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SCREENING WITH CASH DEPOSITS IN DIGITAL CREDIT MARKETS

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ABSTRACT

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Screening with Cash Deposits in Digital Credit Markets*

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Abstract

We study a loan contract that requires borrowers to make a temporary cash deposit prior to disbursement, which is fully refunded and does not alter repayment incentives. In a randomized controlled trial with a digital lender, applicants are offered otherwise identical loans with or without a deposit. The deposit requirement reduces loan take-up but substantially improves repayment and lender profitability. The results indicate that deposits screen borrowers on both observable and unobservable characteristics. Higher-risk borrowers are less likely to take up deposit loans, and among borrowers with the same observable risk profile, those who accept deposit loans repay at higher rates, with the largest differences among low-risk borrowers. These findings show that simple contract features can complement data-driven credit models by adding an additional screening margin.

1 Introduction

The proliferation of digital credit and fintech services over the past decade has transformed financial intermediation, particularly in emerging markets where traditional collateral is often absent (Vives, 2019; Petralia et al., 2019; Berg et al., 2022; Gopal and Schnabl, 2022). While fintech lenders can reduce the costs of loan origination, they also lower barriers to entry for borrowers, potentially

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exacerbating the adverse selection problem (Stiglitz and Weiss, 1981). Lenders therefore face the problem of separating borrower types in "thin-file" environments where formal credit signals are scarce. Recent evidence suggests that non-traditional data can help bridge this information gap (Berg et al., 2020; Chioda et al., 2024); however, the role of contract design as a screening tool remains underexplored.

In this paper, we study an unconventional contract feature: a pre-loan cash deposit. To identify the effect of this contractual feature, we conduct a field experiment using a randomized controlled trial (RCT) design in partnership with a large fintech lender in Eastern Africa. Loan applicants were randomly assigned to an offer that required an upfront deposit ("Deposit"), which was the lender's usual business practice, or to a loan offer with no deposit requirement ("No Deposit"). Because net disbursement and repayment obligations are identical across arms, differences in repayment outcomes identify selection induced by the deposit requirement rather than differences in incentives created by the repayment contract.

We document three main results. First, the deposit requirement significantly reduces loan take-up. While 50% of the No Deposit group accepts the loan, take-up in the Deposit group falls to 30%, implying that the deposit screens out a significant share of applicants. Second, the resulting selection materially improves loan performance. The intent-to-treat (ITT) effect on repayment is 8 percentage points (pp), while the local average treatment effect (LATE), which adjusts for noncompliance due to processing mistakes, is 12 pp among those who pay the deposit. Default falls by 17 pp in ITT estimates and 26 pp in LATE estimates. These improvements naturally translate into higher profitability. To get a sense of the magnitude, the monthly (annualized) rate of return for the portfolio of deposit-treated loans is 2.0 pp (27 pp) higher than that of the No Deposit group. Accounting for imperfect compliance, the deposit increases the monthly (annualized) rate of return by 3.2 pp (47 pp).

Third, we find that the deposit operates as a "dual" screen, filtering applicants on both observable and latent characteristics. Take-up is roughly independent of the lender's risk score in the No Deposit group but declines with risk score in the Deposit group. Borrowers with above-median risk scores are 11 pp less likely to accept the loan when a deposit is required. This pattern indicates that the deposit deters applicants with higher predicted risk. More interestingly, the deposit also screens on characteristics that are orthogonal to the risk score: conditional on risk score, borrowers in the Deposit group outperform those in the No Deposit group throughout the risk score distribution. In fact, the differences are largest among applicants classified as low risk, suggesting that the deposit screens on dimensions of borrower quality that are not captured by the lender's scoring model.

Recent work emphasizes screening by expanding the set of observable borrower characteristics, often using alternative data and machine-learning models (Berg et al., 2020; Björkegren and Grissen, 2020; Agarwal et al., 2023; Chioda et al., 2024). However, data-driven lending can be fragile (Ben-David et al., 2025) and can increase disparities across borrowers (Fuster et al., 2022; Bartlett et al., 2022). We focus on a complementary approach: simple contract features can elicit information about borrower quality that is not captured by standard credit scores. The deposit requirement is a costly upfront action that can reveal latent attributes, such as access to liquidity or ability to save. More broadly, the results suggest that contract design remains a useful tool for screening even as lending becomes increasingly automated and data-intensive.

This paper contributes to the literature on contract-based screening under asymmetric information (Akerlof, 1970; Stiglitz and Weiss, 1981). Our cash deposit requirement is related to, but distinct from, the traditional down payment used in secured lending (Bester, 1985; Besanko and Thakor, 1987). In both cases, borrowers are required to put cash up front. However, a traditional down payment discourages high-risk borrowers because it is forfeited in the event of default, thereby increasing the borrower's equity stake and strengthening repayment incentives. The cash deposit we study does not operate through this channel. In our setting, borrowers effectively receive the deposited funds back before making any loan payments, so the deposit neither increases borrower exposure nor alters repayment incentives. Instead, it functions as a short-term screening device that operates exclusively at the take-up stage. Our results therefore show that down payment requirements embed an additional screening channel beyond borrower equity or collateralization—one that arises from the act of temporarily parting with cash itself.

Theoretical work has examined a variety of other contractual features as screening devices under asymmetric information, including loan maturity and short-term versus long-term debt (Flannery, 1986; Diamond, 1991), interest rates and pricing-based screening (Stiglitz and Weiss, 1981; de Meza and Webb, 1987), loan size menus and staged lending (Ghosh and Ray, 2001), group lending with joint liability (Besley and Coate, 1995; Ghatak, 1999), and mandated savings periods (Skrastins et al., 2024).

A related empirical literature seeks to identify the screening role of contract terms by examining settings in which borrowers select from menus of loan contracts. Karlan and Zinman (2009) and Hertzberg et al. (2018) highlight a key limitation of this approach: contract terms may directly affect repayment incentives after origination, confounding selection with incentives. To address this concern, they propose research designs in which borrowers are exposed to different advertised contract terms prior to take-up but ultimately receive identical contracts ex post. Using this design, Karlan and Zinman (2009) find little evidence of selection on advertised interest rates, whereas Hertzberg et al. (2018) find evidence of selection into loan maturity and argue that their results provide the first direct evidence of screening through debt contract terms. More recently, Gertler et al. (2024) identified selection effects from advertised nonpayment penalties, although these effects are substantially smaller than the corresponding incentive effects of the penalties themselves. Jack et al. (2023) find that a reduction in the down payment led to a significant increase in take-up with only a modest increase in default rates, which they attribute almost entirely to selection, rather than moral hazard. Our approach to quantifying the effects of screening differs from the prior studies. We do not rely on a difference in advertised contract terms and a "surprise" to one of the treatment groups to indirectly identify the selection effect. Instead, we employ an experimental design that maintains fixed repayment incentives between treatment groups, allowing us to provide direct reduced-form evidence on the screening role of contract terms.

