Student Debt Cancellation: Evidence from Credit Reporting Panel Data *

Eduard Nilaj † Laura Beamer‡ Sultana Fouzia§ Jack Landry¶

Sérgio Pinto || Marshall Steinbaum ** Robert Stutchbury ††

October 27, 2026

We estimate the effect of student debt cancellation on borrower financial outcomes using a nationally representative credit panel and a staggered difference-in-differences design, comparing 15 quarterly cohorts of borrowers whose student loans were canceled between 2021 and 2024 to borrowers with outstanding student loans. Cancellation raises the homeownership rate by 1.2 percentage points, averaged over the four years after cancellation, and the resulting increase in mortgage debt comes almost entirely from entry into homeownership rather than from larger balances among existing owners; non-mortgage debt among borrowers who held it does not change. Credit scores rise by 17 points, the share of borrowers with a score of 700 or above rises by 7 percentage points, credit card limits rise by 4 percent, and delinquent non-student balances fall. Because cancellation reduces student debt while holding educational attainment fixed, these estimates isolate the effect of the debt itself, which confounds most estimates in the literature.

*We have departed from the alphabetic norm for author order in acknowledgment of Eduard Nilaj's efforts on this project, which have unanimously earned him priority of authorship. We thank the California Policy Lab for hosting, documenting, and facilitating access to the University of California Consumer Credit Panel.

†The Century Foundation and Protect Borrowers. Protect Borrowers is counsel to the AFT in litigation against MOHELA and the U.S. Department of Education concerning the programs studied here. It had no role in the design, analysis, drafting, or review of this paper.

‡Independent.

§Independent.

¶Independent.

||University of Liverpool and Instituto Universitário de Lisboa (ISCTE-IUL), DINAMIA'CET, Lisbon, Portugal.

**University of Utah. Steinbaum is retained as plaintiff's economics expert in *Maldonado v. MOHELA* referenced herein.

††University of Utah.

Following the Biden administration's proposal to cancel between $10,000 and $20,000 in outstanding student debt for most borrowers in 2022, interest in the topic of student debt forgiveness has grown among academics, policymakers, and the public. Despite this heightened interest, there is limited empirical evidence on the economic impact of loan cancellation, leading to a lack of consensus about its economic effect, as evidenced by the 2023 Supreme Court ruling in *Biden v. Nebraska*. This paper addresses a gap in the literature: it is one of the first to examine the retrospective effects of the large-scale student loan cancellations that occurred between 2021 and 2024, rather than evaluating the prospective effect of a more general cancellation policy, as much of the existing literature does.[1] In contrast, our analysis compares borrowers whose loans were canceled with a control group of borrowers whose loans remained active.

This paper's primary contribution is providing comprehensive empirical evidence on how student loan cancellation impacts a range of financial outcomes, including homeownership, mortgage and non-mortgage debt, credit scores, credit card limits, and delinquency. Using credit panel data, we track individual borrowers' financial outcomes before and after loan cancellation to identify causal effects. We address the challenges of evaluating staggered policy interventions by employing a cohort-specific difference-in-differences (DiD) design, using the estimator developed by Sun and Abraham (2021). We identify canceled loans directly from patterns in balance and payment history in the credit panel data and construct a control group of borrowers with active student loans over the same period.

Beyond cancellation specifically, the financial impact of student debt has received increasing attention from researchers as rising debt levels have become a key issue for policymakers and the public. Findings tend to fall into two groups: those that use variation in access to student debt to estimate the effect of debt on outcomes find it has a positive effect (Black et al., 2023), while findings that use variation in the cost of attendance to vary the amount of student debt

[1]For instance, Catherine and Yannelis (2023) examines the prospective distributional effect of student loan forgiveness assuming full take-up of Income-Driven Repayment plans by eligible borrowers.

find it has negative effects (Mezza et al., 2020; Bleemer et al., 2021). The likely omitted variable in both cases is educational attainment: increased access to debt causes increased attainment, which may be positive for borrower financial outcomes, while increased cost of attendance reduces attainment while increasing debt, making it difficult to disentangle the two channels. In this paper, we are able to vary the amount of student debt (through permanent cancellation) while holding attainment constant.

Our findings reveal improvements in borrowers' financial outcomes following student loan cancellation along two dimensions. The first is housing. Averaged over the four years after cancellation, borrowers whose loans were canceled are 1.2 percentage points more likely to own a home, a 2.6 percent increase over the pre-cancellation rate of 45.6 percent. The event study estimates suggest that the effect rises over time during the first two years, by when treated borrowers are 2 percentage points (4.5 percent) more likely to be homeowners than control borrowers; in later periods, we cannot separate the effect of time from the fact that progressively fewer cohorts are observed. Mortgage debt rises as well, almost entirely through an extensive-margin effect. Among borrowers who already held a mortgage in 2019, mortgage balances rise by only 1.4 percent, so nearly all of the aggregate increase comes from borrowers who entered homeownership after cancellation. Meanwhile, credit card, auto loans, and other non-mortgage, non-student debt did not increase post-cancellation. The second dimension of improvement is credit health: credit scores rise by 17 points, the share of borrowers with a score of 700 or above rises by 7 percentage points, credit card limits among existing cardholders rise by 4 percent, and delinquent non-student balances fall. The credit score gain is present in every cohort and is largest for the cohorts canceled from mid-2023 onward, who have the lowest credit scores pre-cancellation. The credit score and credit card limit outcomes show a modest upward trend before cancellation, which we attribute to the pandemic payment pause, from which treated borrowers benefited more than the control group. We quantify the sensitivity of each estimate to its pre-trend in Section II and find that those results are robust to a linear extrapolation of it.

The magnitude of the homeownership effect varies across cohorts, though its sign does not, and the credit score effect is positive and statistically significant in all fifteen cohorts. Cohort-specific estimates for homeownership are 5.8, 4.4, and 2.1 percentage points for the three cohorts whose loans were canceled in 2021, and under 2 percentage points for most later cohorts. Part of this difference is mechanical: each of the 2021 cohorts is observed for 16 quarters after cancellation, while the 2024 cohorts are observed for 4 to 7; if the effect tends to grow with time, the estimated effects for later-treated cohorts, with short post-treatment periods, would be smaller. However, another potential explanation is that the earlier-canceled borrowers benefited from lower interest rates for new home mortgages, which began increasing in 2022. Both of these sources of variation in the homeownership effect across cohorts are due to the timing of cancellation, not cohort composition.

Our study is most closely related to Dinerstein et al. (2025), who also use credit reporting data to estimate the effects of student loan forgiveness. We differ in three key respects: (i) we apply a more conservative approach to identifying cancellations that yields fewer false positives; (ii) we use a control group that is limited to borrowers with active loans during the payment pause, whose most recent loans originated in 2013 or earlier, and who did not benefit from cancellation during the study period, to better estimate the counterfactual for canceled borrowers; (iii) we examine a longer horizon of up to four years post-cancellation, rather than six months. Examining the pandemic-era student loan repayment pause – as opposed to the outright cancellation that is the focus of both this paper and Dinerstein et al. (2025) – Dinerstein, Yannelis and Chen (2024) show the payment pause raised credit scores and enabled borrowers to take on additional consumer debt.[2] Dinerstein et al. (2025) interpret the difference in the two papers' findings as cancellation having little marginal effect beyond eliminating payments; we disagree, for reasons discussed at length below.

Our findings also appear to conflict with Di Maggio, Kalda and Yao (2026), who study the

[2] Collier et al. (2024*a*) and Collier et al. (2024*b*) come to broadly similar conclusions using a range of methodologies and, additionally, measuring borrower subjective wellbeing.

discharge of private student debt for defaulted borrowers. They document de-leveraging—lower balances and reduced mortgage exposure—rather than the expanded homeownership and credit access we find. But this apparent divergence is to be expected given differences between the two settings and their respective treated (and control) populations. Both papers agree on the first-order effect: student debt relief improves borrowers' financial health, raising credit scores. What differs is how borrowers use that improvement, which likely depends on where they sit relative to their credit constraints. The sample in Di Maggio, Kalda and Yao (2026) consists of severely distressed defaulters who receive relief covering just over half of their debt, measured against a counterfactual of borrowers subject to wage garnishment; for such borrowers, the response to a windfall was to pay down obligations. The beneficiaries studied in this paper are comparatively less constrained, receive a larger discharge covering a greater share of their debt, and are measured against a control group of borrowers with active loans; they are also older and plausibly closer to the stage of the life cycle at which households buy homes. For them, the improvement in financial health relaxes the underwriting constraints—debt-to-income ratios, credit scores, and the ability to save for a down payment—that may have previously kept homeownership out of reach. The two sets of results, therefore, reflect a common mechanism operating on populations at different points relative to the credit constraints that govern home purchase.

A similar tension appears with Folch and Mazzone (2026), who find no effect of student debt on homeownership. This divergence is likely attributable to sample composition: their sample is restricted to graduates of four-year public and private nonprofit institutions, excluding college dropouts as well as graduates of two-year and for-profit institutions. The authors themselves note that these restrictions make their sample positively selected on academic ability and negatively selected on debt balances, relative to the overall population of student borrowers. They also point out that the effect of student debt on homeownership should be more negative once the full sample of college enrollees is considered, consistent with the positive effect of student debt cancellation that we identify.

This paper is organized as follows: Section I details the dataset used in this study, including the process for identifying federal loan cancellations, and presents cohort summary statistics. Section II describes the empirical strategy and presents the main regression estimates. Section III interprets these results and places them in the context of existing research on student debt and financial health. Finally, Section IV summarizes the findings and implications. All tables and figures are included at the end of the paper .

I. Data

We use data from the University of California Consumer Credit Panel (UC-CCP), a longitudinal dataset covering the period from 2004 to the present. UC-CCP provides anonymized credit records for a 2 percent nationally-representative sample of U.S. adults with credit histories. It includes detailed demographic and geographic information, credit scores, and tradeline-level data on loans and credit, such as payment history, credit limits, balances, and loan statuses.

We restrict the dataset to borrowers with student loan tradelines between 2017 and 2025. The quarterly frequency lets us date each loan cancellation to the specific quarter in which the balance is written down on borrowers' credit reports, which both sharpens the event timing in our staggered difference-in-differences design and allows us to align our cancellation series with the timing of the Department of Education's discharge announcements. Subsection I.A describes the major federal loan cancellation programs under which loans were canceled between 2021 and 2024. Subsection I.B outlines our approach to identifying canceled loans and defining the treatment group and a control group that represents a credible counterfactual for estimating the effect of student debt cancellation. Subsection I.C reports baseline characteristics and trends for these groups by cancellation cohort, including student debt balance, homeownership rate, and credit score, as well as demographic characteristics.

A. *Federal Student Loan Cancellation Programs*

The federal student loan programs have long contained provisions under which loans are canceled, either as a reward for repayment and public service or because of the circumstances of the borrower or the institution. Before 2021 these provisions were administered so restrictively that they produced comparatively few discharges, and nearly all of the cancellation we study reflects the Biden administration's decision, beginning in 2021, to deliver relief under the terms of existing programs. We describe the programs in two groups, those that cancel debt on the basis of repayment history and those that cancel it on the basis of the borrower's or the institution's circumstances.

The Public Service Loan Forgiveness (PSLF) program, created in 2007, cancels the remaining balance on federal Direct Loans for borrowers who have made 120 qualifying monthly payments under an income-driven or ten-year standard repayment plan while working full-time for a government or nonprofit employer (Federal Student Aid, 2024*e*; Student Borrower Protection Center, 2024). Payments on loans made under the older Federal Family Education Loan (FFEL) and Perkins programs, and payments made under other repayment plans, did not count, and the program had forgiven the loans of only a few thousand borrowers by 2021.[3] Under the limited PSLF waiver, in effect from October 2021 through October 2022, the Department counted all prior payments regardless of loan type or repayment plan, provided the borrower consolidated any FFEL or Perkins loans into a Direct Loan and certified qualifying employment before the deadline (U.S. Department of Education, 2021*b*). Consolidation creates a new tradeline on the borrower's credit report, a feature of the waiver that bears on our identification of canceled loans in Section I.B. Processing of waiver applications continued into 2023, and the account adjustment described next also credited additional months toward PSLF, producing a second surge of PSLF discharges in the spring of 2023.

[3]The Temporary Expanded PSLF (TEPSLF) program, created by Congress in 2018, offered narrow relief to borrowers who had been in the wrong repayment plan, and the Teacher Loan Forgiveness program cancels up to $17,500 for teachers who complete five years in a low-income school. Both are small relative to the programs described in the text; see Hegji (2023).

Income-driven repayment (IDR) plans, available since 1994 and expanded in 2009, 2012, and 2015, set monthly payments as a share of discretionary income and cancel any remaining balance after 20 or 25 years of qualifying payments. Because IDR payments often fall short of accruing interest, balances in long-term IDR tend to grow rather than amortize. The first borrowers reached the forgiveness threshold in 2019, yet servicers had not tracked qualifying payments, and as of June 2021 the Department had forgiven a total of 157 loans under IDR (U.S. Government Accountability Office, 2022). The IDR account adjustment, announced in April 2022, was a one-time recount of every borrower's progress toward forgiveness: the Department credited any month in a repayment status regardless of plan or payment amount, months in long-term forbearance, and most months in deferment before 2013, and applied the resulting credit toward both IDR forgiveness and PSLF. (U.S. Department of Education, 2022; Federal Student Aid, 2024*d*).

The adjustment was implemented in two phases. Credit toward PSLF was applied first, beginning in late 2022, and it accounts for part of the spring 2023 surge in PSLF discharges noted above. Discharges for borrowers who reached the 20- or 25-year IDR threshold (and whose employment history putatively did not qualify them for PSLF) began in July 2023, when the Department announced a first wave of $39 billion for 804,000 borrowers, and continued in further waves through 2024; borrowers with commercially held FFEL loans had to consolidate into a Direct Loan by June 2024 to benefit (U.S. Department of Education, 2023). The Saving on a Valuable Education (SAVE) plan, introduced in August 2023 as a more generous successor to the 2015-vintage IDR plan, shortened the time to forgiveness to as little as ten years for borrowers with original balances of $12,000 or less, and the Department discharged $1.2 billion for 153,000 such borrowers in February 2024 before the Eighth Circuit enjoined the plan in the summer of 2024 and enrolled borrowers were placed in forbearance (U.S. Department of Education, 2024; U.S. Court of Appeals for the Eighth Circuit, 2024).

A second set of programs cancels loans because of the institution the borrower attended or the borrower's own circumstances, without regard to repayment history. Borrower Defense

to Repayment cancels the loans of borrowers whose institutions engaged in misconduct, such as misrepresenting the value of their programs; its beneficiaries are overwhelmingly former students of for-profit institutions (Federal Student Aid, 2024*a*). Although the authority dates to 1994, the great majority of discharges have come since 2021, through group discharges for former students of Corinthian Colleges, ITT Technical Institute, and other collapsed chains, and through the settlement of *Sweet v. Cardona*, approved in November 2022, which required the Department to discharge the loans of roughly 200,000 borrowers whose applications it had left pending (U.S. District Court for the Northern District of California, 2022). Closed School Discharge cancels the loans of borrowers who were enrolled, or had recently withdrawn, when their institution closed before they could complete their program (Smith, 2021; Federal Student Aid, 2024*b*); the same for-profit chain closures generated both types of claim, and the Department discharged $1.1 billion for former ITT students on this basis in 2021. Total and Permanent Disability (TPD) discharge cancels the loans of borrowers who cannot work because of a disability. Before 2021, eligible borrowers had to apply, and many who were approved later had their loans reinstated for failing to document their earnings during a three-year monitoring period. Beginning in August 2021, the Department discharged loans automatically for borrowers identified through a data match with the Social Security Administration and suspended, then eliminated, the monitoring requirement (U.S. Department of Education, 2021*a*; Federal Student Aid, 2024*f*). Federal loans are also discharged upon the borrower's death and, under a heightened undue-hardship standard, in bankruptcy; because these events coincide with changes in the other balance-sheet outcomes we study, we treat them separately from cancellation in subsection I.B.

It's worth briefly mentioning three further, concurrent policies that we do not study directly in this paper but whose existence manifests on borrowers' credit reports and therefore informs the implementation of the policy evaluation we do undertake. First, the administration's August 2022 plan to cancel $10,000 to $20,000 for most borrowers was struck down by the Supreme Court in June 2023 before any loans were discharged (Supreme Court of the United

States, 2023); it therefore contributes nothing to the cancellations we observe, and the program-based relief described above is what the administration expanded in its place. Second, the pandemic payment pause, from March 2020 through September 2023, set interest to zero and suspended required payments on Department-held loans, so that balances were frozen rather than amortizing; subsection I.B explains how we distinguish paused balances from cancellations. Third, the Fresh Start initiative, announced in April 2022 and in effect until October 2024, allowed borrowers in default to return their loans to good standing, and during that window the Department reported defaulted loans to the credit bureaus as current and deleted the record of default (Federal Student Aid, 2024*c*). Fresh Start thus changed how defaulted loans appear in credit files during our sample period, which bears on the Borrower Defense and TPD discharges that disproportionately reached borrowers in default.

B. Identifying Canceled Loans and Constructing the Analysis Sample

To analyze the impact of student loan cancellation on financial outcomes, we first need to identify canceled loans. To do so, we work with a quarterly panel of the credit records of sampled borrowers, which lets us observe the specific quarter in which a loan balance reaches zero. Credit files do not contain a dedicated code for administrative cancellation, so we infer it from the loan's balance history, payment record, and account-status indicators in the quarters preceding the zero balance. We classify each individual loan (tradeline, in credit bureau parlance) and then aggregate to the borrower. This subsection proceeds in eight steps. Section I.B.i explains what the credit panel does and does not record about actual payments made on student loans, which determines what evidence is available. Section I.B.ii lays out the classification of zero-balance loans, and Section I.B.iii the criteria for cancellation within it. Section I.B.iv then reports what the balance and payment histories of zero-balance loans look like, which is what justifies classifying them as we do rather than as Dinerstein et al. (2025) do. Section I.B.v assigns canceled borrowers to quarterly treatment cohorts, and Section I.B.vi defines the control group and the exclusions applied to both. Section I.B.vii compares the cohorts' pre-cancellation

characteristics with the timing of the relief programs in Section I.A to establish which programs each cohort plausibly reflects. Section I.B.viii describes the estimation sample and reconciles the cancellation we identify with the totals the Department of Education has announced.

i. Payment Reporting in the Credit Panel

The most direct way to distinguish a loan that was repaid from one that was canceled would be to compare the payments the borrower made with the loan balance that was extinguished. The credit panel does not permit this comparison for most loans.[4] Furnishers report each tradeline's balance every month, but not all servicers report the amounts actually paid. Among the student loans in our sample whose balance goes from positive to zero between 2021 and 2024, a payment amount is reported in the quarter of the zero-balance event for only about 2 percent, and the payments reported over the preceding two years are large enough to account for the extinguished balance for only 4.3 percent.[5] We believe that the payments that are reported are accurate, so when we observe payments sufficient to retire a balance we conclude that the loan was repaid. The converse inference is not available. The absence of reported payments is the ordinary condition of a tradeline in the panel and carries no information about whether payments were made, so it cannot serve as evidence that a loan was canceled. We therefore rely primarily on the trajectory of the balance over the eight quarters before it reaches zero, together with the account-condition and account-status codes that furnishers do report, and use reported payments as corroborating evidence where they exist.

ii. Classifying Zero-Balance Loans

Throughout, we consider only terminal zero-balance events, meaning loans whose balance reaches zero and does not subsequently return to a positive value, so as not to mistake transient

[4]The panel does include the monthly payment *owed* for the vast majority of tradelines, but not the payments that were actually *made*. The latter is what's at issue in this section.

[5]The 4.3 percent figure counts a loan as documented repaid if payments reported over quarters $t = -8$ to $t = 0$ sum to at least 90 percent of the balance at $t = -8$; the share ranges from 2.3 to 8.3 percent as that threshold moves from 100 to 75 percent.

reporting gaps, or balances that were temporarily reduced and later reinstated, for permanent cancellations.[6] We assign each terminal zero-balance loan to one of seven categories, applied in a fixed order of precedence so that a loan meeting the criteria for an earlier category is not reconsidered for a later one: (1) insufficient history, (2) refinanced or transferred, (3) discharged in bankruptcy or charged off, (4) a residual "relief candidate" category, (5) repaid, (6) canceled, or (7) unknown.

A loan has insufficient history if the borrower has fewer than 48 months of observed student-borrowing history before the loan first reaches a zero balance, measured from the borrower's earliest observed loan origination, or if a new tradeline appears in the borrower's record in the quarter the balance reaches zero, which is the signature of a consolidation or transfer rather than an extinguished balance. Such loans tell us nothing about cancellation and are set aside. Loans discharged in bankruptcy or charged off are identified from the account-condition and status codes for bankruptcy and charge-off, together with the codes for other derogatory terminal exits such as foreclosure and repossession.[7] Loans are categorized as refinanced or transferred on the basis of the account-status codes for transfer and refinancing, and additionally whenever the individual loan reaches zero but the borrower's aggregate student loan balance persists at 75 percent or more of its pre-zero level in subsequent quarters, since this likely reflects a balance moved to a new servicer or into a new loan rather than one that was extinguished. We measure persistence across the borrower's federal and private student loans together, so that a federal balance refinanced into a private loan is recognized as a transfer, and we apply a lower threshold when the persisting balance is carried by a new private loan, because such refinancing is typically for less than the full outstanding amount.

Loans carrying furnisher codes for settlement for less than the full balance, or for a gov-

[6] This restriction also excludes loans enrolled in the SAVE plan whose anticipated forgiveness was enjoined by the courts in 2024. Those balances were frozen rather than discharged and therefore do not terminate at zero in our data.

[7] We use the account-condition and enhanced-status codes for Chapter 7, 11, 12, and 13 bankruptcy and for charge-off. Although charged-off loans differ legally from those discharged through bankruptcy, with distinct impacts on the borrower's credit record, the financial characteristics of the loans and the borrowers in both cases are sufficiently similar that we group them together for the purposes of this study.

ernment claim on the insured portion of a defaulted loan, form the residual "relief candidate" category, which we treat as described in Section I.B.iii. A loan is categorized as repaid if the payments reported in the preceding quarters are large enough to account for the balance, if the balance declined relative to its origination amount over the preceding four and eight quarters, which is the trajectory of a loan amortizing on schedule and, as Section I.B.iv shows, the trajectory of nearly every loan whose repayment is documented, or if the balance remaining before the zero is a small residual of the amount borrowed.[8] We refer to loans assigned to this category on the strength of their trajectory alone, without sufficient reported payments, as "uncertain repaid."

What remains after these four categories is the pool of cancellation candidates, to which the criteria in Section I.B.iii apply. Loans that meet the criteria for no category are "unknown" and excluded from the analysis entirely; we interpret them as likely refinanced or transferred, with no replacement loan found in the borrower's credit record. For borrowers with multiple loans reaching a zero balance, we classify each loan separately and do not require all of a borrower's loans to reach zero simultaneously, since administrative relief legitimately discharges a borrower's loans in more than one quarter, as when PSLF and the IDR account adjustment credit the same borrower months apart.

iii. Identifying Cancellation

Identifying canceled loans among the remaining zero-balance loans is complicated by the payment pause. From March 2020 through September 2023, most federal student loans carried a zero interest rate and payments were voluntary, so most balances were stagnant regardless of whether the borrower had been repaying on schedule or was on track for forgiveness un-

[8]Reported payments are sufficient if a payment was reported within three months of the zero-balance quarter and three times that quarterly payment plus the prior quarter's payment is at least 50 percent of the pre-zero balance, evaluated for the loan and for the borrower's aggregate student loan payments. The trajectory test requires the ratio of balance to origination amount to be lower at the zero than four or eight quarters earlier, for both the loan and the borrower's aggregate balance. A small residual is a pre-zero balance of at most 10 percent of the origination amount, at most 50 percent of the origination amount and under $200, or under $100.

der PSLF, and a flat balance during the pause is by itself uninformative. We address this in two ways. First, we require affirmative evidence that the borrower was not repaying the loan, through one of three sets of criteria pertaining to its balance history described in detail below. But regardless of those balance-history criteria, in every case in which a zero balance is accompanied by a reported payment that is both recent and large relative to the amortized balance, the loan is designated repaid rather than canceled.[9] Second, we require that the borrower have at least 48 months of observed student-borrowing history before the loan first reaches a zero balance. We measure tenure from the borrower's earliest observed loan origination rather than from the age of the individual tradeline, because the limited PSLF waiver and the IDR account adjustment required many borrowers to consolidate older loans into new Direct Loans, which resets the origination date of the tradeline but not the borrower's repayment history. Of the canceled loans in our sample that were originated in 2020 or later, 98 percent belong to borrowers with an earlier student loan origination in the panel, two-thirds at least four years earlier and half at least ten years earlier, with a median gap of nearly twelve years, which shows that they are held by long-tenured borrowers rather than new entrants to student borrowing.[10]

The first of the three criteria to categorize a zero-balance loan as canceled applies to zero-balance events observed during the pause. Because every federal balance was frozen, comparing a loan's balance to its own value one or two years earlier is uninformative, so we benchmark against the last balance observed before the pause: a loan is deemed canceled if its pre-zero balance was at or above the level it held immediately before March 2020, so that it did not amortize across the period when payments would otherwise have reduced it. For loans originated during the pause, mainly the waiver and account-adjustment consolidations, which have no pre-pause history, we instead require the final balance to sit at or above 95 percent of the origination amount, or to have been flat or rising over the preceding four quarters, on the premise

[9] Specifically, a loan is designated repaid if a payment was reported within six months of the zero-balance quarter and the payments reported in that quarter and the two preceding quarters sum to at least 50 percent of the pre-zero balance.

[10] Because the panel begins in 2017, loans that closed before then are not observed, so these shares are lower bounds.

that a consolidation held at zero interest without payments should look exactly like that.

The second criterion to categorize a zero-balance loan as canceled applies outside the pause, when balance movement is again informative: a loan is deemed canceled if its balance, and the borrower's aggregate federal student loan balance, were at or above their levels four or eight quarters earlier, and the loan carried a delinquency or past-due balance at some point in the previous 24 months.

The third criterion covers loans with no distress flag: a loan is classified as canceled if its balance and the borrower's aggregate balance were at or above their levels eight quarters earlier and its pre-zero balance was at or above the amount originally borrowed, which is the signature of a balance that never amortized, under income-driven repayment or the pause.[11]

This schema is deliberately conservative, in the sense that it minimizes false-positive cancellations at the likely cost of some false negatives: some true cancellations go undetected because the loan did not display unequivocal signs of non-amortization or distress before its balance was written down to zero. We therefore report results under two sets of criteria. The *baseline* criteria are those just described. The *expanded* criteria additionally count the relief-candidate category, meaning loans that carry codes either for settlement for less than the full balance or for a government claim on the insured portion of a defaulted loan, which probably reflect partial or administrative relief but are too ambiguous to include in the baseline.[12]

iv. Balance and Payment Histories of Zero-Balance Loans

The classification above rests on balance trajectories and status codes when reported payments are insufficient to account for balance reductions. Appendix A examines the pre-zero balance histories of every student loan in our sample whose balance goes from positive to zero

[11] For zero-balance events through the end of 2021, a four-quarter window is used in place of eight.

[12] Under the *expanded* criteria, where these codes are present, we treat a terminal zero balance as canceled regardless of the balance criteria, since it is a direct administrative marker rather than a proxy. The code is furnisher-reported and therefore inconsistently and very sparsely populated, so it cannot serve as the sole basis for identification. The expanded criteria also relax the second and third balance-history criteria: the distress test accepts a delinquency or past-due balance on any of the borrower's student loans rather than the loan in question, and the origination test applies without the requirement that the balance was flat or rising over the preceding eight quarters.

during 2021–2024 and that is observed with a positive balance in each of the eight preceding quarters, expressing each loan's balance as a ratio to its origination amount. Reported payments enter the classification only to rule cancellation out, never to establish it. Four findings from that exercise justify the analytical choices described in Section I.B.iii.

First, the trajectories do not partition cleanly into loans that amortize steadily and loans that sit flat until a sudden zero. Roughly 38 percent of zero-balance loans never amortized over the two years before the zero, 20 percent declined steadily in the manner of a conventionally amortizing loan, and the remaining 42 percent fall in between, with balances that declined somewhat but were far from retired when they reached zero. Second, because reported payments are sufficient to account for the extinguished balance for only 4.3 percent of loans, payments cannot serve as the basis for classifying the great majority of loans. Even among these documented repayments, about one in ten was retired with a lump sum after little or partial amortization, so a rule that classified loans by the shape of the balance trajectory alone would misread roughly one in ten documented repayments as something other than repayment. Third, 16.6 percent of loans without sufficient reported payments nonetheless decline steadily toward zero in exactly the manner of the documented repayments. These are the uncertain-repaid loans of Section I.B.ii: borrowers who look like they repaid their loans and whose payments we simply do not observe. Their presence is why we do not treat the absence of observed payments as evidence of cancellation. Fourth, among loans without documented repayment that reach zero suddenly, we find that, based on the account-status codes, nearly half are refinancings, transfers, or discharges. Suddenness of the zero balance is a necessary condition for cancellation in the credit file, not a sufficient one.

These findings bear directly on the approach of Dinerstein et al. (2025). They identify forgiveness in a monthly credit panel by flagging every student loan whose balance goes from positive to zero, retaining those for which the payment reported in the month of closure is smaller than the balance extinguished, and then dropping those where 75 percent or more of the balance then reappears (a screen similar to our transfer rule), and those that coincide with

an unexplained collapse of the borrower's other debt. The payment test is the only step in that procedure that distinguishes repayment from cancellation, and it relies entirely on reported payments. If payment reporting in their panel resembles ours, in which a payment is reported at the zero event for about 2 percent of loans, that test classifies as forgiven every loan retired on schedule by a borrower whose servicer does not furnish payment data, every interest-only loan retired with an unreported balloon payment, and every bankruptcy discharge, charge-off, or transfer to a servicer that does not re-report the balance; it assesses neither the pre-zero balance trajectory, nor the borrower's tenure, nor any sign of distress. We return in Section III to the external evidence that their procedure generates a large number of false positives.

v. Constructing Treatment Cohorts

Borrowers with at least one canceled loan are assigned to one of 15 treatment cohorts, one for each quarter from 2021Q2 through 2024Q4. The window opens with the 2021Q2 archive, the first full quarter after the Borrower Defense group discharges of March 2021 that began the period's cancellations, and closes with 2024Q4. Zero-balance events before April 2021 are not classified; cancellation was rare before 2021 (American Enterprise Institute, 2025), and a borrower whose loans reached zero before the window and who otherwise meets the control criteria remains in the control group. Because the number of treated borrowers in any single quarter is small relative to the control group, the summary statistics in Table 1 and the descriptive figures pool cohorts by the calendar year of cancellation; the estimation itself uses the quarterly cohorts. The quarterly cohort sizes track the rollout of relief. The three largest cohorts, 2024Q1, 2023Q4, and 2023Q2, follow the spring 2023 PSLF processing surge and the waves of IDR account adjustment discharges from mid-2023 through early 2024, while the two smallest after the partially censored first cohort, 2023Q1 and 2022Q4, fall before the adjustment's first discharges started posting in the spring of 2023. This correspondence is independent of the classification rules and is further evidence that the rules are identifying administrative discharges rather than noise.

For borrowers with loans canceled in more than one quarter, we assign them to the quarter of the first observed cancellation. A possible side effect of this rule is a downward bias: a borrower who has a small balance written off early and a larger discharge later is anchored to the earlier, smaller shock, so the estimator looks for a behavioral response before the bulk of the relief arrives. In practice this concern is minor, because the large majority of borrowers in our sample experience cancellation in a single quarter. A related issue is that credit reporting operates with a lag, since servicers may take one or more billing cycles to report administrative changes to the bureaus, so the quarter in which we observe a loan as canceled may trail the date the Department of Education approved the discharge for reasons addressed in section I.B.viii and Appendix B. Our event time zero accordingly represents the realization of the cancellation on the borrower's credit file.

Having established a method for identifying canceled loans, we next define our treatment and control groups to allow for a comparison that quantifies the effects of loan cancellation through a staggered difference-in-differences (DiD) approach. The treatment group includes all borrowers with at least one loan classified as "canceled" in the 2021Q2 through 2024Q4 archives, assigned to a treatment cohort according to the quarter of their first cancellation. Under the expanded criteria, borrowers whose only qualifying event is a "relief candidate" loan are also treated; under the baseline criteria they are excluded.

vi. Control Group

The control group consists of borrowers with active student loans who never experience a cancellation during the sample period, subject to two further restrictions. Importantly, we exclude from both the treatment and control groups any borrower whose student loans include a discharge, a refinancing or transfer, a death-associated closure, or an unknown zero-balance event.[13] This is a stricter rule than simply keeping undetected treated individuals out of the

[13]We identify death-associated closures following Dinerstein et al. (2025): a student-loan closure that coincides with an otherwise unexplained collapse of the borrower's non-student debt, with mortgage, auto, and credit-card balances falling by at least half and trailing payments too small to account for the decline, within a window that

control group. A borrower who refinanced, or whose loan was discharged in bankruptcy, has experienced a balance-sheet event that moves the same outcomes we study, so including them in the control group would defeat our purpose. We need a counterfactual that reveals what would have happened to canceled borrowers absent cancellation, and so including borrowers who experienced something similar in the control would make our counterfactual less credible. A borrower who simply paid a loan off, on the other hand, is a valid untreated comparison and is retained. The exclusions do not leave the control group unusually well-performing: as Table 1 shows, 10.0 percent of control-group borrowers had a delinquent student loan at some point in 2019, against 10.7 to 12.0 percent across the annual treatment cohorts and 5.9 to 22.0 percent across the quarterly ones. What the exclusions remove is the later balance-sheet shock, the discharge, charge-off, or unexplained closure, that would otherwise enter the comparison. That is the counterfactual we want to study the effect of cancellation, since it is free of the shocks that would otherwise confound the comparison. To improve comparability, we also limit the control group to borrowers whose most recent loans originated in 2013 or earlier, aligning the repayment vintage and maturity of the control group more closely with those of the treatment cohorts, whose cancellations disproportionately involve older loans. Since the Sun and Abraham (2021) estimator identifies cohort-specific effects against never-treated borrowers, keeping this comparison group free of refinancings, discharges, and death-associated closures is what prevents non-cancellation shocks from entering the counterfactual.

vii. Validating Treatment Cohorts Against Programmatic Cancellations

While we cannot directly observe the program through which individual borrowers in our sample received loan forgiveness, the timing of the programs together with the pre-cancellation characteristics of the treatment cohorts offer insight into the likely pathways. Table 1 pools the 15 quarterly cohorts by calendar year; Appendix Table B.1 reports each quarter separately. The

allows for the fact that servicers learn of a borrower's death at different times. Such borrowers are removed from the sample, since their post-event outcomes are undefined.

quarterly detail matters, because the annual averages blend populations that differ sharply. The cohorts canceled in 2021Q4 and in the last three quarters of 2022 have homeownership rates of 52 to 56 percent and median non-student debt of $57,000 to $87,000, against 48 percent and $41,000 for the control group, together with credit scores close to the control group's (687 to 713, against 711) and median student debt balances between 1.5 and 2.4 times the control group's. That is the profile of an established borrower with a large, long-standing balance, and it coincides with the limited PSLF waiver, under which the Department processed discharges from late 2021 through early 2023.[14] While many of the 2021 beneficiaries obtained relief through the Total and Permanent Disability and Borrower Defense programs, those 2021 cancellations all took place in the quarters prior to 2021Q4. As further detailed in section I.B.viii, a substantial share of beneficiaries from those two programs cannot be identified in credit reporting data because they are likely to have defaulted on their loans and have had them removed from their credit reporting data by the time cancellation happened; furthermore, administrative delays resulted in a substantial share of these beneficiaries, whose cancellation was announced in 2022, only having it reflected in credit reporting data in subsequent years.

The same profile reappears in the 2023Q2 cohort, which has the highest median canceled balance ($47,332), the highest homeownership rate (62 percent), and median non-student debt of $109,490, and which follows the spring 2023 surge in PSLF processing.

The cohorts from 2023Q3 through 2024Q3 look different. They have credit scores between 623 and 652, homeownership rates between 29 and 37 percent, median non-student debt of $14,000 to $22,000. Between 24 and 36 percent of borrowers have a loan in collections. This is consistent with the population reached by the IDR account adjustment, announced in April 2022 and processed in large waves from July 2023, whose beneficiaries had spent decades in income-driven repayment on balances that did not amortize, together with the Closed School, Borrower Defense, and Total and Permanent Disability discharges that reached borrowers from

[14]The 2022Q1 cohort is the exception within this window, with a credit score of 668, a homeownership rate of 42 percent, and median non-student debt of $23,000, closer to the profile of the later cohorts.

for-profit institutions or in financial distress, to the extent that those loans were still reported to the bureaus at the time of discharge. The shares of women and non-white borrowers are higher in these cohorts than in the 2021 and 2022 cohorts, consistent with the composition of long-term IDR enrollment. The final cohort, 2024Q4, does not fit this pattern: its credit score, homeownership rate, and collections share are close to the control group's, and its share of women is the lowest of any cohort, consistent with the June 2024 consolidation deadline for commercially held FFEL loans, after which borrowers who had consolidated to qualify for the IDR account adjustment received discharges on new Direct Loan tradelines. Sixty-two percent of the loans canceled in 2024Q4 were originated in 2020 or later, against 27 percent in 2023Q4, which is also why that cohort's median canceled balance exceeds its median 2019 balance in Appendix Table B.1.

viii. Analysis Sample

We estimate the model on the sample of borrowers observed for at least 16 quarters over the 2021Q2 to 2025Q4 period, so that each borrower has sufficient pre- and post-cancellation coverage to identify dynamic treatment effects.[15] Within this sample, our procedure identifies $74.3 billion in canceled student debt under the baseline criteria and $97.1 billion under the expanded criteria through the end of 2024.

Both figures are lower than the $180.0 billion in cumulative cancellation the Department of Education had announced by the end of 2024.[16] Part of the gap reflects timing: the Department's figure counts relief at approval, and relief approved late in our window had not yet been executed by servicers and posted to credit files by the end of it. The larger part reflects the fact that a substantial share of the announced total consists of Borrower Defense and Total and

[15] This is a minimum-observation-length restriction rather than a balanced panel, since the Sun and Abraham (2021) estimator accommodates borrowers who are not present in every period. The requirement does exclude borrowers with short observed credit histories, who differ systematically from longer-tenured borrowers, so our estimates describe effects for the population of established credit-file holders.

[16] See Appendix B for a comparison with DOE cumulative totals and a fuller account of which categories of relief are and are not observable in credit-panel data.

Permanent Disability discharges, which disproportionately involve loans that were already in default and had been removed from borrowers' credit files before the relief was granted.[17] A discharge of a loan that is no longer reported cannot be observed as a balance falling to zero, so we do not observe these loans as cancellation candidates. The relief that credit panels do observe, primarily PSLF and the IDR account adjustment, which together make up the majority of the announced total, we capture at a substantially higher rate.

There are other, more systemic reasons beside delay that account for canceled loans not appearing in our credit panel. The record in *Sweet v. Cardona* shows the Department itself missing the settlement's January 2024 deadline for relief to roughly 196,000 class members and reporting substantial compliance only in September 2024, so that relief dated to 2022 in the Department's totals posted to credit files across 2023 and 2024 (U.S. District Court for the Northern District of California, 2022). Private actions by borrowers allege the same at the next steps along the chain: against a servicer, that it continued to bill and to furnish loans as outstanding after the Department had notified it of their discharge (U.S. District Court for the Northern District of California, 2024), and, since September 2026, against the Department itself, that it continues to furnish loans covered by its group discharge findings to the credit bureaus as deferred, outstanding, and growing with interest, years after announcing that they would be discharged automatically (U.S. District Court for the District of Columbia, 2026). How much announced relief has been executed is known only piecemeal, from the Department's status reports in litigation over particular programs: by its own account, as of spring 2026 it had yet to process the discharges of 17 percent of the 79,000 Westwood borrowers and 19 percent of the 560,000 Corinthian borrowers announced in mid-2022 (U.S. District Court for the District of Columbia, 2026, ¶¶ 64–65). In July 2026 the Project on Predatory Student Lending sued to compel the

[17] Briones (2025) is a study of the effect of removing these long-defaulted loans from credit reports, which played out over the mid-to-late 2010s, seven years after the first delinquency (per the provisions of the Fair Credit Reporting Act), largely following the enrollment and attainment surge during the Great Recession (Pinto and Steinbaum, 2023). The paper shows that 87% of defaulted loans disappearing from credit reports due to FCRA aging still carried past-due balances. We surmise that Borrower Defense and Total and Permanent Disability discharges that took place during our study period disproportionately concerned those loans, which would explain why we detect fewer of those cancellations than the Department of Education announced.

records for all ten programs (U.S. District Court for the District of Massachusetts, 2026); its public estimate, released with the *Woods* complaint, is that about $4.6 billion of the $23 billion announced, owed by more than 300,000 borrowers, was still being reported as owed four years after the first of those announcements. Appendix B gives further detail.

Two features of our approach bear on how far the estimates generalize. First, the borrowers we identify as canceled on the basis of their balance histories are, on average, more constrained by their student debt than the average cancellation beneficiary, since a distressed or non-amortizing balance is easier to identify as canceled than a balance that was being paid down on a conventional amortization schedule when it was forgiven. These borrowers could therefore benefit more from the policy than the typical beneficiary, and relative to the full population of beneficiaries the estimates in Section II are best read as an upper bound. Second, as discussed above, many of the loans canceled under Borrower Defense and Total and Permanent Disability were in default and had aged off borrowers' credit reports, so those borrowers do not appear in our treatment group. They were almost certainly more distressed than the borrowers we observe, but because their loans had already left the credit file, their discharge produced no credit-file event. The benefit of cancellation for those borrowers likely resulted from the end of wage garnishment and tax-refund offset, rather than through the credit outcomes we study.

C. Summary Statistics and Trends by Cancellation Cohort

Next, we present baseline characteristics and trends for the treatment and control groups. Table 1 provides an overview of the demographic and financial profiles of borrowers in 2019, before the initiation of student loan cancellation. Overall, borrowers with canceled loans were, on average, financially worse off than those whose loans remained active, though the gap is concentrated in the cohorts from 2023Q3 onward. All four annual cohorts carried higher median student loan balances than the control group. The later cohorts also had lower credit scores, lower homeownership rates, less non-student debt, and a higher share of borrowers with non-student delinquencies or loans in collections. The 2023Q2 cohort, which is the third-

largest and carries the largest canceled balances, is an exception in the other direction, with homeownership and credit scores above the control group in 2019.

We emphasize that the difference-in-differences (DiD) approach we employ does not require identical baseline levels between the treatment and control groups. Our identification strategy relies on the assumption of parallel trends: in the absence of loan cancellation, the treatment and control groups would have evolved similarly over time. Under this assumption, the control group serves as a valid benchmark for the treatment group, allowing us to assess the causal impact of loan cancellation on financial outcomes, even with differences in baseline characteristics.

Figure 1 plots quarterly trends from 2017Q1 through 2025Q4 in the median student loan balance, the mean monthly student loan payment *owed* (as opposed to actually paid), the homeownership rate, and the median credit score. Each panel shows the four annual cancellation cohorts and the control group. Median balances for the treatment cohorts are flat or slightly rising in the years before cancellation, at between $27,000 and $43,000, and fall to zero in the year of cancellation and stay there. By contrast, the control group's median balance declines gradually from $20,000 in 2017 to under $6,000 by 2025. Mean monthly payments for all groups fall sharply at the start of the repayment pause in 2020Q2. When payments resumed in late 2023, the control group's mean payment returned to about $150 while the cohorts already canceled remained near $50, reflecting the smaller remaining balances of borrowers some or all of whose loans had been discharged.

The bottom panels show the outcomes that motivate the analysis. The 2021 and 2022 cohorts start below the control group in homeownership and cross above it in 2021 and 2022 respectively, ending three to four points higher by 2025. The 2023 cohort starts further below and closes about half the gap by 2025. The 2024 cohort, the most constrained at baseline, rises in parallel with the control group without converging. Credit scores show the same ordering with sharper timing: the 2021 and 2022 cohorts overtake the control group in the year their loans are canceled and end 20 points above it, the 2023 cohort converges to the control group's

level in 2023, and the 2024 cohort gains about 25 points after cancellation but remains 20 points below. The control group's trends in both outcomes are smooth, with none of the inflection points visible for the treatment cohorts. These descriptive patterns suggest a relationship between cancellation and improved outcomes net of the repayment pause, but the treatment cohorts differ from each other and from the control group at baseline, so a causal interpretation requires the event-study design of the following section.

II. Empirical Specification and Results

In this section, we examine the effects of student loan cancellation on financial outcomes such as homeownership, credit access, and overall debt burden, utilizing an event study and a difference-in-differences (DiD) approach designed for staggered interventions. We begin by detailing the empirical specification used to estimate the causal effects of loan cancellation. Next, we present the model estimates and describe the event-study plots, which quantify the impact of loan cancellation on financial outcomes.

A. Empirical Specification

Our empirical strategy employs a staggered difference-in-differences (DiD) design to estimate the effects of student loan cancellation on borrower outcomes. This approach leverages variation in the timing of loan cancellations across multiple cohorts from 2021 to 2024, allowing us to capture the dynamic effects of loan forgiveness.

Traditional two-way fixed effects (TWFE) models in staggered treatment settings have been shown to produce biased estimates when treatment effects vary over time or across cohorts (Goodman-Bacon, 2021). To address this, we adopt the method developed by Sun and Abraham (2021), which accounts for heterogeneity in treatment timing and dynamic treatment effects. This approach enables us to estimate cohort-specific average treatment effects on the treated (CATT) for each cancellation cohort, while correcting for biases that arise in TWFE models.

The generalized model is specified as follows:

$$y_{it} = \sum_{\tau \neq -1} \beta_\tau D_{it}^{\tau} + \alpha_i + \delta_{g(i),t} + \varepsilon_{it} \tag{1}$$

where y_{it} is the outcome for borrower i in calendar quarter t, and D_{it}^{τ} is an indicator equal to one when quarter t is τ quarters from the quarter in which the borrower's first canceled loan reached a zero balance. The indicators are zero in every quarter for control-group borrowers. The quarter before cancellation ($\tau = -1$) is the omitted reference period, so each β_τ measures the change in the outcome relative to that quarter. The model includes borrower fixed effects (α_i), which absorb time-invariant differences across borrowers, and fixed effects for calendar quarter interacted with the borrower's 2019 student loan payment group and 2019 student loan balance group ($\delta_{g(i),t}$). The payment groups are quintiles of the mean monthly student loan payment in 2019 among borrowers with a positive payment, with borrowers reporting no payment forming a sixth group; the balance groups are defined the same way from the mean 2019 student loan balance. Both measures are winsorized at the 1st and 99th percentiles before grouping. The groups are computed across treated and control borrowers together, so that each treated borrower is compared with control borrowers who carried a similar payment obligation and balance before cancellation. Standard errors are clustered by borrower.

We estimate equation 1 with the interaction-weighted estimator of Sun and Abraham (2021). The estimator first interacts the relative-time indicators with cohort indicators, using the control group as the comparison in every period, and then averages the cohort-specific coefficients at each τ using each cohort's share of treated borrowers observed at that horizon as weights. The resulting β_τ are free of the contamination that arises in two-way fixed effects event studies when already-treated cohorts serve as controls for later ones. The model is estimated on every relative period observed in the panel, which extends from more than thirty quarters before cancellation for the latest cohorts to eighteen quarters after for the earliest; the event-study figures plot β_τ from eight quarters before cancellation to sixteen quarters after, and the window only

restricts what is shown. The average treatment effect on the treated (ATT) that we report in the tables is the average of the post-cancellation coefficients ($\tau \geq 0$), weighted by cohort shares, and the cohort-specific average treatment effects (CATTs) are the same averages taken within each cohort.

The payment-group and balance-group fixed effects ensure that borrowers with canceled loans are compared with others at similar stages of their economic life cycles. Borrowers with canceled student loans may differ significantly from those without, due to factors like whether they attended college before or after the Great Recession (Pinto and Steinbaum, 2023), their resulting labor market earnings, and the degree to which they relied on student loans to finance their education. Without controlling for these differences, the estimated effects of loan cancellation could be confounded by pre-existing disparities, violating the parallel trends assumption. Interacting the groups with calendar time captures distinct economic trends for borrowers at various stages in their financial lives, so that the comparison is among borrowers with similar *ex ante* student debt burdens as measured both by outstanding balance and monthly payments, since Income-Driven Repayment means borrowers with the same balance nonetheless face very different debt service obligations.

B. Estimated Impact of Loan Cancellation

Figures 2 and 3 plot the event-study coefficients from equation 1 for eight outcomes, four describing housing and debt and four describing credit health. Table 2 reports the ATT for each outcome, together with an estimate adjusted for any linear pre-trend, which we discuss below. Cohort-specific estimates are in Appendix Table B.2, and estimates for nine supplementary outcomes, including auto loans, aggregate non-student debt, and collections, are in Appendix Table B.3 and Appendix Figure B.2. The event-study plots track each outcome from eight quarters before cancellation to sixteen quarters after, with the quarter before cancellation as the reference period. Coefficients at long horizons are identified from fewer cohorts since only the cohorts canceled in 2021 are observed sixteen or more quarters post-cancellation. Therefore,

the right-hand portion of each plot should be read with that in mind.

The housing results are in Figure 2. Cancellation raises the homeownership rate by 1.2 percentage points on average over the entire post-cancellation period, against a pre-cancellation rate of 45.6 percent among treated borrowers. The effect rises from about 1 percentage point one year after cancellation to 2 percentage points at two years, over a period where the cohort composition is nearly constant.[18] The point estimates continue to grow after that point, though we cannot separate the effect of time from the fact that progressively fewer cohorts are observed. First-time homeownership is a quarterly flow rather than a stock (an indicator for the quarter in which a borrower is first observed with a mortgage, defined in the note to Table 1), so its coefficients are read differently. The flow rises by 0.11 percentage points per quarter against a pre-cancellation rate of 0.48 percent, a 23 percent increase. Summing the post-cancellation coefficients gives the cumulative entry into first-time homeownership attributable to cancellation: 1.3 percentage points over the first two years, against a 2.1 percentage point increase in the homeownership rate at the same horizon.[19] About two-thirds of the rise in homeownership therefore comes from borrowers buying a home for the first time. The remainder comes from borrowers who had held a mortgage earlier in the panel and took out a new one, and from fewer exits from homeownership, neither of which the first-time measure counts. Mortgage balances rise by 0.16 on the inverse hyperbolic sine scale. Because 54 percent of treated borrowers had no mortgage before cancellation, this coefficient mixes new mortgages with changes in existing ones, but the two can be separated. Among borrowers who held a mortgage in 2019, balances rise by 1.4 percent (Appendix Table B.3), so nearly all of the aggregate mortgage effect reflects borrowers who entered homeownership after cancellation.

Non-mortgage, non-student debt does not respond. The fourth panel of Figure 2 estimates

[18] At $\tau = 8$, the point estimate is obtained off of the cohorts through to 2023Q4, representing 92 percent of treated borrowers.

[19] The cumulative figure is the sum of the first-time homeownership coefficients for $\tau = 0$ through 8 – as mentioned above, this is the horizon over which all eleven cohorts canceled through 2023Q4, holding 92 percent of treated borrowers, are observed. Each coefficient is the cohort-weighted average flow effect in that quarter, so the sum is the average treated borrower's cumulative probability of entering first-time homeownership because of cancellation.

the effect on the log of that outcome (among borrowers who held such debt in 2019), which is the intensive margin a reader might expect to move if cancellation relaxed a borrowing constraint or if borrowers substituted other credit for the student debt removed from their balance sheets. The resulting ATT is very small and clearly non-significant. The same conclusion holds for the share of borrowers with an auto loan, which is unchanged (Appendix Table B.3). The aggregate non-student debt measures in the appendix do rise, by 0.14 and 0.08 on the inverse hyperbolic sine scale, but those series include mortgages and include borrowers with no debt at baseline, and the decomposition above shows that the increase is mortgage debt held by new homeowners.[20]

The credit results are in Figure 3. Credit scores rise by 17.4 points on average, from a pre-cancellation mean of 701, and the increase is present in every cohort, ranging from 12 to 22 points (Appendix Table B.2). The increase crosses cutoffs that lenders use: The share of treated borrowers with a score of 700 or above rises by 7.1 percentage points from a base of 54 percent, and the share at 660 or above rises by 5.1 percentage points.[21] Therefore, it is not surprising that lenders respond to the credit score increases we document. Among borrowers who held a credit card in 2019, total credit card limits rise by 3.6 percent, and the series shows a visible change in slope at the cancellation quarter. On the other side of the ledger, delinquent non-student balances fall by 0.04 on the inverse hyperbolic sine scale, and the share of borrowers with any delinquent non-student tradeline falls by 0.41 percentage points from a base of 5.6 percent. The effect on balances in collections is of the same sign but smaller and less precisely estimated, a 0.30 percentage point decline in the share with any collections account ($p = 0.052$) and a decline of 0.023 in collections balances on the inverse hyperbolic sine scale ($p = 0.040$), and it does not persist at longer horizons.

[20]The inverse hyperbolic sine transform is sensitive to the share of zeros in the outcome, and a coefficient estimated on a sample with many zeros cannot be read as a proportional change (Chen and Roth, 2024). For that reason, we present the intensive-margin estimates on 2019 debt holders in logs and treat the aggregate asinh measures as supplementary.

[21]The 660 threshold is the conventional boundary between near-prime and prime credit; 700 is a common cutoff for the most favorable mortgage and credit card pricing. The 660 estimate is reported in Appendix Table B.3.

Some of these outcomes show movement before cancellation, so the last column of Table 2 shows the result of implementing an adjustment that corresponds to the linear-trend case of the sensitivity analysis in Rambachan and Roth (2023) and follows the approach of Dobkin et al. (2018): it fits a line through the pre-treatment coefficients, extrapolates it into the post-cancellation period, and reports the average post-cancellation effect after subtracting the extrapolated trend, with a standard error computed from the full covariance matrix of the event-study coefficients.[22] For homeownership, the pre-period coefficients are jointly significant at the 5 percent level ($p = 0.049$) but economically negligible, at most 0.36 percentage points against post-period effects that reach 2 to 5 percentage points, and they show no slope; the detrended estimate is 1.14 percentage points. First-time homeownership and mortgage balances pass the pre-trend test and are essentially unchanged by detrending. The credit score outcomes reject the test, because the standard errors are small enough that a 5-point drift over eight quarters is detectable, but the drift is small relative to the effect. Detrending reduces the credit score effect from 17.4 to 15.5 points and the 700-threshold effect from 7.1 to 6.2 percentage points, and both remain many standard errors from zero. We attribute the pre-treatment credit score drift to the pandemic payment pause, which affected treated borrowers more than the control group since canceled loans had to be eligible for it, and which other research has shown improved credit conditions for its beneficiaries (Dinerstein, Yannelis and Chen, 2024). The credit card limit effect passes the test and loses little to detrending.

Two outcomes are sensitive to the adjustment. Delinquent balances show no significant pre-trend, but the pre-period coefficients lean enough that extrapolating them removes two-thirds of the effect and leaves it insignificant (-0.013, standard error 0.020). We therefore read the independent delinquency effect of cancellation as suggestive rather than definitive. Non-mortgage debt among 2019 debt holders shows a significant downward pre-trend, with these

[22] We fit the line through the pre-period coefficients with the reference period fixed at zero, and average the detrended coefficients over the first nine post-cancellation quarters ($\tau = 0$ to 8), the horizon over which all eleven cohorts canceled through 2023Q4, holding 92 percent of treated borrowers, are observed. The adjustment is a linear function of the estimated coefficients, so its standard error follows from their clustered covariance matrix.

borrowers' balances falling relative to the control group before cancellation; against a continuation of that decline the post-cancellation estimate would be positive (3.1 percent, standard error 1.1). The parallel-trends estimate is zero, and the detrended estimate says the decline stopped. Neither reading supports the view that cancellation led borrowers to take on more non-mortgage debt.

The ATTs average over cohorts that differ in both composition and time horizon. For homeownership, the estimated effect depends on both the length of the post-period and the interest rate environment prevailing at the time of cancellation and just after. The cohort-specific estimates in Appendix Table B.2 show that the homeownership effect is highest in the earliest cohorts. The three cohorts canceled in 2021 have CATTs of 5.8, 4.4, and 2.1 percentage points, and the mortgage balance CATTs for the same cohorts are 0.80, 0.51, and 0.33. Most later cohorts have homeownership CATTs below 2 percentage points, and the 2024 cohorts are close to zero. Three factors contribute. First, as discussed earlier, there is evidence suggesting that the effect likely grows with time since cancellation, and while all the 2021 cohorts are observed for 16 post-cancellation quarters the 2024 cohorts are observed for only four to seven. Second, as Appendix Table B.1 shows, the 2021 and 2022 cohorts have the profile of PSLF beneficiaries, with credit scores and homeownership rates near the control group's and long-standing balances, while the cohorts from 2023Q3 onward have lower scores, lower homeownership, and more distress at baseline. Third, the mortgage market changed under the later cohorts. The three 2021 cohorts were canceled when the 30-year fixed mortgage rate averaged about 3 percent. Beginning in January 2022 the rate rose steeply, passing 5 percent in April, 6 percent in September, and 7 percent in October 2022; it peaked near 7.8 percent in October 2023 and did not fall below 6 percent again through the end of 2025 (Freddie Mac, 2025). The monthly payment on a loan of given size was therefore 50 to 60 percent higher for a borrower canceled in 2023 or 2024 than for one canceled in 2021, and since cancellation operates on homeownership through debt-to-income ratios and the ability to save a down payment, the same improvement in creditworthiness bought less house, or none, in the later environment. The within-profile

comparison is consistent with this: the 2022Q2 and 2022Q3 cohorts share the PSLF profile of the 2021 cohorts but were canceled at rates of 5 to 6 percent, and their homeownership CATTs are 2.3 and 0.3 percentage points. The comparison is confounded with horizon, however, and the small 2023Q1 cohort (3.2 percentage points, on 483 borrowers) runs the other way, so we read it as suggestive. The credit score effect, by contrast, is present in every cohort and is largest for the later ones. The cohort estimates for delinquent balances follow the homeownership pattern, with effects between -0.13 and -0.15 for four of the five cohorts canceled from 2021Q3 through 2022Q3 and little effect for later cohorts, which is part of why that outcome is sensitive to detrending.

The results are robust to the definition of cancellation. Appendix Table B.4 re-estimates every outcome under the expanded criteria, which add 9,339 borrowers whose loans carry settlement or government-claim codes. The outcomes where baseline ATTs were significant remain so when estimated under the expanded criteria, while the new point estimates are typically also very similar to those in the baseline. Finally, Appendix C reports estimates by gender and race. The ordering of effects is stable across outcomes, with men and white borrowers showing somewhat larger homeownership gains and non-white borrowers showing larger credit score gains, and the one subgroup contrast we would highlight is that the first-time homeownership effect is present for white borrowers (0.14 percentage points) and absent for non-white borrowers.

III. Discussion

Our central finding is that student debt cancellation enables previously constrained borrowers to substitute mortgage debt for the unsecured student debt removed from their balance sheets, without taking on more auto, credit card, or other non-mortgage debt. This substitution reflects the financial constraints that borrowers face while their student debt remains outstanding. Removing those constraints frees borrowers to advance through their economic life cycles, with potential downstream effects on household composition, career choices, and asset accu-

mulation. In this sense, student debt cancellation facilitates social reproduction, counteracting the economic stasis imposed by otherwise unresolvable debt overhangs. This section draws out that interpretation and then reconciles our findings with the papers whose results appear to differ from ours.

The first-order effect of cancellation is on credit health. Consistent with prior studies of debt relief, such as Dobbie and Song (2015) and Di Maggio, Kalda and Yao (2026) (but in contrast to Dinerstein et al. (2025)), we find that cancellation produces an immediate and substantial increase in credit scores, only a small part of which reflects the pre-treatment drift attributable to the payment pause. What distinguishes cancellation is how borrowers use the improvement. Dinerstein, Yannelis and Chen (2024) find that the payment pause allowed borrowers to take on additional consumer debt, including auto loans and credit card balances, and read that as an ambiguous outcome. We find something different. Credit card limits rise, delinquent balances fall, and non-mortgage non-student debt among borrowers who held it does not change; the only debt that grows is mortgage debt, and it grows because borrowers who did not own a home before cancellation bought one. Permanent relief, on this evidence, improved borrowers' financial position without inducing them to borrow more using unsecured credit.

The housing result is consistent with the mechanisms identified in the literature on student debt and homeownership. Bleemer et al. (2021) and Mezza et al. (2020) show that increases in student debt reduce the likelihood of becoming a homeowner, and both papers point to three underwriting channels: difficulty saving for a down payment, difficulty meeting debt-to-income requirements, and lower credit scores where borrowers have previously been delinquent on their student loans, as is likely the case for many of the borrowers we identify as canceled. A sudden and large reduction in student debt relaxes all three. Two years after cancellation, homeownership is 2 percentage points higher than it would otherwise have been and about two-thirds of that gain is first-time home purchase, which is where debt-to-income limits bind most tightly, with the rest coming from former mortgage holders returning to the market.

The homeownership gains are concentrated in the cohorts canceled in 2021 and, to a lesser

degree, 2022, which have the profile of PSLF beneficiaries, are observed for the longest period after cancellation, and were canceled into a mortgage market in which the 30-year rate was near 3 percent rather than the 6 to 7 percent that prevailed from late 2022 onward. The credit score gains are present in every cohort and are largest for the cohorts canceled from mid-2023 onward, which entered the period with the lowest scores and the most distress and are the population reached by the IDR account adjustment and the discharge programs aimed at borrowers from for-profit institutions. Cancellation improves credit standing for every cohort; what differs across borrowers is the margin on which the improvement is used: relaxing underwriting constraints for established borrowers close to the margin of home purchase, and repairing credit standing for distressed borrowers who were not. The rate environment also bears on external validity. Most treated borrowers were canceled into an adverse housing market, so the pooled homeownership ATT describes what cancellation did under those conditions, not what it would do at 2021 rates. Future research could explore heterogeneity in how borrowers use the improvement in credit standing cancellation confers; a treatment-intensity framework, examining variation in outcomes by the amount of debt canceled or its share of the borrower's total liabilities, would illuminate the mechanisms through which relief shapes financial behavior and inform policy design.

Our results also underscore the distinction between temporary relief, such as income-driven repayment or the payment pause, and the principal reductions achieved through cancellation. Temporary measures alleviate immediate financial pressure but do not address the economic impairment caused by unresolved debt. Neither IDR nor the payment pause enabled borrowers to make significant asset purchases relative to their counterfactual, as the absence of meaningful pre-trends in homeownership and mortgage balances in Figure 2 shows and as Collier et al. (2024*b*), who find modest-to-nonexistent effects of the pause on these outcomes, confirm. Only permanent reductions in outstanding balances appear to relieve the constraints that thwart wealth accumulation and the economic life cycle.

Finally, these findings bear on the economic consequences of financing higher education

through individually-held debt. Studying a debt-financed system against a counterfactual is usually stymied by the fact that indebtedness and educational attainment are jointly determined, so that it is difficult to vary the debt while holding attainment fixed. Cancellation does exactly that: it reduces student debt for individuals who had, for the most part, already completed their education. The result is suggestive evidence that a higher education system that does not leave students indebted leaves them financially better off than one that does.

These interpretations put us at odds with Dinerstein et al. (2025), the paper closest to ours, on two points. The first is interpretive. Their estimates are broadly similar to ours where the two overlap (with the exception of the credit score outcome), but they read the resemblance between their forgiveness results and the payment-pause results of Dinerstein, Yannelis and Chen (2024) as evidence that cancellation has little marginal effect beyond eliminating payments. The preceding paragraphs are our answer: the pause produced unsecured borrowing and no movement in wealth accumulation, whereas cancellation produced the latter, because only cancellation removes the balance that underwriting standards count against the borrower. The second point is measurement, described in Section I.B.iv. Their procedure treats any zero-balance loan not matched by a reported payment in the month of closure as forgiven, which, given how sparsely payments are reported, classifies loans that are actually repaid as canceled. Two pieces of external evidence indicate that the resulting false positives are numerous. Their Figure C.1 shows a higher cancellation rate in 2019 than in 2020 or 2021, whereas the American Enterprise Institute's Student Debt Forgiveness Tracker shows cancellation close to zero before 2020 (American Enterprise Institute, 2025).[23] The timing is also diagnostic: a measure that counts scheduled payoffs as forgiveness should fall when the payment pause reduced payoffs in 2020 and 2021, which is what their series does, when actual cancellation began to rise. And their summary statistics, which are not disaggregated by cohort or period, show their treated group better off than their control group on delinquency, employment, and earnings,

[23]The tracker covers Borrower Defense, Total and Permanent Disability, PSLF, Temporary Expanded PSLF, the limited PSLF waiver, Teacher Loan Forgiveness, and income-driven repayment forgiveness.

even though nearly half of the cancellation beneficiaries between 2021 and January 2025 came through the Borrower Defense and Total and Permanent Disability programs (Partridge and Weiss, 2024),[24] whose beneficiaries are very unlikely to be better off than the average borrower. Two further design differences follow from these. Our control group is limited to borrowers who had active loans during the pause, did not benefit from cancellation, and whose most recent loans originated in 2013 or earlier, which yields a counterfactual as informative as credit data allow about outcomes in the absence of the pause; theirs appears to include every borrower not classified as a beneficiary. And we follow borrowers for up to four years after cancellation rather than six months, which is what allows the homeownership effect, which builds over time, to appear at all.

Our homeownership finding also contrasts with Di Maggio, Kalda and Yao (2026), who study the discharge of private student debt for defaulted borrowers and find lower mortgage balances and originations alongside improved financial stability. Three differences account for the divergence. Their treated group consists solely of defaulted borrowers, who are substantially younger (average age below 35, against 42 to 44 across our cohorts in 2019) and carry much less student debt (under $14,000, against median balances of between $24,000 and $38,000 across our cohorts). The treatment differs: their relief averages under $8,000, about 58 percent of pre-treatment student debt, whereas the median canceled balance in our cohorts is $24,000 to $33,000 – broadly corresponding to 80 percent or more of the cohort's median 2019 balance (Table 1 – and, as illustrated by Figure 1, in our case the median borrowers in every cohort have the totality of their debt cancelled. And the counterfactual differs: theirs is defaulted borrowers under wage garnishment, ours are borrowers with active loans in good standing.

These differences demarcate populations who stand very differently vis-a-vis the constraints on purchasing a home. Their borrowers were severely distressed, received partial relief, and were measured against garnished peers; for them, a windfall went to paying down obligations.

[24]The Center for American Progress tracker compiles the relevant Department of Education press releases; the same compilation underlies the benchmark series in Appendix B.

Ours were comparatively less constrained, received near-complete relief, and were measured against borrowers repaying on schedule; for them, the same improvement in credit standing relaxed the underwriting constraints that had kept homeownership out of reach. Both papers find the first-order effect, improved credit health. What differs is how borrowers use it, and we read the two sets of results as a coherent joint account of debt relief operating at different points relative to those constraints rather than as a conflict.

Our results likewise contrast with the headline finding of Folch and Mazzone (2026), who find no significant effect of student debt on homeownership, although our results can be reconciled with those as well. In discussing the difference between their results and those of Mezza et al. (2020), they attribute it to sample composition. Their sample excludes college dropouts, about 40 percent of those who enroll (Shapiro et al., 2019),[25] as well as graduates of two-year and for-profit institutions, together more than a quarter of graduates. As they acknowledge, these restrictions make their sample positively selected on academic ability and negatively selected on debt balances relative to the population of student borrowers, and they state that "*the effect of student debt on home ownership should be more negative when the full sample of former college enrollees is considered*," a conclusion corroborated by one of their heterogeneity analyses within their restricted sample. Our results, which concern exactly the borrowers their sample excludes, are consistent with it.

The distinction between temporary and permanent relief should inform the design of debt relief policy. Temporary measures can provide important short-term relief, but our findings point to the particular capacity of principal reduction to foster economic mobility, and by implication, extend to the long-term impact of financing a higher education system with individually-held debt.

[25] Shapiro et al. (2019) report a completion rate of approximately 60 percent after eight years for the fall 2010 cohort.

IV. Conclusion

This paper uses a 2 percent nationally representative panel of credit records, observed quarterly from 2017 through 2025, to estimate the effect of federal student debt cancellation on borrowers' financial outcomes, comparing 15 quarterly cohorts of borrowers whose loans were canceled between 2021 and 2024 with borrowers whose loans remained active and experienced no cancellation, discharge, or refinancing, in a staggered difference-in-differences design that follows borrowers for up to four years after cancellation. Because credit files carry no marker for administrative cancellation and record payments for only a small minority of loans, we identify canceled loans from the trajectory of the balance before it reaches zero and from the account-status codes that distinguish refinancing, transfer, and bankruptcy from discharge; unlike a procedure that rests on the absence of reported payments, ours does not classify ordinary payoffs as cancellation. The procedure identifies $74 billion of the $180 billion in relief the Department of Education reported as approved through 2024, with the remainder likely consisting largely of discharges of defaulted loans that had already left borrowers' credit files, relief approved but not yet executed by servicers, and relief that, as ongoing litigation shows, was executed late or not at all.

Cancellation raises credit scores by 17 points, raises the share of borrowers with a score of 700 or above by 7 percentage points, raises credit card limits among existing cardholders by about 4 percent, and, more tentatively, reduces delinquent non-student balances. It raises the homeownership rate by 1.2 percentage points on average and by 2 points two years out, with the effect even larger for the earliest treated cohorts, and the increase in mortgage debt comes almost entirely from borrowers who did not own a home before cancellation; non-mortgage borrowing does not change. The homeownership gains are concentrated among the established, PSLF-profile borrowers canceled in 2021, who were observed longest and bought into a market with 3 percent mortgage rates, while the credit gains are present in every cohort and largest among the distressed borrowers reached by the IDR account adjustment and the for-

profit discharge programs from mid-2023 onward. We read these results as showing that cancellation relaxes the underwriting constraints that student debt imposes, allowing borrowers to substitute secured, wealth-building debt for an unsecured obligation that many could not retire, a benefit that the payment pause, which suspended payments without reducing principal, did not deliver.

Finally, because cancellation varies indebtedness while holding educational attainment fixed, the estimates isolate the effect of the debt itself, and they constitute suggestive evidence that a higher education system that leaves students indebted leaves them worse off than one that does not. However, three qualifications bound our estimates and implicitly limit their policy significance: our classification captures the cancellations most clearly documented in credit files, so the treated population is likely more constrained than the average beneficiary who was (or would have) paid down their debt in the absence of cancellation. Therefore, we interpret our estimates as an upper bound on the effect of the policy in the entire treated population. Second, most treated borrowers were canceled after the rise in mortgage interest rates in 2022, so the homeownership effects describe cancellation under adverse credit market conditions rather than under those that prevailed when the earliest cohorts were treated. Third, our findings do not speak to the beneficiaries whose defaulted loans had already left their credit files when their debts were canceled by the Department of Education.

References

American Enterprise Institute. 2025. "Student Debt Forgiveness Tracker." *https://www.aei.org/studentdebtforgivenesstracker/*, Maintained by Nat Malkus.

Black, Sandra E., Jeffrey T. Denning, Lisa J. Dettling, Sarena Goodman, and Lesley J. Turner. 2023. "Taking It to the Limit: Effects of Increased Student Loan Availability on Attainment, Earnings, and Financial Well-Being." *American Economic Review*, 113(12): 3357–3400.

Bleemer, Zachary, Meta Brown, Donghoon Lee, Katherine Strair, and Wilbert Van Der Klaauw. 2021. "Echoes of rising tuition in students' borrowing, educational attainment, and homeownership in post-recession America." *Journal of Urban Economics*, 122: 103298.

Briones, Diego A. 2025. "Credit Scars: Credit Reporting Relief for Defaulted Student Loan Borrowers."

Catherine, Sylvain, and Constantine Yannelis. 2023. "The distributional effects of student loan forgiveness." *Journal of Financial Economics*, 147(2): 297–316.

Chen, Jiafeng, and Jonathan Roth. 2024. "Logs with Zeros? Some Problems and Solutions." *Quarterly Journal of Economics*, 139(2): 891–936.

Collier, Daniel, Dan Fitzpatrick, Chloe Fann, Frederick Engram, Carol Bruzzano, and Mecca Keyes. 2024*a*. "Pause and Effect: Examining the Dynamics of the Student Loan Pause and the Challenge of Resuming Payments for Public Service Loan Forgiveness Borrowers." *SSRN Journal*.

Collier, Daniel, Marshall Steinbaum, Dalié Jiménez, Jonathan Glater, Chloe Fann, and Axel Morales Sanchez. 2024*b*. "Student Loan Borrowers and Pandemic-Era Interventions in California."

Di Maggio, Marco, Ankit Kalda, and Vincent W. Yao. 2026. "Second Chance: Life without Student Debt." *The Journal of Finance*, LXXXI(1): 507–550.

Dinerstein, Michael, Constantine Yannelis, and Ching-Tse Chen. 2024. "Debt Moratoria: Evidence from Student Loan Forbearance." *American Economic Review: Insights*, 6(2): 196–213.

Dinerstein, Michael, Samuel Earnest, Dmitri K. Koustas, and Constantine Yannelis. 2025. "Student Loan Forgiveness." National Bureau of Economic Research Working Paper 33462. https://www.nber.org/papers/w33462.

Dobbie, Will, and Jae Song. 2015. "Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection." *American Economic Review*, 105(3): 1272–1311.

Dobkin, Carlos, Amy Finkelstein, Raymond Kluender, and Matthew J. Notowidigdo. 2018. "The Economic Consequences of Hospital Admissions." *American Economic Review*, 108(2): 308–352.

Federal Student Aid. 2024*a*. "Borrower Defense Loan Discharge." [Online; accessed 2024-12-12].

Federal Student Aid. 2024*b*. "Closed School Discharge." https://studentaid.gov/manage-loans/forgiveness-cancellation/closed-school. Accessed October 20, 2026.

Federal Student Aid. 2024*c*. "Fresh Start Program for Borrowers in Default." https://studentaid.gov/announcements-events/default-fresh-start. Accessed October 20, 2026.

Federal Student Aid. 2024*d*. "Income-Driven Repayment and Public Service Loan Forgiveness Program Account Adjustment." https://studentaid.gov/announcements-events/idr-account-adjustment. Accessed October 18, 2026.

Federal Student Aid. 2024*e*. "Public Service Loan Forgiveness." [Online; accessed 2024-12-12].

Federal Student Aid. 2024*f*. "Total and Permanent Disability Discharge." https://studentaid.gov/manage-loans/forgiveness-cancellation/disability-discharge. Accessed October 18, 2026.

Folch, Marc, and Luca Mazzone. 2026. "Go big or buy a home: The impact of student debt on career and housing choices." *Review of Economic Dynamics*, 59: 101317.

Freddie Mac. 2025. "Primary Mortgage Market Survey." Publication Title: Freddie Mac.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics*, 225(2): 254–277.

Hegji, Alexandra. 2023. "Service-Contingent Federal Student Loan Forgiveness and Loan Repayment Programs." Congressional Research Service R47837. https://www.everycrsreport.com/reports/R47837.html.

Mezza, Alvaro, Daniel Ringo, Shane Sherlund, and Kamila Sommer. 2020. "Student Loans and Homeownership." *Journal of Labor Economics*, 38(1): 215–260.

Partridge, Sara, and Madison Weiss. 2024. "Tracker: Student Loan Debt Relief Under the Biden-Harris Administration." Center for American Progress. https://www.americanprogress.org/article/tracker-student-loan-debt-relief-under-the-biden-harris-administration/.

Pinto, Sérgio, and Marshall Steinbaum. 2023. "The Long-Run Impact of the Great Recession on Student Debt." *Labour Economics*, 85(102449).

Rambachan, Ashesh, and Jonathan Roth. 2023. "A More Credible Approach to Parallel Trends." *Review of Economic Studies*, 90(5): 2555–2591.

Shapiro, D, A Dundar, F Huie, P Wakhungu, A Bhimdiwala, and SE Wilson. 2019. "Completing college: Eight year completion outcomes for the Fall 2010 cohort (Signature Report No. 12c)." *Herndon, VA: National Student Clearinghouse Research Center*.

Smith, Robyn. 2021. "Revisiting Relief for Students Harmed by School Closures." *Student Borrower Protection Center Research Paper, Forthcoming*.

Student Borrower Protection Center. 2024. "Accessing Public Service Loan Forgiveness."

Sun, Liyang, and Sarah Abraham. 2021. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics*, 225(2): 175–199.

Supreme Court of the United States. 2023. "Biden v. Nebraska." 600 U.S. 477 (2023). Decided June 30, 2023.

U.S. Court of Appeals for the Eighth Circuit. 2024. "Missouri v. Biden." No. 24-2332 (8th Cir. Aug. 9, 2024). Order granting injunction pending appeal.

U.S. Department of Education. 2021*a*. "Over 323,000 Federal Student Loan Borrowers to Receive $5.8 Billion in Automatic Total and Permanent Disability Discharges." Press release, August 19, 2021. https://www.ed.gov/news/press-releases/over-323000-federal-student-loan-borrowers-receive-58-billion-automatic-total-and-permanent-disability-discharges.

U.S. Department of Education. 2021*b*. "U.S. Department of Education Announces Transformational Changes to the Public Service Loan Forgiveness Program, Will Put Over 550,000 Public Service Workers Closer to Loan Forgiveness." Press release, October 6, 2021. https://www.ed.gov/news/press-releases/us-department-education-announces-transformational-changes-public-service-loan-forgiveness-program-will-put-over-550000-public-service-workers-closer-loan-forgiveness.

U.S. Department of Education. 2022. "Department of Education Announces Actions to Fix Longstanding Failures in the Student Loan Programs." Press release, April 19, 2022. https://www.ed.gov/news/press-releases/department-education-announces-actions-fix-longstanding-failures-student-loan-programs.

U.S. Department of Education. 2023. "Biden-Harris Administration to Provide 804,000 Borrowers with $39 Billion in Automatic Loan Forgiveness as a Result of Fixes to Income Driven Repayment Plans." Press release, July 14, 2023. https://www.presidency.ucsb.edu/node/363775.

U.S. Department of Education. 2024. "Biden-Harris Administration Approves $1.2 Billion in Loan Forgiveness for Over 150,000 SAVE Plan Borrowers." Press release, February 21, 2024. https://www.ed.gov/news/press-releases/biden-harris-administration-approves-12-billion-loan-forgiveness-over-150000-save-plan-borrowers.

U.S. District Court for the District of Columbia. 2026. "Woods v. U.S. Department of Education." Published: Class action complaint, No. 1:26-cv-03335 (D.D.C., filed Sept. 24, 2026).

U.S. District Court for the District of Massachusetts. 2026. "Project on Predatory Student Lending v. U.S. Department of Education." Complaint under the Freedom of Information Act, filed July 1, 2026. Case 1:26-cv-13040. https://www.ppsl.org/cases/ppsl-v-usde.

U.S. District Court for the Northern District of California. 2022. "Sweet v. Cardona." No. 3:19-cv-03674-WHA (N.D. Cal. Nov. 16, 2022). Order granting final approval of class settlement.

U.S. District Court for the Northern District of California. 2024. "Maldonado v. Higher Education Loan Authority of the State of Missouri." Class action complaint, No. 24CV090146 (Cal. Super. Ct. Alameda Cnty., filed Sept. 4, 2024), removed, No. 3:24-cv-07850-VC (N.D. Cal. Nov. 8, 2024). Order denying motion to dismiss, Dkt. 47 (Apr. 9, 2025); order on cross-motions for partial summary judgment, Dkt. 124 (Mar. 10, 2026). https://www.ppsl.org/cases/maldonado-v-mohela.

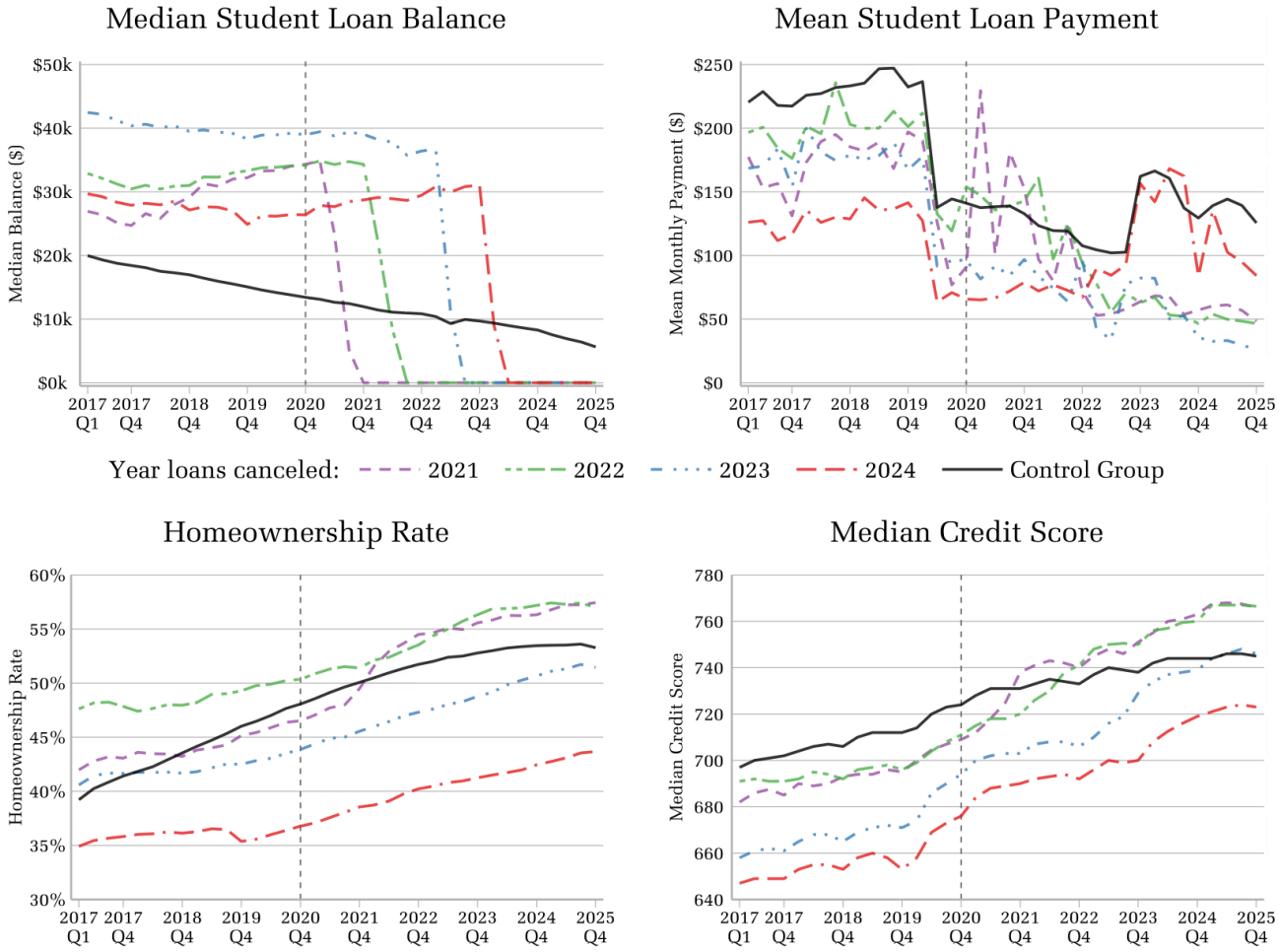
U.S. Government Accountability Office. 2022. "Federal Student Aid: Education Needs to Take Steps to Ensure Eligible Loans Receive Income-Driven Repayment Forgiveness." GAO-22-103720. https://www.gao.gov/products/gao-22-103720.

TABLE 1. 2019 SUMMARY STATISTICS FOR STUDENT LOAN CANCELLATION COHORTS AND CONTROL GROUP

	Cancellation Cohorts				Control Group
	2021	**2022**	**2023**	**2024**	
Number of Individuals	1,967	3,497	10,704	12,139	117,025
Mean Age	42.3	43.7	43.2	41.7	42.6
Share Non-White	22.5%	23.0%	25.8%	27.0%	21.4%
Share Female	53.5%	58.2%	61.1%	61.8%	54.0%
Share Homeowners	46.2%	50.7%	43.9%	36.6%	47.5%
Share New Homeowners	2.7%	2.6%	2.9%	2.6%	3.6%
Share with Auto Loans	58.0%	58.5%	59.1%	56.5%	62.0%
Median Credit Score	693	696	668	652	711
Median Student Debt Canceled	$24,284	$27,496	$32,926	$23,845	$0
Median Student Debt Balance	$31,488	$32,564	$38,128	$24,050	$15,821
Median Non-Student Debt Balance	$36,782	$52,824	$31,746	$20,935	$40,993
Median Auto Loan Balance	$4,904	$4,739	$5,176	$4,101	$5,915
Share with Any Student Loan Delinquency	11.4%	12.0%	10.7%	11.9%	10.0%
Share with Any Non-Student Delinquency	10.1%	10.0%	14.5%	14.6%	10.2%
Mean Delinquent Balance Share (Non-Student)	1.8%	2.1%	3.2%	3.0%	1.8%
Share with Any Loans in Collections	18.6%	19.8%	23.3%	26.0%	17.4%

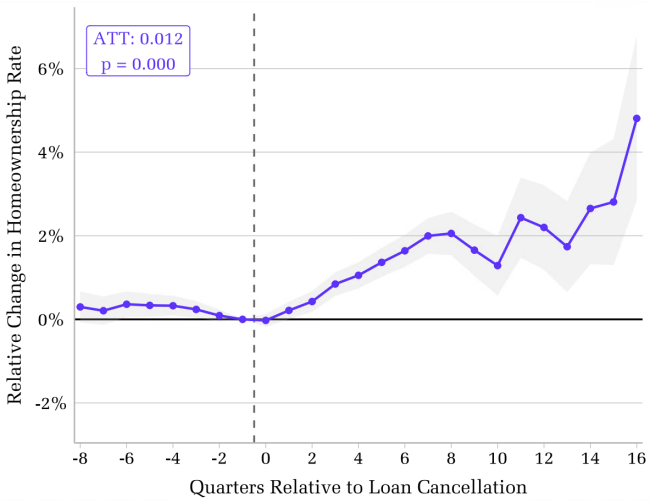
Notes: This table compares the financial and demographic characteristics of student loan cancellation cohorts and the control group in 2019, two years before any loans were canceled. Cohorts are defined under the baseline classification criteria. Treatment is assigned at the quarterly level, giving 15 cohorts from 2021Q2 through 2024Q4; for readability, the table pools cohorts by the calendar year of cancellation. Age is measured at December 2019. Homeownership is inferred from the presence of an open mortgage or home equity line of credit. First-time homeownership is an indicator for the first quarter in which a borrower is observed with a mortgage or home equity line of credit after at least one earlier quarter in the panel without one. Student loan delinquency is an indicator for any student loan tradeline flagged delinquent in any quarter of 2019, the same measure used in the event studies. Delinquency and collections measures for non-student debt are indicators for any tradeline in the relevant status.

FIGURE 1. TRENDS IN STUDENT LOAN BALANCE, PAYMENT, HOMEOWNERSHIP, AND CREDIT SCORE BY CANCELLATION COHORT AND CONTROL GROUP, 2017–2025

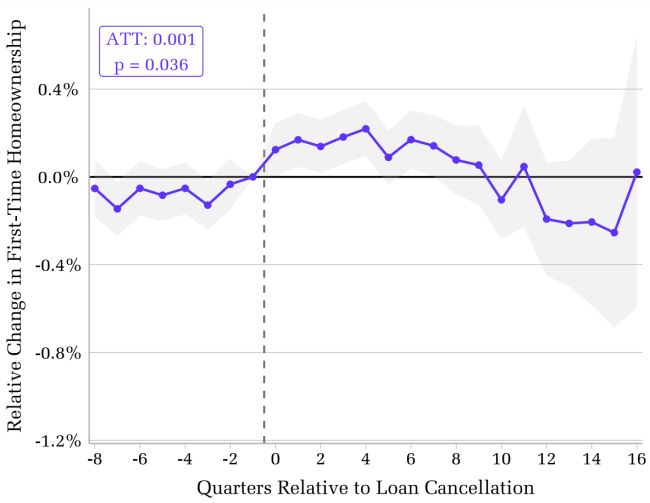


Notes: This figure plots quarterly trends from 2017Q1 through 2025Q4 in the median student loan balance, the mean monthly student loan payment owed, the homeownership rate, and the median credit score, by cancellation cohort and for the control group. The 15 quarterly cohorts are pooled by the calendar year of cancellation. Cohorts are defined under the baseline classification criteria. Homeownership is inferred from the presence of an open mortgage or home equity line of credit. The dashed vertical line marks 2020Q4, the last quarter in the year before our first cancellation cohort.

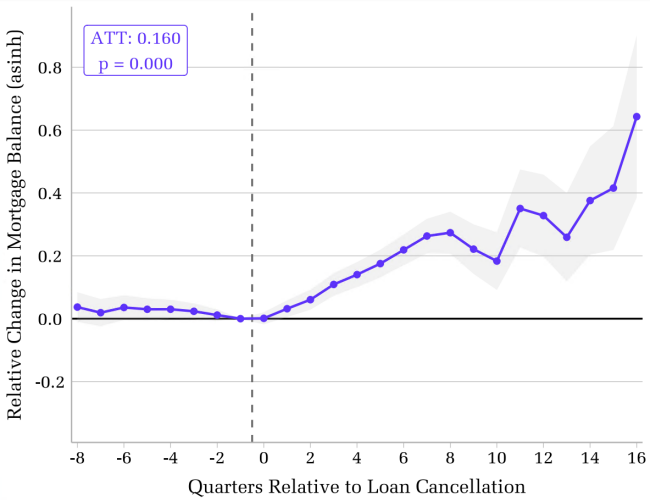
FIGURE 2. EVENT STUDY ESTIMATES: HOMEOWNERSHIP AND DEBT



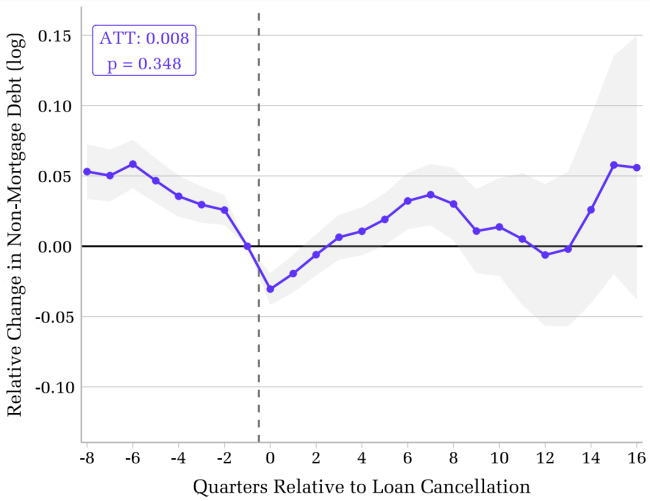
(a) Homeownership (percentage points)



(b) First-time homeownership (percentage points)



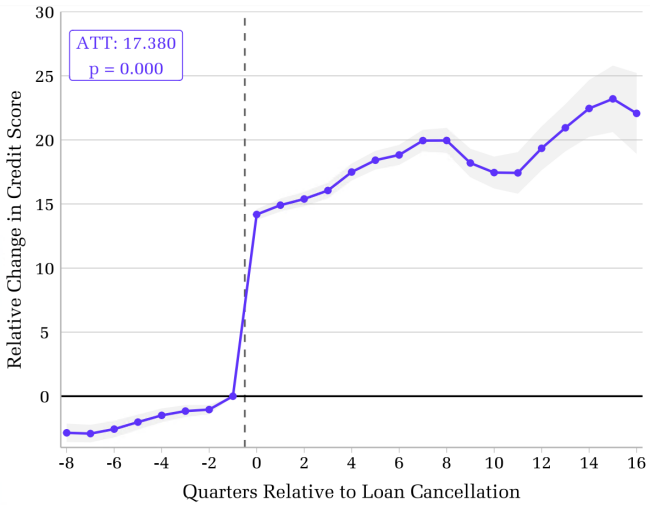
(c) Mortgage balance (asinh)



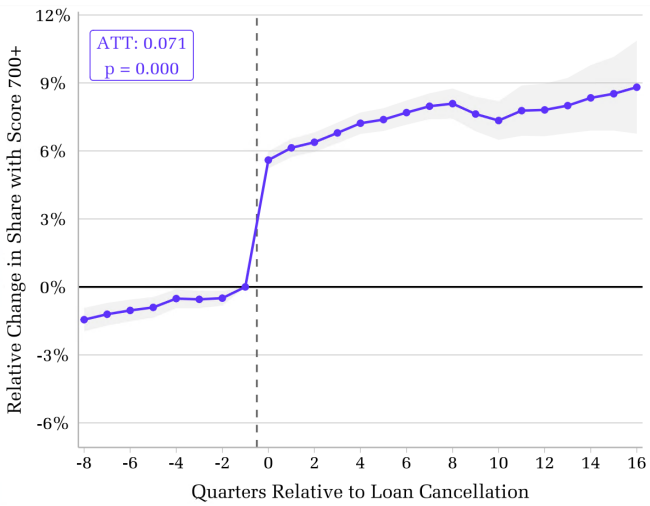
(d) Non-mortgage, non-student debt, 2019 debt holders (log)

Notes: Each panel plots the coefficients β_τ from equation 1, estimated with the interaction-weighted estimator of Sun and Abraham (2021) on the baseline definition of cancellation, from eight quarters before cancellation to sixteen quarters after. The quarter before cancellation ($\tau = -1$) is the omitted reference period and the dashed vertical line marks the cancellation quarter. Shaded areas are 95 percent confidence intervals with standard errors clustered by borrower. The ATT reported in each panel is the average of the post-cancellation coefficients. Panels (a) and (b) are in percentage points; first-time homeownership is a quarterly rate. Panel (c) is the inverse hyperbolic sine of the mortgage balance, estimated on all borrowers, so the coefficient combines new mortgages with changes in existing balances. Panel (d) is the log of non-mortgage, non-student debt, estimated on borrowers who held such debt in 2019 and in the quarter observed, so the coefficient is an approximate proportional change. Estimates at horizons beyond three quarters are identified from progressively fewer cohorts.

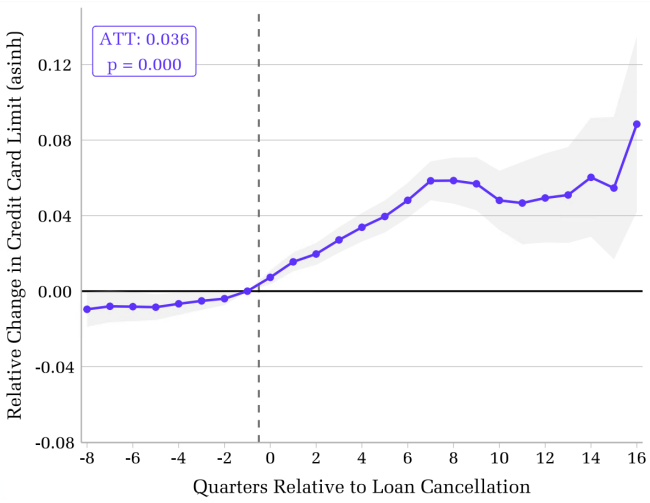
FIGURE 3. EVENT STUDY ESTIMATES: CREDIT HEALTH



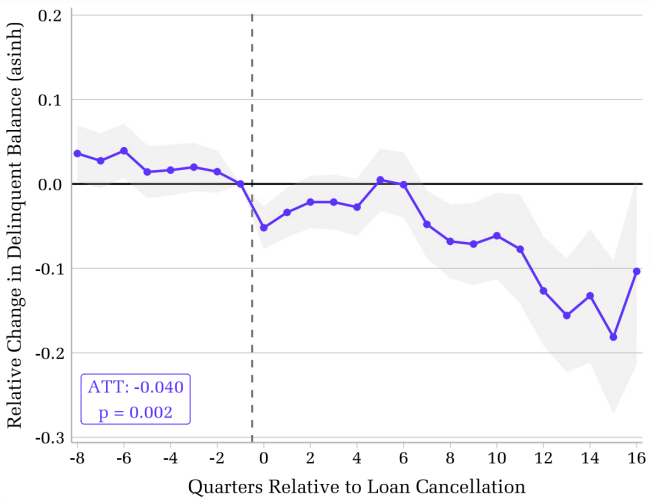
(a) Credit score (points)



(b) Credit score 700 or above (percentage points)



(c) Credit card limit, 2019 cardholders (asinh)



(d) Delinquent non-student balance (asinh)

Notes: See notes to Figure 2. Panel (a) is in points and panel (b) in percentage points. Panel (c) is the inverse hyperbolic sine of total credit card limits, estimated on borrowers who held a credit card in 2019 and in the quarter observed, so the coefficient is an approximate proportional change. Panel (d) is the inverse hyperbolic sine of the delinquent balance on non-student tradelines, estimated on all borrowers; student loans are excluded because a canceled loan cannot be delinquent and including them would produce a mechanical decline.

TABLE 2. AVERAGE POST-TREATMENT EFFECTS OF LOAN CANCELLATION

Outcome	Pre-period mean	ATT	Std. error	p-value	Detrended ATT (std. error)
Homeownership and debt					
Homeownership (pp)	45.6	1.18	0.16	0.000	1.14 (0.21)
First-time homeownership, quarterly (pp)	0.48	0.11	0.05	0.036	0.14 (0.06)
Mortgage balance (asinh)	5.66	0.160	0.021	0.000	0.152 (0.027)
Non-mortgage, non-student debt, 2019 debt holders (log)	9.46	0.008	0.008	0.348	0.031 (0.011)
Credit health					
Credit score (points)	701	17.38	0.30	0.000	15.55 (0.44)
Credit score 700 or above (pp)	54.4	7.09	0.21	0.000	6.23 (0.31)
Credit card limit, 2019 cardholders (asinh)	10.70	0.036	0.004	0.000	0.031 (0.005)
Delinquent non-student balance (asinh)	0.53	−0.040	0.013	0.002	−0.013 (0.020)

Notes: Each row reports the average of the post-treatment event-study coefficients ($\tau \geq 0$) from equation 1, estimated with the interaction-weighted estimator of Sun and Abraham (2021) on the baseline definition of cancellation. The pre-period mean is the mean of the outcome among treated borrowers in the quarter before cancellation. Binary outcomes are in percentage points. Non-mortgage, non-student debt and credit card limits are estimated on borrowers who held the relevant account in 2019 and in the quarter observed, so those coefficients are approximate proportional changes. The detrended ATT fits a line through the pre-cancellation coefficients, extrapolates it into the post-cancellation period, and averages the detrended coefficients over $\tau = 0$ to 8; its standard error is computed from the clustered covariance matrix of the event-study coefficients. Fixed effects are the borrower's 2019 student loan payment and balance quintile interacted with calendar quarter, as well as an individual-borrower constant. Standard errors are clustered by borrower. The estimation sample contains 5,208,815 borrower-quarter observations on 145,453 borrowers over 2017Q1–2025Q4; the non-mortgage debt and credit card limit samples contain 136,462 and 133,872 borrowers. Cohort-specific estimates are in Appendix Table B.2 and supplementary outcomes in Appendix Table B.3.

Appendix

A. Balance Trajectories of Zero-Balance Loans

This appendix documents the pre-zero balance histories that motivate the classification described in the main text. Throughout, we take every student loan in our sample whose balance goes from positive to zero from June 2021 to December 2024 and that is observed with a positive balance in each of the eight preceding quarters, and we express each loan's balance as a ratio to its origination amount.

Figure A.1 assigns each loan to one of 100 centiles according to its balance-to-origination ratio in the quarter before it reaches zero, and plots the mean ratio for each centile over the eight preceding quarters. Two things stand out. First, a large share of loans reach zero suddenly: their balance sits flat, at or above the original amount, for the entire two years before the zero and then disappears in a single quarter. The dense band of centiles at exactly 100 percent, and the centiles above it, are balances that never amortized, which is the profile one would expect of a loan in income-driven repayment or under the repayment pause that was subsequently forgiven. Second, the lower centiles decline steadily toward zero over the full window, which is the profile of a loan being repaid on schedule. The two patterns are not, however, a clean partition. Using the cutoffs described in the figure note, roughly 38 percent of zero-balance loans in this period had a balance that never amortized, and 20 percent lost at least 40 percent of their balance over the two years before reaching zero.[26] The remaining 42 percent fall in between, with balances that declined somewhat but were far from being retired in $t = -1$ before hitting zero in $t = 0$. Those intermediate loans are the ones for which the shape of the balance trajectory alone says least about whether the zero reflects repayment, a refinancing, or a discharge.

The centile means in Figure A.1 are representative of the loans they summarize. Figure A.2

[26] Under stricter cutoffs, requiring a ratio of at least 0.95 at both $t = -8$ and $t = -1$ for the first group and a decline of more than half for the second, the shares are 34 and 15 percent, with the remainder rising to 50 percent.

plots the standard deviation of the $t = -8$ balance ratio within each centile of the $t = -1$ balance ratio distribution. For the centiles that sit at or above 100 percent at $t = -1$, the within-centile standard deviation is roughly 0.1, so the flat trajectories describe nearly every loan in those centiles. Dispersion is about twice as large among the lower centiles, so the gradual declines are an average over loans that started the window at somewhat different points. The implication of the pattern is that the loans that have not amortized before they reach zero suddenly represent a distinct subset of student loans experiencing distress, indicating IDR enrollment or some other underlying factor representing non-repayment. Whereas for the loans that were being repaid, the trajectory toward repayment is more diverse.

Figures A.1 and A.2 suggest an obvious rule: classify the sudden zeroes as canceled and the gradual declines as repaid. Figure A.3 shows the first reason this rule is not enough on its own. It splits the same loans according to whether the payments reported to the bureau over the eight quarters are large enough to retire the balance as it stood at $t = -8$. We call the loans that meet this test "certain repaid," since the credit file itself documents how the balance was extinguished. Two things are evident. First, certain-repaid loans are rare: they make up only 4.3 percent of zero-balance loans, and the share ranges from 2.3 to 8.3 percent as the documented-payment-sufficiency threshold moves from 100 to 75 percent of the $t = -8$ balance. Reported payments therefore cannot serve as the basis for classifying the great majority of loans. Second, the certain-repaid loans that do exist are mostly, but not entirely, gradual decliners. Ninety percent lost at least 40 percent of their balance over the two years before reaching zero. The remaining 10 percent had a flat or only partially amortized balance before being retired with a lump-sum payment. A rule that classified loans by the shape of the balance trajectory alone would therefore misread roughly one in ten documented repayments as something other than repayment, and documented repayments are the easy cases where we know the answer.

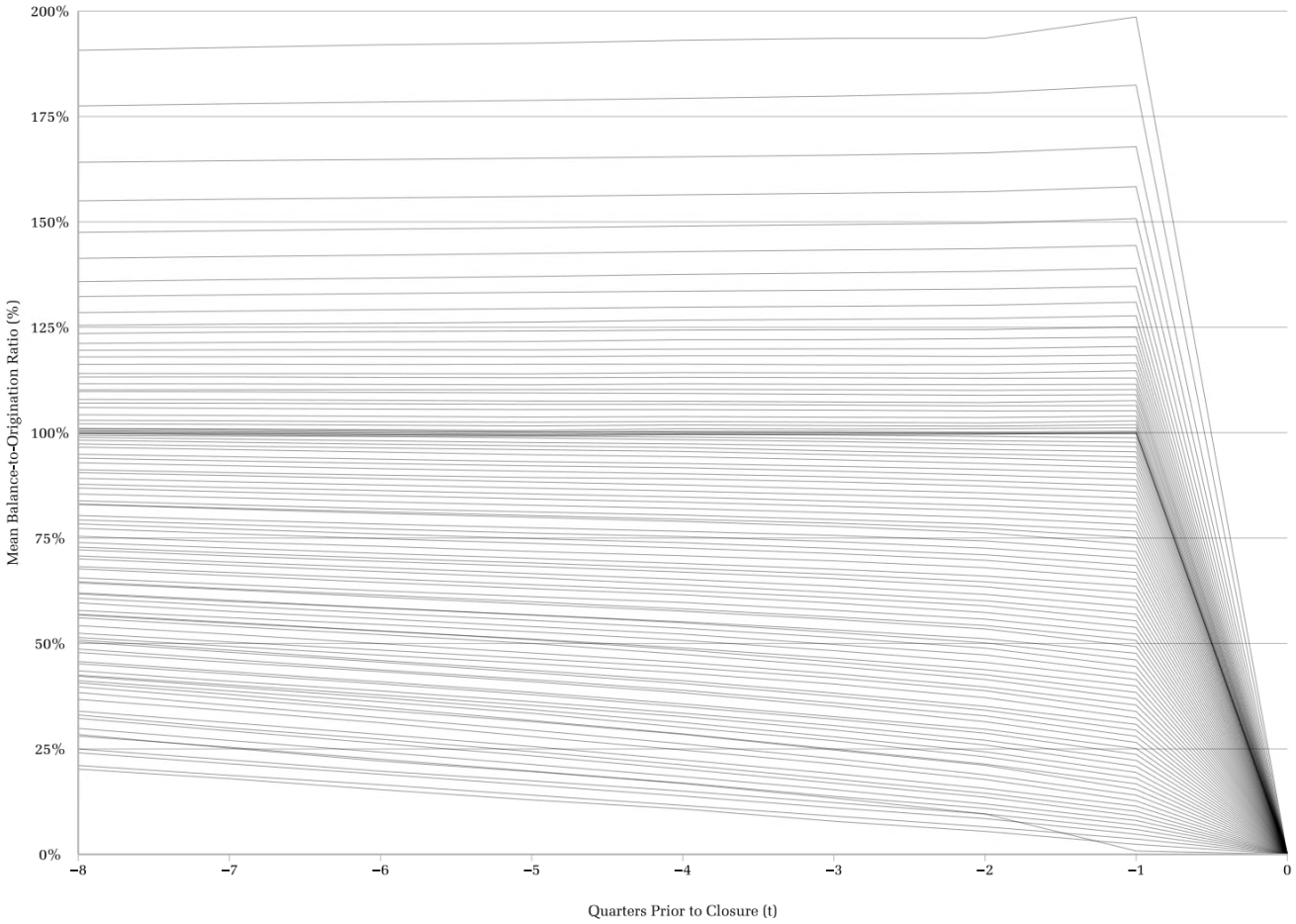
The right panel of Figure A.3 plots the 96 percent of zero-balance loans that are not certain repaid. Figure A.4 isolates that panel and highlights the percentiles that decline gradually. These loans, 16.6 percent of the not-certain group, follow the trajectory of a loan that is being

repaid gradually, but with no reported payments to account for the decline. These borrowers look like they repaid their loans and we simply do not observe the payments, which is not surprising given that not all servicers report actual payments to the bureaus. We call these loans "uncertain repaid" and treat them as repaid. Their presence establishes that the absence of sufficient observed payments does not identify cancellation: a rule that treated every zero without observed payments as a discharge would classify these repayments as relief.

The remaining loans in the not-certain group are the sudden zeroes, 39.7 percent of the group, and the intermediate trajectories, 43.7 percent. A sudden zero can equally be a refinancing, a transfer to a new servicer, or a bankruptcy discharge, and in our data nearly half of the sudden zeroes without documented repayment turn out, on the basis of the account-status codes described in Section I.B, to be one of these rather than a cancellation. Suddenness is a necessary condition for cancellation in the credit file, not a sufficient one.

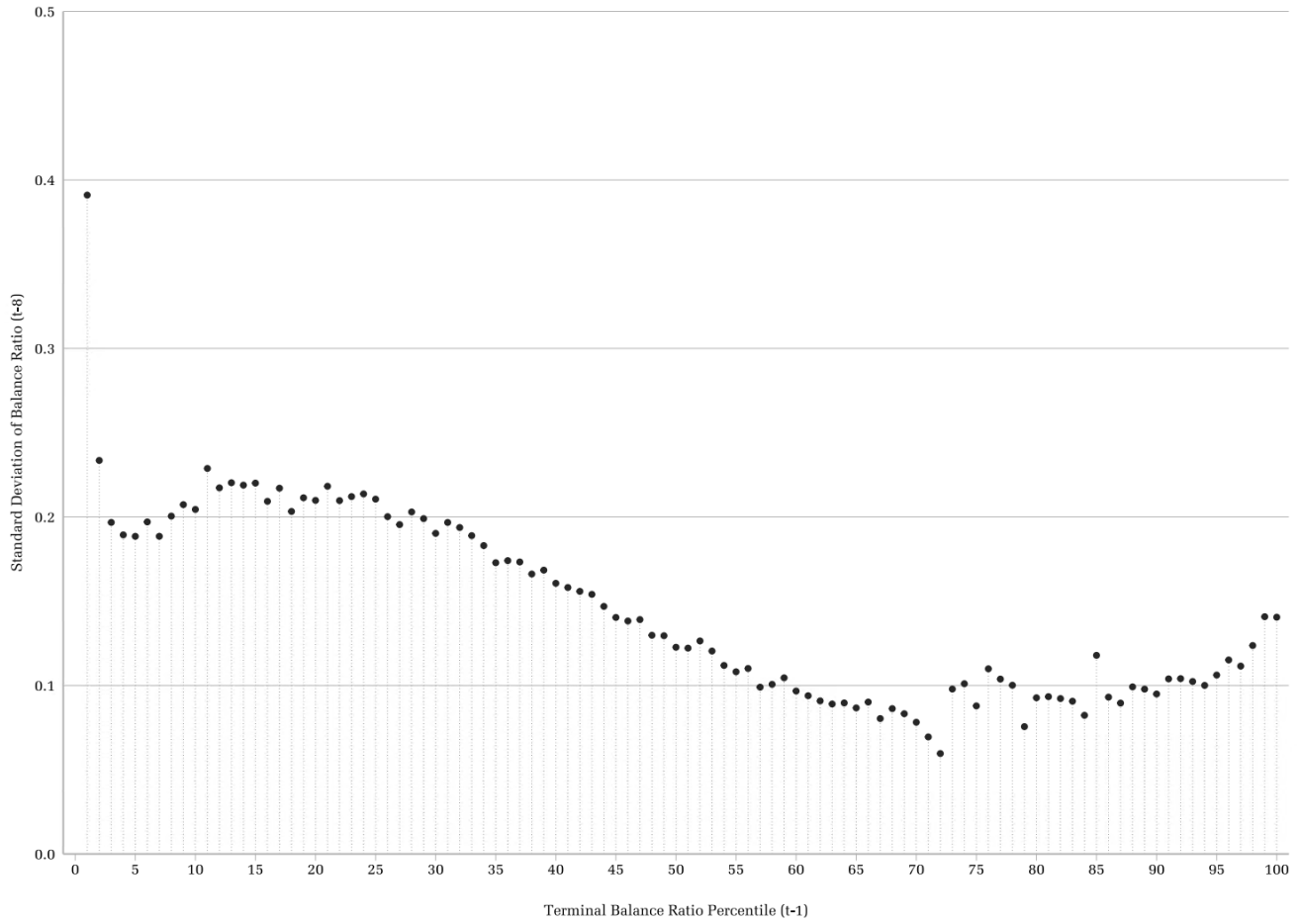
For the reasons described in this appendix, we differ from Dinerstein et al. (2025) in assigning canceled status to zero-balance loans in two respects: we do not require pre-zero payments to be observed in order to classify a loan as repaid, and a sudden zero balance in the absence of documented payments is not sufficient for us to assign canceled status. We additionally require evidence of pre-zero distress, which is why our lookback period is two years before the zero balance is observed. Ours is therefore the more conservative cancellation assignment rule, resulting in fewer false positives but at the cost of potential false negatives, which in our case would be loans that appear to be on the road to repayment prior to zero balance, and which we would interpret as unobserved- or uncertain-repaid, but which are in fact canceled. The fact that we miss those loans in assigning canceled status is what motivates our inference that the effects of cancellation we document are likely upper bounds on the actual effects, because we hypothesize those false-negative canceled borrowers would be less affected by the loan cancellation they experienced than the cancelled borrowers in our treatment group.

FIGURE A.1. PRE-ZERO BALANCE TRAJECTORIES OF STUDENT LOANS REACHING A ZERO BALANCE, 2021–2024



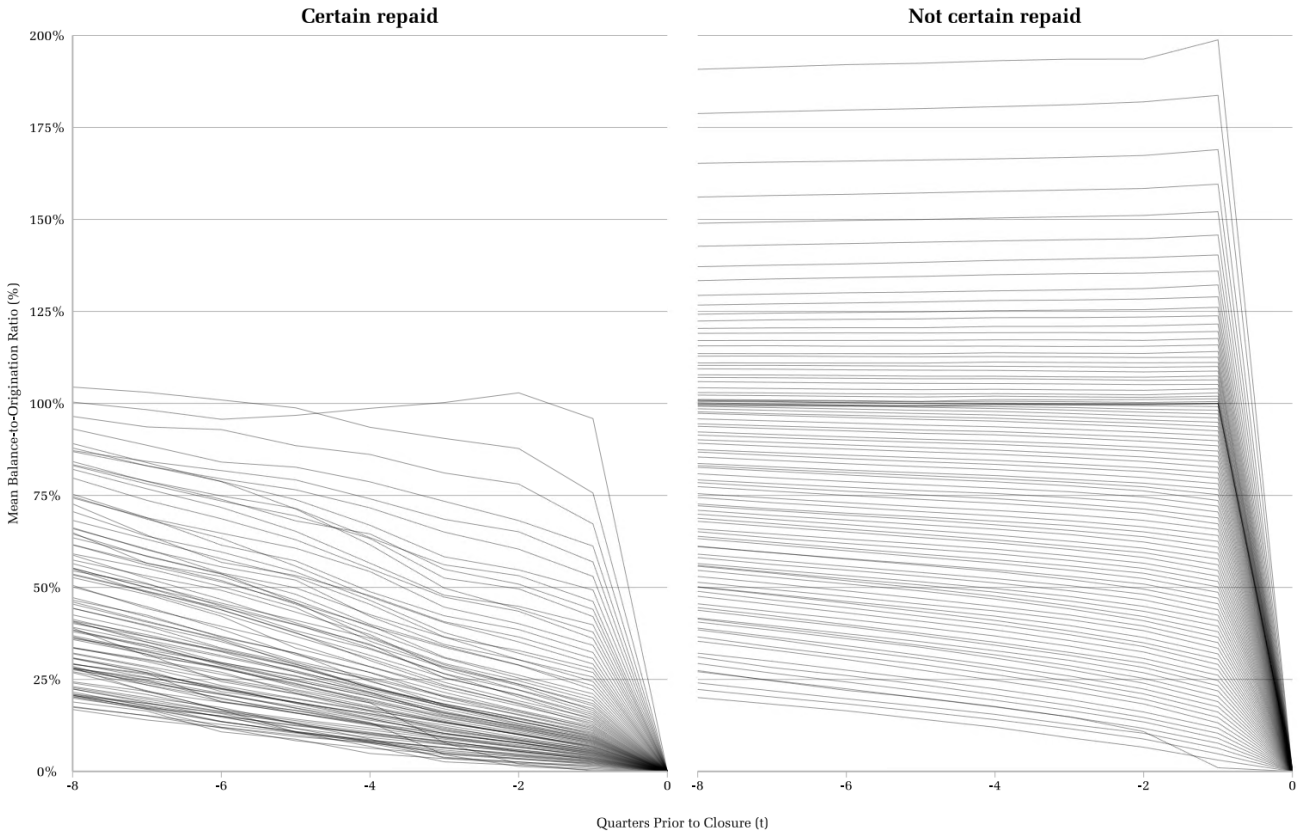
Notes: This figure plots the mean balance-to-origination ratio, by centile, for all student loans in the UC-CCP sample whose balance reaches zero during 2021–2024 and that are observed with a positive balance in each of the eight preceding quarters. Each loan is assigned to a centile according to its balance-to-origination ratio in the quarter before the balance reaches zero ($t = -1$); each line is the mean ratio for one centile over quarters $t = -8$ to $t = 0$. Ratios are winsorized at the 99th percentile. A loan is classified as never having amortized if its balance-to-origination ratio is at least 0.9 at $t = -1$ and at least 90 percent of its ratio at $t = -8$, and as having declined steadily if its ratio at $t = -1$ is less than 60 percent of its ratio at $t = -8$; loans meeting neither condition are intermediate.

FIGURE A.2. WITHIN-CENTILE DISPERSION OF PRE-ZERO BALANCE RATIOS



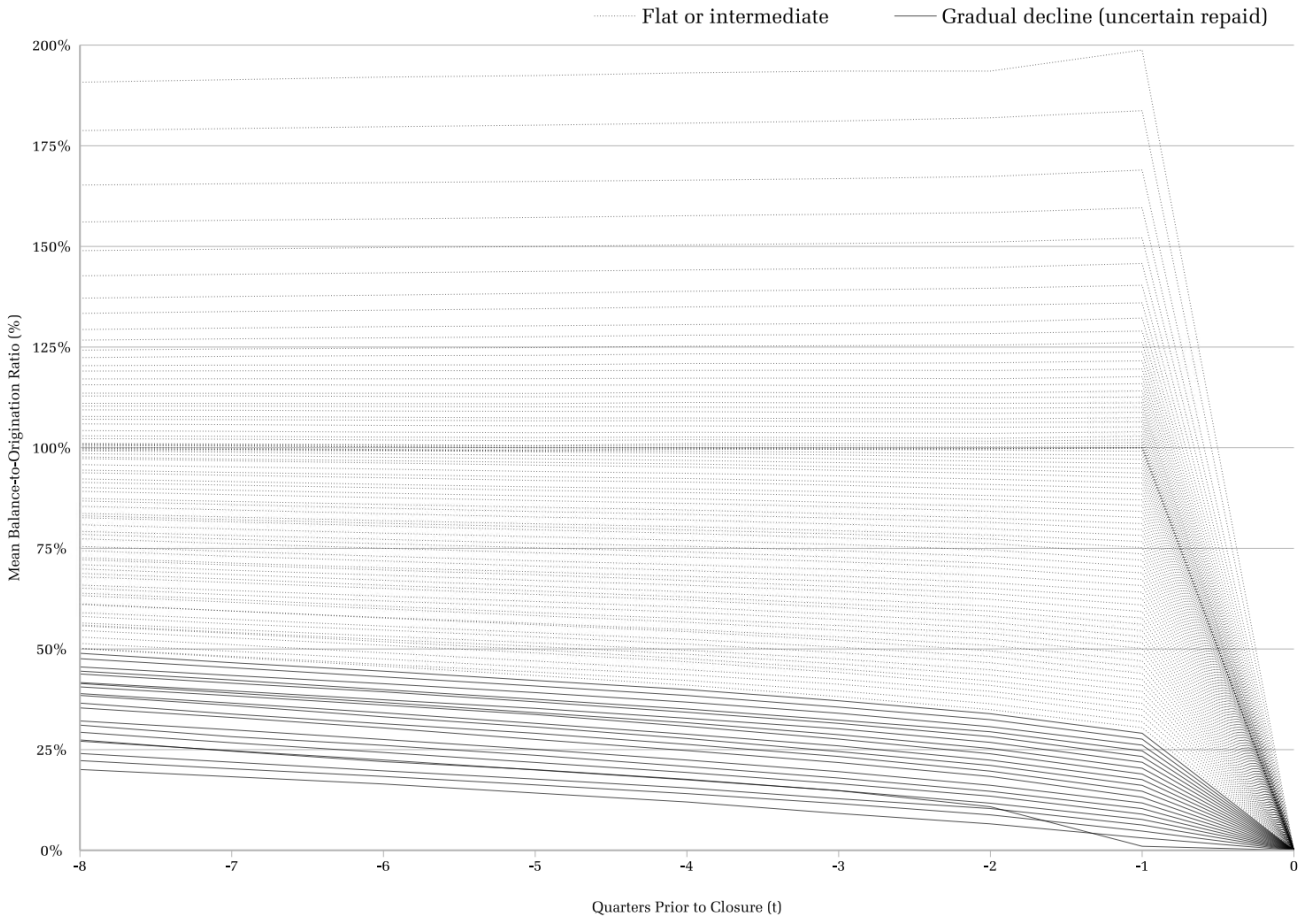
Notes: This figure plots, for each of the 100 centiles in Figure A.1, the standard deviation across loans of the balance-to-origination ratio at $t = -8$. Centiles are defined by the balance-to-origination ratio at $t = -1$, so the horizontal axis runs from loans with the lowest pre-zero balance relative to origination (left) to the highest (right).

FIGURE A.3. PRE-ZERO BALANCE TRAJECTORIES BY SUFFICIENCY OF OBSERVED PAYMENTS



Notes: This figure splits the loans in Figure A.1 according to whether the payments reported to the credit bureau over quarters $t = -8$ to $t = 0$ sum to at least 90 percent of the loan balance at $t = -8$ ("certain repaid," 7,936 loans) or not ("not certain repaid," 176,468 loans). Reported payment amounts are monthly and are multiplied by three to obtain quarterly flows. Centiles are recomputed within each panel according to the balance-to-origination ratio at $t = -1$; each line is the mean ratio for one centile. Ratios are winsorized at the 99th percentile.

FIGURE A.4. PRE-ZERO BALANCE TRAJECTORIES OF LOANS WITHOUT SUFFICIENT OBSERVED PAYMENTS

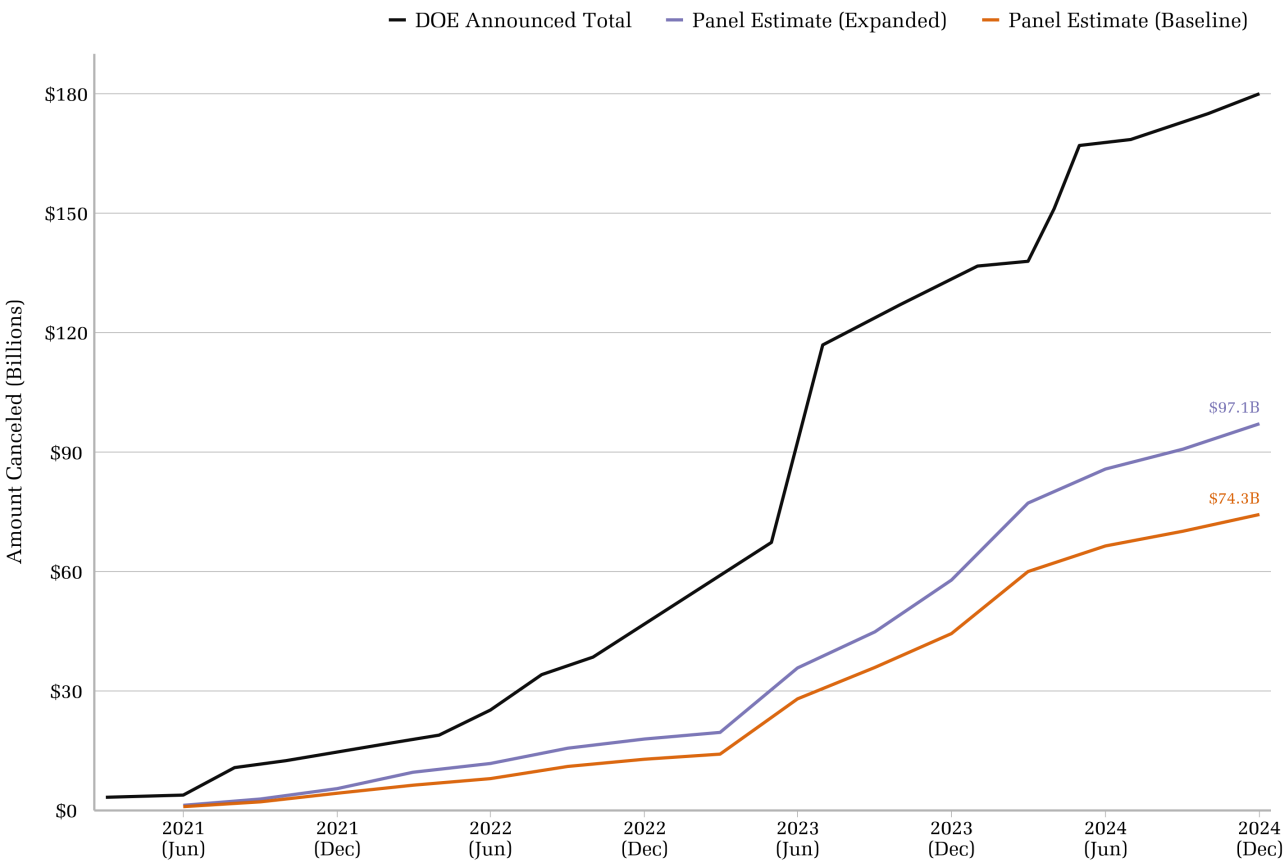


Notes: This figure plots the not-certain-repaid panel of Figure A.3. Solid lines are centiles whose mean balance-to-origination ratio at $t = -1$ is below 60 percent of its value at $t = -8$, the same criterion used to define a steady decline in Figure A.1; dotted lines are all other centiles. At the loan level, 16.6 percent of not-certain-repaid loans decline steadily by this criterion, 39.7 percent are sudden zeroes, and 43.7 percent are intermediate.

B. Reported vs. Estimated Cumulative Cancellation

To gauge whether our method of identifying loan cancellation aligns with external benchmarks, we compare our panel-based cumulative-dollar series (scaled to a national frame) with the U.S. Department of Education's (DOE) announced totals.

FIGURE B.1. CUMULATIVE STUDENT LOAN DEBT CANCELLATION, 2021–2024



Notes: This figure benchmarks our loan-cancellation classification against the U.S. Department of Education's announced totals. The black line is the DOE-reported cumulative total; the orange and purple lines are our scaled credit-panel estimates under the baseline and expanded criteria for determining loan cancellations. The DOE series is dated by announcement (approval), whereas our panel series is dated by realization on the borrower's credit file; the two therefore begin at different points—our series starts in 2021Q2, since the earliest 2021 discharges were not yet visible in credit files in 2021Q1—and the early-2021 divergence between the lines reflects the lag between approval and credit-file reporting rather than missed cancellations. DOE totals are sourced from press releases; a consolidated list of the original press-release URLs was compiled by the Center for American Progress for its "Tracker: Student Loan Debt Relief Under the Biden–Harris Administration" (Partridge and Weiss, 2024).

By the end of 2024, our approach identifies $74.3 billion in canceled student debt under the baseline criteria and $97.1 billion under the expanded criteria, compared to the $180.0 billion the Department of Education had announced by that point. Both series track the timing of the Department's announcements fairly closely, most visibly at the July 2023 income-driven repayment (IDR) account adjustment, which shows the largest single step in either series. However, both series sit below the announced cumulative total, and the gap widens over 2023 and 2024. A natural concern is that this shortfall reflects a failure of our classification rules. We argue instead that most of it is structural, arising from differences between what the Department counts and what a credit panel can observe.

The first difference is one of timing. The Department's figure counts *approved* relief, a portion of which had not yet been discharged and posted to servicers — and therefore to credit files — by the end of our observation window. Our series is dated by when a loan with a zero balance appears in the credit data for the first time, so relief approved late in a quarter enters our data only once it posts.[27] This mechanically places our cumulative total below an approval-dated benchmark at any given date.

The second, and perhaps most important, difference is that a large share of the announced total consists of discharges that are not typically observable in credit data at all. Borrower Defense and Total and Permanent Disability discharges disproportionately involve loans that were already in default and had been removed from borrowers' credit files before the discharge was granted per the findings in Briones (2025). A discharge of a tradeline that is no longer reported cannot appear as a balance falling to zero, so these categories are absent from our series regardless of how the classification rules are set. This is a limitation of the data source rather than of the methodology.

The third difference is between approval and execution. The Department counts a loan as discharged when it approves the discharge and notifies the servicer; the credit panel records

[27] This is the appropriate reference point for the borrower outcomes we study, since a borrower's credit access cannot respond to a discharge that lenders do not yet see.

a discharge when the servicer zeros the balance and reports it. The record in *Sweet v. Cardona* is the clearest illustration of how a longstanding gap can arise between the two events. The settlement approved in November 2022 required discharge, refunds, and deletion of adverse credit reporting for roughly 196,000 borrowers in the automatic relief group within one year; the Department missed that deadline, the plaintiffs moved to enforce in March 2024, and the Department did not report substantial compliance until September 2024, at which point the court found a further breach on the next group of class members and ordered relief by December 2024 (U.S. District Court for the Northern District of California, 2022). Relief that the Department's totals date to 2022 therefore posted to credit files across 2023 and 2024, with a tail beyond the end of our window.

The group discharges show the same pattern, first at the servicer level and then at the Department of Education's. In *Maldonado v. MOHELA*, borrowers covered by the Corinthian, ITT, and Westwood group discharges allege that MOHELA continued to bill them and to furnish their loans as outstanding, with interest accruing, in some cases for two years or more after the Department had notified the borrowers that they owed nothing (U.S. District Court for the Northern District of California, 2024). In *Woods v. U.S. Department of Education*, filed in September 2026, borrowers covered by the ten group discharges announced between April 2022 and January 2025—more than 1.5 million people who attended Marinello, Corinthian, ITT, Westwood, CollegeAmerica, the Art Institutes, CEHE, Drake, Lincoln Tech, and Ashford—allege that the Department, which furnishes its loans to the credit bureaus in its own name through its servicers, continues to report the discharged loans as deferred, with a balance that is owed and increases monthly and a future payment date, for as long as the servicer has not completed the accounting steps that the Department's own instructions say should take 15 to 45 days and end with deletion of the tradeline (U.S. District Court for the District of Columbia, 2026, ¶¶ 36–42, 56–69). The complaint's two named plaintiffs illustrate the lag: one, who borrowed about $65,000 to attend Ashford and was told in January 2025 that her loans would be discharged automatically, saw $71,901 reported as owed in August 2026, about $2,000 more than when

she had disputed the reporting six months earlier; the other, covered by the August 2022 ITT discharge, saw $21,586 reported four years later (¶¶ 87–120). In January 2025 the Department itself wrote to group-discharge borrowers "confirming that if your discharge is still incomplete you do not have to make any more payment on the loan(s) that we are in the process of discharging" (¶ 55).

How much announced relief has been executed is known only piecemeal. The Department has not published execution rates for its group discharges; what exists are its status reports in litigation over particular programs, which the *Woods* complaint collects: as of April 2026 it had yet to complete the discharges of 17 percent of Westwood borrowers (about 13,500 of 79,000) and reported that it "will not have meaningful additional availability during 2026" to do so, and as of June 2026 it had completed 451,300 of the 560,000 Corinthian discharges announced in June 2022, leaving 19 percent (¶¶ 64–65). Over 2025 the Department reduced Federal Student Aid's staff from 1,433 to 777 (¶ 74). In July 2026 the Project on Predatory Student Lending sued the Department under the Freedom of Information Act after it failed for more than two years to produce records on the implementation of all ten discharges, covering more than 1.5 million borrowers and $23 billion (U.S. District Court for the District of Massachusetts, 2026); the organization's public estimate, released with the *Woods* complaint, is that about $4.6 billion of that amount, owed by more than 300,000 borrowers, was still being reported to the credit bureaus as owed in September 2026. For figure B.1, the implication is that the Department's series is a ceiling on realized relief rather than a measure of it, that the steps in our series should lag the corresponding steps in the Department's by anywhere from a quarter to more than two years for the borrower defense programs, and that some announced relief does not enter our series at all because it had not been executed by the end of 2024—nor, for roughly a fifth of the group-discharge borrowers, by the fall of 2026.

The relief that credit panels *do* observe—primarily PSLF and the IDR account adjustment, which together account for the majority of the announced total—we capture at a substantially higher rate. These programs act on active, reported loans held by long-tenured borrowers,

which is the population our balance-trajectory and tenure criteria are designed to identify, and they are responsible for the sharp increases in our canceled series across the PSLF- and adjustment-heavy quarters of 2023 and 2024.

Two further features of our baseline criteria may further contribute to the gap between the cancellations we document and those reported by the Department of Education. First, after the repayment pause ended and interest and some payments resumed in late 2023, some genuine discharges are preceded by small balance declines, which our rules treat as evidence of repayment rather than cancellation and therefore exclude. Second, a minority of PSLF discharges reach zero through consolidation into a new servicer or a replacement tradeline, which our rules classify as a transfer rather than a cancellation. Both choices reflect a deliberate preference for false negatives over false positives, for reasons discussed in Section I.B. The expanded criteria, which additionally count loans carrying settlement and government-claim codes, recover part of this difference, and we report them alongside the baseline in Table B.4.

One consequence of this approach is worth stating explicitly. Because we exclude from both the treatment and control groups any zero-balance loan that does not pass our screens, our estimates apply to borrowers whose cancellations are most clearly documented in their credit files. This ensures that we are not comparing borrowers who received a cancellation to borrowers whose loans were merely misclassified, at the cost of describing a somewhat narrower population than the full set of cancellation beneficiaries.

TABLE B.1. 2019 SUMMARY STATISTICS BY QUARTERLY CANCELLATION COHORT

		Quarter of First Cancellation														
	Control	**2021Q2**	**2021Q3**	**2021Q4**	**2022Q1**	**2022Q2**	**2022Q3**	**2022Q4**	**2023Q1**	**2023Q2**	**2023Q3**	**2023Q4**	**2024Q1**	**2024Q2**	**2024Q3**	**2024Q4**
Number of Individuals	117,025	521	613	833	937	800	1,198	562	483	3,748	2,653	3,820	5,857	2,321	2,027	1,934
Mean Age	42.6	42.4	42.4	42.3	43.2	42.5	44.3	44.6	44.5	44.7	45.5	40.1	41.7	41.1	41.8	42.2
Share Non-White	21.4%	24.6%	22.0%	21.5%	25.8%	19.9%	22.3%	24.6%	27.7%	27.1%	25.9%	24.1%	27.0%	29.5%	30.4%	20.6%
Share Female	54.0%	54.3%	53.0%	53.3%	53.6%	55.6%	63.4%	59.2%	58.9%	67.9%	60.3%	55.4%	64.1%	65.4%	61.8%	50.3%
Share Homeowners	47.5%	37.6%	45.2%	52.3%	41.8%	52.6%	55.5%	52.1%	45.3%	61.5%	35.8%	32.0%	36.0%	36.5%	29.2%	46.4%
Share New Homeowners	3.6%	2.7%	2.8%	2.8%	1.8%	2.8%	2.5%	3.7%	1.7%	3.4%	2.3%	2.9%	2.5%	3.4%	2.8%	1.6%
Share with Auto Loans	62.0%	51.2%	56.4%	63.3%	51.9%	61.6%	60.0%	61.9%	62.5%	67.1%	58.5%	51.3%	55.2%	58.4%	58.6%	56.2%
Median Credit Score	711	666	693	710	668	701	713	687	660	704	639	651	649	652	623	693
Median Student Debt Canceled	$0	$17,167	$20,037	$29,955	$23,184	$24,255	$31,364	$37,667	$32,856	$47,332	$35,429	$21,276	$25,859	$24,560	$18,193	$26,762
Median Student Debt Balance	$15,821	$25,938	$24,440	$37,863	$25,801	$30,110	$34,944	$38,235	$37,116	$52,772	$38,461	$25,012	$28,256	$25,370	$16,020	$12,832
Median Non-Student Debt Balance	$40,993	$21,209	$36,102	$63,803	$23,189	$59,302	$86,979	$57,193	$41,442	$109,490	$21,610	$14,211	$20,602	$21,597	$15,960	$32,202
Median Auto Loan Balance	$5,915	$1,083	$4,229	$6,718	$1,528	$5,243	$5,217	$7,226	$6,892	$8,017	$5,160	$814	$3,461	$4,840	$5,134	$3,784
Share with Any Student Loan Delinquency	10.0%	15.5%	12.6%	7.9%	22.0%	9.9%	6.5%	10.0%	12.4%	7.1%	13.4%	12.0%	11.2%	14.0%	17.1%	5.9%
Share with Any Non-Student Delinquency	10.2%	13.1%	10.1%	8.2%	11.4%	8.5%	9.2%	11.2%	14.5%	11.9%	18.8%	14.2%	14.7%	15.7%	18.0%	9.6%
Mean Delinquent Balance Share (Non-Student)	1.8%	2.4%	1.9%	1.5%	2.5%	1.5%	2.1%	2.4%	2.8%	2.3%	4.4%	3.3%	3.1%	2.9%	4.0%	1.8%
Share with Any Loans in Collections	17.4%	25.7%	17.3%	15.1%	22.7%	17.4%	18.9%	20.3%	28.6%	17.3%	30.5%	23.5%	25.9%	27.8%	35.8%	14.0%

Notes: This table reports the 2019 characteristics of each of the 15 quarterly treatment cohorts used in estimation, together with the control group. Cohorts are defined by the quarter in which a borrower's first canceled loan reaches a zero balance under the baseline classification criteria. Table 1 in the main text pools these cohorts by calendar year. Age is measured at December 2019. Homeownership is inferred from the presence of an open mortgage or home equity line of credit. First-time homeownership is an indicator for the first quarter in which a borrower is observed with a mortgage or home equity line of credit after at least one earlier quarter in the panel without one. Student loan delinquency is an indicator for any student loan tradeline flagged delinquent in any quarter of 2019, the same measure used in the event studies. Delinquency and collections measures for non-student debt are indicators for any tradeline in the relevant status. The median canceled balance exceeds the median 2019 balance in the 2024Q3 and 2024Q4 cohorts because 44 and 62 percent of the loans canceled in those quarters were originated in 2020 or later, mostly consolidations made to qualify for the IDR account adjustment before its June 2024 deadline, and the balances on those tradelines are not part of the 2019 figure. Cohort sizes sum to 145,332 rather than the 145,453 borrowers in estimation because 121 treated borrowers have no 2019 observation and are omitted from the 2019 columns.

TABLE B.2. COHORT-SPECIFIC (QUARTERLY) AVERAGE POST-TREATMENT EFFECTS

Cohort	Home-ownership (pp)	First-time home-ownership (pp)	Mortgage balance (asinh)	Non-mtg debt, 2019 holders (log)	Credit score (points)	Score 700+ (pp)	Card limit, 2019 card-holders (asinh)	Delinquent balance (asinh)
2021Q2	5.83	0.06	0.800	0.070	15.69	6.54	0.048	−0.002
	(1.45)	(0.44)	(0.188)	(0.075)	(2.20)	(1.61)	(0.032)	(0.069)
2021Q3	4.38	−0.14	0.515	−0.083	20.11	8.62	0.074	−0.141
	(1.39)	(0.47)	(0.179)	(0.065)	(1.86)	(1.34)	(0.032)	(0.049)
2021Q4	2.05	−0.19	0.325	0.001	15.08	6.91	0.045	−0.129
	(1.12)	(0.40)	(0.149)	(0.048)	(1.64)	(1.17)	(0.021)	(0.051)
2022Q1	−0.02	0.05	0.054	0.008	18.99	5.77	−0.011	−0.148
	(0.82)	(0.29)	(0.107)	(0.049)	(1.56)	(0.98)	(0.021)	(0.059)
2022Q2	2.26	0.00	0.269	−0.030	17.83	7.06	0.030	−0.066
	(1.01)	(0.31)	(0.130)	(0.047)	(1.48)	(1.15)	(0.018)	(0.044)
2022Q3	0.29	0.28	0.029	−0.034	13.51	5.18	0.014	−0.125
	(0.61)	(0.16)	(0.079)	(0.034)	(1.24)	(0.86)	(0.015)	(0.048)
2022Q4	2.12	0.60	0.302	−0.003	16.29	7.19	0.050	−0.029
	(1.03)	(0.20)	(0.132)	(0.046)	(1.81)	(1.44)	(0.023)	(0.071)
2023Q1	3.20	0.21	0.510	0.015	11.90	8.21	0.039	0.013
	(1.20)	(0.38)	(0.157)	(0.053)	(2.06)	(1.61)	(0.022)	(0.096)
2023Q2	−0.23	0.06	−0.026	0.022	14.23	6.55	0.038	−0.030
	(0.32)	(0.10)	(0.040)	(0.017)	(0.67)	(0.51)	(0.007)	(0.028)
2023Q3	1.14	0.00	0.136	0.008	19.49	7.98	0.063	−0.020
	(0.40)	(0.16)	(0.050)	(0.020)	(0.90)	(0.63)	(0.010)	(0.046)
2023Q4	1.73	0.26	0.218	0.017	22.48	8.76	0.052	−0.044
	(0.32)	(0.12)	(0.042)	(0.018)	(0.74)	(0.52)	(0.008)	(0.036)
2024Q1	0.84	0.10	0.123	0.029	16.56	6.65	0.035	−0.001
	(0.24)	(0.11)	(0.031)	(0.013)	(0.57)	(0.41)	(0.006)	(0.031)
2024Q2	0.66	0.15	0.078	0.042	16.75	6.95	0.013	0.113
	(0.35)	(0.12)	(0.044)	(0.023)	(0.88)	(0.61)	(0.009)	(0.045)
2024Q3	0.02	0.14	−0.009	−0.027	22.20	7.51	0.004	−0.039
	(0.41)	(0.16)	(0.050)	(0.023)	(0.98)	(0.65)	(0.011)	(0.059)
2024Q4	−0.20	0.27	−0.011	−0.033	15.70	6.50	0.016	−0.056
	(0.44)	(0.14)	(0.055)	(0.025)	(0.92)	(0.73)	(0.010)	(0.043)

Notes: Each cell reports the cohort-specific average of the post-treatment event-study coefficients ($\tau \geq 0$) from equation 1, estimated with the interaction-weighted estimator of Sun and Abraham (2021) on the baseline definition of cancellation. Cohorts are defined by the quarter in which a borrower's first canceled loan reaches a zero balance. Standard errors clustered by borrower are in parentheses. Outcomes are defined as in Table 2, so "Non-mtg debt" in column (4) refers to non-mortgage, non-student debt. Later cohorts are observed for fewer post-treatment quarters, so their averages cover shorter horizons than those of earlier cohorts; the 2021Q2 cohort is observed for 18 quarters after cancellation and the 2024Q4 cohort for four. The pooled estimates in Table 2 weight these cohort-specific effects by each cohort's share of treated borrowers at each event time.

TABLE B.3. AVERAGE POST-TREATMENT EFFECTS: SUPPLEMENTARY OUTCOMES

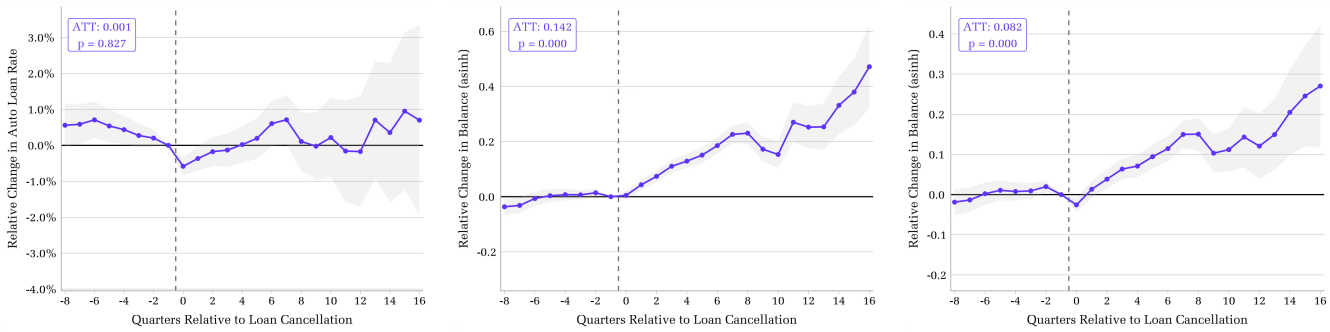
Outcome	Pre-period mean	ATT	Std. error	p-value	Detrended ATT (std. error)
Debt					
Has auto loan (pp)	52.0	0.05	0.25	0.827	0.36 (0.35)
Total non-student debt (asinh)	10.59	0.142	0.012	0.000	0.080 (0.017)
Non-mortgage, non-student debt (asinh)	9.30	0.082	0.013	0.000	0.040 (0.018)
Total non-student debt, 2019 debt holders (log)	10.76	0.051	0.008	0.000	0.059 (0.011)
Mortgage balance, 2019 homeowners (asinh)	12.81	0.014	0.004	0.000	0.013 (0.005)
Credit health					
Credit score 660 or above (pp)	69.4	5.14	0.20	0.000	4.30 (0.30)
Any delinquent non-student tradeline (pp)	5.6	−0.41	0.13	0.002	−0.19 (0.21)
Any tradeline in collections (pp)	8.0	−0.30	0.16	0.052	−0.85 (0.26)
Balance in collections (asinh)	0.55	−0.023	0.011	0.040	−0.055 (0.019)

Notes: See notes to Table 2. The aggregate debt measures (asinh) are estimated on all borrowers and combine extensive- and intensive-margin changes; the measures on 2019 debt holders and 2019 homeowners are estimated on borrowers who held the relevant balance in 2019 and in the quarter observed. The 2019 homeowner sample contains 64,996 borrowers and the 2019 debt holder sample 137,568. Collections balances include all tradelines in collections. Event-study plots for these outcomes are in Figure B.2.

TABLE B.4. AVERAGE POST-TREATMENT EFFECTS UNDER THE BASELINE AND EXPANDED DEFINITIONS OF CANCELLATION

Outcome	Baseline	Expanded
	ATT (std. error)	**ATT (std. error)**
Main outcomes		
Homeownership (pp)	1.18 (0.16)	1.30 (0.15)
First-time homeownership, quarterly (pp)	0.11 (0.05)	0.10 (0.05)
Mortgage balance (asinh)	0.160 (0.021)	0.184 (0.019)
Non-mortgage, non-student debt, 2019 debt holders (log)	0.008 (0.008)	0.014 (0.007)
Credit score (points)	17.38 (0.30)	15.94 (0.26)
Credit score 700 or above (pp)	7.09 (0.21)	6.40 (0.18)
Credit card limit, 2019 cardholders (asinh)	0.036 (0.004)	0.037 (0.003)
Delinquent non-student balance (asinh)	−0.040 (0.013)	−0.039 (0.011)
Supplementary outcomes		
Has auto loan (pp)	0.05 (0.25)	0.11 (0.22)
Total non-student debt (asinh)	0.142 (0.012)	0.147 (0.011)
Non-mortgage, non-student debt (asinh)	0.082 (0.013)	0.083 (0.012)
Total non-student debt, 2019 debt holders (log)	0.051 (0.008)	0.065 (0.007)
Mortgage balance, 2019 homeowners (asinh)	0.014 (0.004)	0.019 (0.003)
Credit score 660 or above (pp)	5.14 (0.20)	4.60 (0.17)
Any delinquent non-student tradeline (pp)	−0.41 (0.13)	−0.39 (0.11)
Any tradeline in collections (pp)	−0.30 (0.16)	−0.24 (0.13)
Balance in collections (asinh)	−0.023 (0.011)	−0.022 (0.010)
Borrowers	145,453	154,792
Borrower-quarter observations	5,208,815	5,542,878

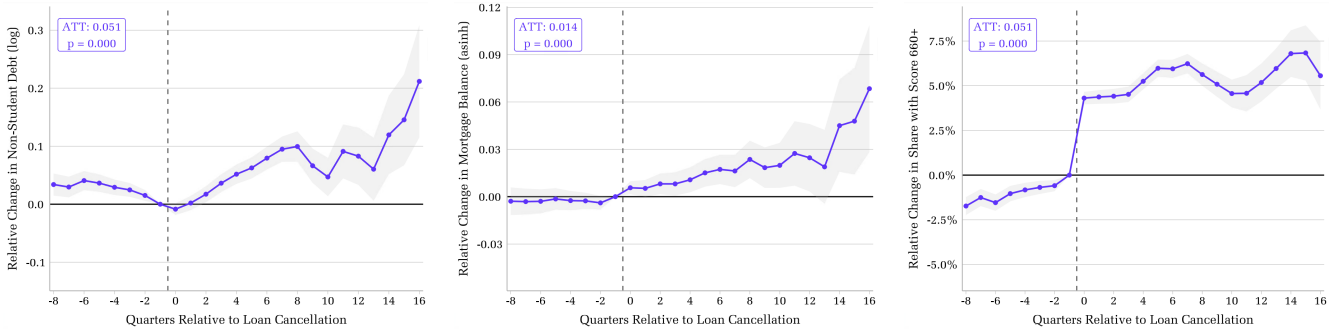
Notes: Each cell reports the average of the post-treatment event-study coefficients ($\tau \geq 0$) from equation 1, estimated with the interaction-weighted estimator of Sun and Abraham (2021), with standard errors clustered by borrower in parentheses. The baseline columns reproduce Table 2 and Table B.3. The expanded columns re-estimate every outcome on the sample defined by the expanded criteria of Section I.B, which additionally treat borrowers whose loans carry settlement or government-claim codes. Outcomes are defined as in Table 2.



(a) Has auto loan (pp)

(b) Total non-student debt (asinh)

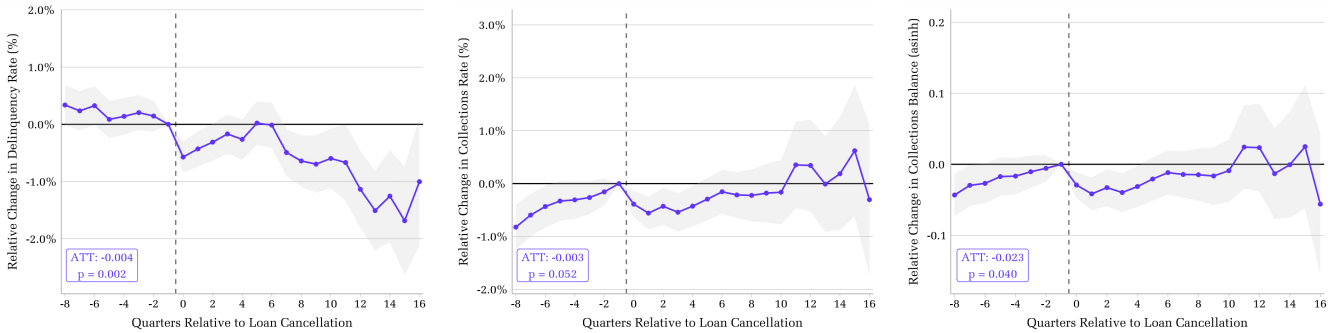
(c) Non-mortgage, non-student debt (asinh)



(d) Total non-student debt, 2019 debt holders (log)

(e) Mortgage balance, 2019 homeowners (asinh)

(f) Credit score 660 or above (pp)



(g) Any delinquent non-student tradeline (pp)

(h) Any tradeline in collections (pp)

(i) Balance in collections (asinh)

FIGURE B.2. EVENT STUDY ESTIMATES: SUPPLEMENTARY OUTCOMES

Notes: See notes to Figure 2. Outcomes are defined as in Table B.3.

C. Loan Cancellation Effects by Race and Gender

This appendix reports estimates of equation 1 on subsamples defined by gender and by race, for the eight outcomes in the main text. The credit panel directly reports individual borrowers' gender, which we believe is imputed using an algorithm based on first names (which are known to the credit bureau but not available to researchers). Gender is missing for 12,290 borrowers, who are omitted from the gender split; race is available for every borrower. Race is reported as a probability distribution over race categories estimated from last names and addresses. We assign the race group (either White or Non-White) based on the plurality of those probabilities. Table C.1 reports the ATTs, and Figures C.1 through C.4 plot the event studies with the two groups on the same axes.

TABLE C.1. AVERAGE POST-TREATMENT EFFECTS BY GENDER AND RACE

Outcome	Men	Women	White	Non-White
Homeownership and debt				
Homeownership (pp)	1.36	1.08	1.29	0.86
	(0.28)	(0.22)	(0.19)	(0.30)
First-time homeownership, quarterly (pp)	0.14	0.12	0.14	0.00
	(0.09)	(0.07)	(0.06)	(0.11)
Mortgage balance (asinh)	0.191	0.147	0.178	0.109
	(0.037)	(0.027)	(0.025)	(0.039)
Non-mortgage, non-student debt, 2019 debt holders (log)	−0.003	0.009	0.004	0.019
	(0.014)	(0.011)	(0.010)	(0.016)
Credit health				
Credit score (points)	17.78	17.29	16.98	18.25
	(0.51)	(0.41)	(0.34)	(0.64)
Credit score 700 or above (pp)	7.42	7.03	7.12	6.85
	(0.36)	(0.29)	(0.24)	(0.42)
Credit card limit, 2019 cardholders (asinh)	0.035	0.034	0.037	0.031
	(0.006)	(0.005)	(0.004)	(0.008)
Delinquent non-student balance (asinh)	−0.033	−0.043	−0.031	−0.063
	(0.020)	(0.018)	(0.014)	(0.030)
Borrowers	59,588	73,575	113,098	32,355
Borrower-quarter observations	2,132,132	2,636,070	4,049,667	1,159,148

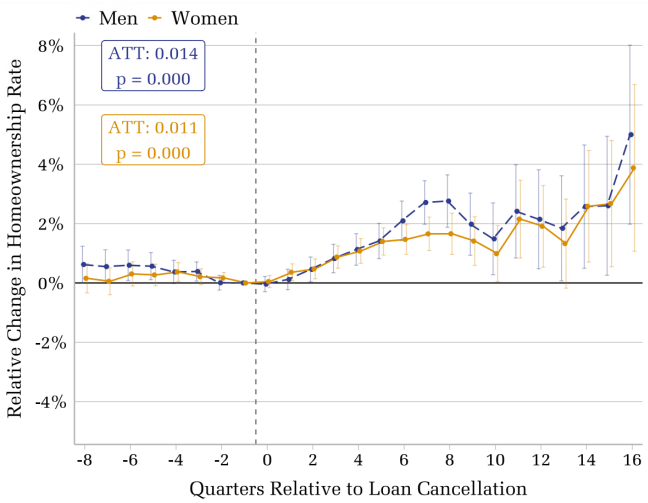
Notes: Each cell reports the average of the post-treatment event-study coefficients ($\tau \geq 0$) from equation 1, estimated separately on the indicated subsample with the interaction-weighted estimator of Sun and Abraham (2021) and the baseline definition of cancellation. Standard errors clustered by borrower are in parentheses. Outcomes are defined as in Table 2. Gender and race are as reported in the credit panel; see the text for how they are constructed. Borrowers with missing gender or race are omitted from the respective splits. Full-sample estimates are in Table 2.

Analysis by Gender

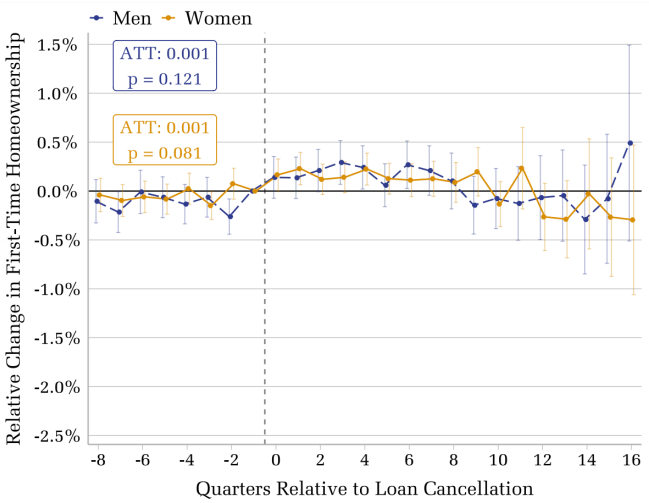
The effects for men and women are similar on every outcome. Men show a somewhat larger homeownership effect (1.36 versus 1.08 percentage points) and a larger mortgage balance effect (0.19 versus 0.15), and the first-time homeownership effects are the same (0.14 and 0.12 percentage points). Credit scores rise by about 17 points for both groups, the share with a score of 700 or above rises by 7 percentage points for both, and credit card limits rise by 3.5 percent for both. Delinquent balances fall for both groups, by somewhat more for women.

Non-mortgage debt among 2019 debt holders does not change for either group. None of the differences between men and women is statistically distinguishable from zero.

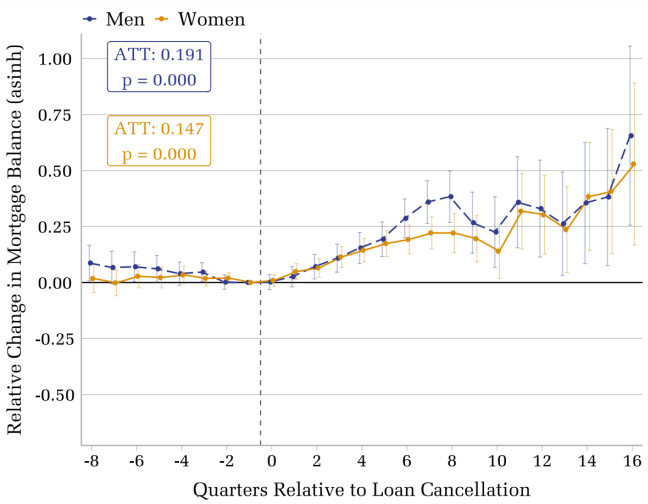
FIGURE C.1. EVENT STUDY ESTIMATES BY GENDER: HOMEOWNERSHIP AND DEBT



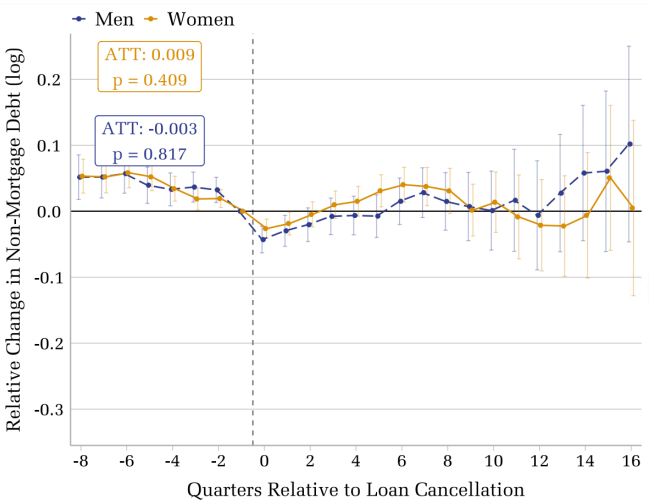
(a) Homeownership (percentage points)



(b) First-time homeownership (percentage points)



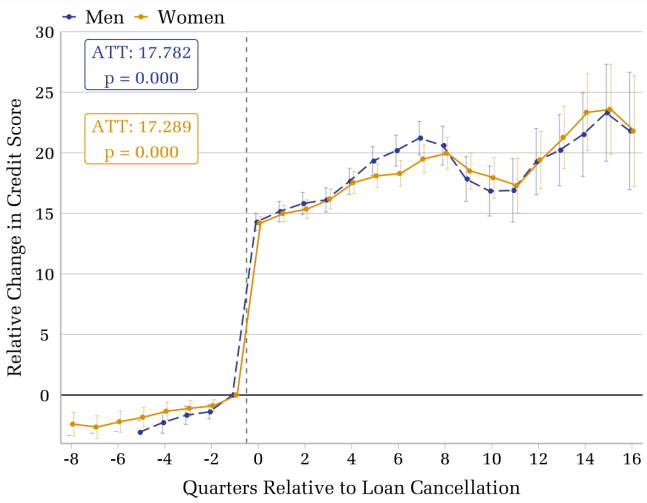
(c) Mortgage balance (asinh)



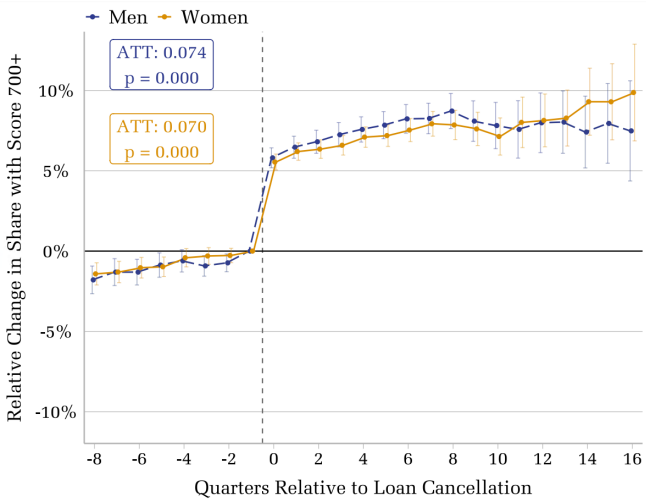
(d) Non-mortgage, non-student debt, 2019 debt holders (log)

Notes: Each panel plots the coefficients β_τ from equation 1, estimated separately for men and women with the interaction-weighted estimator of Sun and Abraham (2021) on the baseline definition of cancellation. The quarter before cancellation ($\tau = -1$) is the omitted reference period and the dashed vertical line marks the cancellation quarter. Error bars are 95 percent confidence intervals with standard errors clustered by borrower. The ATT reported for each group is the average of its post-cancellation coefficients. Outcomes are defined as in Figure 2.

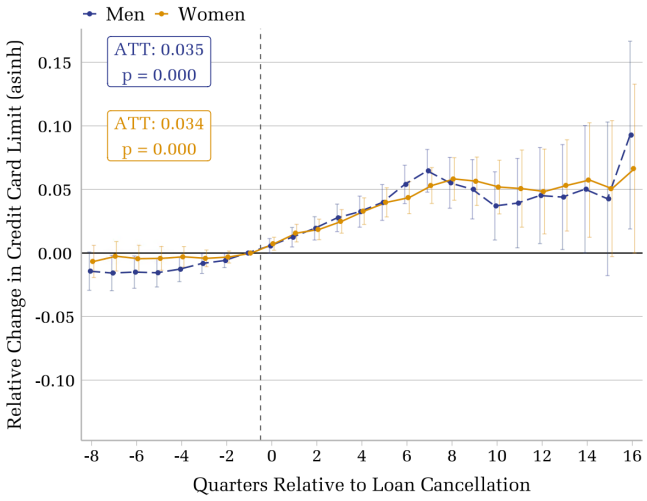
FIGURE C.2. EVENT STUDY ESTIMATES BY GENDER: CREDIT HEALTH



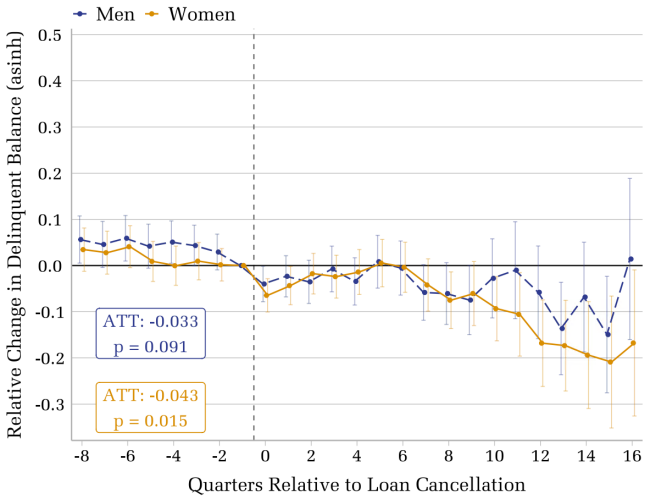
(a) Credit score (points)



(b) Credit score 700 or above (percentage points)



(c) Credit card limit, 2019 cardholders (asinh)



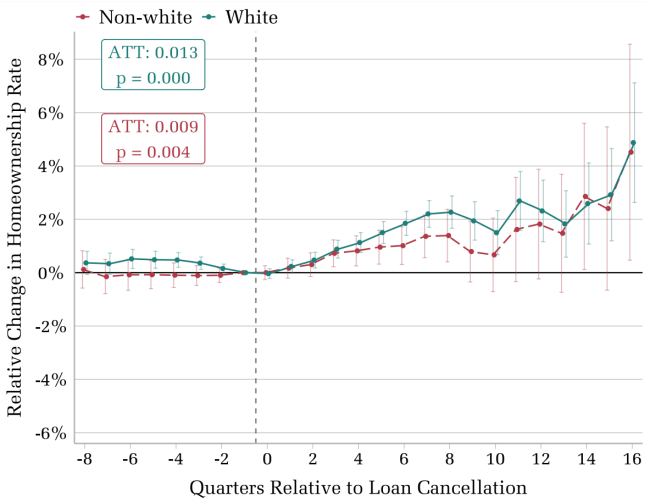
(d) Delinquent non-student balance (asinh)

Notes: See notes to Figure C.1. Outcomes are defined as in Figure 3.

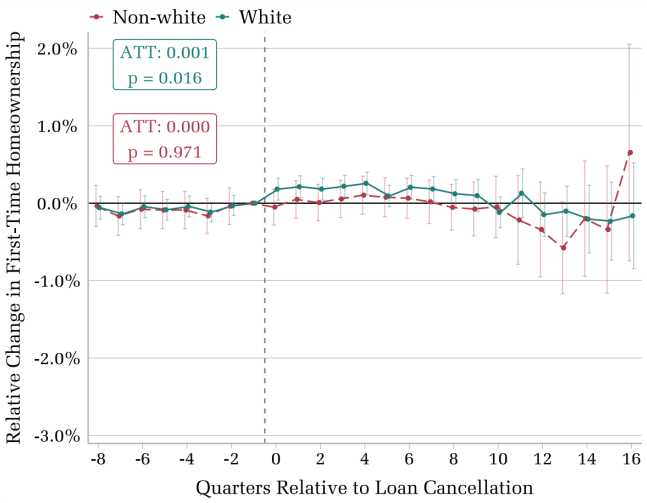
Analysis by Race

The effects by race differ more. White borrowers show a larger homeownership effect than non-white borrowers (1.29 versus 0.86 percentage points) and a larger mortgage balance effect (0.18 versus 0.11), and the first-time homeownership effect is present for white borrowers (0.14 percentage points, $p = 0.01$) and absent for non-white borrowers (0.00, standard error 0.11). On the credit side the ordering reverses. Non-white borrowers show a larger credit score gain (18.3 versus 17.0 points), a larger decline in delinquent balances (-0.063 versus -0.031), and a larger decline in the share with any collections account (-0.61 versus -0.21 percentage points; not shown in the table), while the credit card limit and 700-threshold effects are similar across the two groups. Non-white borrowers in the treated cohorts entered the period with lower credit scores and more distress, so the pattern is consistent with the cohort heterogeneity in the main text: cancellation moved borrowers who were near the margin of homeownership into it, and repaired the credit standing of borrowers who were not. The homeownership and first-time homeownership differences between white and non-white borrowers are each about one standard error apart, so we read them as suggestive rather than established.

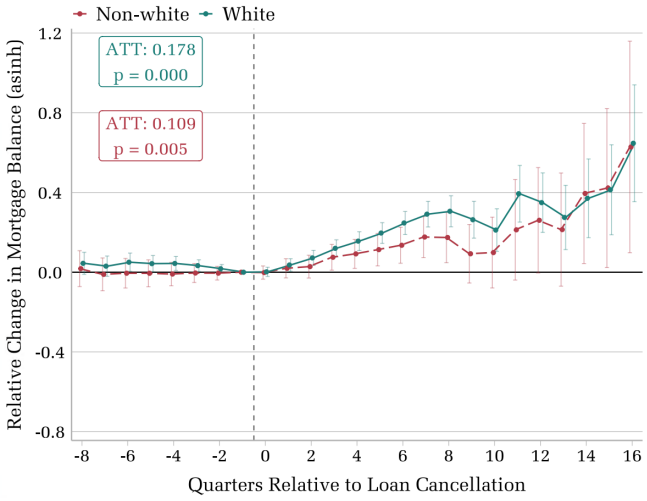
FIGURE C.3. EVENT STUDY ESTIMATES BY RACE: HOMEOWNERSHIP AND DEBT



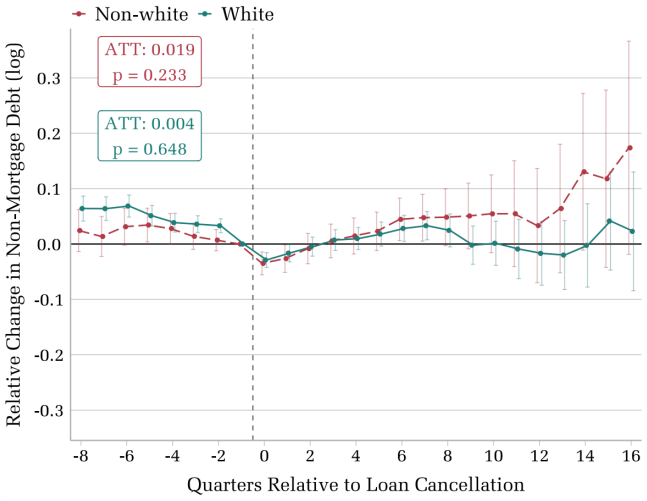
(a) Homeownership (percentage points)



(b) First-time homeownership (percentage points)



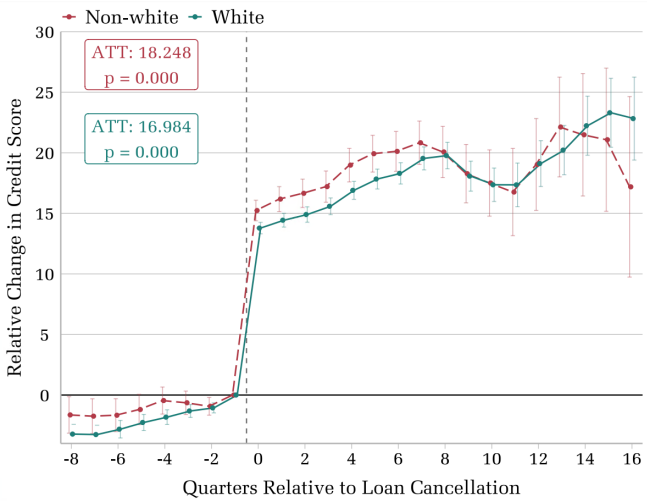
(c) Mortgage balance (asinh)



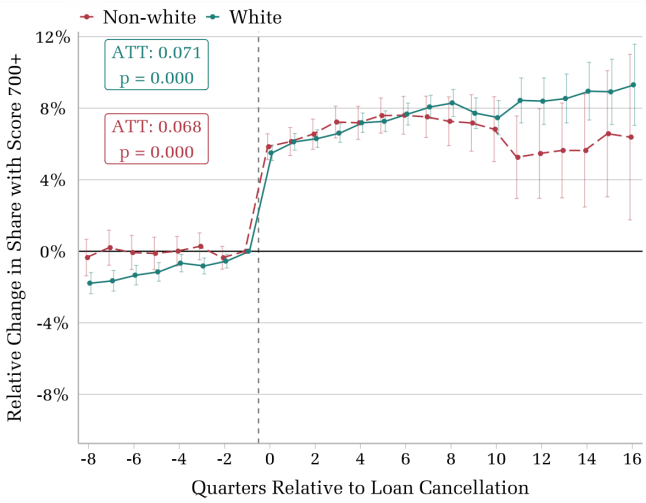
(d) Non-mortgage, non-student debt, 2019 debt holders (log)

Notes: See notes to Figure C.1. Estimates are for white and non-white borrowers separately.

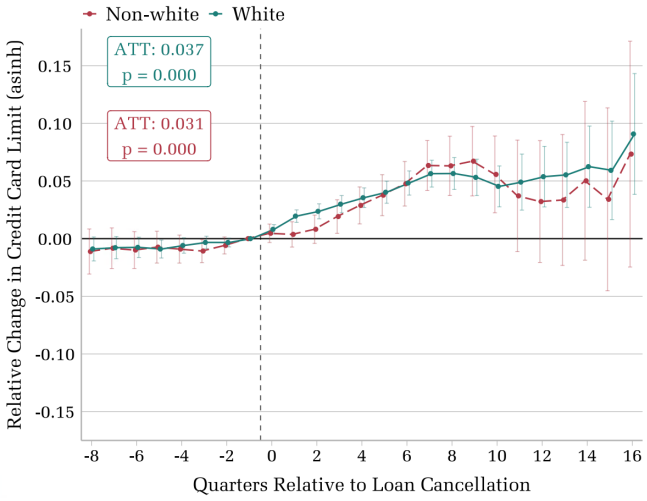
FIGURE C.4. EVENT STUDY ESTIMATES BY RACE: CREDIT HEALTH



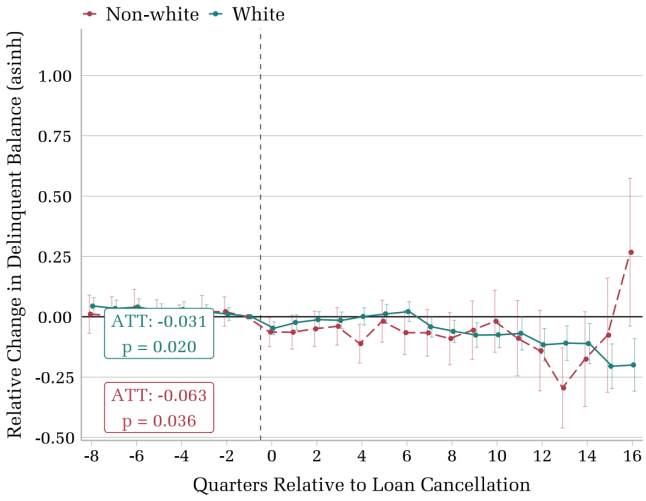
(a) Credit score (points)



(b) Credit score 700 or above (percentage points)



(c) Credit card limit, 2019 cardholders (asinh)



(d) Delinquent non-student balance (asinh)

Notes: See notes to Figure C.1. Estimates are for white and non-white borrowers separately. Outcomes are defined as in Figure 3.