frequency; outcomes are averaged across both months within each bimonth to maintain comparability with the monthly subsidy analysis. Villages receiving an agent in January–May 2021 serve as the control group. Panel A includes all 99 expansion villages. Panel B excludes the 10 villages in the May 2021 cohort, restricting the control group to the 5 villages treated in January 2021. Column (1) is the main specification with village and time fixed effects. Columns (2)–(4) add controls for village-level wealth (Relative Wealth Index), population density, and distance to the nearest market. Standard errors clustered at the village level are in parentheses. The *p*-value for the joint test of pre-trends tests the null that all pre-treatment coefficients are jointly zero. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 5: Subsidy and Mobile Money Agent Effects on Payment Behaviour

|                                                                |                       |                              |                             | Conditional                  |                             |
|----------------------------------------------------------------|-----------------------|------------------------------|-----------------------------|------------------------------|-----------------------------|
|                                                                | Any<br>Payment<br>(1) | Number of<br>Payments<br>(2) | Multiple<br>Payments<br>(3) | Number of<br>Payments<br>(4) | Multiple<br>Payments<br>(5) |
| <i>Panel A. All Villages</i>                                   |                       |                              |                             |                              |                             |
| Subsidy                                                        | -0.025<br>(0.020)     | -0.255***<br>(0.052)         | -0.030**<br>(0.014)         | -0.313***<br>(0.057)         | -0.032**<br>(0.015)         |
| Pre-policy control mean                                        | 0.713                 | 1.318                        | 0.266                       | 1.857                        | 0.376                       |
| <i>p</i> -value: joint test of pre-trends                      | [0.915]               | [0.446]                      | [0.692]                     | [0.082]                      | [0.978]                     |
| Customers                                                      | 7,493                 | 7,493                        | 7,493                       | 6,719                        | 6,719                       |
| Observations                                                   | 46,543                | 46,543                       | 46,543                      | 32,740                       | 32,740                      |
| <i>Panel B. Villages with Access to Mobile Money Agents</i>    |                       |                              |                             |                              |                             |
| Subsidy                                                        | -0.012<br>(0.021)     | -0.319***<br>(0.058)         | -0.050***<br>(0.018)        | -0.389***<br>(0.063)         | -0.050***<br>(0.019)        |
| Pre-policy control mean                                        | 0.733                 | 1.401                        | 0.274                       | 1.922                        | 0.379                       |
| <i>p</i> -value: joint test of pre-trends                      | [0.370]               | [0.228]                      | [0.754]                     | [0.066]                      | [0.558]                     |
| Customers                                                      | 2,784                 | 2,784                        | 2,784                       | 2,494                        | 2,494                       |
| Observations                                                   | 17,493                | 17,493                       | 17,493                      | 12,440                       | 12,440                      |
| <i>Panel C. Villages without Access to Mobile Money Agents</i> |                       |                              |                             |                              |                             |
| Subsidy                                                        | -0.030<br>(0.025)     | -0.185***<br>(0.046)         | -0.012<br>(0.014)           | -0.228***<br>(0.054)         | -0.016<br>(0.017)           |
| Pre-policy control mean                                        | 0.695                 | 1.244                        | 0.258                       | 1.796                        | 0.374                       |
| <i>p</i> -value: joint test of pre-trends                      | [0.850]               | [0.530]                      | [0.487]                     | [0.515]                      | [0.654]                     |
| Customers                                                      | 4,709                 | 4,709                        | 4,709                       | 4,225                        | 4,225                       |
| Observations                                                   | 29,050                | 29,050                       | 29,050                      | 20,300                       | 20,300                      |
| <i>Panel D. All Mobile Money Agent Expansion Villages</i>      |                       |                              |                             |                              |                             |
| Mobile Money Agent Arrival                                     | -0.043<br>(0.044)     | 0.185<br>(0.252)             | 0.045<br>(0.041)            | 0.301<br>(0.267)             | 0.068**<br>(0.032)          |
| Pre-policy control mean                                        | 0.918                 | 2.228                        | 0.533                       | 2.427                        | 0.581                       |
| <i>p</i> -value: joint test of pre-trends                      | [0.298]               | [0.180]                      | [0.010]                     | [0.546]                      | [0.167]                     |
| Customers                                                      | 590                   | 590                          | 590                         | 545                          | 545                         |
| Observations                                                   | 6,363                 | 6,363                        | 6,363                       | 5,287                        | 5,287                       |
| <i>p</i> -value: test of equality (B = C)                      | [0.549]               | [0.020]                      | [0.052]                     | [0.010]                      | [0.124]                     |

*Notes:* This table reports the average effects of subsidy eligibility and mobile money agent arrival on customer-level payment behaviour. Panels A–C estimate the subsidy effect using the interaction-weighted estimator of [Sun and Abraham \(2021\)](#) applied to Equation (11) at the customer-month level for existing customers. Panel A includes all villages; Panel B restricts to villages within 3 km of a mobile money agent at baseline; Panel C restricts to villages without such access. Panel D estimates the effect of mobile money agent arrival using Equation (13) at the customer-bimonth level for existing customers in the 99 expansion villages. Column (1): binary indicator for whether the customer made any payment that period. Column (2): number of payments made. Column (3): binary indicator for whether the customer made more than one payment. Columns (4) and (5) replicate columns (2) and (3) conditional on the customer making at least one payment. The *p*-value for the test of equality (B = C) tests whether the subsidy effect differs across the two subsamples. All specifications include customer and time fixed effects. Standard errors clustered at the customer level are in parentheses. The *p*-value for the joint test of pre-trends tests the null that all pre-treatment coefficients are jointly zero. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

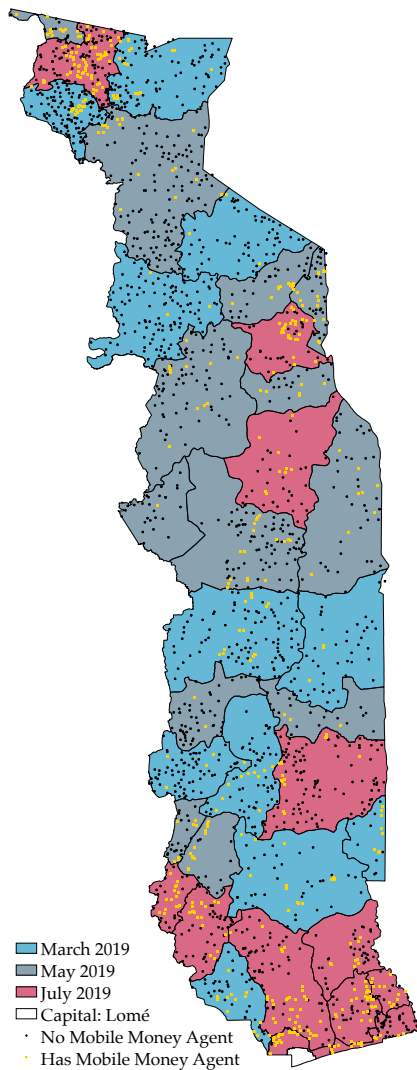
Table 6: Heterogeneous Treatment Effects of Novissi by Mobile Money Access

|                                                                                   | Food<br>Security<br>(1) | Financial<br>Health<br>(2) | Financial<br>Inclusion<br>(3) | Mental<br>Health<br>(4) | Perceived<br>Status<br>(5) | Health Care<br>Access<br>(6) | Labour<br>Supply<br>(7) | All 7<br>Indices<br>(8) |
|-----------------------------------------------------------------------------------|-------------------------|----------------------------|-------------------------------|-------------------------|----------------------------|------------------------------|-------------------------|-------------------------|
| <i>Panel A: Aiken et al. (2025) main treatment effects (full sample, N=9,511)</i> |                         |                            |                               |                         |                            |                              |                         |                         |
| Treatment (original)                                                              | 0.064***<br>(0.022)     | 0.026<br>(0.024)           | 0.007<br>(0.022)              | 0.072***<br>(0.019)     | 0.040*<br>(0.022)          | 0.010<br>(0.023)             | 0.009<br>(0.025)        | 0.061***<br>(0.023)     |
| <i>Panel B: Heterogeneous effects by mobile money access (N=9,284)</i>            |                         |                            |                               |                         |                            |                              |                         |                         |
| Treatment ( $\beta_1$ )                                                           | 0.073***<br>(0.022)     | 0.032<br>(0.024)           | 0.007<br>(0.022)              | 0.073***<br>(0.018)     | 0.038*<br>(0.022)          | 0.004<br>(0.023)             | 0.014<br>(0.025)        | 0.064***<br>(0.023)     |
| Treatment $\times$ MoMo Access ( $\beta_3$ )                                      | 0.018<br>(0.022)        | 0.049**<br>(0.025)         | -0.004<br>(0.023)             | -0.028<br>(0.019)       | 0.019<br>(0.024)           | 0.040*<br>(0.023)            | 0.004<br>(0.027)        | 0.026<br>(0.023)        |
| MoMo Access ( $\beta_2$ )                                                         | -0.024<br>(0.015)       | -0.040**<br>(0.019)        | 0.003<br>(0.016)              | 0.013<br>(0.014)        | 0.014<br>(0.017)           | -0.005<br>(0.017)            | -0.009<br>(0.022)       | -0.013<br>(0.017)       |
| Observations                                                                      | 9,284                   | 9,284                      | 9,284                         | 9,284                   | 9,284                      | 9,284                        | 9,284                   | 9,284                   |
| Control mean                                                                      | -0.004                  | -0.002                     | 0.004                         | 0.007                   | -0.004                     | 0.013                        | 0.005                   | 0.005                   |
| $R^2$                                                                             | 0.283                   | 0.175                      | 0.187                         | 0.430                   | 0.239                      | 0.177                        | 0.151                   | 0.185                   |
| Strata FE                                                                         | Yes                     | Yes                        | Yes                           | Yes                     | Yes                        | Yes                          | Yes                     | Yes                     |
| Enumerator FE                                                                     | Yes                     | Yes                        | Yes                           | Yes                     | Yes                        | Yes                          | Yes                     | Yes                     |
| Survey week FE                                                                    | Yes                     | Yes                        | Yes                           | Yes                     | Yes                        | Yes                          | Yes                     | Yes                     |

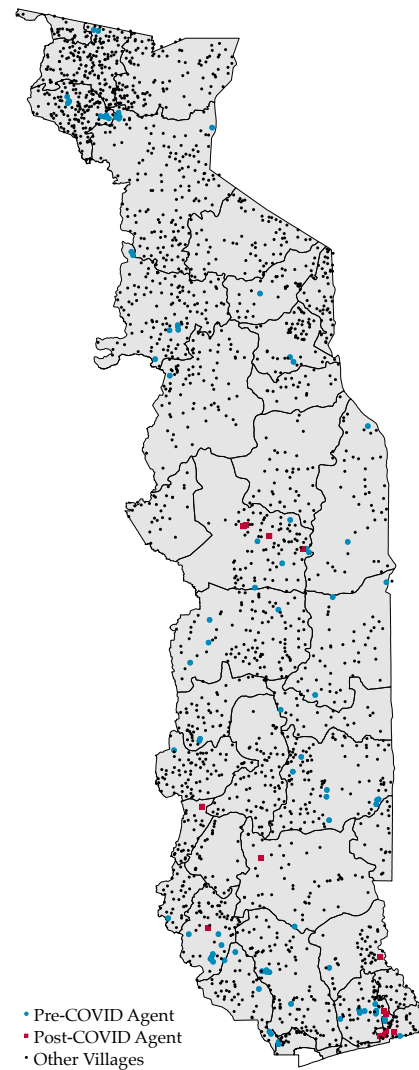
*Notes:* Panel A reproduces the main treatment effects from Aiken et al. (2025), Table 1, Panel A, using the full sample. Panel B reports OLS estimates of equation (A1) on the subsample of individuals in cantons matched to the mobile money data. Each column corresponds to a different outcome, measured as a standardised index (mean zero, unit standard deviation in the control group). Treatment is the randomly assigned indicator for receiving the Novissi cash transfer. MoMo Access is the share of villages in the respondent's canton that have at least one mobile money agent within 3 km, standardised to have mean zero and unit standard deviation across cantons. All regressions include strata, enumerator, and survey week fixed effects, and are weighted by the sampling and response weights from the original study. Heteroskedasticity-robust standard errors are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Figure 1: Districts of Togo by Policy Rollout

