Self-Control and Commitment in Consumer Credit Markets*

Ke Shi[1] and Yi Xin[2]

[1]Shanghai Advanced Institute of Finance (SAIF), SJTU
[2]California Institute of Technology

June 20, 2026

Abstract

This paper studies whether commitment devices mitigate self-control problems in consumer credit markets. Using data from a major fintech lender, we analyze a refinancing policy that introduced a "direct-pay" option, under which funds are sent directly to creditors rather than borrowers. A difference-in-differences design shows that the policy reduces defaults among eligible borrowers by 11%, while adopters cut default risk by roughly half. We then estimate a structural model of borrowing and repayment behavior. Counterfactuals suggest that self-control frictions account for about 35% of pre-policy defaults. Net of borrower selection and interest-rate incentives, commitment meaningfully improves repayment outcomes.

*Shi (corresponding author): Shanghai Advanced Institute of Finance (SAIF), Shanghai Jiao Tong University, 211 West Huaihai Road, Shanghai, China 200030. Email: kshi@saif.sjtu.edu.cn. Xin: Division of the Humanities and Social Sciences, California Institute of Technology, 1200 East California Blvd, MC 228-77, Pasadena, CA 91125, USA. Email: yixin@caltech.edu. We are grateful to Kasper Meisner Nielsen for insightful discussion. We also thank seminar and conference participants at Caltech, SAIF, Asia-Pacific Industrial Organisation Conference, International Industrial Organization Conference, Econometric Society North America Summer Meeting, Econometric Society CSW Asia Meeting, Five-Star Workshop in Finance, and HKUST Finance Symposium.

1 Introduction

Personal and household debt in the United States has increased substantially in recent decades, drawing attention to household financial vulnerability. By mid-2024, total household debt reached a record $4.9 trillion (excluding housing debt), with credit card balances alone surpassing $1.1 trillion.[1] This has fueled a boom in the consumer finance market for *debt refinancing* (or *debt consolidation*), as households increasingly turn to personal loans to consolidate high-interest obligations.[2] This segment has also experienced record growth, with unsecured personal loan balances reaching $245 billion by early 2024.[3] While often marketed as a tool for financial prudence, this borrow-to-repay behavior can paradoxically lead to a "debt trap." Borrowers may consolidate their debts only to immediately accumulate new ones, often due to underlying issues with overspending.[4] We study whether persistent borrow-to-repay cycles reflect behavioral frictions, most notably self-control problems that can widen the gap between a borrower's long-run financial intentions and short-run spending impulses, rather than financial literacy deficits alone.

While classical models of credit markets focus on asymmetric information in the form of adverse selection and moral hazard (Akerlof, 1970; Stiglitz and Weiss, 1981), self-control problems represent a fundamentally different friction. Rather than arising from conflicts of interest between lenders and borrowers, self-control reflects an internal conflict within the borrower between immediate temptation and long-run financial health. For borrowers susceptible to self-control failures, this tension *aligns* the incentives of principal and agent, as both lenders, who seek to reduce default risk, and borrowers, who value financial stability, share an interest in mitigating it. This paper provides new field evidence on this friction by studying a policy innovation from a large fintech lending platform. To combat the misuse of funds, the platform introduced a "direct-pay" option for borrowers seeking to refinance existing debt. This option transfers the loan proceeds directly to the borrower's existing creditors, such as commercial banks or credit card companies, bypassing the borrower's bank account. By limiting borrowers' discretion over

[1]See the Quarterly Report on Household Debt and Credit by the Federal Reserve Bank of New York: https://www.newyorkfed.org/microeconomics/hhdc.

[2]Debt refinancing differs from a "balance transfer" in the context of credit card lending, which is typically offered by commercial banks to attract customers from competitors. Debt-refinancing loans are unsecured personal loans and are primarily offered by digital lenders and fintech companies such as SoFi.

[3]See the Q1 2024 Quarterly Credit Industry Insights Report (CIIR) by TransUnion: https://www.transunion.com/blog/q1-2024-credit-industry-insights-report.

[4]For example, the approval of P2P lending has been shown to lead to more bankruptcy filings, suggesting that borrowers may overextend themselves financially and fall into debt traps (Wang and Overby, 2022).

the use of funds, it functions as a commitment device to curb impulsive spending, with an accompanying interest-rate discount designed to incentivize take-up.[5]

We develop a theoretical framework to formalize this institutional setting, focusing on three core components: the self-control problem, the commitment device, and contract design under asymmetric information. We apply the classic quasi-hyperbolic discounting framework (Laibson, 1997; O'Donoghue and Rabin, 1999) to a multi-period debt refinancing decision. This captures the central behavioral friction: a conflict between the borrower's ex-ante "planner" self at $t = 0$, who intends to use the loan responsibly, and the ex-post "doer" self at $t = 1$, who receives the cash and is tempted by immediate consumption. Our model formalizes this preference reversal, showing how the doer self can subvert the planner's optimal intention. The direct-pay option functions as a commitment device by allowing the planner self to preemptively restrict the future self's choice set. Following the behavioral literature, we consider sophisticated borrowers who are aware of their own time-inconsistency and able to anticipate their preference reversal. This sophistication generates an endogenous demand for commitment: the planner self, anticipating the doer's costly mistake, voluntarily chooses the direct-pay option at $t = 0$, thereby aligning the ex-post action with the ex-ante plan.

Beyond being a behavioral intervention, the direct-pay option also functions as a screening device in a market with asymmetric information. We model a borrower's unobserved "type" as their private cost of default. Borrowers with high default costs are more creditworthy; those with low default costs are more prone to default. The direct-pay option can screen on this dimension because high- and low-default-cost borrowers value the menu differently: those with high default costs place greater weight on repayment incentives, whereas those with low default costs, for whom default is relatively cheap, gain less and prefer cash, which preserves discretionary spending. In the model, under frictionless sorting on default cost, high-default-cost borrowers disproportionately choose direct-pay.

The theoretical framework yields clear, testable implications for borrower sorting and repayment outcomes, which we examine using loan-level data from the platform covering the period before (2017) and after (2018) the policy's introduction. We first estimate an intent-to-treat (ITT) effect of policy availability using a difference-in-differences (DiD) design. The direct-pay menu was offered only for debt-refinancing loans (treated) and not for general-purpose consumption loans (control). Eligibility is the treatment regardless of adoption. Contemporaneous general-purpose loans on the same platform supply the

[5]Our study focuses on the early experimentation stage of the policy in 2018. The direct-pay option has since become an industry-standard feature among fintech lenders, e.g., SoFi; see https://www.sofi.com/personal-loans/credit-card-consolidation-loans/.

counterfactual trend, identifying how eligible borrowers' defaults changed from 2017 to 2018 net of platform-wide drift among borrowers not offered the direct-pay menu in the same origination months. We find that the introduction of the direct-pay option significantly reduced the one-year default probability among eligible borrowers by 0.6 percentage points (roughly 11% relative to the pre-policy mean). The estimated effect is largest among borrowers with lower FICO scores but higher incomes, consistent with the policy targeting those with the means to repay but lacking the discipline to do so, rather than those who are simply liquidity constrained.

We then compare default rates between borrowers who adopt the direct-pay option and those who choose the cash option among eligible loans. The one-year default rate among direct-pay borrowers is 2.6%, substantially lower than that of cash borrowers at 6.2%. This gap reflects a combination of behavioral improvement, the interest-rate discount offered to direct-pay borrowers, and the selection of more creditworthy borrowers into direct-pay. To disentangle these channels, we use the 2017 pre-policy cohort of eligible borrowers, when only cash disbursement was available, as the product-specific benchmark. We find that 2018 cash borrowers perform significantly worse than this 2017 pool, consistent with adverse selection into the remaining cash pool as higher-quality borrowers sort into direct-pay, as predicted by the model. The improvement among 2018 direct-pay adopters exceeds what this sorting alone can explain. A back-of-the-envelope decomposition attributes about one-quarter of the cash–direct gap to selection and about three-quarters to a residual adopter effect that combines commitment and pricing.

Building on these empirical findings, we develop and estimate a dynamic structural model of borrowers' disbursement and repayment choices. In the model, borrowers have a private cost of default and choose between the cash option and the direct-pay option at loan origination. The key trade-off is that the cash option permits immediate discretionary spending but increases the debt burden in subsequent repayment periods. After loan origination, borrowers make monthly repayment decisions within a dynamic discrete choice framework. In estimation, we incorporate rich borrower and loan characteristics that affect both the distribution of borrower types and their propensity to consume.

We estimate the structural parameters using the 3-year loan subsample and match four key moments that capture the policy's impact: the 2017 pre-policy default rate as a baseline, the 2018 direct-pay take-up rate, which identifies selection, and the separate 2018 default rates for cash and direct-pay borrowers, which allow us to disentangle selection from behavioral effects. Based on our estimates, the default cost is \$2.7 thousand for low-type borrowers and \$4.5 thousand for high-type borrowers. As expected, borrowers with higher annual income, higher FICO scores, and longer credit histories are more likely to be creditworthy. We also find a model-implied average discretionary share of 33.0% of cash loan proceeds, which amplifies future repayment burdens and contributes to default risk in the absence of commitment.

Using our structural estimates, we conduct a set of counterfactual experiments to (1) quantify the role of self-control problems, (2) evaluate the overall impact of the direct-pay option and decompose its selection, commitment, and pricing effects, and (3) assess alternative contract designs. Counterfactual simulations indicate that self-control failures account for approximately 35% of all defaults in the pre-policy market. The direct-pay policy lowers the simulated aggregate default rate from 5.75% to 5.00%. Decomposing the 3.18 percentage-point cash–direct gap under the current policy, the model attributes about 8% to selection, with the remainder explained by behavioral commitment and interest-rate discounts. This ranking is consistent with the reduced-form decomposition, which likewise attributes the majority of the gap to policy-induced improvement rather than selection. Holding sorting fixed, behavioral commitment accounts for 0.44 percentage points of the 0.75 percentage-point reduction in the overall default rate, while interest-rate discounts account for the remaining 0.31 percentage points.

We then evaluate alternative contract designs. We first consider direct-pay without an interest-rate discount, letting adoption respond endogenously to the menu. Take-up falls only slightly when the discount is removed, indicating that the interest-rate discount is not a major driver of adoption; direct-pay nonetheless improves repayment through the commitment channel. We next require all eligible borrowers to use direct-pay, eliminating the cash option entirely. This mandatory routing substantially reduces defaults, but removes liquidity that some borrowers may need for legitimate purposes. We also simulate an upfront adoption bonus for borrowers who choose direct-pay, modeled as a reduction in the taste for immediate cash. Because present-biased borrowers overweight short-run rewards, this design raises take-up more sharply than the interest-rate discount while also improving repayment. Finally, we reduce borrowers' propensity to divert loan proceeds to discretionary spending, capturing policies that limit impulsive use of cash at the moment of temptation. This intervention improves outcomes, though by less than full commitment routing.

Related literature. Our research is situated at the intersection of three related streams of literature: (1) the broad and growing field of behavioral household finance, which identifies behavioral frictions as important drivers of financial outcomes; (2) the theoretical and empirical work on commitment devices as a solution to self-control problems; and (3) the classic literature on contract design and screening under asymmetric information.

This paper adds to the growing literature on behavioral household finance, which has moved beyond traditional life-cycle models to incorporate psychological frictions in explaining household financial decisions (Campbell, 2006; Hirshleifer, 2015; Zinman, 2015; Campbell and Ramadorai, 2026). Seminal reviews by Beshears et al. (2018) and Gomes, Haliassos, and Ramadorai (2021) document a wide range of suboptimal behaviors, including in household liability management, that are difficult to reconcile with the standard model. Our work focuses on the unsecured personal loan market for debt refinancing, a fast-growing sector fueled by the growth of household debt and reshaped by fintech platforms (Fuster et al., 2019; Buchak et al., 2018; Jagtiani, Lemieux, and Goldstein, 2023).

The specific friction we analyze is the self-control problem, rooted in the "planner–doer" framework of time-inconsistent preferences (Thaler and Shefrin, 1981; Laibson, 1997; O'Donoghue and Rabin, 1999). This conflict can lead to over-borrowing and default. Empirical work establishes that present-biased preferences correlate with credit card borrowing and debt accumulation (Meier and Sprenger, 2010; Meier and Sprenger, 2013; Allcott et al., 2022; Kuchler and Pagel, 2021; deHaan et al., 2024), and that self-control predicts financial behavior and well-being (Gathergood, 2012; Strömbäck et al., 2017; DellaVigna, 2009). The theoretical literature shows that firms design contracts to exploit these biases (DellaVigna and Malmendier, 2004; Heidhues and Kőszegi, 2010), while consumers may demand commitment devices to constrain future choices (Gul and Pesendorfer, 2001; Galperti, 2015; Toussaert, 2018; Attanasio, Kovacs, and Moran, 2020; Martinez, Meier, and Sprenger, 2023; Exler et al., 2025). We provide large-scale field evidence on whether a cash-versus-direct-pay menu at origination curbs impulsive spending on loan proceeds and improves repayment, and we quantify how present-biased diversion contributes to default.

A second stream of literature examines commitment devices as a solution for sophisticated agents who are aware of their self-control problems (Bryan, Karlan, and Nelson, 2010). This literature provides extensive field evidence showing that voluntary commitment mechanisms can improve outcomes in domains like workplace productivity (Kaur, Kremer, and Mullainathan, 2015; Augenblick, Niederle, and Sprenger, 2015), health (Giné, Karlan, and Zinman, 2010; Schilbach, 2019; Derksen et al., 2025; Avery, Giuntella, and Jiao, 2025), and savings (Thaler and Benartzi, 2004; Ashraf, Karlan, and Yin, 2006; Beshears et al., 2020). This literature is thinner in the context of debt repayment. A closely related paper by Vihriälä (2023) finds that households voluntarily forgo payment flexibility to create self-imposed liquidity constraints. By contrast, we study lender-designed upfront commitment at origination on refinancing loans and use reduced-form and structural evidence to separate behavioral gains from borrower sorting and take-up incentives.

Finally, our work integrates this behavioral framework with the classic literature on asymmetric information and screening in credit markets. Beginning with Akerlof (1970), Rothschild and Stiglitz (1976), and Stiglitz and Weiss (1981), this literature demonstrates how lenders use contract terms to screen unobserved borrower "types" and mitigate adverse selection. This remains a central theme in modern consumer finance, where contract menus (e.g., loan maturity or pricing) are designed to induce separation (Einav, Jenkins, and Levin, 2012; Galperti, 2015; Hertzberg, Liberman, and Paravisini, 2018; Kawai, Onishi, and Uetake, 2022; Yannelis and Zhang, 2023; Agarwal et al., 2023; Polo, Taburet, and Vo, 2025; Xin, 2025). Our model allows self-control problems alongside unobserved default costs and treats the disbursement menu as both a screening device and a commitment device, jointly quantifying selection and behavioral channels that align borrower and lender incentives.

Outline. The remainder of the paper is organized as follows. Section 2 describes the institutional setting, the direct-pay disbursement policy, and our loan-level data. Section 3 develops a theoretical framework in which the direct-pay option serves both as a commitment device for borrowers and as a screening device under asymmetric information. Section 4 presents reduced-form evidence on direct-pay adoption, the effect of policy availability, and the selection and behavioral channels. Section 5 develops and estimates a dynamic structural model and reports counterfactual experiments. Section 6 concludes.

2 Institutional Background and Data

2.1 Personal Loans

We draw on data from a large U.S. online fintech company (the "*platform*") that offers unsecured personal loans. The platform initially focused on lower-prime borrowers with limited access to brick-and-mortar bank credit, but in recent years has expanded to a broader customer base with stronger credit profiles. Borrowers can request unsecured loans of up to $40,000 with terms of either 3 or 5 years.[6] Loans feature fixed interest rates and equal monthly installments determined at origination. The platform charges an origination fee as a percentage of the loan amount, while interest income accrues to

[6] A concern is that opportunistic borrowers might exploit fintech lending by borrowing with the deliberate intention of defaulting. To mitigate this risk, the platform requires borrowers to have at least three years of credit history and a minimum FICO score of 660 (often considered "fair" or "good" on the 300-850 scale). In the data, very few borrowers default in the first payment period, indicating that the borrower pool is reasonably creditworthy.

investors.[7]

To apply for a loan, a prospective borrower first specifies the desired amount and term, then provides additional information to the platform, including the stated purpose of the loan, home address, social security number, employment details, and income. Around 82% of borrowers on the platform seek loans to refinance existing debts, meaning they already hold credit card balances or personal loans and are borrowing to repay these obligations. This borrow-to-repay behavior can arise for several reasons, such as short-term liquidity needs, more favorable terms offered by the platform, or a desire to consolidate multiple debts for easier management. The platform does not require borrowers to justify their specific motivation for debt refinancing.

The platform then retrieves the borrower's credit report from a consumer credit reporting agency to determine loan eligibility. If the borrower meets the underwriting criteria, the platform sets the interest rate using an internal algorithm based on the borrower's credit history and repayment capacity. After observing the quoted rate, the borrower decides whether to accept the terms. If the loan is accepted, the listing is posted on the platform's website, where individual (retail) investors can browse approved loans and invest based on the borrower's information. A key feature of this fintech platform is that investors can purchase fractional shares of loans, and a loan is originated only when it is fully funded.

2.2 Direct-Pay Disbursement

If a borrower's loan application is approved, the loan amount is deposited into their bank account. Afterward, there is no effective mechanism for the platform or lenders to verify whether the funds are actually used for the stated purpose at application. Borrowers may genuinely intend to consolidate existing debts, but once they receive the cash, they can divert the money to other uses. This behavior can undermine their financial health and increase default risk for lenders.

To combat loan misuse and help borrowers follow through on their initial intentions, the platform introduced a new policy. Traditionally, loans were disbursed as cash deposits into the borrower's personal bank account. In 2018, the platform added an alternative disbursement method, the *direct-pay* option, which wires the loan proceeds directly to the borrower's other creditors, such as banks or credit card issuers. In practice, after

[7] This study focuses on the borrower side of the market, examining self-control problems and borrowers' commitment to responsible financial behavior. We abstract from the incentives and behavior of the platform and investors. Although the platform does not earn interest income directly, it remains strongly incentivized to screen for creditworthy borrowers to preserve its appeal to investors.

submitting a loan request and the required information, borrowers are offered a choice between cash and direct-pay. The cash option works as before, while the direct-pay option requires borrowers to provide account details for their third-party creditors so that the platform can transfer funds on their behalf.

