Rehabilitating Delinquent Digital Borrowers*

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Abstract

We partner with a digital lender in Africa to examine how offering delinquent digital borrowers a strategy to repay their overdue loan (payment plan) and the possibility of regaining access to future credit (renewed eligibility) affects repayment behavior and welfare. The payment plan significantly increases repayment, settlement, and re-borrowing, while eligibility alone has no effect. Although the payment plan has no impact on welfare, the eligibility treatment raises stress and perceived financial insecurity. Our analysis suggests that impatience, time inconsistency, and liquidity constraints could play a role in explaining our results.

Keywords: digital credit; default; payment plans; credit markets

JEL Classifications: D14, D18, G51, O16

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1 Introduction

Digital credit has quickly emerged as a transformative force in the financial landscape of low- and middle-income countries (LMICs). Delivered via mobile platforms, and often linked to mobile-money accounts, digital loans offer a compelling promise: rapid access to credit for consumers outside the reach of traditional financial institutions. As noted in Berg et al. (2022), this expansion has been driven more by convenience – offering near-instant, low-friction credit – and less by advances in screening or monitoring. Indeed, while fast delivery speeds and ease of access can make digital loans very attractive to borrowers, much of the industry is characterized by high interest rates and default rates, while loan amounts are typically small (Brailovskaya et al. 2021, Suri et al. 2021, Carlson 2017, Burlando et al. 2025).[1] These characteristics are consistent with a credit market affected by limited enforcement problems (Gertler et al. 2024). Limited enforcement arises from the inability of digital lenders to induce repayments through standard strategies available to traditional formal and informal lenders such as in person monitoring of effort, threat of expropriation of collateral, or reliance on social collateral.

Absent obvious enforcement mechanisms, loan contracts must be self-reinforcing, meaning that borrowers have an incentive to repay (Ghosh et al. 2000). A common strategy, grounded in both theory and observation, is for lenders to exclude delinquent borrowers from future credit. While this approach may deter strategic default, it also poses an important problem: to the extent that some non-repayment is involuntary and stems from temporary shocks, this practice causes borrowers to lose access to a valuable source of credit and may deprive digital lenders of a future stream of income. What can be done to "rehabilitate" delinquent digital borrowers, that is, to help them regain access to credit?

We address this problem by studying the effects of offering delinquent digital borrowers a strategy to repay their overdue loan (through a payment plan) and the possibility of regaining access to future digital credit (through renewed eligibility). We focus on the effects of these interventions on repayment behavior and short-run welfare. Ex ante, we

[1] Digital loans, delivered directly to mobile money accounts are most prevalent in Sub-Saharan Africa, Latin America, and parts of Asia. Across countries, they share similar characteristics and high default rates due to limited enforcement. Other countries, such as India, also offer fast digital loans, but have lower default rates and an Open-Banking based public digital payment infrastructure (UPI) that allows borrowers to quickly share with lenders their digitally verifiable financial history with low transaction costs (Alok et al. 2024).

expect them to generate repayment and new borrowing largely from involuntary defaulters who are liquidity constrained. Moreover, we expect these interventions to have limited or no impact on strategic defaulters.

We partnered with a digital lender operating in a sub-Saharan African country to conduct a randomized controlled trial involving borrowers whose loans were 90-150 days overdue. We randomized these delinquent borrowers into four treatment arms: a payment plan treatment group in which borrowers were offered a plan breaking down a full repayment into four weekly installments; an eligibility treatment group in which borrowers were notified that they will be eligible for a future loan if the delinquent loan was repaid; a group that was offered both treatments; and a control group that received weekly generic reminders encouraging them to repay their overdue loan. An additional set of delinquent borrowers were randomly selected to be observed but never contacted, allowing us to measure the natural rate of repayment outside of the study and absent communications. We refer to these borrowers as the "reference group" to distinguish them from the control group in the study.

Some elements of these interventions were common across the four treatment arms. All notifications were provided by the lender through text messaging. Each study participant was provided with a new "due-by-date" four weeks in the future, received the same number of messages over the repayment period, and on the same day and time. Thus, the only variation across the treatment arms stems from the content of messages, and not from their reminder effects. Finally, all messages were light-touch, in the sense that all borrowers had the ability to create payment plans by making partial payments, and all borrowers who repaid in full became eligible for future loans.[2]

Random assignment coupled with administrative data from the digital lender enables us to identify the causal impact of these treatments on repayment rates and future credit-seeking behavior. In addition, we also identify the impacts of treatments on borrowers' well-being using data collected through phone surveys.

Our study yields several findings. First, offering delinquent borrowers a payment plan significantly improves repayment outcomes. Compared to the control group, borrowers with access to a payment plan were 9.3 percentage points (p.p.) more likely to make any

[2]In the payment plan and control treatment arms, borrowers were notified that they could access new loans only after they completed all payments. Eligibility was restored even if payments were completed after the new pay-by-date.

repayment (57% more than the control group), 2 p.p. (41%) more likely to settle their loan within 30 days, and 3.3 p.p. (48%) more likely to do so within 60 days. 79% of the "rehabilitated" borrowers reborrowed from the digital lender. Second, the eligibility treatment informing borrowers that repayment would restore credit access proved ineffective on average. However, we find positive effects on borrowers whose repayment amounts were not large, and on borrowers who were liquidity constrained before the study. Third, contrary to our expectations, we find that combining the payment plan and eligibility treatments has similar, albeit smaller-in-magnitude effects to the payment plan treatment. Fourth, using the reference group's repayment behavior, we show that reminders have a large positive effect on repayment. For example, about one third of the overall effect of the payment plan treatment on settlement during the study is explained by the payment plan feature alone, and two thirds by reminders. Finally, we find some negative welfare effects of eligibility notices.

We investigate the potential mechanisms through which the treatments affect the repayment behavior of delinquent digital borrowers. We consider five potential channels: liquidity constraints, time inconsistency, time preferences, risk aversion, and cognitive reasoning. The payment plan treatment could help liquidity-constrained borrowers who cannot repay the full overdue amount at once but can manage smaller installments. We find some suggestive evidence that borrowers who are liquidity constrained at baseline are more likely to respond positively to this treatment. Considering time inconsistency, a payment plan can serve as a "soft commitment" device for sophisticated time-inconsistent borrowers, allowing them to overcome procrastination and increase repayment. Our results do not support this mechanism. The eligibility treatment, on the other hand, can be less effective for sophisticated time-inconsistent borrowers that might have previously decided not to repay their loan as a commitment strategy to avoid future digital loans. We find some support for this channel. We also show that inpatient borrowers are more affected by the payment plan treatment, but not by the eligibility treatment. We hypothesize that this is because the delayed repayment structure appeals to those who struggle with immediate self-control. Considering risk aversion, we find, contrary to expectations, that risk-averse individuals are less responsive to the payment plan treatment, potentially because they prefer to retain liquidity for unexpected shocks. Finally, we explore the role of cognition. People with lower cognition levels may choose dominated policies because they are less complicated (Puri 2025). If that is the case, we should expect a lower impact of the more complex combined treatment. We find no evidence of this channel. Overall, our study suggests that impatience, time inconsistency, and liquidity constraints play a larger role than risk aversion or cognitive reasoning in shaping repayment behavior of delinquent borrowers.

To our knowledge, this study is the first to address the high default rates in digital credit by testing the effects of light-touch interventions such as payment plans and reminders on delinquent borrowers. Previous studies in household finance have looked at the role of reminders in improving timely repayment of loan installments (Barboni et al. 2022; Cadena and Schoar 2011; Karlan et al. 2015; Kuan et al. 2025) or credit card payments (Medina 2021) for non-delinquent borrowers.[3]

Our work also advances the nascent literature on the nuanced welfare implications of light-touch or "nudge" interventions, and our paper raises some concerns about their potential unintended consequences (Allcott et al. 2025). In fact, we find that while our treatments lead to statistically significant increases in repayment, these financial gains may have come at a cost to borrowers' welfare. Three months after the intervention ended, we find that the eligibility treatment heightened stress and perceived financial insecurity among borrowers who remained in default. We find null effects of the payment plan treatment on welfare.

In addition, our study engages with several other strands of economic literature, including the microfinance literature on repayment frequency. Fischer and Ghatak (2016) use a theoretical framework to highlight the ambiguous effect of repayment frequency for microfinance loans. Field and Pande (2008) find that installments' frequency is not a significant determinant of repayment for microcredit loans. In the context of digital loans, our results suggest that payment frequency can be profitably leveraged.

Our research is also linked to the experimental (e.g., Brown and Lahey 2015; Kettle et al. 2016) and observational (e.g., Gal and McShane 2012) work suggesting that achieving smaller subgoals ("small victories") increases motivation to complete future subgoals, and increases the likelihood of completing the overall task. The fact that the payment plan treatment continues to generate repayments after our study period ends is consistent with

[3]Additional studies have looked at the role of text message reminders to increase savings deposits (Karlan et al. 2016; Kast et al. 2018), other types of reminders to avoid bank overdraft fees Stango and Zinman (2014) and increase refinancing Byrne et al. (2023).

the explanation that establishing smaller sub-goals allows borrowers to build repayment momentum.

Our eligibility treatment is also related to existing work on dynamic incentives as a way to reduce moral hazard in loan repayments (Karlan and Zinman 2009). In contrast, we do not find that dynamic incentives, via the eligibility treatment, improve repayment. We attribute this to the fact that our study sample is composed entirely of delinquent borrowers.

Finally, our study contributes to the recent digital credit literature (e.g., Björkegren and Grissen 2018; Bharadwaj et al. 2019; Berg et al. 2020; Bjorkegren et al. 2021; Brailovskaya et al. 2021; Di Maggio and Yao 2021; Hau et al. 2024), including the limited number of studies focused on repayments (Gertler et al. 2024; Burlando et al. 2024, 2025).

2 Setting

A large share of digital loans in sub-Saharan Africa are provided through mobile money operators (MMOs), which are also telecommunications providers (i.e., they are cellular phone carriers). MMO clients use mobile money, which is stored in a digital wallet on the SIM card of the client's phone and do not require a smarphone to opearate. Clients use the mobile money network and their digital wallet to deposit, withdraw, and transfer funds to other digital wallets. Transfers between mobile money and physical cash are carried out by a deep network of community-based mobile money agents. Among the various services that clients can access through their mobile money wallets are quick digital loans, which may be provided by separate commercial banks or private lending companies. Requesting a loan takes seconds and can be done anywhere the client has network coverage. The screening and approval processes are entirely automatized and are instantaneous. Clients who are approved immediately receive the loan directly into their digital wallet, and the funds are then available for withdrawal (through a mobile money agent) or transfer (through the mobile money network). Repayment of the loan happens in the same way: a client deposits funds into his or her digital wallet, and then initiates the repayment within the wallet.

In the country of study, our digital lender provides loans through a digital credit product operated through a large telecom company.[4] Eligibility for loans is determined by the internal credit "scorecards" created by the digital lender for each potential client. The loan sizes vary substantially: \$0.56 to \$252 at the current exchange rate, roughly \$2 to \$1006 at the 2018 PPP rate. The vast majority of loans are for 30 day terms, although some are for 14 days. The cost structure consists of a service fee that ranges from 10-19% (313-806% APR) and a one-time late fee of 10%. The collection strategy is light-touch: borrowers get SMS reminders about payment in advance of the due date, and monthly SMS follow-ups afterwards and for a duration of 90 days. They can make full or partial repayment at any time before the due date, although full repayment near the end of the term is most common. Successful repayments open the door for larger loans and lower service fees going forward.

2.1 Conceptual framework

Why might long-delinquent borrowers respond to payment plans or eligibility notices? We consider a simple conceptual framework that allows digital borrowers to differ along three dimensions: (1) the value they place on access to digital credit, i.e., the expected present-discounted value of future loans (which includes the insurance value of access to loans); (2) the disutility they experience from holding unpaid debt apart from its impact on future credit access; and (3) their available liquidity, which fluctuates over time.

Two types of borrowers will default at the end of their initial loan term. Borrowers with sufficient liquidity at that time to repay their loan, but with low values of continued access to digital credit relative to the disutility of debt, will *strategically* default. Additionally, borrowers with high values of continued access to digital credit relative to the disutility of debt, but facing liquidity constraints at the due date will *involuntarily* default.[5] In this framework, we would expect some repayment occurring after default, arising from involuntary defaulters whose disutility from holding debt is higher than the value of the outstanding loan and who have gained sufficient liquidity to pay off the loan.

We expect our treatments to affect these two types of defaulters differently. Consider

[4] There is a rich market of digital lenders in the market. In our sample, 17.5% of borrowers in our sample reported trying to borrow from another source at the same time they borrowed from our lender, and 79% of them did so by looking for another digital lender.

[5] A final group of delinquent borrowers have low liquidity and low values of continued access to digital credit, and can be classified as being strategic defaulters as they would not have made a payment even if able to do so.

first the impact of offering conditional eligibility. Eligibility causes borrowers to face the same decision problem they faced during the initial loan term. Strategic defaulters have already revealed their preference in this case, and they will not be induced to repay regardless of their available liquidity. Among the set of involuntary defaulters, some will return to the initial repayment decision problem with higher liquidity than they had the first time, and repay in response to the eligibility offer. If the value of credit access is large relative to the utility costs of holding debt, offering second chances matters in a world of fluctuating liquidity.[6]

Now consider the impact of offering a payment plan. The plan lowers the liquidity requirement to *begin* the repayment process, and may therefore induce some of those who have enough liquidity to pay an installment to begin to repay their loan. Additionally, payment plans can reduce the overall perceived costs of repayment through a number of channels. For example, this could be via discounting (under the assumption that the benefits of repayment are not tightly yoked to when a loan is repaid), sophisticated time-inconsistency (via reduced default temptation) or risk-aversion (if borrowers face potential expenditure or income shocks). And even if borrowers are already aware of the installment strategy for repayment, perhaps having the lender automatically calculate and implement the plan lowers the cognitive cost of repaying.[7] These lower perceived costs of repaying through an installment plan could induce both involuntary and strategic defaulters to begin repayment. Our priors about whether borrowers that begin a payment plan will successfully complete them are less clear. For example, if time-inconsistent borrowers are only *partially* sophisticated, then the soft commitment offered by the installment plan may not be sufficient to induce higher rates of settlement. On the other hand, if small victories build repayment momentum towards the goal of settlement, then it could be that being induced to start a plan often leads to full repayment.