Panel A. Subsidy Rollout by District

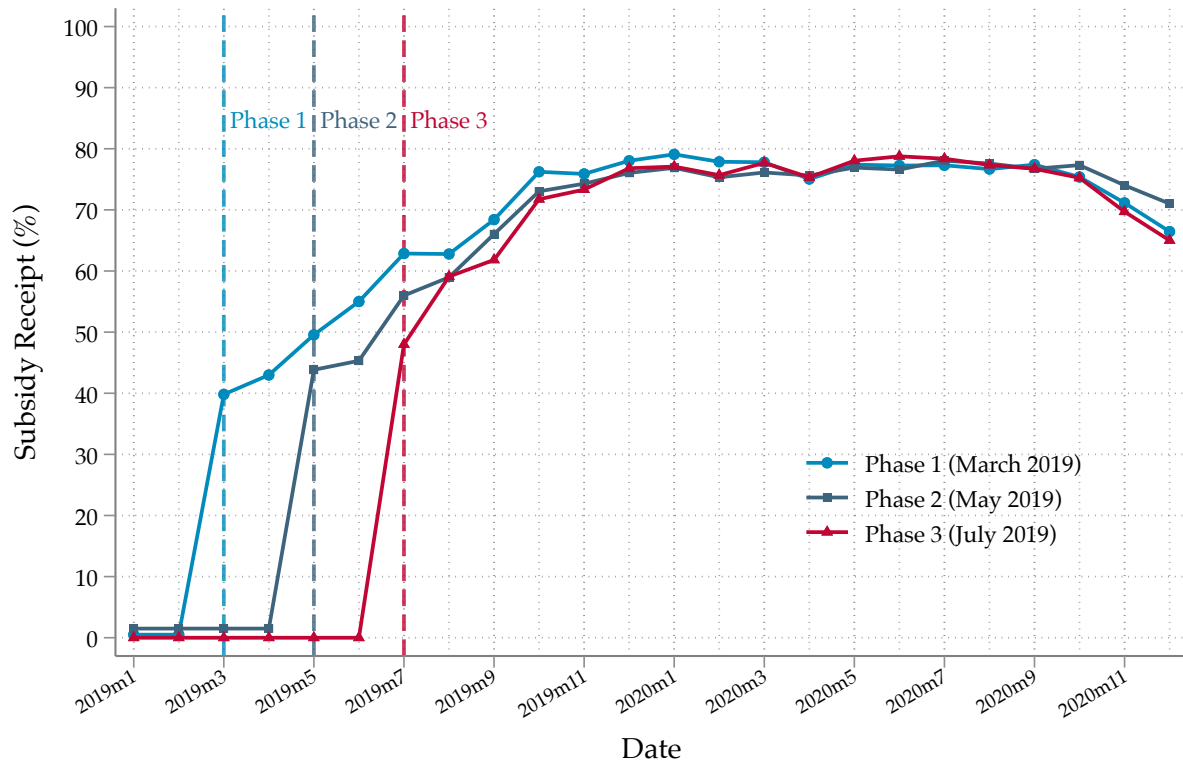


Panel B. Mobile Money Agent Villages



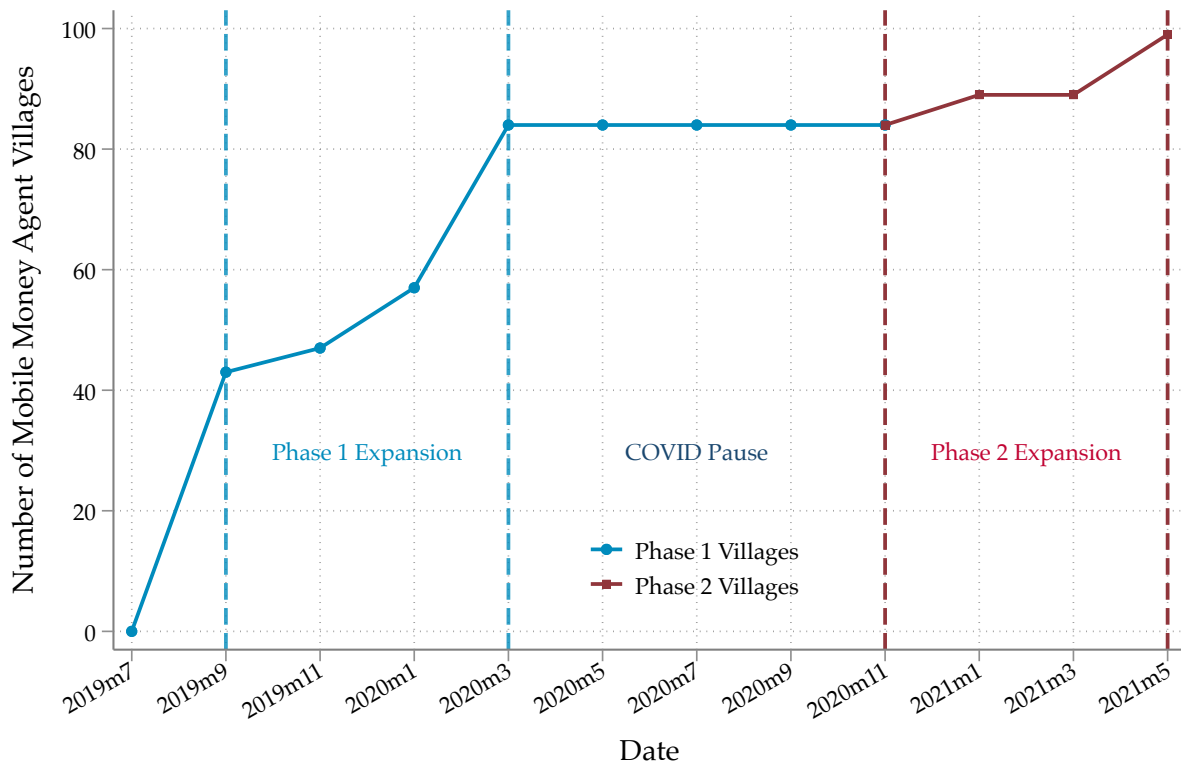
*Notes:* Panel A illustrates the spatial distribution of the subsidy programme rollout across Togo's districts. Districts shaded in the light blue represent the first phase, where the programme launched in March 2019. Districts in dark blue indicate the second phase, beginning in May 2019. The red districts correspond to the final phase, initiated in July 2019. Panel B depicts the locations of villages that were part of the mobile money agent expansion programme.

Figure 2: Subsidy Policy Rollout



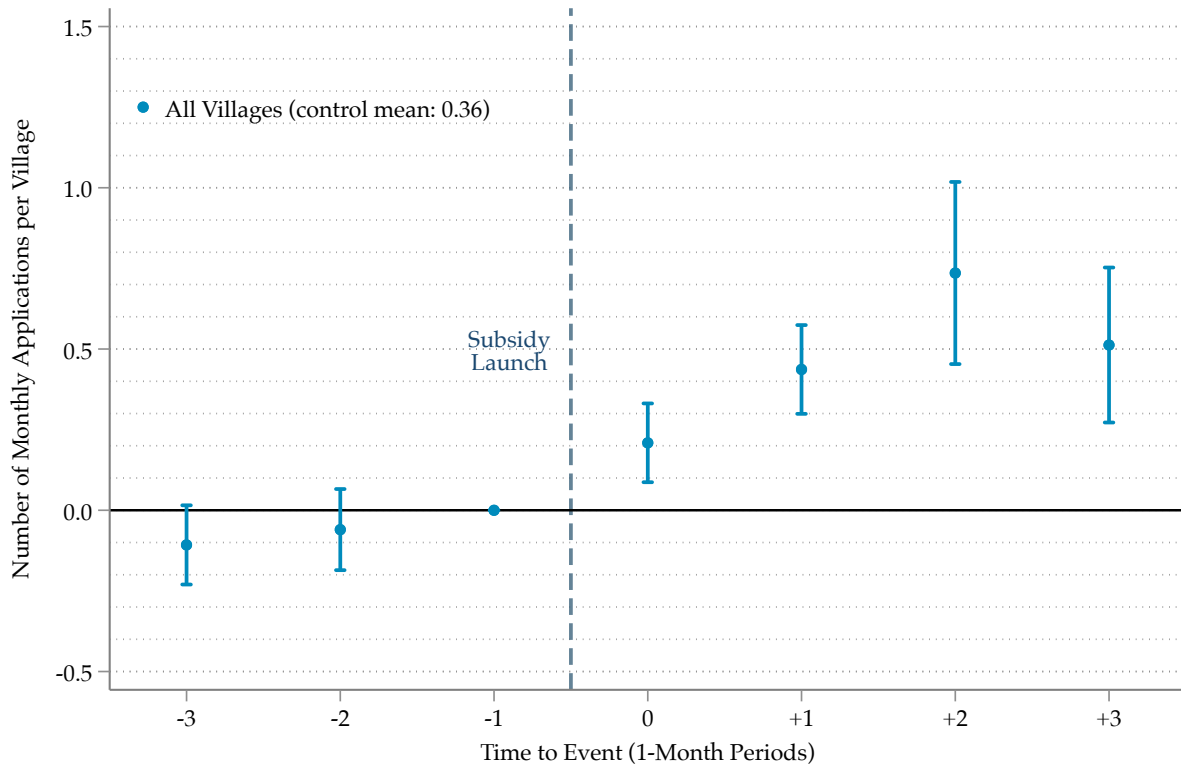
*Notes:* The figure illustrates the spatial distribution of subsidy program rollout across Togo's districts. Districts shaded in the lightest hue represent the first phase, where the program launched in March 2019. Districts in a slightly darker shade indicate the second phase, beginning in May 2019. The darkest shaded districts correspond to the final phase, initiated in July 2019.

Figure 3: Mobile Money Agent Policy Rollout



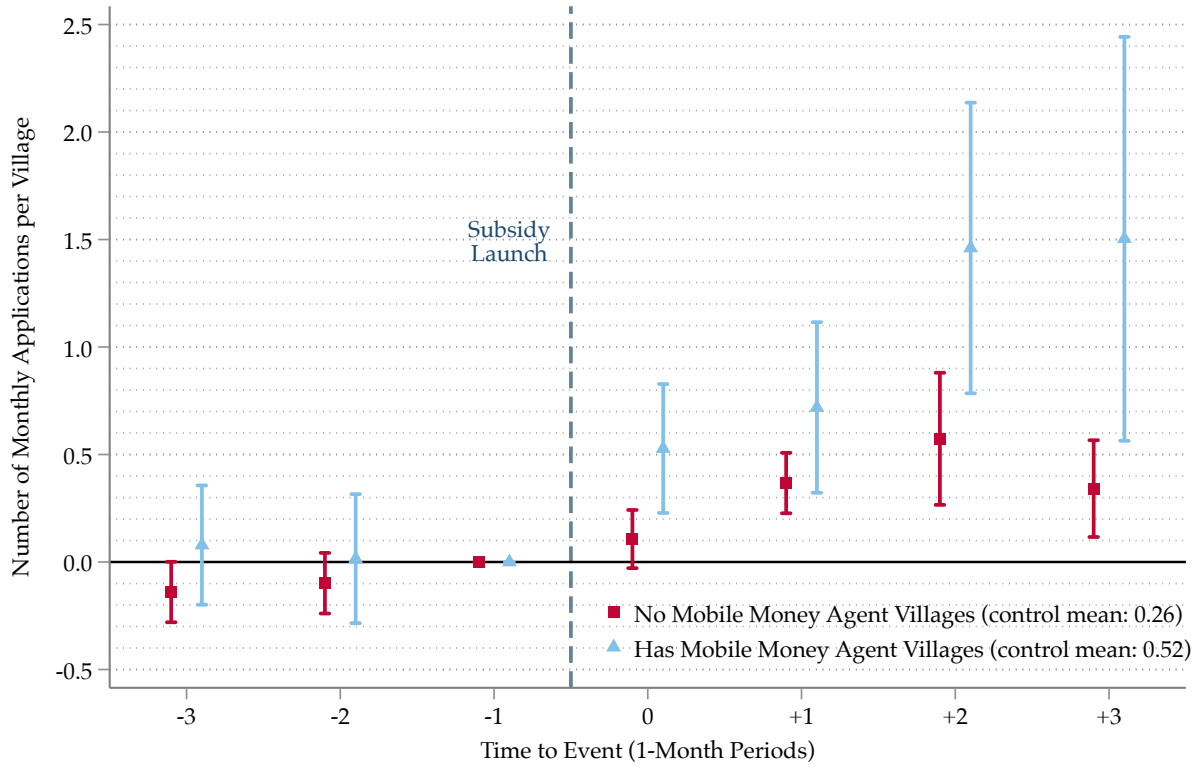
Notes: This figure plots the evolution of the number of villages with a mobile money agent within 3km, due to the expansion campaign described in Section 1.5. The first wave of the expansion took place between September 2019 and March 2020. The second wave of the expansion took place between January and May 2021.

Figure 4: Subsidy Effects on Adoption



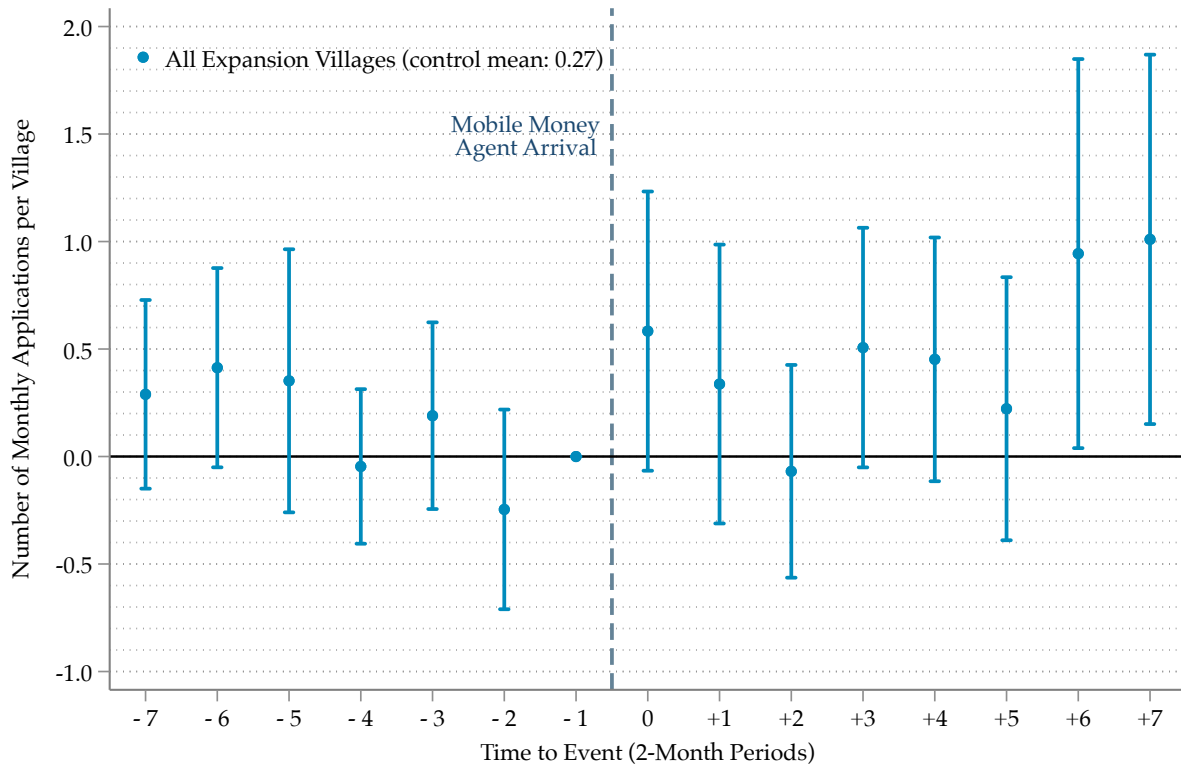
*Notes:* This figure displays village-level dynamic ITT effects of subsidy eligibility on solar home system (SHS) adoption, estimated by equation 10, following Sun and Abraham (2021). Our empirical strategy compares villages in districts that launched the subsidy in March and May 2019 (treatment group) to villages in districts that launched it in July 2019 (control group). The outcome variable is the number of SHS applications per month in each village. The number in parentheses at the point (0,0) denotes the mean value of the outcome in the control group prior to subsidy launch. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the village level. Table 2 aggregates the dynamic effects into a single average effect.