To encourage take-up of the direct-pay option, the platform offers an interest rate discount to borrowers who select it.[8] When the product was new, one plausible rationale for the discount is adoption friction: borrowers might otherwise hesitate to try an unfamiliar disbursement method even when it was beneficial. We study the early experimentation phase, during which the recovered discounts were especially large and idiosyncratic, appearing to include deeper reductions for borrowers with weaker credit profiles, consistent with the view that these borrowers stood to benefit most from a commitment to disciplined spending. Because this pricing schedule is specific to the pilot and was not set in a systematic, replicable way, we deliberately do not center the paper on interest rates: we treat the discount as a feature of the rollout rather than as an object of independent interest, and we read reduced-form estimates of its effect with caution. Our focus is instead on the commitment channel that remains once pricing is accounted for.[9]

2.3 Data

Our primary dataset consists of personal loans originated on the platform in the United States during 2017 and 2018, covering the period before and after the introduction of the direct-pay option.[10] We focus on debt-refinancing loans declared for "credit card repayment" or "debt consolidation", which represent approximately 80% of the platform's loans. The remaining loans are general-purpose consumption loans (e.g., weddings, holidays). In the DiD analysis below, these ineligible loans supply a within-platform comparison for common time shocks; we do not assume that they share refinancing borrowers' default dynamics in levels. The data include detailed loan terms and borrower character-

[8] We do not directly observe the interest-rate discount for choosing the direct-pay option. To recover it, we use a random forest algorithm to estimate the relationship between the contractual interest rate, the disbursement option, and other key loan and borrower characteristics. We then predict the interest rate for the non-chosen disbursement method for each loan and use this to calculate the implied discount for the direct-pay option. The observations about the discount below should therefore be read as conditional on these recovered values.

[9] As of early 2026, most platforms either do not offer a discount for direct-pay or offer only a small one to borrowers with strong credit profiles, for example, SoFi with about 0.25 percentage points (https://www.sofi.com/personal-loans/credit-card-consolidation-loans/). The large pilot-era discounts we study are therefore not representative of current market practice.

[10] All loans are fully funded and observations are made at the loan level rather than at the individual borrower level. Coarse matching based on borrower characteristics (such as zip code, job title and length, and income) indicates that repeated borrowing is uncommon during the sample period.

Table 1: Summary Statistics

Variable	N	Mean	SD	Min	Max
Loan characteristic					
Loan amount (1,000s)	339,662	15.64	9.88	1.00	40.00
Term (1 = 3-year, 0 = 5-year)	339,662	0.66	0.47	0.00	1.00
Debt consolidation	339,662	0.82	0.38	0.00	1.00
Direct-pay	339,662	0.19	0.40	0.00	1.00
Contractual interest rate	339,662	13.78	4.59	5.31	27.27
Discount	339,662	3.45	2.08	0.00	17.58
Repayment outcome					
Default (in 1st year)	339,662	0.057	0.23	0.00	1.00
Borrower characteristic					
Annual income (1,000s)	339,662	81.05	94.53	1.90	9930.48
Home ownership (1 = yes)	339,662	0.12	0.33	0.00	1.00
Debt-to-income ratio (DTI)	339,662	18.19	8.74	0.00	39.99
Credit score (FICO)	339,662	700.46	28.97	664.00	850.00
Credit history length (years)	339,662	15.72	7.71	3.17	69.00
Credit inquiries (past 6 months)	339,662	0.48	0.75	0.00	5.00
Open credit accounts	339,662	11.52	6.01	0.00	101.00
Revolving credit utilization	339,662	47.49	24.16	0.00	183.80

Notes: This table reports summary statistics for the main variables used in the analysis. The sample includes all approved loans originated in 2018. Variable definitions are as follows: default equals one if the loan defaults within one year of origination; direct-pay equals one if the loan proceeds are disbursed directly to creditors; loan amount is measured in thousands of dollars; interest rate is expressed in percent; annual income is in thousands of dollars; debt-to-income (DTI) ratio (monthly debt repayment as a percentage of gross monthly income), credit score (FICO), credit history length, open credit accounts, and revolving utilization are as reported by the platform.

istics, such as annual income, home ownership, debt-to-income ratio, credit score (FICO), delinquency record, and credit history length, that capture borrowers' repayment ability and creditworthiness. We track monthly repayment behavior through the beginning of 2020. For robustness checks, we also include loans from the 2014-2016 period.

Table 1 presents summary statistics for loans originated in 2018. Each loan is characterized by loan amount, term (3 or 5 years), disbursement option (cash or direct-pay), contractual interest rate, and the recovered discount for the direct-pay option. The sample shows an average loan amount of approximately $16,000, with 65% of loans having a 3-year term. About 20% of all loans (a quarter among those who are eligible) use the direct-pay disbursement method. Across all credit grades, the average interest rate is

13.8%, with direct-pay loans receiving an average recovered discount of 3.5 percentage points.

For repayment outcomes, we observe partial repayment histories through early 2020.[11] The number of observed repayments varies depending on when the loan was issued during 2017-2018. To ensure comparability across all loans, we take a conservative approach and report loan status (either current, paid off, or defaulted) one year after origination. The first-year default rate in the sample is approximately 5.7%.[12]

Borrower characteristics reveal a mixed credit profile. The average debt-to-income ratio is 18.2%, indicating nontrivial debt burdens. Only 12% of borrowers are homeowners, and average annual income is approximately $81,000. The average FICO credit score is 700, placing borrowers in the "good" credit range. Borrowers have an average credit history length of 15.7 years and hold 11.5 open credit accounts on average. Revolving credit utilization averages 47.5%, suggesting borrowers are carrying substantial credit card debt, which aligns with their stated purpose of debt refinancing.

3 Theoretical Framework

This section develops a multi-period model of debt refinancing behavior in the presence of self-control problems. The framework captures the tension between ex-ante financial discipline and ex-post temptation that arises when loan proceeds are disbursed as cash, as well as the endogenous demand for commitment that this tension creates. From this, the model delivers testable implications for the behavioral changes in repayment that commitment induces and for borrower sorting across the cash and direct-pay options. It also provides the conceptual foundation for the dynamic structural model we estimate in Section 5.

3.1 Model Setup

At $t = 0$, the borrower faces an outstanding debt of size s. The borrower requests a refinancing loan of the same amount s, intended for debt consolidation.[13] No consumption

[11]Although the dataset includes information from early 2020, the onset of COVID-19 significantly disrupted household consumption and financial behavior. To avoid confounding effects, we choose not to conduct a more detailed analysis of this period.

[12]By the end of the sample period in early 2020, approximately 40% of the loans had concluded, either being paid off or defaulted. Among these, about a quarter were defaults.

[13]For simplicity, we assume the refinancing loan amount s equals the outstanding debt. This assumption maps directly to our empirical implementation, where we observe borrowers requesting loans of size s for debt refinancing but lack detailed information on their pre-existing debt obligations.

occurs during this period.

At $t = 1$, the refinancing loan is disbursed with a term of T. Although the loan is intended for debt repayment, the disbursement is made in cash unless otherwise constrained. The borrower then faces the following choices: (1) *abstain*, applying the funds toward debt repayment as intended, or (2) *spend*, diverting a proportion α of the loan to discretionary consumption. If the borrower abstains, the new debt burden remains s. If the borrower spends, the debt burden increases to $(1 + \alpha)s$. For now, we treat the propensity to spend, α, as exogenous.

For all subsequent periods $t \geq 2$, the borrower enters a dynamic repayment subgame, choosing monthly whether to make the installment payment or to default. Defaulting incurs a private cost $c > 0$, representing the economic and reputational consequences, such as future credit exclusion or collection penalties. This cost c serves as our measure of a borrower's underlying creditworthiness. Importantly, this cost is unobservable to the lender and is thus the borrower's private information, or *type*, in the context of asymmetric information and adverse selection (Akerlof, 1970; Rothschild and Stiglitz, 1976).

Following the classic model of present-biased preferences (Laibson, 1997; O'Donoghue and Rabin, 1999), the borrower's intertemporal utility at time t is given by a quasi-hyperbolic discounting function:

$$U_t(x_t, x_{t+1}, \cdots, x_T) = u(x_t) + \beta \sum_{\tau=t+1}^{T} \delta^{\tau-t} u(x_\tau), \tag{1}$$

where δ is the standard exponential discount factor and $\beta \leq 1$ captures their *present bias*. When $\beta = 1$, preferences are *time-consistent*. Conversely, when $\beta < 1$, the agent exhibits a present bias, over-weighting immediate gratification relative to all future utilities.[14]

We analyze the borrower's decision from two distinct temporal perspectives: ex-ante (the "planner" self at $t = 0$) and ex-post (the "doer" self at $t = 1$). Let $V_1(s, r, c)$ denote the continuation value from the perspective of period $t = 1$ prior to the repayment subgame, given the principal s, interest rate r, and default cost c. This $V_1(\cdot)$ is the outcome of the dynamic repay-vs-default problem, which we leave unspecified for now and assume it to be well-behaved and differentiable. The borrower's choice between spending and

[14]We consider borrowers to be *sophisticated* in the sense of O'Donoghue and Rabin (2001): aware of their time inconsistency and able to anticipate future deviations from their ex-ante plans. We do not rule out *naivete* (John, 2020), but for tractability we follow the standard practice of imposing sophistication, as several empirical studies do (e.g., Paserman (2008)). More recently, Laibson et al. (2024) find broadly similar lifecycle discount-function estimates under naivete and sophistication specifications, while Maxted (2025) emphasizes that separating the two in field data nevertheless remains difficult.

abstaining leads to two possible continuation values

$$\begin{cases} V_1^{\text{spend}} = V_1((1+\alpha)s, r, c) & \text{if spend,} \\ V_1^{\text{abstain}} = V_1(s, r, c) & \text{if abstain.} \end{cases} \tag{2}$$

From the $t = 0$ ("planner" self) perspective, when they submit the loan request, all potential outcomes are in the future and are thus discounted. The utilities following either action are

$$\begin{cases} U_0^{\text{spend}} = \beta(\delta u(\alpha s) + \delta^2 V_1^{\text{spend}}) & \text{if spend,} \\ U_0^{\text{abstain}} = \beta\delta^2 V_1^{\text{abstain}} & \text{if abstain.} \end{cases} \tag{3}$$

From the $t = 1$ ("doer" self) perspective, after receiving the loan, the utility from consumption $u(\alpha s)$ is now immediate and is not discounted, while the future consequences V_1 remain discounted by $\beta\delta$. The utilities following either action are

$$\begin{cases} U_1^{\text{spend}} = u(\alpha s) + \beta\delta V_1^{\text{spend}} & \text{if spend,} \\ U_1^{\text{abstain}} = \beta\delta V_1^{\text{abstain}} & \text{if abstain.} \end{cases} \tag{4}$$

3.2 Self-Control Problem

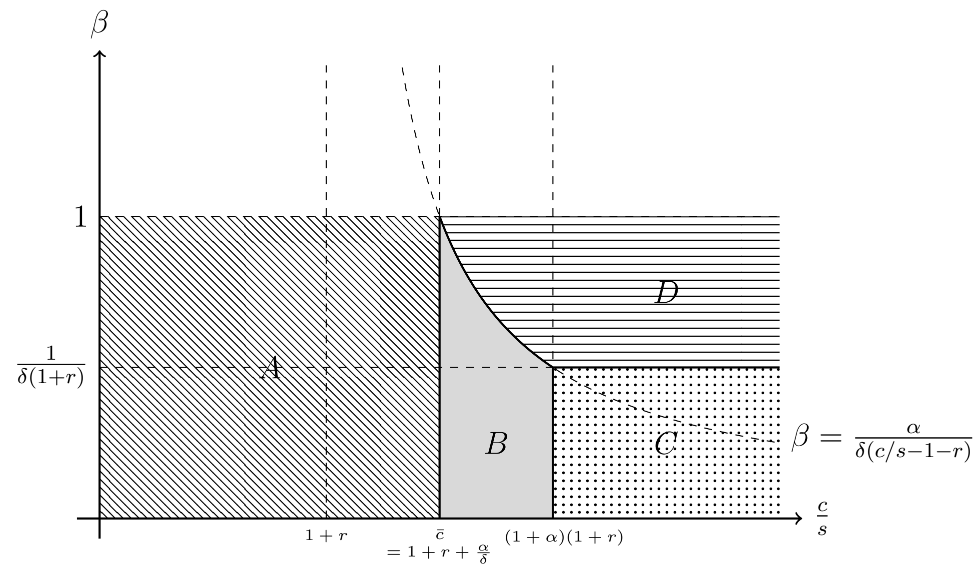
A self-control problem arises if the borrower's preference for spending reverses between $t = 0$ and $t = 1$. This conflict is analyzed by comparing the "planner" self at $t = 0$ with the "doer" self at $t = 1$. Let $\Delta V_1 = V_1^{\text{abstain}} - V_1^{\text{spend}}$ be the net continuation value lost from discretionary spending.

At $t = 0$, the "planner" intends to abstain if the discounted future cost of spending outweighs its utility. At $t = 1$, the "doer" is tempted by immediate gratification. This "doer" will choose to spend if the immediate utility outweighs the present-biased discounted future cost. This conflict, where the ex-ante plan to abstain is overturned by the ex-post action to spend, defines the self-control problem. It arises precisely when the utility of gratification falls between the planner's and the doer's thresholds, as formalized in the following proposition.

Proposition 1 (Self-control problems). *A sophisticated borrower exhibits a self-control problem, i.e., $U_0^{abstain} > U_0^{spend}$ but $U_1^{spend} > U_1^{abstain}$, if their present-bias parameter $\beta < 1$ is sufficiently low such that the immediate gratification αs falls within the following region:*

$$\beta\delta\Delta V_1 < u(\alpha s) < \delta\Delta V_1.$$

Figure 1: Parameter Space of Default Cost (c) and Present Bias (β) in Example



Notes: This figure illustrates the borrower's optimal strategy in Example based on their present-bias parameter (β, y-axis) and their default cost scaled by the loan size (c/s, x-axis). Details of the derivation are given in Appendix A.1. The parameter space is divided into four regions. Region A (plan-following defaulters): Borrowers with low default costs (c/s) who will default regardless of their spending decision. They plan to spend and default, and ex-post follow through. Region D (plan-following repayers): Borrowers with high default costs (c/s) and high patience (high β). They plan to abstain from discretionary spending and repay, and ex-post follow through. Regions B & C (Preference-reversing borrowers): Borrowers with moderate-to-high default costs but low patience (low β). Ex-ante (at $t = 0$), they prefer to abstain and repay. However, ex-post (at $t = 1$), their present bias causes them to reverse this preference and choose to spend. These are the borrowers who suffer from a self-control problem and thus have a demand for the direct-pay commitment device.

Example. We illustrate this tension in the following example. Consider a three-period model ($T = 2$) where the borrower has linear utility, $u(x) = x$. The repayment subgame at $t = 2$ consists of a single, binary choice: repay the loan (incurring the utility cost of the principal plus interest) or default (incurring the utility cost $-c$). The continuation values contingent on the spending decision are thus

$$\begin{cases} V_1^{\text{spend}} = \max\{-(1+\alpha)(1+r)s, -c\} & \text{if spend,} \\ V_1^{\text{abstain}} = \max\{-(1+r)s, -c\} & \text{if abstain.} \end{cases} \tag{5}$$

The borrower's optimal strategy depends on two key parameters: the present bias parameter β and the default cost c scaled by the loan size s. Figure 1 illustrates the parameter space based on these two dimensions. This space is divided into borrowers whose ex-ante plans align with their ex-post choices (Regions A and D) and borrowers who exhibit preference reversals (Regions B and C), with the corresponding parameter bounds provided in Appendix A.1.

For borrowers in Regions A and D, ex-ante plans align with their ex-post choices. Borrowers in Region A (low cost) plan to spend and default and subsequently follow through on those plans. Borrowers in Region D (high cost, high β) plan to abstain and repay and ultimately do so.[15]

The key behavioral conflict, and the focus of our analysis, is captured by Regions B and C. These regions represent sophisticated borrowers who suffer from a self-control problem. Their behavior is driven by the interaction of two factors. On the one hand, moderate-to-high default cost (c/s) makes the future consequences of over-borrowing severe. From the ex-ante "planner" perspective at $t = 0$, the optimal plan is to abstain to avoid this high cost. On the other hand, low patience (low β) increases the attractiveness of immediate consumption. From the ex-post "doer" perspective at $t = 1$, this temptation outweighs the present-biased discounted future cost, causing the borrower to reverse their plan and spend. The two regions differ in their repayment outcomes. In Region C, default costs are sufficiently high that borrowers repay the loan in period 2 despite having spent in period 1. In Region B, default costs are lower, and borrowers ultimately choose to default in period 2. Thus, borrowers in both regions exhibit a preference reversal in their spending decisions, but only Region B borrowers also default on the loan.

The example highlights the central challenge in the refinancing market: while borrowers may ex-ante intend to use refinancing responsibly, the ex-post disbursement of cash exposes those with low patience to self-control failures. The model thus underscores the value of commitment mechanisms that can align these borrowers' ex-post actions with their ex-ante intentions.

3.3 Direct-Pay and Commitment Device

The direct-pay option functions as a commitment device to help borrowers preemptively restrict their future choices. The disbursement choice is made by the planner at $t = 0$ (when the loan is contracted) and implemented at $t = 1$ (when funds are disbursed). Under the cash option, the borrower receives the funds directly, thereby retaining the opportunity to divert a portion ($\alpha > 0$) to immediate discretionary consumption in period

[15] Region D borrowers, who face no self-control problems, never change their default outcome, so both the cash and direct-pay options leave their repayment unchanged. If they always choose the commitment contract, they enter the treated group but contribute a zero treatment effect, mechanically attenuating the reduced-form estimate toward zero. If they always abstain, they do not affect the treatment-control difference among compliers, and their role is solely to reduce take-up and thus attenuate the ITT.

$t = 1$. Under the direct-pay option, the lender bypasses the borrower and remits the funds directly to outstanding creditors. This action eliminates the possibility of discretionary spending altogether.[16]

The intervention is designed to target sophisticated borrowers in Regions B and C who exhibit preference reversal and correctly anticipate their ex-post deviation from their ex-ante plan. This anticipation creates an endogenous demand for commitment. For the sophisticated planner, the utility loss from their future self's mistake is a salient and predictable cost. The direct-pay option provides a mechanism to mitigate this cost. By voluntarily selecting the direct-pay contract at $t = 0$, the sophisticated borrower makes a rational, ex-ante decision to preemptively restrict their future self's choice set. This action aligns their ex-post behavior (forcing $\alpha = 0$) with their ex-ante intention, thereby maximizing the "planner" self's utility.

