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One Size Does Not Fit All: Contract Design in FinTech Lending

Jianfeng Hu
Singapore Management University

Gloria Yang Yu
University of Florida

Changcheng Song
University of Florida

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Jianfeng Hu,† Changcheng Song,‡ Gloria Yang Yu§

Abstract

Many fintech lenders rely on standardized contract terms to support scale and operational speed. This paper examines whether modest tailoring of loan contracts can improve credit market outcomes. We conduct a randomized field experiment with a large fintech lender that varies loan due dates relative to borrowers' salary paydays. Synchronizing repayment schedules with income cycles reduces delinquency by 29.1% in the experiment and 15.7% in the administrative data. Effects concentrate among liquidity-constrained borrowers: young, low-income, and low-credit-limit individuals. Tailored repayment timing generates substantial economic benefits: borrowers save on overdue penalties, lenders accelerate cash flows, and improved repayment increases future credit access. The benefits persist over time and strengthen with borrower experience. Overall, the findings show that contract tailoring can enhance fintech's contribution to financial inclusion.

Keywords: FinTech, Contract Design, Payday, Household Finance, Financial Inclusion,

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† jianfenghu@smu.edu.sg; Singapore Management University, 50 Stamford Road, Singapore 178899, Singapore

‡ changcheng.song@warrington.ufl.edu;University of Florida, 301D Stuzin Hall, Gainesville, FL 32611, USA

§ gloriayu@smu.edu.sg; Singapore Management University, 50 Stamford Road, Singapore 178899, Singapore

1 Introduction

Fintech lending has expanded rapidly in consumer credit markets, enabled by automated underwriting technologies that support fast and low-cost origination at scale (Fuster, Plosser, Schnabl, and Vickery, 2019; Berg, Fuster, and Puri, 2022). Repayment performance nonetheless remains a central challenge, with delinquencies imposing financial costs, emotional stress, and credit access restrictions for borrowers. An extensive literature examines screening and monitoring mechanisms (Hertzberg, Liberman, and Paravisini, 2018; Berg, Burg, Gombović, and Puri, 2020), but comparatively little is known about how contract design influences repayment behaviour. Theoretical work suggests that optimal credit contracts should be state-contingent, aligning debt obligations with borrowers' ability to repay (Albuquerque and Hopenhayn, 2004; Piskorski and Tchistyi, 2010). Yet, driven by the need for speed and scalability, many fintech lenders rely on standardized, calendar-based contracts that do not tailor to borrowers' heterogeneous liquidity cycles.[1]

This paper is the first to estimate the causal impact of contract tailoring in a fintech setting.[2] We examine whether synchronizing repayment schedules with borrowers' salary pay cycles, a simple form of tailoring compatible with automation, can reduce delinquency. We investigate this question in Indonesia, a setting characterized by prevalent repayment difficulties and commonly binding liquidity constraints (Bursztyn, Fiorin, Gottlieb, and Kanz, 2019). Our study demonstrates that modestly adapting contract terms to borrower profiles meaningfully improves repayment performance and future credit access. By addressing frictions stemming from the mismatch between income cycles and contractual liabilities, small deviations from uniform contract design can generate substantial gains. The findings offer a scalable solution for fintech lenders to enhance financial inclusion without compromising automation efficiency.

[1] Berg, Fuster, and Puri (2022) note that "an increase in convenience and speed appears to have been more central to fintech lending's growth than improved screening or monitoring."

[2] Related works examine tailored covenants in corporate lending (Matvos, 2013; Ganglmair and Wardlaw, 2018), individualized credit limits in credit cards (Matcham, 2025), flexible repayment structures in microfinance (Battaglia, Gulesci, and Madestam, 2024; Barboni and Agarwal, 2023), and repayment timing flexibility in mortgage lending (Garmaise, 2013).

Empirical research on contract tailoring faces two hurdles. First, researchers rarely observe the cross-sectional variation needed to study contract design. Although fintech lenders possess granular borrower-level data that could, in principle, support tailored contracts, organizational frictions often sustain uniform contracts rather than cash-flow-sensitive designs. Lenders that adopt standardized products to scale rapidly in their early stages subsequently face inertia when updating underwriting systems, and may encounter coordination frictions with funding partners.[3] Second, identification is challenging because contract terms are not randomly assigned. Observed relationships between contract features and repayment may reflect selection on borrower characteristics or other unobservables. Borrowers' endogenous sorting into contract terms based on private information further complicates identification (Garmaise, 2013; Vihriälä, 2023; Barboni and Agarwal, 2023). Thus, administrative data alone offer limited leverage for recovering the causal treatment effects of contract design.

In collaboration with a leading digital lender, we overcome both challenges by combining a field experiment that randomizes contract terms and large administrative data to explore the potential benefits of integrating tailored contracts into fintech lending models. We focus on borrowers' salary paydays relative to loan maturity. Borrowers' paydays are heterogeneous and paychecks represent substantial liquidity (Olafsson and Pagel, 2018), making payday an obvious anchor for tailoring loan terms to individual cash flow cycles. Drawing on extensive empirical evidence that salary payments improve borrowers' liquidity and the established practice of payday lending, we hypothesize that aligning loan due dates with salary paydays will improve repayment rates.[4]

In the field experiment, we randomly vary loan due dates to create *payday alignment* defined as a borrower's payday preceding the due date. We only sample approved loans to control for the potential selection effect due to different loan approval rates across experiment groups. We treat

[3] For example, many fintech lenders work with bank partners whose program agreements restrict modifications to loan terms without prior written consent.

[4] While payday loans may facilitate consumption smoothing, critics highlight risks of over-borrowing and debt cycles. Gathergood, Guttman-Kenney, and Hunt (2019) find that payday loans provide short-term liquidity but increase future default risk.

randomly drawn borrowers by extending their loan maturity at no cost so that the loan due date falls after a positive liquidity shock from paychecks to the borrower. For example, consider borrowers whose newly approved loans mature one day before the salary payday. These borrowers belong to the Match group. Before they are notified about loan terms, we randomly select one third of the borrowers and assign a two-day loan extension with no additional interest cost. In this case, these treated loans mature one day after the salary is disbursed. Since the loan extensions incur no additional interest costs and align the loan due date with the salary payday, the treatment effects on loan repayment include both the liquidity effect due to salary *payday alignment* and the income effect due to a longer tenor.

To sharpen our identification and address the concern that a longer tenor itself may improve repayment (Carter, Liu, Skiba, and Sydnor, 2022), we include placebo subjects that also receive loan extensions but do not experience liquidity shocks from payday because of the extension. Such borrowers whose salary payday is far away from loan due date belong to the Placebo group.[5] Because the loan extension in the Placebo group only captures the income effect, the treatment effect difference between the Match and Placebo groups captures the liquidity effect on loan outcomes, defined as the *payday alignment* effect.

We conduct the experiment from April 2021 to October 2021 with 2809 subjects in total. We find strong empirical support for our conjecture that *payday alignment* increases the cash adequacy and thus increases borrowers' repayment propensity. The difference-in-difference (DiD) estimators suggest that *payday alignment* reduces the likelihood of loans being overdue by 5.9 percentage points. Such effect is economically large. Given the overdue rate of 20.3% in the control group, *payday alignment* reduces the overdue propensity by 29.1%.

Next, we estimate the *payday alignment* effect on the timing of repayments across the entire repayment distribution using a hazard model. Across all repayment dates (relative to maturity), loans maturing after the borrower's salary payout date on average are 36% more likely to receive

[5] We formally define the Match and Placebo groups in Section 3.1.

repayment. Hence, synchronizing loan due dates with salary paydays not only significantly reduces the overdue rate but also advances borrowers' repayments in general. This result is consistent with the mechanism that anticipated or realized salary income prior to due dates relaxes borrowers' effective liquidity constraints, prompting earlier repayment.

Our further analysis of the experiment uncovers rich heterogeneous effects of *payday alignment* with respect to borrower characteristics. Consistent with our hypothesis that *payday alignment* increases borrowers' cash adequacy, the effect mentioned above is stronger for young, low-income, low-credit-limit borrowers, and for borrowers having more late repayment records in the past. The results indicate that a borrower's liquidity shock has the strongest effect on the likelihood of loan repayment for these underserved borrowers who need liquidity and credit the most.

After establishing the causal impact of contract tailoring on loan delinquency using the field experiment, we further explore more than 1 million loans in the lender's full loan book from 2018 to 2020 to deepen the insights. Our analyses include all borrowers with regular monthly salary payments taking out 28-day cash loans. Using stringent specifications that account for time trends and borrower heterogeneity, we find that *payday alignment* reduces the likelihood of delinquency by 4.1–4.9 percentage points, equivalent to a 15.7%–18.7% reduction relative to the delinquency rate in the sample. Payday-aligned loans also incur Rp 5,787–Rp 12,515 less overdue interest and fees per loan, which is a 5.2%–11.3% reduction relative to the average charges.

Although reduced overdue interest and fees seemingly imply income loss for lenders, *payday alignment* generates other important benefits for the lender. We show that *payday alignment* accelerates cash inflows by approximately Rp 5,163 million (US$366,733) per month for the 28-day loan product, equivalent to 4.8% of monthly loan maturities and 5.6% of monthly disbursements.[6] More timely and predictable repayment patterns are also consistent with easing liquidity management, shortening the capital cycle, and reducing the debt collection costs for lenders.[7]

[6] For currency conversion, we apply the sample average of 14,080 for USD/IDR.

[7] For example, more predictable cash inflows may reduce the need for large liquidity buffers and allow engineering and risk-management teams to reallocate effort to other productive activities. In practice, lenders devote substantial

The breadth of administrative data enables more comprehensive analysis of estimating the effect on infrequent outcomes such as default. We find that *payday alignment* significantly lowers the likelihood of default by 0.3–1 percentage points, or 2.6%–8.6% relative to the unconditional default rate in the sample. The effect on default is smaller than that on delinquency because *payday alignment* enhances borrowers' ability to repay but does not affect the willingness to pay, strategic default propensity, or fraud risk.[8]

Payday alignment also yields important long-term effects. Our Two-Stage Least Squares (2SLS) estimates indicate that avoiding delinquency records through *payday alignment* increases a borrower's subsequent loan approval rate by 0.56–1.04 percentage points, or 1.1%–2% relative to the unconditional approval rate. In other words, if borrowers are allowed to align the loan maturity date with their payday, their future access to credit also improves due to better performance of the current loan. This is particularly important in financial inclusion for borrowers with a thin credit history. Back-of-the-envelope estimates indicate that the effect corresponds to roughly 0.45–0.83 million additional loans approved and 0.15–0.29 million new borrowers granted access to credit annually in Indonesia if the *payday alignment* feature were employed by all fintech lenders and the effects we estimate persist across lenders over the sample period.

We uncover heterogeneous effects of *payday alignment* over salary cycles. When we split the positive maturity-payday gap into close (1–3 days), medium (4–7 days), and distant (8+ days) bands, *payday alignment* yields a 22.5% drop in overdue rates in the close band, closely matching our experimental estimate, yet the effect steadily attenuates as the gap widens. These results underscore that proximity to payday is key for easing short-term cash constraints.

The administrative data further allow us to study first-time borrowing excluded from the exper-

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resources to debt collection and to developing new collection technologies (Drozd and Serrano-Padial, 2017; Zhou, 2024; Choi, Huang, Yang, and Zhang, 2025). Timelier repayment also implies savings in debt collection costs and lower unobserved legal and reputational risks associated with delinquency management. These implications are suggestive and quantifying the full lender-side gains lies beyond the scope of this paper.

[8] Balyuk and Davydenko (2024) highlight the prevalence of fraud and borrower misreporting in fintech lending. Examples of studies on strategic default in the mortgage context include Guiso, Sapienza, and Zingales (2013) and Gerardi, Herkenhoff, Ohanian, and Willen (2018).

iment but critical for financial inclusion. Brune, Giné, and Karlan (2022) emphasize that first-time borrowing differ significantly from repeat borrowing in repayment behavior. We find *payday alignment* also significantly reduces delinquency rates among first-time borrowing. However, the effect size is smaller than that for repeat borrowing, potentially due to limited debt management experience and unfamiliarity with repayment procedures.

We then examine how the effect of *payday alignment* evolves as borrowers' experiences of such alignment accrue and how it may interact with past borrowing experiences. We find that the *payday alignment* effect strengthens when borrowers experience a higher share of such loans in the past. We also find stronger effects for borrowers with more experience in loan disbursements and loan applications, consistent with these groups accumulating greater familiarity with loan management over time. This evidence suggests that the benefit of contract tailoring is not transitory, but rather persists and gets reinforced through borrowers' experiences in lending.

Consistent with the findings of the experiment, the *payday alignment* effect also intensifies for borrowers with a history of overdue loans. Furthermore, the reduction in delinquency rates is more pronounced among young, low-income, and low-credit-limit borrowers. These heterogeneous effects paint a clear picture: aligning due dates with paydays meaningfully boosts cash adequacy and repayment performance, particularly for those who need it most.

We interpret the experimental and administrative patterns through a liquidity constraint mechanism. Alternative explanations, such as salience and present bias, could matter but cannot explain all the observed patterns. First, while payday receipts may be salient events acting as reminders that aid repayment (Cadena and Schoar, 2011), the lender in our setting issues standard, daily reminder notifications to all borrowers before the due date, making reminder exposure invariant to *payday alignment*. Second, *payday alignment* may propel present-biased borrowers to clear pressing debt obligations immediately using salary funds rather than delay consumption reductions that go toward repaying debt (O'donoghue and Rabin, 1999; Kuchler and Pagel, 2021). This mechanism would predict repayment bunching on the due date, and reversed effects when the due date is well after payday. However, we observe the opposite empirically.

While precise magnitudes may vary beyond our setting of Indonesian regularly paid workers, the underlying mechanism has broad implications. Lender-driven tailoring of repayment timing meaningfully improves loan performance in settings where intra-month liquidity swings clash with rigid, standardized loan contracts. This approach differs fundamentally from borrower-initiated rescheduling that is often restrictive and limited by borrower awareness. Furthermore, it is unlikely that borrowers can strategically time applications to engineer synchronization between their loan obligation and income cycle, as the lender's algorithms incorporate discretionary rules that render the final maturity date opaque and unpredictable to borrowers. Finally, this automated tailoring avoids regulatory concerns, such as the prohibition on wage assignments or the use of salary as collateral often associated with payday lending.

This paper speaks to several strands of literature. First, we contribute to the literature on information asymmetries in the credit market (Karlan and Zinman, 2009; Adams, Einav, and Levin, 2009; Dobbie and Skiba, 2013). To overcome adverse selection, existing literature focuses on screening mechanisms using the borrower information and choice (Hertzberg, Liberman, and Paravisini, 2018; Cespedes, 2019; Berg, Burg, Gombović, and Puri, 2020). To overcome moral hazard, existing literature studies dynamic incentives (Karlan and Zinman, 2009; Giné, Goldberg, and Yang, 2012), peer enforcement (Bryan, Karlan, and Zinman, 2015), and information provision (Cadena and Schoar, 2011; Karlan, Morten, and Zinman, 2016; Du, Li, Lu, and Lu, 2020). Instead of using borrower information for screening, we embed it in the loan contract to set terms that prompt timely repayment. We show novel evidence that tailored contract design effectively improves borrower repayment ability.

Second, we contribute to the literature on repayment flexibility. Carter, Liu, Skiba, and Sydnor (2022) show that payday borrowers suboptimally use grace periods due to heuristic decision-making. A series of studies show that repayment flexibility in microfinance contracts raise investment (Field, Pande, Papp, and Rigol, 2013; Brune, Giné, and Karlan, 2022; Battaglia, Gulesci, and Madestam, 2024). Barboni and Agarwal (2023) demonstrate that flexible microfinance contracts improve business outcomes without harming repayment through the selection of sophisticated borrowers. Garmaise (2013) show that repayment flexibility increases mortgage volume and highlight the interplay between ex ante adverse selection effects and the bank's ex post sorting of stronger borrowers. Our setting is conceptually different. Rather than allowing borrowers to opt into contract flexibility, we randomly assign due dates to align with salary paydays after loan approvals, ensuring full take-up and eliminating endogenous sorting into contract types. This design enables clean identification of the treatment effect of contract terms.

Third, we add to the growing fintech lending literature (e.g., Buchak, Matvos, Piskorski, and Seru 2018; Tang 2019; Di Maggio and Yao 2021), particularly the debate on fintech's distributional effects in credit markets. Prior work documents mixed effects: digital footprints can expand access where bureau data are limited (Berg, Burg, Gombović, and Puri, 2020) and automation may reduce racial disparities (Howell, Kuchler, Snitkof et al., 2024), yet algorithmic flexibility can widen demographic gaps (Fuster, Goldsmith-Pinkham, Ramadorai, and Walther, 2022) and faster processing does not necessarily benefit thin-file borrowers (Fuster, Plosser, Schnabl, and Vickery, 2019). Our results highlight a nuanced channel of contract design. When fintech is paired with tailored contract design that addresses borrower constraints, the gains are especially pronounced for liquidity-constrained, thin-credit borrowers, thereby mitigating disparities in credit performance.

Finally, we add to the literature on payday effects in household finance decisions (Huffman and Barenstein, 2005; Mastrobuoni and Weinberg, 2009; Skiba, 2014; Leary and Wang, 2016; Baugh and Correia, 2022). Olafsson and Pagel (2018) study the payday effects on consumption and show significant spending responses to the arrival of both regular and irregular income. Bertrand and Morse (2009) find a persistent decline in payday borrowing following the receipt of a tax rebate. We complement this literature by examining payday effects on fintech loan repayment, showing how incorporating payday information into loan design can improve performance through financial innovation.

2 Institutional Background

Indonesia. In 2019, Indonesia's financial inclusion index was ranked 64th out of 153 economies (Park and Mercado, 2021). Traditional banks heavily rely on credit scores and collateral, which underserve low-income individuals and those with thin credit histories. This gap in financial access has created a pressing need for innovative solutions to expand credit availability to underserved populations.