Figure 5: Heterogeneous Subsidy Effect on Adoption by Mobile Money Agent Access



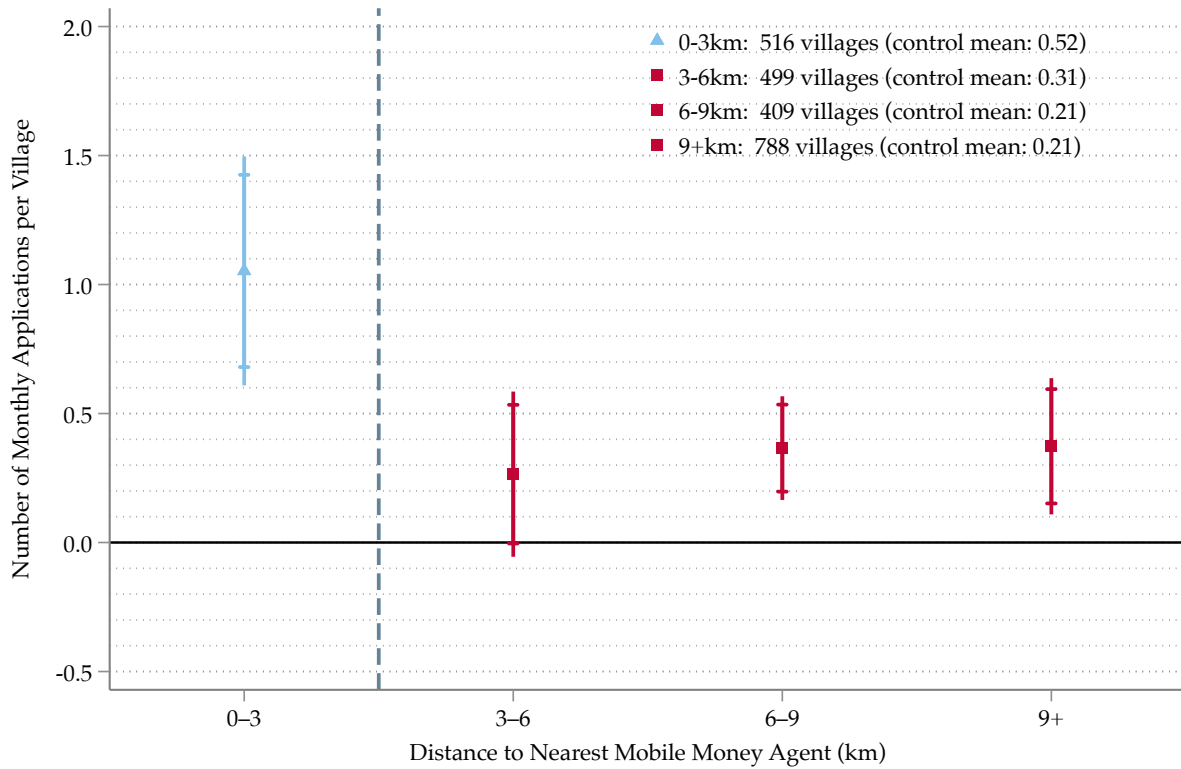
Notes: This figure displays village-level heterogeneous ITT effects of subsidy eligibility on solar home system (SHS) adoption, estimated by equation 10 separately for each subset of villages, following Sun and Abraham (2021). Our empirical strategy compares villages in districts that launched the subsidy in March and May 2019 (treatment group) to villages in districts that launched it in July 2019 (control group). The subsidy's impact for villages that had a mobile money agent prior to subsidy launch is shown in red, and the impact for villages without an agent in blue. The outcome variable is the number of SHS applications per month in each village. The number in parentheses at the point (0,0) denotes the mean value of the outcome in the control group prior to subsidy launch. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the village level. Table 2 aggregates the dynamic effects into a single average effect and tests whether it differs between villages with and without a mobile money agent, defined as having an agent within a 3km radius of the village centre.

Figure 6: Mobile Money Agent Effect on Adoption



*Notes:* This figure displays village-level dynamic ITT effects of mobile money agent arrival on solar home system (SHS) adoption, estimated by equation 12, following Sun and Abraham (2021). Our empirical strategy compares villages that received a mobile money agent between September 2019 and March 2020 through the agent expansion policy (treatment group) to villages that received an agent later, between January and May 2021 (control group). The analysis is conducted at the bimonthly frequency; the outcome variable is the average number of SHS applications per month in each village. The number in parentheses at the point (0,0) denotes the mean value of the outcome in the control group prior to agent arrival. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the village level. Table 3 aggregates the dynamic effects into a single average effect.

Figure 7: Subsidy Effect on Adoption by Distance to Nearest Mobile Money Agent



*Notes:* This figure displays village-level heterogeneous ITT effects of subsidy eligibility on solar home system (SHS) adoption, estimated by equation 10 separately for four groups of villages defined by their pre-subsidy distance to the nearest mobile money agent, following Sun and Abraham (2021). Our empirical strategy compares villages in districts that launched the subsidy in March and May 2019 (treatment group) to villages in districts that launched it in July 2019 (control group). Villages are grouped into four distance bands: 0–3 km (with a mobile money agent), 3–6 km, 6–9 km, and 9+ km. The outcome variable is the number of SHS applications per month in each village. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the village level. Table 2 reports the corresponding average effects.

# Appendix

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# A1 Appendix Tables and Figures

Table A1: Balance Table Across Groups of Districts

| February 2019                                        | (1)<br>Group 1   | (2)<br>Group 2   | (3)<br>Group 3   | (4)<br>F-stat ANOVA | (5)<br>N | All districts    |
|------------------------------------------------------|------------------|------------------|------------------|---------------------|----------|------------------|
| <i>Panel A. Demographics</i>                         |                  |                  |                  |                     |          |                  |
| HH's age at adoption                                 | 40.25<br>(2.63)  | 39.92<br>(2.79)  | 41.64<br>(1.62)  | (1.74)              | 36       | 40.59<br>(2.46)  |
| Female-headed Households (%)                         | 0.74<br>(1.21)   | 1.44<br>(1.56)   | 3.35<br>(2.79)   | (5.45***)           | 36       | 1.86<br>(2.22)   |
| Customer income: agriculture (%)                     | 83.04<br>(15.21) | 77.22<br>(17.59) | 66.88<br>(19.60) | (2.50*)             | 36       | 75.55<br>(18.37) |
| Customer income: own biz (%)                         | 8.13<br>(8.22)   | 10.20<br>(8.66)  | 15.71<br>(9.28)  | (2.35)              | 36       | 11.40<br>(9.07)  |
| Customer income: employee (%)                        | 7.28<br>(6.49)   | 10.14<br>(8.54)  | 14.58<br>(11.86) | (1.82)              | 36       | 10.75<br>(9.50)  |
| Customer income: other (%)                           | 1.56<br>(2.08)   | 2.44<br>(2.80)   | 2.83<br>(2.82)   | (0.71)              | 36       | 2.30<br>(2.59)   |
| <i>Panel B. Previous Energy Sources for Lighting</i> |                  |                  |                  |                     |          |                  |
| Any Electricity Sources (%)                          | 2.53<br>(3.66)   | 1.46<br>(2.28)   | 2.38<br>(2.50)   | (0.51)              | 36       | 2.09<br>(2.80)   |
| Solar (%)                                            | 1.26<br>(2.43)   | 0.91<br>(2.09)   | 1.23<br>(1.64)   | (0.11)              | 36       | 1.12<br>(2.01)   |
| Grid (%)                                             | 0.79<br>(1.15)   | 0.07<br>(0.17)   | 0.75<br>(1.22)   | (2.27)              | 36       | 0.52<br>(0.99)   |
| Generator (%)                                        | 0.47<br>(0.78)   | 0.49<br>(1.26)   | 0.40<br>(0.95)   | (0.02)              | 36       | 0.45<br>(1.00)   |
| Flashlight (%)                                       | 48.04<br>(29.42) | 42.22<br>(29.83) | 50.57<br>(26.08) | (0.28)              | 36       | 46.78<br>(27.92) |
| Kerosene/Lantern/None (%)                            | 48.44<br>(26.82) | 55.49<br>(30.60) | 46.32<br>(27.04) | (0.36)              | 36       | 50.28<br>(27.80) |
| <i>Panel C. Electricity Behavior</i>                 |                  |                  |                  |                     |          |                  |
| Basic Kit (Light+Charger) (%)                        | 28.32<br>(14.45) | 29.93<br>(21.25) | 22.86<br>(8.90)  | (0.66)              | 36       | 27.08<br>(15.78) |
| Plus Kit (Above+Radio) (%)                           | 25.60<br>(16.47) | 32.16<br>(20.36) | 20.83<br>(11.49) | (1.46)              | 36       | 26.38<br>(16.86) |
| Premium Kit (Above+TV) (%)                           | 46.08<br>(24.14) | 37.91<br>(19.51) | 56.31<br>(16.83) | (2.59*)             | 36       | 46.54<br>(21.13) |
| Number of Payment Days                               | 1.60<br>(0.33)   | 1.53<br>(0.27)   | 1.90<br>(0.36)   | (4.44**)            | 36       | 1.67<br>(0.35)   |
| Average Payment Size (in days)                       | 35.90<br>(21.94) | 30.79<br>(8.27)  | 26.11<br>(3.74)  | (1.57)              | 36       | 30.79<br>(13.46) |
| Utilization Rate (%)                                 | 91.81<br>(3.77)  | 92.29<br>(4.29)  | 92.54<br>(3.28)  | (0.11)              | 36       | 92.23<br>(3.72)  |
| # of districts                                       | 11               | 13               | 12               |                     |          | 36               |
| # of consumers per district                          | 154              | 115              | 94               |                     |          | 120              |

Notes: The table summarizes the characteristics of customers who joined by February 2019, before the subsidy, and who receive the subsidy at some point by December 2019. Panel A displays demographic, population, and past energy sources variables, and Panel B electricity behavior variables. The first three columns show the mean values of the district averages for each of the three groups of districts treated at different times. This is measured at baseline, before the subsidy intervention in February 2019, with standard deviations in parentheses. The fourth column shows the F-stat of the one-way analysis-of-variance (ANOVA) model to test for differences in means across the three groups. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A2: Subsidy Effects on Adoption with Different Distance Cutoffs

|                                                                | Monthly Applications for Solar Home Systems              |                     |                     |                     |                     |
|----------------------------------------------------------------|----------------------------------------------------------|---------------------|---------------------|---------------------|---------------------|
|                                                                | Cutoff Distance ( $d$ ) for Access to Mobile Money Agent |                     |                     |                     |                     |
|                                                                | $d \leq 1km$<br>(1)                                      | $d \leq 2km$<br>(2) | $d \leq 3km$<br>(3) | $d \leq 4km$<br>(4) | $d \leq 5km$<br>(5) |
| <i>Panel A. Villages with Access to Mobile Money Agents</i>    |                                                          |                     |                     |                     |                     |
| Subsidy                                                        | 1.682***<br>(0.468)                                      | 1.277***<br>(0.372) | 1.053***<br>(0.299) | 0.918***<br>(0.205) | 0.720***<br>(0.177) |
| Pre-policy control mean                                        | 0.759                                                    | 0.663               | 0.558               | 0.526               | 0.465               |
| $p$ -value: joint test of pre-trends                           | [0.571]                                                  | [0.815]             | [0.869]             | [0.715]             | [0.909]             |
| Villages                                                       | 260                                                      | 394                 | 516                 | 682                 | 863                 |
| Observations                                                   | 1,820                                                    | 2,758               | 3,612               | 4,774               | 6,041               |
| <i>Panel B. Villages without Access to Mobile Money Agents</i> |                                                          |                     |                     |                     |                     |
| Subsidy                                                        | 0.363***<br>(0.073)                                      | 0.353***<br>(0.083) | 0.347***<br>(0.085) | 0.336***<br>(0.092) | 0.369***<br>(0.102) |
| Pre-policy control mean                                        | 0.315                                                    | 0.295               | 0.301               | 0.272               | 0.296               |
| $p$ -value: joint test of pre-trends                           | [0.381]                                                  | [0.402]             | [0.397]             | [0.328]             | [0.376]             |
| Villages                                                       | 1,952                                                    | 1,818               | 1,696               | 1,530               | 1,349               |
| Observations                                                   | 13,664                                                   | 12,726              | 11,872              | 10,710              | 9,443               |
| $p$ -value: test of equality ( $A = B$ )                       | [0.006]                                                  | [0.021]             | [0.033]             | [0.015]             | [0.124]             |
| Village fixed effects                                          | Yes                                                      | Yes                 | Yes                 | Yes                 | Yes                 |
| Time fixed effects                                             | Yes                                                      | Yes                 | Yes                 | Yes                 | Yes                 |