An alternative approach studies screening through structural models of credit demand and repayment. Adams et al. (2009) show that loan size itself can generate selection, with riskier borrowers disproportionately choosing larger loans. Einav et al. (2012) show that higher minimum down payment requirements exclude borrowers with a poorer repayment ability. Gertler et al.

(2025) find that minimum down payments and interest rates screen out both low-income consumers, who cannot afford credit, and high-income consumers, who prefer to pay cash.

2 Experimental Setting

Our partner for the study (the lender) was a technology company operating in East Africa that offers financing for its solar home systems, which provide a modest amount of electricity to households without reliable access to the grid. At the time of our study, the lender had more than half a million customers across six countries in Sub-Saharan Africa. Like many SHS providers, the lender sold most of its units through a pay-as-you-go model.[1] Customers make a down payment to take possession of the SHS and then make small regular payments using mobile money until they have paid off the loan. If a customer misses a payment, the SHS is remotely locked so that the battery will not discharge electricity until the next payment is made.

In addition to offering financing for their SHS, the lender also offered secondary cash loans. Their most popular follow-up product was a school-fee loan. These are cash loans offered to customers in good standing at the beginning of each school term. The lender's normal business practice requires that customers make a deposit of 20% of the loan amount. Several days after making the deposit, the loan funds were disbursed to the customer via mobile money. Customers received a seven-day grace period, after which they were responsible for making daily payments.[2] The lender considered the loan to be paid off as long as the customer made payments totaling the gross loan amount within 145 days of the loan issue date. This arrangement implied that customers who took longer to repay faced a lower effective interest rate.[3]

Customers who do not pay off the loan within 45 days of the target repayment date face

[1] Over 85% of solar home systems sold in the second half of 2018 were sold on PAYGO (see *Global Off-Grid Solar Market Report: Semi-Annual Sales and Impact Data*, 2018. Available at `https://www.gogla.org/publications`).

[2] Most customers chose to pay for several days or a week at a time instead of making daily payments.

[3] For instance, a customer who made a payment every day paid an annual percentage rate (APR) of 168%, whereas a customer who made a payment only two out of every three days paid an APR of only 112%. Of course, the latter APR does not reflect the cost of losing access to the SHS on locked days.

interest charges of 2% per month on any remaining principal. In addition, failure to repay the loan in a timely manner renders customers ineligible for future loan offers. After 180 days of no payments, the loan is considered to be in default and the lender reserves the right to repossess the SHS system. In practice, only a very small fraction of defaults (less than 5%) result in physical repossession. Rather, it is the lock and access to future products and services that serve as the primary mechanisms through which the lender provides repayment incentives.

Our prior work found that access to the school-fee loan helped increase school enrollment largely by solving a timing mismatch between income and expenditures (Gertler et al., 2024). School fees and school-related expenditures constitute a non-trivial portion of household expenses in Uganda and are typically due three times per year.[4] Two of the three due dates are not proximate to harvest season and hence are periods of low income across rural Uganda. In one study, 53% of families reported having their children sent home because they were unable to pay school fees (Intermedia, 2016). The current study was not powered to estimate the impacts of school-fee loan access on education, but the target population and the loan product are the same as in Gertler et al. (2024).

An important feature of this setting is the lender's limited information about borrowers. Although Uganda has credit reference bureaus, coverage is sparse among the rural customers served by the lender. This information environment is representative of many digital lending markets in low-income countries, where thin credit files make traditional screening difficult. The lender therefore relies primarily on a risk score derived from each customer's repayment history on their existing loan. This information can potentially be highly informative: the lender observes the full time series of payments on a comparable loan financing the same asset under similar terms. Nevertheless, contractual design may still play an additional role in screening borrowers beyond what can be captured by risk scores alone.

[4] Conditional on having a student enrolled, the median household spends 14% of income on primary education and 21% of income on secondary education based on data from the 2019 nationally representative Living Standards Measurement Study (LSMS).

3 Experimental Design

Our study focused on a 100,000 Ugandan Shilling (UGX, \$27) school-fee loan.[5] The universe of eligible loan recipients consisted of the lender's customers who were still paying off their initial loan from the lender on their solar home system and did not have an outstanding secondary loan. We further limited the sample to customers whose primary loan was originated at least 180 days before the study began and who had been locked out of their solar home system for fewer than 30% of the days of their loan.

In May 2019, we sent an SMS message to the 39,578 customers who met these criteria inviting them to respond if they were interested in a loan. Compared to the population of rural Uganda, eligible customers are younger, more likely to be male and married, and have more children. They are also more likely to be employed outside the agricultural sector in non-professional occupations.[6] Of the 39,578 eligible customers, 4,594 (12%) responded affirmatively, and we randomly selected 2,488 for the experiment. The interested customers are younger, more likely to be male, and have fewer children than the sample of eligible households. They are also more likely to be employed outside the agricultural sector in professional occupations.

We randomly allocated the interested customers into 2 groups. The customers in the first group were required to make a deposit ("Deposit"), whereas customers in the second group were not ("No Deposit").[7] Customers in the Deposit group received a \$27 loan but were required to post a \$5.50 deposit prior to disbursement; customers in the No Deposit group received a \$21.50 loan without a deposit. Net proceeds (\$21.50) and repayment obligations were identical in the two loan-offer arms.

Our call center attempted to reach the households in each treatment group using the same phone number to which we had sent the SMS messages. The call center reached over 80% of households, explained that the customers were eligible for a loan, and asked if they were interested

[5]All conversions from UGX to USD in this paper use the 2019 average exchange rate of 3,704 UGX per USD (source: `https://data.worldbank.org/indicator/PA.NUS.FCRF?locations=UG`).

[6]Table A.1 in the Supplemental Appendix compares eligible customers and SMS respondents with the population of rural Uganda using the 2019 World Bank LSMS survey.