Finally, consider offering both a payment plan and eligibility. This combined treat-

[6]This prediction is predicated on long-term delinquent borrowers assuming that they have missed their chance to re-gain credit access from the lender upon repayment. During our baseline survey we asked borrowers, "If a borrower repays a digital loan more than 90 days late, can she borrow again from the same digital lender?" 40.6% of the sample responded "yes," suggesting substantial scope for this offer to matter. We deliberately asked about this in general, rather than in reference to the specific delinquent loan that made the borrower eligible for our sample.

[7]While this is unlikely an exhaustive list of pathways through which payment plans could reduce the perceived costs of repayment, these were the potential causal pathways we pre-registered for investigation in this study.

ment reduces liquidity requirements and raises the value of repayment. For the reasons described above, we expect it to increase repayment, implying complementarity between the two components.

3 Experimental Design

3.1 Study population

Our study population consists of mobile money users who had borrowed money from our digital lender and met the following criteria: (1) they were delinquent with a loan 90-150 days overdue loan; (2) their overdue load had a balance of least \$2.24.

3.2 Treatment arms

Study participants were randomly assigned to one of four treatment arms: payment plan (T1), eligibility notice (T2), payment plan and eligibility notice (T3), and control (C). In all treatment arms, participants receive a total of 10 text messages over a four-week period. The treatment assignment determines the content of the messages that the participant receives from the lender.[8]

T1: Payment plan Participants assigned to T1 were enrolled in a payment plan, in which repayment of the overdue loan amount occurred over four equally sized weekly installments. Clients first receives an introductory SMS alerting them of the payment plan, and providing them with a new 'due-by-date' four weeks in the future. Twice weekly, clients receives one message detailing the next installment amount due and the next installment due date, and one reminder about the upcoming deadline. When installments were under- or over-paid, the remaining installment amounts were recalculated by dividing equally the residual balance over the remaining weeks.

T2: Eligibility Participants assigned to T2 were informed via text messages that their loan eligibility will be restored if the loan is paid in full.

[8] All messages sent to study participants by the digital lender are in English. English is a language of instruction in the country of study, it is widely spoken, and most people prefer to receive SMS messages in English rather than local languages. 86% of the respondents reported that they were comfortable speaking and reading English.

Clients first received an introductory SMS alerting them of the amount due, of the new 'due-by-date' four weeks in the future, and the eligibility offer. Borrowers then received similar messages twice a week for the remaining weeks.

T3: Payment plan and eligibility Participants assigned to T3 were enrolled in the payment plan with four installments (as in T1), and learned about becoming eligible for future loans conditional on repaying their overdue loan (as in T2). The first message contained the same information as the introductory messages in T1 and T2. Then borrowers received twice weekly messages for the remaining weeks reminding them of the installment amounts, due dates, and eligibility.

C: Control Participants assigned to C received weekly generic reminders encouraging them to repay their overdue loan. The first message alerted the borrower of the existence of the overdue loan amount and provided the new 'due-by-date' four weeks in the future. Twice weekly generic messages reminded the borrower of the final due date and residual loan amount.

Common features Importantly, the only difference between the four treatment arms was the content of the messages–everything else was held constant. To guarantee this, the following features of the intervention were common to all participants. First, everyone received 10 messages over a four-week period, and everyone enrolled on the same day received those messages on the same day and at the same time. This was done to ensure that the treatment effects we measure are not the result of reminders only (Barboni et al. 2022). Since the intervention schedule was the same for those enrolled on the same day, the loan due date was also the same. Thus, repayment differences are not driven by differences in due dates. Second, all participants were able to make partial or full payments at any time if they so chose. Partial payments were as easy to make as a full payment and they did not cost more (i.e., there were no per-payment fees).[9] Third, all participants in the study regained eligibility (i.e., they regained access to a loan from this digital lender) if they fully repaid. Regained eligibility worked immediately upon

[9]This is a feature of the digital lender's payment system, not of our research. While partial payments are not discouraged, the digital lender reports that the vast majority of borrowers outside the study make a single payment to repay.

repayment, but loan amounts were reset to a low value. This was likely costly to those borrowers with a long credit history with the lender.

Reference group An additional set of delinquent borrowers with the same characteristics of the study sample were randomly selected by the lender and are used as a reference group. The repayment rates of this group are used as a no-contact benchmark, as these borrowers were not contacted by the research team, did not consent to participate in the study, and did not receive any of the interventions in the study.

3.3 Sampling and study implementation

Our sampling protocol is described in Figure 1. The total sample consists of 8,000 randomly selected 90-150 days delinquent borrowers of our digital lender who fit the study eligibility criteria. Of this group, 1,000 were randomly selected (through a random number generator) and set aside by the lender to be part of the reference group, while 7,000 were identified as potential study participants.

Enrollment of study participants took place in five non-continuous weeks, over a seven-week period (April 8 to May 13, 2024). On Mondays, the digital lender would select a fresh sample of 1,400 potential participants for interview and share it with the data collection firm in charge of recruiting the study sample (i.e., Innovations for Poverty Action, IPA). The list of 1,400 respondents is a cohort, which we label as a "batch". Enumerators called potential participants on the phone to enroll them in the study and conducted a baseline interview.[10] Up to six call attempts were made before the potential participant was dropped from the study due to non-response. To mitigate non-response, enumerators allowed participants to request callbacks at other times, and gave participants a compensation (approximately $1.10) for the time taken to complete the survey.[11] Compensation payments were deposited into the mobile money wallets of all study participants.

Randomization into treatment arms was done at the batch level. In our randomization procedure, we produced 100 random treatment assignments and selected the assignment with the largest minimum p-value when testing for covariate balance across treatment

[10]Our consent materials mentioned that participants in the study were going to receive up to 10 messages as part of the study. Interviews were conducted in English and in other official languages used in the country.

[11]GDP per capita per day in the country of study is below $4.00 (Source: FRED, 2021).

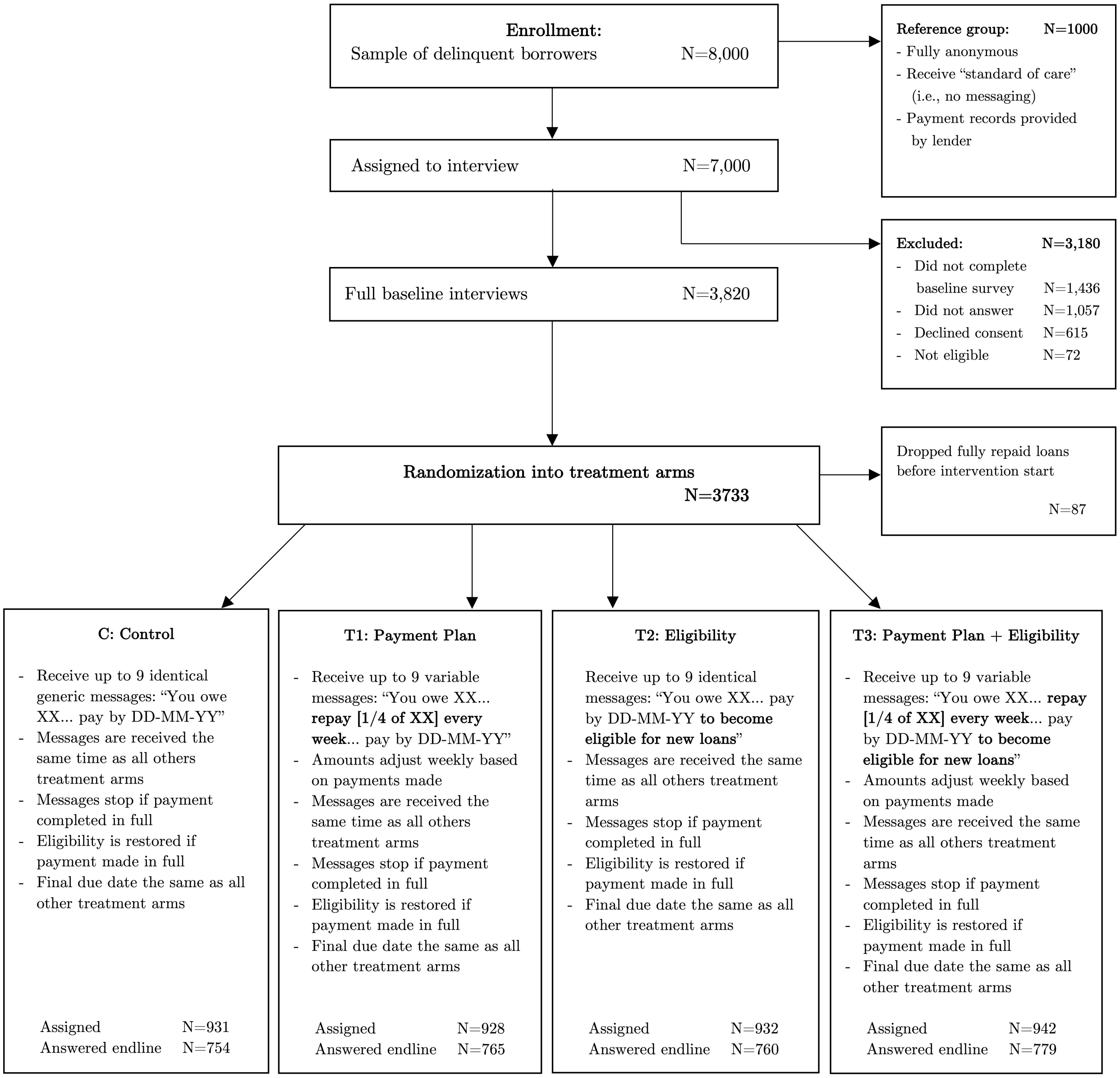


FIGURE 1: Enrollment and assignment to treatment arms

arms. Following randomization, treatment assignments were handed to the digital lender for the implementation phase. The lender would identify and drop those study participants who had fully repaid their loan prior to the start of the intervention. This was done on Fridays, 11 days after the initial batch of 1,400 potential participants was created. A total of five batches were rolled out and the intervention started for the last batch on May 24, 2024. Endline interviews took place in August 2024, i.e., two to four months after the baseline and one to three months after the completion of the intervention. As with the baseline, up to six attempts were made to contact each participant. Endline interviews took place in August 2024, i.e., two to four months after the baseline and one to three months after the completion of the intervention. As with the baseline, up to six attempts were made to contact each participant.

Considering the total of five weekly batches of 1,400 potential participants each (i.e. 7,000 potential participants in total), 3,820 study participants completed the baseline interview (55% of the initial sample of 7,000). The 87 participants fully repaid their overdue loan prior to the start of the intervention were dropped from the study sample and did not receive the intervention. The remaining sample of 3,733 study participants was randomized into the four treatment arms. A total of 3,172 respondents (82% of the baseline respondents) responded to the endline survey. Appendix Table A.1 shows nearly identical endline follow-up rates across treatments. Thus, there appear to be no concern for differential attrition.

3.4 Data

Our study uses three data sources: a baseline survey, an endline survey, and administrative data from the digital lender. The baseline survey includes socio-economic characteristics; digital borrowing history, including how the defaulted loan was used and whether the borrower has any other digital and non-digital debt; and access to other digital credit products. In addition, the baseline survey measures financial security of the borrower's household and the borrower's mental, financial literacy, and present bias. The endline survey contains most of the baseline survey questions and measures borrowers' perceptions about the consequences of default.[12]

The administrative data from the digital lender contain information on borrowers at the time the sampling was taken, including the repayment status of their overdue digital loan, the amount overdue, the date the client borrowed the overdue loan, the amount borrowed initially. In addition, we have information on dates and repayments amounts, whether loan was repaid in full, and partial and full repayment dates. Our survey partner, Innovations for Poverty Action (IPA), matched the lender data to the completed surveys and then provided us with an anonymized dataset containing matched data from both

[12]These questions were not asked at baseline as they could have primed repayment behavior had they appeared in the baseline survey.

sources. For the reference group we only have access to the administrative data as they were never contacted.

Table 1: Pre-intervention balance across treatment arms

	Treatment arms			
	C: Control	T1: Payment plan	T2: Eligibility	T3: Payment plan and eligibility
Panel A: Borrower characteristics				
Female	0.43	0.43 (0.8)	0.41 (0.25)	0.46 (0.23)
Age	36.83	37.19 (0.53)	37.09 (0.65)	37.72 (0.12)
Education (above secondary)	0.71	0.71 (0.88)	0.71 (0.87)	0.71 (0.95)
English proficiency	0.86	0.86 (0.86)	0.87 (0.63)	0.86 (0.87)
Marital status	0.6	0.6 (0.89)	0.6 (0.95)	0.6 (0.91)
Employed	0.6	0.63 (0.15)	0.59 (0.72)	0.63 (0.25)
Has electricity	0.8	0.82 (0.36)	0.81 (0.76)	0.81 (0.59)
Owning a bicycle	0.25	0.27 (0.5)	0.27 (0.3)	0.26 (0.78)
Owning a motorcycle	0.07	0.08 (0.84)	0.05 (0.01)	0.06 (0.17)
Owning an automobile	0.19	0.19 (0.74)	0.17 (0.43)	0.18 (0.68)
Owning a stove	0.49	0.5 (0.56)	0.48 (0.8)	0.51 (0.24)
Owning a TV	0.72	0.76 (0.03)	0.71 (0.52)	0.73 (0.53)
Owning a smartphone	0.71	0.72 (0.75)	0.71 (0.79)	0.72 (0.64)
Has other outstanding loans	0.78	0.78 (0.75)	0.78 (0.95)	0.78 (0.96)
Difficult to borrow (scale 1-10)	6.1	5.94 (0.36)	5.87 (0.2)	6.06 (0.83)
Financial security (scale 1-5)	2.85	2.82 (0.66)	2.8 (0.42)	2.84 (0.82)
Panel B: Baseline administrative records				
Loan number	15.71	16.3 (0.42)	16.41 (0.33)	16.96 (0.08)
Disbursed amount (USD)	39.93	39.2 (0.79)	43.38 (0.23)	44.97 (0.08)
Self-reported amount borrowed (USD)	30.33	31.63 (0.63)	31.42 (0.68)	38.2 (0.01)
Overdue loan balance (USD)	46.36	44.59 (0.58)	49.84 (0.3)	51.48 (0.13)
Weekend disbursement	0.23	0.26 (0.09)	0.26 (0.08)	0.25 (0.21)
Joint test p-value		0.82	0.87	0.87
Observations	931	928	932	942

Notes: Treatment group means reported, with p-values for the t-test against Control in parentheses. Joint tests compare all variables to Control with an F-test. Panel A: Characteristics collected from the baseline phone survey. Difficult to borrow describes the difficulty to borrow USD 8 by tomorrow, on a scale from 1 to 10. Panel B: Baseline administrative records were captured at the time the sampling was taken (see section 3.3 for details). Loan number is a counter of the total number of loans taken by the borrower over their entire borrowing history with the lender. Weekend disbursement is an indicator for whether the loan was requested and disbursed on Saturday or Sunday.

Table 1 reports baseline characteristics by treatment arms and the balance tests. The frequency of significant differences is low, and the overall F-tests suggests no differences between our treatment and control groups. 43% of the study sample is female with an average age of 37 years, and 60% is married. More than 70% of the sample has a level of education about secondary school and 86% speaks English. In addition, they appear to be relatively assets-rich (71% smartphone ownership, 18% car ownership, 80% electricity). 75% of the sample reports having other outstanding loans aside from the one with our digital lender. Administrative data show that the average delinquent borrower in our sample has an overdue loan amount of roughly \$50 and is on their 16th loan. About a quarter of the overdue loans were taken during the weekend.

Additional data show that our digital lender operates in a competitive environment. In fact, at the time of borrowing, 17.5% of respondents mentioned seeking additional credit from other sources to cover the same need: 79% of them sought credit from another digital lender, 11% from microfinance institutions, and 10% from informal sources (e.g., family and friends). Borrowers were intentional about their loan, with 88.4% saying they had a plan for what to do with the money, and Appendix Figure A.1 shows that business expenses and food are the most commonly reported uses for the loans. Similar to Hau et al. (2024) for Alipay in China, in our setting borrowers largely (86.1%) understand that if they default from a digital lender they will not be able to get another loan from the same lender.

3.5 Analysis

For each study participant i in batch wave w, we run the following OLS regression:

$$\begin{aligned} Y_{iw} = \quad & \alpha + \beta_1 \times T1 : \mathit{PaymentPlan}_i + \beta_2 \times T2 : \mathit{Eligibility}_i + \\ & \beta_3 \times T3 : \mathit{PaymentPlan\ and\ Eligibility}_i + \epsilon_{iw} \end{aligned} \tag{1}$$

where Y_{iw} is an outcome variable for participant i in batch w. $T1 : \mathit{PaymentPlan}$ is an indicator equal to one if the participant was assigned to the payment plan treatment arm, $T2 : \mathit{Eligibility}$ is an indicator equal to one if the participant was assigned to the eligibility treatment arm, and $T3 : \mathit{PaymentPlan\ and\ Eligibility}$ is an indicator equal to one if the participant received both treatments. We report heteroskedasticity-robust standard errors, under the assumption that observations are independent from one another. The assumption is valid in this context, as clients are geographically dispersed across the country, randomization was carried out at the individual level, and there are unlikely to be spillovers between participants.

The coefficients β_1, β_2, and β_3 represent the causal effects of being assigned to T1, T2, and T3, respectively, relative to being assigned to the control group α represents the mean outcome in the control group. This means that if the effects of the payment plan and eligibility notice are additive, we should observe that $\beta_1 + \beta_2 = \beta_3$.

We also run a second specification with batch fixed effects, α_w, and controls at the individual level, X_{iw}:[13]

$$\begin{aligned} Y_{iw} = \quad & \alpha_w + \beta_1 \times T1 : PaymentPlan_i + \beta_2 \times T2 : Eligibility_i + \\ & \beta_3 \times T3 : PaymentPlan\ and\ Eligibility_i + X_{iw} \cdot \Gamma + \epsilon_{iw} \end{aligned} \tag{2}$$

4 Results

4.1 Impacts on repayment

Figure 2 provides visual evidence of the impact of each treatment arm on total gross collections over time, starting from the day participants receive their first message. The reference group is included as well. As Figure 2, collections increased rapidly during the study period of 30 days in the three treatment arms as well as in the control group, relative to the reference group. The rate of collections is highest in the first 30 days, i.e., while participants were receiving messages. Second, collections are highest for the two treatment arms assigned to the payment plan (T1 and T3), and are similar for the control group (C) and the eligibility treatment (T2). Third, while collections slow down after 30 days, they continue to accumulate for some time afterwards with the accumulation rate being faster in the treatment arms with a payment plan (T1 and T3).

Next, Table 2 shows the regression estimates of Equations 1 (odd columns) and 2 (even columns) on six different loan outcomes. The outcome measures are: whether the borrower makes any repayment (columns 1 and 2); the fraction of the amount due that is repaid by the end of the study period, i.e. 30 days from the start of the intervention

[13]The control variables include the set of baseline variables we used to ensure balance across treatments during our randomization process for each batch: sex, age, urban vs. rural location, financial security, english reading proficiency, marital status, employment status, educational attainment, whether the respondent has electricity, a bicycle, a motorcycle, a car, a stove, a tv, a smartphone, a mobile money SIM card, or outstanding loans, and information about the current loan they are delinquent on, including its sequential loan number from the lender, the current balance, the disbursed amount, days since the disbursed date, the self-reported amount borrowed, whether the disbursed date was a weekend, and whether the loan was disbursed during the Christmas season.

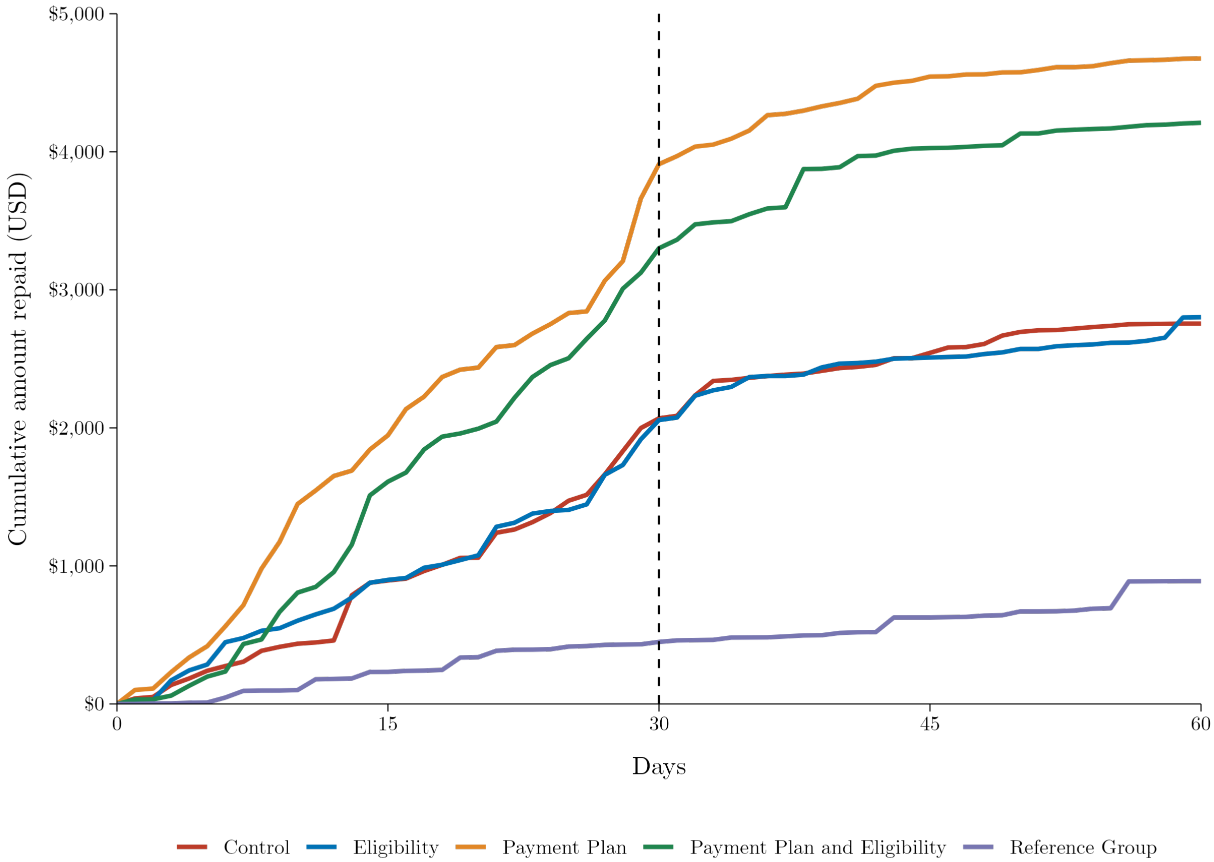


FIGURE 2: Cumulative amounts repaid by treatment arm from the start of intervention

(columns 3 and 4) and 60 days from the start of the intervention (columns 5 and 6); whether the loan is settled within 30 days (columns 7 and 8) and within 60 days (columns 9 and 10); and whether the borrower has borrowed again from the digital lender (columns 11 and 12). For comparison, Table 2 also includes the average value of each outcome variable for the reference group.

Table 2: Loan repayment by treatment arms

	Any repayment		Fraction paid (30 days)		Fraction paid (60 days)		Settled loan (30 days)		Settled loan (60 days)		Borrowed again	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
T1: Payment plan	0.0932***	0.0823***	0.0471***	0.0389***	0.0587***	0.0490***	0.0196*	0.0135	0.0326**	0.0248**	0.0271**	0.0210*
	(0.0187)	(0.0178)	(0.0121)	(0.0116)	(0.0136)	(0.0131)	(0.0109)	(0.0107)	(0.0129)	(0.0126)	(0.0115)	(0.0112)
T2: Eligibility	0.0267	0.0215	0.0165	0.0151	0.0207	0.0193	0.0182*	0.0180*	0.0192	0.0190	0.0193*	0.0187*
	(0.0176)	(0.0169)	(0.0115)	(0.0113)	(0.0129)	(0.0126)	(0.0109)	(0.0108)	(0.0124)	(0.0123)	(0.0112)	(0.0111)
T3: Payment plan and eligibility	0.0799***	0.0709***	0.0325***	0.0293***	0.0328**	0.0288**	0.0143	0.0129	0.0151	0.0130	0.0132	0.0114
	(0.0185)	(0.0175)	(0.0116)	(0.0112)	(0.0129)	(0.0124)	(0.0107)	(0.0104)	(0.0123)	(0.0120)	(0.0109)	(0.0107)
C: Control	0.1622***	0.2006***	0.0730***	0.0944***	0.0930***	0.1205***	0.0494***	0.0602***	0.0687***	0.0828***	0.0526***	0.0662***
	(0.0121)	(0.0202)	(0.0077)	(0.0127)	(0.0086)	(0.0144)	(0.0071)	(0.0119)	(0.0083)	(0.0138)	(0.0073)	(0.0126)
Reference group	0.0410	0.0410	0.0151	0.0151	0.0264	0.0264	0.0170	0.0170	0.0280	0.0280	0.0240	0.0240
Observations	3,733	3,733	3,733	3,733	3,733	3,733	3,733	3,733	3,733	3,733	3,733	3,733
Controls		✓		✓		✓		✓		✓		✓
Batch fixed effects		✓		✓		✓		✓		✓		✓
H0: T1 = T2	0	0	0.02	0.05	0.01	0.03	0.91	0.7	0.33	0.66	0.52	0.85
H0: T1 = T3	0.5	0.55	0.25	0.43	0.07	0.14	0.65	0.96	0.19	0.36	0.25	0.41
H0: T2 = T3	0.01	0.01	0.19	0.23	0.37	0.46	0.73	0.65	0.75	0.63	0.6	0.53
H0: T3 = T1 + T2	0.13	0.19	0.07	0.14	0.02	0.03	0.14	0.23	0.05	0.09	0.04	0.08

Notes: $^{***} \Rightarrow p < 0.01, ^{**} \Rightarrow p < 0.05, ^{*} \Rightarrow p < 0.10$. Sample includes all borrowers enrolled in the study at baseline that had not settled their loans prior to the intervention start date. Estimates are from OLS regressions with heteroskedasticity-robust standard errors shown in parentheses below the estimates. Any repayment is an indicator variable for whether the borrower made at least one payment. Fraction paid (30 days) measures the amount of the loan paid as a fraction of the initial overdue amount at the end of the study period. Fraction paid (45 days) measures the amount of the loan paid as a fraction of the initial overdue amount at 60 days from the start of the intervention. Settled (30 days) is a dummy variable equal to one if the loan was fully repaid by the end of the study period. Settled (60 days) is a dummy variable equal to one if the loan was fully repaid by 60 days from the start of the intervention. Borrowed again is a dummy variable equal to one if the borrower borrowed again at any time after repayment. The reference group is a separate sample of delinquent borrowers who were not included in the study sample for which we have access to administrative data. The reference group row in the table reports the mean outcome among this group. The reference group sample is not included in any of the regressions. Control variables are listed in Footnote 13.

We discuss our results sequentially for each treatment arm, using the estimates corresponding to Equation 1. The payment plan (T1) improved repayment behavior for all outcome variables. Specifically, being offered a payment plan increased the likelihood of a borrower making any payment by 9.3 percentage points (p.p.). It also raised by 4.7 p.p. the fraction paid by the end of the study period, i.e. 30 days, and by 5.9 p.p. the fraction paid within 60 days. Moreover, it increased by 2 p.p. the likelihood of a loan being settled within 30 days and by 3.3 p.p within 60 days. Finally, it raised the likelihood of re-borrowing by 2.7 p.p. All of these estimates are statistically significant at either the 5% or 1% level, with the exception of 30-day settlement, which is marginally statistically significant at the 10% level. Results using the estimates corresponding to Equation 2 are smaller in magnitude, but tell a similar story: the largest proportional difference across specifications is for settlement within 30 days, where the estimate is not statistically significant in the fully-specified model. However, we do estimate a statistically significant effect on settlement within 60 days with this model. The effects of the payment plan treatment are large relative to the mean in the control group. For example, 16.2% of borrowers in the control group made any repayment, meaning that the 9.3 p.p. increase due to the payment plan (T1) represents a 57% increase. Similarly, 4.9% of borrowers in the control group settled during the study period, meaning that the 2 p.p. increase due to the payment plan (T1) represents a 41% increase. That said, it is notable that the fraction of borrowers who settled their loan is significantly smaller than the fraction who began payments, suggesting a high rate of abandonment of the plan.

The eligibility treatment (T2) did not have statistically significant average impacts on most repayment behaviors. While all coefficients are positive, they are marginally statistically significant only settlements within 30 days and for re-borrowing. As shown at the bottom of Table 2, t-tests reject the hypothesis of equality of the effects of the payment plan and the eligibility treatments (i.e., H0: T1 = T2) for the outcome variables any repayment (columns 1 and 2) and fraction paid (columns 3-6). On the other hand, we cannot reject the same hypothesis for settled loans (columns 7-10) and borrowed again (columns 11-12). Overall, while there is suggestive evidence that the eligibility notice induced a small number of borrowers to settle during the study, this treatment arm did not have a widespread effect on repayment behavior.

Combining the payment plan and eligibility treatments (T3) has similar but smaller in magnitude effects to the payment plan treatment alone (T1) across some, but not all outcomes. As with T1, borrowers assigned to T3 have a relatively large and statistically significant increase in the likelihood of making at least one payment relative to the control group (8 p.p., a 49% increase). T-tests show that the effect size of combining the payment plan and eligibility (T3) is similar to the payment plan treatment alone (T1), and statistically different from the eligibility treatment alone (T2). While the fraction repaid increased by 3.3 p.p., combining the two treatments (T3) has no statistically significant effects on loan settlement or re-borrowing. In fact, the estimated effect on loan settlement is lower in T3 than in T1, although t-tests do not reject the hypothesis of equality of the effects of T1 being equal to T3 (i.e., H0: T3 = T1). Rather than being complementary, these results suggest that the eligibility notice may have partly undermined the effect of the payment plan. A potential explanation could be that a more comprehensive dominant alternative (T3 in our context) may not always be more powerful than a simpler option (Puri 2025). Bertrand et al. (2010) also shows that offering borrowers larger menus of loan options can trigger choice avoidance and/or deliberation that makes the advertised loan less appealing. Alternatively, it is possible that borrowers in T3 use the payment plan to pay down their overdue loan but are unwilling to repay in full as a way to commit to not borrow again. We will test for these possible explanations in section 4.3. Finally, it is important to note that we can either reject or nearly reject that the effect of combining the two treatments (T3) is equal to the sum of the effects of the payment plan (T1) and the eligibility (T2).

Moreover, all coefficients of all outcome measures are higher for the control group when compared with the averages for the reference group. For example, 16.2% of the control group made at least one payment compared to 4.1% for the reference group (column 1). That said, the absolute effects are modest. For every 100 long-term delinquent borrowers, we would expect three of them to settle without receiving any messages from the lender within a 60-day period. The digital lender could increase the number of delinquent borrowers who settle their loans to seven with SMS reminders, and increase it to ten with a payment plan.[14]

[14]This assumes no selection in responding to our survey. We re-visit this issue with a bounding exercise in Section 4.4.

Frequency of payments In Table 3 we now study whether the payment plan and eligibility treatments lead to more frequent payments. In principle, a payment plan by breaking up a large one-time payment into more payments of smaller amounts should increase the number of payments. In contrast, we do not expect the eligibility to have any impact on the number of payments. Using indicator variables for making two or more, three or more, and four or more payments in columns 3-8, we estimate the effects of each treatment using the same regression specifications as in Table 2. Panel A shows the impacts on payments made during the intervention (i.e., 0-30 days) and Panel B shows the impacts on payments made in the 30 days following the intervention (i.e. 31-60 days).

During the study period (i.e. 30 days), we find that, relative to the control group, T1 (T3) leads to an increase in the likelihood of two or more payments by 89% (78%), three or more payments by 148% (107%), and four or more payments by 93% (90%), considering the estimates of equation 1. All coefficient estimates are statistically significant. As expected, there are no effects –we obtain precise zeros– for the effects of the eligibility treatment (T2) on making more than one payment. Importantly, Panel B shows that in the 30 days after the intervention, borrowers in the payment plan treatments (i.e., T1 and T3) are significantly more likely to make at least two payments even if the intervention period has ended, consistent with the small victories model, and the post-study period repayment momentum shown in Figure 2.

Table 3: Number of payments by treatment arm

	Number of payments							
	= 1		≥ 2		≥ 3		≥ 4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Intervention period (0-30 days)								
T1: Payment plan	0.0413***	0.0367**	0.0519***	0.0457***	0.0335***	0.0299***	0.0130**	0.0106*
	(0.0153)	(0.0150)	(0.0128)	(0.0125)	(0.0090)	(0.0088)	(0.0066)	(0.0064)
T2: Eligibility	0.0214	0.0200	0.0053	0.0015	0.0096	0.0084	0.0011	0.0002
	(0.0148)	(0.0146)	(0.0111)	(0.0108)	(0.0076)	(0.0075)	(0.0055)	(0.0054)
T3: Payment plan and eligibility	0.0349**	0.0314**	0.0450***	0.0396***	0.0242***	0.0212***	0.0126*	0.0103
	(0.0151)	(0.0148)	(0.0125)	(0.0121)	(0.0084)	(0.0082)	(0.0065)	(0.0064)
Control	0.1042***	0.1171***	0.0580***	0.0835***	0.0226***	0.0385***	0.0140***	0.0244***
	(0.0100)	(0.0174)	(0.0077)	(0.0135)	(0.0049)	(0.0099)	(0.0038)	(0.0073)
Reference Group	0.0290	0.0290	0.0120	0.0120	0.0070	0.0070	0.0060	0.0060
H0: T1 = T2	0.21	0.29	0	0	0.01	0.02	0.07	0.11
H0: T1 = T3	0.69	0.74	0.63	0.66	0.36	0.39	0.96	0.96
H0: T2 = T3	0.39	0.46	0	0	0.11	0.15	0.08	0.12
H0: T3 = T1 + T2	0.21	0.24	0.5	0.67	0.14	0.18	0.88	0.95
Panel B: 30 days after the intervention (31-60 days)								
T1: Payment plan	0.0174	0.0136	0.0119**	0.0108*	0.0022	0.0019	0.0022	0.0020
	(0.0116)	(0.0115)	(0.0059)	(0.0058)	(0.0040)	(0.0039)	(0.0022)	(0.0021)
T2: Eligibility	0.0032	0.0005	0.0086	0.0077	0.0064	0.0060	0.0032	0.0031
	(0.0110)	(0.0109)	(0.0056)	(0.0055)	(0.0045)	(0.0044)	(0.0024)	(0.0023)
T3: Payment plan and eligibility	-0.0060	-0.0101	0.0158**	0.0143**	0.0010	0.0005	0.0021	0.0020
	(0.0105)	(0.0105)	(0.0062)	(0.0061)	(0.0038)	(0.0038)	(0.0021)	(0.0022)
Control	0.0580***	0.0815***	0.0107***	0.0074	0.0064**	0.0050	0.0011	0.0031
	(0.0077)	(0.0134)	(0.0034)	(0.0061)	(0.0026)	(0.0050)	(0.0011)	(0.0033)
Reference Group	0.0240	0.0240	0.0070	0.0070	0.0050	0.0050	0.0030	0.0030
Observations	3,733	3,733	3,733	3,733	3,733	3,733	3,733	3,733
Controls		✓		✓		✓		✓
Batch fixed effects		✓		✓		✓		✓

Notes: *** ⇒ $p < 0.01$,** ⇒ $p < 0.05$,* ⇒ $p < 0.10$. Sample includes all borrowers enrolled in the study at baseline that had not settled their loans prior to the intervention start date. Estimates are from OLS regressions with heteroskedasticity-robust standard errors shown in parentheses below the estimates. Columns (1) and (2) estimate the treatment effects on whether a borrower makes exactly one payment; columns (3) and (4) show two or more payments; columns (5) and (6) show three or more payments; columns (7) and (8) show four or more payments. The reference group is a separate sample of delinquent borrowers who were not included in the study sample for which we have access to administrative data. The reference group row in the table reports the mean outcome among this group. The reference group sample is not included in any of the regressions. Control variables are listed in Footnote 13.

Impact on settlement by loan size Next, we consider whether treatment effects on loan settlement vary by the size of the overdue loan amount. Table 4 reports the estimates of Equations 1 and 2 estimated separately for each quartile of amount overdue at the time of the baseline survey. Panel A shows settlement during the study period, and Panel B shows settlement during the 30 days after. Estimates from these models are less precise due to the sub-sample sizes. The positive effects of payment plans on settlement both during and after the intervention period are concentrated among loans in the middle quartiles. Our interpretation is that small loans do not respond to payment plans either because the lack of repayment is likely driven by unwillingness to repay, or because only completely constrained borrowers do not repay these loans. Interestingly, we find that for the eligibility treatment (T2) there is a positive and statistically significant effect for the second quartile of overdue amounts. The point estimate for T2 is statistically significant at the 1% level and indicates a 6.4 p.p. increase in repayment, while all other estimates are very close to zero. We interpret this to indicate that the possibility of regaining eligibility may be appealing to some if the cost of meeting the rehabilitation criteria is not too high. However, this does not explain the lack of an eligibility effect for the bottom quartile. Again, it could be the case that voluntary defaulters are overrepresented in the bottom quartile of loan sizes.

Table 4: Quartile analysis of settled loans by treatment arm

	Settled loans by percentiles of loan size at baseline							
Percentiles:	0-25%		25-50%		50-75%		75-100%	
Loan sizes (USD):	1-4		4-14		14-57		57-314	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Intervention period (0-30 days)								
T1: Payment plan	-0.0179	-0.0437*	0.0343	0.0289	0.0421**	0.0234	0.0235	0.0210
	(0.0252)	(0.0230)	(0.0225)	(0.0220)	(0.0210)	(0.0198)	(0.0163)	(0.0176)
T2: Eligibility	-0.0112	-0.0176	0.0636***	0.0649***	0.0195	0.0138	0.0074	0.0046
	(0.0262)	(0.0250)	(0.0243)	(0.0234)	(0.0192)	(0.0188)	(0.0136)	(0.0135)
T3: Payment plan and eligibility	0.0086	-0.0036	0.0180	0.0190	0.0290	0.0187	0.0129	0.0108
	(0.0274)	(0.0238)	(0.0212)	(0.0212)	(0.0202)	(0.0204)	(0.0140)	(0.0140)
Control mean	0.0941	0.0941	0.0442	0.0442	0.0351	0.0351	0.0180	0.0180
Panel B: 30 days after the intervention (31-60 days)								
T1: Payment plan	0.0167	-0.0079	0.0252	0.0275	0.0609**	0.0413*	0.0284	0.0254
	(0.0284)	(0.0266)	(0.0272)	(0.0261)	(0.0257)	(0.0250)	(0.0200)	(0.0213)
T2: Eligibility	0.0040	-0.0023	0.0531*	0.0582**	0.0144	0.0101	0.0108	0.0084
	(0.0283)	(0.0269)	(0.0284)	(0.0270)	(0.0227)	(0.0228)	(0.0176)	(0.0171)
T3: Payment plan and eligibility	0.0141	0.0012	0.0092	0.0116	0.0327	0.0273	0.0148	0.0119
	(0.0286)	(0.0255)	(0.0262)	(0.0249)	(0.0242)	(0.0241)	(0.0176)	(0.0177)
Control mean	0.1020	0.1020	0.0796	0.0796	0.0570	0.0570	0.0315	0.0315
Observations	932	932	921	921	946	946	934	934
Controls		✓		✓		✓		✓
Batch fixed effects		✓		✓		✓		✓

Notes: $^{***} \Rightarrow p < 0.01, ^{**} \Rightarrow p < 0.05, ^{*} \Rightarrow p < 0.10$. Sample includes all borrowers enrolled in the study at baseline that had not settled their loans prior to the intervention start date. Estimates are from OLS regressions with heteroskedasticity-robust standard errors shown in parentheses below the estimates. The outcome variable is an indicator that takes value one if the loan was settled by the end of the study period in Panel A, and in the 30 days following the study period in Panel B. Because the control variables are standardized at the full sample level, we report the mean outcome from the control treatment rather than the estimated constant term, to maintain comparability across models with and without control variables. Control variables are listed in Footnote 13.

4.2 Are borrowers rehabilitated?

Columns (9) and (11) of Table 2 show that the payment plan treatment (T1) increased the 60-day settlement rate by 3.3 p.p. and the likelihood that a new loan was issued by 2.7 p.p. relative to the control group. This means that the rate of re-borrowing is high among delinquent borrowers who settled their overdue loan in T1 (i.e., 79%). Rates of reborrowing are similar in T2 and T3 (82% in T2 and 79% in T3).

How quickly do borrowers return to borrowing after repayment? Elsewhere, researchers have observed rapid credit cycling among digital borrowers. Carlson (2017) and Burlando et al. (2025) both find that the median time between digital loans is one day. Administrative data from the digital lender allows us to show a similar pattern. By comparing the date of repayment with the date in which the borrower takes out a new loan, we find that immediate re-borrowing is the modal behavior in our study, although re-borrowing happens at a slower pace than Carlson (2017) and Burlando et al. (2025). Figure 3 shows that about 17% of the new loans are generated the same day the previous loan was repaid, but the median wait is over a week.

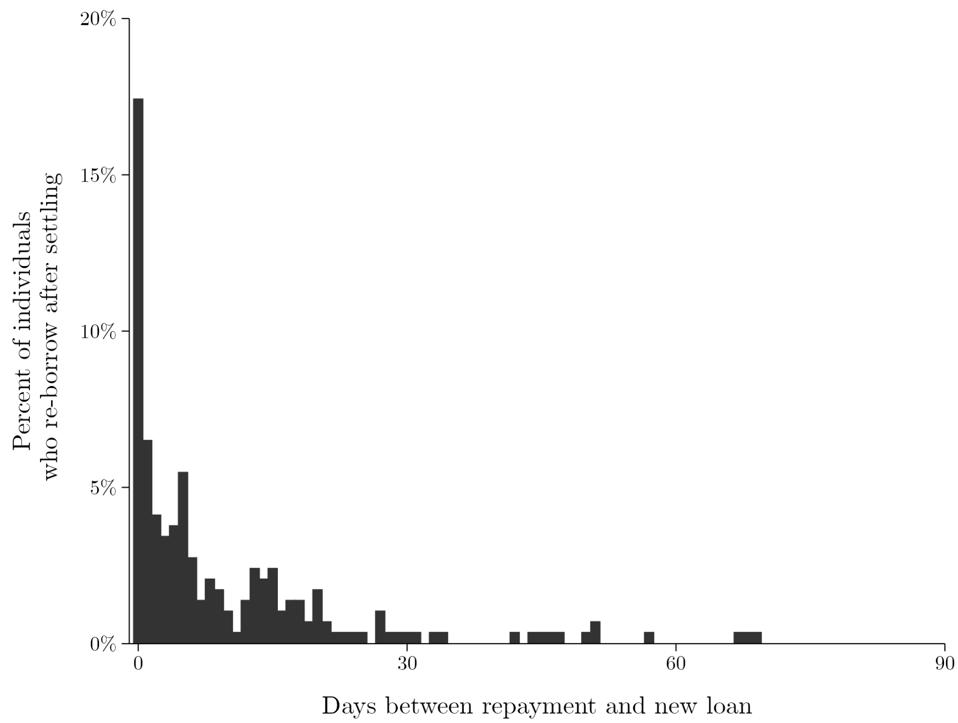


FIGURE 3: Days between repayment and a new loan delinquent borrowers who fully repay and borrow again

4.3 Mechanisms and treatment effect heterogeneity

Next, we explore the role of economic and behavioral factors in explaining our results. Specifically, we test whether impacts are heterogeneous along five pre-specified self-reported characteristics measured at baseline: access to liquidity; patience; spending discipline; risk aversion; and cognitive reasoning. These tests are presented in Appendix Tables A.2-A.6.

First, we explore the role of liquidity constraints. As discussed in Section 2.1, we hypothesized that payment plans could help to start the repayment process among borrowers with enough liquidity to pay a loan installment, but not the full amount overdue. If this is true, we expect that the effects of the payment plan treatment in T1 and T3 on repayment are concentrated among those who are liquidity-constrained at baseline. Our proxy for liquidity measures whether the respondent reported having sufficient savings to deal with an emergency that required $35.[15] In Appendix Table A.2 we report the

[15] The baseline survey asked, "Imagine your household had an emergency that required [roughly $35] to cover, would you have enough savings to deal with it?"

interactive effects of each treatment arm with this liquidity indicator. While we lack the precision to reject the null hypothesis of no differential effects by liquidity, the pattern of results is consistent with liquidity-constrained borrowers being more affected. In fact, nearly every interaction term point estimate is negative, and the magnitudes are comparable to the un-interacted point estimates. For example, our main results in Table 2 show a 2 p.p. effect of the payment plan alone on loan settlement within 30 days (column 7). The corresponding estimate for liquidity-constrained borrowers in Appendix Table A.2 is 3 p.p. and for non-constrained borrowers is 0.6 p.p. This pattern holds for our other outcome measures, for the combined treatment (T3) and the eligibility treatment (T2).

Second, we study the role of time preferences. Payment plans shift the timing of costly repayment and represent a soft commitment device that might be appealing to sophisticated time-inconsistent borrowers. Eligibility, on the other hand, could make repayment *less* appealing to a sophisticated time-inconsistent borrower that had previously decided not to repay their loan as a commitment strategy to avoid future borrowing. To test for these potential mechanisms, we use the self-reported measures of patience and awareness of spending discipline problems collected at baseline.[16]

The interaction coefficients of each treatment arm with patience are reported in Appendix Table A.3. We find that inpatient borrowers are more affected by the payment plan along (T1) as this treatment arm increases the probability of any repayment by 14.3 p.p, the fraction paid within 30 (60) days by 8.0 p.p (9.5 p.p), and the settlement rate within 30 (60) days by 3.1 p.p (5.1 p.p). These effects are 93% to 154% larger than those for borrowers with below median patience, although we can only statistically reject equal effect sizes in the case of any repayment and the fraction of the overdue amount repaid. We observe no statistically significant heterogeneity by patience in the eligibility treatment (T2) or the combined payment plan and eligibility treatment (T3).

Regarding time inconsistency, we define borrowers that report being disciplined with their spending to be either time-consistent or unaware of their inconsistency, and we consider borrowers that report otherwise to be sophisticated about their time-inconsistency.

[16]The baseline survey asked, “On a scale from 1 to 10, where 1 is extremely impatient and 10 is extremely patient, how patient of a person would you say you are, relative to others?” We then created an indicator variable for above-median patience. The baseline survey also asked borrowers to rate how often they “have difficulty sticking to the plans [they] make about my money, even when emergencies do not happen” on a scale from one (“never”) to five (“very often”). We then created an indicator variable for above-median discipline (a one or two on the scale).

Appendix Table A.4 does not find evidence that sophistication matters for the payment plan alone (T1). Estimates of the interaction coefficient for the eligibility (T2) tentatively suggest that the weak overall effects of the eligibility notice might be driven by borrowers that perceive themselves to be disciplined spenders. That is because, despite our lack of precision, the estimates for eligibility (T2) among borrowers with discipline problems are very close to zero, but the interactive effects with spending discipline are positive and of substantial magnitudes. The same is true for the combined treatment (T3). These results suggest that payment plans do not offer value as a soft commitment device for sophisticated time-inconsistent borrowers. Moreover, eligibility is also not an attractive offer for those borrowers, possibly because they use default as a commitment device to avoid borrowing again in the future. Because our measures of sophistication and patience are correlated, we believe this also helps explain why, for impatient borrowers, the treatment that combines the payment plan and the eligibility (T3) is not as attractive as the treatment with the payment plan alone (T1).

Third, we consider the role of risk preferences.[17] Payment plans might be appealing to risk-averse borrowers if they face time-varying liquidity shocks. Loan installments help maintain access to precautionary savings needed to confront potential negative shocks. Interaction coefficient estimates reported in Appendix Table A.5 do not support this view. We find that payment plan treatments (T1 and T3) may be less effective at stimulating repayments for more risk-averse borrowers. This is most pronounced when we consider whether a loan is settled within 30 days: the interaction coefficients of our risk aversion measure with T1 and T3 are zero or negative for the risk-averse group, but positive and significant for the risk-tolerant group. This difference in effects is marginally statistically significant for T3.[18] There is no systematic relationship between risk aversion and the effects of the eligibility notice (T2).

Fourth, we explore the role of cognitive reasoning, using a cognitive indicator built using two cognitive reflection test questions.[19] Cognition may be particularly important

[17] The baseline survey asked, "On a scale from 1 to 10, where 1 is completely unwilling and 10 is completely willing, how willing are you to take risks in general?" We then create a risk-averse indicator variable for below-median willingness to take risks.

[18] Using the fully-specified model (column 8), this is also true for T1.

[19] The baseline survey used one question from the Cognitive Reflection Test (CRT) (Frederick 2005) and another from the CRT2 (Thomson and Oppenheimer 2016). Borrowers were first asked, "If you're running a race and you pass the person in second place, what place are you in? and then asked "A meal and a drink cost [roughly $4.40] in total. The meal costs [roughly $4] more than the drink. How

in explaining why the treatment combining the payment plan and the eligibility (T3) is, on average, somewhat less effective than the payment plan alone (T1). As shown by Puri (2025), people with lower cognition levels may choose dominated policies that are less complicated. Thus, in our setting, if the combination of the two treatments is perceived as more complicated, we should expect a lower impact among those with lower cognitive reasoning skills. However, estimates in Appendix Table A.6 do not show that cognitive reasoning plays a role.

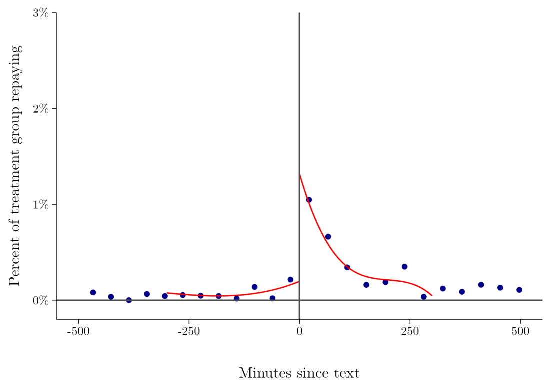
4.4 The role of reminders

So far, our analysis has focused on the effects and mechanisms of our treatments compared to the control group, taking advantage of the design of our randomized control trial. All study participants, including the control group, received the same number of messages from the lender with the same frequency, allowing us to abstract from any "reminder" effects of our interventions. We can analyze the impact of reminders by comparing repayment behavior of the control group (which received reminders about their overdue loan amount and the new due date) with the reference group (that was never contacted during the intervention). As Table 2 shows, repayment behaviors are more common among delinquent borrowers in the control group than among those in the reference group. For example, 16.2% of delinquent borrowers in the control group made a payment, while only 4.1% of borrowers in the reference group did. Similarly, 4.9% of borrowers in the control group settled their loan during the study period compared to 1.7% in the reference group. These comparisons suggest that the timing and frequency of messages played an important role in driving repayment by reminding borrowers of their overdue loan.

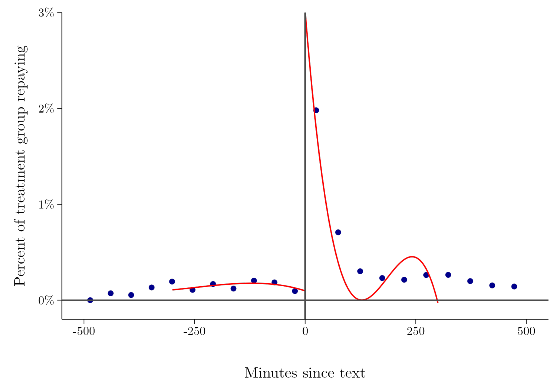
In this section, we quantify the relative importance of frequent reminders about the overdue loan against message content (i.e. payment plan and eligibility). We start by showing that receiving messages reminds borrowers of their overdue debt and induces them to make payments. We use timestamps to compare the time when messages are sent with the time when payments are received. Figure 4 shows the likelihood of a payment being received 500 minutes before and after a message was sent to the delinquent borrowers in our sample. Across all treatment arms, we see that repayments spike within minutes of a

much does the drink cost?" We then split the sample by an indicator variable for whether a respondent answered either question correctly.

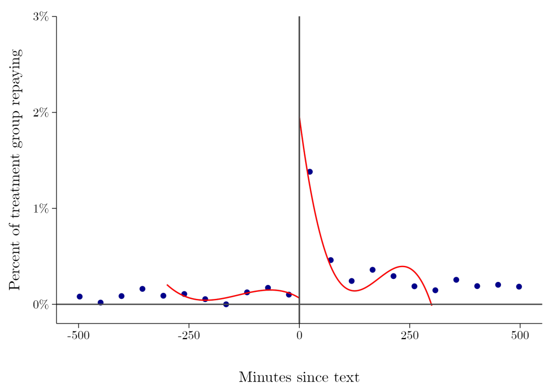
message being sent, but that the effect fades out within four hours.



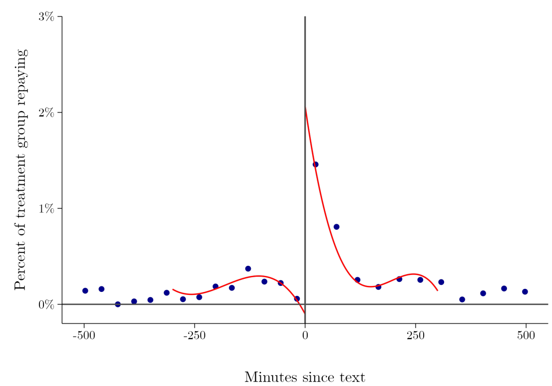
(a) Control



(b) T1: Payment Plan



(c) T2: Eligibility



(d) T3: Payment Plan and Eligibility

FIGURE 4: Repayment before and after receiving a message

Next, we decompose the overall effect of each treatment arm relative to the reference group into the "content" effect and the "reminder" effect. To do this, we need an accurate measurement of repayments that would have occurred, absent the intervention, for the delinquent borrowers in our study. However, repayments among the reference group are not a fully accurate measure of this counterfactual because the study sample includes *only* individuals who consented to participate in the study (55% of the initial sample of potential study participants), and does not include non-consenters (i.e. those who chose not to participate or could not be reached, which represents 45% of the initial sample of potential study participants). Since the reference group was never contacted, the comparison between these two samples suffers from selection bias.

To overcome this issue, we use a bounding procedure shown in Appendix Table A.7 to adjust for participants' selection into the study. We start with the estimates of the effect of our treatment arms on any repayment and settlement during the study period (columns 1 and 7 in Table 2). We use these estimates to calculate the overall rates of payment for participants in our study. Then, we bound these rates to account for the fact that 45% of the initial sample of potential participants are non-consenters. In our adjustment, we make use of the fact that the rates of making any payment and settlement for non-consenters are 3.2% and 3.1%, respectively. We therefore compute the weighted average between repayments of study participants and non-consenters. These adjusted means are comparable with the reference group under the assumption that non-consenters in the study do not change their payment behavior if treated. Since we expect that some non-consenters would have made additional repayments if we had been able to enroll them in the study, we see these as conservative bounds.

Next, we use the repayment rates of the reference group (also reported in Table 2) to net out repayments that would have occurred absent the intervention, and compute the fraction of each treatment effect *relative to the reference group* attributable to the content and reminder effect respectively. The bottom panel of Appendix Table A.7 shows that the overall effect of the payment plan on any payment (settlement) is explained by at least 43% (31%) the payment plan feature and at most 57% (69%) by reminders. Considering the eligibility treatment and the combined treatment, we also see that reminders play a large positive role in repayment.

4.5 Welfare effects

Table 5 reports the impacts on welfare measures collected three months after the end of the intervention. Following our pre-analysis plan, we consider six outcome variables, two from administrative data and four from the endline survey. The variables are: any repayment (columns 1 and 2) and borrowed again (columns 3 and 4);[20] whether the respondent reports having difficulty in borrowing again (columns 5 and 6); whether they feel financially insecure (columns 7 and 8); their overall amount of debt (columns 9 and 10); and an index of stress (columns 11 and 12). Additionally, we also report an overall index of the four survey welfare outcomes (columns 13 and 14).[21] As for the previous tables, es-

[20]These two outcome variables are the same outcome variables used in Table 2.

[21]We note that because delinquent borrowers who repay mostly end up borrowing again, it makes sense that the overall rate of indebtedness is unchanged despite our observed repayment effects.

timates of Equation 1 (Equation 2) are in odd-numbered (even-numbered) columns. We also report sharpened q-values that adjust our p-values for multiple hypothesis testing over the six pre-registered measures (Anderson 2008).

As the coefficient estimates in Table 5 show, the payment plan treatment (T1) does not affect any of the endline welfare measures, using either the q-values or standard p-values. We find evidence however, that borrowers given the eligibility treatment (T2 and T3) felt less financially secure and more stressed at endline. In the combined payment plan and eligibility treatment (T3), we find a statistically significant increase in stress of 0.23 standard deviations (column 11) even when adjusting for multiple hypothesis testing. We also find large point estimates for the effect of eligibility alone (T2) on stress and financial insecurity. The estimates are statistically significant based on standard inference, but not according to the q-values. That said, given that the direction of the effect is to reduce well-being, we should be conservative about concluding that there are no negative welfare effects. To that end, we consider the overall index of survey-based welfare measures with standard p-values, and find statistically significant reductions for both the eligibility treatment alone (0.156 standard deviations) and the combined treatment payment plan and eligibility (0.12 standard deviations).

We interpret the negative effects of eligibility notices on stress and financial security as the result of borrowers being reminded that, as long as their loan is overdue, they will not be able to borrow from our digital lender. As the fraction of delinquent borrowers who settle their overdue loan is low, these effects are likely driven by borrowers that do not successfully repay. Considering the payment plan, we hypothesize that it gives a certain degree of protection against financial insecurity, as it provides delinquent borrowers with an instrument to make those payments in the future. By learning that overdue amounts can be "chipped away" until full repayment, borrowers do not see their financial security further eroded by the eligibility notifications. While sending text messages about overdue loans is a low-cost strategy for the lender, a key takeaway from Table 5 is that it can have negative welfare effects for borrowers.

Table 5: Impacts on consumer well-being at endline

	Any repayment		Borrowed again		Difficult to borrow		Financially insecure		Overall debt		Stress index		Overall index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
T1: Payment plan	0.0932***	0.0823***	0.0271**	0.0210*	0.1421	0.0667	-0.0043	0.0115	0.0496	0.1016	0.0369	0.0683	-0.0249	-0.0376
	(0.0187)	(0.0178)	(0.0115)	(0.0112)	(0.2021)	(0.1944)	(0.0603)	(0.0548)	(4.067)	(3.222)	(0.0910)	(0.0867)	(0.0556)	(0.0486)
	[0.001]	[0.001]	[0.05]	[0.18]	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]		
T2: Eligibility	0.0267	0.0215	0.0193*	0.0187*	0.2413	0.1660	0.1430**	0.1336**	3.794	2.406	0.1776*	0.1638*	-0.1560***	-0.1338***
	(0.0176)	(0.0169)	(0.0112)	(0.0111)	(0.2038)	(0.1966)	(0.0608)	(0.0561)	(4.092)	(3.191)	(0.0911)	(0.0876)	(0.0549)	(0.0488)
	[0.183]	[0.227]	[0.167]	[0.182]	[0.244]	[0.253]	[0.127]	[0.116]	[0.244]	[0.253]	[0.148]	[0.182]		
T3: Payment plan + eligibility	0.0799***	0.0709***	0.0132	0.0114	0.2662	0.1930	0.0377	0.0207	5.133	1.243	0.2297**	0.1882**	-0.1196**	-0.0765
	(0.0185)	(0.0175)	(0.0109)	(0.0107)	(0.2026)	(0.2001)	(0.0610)	(0.0562)	(4.116)	(3.019)	(0.0927)	(0.0893)	(0.0561)	(0.0489)
	[0.001]	[0.001]	[0.233]	[0.413]	[0.233]	[0.413]	[0.396]	[0.779]	[0.233]	[0.413]	[0.035]	[0.097]		
Control	0.1622***	0.2006***	0.0526***	0.0662***	5.530***	4.425***	-2.707***	-2.664***	42.02***	43.57***	-0.1095*	-0.1990*	0.0755*	0.0474
	(0.0121)	(0.0202)	(0.0073)	(0.0126)	(0.1442)	(0.2609)	(0.0442)	(0.0670)	(2.918)	(3.913)	(0.0658)	(0.1043)	(0.0397)	(0.0582)
Observations	3,733	3,733	3,733	3,733	2,893	2,728	3,013	3,013	2,813	2,619	3,006	3,006	2,618	2,618
Controls		✓		✓		✓		✓		✓		✓		✓
Batch fixed effects		✓		✓		✓		✓		✓		✓		✓

Notes: $^{***} \Rightarrow p < 0.01, ^{**} \Rightarrow p < 0.05, ^{*} \Rightarrow p < 0.10$. Estimates are from OLS regressions with heteroskedasticity-robust standard errors shown in parentheses below the estimates. Sharpened q-values that adjust for multiple hypothesis testing over the six pre-registered measures (columns (1)-(12)) are in brackets below the standard errors. Columns (1)-(4) are identical to our models of any repayment and re-borrowing in Table 2. Difficulty borrowing is measured on a Likert scale from 1-10. Financial insecurity is a measured on a (negated) Likert scale from 1-5. Overall debt is converted to USD and winsorized at the 95th percentile. Precise details on the survey questions and outcome variable definitions for columns (5)-(14) are in Appendix Section A.2. The stress index is a standardized combination of two survey questions on stress and anxiety. The overall index is a standardized combination of the preceding four measures. Control variables are listed in Footnote 13. This set includes baseline financial security, and we have baseline survey measures of borrowing difficulty and overall debt, which we add as controls in columns (6) and (10), respectively.

5 Conclusion

Digital credit has revolutionized access to finance in low- and middle-income countries, offering speed and convenience. Yet, the promise of instant, low-friction lending is tempered by high default rates and the absence of conventional enforcement mechanisms. Our study directly tackles this tension by exploring the effects two light-touch interventions aimed at increasing repayment of delinquent digital loans: the notification of the regaining eligibility for a future loan if the delinquent loan is repaid, and the offer of a payment plan, which breaks down a full repayment into four equal installments.

Our findings show that offering a payment plan, delivered via SMS and requiring no in-person interaction, significantly improved repayment outcomes among borrowers severely delinquent. By contrast, the eligibility treatment–informing borrowers they would regain access to future loans upon repayment–had limited effects but worsened borrower welfare in the short-run, making borrowers feel less financially secure and more stressed. The behavioral impacts of our interventions were not related to their short-run welfare impacts in the obvious way, which should be a caution against overlooking the measurement of well-being outcomes in digital credit research. Combining the two interventions did not amplify their effects; if anything payment plans offered in conjunction with the eligibility notices were slightly less effective than payment plans alone. Most borrowers that repay their loans go on to borrow again from the lender, so that none of the interventions changed the overall debt burden of study participants.

Our study highlights the potential of low-cost, light-touch interventions in rehabilitating *some* delinquent borrowers back into the digital credit ecosystem. Indeed, regular reminders substantially increased repayment behaviors in our control group relative to the un-contacted reference group, even when we use a conservative non-response bounding procedure; there are low-hanging fruit out there among the population of long-term delinquent borrowers. While the absolute magnitudes of settlement remain small among borrowers in our study, it is altogether possible that there are other interventions that would be able rehabilitate additional segments of this population. Future work should consider what such interventions could be, and what the effective ceiling is for the share of long-term delinquent borrowers that can be re-financially included. Changing the behavior of strategic defaulters would likely yield substantial benefits for digital credit firms, for example. In addition, further work is needed to understand the dynamic effects of interventions that rehabilitate delinquent borrowers, including long-run repayment behavior and welfare for rehabilitated borrowers, and whether the existence of rehabilitation options dilutes the incentives already in place to repay loans on time.

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A Appendix for online publication

A.1 Supplemental Figures and Tables

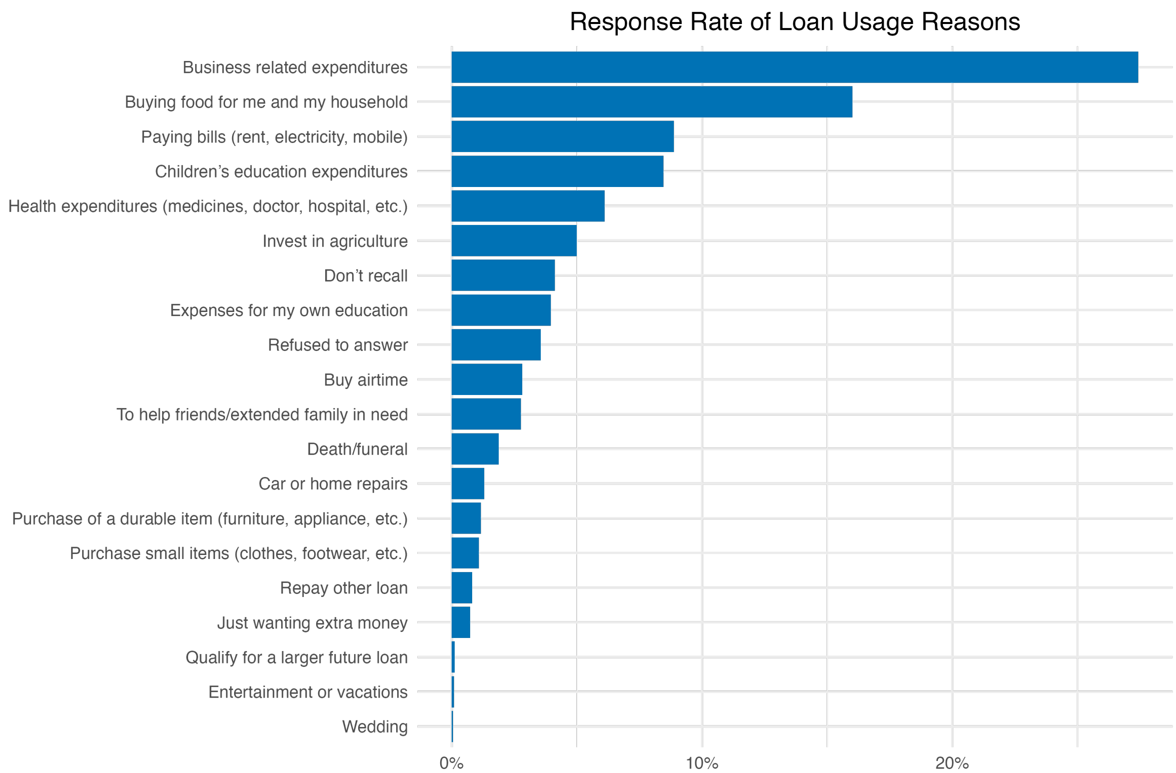

FIGURE A.1: Percent of sample reporting each loan use

Table A.1: Attrition

	Attrition (Percent of responses at endline) (1)
T1: Payment plan	0.0145
	(0.0179)
T2: Eligibility	0.0056
	(0.0181)
T3: Payment plan and eligibility	0.0171
	(0.0178)
Control	0.8099***
	(0.0129)
Observations	3,733

Notes: $^{***} \Rightarrow p < 0.01, ^{**} \Rightarrow p < 0.05, ^{*} \Rightarrow p < 0.10$. Estimates from OLS regressions with heteroskedasticity-robust standard errors shown in parentheses below the estimates.

Table A.2: Heterogeneous effects by liquidity constraint

	Any repayment		Fraction paid (30 days)		Fraction paid (60 days)		Settled loan (30 days)		Settled loan (60 days)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
T1: Payment plan	0.0914***	0.0903***	0.0575***	0.0501***	0.0626***	0.0538***	0.0295**	0.0228	0.0401**	0.0323*
	(0.0260)	(0.0247)	(0.0160)	(0.0153)	(0.0182)	(0.0175)	(0.0143)	(0.0139)	(0.0174)	(0.0169)
T2: Eligibility	0.0203	0.0196	0.0250*	0.0266*	0.0258	0.0284*	0.0275**	0.0293**	0.0227	0.0253
	(0.0240)	(0.0231)	(0.0149)	(0.0147)	(0.0169)	(0.0165)	(0.0139)	(0.0139)	(0.0162)	(0.0161)
T3: Payment plan + eligibility	0.0831***	0.0775***	0.0491***	0.0456***	0.0437**	0.0395**	0.0234*	0.0214	0.0274*	0.0242
	(0.0252)	(0.0240)	(0.0152)	(0.0148)	(0.0171)	(0.0166)	(0.0135)	(0.0133)	(0.0163)	(0.0160)
Control	0.1673***	0.2006***	0.0633***	0.0860***	0.0870***	0.1148***	0.0383***	0.0515***	0.0605***	0.0781***
	(0.0168)	(0.0232)	(0.0096)	(0.0138)	(0.0112)	(0.0162)	(0.0086)	(0.0128)	(0.0107)	(0.0157)
Has $35 in emergency savings	-0.0103	0.0077	0.0216	0.0223	0.0141	0.0126	0.0245*	0.0216	0.0192	0.0138
	(0.0245)	(0.0244)	(0.0159)	(0.0162)	(0.0178)	(0.0181)	(0.0147)	(0.0152)	(0.0171)	(0.0175)
T1 × has savings	0.0081	-0.0137	-0.0227	-0.0239	-0.0094	-0.0107	-0.0234	-0.0211	-0.0201	-0.0186
	(0.0381)	(0.0361)	(0.0247)	(0.0239)	(0.0278)	(0.0268)	(0.0223)	(0.0221)	(0.0263)	(0.0258)
T2 × has savings	0.0082	-0.0034	-0.0210	-0.0280	-0.0135	-0.0226	-0.0201	-0.0246	-0.0097	-0.0157
	(0.0357)	(0.0342)	(0.0236)	(0.0230)	(0.0263)	(0.0255)	(0.0224)	(0.0221)	(0.0255)	(0.0251)
T3 × has savings	-0.0082	-0.0168	-0.0341	-0.0329	-0.0256	-0.0241	-0.0166	-0.0143	-0.0272	-0.0238
	(0.0376)	(0.0358)	(0.0239)	(0.0232)	(0.0264)	(0.0257)	(0.0222)	(0.0218)	(0.0251)	(0.0248)
Observations	3,657	3,657	3,657	3,657	3,657	3,657	3,657	3,657	3,657	3,657
Controls		✓		✓		✓		✓		✓
Batch fixed effects		✓		✓		✓		✓		✓

Notes: $^{***} \Rightarrow p < 0.01, ^{**} \Rightarrow p < 0.05, ^{*} \Rightarrow p < 0.10$. Sample includes all borrowers enrolled in the study at baseline that had not settled their loans prior to the intervention start date. Estimates are from OLS regressions with heteroskedasticity-robust standard errors shown in parentheses below the estimates. Any repayment is an indicator variable for whether the borrower made at least one payment. Fraction paid (30 days) measures the amount of the loan paid as a fraction of the initial overdue amount at the end of the study period. Fraction paid (45 days) measures the amount of the loan paid as a fraction of the initial overdue amount at 60 days from the start of the intervention. Settled (30 days) is a dummy variable equal to one if the loan was fully repaid by the end of the study period. Settled (60 days) is a dummy variable equal to one if the loan was fully repaid by 60 days from the start of the intervention. The reference group is a separate sample of delinquent borrowers who were not included in the study sample for which we have access to administrative data. The reference group row in the table reports the mean outcome among this group. The reference group sample is not included in any of the regressions. Control variables are listed in Footnote 13.

Table A.3: Heterogeneous effects by patience

	Any repayment		Fraction paid (30 days)		Fraction paid (60 days)		Settled loan (30 days)		Settled loan (60 days)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
T1: Payment plan	0.1434***	0.1222***	0.0795***	0.0636***	0.0953***	0.0770***	0.0311	0.0197	0.0512**	0.0370
	(0.0319)	(0.0305)	(0.0216)	(0.0207)	(0.0241)	(0.0232)	(0.0196)	(0.0192)	(0.0232)	(0.0226)
T2: Eligibility	0.0039	-0.0080	0.0093	0.0042	0.0153	0.0114	0.0134	0.0118	0.0120	0.0114
	(0.0279)	(0.0271)	(0.0190)	(0.0186)	(0.0214)	(0.0209)	(0.0182)	(0.0178)	(0.0209)	(0.0205)
T3: Payment plan + eligibility	0.0757**	0.0623**	0.0319	0.0289	0.0287	0.0261	0.0194	0.0187	0.0069	0.0072
	(0.0301)	(0.0291)	(0.0197)	(0.0193)	(0.0216)	(0.0212)	(0.0187)	(0.0184)	(0.0206)	(0.0203)
Control	0.1566***	0.2022***	0.0740***	0.0993***	0.0945***	0.1262***	0.0542***	0.0665***	0.0753***	0.0911***
	(0.0200)	(0.0257)	(0.0132)	(0.0170)	(0.0149)	(0.0191)	(0.0124)	(0.0161)	(0.0145)	(0.0186)
Above median patience	0.0060	-0.0064	-0.0011	-0.0095	-0.0016	-0.0101	-0.0053	-0.0099	-0.0071	-0.0123
	(0.0252)	(0.0244)	(0.0165)	(0.0163)	(0.0185)	(0.0182)	(0.0154)	(0.0153)	(0.0179)	(0.0177)
T1 × patient	-0.0692*	-0.0562	-0.0482*	-0.0370	-0.0542*	-0.0419	-0.0182	-0.0099	-0.0293	-0.0196
	(0.0397)	(0.0378)	(0.0262)	(0.0253)	(0.0295)	(0.0285)	(0.0238)	(0.0235)	(0.0282)	(0.0277)
T2 × patient	0.0426	0.0519	0.0134	0.0187	0.0111	0.0141	0.0068	0.0090	0.0107	0.0111
	(0.0363)	(0.0350)	(0.0243)	(0.0237)	(0.0272)	(0.0265)	(0.0231)	(0.0227)	(0.0264)	(0.0260)
T3 × patient	0.0069	0.0132	0.0008	0.0009	0.0067	0.0046	-0.0098	-0.0101	0.0120	0.0085
	(0.0384)	(0.0369)	(0.0247)	(0.0241)	(0.0273)	(0.0265)	(0.0230)	(0.0227)	(0.0260)	(0.0256)
Observations	3,631	3,631	3,631	3,631	3,631	3,631	3,631	3,631	3,631	3,631
Controls		✓		✓		✓		✓		✓
Batch fixed effects		✓		✓		✓		✓		✓

Notes: $^{***} \Rightarrow p < 0.01$, $^{**} \Rightarrow p < 0.05$, $^{*} \Rightarrow p < 0.10$. Sample includes all borrowers enrolled in the study at baseline that had not settled their loans prior to the intervention start date. Estimates are from OLS regressions with heteroskedasticity-robust standard errors shown in parentheses below the estimates. Any repayment is an indicator variable for whether the borrower made at least one payment. Fraction paid (30 days) measures the amount of the loan paid as a fraction of the initial overdue amount at the end of the study period. Fraction paid (45 days) measures the amount of the loan paid as a fraction of the initial overdue amount at 60 days from the start of the intervention. Settled (30 days) is a dummy variable equal to one if the loan was fully repaid by the end of the study period. Settled (60 days) is a dummy variable equal to one if the loan was fully repaid by 60 days from the start of the intervention. The reference group is a separate sample of delinquent borrowers who were not included in the study sample for which we have access to administrative data. The reference group row in the table reports the mean outcome among this group. The reference group sample is not included in any of the regressions. Control variables are listed in Footnote 13.

Table A.4: Heterogeneous effects by spending discipline

	Any repayment		Fraction paid (30 days)		Fraction paid (60 days)		Settled loan (30 days)		Settled loan (60 days)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
T1: Payment plan	0.0866***	0.0639**	0.0563***	0.0420**	0.0658***	0.0485**	0.0249	0.0151	0.0401*	0.0268
	(0.0318)	(0.0299)	(0.0215)	(0.0208)	(0.0239)	(0.0231)	(0.0194)	(0.0191)	(0.0231)	(0.0227)
T2: Eligibility	-0.0094	-0.0174	0.0001	-0.0030	-0.0036	-0.0070	0.0060	0.0054	-0.0048	-0.0053
	(0.0292)	(0.0276)	(0.0193)	(0.0188)	(0.0213)	(0.0208)	(0.0180)	(0.0181)	(0.0207)	(0.0205)
T3: Payment plan + eligibility	0.0419	0.0358	0.0132	0.0076	0.0113	0.0041	0.0016	-0.0018	-0.0070	-0.0122
	(0.0303)	(0.0287)	(0.0191)	(0.0183)	(0.0212)	(0.0203)	(0.0176)	(0.0171)	(0.0204)	(0.0197)
Control	0.1967***	0.2303***	0.0893***	0.1124***	0.1117***	0.1400***	0.0601***	0.0724***	0.0874***	0.1026***
	(0.0208)	(0.0262)	(0.0136)	(0.0167)	(0.0152)	(0.0189)	(0.0124)	(0.0156)	(0.0148)	(0.0184)
Disciplined with spending	-0.0580**	-0.0471*	-0.0279*	-0.0271*	-0.0320*	-0.0311*	-0.0187	-0.0196	-0.0316*	-0.0323*
	(0.0255)	(0.0243)	(0.0163)	(0.0160)	(0.0184)	(0.0179)	(0.0150)	(0.0149)	(0.0177)	(0.0174)
T1 × disciplined	0.0180	0.0345	-0.0119	-0.0032	-0.0092	0.0016	-0.0073	-0.0016	-0.0118	-0.0034
	(0.0394)	(0.0373)	(0.0259)	(0.0251)	(0.0290)	(0.0280)	(0.0234)	(0.0231)	(0.0277)	(0.0272)
T2 × disciplined	0.0586	0.0629*	0.0272	0.0300	0.0405	0.0434*	0.0217	0.0227	0.0394	0.0400
	(0.0366)	(0.0349)	(0.0240)	(0.0236)	(0.0267)	(0.0262)	(0.0226)	(0.0226)	(0.0259)	(0.0257)
T3 × disciplined	0.0662*	0.0610*	0.0344	0.0380	0.0368	0.0418	0.0228	0.0260	0.0371	0.0421*
	(0.0384)	(0.0365)	(0.0242)	(0.0234)	(0.0268)	(0.0259)	(0.0222)	(0.0219)	(0.0256)	(0.0250)
Observations	3,680	3,680	3,680	3,680	3,680	3,680	3,680	3,680	3,680	3,680
Controls		✓		✓		✓		✓		✓
Batch fixed effects		✓		✓		✓		✓		✓

Notes: $^{***} \Rightarrow p < 0.01, ^{**} \Rightarrow p < 0.05, ^{*} \Rightarrow p < 0.10$. Sample includes all borrowers enrolled in the study at baseline that had not settled their loans prior to the intervention start date. Estimates are from OLS regressions with heteroskedasticity-robust standard errors shown in parentheses below the estimates. Any repayment is an indicator variable for whether the borrower made at least one payment. Fraction paid (30 days) measures the amount of the loan paid as a fraction of the initial overdue amount at the end of the study period. Fraction paid (45 days) measures the amount of the loan paid as a fraction of the initial overdue amount at 60 days from the start of the intervention. Settled (30 days) is a dummy variable equal to one if the loan was fully repaid by the end of the study period. Settled (60 days) is a dummy variable equal to one if the loan was fully repaid by 60 days from the start of the intervention. The reference group is a separate sample of delinquent borrowers who were not included in the study sample for which we have access to administrative data. The reference group row in the table reports the mean outcome among this group. The reference group sample is not included in any of the regressions. Control variables are listed in Footnote 13.

Table A.5: Heterogeneous effects by risk aversion

	Any repayment		Fraction paid (30 days)		Fraction paid (60 days)		Settled loan (30 days)		Settled loan (60 days)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
T1: Payment plan	0.0963***	0.0834***	0.0666***	0.0602***	0.0671***	0.0593***	0.0362**	0.0327**	0.0451**	0.0401**
	(0.0279)	(0.0262)	(0.0179)	(0.0171)	(0.0200)	(0.0190)	(0.0163)	(0.0159)	(0.0190)	(0.0184)
T2: Eligibility	0.0149	0.0074	0.0167	0.0166	0.0193	0.0196	0.0176	0.0189	0.0227	0.0250
	(0.0262)	(0.0244)	(0.0162)	(0.0159)	(0.0186)	(0.0181)	(0.0153)	(0.0153)	(0.0180)	(0.0178)
T3: Payment plan + eligibility	0.0986***	0.0876***	0.0464***	0.0437***	0.0427**	0.0387**	0.0293*	0.0283*	0.0266	0.0249
	(0.0279)	(0.0265)	(0.0169)	(0.0163)	(0.0190)	(0.0181)	(0.0159)	(0.0155)	(0.0180)	(0.0174)
Control	0.1659***	0.2050***	0.0649***	0.0837***	0.0882***	0.1132***	0.0427***	0.0507***	0.0616***	0.0727***
	(0.0181)	(0.0241)	(0.0106)	(0.0144)	(0.0123)	(0.0165)	(0.0098)	(0.0135)	(0.0117)	(0.0159)
Risk averse	-0.0083	-0.0060	0.0182	0.0215	0.0127	0.0164	0.0162	0.0177	0.0182	0.0215
	(0.0247)	(0.0236)	(0.0158)	(0.0156)	(0.0178)	(0.0175)	(0.0146)	(0.0146)	(0.0171)	(0.0169)
T1 × risk averse	-0.0012	0.0016	-0.0377	-0.0412*	-0.0174	-0.0213	-0.0324	-0.0370*	-0.0256	-0.0309
	(0.0383)	(0.0364)	(0.0248)	(0.0239)	(0.0280)	(0.0269)	(0.0226)	(0.0222)	(0.0266)	(0.0260)
T2 × risk averse	0.0213	0.0249	-0.0083	-0.0104	-0.0042	-0.0070	-0.0056	-0.0078	-0.0151	-0.0194
	(0.0359)	(0.0344)	(0.0233)	(0.0230)	(0.0263)	(0.0256)	(0.0221)	(0.0220)	(0.0254)	(0.0251)
T3 × risk averse	-0.0386	-0.0357	-0.0347	-0.0355	-0.0277	-0.0280	-0.0358*	-0.0362*	-0.0290	-0.0300
	(0.0377)	(0.0359)	(0.0236)	(0.0230)	(0.0263)	(0.0254)	(0.0217)	(0.0215)	(0.0251)	(0.0245)
Observations	3,596	3,596	3,596	3,596	3,596	3,596	3,596	3,596	3,596	3,596
Controls		✓		✓		✓		✓		✓
Batch fixed effects		✓		✓		✓		✓		✓

Notes: $^{***} \Rightarrow p < 0.01,^{**} \Rightarrow p < 0.05,^{*} \Rightarrow p < 0.10$. Sample includes all borrowers enrolled in the study at baseline that had not settled their loans prior to the intervention start date. Estimates are from OLS regressions with heteroskedasticity-robust standard errors shown in parentheses below the estimates. Any repayment is an indicator variable for whether the borrower made at least one payment. Fraction paid (30 days) measures the amount of the loan paid as a fraction of the initial overdue amount at the end of the study period. Fraction paid (45 days) measures the amount of the loan paid as a fraction of the initial overdue amount at 60 days from the start of the intervention. Settled (30 days) is a dummy variable equal to one if the loan was fully repaid by the end of the study period. Settled (60 days) is a dummy variable equal to one if the loan was fully repaid by 60 days from the start of the intervention. The reference group is a separate sample of delinquent borrowers who were not included in the study sample for which we have access to administrative data. The reference group row in the table reports the mean outcome among this group. The reference group sample is not included in any of the regressions. Control variables are listed in Footnote 13.

Table A.6: Heterogeneous effects by cognitive reasoning

	Any repayment		Fraction paid (30 days)		Fraction paid (60 days)		Settled loan (30 days)		Settled loan (60 days)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
T1: Payment plan	0.1068***	0.0912***	0.0585***	0.0469***	0.0777***	0.0641***	0.0235*	0.0155	0.0467***	0.0360**
	(0.0225)	(0.0212)	(0.0143)	(0.0137)	(0.0164)	(0.0157)	(0.0129)	(0.0126)	(0.0155)	(0.0152)
T2: Eligibility	0.0392*	0.0363*	0.0228*	0.0241*	0.0210	0.0227	0.0214*	0.0234*	0.0159	0.0182
	(0.0210)	(0.0199)	(0.0134)	(0.0130)	(0.0147)	(0.0142)	(0.0126)	(0.0125)	(0.0141)	(0.0139)
T3: Payment plan and eligibility	0.0938***	0.0807***	0.0370***	0.0333**	0.0344**	0.0296**	0.0148	0.0137	0.0162	0.0136
	(0.0224)	(0.0210)	(0.0137)	(0.0132)	(0.0151)	(0.0144)	(0.0124)	(0.0122)	(0.0143)	(0.0139)
Control	0.1605***	0.2032***	0.0672***	0.0892***	0.0864***	0.1116***	0.0457***	0.0566***	0.0633***	0.0751***
	(0.0141)	(0.0219)	(0.0087)	(0.0137)	(0.0098)	(0.0155)	(0.0080)	(0.0128)	(0.0094)	(0.0149)
Higher reasoning score	0.0101	0.0178	0.0205	0.0275	0.0275	0.0348	0.0112	0.0152	0.0220	0.0264
	(0.0295)	(0.0290)	(0.0196)	(0.0198)	(0.0223)	(0.0223)	(0.0179)	(0.0181)	(0.0214)	(0.0216)
T1 × higher score	-0.0212	-0.0041	-0.0193	-0.0075	-0.0490	-0.0347	0.0022	0.0105	-0.0370	-0.0250
	(0.0444)	(0.0429)	(0.0297)	(0.0290)	(0.0327)	(0.0319)	(0.0271)	(0.0270)	(0.0311)	(0.0307)
T2 × higher score	-0.0402	-0.0496	-0.0124	-0.0241	0.0093	-0.0037	0.0000	-0.0095	0.0248	0.0148
	(0.0416)	(0.0407)	(0.0288)	(0.0286)	(0.0332)	(0.0328)	(0.0270)	(0.0271)	(0.0324)	(0.0323)
T3 × higher score	-0.0520	-0.0418	-0.0234	-0.0228	-0.0189	-0.0171	-0.0057	-0.0065	-0.0136	-0.0116
	(0.0423)	(0.0407)	(0.0274)	(0.0267)	(0.0310)	(0.0301)	(0.0252)	(0.0249)	(0.0295)	(0.0290)
Observations	3,556	3,556	3,556	3,556	3,556	3,556	3,556	3,556	3,556	3,556
Controls		✓		✓		✓		✓		✓
Batch fixed effects		✓		✓		✓		✓		✓

Notes: $^{***} \Rightarrow p < 0.01, ^{**} \Rightarrow p < 0.05, ^{*} \Rightarrow p < 0.10$. Sample includes all borrowers enrolled in the study at baseline that had not settled their loans prior to the intervention start date. Estimates are from OLS regressions with heteroskedasticity-robust standard errors shown in parentheses below the estimates. Any repayment is an indicator variable for whether the borrower made at least one payment. Fraction paid (30 days) measures the amount of the loan paid as a fraction of the initial overdue amount at the end of the study period. Fraction paid (45 days) measures the amount of the loan paid as a fraction of the initial overdue amount at 60 days from the start of the intervention. Settled (30 days) is a dummy variable equal to one if the loan was fully repaid by the end of the study period. Settled (60 days) is a dummy variable equal to one if the loan was fully repaid by 60 days from the start of the intervention. The reference group is a separate sample of delinquent borrowers who were not included in the study sample for which we have access to administrative data. The reference group row in the table reports the mean outcome among this group. The reference group sample is not included in any of the regressions. Control variables are listed in Footnote 13.

Table A.7: Bounding the treatment effect sizes relative to reminder effect

	Any repayment	Settled loan (30 days)
Panel A: Point estimates from OLS estimates in Table 2		
T1: Payment plan	9.32 p.p.	1.96 p.p
T2: Eligibility	2.67 p.p	1.82 p.p
T3: Payment plan + eligibil.	7.99 p.p.	1.43 p.p
Control mean	16.22%	4.94%
Panel B: Repayment rates by treatment		
T1: Payment plan	$16.22 + 9.32 = 25.54\%$	$4.94 + 1.96 = 6.90\%$
T2: Eligibility	$16.22 + 2.67 = 18.89\%$	$4.94 + 1.82 = 6.76\%$
T3: Payment plan + eligibil.	$16.22 + 7.99 = 24.21\%$	$4.94 + 1.43 = 6.37\%$
Control	16.22%	4.94%
Panel C: Repayment rates adjusted for non-response (lower bound)		
Fraction of non-participants (non-response rate): 0.45		
Any payment among non-participants: 3.2%		
30-day settlement among non-participants: 3.1%		
T1: Payment plan	$25.54 \cdot 0.55 + 3.2 \cdot 0.45 = 15.49\%$	$6.90 \cdot 0.55 + 3.1 \cdot 0.45 = 5.19\%$
T2: Eligibility	$18.89 \cdot 0.55 + 3.2 \cdot 0.45 = 11.83\%$	$6.76 \cdot 0.55 + 3.1 \cdot 0.45 = 5.11\%$
T3: Payment plan + eligibil.	$24.21 \cdot 0.55 + 3.2 \cdot 0.45 = 14.76\%$	$6.37 \cdot 0.55 + 3.1 \cdot 0.45 = 4.90\%$
Control	$16.22 \cdot 0.55 + 3.2 \cdot 0.45 = 10.36\%$	$4.94 \cdot 0.55 + 3.1 \cdot 0.45 = 4.11\%$
Panel D: Comparison with reference group		
Any payment among reference group: 4.10%		
30-day settlement among reference group: 1.70%		
T1: Payment plan	$15.49 - 4.10 = 11.39$ p.p.	$5.19 - 1.70 = 3.49$ p.p.
T2: Eligibility	$11.83 - 4.10 = 7.73$ p.p.	$5.11 - 1.70 = 3.41$ p.p.
T3: Payment plan + eligibil.	$14.76 - 4.10 = 10.66$ p.p.	$4.90 - 1.70 = 3.20$ p.p.
Control	$10.36 - 4.10 = 6.26$ p.p.	$4.11 - 1.70 = 2.41$ p.p.
Panel E: Netting out the effect of reminders (as % of total treatment effect)		
T1: Payment plan	$(11.39 - 6.26)/11.39 = 43.11\%$	$(3.49 - 2.41)/3.49 = 30.95\%$
T2: Eligibility	$(7.73 - 6.26)/7.73 = 19.01\%$	$(3.41 - 2.41)/3.41 = 29.41\%$
T3: Payment plan + eligibil.	$(10.66 - 6.26)/10.66 = 41.23\%$	$(3.20 - 2.41)/3.20 = 24.64\%$

A.2 Consumer Well-Being Variables

In Table 5, we use survey variables and create two index variables –a "stress index" and an "overall index"– to help capture the effect of treatment on consumer's well-being at the time of our endline survey. Here we describe the underlying survey questions and construction of these measures.

A.2.1 Borrowing difficulty

Difficulty borrowing is measured using the following question from the survey:

- $Q2_{21}$: *"On a scale from 1 to 10, where 1 is very easy and 10 is very difficult, if tomorrow you needed a loan of [roughly $7.50], how difficult would it be to get that loan from any source?"*

A.2.2 Financial insecurity

Financial insecurity is measured using -1 times a subject's response to the following question from the survey:

- $Q4_1$: *"On a scale of 1 to 5, where 1 is very insecure and 5 is very secure, how secure do you feel with the financial situation of you household today?"*

A.2.3 Overall debt

We convert to USD and winsorize the responses to the following question at the 95th percentile to measure overall debt:

- $Q2_{20}$: *"Roughly, what is the total amount of [your] outstanding loans?"*

A.2.4 Stress index

To create the stress index we rely on two questions from the survey:

- $Q5_1$: *"In the past 7 days, how often have you felt nervous and stressed?"*
- $Q5_2$: *"In the past 7 days, how often have you felt difficulties were piling up so high that you could not overcome them?"*

Individuals responded using a Likert scale from 1 to 5. We standardize each variable, add them up, and standardize again to create the stress index.

A.2.5 Overall index

To create the overall index we standardize the difficulty borrowing, financial insecurity, and overall debt variables, we then sum these standardized measures and the stress index (which is already standardized), and then standardize again to create the overall index.