*Notes:* This table reports the average effect of subsidy eligibility on monthly village-level applications for solar home systems, estimated using the interaction-weighted estimator of [Sun and Abraham \(2021\)](#) applied to Equation (11). The sample covers 2,212 villages observed monthly from December 2018 to October 2019. Treatment is defined by the staggered rollout of subsidy eligibility across three groups of districts (March, May, and July 2019), with the last group serving as the control. Panel A includes all villages. Panel B restricts to villages within 3 km of a mobile money agent at baseline; Panel C restricts to villages without such access. The  $p$ -value for the test of equality ( $B = C$ ) tests whether the subsidy effect differs across the two subsamples. Column (1) is the main specification with village and time fixed effects. Columns (2)–(4) add controls for village-level wealth (Relative Wealth Index), population density, and distance to the nearest market. Standard errors clustered at the village level are in parentheses. The  $p$ -value for the joint test of pre-trends tests the null that all pre-treatment coefficients are jointly zero. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table A3: Summary Statistics Pre- and Post-Subsidy Customers

|                                         | (1)<br>Pre-subsidy<br>(February 2019) | (2)<br>Post-subsidy (May 2020) | (3)<br>Test of Difference |
|-----------------------------------------|---------------------------------------|--------------------------------|---------------------------|
| <i>Panel A. Demographics</i>            |                                       |                                |                           |
| HH's age at adoption                    | 40.71<br>(11.26)                      | 39.80<br>(11.75)               | (-3.36***)                |
| Female-headed Households (%)            | 1.60<br>(12.54)                       | 2.48<br>(15.56)                | (2.03*)                   |
| Customer income: agriculture (%)        | 77.53<br>(41.74)                      | 70.54<br>(45.59)               | (-3.16***)                |
| Customer income: own biz (%)            | 9.86<br>(29.81)                       | 14.51<br>(35.22)               | (3.43***)                 |
| Customer income: employee (%)           | 10.53<br>(30.69)                      | 11.47<br>(31.87)               | (1.59)                    |
| Customer income: other (%)              | 2.08<br>(14.28)                       | 3.48<br>(18.33)                | (2.98***)                 |
| <i>Panel B. Previous Energy Sources</i> |                                       |                                |                           |
| Any Electricity Sources (%)             | 2.22<br>(14.74)                       | 1.34<br>(11.48)                | (-1.82*)                  |
| Solar (%)                               | 1.27<br>(11.21)                       | 0.68<br>(8.20)                 | (-1.63)                   |
| Grid (%)                                | 0.44<br>(6.62)                        | 0.36<br>(5.95)                 | (-0.74)                   |
| Generator (%)                           | 0.51<br>(7.12)                        | 0.30<br>(5.50)                 | (-1.58)                   |
| Flashlight (%)                          | 44.01<br>(49.65)                      | 26.56<br>(44.17)               | (-4.57***)                |
| Kerosene/Lantern/None (%)               | 52.75<br>(49.93)                      | 71.46<br>(45.16)               | (5.03***)                 |
| <i>Panel C. Electricity Behavior</i>    |                                       |                                |                           |
| Basic Kit (Light+Charger) (%)           | 27.53<br>(44.67)                      | 39.04<br>(48.79)               | (6.91***)                 |
| Plus Kit (Above+Radio) (%)              | 29.48<br>(45.60)                      | 20.64<br>(40.47)               | (-4.19***)                |
| Premium Kit (Above+TV) (%)              | 42.99<br>(49.51)                      | 40.32<br>(49.06)               | (-1.49)                   |
| Number of Payment Days                  | 1.65<br>(1.41)                        | 1.94<br>(1.76)                 | (6.12***)                 |
| Average Payment Size (in days)          | 28.02<br>(26.36)                      | 16.49<br>(12.33)               | (-14.82***)               |
| Utilization Rate (%) (%)                | 90.98<br>(20.31)                      | 87.96<br>(22.07)               | (-5.66***)                |
| Number of Customers                     | 4322                                  | 11523                          |                           |

*Notes:* The table summarizes the characteristics of the consumers available in our data, for demographic variables (Panel A), energy sources (Panel B) and electricity behavior (Panel C). Column (1) show the mean values of each variable for customers who joined by February 2019, before the subsidy, and who received the subsidy by December 2019. Column (2) shows the mean values for all customers who had joined by May 2020, nearly a year after the subsidy. Standard deviations in brackets. The third column shows the t-stat of the difference between the baseline and endline group of consumers, where the following OLS regression equation, controlling for district fixed effect, is performed:  $Y_i = \beta May2020 + District_i + \epsilon_i$ , with standard errors clustered at the district-level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A4: Balance Table Across Groups of Districts - Government Data

|                            | (1)<br>Group 1 | (2)<br>Group 2  | (3)<br>Group 3  | (4)<br>F-stat ANOVA | (5)<br>N | All districts   |
|----------------------------|----------------|-----------------|-----------------|---------------------|----------|-----------------|
| Electrified households (%) | 5.00<br>(0.00) | 16.54<br>(6.58) | 23.18<br>(7.83) | (26.37***)          | 35       | 15.00<br>(9.39) |
| # of districts             | 11             | 13              | 11              |                     |          | 35              |

*Notes:* The table summarizes the electrification rates of the districts within the three groups. This is based on data collected in 2016 from the Government of Togo (data is unavailable for one out of thirty-six districts). The first three columns show the mean values of the district averages for each of the three groups of districts treated at different times, with standard deviations in parentheses. The electrification rates are provided by the Government in ranges, and so the midpoint of the range is used for each district average (all districts in Group 1 have electrification rates between 0 and 10%, hence the 5% mean with no standard deviation). The fourth column shows the F-stat of the one-way analysis-of-variance (ANOVA) model to test for differences in means across the three groups. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A5: Subsidy and Mobile Money Agent Effects on Installations

|                                                                | Monthly Installations for Solar Home Systems |                           |                     |                     |
|----------------------------------------------------------------|----------------------------------------------|---------------------------|---------------------|---------------------|
|                                                                | Main Specification                           | Robustness Specifications |                     |                     |
|                                                                | (1)                                          | (2)                       | (3)                 | (4)                 |
| <i>Panel A. All Villages</i>                                   |                                              |                           |                     |                     |
| Subsidy                                                        | 0.384***<br>(0.081)                          | 0.413***<br>(0.085)       | 0.412***<br>(0.084) | 0.426***<br>(0.097) |
| Pre-policy control mean                                        | 0.393                                        | 0.393                     | 0.393               | 0.393               |
| <i>p</i> -value: joint test of pre-trends                      | [0.411]                                      | [0.520]                   | [0.509]             | [0.491]             |
| Villages                                                       | 2,212                                        | 2,212                     | 2,212               | 2,212               |
| Observations                                                   | 15,484                                       | 15,484                    | 15,484              | 15,484              |
| <i>Panel B. Villages with Access to Mobile Money Agents</i>    |                                              |                           |                     |                     |
| Subsidy                                                        | 0.992***<br>(0.301)                          | 1.054***<br>(0.320)       | 1.058***<br>(0.321) | 1.062***<br>(0.315) |
| Pre-policy control mean                                        | 0.563                                        | 0.563                     | 0.563               | 0.563               |
| <i>p</i> -value: joint test of pre-trends                      | [0.725]                                      | [0.703]                   | [0.691]             | [0.693]             |
| Villages                                                       | 516                                          | 516                       | 516                 | 516                 |
| Observations                                                   | 3,612                                        | 3,612                     | 3,612               | 3,612               |
| <i>Panel C. Villages without Access to Mobile Money Agents</i> |                                              |                           |                     |                     |
| Subsidy                                                        | 0.253***<br>(0.091)                          | 0.256***<br>(0.090)       | 0.234***<br>(0.090) | 0.244**<br>(0.096)  |
| Pre-policy control mean                                        | 0.291                                        | 0.291                     | 0.291               | 0.291               |
| <i>p</i> -value: joint test of pre-trends                      | [0.212]                                      | [0.216]                   | [0.213]             | [0.186]             |
| Villages                                                       | 1,696                                        | 1,696                     | 1,696               | 1,696               |
| Observations                                                   | 11,872                                       | 11,872                    | 11,872              | 11,872              |
| <i>Panel D. All Mobile Money Agent Expansion Villages</i>      |                                              |                           |                     |                     |
| Mobile Money Agent Arrival                                     | 0.485**<br>(0.220)                           | 0.469**<br>(0.233)        | 0.463**<br>(0.232)  | 0.452*<br>(0.241)   |
| Pre-policy control mean                                        | 0.381                                        | 0.381                     | 0.381               | 0.381               |
| <i>p</i> -value: joint test of pre-trends                      | [0.114]                                      | [0.078]                   | [0.073]             | [0.120]             |
| Villages                                                       | 99                                           | 99                        | 99                  | 99                  |
| Observations                                                   | 1,484                                        | 1,484                     | 1,484               | 1,484               |
| <i>p</i> -value: test of equality (A = B)                      | [0.021]                                      | [0.018]                   | [0.015]             | [0.012]             |
| Village fixed effects                                          | Yes                                          | Yes                       | Yes                 | Yes                 |
| Time fixed effects                                             | Yes                                          | Yes                       | Yes                 | Yes                 |
| Wealth controls                                                | No                                           | Yes                       | Yes                 | Yes                 |
| Population density controls                                    | No                                           | No                        | Yes                 | Yes                 |
| Distance to market controls                                    | No                                           | No                        | No                  | Yes                 |

*Notes:* This table replicates the adoption analysis of Table 2 using installations (SHS units physically installed) rather than applications as the outcome variable. Panels A–C estimate the subsidy effect using the interaction-weighted estimator of Sun and Abraham (2021) applied to Equation (11) at the village-month level. Panel A includes all 2,212 villages; Panel B restricts to villages within 3 km of a mobile money agent at baseline (516 villages); Panel C restricts to villages without such access (1,696 villages). Panel D estimates the effect of mobile money agent arrival using Equation (13) at the bimonthly level for the 99 expansion villages. The *p*-value for the test of equality (B = C) tests whether the subsidy effect differs across the two subsamples. Column (1) is the main specification with village and time fixed effects. Columns (2)–(4) add controls for village-level wealth, population density, and distance to the nearest market. Standard errors clustered at the village level are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table A6: Subsidy Effects on Adoption for Mobile Money Agent Expansion Villages

|                                                       | Monthly Applications for Solar Home Systems |                           |                  |                  |
|-------------------------------------------------------|---------------------------------------------|---------------------------|------------------|------------------|
|                                                       | Main Specification                          | Robustness Specifications |                  |                  |
|                                                       | (1)                                         | (2)                       | (3)              | (4)              |
| <i>Panel A. Mobile Money Agent Expansion Villages</i> |                                             |                           |                  |                  |
| Subsidy                                               | 0.384<br>(0.289)                            | 0.356<br>(0.301)          | 0.361<br>(0.292) | 0.379<br>(0.294) |
| Pre-policy control mean                               | 0.478                                       | 0.478                     | 0.478            | 0.478            |
| <i>p</i> -value: joint test of pre-trends             | [0.332]                                     | [0.289]                   | [0.313]          | [0.254]          |
| Villages                                              | 99                                          | 99                        | 99               | 99               |
| Observations                                          | 693                                         | 693                       | 693              | 693              |
| Village fixed effects                                 | Yes                                         | Yes                       | Yes              | Yes              |
| Time fixed effects                                    | Yes                                         | Yes                       | Yes              | Yes              |
| Wealth controls                                       | No                                          | Yes                       | Yes              | Yes              |
| Population density controls                           | No                                          | No                        | Yes              | Yes              |
| Distance to market controls                           | No                                          | No                        | No               | Yes              |

*Notes:* This table estimates the subsidy effect on monthly applications for the 99 villages that later received a mobile money agent through the government-led expansion programme. Panel A reports the average effect of subsidy eligibility using the interaction-weighted estimator of [Sun and Abraham \(2021\)](#) applied to Equation (11) at the village-month level. This specification tests whether the subsidy effect in the expansion villages prior to agent arrival resembles that of other non-mobile-money villages, helping to verify that the mobile money agent expansion villages are an appropriate comparison group. Column (1) is the main specification with village and time fixed effects. Columns (2)–(4) add controls for village-level wealth, population density, and distance to the nearest market. Standard errors clustered at the village level are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table A7: Transaction Costs of Paying for Electricity

|                                                   | Pre-expansion<br>(1) | Post-expansion<br>(2) | Change<br>(3) |
|---------------------------------------------------|----------------------|-----------------------|---------------|
| <i>Panel A. Inputs</i>                            |                      |                       |               |
| Monthly electricity bill (CFA)                    | 4,800                | 4,800                 | 0             |
| Distance to nearest MM agent (km)                 | 8.0                  | 1.4                   | -6.6          |
| Payments per month                                | 1.6                  | 1.6                   | 0             |
| Fixed cost of transportation (CFA)                | 350                  | 350                   | 0             |
| Travel cost per km (CFA)                          | 50                   | 50                    | 0             |
| <i>Panel B. Implied monthly transaction costs</i> |                      |                       |               |
| Travel cost of payments (CFA)                     | 2,400                | 1,344                 | -1,056        |
| Travel cost as % of electricity bill              | 50.0%                | 28.0%                 | -22.0%        |

Notes: This table illustrates how the expansion of mobile money agent access reduces the transaction costs of paying for electricity. Panel A reports the inputs: the monthly electricity bill (4,800 CFA, held constant), the average distance to the nearest mobile money agent before and after the expansion, the average number of payment trips per month, and the median travel cost per kilometer from our transport cost data. Panel B reports the implied monthly travel cost and its share of the electricity bill. The key comparison is the bottom row: before the expansion, households spent an additional 53.3% of their electricity bill on transportation to make payments; after the expansion, this fell to 9.3%. Travel costs assume a round trip for each payment.

