Credit Scars: Credit Reporting Relief for Defaulted Student Loan Borrowers*

Diego A. Briones†

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Abstract

Over one in five student loan borrowers eventually defaults, leaving a persistent black mark on their credit reports. These borrowers disproportionately attended low-return institutions and come from disadvantaged backgrounds, raising concerns that credit record penalties may be restricting economic mobility. I estimate the causal effect of removing student loan default records from credit reports by leveraging their automatic removal under the seven-year Fair Credit Reporting Act (FCRA) threshold, which I establish as the primary mechanism for student loan default record clearance. Using a nationally representative panel of quarterly administrative credit bureau records and a stacked difference-in-differences design, I find that record removal boosts credit scores by 15 points and spurs significant new borrowing across mortgage, auto, and credit card markets, primarily on the extensive margin. These results indicate that default records continued to function as barriers to credit long after the initial financial distress. The value that individuals place on increased access to credit more than seven years after the initial default event suggests significant private and social returns to programs that successfully rehabilitate defaults or to programs intended to prevent default.

Keywords: Student Debt, Higher Education, Default, Credit Access, Consumer Lending
JEL Codes: G51, I22, I23

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†University of Virginia, Department of Economics and UC-Irvine, Student Loan Law Initiative. dab5xq@virginia.edu

1 Introduction

Federal student loans provide liquidity for individuals to invest in postsecondary education and achieve greater economic opportunity. By eliminating traditional underwriting, these loans alleviate credit constraints for economically disadvantaged groups that the private market would otherwise underserve. Yet, more than one in five borrowers defaults, and by the end of 2019, over 11 million borrowers were in default on $288 billion in debt.

This negative outcome is concentrated in a specific, vulnerable population. Defaulters differ substantially from typical students—they disproportionately attend less-selective, low-return institutions, leave school without degrees, and come from low-income families and minority groups (Ahlman and Gonzalez, 2019; Looney and Yannelis, 2015; Scott-Clayton, 2018; Stratford, 2023). Upon default, a derogatory flag appears on their credit reports, where it remains visible to prospective employers, landlords, and creditors for up to seven years under the Fair Credit Reporting Act (FCRA). A concern is that this credit reporting penalty further restricts economic mobility for an already disadvantaged group.

This paper examines how negative information and its removal from credit records affects credit utilization for defaulted student borrowers. In principle, credit access allows individuals to smooth consumption over time and states of the world and facilitates access to productive assets. Lenders primarily allocate credit through measures of perceived creditworthiness such as credit scores, and so, negative credit events such as a default can severely restrict this source of liquidity. Moreover, student loans are an increasing share of borrowers' first interactions with the credit market (Bakker et al., 2025), so default can become a defining, and overwhelmingly negative, signal of their creditworthiness. On the other hand, if defaulters have no underlying demand for credit, then information disclosure will matter little for this channel of economic mobility.

To test whether default records impose binding constraints, I leverage the seven-year FCRA threshold for automatic removal of student loan default information, combined with a large, nationally representative panel of administrative credit bureau records. My main empirical finding is that borrowers significantly increase their total stock of debt across all major loan categories following default record removal. Using a stacked difference-in-differences (DiD) design comparing treated borrowers to not-yet-treated consumers, I estimate a sharp, 15-point increase in credit scores in the quarter when the default flag is removed. By the fourth quarter without a default record, treated borrowers increase their balances on mortgages by $1,801 (36%), auto loans by $404 (13%), and credit cards by $131 (25%). Extensive margin responses primarily drive these impacts: baseline borrowing levels for defaulters are strikingly low when compared to the general population of consumers and student loan borrowers. For example, only 27% of defaulters in the comparison group have an open credit card prior to flag removal, compared to 73% of all student borrowers.

Using credit inquiry data, I do not find strong evidence of differential changes in underlying demand between treated and comparison borrowers, suggesting that borrowing impacts are driven by higher likelihoods of credit approvals. Nor do I find evidence of systematic increases in delinquencies that would otherwise explain growing balances or borrowers taking on unmanageable debt within the first year of record removal. Treated and not-yet-treated borrowers exhibit similar borrowing trajectories prior to flag removal, and the main estimates are robust to alternative specifications, including a parametric event study approach used in related analyses of bankruptcy flag removals (e.g., Dobbie et al., 2017; Gross et al., 2020), and to allowing for deviations between 1.9 and 2 times the maximum violation of parallel trends observed in the pre-period (Rambachan and Roth, 2023).

A complementary analysis of student loan repayment cohorts provides crucial context for interpreting the magnitude of these borrowing responses. This analysis establishes two key facts. First, the initial default represents a severe and persistent shock: defaults occur early in the repayment cycle and are correlated with credit score drops exceeding 50 points. These borrowers have relatively thin credit files before defaulting, meaning the default record becomes a defining negative signal of creditworthiness. Second, I establish that the FCRA threshold is the relevant margin for removal, as default flags largely persist for the full seven years, indicating that borrowers rarely use preemptive options like rehabilitation.

Taken together, these results imply that the default flag itself serves as a key barrier to credit access. Furthermore, as treated borrowers appear to manage these debts within the first year of record removal, the flag becomes a nosier signal of creditworthiness, leading to potential misclassifications of borrowers who are, in fact, able to repay (Blattner and Nelson, 2021). This potential for inefficient lock-out from credit markets is perhaps most evident in the positive estimated treatment effect on mortgages—a market characterized by extensive underwriting that relies on income and asset information in addition to credit records.[1]

These findings provide direct evidence on a central claim in the student loan debt crisis: that education loans restrict struggling borrowers' capacity to participate fully in the economy through access to credit.[2] This paper demonstrates that default leaves a defining, seven-year imprint on credit reports of consumers who begin with very low participation in formal credit

[1]In this context, "lock-out" refers to situations where a prospective borrower is either explicitly denied credit (facing infinite prices) or is not offered a loan at a price that meets their willingness-to-pay.

[2]For example, in a December 1, 2020 Senate Banking Committee hearing, Federal Reserve Chairman Jerome Powell stated, "So, I think...over longer periods of time people who take on student debt...and if it doesn't work out...they drag that debt down through their economic lives, and it can get in the way of their credit history...their ability to own a home, and their whole economic life for many years..."(Warren, 2020)

markets. The persistence of default records and the large borrowing responses to their removal imply substantial unmet credit demand among these consumers. Given that defaulters are more likely to have faced weak labor market outcomes after leaving school, restrictions on credit for potentially productive investments—such as vehicles for employment—or for consumption smoothing during unemployment may magnify the costs of default and reduce the likelihood of repayment.

The large borrowing responses to FCRA-mandated record removal also raise questions about why so few borrowers use rehabilitation to remove these records sooner. More broadly, given well-documented frictions in the student loan repayment process (Herbst, 2023; Looney and Yannelis, 2015; Monarrez and Turner, 2024; Mueller and Yannelis, 2022), many borrowers could have avoided these derogatory credit flags by entering income-driven repayment plans and qualified for zero-dollar payments, underscoring the large potential returns to policies that prevent borrowers from defaulting in the first place. The relevance of these findings is particularly acute in the current policy environment: following nearly five years of federal payment moratorium, delinquency rates have surged back to pre-pandemic levels, meaning a new wave of borrowers now faces default.

These broad conclusions come with two important considerations. First, removing default information potentially reduces lenders' ability to price risk accurately which could generate spillover effects on other borrowers. My identification strategy limits the extent to which I can estimate causal impacts on debt management beyond one-year from the removal event. If borrowers systematically fall into delinquency on new debt, student loan defaults may have accurately signaled low underlying creditworthiness. Second, and related to this point, the FCRA threshold applies to all student loan defaulters regardless of underlying risk. The policy likely lifts credit access constraints for both high-risk consumers experiencing persistent negative shocks and student borrowers who have become lower-risk since the initial delinquency. The resulting welfare consequences are therefore sensitive to the policy's timing, as it interacts with the dynamic nature of borrower risk. While a seven-year threshold allows time for defaulters to transition to lower-risk types, a more swift removal—for example, immediately following default—would likely grant credit access to a consumer pool that remains predominantly high-risk.

These tradeoffs in information disclosure appear in a range of contexts, from 'ban the box' laws to regulations on credit reporting after bankruptcy and the suppression of medical debt information. In credit markets, information mitigates issues of adverse selection and moral hazard, leading to a more efficient allocation of capital. However, the use of negative historical information can penalize individuals for temporary shocks that lead to an inefficient, persistent exclusion from credit markets and limit economic mobility.[3]

I conclude with a preliminary analysis of the welfare implications of record removal using a framework that considers both the redistribution to defaulted borrowers and the potential efficiency costs from removing risk information (Jansen et al., 2025). Applying this approach to auto loan borrowing, I find that the social deadweight loss from record removal is relatively modest: approximately 5.74 cents of social surplus are lost for each dollar transferred to defaulters. This estimate suggests that even under the conservative assumption that seven-year-old flags remain informative about current risk and that data disclosure always improves social welfare, the efficiency cost per dollar redistributed remains small.

Prior research on student loan default has examined both its predictors—including borrower characteristics, institutional factors, and policy interventions like income-driven repayment (e.g., Herbst, 2023; Looney and Yannelis, 2015; Mueller and Yannelis, 2022)—and the broader pattern of suboptimal financial decisions among borrowers, such as low take-up of forgiveness programs and income-driven plans (e.g., Briones et al., 2024; Monarrez and Turner, 2024). But little evidence exists on the long-term consequences of student loan default, even as there is robust evidence on the effects of other forms of financial distress, including bankruptcy, on consumer behavior (Dobbie et al., 2017, 2020; Gross et al., 2020; Herkenhoff et al., 2021; Jansen et al., 2025; Musto, 2004). This paper demonstrates that the removal of default indicators more than seven years after initial financial distress meaningfully affects credit usage for student loan borrowers. Crucially, student loan defaulters differ from bankruptcy filers in ways that explain both the magnitude and persistence of these effects: defaulters enter default with minimal credit market experience and lower baseline credit scores, leading to a pronounced lock-out effect that persists until automatic FCRA removal. This distinction—limited initial credit usage combined with severe informational penalties—helps explain why the borrowing responses I estimate are substantially larger than those found in the bankruptcy literature and why increased credit access remains valuable more than seven years after the initial default event.

2 Background and Institutional Context

Federal student loans are unique in the household debt market. The rules dictating their supply, repayment, and credit reporting practices mean that the consequences of non-payment and the affected population are likely distinct from other financially distressed groups. Defaulting student loan borrowers are also atypical postsecondary students. Understanding

[3] See Blattner et al. (2022) for a discussion and analysis on credit registry design.

their characteristics provides important context in considering the implications of credit reporting relief for this group. This section outlines the institutional features of federal student loan repayment and default, describes the profile of borrowers who default, and details the mechanisms through which borrowers can exit default status and remove derogatory marks from their credit reports.

2.1 Student Loan Repayment

Student loans were the largest source of non-mortgage liabilities for U.S. households between 2010 and 2023 and the vast majority of these debt obligations are disbursed by the federal government.[4] The public provision of these loans serves to offer liquidity for constrained students that the private market would otherwise underserve, allowing them to make potentially high-return investments. Since postsecondary investments cannot be collateralized, in contrast to tangible, asset-linked loan markets, issues of moral hazard and adverse selection are instead at least partially addressed through wage garnishment and tax refund withholding in the case of non-payment.[5] Further, federal student loans, in the main, are not dischargeable through bankruptcy.[6]

Federal higher education loans do not have a traditional underwriting process. Students must complete the Free Application for Federal Student Aid which the government uses to assess student need and allows borrowers to access funds. Conditional on level of enrollment and loan type, borrowers face the same fixed interest rates and borrowing limits.[7]

Borrowers enter repayment once they graduate, drop below half-time enrollment, or leave school and typically have a six-month grace period before being required to make payments. Loans entering repayment are automatically enrolled in the "standard" 10-year, fixed monthly payment plan, but borrowers have alternative payment options through income-based-repayment and temporary cessation of payments through forbearance and deferrals.

[4]See the Quarterly Report on Household Debt and Credit provided by the Federal Reserve Bank of New York. Approximately 7.5% of the student debt market is private loans. This comes from a comparison of the total balances of loans owned and securitized against the aggregate federal loan portfolio as of June 30, 2021 (Board of Governors of the Federal Reserve System, 2024).

[5]See Hoxby (2015).

[6]Discharge of student debt through bankruptcy has been exceedingly rare with an estimated success rate for those who do file of approximately 0.1 percent (Iuliano, 2020). In November 2022, federal student loan rules for discharge through bankruptcy were changed in order to make the process less burdensome (Department of Justice, 2022), though the event remains rare compared to the number borrowers in default (Bernard, 2023).

[7]The William D. Ford Federal Direct Loan (Direct Loan) program is the primary source of federal postsecondary debt. The three main Direct Loan types are subsidized, unsubsidized, and PLUS. Subsidized loans have an interest subsidy benefit and are only available to undergraduates with demonstrated financial need. Eligibility for unsubsidized and PLUS loans are unconditional on need and are available to all enrollment levels. Loan limits and interest rates vary across loan types and level of enrollment. These rules are set by Congress. For a comprehensive review of these program details see (Hegji, 2023).

A student borrower becomes delinquent on loans the first day a payment is missed. Loan servicers – government contractors that handle billing and repayment with borrowers – report delinquent loans to the three major national credit bureaus after the loan has been delinquent for 90 days. This is more generous than other liability accounts where missed payments are typically reported after 30 days (Akin, 2020). Most federal student loans are considered in default after 270 consecutive days of missed payments and then are transferred to a default servicer after about a year. Again, federal loans have a relatively higher time threshold before being considered seriously delinquent compared to other debt categories such as auto loans where lenders might pursue repossession as soon as 60 or 30 days of missed payments.

Any reported delinquency can yield significant restrictions in a borrower's access to credit, with delinquencies of 90 days or more appearing to have persistent, negative relationships with borrowers' credit, homeownership status, and income (De Giorgi and Naguib, 2024). Entering collections or default can more severely hamper a borrower's credit access. The exact impact of default on an individual's credit score, the primary measure of a borrower's creditworthiness, depends on a number of known factors, but the precise algorithm producing these estimates is proprietary information (Akin, 2023). In addition, default on federal student loans results in borrowers losing access to federal student aid, repayment plans, and debt forgiveness options. The federal government can also withhold tax refunds and federal payments, and garnish up to 15 percent of your disposable wages (Federal Student Aid, n.d.a,n).

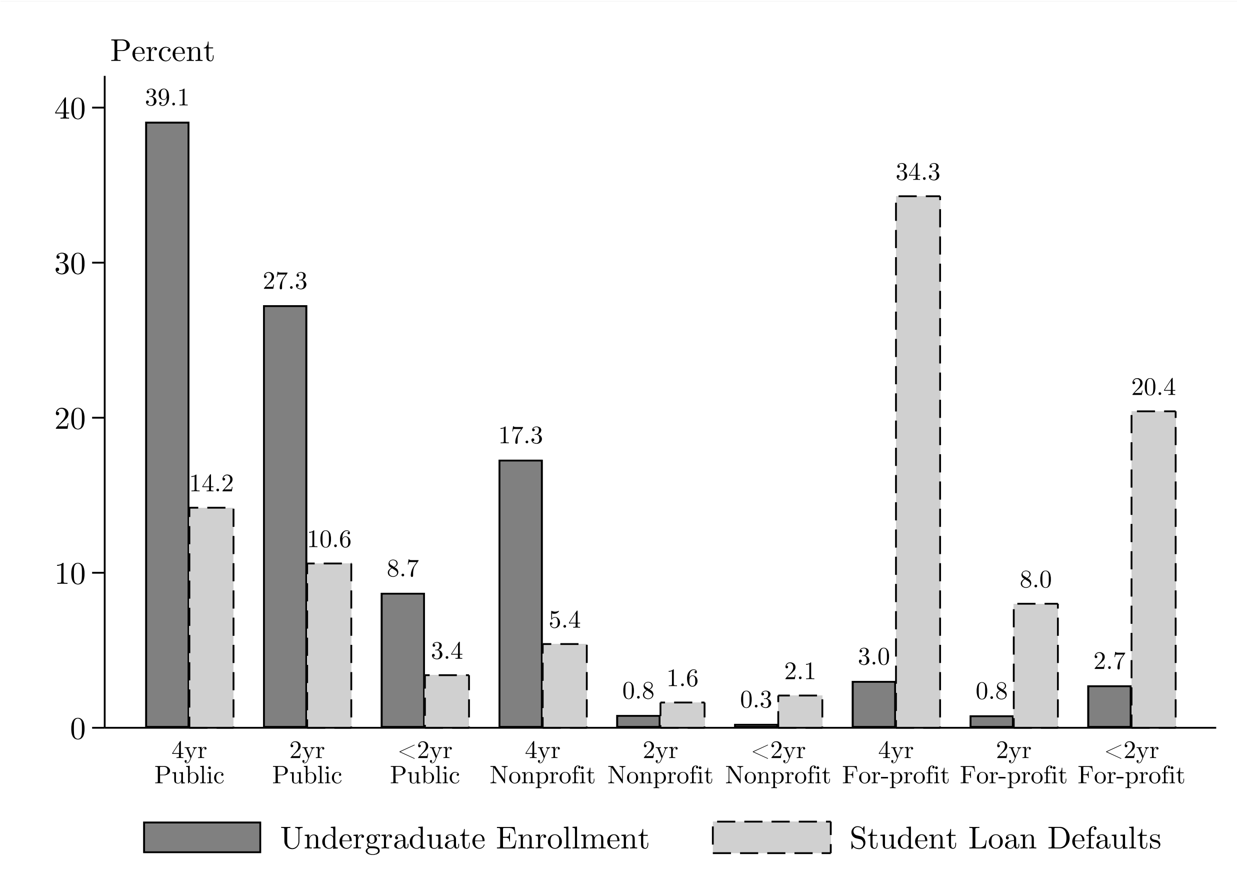
Delinquency and default were prominent features of the student debt market prior to the federal pause on loan payments and collections in March 2020. Between 2013 and 2020, student debt had the highest 90-plus-day delinquency rate across all other household liabilities (Figure A1) and over 20 percent of debt recipients were in default by the end of 2020 (see Figure A2). Following the end of the federal student loan payment moratorium in October 2023, delinquency rates have quickly returned near pre-pandemic levels, ensuring that student default and its consequences for credit access remain a significant policy challenge.

2.2 Who Defaults?

Student loan default is not evenly distributed across the diverse landscape of U.S. postsecondary education; rather, the problem is acute in specific institutional sectors. Data from the Department of Education's 2018-2019 College Scorecard reveals a clear profile of institutions with the highest default rates: they enroll a disproportionately high share of low-income students, are substantially less selective, and exhibit poor student completion outcomes.