This framework yields two related empirical predictions for the commitment mechanism developed above. First, when preferences are time-consistent ($\beta = 1$), direct-pay offers little commitment value, so take-up should be limited absent other benefits. Second, among present-biased borrowers ($\beta < 1$), the option is especially valuable to those in Regions B and C of Figure 1, who ex-ante plan to abstain but reverse course at disbursement. By remitting loan proceeds directly to creditors, the option forecloses discretionary spending at $t = 1$, aligning ex-post behavior with the planner's intentions and, for Region B borrowers, reducing default. Therefore, voluntary adoption of direct-pay together with improved repayment outcomes among adopters would be consistent with the presence of self-control frictions.

3.4 Contract Design and Borrowers' Self-Selection

Beyond its role as a commitment device, the direct-pay option also functions as a screening mechanism in the face of asymmetric information (Rothschild and Stiglitz, 1976; Wilson, 1977; Myerson, 1981). From the lender's perspective, the borrower's default cost, c, is unobserved private information. This c reflects the borrower's intrinsic creditworthiness, or "type." The introduction of contract choice alters this information environment and allows the lender to address this information problem by inducing borrowers to self-select. The borrower's observable choice of disbursement method thus acts as a signal of their unobservable type.

This selection process can generate a separating equilibrium, analogous to classic

[16] The commitment device we study eliminates the undesirable option entirely, rather than relying on incentives or penalties conditional on borrower actions. This approach contrasts with more traditional designs that depend on monitoring and enforcement; see Section 1 for a review of related work.

models of contract design. To formalize the mechanism, consider the cutoff $\bar{c} = (1+r+\frac{\alpha}{\delta})s$ derived from the Example above. This threshold divides borrowers into two types based on their default costs. Low-cost borrowers ($c < \bar{c}$, e.g., Region A) are low-types who are prone to default. They have no incentive to restrict their future choices, are insensitive to the future debt burden, and prefer cash for immediate discretionary consumption ($u(\alpha s)$); they will choose the cash option. High-cost borrowers ($c \geq \bar{c}$, e.g., Regions B and C) are high-types with salient repayment consequences who are susceptible to self-control failures ($\beta < 1$). They face a strong incentive to opt into the direct-pay method, which provides commitment value to align their ex-post behavior with their ex-ante repayment intentions.

The example above illustrates the separating equilibrium in a simplified, frictionless setting; the result extends to the general model. Offering cash and direct-pay options generates a separating equilibrium at a threshold $\bar{c}$, with low-cost types choosing cash and high-cost types choosing direct-pay. For this separation to occur in the general case, the continuation value function $V_1(s, r, c)$ must satisfy the standard single-crossing condition in both (s, c) and (r, c). This condition holds naturally because high-cost borrowers are more responsive to changes in the total debt burden s and interest rate r precisely because default is costly for them.[17] The differential sensitivity enables separation: immediate consumption utility $u(\alpha s)$ entices low-cost borrowers to choose cash, while high-cost borrowers prefer direct-pay for commitment value.

From a practical perspective, the lender can expand the set of borrowers choosing direct-pay by offering a lower interest rate for direct-pay borrowers, r_d, than that for cash borrowers, r_c. This discount serves two purposes. First, it ensures separation occurs: if rates are too high, the threshold $\bar{c}$ may exceed all borrowers' default costs, resulting in a corner solution where everyone chooses cash. Second, conditional on separation, the discount shifts the threshold downward, expanding the pool selecting direct-pay. Borrowers with moderate default costs who would have chosen cash at equal rates are now enticed by commitment value and lower repayment burden. We illustrate this effect in Figure A3 in Appendix A.1.

Proposition 2 (Separating equilibrium by default cost)**.** *A contract menu consisting of cash disbursement at interest rate r_c and direct-pay disbursement at interest rate r_d induces a separating equilibrium at some cutoff $\bar{c}$ in which high-cost borrowers ($c \geq \bar{c}$) choose direct-pay and low-cost borrowers ($c < \bar{c}$) choose cash.*

[17] Formally, higher sensitivity means that the partial derivatives $\frac{\partial}{\partial s}V_1(s, r, c)$ and $\frac{\partial}{\partial r}V_1(s, r, c)$ (which are both negative) become more negative as c increases, i.e., $\frac{\partial^2 V_1}{\partial s \partial c} < 0$ and $\frac{\partial^2 V_1}{\partial r \partial c} < 0$. This submodularity property satisfies single-crossing.

This separation is not enforced externally but arises endogenously through the alignment of borrowers' private incentives and the constraints imposed by the policy. For lenders, observing which borrowers choose the commitment device provides a valuable informational cue, allowing them to infer the borrower's unobservable default risk. Thus, the direct-pay policy functions not only as a behavioral intervention but also as a screening device that enhances the efficiency of the credit market by reducing information asymmetry.

Self-selection along the default-cost cutoff generates additional empirical predictions. If borrowers sort as predicted, high-type borrowers with stronger repayment incentives and lower intrinsic default risk will disproportionately select direct-pay, while lower-type borrowers will remain with cash. Relative to a pre-policy baseline in which all borrowers received cash, average default among cash borrowers should therefore rise as the more creditworthy pool self-selects out, while adopters of direct-pay should exhibit lower average default rates. These comparisons are consistent with the screening mechanism but do not by themselves identify a causal effect: observed differences may reflect both favorable selection and genuine behavioral improvement from restricting discretionary spending. In Section 4.4, we will decompose the respective roles of selection and behavioral improvement.

4 Empirical Evidence

This section presents empirical evidence on how the introduction of the direct-pay disbursement option affects borrower behavior and loan performance. Guided by the theoretical framework, we test two key mechanisms through which the policy operates: (i) a behavioral channel, through which the device directly mitigates self-control failures, and (ii) a selection channel, by which more creditworthy or self-aware borrowers self-select into the commitment device. We organize the analysis into four parts. We first examine the determinants of borrowers' choice of direct-pay, documenting who adopts the commitment device and why. Next, we estimate an intent-to-treat effect of policy availability using a difference-in-differences comparison of eligible and ineligible loans on the same platform, and we complement the DiD estimate with within-eligible comparisons to the 2017 pre-policy cohort. Third, we analyze heterogeneity in the DiD estimates across borrower characteristics. Finally, we conduct a back-of-the-envelope decomposition analysis that separates the role of selection from behavioral improvement.

4.1 Determinants of Direct-Pay Adoption

We begin by examining the determinants of borrowers' take-up of the direct-pay disbursement option, which accounts for approximately 20% of loans, as reported in the summary statistics in Table 1. Specifically, we estimate linear probability model (LPM) and logit specifications of the borrower's choice between the direct-pay and cash disbursement methods:

$$Direct_i = \beta_1 Discount_i + X_i'\gamma + \mu_t + \epsilon_i, \tag{6}$$

where i is the index for each loan, $Direct_i$ is an indicator of whether the borrower has chosen the direct-pay option, $Discount_i$ is the estimated interest-rate reduction associated with the direct-pay option, X_i includes borrower and loan characteristics, and μ_t denotes month-of-origination fixed effects. Table 2 reports eight specifications: Columns (1)–(4) present linear-probability estimates, and Columns (5)–(8) report the corresponding logit estimates. Each successive specification adds richer controls; Columns (4) and (8) include full borrower covariates and month fixed effects.

The estimates show a strong positive association between the offered discount and the likelihood of choosing direct-pay. A one-percentage-point larger discount raises the probability of adoption by about 5 percentage points after controlling for borrower characteristics and month effects. This elasticity confirms that borrowers respond predictably to the financial incentive embedded in the pricing menu. The negative coefficient on the undiscounted interest rate further suggests that, holding the discount constant, higher underlying rates discourage adoption.

Beyond the price incentive, adoption is also related to borrower balance-sheet characteristics. Borrowers with larger loan amounts and higher debt-to-income (DTI) ratios are significantly more likely to select direct-pay, consistent with the notion that borrowers facing heavier debt burdens have stronger motives to pre-commit to debt repayment and to avoid the temptation to spend newly borrowed funds. Conversely, higher-income borrowers are less likely to adopt, indicating that they value liquidity and flexibility more than the commitment benefit of direct-pay. The negative coefficients on FICO score, credit-history length, and home ownership imply that financially more established borrowers are less inclined to restrict their discretion, again consistent with smaller perceived self-control frictions in this group. Finally, borrowers with more open accounts or higher revolving utilization are more likely to choose direct-pay, reinforcing the interpretation that the option appeals to borrowers managing complex debt portfolios.

Taken together, this set of results shows that both price incentives and behavioral motives drive adoption of the commitment device. The platform's design effectively screens

Table 2: Determinants of Direct-Pay Adoption

	Direct							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Discount	0.015***	0.054***	0.056***	0.050***	0.093***	0.427***	0.444***	0.398***
	(0.000)	(0.000)	(0.000)	(0.003)	(0.002)	(0.003)	(0.003)	(0.015)
Undiscounted interest rate		−0.035***	−0.044***	−0.044***		−0.279***	−0.362***	−0.374***
		(0.000)	(0.000)	(0.004)		(0.002)	(0.002)	(0.008)
Loan amount			0.004***	0.004***			0.022***	0.023***
			(0.000)	(0.000)			(0.001)	(0.001)
Term 3-year			−0.064***	−0.066***			−0.516***	−0.544***
			(0.002)	(0.010)			(0.013)	(0.054)
log(Annual income)			−0.059***	−0.065***			−0.476***	−0.536***
			(0.001)	(0.005)			(0.011)	(0.022)
Debt-to-income			0.000**	0.000			0.003***	0.002*
			(0.000)	(0.000)			(0.001)	(0.001)
Credit score (FICO)			−0.000***	−0.001***			−0.004***	−0.005***
			(0.000)	(0.000)			(0.000)	(0.001)
Home ownership			−0.006**	−0.002			−0.036*	−0.005
			(0.002)	(0.006)			(0.014)	(0.045)
Credit history length (years)			−0.002***	−0.002***			−0.016***	−0.017***
			(0.000)	(0.000)			(0.001)	(0.001)
Credit inquiries (past 6 months)			−0.006***	−0.002*			−0.037***	−0.011
			(0.001)	(0.001)			(0.007)	(0.008)
Open credit accounts			0.005***	0.005***			0.038***	0.041***
			(0.000)	(0.000)			(0.001)	(0.001)
Revolving credit utilization			0.002***	0.002***			0.018***	0.019***
			(0.000)	(0.000)			(0.000)	(0.000)
Constant	0.142***	0.510***	1.075***	1.073***	−1.758***	0.920***	5.907***	5.785***
	(0.001)	(0.003)	(0.023)	(0.144)	(0.009)	(0.019)	(0.175)	(0.743)
Month fixed effects	No	No	No	Yes	No	No	No	Yes
R2	0.006	0.073	0.104	0.131				
Observations	339662	339662	339662	339662	339662	339662	339662	339662
Log-likelihood					−166062.343	−152923.651	−147045.273	−141752.550

Notes: This table reports estimates of borrowers' choices between the direct-pay and cash disbursement options. The dependent variable equals one if the borrower selects the direct-pay option and zero otherwise. Columns (1)–(4) present linear-probability models (LPM) and Columns (5)–(8) report logit specifications. Columns (4) and (8) include month-of-origination fixed effects. Robust standard errors clustered by origination month are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

borrowers along dimensions of indebtedness and liquidity preference: those with greater outstanding debt or potential self-control concerns self-select into the direct-pay option, whereas borrowers with stronger balance sheets or greater demand for flexibility favor cash disbursement.

4.2 Effect of Policy Availability

We next estimate the effect of making the direct-pay menu available on borrower default. The policy became available in 2018 for refinancing loans, i.e., those used to consolidate or repay existing debts, but not for general-purpose loans such as home improvement or major purchases. This institutional feature allows us to implement a difference-in-differences (DiD) design that compares the change in default rates for eligible loans (refinancing) to that for ineligible loans (general-purpose consumption) before and after the policy introduction.

The difference-in-differences design requires that eligible (treated) and ineligible (control) borrowers would have followed parallel default trends absent the policy. A natural concern is that refinancing and general-purpose loans may respond differently to macroeconomic shocks. Several features of our setting address this concern. Both groups are originated on the same platform under the same underwriting algorithm, and our specifications include rich borrower controls and month-of-origination fixed effects. Covariate balance tests confirm that the two groups are broadly comparable on observable characteristics before the policy (Table A9a) and remain so in 2018 (Table A9b). Further, an event study over the 2014–2017 pre-period confirms parallel default trends prior to the policy; divergence emerges only after direct-pay became available in 2018 (Appendix A.3 and Figure A4). Taken together, these results are broadly consistent with the parallel trends assumption underlying the DiD design.

Formally, we estimate the following model:

$$Default_{it} = \beta_0 + \beta_1 Post_t + \beta_2 Treated_i + \beta_3 (Post_t \times Treated_i) + X_{it}'\gamma + \epsilon_{it}, \qquad (7)$$

where $Default_{it}$ is an indicator equal to one if loan defaults within one year of origination, $Post_t$ equals one for loans originated after the policy (2018) and zero before the policy (2017), and $Treated_i$ equals one for loans whose stated purpose qualifies for the direct-pay option (e.g., refinancing) and zero otherwise (e.g., consumption loans). Note that being eligible does not mean that the borrower must use the direct-pay option. The coefficient of interest, β_3, measures the differential change in default rates for eligible versus ineligible loans following the policy introduction. In this specification, eligibility for

Table 3: Effect of Direct-Pay Policy Introduction on Loan Default: Difference-in-Differences Estimates

	One-year default							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	0.008***	0.008***	0.008***	0.008***	0.138***	0.120***	0.120***	0.114**
	(0.001)	(0.001)	(0.002)	(0.002)	(0.022)	(0.023)	(0.035)	(0.035)
Treated	−0.009***	−0.014***	−0.014***	−0.014***	−0.165***	−0.256***	−0.255***	−0.249***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.018)	(0.018)	(0.015)	(0.015)
Post × Treated	−0.007***	−0.006***	−0.006**	−0.020***	−0.114***	−0.096***	−0.098**	−0.372***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.025)	(0.026)	(0.031)	(0.028)
Post × Treated × Discount				0.004***				0.074***
				(0.000)				(0.003)
Loan amount		0.001***	0.001***	0.001***		0.028***	0.028***	0.028***
		(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)
Term 3-year		−0.005***	−0.004***	−0.004***		−0.087***	−0.086***	−0.079***
		(0.001)	(0.001)	(0.001)		(0.013)	(0.015)	(0.016)
log(Annual income)		−0.015***	−0.015***	−0.015***		−0.307***	−0.308***	−0.303***
		(0.001)	(0.001)	(0.001)		(0.013)	(0.026)	(0.025)
Debt-to-income		0.001***	0.001***	0.001***		0.013***	0.013***	0.013***
		(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)
Credit score (FICO)		−0.000***	−0.000***	−0.000***		−0.007***	−0.007***	−0.007***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Home ownership		0.003**	0.003***	0.003***		0.049**	0.049***	0.045***
		(0.001)	(0.001)	(0.001)		(0.016)	(0.009)	(0.010)
Credit history length (years)		−0.001***	−0.001***	−0.000***		−0.011***	−0.010***	−0.010***
		(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)
Credit inquiries (past 6 months)		0.018***	0.018***	0.017***		0.291***	0.290***	0.287***
		(0.000)	(0.001)	(0.001)		(0.006)	(0.009)	(0.009)
Open credit accounts		−0.001***	−0.001***	−0.001***		−0.016***	−0.016***	−0.017***
		(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)
Revolving credit utilization		−0.000***	−0.000***	−0.000***		−0.004***	−0.004***	−0.005***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Constant	0.061***	0.359***	0.357***	0.343***	−2.727***	3.134***	3.105***	2.886***
	(0.001)	(0.008)	(0.009)	(0.009)	(0.016)	(0.167)	(0.210)	(0.213)
Month fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
R2	0.001	0.009	0.009	0.009				
Observations	660969	660969	660969	660969	660969	660969	660969	660969
Log-likelihood					−141719.546	−139098.260	−139082.471	−138923.075

Notes: This table reports difference-in-differences estimates of the effect of the direct-pay policy on borrowers' one-year default probability. The dependent variable equals one if the loan defaults within one year of origination. Post is an indicator for loans originated after the policy introduction in 2018. Treated equals one for loans whose stated purpose qualifies for the direct-pay option (debt-refinancing or credit-card-repayment loans) and zero for ineligible, general-purpose loans. The interaction term Post × Treated measures the policy's impact on default rates among eligible borrowers relative to ineligible ones. Columns (1)–(4) present linear-probability models, and Columns (5)–(8) report logit specifications. Columns (2)–(4) and (6)–(8) add borrower and loan controls, Columns (3)–(4) and (7)–(8) include month-of-origination fixed effects, and Columns (4) and (8) additionally interact the treatment effect with the borrower-level direct-pay discount. Robust standard errors are clustered by origination month. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

the direct-pay option is the treatment, regardless of whether a borrower subsequently adopts it. We therefore interpret β_3 as an intent-to-treat (ITT) effect of making the commitment device available to eligible borrowers on their one-year default probability. We include borrower- and loan-level controls X_{it} (loan amount, term, log income, debt-to-income ratio, and FICO score) and, in the preferred specification, month fixed effects μ_t. In augmented specifications, we additionally include $Post_t \times Treated_i \times Discount_i$, where $Discount_i$ is the borrower-level predicted direct-pay discount, to allow the policy effect to vary with the pricing incentive. Standard errors are clustered by month of origination to allow for arbitrary correlation in residuals across loans originated in the same month, capturing common shocks in underwriting standards, macro conditions, and other month-specific factors.

Table 3 reports the regression results for Equation (7). Across the specifications, the coefficient on the interaction term $Post \times Treated$ is negative and statistically significant, indicating that default rates declined for borrowers eligible for the direct-pay option relative to those who were not.[18] In the preferred baseline linear-probability specification with full controls and month fixed effects (Column 3), the estimated coefficient of -0.006 corresponds to roughly a 0.6 percentage-point reduction in one-year default probability, relative to a pre-policy mean of about 5.2%. Economically, this corresponds to about an 11% decline in default risk relative to the pre-policy mean under the DiD normalization. The logit specification in Column (7) (-0.098) is similar in percentage terms and is reported as a robustness check. Columns (4) and (8) additionally allow the treatment effect to vary with the predicted discount offered to eligible borrowers, and the policy effect remains negative and statistically significant in these augmented specifications as well.