Table A8: Mobile Money Agent Effects on Adoption Pre-COVID

|                                                                          | Monthly Applications for Solar Home Systems |                           |                  |                  |
|--------------------------------------------------------------------------|---------------------------------------------|---------------------------|------------------|------------------|
|                                                                          | Main Specification                          | Robustness Specifications |                  |                  |
|                                                                          | (1)                                         | (2)                       | (3)              | (4)              |
| <i>Panel A. Mobile Money Agent Expansion Villages (up to May 2020)</i>   |                                             |                           |                  |                  |
| Subsidy                                                                  | 0.483**<br>(0.241)                          | 0.426*<br>(0.258)         | 0.409<br>(0.258) | 0.394<br>(0.290) |
| Pre-policy control mean                                                  | 0.381                                       | 0.381                     | 0.381            | 0.381            |
| <i>p</i> -value: joint test of pre-trends                                | [0.143]                                     | [0.102]                   | [0.087]          | [0.143]          |
| Villages                                                                 | 99                                          | 99                        | 99               | 99               |
| Observations                                                             | 1,187                                       | 1,187                     | 1,187            | 1,187            |
| <i>Panel B. Mobile Money Agent Expansion Villages (up to March 2020)</i> |                                             |                           |                  |                  |
| Subsidy                                                                  | 0.376<br>(0.239)                            | 0.319<br>(0.267)          | 0.297<br>(0.266) | 0.257<br>(0.300) |
| Pre-policy control mean                                                  | 0.381                                       | 0.381                     | 0.381            | 0.381            |
| <i>p</i> -value: joint test of pre-trends                                | [0.143]                                     | [0.102]                   | [0.087]          | [0.143]          |
| Villages                                                                 | 99                                          | 99                        | 99               | 99               |
| Observations                                                             | 1,088                                       | 1,088                     | 1,088            | 1,088            |
| Village fixed effects                                                    | Yes                                         | Yes                       | Yes              | Yes              |
| Time fixed effects                                                       | Yes                                         | Yes                       | Yes              | Yes              |
| Wealth controls                                                          | No                                          | Yes                       | Yes              | Yes              |
| Population density controls                                              | No                                          | No                        | Yes              | Yes              |
| Distance to market controls                                              | No                                          | No                        | No               | Yes              |

*Notes:* This table tests the robustness of the mobile money agent expansion results (Table 3) to restricting the post-treatment window to the pre-COVID period. Panel A uses data through May 2020 (two months after the onset of COVID in Togo); Panel B uses data through March 2020 (the month COVID restrictions began). Both panels estimate the average effect of mobile money agent arrival using the interaction-weighted estimator of [Sun and Abraham \(2021\)](#) applied to Equation (13) at the bimonthly village level. Column (1) is the main specification with village and time fixed effects. Columns (2)–(4) add controls for village-level wealth, population density, and distance to the nearest market. Standard errors clustered at the village level are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table A9: Subsidy and Mobile Money Agent Effects on Adoption by Customer Types

|                                                                | Monthly Applications for Solar Home Systems |                     |                     |                     |                     |                     |
|----------------------------------------------------------------|---------------------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
|                                                                | Primary Income                              |                     | Age                 |                     | Gender              |                     |
|                                                                | Agriculture<br>(1)                          | Other<br>(2)        | 18-34<br>(3)        | 35+<br>(4)          | Men<br>(5)          | Women<br>(6)        |
| <i>Panel A. Villages with Access to Mobile Money Agents</i>    |                                             |                     |                     |                     |                     |                     |
| Subsidy                                                        | 0.682***<br>(0.184)                         | 0.371***<br>(0.138) | 0.370***<br>(0.123) | 0.680***<br>(0.188) | 0.972***<br>(0.283) | 0.080***<br>(0.024) |
| Pre-policy control mean                                        | 0.391                                       | 0.167               | 0.189               | 0.369               | 0.509               | 0.049               |
| <i>p</i> -value: joint test of pre-trends                      | [0.737]                                     | [0.393]             | [0.793]             | [0.707]             | [0.889]             | [0.670]             |
| Villages                                                       | 516                                         | 516                 | 516                 | 516                 | 516                 | 516                 |
| Observations                                                   | 3,612                                       | 3,612               | 3,612               | 3,612               | 3,612               | 3,612               |
| <i>Panel B. Villages without Access to Mobile Money Agents</i> |                                             |                     |                     |                     |                     |                     |
| Subsidy                                                        | 0.298***<br>(0.088)                         | 0.049*<br>(0.027)   | 0.189***<br>(0.054) | 0.159***<br>(0.041) | 0.327***<br>(0.081) | 0.020**<br>(0.009)  |
| Pre-policy control mean                                        | 0.247                                       | 0.054               | 0.100               | 0.201               | 0.269               | 0.032               |
| <i>p</i> -value: joint test of pre-trends                      | [0.395]                                     | [0.582]             | [0.300]             | [0.476]             | [0.404]             | [0.503]             |
| Villages                                                       | 1,696                                       | 1,696               | 1,696               | 1,696               | 1,696               | 1,696               |
| Observations                                                   | 11,872                                      | 11,872              | 11,872              | 11,872              | 11,872              | 11,872              |
| <i>Panel C. All Mobile Money Agent Expansion Villages</i>      |                                             |                     |                     |                     |                     |                     |
| Mobile Money Agent Arrival                                     | 0.336*<br>(0.201)                           | 0.162<br>(0.102)    | 0.268**<br>(0.135)  | 0.230*<br>(0.128)   | 0.354*<br>(0.203)   | 0.144**<br>(0.068)  |
| Pre-policy control mean                                        | 0.276                                       | 0.105               | 0.129               | 0.252               | 0.324               | 0.057               |
| <i>p</i> -value: joint test of pre-trends                      | [0.257]                                     | [0.783]             | [0.590]             | [0.123]             | [0.113]             | [0.271]             |
| Villages                                                       | 99                                          | 99                  | 99                  | 99                  | 99                  | 99                  |
| Observations                                                   | 1,484                                       | 1,484               | 1,484               | 1,484               | 1,484               | 1,484               |
| <i>p</i> -value: test of equality (A = B)                      | [0.082]                                     | [0.015]             | [0.169]             | [0.012]             | [0.038]             | [0.027]             |

*Notes:* This table reports the effects of subsidy eligibility and mobile money agent arrival on monthly village-level applications, disaggregated by customer type. The outcome in each column is the number of monthly applications from customers in the indicated demographic group: primary income source (agriculture vs. other, columns 1–2), age group (18–34 vs. 35+, columns 3–4), and gender (men vs. women, columns 5–6). Panels A and B estimate subsidy effects using the interaction-weighted estimator of [Sun and Abraham \(2021\)](#) applied to Equation (11), for villages with and without baseline mobile money agent access ( $\leq 3$  km), respectively. Panel C estimates the effect of mobile money agent arrival using Equation (13) at the bimonthly level for the 99 expansion villages. The *p*-value for the test of equality (A = B) tests whether the subsidy effect differs across the two subsamples. All specifications include village and time fixed effects. Standard errors clustered at the village level are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table A10: Subsidy and Mobile Money Agent Effects on Usage

|                                                                | Any Usage<br>(1)  | Majority Usage<br>(2) | Usage Share<br>(3) |
|----------------------------------------------------------------|-------------------|-----------------------|--------------------|
| <i>Panel A. Villages with Access to Mobile Money Agents</i>    |                   |                       |                    |
| Subsidy                                                        | -0.014<br>(0.011) | -0.025<br>(0.016)     | -0.020<br>(0.014)  |
| Pre-policy control mean                                        | 0.990             | 0.976                 | 0.957              |
| <i>p</i> -value: joint test of pre-trends                      | [0.074]           | [0.010]               | [0.029]            |
| Customers                                                      | 2,784             | 2,784                 | 2,784              |
| Observations                                                   | 17,493            | 17,493                | 17,493             |
| <i>Panel B. Villages without Access to Mobile Money Agents</i> |                   |                       |                    |
| Subsidy                                                        | -0.006<br>(0.014) | -0.025<br>(0.018)     | -0.018<br>(0.016)  |
| Pre-policy control mean                                        | 0.987             | 0.967                 | 0.941              |
| <i>p</i> -value: joint test of pre-trends                      | [0.840]           | [0.155]               | [0.064]            |
| Customers                                                      | 4,709             | 4,709                 | 4,709              |
| Observations                                                   | 29,050            | 29,050                | 29,050             |
| <i>Panel C. All Mobile Money Agent Expansion Villages</i>      |                   |                       |                    |
| Mobile Money Agent Arrival                                     | -0.019<br>(0.031) | -0.003<br>(0.046)     | 0.012<br>(0.045)   |
| Pre-policy control mean                                        | 0.982             | 0.927                 | 0.893              |
| <i>p</i> -value: joint test of pre-trends                      | [0.010]           | [0.268]               | [0.406]            |
| Customers                                                      | 590               | 590                   | 590                |
| Observations                                                   | 6,363             | 6,363                 | 6,363              |
| <i>p</i> -value: test of equality (A = B)                      | [0.631]           | [0.999]               | [0.919]            |

*Notes:* This table reports the effects of subsidy eligibility and mobile money agent arrival on customer-level energy usage. Column (1): binary indicator for whether the customer used any electricity that period. Column (2): binary indicator for whether the customer used electricity on the majority of days. Column (3): share of days with electricity usage. Panels A and B estimate subsidy effects using the interaction-weighted estimator of [Sun and Abraham \(2021\)](#) at the customer-month level, for villages with and without baseline mobile money agent access ( $\leq 3$  km), respectively. Panel C estimates the effect of mobile money agent arrival using Equation (13) at the customer-bimonth level for existing customers in the 99 expansion villages. The *p*-value for the test of equality (A = B) tests whether the subsidy effect differs across the two subsamples. All specifications include customer and time fixed effects. Standard errors clustered at the customer level are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table A11: Subsidy and Mobile Money Agent Effects on Payment Coordination

|                                                                | Share of Paying<br>Customers Using<br>Third Party Payments<br>(1) | Share of<br>Payments by<br>Third Parties<br>(2) | Share of New<br>Customers Using<br>Third Party Payments<br>(3) |
|----------------------------------------------------------------|-------------------------------------------------------------------|-------------------------------------------------|----------------------------------------------------------------|
| <i>Panel A. Villages with Access to Mobile Money Agents</i>    |                                                                   |                                                 |                                                                |
| Subsidy                                                        | -0.126***<br>(0.040)                                              | -0.121***<br>(0.037)                            | -0.102<br>(0.113)                                              |
| Pre-policy control mean                                        | 0.388                                                             | 0.357                                           | 0.466                                                          |
| <i>p</i> -value: joint test of pre-trends                      | [0.838]                                                           | [0.879]                                         | [0.063]                                                        |
| Villages                                                       | 370                                                               | 370                                             | 188                                                            |
| Observations                                                   | 2,218                                                             | 2,218                                           | 708                                                            |
| <i>Panel B. Villages without Access to Mobile Money Agents</i> |                                                                   |                                                 |                                                                |
| Subsidy                                                        | -0.090*<br>(0.047)                                                | -0.081*<br>(0.047)                              | -0.153*<br>(0.092)                                             |
| Pre-policy control mean                                        | 0.456                                                             | 0.422                                           | 0.630                                                          |
| <i>p</i> -value: joint test of pre-trends                      | [0.076]                                                           | [0.032]                                         | [0.345]                                                        |
| Villages                                                       | 1,110                                                             | 1,110                                           | 480                                                            |
| Observations                                                   | 6,050                                                             | 6,050                                           | 1,432                                                          |
| <i>Panel C. All Mobile Money Agent Expansion Villages</i>      |                                                                   |                                                 |                                                                |
| Mobile Money Agent Arrival                                     | 0.022<br>(0.068)                                                  | -0.021<br>(0.052)                               | 0.063<br>(0.113)                                               |
| Pre-policy control mean                                        | 0.394                                                             | 0.291                                           | 0.407                                                          |
| <i>p</i> -value: joint test of pre-trends                      | [0.514]                                                           | [0.495]                                         | [0.015]                                                        |
| Villages                                                       | 95                                                                | 95                                              | 79                                                             |
| Observations                                                   | 1,123                                                             | 1,123                                           | 492                                                            |
| <i>p</i> -value: test of equality (A = B)                      | [0.482]                                                           | [0.450]                                         | [0.746]                                                        |

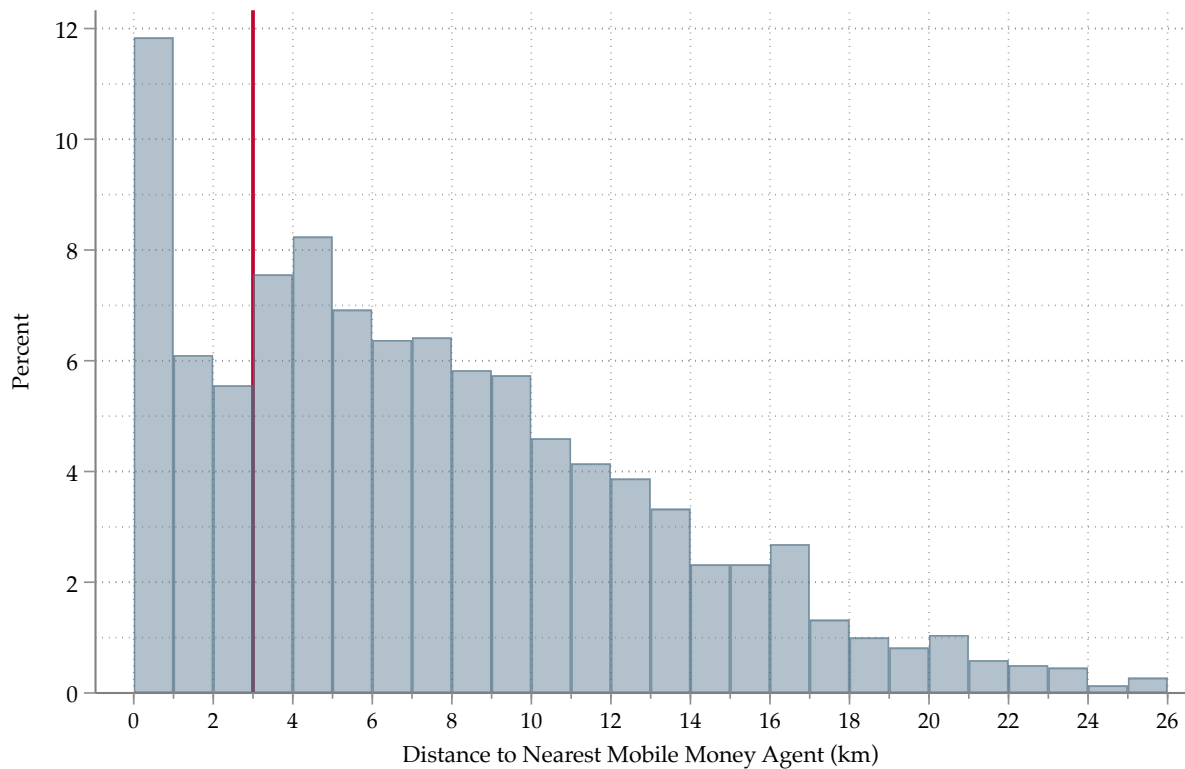
*Notes:* This table reports the effects of subsidy eligibility and mobile money agent arrival on third-party payment coordination at the village level. Column (1): share of paying customers who use a third party's phone to make at least one payment. Column (2): share of total payments made via a third party's phone. Column (3): share of new customers whose first payment is made via a third party's phone. Panels A and B estimate subsidy effects using the interaction-weighted estimator of [Sun and Abraham \(2021\)](#) at the village-month level, for villages with and without baseline mobile money agent access ( $\leq 3$  km), respectively. The sample in each panel is restricted to village-months with at least one paying customer (columns 1–2) or at least one new customer (column 3). Panel C estimates the effect of mobile money agent arrival using Equation (13) at the bimonthly village level for the 99 expansion villages, with analogous sample restrictions. The *p*-value for the test of equality (A = B) tests whether the subsidy effect differs across the two subsamples. All specifications include village and time fixed effects. Standard errors clustered at the village level are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table A12: Mobile Money Agent Effects on Subsidy Disbursement

|                                                           | Share of<br>Paying Customers<br>Getting the Subsidy<br>(1) | Share of<br>Payments Getting<br>the Subsidy<br>(2) | Share of<br>New Customers<br>Getting the Subsidy<br>(3) |
|-----------------------------------------------------------|------------------------------------------------------------|----------------------------------------------------|---------------------------------------------------------|
| <i>Panel A. All Mobile Money Agent Expansion Villages</i> |                                                            |                                                    |                                                         |
| Mobile Money Agent Arrival                                | -0.052<br>(0.076)                                          | 0.017<br>(0.067)                                   | -0.000<br>(0.143)                                       |
| Pre-policy post-subsidy control mean                      | 0.679                                                      | 0.664                                              | 0.526                                                   |
| <i>p</i> -value: joint test of pre-trends                 | [0.104]                                                    | [0.081]                                            | [0.610]                                                 |
| Villages                                                  | 95                                                         | 95                                                 | 79                                                      |
| Observations                                              | 1,123                                                      | 1,123                                              | 492                                                     |