The for-profit sector accounts for a vastly disproportionate share of student loan defaulters. As shown in Figure 1, for-profit institutions enrolled just 6.5% of undergraduates but were responsible for 62.7% of all defaulters. Conversely, public and nonprofit institutions, which enroll the majority of students, represent a comparatively small share of those who default. This finding is consistent with a large body of research showing that for-profit institutions—a diverse sector including large online universities and smaller vocational schools—are associated with poor subsequent employment and earnings outcomes for their students.[8]

Figure 1. Distribution of Undergraduates and Defaulters by Institutional Sector, (College Scorecard, 2018-2019)



Notes: Figure shows the share of undergraduates and student loan defaulters within each higher education sector. The first three pair of bars are public institutions, followed by nonprofits, and then for-profit institutions. The small share of institutions that are graduate-only serving or have an "unknown" sector are excluded. The share of defaulters is estimated using the institution-level 3-year cohort default rate multiplied by the number of students in the cohort to calculate the total number of defaulters. Data come from the 2018-2019 College Scorecard.

Figure 2 uses binned scatter plots to illustrate the relationship between institutional characteristics and three-year cohort default rates. Default rates fall as institutional selectivity (proxied by average SAT scores, panel a) and student completion rates (panel b) rise.

[8]See Cellini (2021) for a review.

Conversely, default rates are positively correlated with the percentage of undergraduates receiving Pell Grants (panel c) - need-based aid where 92% of recipients had total incomes at or below $60,000 in 2021-2022 (Dortch, 2024) - indicating that the student bodies at high-default institutions have fewer financial resources to begin with.

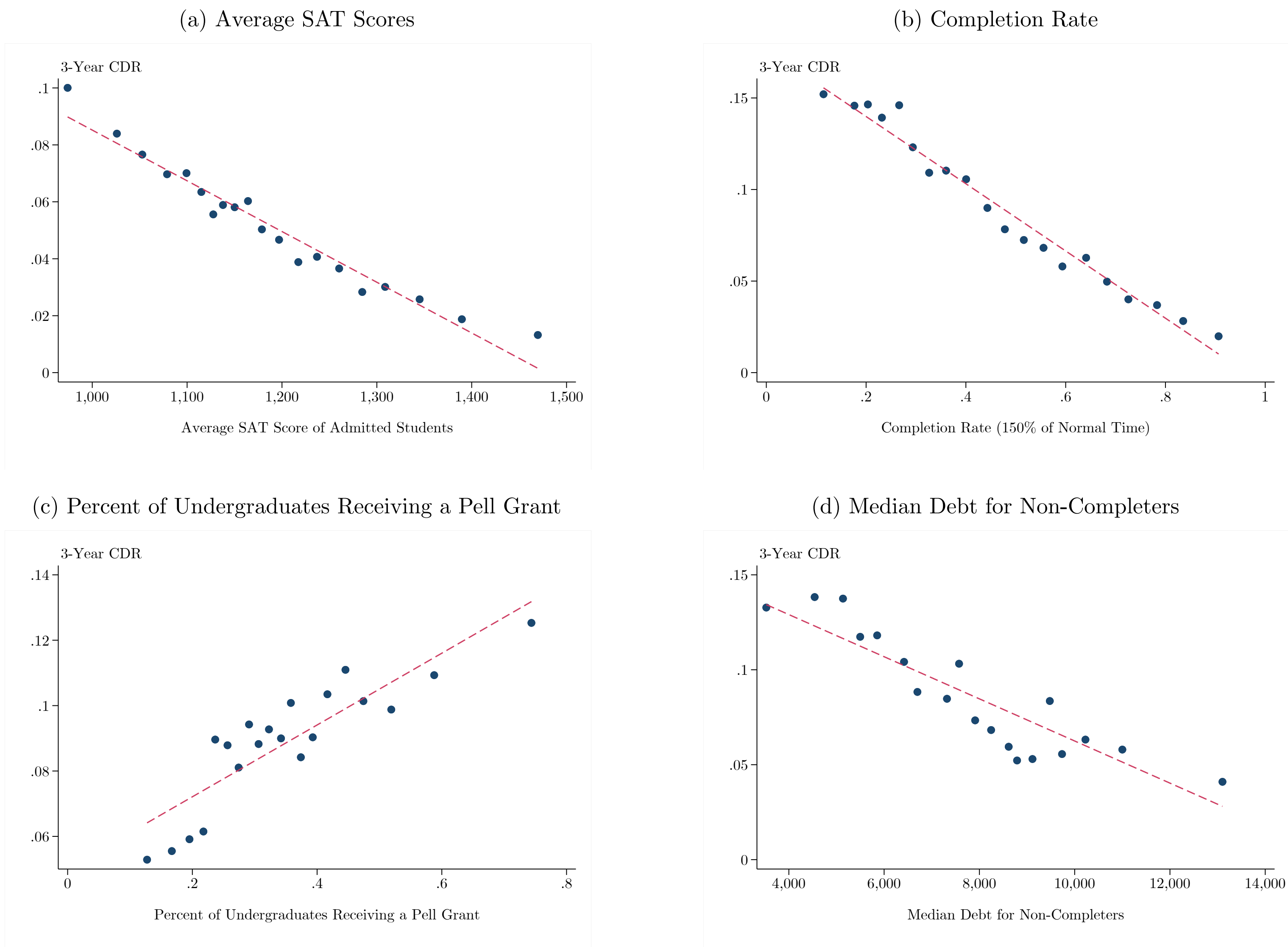
A central finding is that the institutions at the heart of the default crisis are those where students are least likely to graduate, and so even modest loan balances become unmanageable. High levels of student debt are not the primary driver of default. In fact, institutions with higher median student debt tend to have lower default rates (panel d), as these larger balances are often associated with graduate and professional degrees that lead to higher earnings.[9] A core issue lies with borrowers who have smaller balances from less-selective institutions that fail to provide the necessary economic mobility for repayment.

These characteristics are consistent with findings from an extensive set of research seeking to understand what borrower characteristics and market conditions are associated with default. Knapp and Seaks (1992) and Wilms et al. (1987) represent early examples of this research, using borrower samples from Pennsylvania colleges and California two-year institutions, respectively, to predict default based on borrower and institutional characteristics. Higher probabilities of default among lower-income, Black, and non-graduating borrowers found in these studies continue to be present in the student loan population today. Looney and Yannelis (2015) drew further attention to borrower disparities in default, and highlighted the rise in enrollment across low-performing institutions that explains a large share of the increase in default rates between 2000 and 2011. A second strand evaluates causal factors that affect default rates, for example income-driven-repayment (Herbst, 2023; Mueller and Yannelis, 2022), payment burdens (Monarrez and Turner, 2024), and debt balances (Barr et al., 2021; Black et al., 2023).[10] Much of the prior work on student loan default is motivated by the size of the delinquent population and its potentially severe consequences for borrowers, yet surprisingly little empirical evidence exists on these costs.

[9] Note that panel (d) plots the median debt balances for non-completers as defaulters are more likely to be a part of this group, but this relationship between default and debt is consistent whether one uses median debt of all borrowers or just Pell Grant recipients.

[10] Other relevant work within these two areas of focus include Armona et al. (2022); Blagg (2018); Cox et al. (2020); Dynarski (1994); Flint (1997); Gross et al. (2010); Kuan et al. (2025); Lochner et al. (2021); Looney and Yannelis (2019, 2022); Mangrum (2022); Mueller and Yannelis (2019); Pinto and Steinbaum (2023); Scott-Clayton (2018).

Figure 2. Cohort Default Rates and Institutional Characteristics (College Scorecard, 2018-2019)



Notes: Each panel plots the relationship between institution-level three-year cohort default rate and a selected characteristic in a binned scatter plot. Panels are formed by dividing each x-axis variable of interest into twenty equal-sized groups and then plotting the mean cohort default rate within each bin against the mean value of the characteristic within each bin. The dashed line shows the best linear fit estimated on the underlying institution-level data. Plots are weighted by the number of undergraduates at each institution. Data come from the 2018-2019 College Scorecard. Note that some institutions can have missing characteristic data due to privacy suppression for small cell sizes or the characteristic is not applicable to the type of institution. For example, some institutions such as trade schools do not require SAT scores. Therefore, the institution count can vary across panels.

2.3 Exiting Default and Credit Reporting Benefits

Per the Fair Credit Reporting Act (FCRA), most types of derogatory information on consumer credit reports is removed after seven years. Federal student loan borrowers have several options to exit default status: rehabilitation, consolidation, or full payoff. However, only rehabilitation clears the default record from their report before the seven-year threshold.

In rehabilitation, borrowers make nine consecutive payments, after which the default record is removed from their credit report and their loan returns to good-standing, regaining eligibility for forgiveness and income-driven repayment plans.There are no readily available administrative estimates on the take-up of rehabilitation, though it appears to be a minority, and for those who do successfully rehabilitate, as many as third will re-default again within two years.[11] Borrowers can only use rehabilitation once, so upon re-default, their only recourse for credit reporting relief is the seven-year FCRA threshold.

Consolidation requires that borrowers have an eligible student loan that is in good-standing, in addition to their defaulted loan(s). Borrowers can consolidate these loans together only if they agree to an income-driven repayment plan upon consolidating or make three voluntary monthly payments on their defaulted loan prior to consolidation. A borrower who re-defaults on a consolidated loan can only use consolidation again if they have another loan in good-standing. Finally, borrowers can fully repay their debt through involuntary or voluntary payments. Involuntary payments include wage garnishment and/or seizure of federal income tax refunds.

Borrowers are not forced to pursue rehabilitation or consolidation, and it is at the discretion of the Department of Education when and how they pursue collection efforts, if it does so at all. Even after a successful full repayment or consolidation, borrowers' credit reports will still have the derogatory marks until they age off.

3 Data and Evidence of Credit Constraints

This section describes my data and provides descriptive evidence that default records impose meaningful restrictions on credit access. The first part outlines the data source and sample construction, with additional details in Appendix Section B. I then examine the entry and evolution of student loan repayment over time to document two key facts: first, that

[11] See Consumer Financial Protection Bureau (2016). At the time when the report was released, over eight million borrowers were in default and 650,000 had rehabilitated in the past year by making $5 monthly payments. Delisle et al. (2018) study a subsample of borrowers who default from a representative sample of students who began postsecondary education in 2003-04. Fifty-two percent of their default sample exits default after three years and 39% of those who exited used rehabilitation. As the authors note, however, their sample was not drawn to be representative of defaulted borrowers and there were large increases in the default rate in latter cohorts.

default is correlated with large, persistent drops in credit scores and relatively low credit market participation; and second, that the seven-year FCRA threshold—rather than borrower-initiated rehabilitation—is the primary mechanism through which default records are removed.

3.1 Data Source and Sample Construction

This study draws on longitudinal, administrative records from the University of California Consumer Credit Panel (UC-CCP). I utilize a representative 2% random sample of all individuals in the U.S. with credit files, covering approximately six million consumers quarterly from 2004 to 2024. The panel is dynamic, with about 5% of the sample being refreshed each quarter with new consumers.

The data provide a comprehensive view of credit health, and include basic information about borrowers such as their credit scores, birth dates, and geography. Detailed loan-level, or "tradeline," information allow me to observe the open date, scheduled payments, balances, and delinquency status on all debts reported to the credit bureau. Crucially, I observe student loans when they are in default and can trace the repayment history leading to default and how long that default record persists on a credit report. Anonymized identifiers allow for tracking both individuals and tradelines over time. Due to a 2012 change in how the credit bureau codes defaulted debt, the primary analysis is restricted to the 2012-2024 period to ensure consistency.

My main outcomes of interest are credit score and utilization in the three primary non-student loan household debt types: mortgages, auto loans, and credit cards. Combined, these debts make-up about 86% of all consumer debt in the U.S (Federal Reserve Bank of New York, 2025). I aggregate tradeline data to the borrower level such that a consumer's credit card balance, for example, is the sum of all individual, open credit card balances observed in that quarter.

Additional sample restrictions follow recommendations from Gibbs et al. (2025), where these data, along with similar credit panel data, are described in more detail. Throughout my analysis, I drop deceased individuals and those with missing birth year information.

3.2 Credit Access and Student Loan Default in the Credit Bureau Data

This section motivates the paper's central analysis. Before estimating the causal impact of removing default records via FCRA, I must first establish whether this removal represents a meaningful economic event. The answer hinges on two empirical questions.

First, to what extent does student loan default restrict credit supply? If defaulters experience modest credit score declines and recover quickly, then seven-year record removal would likely have minimal effects on borrowing. Conversely, if default causes large, persistent drops in credit scores and access to credit, the default record itself may act as persistent barrier to credit long after the initial financial distress.

Second, is the seven-year FCRA threshold the primary mechanism through which default records are cleared? If most borrowers successfully use rehabilitation to remove default records preemptively, then FCRA-mandated removal would affect only a small, selected subset of defaulters, limiting the generalizability and policy relevance of my estimates.

Answering these questions is essential for interpreting the welfare implications of my main results. As Bakker et al. (2025) document, student loans are increasingly borrowers' first interaction with formal credit markets. For consumers with thin credit files, default can become an overwhelmingly negative signal of creditworthiness. If the distress leading to default is temporary but the penalty is persistent, borrowers may face inefficient long-term exclusion from credit markets even as their underlying risk profile improves (Blattner et al., 2022). Establishing the severity and persistence of the initial default shock, along with the prevalence of FCRA as the primary removal mechanism, is therefore critical for understanding whether record removal can meaningfully expand credit access for this population.

To address these questions, I document the repayment patterns, credit score dynamics, and credit market participation for student loan borrowers. For the moment, I focus on borrowers who entered repayment in 2012, as this is earliest cohort I can track consistently over a sufficiently long horizon to observe both the initial impact of default and the timing of record removals.

3.2.1 Repayment

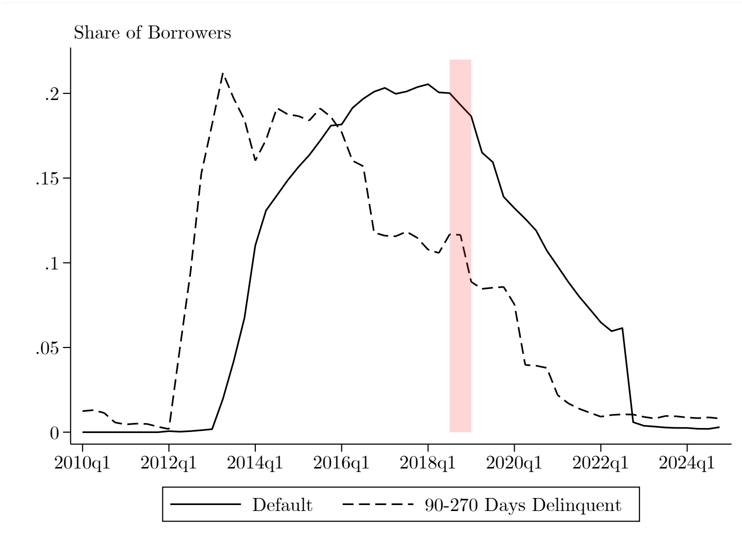
I classify borrowers as entering repayment in 2012 if they exit a deferment period in that year. I then track the status of their student loans—in school, in repayment, delinquent, or in default—at the borrower-quarter level.[12] Figure 3 plots the repayment trajectories for this cohort, revealing three facts. First, financial distress materializes quickly; the share of borrowers with a delinquency rises to over 15% within the first year of repayment (panel a). Second, a substantial fraction of the cohort - peaking at over 20% - ultimately has a default record on their credit report. Third, the decline in default records is not gradual but begins sharply around late 2018, seven years after the initial wave of delinquencies (indicated

[12]There are no codes in the credit bureau data that specifically denote when a borrower is in school or entered repayment, but these can be readily approximated using balance and payment histories, as well account status and condition codes. My approach is similar to Blagg (2018), though I can observe individual tradelines whereas Blagg (2018) observes consumer-level aggregates. Borrowers can have many student loan tradelines (e.g., one loan disbursed in the fall semester and one in the spring semester), so at the consumer-level, borrowers could be in different statuses during the same quarter (e.g., in deferment on one loan, but delinquent on another).

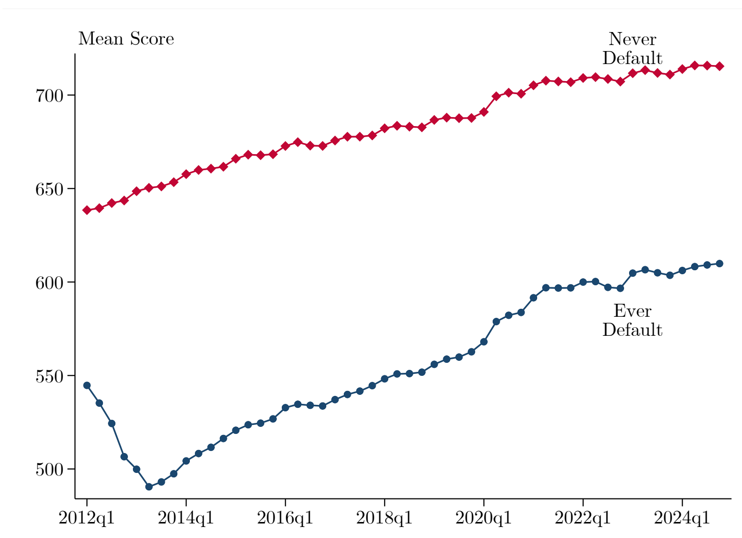
by shaded region in panel a). This timeline suggests that the primary removal mechanism is FCRA, rather than take-up of rehabilitation. The ongoing decline past March 2020 was partially influenced by the pandemic payment moratorium, which cured existing delinquencies preventing individuals from entering default but did not remove default records.

Figure 3. Student Borrower Repayment Trends and Credit Scores

(a) 2012 Cohort: Trends in Delinquency



(b) 2012 Cohort: Credit Score



Notes: The sample is student borrowers who enter repayment on a student loan in 2012 identified in the University of California Consumer Credit Panel (UC-CCP). In panels (a) and (b), borrowers can have loans in multiple statuses at point in time (e.g., one loan in delinquency and one loan in default). The shaded region corresponds to the initial decline in the share of borrowers with a default record in the credit data.

This distinction between a default record on a credit report and the underlying loan status is crucial. The decline shown in the credit data (panel a) is primarily a reporting feature. In subsequent analysis, I find that 87% of these loans still have past-due balances when the record is removed from the credit file. As Figure A2 shows, the share of borrowers officially in default (per Department of Education data) features no stark changes over this period consistent with information disclosure rules driving the trends in the credit reporting data, not loans being cured.[13]

3.2.2 Evidence of Credit Constraints

Delinquencies and default appear to have severe consequences for access to credit. As shown in panel (b) of Figure 3, an initial 50-point drop causes the average score gap relative to non-defaulters to widen from 94 to 160 points by mid-2013. The recovery is slow and incomplete; it takes until December 2017 for the average defaulter's score just to return to its pre-delinquency level.