The main effects are also informative. The positive and significant coefficient on $Post$ shows that default rates increased slightly for ineligible loans after 2018, which is consistent with a modest deterioration in overall credit quality or macro conditions. The negative coefficient on $Treated$ indicates that refinancing loans, typically used to repay existing debts, exhibited a lower baseline default risk even before the policy. The incremental decline captured by β_3 is the DiD estimate of policy availability net of the ineligible-group time trend, not a comparison of levels between products. In the discount-interacted specifications, the coefficient on $Post \times Treated \times Discount$ is positive. We do not interpret this as evidence that larger discounts worsen repayment. Rather, the discount was not

[18] For robustness checks, in Table A11 in Appendix A.3, we enlarge the pre-policy period to 2014–2017 and re-estimate both the baseline DiD and an event-study specification; the 2018 interaction remains negative and statistically significant, and pre-2018 coefficients are small (see Figure A4). Consistent with the default regressions, the survival analysis in Table A12 in Appendix A.3 shows lower default hazards among direct-pay adopters than among comparable cash borrowers.

randomly assigned: during the 2018 pilot, larger discounts appear to have been offered to observably and unobservably riskier eligible borrowers. This endogenous pricing induces a positive correlation between the offered discount and underlying default risk, so the interaction term partly absorbs risk-based targeting rather than a clean causal price effect. The estimates are therefore best read as showing that the policy effect remains negative even after accounting for this endogenous discount variation.

4.3 Heterogeneity

We next examine whether the effect of the direct-pay policy differs across borrower risk and liquidity profiles. Theory suggests that borrowers with greater self-control problems, but not necessarily tighter liquidity constraints, should benefit most from the commitment device. To test this prediction, we re-estimate the difference-in-differences specification separately by (i) credit quality, (ii) indebtedness, (iii) income, and (iv) revolving credit utilization. Specifically, we split the sample into three tiers of FICO score, two groups of debt-to-income (DTI) ratio (below- and above-median), three income tiers (bottom, middle, and top terciles), and three revolving utilization groups (low, medium, and high). Table 4 reports the results. All regressions include the same borrower- and loan-level controls as before and month-of-origination fixed effects.

Across subsamples, the estimated interaction coefficient Post × Treated remains negative for most groups, but its magnitude and significance vary systematically with borrower characteristics. The policy effect is strongest among low-FICO borrowers, where the coefficient of -0.010 (Column 1) implies roughly a one-percentage-point decline in one-year default probability relative to the pre-policy mean of about 10%. The effect diminishes with credit quality and becomes statistically insignificant in the higher-FICO terciles, consistent with the notion that high-score borrowers already exhibit greater financial discipline and hence have less scope for behavioral improvement. When splitting by debt-to-income ratio, the coefficient is negative and significant only for low-DTI borrowers (Column 4), suggesting that the policy is most effective for borrowers who are relatively liquid but prone to self-control failures rather than those already financially constrained. Similarly, by income group, the policy has the largest and most significant impact among high-income borrowers (Column 8), while the effect is weaker and statistically indistinguishable from zero for the lower-income groups.

The revolving credit utilization results in Table 4 provide additional insight into which borrowers benefit most from the commitment device. The policy effect is strongest for borrowers with medium revolving utilization (Column 10), where the $Post \times Treated$ co-

Table 4: Heterogeneity in the Effect of the Direct-Pay Policy on Default

	One-year default										
	FICO			DTI		Income			Revolving utilization		
	low (1)	medium (2)	high (3)	low (4)	high (5)	low (6)	medium (7)	high (8)	low (9)	medium (10)	high (11)
Post	0.008*	0.005	0.009**	0.008***	0.002	0.006*	0.004	0.011***	0.009**	0.007***	0.003
	(0.004)	(0.004)	(0.003)	(0.002)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
Treated	−0.010***	−0.016***	−0.017***	−0.014***	−0.009*	−0.014***	−0.016***	−0.013***	−0.011***	−0.014***	−0.011***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Post × Treated	−0.010**	−0.001	−0.005	−0.007**	−0.004	−0.004	−0.002	−0.011***	−0.002	−0.007**	−0.005**
	(0.004)	(0.003)	(0.003)	(0.002)	(0.006)	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.002)
Loan amount	0.002***	0.001***	0.001***	0.002***	0.001***	0.002***	0.002***	0.001***	0.003***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Term 3-year	−0.008***	−0.004**	−0.001	−0.004***	−0.008***	−0.004*	−0.004**	−0.002	−0.001	−0.007***	−0.001*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
log(Annual income)	−0.016***	−0.014***	−0.014***	−0.016***	−0.008*	−0.024***	−0.014***	−0.011***	−0.023***	−0.012***	−0.012***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)	(0.002)	(0.004)	(0.001)	(0.002)	(0.001)	(0.001)
Debt-to-income	0.001***	0.001***	0.000***	0.000***	0.001*	0.001***	0.001***	0.001***	0.000**	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Credit score (FICO)	−0.001***	−0.000***	−0.000***	−0.000***	−0.001***	−0.000***	−0.000***	−0.000***	−0.000***	−0.001***	−0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Home ownership	0.001	0.003	0.005***	0.003***	−0.001	−0.002***	0.000	0.010***	0.002	0.002*	0.004*
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Credit history length (years)	−0.001***	−0.000***	−0.000***	−0.001***	−0.000	−0.000	−0.001***	−0.001***	−0.001***	−0.000***	−0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Credit inquiries (past 6 months)	0.017***	0.017***	0.019***	0.018***	0.016***	0.019***	0.018***	0.016***	0.017***	0.016***	0.021***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Open credit accounts	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Revolving credit utilization	−0.000***	−0.000***	−0.000***	−0.000***	−0.000**	−0.000***	−0.000***	−0.000***	−0.001***	−0.000***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.474***	0.367***	0.262***	0.347***	0.475***	0.372***	0.378***	0.331***	0.344***	0.451***	0.411***
	(0.039)	(0.052)	(0.010)	(0.009)	(0.028)	(0.017)	(0.022)	(0.015)	(0.018)	(0.013)	(0.017)
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.008	0.008	0.012	0.009	0.007	0.009	0.010	0.010	0.015	0.008	0.008
Observations	276290	169450	215229	596244	64725	241624	186718	232627	185100	302233	173636

Notes: This table reports difference-in-differences estimates of the policy effect across borrower risk and liquidity groups. The dependent variable equals one if the loan defaults within one year of origination. Post is an indicator for loans originated after the 2018 introduction of the direct-pay policy. Treated equals one for loans whose stated purpose qualifies for the direct-pay option (debt-refinancing or credit-card-repayment loans). The coefficient on Post × Treated measures the change in default probability for treated (eligible) loans relative to untreated loans following the policy introduction. Columns (1)–(3) split the sample by FICO-score terciles (low, medium, high), Columns (4)–(5) by debt-to-income (DTI) ratio (below- and above-median), Columns (6)–(8) by income terciles (low, medium, high), Columns (9)–(11) by revolving utilization terciles (low, medium, high). All specifications include the full set of borrower- and loan-level controls, loan amount, term, log annual income, DTI, FICO, home ownership, credit-history length, credit inquiries in the past six months, number of open accounts, and revolving credit utilization, as well as month-of-origination fixed effects. Robust standard errors are clustered by origination month. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

efficient of about -0.007 is negative and statistically significant. In contrast, the effect is smaller and statistically insignificant for low-utilization borrowers (Column 9) and somewhat weaker, though still negative and significant, for high-utilization borrowers (Column 11). This pattern suggests that borrowers with moderate to high credit card debt are the primary beneficiaries of the commitment mechanism. These borrowers have the capacity to further increase their debt burden through discretionary spending, making them most vulnerable to self-control failures and thus most likely to benefit from the direct-pay option. High-utilization borrowers, who are already heavily indebted, may face binding liquidity constraints that limit their ability to spend further, tempering (but not eliminating) the gains from commitment.

The heterogeneity results are most consistent with a commitment interpretation of the DiD estimates. The interaction is largest for borrowers who appear liquid but risky: higher incomes, lower DTI ratios, weaker credit histories, and moderate revolving utilization, i.e., borrowers with scope to add discretionary debt rather than those already at binding liquidity limits. We read this pattern as supporting, though not by itself proving, the self-control channel; the within-eligible decomposition in Section 4.4 provides the sharper product-level evidence.

4.4 Effect of Direct-Pay Adoption: Selection vs. Behavioral Channels

Having established that the policy's availability improved market-wide outcomes, we now examine the effect on those who adopted the direct-pay option among borrowers eligible for the policy. We focus on the cross-section of 2018 debt-refinancing loans that are eligible for the direct-pay option and first estimate

$$Default_i = \beta_1 Direct_i + X_i'\gamma + \mu_t + \epsilon_i, \tag{8}$$

where $Direct_i$ is an indicator for the disbursement method, X_i is a vector of borrower and loan controls, and μ_t denotes month-of-origination fixed effects. In additional specifications, we interact $Direct_i$ with the borrower-level predicted direct-pay discount.

Table 5a reports the estimates for Equation (8). In the baseline specifications with full controls and month fixed effects, direct-pay is associated with a 3.2 percentage-point lower default probability in the LPM (Column 3) and a log-odds coefficient of -0.816 in the logit model (Column 7). This raw coefficient should be read with two caveats. First, because adopters are also offered a lower interest rate, the estimate conflates the behavioral commitment effect with this pricing benefit, so the coefficient in Columns (1)–(3) should not be read as a pure commitment effect. Columns (4) and (8) interact direct-pay with the offered discount to partial out the pricing channel; once we account for the discount directly, the coefficient on direct-pay falls in magnitude (from -0.032 to -0.015 in the LPM) but remains negative and statistically significant, indicating that the better repayment of direct-pay borrowers is not merely an interest-rate effect. The negative interaction term further suggests that lower pricing is associated with even better repayment among direct-pay users, although this reduced-form correlation mixes the causal pricing effect with endogenous discount assignment and likely overstates the role of the discount (we return to this below and in the structural model). Second, even net of pricing, this within-2018 comparison conditions only on observable characteristics and does not remove selection on unobservable borrower type, as safer borrowers may sort into direct-pay for reasons our controls do not capture.

To address this concern, we separate the behavioral effect (the commitment device making borrowers less risky) from the selection effect (less risky borrowers choosing the device), which the within-2018 comparison cannot do on its own. We use the 2017 pre-policy cohort of debt-refinancing loans (i.e., those who would have been eligible to use the direct-pay option in 2018) as a baseline.[19] The introduction of the direct-pay option in 2018 partitions borrowers into two observed groups: cash borrowers and direct-pay borrowers. Comparing their default outcomes, both within 2018 and relative to the 2017 benchmark, provides the basis for decomposing the overall effect into selection and behavioral components. We implement this idea by estimating the following model on the pooled 2017–2018 debt-refinancing loans:

$$Default_i = \beta_1 PostCash_i + \beta_2 PostDirect_i + X_i'\gamma + \mu_t + \epsilon_i, \tag{9}$$

where $PostCash_i$ and $PostDirect_i$ are indicators for 2018 debt-refinancing loans disbursed through the cash and direct-pay options, respectively, with 2017 refinancing cash loans serving as the omitted baseline group. We also interact $PostDirect_i$ with the borrower-level predicted direct-pay discount in additional specifications.

Table 5b reports the regression results of Equation (9). The estimates are consistent with the separating-equilibrium and adverse-selection patterns predicted by our theoretical framework. In the preferred LPM with full controls and month fixed effects (Column 3), the coefficient on $PostCash$ is 0.009 and statistically significant. Relative to the 0.008 common time trend estimated in the DiD regressions, this indicates that the 2018 cash-

[19] We conduct a covariate balance test reported in Appendix A.2. Table A10 compares the characteristics of eligible borrowers from 2017 and 2018 and finds only small, statistically insignificant differences across income, credit scores, debt burdens, and other observables. The stability of observables supports the maintained assumption that the borrower pool did not materially change.

Table 5: Decomposition of Selection and Behavioral Effects

(a) Within-2018 Comparison of Direct-Pay and Cash Borrowers

	One-year default							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Direct-pay	−0.035***	−0.031***	−0.032***	−0.015***	−0.880***	−0.795***	−0.816***	−0.332***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.026)	(0.026)	(0.030)	(0.069)
Direct-pay × Discount				−0.004***				−0.131***
				(0.000)				(0.016)
Loan amount		0.001***	0.001***	0.001***		0.025***	0.025***	0.025***
		(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)
Term 3-year		−0.006***	−0.006***	−0.007***		−0.118***	−0.113***	−0.130***
		(0.001)	(0.001)	(0.001)		(0.019)	(0.016)	(0.015)
log(Annual income)		−0.013***	−0.014***	−0.014***		−0.277***	−0.282***	−0.283***
		(0.001)	(0.001)	(0.001)		(0.020)	(0.028)	(0.028)
Debt-to-income		0.000***	0.000***	0.001***		0.010***	0.010***	0.010***
		(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)
Credit score (FICO)		−0.000***	−0.000***	−0.000***		−0.008***	−0.008***	−0.008***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Home ownership		0.001	0.001	0.001		0.016	0.017	0.018
		(0.001)	(0.001)	(0.001)		(0.026)	(0.024)	(0.024)
Credit history length (years)		−0.001***	−0.001***	−0.001***		−0.011***	−0.011***	−0.011***
		(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)
Credit inquiries (past 6 months)		0.017***	0.017***	0.017***		0.284***	0.284***	0.286***
		(0.001)	(0.001)	(0.001)		(0.010)	(0.016)	(0.016)
Open credit accounts		−0.001***	−0.001***	−0.001***		−0.017***	−0.017***	−0.016***
		(0.000)	(0.000)	(0.000)		(0.002)	(0.002)	(0.002)
Revolving credit utilization		−0.000***	−0.000***	−0.000***		−0.006***	−0.005***	−0.005***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Constant	0.062***	0.376***	0.369***	0.384***	−2.715***	3.598***	3.467***	3.679***
	(0.000)	(0.013)	(0.010)	(0.009)	(0.009)	(0.261)	(0.236)	(0.223)
Month fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
R2	0.004	0.012	0.012	0.013				
Observations	278331	278331	278331	278331	278331	278331	278331	278331
Log-likelihood					−57570.105	−56567.484	−56535.076	−56482.512

Table 5: Decomposition of Selection and Behavioral Effects (cont.)

(b) Comparison with 2017 Baseline: Separating Selection and Commitment Effects

	One-year default							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post cash	0.010***	0.009***	0.009***	0.009***	0.177***	0.169***	0.168***	0.169***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.013)	(0.013)	(0.021)	(0.020)
Post direct-pay	−0.026***	−0.022***	−0.023***	−0.005**	−0.703***	−0.630***	−0.644***	−0.154*
	(0.001)	(0.001)	(0.001)	(0.002)	(0.026)	(0.026)	(0.027)	(0.062)
Post direct-pay × Discount				−0.005***				−0.132***
				(0.000)				(0.016)
Loan amount		0.001***	0.001***	0.001***		0.025***	0.025***	0.025***
		(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)
Term 3-year		−0.005***	−0.005***	−0.006***		−0.103***	−0.099***	−0.107***
		(0.001)	(0.001)	(0.001)		(0.014)	(0.016)	(0.016)
log(Annual income)		−0.013***	−0.014***	−0.014***		−0.286***	−0.288***	−0.289***
		(0.001)	(0.001)	(0.001)		(0.015)	(0.026)	(0.026)
Debt-to-income		0.001***	0.001***	0.001***		0.015***	0.015***	0.015***
		(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)
Credit score (FICO)		−0.000***	−0.000***	−0.000***		−0.008***	−0.008***	−0.008***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Home ownership		0.002	0.002*	0.002*		0.032	0.031*	0.032*
		(0.001)	(0.001)	(0.001)		(0.019)	(0.013)	(0.013)
Credit history length (years)		−0.000***	−0.000***	−0.000***		−0.010***	−0.010***	−0.010***
		(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)
Credit inquiries (past 6 months)		0.017***	0.017***	0.017***		0.285***	0.284***	0.285***
		(0.000)	(0.001)	(0.001)		(0.007)	(0.008)	(0.008)
Open credit accounts		−0.001***	−0.001***	−0.001***		−0.016***	−0.016***	−0.016***
		(0.000)	(0.000)	(0.000)		(0.001)	(0.002)	(0.002)
Revolving credit utilization		−0.000***	−0.000***	−0.000***		−0.004***	−0.004***	−0.004***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Constant	0.053***	0.372***	0.369***	0.378***	−2.892***	3.786***	3.732***	3.853***
	(0.000)	(0.009)	(0.007)	(0.007)	(0.009)	(0.198)	(0.181)	(0.176)
Month fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
R2	0.002	0.010	0.010	0.010				
Observations	529346	529346	529346	529346	529346	529346	529346	529346
Log-likelihood					−109254.205	−107333.666	−107305.733	−107251.270

Notes: This table decomposes the decline in default associated with the direct-pay policy into selection and behavioral components. Table 5a estimates Equation (8) using 2018 loans only; the dependent variable equals one if the loan defaults within one year of origination. Direct-pay equals one for loans disbursed directly to creditors and zero for cash disbursement. Columns (1)–(4) present linear-probability models and Columns (5)–(8) report logit specifications; Columns (4) and (8) additionally include $Direct_i \times Discount_i$. Table 5b estimates Equation (9) on pooled 2017–2018 data, with 2017 cash borrowers as the omitted group. Post Cash and Post Direct are indicators for 2018 cash and 2018 direct-pay loans, respectively, and Columns (4) and (8) additionally include $PostDirect_i \times Discount_i$. All regressions control for loan amount, term (3-year indicator), log annual income, debt-to-income ratio, FICO score, home ownership, credit-history length, credit inquiries in the past six months, number of open credit accounts, and revolving-credit utilization, and Columns (3)–(4) and (7)–(8) include month-of-origination fixed effects. Robust standard errors are clustered by origination month. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

only pool performed modestly worse than the 2017 eligible baseline even after controlling for observables, consistent with adverse selection into the remaining cash pool.

The complementary side of this pool separation appears in the $PostDirect$ coefficient, which is -0.023 in Column (3): 2018 adopters default 2.3 percentage points below the 2017 baseline. Part of this improvement may reflect the high-type borrowers who sorted into direct-pay, so we do not yet read it as a pure behavioral effect. Its magnitude is nonetheless large and difficult to attribute to selection alone, since the cash pool deteriorated only modestly above, suggesting a behavioral commitment effect over and above sorting. To make the selection-versus-behavioral split explicit, we now turn to a back-of-the-envelope decomposition.

Decomposition. We decompose the total policy effect into selection and behavioral components using the framework illustrated in Figure 2. The figure plots average one-year default rates for different borrower groups before (2017) and after (2018) the policy introduction. Panel (a) summarizes the DiD design, while Panel (b) illustrates the decomposition for eligible borrowers. All values are group means without adjusting for borrower characteristics.

We begin by documenting the raw 2018 default gap between cash and direct-pay borrowers. When direct-pay became available, eligible borrowers split into a cash group with a one-year default rate of 6.2% and a direct-pay group with a rate of 2.6%, a gap of 3.6 percentage points (points C′ and D′ in Panel (b)). For reference, the pre-policy default rate for all eligible debt-refinancing loans in 2017 was 5.2% (point A).

We next recover the *implied* 2017 default rate for borrowers who chose cash in 2018. The DiD estimates in Equation (7) imply a 0.8 percentage point market-wide increase in defaults from 2017 to 2018, measured by the control-group change in Panel (a) from point B to point B′. Removing this aggregate trend yields an implied 2017 default rate of 5.4% for future cash borrowers (point C in Panel (b)).

We then infer the *implied* 2017 default rate for future direct-pay borrowers from the 2017 pool average. Because the 5.2% aggregate rate is a weighted average of the two groups and direct-pay accounted for roughly 24% of 2018 disbursements, the implied 2017 default rate for adopters is 4.5% (point D in Panel (b)).[20]

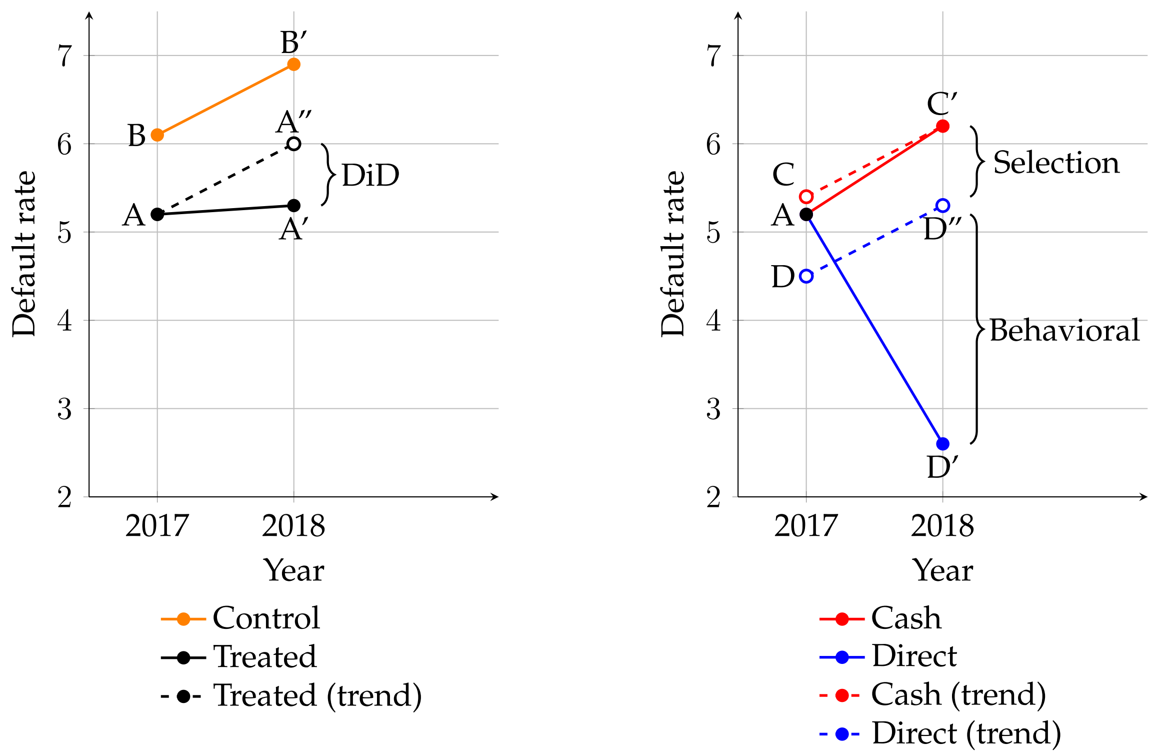
Finally, we compare these pre-policy baselines to decompose the 2018 gap into selection and behavioral components. The 0.9 percentage point gap between the two baselines (points C and D) reflects selection: even before direct-pay existed, future adopters were lower-risk than future cash borrowers. Relative to the total 2018 gap of 3.6 points, selec-

[20] $5.2\% = 0.76 \times 5.4\% + 0.24 \times$ Default Rate, which yields Default Rate $\approx 4.5\%$.

Figure 2: Default Rate Dynamics and Decomposition

(a) Policy Effect: Eligible (Treated) vs. Ineligible (Control) Loans

(b) Decomposition: Cash vs. Direct-Pay Defaults (Eligible Loans Only)



Notes: This figure plots average one-year default rates for various groups of borrowers in 2017 (pre-policy) and 2018 (post-policy). All values are group means without adjusting for borrower characteristics.

Panel (a) illustrates the difference-in-differences design from Section 4.2. The treated group consists of debt-refinancing loans eligible for the direct-pay option (A and A′), while the control group consists of consumption loans that were ineligible (B and B′). Point A″ denotes the counterfactual 2018 default rate for eligible loans under the parallel-trends assumption. The DiD estimate is $A' - A''$, equivalently $(A' - A) - (B' - B)$.

Panel (b) illustrates the decomposition of the direct-pay effect. Point A denotes the overall default rate of eligible borrowers in 2017. Points C′ and D′ denote the observed 2018 default rates of cash and direct-pay borrowers, respectively. Point C is the implied 2017 default rate of future cash borrowers, obtained by removing the aggregate time trend estimated in Panel (a). Point D is the implied 2017 default rate of future direct-pay borrowers, recovered from the weighted-average relationship between A, C, and the 2018 adoption share. Point D″ is the counterfactual 2018 default rate of direct-pay borrowers absent any behavioral improvement. The difference between C and D captures selection, while the difference between D″ and D′ captures behavioral improvement.

tion accounts for about 0.9 points (25%), leaving 2.7 points (75%) attributable to behavioral improvement associated with direct-pay.

A key caveat is that the residual improvement attributed to direct-pay may reflect both commitment effects and the interest-rate discounts offered to adopters. Moreover, interest-rate discounts may also influence borrowers' take-up decisions, affecting the composition of the direct-pay and cash pools and thereby complicating the measurement of selection effects. Because these channels are difficult to disentangle in reduced form, we return to this issue in the next section, where we estimate a structural model that incorporates borrowers' disbursement and repayment decisions and allows us to separate the effects of commitment and pricing incentives.

5 Structural Model and Estimation

The empirical patterns in Section 4 suggest that the direct-pay feature both attracts borrowers with stronger repayment discipline and improves repayment behavior through a commitment effect. To quantify these channels and evaluate alternative designs, we estimate a structural model that embeds repayment and consumption in a dynamic discrete-choice environment. This approach builds on dynamic structural estimation in finance, where models are used to recover economic primitives and evaluate institutional frictions such as financing costs and governance frictions (Hennessy and Whited, 2005; Hennessy and Whited, 2007; Strebulaev, 2007; Morellec, Nikolov, and Schürhoff, 2012). A growing parallel literature applies the same methodology to household and consumer credit settings, including life-cycle consumption choices, payday borrowing, and over-optimistic beliefs (Allcott et al., 2022; Laibson et al., 2024; Exler et al., 2025). We bring this framework to debt refinancing and use it to quantify how commitment mitigates self-control frictions in consumer credit markets.

5.1 Model and Specification

The model has three main features. First, we allow for variation among borrowers in their underlying unobserved creditworthiness, which we interpret as the default cost. Borrowers' unobserved types influence their optimal choices for the disbursement method and their repayment behavior. Second, we explicitly account for the trade-off borrowers face between the direct-pay and cash options. If the borrower selects the cash option, they enjoy an immediate gratification by spending a proportion of the loan amount, leading to an increased future repayment burden. In contrast, the direct-pay option compels borrowers to adhere to their original plan, maintaining a smaller debt burden. Third, we model the borrowers' monthly repayment behavior using a dynamic discrete choice model. The disbursement method affects the total debt burden once the loan originates, and the default cost affects borrowers' incentives to repay their loan.

Let X denote the set of borrower and loan characteristics, such as loan size, annual income, credit score (FICO), debt-to-income ratio, and the length of credit history. We model default cost as a binary type $c \in \{c_H, c_L\}$ to align with our theoretical framework, where separation occurs at a threshold $\bar{c}$ that divides borrowers into two groups. This binary structure captures the essential heterogeneity needed to identify the selection and behavioral effects. The distribution of the default cost c depends on characteristics X and is parameterized by the logistic function,

$$P(H) = \frac{\exp(X\theta_p)}{1 + \exp(X\theta_p)}. \tag{10}$$

The borrower has an existing debt of size s and approaches the lending platform to refinance this debt. They request a loan of equal size s and term $T = 36$. The platform offers two disbursement options, either cash or direct-pay, each with different interest rates. The cash disbursement deposits the loaned amount s to the borrower's account. Once the loan originates, the borrower uses some proportion $\alpha \in [0, 1]$ of the loan for immediate consumption and the platform has no control over this behavior. Their debt burden increases to $(1 + \alpha)s$. The direct-pay option, on the other hand, forwards the loan amount directly to the borrower's existing creditors. To capture heterogeneity in borrowers' propensity to consume, we allow the parameter α to depend on borrowers' observable characteristics with the following specification,

$$\alpha = \frac{\exp(X\theta_\alpha)}{1 + \exp(X\theta_\alpha)}. \tag{11}$$

Throughout the structural model, we specify a linear utility over monetary payoffs, such that $u(x) = x$. The platform determines the interest rates of the loans based on the borrowers' observable characteristics and the disbursement methods they choose. Let r_c and r_d denote the interest rates associated with the cash and direct-pay options, respectively. The platform's interest rate algorithm is considered exogenous in our model and can be estimated from the data. Once the loan originates, the borrower makes monthly repayment decisions until either default or the end of the loan term T. The borrower's strategy includes choosing between cash and direct-pay disbursement options $DP \in \{0, 1\}$, and making repayment decisions $a_t \in \{0, 1\}$ after the loan is originated for $t = 1, \cdots, T$.

Repayment decisions. The repayment decisions follow the models of Kawai, Onishi, and Uetake (2022) and Xin (2025). Once the loan originates, the borrower makes a binary repayment decision a_t at $t = 1, 2, \cdots, T$. The borrower chooses to either repay a fixed monthly installment m ($a_t = 0$) or default ($a_t = 1$). The size of the monthly installment m

is determined by the loan size s, term T, and interest rate r.[21]

The utilities of the two actions at $t < T$ are given by:

$$\begin{cases} u(-m) + \delta V_{t+1}(s, r, c) + \epsilon_{t,0} & \text{for paying installment,} \\ u(-c) + \epsilon_{t,1} & \text{for default,} \end{cases} \tag{12}$$

where $\epsilon_t = (\epsilon_{t,0}, \epsilon_{t,1})$ are the idiosyncratic utility shocks associated with each alternative and are assumed to be i.i.d. and to follow the Type I extreme value distribution with a scale parameter σ_ϵ. We allow σ_ϵ to be different for the cash and direct-pay options and denote them $\sigma_{\epsilon,\text{cash}}$ and $\sigma_{\epsilon,\text{direct}}$. Let δ denote the discount factor. The value function $V_{t+1}(s, r, c)$ is the ex-ante expected continuation value at the beginning of period $t + 1$ if the borrower chooses to repay an installment at t. Specifically,

$$V_{t+1}(s, r, c) = \mathbb{E}\left(\max \left\{ u(-m) + \delta V_{t+2}(s, r, c) + \epsilon_{t+1,0}, u(-c) + \epsilon_{t+1,1} \right\} \right). \tag{13}$$

In the last repayment period T, the continuation value of the loan is zero, so the borrower's utility for the two options is

$$\begin{cases} u(-m) + \epsilon_{T,0} & \text{for paying installment,} \\ u(-c) + \epsilon_{T,1} & \text{for default.} \end{cases} \tag{14}$$

By backward induction, we can derive the ex-ante value of a loan before the borrowers make any repayments $V_1(s, r, c)$. This value is important for the borrowers' choices of disbursement method, as we will explain below.[22]

Choice of disbursement method. When choosing the disbursement method, the borrower takes into account the value of the loan at the repayment stage and their immediate gratification. If the borrower chooses the direct-pay option, they face a loan of size s at the interest rate r_d. So, the value of the loan is $V_1(s, r_d, c)$. For those who choose the cash option, they receive a cash amount of s at the interest rate r_c. After receiving the cash, the borrower uses a certain proportion $\alpha \in [0, 1]$ for immediate discretionary spending and enjoys the utility of spending αs and a fixed benefit of choosing the cash option ω.

The parameter ω is a reduced-form utility shifter that captures non-pecuniary forces

[21] The fixed monthly installment m is given by $s\left[\frac{r(1+r)^T}{(1+r)^T-1}\right]$. The outstanding balance in period t, given that the borrower has paid the monthly installments up to period $t-1$, is $B_t = s\left[\frac{(1+r)^{T+1}-(1+r)^t}{(1+r)^T-1}\right]$.

[22] Note also that $V_1(s, r, c)$ satisfies the submodularity condition in the theoretical framework. See Appendix A.1 for the explanation.

favoring cash disbursement and frictions in adopting the direct-pay option, including preferences for liquidity and flexibility, liquidity constraints, naivete about future self-control problems, inattention to the direct-pay option, and limited understanding of its implications. We allow ω to vary across borrower types and denote them ω_H and ω_L. Because of the spending, the borrower now has a larger debt burden of $(1+\alpha)s$ to pay later. The expected utility of choosing the two kinds of disbursement methods is given by

$$\begin{cases} u(\alpha s+\omega)+V_1((1+\alpha)s, r_c, c)+\eta_0 & \text{for cash,} \\ V_1(s, r_d, c)+\eta_1 & \text{for direct-pay,} \end{cases} \tag{15}$$

where $\eta=(\eta_0,\eta_1)$ is the random utility shocks associated with either option. We similarly let η follow the Type I extreme value distribution with scale parameter σ_η.

Connecting the structural and theoretical models. A key feature of our structural estimation is the use of the propensity to consume, α, to capture the severity of the self-control problem, rather than directly estimating the present-bias parameter, β. This modeling choice is deliberate and rests on two justifications.

First, β is not separately identified in our data. A clean estimation of β would require observing the same individual's conflicting choices between immediate and delayed rewards, typically under ex-ante and ex-post conditions, as is common in other field or experimental settings (Fang and Silverman, 2009; Fang and Wang, 2015; Augenblick and Rabin, 2019; Martinez, Meier, and Sprenger, 2023; Laibson et al., 2024). Our observational data, however, only reveal the outcome of this internal conflict (i.e., the choice of contract and subsequent default), not the preference reversal itself.

Second, and more importantly, α serves as the reduced-form behavioral consequence of the deep present-bias parameter β. While we treated α as exogenous in our simplified theoretical model (Section 3), it can be micro-founded as an endogenous choice variable of the agent. In Equation (4), the "doer" self at $t=1$ chooses an optimal α^* to maximize U_1^{spend}. This optimal α^* is a direct function of β.

Proposition 3. *Assuming u and V_1 are concave in s, the optimal propensity to consume, α^*, is decreasing in β, i.e., $\frac{\partial\alpha^*}{\partial\beta}<0$.*

The intuition is straightforward: as a borrower becomes more present-biased (a lower β), they discount the future negative consequences of debt more heavily. This lowers the perceived marginal cost of spending, leading them to choose a larger α^*. Therefore, α and β are inversely related. In our data, these parameters are observationally equivalent, and

α is the channel through which self-control problems manifest as default risk. Estimating the reduced-form parameter α is therefore sufficient for matching observed choices and default outcomes. Accordingly, the structural model is a behaviorally consistent reduced form of the theoretical planner–doer framework: it preserves the same revealed-choice and default implications through α, but it does not separately identify the underlying present-bias parameter β or the planner–doer welfare wedge.

5.2 Estimation

We estimate the structural parameters of the model using the Generalized Method of Moments (GMM). The estimation is performed on the subsample of 3-year loans to maintain consistency with the model's fixed term structure ($T = 36$ months). The vector of parameters to be estimated includes the default costs (c_H, c_L), the borrower type distribution (θ_p), the propensity for discretionary consumption (θ_α), the type-specific preferences for cash (ω_H, ω_L), and the scale parameters for the idiosyncratic utility shocks ($\sigma_\eta, \sigma_{\epsilon,\text{cash}}, \sigma_{\epsilon,\text{direct}}$). We estimate these parameters by minimizing the weighted quadratic distance between a set of empirical moments from the data and the corresponding moments simulated from the model.

The model is identified by matching key moments that capture the central features of the data and the policy's impact, as shown in the model fit results in Appendix A.5. Specifically, we target four main moments: (1) the average one-year default rate in the 2017 pre-policy cohort; (2) the probability of choosing the direct-pay option in the 2018 post-policy cohort; (3) the one-year default rate for cash borrowers in 2018; and (4) the one-year default rate for direct-pay borrowers in 2018. Identification does not rely solely on these aggregate moments. Each moment is additionally interacted with borrower characteristics X, allowing the estimator to exploit rich cross-sectional variation in adoption and default behavior across observable borrower types.

Specifically, the default-cost parameters are primarily identified by default rates and their variation with observable borrower characteristics, which determine the distribution of default costs. The cash-preference parameters are then identified from adoption decisions after accounting for borrower type composition. Finally, the self-control parameter α is identified by the excess default risk among cash borrowers relative to direct-pay borrowers, after controlling for selection. The intuition parallels our reduced-form decomposition exercise: after accounting for sorting into disbursement options, the remaining difference in default outcomes is attributed to the behavioral effect of unrestricted access to loan proceeds.

Table 6: Parameter Estimates of the Structural Model

Parameter	Estimate	SE
c_L	2.721^{***}	(0.000)
c_H	4.450^{***}	(0.000)
$P(H)$	0.698^{***}	(0.003)
α	0.330^{***}	(0.003)
ω_L	0.393^{***}	(0.016)
ω_H	0.769^{***}	(0.012)
σ_η	0.492^{***}	(0.004)
$\sigma_{\epsilon,\text{direct}}$	0.201^{***}	(0.007)
$\sigma_{\epsilon,\text{cash}}$	0.172^{***}	(0.027)

Notes: This table reports the parameter estimates and average fitted values from the structural model in Section 5.1 using the subsample of 3-year loans. c_L and c_H are the fixed default costs for low- and high-type borrowers, respectively. $P(H)$ and α are the average fitted values for the probability of being a high-type borrower (Equation (10)) and the propensity to consume (Equation (11)), respectively. ω_L and ω_H represent the non-pecuniary utility (preference for flexibility) of the cash option for low- and high-types (Equation (15)). σ_η is the scale parameter for the disbursement choice utility shock (Equation (15)). $\sigma_{\epsilon,\text{direct}}$ and $\sigma_{\epsilon,\text{cash}}$ are the scale parameters for the dynamic repayment (pay vs. default) utility shocks for borrowers in the direct-pay and cash paths, respectively (Equation (12)). Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

5.3 Estimation Results

Table 6 presents the parameter estimates for the structural model, with the corresponding estimates for borrower heterogeneity (covariate effects) detailed in Table 7. Model fit is presented in Table A13 in Appendix A.5.

The first set of parameters provides the intercept terms for the model's core components. Based on our estimates, the default cost for low-type borrowers (c_L) is 2.721 thousand dollars, while the default cost for high-type borrowers (c_H) is 4.450 thousand dollars. The probability of being a high-type borrower varies with observable characteristics: as shown in Table 7, borrowers with higher annual income, higher FICO scores, lower debt-to-income ratios, and longer credit histories are significantly more likely to be the high-type. On average, 69.8% of the borrowers in our sample are estimated to be the high-type.

We also find a model-implied average discretionary share $\hat{\alpha} = 0.33$. This suggests that, on average, borrowers spend approximately 33.0% of the cash loan on discretionary consumption. Table 7 indicates that borrowers with higher annual incomes and longer credit histories have lower α. Interestingly, borrowers with higher debt-to-income ratios also exhibit lower α, perhaps reflecting a stronger incentive to avoid wasteful spending

Table 7: Covariate Effects on Type Distribution ($P(H)$), Consumption (α), and Marginal Probabilities

	$P(H)$			α			P(Direct)	P(Default)
	Est	SE	ME	Est	SE	ME	ME	ME
Loan amount	1.000***	(0.016)	0.177	−0.915***	(0.014)	-0.173	-0.030	-0.016
Log annual income	0.294***	(0.008)	0.052	−0.234***	(0.002)	-0.044	-0.010	-0.007
FICO score	0.025**	(0.013)	0.004	0.000	(0.017)	0.000	-0.001	-0.000
Debt-to-income ratio	0.094***	(0.001)	0.017	−0.045***	(0.003)	-0.009	-0.003	-0.002
Own home (indicator)	0.013**	(0.005)	0.002	0.004	(0.004)	0.001	-0.000	-0.000
Credit history length	0.048***	(0.013)	0.009	−0.051***	(0.010)	-0.010	-0.002	-0.001
Inquiries last 6 months	−0.028**	(0.011)	-0.005	−0.022	(0.018)	-0.004	0.000	0.000
Open accounts	0.091***	(0.007)	0.016	−0.066***	(0.002)	-0.012	-0.003	-0.002
Revolving utilization	0.115***	(0.013)	0.020	−0.104***	(0.014)	-0.020	-0.004	-0.003
Intercept	1.127***	(0.016)	0.200	−0.923***	(0.014)	-0.175	0.000	-0.000

Notes: This table reports the coefficient estimates for the heterogeneity in the structural model. The table reports the coefficients (θ_p, θ_α) showing the effect of observables on the probability of being a high-type, $P(H)$ (Equation (10)), and on the propensity to consume, α (Equation (11)). The "ME" columns report the marginal effects of each characteristic on the simulated probability of choosing the direct-pay option and the overall probability of default. "Est" denotes estimates, "SE" denotes standard errors, and "ME" denotes marginal effects. Standard errors are computed using GMM. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

given their precarious financial state.

The subsequent parameters in Table 6 shed light on the non-pecuniary drivers of selection and repayment. The estimates for ω_L and ω_H quantify the utility of liquidity and flexibility, the "cash preference", for low- and high-type borrowers, respectively. We find this preference is positive and highly significant for both types but substantially larger for high-type borrowers ($\hat{\omega}_H = 0.769$ thousand dollars) compared to low-type borrowers ($\hat{\omega}_L = 0.393$ thousand dollars). Despite this higher cash preference, high-type borrowers are still more likely to choose the direct-pay option because their higher default costs ($c_H > c_L$) make the commitment value of direct-pay more valuable relative to the foregone cash utility.

Borrower heterogeneity. Table 7 outlines the determinants of the borrower's type and their propensity for discretionary consumption. The probability of being a high-type, $P(H)$, is positively associated with standard measures of financial stability and creditworthiness, such as higher loan amounts, higher log annual income, higher FICO score, lower debt-to-income ratio, home ownership, longer credit history, more open accounts, and higher revolving utilization. This aligns with the interpretation of the high-type as being more creditworthy and reliable. The model also shows that the propensity for discretionary spending (α), which captures self-control problems, is significantly lower for borrowers with higher loan amounts, higher incomes, higher DTI, longer credit histories, more open accounts, and higher revolving utilization. This suggests that both financial discipline (income, history) and financial constraints (DTI, loan amount) can reduce impulsive spending. Homeownership has little additional explanatory power for α in the fitted model.

Finally, the marginal effects (ME) in Table 7 reveal the net impact of these characteristics on the two key outcomes: disbursement choice and default probability. We find that larger loan amounts are associated with lower direct-pay adoption (ME = -0.030), while characteristics like income, FICO, and credit history have smaller negative effects. The marginal effects on the overall probability of default, $P(\text{default})$, synthesize these competing channels. Consistent with standard intuition, higher income, higher FICO scores, longer credit history, lower DTI, more open accounts, and lower revolving utilization are all associated with a lower default probability. These results are driven by the structural channels we have identified: borrowers with better financial characteristics are more likely to be high-types (higher $P(H)$) and have lower propensities to consume (lower α), both of which reduce default risk.

5.4 Counterfactual Analysis

With the estimated parameters, we now conduct counterfactual simulations to decompose the policy's effects and evaluate alternative policy designs. We simulate borrower choices and repayment decisions under several scenarios with 3-year debt-refinancing loans in the 2018 cohort. The results are presented in Table 8. We remind the reader that all figures are simulations based on the estimated model parameters and not descriptive shares.

Self-control problems. The results in Table 8, Panel A, quantify the role of self-control problems in driving default. The "rational benchmark" scenario (row 1) shows that without self-control problems ($\alpha = 0$), the market's default rate would be 3.75%. The "behavioral baseline" scenario (row 2) shows that introducing self-control problems ($\alpha > 0$) increases the default rate to 5.75%. This 2.00 percentage point increase implies that self-control issues account for approximately 35% of all defaults in the pre-policy market.

Policy effects and decomposition. We next decompose the policy's effects into selection, behavioral commitment, and price incentives. Panel B holds sorting fixed at the current-policy take-up rate (26.0%) and compares three scenarios. Row 3 simulates the

Table 8: Simulation Results: Decomposition and Policy Designs

Scenario	Take-up rate	Default rate		
		Cash	Direct-pay	Overall
Panel A: Self-control problems				
(1) Rational benchmark ($\alpha = 0$)	-	3.75%	-	3.75%
(2) Behavioral baseline (pre-intervention)	-	5.75%	-	5.75%
Panel B: Policy effects and decomposition				
(3) Current policy	26.0%	5.82%	2.64%	5.00%
(4) Selection only (no behavioral improvement)	26.0%	5.82%	5.55%	5.75%
(5) Behavioral improvement (no discount)	26.0%	5.82%	3.85%	5.31%
Panel C: Counterfactual policy simulations				
(6) Zero discount (with endogenous adoption)	23.3%	5.87%	3.72%	5.37%
(7) Mandatory direct-pay (100% take-up)	100%	-	3.01%	3.01%
(8) Reduced adoption friction (ω down by $200)	34.3%	5.84%	2.68%	4.76%
(9) Reduced discretionary spending (α down by 10%)	25.8%	5.54%	2.70%	4.80%

Notes: This table reports the simulated one-year default rates from the estimated structural model under various counterfactual scenarios, using the subsample of 3-year eligible (debt-refinancing) loans in 2018. "Take-up rate" reports the percentage of borrowers who choose the direct-pay option in that scenario. "Default rate," which is split into "cash," "direct-pay," and "overall," reports the average one-year default rates for the respective groups. Panel A compares scenarios with and without self-control problems. Panel B decomposes the policy effects by comparing scenarios with different policy features. Panel C evaluates alternative policy designs.

current policy. Row 4 preserves the current-policy sorting pattern but shuts down all policy-induced improvement, including both the behavioral commitment device and the interest-rate discount, so that direct-pay borrowers follow the same repayment pathway they would have under the pre-intervention regime. Row 5 restores behavioral improvement but removes the interest-rate discount.

Under the current policy (row 3), the aggregate default rate falls from 5.75% in the behavioral baseline to 5.00%, a total reduction of 0.75 percentage points. Cash borrowers default at 5.82%, while direct-pay borrowers default at 2.64%, a gap of 3.18 percentage points.

Row 4 isolates the role of sorting. Because all policy-induced improvement is switched off, the overall default rate equals the behavioral baseline of 5.75%. Under this counterfactual, cash borrowers default at 5.82% while direct-pay borrowers default at 5.55%. The 0.27 percentage-point gap between the two groups therefore reflects selection alone: given current-policy sorting, the borrowers who choose direct-pay would have been lower-risk even absent any improvement from the policy.

We then introduce behavioral improvement while holding sorting fixed and keeping the discount at zero (row 5). Direct-pay default falls from 5.55% to 3.85%, so the gain from row 4 to row 5, 1.70 percentage points among adopters and 0.44 percentage points at the aggregate level, is attributable entirely to behavioral commitment. Finally, comparing row 5 to the current policy (row 3) introduces the observed interest-rate discount. Direct-pay default falls by a further 1.21 percentage points, to 2.64%, and aggregate default falls by 0.31 percentage points, to 5.00%; these increments isolate the additional benefit from the rate discount. Decomposing the 3.18 percentage-point cash–direct gap under the current policy accordingly yields three pieces: 0.27 percentage points from selection (8%), 1.70 percentage points from behavioral commitment (53%), and 1.21 percentage points from the discount (38%).

It is instructive to compare the Panel B decomposition with the reduced-form decomposition exercise in Section 4.4. Both approaches attribute the majority of the default-rate gap to policy-induced improvement rather than selection. Quantitatively, however, the structural model assigns a smaller role to selection. A likely reason is that the reduced-form decomposition cannot separately account for the effects of interest-rate discounts on take-up and repayment outcomes, whereas these channels are explicitly modeled in the structural framework. In addition, the structural model allows for richer borrower heterogeneity in default costs, cash preferences, and responses to commitment and pricing incentives. This comparison illustrates how the structural model complements the reduced-form evidence by providing a more nuanced decomposition of the policy effect into its underlying selection, commitment, and pricing components.

Other policy designs. We now consider several stylized counterfactual policies to evaluate alternative designs. The simulation results are presented in Table 8, Panel C.

First, we simulate a zero-discount policy with endogenous adoption (row 6). Unlike the behavioral improvement counterfactual in row 5, which holds adoption fixed as the current policy at 26%, borrowers here choose whether to adopt direct-pay given the terms offered, so both take-up and the composition of the cash and direct-pay pools respond to the policy. Relative to the current policy (row 3), removing the interest-rate discount raises aggregate default from 5.00% to 5.37% (so the discount contributes 0.37 percentage points of the total 0.75 percentage-point reduction in the overall default rate) and lowers take-up from 26.0% to 23.3%. The limited take-up response helps explain why lenders have shifted to pairing direct-pay disbursement with only modest rate reductions rather than offering large discounts as in the pilot stage. The commitment value alone is sufficient to attract the relevant borrowers, while larger discounts do little to expand adoption because the resulting interest savings are realized only gradually through future repayments.[23] We next consider alternative designs that target adoption frictions more directly.

Second, we simulate a "mandatory direct-pay" policy where all borrowers are required to take the direct-pay option. This simulates a full intervention among the 2018 applicant pool, holding loan volume fixed. Because borrowers declare they are borrowing to restructure debt, this policy could help filter out those who may misrepresent their loan's purpose. Row (7) shows that this policy reduces the overall default rate to 3.01%, a 1.99 percentage point improvement over the current policy's 5.00%. Two important caveats qualify this result. First, eliminating cash removes liquidity that some borrowers may need for legitimate reasons (for example, urgent expenses unrelated to impulsive spending), and our binary-type model does not quantify the associated borrower welfare loss. Second, the simulation does not incorporate platform-level selection at the application margin: if mandatory direct-pay were announced, borrowers with strong cash needs might not apply at all or might seek outside options, so the 3.01% default rate is best interpreted as a stylized in-sample benchmark rather than a forecast of realized portfolio performance once loan volume and applicant composition adjust. A more realistic implementation might require a minimum percentage of the loan to be direct-pay, which would place the default rate between 3.01% and 5.00% while preserving partial liquidity.

Third, we simulate a one-off adoption bonus: borrowers who choose direct-pay receive \$200 at origination (row 8), modeled as a \$200 reduction in the cash preference ω. We set the bonus at \$200 to match the magnitude of the typical sign-up bonuses that credit card issuers and commercial banks commonly offer for opening a new account.[24] This design targets the same friction that deters commitment (borrowers' taste for immediate liquidity) but delivers the incentive upfront rather than through a stream of future interest savings. Because present-biased borrowers overweight short-run consumption, a lump-sum bonus may be especially effective at encouraging take-up. Row (8) shows that take-up rises from 26.0% under the current policy to 34.3%, an increase of more than eight percentage points, while the overall default rate falls to 4.76%, a 0.24 percentage point improvement over the current policy. Relative to the interest-rate discount, a modest upfront bonus can therefore deliver substantially larger adoption gains.

Fourth, we simulate a "reduced discretionary spending" scenario by assuming borrowers use a 10% smaller proportion α of the loan for discretionary consumption. In our

[23] As of early 2026, for example, SoFi's credit-card consolidation direct-pay option offers only a 0.25% rate reduction; see https://www.sofi.com/personal-loans/credit-card-consolidation-loans/.

[24] As of 2026, many major credit card issuers (such as Discover and Chase) advertise sign-up bonuses of about \$200 on selected card products for new accounts. See https://www.nerdwallet.com/credit-cards/learn/cash-back-credit-cards-offer-signup-bonus.

model, α captures the share of the loan that the $t = 1$ "doer" self diverts to immediate consumption instead of debt repayment, so a lower α corresponds to policies that limit the scope for impulsive spending, such as faster disbursement that shortens the gap between approval and funding (keeping the borrower closer to the $t = 0$ "planner" state), installment disbursement of the cash portion rather than a lump-sum payout, or routing the funds through a controlled spending channel (e.g., a card or account that restricts high-temptation categories). These interventions effectively reduce the amount of cash that is freely available at the moment of temptation. In this conservative scenario (row 9), reducing α by 10% lowers the overall default rate to 4.8%, a 0.2 percentage point improvement relative to the current policy.

6 Conclusion

This paper provides new field evidence that self-control problems are significant and costly frictions in consumer credit markets, distinct from traditional models of asymmetric information. We analyze the introduction of a "direct-pay" disbursement option by a large fintech lender, a policy innovation that functions as both a commitment device for behavioral frictions and a screening mechanism for information frictions. Our contribution is to integrate these two frameworks theoretically and empirically, developing and estimating a structural model that simultaneously features unobserved borrower heterogeneity and self-control problems, allowing us to separately quantify the selection and behavioral channels that drive the policy's effectiveness.

Our theoretical framework, based on time-inconsistent preferences, shows that sophisticated, present-biased borrowers have an ex-ante incentive to adopt commitment to constrain their ex-post selves from misusing funds. The model also demonstrates how the direct-pay option induces a separating equilibrium on default cost: high-cost borrowers disproportionately adopt direct-pay despite self-control problems, while low-cost borrowers prefer cash for discretionary spending. This dual role aligns borrower and lender incentives in ways that differ from zero-sum screening in classic adverse-selection models.

Our empirical analysis documents improved repayment under the policy and disentangles its dual effects. A difference-in-differences comparison on the same platform implies that making the direct-pay option available reduced default rates for eligible borrowers by approximately 0.6 percentage points (about an 11% decline relative to the pre-policy mean). A reduced-form decomposition attributes about one-quarter of the eligible-sample cash–direct gap to selection and about three-quarters to a residual adopter effect that combines commitment and pricing; we treat these figures as illustrative under maintained separation assumptions.

To quantify these mechanisms and evaluate counterfactuals, we develop and estimate a dynamic structural model of borrower choice and repayment. Our estimates reveal that self-control problems account for approximately 35% of all defaults in the pre-policy market. The model implies an average discretionary share of 33.0% of cash loan proceeds, and that 69.8% of borrowers are high-types who benefit from commitment. On the 3-year subsample, the structural decomposition yields a similar residual magnitude (2.91 versus 2.7 percentage points) but a smaller selection share (about 8% versus 25%). Holding sorting fixed, behavioral commitment accounts for 0.44 percentage points of the 0.75 percentage-point total improvement and the discount for 0.31 percentage points, reducing aggregate default from 5.75% to 5.00%. A zero-discount counterfactual with endogenous adoption still lowers defaults to 5.37%, but the modest incremental gain from the observed discount and the limited take-up response suggest that pricing is a weak lever relative to commitment. A mandatory direct-pay policy could further reduce defaults to 3.01% among current applicants in a stylized benchmark that eliminates cash liquidity. We also find that offering an upfront bonus for direct-pay adoption and reducing borrowers' propensity to divert loan proceeds toward discretionary spending are both effective in lowering the overall default rate.

Our findings are consistent with the view that self-control problems impose costs on both borrowers and lenders and that commitment-oriented contract design can improve repayment outcomes. Because we do not quantify borrower welfare (including the utility cost of reduced liquidity), lender profits, or investor returns, however, stronger claims of mutually beneficial ("win-win") market design remain partial-equilibrium hypotheses supported by default evidence rather than welfare rankings. This paper nonetheless shows that financial contracts can be powerful tools to address behavioral biases, suggesting avenues for credit-market innovation aimed at mitigating self-control. The integration of behavioral frameworks with asymmetric information, both theoretically and empirically, opens new directions for understanding and designing credit contracts that address the complex frictions present in modern consumer finance markets.

References

Agarwal, Sumit, Muris Hadzic, Changcheng Song, and Yildiray Yildirim (2023). "Liquidity Constraints, Consumption, and Debt Repayment: Evidence from Macroprudential Policy in Turkey". *The Review of Financial Studies* 36.10, pp. 3953–3998.

Akerlof, George A. (1970). "The Market for "Lemons": Quality Uncertainty and the Market Mechanism". *The Quarterly Journal of Economics* 84.3, pp. 488–500.

Allcott, Hunt, Joshua Kim, Dmitry Taubinsky, and Jonathan Zinman (2022). "Are High-Interest Loans Predatory? Theory and Evidence from Payday Lending". *The Review of Economic Studies* 89.3, pp. 1041–1084.

Ashraf, Nava, Dean Karlan, and Wesley Yin (2006). "Tying Odysseus to the Mast: Evidence From a Commitment Savings Product in the Philippines". *The Quarterly Journal of Economics* 121.2, pp. 635–672.

Attanasio, Orazio, Agnes Kovacs, and Patrick Moran (2020). *Temptation and Commitment: A Model of Hand-to-Mouth Behavior*. Working Paper.

Augenblick, Ned, Muriel Niederle, and Charles Sprenger (2015). "Working over Time: Dynamic Inconsistency in Real Effort Tasks". *The Quarterly Journal of Economics* 130.3, pp. 1067–1115.

Augenblick, Ned and Matthew Rabin (2019). "An Experiment on Time Preference and Misprediction in Unpleasant Tasks". *The Review of Economic Studies* 86.3, pp. 941–975.

Avery, Mallory, Osea Giuntella, and Peiran Jiao (2025). "Why Don't We Sleep Enough? A Field Experiment among College Students". *The Review of Economics and Statistics* 107.1, pp. 65–77.

Beshears, John, James J. Choi, Christopher Harris, David Laibson, Brigitte C. Madrian, and Jung Sakong (2020). "Which early withdrawal penalty attracts the most deposits to a commitment savings account?" *Journal of Public Economics* 183, p. 104144.

Beshears, John, James J. Choi, David Laibson, and Brigitte C. Madrian (2018). "Behavioral Household Finance". *Handbook of Behavioral Economics: Applications and Foundations 1*. Ed. by B. Douglas Bernheim, Stefano DellaVigna, and David Laibson. Vol. 1. Handbook of Behavioral Economics - Foundations and Applications 1. North-Holland, pp. 177–276.

Bryan, Gharad, Dean Karlan, and Scott Nelson (2010). "Commitment Devices". *Annual Review of Economics* 2.1, pp. 671–698.

Buchak, Greg, Gregor Matvos, Tomasz Piskorski, and Amit Seru (2018). "Fintech, regulatory arbitrage, and the rise of shadow banks". *Journal of Financial Economics* 130.3, pp. 453–483.

Campbell, John Y. (2006). “Household Finance”. *The Journal of Finance* 61.4, pp. 1553–1604.

Campbell, John Y. and Tarun Ramadorai (2026). *Household Finance in Retrospect and Prospect*. Working Paper.

deHaan, Ed, Jungbae Kim, Ben Lourie, and Chenqi Zhu (2024). “Buy Now Pay (Pain?) Later”. *Management Science* 70.8, pp. 5586–5598.

DellaVigna, Stefano (2009). “Psychology and Economics: Evidence from the Field”. *Journal of Economic Literature* 47.2, pp. 315–372.

DellaVigna, Stefano and Ulrike Malmendier (2004). “Contract Design and Self-Control: Theory and Evidence”. *The Quarterly Journal of Economics* 119.2, pp. 353–402.

Derksen, Laura, Jason T Kerwin, Natalia Ordaz Reynoso, and Olivier Sterck (2025). “Healthcare Appointments as Commitment Devices”. *The Economic Journal* 135.665, pp. 81–118.

Einav, Liran, Mark Jenkins, and Jonathan Levin (2012). “Contract Pricing in Consumer Credit Markets”. *Econometrica* 80.4, pp. 1387–1432.

Exler, Florian, Igor Livshits, James MacGee, and Michèle Tertilt (2025). “Consumer Credit with Over-Optimistic Borrowers”. *Journal of the European Economic Association* 23.4, pp. 1431–1478.

Fang, Hanming and Dan Silverman (2009). “Time-Inconsistency and Welfare Program Participation: Evidence from the Nlsy”. *International Economic Review* 50.4, pp. 1043–1077.

Fang, Hanming and Yang Wang (2015). “Estimating Dynamic Discrete Choice Models with Hyperbolic Discounting, with an Application to Mammography Decisions”. *International Economic Review* 56.2, pp. 565–596.

Fuster, Andreas, Matthew Plosser, Philipp Schnabl, and James Vickery (2019). “The Role of Technology in Mortgage Lending”. *The Review of Financial Studies* 32.5, pp. 1854–1899.

Galperti, Simone (2015). “Commitment, Flexibility, and Optimal Screening of Time Inconsistency”. *Econometrica* 83.4, pp. 1425–1465.

Gathergood, John (2012). “Self-control, financial literacy and consumer over-indebtedness”. *Journal of Economic Psychology* 33.3, pp. 590–602.

Giné, Xavier, Dean Karlan, and Jonathan Zinman (2010). “Put Your Money Where Your Butt Is: A Commitment Contract for Smoking Cessation”. *American Economic Journal: Applied Economics* 2.4, pp. 213–235.

Gomes, Francisco, Michael Haliassos, and Tarun Ramadorai (2021). “Household Finance”. *Journal of Economic Literature* 59.3, pp. 919–1000.

Gul, Faruk and Wolfgang Pesendorfer (2001). “Temptation and Self-Control”. *Econometrica* 69.6, pp. 1403–1435.

Heidhues, Paul and Botond Kőszegi (2010). “Exploiting Naïvete about Self-Control in the Credit Market”. *American Economic Review* 100.5, pp. 2279–2303.

Hennessy, Christopher A. and Toni M. Whited (2005). “Debt Dynamics”. *The Journal of Finance* 60.3, pp. 1129–1165.

Hennessy, Christopher A. and Toni M. Whited (2007). “How Costly Is External Financing? Evidence from a Structural Estimation”. *The Journal of Finance* 62.4, pp. 1705–1745.

Hertzberg, Andrew, Andres Liberman, and Daniel Paravisini (2018). “Screening on loan terms: evidence from maturity choice in consumer credit”. *The Review of Financial Studies* 31.9, pp. 3532–3567.

Hirshleifer, David (2015). “Behavioral Finance”. *Annual Review of Financial Economics* 7.1, pp. 133–159.

Jagtiani, Julapa, Catharine Lemieux, and Brandon Goldstein (2023). *Did Fintech Loans Default More During the COVID-19 Pandemic? Were Fintech Firms “Cream-Skimming” the Best Borrowers?* Working paper (Federal Reserve Bank of Philadelphia) 23-26. Federal Reserve Bank of Philadelphia, pp. 23–26.

John, Anett (2020). “When Commitment Fails: Evidence from a Field Experiment”. *Management Science* 66.2, pp. 503–529.

Kaur, Supreet, Michael Kremer, and Sendhil Mullainathan (2015). “Self-Control at Work”. *Journal of Political Economy* 123.6, pp. 1227–1277.

Kawai, Kei, Ken Onishi, and Kosuke Uetake (2022). “Signaling in Online Credit Markets”. *Journal of Political Economy* 130.6, pp. 1585–1629.

Kuchler, Theresa and Michaela Pagel (2021). “Sticking to your plan: The role of present bias for credit card paydown”. *Journal of Financial Economics* 139.2, pp. 359–388.

Laibson, David (1997). “Golden Eggs and Hyperbolic Discounting”. *The Quarterly Journal of Economics* 112.2, pp. 443–477.

Laibson, David, Sean Chanwook Lee, Peter Maxted, Andrea Repetto, and Jeremy Tobacman (2024). “Estimating Discount Functions with Consumption Choices over the Lifecycle”. *The Review of Financial Studies*, hhae035.

Martinez, Seung-Keun, Stephan Meier, and Charles Sprenger (2023). “Procrastination in the Field: Evidence from Tax Filing”. *Journal of the European Economic Association* 21.3, pp. 1119–1153.

Maxted, Peter (2025). “Present bias unconstrained: Consumption, welfare, and the present-bias dilemma”. *The Quarterly Journal of Economics* 140.4, pp. 2963–3013.

Meier, Stephan and Charles Sprenger (2010). “Present-Biased Preferences and Credit Card Borrowing”. *American Economic Journal: Applied Economics* 2.1, pp. 193–210.

Meier, Stephan and Charles D. Sprenger (2013). “Discounting financial literacy: Time preferences and participation in financial education programs”. *Journal of Economic Behavior & Organization* 95, pp. 159–174.

Morellec, Erwan, Boris Nikolov, and Norman Schürhoff (2012). “Corporate Governance and Capital Structure Dynamics”. *The Journal of Finance* 67.3, pp. 803–848.

Myerson, Roger B. (1981). “Optimal Auction Design”. *Mathematics of Operations Research* 6.1, pp. 58–73.

O’Donoghue, Ted and Matthew Rabin (1999). “Doing It Now or Later”. *The American Economic Review* 89.1, pp. 103–124.

O’Donoghue, Ted and Matthew Rabin (2001). “Choice and Procrastination”. *The Quarterly Journal of Economics* 116.1, pp. 121–160.

Paserman, M Daniele (2008). “Job search and hyperbolic discounting: Structural estimation and policy evaluation”. *The Economic Journal* 118.531, pp. 1418–1452.

Polo, Alberto, Arthur Taburet, and Quynh-Anh Vo (2025). “Screening using a menu of contracts: A structural model for lending markets”. *Journal of Financial Economics* 169, p. 104056.

Rothschild, Michael and Joseph Stiglitz (1976). “Equilibrium in Competitive Insurance Markets: An Essay on the Economics of Imperfect Information*”. *The Quarterly Journal of Economics* 90.4, pp. 629–649.

Schilbach, Frank (2019). “Alcohol and Self-Control: A Field Experiment in India”. *American Economic Review* 109.4, pp. 1290–1322.

Stiglitz, Joseph E. and Andrew Weiss (1981). “Credit Rationing in Markets with Imperfect Information”. *The American Economic Review* 71.3, pp. 393–410.

Strebulaev, Ilya A. (2007). “Do Tests of Capital Structure Theory Mean What They Say?” *The Journal of Finance* 62.4, pp. 1747–1787.

Strömbäck, Camilla, Thérèse Lind, Kenny Skagerlund, Daniel Västfjäll, and Gustav Tinghög (2017). “Does self-control predict financial behavior and financial well-being?” *Journal of Behavioral and Experimental Finance* 14, pp. 30–38.

Thaler, Richard H. and Shlomo Benartzi (2004). “Save More Tomorrow™: Using Behavioral Economics to Increase Employee Saving”. *Journal of Political Economy* 112.S1, S164–S187.

Thaler, Richard H. and H. M. Shefrin (1981). “An Economic Theory of Self-Control”. *Journal of Political Economy* 89.2, pp. 392–406.

Toussaert, Séverine (2018). “Eliciting Temptation and Self-Control Through Menu Choices: A Lab Experiment”. *Econometrica* 86.3, pp. 859–889.

Vihriälä, Erkki (2023). “Self-imposed liquidity constraints via voluntary debt repayment”. *Journal of Financial Economics* 150.2, p. 103708.

Wang, Hongchang and Eric M. Overby (2022). “How Does Online Lending Influence Bankruptcy Filings?” *Management Science* 68.5, pp. 3309–3329.

Wilson, Charles (1977). “A model of insurance markets with incomplete information”. *Journal of Economic Theory* 16.2, pp. 167–207.

Xin, Yi (2025). “Asymmetric Information, Reputation, and Welfare in Online Credit Markets”. *Rand Journal of Economics*, Forthcoming.

Yannelis, Constantine and Anthony Lee Zhang (2023). “Competition and selection in credit markets”. *Journal of Financial Economics* 150.2, p. 103710.

Zinman, Jonathan (2015). “Household Debt: Facts, Puzzles, Theories, and Policies”. *Annual Review of Economics* 7.1, pp. 251–276.

Appendix A

A.1 Model Details

Proof of Proposition 1. Let $\Delta V_1 = V_1^{\text{abstain}} - V_1^{\text{spend}}$ be the net continuation value lost from discretionary spending.

The $t = 0$ "planner" self prefers to abstain if $U_0^{\text{abstain}} > U_0^{\text{spend}}$. Using Equation (3), this implies $u(\alpha s) < \delta \Delta V_1$. The $t = 1$ "doer" self prefers to spend if $U_1^{\text{spend}} > U_1^{\text{abstain}}$. Using Equation (4), this implies $u(\alpha s) > \beta\delta\Delta V_1$. Thus, a self-control problem exists if both conditions are met, such that the utility of gratification $u(\alpha s)$ falls within the region:

$$\beta\delta\Delta V_1 < u(\alpha s) < \delta\Delta V_1.$$

This region is non-empty for any $\beta < 1$.

Details of Example in Section 3.2. Assume that $r > \frac{1}{\delta} - 1$. In other words, the interest rate is sufficiently high with respect to the discount factor. This assumption ensures that repayment is sufficiently costly such that the borrower would not always choose to consume.

We search for the region of time-inconsistent preferences.

Case 1: $c < (1 + r)s$. The default cost is so low that the borrower will always default in period 2, so $V_1^{\text{spend}} = V_1^{\text{abstain}} = -c$. Therefore, $U_1^{\text{spend}} > U_1^{\text{abstain}}$ and $U_0^{\text{spend}} > U_0^{\text{abstain}}$. Spending is preferable from the perspectives of both periods 0 and 1.

Case 2: $(1+r)s < c < (1+\alpha)(1+r)s$. First, observe that spending implies default, and abstinence implies no default. $V_1^{\text{spend}} = -c$ and $V_1^{\text{abstain}} = -(1 + r)s$. From the perspective of period 0, when $c/s < 1 + r + \frac{\alpha}{\delta}$, $U_0^{\text{spend}} > U_0^{\text{abstain}}$. From the perspective of period 1, when $\beta < \frac{\alpha}{\delta(c/s-1-r)}$, $U_1^{\text{spend}} > U_1^{\text{abstain}}$.

Case 3: $c > (1 + \alpha)(1 + r)s$. The default cost is so high that the borrower will always repay in period 2, so $V_1^{\text{spend}} = -(1 + \alpha)(1 + r)s$ and $V_1^{\text{abstain}} = -(1 + r)s$. Under the assumption $r > \frac{1}{\delta} - 1$, $U_0^{\text{spend}} < U_0^{\text{abstain}}$, and the borrower always prefers to abstain from the perspective of period 0. From the perspective of period 1, however, when $\beta < \frac{1}{\delta(1+r)}$, i.e., the borrower is more present-biased, $U_1^{\text{spend}} > U_1^{\text{abstain}}$. So, the borrower prefers to spend from the perspective of period 1. In other words, these borrowers have their spending preferences reversed.

Proof of Proposition 2. The single-crossing assumption implies an increasing-difference relationship in future value derived from the two options. Let $\Delta V_1(c) = V_1(s, r_d, c) -$

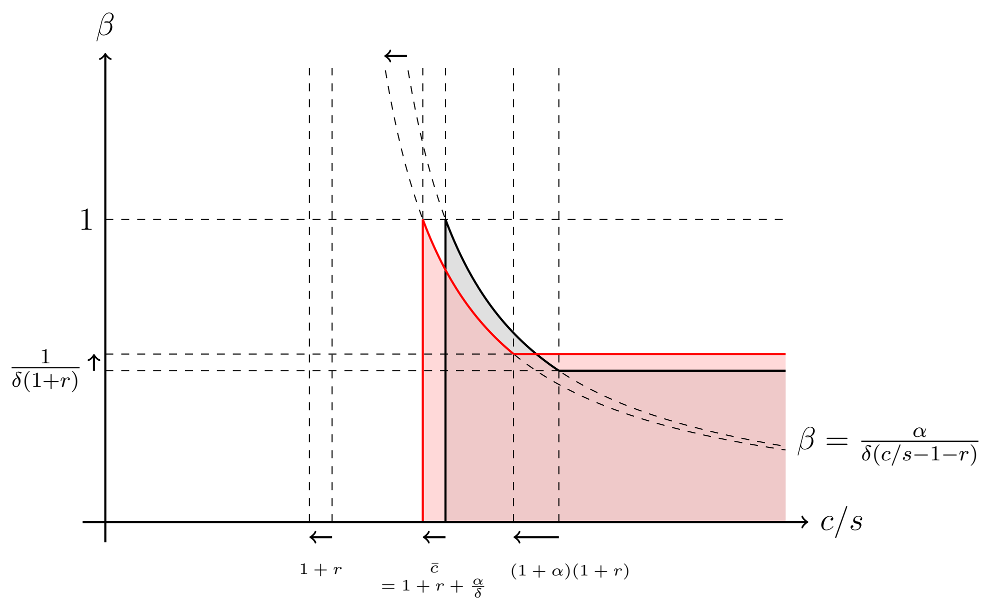
$V_1((1+\alpha)s, r_c, c)$ be the net gain in continuation value from abstaining. The single-crossing assumption means this net future gain is greater for the high-type:

$$\Delta V_1(c_H) > \Delta V_1(c_L) \tag{A.1}$$

A separating equilibrium in the frictionless benchmark is achieved by satisfying two incentive-compatibility (IC) constraints based on the borrowers' ex-ante ($t = 0$) utilities from Equation (3). A borrower of type c will choose direct-pay if $U_0^{\text{abstain}} > U_0^{\text{spend}}$. The condition to choose direct-pay (Abstain) is $\delta \Delta V_1(c) > u(\alpha s)$. If the immediate consumption utility $u(\alpha s)$ falls between these two values: $\delta \Delta V_1(c_L) < u(\alpha s) < \delta \Delta V_1(c_H)$, the high-type borrowers will choose direct-pay, and low-type borrowers will choose cash.

Effect of lower interest rate. We illustrate the effect of a lower interest rate on the separating equilibrium in Figure A3. The lower interest rate shifts the cutoff $\bar{c}$ downward, expanding the pool of borrowers who choose direct-pay.

Figure A3: Effect of lower interest rate



Notes: The figure illustrates the effect of a lower interest rate on the separating equilibrium. The x-axis is the default cost c/s, and the y-axis is the present-bias parameter β.

Proof of Proposition 3. The first order condition yields

$$\frac{\partial u(\alpha s)}{\partial \alpha} + \beta\delta \frac{\partial V_1^{\text{spend}}}{\partial \alpha} = 0, \tag{A.2}$$

which characterizes the optimal propensity to spend α^* as an implicit function of β. Differentiating the FOC with respect to β,

$$\delta \frac{\partial V_1^{\text{spend}}}{\partial \alpha} + \frac{\partial \alpha}{\partial \beta}\left(\frac{\partial^2 u(\alpha s)}{\partial \alpha^2} + \beta\delta \frac{\partial^2 V_1^{\text{spend}}}{\partial \alpha^2}\right) = 0 \tag{A.3}$$

Because $\frac{\partial V_1^{\text{spend}}}{\partial \alpha} < 0$ and the term in the parentheses is also negative, $\frac{\partial \alpha^*}{\partial \beta} < 0$.

Submodularity of $V_1(\cdot)$ in Equation (13) in the structural model. The value of the loan at the beginning of the repayment stage, $V_1(s, r, c)$, is submodular in both (s, c) and (r, c). That is,

$$\frac{\partial^2}{\partial s \partial c} V_1(s, r, c) < 0 \text{ and } \frac{\partial^2}{\partial r \partial c} V_1(s, r, c) < 0. \tag{A.4}$$

We provide an intuitive explanation of this result, which follows the same logic as Kawai, Onishi, and Uetake (2022). First, note that the value of the loan decreases in s and r, *i.e.*, $\frac{\partial}{\partial s} V_1(s, r, c) < 0$ and $\frac{\partial}{\partial r} V_1(s, r, c) < 0$. Additionally, good borrowers with a high default cost c are more likely to choose to make repayments, so their loan value is more responsive to changes in the monthly installment m. Because the monthly installment m increases monotonically with the loan size s and the interest rate r, the partials $\frac{\partial}{\partial s} V_1(s, r, c)$ and $\frac{\partial}{\partial r} V_1(s, r, c)$ are decreasing in c, giving rise to submodularity.

A.2 Borrower Characteristics Balance Test

Table A9: Borrower Characteristics: Eligible vs. Ineligible Loan Types

(a) Pre-Policy (2017)

Variable	Eligible	Ineligible	StdDev	Cohen's d
Annual Income	79.30	81.00	236.00	-0.01
Debt-to-Income Ratio	18.70	16.80	8.21	0.23
FICO Score	695.00	700.00	27.90	-0.18
Homeowner	0.11	0.15	0.33	-0.10
Credit History Length	16.20	16.20	7.68	0.01
Inquiries (6 months)	0.54	0.59	0.82	-0.06
Open Accounts	11.70	10.80	5.79	0.16
Revolving Utilization	52.40	44.90	24.10	0.31

(b) Post-Policy (2018)

Variable	Eligible	Ineligible	StdDev	Cohen's d
Annual Income	80.60	83.00	94.50	-0.03
Debt-to-Income Ratio	18.60	16.60	8.74	0.23
FICO Score	700.00	703.00	29.00	-0.12
Homeowner	0.12	0.14	0.33	-0.05
Credit History Length	15.70	15.80	7.71	-0.01
Inquiries (6 months)	0.47	0.54	0.75	-0.09
Open Accounts	11.70	10.70	6.01	0.16
Revolving Utilization	48.80	41.30	24.20	0.31

Notes: This table compares borrower and loan characteristics between loans eligible for the direct-pay option (e.g., debt-refinancing or credit-card-repayment loans) and those ineligible (general-purpose or other consumer loans). Eligibility is determined by loan purpose categories defined prior to the policy introduction. Panel (a) reports the comparison for the pre-policy year 2017, before the direct-pay option was introduced; because neither group is treated in 2017, this comparison reflects the groups' pre-policy comparability and is the most relevant balance check for the parallel-trends assumption. Panel (b) repeats the comparison for the post-policy year 2018, and the eligible-versus-ineligible differences are similar in magnitude to those in 2017, indicating that the introduction of the direct-pay option did not materially reshape the composition of the eligible group. Across both years, eligible and ineligible borrowers are broadly similar across most observable dimensions, including income, credit score, and credit history, although eligible borrowers tend to have slightly higher debt burdens (DTI and revolving utilization). Together, these comparisons indicate that eligible and ineligible borrowers are observably similar enough for ineligible loans to gauge common platform time shocks in the DiD design; they do not assert that the two segments would have shared the same default rate in levels absent the policy.

Table A10: Borrower Characteristics Before and After the Policy Introduction

Variable	Year 2017	Year 2018	StdDev	Cohen's d
Annual Income	79.63	81.05	177.69	-0.01
Debt-to-Income Ratio	18.29	18.19	8.49	0.01
FICO Score	696.06	700.46	28.56	-0.15
Homeowner	0.12	0.12	0.33	-0.01
Credit History Length	16.23	15.72	7.70	0.07
Inquiries (6 months)	0.55	0.48	0.79	0.08
Open Accounts	11.53	11.52	5.90	0.00
Revolving Utilization	50.73	47.49	24.17	0.13

Notes: This table compares key borrower and loan characteristics for loans originated in 2017 (pre-policy) and 2018 (post-policy). Variables include loan amount, interest rate, annual income, debt-to-income (DTI) ratio, credit score (FICO), home ownership, credit-history length, recent credit inquiries, number of open credit accounts, and revolving credit utilization. Differences are generally small and statistically insignificant, indicating that the composition of borrowers did not change materially around the introduction of the direct-pay policy. These results suggest that the observed changes in default rates are unlikely to be driven by shifts in borrower quality or loan mix.

A.3 Robustness and Parallel Trends in DiD Analysis

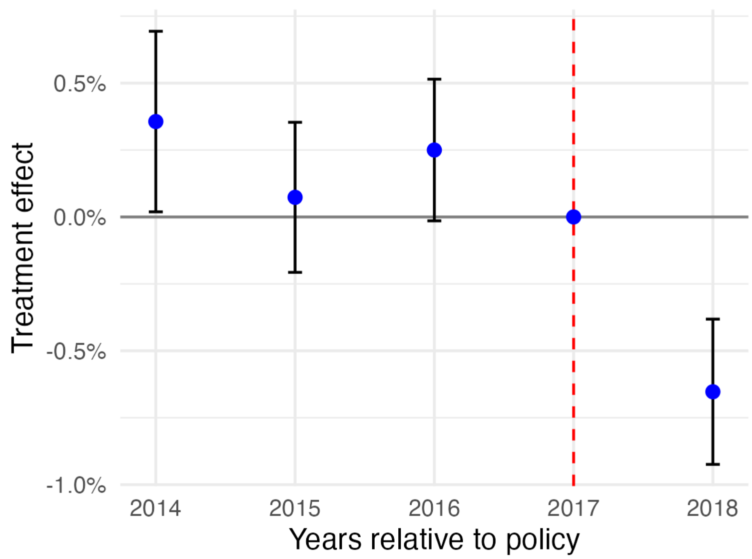
Table A11 reports robustness checks extending the pre-policy period to 2014–2017 and estimating both the baseline DiD and a time-varying treatment-effect specification, including versions that allow the 2018 treated effect to vary with the predicted discount. The estimated coefficients remain negative and statistically significant under alternative sample definitions. Figure A4 plots the dynamic treatment effects and shows no differential pre-trend before 2018 and a decline in defaults thereafter, consistent with using ineligible loans to net out common platform drift rather than as a behavioral counterfactual in levels.

Table A11: Robustness of the Direct-Pay Policy Effect: Extended Pre-Period and Dynamic Treatment Estimates

	One-year default									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post	0.011***	0.012***	0.012***			0.182***	0.203***	0.206***		
	(0.001)	(0.001)	(0.002)			(0.018)	(0.018)	(0.036)		
Treated	−0.008***	−0.014***	−0.014***	−0.014***	−0.014***	−0.147***	−0.263***	−0.261***	−0.271***	−0.269***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.010)	(0.010)	(0.015)	(0.018)	(0.018)
Post × Treated	−0.008***	−0.007***	−0.007***			−0.132***	−0.104***	−0.110***		
	(0.001)	(0.001)	(0.002)			(0.021)	(0.021)	(0.031)		
2014				−0.019***	−0.019***				−0.379***	−0.376***
				(0.002)	(0.002)				(0.042)	(0.043)
2015				−0.007***	−0.007***				−0.141***	−0.139***
				(0.001)	(0.001)				(0.024)	(0.024)
2016				−0.000	−0.000				−0.006	−0.006
				(0.001)	(0.001)				(0.023)	(0.023)
2018				0.008***	0.008***				0.130***	0.128***
				(0.002)	(0.002)				(0.036)	(0.036)
2014 × Treated				0.004*	0.004*				0.044	0.045
				(0.002)	(0.002)				(0.034)	(0.034)
2015 × Treated				0.001	0.001				0.007	0.007
				(0.001)	(0.001)				(0.021)	(0.021)
2016 × Treated				0.003	0.003				0.051*	0.051*
				(0.002)	(0.002)				(0.026)	(0.026)
2018 × Treated				−0.007***	−0.020***				−0.107***	−0.355***
				(0.002)	(0.002)				(0.031)	(0.030)
2018 × Treated × Discount					0.004***					0.066***
					(0.000)					(0.003)
Loan amount		0.001***	0.001***	0.001***	0.001***		0.025***	0.025***	0.025***	0.025***
		(0.000)	(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)	(0.001)
Term 3-year		−0.003***	−0.003***	−0.004***	−0.004***		−0.066***	−0.066***	−0.082***	−0.079***
		(0.000)	(0.001)	(0.001)	(0.001)		(0.008)	(0.014)	(0.013)	(0.013)
log(Annual income)		−0.013***	−0.013***	−0.014***	−0.014***		−0.277***	−0.279***	−0.299***	−0.298***
		(0.000)	(0.001)	(0.001)	(0.001)		(0.009)	(0.012)	(0.012)	(0.012)
Debt-to-income		0.001***	0.001***	0.001***	0.001***		0.019***	0.019***	0.019***	0.019***
		(0.000)	(0.000)	(0.000)	(0.000)		(0.000)	(0.001)	(0.001)	(0.001)
Credit score (FICO)		−0.000***	−0.000***	−0.000***	−0.000***		−0.007***	−0.007***	−0.007***	−0.007***
		(0.000)	(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)	(0.000)
Home ownership		0.002***	0.002***	0.002**	0.002**		0.040***	0.039***	0.032**	0.031**
		(0.001)	(0.001)	(0.001)	(0.001)		(0.011)	(0.011)	(0.012)	(0.011)
Credit history length (years)		−0.001***	−0.001***	−0.001***	−0.001***		−0.014***	−0.014***	−0.014***	−0.013***
		(0.000)	(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)	(0.001)
Credit inquiries (past 6 months)		0.013***	0.013***	0.013***	0.013***		0.220***	0.221***	0.233***	0.232***
		(0.000)	(0.000)	(0.000)	(0.000)		(0.004)	(0.007)	(0.005)	(0.005)
Open credit accounts		−0.001***	−0.001***	−0.001***	−0.001***		−0.012***	−0.012***	−0.011***	−0.012***
		(0.000)	(0.000)	(0.000)	(0.000)		(0.001)	(0.001)	(0.001)	(0.001)
Revolving credit utilization		−0.000***	−0.000***	−0.000***	−0.000***		−0.004***	−0.004***	−0.003***	−0.004***
		(0.000)	(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.059***	0.340***	0.338***	0.345***	0.339***	−2.771***	3.066***	3.022***	3.162***	3.063***
	(0.000)	(0.006)	(0.005)	(0.005)	(0.005)	(0.009)	(0.117)	(0.129)	(0.136)	(0.136)
Month fixed effects	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
R2	0.000	0.007	0.007	0.008	0.008					
Observations	1530704	1530703	1530703	1530703	1530703	1530704	1530703	1530703	1530703	1530703
Log Likelihood						−319391.485	−314231.172	−314169.218	−313704.797	−313574.079

Notes: This table reports robustness checks using an extended pre-policy sample (2014–2017) and an event-study specification that allows time-varying treatment effects around the 2018 policy introduction. The dependent variable equals one if the loan defaults within one year of origination. Columns (1)–(3) and (6)–(8) report baseline DiD estimates in LPM and logit form, respectively. Columns (4)–(5) and (9)–(10) report event-study versions with year-specific treatment effects, and Columns (5) and (10) additionally interact the 2018 treatment effect with the borrower-level direct-pay discount. All regressions include borrower- and loan-level controls (loan amount, term, log income, debt-to-income ratio, FICO, home ownership, credit history length, inquiries, open accounts, revolving utilization) and month fixed effects. Robust standard errors are clustered by origination month. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Figure A4: Time-Varying Treatment Effects and Parallel Trends



Notes: This figure plots the estimated coefficients and 95% confidence intervals from an event-study version of the difference-in-differences model, where each coefficient represents the relative change in one-year default rates for treated (eligible) loans compared with untreated loans in year t, using 2017 as the omitted baseline.

A.4 Survival Analysis of Loan Default

Table A12: Survival Analysis of Loan Default

	Hazard ratio							
	2018				2017 and 2018			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Direct-pay	-0.848^{***}	-0.759^{***}	-0.779^{***}	-0.327^{***}				
	(0.024)	(0.024)	(0.024)	(0.048)				
Direct-pay × Discount				-0.121^{***}				
				(0.012)				
Post cash					0.150^{***}	0.139^{***}	0.138^{***}	0.139^{***}
					(0.012)	(0.012)	(0.012)	(0.012)
Post direct-pay					-0.699^{***}	-0.625^{***}	-0.637^{***}	-0.179^{***}
					(0.024)	(0.024)	(0.024)	(0.047)
Post direct-pay × Discount								-0.122^{***}
								(0.012)
Loan amount		0.023^{***}	0.024^{***}	0.023^{***}		0.024^{***}	0.024^{***}	0.024^{***}
		(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)
Term 3-year		-0.109^{***}	-0.105^{***}	-0.121^{***}		-0.095^{***}	-0.091^{***}	-0.099^{***}
		(0.017)	(0.018)	(0.018)		(0.013)	(0.013)	(0.013)
log(Annual income)		-0.266^{***}	-0.270^{***}	-0.271^{***}		-0.279^{***}	-0.280^{***}	-0.280^{***}
		(0.019)	(0.019)	(0.019)		(0.014)	(0.014)	(0.014)
Debt-to-income		0.010^{***}	0.009^{***}	0.010^{***}		0.014^{***}	0.014^{***}	0.014^{***}
		(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)
Credit score (FICO)		-0.008^{***}	-0.008^{***}	-0.008^{***}		-0.008^{***}	-0.008^{***}	-0.008^{***}
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Home ownership		0.007	0.008	0.009		0.024	0.024	0.024
		(0.024)	(0.024)	(0.024)		(0.017)	(0.017)	(0.017)
Credit history length (years)		-0.011^{***}	-0.011^{***}	-0.011^{***}		-0.010^{***}	-0.010^{***}	-0.010^{***}
		(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)
Credit inquiries (past 6 months)		0.272^{***}	0.273^{***}	0.274^{***}		0.277^{***}	0.276^{***}	0.277^{***}
		(0.009)	(0.009)	(0.009)		(0.006)	(0.006)	(0.006)
Open credit accounts		-0.015^{***}	-0.015^{***}	-0.015^{***}		-0.015^{***}	-0.015^{***}	-0.014^{***}
		(0.002)	(0.002)	(0.002)		(0.001)	(0.001)	(0.001)
Revolving credit utilization		-0.006^{***}	-0.006^{***}	-0.006^{***}		-0.005^{***}	-0.005^{***}	-0.005^{***}
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Month fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Pseudo-R2	0.006	0.014	0.014	0.014	0.003	0.011	0.011	0.011
Observations	278046	278046	278046	278046	528819	528819	528819	528819

Notes: This table reports Cox proportional-hazard estimates of the effect of the direct-pay policy on the timing of loan default. The dependent variable is the time (in months) from origination to default or censoring at loan maturity. The hazard ratio below one indicates a lower risk of default. Columns (1)–(4) use the 2018 cohort and compare cash and direct-pay borrowers' default hazards through 2019, controlling for borrower and loan characteristics. Columns (5)–(8) pool the 2017 and 2018 cohorts to compare 2018 cash and direct-pay borrowers against the 2017 baseline. The key variable direct-pay equals one for loans disbursed directly to creditors; in the pooled regressions, Post Cash and Post Direct indicate 2018 cash and 2018 direct-pay loans, respectively, with 2017 cash loans as the omitted group. Columns (4) and (8) additionally include the interaction between direct-pay status and the borrower-level predicted discount. All specifications include controls for loan amount, term (3-year indicator), log annual income, debt-to-income ratio, FICO score, home ownership, credit-history length, credit inquiries in the past six months, number of open credit accounts, and revolving-credit utilization, and Columns (3)–(4) and (7)–(8) include month-of-origination fixed effects. Standard errors are clustered by origination month. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

A.5 Model Fit of Structural Estimation

Table A13: Model Fit of Targeted Moments in Structural Estimation

Moment	Empirical	Fitted
Default in 2017	5.14%	5.18%
Direct-Pay Choice 2018	25.9%	26.0%
Default (Cash) in 2018	5.81%	5.82%
Default (Direct-Pay) in 2018	2.61%	2.64%
Default (Overall) in 2018	4.98%	5.00%

Notes: This table compares the empirical moments from the data (column "Empirical") to the corresponding moments generated by the estimated structural model (column "Fitted") for loans with a term of 3 years. The structural parameters are estimated via the Generalized Method of Moments (GMM), which minimizes the distance between these two sets of moments. The four targeted moments are: (1) the 2017 pre-policy default rate, (2) the 2018 direct-pay adoption rate, (3) the 2018 default rate for cash borrowers, and (4) the 2018 default rate for direct-pay borrowers. The "Default in 2018" row reports the aggregate default rate, which is an outcome of the four targeted moments.