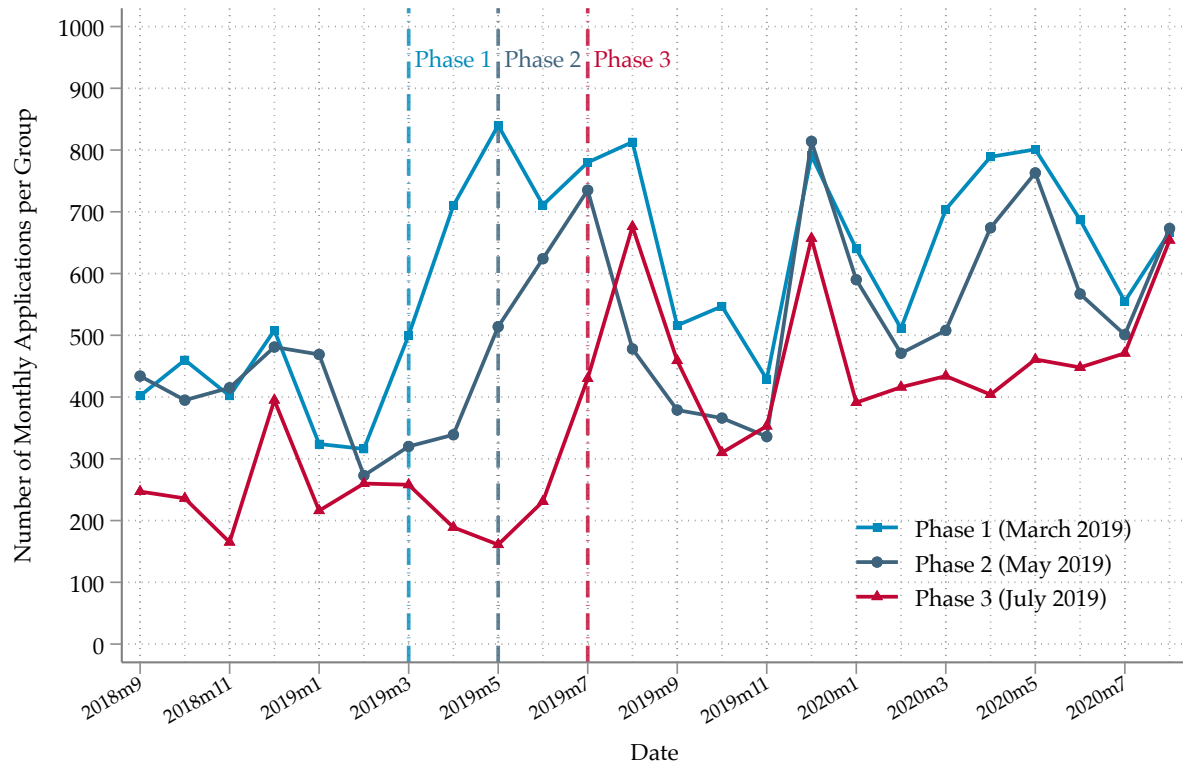
*Notes:* This table tests whether mobile money agent arrival affects subsidy disbursement rates at the village level. The analysis uses only the post-subsidy period (after March 2019) within the mobile money agent expansion sample. Column (1): share of paying customers who received the subsidy. Column (2): share of payments that received the subsidy. Column (3): share of new customers who received the subsidy on their first payment. Panel A estimates the effect of agent arrival using the interaction-weighted estimator of [Sun and Abraham \(2021\)](#) applied to Equation (13) at the bimonthly village level for the 95 expansion villages with post-subsidy observations (79 villages for column 3, which requires at least one new customer). The sample in column (3) is smaller because it conditions on village-bimonths with at least one new customer. All specifications include village and time fixed effects. Standard errors clustered at the village level are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Figure A1: Distance to Nearest Mobile Money Agent Across all Villages



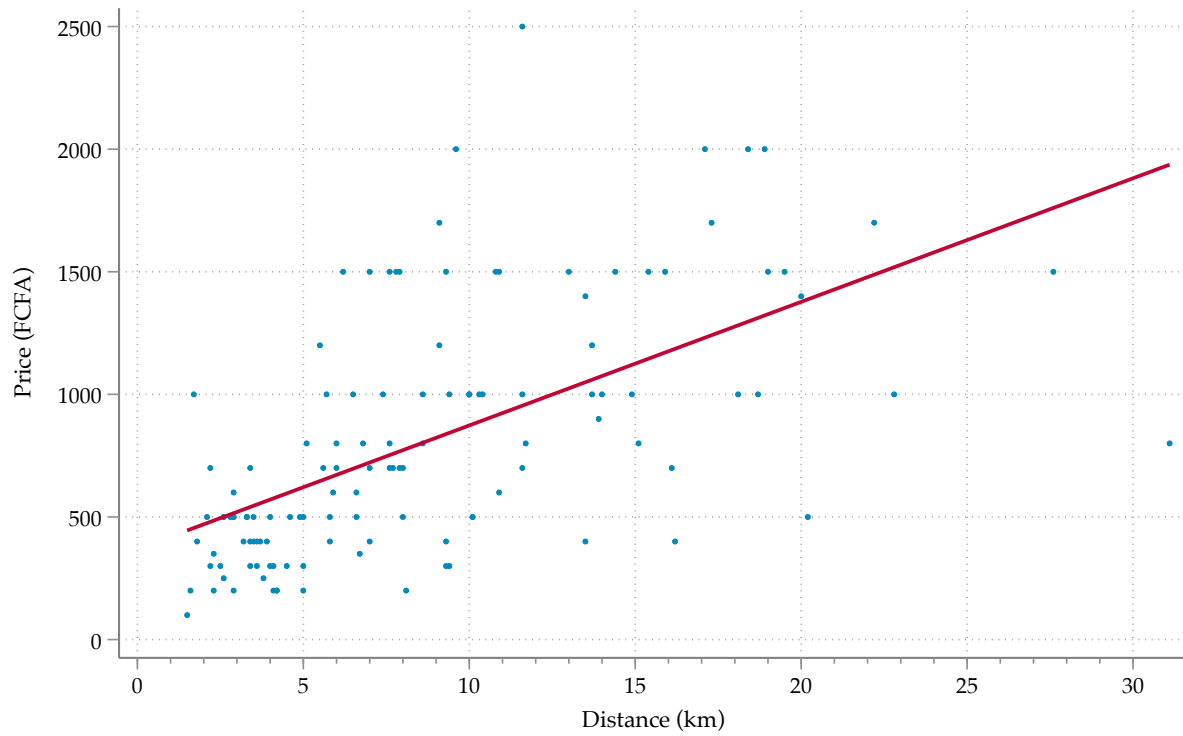
*Notes:* This figure plots the distribution of village distance to the nearest mobile money agent in January 2019, two months before the subsidy was rolled out. This includes the entire universe of villages we observe in our data at any point. To calculate the distance to the nearest mobile money agent, we combine data from the Togolese government, detailing the longitude and latitude of all mobile money agents in the country from 2010 to 2024, on a monthly or yearly basis, with the coordinates of villages in our sample.

Figure A2: Aggregate Adoption of Solar Home Systems



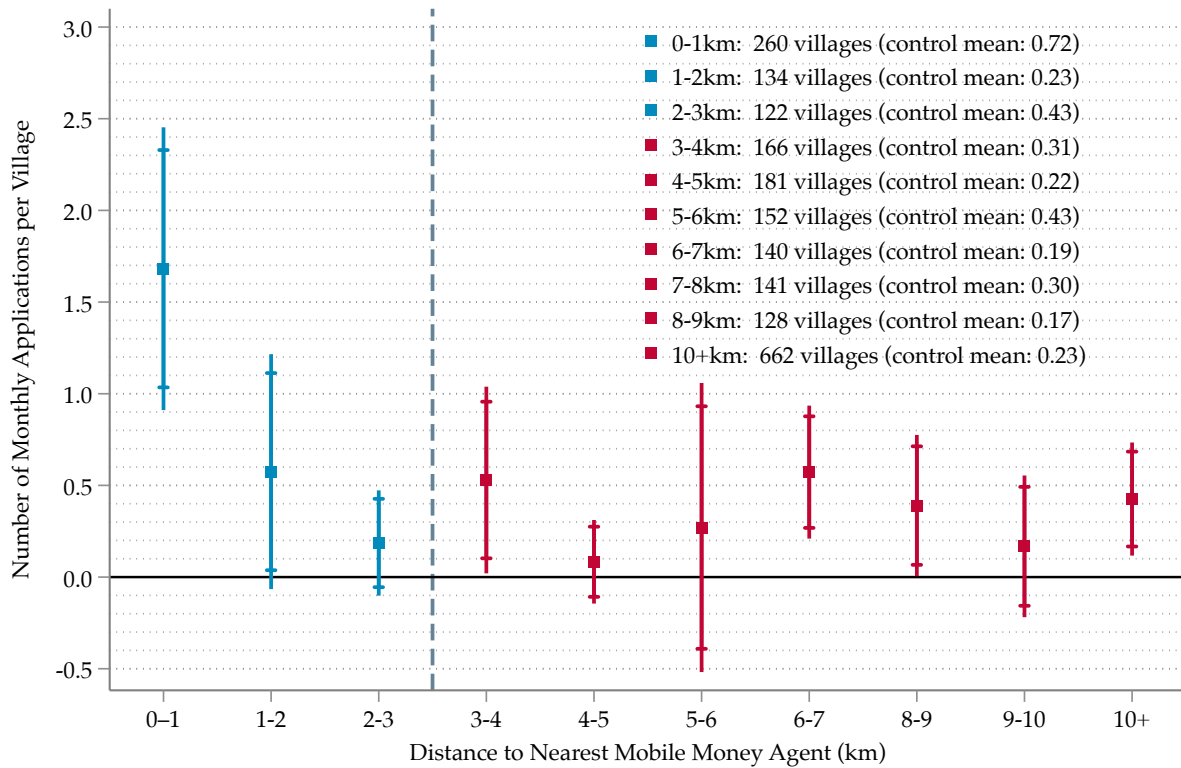
Notes: This figure plots the total number of new SHS applicants by subsidy rollout group. The first set of districts implemented the subsidy in March 2019, a second group in May 2019 and the last group in July 2019, as outlined in Section 4.1.

Figure A3: Travel Distance and Travel Cost



*Notes:* This figure shows the relationship between travel distance and monetary travel cost based on the transport cost data collected in rural Togo (see Section 3.5). Each observation corresponds to one hypothetical trip elicited from respondents during the transport cost survey. Distance is measured in kilometers and travel cost in CFA francs.

Figure A4: Subsidy Effect on Adoption by Distance to Nearest Mobile Money Agent by 1km Increments



Notes: This figure displays village-level heterogeneous ITT effects of subsidy eligibility on solar home system (SHS) adoption, estimated by equation 10 separately for four groups of villages defined by their pre-subsidy distance to the nearest mobile money agent, following Sun and Abraham (2021). Our empirical strategy compares villages in districts that launched the subsidy in March and May 2019 (treatment group) to villages in districts that launched it in July 2019 (control group). Villages are grouped into four distance bands: 0–3 km (with a mobile money agent), 3–6 km, 6–9 km, and 9+ km. The outcome variable is the number of SHS applications per month in each village. The dashed vertical lines are 95% confidence intervals using standard errors clustered at the village level. Table 2 reports the corresponding average effects.

## A2 Information Campaigns

A potential concern is that the estimated effect of the subsidy may partly reflect changes in information rather than changes in price. This concern is especially relevant in rural settings, where awareness of new technologies may be limited. To assess this possibility, we examine the timing of marketing campaigns conducted by the solar provider around the subsidy rollout. Because the available data cover only the provider’s campaigns, this exercise should be interpreted as a partial check on information-based confounding rather than a full test of all information channels.

The campaign data are aggregated at the store level. The provider operated 22 stores, each of which was assigned responsibility for one or more districts, yielding campaign information for 29 districts in total. These data do not allow us to observe which villages within a district were targeted in a given period. Our analysis therefore captures broad district-level marketing activity rather than village-level exposure.

We group campaigns into three categories: (i) *market* campaigns, which involve proactive outreach in major market areas; (ii) *radio* campaigns, including advertisements and radio shows; and (iii) *other* campaigns, which combine festivals, children-focused events, and door-to-door outreach. Figure A5 plots the number and type of campaigns conducted in each bi-month period, while Table A13 reports the share of District  $\times$  Bi-month observations in which each type of campaign occurred. Market visits and radio campaigns are the most common forms of outreach.

The key question is whether provider marketing intensified at the time the subsidy was introduced. Columns (2) and (3) of Table A13 compare the two bi-month periods immediately before and after the subsidy launch in each district. We find no evidence of an increase in market campaigns or in the residual “other” category around rollout. If anything, the share of districts receiving radio campaigns declines modestly in the post-launch period, with the difference significant at the 10% level. Overall, the descriptive evidence suggests that the subsidy was not systematically accompanied by a contemporaneous increase in the provider’s recorded marketing activity.