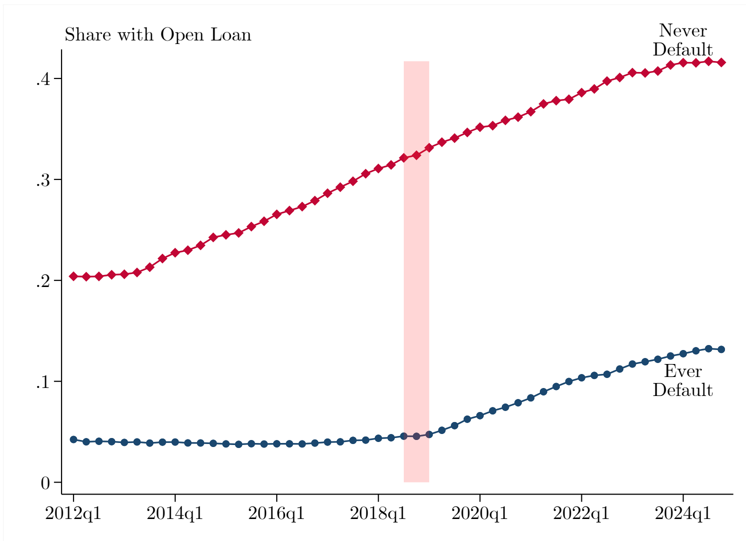
Figure 4 provides suggestive evidence that default leads to credit market exclusion. Throughout the repayment period, defaulters are far less likely than their non-defaulting peers to hold a mortgage, auto loan, or credit card. The gaps are wide and persist; by 2018, less than 30% of defaulters have a credit card relative to 79% of non-defaulters. However, corresponding with the drop in default records (shaded region), the rate of borrowing on the extensive margin begins to accelerate across all debt categories. While these trends could be confounded by life-cycle effects or other unobservables, the timing provides suggestive evidence that the default record itself acts as a binding constraint on credit access.

This apparent credit constraint is particularly concerning given the characteristics of the borrowers who default. As established previously, these are not typical student borrowers. They disproportionately come from low-income backgrounds, attended less-selective and for-profit institutions, and often left without a degree that provides economic mobility. These borrowers often hold modest loan balances, yet the penalty for non-repayment—exclusion from the formal credit system—is severe.

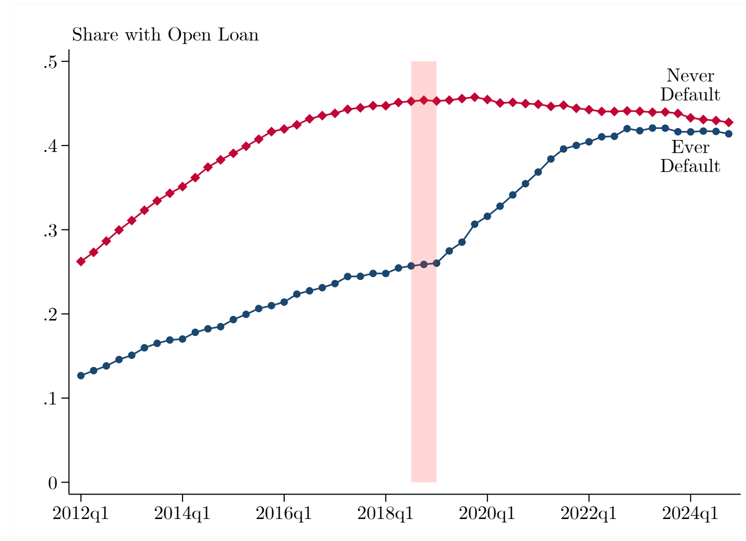
[13] As shown in panel (a), the share with a default record drops to near zero in December 2024. This corresponds to a temporary program effectively allowing borrowers expedited rehabilitation, though for this cohort, any credit reporting benefits from the program appear minimal with only 6% having a default record by this time. Again, the steep drop is a credit reporting benefit, there is no corresponding drop in the default share reported by ED (Figure A2). Gibbs (2023) analyzes the early implementation of the program and notes that, by August 2022, a significant majority of the borrowers in default no longer had a record of the event on their credit report.

Figure 4. Share of Borrowers with an Open Loan, 2012 Repayment Cohort

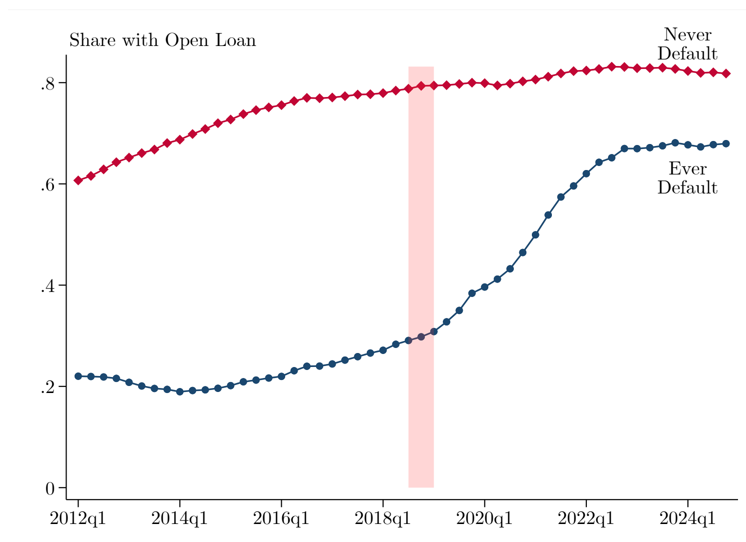
(a) Mortgage



(b) Auto Loan



(c) Credit Card



Notes: Vertical axis is the share of borrowers in the 2012 cohort with an open loan. Each panel illustrates trends on the extensive margin of borrowing in a loan category. Ever default are those borrowers who have a defaulted student loan at any point in the panel. The shaded region corresponds to the initial decline in the share of borrowers with a default record in the credit data.

This lock-out can magnify the initial negative labor market impacts of a poor educational investment. For a population with few family resources to draw upon, the inability to access credit for a potentially productive investment like a vehicle, or to smooth consumption during unemployment, can create a durable barrier to economic recovery. This raises the paper's core research question: What is the causal effect of removing a student loan default record on a borrower's use of credit? A finding of no significant borrowing response would be consistent with two distinct possibilities: either the record is a redundant signal of persistent high risk that lenders identify through other means, or these borrowers have no underlying demand for new credit. In contrast, a positive borrowing response—as suggested by the descriptive patterns—would provide strong evidence that the record itself functions as a binding constraint, and its removal unlocks unmet credit demand.

4 Empirical Strategy

The descriptive evidence from the 2012 cohort indicates that FCRA, not borrower-initiated rehabilitation, is the primary mechanism for default record removal. Estimating the causal effect of these removals is challenging, as defaulters are observably different from typical student borrowers, and simple pre-post comparisons could be biased by confounding life-cycle trends. Accordingly, my empirical approach compares defaulted borrowers receiving a FCRA record removal to those who will have their records removed in the near future, an approach similar in research studying the impacts of bankruptcy flag removals (e.g., Dobbie et al., 2020; Gross et al., 2020; Jansen et al., 2025; Musto, 2004)

I implement these comparisons as follows. I identify all student borrowers with defaulted student loans between 2012 and 2022, and measure the age of the defaulted tradeline using a combination of the number of quarters the tradeline is present in the data and the number of observed months of payment history. Only borrowers where the age of the tradeline prior to removal is consistent with FCRA are considered in the empirical analysis, as early removals potentially indicate the use of rehabilitation. I find that over 91% of borrowers have a default removal event consistent with FCRA. Later, in the analysis, I confirm that trends in listed student loan balances on credit records are consistent with FCRA age-off, lending validity to my sampling approach. I provide further details on identifying default record removals and distinguishing tradelines that age-off due to FCRA in Appendix Section B.

Borrowers with removal events in December 2022 and beyond are excluded to avoid potential confounding bias from an accelerated rehabilitation program implemented at that time. Finally, I limit the analysis to borrowers with records that were still actively in default before they age-off, approximately 87% of defaulted tradelines. Similar to the case of rehabilitation, this restriction is to limit potential bias from borrowers who have paid off the debt or consolidated before the record is removed.[14] In cases where borrowers have multiple removal events, their first removal within the sample period is defined as the quarter of treatment.

Summary statistics are presented in Table 1, with all characteristics measured in the quarter prior to record removal. For comparison with the default sample (column 1), the table includes statistics for approximately 10% random samples of all student borrowers (column 2) and all consumers (column 3).[15] The characteristics of the default sample highlight their lower credit access and usage. Their average credit score is approximately 560, more than 100 points below the average student borrower and over 120 points below the average consumer. This low creditworthiness is reflected in their limited participation in formal credit markets. Defaulters have lower balances and scheduled payments across all debt categories driven by their lower likelihood of holding an auto loan, mortgage, or credit card. Student borrowers and consumers more generally, have greater capacities to smooth consumption using unsecured credit as only 31% of defaulters have an open credit card. The average credit limit for non-defaulters is more than nine times the average limit for defaulters.

[14] Borrowers who exited student loan default by consolidating or through full repayment (either voluntarily and/or through collections) will still have the record of default on their credit report until the seven-year threshold. Thus, it is plausible that they will also benefit from a credit reporting standpoint due to FCRA, but they are likely different borrowers than the majority of defaulters.

[15] Given the variation in the treatment dates of my default sample, I select the random student borrower and all consumer groups through stratified sampling such that the distributions of the groups match the distribution of the default group in time. Comparison of credit score distributions across these three samples is illustrated in Figure A3.

Table 1. Summary Statistics

	(1) Default Sample			(2) Student Loan Borrowers			(3) All Borrowers		
	Mean	SD	Median	Mean	SD	Median	Mean	SD	Median
Age	40.10	10.45	38	38.96	13.76	36	49.91	18.45	49
Credit Score	559.80	56.56	554	664.75	108.46	673	683.35	105.54	691
Default on Student Loan	1.00	0.00	1	0.10	0.30	0	0.02	0.14	0
Auto Loan Balance	3453.23	8524.74	0	7270.55	13361.40	0	5119.43	12100.11	0
Auto Loan Scheduled Payment	97.55	216.42	0	187.07	311.48	0	133.74	285.43	0
Any Open Auto Loan	0.22	0.41	0	0.38	0.49	0	0.26	0.44	0
Mortgage Balance	4948.49	31298.96	0	61929.81	143088.46	0	48673.21	139919.92	0
Mortgage Scheduled Payment	45.22	473.72	0	476.22	1040.44	0	386.72	1113.26	0
Any Open Mortgage	0.04	0.19	0	0.29	0.45	0	0.24	0.43	0
Credit Card Balance	623.86	2290.44	0	3701.10	7839.50	497	2713.65	7029.46	67
Credit Card Scheduled Payment	27.05	73.70	0	125.46	209.00	50	95.85	186.90	27
Any Open Credit Card	0.31	0.46	0	0.73	0.44	1	0.64	0.48	1
Credit Card Limit	1399.41	4762.04	0	13468.68	20416.05	4600	12928.94	20692.43	3000
Number of Borrowers	48,314			117,604			549,175		

Notes: Sample in panel (1) are student loan borrowers whose default record is removed between 2013q1 and 2021q3. These represent the sample of treated borrowers in my analytical sample. Summary statistics are drawn from the quarter before record removal. Panels (2) and (3) are approximately 10% random samples of all student loan borrowers and all borrowers, respectively, in the credit panel. Random samples are drawn to match the calendar quarter distribution of the default sample.

4.1 Difference-in-Differences

I estimate impacts of record removals in a stacked difference-in-differences research design (e.g., Butters et al., 2022; Cengiz et al., 2019; Deshpande and Li, 2019) using specifications of the following form:

$$Y_{iqae} = \sum_{\substack{h=-4...3 \\ h\neq -1}} \Big[\delta_e RecDrop_{iqa} \times 1[e=h]\Big] + \lambda_{qa} + \gamma_{ae} + \epsilon_{iqae}. \tag{1}$$

Notation follows Wing et al. (2024). Y_{iqae} is a credit outcome (e.g., credit score) for consumer i in treatment cohort q, sub-experiment a, and event time e. The λ_{qa} are cohort-by-sub-experiment fixed effects, and γ_{ae} are sub-experiment-by-event-time fixed effects.

The stacked model takes borrower, i, receiving treatment in calendar quarter q where event time $e = 0$, and groups them with borrowers receiving their treatment between a year and 1.75 years in the future. These groupings of treated and comparison units represent separate "sub-experiments" denoted by a. For example, borrowers who have their record removal in $s = 2014q1$ are grouped with individuals where $q \in [2015q1, 2015q4]$, and this set of treated and control observations represent the sample for sub-experiment $a = 2014q1$. I use $e \in [-4, 3]$ for my main specification and require calendar-time balance across the event window within a. My control restriction of $q_{control} \in [q_{treated} + 4, q_{treated} + 7]$ means I avoid comparisons between newly treated and previously treated borrowers (Goodman-Bacon, 2021; Roth et al., 2023), while balance within the event window ensures estimates of the parameters of interest are not biased by compositional changes across e (Wing et al., 2024). The restriction on how far in the future control borrowers can be drawn from is to limit treatment and control borrowers being at different points in the life cycle. Given the observed repayment patterns in Section 3.2, defaulters having their records removed in 2014q1 are likely older than those have their records removed eight years in the future between 2013q1 and 2014q4.

The variable $RecDrop_{iqa}$ is an indicator equal to 1 if the borrower is in treatment cohort (default record is removed) where $q = a$. Note that borrowers can appear as controls in different sub-experiments if they satisfy $q_{control} \in [q_{treated} + 4, q_{treated} + 7]$ and are present in the credit reporting data throughout $e \in [-4, 3]$. The indicator $1[e = h]$ is equal to 1 if event time (calendar quarter in each a) is h quarters before or after the record removal and 0 otherwise. I normalize the quarter, δ_{-1}, prior to the removal event to zero. The parameters of interest are δ_e which capture estimates of the average treatment effect on the treated (ATT) in each period before and after the record removal. I follow recommendations from Wing et al. (2024) and estimate equation (1) using weighted least squares where each group-time ATT is weighted by its share of the treatment group. Thus, my specific target parameter is the "trimmed aggregate ATT."[16] δ_e will capture unbiased estimates under the assumptions that treated and yet-to-be treated borrower outcomes would have evolved in parallel if not for the record removal (parallel trends) and no anticipatory effects from the expectation of the record removal (no anticipation). In the proceeding sections, I present figures plotting the δ_e estimates to study the evolution of potential treatment effects and assess the validity of the research design. In assessing the robustness of my results, I also include payment-group fixed effects interacted with time and sub-experiments following Dinerstein et al. (2024). Payment groups are defined as the borrower's decile in the distribution of minimum payments across mortgages, auto loans, and credit cards in the period right before the record removal.[17] In practice, these fixed effects do little to change point estimates, but they do increase precision.

The ϵ captures the error term, and I cluster standard errors at the treatment cohort level since that is the level of my variation. Clustering at the cohort level accounts for repeated appearances of cohorts across sub-experiments. Table A1 summarizes the stacked dataset where each row presents the sub-experiment's calendar quarter range, its share of the dataset and treated borrowers, and the cohorts that are eligible to be in the comparison group.

Variation in $RecDrop_{iqa}$ comes from two sources: (i) preemptive actions by borrowers to remove the default record from their credit report, and (ii) the date when borrowers originally entered 90-day delinquency on the eventually defaulted loan. Since I restrict the removal events of interest to those where the tradeline stays on the borrowers credit report around the FCRA timing threshold for derogatory information, I reduce the chances that treated or comparison borrowers are rehabilitating their loans. Otherwise, the main concern would be that estimates would be biased by comparisons of rehabilitators and non-rehabilitators who might exhibit differential trends on unobservables. For example, rehabilitators might seek record removal in anticipation of lower borrowing costs facilitating new borrowing.

Since I only focus on borrowers who have removal events consistent with FCRA, the primary source of my variation in the stacked DiD model comes from (ii). Thus, identification requires that borrowers did not strategically time their initial delinquency with future record removal in mind - a plausible assumption given the long seven-year horizon and the automatic nature of the removal process. A key remaining assumption, beyond ruling out explicit anticipation, is that the exact timing of initial delinquency for borrowers, seven years before

[16]As Wing et al. (2024) discuss, "the trimmed aggregate ATT is similar to the balanced event time aggregate presented by Callaway and Sant'Anna (2021)," except that compositional balance is imposed in the trimmed aggregate case.

[17]As discussed in Dinerstein et al. (2024), the specification's additive structure may be a poor fit in the context of loans where the nonlinearity of repayment plans would result in differential trends in payments even in the absence of treatment if both groups have underlying differences in balances.

the record is removed, is effectively random with respect to later borrowing behavior. I argue that this assumption is more plausible when drawing comparisons between treated cohorts within a narrow window of time as treatment timing differs by at most, seven quarters. The main tradeoff to this approach is the ability to look at longer horizons of post-removal outcomes, since increasing the post-period event window requires using comparison units who are treated further in the future.

5 Results

This section presents the main empirical findings. I begin by documenting the immediate effects of record removal on credit scores. I then examine borrowing responses across mortgages, auto loans, and credit cards, showing significant increases driven primarily by the extensive margin. Next, I assess whether these borrowing patterns reflect improved credit conditions or increased financial distress by examining delinquency rates, available credit, and credit inquiries. Finally, I explore heterogeneity by baseline credit score, demonstrating that positive effects on credit access extend to subprime borrowers who comprise over 80% of the sample.

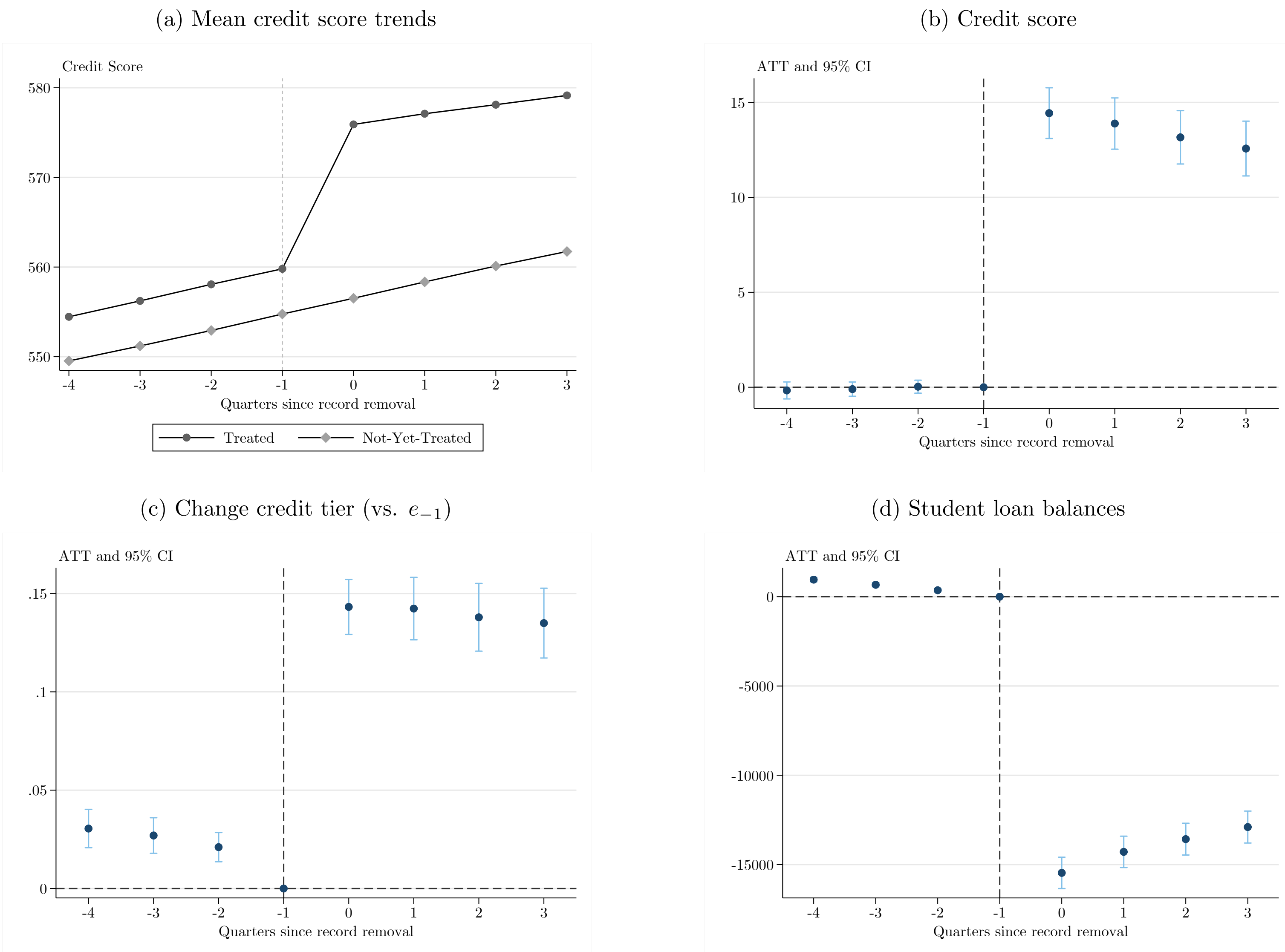
5.1 Credit Scores and Student Loan Balances

The removal of default records significantly increases borrowers' credit ratings immediately in the period where the record drops and this initial increase persists over the year since the default was last present. The magnitudes of score increases are very similar to the existing empirical literature on bankruptcy flag removals, though because my sample of student loan defaulters generally start at lower credit scores, the implied effect on the predicted probability of default from the credit score is greater relative to bankruptcy flag samples. Further, I demonstrate that these are economically meaningful improvements in scores with student debtors significantly more likely to cross into a new credit tier. Credit tiers follow grouping by credit reporting agencies and are defined (in ascending order of creditworthiness) as subprime, near prime, prime, and super prime.

Panel (a) in Figure 5, illustrates the raw means for the treatment and comparison group in credit scores over event time. Leading up to the removal, both treated and yet-to-be-treated borrowers scores move in parallel to one another with both groups showing positive trends in their mean scores. This general, upward trend is not surprising as the relevance of borrowers' derogatory information diminishes over time even if it is present on their credit report. However, in the first period where the record is removed, (event time 0), treated borrowers mean scores improve by about 15 points while comparison borrowers continue their trajectory through the end of the event window.

In the remaining panels of Figure 5, I plot estimates of equation (1) where the outcomes are credit score (b), change in credit tier (c), and student loan balances (d). Each circle corresponds to the δ_e estimates (the trimmed aggregate ATT) and their associated 95% confidence intervals. Panel (b) confirms the observed trends presented in panel (a). There are precise, null differences in score trends prior to the record removal and then a sharp increase in event time 0. The treatment effect appears to attenuate slightly over the year, though score gains are still large, roughly 13 points at the end of the year. This reflects partial convergence of the two groups rather than deteriorating performance among the treated. Panel (a) shows that the treated group's credit scores continue to improve on average, while the comparison group follows its own positive trajectory, gradually narrowing the gap before its own default records are removed.

Figure 5. Changes in Credit Scores and Student Loan Balances



Notes: The sample is all borrowers with defaulted student loan record removals between 2013q1 and 2022q3. Horizontal axis is event time relative to the record removal event where $e = 0$ and is measured in calendar quarters. Panel (a) plots the mean credit score trends for the treated group (receiving record removals) and the comparison group (not-yet-treated), with a vertical line marking the period before and after the record removal. Panels (b)-(d) show coefficients and 95% confidence intervals from the stacked event-study regressions described in the paper for credit scores, probability of changing credit tiers relative to $e = -1$, and student loan balances. Student loan balances are the total listed balances on borrowers' credit reports, which is different than what these borrowers actually owe.

One way to consider the magnitude of these score gains, is to measure the likelihood of treated borrowers to cross a new credit tier threshold. Crossing into a new credit tier potentially translates into larger changes in loan terms and marketing efforts by lenders. Yet, given that the vast majority of this population are in the lowest bracket of creditworthiness, its plausible that these score improvements do little to change how lenders consider these borrowers, and in turn, borrowing outcomes. In panel (c), the outcome is equal to one when the borrower is in a different credit tier relative to e_{-1}. Treated borrowers are nearly 15 percentage points more likely to be in a new credit tier following record removal. In the context of baseline credit worthiness and the observed score gains, the record removal translates to a large change in subprime borrowers moving to near prime status.

Finally, estimates shown in panel (d) serve as an initial data validity check. I identify record drops that are consistent with FCRA using the timing of the disappearance of a defaulted tradeline. It is then no surprise, that I find a large initial drop of about $15,000 in listed student balances at the same time of the record removal.[18] These are records aging off the credit report, but debtors still owe these dollars. The concern is, following the initial drop, listed balances would spike near the levels they were prior to the removal. This change in balance would be consistent with reporting practices once a borrower consolidates or rehabilitates their loans. Estimates from the event study reveal that treated borrowers have a persistent large drop in balances suggesting that removal events driven outside of FCRA are not a first-order concern for the analysis.

5.2 Borrowing

I find that improvements in creditworthiness among treated borrowers leads to significant increases in indebtedness across all three major loan categories. Figure 6 plots the event study estimates where the outcomes are aggregate balances, scheduled payments, and whether the borrower has an open loan separated by loan type. Rather than the sharp increases in credit scores and decreases in student balances, debt accumulation effects increase through the end of the event window. The balance outcome measures an individual's stock of debt and so these dynamics appear natural as borrowers learn of their credit score improvements, receive new marketing from lenders, and/or get approved for new debt at higher rates relative to the comparison group. Mean trends in balances across loan types are illustrated in Figure A4.

Relative to pre-period means, the increases in balances are substantial. By the end of the year without the default record, the ATT estimate for mortgages is approximately $1,801, a 37 percent increase. Auto and credit card balance differences at e_3 are $404 (14.6 percent) and $131 (27 percent), respectively. At baseline, these are a group of borrowers that are less likely to have open debt and so even small changes in absolute terms lead to large effects.

Impacts on auto loans are potentially important for these borrowers who were already

[18]While initial borrowing amounts among defaulters are relatively low, long periods of non-repayment means interest has accumulated over many years. Upon default, borrowers may also be charged court costs, collection fees, attorney's fees, and other costs associated with the collection process.

relatively low-resourced and more likely to have had poor labor market outcomes upon leaving higher education. Individual mobility is a long-standing focus of labor research seeking to understand worker outcomes, and previous work has shown that vehicle ownership and access can improve employment outcomes (e.g., Baum, 2009; Blumenberg et al., 2015; Johnson, 2006; Ong and Miller, 2005; Raphael and Rice, 2002).[19] Car ownership may also expand the feasible radius for housing searches, enabling individuals a wider range of potential neighborhoods (Blumenberg and Pierce, 2017). One potential implication of this analysis is that the initial credit lockout from a student loan default may create a longer-term barrier to economic recovey, limiting employment opportunities, and, consequently, the borrower's capacity to repay their student debt.

The mortgage balance impacts are also interesting given the significant underwriting in the housing market implying relative financial security for these borrowers around the removal period. In light of recent research showing that increases in student debt balances have caused lower homeownership rates (e.g., Bleemer et al., 2021; Mezza et al., 2020), these findings are suggestive that default records might also have some role to play.

The patterns observed in balances are largely mimicked in reported payments due and whether borrowers have an open loan. These provide important context to the balance findings. Balances could increase if existing borrowers have lower scheduled payments (perhaps due to forbearance). Instead, the primary driver of balance increases appear to be differences at the extensive margin of borrowing. Alternatively, borrowers could become delinquent on their debts at higher rates yielding larger balances while still seeing positive impacts on scheduled payments and likelihood of borrowing. While this is unlikely given the stability in the credit score impacts, I explore impacts on delinquency in the proceeding section.

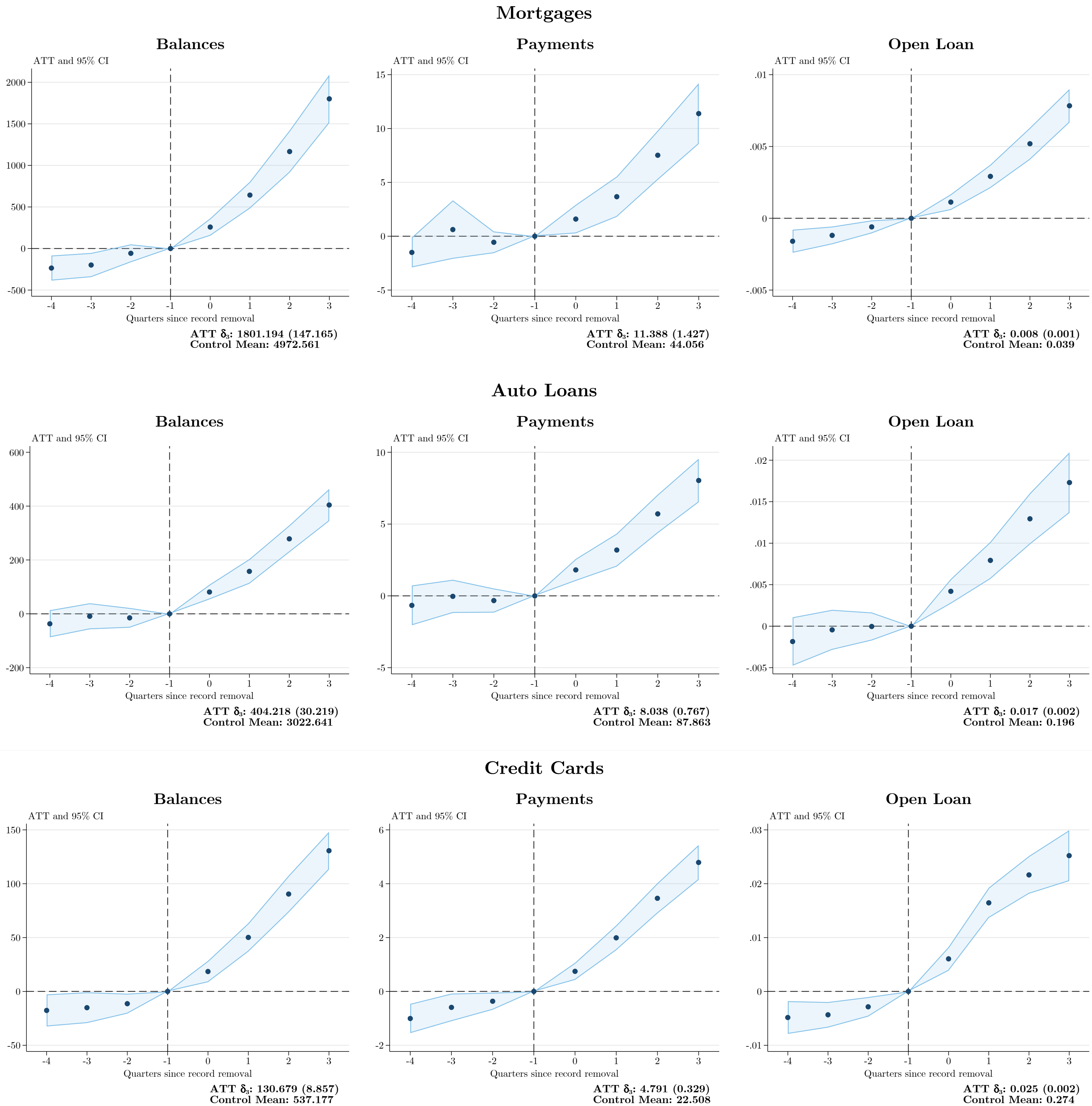
In assessing the pre-period estimates, some of the outcome estimates hint at slight upward trends though estimates are precise and the slope change in the post-period is stark enough to largely rule out a continuation of purely positive, linear pre-trends. I test the robustness of these estimates more formally in Section 6.

5.3 Consumer Credit Conditions

On average, I find that the the overall improvement in credit scores translates to increases in borrowing across all three major debt categories within a year of default record removals. The potential for lower borrowing costs and higher limits implied by the changes in credit

[19]A related research agenda stems from the spatial mismatch hypothesis which posits that there is a geographic disconnect between where many low-income and minority workers live and where well-matched jobs are located. See Andersson et al. (2018) for a discussion and related work. See also Bastiaanssen et al. (2020) for a review of the empirical research exploring the relationship between transportation and employment.

Figure 6. Stacked Event-Study: Balances and Payments Due



Notes: Horizontal axis is event time relative to the record removal event where $e = 0$ and is measured in calendar quarters. Each sub-figure plots estimated coefficients and 95% confidence intervals from the stacked event-study regressions described in the paper for balances, scheduled payments, and probability of having an open loan, separated by each major category of debt: mortgages, auto loans, and credit cards. The quarter right before the record removal event is the reference period.

scores could mean overall improvements in credit conditions for these borrowers. I explore this using the framework outlined in Brennecke et al. (2025).

Ability to absorb economic shocks— Brennecke et al. (2025) construct measures of an individual's ability to absorb an economic shock centered on a borrower's available credit on general-purpose cards. Figure A5 shows stacked-DiD estimates for these measures. By the end of the first year without the default record, credit limits rise by \$326, a 28% increase relative to the control mean (panel a). Credit limit impacts are not surprising given the increases on the extensive margin of borrowing, though consumers may also be receiving higher limits on existing cards. However, the capacity to absorb economic shocks with unsecured cards will depend on the borrower's available credit. If borrowers immediately max-out their new credit cards, limits will not provide any protection from a negative shock. Estimates in panel (b) suggest this is not occurring within the first year of treatment. The ATT estimate is about \$200, a 31% increase.

Ability to manage daily finances— A key concern is whether the documented increases in debt are accompanied by a rise in financial distress. To investigate this, Figure A6 presents event study estimates on the share of outstanding balances that are past-due. The results show no significant increase in delinquency for total balances, mortgages, or auto loans. This suggests the observed impacts on balances are not driven by defaults and that, on average, consumers are managing this new credit responsibly.

The estimates for credit cards warrant closer inspection. As shown in Figure A6, the estimate on δ_{-4} is positive and statistically significant, suggesting a potential violation of the parallel trends assumption for this specific outcome. However, this pre-trend is likely a mechanical artifact of compositional differences between the groups. Table 2 shows that just prior to treatment, the treated group has a higher rate of credit card ownership (31%) than the comparison group (27%). Because only individuals with a credit card can become delinquent, the treated group has a larger base of consumers at risk of delinquency.

This same compositional dynamic complicates the interpretation of the post-period coefficients. My primary findings show that the treatment significantly increases the likelihood to have an open loan across debt categories. Therefore, the post-treatment increase in the delinquency share for the treated group is driven, at least in part, by the mechanical fact that more treated individuals now hold a credit card and are able to become delinquent. Given the economically modest point estimates, which represent an increase of less than 0.08 percentage points, and these significant compositional effects, I conclude that there is no strong evidence of a widespread increase in credit card distress. One key caveat to reemphasize is the trade-off between constructing the ideal treatment and not-yet-treated comparisons and estimating impacts of a longer horizon. Ideally, I would be able to understand debt management past a year from record removal. I plan to address this in future iterations of this work.

Table 2. Summary Statistics: Treated and Not-Yet-Treated

	(1) Treated			(2) Not-Yet-Treated		
	Mean	SD	Median	Mean	SD	Median
Age	40.10	10.45	38	38.91	10.56	37
Credit Score	559.80	56.56	554	554.76	53.40	550
Default on Student Loan	1.00	0.00	1	1.00	0.00	1
Auto Loan Balance	3453.23	8524.74	0	3022.64	7896.25	0
Auto Loan Scheduled Payment	97.55	216.42	0	87.86	212.59	0
Any Open Auto Loan	0.22	0.41	0	0.20	0.40	0
Mortgage Balance	4948.49	31298.96	0	4972.56	32476.07	0
Mortgage Scheduled Payment	45.22	473.72	0	44.06	322.74	0
Any Open Mortgage	0.04	0.19	0	0.04	0.19	0
Credit Card Balance	623.86	2290.44	0	537.18	2213.10	0
Credit Card Scheduled Payment	27.05	73.70	0	22.51	68.00	0
Any Open Credit Card	0.31	0.46	0	0.27	0.45	0
Credit Card Limit	1399.41	4762.04	0	1153.14	4291.35	0
Observations	48,314			213,315		

Notes: Sample in panel (1) are student loan borrowers whose default record is removed between 2013q1 and 2021q3. These represent the sample of treated borrowers in my analytical sample. Panel (2) are the set of not-yet-treated (control) borrowers. Summary statistics for both groups are drawn from the quarter before record removal for the treated. Observation count in panel (1) represents the number of individual borrowers. In panel (2) individual borrowers can serve as controls in multiple sub-experiments, but can only appear once in any given sub-experiment.

Ability to get new credit— To explore whether the observed borrowing responses are driven by changes in credit demand, I examine hard credit inquiries. Inquiries are an imperfect proxy for demand, especially for the low-credit-score population in this study. A hard inquiry is recorded when a lender accesses a consumer's credit file to evaluate a loan application, typically causing a small, temporary drop in their credit score.

This measure has two key limitations. First, inquiries are not consistently reported to all credit bureaus (Gibbs et al., 2025). Second, and more importantly, inquiries may significantly understate true credit demand due to discouragement, where individuals do not apply for loans because they anticipate rejection. This discouragement is highly relevant for my sample. Data from the Federal Reserve Bank of New York's Survey of Consumer Expectations (June 2025) show that consumers with credit scores below 680 were much more likely to be discouraged from applying for credit than those with scores between 681 and 759 (22% vs. 1.8%). Given these caveats, the inquiry analysis should be interpreted with caution.

Figure A7 plots event study estimates where the outcome is a binary variable equal to one if the consumer has a new credit inquiry on their report in the quarter of interest. Each panel is separated by inquiry loan type. Overall, there does not appear to be clear, meaningful differences in inquiries. In the quarter of record removal, there is a 0.3 percentage point increase in the probability of having an auto loan inquiry, but this estimate approaches zero by the end of the panel. There is also a small increase in credit card inquiries of approximately 0.2 percentage points a year without the default record.

This muted response in inquiries, when contrasted with the significant increase in actual borrowing, suggests that the primary mechanism is an improvement in consumer ability to be approved for credit, not a surge in demand. The stability of the post-removal credit score gains documented earlier provides further support for this view. A large spike in applications would likely have put downward pressure on scores, a pattern not observed in the data. Furthermore, even a large, positive effect on inquiries would be difficult to interpret. Such a response could be driven by previously discouraged borrowers re-engaging with the market after seeing their scores improve. Alternatively, it could reflect a supply-side response, where lenders proactively market to these newly creditworthy consumers, providing a positive signal that induces applications.

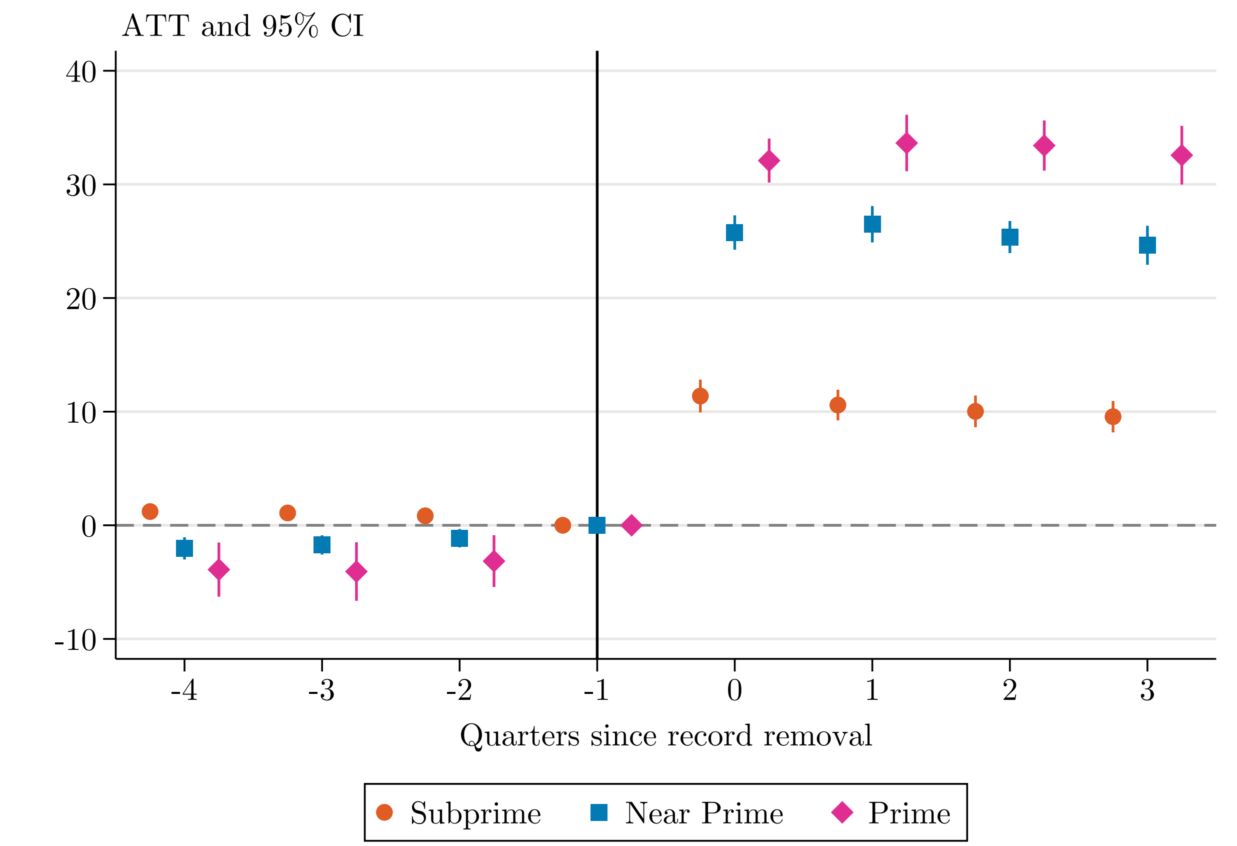
5.4 Estimated Impacts by Baseline Credit Score

Credit score profiles or tiers can play an important role in lending decisions (Consumer Financial Protection Bureau, n.d.; Laufer and Paciorek, 2022). Thus, borrowing responses might differ by defaulter's baseline credit scores. Over 80% of defaulters in my sample would be classified as subprime (credit scores at or below 600) prior to the record removal event. Conditional on demand, if lenders use score cutoffs, then subprime borrowers would be more likely to be outside of the formal credit market prior to the removal event. Near prime (scores: 601-660) or prime (scores: 661-780) borrowers, on the other hand, will likely be approved for higher balances.

Figure 7 plots the stacked event-study coefficients from three separate regressions where the sample is either composed of pre-removal subprime, near prime, or prime borrowers.[20] All exhibit a discontinuous jump in their credit scores, although near prime and prime borrowers have noticeably higher gains with these groups experiencing close to 27 and 31 point increases, respectively. However, the absolute point change can be a misleading metric of economic significance. A smaller increase for a subprime borrower may represent a larger proportional reduction in their perceived default risk. Therefore, the smaller point gains for subprime borrowers do not necessarily imply a smaller improvement in their creditworthiness from a

[20] Super prime borrowers are excluded from the analysis because so few borrowers in my sample are within this credit score range.

Figure 7. Credit Score Impacts by Baseline Credit Tier



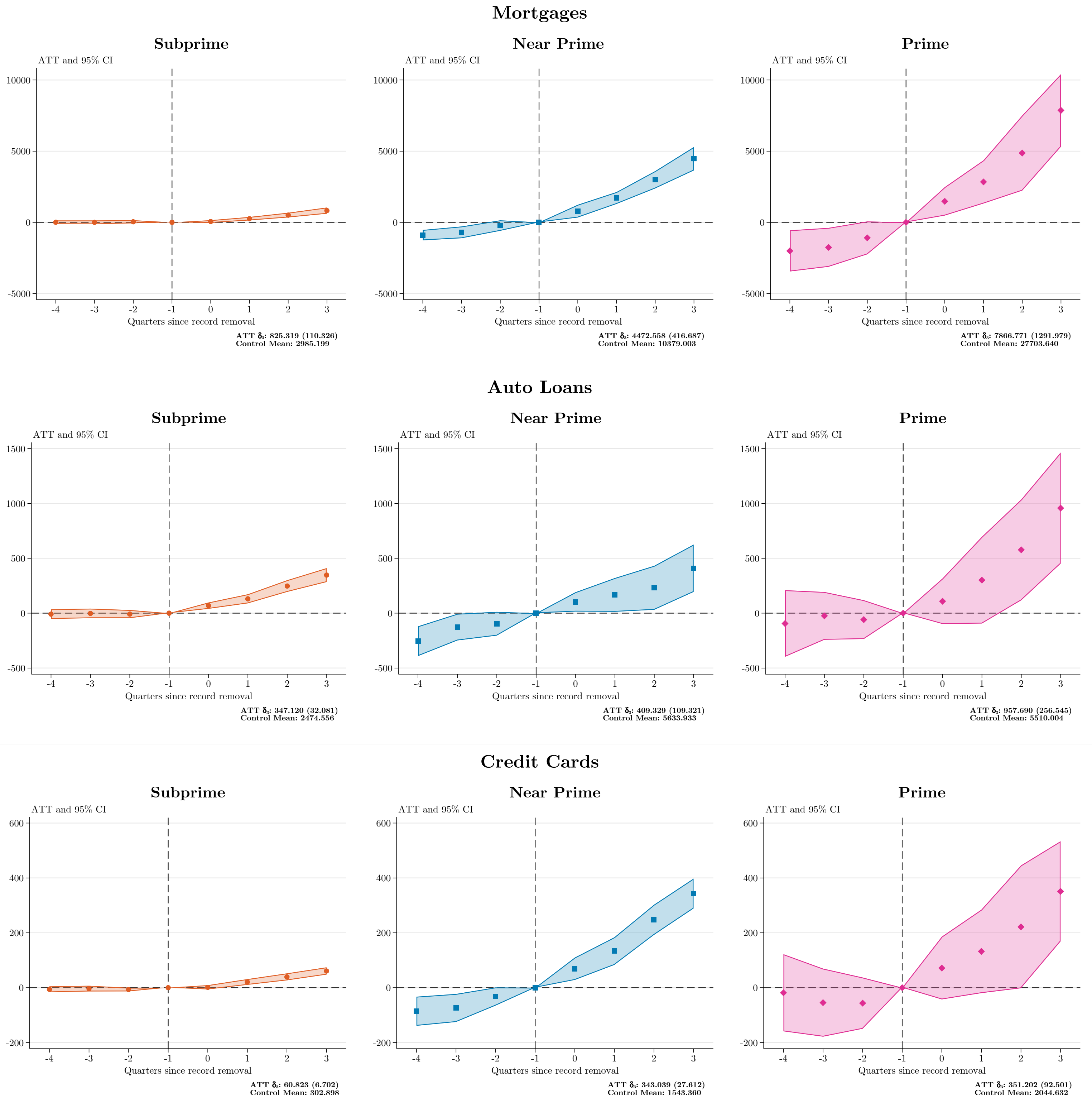
Notes: Horizontal axis is event time relative to the record removal event where e = 0 and is measured in calendar quarters. The figure plots estimated coefficients and 95% confidence intervals from three separate stacked event-study regressions where the sample is restricted to borrowers in each baseline credit tier (subprime, near prime, or prime) in the quarter prior to record removal. Credit tiers follow standard industry definitions: subprime (scores 600), near prime (scores 601-660), and prime (scores 661-780). The quarter right before the record removal event is the reference period. Super prime borrowers (scores 780-850) are excluded from the analysis due to small sample size..

lender's perspective.

An analysis of borrowing across credit tiers reveals that the positive effects on credit access are not confined to the small subset of defaulters who already had a greater capacity to borrow. Figure 8 illustrates the ATT estimates on the stock of debt across baseline credit tier. Each panel is organized by the loan type, and each sub-figure's title denotes the credit tier. The vertical axis in each loan panel is common across the credit tiers for ease of comparison. While near-prime and prime borrowers experience larger absolute increases in debt balances (though with less statistical precision), the subprime majority also shows a clear and significant positive borrowing response. This analysis also reveals that small differences in borrowing prior to record removal that were observed in the full sample are driven by the relatively few consumers that have higher baseline credit scores. For example, auto loan balances for near prime borrowers appear to follow a smooth upward trajectory throughout the panel.

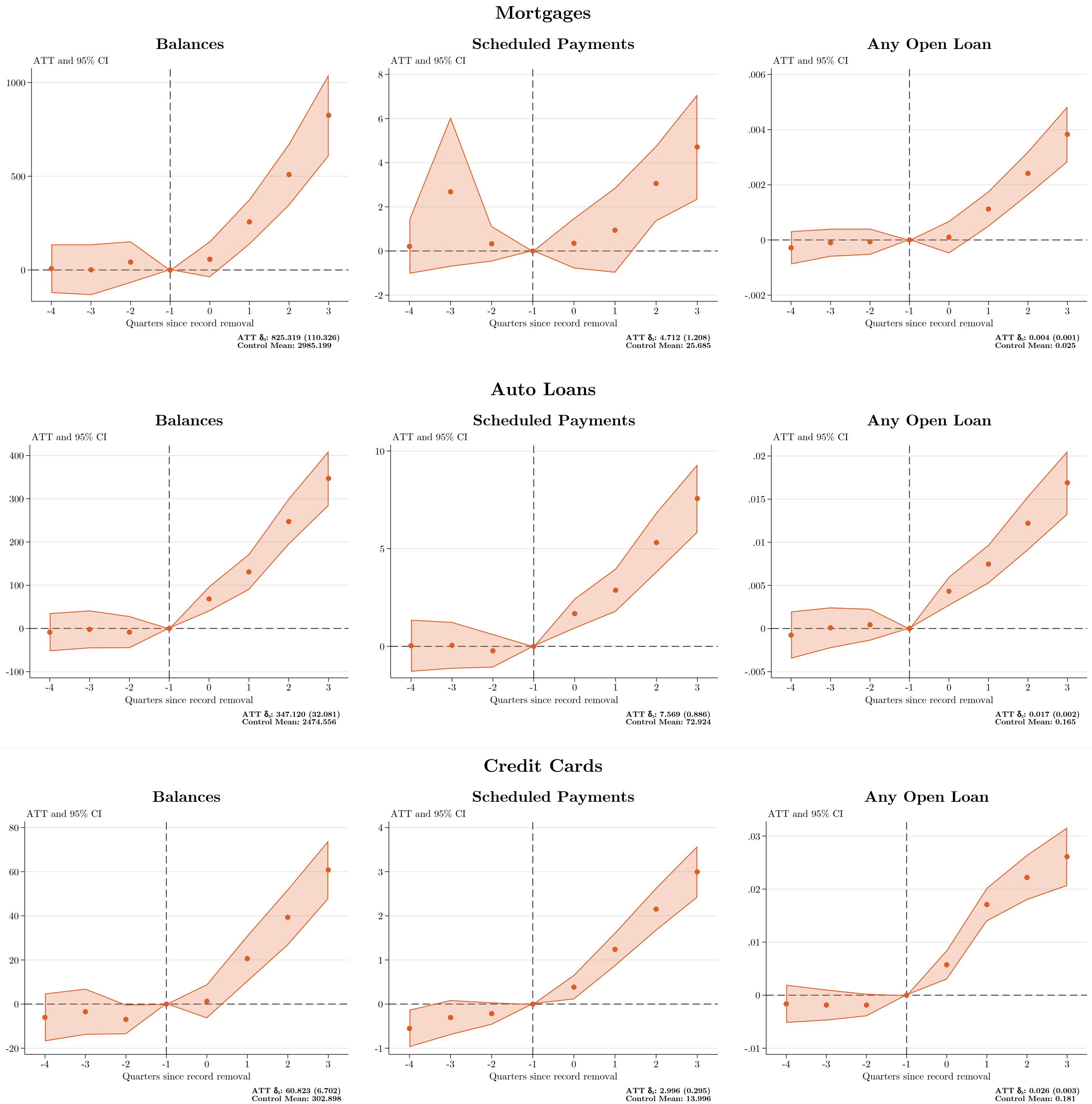
Isolating the analysis on subprime borrowers confirms that the extensive margin borrowing response is not driven by more creditworthy individuals (Figure 9). For this group, the probability of holding an auto loan and credit card increases by 1.7 and 2.6 percentage points, respectively. These effects are economically meaningful, implying large percentage increases over a low baseline. The impact on mortgages is smaller at 0.4 percentage points but still notable, given that only 2.5% of this population had a mortgage prior to record removal.

Figure 8. Stacked Event-Study: Balances by Baseline Credit Tier



Notes: Horizontal axis is event time relative to the record removal event where e = 0 and is measured in calendar quarters. Each panel plots estimated coefficients and 95% confidence intervals from separate stacked event-study regressions for balances on mortgages (top row), auto loans (middle row), and credit cards (bottom row), with each column representing a different baseline credit tier. The vertical axis scale is consistent within each loan type (across columns) to facilitate comparison across credit tiers. The quarter right before the record removal event is the reference period.

Figure 9. Stacked Event-Study: Balances and Payments Due for Subprime Borrowers



Notes: Horizontal axis is event time relative to the record removal event where $e = 0$ and is measured in calendar quarters. Each sub-figure plots estimated coefficients and 95% confidence intervals from the stacked event-study regressions described in the paper for balances, scheduled payments, and probability of having an open loan, separated by each major category of debt: mortgages, auto loans, and credit cards. The quarter right before the record removal event is the reference period.

6 Threats to Identification and Robustness

This section examines the robustness of the main results, addressing potential threats to identification and exploring sensitivity to alternative specifications.

No anticipation — A key assumption of the research design is that borrowers do not strategically alter their behavior in anticipation of the record removal. There are no available estimates on whether student loan borrowers in default are aware of FCRA timing threshold, although credit bureaus do not actively inform borrowers of these actions.[21] Even if borrowers are generally aware that most derogatory records are removed after seven years, they might not be aware of the backdating to the original loan delinquency before the loan was transferred to the federal default servicer. These individuals then might overestimate how long the record will remain on their report.

In evaluating the path of the outcomes observed in Figure 6, it is unlikely that anticipation is a first-order concern. Similarly argued by Gross et al. (2020), we should expect a downward trend in outcomes if anticipatory behavior is present. Borrowers should reserve their borrowing until after the record removal in anticipation of lower borrowing costs and improved terms.

Sensitivity analysis — In order to interpret these effects as causal, a key assumption is that treated and comparison borrowers would have followed the same borrowing trajectory in absence of the record removal event. The main results presented in Figure 6 provide some evidence in support of parallel trends. Treated and not-yet-treated borrowers also do not exhibit economically meaningful differences in their outcomes or age prior to the record removal (Table 2). Still, there are established limitations to assessing the validity of the parallel trends assumption based on pre-period estimates (Freyaldenhoven et al., 2019; Rambachan and Roth, 2023; Roth, 2022). Following Rambachan and Roth (2023), I estimate breakdown values, $\bar{M}$, representing how large post-treatment violations of parallel trends could be relative to the maximum observed violation observed in the pre-treatment period and still estimate significant effects. Figure A8 illustrates results from this analysis focusing on the main balance outcomes. Mortgage and auto loan balance outcomes are robust to violations between 1.9 and 2 times as large as the maximum deviation in the pre-period, and credit card estimates are robust to violations between 1.6 and 1.7. As estimated ATTs in the pre-period are closer to zero in the subprime group (Figure 8), estimates of $\bar{M}$ are larger (more robust) for this group.

Parametric event study — The nonparametric approach I use in my main analysis will

[21] Gross et al. (2020) field a survey of bankruptcy filers asking respondents when their bankruptcy flags would be removed from credit reports. Only 9.2% of their sample correctly predicted when this would occur. The student loan defaulters I study are arguably more disconnected from the credit market than bankruptcy filers who necessarily had debts to be discharged to begin with.

overstate the impact of record removals if there are pre-existing, positive trends in the outcomes between the treated and control group. Related papers studying bankruptcy flag removal use a parametric event study controlling for observed linear trends, arguing that the initial bankruptcy event represents a particularly distressing event in the borrower's life that they slowly recover from (e.g., Dobbie et al., 2017; Gross et al., 2020). Revisiting Figure 6, the nonparametric estimates suggest that a linear trend captures any secular trends well. I assess the robustness of my results by estimating specifications of the following form:

$$Y_{iqae} = \alpha \times RecDrop_{iqa} + \sum_{h=0}^{3} \Big[\delta_e RecDrop_{iqa} \times 1[e = h] \Big] + \lambda_{qa} + \gamma_{ae} + \epsilon_{iqae}. \quad (2)$$

Here, $\alpha \times RecDrop_{iqa}$ captures the pre-record-removal trend in outcomes and δ_e coefficients are only estimated for the lagged effect of record removal. The post-event study indicators now measure deviations from a linear projection of pre-record removal differences. The identification assumption in this setting is that the timing of the removal event is uncorrelated with deviations of the outcome from a linear trend in event time. In the absence of differential trends, the parametric and nonparametric estimates will yield the same results. Overall, the main results are robust to this linear extrapolation and statistically indistinguishable from the non-parametric estimates (Figure A9).

7 Welfare Implications of Record Removal

In principle, removing student loan default records decreases lenders' ability to price according to borrowers' true underlying risk. Removal of this information can effectively pool previously high-risk borrowers with observably similar consumers creating two opposing effects: it transfers surplus to defaulted borrowers through better loan terms but can also generate a social welfare loss from inefficient credit allocation.

Jansen et al. (2025) provides a framework, built on the cost curve approach developed in Einav et al. (2010), to consider the welfare consequences of credit information. A key attraction of the model is that it only requires prices and loan quantities, and thus, I can readily translate my empirical results to this framework. Note that my main results provide estimates on loan quantity effects, but not interest rates as I do not observe these directly in the credit bureau data. Approaches to back-out interest rates can be reliably applied in the auto loan market which will be incorporated in future iterations of this work. I therefore focus on my auto loan estimates. Results from this proceeding analysis should be considered preliminary.

Following notation from Jansen et al. (2025), the welfare calculation requires four primary inputs: the quantity and price of credit before and after the record removal. The price is defined as the monthly payment as a fraction of the principal, $\phi(r)$. The before state corresponds to the fair-price equilibrium for defaulters where the record is present ($\phi(r_{flag})$, $\Lambda(\phi(r_{flag}))$), while the after state is the pooled-price equilibrium where the record is removed ($\phi(r_{pool})$, $\Lambda(\phi(r_{pool}))$). I map my estimates to these parameters as follows: $\Lambda(\phi(r_{flag}))$ is the baseline auto loan balance prior to flag removal in Table 1, and the post-removal quantity, $\Lambda(\phi(r_{pool}))$, is this baseline plus my estimated ATT a year without record, δ_3. Because my estimates are quarterly and the estimates effects are driven by the extensive margin, I convert the estimated annual increase in the stock of debt into an average monthly flow. As a starting point, I use the price changes estimated by Jansen et al. (2025) in their empirical application to bankruptcy flag removals which they find yields a 23-basis-point decrease in interest rates. This translates to repayment fractions of 2.077% and 2.066% for $\phi(r_{flag})$ and $\phi(r_{pool})$, respectively.[22] Using these inputs, I calculate the change in consumer surplus (ΔCS)—the transfer to defaulted borrowers—and the change in social welfare (ΔSW), which represents the deadweight loss from inefficient allocation when information is removed. $\frac{|\Delta SW|}{\Delta CS}$ provides a dollar measure of the social welfare lost per dollar in surplus transferred to defaulted student borrowers.

Similar to bankruptcy flag removal estimates in Jansen et al. (2025), I find that the social deadweight loss from record removal is relatively small: 5.74 cents of social surplus are destroyed for each dollar transferred to defaulters. The comparable estimate in Jansen et al. (2025) is 3.15 cents. This difference is primarily attributable to the larger annual borrowing response, \$404 relative to \$213.50. From the perspective of the model, a greater borrowing elasticity implies that the price signal from record removal draws in a larger volume of marginal credit, which, under the model's assumptions, is inefficiently priced. Social welfare loss is still modest since student loan defaulters, similar to bankruptcy, are a small group relative to all consumers.[23]

There are two assumptions in the framework that are relevant in my setting when interpreting the welfare calculations. First, the price with the default flag present, $\phi(r_{flag})$, reflects the true marginal cost of lending to these borrowers. Under this assumption, data

[22] Jansen et al. (2025) use estimates of expected non-default periods of a 60-month loan term, ψ to convert monthly welfare changes into a total lifetime expected value. I use their estimates of the high cost, $\psi_H = 57.29$, and low cost groups, $\psi_L = 58.34$, but student defaulters may have lower or higher probabilities of default on car loans than bankruptcy filers. Aside from the loan quantity estimates, the only underlying parameters I change are the number of student loan defaults each year, assumed to be approximately 500,000, and the relative size of student loan defaulters, about 3.8% of consumers.

[23] If the true price change for student loan defaulters is larger than the 23-basis-point assumption, the efficiency ratio would rise, since the welfare loss area scales approximately with the square of the price distortion, whereas the consumer surplus transfer area grows linearly. Thus, larger price distortions imply a higher social cost per dollar redistributed.

disclosure is always welfare-improving because it enables efficient pricing based on true risk. However, if the seven-year persistence of default records causes them to become stale signals of current creditworthiness, then record removal may actually improve allocative efficiency by correcting lender overestimation of risk (Blattner and Nelson, 2021). Second, the framework assumes borrowers face continuous menu of interest rate-loan size combinations and that "data policy primarily influences outcomes through shifting interest rates." Borrowing responses following default flag removal are primarily driven by the extensive margin which suggests borrowers may have faced credit rationing rather than high prices, alone. In these cases, the model cannot account for borrowers who transition from infinite (rationing) to finite prices and will underestimate welfare gains. Still, the framework provides a useful benchmark to consider the efficiency cost of record removal as a redistributive tool: even under the conservative assumption that seven-year-old flags provide perfect information about current risk, the social cost per dollar transferred to defaulters remains modest at 5.74 cents.

8 Conclusion

Removal of default records from credit reports yields discontinuous increases in consumer credit scores. This positive shock leads to significant and economically meaningful increases in borrowing across mortgage, auto, and credit card markets, primarily on the extensive margin. The results point to substantial unmet demand among defaulted borrowers where only 31% have an open credit card prior to record removal. Descriptive evidence suggests that credit constraints faced by defaulted borrowers stem from the initial, severe shock to their credit scores upon default. This leads to a scarring effect reflected in their persistently low credit market participation compared to their peers until FCRA-mandated removal of this information.

These findings provide new information on defaulted student loan borrowers who have been a central concern in the U.S. student loan debt crisis. Previous research has documented the major factors contributing to default, including poor labor market outcomes from low-return institutions and frictions in the repayment process in which borrowers do not take-up programs that would reduce the risk of default. I present evidence that is both consistent with this previous work, and provides new estimates on the consequences of default of which we knew little. The results suggest that default leads to a persistent lockout from credit markets, a particularly severe penalty for a population that could benefit from unsecured credit to smooth consumption in unemployment or from an auto loan to improve job access.

The large borrowing responses to FCRA-mandated record removal raise a critical question: why do so few borrowers use rehabilitation to remove these records sooner? While a full analysis of rehabilitation take-up is beyond the scope of this paper, the results demonstrate that the private returns to completing the program, in the form of improved credit access, are substantial. More broadly, the findings highlight the significant costs of default and underscore the large potential returns to policies—such as institutional accountability or improved repayment processes—that prevent borrowers from defaulting in the first place.

The relevance of these findings is particularly acute in the current policy environment. For nearly five years, a federal moratorium on student loan payments drove delinquencies to historic lows. Since that pause ended, however, delinquency rates have surged from below one percent in 2024 to over 10 percent in the second quarter of 2025, near pre-2020 levels (Figure A1). This resurgence means a new wave of borrowers is now at risk of default and its severe consequences for credit access.

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A Appendix Tables and Figures

Details on staggered default record removal design

My analysis draws comparisons between defaulted student loan borrowers who have their records removed (treated) to those who will have their records removed in the future (not-yet-treated). I implement this approach using a stacked difference-in-differences design following recommendations from Wing et al. (2024).

I directly construct comparisons of interest as separate datasets. Each of these datasets represents a "sub-experiment," which I then append into a single data file. In each sub-experiment, one cohort of borrowers has their student loan default record removed in a calendar-quarter. Two features dictate whether not-yet-treated borrowers can serve as a comparison borrower in the sub-experiment. First, the comparison cohort must receive treatment outside of the event window to avoid contamination from their own treatment effects. Second, I restrict comparison cohorts to those treated within a relatively narrow time window after the treated cohort—specifically, between four and seven quarters in the future. This restriction ensures that treated and comparison borrowers are at similar points in their life cycle and limits the potential for differential secular trends to bias my estimates.

Appendix Table A1 provides details on each sub-experiment used in my main analysis. Each row corresponds to a single sub-experiment defined by the calendar quarter in which the treated cohort receives record removal. The table shows the start and end quarters of the event window, the share of the stacked dataset and treated borrowers represented by that sub-experiment, the treated cohort quarter, and the eligible control cohorts (those receiving treatment between four and seven quarters after the treated cohort). Cohorts from 2012 are trimmed because there is an insufficient number of pre-treatment quarters to construct balanced event windows. Similarly, sub-experiments after 2021q3 are trimmed because there are no eligible control cohorts that receive treatment outside the event window while still falling within the four-to-seven quarter control restriction. The table reveals substantial variation in the number of defaulters across cohorts, with the largest shares of treated borrowers concentrated in the 2016-2018 period. This variation reflects underlying trends in student loan default rates during this period and determines the relative weight each sub-experiment receives in the aggregate treatment effect estimates. The stacked design ensures that each treated cohort is compared only to appropriate control cohorts, avoiding the contamination bias that can arise when using previously treated units as controls in traditional two-way fixed effects specifications.

Appendix Table A1. Sub-Experiments in the Staggered Default Record Removal Design

Sub-Exp.	**Start Qtr**	**End Qtr**	**Stack Share**	**Treated Share**	**Treated Cohort**	**Eligible Control Cohorts**
2012q1	Trimmed	Trimmed				
2012q2	Trimmed	Trimmed				
2012q3	Trimmed	Trimmed				
2012q4	Trimmed	Trimmed				
2013q1	2012q1	2013q4	1.44	0.95	2013q1	2014q1 – 2014q4
2013q2	2012q2	2014q1	1.57	1.72	2013q2	2014q2 – 2015q1
2013q3	2012q3	2014q2	1.48	1.11	2013q3	2014q3 – 2015q2
2013q4	2012q4	2014q3	1.74	2.17	2013q4	2014q4 – 2015q3
2014q1	2013q1	2014q4	1.62	1.37	2014q1	2015q1 – 2015q4
2014q2	2013q2	2015q1	1.79	2.27	2014q2	2015q2 – 2016q1
2014q3	2013q3	2015q2	1.55	1.26	2014q3	2015q3 – 2016q2
2014q4	2013q4	2015q3	1.76	1.93	2014q4	2015q4 – 2016q3
2015q1	2014q1	2015q4	1.73	1.27	2015q1	2016q1 – 2016q4
2015q2	2014q2	2016q1	1.91	2.38	2015q2	2016q2 – 2017q1
2015q3	2014q3	2016q2	1.76	1.57	2015q3	2016q3 – 2017q2
2015q4	2014q4	2016q3	1.88	2.13	2015q4	2016q4 – 2017q3
2016q1	2015q1	2016q4	1.91	1.24	2016q1	2017q1 – 2017q4
2016q2	2015q2	2017q1	2.35	2.08	2016q2	2017q2 – 2018q1
2016q3	2015q3	2017q2	3.32	2.07	2016q3	2017q3 – 2018q2
2016q4	2015q4	2017q3	3.80	2.61	2016q4	2017q4 – 2018q3
2017q1	2016q1	2017q4	4.16	1.07	2017q1	2018q1 – 2018q4

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Appendix Table A1. (Continued)

Sub-Exp.	**Start Qtr**	**End Qtr**	**Stack Share**	**Treated Share**	**Treated Cohort**	**Eligible Control Cohorts**
2017q2	2016q2	2018q1	4.34	2.09	2017q2	2018q2 – 2019q1
2017q3	2016q3	2018q2	3.86	2.18	2017q3	2018q3 – 2019q2
2017q4	2016q4	2018q3	3.81	3.67	2017q4	2018q4 – 2019q3
2018q1	2017q1	2018q4	3.25	2.60	2018q1	2019q1 – 2019q4
2018q2	2017q2	2019q1	4.34	7.30	2018q2	2019q2 – 2020q1
2018q3	2017q3	2019q2	3.62	4.20	2018q3	2019q3 – 2020q2
2018q4	2017q4	2019q3	4.13	7.04	2018q4	2019q4 – 2020q3
2019q1	2018q1	2019q4	3.26	2.49	2019q1	2020q1 – 2020q4
2019q2	2018q2	2020q1	3.67	4.55	2019q2	2020q2 – 2021q1
2019q3	2018q3	2020q2	3.33	2.43	2019q3	2020q3 – 2021q2
2019q4	2018q4	2020q3	4.20	5.01	2019q4	2020q4 – 2021q3
2020q1	2019q1	2020q4	3.81	3.71	2020q1	2021q1 – 2021q4
2020q2	2019q2	2021q1	3.86	3.83	2020q2	2021q2 – 2022q1
2020q3	2019q3	2021q2	3.59	2.44	2020q3	2021q3 – 2022q2
2020q4	2019q4	2021q3	3.93	4.90	2020q4	2021q4 – 2022q3
2021q1	2020q1	2021q4	3.11	3.84	2021q1	2022q1 – 2022q4
2021q2	2020q2	2022q1	2.40	4.08	2021q2	2022q2 – 2023q1
2021q3	2020q3	2022q2	1.70	4.44	2021q3	2022q3 – 2023q2
2021q4	Trimmed	Trimmed				
2022q1	Trimmed	Trimmed				
2022q2	Trimmed	Trimmed				

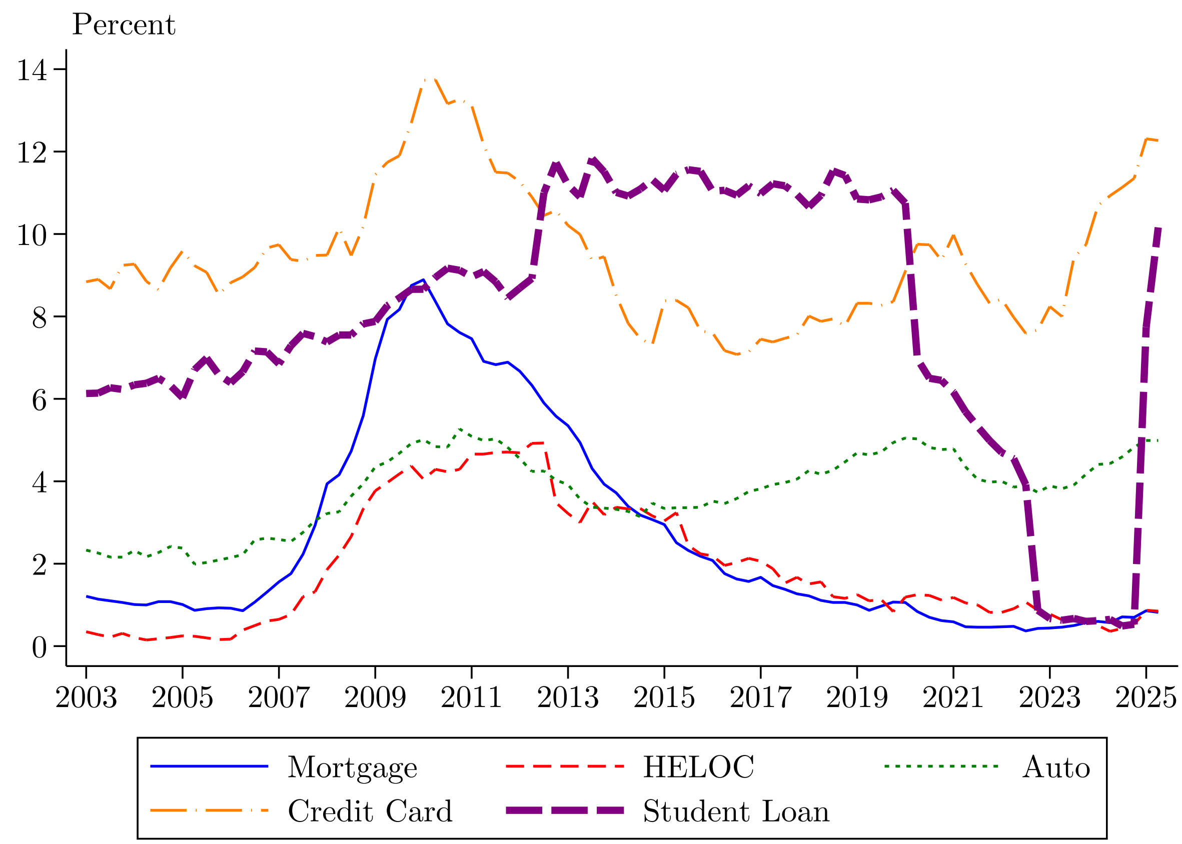
Continued on next page

Appendix Table A1. (Continued)

Sub-Exp.	**Start Qtr**	**End Qtr**	**Stack Share**	**Treated Share**	**Treated Cohort**	**Eligible Control Cohorts**
2022q3	Trimmed	Trimmed				

Notes: Each row provides details on each sub-experiment in the stacked difference-in-differences design. In my main analysis I define a uniform event window of four quarters pre and post record removal. The Treated Cohort (blue) is the cohort receiving treatment in that sub-experiment. The Eligible Control Cohorts (green) are defined as cohorts treated outside of the sub-experiment's event window and less than two years after the treated cohort. Cohorts from 2012 are trimmed due to an insufficient pre-treatment period. Sub-experiments after 2021q3 are trimmed as there would be no eligible control groups that receive treatment outside of the event window.

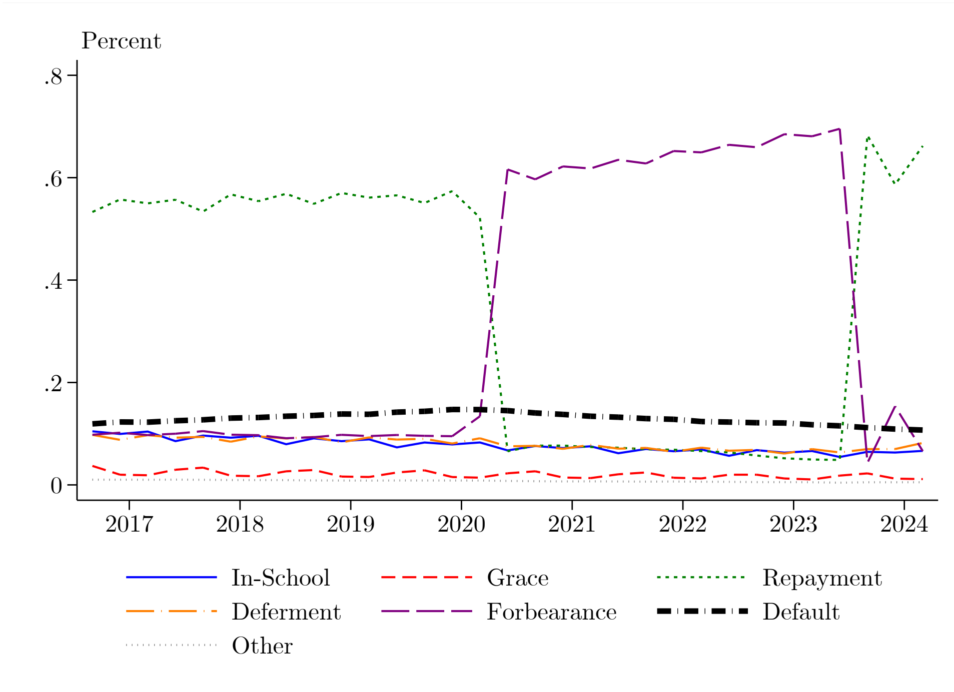
Appendix Figure A1. Percent of Balance 90+ Day Delinquency Rates by Loan Type



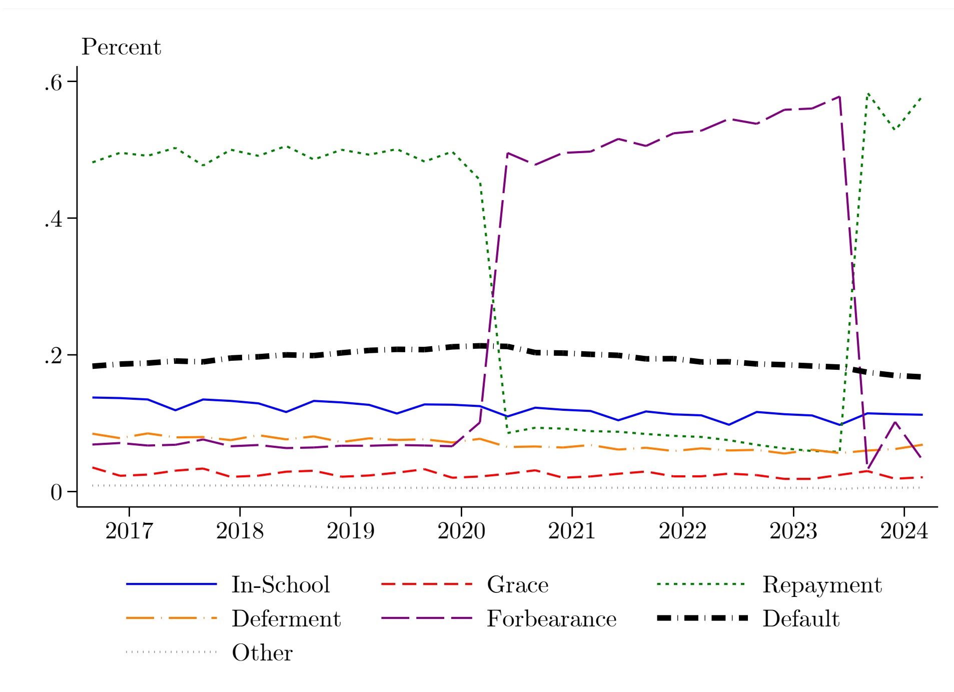
Notes: Figure illustrate the quarterly trends in the percent of balances reported as delinquent for 90 days or more. Data come from the Federal Reserve Bank of New York's Quarterly Report on Household Debt and Credit and are derived from the organization's Equifax consumer credit panel.

Appendix Figure A2. Trends in Federal Student Loan Portfolio by Status

(a) Balances

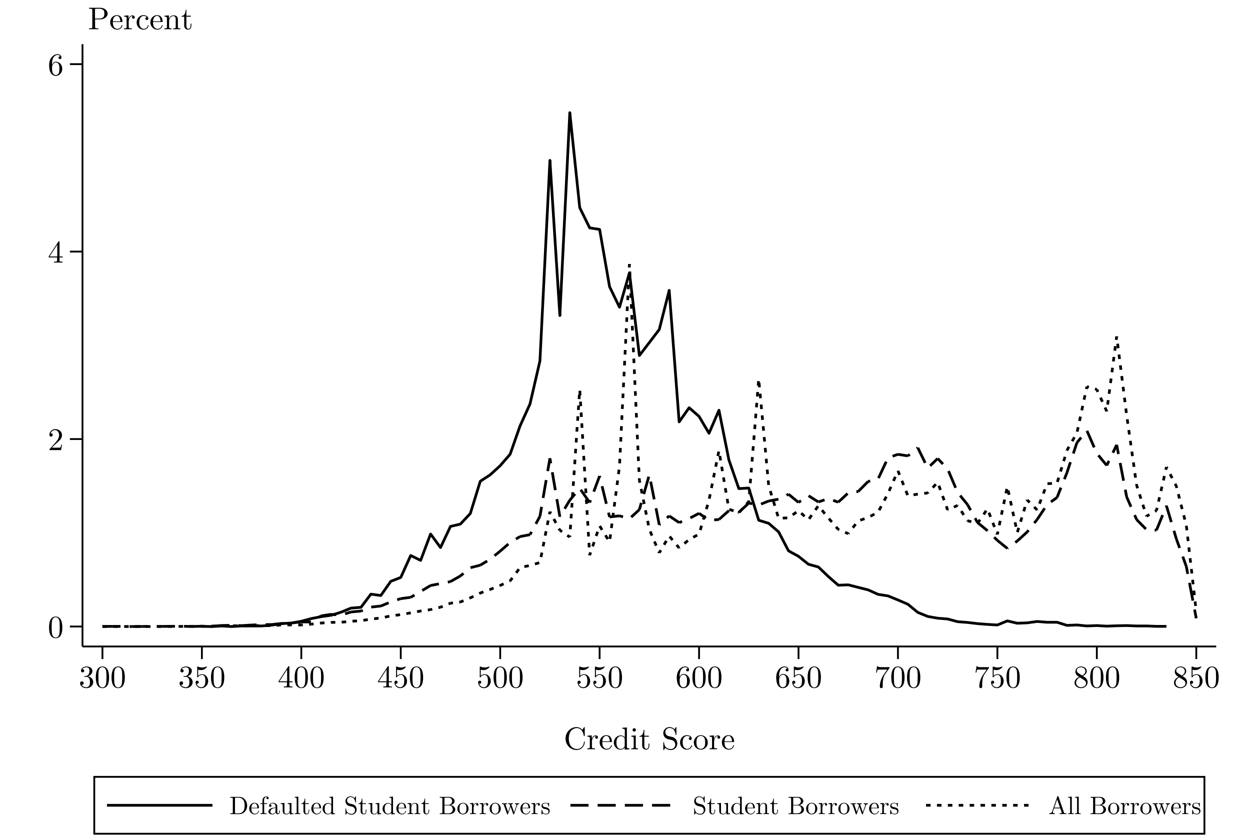


(b) Recipients



Notes: Figures illustrate the trends in the proportion federal student loan balances and recipients across loan statuses. Shares are relative to the aggregate outstanding ED-held and FFEL loans. Data come from Federal Student Aid portfolio reports which are tabulations from the National Student Loan Data System by fiscal year-quarter. Loan status categorizations are provided by Federal Student Aid.

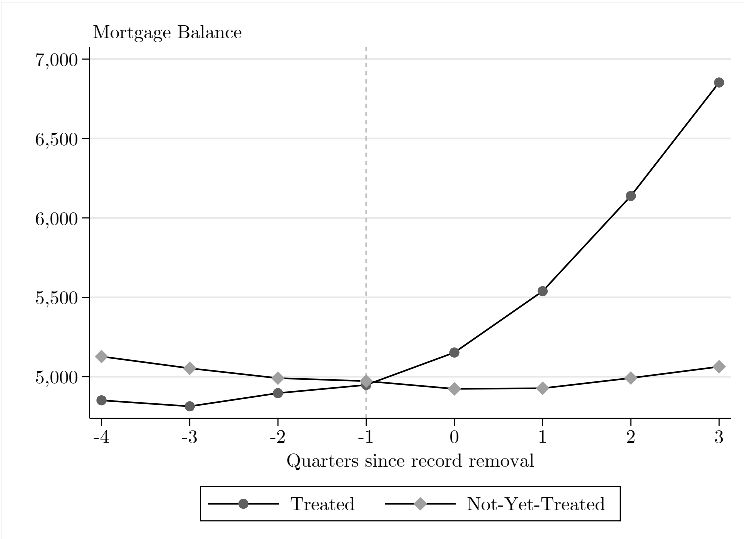
Appendix Figure A3. Credit Score Distribution



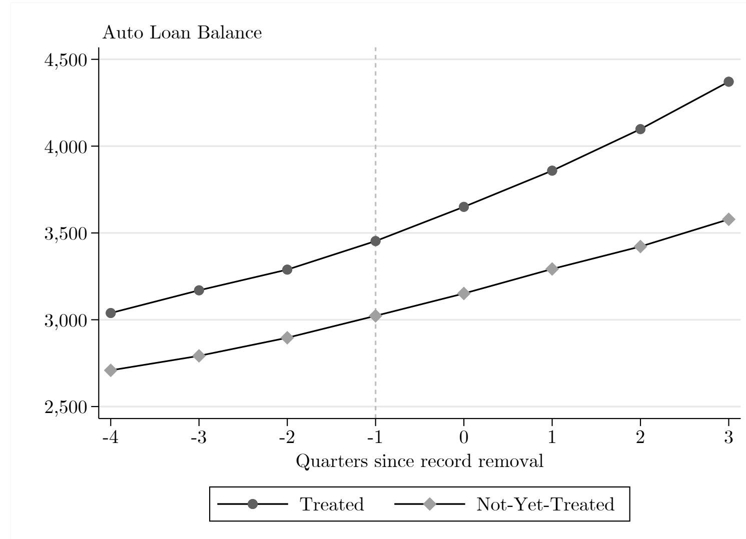
Notes: Horizontal axis is the range of credit scores. Higher scores signal higher creditworthiness. Defaulted student borrowers are student loan borrowers whose default record is removed between 2013q1 and 2021q3. These represent the sample of treated borrowers in my analytical sample. Credit score distribution is from the quarter before record removal. Student loan borrower and all borrower samples are approximately 10% random samples of each population in the credit panel. Random samples are drawn to match the calendar quarter distribution of the default sample. Borrowers are grouped into 5-point bins. For ease of comparison, distributions are illustrated as overlapping lines.

Appendix Figure A4. Trends in Loan Balances

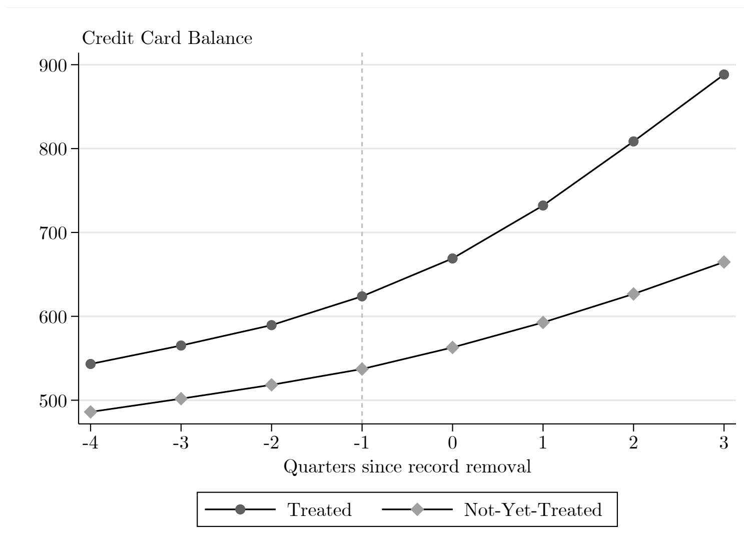
(a) Mortgage



(b) Auto Loan



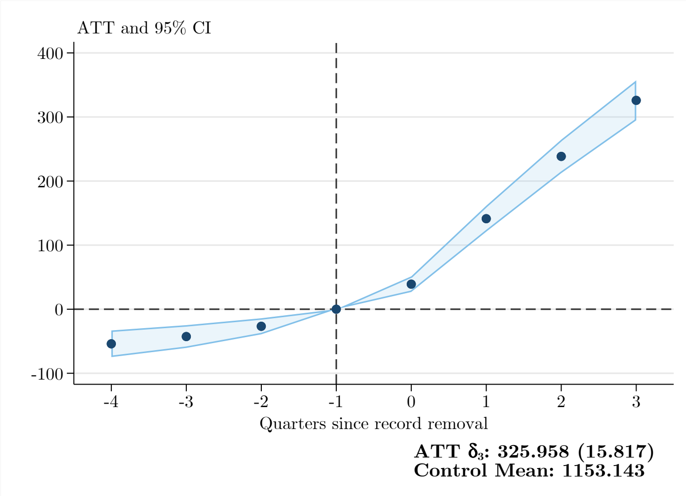
(c) Credit Card



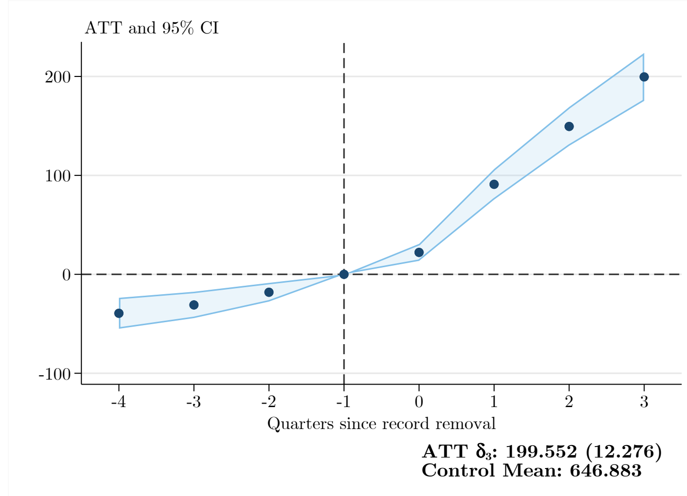
Notes: Vertical axis is the mean loan balance. Each panel illustrates trends in balances between treated and not-yet-treated borrowers across loan category.

Appendix Figure A5. Ability to Absorb Economic Shocks

(a) Credit limits

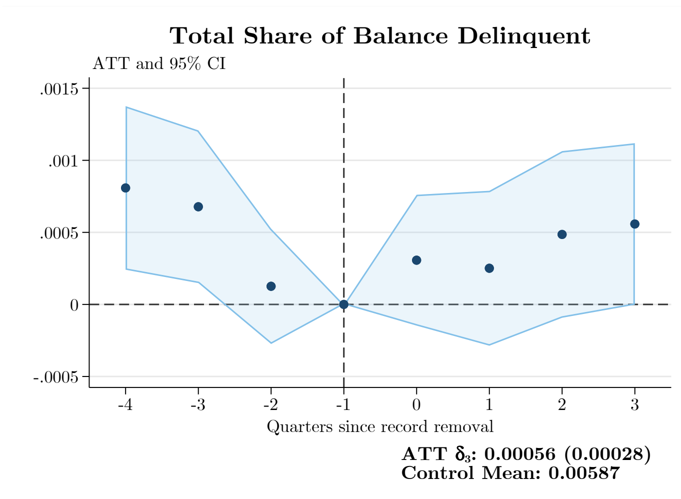


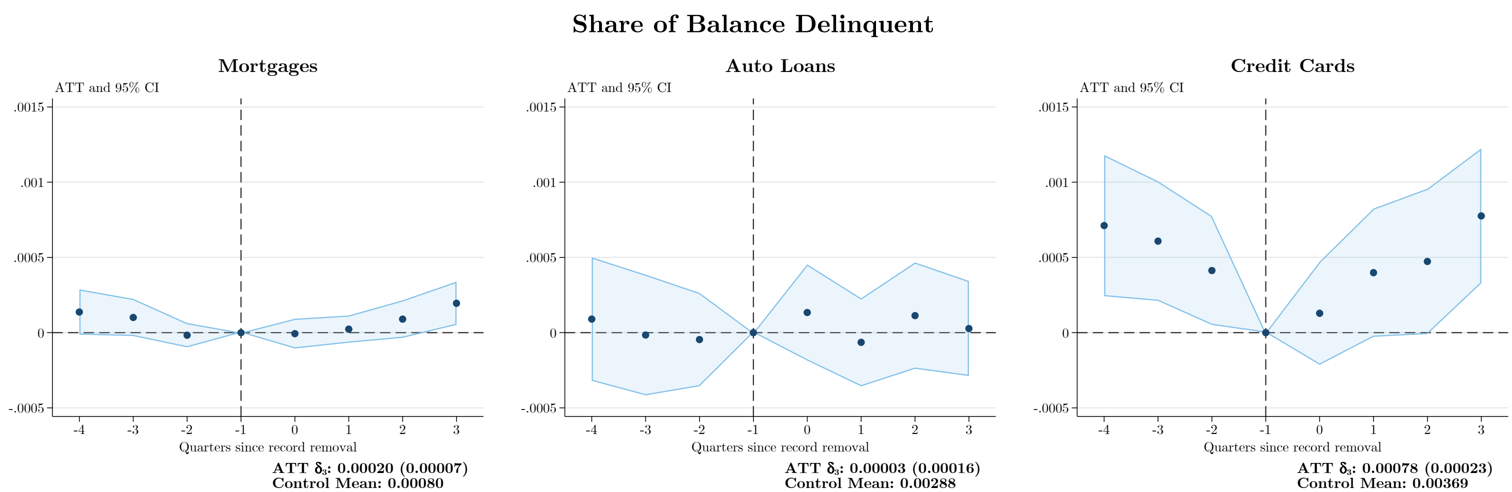
(b) Available credit



Notes: Horizontal axis is event time relative to the record removal event where $e = 0$ and is measured in calendar quarters. Each sub-figure plots estimated coefficients and 95% confidence intervals from the stacked event-study regressions described in the paper for the share of outstanding balances that are past due. The quarter right before the record removal event is the reference period.

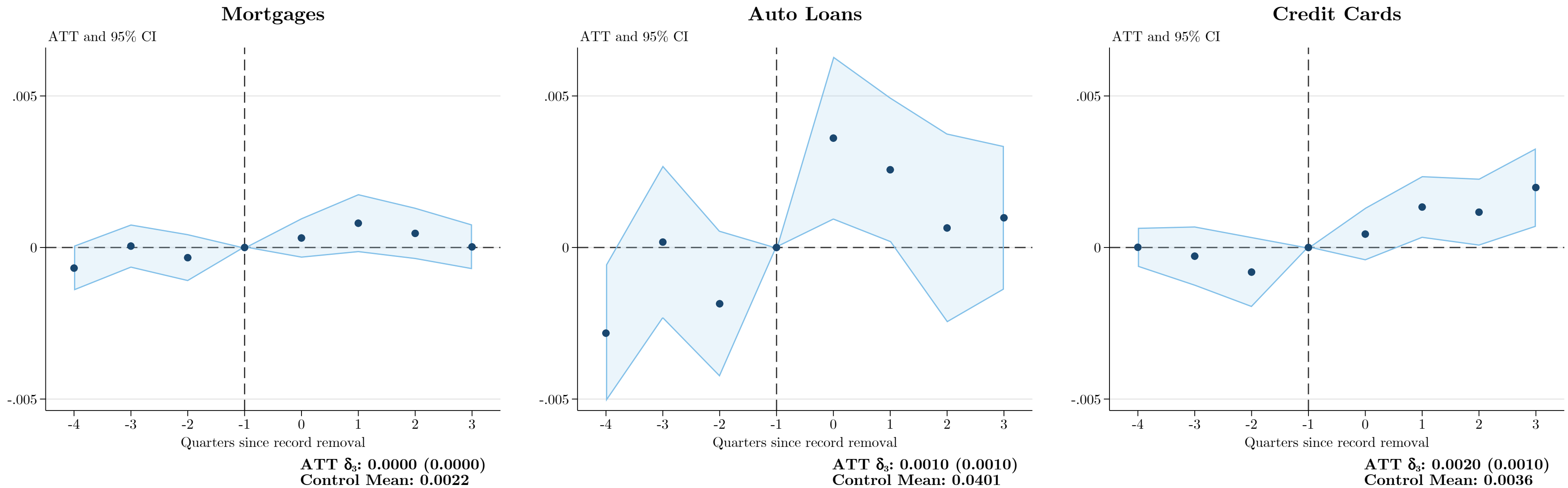
Appendix Figure A6. Delinquency





Notes: Horizontal axis is event time relative to the record removal event where $e = 0$ and is measured in calendar quarters. Each sub-figure plots estimated coefficients and 95% confidence intervals from the stacked event-study regressions described in the paper for the share of outstanding balances that are past due. The quarter right before the record removal event is the reference period.

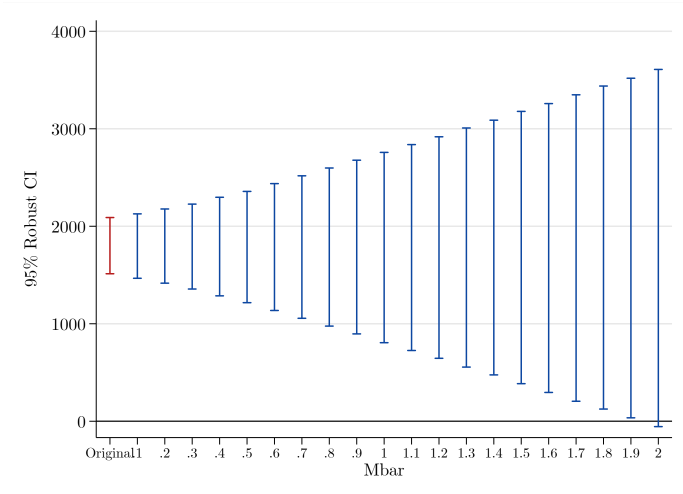
Appendix Figure A7. Credit Inquiries



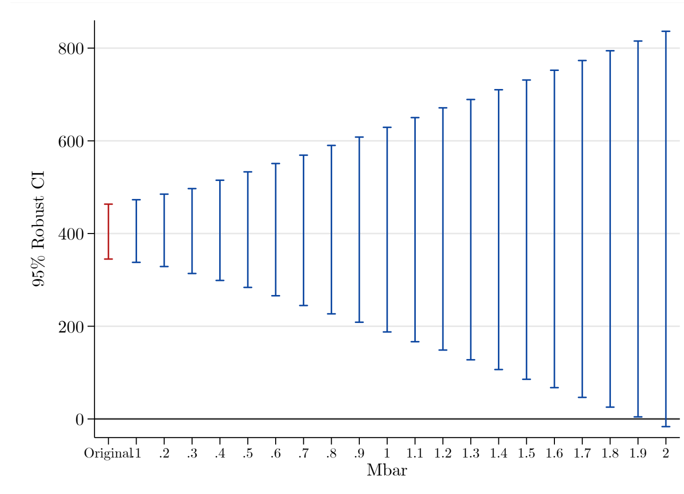
Notes: Horizontal axis is event time relative to the record removal event where $e = 0$ and is measured in calendar quarters. Each sub-figure plots estimated coefficients and 95% confidence intervals from the stacked event-study regressions described in the paper for balances, scheduled payments, and probability of having an open loan, separated by each major category of debt: mortgages, auto loans, and credit cards. The quarter right before the record removal event is the reference period. Parametric estimates measure the deviations from what would be expected if outcomes trended linearly from the pre-removal period.

Appendix Figure A8. Sensitivity Analysis for Main Estimates δ_3: Balances

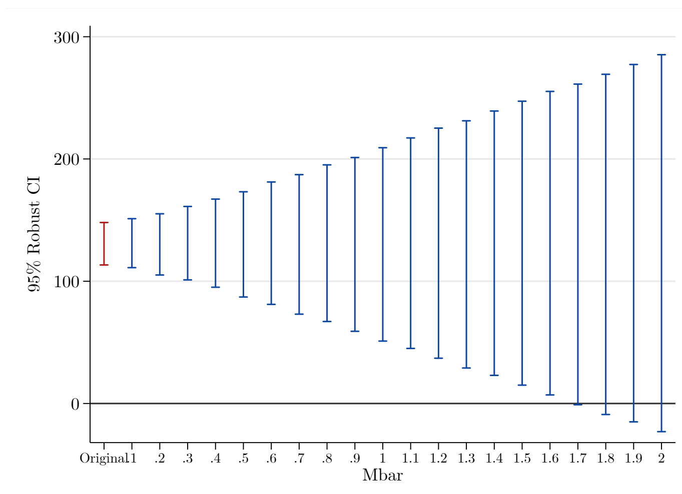
(a) Mortgage



(b) Auto Loan

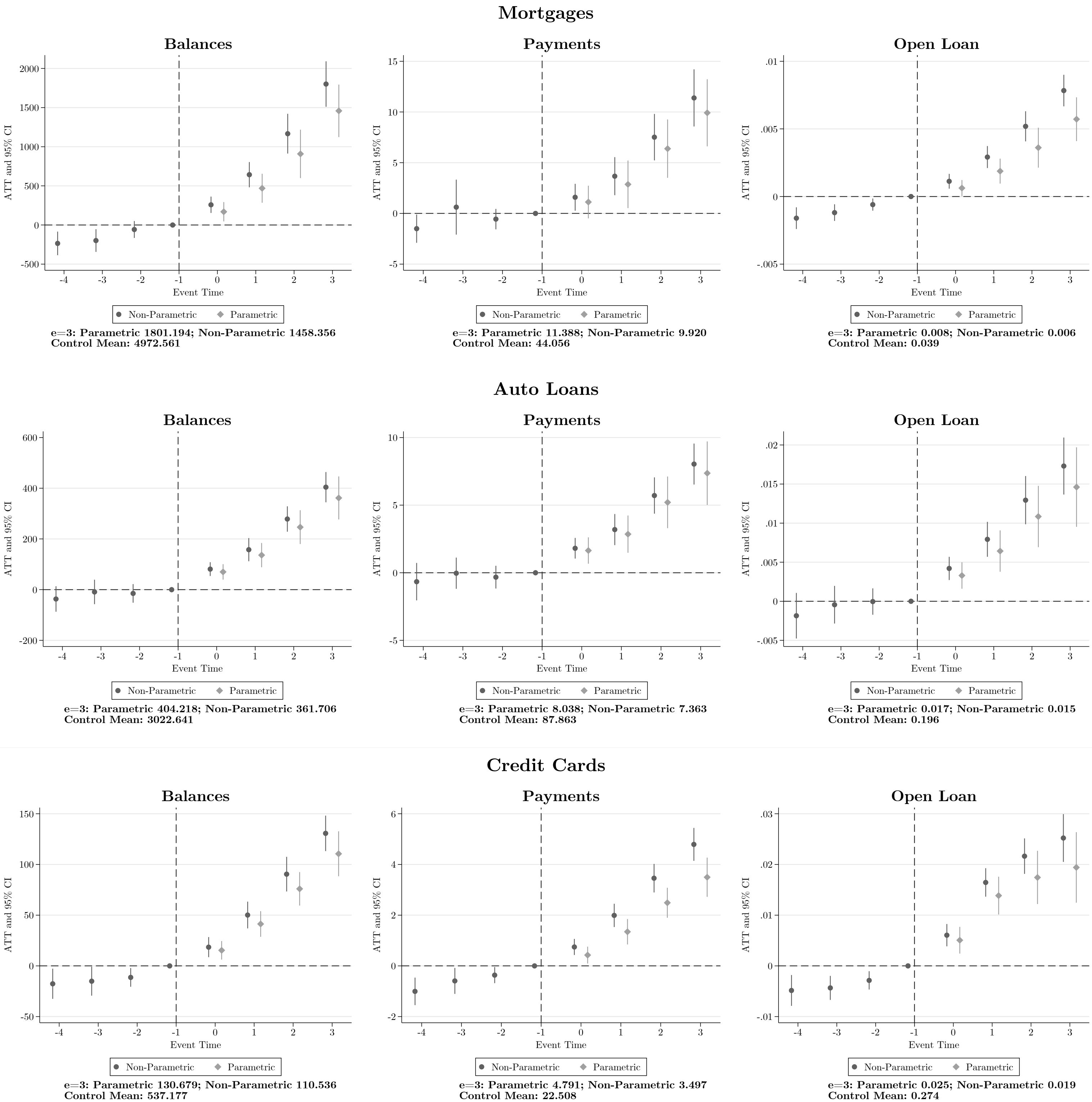


(c) Credit Card



Notes: Sensitivity tests are done for the δ_3 coefficients (fourth quarter without the default record). Estimates are calculated using the relative magnitude method described in Rambachan and Roth (2023) and implemented using the HonestDiD package in Stata. Estimates assume that there is a violation in parallel trends in the post-period that is as large as the maximum deviation in the pre-preiod multiplied by a constant $\bar{M}$.

Appendix Figure A9. Parametric and Non-Parametric Event-Study Estimates



Notes: Horizontal axis is event time relative to the record removal event where $e = 0$ and is measured in calendar quarters. Each sub-figure plots estimated coefficients and 95% confidence intervals from the stacked event-study regressions described in the paper for balances, scheduled payments, and probability of having an open loan, separated by each major category of debt. The quarter right before the record removal event is the reference period. Parametric estimates measure the deviations from what would be expected if outcomes trended linearly from the pre-removal period.

B Data Appendix

B.1 Data Construction

My primary data source is the University of California Consumer Credit Panel (UC-CCP), a nationally representative 2% sample of all individuals in the United States with credit files maintained by a major credit bureau. Credit bureau data can often contain measurement error, fragmented records, reporting lags, and stale information and so I follow recommendations from Gibbs et al. (2025) to ensure a clean and accurate sample.

The panel is dynamic, with approximately 5% of the sample refreshed each quarter as new consumers establish credit files and others are removed, for example, after passing away. The data span 2004 to 2024, covering approximately six million consumers per quarter. I begin my sample construction by identifying all borrowers with student loan tradelines appearing in the data between 2012 and 2024. I restrict the analysis to this period due to a change in how the credit bureau codes defaulted student loan debt in 2012, which is necessary to ensure consistency in measuring default status across cohorts.

From this initial sample, I apply several restrictions. First, I remove deceased individuals and borrowers with missing birth year information as these records are likely to have inaccurate debt information Gibbs et al. (2025). Second, I identify borrowers who have at least one student loan tradeline that enters default status during the sample period. Default status is identified using a payment code that indicates the loan has been transferred to collections. I then identify the sample of borrowers that have FCRA-generated removal of their student loan default record(s). I detail this process in the proceeding section.

The bulk of the analysis is done at the borrower-quarter level, and so I aggregate tradeline-level information to this level. I focus on active mortgage, auto loan, and credit card lines defined as open accounts and that have been updated by the data furnisher within the past year. Payment and balance information on these active tradelines are then aggregated. A borrower's total credit card balance, for example, is the sum of all open credit card tradeline balances in that quarter. My primary outcomes are credit scores, total balances, and scheduled payments. For borrowers with no reported tradelines in a given category, I assign a balance and scheduled payment of zero.

B.2 Identifying Student Loan Default Record Removals

A key objective in this paper is to measure how long student loan default records persist on credit reports, and then use this estimate as a proxy for whether borrowers' records drop due to FCRA or rehabilitation. It is useful to briefly summarize the credit reporting processes of federal student loans.

When a borrower takes out a loan they are assigned to a servicer, a company contracted by the federal government to manage and facilitate borrowers' repayment on the debt. These servicers are required to report balances, payments, and statuses on the loan to the three major national credit reporting agencies. These entities form the basis for the underlying data generating process in my sample. Leading up to default, the servicer reports the successive delinquency statuses (90-days past due, 120-days past due, etc.) until 270 or 360 days, at which point, the loan is considered in default. The loan is then transferred to a default servicer and appears in the credit reporting data with a new identifier. It is common for these transfers to result in reporting lags between when a loan is closed with one servicer and opened with the new servicer (Gibbs et al., 2025).

Using the sample of student borrowers between 2012 and 2024 with defaulted student loans, I measure the age of the defaulted tradeline using a combination of the number of quarters the tradeline is present in the data and the number of observed months of payment history.[24] In practice, age of defaulted loans that appear early in the panel (e.g., 2012) is necessarily calculated using the payment history information as there are not a sufficient number of prior quarters to accurately measure age.[25] I then drop record record removal events that occur after 2022q3 to prevent measurement error induced by aforementioned federal accelerated rehabilitation program that began in 2022q4. Finally, I only consider record drop events in which, after the event, no new default record appears within the year to prevent assigning removals that are actually transfers.

I find that default information affects student borrowers through their credit history for many years. Relatively few student debtors appear to proactively and successfully use avenues to remedy delinquency flags on their credit reports, and so derogatory marks persist until they are removed under statutorily determined time horizons. In Appendix Figure B1, I plot the number of quarters that a student loan default record is observed on an individual's credit report. There is a distinct mass around 22 quarters, or 5.50 years. I find that over 91 percent of borrowers in my sample have a default record lasting five or more years on their report and 83 percent between 5.25 and 5.75 years. This aligns with the observed decline in default records in the 2012 cohort. The decline in the share of borrowers with default records

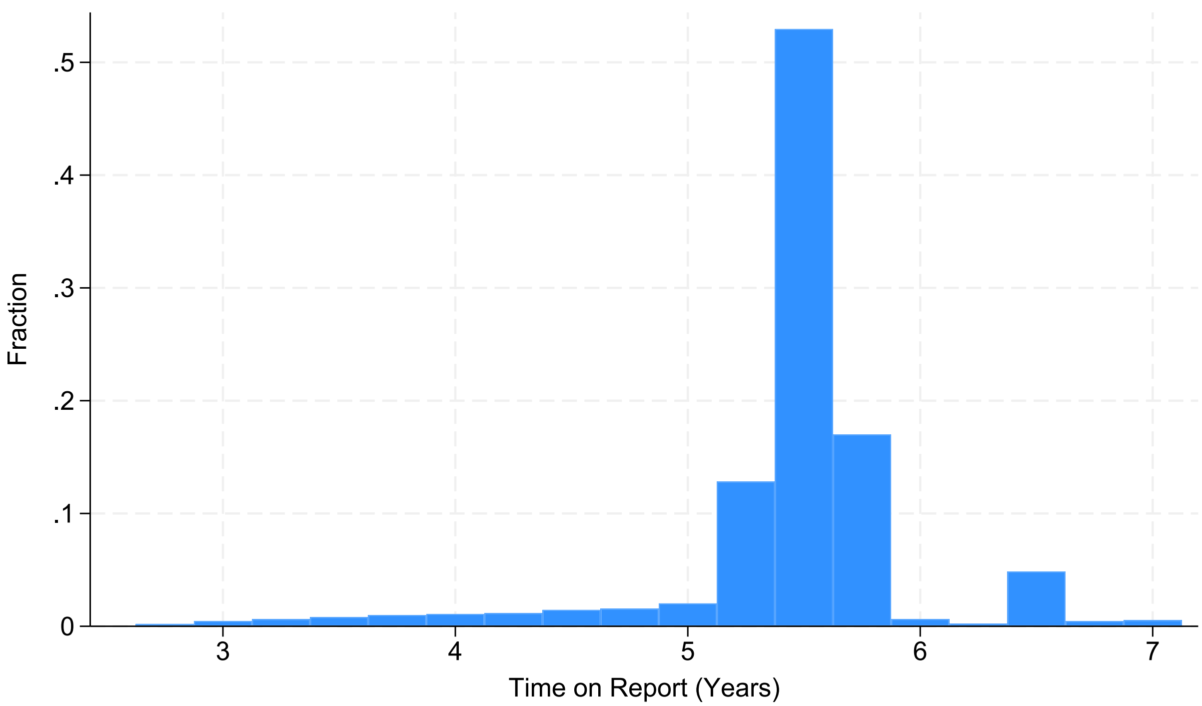
[24]I do not link default tradelines to their original delinquent tradelines to reduce processing time, and in practice, linking these loans across transfers using matched origination dates and amounts appears to leave a majority of the default records without a linked delinquent record. Origination dates are typically a straightforward to measure the age of any loan. However, default records appear to have some inconsistency in how this is recorded with the origination date of the original student loan matching when the original student loan was taken out for school or some later date, perhaps when the defaulted tradeline was created.

[25]Tradelines have a payment grid variable that shows up to 84 months of payment statuses. Thus, if these loans have been updated by servicers recently, age of the record can be approximated by the count of observed statuses within the grid.

follows seven years from the initial delinquencies, rather than the initial rise in defaults which happens approximately 5.50 years prior. This is consistent with credit reporting requirements outlined by the Higher Education Act with record age-off for defaults back-dated to their original delinquencies. Thus, this approximation around the 5.50 year mark makes sense allowing for 270-360 days in delinquency and then reporting lags between transfers to default servicers, at which point, I observe the defaulted tradeline.

These results imply that, from a credit reporting standpoint, the seven-year threshold applies to the vast majority of defaulted borrowers rather than early record removals through the use of rehabilitation. This presents an opportunity for a natural experiment. While the initial decision to default is likely endogenous, the precise timing of when this happens is assumed to not be strategically calculated with the FCRA rule in mind.

Appendix Figure B1. Persistence of Student Loan Default Records on Credit Reports



Notes: The horizontal axis shows the length of time a borrower's default record stays on their credit report. Underlying data is measured in calendar quarters and converted to years for illustration. Borrower is the level of observation. A borrower's longest record length is used for those with multiple default record removals.