[7]Tables A.4 and A.5 in the Supplemental Appendix show that the randomization was balanced.

in proceeding. The No Deposit treatment group was informed that it would not be required to post a deposit to obtain the loan, whereas the Deposit treatment group was informed that it would need to make a deposit, following the terms outlined above. Households who received a loan were sent regular SMS payment reminders: on the payment due date, if they were two days late, and again if they were one week late in making a payment. This is standard practice for the lender.

Some customers assigned to the No Deposit group erroneously received a message from the lender's automated system that directed them to the lender's portal to apply for the loan, rather than through the experimental portal. Customers who applied through the lender's portal received the standard contract terms, which included a deposit. We refer to customers assigned to the No Deposit group who applied through the lender's portal as noncompliers. Based on observable characteristics, the difference between compliers and noncompliers is small and statistically insignificant in a multivariate comparison. We discuss noncompliance in more detail in Section 4.3.

4 Results

We first document the effect of the deposit on take-up and loan performance. We then examine the extent to which the deposit screens on observable versus unobservable characteristics. Finally, we discuss the additional insights gleaned from the noncompliers in our experiment.

4.1 Take-up and Loan Performance

Of the 2,230 customers who were offered a loan with a deposit, 667 (30%) accepted. Of the 258 customers who were offered a loan with no deposit, 130 (50%) accepted.[8] The take-up differential is statistically different from zero ($p < 0.001$, see Table 3). Of the 130 customers who accepted a no deposit loan, 84 followed the experimental protocol and received a no deposit loan (the No Deposit group), 46 applied through the lender's portal and therefore paid a deposit (the noncompliers).

[8] At the lender's request, the no deposit treatment group was limited to 10% of the customers in the experiment.

Figure 1, panel (a) plots the share of the principal repaid for customers in the Deposit group (red line), the No Deposit group (purple dotted line), and the noncompliers (blue dashed line). No Deposit borrowers repay less than borrowers in the Deposit group, while repayment among noncompliers lies in between. Panel (b) shows the fraction of customers who had completed loan payments as a function of days since loan origination.[9] Again, the No Deposit borrowers are significantly less likely to have completed the loan than borrowers in the Deposit group, with noncompliers in between. In both panels (a) and (b), Kolmogorov–Smirnov tests reject equality of any pair of distributions with p-values less than 0.02 (see figure notes).

Table 1 reports ITT and LATE estimates. The ITT specification in column (2) is:

$$y_{it} = \alpha_t + \beta_t Deposit_i + \epsilon_{it} \tag{1}$$

where y_{it} is either the fraction of principal repaid by household i at date t (Panel A) or an indicator for whether household i completed loan payments by date t (Panel B). The treatment effect is β_t, α_t is a constant, and ϵ_{it} is an error term. Rows correspond to days since loan origination.

In column (3), we report local average treatment effect (LATE) estimates, which correct for imperfect compliance by instrumenting for deposit payment with a dummy indicating that the customer was randomized into the deposit treatment group. As discussed earlier, imperfect compliance arose from administrative errors that resulted in some No Deposit customers having to pay a deposit. LATE results are consistently larger than ITT.

The LATE results in Table 1, Panel A indicate that the deposit requirement increased repayment by 12 pp at 150 days (from 55% to 67%) and by 13 pp at 200 days (from 63% to 76%). The LATE results in Panel B indicate that the deposit requirement approximately doubled loan completion rates, increasing them by 9 pp at 110 days (from 4% to 13%), 14 pp at 150 days (from 18% to 32%), and 26 pp at 200 days (from 31% to 56%).

To translate improved repayment performance into loan profitability, Table 2 reports monthly

[9] An on-time repayer would have completed the loan after 107 days, which includes 100 days of payments and the seven-day grace period.

internal rates of return on various loan portfolios. The first column, which includes all loans in our experimental treatment groups, shows that the deposit requirement increased the monthly IRR by 2.0 pp (27 pp annualized). When we restrict attention to loans with perfect compliance, the increase in monthly profitability for loans with deposits is larger (3.2 pp, 47 pp annualized). The latter difference is statistically significant at the 1% level. When we sort loans into the upper and lower halves for each treatment group, differences are much starker for the lower half: there is a 7.1 pp differential between loans with deposits and loans to No Deposit compliers, compared to a 1.5 pp differential for loans in the upper half.

4.2 Screening

Next, we provide evidence that the deposit requirement acts as a dual screening device, selecting borrowers along both observable and latent characteristics. Because the deposit is refunded immediately upon loan disbursement, it does not function as collateral and does not directly change incentives to repay after origination. Any differences in repayment therefore reflect selection rather than incentive effects. The patterns we document are consistent with selection on borrower characteristics that are only imperfectly captured by the lender's observables.

The deposit screens, at least partially, on observable characteristics.[10] Borrowers who accept loans with a deposit are less likely to be classified as high risk, have older accounts, and are more likely to exhibit characteristics associated with income stability than those who accept without a deposit. For example, they are more likely to be employed in professional occupations and less likely to work in "non-professional, non-agricultural" jobs (such as market trading or construction). In a multivariate regression, the coefficient on high-risk is no longer significant, suggesting it is correlated with other characteristics.

In Table 3, we estimate the effect of the deposit on take-up and default, including different sets of controls and interactions. We define the loan as in default if the borrower has not completed

[10] See Table A.2 in the Supplemental Appendix for pairwise comparisons and a multivariate regression predicting deposit loan take-up.

payments after 200 days from origination, which is almost twice the amount of time it would take an on-time repayer to complete loan payments. Column (1) reports the unconditional effect of the deposit on take-up: a 20 pp reduction. Comparing columns (1) and (2) reveals that adding borrower controls barely changes the deposit coefficient, indicating that the deposit's effect on take-up operates primarily through unobservable channels rather than observable screening. Column (3) shows that the reduction is especially pronounced among higher-risk applicants: the interaction term indicates that borrowers with above-median risk scores are 11 pp less likely to take up a loan when a deposit is required compared to borrowers with below-median risk scores. Columns (4)–(6) report analogous results for default. The unconditional effect in column (4) is a 26 pp reduction, which attenuates somewhat to 22 pp with controls in column (5), suggesting that observable characteristics account for a modest share of the deposit's effect on default. Perhaps surprisingly, column (6) shows that the reduction in default is concentrated among borrowers with below-median risk scores.