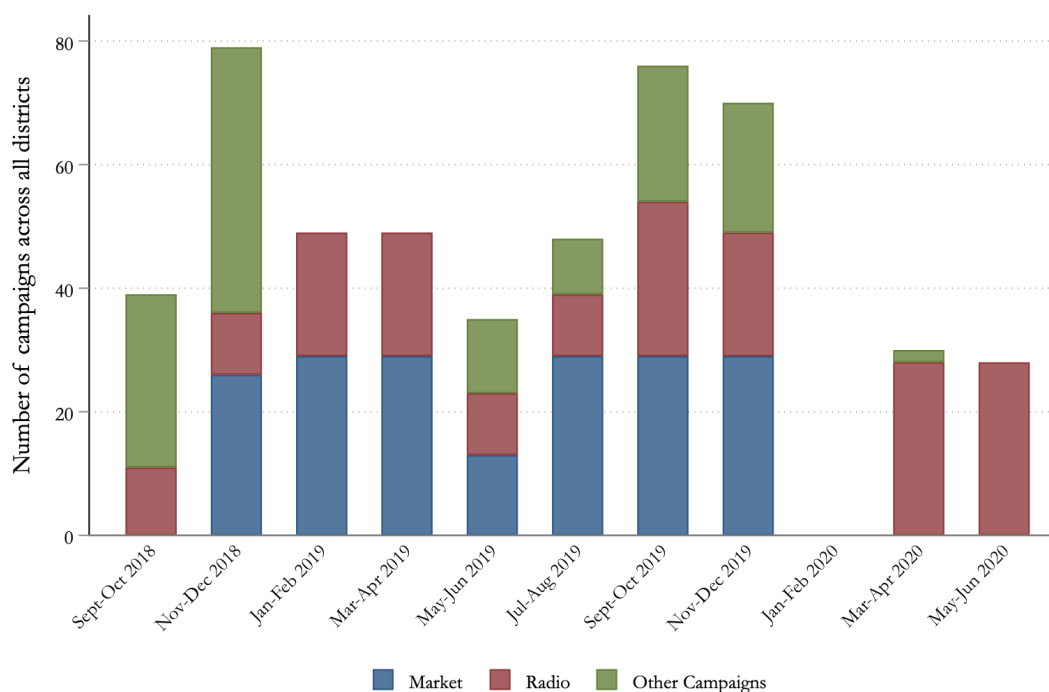
Taken together, these patterns reduce the concern that our subsidy estimates are driven by a coincident increase in observed provider-led campaigns. At the same time, this evidence has important limitations. First, the data are measured at the store/district level and cannot capture within-district targeting. Second, the data cover campaigns conducted by the solar company, not broader information efforts by the government or other actors. We therefore view this section as a descriptive check showing that the provider’s recorded district-level marketing activity does not appear to rise at subsidy launch, rather than as a definitive test that information played no role.

Table A13: Percentage of District x Bi-months targeted by marketing campaigns

|                 | (1)<br>September 2018<br>- May 2020 | (2)<br>2 months<br>pre-subsidy | (3)<br>2 months<br>post-subsidy | (4)<br>T-stat difference |
|-----------------|-------------------------------------|--------------------------------|---------------------------------|--------------------------|
| Market          | 55.76<br>(49.74)                    | 73.33<br>(44.98)               | 83.33<br>(37.90)                | (1.14)                   |
| Radio           | 55.15<br>(49.81)                    | 56.67<br>(50.40)               | 50.00<br>(50.85)                | (-1.86*)                 |
| Other campaigns | 33.86<br>(47.40)                    | 17.24<br>(38.44)               | 20.69<br>(41.23)                | (-0.33)                  |
| N               | 319                                 | 29                             | 29                              |                          |

Notes: The table summarizes the proportion of districts targeted by various marketing campaigns across each bi-month period. The first column shows this proportion over the full sample period (September 2018 - May 2020). The second and third columns show the proportion in the two months pre- and post- subsidy launch (these specific months vary by prefecture). The fourth column shows the t-stat of the difference between the two months pre- and post-subsidy, where the following OLS regression equation is performed  $Y_i = \beta Post - Subsidy + \epsilon_i$ , with standard errors clustered at the district-level (with the sample restricted only to two months pre- and post- subsidy launch). \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Figure A5: Number of District-level Information Campaigns



Notes: This figure plots the number and type of information campaigns conducted by the solar company around the time of the subsidy rollout.

## A3 Extension: Payment Infrastructure and Cash Transfers

The previous sections establish that transaction costs—driven by distance to mobile money agents—limit the effectiveness of the solar electrification subsidy. A natural question is whether this complementarity between payment infrastructure and public policy extends beyond electrification. In this section, we provide suggestive evidence that it does, by examining the Novissi emergency cash transfer programme in Togo.

### A3.1 The Novissi Programme

In April 2020, the Togolese government launched *Novissi* (“solidarity” in Ewe), a nationwide digital cash transfer programme designed to support informal workers affected by the COVID-19 pandemic ([Aiken et al., 2022](#)). The programme targeted the 100 poorest cantons in Togo, identified through a machine learning algorithm that combined satellite imagery and mobile phone metadata with survey data to produce a poverty map. Within these cantons, eligible individuals were identified using phone-based proxy means testing and received monthly transfers of 12,250 CFA (approximately \$21) for women and 10,500 CFA (\$18) for men, disbursed directly to beneficiaries’ mobile money accounts.

Critically, Novissi transfers were delivered exclusively through mobile money. To receive a payment, beneficiaries needed a registered mobile money account, and to convert the digital transfer into cash for daily purchases, they needed to visit a mobile money agent. This design feature creates a direct link between the density of the mobile money agent network and the effective value of the transfer: in cantons with limited agent access, beneficiaries face higher costs—in time, transport, and forgone earnings—to cash out their transfers.

[Aiken et al. \(2025\)](#) evaluate the impacts of Novissi using a randomised controlled trial embedded within the programme, in which approximately 49,000 individuals were randomly assigned to receive or not receive the transfer. Using follow-up phone surveys, the authors find positive and significant effects on food security, mental health, and a combined index of seven well-being outcomes, though they document geographic heterogeneity, with stronger effects in the Savanes region.

### A3.2 Data and Empirical Strategy

We combine two data sources. First, the individual-level experimental data from [Aiken et al. \(2025\)](#), which contains treatment assignment, sampling weights, enumerator and strata identifiers, and seven standardised outcome indices for 9,511 individuals across 260 cantons: food security, financial health, financial inclusion, mental health, perceived social status, health care access, and labour supply, as well as a combined index of all

seven measures.<sup>20</sup> Second, we use government administrative data on the GPS location of every mobile money agent in Togo, matched to the universe of 2,686 villages across 385 cantons. For each village, we compute the distance to the nearest mobile money agent of any operator in 2020, the year of the Novissi programme.

We construct a canton-level measure of mobile money access: the share of villages within each canton that have at least one mobile money agent within 3 kilometres. This distance threshold is consistent with the definition used in our main analysis of the agent expansion policy (Section 1.5), where we define a village as having direct access when an agent operates within 3 km. We match canton names across the two datasets, successfully linking 236 of 260 cantons (covering 9,284 of 9,511 individuals, or 97.6%).

Table A14 reports summary statistics for mobile money access across the cantons in the Novissi sample. There is substantial variation: the average canton has 42.4% of its villages within 3 km of a mobile money agent, but the distribution ranges from 0% to 100%. Roughly one in five cantons (21.6%) has no village within 3 km of an agent, while only 13.6% have full coverage. The median canton-level average distance to the nearest agent is 3.9 km, but the 75th percentile is 6.4 km, indicating that a sizeable fraction of the population in the poorest cantons faces non-trivial travel distances to access mobile money services.<sup>21</sup> These distances are comparable to those documented in Section 1.3 for the broader population of Togolese villages.

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<sup>20</sup>Following Aiken et al. (2025), outcome indices are constructed using the methodology of Bryan, Chowdhury and Mobarak (2021). *Food security* captures the number of meals, frequency of food restrictions, and consumption of specific food items over the past week. *Financial health* measures savings, the duration a household could sustain itself without income, and the amount of money obtainable in an emergency. *Financial inclusion* is the fraction of household adults with a bank account or mobile money account. *Mental health* is the Kessler K6 nonspecific distress scale, measuring the frequency of feeling nervous, hopeless, restless, that everything was an effort, extremely sad, or worthless over the past seven days. *Perceived status* captures self-rated and community-perceived socioeconomic standing on a 1–5 scale. *Health care access* measures whether the household obtained health care when needed, whether it was received at a hospital, and the ability to bring a child to the hospital. *Labour supply* measures hours worked and income received in the past week. Each index is standardised to have zero mean and unit variance in the control group.

<sup>21</sup>Since we observe only each respondent’s canton, not their village, we assign each canton the mean distance to the nearest agent across its constituent villages. At the individual level, the sampling-weighted median distance is 3.6 km.

Table A14: Summary Statistics: Mobile Money Access in Novissi Cantons

|                                                                     | Mean                          | SD    | P25   | Median | P75   | N   |
|---------------------------------------------------------------------|-------------------------------|-------|-------|--------|-------|-----|
| <i>Panel A: Share of villages with agent within threshold</i>       |                               |       |       |        |       |     |
| Within 1 km                                                         | 0.271                         | 0.289 | 0.000 | 0.200  | 0.414 | 236 |
| Within 2 km                                                         | 0.344                         | 0.316 | 0.000 | 0.286  | 0.500 | 236 |
| Within 3 km                                                         | 0.424                         | 0.344 | 0.106 | 0.400  | 0.667 | 236 |
| Within 5 km                                                         | 0.600                         | 0.351 | 0.333 | 0.636  | 1.000 | 236 |
| Within 10 km                                                        | 0.841                         | 0.295 | 0.833 | 1.000  | 1.000 | 236 |
| <i>Panel B: Canton-level average distance to nearest agent (km)</i> |                               |       |       |        |       |     |
| Mean across villages                                                | 5.38                          | 5.48  | 2.36  | 3.93   | 6.41  | 236 |
| <i>Panel C: Canton characteristics</i>                              |                               |       |       |        |       |     |
| Number of villages                                                  | 8.11                          | 5.63  | 4.00  | 7.00   | 11.00 | 236 |
| Share with no agent $\leq 3$ km                                     | 21.6% of cantons (51 of 236)  |       |       |        |       | 236 |
| Share with any agent $\leq 3$ km                                    | 78.4% of cantons (185 of 236) |       |       |        |       | 236 |
| Share with all villages $\leq 3$ km                                 | 13.6% of cantons (32 of 236)  |       |       |        |       | 236 |

*Notes:* Unit of observation is a canton. Mobile money access is measured in 2020, the year of the Novissi programme, using government administrative data on the GPS location of all mobile money agents. Share of villages is computed as the fraction of villages within a canton that have at least one agent (of any operator) within the given distance threshold. Distance to nearest agent is the canton-level average of village-level distances to the nearest agent. The sample consists of the cantons in the Novissi experimental sample that could be matched to the village-level mobile money data.

We estimate the following specification, which interacts the randomly assigned treatment indicator with canton-level mobile money access:

$$Y_i = \beta_1 \text{Treatment}_i + \beta_2 \text{MoMoAccess}_{c(i)} + \beta_3 (\text{Treatment}_i \times \text{MoMoAccess}_{c(i)}) + \alpha_s + \gamma_t + \delta_e + \varepsilon_i, \quad (\text{A1})$$

where  $Y_i$  is a standardised outcome index for individual  $i$ ;  $\text{Treatment}_i$  is the randomly assigned indicator;  $\text{MoMoAccess}_{c(i)}$  is the share of villages with an agent within 3 km in canton  $c$ , standardised to have mean zero and unit standard deviation;  $\alpha_s$  are strata fixed effects;  $\gamma_t$  are survey week fixed effects; and  $\delta_e$  are enumerator fixed effects. Observations are weighted using the sampling and response weights from [Aiken et al. \(2025\)](#). The coefficient of interest is  $\beta_3$ : if payment infrastructure is complementary to cash transfer effectiveness, we expect  $\beta_3 > 0$  for outcomes where the transfer operates through monetary channels.

### A3.3 Results

Table 6 presents the results. The first row reports the main treatment effect ( $\beta_1$ ), which closely reproduces the estimates in [Aiken et al. \(2025\)](#): the transfer significantly improves food security (0.073 SD) and mental health (0.073 SD), with a positive combined effect across all seven indices (0.064 SD).

The second row reports the interaction between treatment and mobile money access ( $\beta_3$ ). The pattern of results is consistent with the hypothesis that payment infrastruc-

ture complements cash transfer effectiveness for outcomes that operate through monetary channels. Financial health, which captures household savings, debt, and the ability to cope with shocks, shows a positive and significant interaction of 0.049 SD ( $p < 0.05$ ), indicating that a one-standard-deviation increase in mobile money access amplifies the treatment effect on financial health by about 0.05 SD. Health care access, which measures households' ability to afford and obtain medical care, shows a similar positive interaction of 0.040 SD ( $p < 0.10$ ). The combined index of all seven outcomes is also positive (0.026 SD), though not statistically significant at conventional levels.

The exception is mental health, where the interaction coefficient is negative ( $-0.028$  SD) but statistically insignificant ( $p = 0.14$ ).

The remaining outcomes—food security, perceived status, and labour supply—show positive but statistically insignificant interaction coefficients, suggesting directionally consistent but weaker complementarity on these dimensions. Financial inclusion shows a near-zero interaction ( $-0.004$  SD), which is unsurprising: this index measures the share of household adults with a bank or mobile money account, and [Aiken et al. \(2025\)](#) themselves find no significant treatment effect on this outcome in their main results (0.007 SD,  $p = 0.74$ ; see Panel A of Table 6). Since Novissi required mobile money registration for all beneficiaries in both treatment and control groups, the programme was unlikely to generate differential financial inclusion gains that could interact with pre-existing agent density.

Overall, these results provide evidence that the complementarity between payment infrastructure and public policy effectiveness extends beyond the electrification context studied in the main analysis. In both settings—solar subsidies and emergency cash transfers—the presence of nearby mobile money agents amplifies the economic benefits of the policy, consistent with the mechanism that transaction costs reduce the effective value of transfers when beneficiaries rely on mobile money to access funds.