The Indonesia Financial Services Authority (OJK) has promoted fintech adoption to improve the financial system efficiency and broaden access to financial services. The volume of digital lending has increased significantly, reaching 76 million borrowers in recent years (Asian Development Bank, 2024). Fintech lenders leverage mobile technology, big data analytics, and alternative credit scoring methods—such as behavioral data or smartphone metadata—to assess borrower risk and disburse loans quickly and remotely.

Indonesia's demographic structure presents a compelling case for fintech lending expansion, particularly due to its large, youthful, and digitally connected population. With a population nearing 278 million and a median age around 30, Indonesia is a workforce-dominated economy with rising financial needs and limited formal credit access among younger cohorts. A key feature shaping digital credit adoption is the rapid digitization of the population. As of 2023, over 220 million Indonesians—approximately 79% of the population—are active internet users, and smartphone penetration exceeds 75%.

Lender. Our cooperating lender (*Lender*, hereafter) is a leading digital lending company that operates in most Asean countries as well as India, Hong Kong, and Taiwan, with business lines spanning credit risk analysis and alternative credit provision. The *Lender's* retail business originated in Indonesia, where the loan book is still the largest within the group.

Lender is registered and supervised by the OJK. Its mobile application is one of Indonesia's most popular financial services apps with over 20 million downloads, 1.4 million reviews and a

4.8-star rating as of July 2021. The *Lender* has disbursed loans exceeding Rp 16 trillion since its establishment in 2017. As of July 2021, the active loan book has Rp 1.2 trillion outstanding with over 700,000 active users. The loan size ranges between US$50 and US$200 with fixed maturities of two weeks, four weeks, three months, and six months. All loans are unsecured, with daily interest rates beginning at 0.1% depending on the product selected.[9]

A distinctive feature of the Indonesian credit market is the absence of a nationwide credit agency covering the majority of the population. The standardized consumer score from public registry and private credit bureau effectively became available only around 2016. Therefore, it is hard to screen customers based on credit histories. Furthermore, personal penalties in the case of default are difficult to enforce. Compared to lenders in developed markets, our partner *Lender* faces heightened fraud risk and loss given default. Applicants are required to provide personal information including the national ID, bank account details, education, employment, home address, and purpose of the loan. The *Lender* employs an internal risk assessment system that flags potential fraudulent applicants and calculates an internal credit score. Only borrowers surpassing a minimum score threshold have their applications approved.

Borrowers. To be eligible for loan applications with *Lender*, borrowers must be Indonesian citizens between 18-60 years of age, possess a valid Indonesian ID card, maintain a bank account linked to their Indonesian ID card, and have established residency records in Indonesia. Stated loan purposes, ranked by frequency, include working capital, livelihood needs, education, healthcare, home renovation, consumption, and holiday travel. We provide more details about borrowers in our sample and the loan application process in Section 3.

[9] Interest rates are fixed at the product level rather than individualized through risk-based pricing (common in U.S. consumer lending markets).

3 Experiment

3.1 Experimental Design

We conduct a randomized field experiment across seven months from April to October 2021, encompassing 2,809 loans and distinct borrowers. We focus on repeat borrowers with regular paychecks. Borrowers' paydays vary considerably, most commonly falling on the 5th, 10th, 25th, or 28th of each month. Since most Indonesian workers receive monthly salaries, we exclusively examine loans with 28-day tenors issued to monthly paid workers to align repayment cycles with paycheck cycles.

Figure A1 illustrates the experimental timeline. Borrowers submit applications through the lender's mobile application, which requires only a valid ID card and a linked bank account—no collateral is necessary. The *Lender* processes applications with an automated pipeline that yields a median decision time of about 18 minutes, although decision time varies from instantly to as long as 12 hours in the experimental sample.

On the experiment day, we sample approved loans with reported salary paydays that are ready for fund disbursements and randomly extend loan tenors by one or two days at no additional interest cost. Borrowers then receive in-app notifications confirming loan approval along with loan details such as loan size, interest rate, tenor, and due date. Funds are then disbursed directly to borrowers' registered bank accounts within minutes of receiving approval. Borrowers can repay through multiple channels: bank transfer, e-wallet, or cash/QR code payments at convenience stores partnered with *Lender*.[10] After loans become due, *Lender* shares loan repayment data with us and we analyze the impact of our intervention on various loan performance metrics. For overdue loans, *Lender* conducts standard collection activities unaffected by our experiment: in-app notifications, text messages, emails, mails, phone calls, and in-person visits.

[10] Payments are typically processed and verified immediately, with borrowers receiving in-app confirmation. Occasional processing delays may occur, but payments are reflected within 24 hours.

Because randomization occurs only after loan approvals, key determinants of the credit decision such as borrower type and credit risk are held fixed, allowing for a cleaner identification of the *payday alignment* treatment effect. We define *Reference* dates corresponding to common Indonesian salary paydays: the 5th, 10th, 25th, and 28th of each month (adjusted to the preceding business day when these fall on weekends or holidays).[11] The experimental intervention is applied to approved loans whose original maturity dates fall 0–3 days before each *Reference* date. For example, if the *Reference* date is May 5, we sample 28-day loans approved between April 4 and April 7 whose scheduled maturity dates fall on May 2, 3, 4, and 5 respectively.

Loan extensions affect repayment through two channels: liquidity effects from *payday alignment* and income effects from cost-free extensions. To distinguish these mechanisms, we split approved loans with the same disbursement date into two groups based on borrowers' paydays relative to the focal *Reference* date. The Match group refers to borrowers whose salary payday coincides with the *Reference* date—for whom loan extensions affect liquidity on the due date and incur income effects from free tenor increase. The Placebo group refers to borrowers whose payday falls at least 10 days from the *Reference* date—for whom extensions create no material liquidity changes on the due date but still incur income effects. Therefore, the differential treatment effects between the Match and Placebo group capture the liquidity effects due to *payday alignment* and cancel out the income effect.

Within each disbursement date, separately for the Match and Placebo groups, we stratify by disbursement amount, age, and credit limit (when sample sizes permit) to form randomization blocks. We randomly allocate borrowers within each block into three equal-sized groups: control, one-day extension, and two-day extension. Extensions are executed silently in the system without additional borrower communication. At the same time, *Lender* disburses funds and informs borrowers of loan details. Crucially, borrowers only observe the post-intervention due date on the platform. All borrowers receive identical, daily reminder messages around the due date. *Lender*

[11] In our data, payday dates do not systematically correlate with borrowers' employer type or income level.

shares the loan repayment data up to 35 days after the loan due date. Because very few loans in the experimental sample transition into formal default within this short window, the experiment does not generate enough default events for reliable estimates. We therefore focus on the outcome of short-term delinquency. We repeat this procedure on other disbursement dates over the 7 months.

Table A1 summarizes the design and variable definitions. Panel A defines key variables: *Match* equals one if payday coincides with the *Reference* date, *Treatment* equals one if extension shifts maturity beyond the *Reference* date. We conservatively classify *Treatment* as zero when the due date falls exactly on the *Reference* date. Because we do not observe the precise timestamp of salary credit and intraday transfer or processing delays may prevent same-day salary receipts from being immediately available for repayment, classifying these cases as $Treatment = 1$ for the Match group risks overstating the benefit of *payday alignment*. Nonetheless, robustness checks in Section 3.6 use the alternative definition for these observations and results remain similar or even stronger. Control in the Match and Placebo groups provides respective counterfactual benchmark. Treatment in the Match group are subject to both income and liquidity effects. Treatment in the Placebo group is only subject to the income effect from free loan extensions. Thus, the difference of Treatment-Control difference between Match and Placebo groups cleanly isolates the liquidity effect from randomly assigned experimental interventions.

Panel B of Table A1 provides a specific example: for loans originally due on the 4th with a *Reference* date of the 5th, we assign Match group borrowers (payday on 5th) and Placebo group borrowers (payday on 15th-25th). For each group, we stratify on disbursement amount, age, and credit limit if observation counts permit and form the randomization block. We assign one third of each block to receive 0-, 1-, or 2-day extensions, resulting in new due dates of 4th-6th. For Match group, their new maturity dates are -1 to 1 day relative to their own salary payday. For Placebo group, the new maturity dates are still far away. In this example, only the 2-day extension constitutes *Treatment* = 1 for both groups.

Figure 1 generalizes the example in Panel B of Table A1 to all *Reference* dates. Panel A shows the Match group with loans originally due one day before salary payday. We randomly allocate borrowers into three groups: control (no extension, due date remains one day before payday), one-day extension (new due date falls on payday), and two-day extension (new due date is one day after payday). The two-day extension group receives cost-free extensions that shift due dates beyond paydays, capturing both income and liquidity effects. Panel B shows the Placebo group with borrowers' salary payday far away from the due date. We use the same three-group randomization, but because due dates remain far from paydays, extensions provide only income effects without liquidity benefits. Thus, in the following analysis for the experiment, we focus on the difference-in-difference estimators. The same experimental design applies to the other three samples in which loans mature 3, 2, or 0 days before the *Reference* date prior to the intervention.

3.2 Data and Summary Statistics

Loans were disbursed from April 3 to October 29, 2021. Our sample comprises 2,809 loans from 2,809 distinct borrowers. Panel A of Table 1 presents summary statistics. The average borrower is slightly above 30 years old (32 on average), is married, and reports monthly income of Rp 5.1 million. Half of the borrowers in the sample have attended technical college or higher. The average loan size is Rp 1.3 million, 26% of the average monthly income. The delinquency rate is 18.9 percent, with 6.9 percent of loans past due by exactly one day (*DPD1*) and 10.4 percent past due by 1-7 days (*DPD1t7*).

Lender employs several internal credit assessment metrics through proprietary credit models. For instance, *Lender* assigns and updates *Credit Limit* and *Behavior* scores for repeat borrowing. *Credit Limit* is an internal scoring metric that maps to the maximum credit amount a borrower is eligible for, based on various criteria including demographic information, loan application frequency, repayment punctuality, referral activities, and social media engagement in terms of user experience sharing. While borrowers do not know their exact *Credit Limit*, they can learn general strategies to improve their credit limit through in-app and online resources and the benefits of higher credit limit such as better loan offers and vouchers. *Credit Limit* varies significantly across borrowers, ranging from 3.2 at the bottom decile to 15.5 at the top decile.[12] *Lender* also assigns *Behavior* scores to borrowers based on their repayment patterns. Timely repayment generally increases this score, though borrowers remain unaware of this metric. By definition, this score is only available for repeat borrowers. Its mean and standard deviation stand at 0.546 and 0.048 in the experiment sample.

Lender also shares with us data on borrowers' complete loan histories, including application dates, approval dates, disbursement details, loan terms, and repayment timing, etc. These data enable measurement of past repayment difficulties. For example, borrowers' average days past due across prior loans has a mean of 0.37 days (*Past Overdue*).

Compared to Indonesia's general population, experimental borrowers are similar in age but have higher average income.[13] This income difference likely reflects the lender's technology requirements, which exclude lowest-income groups lacking smartphone access and bank accounts. For context, we also draw on *Lender*'s full administrative book (formally introduced in Section 4), covering more than 1 million 28-day loans from January 2018 through July 2020. Compared to the administrative sample, borrowers in the experimental sample are older, are more educated, have higher incomes, and have better repayment histories. The *Overdue* rate in the experimental sample is 18.9%, lower than that of the administrative sample (Section 4.1 provides more details). These differences are primarily due to two reasons. First, we conduct experiments in 2021 after the administrative sample ends. *Lender*'s borrower pool improves over time in terms of credit profile as the business matures. Second, the experimental sample exclusively focus on repeat borrowing whereas the administrative sample also includes riskier first-time borrowing.

Despite having regular salaries and formal banking relationships, sample borrowers repeatedly

[12] During 2021 when we conducted the experiment, *Lender* increased *Credit Limit* market-wide. Consequently, the *Credit Limit* in our experiment sample has a mean of 6.8, substantially higher than the mean of 0.27 from our administrative sample ending in July 2020.

[13] According to BPS-Statistics Indonesia (Badan Pusat Statistik), the national average age was approximately 30 years and average employee wages were Rp 2.9 million in 2021.

use fintech lending, indicating unmet liquidity needs that conventional financial institutions fail to address. This demographic profile represents an underserved market segment: individuals with basic financial infrastructure who remain underbanked relative to their credit and liquidity requirements.

Table A3 reports balance tests across extension groups. No detectable differences exist for a variety of loan and borrowers characteristics ranging from loan size, age, gender, marital status, income, credit limit, education, behavior score, and and past due history across 0-, 1-, and 2-day extension assignments at the 1 percent significance level.

3.3 Loan Performance

Our central hypothesis posits that aligning loan maturity with borrowers' paydays increases cash adequacy and enhances repayment propensity. Under the conjecture that cash adequacy improves repayment, we expect lower probabilities of overdue outcomes for loans maturing after borrowers' salary payday.

As discussed in Section 3.1, the experiment focuses on *payday alignment* effect on loan delinquency behavior (we assess default using the administrative sample in Section 4.1). We examine three delinquency measures provided by *Lender*: *Overdue* (any days past due), *DPD1* (exactly one day past due), and *DPD1t7* (1-7 days past due). As a first step, Figure 2 plots these outcomes against the distance between post-intervention due dates and the *Reference* date for Match and Placebo groups separately. Panel A1 shows that for the Match group, overdue rates decrease from 30% to 15% as the distance shifts from −3 (loans maturing 3 days before the *Reference* date) to +2 (loans maturing 2 days after the *Reference* date). This pattern is absent in Panel B1 for the Placebo group, whose paydays fall more than 10 days from the *Reference* date. Panel A2 demonstrates that *DPD1* drops sharply when the distance turns positive (loans maturing after the *Reference* date) for the Match group, while Panel B2 shows no such pattern for the Placebo group. Panels A3 and B3 present similar diverging patterns for *DPD1t7* between the Match and Placebo groups.

We then formally evaluate *payday alignment* effects using the following difference-in-differences specification:

$$y_{l,t} = \alpha_{\text{Match}_l,\,t} + \beta\text{Treatment}_l \times \text{Match}_l + \gamma\text{Treatment}_l + \text{X'}_l + \epsilon_{l,t} \tag{1}$$

where $y_{l,t}$ represents loan outcomes for loan l disbursed on date t. $\alpha_{\text{Match}_l,\,t}$ denotes Disbursement Date × Match fixed effects which absorb the standalone effect from $Match$. $Treatment_l$ indicates loans with maturity extended beyond the *Reference* date. $Match_l$ equals one if the borrower's payday coincides with the *Reference* date, zero if paydays fall at least 10 days away. X_l' includes loan and borrower controls: loan size, age, gender, marital status, income, credit limit level, education, behavior score, and overdue history. Our parameter of interest is β, capturing the differential treatment effect between the Match and Placebo groups.

The specification includes Disbursement Date × Match fixed effects. These fixed effects align with our experiment procedure, under which interventions are implemented separately for the Match and Placebo groups at the disbursement date level. Because all loans in the experiment sample are disbursed within 12 hours of application, these fixed effects absorb variation arising from borrowers' self-selected application timing. Moreover, same-day disbursements in the Match group correspond to the same payday, so the fixed effects also account for borrowers' position in the payday cycle at the time of application. Together, these fixed effects reinforce the stratified randomization structure and support a causal interpretation of the DiD estimates.

Before presenting the DiD estimates, we assess the balance of observable characteristics. Following specification analogous to Equation 1, we regress the *Treatment* indicator on borrower and loan covariates. Table A4 reports the results for the pooled DiD sample, the Match group, and the Placebo group in columns (1)–(3), respectively. All covariates are statistically indistinguishable, except for credit limit in the Placebo group which is statistically significant at the 10% level. We therefore control for credit limit throughout the analysis. These findings validate the stratified randomization procedure.

Table 2 reports the main results. Columns (1)–(3) present baseline DiD estimates following specification 1 for the pooled sample. Columns (4)–(6) and (7)–(9) report subsample results for the Match and Placebo groups, respectively. The dependent variables are *Overdue*, *DPD1*, and *DPD1t7* in columns (1)(4)(7), (2)(5)(8), and (3)(6)(9), respectively. Panel A includes the full sample. Across all delinquency measures, the interaction coefficient *Treatment* $\times$ *Match* is significantly negative, consistent with our hypothesis. In column (1), aligning due dates with paydays reduces delinquency by 5.9 percentage points in the Match group relative to Placebo, representing a 29.1% decline from the control group overdue rate of 20.3%. Similar patterns hold for the other delinquency measures in columns (2) and (3).

Validation checks in columns (4)–(9) further show that treatment effects remain significantly negative for the Match group, but are statistically and economically insignificant for the Placebo group. This heterogeneity indicates that the results reflect liquidity effects unique to the Match group rather than general income effects from longer loan maturities.

The baseline DiD estimate in column (1) of Panel A implies that loans due one to two days after the *Reference* date are 5.9 percentage points less likely to become delinquent than loans due 0-3 days before the *Reference* date, for the Match group relative to the Placebo group. As discussed in Carter, Liu, Skiba, and Sydnor (2022), differential time to repay may or may not affect loan performance. To address the concern that the *Treatment* indicator equals one for both one- and two-day extensions, Panel B restricts the sample to loans maturing 0-1 days before the *Reference* date, where only two-day extensions generate treatment status. The estimates in Panel B are nearly identical to Panel A. Moreover, robustness checks (Table A5) that control directly for the number of extension days yield unchanged results.

Late repayment carries substantial consequences. Beyond contractual late fees, payment failures impair borrowers' credit quality, affecting future loan applications with both the current lender and other financial institutions. Section 4 examines these pecuniary outcomes using administrative data. Non-pecuniary costs include reputation damage and emotional stress from collection activities, including contact with borrowers' emergency contacts.

Overall, this section provides causal evidence that synchronizing loan due dates with borrowers' salary cycles substantially reduces delinquency.

3.4 Heterogeneous Effects

We then study the heterogeneous effects of *payday alignment* with respect to borrower characteristics. Young consumers tend to face stronger cash flow volatility, impairing timely repayment. Income level is a classic proxy for liquidity buffer and risk tolerance, while credit limit level assigned by the lenders, akin to credit score in our setting, captures historical repayment discipline and predict future delinquency. If *payday alignment* works through improving borrowers' cash adequacy, young, low-income, and low-credit borrowers are more likely to be liquidity-constrained and hence the treatment of *payday alignment* will especially improve their chances of repayment on time. Along the same line, borrowers who were more severely late for repayment as measured by the average overdue days over past loans tend to benefit more from the *payday alignment* treatment. Thus, we focus on borrowers' age, income, credit limit level and their late repayment records.

We construct binary indicators to capture these dimensions. *Young* equals one for borrowers aged 30 or below. For income, we construct *High Income* as an indicator for borrowers whose monthly income at account registration falls in the top decile of the experimental sample.[14] We construct *Low Credit* as an indicator for borrowers whose credit limit falls below the sample median. *High Past Overdue* indicates borrowers whose mean days past due across prior loans exceeds the sample median. We then interact the difference-in-differences term ($Treatment \times Match$) with these borrower characteristic indicators.

Table 3 reports results. Panel A examines age and income; Panel B focuses on credit limit level and delinquency history. Outcome variables are *Overdue*, *DPD1*, and *DPD1t7* in columns (1)(4),

[14] The lender only collects income information at account registration. We define *High Income* as top decile rather than above median because the median and 75th percentile income levels remain low—Rp 4.05 and 5.5 million (approximately US$288 and US$391) as shown in Table 1.

(2)(5), and (3)(6), respectively. We control for lower-level interaction terms, borrower and loan characteristics, and Disbursement Date × Match fixed effects. Across loan outcomes, coefficient estimates on triple-interaction terms are negative and statistically significant at conventional levels for *Young*, *Low Credit*, and *High Past Overdue*, confirming that *payday alignment* effects are stronger for young and low-credit borrowers with repayment difficulties. For *High Income*, coefficients on triple interaction terms are positive and marginally significant, implying that low-income borrowers benefit more from *payday alignment* than top earners, though statistical power is limited. The relatively lower statistical power likely reflects limited sample size and income variation in the experimental sample. Section 4.4.4 revisits and further discusses the heterogeneous effects with respect to income.

These findings demonstrate that contract tailoring plays a more pronounced role for borrowers who face higher liquidity constraints and baseline delinquency risk—demographics central to financial inclusion efforts: young individuals and those with low income and credit limits. The results contribute to the ongoing debate on fintech's distributional impact in credit markets, where prior research shows mixed patterns (Fuster, Plosser, Schnabl, and Vickery, 2019; Berg, Burg, Gombović, and Puri, 2020; Howell, Kuchler, Snitkof et al., 2024; Fuster, Goldsmith-Pinkham, Ramadorai, and Walther, 2022). Our evidence suggests that fintech, when combined with contract design addressing borrower constraints, can generate mores gains for the most underserved populations and mitigate disparities in credit performance.

3.5 Repayment Distribution

We next examine whether *payday alignment* affects the timing of repayments across the entire repayment distribution in addition to the likelihood of being overdue. Figure 3 plots the share of repaid loans against repayment date relative to loan maturity following experimental interventions. Panel A shows the Match group, where the borrower's payday equals the *Reference* date. The treatment group's repayment distribution is left-shifted relative to the control, indicating that borrowers repay earlier when their loans mature immediately after payday. In contrast, Panel B shows the Placebo group, where payday is distant from the *Reference* date. Here, repayment distributions are indistinguishable between treatment and control, consistent with our *payday alignment* hypothesis.

We formally test repayment propensity using the Cox proportional hazard model, where repayment constitutes the "failure" event and time-to-maturity defines the analysis time. The unit of observation is loan-calendar-day. Table 4 reports the results and parallels Table 2. Estimated coefficients are expressed as the natural logarithm of the hazard ratios. Panel A includes the full sample; Panel B restricts to loans originally maturing 0-1 days before the *Reference* date prior to experimental interventions. Column (1) estimates the difference-in-differences specification from Equation 1. The positive, statistically significant coefficient on *Treatment* × *Match* confirms our main hypothesis. The economic magnitude indicates *payday alignment* increases Match group repayment likelihood by 36% ($= e^{0.306} - 1$) relative to the Placebo group. Columns (2) and (3) provide validation using Match and Placebo groups separately. The *Treatment* coefficient is positive and significant at 1% for the Match group in column (2) but economically and statistically insignificant for the Placebo group in column (3). The estimated survival curves in Figure 4 illustrate this contrast: treated loans in the Match group exhibit faster decay of the survival function than the controls, reflecting earlier repayment, whereas the treated and control curves for the Placebo group are nearly identical.

We complement the hazard-based inference with tests in distributions. Table A6 reports two-sample Kolmogorov-Smirnov statistics comparing the distributions of repayment-to-maturity gaps. The distributions differ significantly between treatment and control loans in the Match group but not in the Placebo group, and the size of treatment–control difference (reported as Combine K-S in the table) in the Match group is roughly twice as large as in Placebo.

Together, these findings show that *payday alignment* shifts the entire repayment-timing distribution, not merely the binary outcome of delinquency. Borrowers with payday-aligned loans repay sooner across the repayment horizon, suggesting that anticipated or realized salary income prior to loan due dates relaxes their short-term cash constraints and accelerates repayment. This evidence corroborates the findings in Section 3.3 and strengthens the paper's main mechanism of liquidity constraints.

3.6 Robustness

We conduct several robustness tests by varying treatment definitions and estimation methods. Table A7 presents results using alternative treatment specifications. In our main specification, *Treatment* equals one only if the *Reference* date precedes the loan due date. This definition is strict and conservative for detecting *payday alignment* effects because borrowers in the Match group also experience liquidity improvement when the two dates exactly coincide. To relax this definition, Panel A defines *Treatment Alt* as an indicator equal to one if loans mature on or after the *Reference* date following experimental intervention. To address concerns that 1-2 day loan extension assignments may not always result in *Treatment* equaling one and may introduce noise into the identification, Panel B defines *Treatment Alt2* as one if loans mature after the *Reference* date following a 1–2 day extension, missing if loans mature on or before the *Reference* date after extension, and zero if loans receive no extension. To address potential confounding factors from non-working days, we exclude the experiment sample in which the *Reference* dates fall on a weekend or holiday and are adjusted to the preceding business day, and report results in Table A8. Table A9 provides results using an alternative clustering approach: clustering by Disbursement Date $\times$ *Match*. Table A10 reports Probit estimates that account for the binary nature of the outcome variables.

All robustness tests yield results that are qualitatively and quantitatively consistent with baseline estimates, confirming the stability of our main findings across alternative specifications and estimation methods.

4 Administrative Sample

Section 3 establishes and quantifies the causal effect of *payday alignment* through a randomized field experiment. However, the experiment sample size limits our ability to conduct more granular analyses and extract deeper insights. For example, the treatment is confined to maturities within 3 days before and 2 days after a borrower's payday, capturing only a subset of tailoring: in practice, loan maturity dates can lead or lag the nearest payday by up to 16 days. Moreover, the experiment subjects consist exclusively of repeat borrowing, whose responses to the treatment could differ from those of first-time borrowing. To address these constraints and extend our analysis, we exploit the *Lender*'s complete administrative dataset, focusing on 28-day loans made to borrowers who receive regular paychecks and explicitly report their payday.

4.1 Summary Statistics

Panel B of Table 1 reports summary statistics for the administrative sample. This sample comprises 1,002,292 loans with a tenor of 28 days disbursed to 364,458 borrowers between January 2018 and July 2020. The average borrower is below 30 years of age, has not attended technical college or above, is married, and reports monthly income of Rp 4.2 million.

We examine an array of loan outcomes. Our main outcome of interest is *Overdue*, a dummy variable equals one when a borrower fails to pay off the loan on the loan maturity date. *Overdue* rate is 26.2 percent in the sample. The administrative data include detailed information on overdue penalties. When a loan becomes overdue, the lender charges an overdue fee plus penalty interest on the outstanding amount until full repayment. We define *Overdue Penalty* as the sum of interest charges accrued during the late payment period plus overdue fees; this variable has a mean value of Rp 110,726. The administrative data also enable analysis of loan default behavior. Following the *Lender*'s classification, we define default as loans overdue by more than 30 days.[15] The default

[15] For loans overdue by more than 30 days, the mean overdue period is 330 days and the write-off rate is 97 percent, confirming that this measure captures true borrower default. As a robustness check, we also apply a 90-day cutoff for

rate is 11.7 percent, reflecting the elevated risk profile characteristic of fintech lending.

Borrowers in the administrative sample are slightly younger, less educated, and have lower incomes compared to those in the experimental sample. The *Overdue* rate in the administrative sample also exceeds that of the experimental sample. These differences reflect two primary factors. First, the administrative sample provided by the *Lender* ends eight months before the experiment begins and includes riskier borrowers from the early operational period when the lender first launched operations in Indonesia. Second, the administrative sample includes first-time borrowing, representing 18.2 percent of the total sample. Table A11 presents summary statistics by borrowing history. The *Overdue* and *Default* rates are 24.1 percent and 9.2 percent for repeat borrowing, compared to 35.4 percent and 23.0 percent for first-time borrowing, respectively.

In the administrative data, loan due dates are distributed throughout the salary cycle, as borrowers can originate loans at any point between consecutive paydays. To capture the liquidity effect of salary payments, we compare each loan's maturity date to the preceding and following paydays and focus on the nearest payday. We calculate *Maturity Relative to Payday* as the number of days between loan maturity and the nearest payday, where negative values indicate maturity before the nearest payday and positive values indicate maturity after the nearest payday. This measure ranges from -16 to +16 days, with a sample mean of 0.436 days and median of 0 days.

We define our key variable of interest *Alignment* as one if a loan matures after the borrower's nearest payday (*Maturity Relative to Payday*>0), and zero otherwise. This essentially captures whether loans mature in the early half of a salary cycle, when borrowers have relatively greater cash availability following salary receipt. Compared to our intervention focusing on a narrow window around payday (contrasting loans maturing 1-2 days after salary receipt with those maturing 0-3 days before) in the experiment, the administrative analysis captures liquidity effects across the broader salary cycle. *Alignment* has an average of 0.5, indicating that half of sample loans are due in the early half of a salary cycle.

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default classification.

4.2 Overdue, Penalty, and Default

We examine the *payday alignment* effect on loan outcomes to assess the value of tailored contract design. As a preliminary analysis, Figure 5 plots *Overdue* and *Default* rates against *Maturity Relative to Payday*. The figure reveals that both rates are substantially lower when *Maturity Relative to Payday* is positive (i.e., when *Alignment* equals one), providing initial evidence consistent with the *payday alignment* effect.

We next turn to formal regression analysis. Specifically, we estimate the following regressions specification:

$$y_{b,l,t} = \alpha_t + \alpha_b + \beta \text{Alignment}_{b,l,t} + \text{X'}_{b,t} + \text{X'}_{l} + \epsilon_{b,l,t} \tag{2}$$

where b, l and t denote borrower, loan and loan disbursement date; $y_{b,l,t}$ is the loan outcome variables; and α_t and α_b are the disbursement date and borrower fixed effects. X'_l and $X'_{b,t}$ contain vectors of loan-level and borrower-level controls including the loan size, and borrowers' age, gender, marriage status, income, and education background.[16]

The panel regression specification leverages both disbursement date and borrower fixed effects to address endogeneity concerns. Disbursement date fixed effects restrict comparisons to loans disbursed on the same day, thereby controlling for unobserved time-varying factors and addressing self-selection issues related to application timing.[17] Borrower fixed effects absorb time-invariant borrower characteristics that may jointly influence *payday alignment* and repayment behaviour. Although the administrative setting does not offer identification as clean as the randomized experiment, these stringent controls substantially alleviate omitted variable concerns and support credible inference from observational data.

Table 5 presents the panel regression results. Columns (1)–(2) estimate the effect of *Alignment*

[16] Since the administrative panel sample includes first-time borrowing, we drop the control variables of *Behaviour*, *Credit Limit*, and *Past Overdue Max* that are only available for repeat borrowing.

[17] In the administrative sample, the decision time has a median of 28 minutes, ranging from instantaneous disbursement to 12 days for cases requiring manual review. As a robustness check, we also estimate specifications including application date fixed effects and obtain nearly identical results.

on overdue incidence, first with disbursement date fixed effects and then additionally with borrower fixed effects. In both specifications, the coefficient on *Alignment* is negative and statistically significant at the 1% level, indicating that aligning the loan maturity with borrowers' salary paydays meaningfully reduces delinquency. The estimated magnitudes are sizable: *payday alignment* lowers the likelihood of being overdue by 4.9 and 4.1 percentage points in columns (1) and (2), corresponding to a reduction of 18.7% and 15.7% relative to the sample mean.

The estimate in column (2) is smaller in magnitude than that in column (1). Borrower fixed effects strengthen identification by absorbing time-invariant borrower characteristics. Yet, within-borrower estimation places greater weight on borrowers with more within-variation in *Alignment*. Borrowers who experience more switches in alignment status—likely driven by features of the lender's processing algorithm rather than their own choices—may adapt their liquidity management and repayment behavior to be less reliant on salary receipts.[18] As a result, the within-borrower estimator may overweight borrowers who are less sensitive to *payday alignment*, potentially understating the average effect. We therefore report estimates with and without borrower fixed effects to bound the effect size.

Columns (3)–(4) report analogous regressions for *Overdue Penalty*. The coefficients again indicate economically meaningful improvements. Payday-aligned loans incur Rp 12,515 and Rp 5,787 less in overdue interest and fees, representing a reduction of 11.3% and 5.2% relative to average penalty charges. The consistency across outcomes and specifications highlights that modest timing adjustments materially improve repayment performance and reduce the associated financial costs.

Although reduced overdue interest and fees seemingly imply income loss for lenders, aligning repayment with salary cycles generates other important benefits for the lender. By reducing delinquency, *payday alignment* accelerates cash inflows, expanding the *Lender*'s capacity to origi-

[18] Section 4.4.3 discusses why strategic selection into *payday alignment* appears unlikely given the unpredictable and opaque loan processing timeline and provides supporting empirical evidence.

nate new loans. A back-of-the-envelope calculation using the administrative sample indicates that aligning all due dates with paydays would raise on-time monthly cash receipts by about Rp 5,163 million (US$366,733)—equal to 4.8% of the value of loans maturing in a month and 5.6% of monthly disbursements. We compute this by multiplying the number of loans due per month by the coefficient from regressing overdue dollars (sum of overdue principal, interest, and fees) on *Alignment*, estimated as in Equation 2. The corresponding regression result is reported in column (2) of Table A12Column (2) of Table A12 tabulates the corresponding regression result.

Quantifying the full lender-side gains lies beyond the scope of this paper. Nonetheless, we discuss several benefits qualitatively. First, accelerated and timelier cash flows shorten the capital cycle, boosting fund velocity and enhancing the efficiency and return on capital. Second, predictable cash inflows from timely repayments reduce liquidity risk and the need for large cash buffers, freeing up financial and human capital for investment and expansion. Finally, reduced delinquency rates decrease the volume of accounts requiring active collection. This lowers direct debt collection costs which require multilayered teams, and mitigates unobserved legal and reputational risks associated with debt collection.

The larger scale and comprehensive outcome tracking in the administrative data allow for more precise estimation of the *payday alignment* effect on default. Columns (5) and (6) show that *payday alignment* reduces the probability of default by 1 and 0.3 percentage points, corresponding to a 8.6% and 2.6% decrease relative to the unconditional default rate in the sample. Although the economic magnitudes are smaller than those for *Overdue*, both estimates are statistically significant at the 1% level. These results indicate that the liquidity boost associated with salary receipt not only improves short-term repayment behavior but also lowers the lender's longer-term credit risk.

Overall, our analysis in Table 5 generates consistent results of *payday alignment* in the administrative data as in the field experiment. Regardless of the loan performance measure examined and various fixed effects used in the panel regression, we find that *payday alignment* improves the loan performance with strong statistical and economic significance.

4.3 Future Credit Access

Access to credit on an ongoing basis is a cornerstone of financial inclusion: missing a payment today can raise the cost of or even prohibit tomorrow's loans, trapping households in a cycle of liquidity scarcity and high-cost borrowing (Karlan and Zinman, 2009; Adams, Einav, and Levin, 2009). In the context of digital lending, where platforms often automatically rely on repayment histories to underwrite future applications, a single delinquency can materially erode a borrower's ability to obtain new financing (Berg, Fuster, and Puri, 2022). To quantify how *payday alignment* today influences credit access in the future, we turn to the platform's full application dataset and implement a Two-Stage Least Squares (2SLS) design. In the first stage, we predict each loan's incidence of delinquency and default by regressing *Overdue* and *Default* respectively on *Alignment*, corresponding to the specifications reported in columns (1)(2) and (5)(6) of Table 5. In the second stage, we use these predicted repayment outcomes to explain the probability that the subsequent loan application is approved (*Next Approval*).

Table 6 reports the second stage results. Columns (1) and (2) report the second stage results using predicted *Overdue*; their corresponding first stage with *Overdue* as the dependent variable are reported in columns (1) and (2) of Table 5, respectively. In both specifications, the coefficient on the predicted *Overdue* measure is statistically significant and has the expected sign. The 2SLS estimates indicate that reduced overdue records triggered by *payday alignment* improves subsequent loan approval rates by 1.04 and 0.56 percentage points in columns (1) and (2), or 2.0% and 1.1% relative to the unconditional approval rate.[19]

Columns (3) and (4) report the second stage results using predicted *Default*; their corresponding first stage with *Default* as the dependent variable are reported in columns (5) and (6) of Table 5. The estimates in columns (3) and (4) indicate that default records explained by *payday alignment* improve subsequent loan approval rates by 1 and 0.5 percentage points, or 2% and 1% relative to

[19] 1.04 percentage points = $(-0.049) \times (-0.213)$; 0.56 percentage points = $(-0.041) \times (-0.136)$.

the unconditional approval rate.[20] In other words, if borrowers have loan maturity date tailored to their payday, their future access to credit also improves with reduced likelihood of delinquency and default on the current loan. This is particularly important in financial inclusion for borrowers without a credit history.

Over the sample period, the *Lender* received 19,646,866 loan applications from 6,804,013 distinct borrowers, representing a share of 8.2% in Indonesia's online lending market. Putting the effect into perspective, we can estimate the number of beneficiaries from *payday alignment* in the whole country by multiplying the 2SLS estimates in Table 6, and then grossing up by dividing by the *Lender*'s market share. Our back-of-the-envelope calculation using columns (1) and (2) suggest that *payday alignment* improves credit access by approximately 0.83 and 0.45 million additional applications and 0.29 and 0.15 million additional borrowers through reducing overdue records annually in Indonesia. As borrowers who avoid delinquency remain eligible for future credit, lenders also benefit from increased lifetime customer value and reduced portfolio churn.

One caveat in interpreting these results is that this study draws on data from one major lender in Indonesia and hence we cannot observe whether the applicant receives a loan elsewhere. Although the *Lender* is the leading fintech lender, borrowers rejected by the *Lender* may possibly obtain credit from other lenders.

4.4 Heterogeneous Effects of Payday Alignment

This section explores heterogeneous *payday alignment* effects along different dimensions.

4.4.1 Distance Bands

We start with the conditional effects based on the distance between payday and loan maturity. Our main variable *Alignment* identifies whether loan maturity falls in the first or second half of a salary cycle. Since liquidity constraints increase as financial resources deplete over time after salary

[20] 1 percentage points = $(-0.01) \times (-1.041)$; 0.5 percentage points = $(-0.003) \times (-1.699)$.

payment, we expect *payday alignment* effects to attenuate as the distance between loan maturity and payday increases.

Figure 5 shows that overdue and default rates steadily decrease as *Maturity Relative to Payday* decreases from 16 to 1, indicating improved loan performance as due dates move closer to but follow payday. Once due dates precede payday (*Maturity Relative to Payday* turns negative), both rates spike sharply up. To formally probe *payday alignment* effects along *Maturity Relative to Payday*, we decompose *Alignment* into four binary variables based on the distance between due date and payday: *Close* (1–3 days), *Medium* (4–7 days), *Distant* (8–16 days), and *Benchmark* (due date precedes payday).[21] The first three variables represent $Alignment = 1$; the last identifies $Alignment = 0$. We repeat our panel regression in Table 5 using these interval variables.

Table 7 reports the results. We omit the *Benchmark* category, so coefficients represent effects relative to $Alignment = 0$. Columns (1)(2) examine *Overdue* as the dependent variable. The coefficient estimates for *Close*, *Medium*, and *Distant* are all negative and statistically significant, indicating that *payday alignment* effects persist beyond immediate post-payday periods. The point estimates in column (2) imply that *Close* reduces the overdue probability by 5.9 percentage points (22.5% of the unconditional rate), while *Medium* and *Distant* reduce the overdue probability by 4.4 and 2.7 percentage points, respectively. The diminishing magnitudes suggest that *payday alignment* effects decay as due dates move further beyond payday. Columns (3)(4) show similar patterns for overdue penalties. Columns (5)(6) examine *Default*. The effect is consistently statistically significant only in the *Close* and *Medium* categories, although the longer-term effect in the *Distant* category remains negative. Consistent with our hypothesis based on liquidity constraints and cash adequacy, *payday alignment* effect magnitudes increase as temporal proximity to payday increases.

Importantly, the *Close* coefficient in column (2) closely matches experimental results in Table 2 (5.9 percentage point reduction, 29.1% relative to experimental control group overdue rate). This

[21] The administrative *Distant* band and experimental Placebo group use different definitions and are not directly comparable. The *Distant* band restricts to loans maturing after the nearest payday, whereas the Placebo group does not (loans mature 10 days before or after *Referenece* dates).

correspondence is intuitive: experimental interventions result in loans maturing 1–2 days after payday for the Match group, capturing the *Close* category effect. Restricting the analysis to repeat borrowers—the same population targeted in the experiment—yields even closer tracking: coefficient estimate of *Close* in column (2) of Table A13 is −0.062, 25.7% reduction from the overdue rate for repeat borrowers. This consistency validates effect sizes documented and indirectly supports the causal interpretation of *payday alignment* in the administrative sample tests.

In summary, our evidence supports the role of *payday alignment* as a liquidity shock to borrowers and shows that such effect is strongest if loan due dates immediately follow salary payments.

4.4.2 First-time Borrowing

The administrative data also allow us to study first-time borrowing—excluded from the experiment but critical for financial inclusion. Without a repayment history, first-time borrowers often confront higher interest rates, stricter underwriting, and greater risk of exclusion due to higher perceived risk (Brune, Giné, and Karlan, 2022). In our context, first-time borrowing is indeed associated with higher delinquency and default rates. If contract tailoring and flexibility benefits require frequent repayments to screen borrowers, build financial discipline, and establish repayment norms, then tailored contracts should not be offered to first-time borrowers. Yet they may also gain the most from credit access. To understand whether aligning loan maturity with payday can help even those taking their first loan, we repeat our core specification on the subset of borrowers with no prior loans.

Table 8 reports the subsample results for first-time borrowing and the interaction effects between *Alignment* and *First-time*, a binary variable for first-time borrowing. Columns (1), (3), and (5) of Table 8 report the results for *Overdue*, *Overdue Penalty*, and *Default* in the sample of first-time borrowers, respectively. We find that *payday alignment* significantly reduces overdue, overdue penalties, and default probabilities among first-time borrowers. To compare the economic magnitude on first-time borrowers with that on repeat borrowers, we interact *Alignment* with *First-time* in the full sample and repeat the panel regression specification 2. Columns (2), (4) and (6) show that the coefficients of the interaction terms are positive and statistically significant, suggesting that the *payday alignment* effect weakens for first-time borrowing.[22]

First-time borrowing may benefit less from tailored loans and *payday alignment* for several reasons. First-time borrowers may be unfamiliar with *Lender*'s App and repayment procedures, leading to technical difficulties that prevent timely repayment despite adequate cash flow from salary disbursements. Additionally, they may lack the financial discipline to manage loan repayments effectively and exhaust the liquidity buffer provided by salary payments fast. These factors suggest that *payday alignment* effects may strengthen with borrowing experience, which we examine in the next section. In summary, the results suggest that even borrowers without a loan track record derive meaningful benefits from *payday alignment*.

4.4.3 Past Experiences

We then examine how *payday alignment* effect varies as borrowers' experiences accrue and how it can interact with past experiences. The first dimension of experience we study is exposure to *payday alignment* in the past. We expect the *payday alignment* effect to last. On one hand, repeat exposure could reflect learning from a random treatment of alignment, where those who have experienced the benefit of alignment act on it more effectively; on the other, it could reflect a selection effect due to borrower awareness of cash flow management. Although our data cannot fully disentangle learning from selection, we find limited evidence of the latter. We therefore focus on the net effect of past alignment experiences to assess whether the payoff from tailoring repayment timing endures.

Column (1) of Table 9 explores the net effect of past exposure to alignment by interacting *Alignment* with *Payday Alignment Experience*, defined as the share of a borrower's prior loans

[22] The coefficient estimate on the *First-time* dummy is significantly negative due to borrower fixed effects, which excludes borrowers with only one disbursed loan and makes identification within borrower for repeat borrowers only. Without borrower fixed effects, Table A14 in the appendix shows that the coefficient estimate on *First-time* dummy becomes significantly positive, consistent with the summary statistics in Table A11 indicating that first-time borrowers have higher delinquency and default rates along with their associated fees and penalties.

that were aligned. The interaction coefficient of -0.055 (t-statistic = -12.45) indicates that the *payday alignment* effect strengthens for borrowers with greater past alignment exposure, suggesting that the benefits of contract tailoring become reinforced rather than diminished over time. The economic magnitude of this interaction is sizeable: a one standard deviation increase in *Payday Alignment Experience* strengthens the *payday alignment* effect by 0.018 in absolute value ($0.326 \times (-0.055)$), which is 69% of the absolute magnitude of the baseline *Alignment* coefficient (-0.026) for inexperienced borrowers.

To examine whether borrowers can proactively seek alignment by timing their applications, we analyze autocorrelation in *Alignment* status across loans in unreported tables. Regressing current *Alignment* on lagged *Alignment* with specification 2 yields a coefficient of 0.006 (t-statistic = 1.47), indicating minimal within-borrower persistence in alignment status. This pattern is consistent with two interpretations: either borrowers have not learned to replicate alignment in subsequent applications, or borrowers cannot systematically achieve favorable timing through application, even if they recognize alignment benefits.

The loan processing timeline is sufficiently unpredictable and opaque that borrowers cannot manipulate alignment outcomes. Although *Lender*'s automated system often enables same-day approvals, borrowers face uncertainty regarding the exact timing of approval, disbursement, and the due date. Moreover, the mapping from application date to maturity is non-transparent: *Lender*'s internal algorithm applies discretionary rules related to processing cutoffs, weekends, and holidays, making the final due date effectively unpredictable from the borrower's perspective. These features prevent borrowers from strategically timing their applications to align maturities with salary cycles. As a result, meaningful contract tailoring must be engineered deliberately by the fintech lender rather than initiated by borrowers.

We then examine borrowers' past borrowing experience measured by the number of disbursed loans and loan applications, which signals familiarity with the lending process and reliance on credit (Brune, Giné, and Karlan, 2022; Gathergood, Guttman-Kenney, and Hunt, 2019). Columns

(2) and (3) interact *Alignment* with *Disbursement Experience* (the logarithm of the number of past approved loans) and *Application Experience* (the logarithm of the number of past loan applications), respectively. These two measures are positively correlated by construction. In both columns, the interaction coefficients are -0.004 and statistically significant, suggesting that past borrowing experience reinforces rather than weakens the *payday alignment* effect.

Finally, We examine late repayment histories as another dimension of past experiences. Column (4) of Table 9 interacts *Alignment* with *Overdue Experience*, the share of past loans that became overdue. The significantly negative interaction coefficient -0.103 (t-statistic = -7.70) shows that alignment effects are particularly pronounced for borrowers with histories of repayment difficulties. The *payday alignment* effect for borrowers with one standard deviation higher *Overdue Experience* increases by 0.018 in absolute value ($0.179 \times (-0.103)$), representing a substantial increment of 54% from the baseline effect size for borrowers without overdue records (-0.034).

These patterns suggest that repayment design aligned with borrowers' liquidity shocks provides sustained rather than transitory benefits, especially for financially vulnerable borrowers. Loan maturity timing synchronized with salary cycles continues to generate value as borrowers accumulate experience with the loan product.

4.4.4 Borrower Characteristics

We now examine whether *payday alignment* has differential impacts across borrower characteristics that shape liquidity constraints and repayment behavior. Section 3.4 demonstrates that *payday alignment* effects are stronger for young, low-income, and low-credit borrowers who face greater cash flow volatility and liquidity constraints. The larger administrative sample provides more variation across these characteristics than the experimental sample, enabling quintile-based comparisons that illuminate the full range of distributional effects.

To visualize heterogeneous effects, we rank borrowers by each characteristic into quintiles and estimate our baseline panel regression of *Overdue* on interactions between *Alignment* and four quintile dummies (omitting the top quintile), controlling for all covariates and fixed effects. Figure 6 plots interaction coefficients with 95% confidence intervals across three dimensions: age, income, and credit limit. This approach reveals how *payday alignment* benefits vary systematically with borrower profiles.

Panel A uses borrowers over 35 as the benchmark. All coefficients from first through fourth quintiles are negative and statistically significant (marginally significant for the fourth quintile), becoming progressively more negative for age brackets of 31–35, 26–30, 21–25, and 20. This confirms experimental findings and reveals a linear relationship between *payday alignment* effects and age. Panel B shows that low-income quintiles (first through fourth) exhibit statistically significant and negative coefficients relative to the top quintile, indicating heightened sensitivity to loan maturity timing among lower-income households. The third quintile shows the largest magnitude. These patterns confirm and unpack Section 3.4 findings: the top income group benefits least from *payday alignment*; middle-income borrowers benefit most as they have repayment capacity but face binding liquidity constraints, while the bottom group lacks repayment ability. Panel C uses the top credit-limit quintile as the benchmark group, and shows that other quintiles experience significantly negative coefficients and thus stronger *payday alignment* effects. The second and third quintiles exhibit the greatest magnitudes. These results underscore the value of *payday alignment* for credit-constrained borrowers. Table A15 reports the regression results underlying Figure 6.

These findings confirm Section 3.4 results and provide additional evidence that tailored repayment timing is most valuable for borrowers that fintech lenders target in financial inclusion efforts by aligning due dates with payday cycles.

4.5 Robustness

We conduct a battery of robustness checks to our key results in Table A16. In column (1), we re-estimate the overdue regression after dropping all loans disbursed from March 2020 onward to rule out any confounding effect of COVID-related shocks. The treatment coefficient remains virtually unchanged (–0.041), confirming that our *payday alignment* effect is not driven by the pandemic. In column (2), we broaden the *Alignment* definition to include any loan maturing on or after the payday (rather than only after the payday as in the main test), and again recover a very similar point estimate (–0.046). Finally, column (3) applies a more stringent definition of default, requiring loans to be overdue by at least 90 days, and still uncovers a significant and negative effect (−0.003) that echoes our main default results. Across all these variations, *payday alignment* continues to deliver economically meaningful and statistically robust reductions in both short-term delinquency and deep default. Finally, Table A17 replicates the baseline analysis in Table 5 by clustering standard errors at both disbursement date and borrower levels and the results remain robust.

5 Alternative Explanations

We interpret the evidence in Sections 3 and 4 as operating through a liquidity–timing channel whereby aligning maturity with payday relaxes liquidity constraints and improves repayment. Delinquency may also respond to *payday alignment* via non-liquidity channels. In this section we show that alternative explanations cannot account for the full set of patterns in the data.

Inattention and reminder. Inattention due to limited cognitive resources can impede debt repayment (Bordalo, Gennaioli, and Shleifer, 2020), and reminders can partially offset these frictions (Cadena and Schoar, 2011; Karlan, McConnell, Mullainathan, and Zinman, 2016; Medina, 2021). Because payday receipts are salient events, one alternative explanation is that *payday alignment* merely acts as a reminder that helps refocus borrowers' attention on repaying the debt. However, this mechanism is unlikely to drive the results in our setting. Operationally, the *Lender* delivers standard repayment notifications to all borrowers at fixed intervals before the due date, so reminder exposure is invariant to *payday alignment*. Thus, this operational practice is difficult to reconcile with a reminder mechanism.

Present bias. Naïve present bias (O'donoghue and Rabin, 1999) provides a potential, yet insufficient, explanation for the observed debt repayment dynamics. Present-biased borrowers, characterized by limited self-control and short-run impatience, tend to consume immediately upon receiving paychecks, postponing consumption reductions until the due date deadline (Kuchler and Pagel, 2021; Carter, Liu, Skiba, and Sydnor, 2022). If maturity falls immediately after payday, the deadline enters the post-pay window, compressing the procrastination horizon. This forces borrowers to address debt obligations immediately using available funds, thereby mitigating the consumption temptation driven by present bias. In this context, *payday alignment* would function as a soft commitment device that counteracts the inhibitory effect of present bias on timely repayment.

However, several empirical patterns are inconsistent with the present bias mechanism serving as the primary driver. A mechanism based purely on present bias predicts that repayment for payday-aligned loans should spike on the due date, with minimal pre-maturity repayment. In contrast, Section 3.5 demonstrates that payday alignment leads to earlier repayment across the entire distribution of repayment timing (relative to the due date), suggesting that borrowers who receive (or anticipate) cash inflows before the due date exhibit proactive pre-maturity repayment behavior.

Under a present bias framework, if the due date falls well after the payday, borrowers are exposed to a prolonged window for consumption temptations. The immediate utility of spending cash-on-hand can override the future motive to repay, leading borrowers to deplete their salary on short-term gratification before the obligation matures. In this case, *payday alignment* effect is reversed (i.e., increased delinquency). Contrary to this prediction, Section 4.4.1 shows that *payday alignment* continues to reduce delinquency even when the due date follows distantly after the payday.

Collectively, these evidence suggest that present bias cannot be the sole mechanism underlying the efficacy of *payday alignment*.

6 Conclusion

In this paper, we analyze how tailored contract design affects loan outcomes. Our study provides clear, causal evidence that simple tweaks to fintech loan contracts by aligning due dates with borrowers' paydays can significantly improve repayment outcomes. In a randomized control trial on a large and leading fintech lender's platform, we find that shifting maturity by just a few days around payday cuts delinquency by roughly 30%. Leveraging the full administrative sample from the same lender of over one million loans, we comprehensively analyze the impact of such contract tailoring on borrower behavior and loan outcomes both in the short run and long run. We confirm that the *payday alignment* effect persists over time, and is more pronounced for groups central to the financial inclusion efforts: young, low income, and low credit borrowers. More importantly, the *payday alignment* effect extends well into future borrowing abilities. By avoiding delinquency on the current loan, the borrower enjoys better access to credit later. We also find that *payday alignment* generally advances cash flow for the lender, improving the lender's liquidity overall.

Taken together, these results suggest that incorporating basic payday information into automated underwriting and scheduling systems can improve both borrower outcomes and lender liquidity. Rather than viewing flexibility as a costly deviation from standardized contracts, lenders might see it as a way to stabilize cash flows, reduce collection burdens, and build goodwill—especially among borrowers for whom a few days' misalignment can mean the difference between on-time repayment and costly delinquency. Future work could explore additional forms of contract tailoring, such as adjustable installment cycles, and examine how these innovations interact with borrower behavior over longer horizons. By balancing automation with personalization, fintech lenders can scale efficiently while better matching the financial rhythms of their customers.

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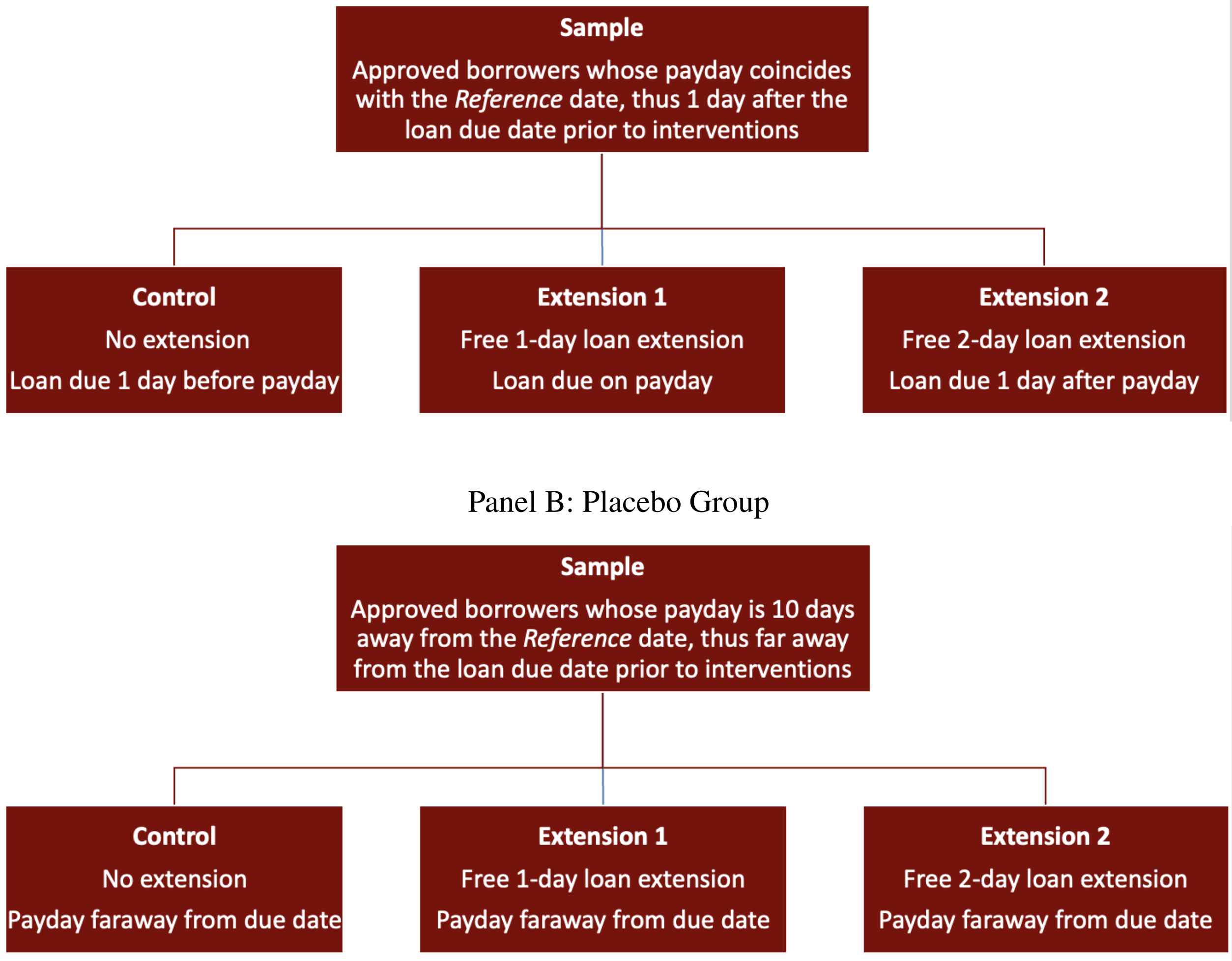
Figures

Figure 1: Research Design

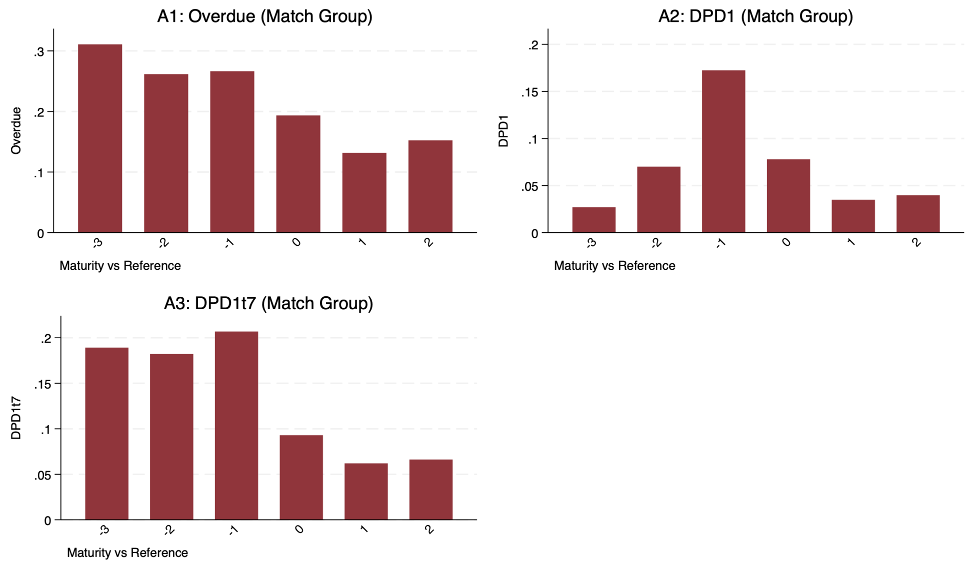
Panel A: Match Group



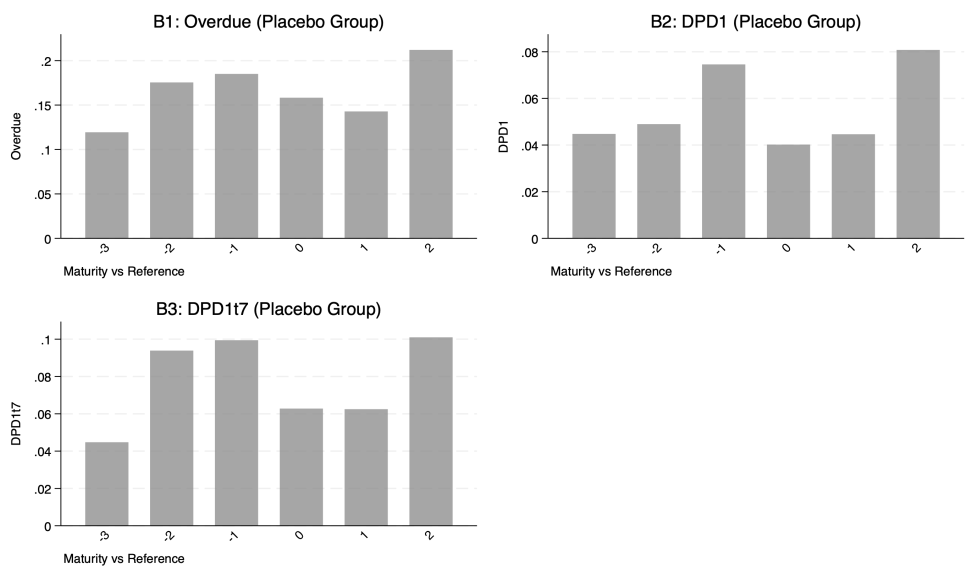
This figure illustrates the experimental design with a subsample of loans in the experiment (those mature one day before the *Reference* date prior to interventions). Panel A and B describe experimental interventions and outcomes for the Match and Placebo group respectively.

Figure 2: Experiment Outcomes

Panel A: Match Group

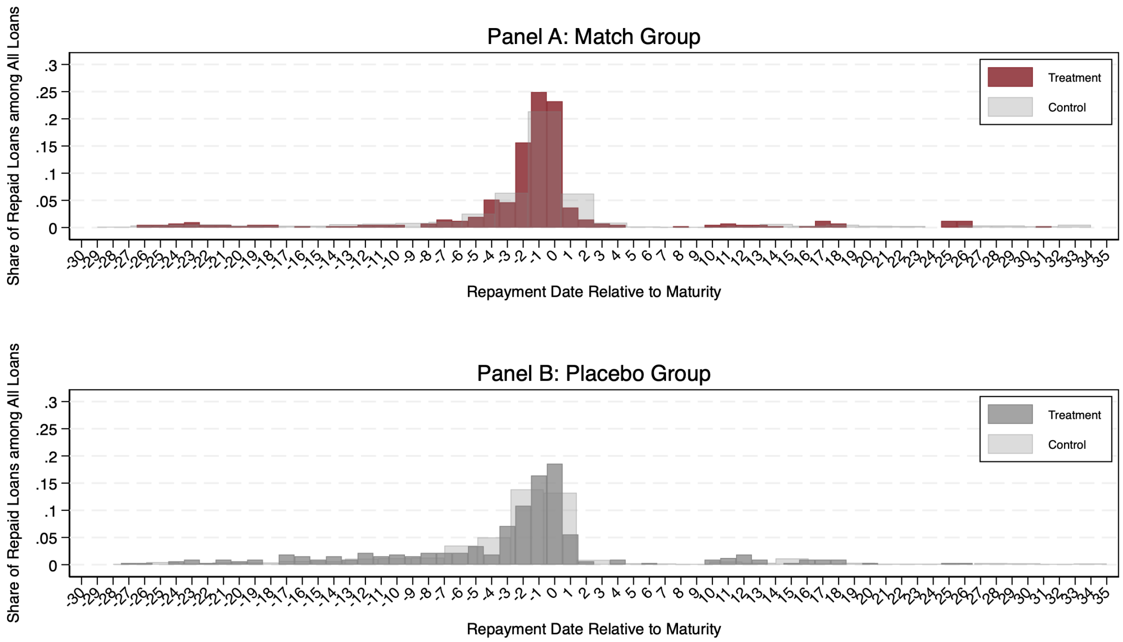


Panel B: Placebo Group



This figure plots the share of overdue loans relative to the distance between the loan maturity date (post-intervention) and the *Reference* date (5th, 10th, 25th, 28th). Overdue outcomes are: *Overdue* (any overdue) in A1 and B1, *DPD* (overdue by one day) in A2 and B2, and *DPD1t7* (overdue by 1–7 days) in A3 and B3. Panel A shows the Match group; Panel B shows the Placebo group.

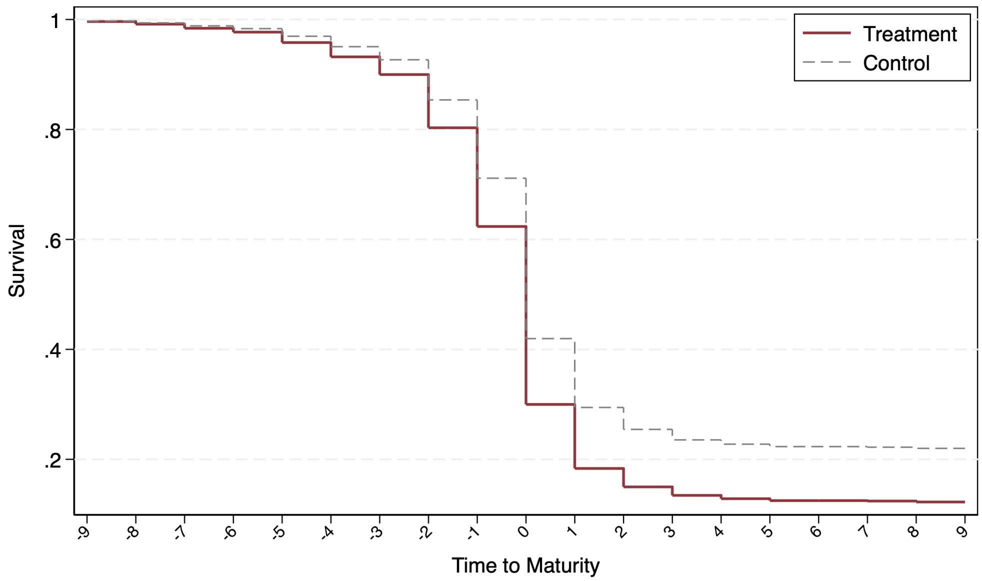
Figure 3: Share of Repaid Loans



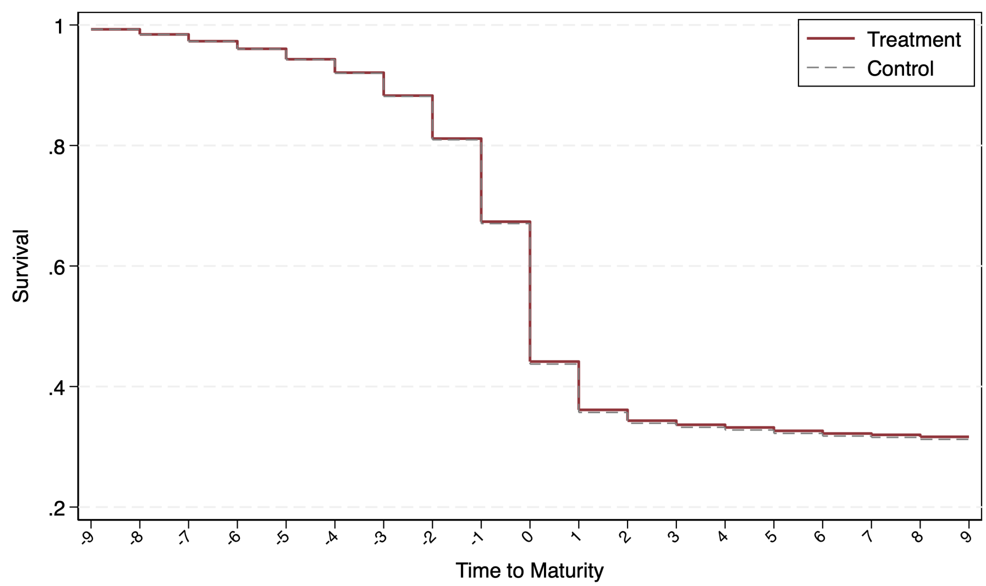
This figure displays the ratio of repaid loans to total loans, plotted against repayment date relative to loan maturity following the experimental intervention. Panel A presents the Match group, with treatment shown in red and control in light grey. Panel B presents the Placebo group, with treatment shown in dark grey and control in light grey.

Figure 4: Survival Function after Cox Regression

Panel A: Match Group

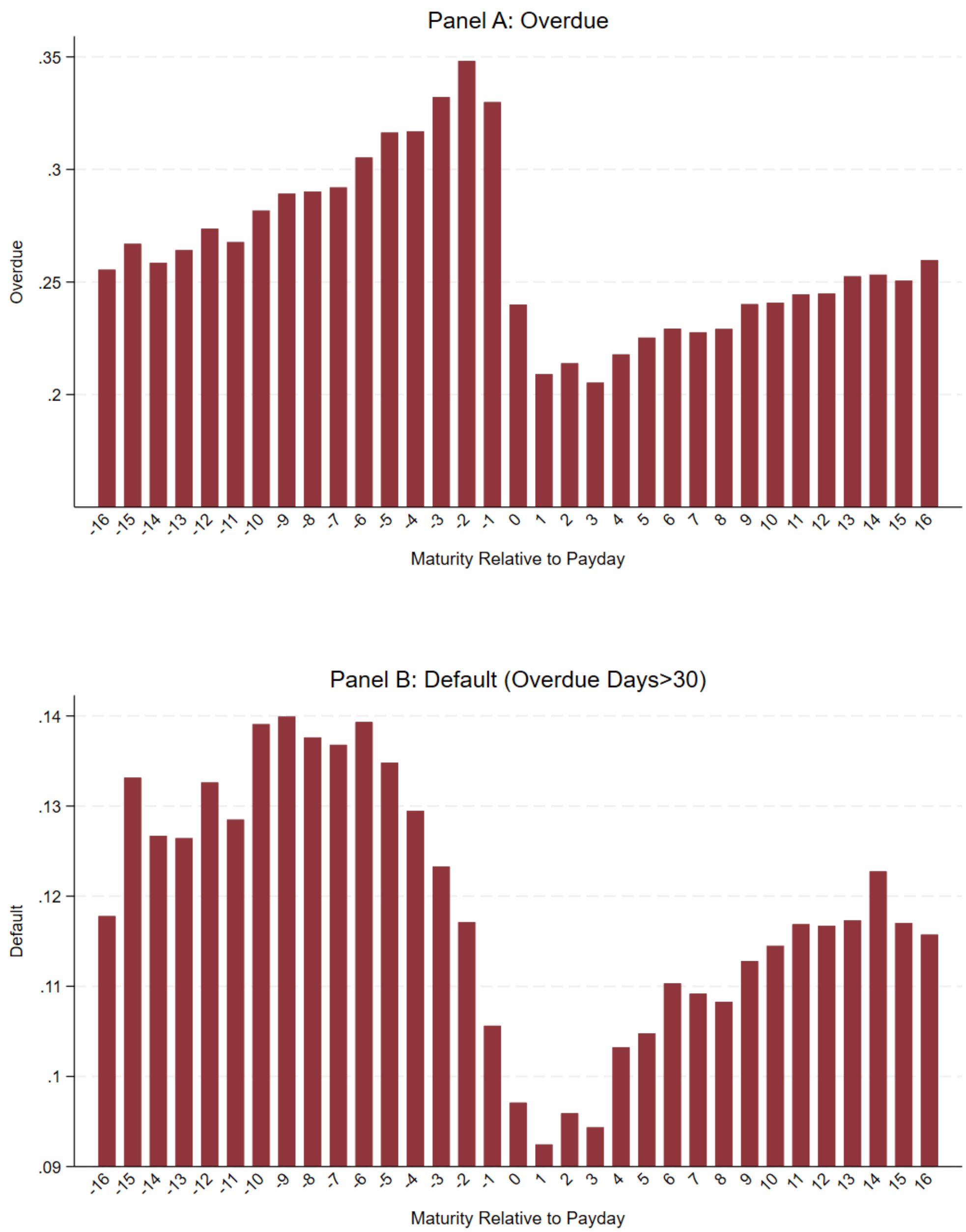


Panel B: Placebo Group



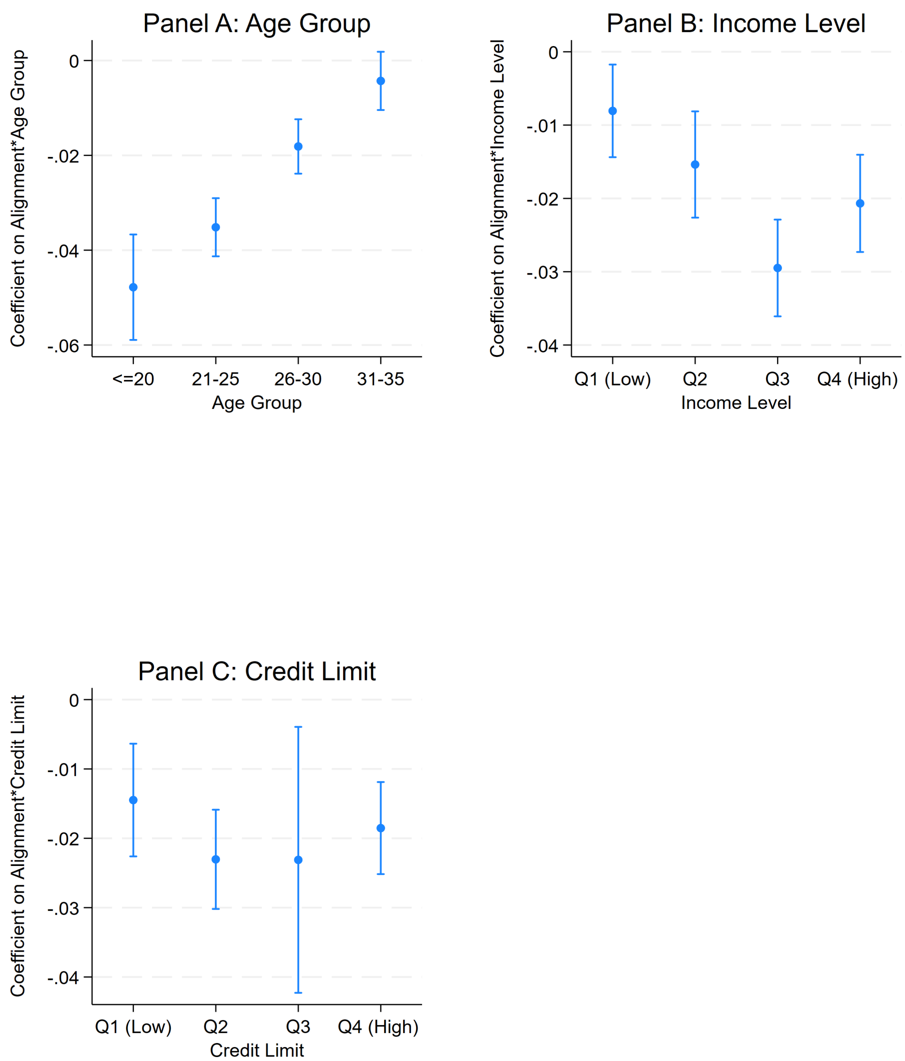
This figure plots survival functions estimated from the Cox regression in Table 4. Panel A shows the Match group; Panel B shows the Placebo group.

Figure 5: Overdue and Default Loans



This figure shows loan performance in the administrative sample. Panel A plots the share of loans overdue against maturity–payday distance. Panel B plots the share of loans in default (defined as 30+ days past due) against maturity–payday distance.

Figure 6: Coefficient Plots of Heterogeneous Effects



This figure reports heterogeneous effects of *Alignment* on overdue outcomes in the administrative sample. Panels A–C show coefficient estimates from interactions of *Alignment* with quintile dummies for age, income, and credit limit. Quintile 5—borrowers older than 35, in the top income group, and with the highest credit limit—is the omitted category. The specification follows column (2) of Table 5. Dots denote point estimates; caps indicate 95% confidence intervals. Table A15 in the Appendix reports the regression results underlying Figure 6.

Tables

Table 1: Summary Statistics

This table reports loan-level summary statistics for key variables. Panel A includes the experiment sample: loans disbursed from April 2021 to October 2021. Panel B includes full administrative sample from the *Lender*: loans disbursed from January 2019 to July 2020. All variables are defined in Table A2.

	Panel A: Experiment Sample							
	count	mean	sd	p10	p25	p50	p75	p90
Overdue	2809	0.189	0.392	0.000	0.000	0.000	0.000	1.000
DPD1	2809	0.069	0.254	0.000	0.000	0.000	0.000	0.000
DPD1t7	2809	0.104	0.306	0.000	0.000	0.000	0.000	1.000
Repayment Relative to Maturity	2809	-1.187	8.443	-10.000	-3.000	-1.000	0.000	3.000
Treatment	2809	0.261	0.439	0.000	0.000	0.000	1.000	1.000
Match	2809	0.503	0.500	0.000	0.000	1.000	1.000	1.000
Young	2809	0.348	0.476	0.000	0.000	0.000	1.000	1.000
Female	2809	0.413	0.492	0.000	0.000	0.000	1.000	1.000
Size	2809	1.313	0.438	0.600	0.900	1.200	1.800	1.800
High Education	2809	0.506	0.500	0.000	0.000	1.000	1.000	1.000
Married	2809	0.551	0.497	0.000	0.000	1.000	1.000	1.000
Credit Limit	2809	6.796	4.284	3.200	5.205	5.550	5.800	15.501
Income	2809	5.066	3.577	2.500	3.200	4.050	5.500	8.000
Behaviour	2802	0.546	0.048	0.481	0.514	0.550	0.580	0.605
Past Max Overdue	2809	0.352	0.851	0.000	0.000	0.000	0.000	1.386
Past Overdue	2809	0.374	0.754	0.000	0.000	0.077	0.467	1.000
	Panel B: Full Administrative Sample							
	count	mean	sd	p10	p25	p50	p75	p90
Overdue	1002292	0.262	0.439	0.000	0.000	0.000	1.000	1.000
Overdue Penalty	1002292	110726.297	301265.874	0.000	0.000	0.000	12841.000	502400.000
Default	1002292	0.117	0.321	0.000	0.000	0.000	0.000	1.000
Repayment Relative to Maturity	1002292	35.608	111.730	-9.000	-2.000	0.000	1.000	170.000
Alignment	1002292	0.506	0.500	0.000	0.000	1.000	1.000	1.000
Maturity Relative to Payday	1002292	0.436	8.454	-12.000	-6.000	1.000	7.000	12.000
Young	1002292	0.606	0.489	0.000	0.000	1.000	1.000	1.000
Female	1002292	0.482	0.500	0.000	0.000	0.000	1.000	1.000
Size	1002292	1.213	0.414	0.600	1.000	1.200	1.500	1.800
High Education	1002292	0.451	0.498	0.000	0.000	0.000	1.000	1.000
Married	1002292	0.524	0.499	0.000	0.000	1.000	1.000	1.000
Income	1002292	4.249	3.331	2.000	2.900	3.600	5.000	6.850
Credit Limit	820642	0.270	0.677	0.110	0.130	0.160	0.235	0.260
Behaviour	817719	0.548	0.042	0.496	0.518	0.546	0.576	0.606
Past Overdue Max	637840	0.243	0.460	0.000	0.000	0.000	0.000	1.099
Past Overdue	637840	0.198	0.452	0.000	0.000	0.000	0.000	0.800
Payday Alignment Experience	637840	0.331	0.326	0.000	0.000	0.250	0.500	1.000
Overdue Experience	637840	0.075	0.179	0.000	0.000	0.000	0.000	0.250
Disburse Experience	637840	1.464	0.804	0.000	0.693	1.609	2.079	2.485
Application Experience	637547	1.635	0.768	0.693	1.099	1.792	2.197	2.565
First-time	1002292	0.182	0.386	0.000	0.000	0.000	0.000	1.000

Table 2: Payday Alignment and Loan Performance

This table reports OLS difference-in-differences (DiD) estimates from the experimental sample covering April–October 2021. The unit of observation is a loan. Panel A includes loans originally maturing 0–3 days before each *Reference* date (5th, 10th, 25th, 28th) prior to the intervention; Panel B restricts to loans maturing 0–1 days before the *Reference* date. Columns (1)–(3) use the full sample; (4)–(6) restrict to the Match group (borrowers whose paydays coincide with the *Reference* date); and (7)–(9) restrict to the Placebo group (paydays at least 10 days away from the *Reference* date). *Treatment* equals one if the experiment intervention shifts maturity beyond the *Reference* date; *Match* equals one for the Match group and zero for the Placebo group. The coefficient of interest is *Treatment* × *Match*. Dependent variables are: *Overdue* (columns (1)(4)(7)), *DPD1* (columns (2)(5)(8)), and *DPD1t7* (columns (3)(6)(9)). All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, *Credit Limit*, *Income*, *Behaviour*, *Past Max Overdue*. All variables are defined in Table A2. Columns (1)–(3) include Disbursement Date × Match fixed effects (match × disbursement date), while (4)–(9) include disbursement date fixed effects. Standard errors are heteroskedasticity robust. t-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Panel A: Full Sample								
	DiD			Match Group			Placebo Group		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Overdue	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*
*Treatment*Match*	-0.059***	-0.084***	-0.064**						
	(-2.62)	(-3.12)	(-2.11)						
Treatment	0.005	0.008	-0.004	-0.054***	-0.076***	-0.068***	0.004	0.007	-0.007
	(0.35)	(0.47)	(-0.19)	(-3.14)	(-3.73)	(-3.11)	(0.27)	(0.40)	(-0.32)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dis. Date × Match FE	Yes	Yes	Yes	-	-	-	-	-	-
Disbursement Date FE	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2801	2801	2801	1408	1408	1408	1393	1393	1393
R^2	0.726	0.081	0.168	0.700	0.088	0.187	0.758	0.067	0.135
	Panel B: Subsample								
	DiD			Match Group			Placebo Group		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Overdue	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*
*Treatment*Match*	-0.057**	-0.083***	-0.059*						
	(-2.52)	(-3.07)	(-1.95)						
Treatment	0.003	0.007	-0.008	-0.053***	-0.075***	-0.066***	0.003	0.004	-0.008
	(0.22)	(0.42)	(-0.38)	(-3.10)	(-3.69)	(-3.04)	(0.19)	(0.24)	(-0.40)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dis. Date × Match FE	Yes	Yes	Yes	-	-	-	-	-	-
Disbursement Date FE	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1435	1435	1435	765	765	765	670	670	670
R^2	0.731	0.085	0.140	0.692	0.104	0.150	0.773	0.071	0.136

Table 3: Heterogeneous Effects

This table tabulates the heterogenous effect of the DiD test in Table 2 with respect to borrowers' age, income, credit limit, and prior overdue records. The unit of observation is at loan level. *Treatment* equals one if the experimental intervention shifts loan maturity beyond the *Reference* date and zero otherwise. *Match* equals one for the Match group and zero for the Placebo group. *Low Credit* equals one if borrower's last credit limit is lower than the sample median and zero otherwise. *High Past Overdue* equals one if the borrower's mean days past due across prior loans is higher than the sample median and zero otherwise. *Young* equals one if borrower is age 30 or younger at loan application. *High Income* equals one if borrower's monthly income at account registration is in the top decile of the sample. In Panel A, the coefficient of interest is the triple interaction *Young* × *Treatment* × *Match* in columns (1)–(3), and *High Income* × *Treatment* × *Match* in columns (4)–(6). In Panel B, the coefficient of interest is the triple interaction *Low Credit* × *Treatment* × *Match* in columns (1)–(3), and *High Past Overdue* × *Treatment* × *Match* in columns (4)–(6). All columns include lower level interactions. Dependent variables are *Overdue* in columns (1) and (4); *DPD1* in columns (2) and (5); and *DPD1t7* in columns (3) and (6). All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, *Credit Limit*, *Income*, *Behaviour*, *Past Max Overdue*. All variables are defined in Table A2. All columns include Disbursement Date × Match fixed effect. Standard errors are heteroskedasticity-consistent. Numbers in parentheses are t-statistics. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	Panel A: Age and Income					
	(1)	(2)	(3)	(4)	(5)	(6)
	Overdue	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*
Young × *Treatment* × *Match*	-0.080**	-0.086*	-0.122**			
	(-2.08)	(-1.89)	(-2.35)			
High Income × *Treatment* × *Match*				0.079	0.047	0.136*
				(1.36)	(0.69)	(1.71)
All Lower-level Interactions	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Dis. Date × Match FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2801	2801	2801	2801	2801	2801
R^2	0.727	0.082	0.170	0.727	0.083	0.169
	Panel B: Credit Limit and Overdue History					
	(1)	(2)	(3)	(4)	(5)	(6)
	Overdue	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*
Low Credit × *Treatment* × *Match*	-0.099**	-0.097*	-0.125**			
	(-2.23)	(-1.81)	(-2.09)			
High Past Overdue × *Treatment* × *Match*				-0.073**	-0.086**	-0.075
				(-2.11)	(-2.07)	(-1.57)
All Lower-level Interactions	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Dis. Date × Match FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2801	2801	2801	2801	2801	2801
R^2	0.729	0.090	0.178	0.730	0.091	0.180

Table 4: Hazard Model

This table tabulates experiment results estimated from the cox hazard model. The unit of observation is at loan-calendar-day level. We define repayment as the "failure" event and time-to-maturity as analysis time in the cox model. Estimated coefficients are expressed as the natural logarithm of the hazard ratios. Panel A uses the full sample, while Panel B restricts to loans originally maturing 0–1 days before the *Reference* date prior to experiment interventions. Column (1) reports results for the full DiD sample; column (2) restricts to the Match group, where borrowers' paydays coincide with the *Reference* date; and column (3) restricts to the Placebo group, where borrowers' paydays fall at least 10 days away from the *Reference* date. *Treatment* equals one if the experimental intervention shifts loan maturity beyond the *Reference* date and zero otherwise. *Match* equals one for the Match group and zero for the Placebo group. The coefficient of interest is the interaction *Treatment* × *Match*. All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, *Credit Limit*, *Income*, *Behaviour*, *Past Max Overdue*. All variables are defined in Table A2. Column (1) includes Disbursement Date × Match fixed effect, while columns (2)–(3) include disbursement date fixed effects. Standard errors are heteroskedasticity-robust. Numbers in parentheses are t-statistics. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	Panel A: Full Sample		
	(1)	(2)	(3)
	DiD	Match	Placebo
*Treatment*Match*	0.306***		
	(2.68)		
Treatment	-0.030	0.303***	-0.020
	(-0.32)	(4.28)	(-0.22)
Controls	Yes	Yes	Yes
Dis. Date × Match FE	Yes	-	-
Disbursement Date FE	-	Yes	Yes
Observations	29882	15091	14791
	Panel B: Subsample		
	(1)	(2)	(3)
	DiD	Match	Placebo
*Treatment*Match*	0.298***		
	(2.65)		
Treatment	-0.022	0.303***	-0.014
	(-0.24)	(4.48)	(-0.16)
Controls	Yes	Yes	Yes
Dis. Date × Match FE	Yes	-	-
Disbursement Date FE	-	Yes	Yes
Observations	15249	8009	7240

Table 5: Overdue, Penalty, and Default

This table reports the effect of *payday alignment* on loan outcomes estimated from OLS estimations based on administrative data from the lender. The sample includes loans disbursed from January 2018 to July 2020. The key independent variable of interest is *Alignment*, which equals one if the loan matures after the borrower's payday and zero otherwise. The dependent variables in columns (1)(2), (3)(4), and (5)(6) are (respectively) *Overdue* which equals one for overdue loans and zero otherwise, *Overdue Penalty* which include late interest charges and fees in Indonesia Rupiah, and *Default* which equals one for loans overdue by 30 days or more. All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, and *Income*. All variables are defined in Table A2. All columns include disbursement date fixed effects; even-numbered columns additionally include borrower fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the disbursement date level. Numbers in parentheses are t-statistics. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Overdue	*Overdue*	*Overdue Penalty*	*Overdue Penalty*	*Default*	*Default*
Alignment	-0.049***	-0.041***	-12515.174***	-5787.157***	-0.010***	-0.003***
	(-16.89)	(-20.35)	(-9.82)	(-6.08)	(-7.46)	(-3.98)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Disbursement Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Borrower FE	No	Yes	No	Yes	No	Yes
Observations	1002250	855711	1002250	855711	1002250	855711
R^2	0.028	0.403	0.013	0.343	0.032	0.347

Table 6: Subsequent Loan Approval

This table reports the effect of *payday alignment* on subsequent loan approval likelihood estimated from two stage least Squares (2SLS) estimations based on administrative data from the lender. The sample includes loans disbursed from January 2018 to July 2020. Columns (1) and (2) present the second stage results, where the dependent variable is *Next Approval*—an indicator equal to one if the borrower's subsequent loan application is approved and the independent variable of interest is $\hat{Overdue}$ predicted from the first stage regression of *Overdue* on *Alignment* as reported in columns (1) and (2) of Table 5. Columns (3) and (4) report the second stage results, where the dependent variable is *Next Approval* and the key independent variable is $\hat{Default}$ predicted from the first stage regression of *Default* on *Alignment*, as reported in columns (5) and (6) of Table 5. All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, and *Income*. All variables are defined in Table A2. All columns include disbursement date fixed effects; even-numbered columns additionally include borrower fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the disbursement date level. Numbers in parentheses are t-statistics. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)
	Next Approval	*Next Approval*	*Next Approval*	*Next Approval*
$\hat{Overdue}$	-0.213***	-0.134***		
	(-11.35)	(-6.06)		
$\hat{Default}$			-1.041***	-1.676***
			(-11.35)	(-6.06)
Controls	Yes	Yes	Yes	Yes
Disburse Day FE	Yes	Yes	Yes	Yes
Borrower FE	No	Yes	No	Yes
Observations	820404	723100	820404	723100
R^2	0.166	0.468	0.166	0.468

Table 7: Payday Alignment by Distance Bands

This table reports the potential nonlinear effect of *payday alignment* on loan outcomes estimated from OLS estimations based on administrative data from the lender. The sample includes loans disbursed from January 2018 to July 2020. The key independent variable of interest is *Close* (*Medium*, *Distant*), which equals one if the loan matures 1–3 (4–7, 8 or more) days after the borrower's payday and zero otherwise. The dependent variables in columns (1)(2), (3)(4), and (5)(6) are (respectively) *Overdue*, *Overdue Penalty*, and *Default*. All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, and *Income*. All variables are defined in Table A2. All columns include disbursement date fixed effects; even-numbered columns additionally include borrower fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the disbursement date level. Numbers in parentheses are t-statistics. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Overdue	*Overdue*	*Overdue Penalty*	*Overdue Penalty*	*Default*	*Default*
Close	-0.071***	-0.059***	-21978.598***	-11098.606***	-0.018***	-0.007***
	(-20.31)	(-22.57)	(-13.70)	(-8.35)	(-10.59)	(-6.06)
Medium	-0.051***	-0.044***	-11127.997***	-5151.795***	-0.009***	-0.003*
	(-16.76)	(-18.76)	(-7.44)	(-3.82)	(-5.28)	(-2.45)
Distant	-0.035***	-0.027***	-7595.708***	-2434.558*	-0.006***	-0.001
	(-11.85)	(-14.47)	(-5.00)	(-2.18)	(-3.48)	(-0.82)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Disbursement Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Borrower FE	No	Yes	No	Yes	No	Yes
Observations	1002250	855711	1002250	855711	1002250	855711
R^2	0.028	0.404	0.013	0.343	0.032	0.347

Table 8: First-time and Repeat Borrowing

This table reports the effect of *payday alignment* on loan outcomes for the first-time borrowing estimated from OLS estimations based on administrative data from the lender. The sample includes loans disbursed from January 2018 to July 2020. Odd-numbered columns focus on first-time borrowing and even-numbered columns focus on the full sample. The dependent variables in columns (1)(2), (3)(4), and (5)(6) are *Overdue* which equals one for overdue loans and zero otherwise, *Overdue Penalty* which include late interest charges and fees in Indonesia Rupiah, and *Default* which equals one for loans overdue by 30 days or more. The key independent variable of interest in odd-numbered columns is *Alignment*, which equals one if the loan matures after the borrower's payday and zero otherwise. The key independent variable of interest in even-numbered columns is the interaction term between *Alignment*, and *First-time* which equals one for first-time borrowing and zero otherwise. Control variables in all specifications are *Young*, *Female*, *Size*, *High Education*, *Married*, and *Income*. All variables are defined in Table A2. All columns include disbursement date fixed effects; even-numbered columns additionally include borrower fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the disbursement date level. Numbers in parentheses are t-statistics. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Overdue	*Overdue*	*Overdue Penalty*	*Overdue Penalty*	*Default*	*Default*
Alignment	-0.031***	-0.044***	-6460.344***	-7219.038***	-0.009***	-0.004***
	(-8.39)	(-21.06)	(-3.32)	(-7.45)	(-2.99)	(-5.40)
Alignment × *First*		0.033***		15377.539***		0.014***
		(7.25)		(5.26)		(4.78)
First-time		-0.130***		-69332.245***		-0.166***
		(-15.44)		(-12.02)		(-27.63)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Disbursement Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Borrower FE	No	Yes	No	Yes	No	Yes
Observations	182031	855711	182031	855711	182031	855711
R^2	0.032	0.406	0.028	0.346	0.041	0.355

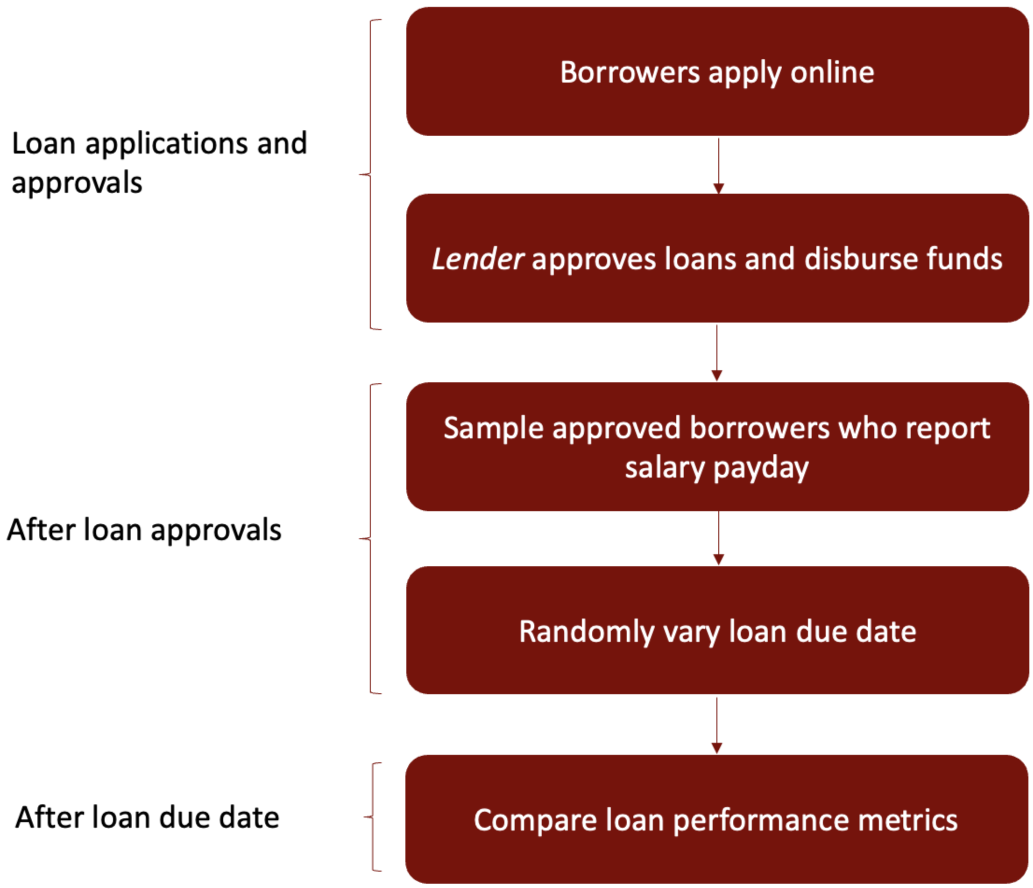
Table 9: Past Experiences

This table reports the relationship between *payday alignment* effects on loan outcomes and borrower past experiences, estimated by OLS on administrative data from the lender. The sample includes loans disbursed from January 2018 to July 2020. The dependent variable in all columns is *Overdue*, which equals one for overdue loans and zero otherwise. In columns (1)–(4), we interact *Alignment* (respectively) with the share of loans maturing after salary payday out of the total approved loans experienced by the borrower in the past (*Payday Alignment Experience*), the logarithm of the number of borrower's prior disbursements (*Disburse Experience*), the logarithm of the number of borrower's prior applications (*Application Experience*), and the share of overdue loans out of the total approved loans experienced by the borrower in the past (*Overdue Experience*). All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, and *Income*. All variables are defined in Table A2. All regressions include disbursement date and borrower fixed effects. Standard errors are clustered at the disbursement-day level. *, **, *** denote significance at the 10%, 5%, and 1% levels.

	(1) *Overdue*	(2) *Overdue*	(3) *Overdue*	(4) *Overdue*
Alignment × *Payday Alignment Experience*	-0.055***			
	(-12.45)			
Alignment × *Disburse Experience*		-0.004**		
		(-2.24)		
Alignment × *Application Experience*			-0.004*	
			(-1.87)	
Alignment × *Overdue Experience*				-0.103***
				(-7.70)
Alignment	-0.026***	-0.036***	-0.036***	-0.034***
	(-10.15)	(-8.89)	(-7.74)	(-16.46)
Controls	Yes	Yes	Yes	Yes
Disbursement Date FE	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes
Observations	564468	564468	564148	564468
R^2	0.411	0.417	0.417	0.439

Online Appendix

Figure A1: Experiment Timeline



This figure outlines the timeline of the experimental design.

Table A1: Experimental Design

This table illustrates the experimental design for *payday alignment* interventions. We sample approved loans originally maturing 0-3 days before each *Reference* date (5th, 10th, 25th, 28th). Panel A describes the Match and Treatment conditions. Panel B illustrates specific intervention scenarios for loans originally due 1 day before the *Reference* date of 5th. Shaded rows indicate placebo borrowers (Match equals 0).

Panel A

Individual Payday	Match	Maturity after Reference	Treatment	Economic Effect
Reference	1	No	0	Control
Reference	1	Yes	1	Control + Income + Liquidity
10 days from Reference	0	No	0	Placebo Control
10 days from Reference	0	Yes	1	Placebo Control + Income

Panel B

Reference	Individual Payday	Original Loan Due Date	Extension Days	Maturity	Maturity vs Individual Payday	Match	Maturity vs Reference	Treatment
5th	5th	4th	0	4th	-1	1	-1	0
5th	5th	4th	1	5th	0	1	0	0
5th	5th	4th	2	6th	1	1	1	1
5th	15th–25th	4th	0	4th	Far	0	-1	0
5th	15th–25th	4th	1	5th	Far	0	0	0
5th	15th–25th	4th	2	6th	Far	0	1	1

Table A2: Variable Definition

This table tabulates definitions of key variables.

Variable	Definition
Dependent Variables	
DPD1	A binary variable indicating that the number of days past due day equals 1
DPD1t7	A binary variable indicating that the number of days past due day ranges from 1 to 7
Overdue	A binary variable indicating that the number of days past due day is greater than 0
Default	A binary variable indicating that the number of days past due day is greater than 30
Overdue Penalty	Late interest charges and fees in Indonesia Rupiah
Next Approval	A binary variable that equals one if the subsequent loan application is approved and zero otherwise
Independent Variables	
Treatment	A binary variable indicating that experimental intervention shifts loan maturity beyond the *Reference* date
Alignment	A binary variable indicating that a borrower's payday falls before the loan maturity date
Match	A binary variable indicating that a borrower's payday matches the *Reference* date targeted in the experiment
Repay Relative to Maturity	Repayment date minus the maturity date
Close	A binary variable that equals one if the loan matures 1-3 days after the borrower's payday and zero otherwise
Medium	A binary variable that equals one if the loan matures 4-7 days after the borrower's payday and zero otherwise
Distance	A binary variable that equals one if the loan matures 8 or more days after the borrower's payday and zero otherwise
Low Credit	A binary variable indicating that the borrower's credit limit is lower than the sample median
High Past Overdue	A binary variable indicating that the borrower's mean days past due across prior loans is higher than the sample median
High Income	A binary variable indicating that the borrower's monthly income reported at account registration is in the top decile of the sample
Payday Alignment Experience	The share of loans maturing after salary payday out of the total approved loans experienced by the borrower in the past
Overdue Experience	The share of overdue loans out of the total approved loans experienced by the borrower in the past
Disburse Experience	Log (number of borrower's prior loan applications)
Application Experience	Log (number of borrower's prior loan disbursements)
First-time	A binary variable that equals one for first-time borrowing and zero otherwise
Control Variables	
Young	A binary variable indicating that the borrower is age 30 or younger at loan application
Female	A binary variable indicating that the borrower is a female
Size	Loan size (in Rp 1million)
High Education	A binary variable indicating that the borrower obtains a technical college, undergraduate or master degree
Married	A binary variable indicating that the borrower is married
Credit Limit	Borrowers' credit limit assigned by the lender
Income	Borrowers' monthly income (Rp 1million) reported at account registration
Behaviour	Borrowers' behaviour score assigned by the lender to assess fraud risk
Past Overdue	Mean days past due across the borrower's prior loans
Past Overdue Max	Log (1 + borrower's maximum days past due on any prior loan)

Table A3: The Balance of Characteristics by Extension Days

This table tabulates the balance of characteristics by the number of extension days. Variable definitions are listed in Appendix Table A2. Standard deviation is in the parentheses. P-value is the significance level of the difference in the mean across groups.

	Extension 0 Day	Extension 1 Day	Extension 2 Days	P-value
Size	1.304	1.309	1.325	0.577
	(0.440)	(0.440)	(0.435)	
Young	0.349	0.349	0.346	0.985
	(0.477)	(0.477)	(0.476)	
Female	0.430	0.402	0.408	0.426
	(0.495)	(0.490)	(0.492)	
Income	5.150	5.012	5.043	0.684
	(3.715)	(3.425)	(3.604)	
High Education	0.523	0.475	0.523	0.053
	(0.500)	(0.500)	(0.500)	
Married	0.543	0.557	0.554	0.823
	(0.498)	(0.497)	(0.497)	
Credit Limit	6.774	6.803	6.811	0.982
	(4.365)	(4.301)	(4.190)	
Behaviour	0.548	0.545	0.545	0.241
	(0.049)	(0.048)	(0.047)	
Past Max Overdue	0.364	0.357	0.337	0.774
	(0.832)	(0.869)	(0.850)	
Observations	893	991	925	

Table A4: Balance Tests

This table tabulates the balance of characteristics by *Treatment* following the baseline specification and table structure in Table 2. The dependent variable is *Treatment* and independent variables are borrower and loan characteristics. Variable definitions are listed in Appendix Table A2. The unit of observation is a loan. Columns (1) uses the full DiD sample; column (2) restricts to the Match group, where borrowers' paydays coincide with the *Reference* date; and column (3) restrict to the Placebo group, where borrowers' paydays fall at least 10 days away from the *Reference* date. The dependent variable is *Treatment* which equals one if loans mature after the *Reference* date following the experimental intervention and zero otherwise. The independent variables in all specifications include *Size*, *Young*, *Female*, *High Education*, *Married*, *Credit Limit*, *Income*, *Behaviour*, and *Past Max Overdue*. All variables are defined in Table A2. Column (1) includes Disbursement Date × Match fixed effect, while columns (2)–(3) include disbursement date fixed effects. Standard errors are heteroskedasticity-robust.

	DiD	Match Group	Placebo Group
	(1)	(2)	(3)
	Treatment	*Treatment*	*Treatment*
Size	-0.006	0.024	-0.035
	(-0.35)	(0.91)	(-1.48)
Young	0.018	0.043	-0.007
	(0.98)	(1.61)	(-0.30)
Female	-0.018	-0.011	-0.025
	(-1.29)	(-0.54)	(-1.32)
Income	-0.000	0.000	-0.002
	(-0.22)	(0.12)	(-0.59)
High Education	0.006	-0.006	0.020
	(0.43)	(-0.31)	(1.07)
Married	0.010	-0.000	0.023
	(0.67)	(-0.02)	(1.08)
Credit Limit	0.007	-0.001	0.015*
	(1.20)	(-0.07)	(1.90)
Behaviour	-0.068	-0.176	0.011
	(-0.43)	(-0.73)	(0.05)
Past Max Overdue	-0.001	0.002	-0.004
	(-0.14)	(0.19)	(-0.38)
Dis. Date × Match	Yes	No	No
Disbursement Date FE	-	Yes	Yes
Observations	2801	1408	1393
R^2	0.419	0.425	0.410

Table A5: Controlling for Extension Days

This table reports robustness checks for Table 2 with a control for *Extension Days*, defined as the experimental extension of loan maturities in days (0–2). Results are estimated from an OLS difference-in-differences (DiD) specification. The sample consists of approved loans maturing 0–3 days before each *Reference* date (5th, 10th, 25th, 28th). The unit of observation is a loan. Columns (1)–(3) use the full sample; (4)–(6) restrict to the Match group (borrowers whose paydays coincide with the *Reference* date); and (7)–(9) restrict to the Placebo group (paydays at least 10 days away from the *Reference* date). *Treatment* equals one if the experiment intervention shifts maturity beyond the *Reference* date; *Match* equals one for the Match group and zero for the Placebo group. The coefficient of interest is *Treatment* × *Match*. Dependent variables are: *Overdue* (columns (1)(4)(7)), *DPD1* (columns (2)(5)(8)), and *DPD1t7* (columns (3)(6)(9)). All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, *Credit Limit*, *Income*, *Behaviour*, *Past Max Overdue*. All variables are defined in Table A2. Columns (1)–(3) include Disbursement Date × Match fixed effects (match × disbursement date), while (4)–(9) include disbursement date fixed effects. Standard errors are heteroskedasticity robust. t-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	DiD			Match Group			Placebo Group		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Overdue	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*
*Treatment*Match*	-0.059***	-0.084***	-0.064**						
	(-2.63)	(-3.12)	(-2.12)						
Treatment	0.031*	0.013	0.045*	-0.020	-0.083***	-0.004	0.021	0.021	0.026
	(1.83)	(0.63)	(1.87)	(-0.93)	(-3.28)	(-0.15)	(1.18)	(0.99)	(1.05)
Extension Days	-0.018***	-0.003	-0.033***	-0.023**	0.005	-0.043***	-0.012	-0.010	-0.022**
	(-2.77)	(-0.40)	(-3.77)	(-2.24)	(0.44)	(-3.07)	(-1.49)	(-1.01)	(-2.10)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dis. Date × Match FE	Yes	Yes	Yes	-	-	-	-	-	-
Disbursement Date FE	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2801	2801	2801	1408	1408	1408	1393	1393	1393
R^2	0.727	0.081	0.173	0.701	0.088	0.193	0.758	0.067	0.138

Table A6: Two-sample Kolmogorov-Smirnov Test for Equality of the Distribution Functions of Days between Repayment Day and Maturity Date

This table tabulates results from a two-sample Kolmogorov-Smirnov test for the equality of the distribution functions of the days between repayment date and loan maturity date across the treatment and control group. Borrowers in the Match group have their payday matching *Reference* dates in the experiment and the payday of the Placebo group falls at least 10 days away from Reference. *Treatment* (*Control*) equals one (zero) for loans maturing after Reference and zero (one) otherwise.

	Match		Placebo	
	D	P-value	D	P-value
Control	0.031	0.580	0.007	0.978
Treatment	-0.155	0.000	-0.077	0.052
Combine K-S	0.155	0.000	0.077	0.105

Table A7: Alternative Definitions

This table reports robustness checks for Table 2 using alternative definitions of *Treatment*. Results are estimated from an OLS difference-in-differences (DiD) specification. The sample consists of approved loans maturing 0–3 days before each *Reference* date (5th, 10th, 25th, 28th). The unit of observation is a loan. Columns (1)–(3) use the full DiD sample; columns (4)–(6) restrict to the Match group, where borrowers' paydays coincide with the *Reference* date; and columns (7)–(9) restrict to the Placebo group, where borrowers' paydays fall at least 10 days away from the *Reference* date. In Panel A, *Treatment Alt* equals one if loans mature on or after the *Reference* date following the experimental intervention and zero otherwise. In Panel B, *Treatment Alt2* equals one if loans mature after the *Reference* date following a 1–2 day extension, is missing if loans mature on or before the *Reference* date after such an extension, and equals zero if loans receive no extension. The coefficient of interest is the interaction *Treatment* × *Match*. Dependent variables are *Overdue* in columns (1), (4), and (7); *DPD1* in columns (2), (5), and (8); and *DPD1t7* in columns (3), (6), and (9). All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, *Credit Limit*, *Income*, *Behaviour*, *Past Max Overdue*. All variables are defined in Table A2. Columns (1)–(3) include Disbursement Date × Match fixed effect, while columns (4)–(9) include disbursement date fixed effects. Standard errors are heteroskedasticity-robust.

	Panel A: Alternative Definition								
	DiD			Match Group			Placebo Group		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Overdue	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*
*Treatment Alt*Match*	-0.067***	-0.063**	-0.095***						
	(-2.76)	(-2.11)	(-2.89)						
Treatment Alt	-0.032**	-0.033*	-0.051***	-0.098***	-0.096***	-0.146***	-0.033**	-0.034*	-0.054***
	(-2.28)	(-1.93)	(-2.61)	(-4.87)	(-3.90)	(-5.50)	(-2.36)	(-1.96)	(-2.74)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dis. Date × Match FE	Yes	Yes	Yes	-	-	-	-	-	-
Disbursement Date FE	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2801	2801	2801	1408	1408	1408	1393	1393	1393
R^2	0.729	0.085	0.180	0.704	0.092	0.203	0.759	0.070	0.141

	Panel B: Alternative Definition2								
	DiD			Match Group			Placebo Group		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Overdue	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*
*Treatment Alt2*Match*	-0.070***	-0.099***	-0.077**						
	(-2.71)	(-3.10)	(-2.20)						
Treatment Alt2	-0.002	-0.002	-0.017	-0.073***	-0.100***	-0.094***	-0.006	-0.005	-0.022
	(-0.13)	(-0.10)	(-0.67)	(-3.82)	(-4.25)	(-3.81)	(-0.31)	(-0.24)	(-0.89)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dis. Date × Match FE	Yes	Yes	Yes	-	-	-	-	-	-
Disbursement Date FE	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1607	1607	1607	850	850	850	757	757	757
R^2	0.741	0.113	0.234	0.732	0.147	0.298	0.756	0.077	0.157

Table A8: Weekends and Holidays

This table reports robustness checks for Table 2 by excluding experiment samples where the *Reference* dates fall on a weekend or holiday and are adjusted to the preceding business day. Results are estimated from an OLS difference-in-differences (DiD) specification. The sample consists of approved loans maturing 0–3 days before each *Reference* date (5th, 10th, 25th, 28th). The unit of observation is a loan. Columns (1)–(3) use the full DiD sample. Columns (4)–(6) restrict to the Match group, where borrowers' paydays coincide with the *Reference* date. Columns (7)–(9) restrict to the Placebo group, where borrowers' paydays fall at least 10 days away from the *Reference* date. *Treatment* equals one when the experimental intervention shifts loan maturity past the *Reference* date and zero otherwise. The coefficient of interest is the interaction term *Treatment* × *Match*. The dependent variables are *Overdue* in columns (1), (4), and (7); *DPD1* in columns (2), (5), and (8); and *DPD1t7* in columns (3), (6), and (9). Control variables in all columns are *Young*, *Female*, *Size*, *High Education*, *Married*, *Credit Limit*, *Income*, *Behaviour*, and *Past Max Overdue*. All variables are defined in Table A2. Columns (1)–(3) include Disbursement Date × Match fixed effect, while columns (4)–(9) include disbursement date fixed effects. Standard errors are heteroskedasticity-robust.

	DiD			Match Group			Placebo Group		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Overdue	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*
*Treatment*Match*	-0.043*	-0.075***	-0.051						
	(-1.83)	(-2.60)	(-1.57)						
Treatment	-0.000	0.006	-0.008	-0.043**	-0.068***	-0.059**	-0.002	0.005	-0.012
	(-0.01)	(0.31)	(-0.32)	(-2.27)	(-2.84)	(-2.36)	(-0.15)	(0.36)	(-0.70)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dis. Date × Match FE	Yes	Yes	Yes	-	-	-	-	-	-
Disbursement Date FE	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2219	2219	2219	1134	1134	1134	1085	1085	1085
R^2	0.724	0.073	0.159	0.699	0.080	0.191	0.754	0.061	0.107

Table A9: Alternative Clustering Choices

This table reports robustness checks for Table 2 under an alternative clustering choice: standard errors are clustered by Disbursement Date × Match. Results are estimated from an OLS difference-in-differences (DiD) specification. The sample consists of approved loans maturing 0–3 days before each *Reference* date (5th, 10th, 25th, 28th). The unit of observation is a loan. Columns (1)–(3) use the full DiD sample. Columns (4)–(6) restrict to the Match group, where borrowers' paydays coincide with the *Reference* date. Columns (7)–(9) restrict to the Placebo group, where borrowers' paydays fall at least 10 days away from the *Reference* date. *Treatment* equals one when the experimental intervention shifts loan maturity past the *Reference* date and zero otherwise. The coefficient of interest is the interaction term *Treatment* × *Match*. The dependent variables are *Overdue* in columns (1), (4), and (7); *DPD1* in columns (2), (5), and (8); and *DPD1t7* in columns (3), (6), and (9). Control variables in all columns are *Young*, *Female*, *Size*, *High Education*, *Married*, *Credit Limit*, *Income*, *Behaviour*, and *Past Max Overdue*. All variables are defined in Table A2. Columns (1)–(3) include Disbursement Date × Match fixed effect, while columns (4)–(9) include disbursement date fixed effects.

	DiD			Match Group			Placebo Group		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Overdue	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*	*Overdue*	*DPD1*	*DPD1t7*
*Treatment**Match*	-0.059***	-0.084***	-0.064**						
	(-2.71)	(-3.20)	(-2.26)						
Treatment	0.005	0.008	-0.004	-0.054***	-0.076***	-0.068***	0.004	0.007	-0.007
	(0.45)	(0.60)	(-0.26)	(-2.92)	(-3.40)	(-2.90)	(0.34)	(0.51)	(-0.43)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dis. Date × Match FE	Yes	Yes	Yes	-	-	-	-	-	-
Disbursement Date FE	-	-	-	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2801	2801	2801	1408	1408	1408	1393	1393	1393
R^2	0.726	0.081	0.168	0.700	0.088	0.187	0.758	0.067	0.135

Table A10: Probit Model

This table reports marginal effects from Probit regressions as a robustness check for the OLS DiD estimation reported in Table 2. The sample consists of approved loans maturing 0–3 days before each *Reference* date (5th, 10th, 25th, 28th). The unit of observation is a loan. Columns (1)–(3) use the full DiD sample. *Treatment* equals one when the experimental intervention shifts loan maturity past the *Reference* date and zero otherwise. The coefficient of interest is the interaction term *Treatment* × *Match*. The dependent variables are *Overdue* in column (1), *DPD1* in column (2), and *DPD1t7* in column (3). Control variables in all columns are *Young*, *Female*, *Size*, *High Education*, *Married*, *Credit Limit*, *Income*, *Behaviour*, and *Past Max Overdue*. All variables are defined in Table A2. All columns include Disbursement Date × Match fixed effect. Standard errors are heteroskedasticity-robust.

	DiD		
	(1) *Overdue*	(2) *DPD1*	(3) *DPD1t7*
*Treatment*Match*	-0.039**	-0.106***	-0.073**
	(0.017)	(0.034)	(0.034)
Treatment	0.004	0.015	0.043
	(0.006)	(0.027)	(0.028)
Controls	Yes	Yes	Yes
Dis. Date × Match FE	Yes	Yes	Yes
Observations	2686	2228	2442

Table A11: Summary Statistics by First-time at Loan Level

This table reports loan-level summary statistics for key variables for the administrative sample. Panel A includes repeat borrowing only. Panel B includes first-time borrowing only. All variables are defined in Table A2.

	Panel A: Repeat							
	count	mean	sd	p10	p25	p50	p75	p90
Overdue	820241	0.241	0.428	0.000	0.000	0.000	0.000	1.000
Overdue Penalty	820241	103042.948	307641.483	0.000	0.000	0.000	0.000	260598.000
Default	820241	0.092	0.289	0.000	0.000	0.000	0.000	0.000
Repayment Relative to Maturity	820241	24.553	91.316	-8.000	-2.000	0.000	0.000	15.000
Alignment	820241	0.508	0.500	0.000	0.000	1.000	1.000	1.000
Maturity Relative to Payday	820241	0.495	8.357	-12.000	-6.000	1.000	7.000	12.000
Young	820241	0.604	0.489	0.000	0.000	1.000	1.000	1.000
Female	820241	0.479	0.500	0.000	0.000	0.000	1.000	1.000
Size	820241	1.305	0.383	0.600	1.200	1.200	1.500	1.800
High Education	820241	0.453	0.498	0.000	0.000	0.000	1.000	1.000
Married	820241	0.524	0.499	0.000	0.000	1.000	1.000	1.000
Income	820241	4.246	3.310	2.000	2.900	3.600	5.000	6.737
Credit Limit	817968	0.270	0.678	0.110	0.130	0.160	0.235	0.260
Behaviour	817624	0.548	0.042	0.496	0.518	0.546	0.576	0.606
Past Overdue Max	637840	0.243	0.460	0.000	0.000	0.000	0.000	1.099
Past Overdue	637840	0.198	0.452	0.000	0.000	0.000	0.000	0.800
Payday Alignment Experience	637840	0.331	0.326	0.000	0.000	0.250	0.500	1.000
Overdue Experience	637840	0.075	0.179	0.000	0.000	0.000	0.000	0.250
Disburse Experience	637840	1.464	0.804	0.000	0.693	1.609	2.079	2.485
Application Experience	637547	1.635	0.768	0.693	1.099	1.792	2.197	2.565
	Panel B: First-time							
Overdue	182051	0.354	0.478	0.000	0.000	0.000	1.000	1.000
Overdue Penalty	182051	145344.053	267966.240	0.000	0.000	0.000	113400.000	502400.000
Default	182051	0.230	0.421	0.000	0.000	0.000	0.000	1.000
Repayment Relative to Maturity	182051	85.417	167.711	-11.000	-3.000	0.000	10.000	414.000
Alignment	182051	0.499	0.500	0.000	0.000	0.000	1.000	1.000
Maturity Relative to Payday	182051	0.170	8.870	-12.000	-8.000	0.000	8.000	12.000
Young	182051	0.618	0.486	0.000	0.000	1.000	1.000	1.000
Female	182051	0.494	0.500	0.000	0.000	0.000	1.000	1.000
Size	182051	0.797	0.265	0.600	0.600	0.600	1.000	1.200
High Education	182051	0.441	0.497	0.000	0.000	0.000	1.000	1.000
Married	182051	0.525	0.499	0.000	0.000	1.000	1.000	1.000
Income	182051	4.266	3.425	1.800	2.800	3.600	5.000	7.000

Table A12: Cash Flow

This table reports effect of *payday alignment* on overdue loan amount estimated from OLS estimations based on administrative data from the lender. The sample includes loans disbursed from January 2018 to July 2020. The dependent variables are *Overdue Amount* which include overdue loan principal, interest charges and fees in Indonesia Rupiah. The key independent variable of interest is *Alignment*, which equals one if the loan matures after the borrower's payday and zero otherwise. All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, and *Income*. All variables are defined in Table A2. Column (1) includes disbursement date fixed effects and and column (2) additionally includes borrower fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the disbursement date level. Numbers in parentheses are t-statistics. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	(1)	(2)
	Overdue Amount	*Overdue Amount*
Alignment	-74028.643***	-65828.519***
	(-16.83)	(-19.70)
Controls	Yes	Yes
Disbursement Date FE	Yes	Yes
Borrower FE	No	No
Observations	1002250	855711
R^2	0.037	0.390

Table A13: Distance Bands for Repeat Borrowing

This table reports the potential nonlinear effect of *payday alignment* on loan outcomes for repeat borrowing estimated from OLS estimations based on administrative data from the lender. The sample includes loans disbursed from January 2018 to July 2020. The key independent variable of interest is *Close* (*Medium*, *Distant*), which equals one if the loan matures 1–3 (4–7, 8 or more) days after the borrower's payday and zero otherwise. The dependent variable is *Overdue*. All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, and *Income*. All variables are defined in Table A2. Column (1) includes disbursement date fixed effects and and column (2) additionally includes borrower fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the disbursement date level. Numbers in parentheses are t-statistics. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	(1)	(2)
	Overdue	*Overdue*
Close	-0.075***	-0.062***
	(-20.68)	(-23.11)
Medium	-0.055***	-0.045***
	(-17.35)	(-18.14)
Distant	-0.038***	-0.028***
	(-12.88)	(-14.02)
Controls	Yes	Yes
Disbursement Date FE	Yes	Yes
Borrower FE	No	Yes
Observations	820196	721203
R^2	0.026	0.406

Table A14: First-time Borrowing Robustness

This table reports the effect of *payday alignment* on loan outcomes for the first-time borrowing estimated from OLS estimations based on administrative data from the lender. The sample includes loans disbursed from January 2018 to July 2020. The dependent variables are *Overdue* which equals one for overdue loans and zero otherwise, *Overdue Penalty* which include late interest charges and fees in Indonesia Rupiah, and *Default* which equals one for loans overdue by 30 days or more. The key independent variable of interest in odd-numbered columns is *Alignment*, which equals one if the loan matures after the borrower's payday and zero otherwise. The key independent variable of interest in even-numbered columns is the interaction term between *Alignment*, and *First-time* which equals one for first-time borrowing and zero otherwise. Control variables in all specifications are *Young*, *Female*, *Size*, *High Education*, *Married*, and *Income*. All variables are defined in Table A2. All columns include disbursement date fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the disbursement date level. Numbers in parentheses are t-statistics. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)
	Overdue	*Overdue Penalty*	*Default*
Alignment × *First*	0.018***	4262.779**	-0.004
	(4.90)	(2.00)	(-1.28)
Alignment	-0.053***	-13381.718***	-0.010***
	(-17.31)	(-10.17)	(-7.76)
First-time	0.201***	39167.480***	0.241***
	(20.00)	(7.13)	(33.94)
Controls	Yes	Yes	Yes
Disbursement Date FE	Yes	Yes	Yes
Observations	1002250	1002250	1002250
R^2	0.035	0.020	0.050

Table A15: Heterogeneous Effects Coefficients

This table reports the underlying regression coefficients for Figure 6. This table examines heterogeneous effects of *Alignment* on overdue outcomes in the administrative sample, following the specification in column (2) of Table 5. The sample period spans 2018–2020. Columns (1)-(3) interact *Alignment* with quintile dummies for age, income, and credit limit, respectively. In all columns, Quintile 5—borrowers older than 35, in the top income group, and with the highest credit limit—is the omitted category. All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, and *Income*. All variables are defined in Table A2. All columns include disbursement date and borrower fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the disbursement date level. Numbers in parentheses are t-statistics. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)
	Age	Income	Credit Limit
Q1 × *Alignment*	-0.048***	-0.008**	-0.014***
	(-8.44)	(-2.51)	(-3.50)
Q2 × *Alignment*	-0.035***	-0.015***	-0.023***
	(-11.26)	(-4.16)	(-6.32)
Q3 × *Alignment*	-0.018***	-0.029***	-0.023**
	(-6.20)	(-8.78)	(-2.37)
Q4 × *Alignment*	-0.004	-0.021***	-0.019***
	(-1.37)	(-6.12)	(-5.48)
Alignment	-0.023***	-0.025***	-0.026***
	(-9.28)	(-9.24)	(-11.06)
Controls	Yes	Yes	Yes
Disburse Day FE	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes
Observations	855711	855711	855711
R^2	0.403	0.403	0.412

Table A16: Other Robustness Checks

This table reports the robustness checks based on administrative data from the lender. The sample includes loans disbursed from January 2018 to July 2020. Column (1) drops the COVID period which started in March 2020 in Indonesia. Column (2) redefines *Alignment* which equals one for loans maturing on or after salary payday and zero otherwise. Column (3) redefines *Default* that equlas one for loans overdue by 90 or more days. All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, and *Income*. All variables are defined in Table A2. All columns include disbursement date and borrower fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the disbursement date level. Numbers in parentheses are t-statistics. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	(1) *Overdue* (Pre-COVID)	(2) *Overdue*	(3) *Default Alt.*
Alignment	-0.042***		-0.003***
	(-19.31)		(-3.72)
Alignment Alt.		-0.047***	
		(-20.58)	
Controls	Yes	Yes	Yes
Disbursement Date FE	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes
Observations	796078	855711	855711
R^2	0.410	0.404	0.347

Table A17: Double Clustering

This table reports the effect of *payday alignment* on loan outcomes estimated from OLS estimations based on administrative data from the lender. The sample includes loans disbursed from January 2018 to July 2020. The key independent variable of interest is *Alignment*, which equals one if the loan matures after the borrower's payday and zero otherwise. The dependent variables in columns (1)(2), (3)(4), and (5)(6) are (respectively) *Overdue* which equals one for overdue loans and zero otherwise, *Overdue Penalty* which include late interest charges and fees in Indonesia Rupiah, and *Default* which equals one for loans overdue by 30 days or more. All regressions control for *Size*, *Young*, *Female*, *High Education*, *Married*, and *Income*. All variables are defined in Table A2. All columns include disbursement date fixed effects; even-numbered columns additionally include borrower fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the disbursement date and borrower level. Numbers in parentheses are t-statistics. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Overdue	*Overdue*	*Overdue Penalty*	*Overdue Penalty*	*Default*	*Default*
Alignment	-0.049***	-0.041***	-12515.174***	-5787.157***	-0.010***	-0.003***
	(-16.76)	(-22.08)	(-9.82)	(-6.18)	(-7.45)	(-3.98)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Disbursement Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Borrower FE	No	Yes	No	Yes	No	Yes
Observations	1002250	855711	1002250	855711	1002250	855711
R^2	0.028	0.403	0.013	0.343	0.032	0.347