These findings are further corroborated by Figure 2, which plots loan take-up and repayment outcomes as flexible functions of the borrower risk score using a generalized additive model (GAM; Hastie and Tibshirani, 1986). Specifically, for each group $g \in \{\text{Deposit}, \text{No Deposit}\}$, we estimate

$$P(Y_i = 1) = F\big(\alpha_g + s_g(\text{RiskScore}_i)\big),$$

where $F(\cdot)$ is the logistic function and $s_g(\cdot)$ is a smooth function estimated via a penalized cubic regression spline. This approach allows the relationship between outcomes and risk scores to vary flexibly across the support of the score, without imposing linearity or parametric restrictions. Panel (a) shows that take-up in the No Deposit group is largely flat across the risk score distribution, whereas take-up in the Deposit group declines with the borrower's risk score. This pattern indicates that the deposit screens borrowers based on observable risk that is already incorporated into the firm's score.

Panel (b) shows that, conditional on the risk score, Deposit loans outperform No Deposit loans. These within-score differences indicate that the deposit treatment selects better payers even among borrowers with similar predicted risk, pointing to selection on latent traits—such as patience, self-control, or unobserved liquidity—that are not fully captured by the firm's credit scoring model. Moreover, the performance gap is largest among borrowers who are least risky according to the risk score, suggesting that the deposit is particularly effective at screening within the ostensibly safest segment of the applicant pool.

Taken together, the evidence shows that the cash deposit plays an important screening role by shaping the borrower pool at origination. Rather than substituting for a credit risk assessment, the deposit requirement complements data-driven screening by allowing the lender to screen on unobservable characteristics that predict loan performance.

4.3 Noncompliance

The noncompliers in our experiment were initially offered a no-deposit loan but subsequently applied through the lender's portal, where they were required to pay a deposit in order to receive the loan. Repayment outcomes for noncompliers are better than those of borrowers in the No Deposit group, but worse than those in the Deposit group. Relative to borrowers in the Deposit group, noncompliers are more likely to have an above-median risk score and less stable sources of income, although these differences are only marginally significant in a multivariate regression.[11] Noncompliers applied for and received their loans earlier than compliers in the No Deposit group. Yet, the distribution of loan start dates for noncompliers closely resembles that of borrowers in the Deposit group, suggesting that differences in repayment are unlikely to be driven solely by timing or exposure length.[12]

These patterns are informative about the mechanism through which the deposit screens borrowers. If screening operated solely through borrowers' ability or willingness to pay the deposit, repayment among noncompliers—who ultimately paid the deposit—should resemble that of the Deposit group. Conversely, if screening operated only through the initial offer, repayment among noncompliers should resemble that of the No Deposit group. Instead, we find that repayment among noncompliers lies squarely between the two. This evidence is consistent with a two-stage

[11] See Table A.3 in the Supplemental Appendix.
[12] See Figure A.2 in the Supplemental Appendix.

screening process: borrowers are first screened by the deposit requirement in the initial offer and then screened again by whether they are required to pay a deposit after their application is approved. Borrowers in the Deposit group are subject to both screens, borrowers in the No Deposit group are subject to neither, and noncompliers are subject only to the second.

5 Conclusion

This paper studies an unconventional contract feature in digital credit markets: requiring borrowers to make a cash deposit prior to loan disbursement. Using a randomized controlled trial with a digital lender, we show that deposits act as a screening device. Deposits reduce loan take-up, improve repayment, and increase profitability. More importantly, they operate as a dual screen—deterring borrowers with higher observable risk while also revealing latent borrower quality that is orthogonal to the lender's risk scores.

These findings underscore the power of contractual design as a screening tool. Even sophisticated credit models may fail to capture important dimensions of borrower risk that can instead be elicited through simple contractual features. In this sense, screening in digital credit markets remains a mechanism design problem, not merely a prediction problem. Even when deposits are not refunded and incentivize repayment, our results suggest that they can also help screen borrowers by inducing them to self-select.

From a welfare perspective, the reduction in loan take-up induced by deposits does not necessarily imply inefficiency. On the one hand, the evidence suggests that deposits reallocate credit toward borrowers more likely to repay, potentially mitigating concerns about overborrowing and unsustainable lending that have accompanied the rapid expansion of digital credit. On the other hand, deposit requirements may also screen out some creditworthy borrowers who are liquidity constrained or unwilling to temporarily part with cash, generating potential welfare losses. A natural question for future research is to quantify the relative importance of these Type I and Type II screening errors and to characterize the deposit level that optimally trades off improved repayment and lender sustainability against the exclusion of otherwise creditworthy borrowers.

Our results should be interpreted in the context of the the information environment. In our setting, the lender's screening relies largely on repayment histories from prior loans, rather than the richer borrower metadata increasingly used by many fintech lenders. To the extent that such metadata improve prediction, the incremental value of contractual screening mechanisms like deposits may differ in those environments. At the same time, the opposite case is also important: when lenders extend credit to first-time borrowers with no prior relationship, repayment histories are unavailable and contractual screening may become even more central. Understanding how mechanisms like deposits interact with both richer data environments (e.g., the digital footprints studied by Berg et al. (2020)) and more informationally sparse settings remains an important question for future research.

More broadly, our results highlight a role for simple, transparent contract features as complements to data-intensive screening technologies. While the specific deposit level we study is context-dependent, the underlying principle—using upfront costly actions to elicit private information—applies more broadly. Similar mechanisms may operate in other credit markets such as buy-now-pay-later products, asset financing, or small business lending. Understanding how contract design shapes access, efficiency, and consumer protection will remain central as credit markets become increasingly digital and automated.

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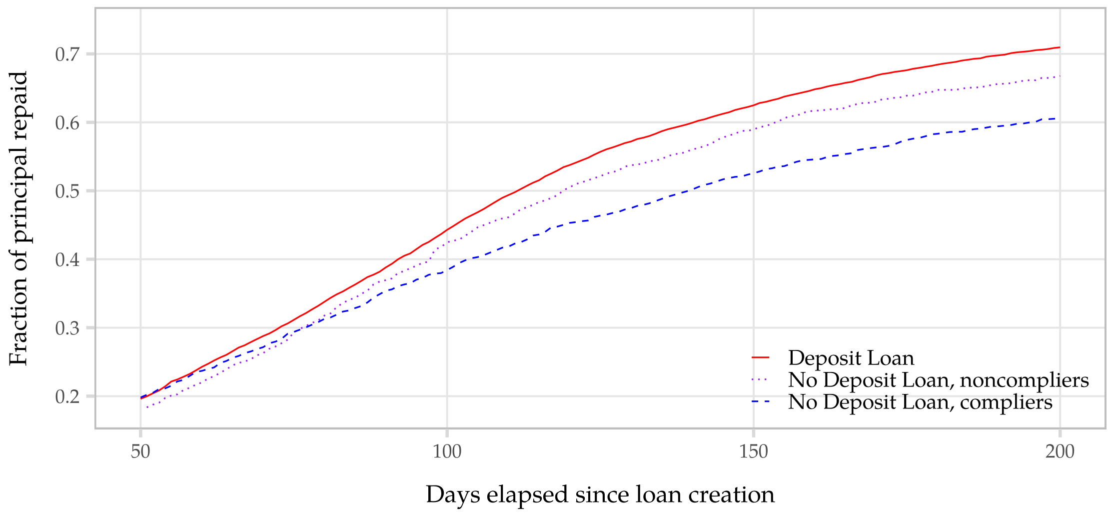
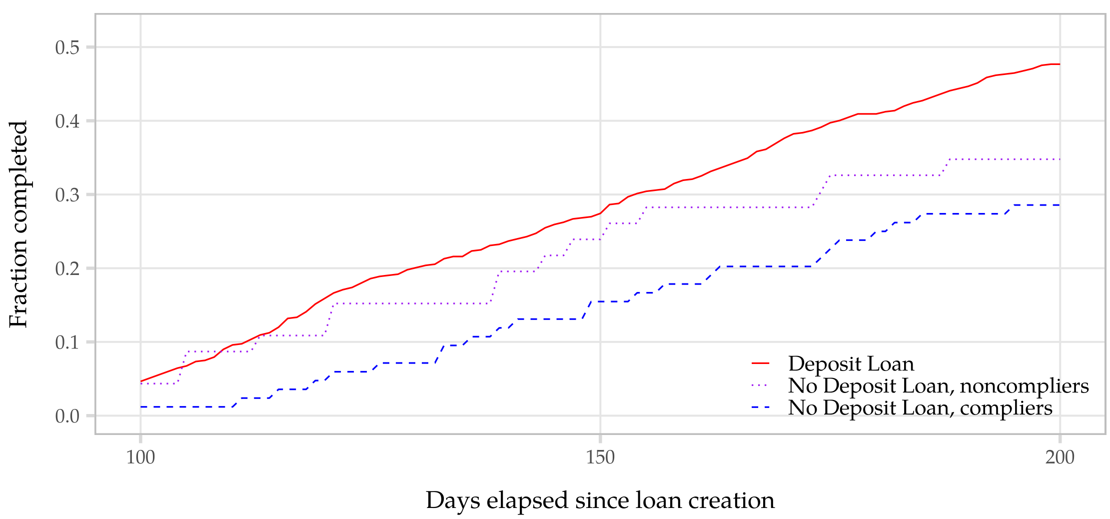
6 Figures and Tables

Figure 1: Loan Repayment and Completion



(a) Loan repayment



(b) Loan completion

Note: Panel (a) plots the average fraction of principal repaid by days since loan creation for Deposit takers ($n = 667$), No Deposit compliers ($n = 84$), and noncompliers ($n = 46$). Panel (b) plots the fraction of loans completed. Kolmogorov–Smirnov tests reject equality of every pairwise distribution in both panels ($p < 0.001$), except Deposit vs. noncompliers in panel (a) ($p = 0.017$).

Table 1: Loan Repayment and Completion on Deposit

Loan day	Mean, No Deposit	ITT	LATE
	(1)	(2)	(3)
Panel A: Loan Repayment			
100	0.40	0.04	0.07
		(0.03)	(0.04)
150	0.55	0.08**	0.12**
		(0.03)	(0.05)
200	0.63	0.08**	0.13**
		(0.03)	(0.05)
Panel B: Loan Completion			
110	0.04	0.06**	0.09**
		(0.03)	(0.04)
150	0.18	0.09**	0.14**
		(0.04)	(0.06)
200	0.31	0.17***	0.26***
		(0.05)	(0.07)
n		797	797

Note: Standard errors in parentheses. Loan repayment is measured by the cumulative proportion of the loan principal repaid (Panel A). Loan completion describes whether the loan principal has been repaid (Panel B). The Intention to Treat (ITT) measures the average effect of treatment assignment on loan repayment (completion), while the Local Average Treatment Effect (LATE) measures the average treatment effect on loan repayment (completion) for compliers, using actual payment of the deposit as the endogenous variable. The analysis is run on samples at either the 100th, 110th, 150th, or 200th day from loan origination. We use 110 days for loan completion instead of 100, as all customers were given several days of free light at the beginning of the loan, such that a customer paying on time would not be fully repaid until about 110 days. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 2: Monthly IRRs of Loan Portfolios

Treatment Group	All	Portfolios sorted by IRR of individual loans		n
		Below Median	Above Median	
Deposit	0.3%	-5.6%	8.9%	667
	(0.5%)	(0.9%)	(0.2%)	
No Deposit, All	-1.7%	-9.4%	7.8%	130
	(1.2%)	(2.5%)	(0.5%)	
No Deposit, Compliers only	-2.9%	-12.7%	7.4%	84
	(1.4%)	(4.1%)	(0.8%)	
p-value from comparison between Deposit and all No Deposit	0.06	0.07	0.01	
p-value from comparison between Deposit and Compliers in No Deposit	0.01	0.03	0.02	

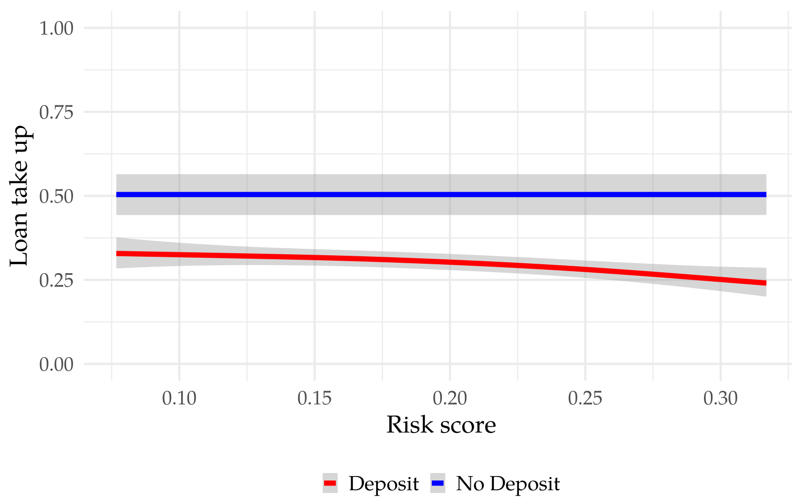
Note: The internal rate of return (IRR) is the discount rate that makes the net present value of cash flows on the loan or the portfolio equal to zero. For a typical 100,000 UGX loan, the initial cash outflow incurred by the lender is 80,000 UGX. The cash inflows are the periodic repayment made by customers, for which we use the whole repayment history without truncation. We then sort loans within each treatment group into upper and lower halves and form portfolios using each half. This table reports the IRRs on these portfolios along with portfolios of all loans in each treatment group. The standard errors are obtained via redrawing the sample with replacement 1,000 times. The p-value is from a one-sided t-test with the null hypothesis that the IRR for loans with Deposit is not higher than the IRR for loans without Deposit.

Table 3: Loan Take Up and Default on Deposit

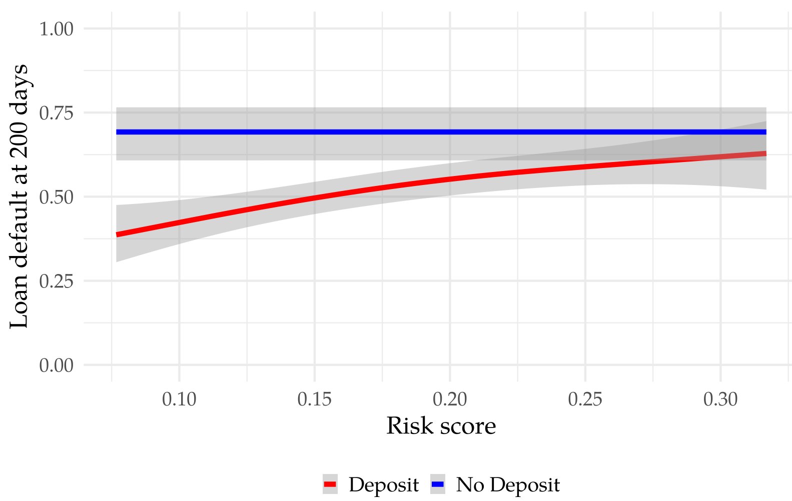
	Take Up			Default		
	(1)	(2)	(3)	(4)	(5)	(6)
Deposit	−0.20***	−0.21***	−0.15***	−0.26***	−0.22***	−0.34***
	(0.03)	(0.03)	(0.04)	(0.07)	(0.07)	(0.11)
Deposit × Median risk score or above			−0.11*			0.21
			(0.06)			(0.15)
Median risk score or above			0.07			−0.20
			(0.06)			(0.14)
Additional Covariates	No	Yes	Yes	No	Yes	Yes
p-value for $\beta_1+\beta_2$			<0.01			0.16
Outcome Mean (No Deposit)	0.50	0.50	0.50	0.69	0.69	0.69
n	2,488	2,487	2,487	797	797	797

Note: Standard errors in parentheses. "Take up" refers to loan take up on deposit Intent-To-Treat (ITT) results. "Default" refers to the Local Average Treatment Effect (LATE) of loan default on deposit, which measures the average treatment effect for compliers, using actual payment of the deposit as the endogenous variable. The analysis for loan default is run on the sample at the 200th day from loan origination. Additional customer covariates include: number of children; female; married; occupation category indicators; region indicators; proportion of days locked at SMS; account age; and equity ratio. Mean and mode imputation is performed where the listed covariate data was missing (less than 0.2% of the data) and indicators for missingness are included. Variable definitions are provided at the beginning of the appendix. * $p < .10$, ** $p < .05$, *** $p < .01$

Figure 2: Loan Take-up and Default by Risk Score



(a) Loan take-up

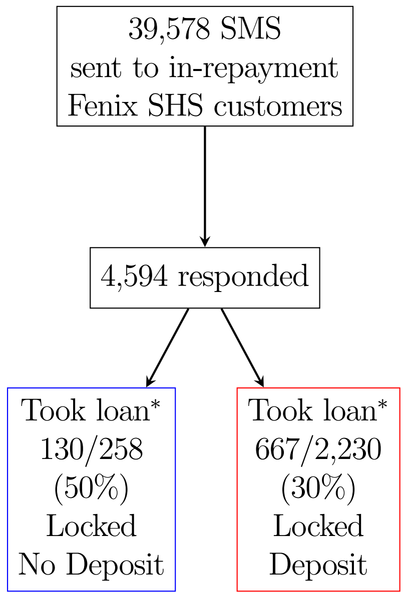


(b) Loan default

Note: This figure shows generalized additive model (GAM) estimates with thin plate regression splines (Wood, 2017) for (a) loan take-up and (b) loan default as functions of the borrower's risk score, with 95% pointwise confidence intervals. The No Deposit group includes noncompliers. Loan default is defined as one minus loan completion. "Risk score" is one minus the sum of the interest paid and the principal paid on the solar home system account overall, over the amount expected to date, recorded on May 12, 2019, prior to the experiment. The risk score variable is winsorized at the 5th and 95th percentile.

A Supplemental Appendix (For Online Publication Only)

Figure A.1: Experimental Design



* Note: "Took loan" refers to accepting the loan and completing the necessary paperwork.

Table A.1: Descriptive Statistics of Enrollee Characteristics from Administrative Data

Characteristic	Uganda LSMS	SMS sent to	SMS respondents
	(1)	(2)	(3)
Median risk score or above	-	-	0.50
			(0.50)
Proportion of days locked at SMS	-	0.19	0.19***
		(0.06)	(0.06)
Account age (years)	-	1.4	1.4*
		(0.9)	(0.9)
Age (years)	45	41***	40***
	(22)	(11)	(10)
Female (proportion)	0.33	0.26***	0.19***
	(0.69)	(0.44)	(0.39)
Married (proportion)	0.72	0.87***	0.88
	(0.67)	(0.33)	(0.33)
Number of children	2.9	4.0***	3.6***
	(3.3)	(2.9)	(2.8)
Agricultural (proportion)	0.58	0.35***	0.24***
	(0.74)	(0.48)	(0.43)
Professional (proportion)	0.12	0.13	0.23***
	(0.54)	(0.33)	(0.42)
Non-Professional & Non-Agricultural (proportion)	0.26	0.46***	0.45
	(0.65)	(0.50)	(0.50)
n	3,174	39,578	2,488

Note: Standard deviations in parentheses. The World Bank Uganda LSMS information in (1) comes from the 2018/2019 wave and uses probability weights. (2) and (3) come from the lender's administrative data. LSMS demographics relate to the household head, while the lender's demographics relate to the customer signing with the lender. LSMS sample occupations follow similarly. (3) is a subset of (2). The results from tests of differences comparing (1) to (2) and (2) to (3) are displayed in (2) and (3), respectively. Variable definitions are provided at the beginning of the appendix. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.2: Characteristics of Loan Takers

Characteristic	Deposit	No Deposit	Regression (DV=Deposit)	n
	(1)	(2)	(3)	
Median risk score or above	0.46	0.55*	−0.028	797
	(0.50)	(0.50)	(0.036)	
Proportion of days locked at SMS	0.18	0.19*	−0.285	797
	(0.06)	(0.06)	(0.329)	
Account age (years)	1.5	1.3*	0.032**	797
	(0.9)	(0.7)	(0.015)	
Age (years)	39	39		610
	(10)	(11)		
Female (proportion)	0.19	0.19	−0.004	797
	(0.39)	(0.40)	(0.034)	
Married (proportion)	0.87	0.92	−0.051	797
	(0.33)	(0.28)	(0.042)	
Number of children	3.5	3.7	−0.002	797
	(2.8)	(2.7)	(0.005)	
Agricultural (proportion)	0.22	0.18	0.063*	797
	(0.41)	(0.38)	(0.035)	
Professional (proportion)	0.25	0.18*	0.061**	797
	(0.44)	(0.39)	(0.030)	
Non-Professional & Non-Agricultural (proportion)	0.43	0.55**		797
	(0.50)	(0.50)		
F-test p-value			0.04	

Note: Standard deviations in parentheses. The results from tests of differences comparing (1) to (2) are displayed in (2). The p-value from the F-test is 0.04. Variable definitions are provided at the beginning of the appendix.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table A.3: Characteristics of Deposit Takers vs. No Deposit Takers (Compliers and Non Compliers)

Characteristic	Deposit	No Deposit	Non-compliers	Regression (DV = Deposit)	Regression (DV = Deposit)	Regression (DV = Compliance)
	(1)	(2)	(3)	(4)	(5)	(6)
Median risk score or above	0.46	0.55*	0.50	0.14	0.10	0.04
	(0.50)	(0.50)	(0.51)	(0.20)	(0.18)	(0.14)
Proportion of days locked at SMS	0.18	0.19*	0.19	-0.63	-0.52	-0.18
	(0.06)	(0.06)	(0.05)	(0.29)	(0.25)	(0.20)
Account age (years)	1.5	1.3*	1.4	0.03	0.02	0.01
	(0.9)	(0.7)	(0.8)	(0.02)	(0.02)	(0.01)
Age (years)	39	39	39			
	(10)	(11)	(12)			
Female (proportion)	0.19	0.19	0.24	0.01	0.02	-0.02
	(0.39)	(0.40)	(0.43)	(0.03)	(0.03)	(0.02)
Married (proportion)	0.87	0.92	0.93	-0.05	-0.02	-0.03
	(0.33)	(0.28)	(0.25)	(0.04)	(0.04)	(0.03)
Number of children	3.5	3.7	3.8	-0.004	-0.003	-0.002
	(2.8)	(2.7)	(3.0)	(0.005)	(0.004)	(0.004)
Agricultural (proportion)	0.22	0.18	0.15	-0.02	-0.01	-0.01
	(0.41)	(0.38)	(0.36)	(0.04)	(0.03)	(0.03)
Professional (proportion)	0.25	0.18*	0.13*			
	(0.44)	(0.39)	(0.34)			
Non-Professional & Non-Agricultural (proportion)	0.43	0.55**	0.61**	-0.07	-0.03	-0.05
	(0.50)	(0.50)	(0.49)	(0.03)	(0.03)	(0.02)
F-test p-value				0.04	0.10	0.52

Note: Standard deviations in parentheses for (1), (2), and (3), but standard errors in parentheses in (4), (5), and (6). Results from tests of differences comparing (1) and (2) are in (2), and results from tests of differences comparing (1) and (3) are in (3). Columns (4), (5), and (6) report multivariate regressions: (4) compares Deposit takers (1) to No Deposit compliers (2), (5) compares Deposit takers (1) to No Deposit noncompliers (3), and (6) compares No Deposit compliers (2) to No Deposit noncompliers (3). Regressions feature the listed variables as well as indicators for customer region. Variable definitions are provided at the beginning of the appendix. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.4: Baseline Characteristics of Responders to SMS

Characteristic	Deposit (1)	No Deposit (2)	Control (3)	n
Median risk score or above	0.50	0.52	0.50	4594
	(0.50)	(0.50)	(0.50)	
Proportion of days locked at SMS	0.19	0.19	0.19	4594
	(0.06)	(0.05)	(0.06)	
Equity ratio at SMS	0.45	0.45	0.45	4594
	(0.20)	(0.19)	(0.20)	
Account age (years)	1.4	1.4	1.5	4594
	(0.9)	(0.8)	(0.9)	
Age (years)	40	38	40	3704
	(10)	(10)	(11)	
Female (proportion)	0.19	0.19	0.19	4592
	(0.39)	(0.39)	(0.39)	
Married (proportion)	0.88	0.89	0.87	4582
	(0.33)	(0.32)	(0.34)	
Number of children	3.6	3.6	3.6	4594
	(2.8)	(2.7)	(2.7)	
Agricultural (proportion)	0.25	0.21	0.24	4592
	(0.43)	(0.41)	(0.42)	
Professional (proportion)	0.23	0.21	0.25	4592
	(0.42)	(0.41)	(0.43)	
Non-Professional & Non-Agricultural (proportion)	0.45	0.50	0.43	4592
	(0.50)	(0.50)	(0.49)	

Note: Standard deviations in parentheses.

Table A.5: p-values from Pairwise Comparisons of Baseline Characteristics of Responders to SMS

Characteristic	Deposit - No Deposit	Deposit - Control	No Deposit - Control
	(1)	(2)	(3)
Median risk score or above	0.48	0.70	0.60
Proportion of days locked at SMS	0.57	0.52	0.79
Equity ratio at SMS	0.90	0.11	0.55
Account age (years)	0.70	0.24	0.36
Age (years)	0.01	0.25	0.01
Female (proportion)	0.98	0.81	0.94
Married (proportion)	0.68	0.43	0.45
Number of children	0.80	0.96	0.81
Agricultural (proportion)	0.17	0.34	0.35
Professional (proportion)	0.54	0.19	0.23
Non-Professional & Non-Agricultural (proportion)	0.09	0.17	0.02

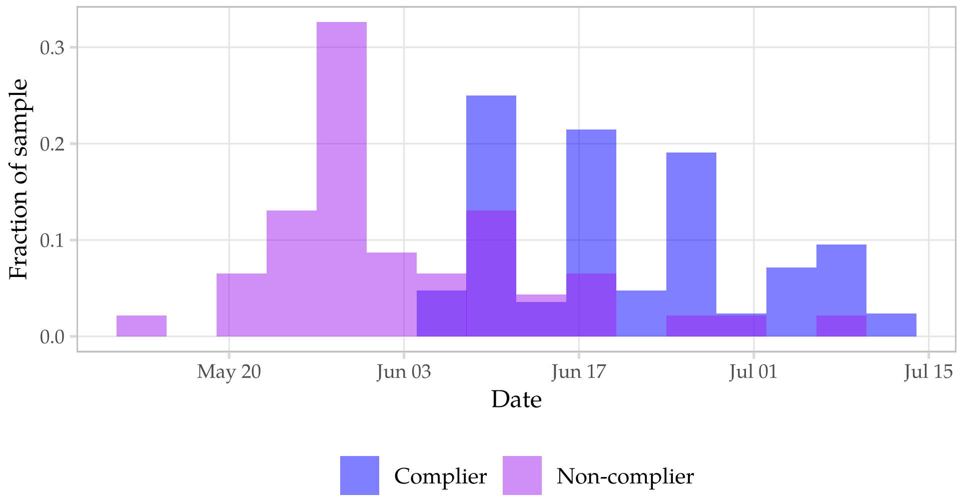
Note: p-values from *t*-tests between treatment group pairs from Table A.3 are included in the above table.

Table A.6: Baseline Characteristics of Deposit Takers vs. Deposit Non-takers

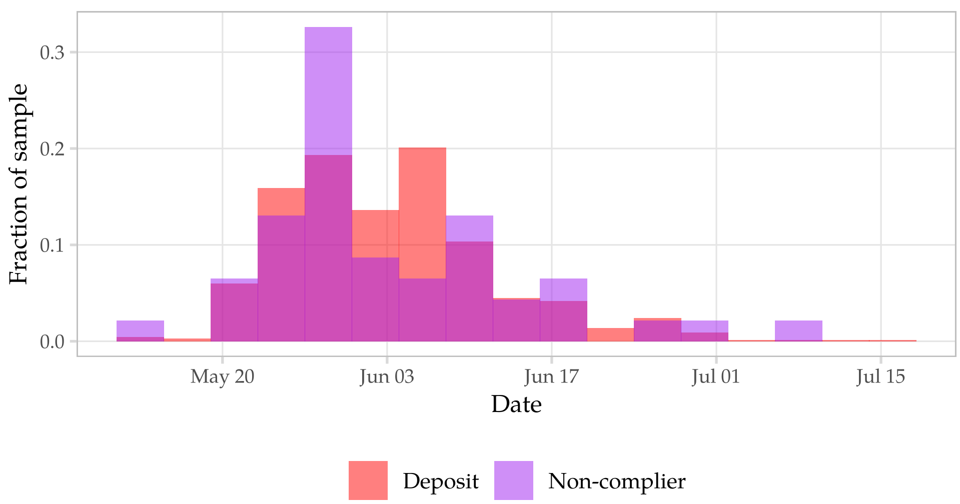
Characteristic	Taker	Non-Taker	n
	(1)	(2)	
Risk score at SMS	0.19	0.20***	2230
	(0.09)	(0.10)	
Proportion of days locked at SMS	0.18	0.19	2230
	(0.06)	(0.06)	
Equity ratio at SMS	0.47	0.43***	2230
	(0.19)	(0.20)	
Account age (years)	1.5	1.4	2230
	(0.9)	(0.9)	
Age (years)	39	40	1826
	(10)	(10)	
Female (proportion)	0.19	0.18	2229
	(0.39)	(0.39)	
Married (proportion)	0.87	0.88	2226
	(0.33)	(0.32)	
Number of children	3.5	3.7	2230
	(2.8)	(2.7)	
Agricultural (proportion)	0.22	0.26**	2229
	(0.41)	(0.44)	
Professional (proportion)	0.25	0.22*	2229
	(0.44)	(0.41)	
Non-Professional & Non-Agricultural (proportion)	0.43	0.45	2229
	(0.50)	(0.50)	
Not classified (proportion)	0.09	0.07**	2229
	(0.29)	(0.25)	
Central (proportion)	0.32	0.45***	2229
	(0.47)	(0.50)	
Eastern (proportion)	0.38	0.32**	2229
	(0.49)	(0.47)	
Western (proportion)	0.24	0.19***	2229
	(0.43)	(0.39)	
Northern (proportion)	0.06	0.04**	2229
	(0.24)	(0.19)	

Note: Standard deviations in parentheses. The results from tests of differences comparing (1) to (2) are displayed in (2). Variable definitions are provided at the beginning of the appendix. * $p < .10$, ** $p < .05$, *** $p < .01$

Figure A.2: Distribution of Loan Start Dates



(a) No Deposit compliers vs. noncompliers



(b) Deposit vs. noncompliers

Note: This figure shows the distribution of loan start dates for borrowers who took a loan, by group. Each bar represents the fraction of borrowers within that group whose loan began in a given 4-day bin. Panel (a) compares No Deposit compliers ($n=84$) to noncompliers ($n=46$). Panel (b) compares Deposit borrowers ($n=667$) to noncompliers ($n=46$). Noncompliers received their loans earlier, and their timing distribution more closely resembles that of the Deposit group.