### A3.4 Transport Costs Data

To quantify the transaction costs that beneficiaries face when cashing out Novissi transfers, we use transport cost data that we collected from 34 semi-urban and rural villages across all seven regions of Togo (Section 3). For each origin village, enumerators identified the nearest village with a mobile money agent and up to three additional nearby villages, recording the distance (from Google Maps), the moto-taxi fare for a one-way trip, the travel time by moto-taxi, and the walking time. Table A15 reports summary statistics for the 126 origin–destination routes in the survey.

The median one-way moto-taxi fare is 700 CFA for a median distance of 7.5 km, implying a median cost of approximately 100 CFA per kilometre. Travel times are substantial: the median one-way moto-taxi trip takes 25 minutes, while walking takes a median of 85 minutes. These costs—both monetary and in time—are borne each time a beneficiary needs to visit a mobile money agent.

Table A15: Summary Statistics: Transport Costs Between Villages

|                          | Mean | SD  | P25 | Median | P75   | Min | N   |
|--------------------------|------|-----|-----|--------|-------|-----|-----|
| Distance (km)            | 8.8  | 6.0 | 4.0 | 7.5    | 11.6  | 1.5 | 126 |
| One-way moto fare (CFA)  | 870  | 706 | 400 | 700    | 1,000 | 100 | 127 |
| Cost per km (CFA/km)     | 115  | 73  | 68  | 100    | 139   | 25  | 126 |
| Travel time, moto (min.) | 35   | 37  | 12  | 25     | 45    | 5   | 127 |
| Walking time (min.)      | 97   | 80  | 35  | 85     | 120   | 10  | 126 |

*Notes:* Data from a transport cost survey of origin villages across all seven regions of Togo. For each origin, enumerators identified the nearest village with a mobile money agent and up to three additional nearby villages. Distance is computed from Google Maps. Cost per km is the ratio of the one-way moto-taxi fare to the Google Maps distance. Walking time excludes routes where the respondent reported “don’t know.”

### A3.5 Transaction Costs and the Effective Value of Cash Transfers

The complementarity between mobile money access and cash transfer effectiveness documented above is consistent with the transaction cost mechanism analysed in the main body of this paper. To make this link concrete, we compare the magnitude of the Novissi transfers to the transport costs that beneficiaries face when cashing out.

Novissi transfers ranged from 10,500 CFA (\$18) for men to 12,250 CFA (\$21) for women per month. To convert these digital transfers into cash, beneficiaries had to travel to the nearest mobile money agent. Using the transport cost data described above, where a round trip costs approximately 700 CFA plus 100 CFA per kilometre (a 350 CFA fixed fare plus 50 CFA per kilometre, each way), we can estimate the share of each transfer consumed by transport costs at different distances.

At the median canton-level distance to the nearest agent (3.9 km), the estimated round-trip moto-taxi fare is approximately 1,100 CFA, representing about 10% of the average transfer.<sup>22</sup> At the 75th percentile (6.4 km), the round-trip cost rises to approximately 1,340 CFA, or 12% of the transfer. For the 51 cantons (20.7%) where no village has an agent within 3 km—where the median distance is 10.3 km—a single round trip costs approximately 1,730 CFA, consuming 15% of the transfer value. In the most remote cantons (90th percentile, 11.2 km), transport costs account for roughly 16% of the transfer.

Moreover, the cost is not purely monetary: at the median distance, a round trip by moto-taxi takes approximately 50 minutes, while walking would require nearly three hours. For the 20.7% of cantons with no agent within 3 km, travel times are even longer. This means that the *effective* value of the transfer—the amount actually available for consumption after accounting for cash-out costs—is systematically lower in areas with poor mobile money coverage, and beneficiaries face a non-trivial time cost on top of the monetary one.

This mechanism provides a natural explanation for the heterogeneous effects documented in Table 6. In cantons with better mobile money access, beneficiaries can cash out more cheaply and more frequently, retaining a larger share of the transfer for consump-

<sup>22</sup>The average transfer is 11,375 CFA, computed as the midpoint of the men’s (10,500 CFA) and women’s (12,250 CFA) amounts.

tion, savings, and health care expenditures. The positive and significant interactions on financial health and health care access—both outcomes that depend on the ability to convert digital money into purchasing power—are consistent with this channel.

### A3.6 Placebo: Alternative Infrastructure Measures

A natural concern with the results in Table 6 is that mobile money access may proxy for general remoteness or economic development, rather than capturing the specific channel through which payment infrastructure affects transfer effectiveness. Cantons with more mobile money agents also tend to be closer to markets and financial institutions, and to have higher population density, so the interaction we estimate could reflect a broader “proximity to services” or urbanisation effect rather than mobile money access per se.

To address this concern, we re-estimate equation (A1) replacing mobile money access with three alternative canton-level measures: proximity to the nearest financial institution, proximity to the nearest market, and population density.<sup>23</sup> If the heterogeneous effects documented above reflect general remoteness rather than mobile money infrastructure specifically, we would expect similar positive interactions for these alternative measures.

Table A16 reports the Treatment  $\times$  Infrastructure interaction coefficient ( $\beta_3$ ) for each measure and outcome. None of the three placebo measures produce significant interactions on financial health or health care access—the two outcomes where mobile money access shows significant effects. Proximity to financial institutions yields a positive but insignificant interaction on financial health (0.010,  $p = 0.68$ ), less than a fifth of the mobile money coefficient. Population density shows a similarly small and insignificant coefficient (0.021,  $p = 0.33$ ). Proximity to markets shows a near-zero interaction ( $-0.006$ ,  $p = 0.82$ ).

These results suggest that the heterogeneous effects in Table 6 are specific to mobile money infrastructure, consistent with the transaction cost mechanism: what matters for the effective value of a digital cash transfer is proximity to an agent where the transfer can be converted into cash, not general proximity to services or degree of urbanisation.

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<sup>23</sup>Proximity measures are the negative of the canton-level average distance to the nearest facility, standardised to have mean zero and unit standard deviation, so that higher values indicate better access—as for the mobile money variable. Population density is the average population per km<sup>2</sup> within 5 km of each village, aggregated to the canton level and standardised. Correlations with mobile money access range from 0.33 (markets) to 0.49 (financial institutions) to 0.38 (population density).

Table A16: Placebo Test: Heterogeneous Treatment Effects by Alternative Infrastructure Measures

|                                                              | Food<br>Security<br>(1) | Financial<br>Health<br>(2) | Financial<br>Inclusion<br>(3) | Mental<br>Health<br>(4) | Perceived<br>Status<br>(5) | Health Care<br>Access<br>(6) | Labour<br>Supply<br>(7) | All 7<br>Indices<br>(8) |
|--------------------------------------------------------------|-------------------------|----------------------------|-------------------------------|-------------------------|----------------------------|------------------------------|-------------------------|-------------------------|
| <i>Panel A: Mobile money access (baseline, from Table 6)</i> |                         |                            |                               |                         |                            |                              |                         |                         |
| Treatment $\times$ MoMo Access                               | 0.018<br>(0.022)        | 0.049**<br>(0.025)         | −0.004<br>(0.023)             | −0.028<br>(0.019)       | 0.019<br>(0.024)           | 0.040*<br>(0.023)            | 0.004<br>(0.027)        | 0.026<br>(0.023)        |
| <i>Panel B: Placebo measures</i>                             |                         |                            |                               |                         |                            |                              |                         |                         |
| Treatment $\times$ Prox. Financial Inst.                     | −0.001<br>(0.023)       | 0.010<br>(0.025)           | 0.005<br>(0.022)              | −0.018<br>(0.018)       | 0.034<br>(0.023)           | 0.027<br>(0.022)             | 0.004<br>(0.025)        | 0.016<br>(0.023)        |
| Treatment $\times$ Prox. Market                              | −0.026<br>(0.022)       | −0.006<br>(0.026)          | 0.005<br>(0.020)              | −0.027<br>(0.020)       | 0.007<br>(0.022)           | 0.019<br>(0.022)             | −0.008<br>(0.026)       | −0.009<br>(0.023)       |
| Treatment $\times$ Pop. Density                              | 0.015<br>(0.022)        | 0.021<br>(0.022)           | −0.005<br>(0.023)             | −0.017<br>(0.020)       | 0.023<br>(0.023)           | 0.023<br>(0.023)             | 0.005<br>(0.023)        | 0.017<br>(0.023)        |
| Observations                                                 | 9,284                   | 9,284                      | 9,284                         | 9,284                   | 9,284                      | 9,284                        | 9,284                   | 9,284                   |

Notes: Each cell reports the coefficient on Treatment  $\times$  Infrastructure from a separate regression of the form of equation (A1), replacing mobile money access with an alternative canton-level measure. Panel A reproduces the baseline interaction from Table 6. In Panel B, Proximity to Financial Institutions is the negative of the canton-level average distance to the nearest bank or microfinance institution; Proximity to Market is the negative of the average distance to the nearest marketplace; Population Density is the average population per km<sup>2</sup> within 5 km of each village. All measures are standardised to have mean zero and unit standard deviation, so that higher values indicate better access or greater density. All regressions include strata, enumerator, and survey week fixed effects, and are weighted by sampling and response weights. Heteroskedasticity-robust standard errors are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

### A3.7 Testing Non-linearity and the Pattern Across Outcomes

The results in Table 6 are consistent with Prediction P5: treatment effectiveness is increasing in mobile money access, concentrated in spending-dependent outcomes. Under Equation 8, aggregated linearly across villages, the treatment effect is *linear* in the canton access share; curvature in this relationship is therefore not a prediction of the framework, but it is informative about the within-canton distribution of distances and about possible thresholds in cash-out behaviour. We explore it as a descriptive exercise.

**Non-linearity.** We test for non-linearity in two ways. First, we augment equation (A1) with a quadratic interaction term:

$$Y_i = \beta_1 T_i + \beta_2 M_c + \beta_3 (T_i \times M_c) + \beta_4 M_c^2 + \beta_5 (T_i \times M_c^2) + \text{FE} + \varepsilon_i \quad (\text{A2})$$

where  $\beta_5 > 0$  would indicate a convex treatment–access relationship. Table A17 Panel A reports the results. The quadratic interaction terms are positive for most spending-dependent outcomes—food security (0.041,  $p = 0.10$ ), financial health (0.039,  $p = 0.17$ ), and the combined index (0.033,  $p = 0.22$ )—though individually imprecise, consistent with some convexity.

Second, we estimate treatment effects separately by tercile of mobile money access (Table A17, Panel B). For financial health, the treatment effect is −0.022 in the bottom ter-

cile (cantons where fewer than 17% of villages have an agent within 3 km), increases to 0.061 in the middle tercile, and reaches 0.072 ( $p < 0.1$ ) in the top tercile. Health care access follows a similar pattern:  $-0.028$  in the bottom tercile, rising to  $0.053$  in the top. The combined index increases monotonically from  $0.035$  to  $0.094$  ( $p < 0.05$ ). These patterns confirm that transfer effectiveness increases with access. The mild convexity—largest gains where access is best—is not a prediction of the linear framework; it may reflect complementarities in local cash-out infrastructure or merchant acceptance, which we leave to future work.

Table A17: Non-Linearity in the Treatment–Mobile Money Access Relationship

|                                                                     | Food<br>Security<br>(1) | Financial<br>Health<br>(2) | Financial<br>Inclusion<br>(3) | Mental<br>Health<br>(4) | Perceived<br>Status<br>(5) | Health Care<br>Access<br>(6) | Labour<br>Supply<br>(7) | All 7<br>Indices<br>(8) |
|---------------------------------------------------------------------|-------------------------|----------------------------|-------------------------------|-------------------------|----------------------------|------------------------------|-------------------------|-------------------------|
| <i>Panel A: Quadratic interaction (equation A2)</i>                 |                         |                            |                               |                         |                            |                              |                         |                         |
| Treatment $\times$ MoMo Access                                      | 0.000<br>(0.025)        | 0.033<br>(0.029)           | $-0.018$<br>(0.025)           | $-0.031$<br>(0.021)     | 0.018<br>(0.026)           | 0.038<br>(0.027)             | 0.005<br>(0.033)        | 0.012<br>(0.026)        |
| Treatment $\times$ MoMo Access <sup>2</sup>                         | 0.041<br>(0.025)        | 0.039<br>(0.028)           | 0.035<br>(0.027)              | 0.006<br>(0.023)        | 0.001<br>(0.027)           | 0.004<br>(0.027)             | $-0.001$<br>(0.030)     | 0.033<br>(0.027)        |
| <i>Panel B: Treatment effects by tercile of mobile money access</i> |                         |                            |                               |                         |                            |                              |                         |                         |
| Bottom tercile                                                      | 0.053<br>(0.037)        | $-0.022$<br>(0.043)        | 0.010<br>(0.038)              | 0.117***<br>(0.030)     | $-0.012$<br>(0.036)        | $-0.028$<br>(0.040)          | 0.015<br>(0.046)        | 0.035<br>(0.039)        |
| Middle tercile                                                      | 0.051<br>(0.035)        | 0.061<br>(0.040)           | 0.027<br>(0.038)              | 0.038<br>(0.031)        | 0.053<br>(0.035)           | 0.025<br>(0.038)             | 0.009<br>(0.037)        | 0.070*<br>(0.038)       |
| Top tercile                                                         | 0.109***<br>(0.036)     | 0.072*<br>(0.038)          | $-0.029$<br>(0.038)           | 0.065**<br>(0.032)      | 0.064<br>(0.040)           | 0.053<br>(0.038)             | 0.018<br>(0.040)        | 0.094**<br>(0.039)      |
| Observations                                                        | 9,284                   | 9,284                      | 9,284                         | 9,284                   | 9,284                      | 9,284                        | 9,284                   | 9,284                   |

Notes: Panel A reports estimates from equation (A2), which augments the baseline specification with a quadratic interaction between treatment and mobile money access. MoMo Access is standardised to mean zero and unit standard deviation. Panel B reports treatment effects estimated separately within each tercile of canton-level mobile money access (share of villages with agent within 3 km). Bottom tercile: 0.00–16.67% of villages covered (mean: 3.82%). Middle: 18.18–55.56% (mean: 37.17%). Top: 57.14–100.00% (mean: 82.94%). All regressions include strata, enumerator, and survey week fixed effects, and are weighted by sampling and response weights. Heteroskedasticity-robust standard errors are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .