Debt Moratoria:
Evidence from Student Loan Forbearance*

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Abstract

We evaluate the effects of the 2020 student debt moratorium that paused payments for student loan borrowers. Using administrative credit panel data, we show that the payment pause led to a sharp drop in student loan payments and delinquencies for borrowers subject to the debt moratorium, as well as an increase in credit scores. We find a large stimulus effect, as borrowers substitute increased private debt for paused public debt. Comparing borrowers whose loans were frozen with borrowers whose loans were not frozen due to differences in whether the government owned the loans, we show that borrowers used the new liquidity to increase borrowing on credit cards, mortgages, and auto loans rather than avoid delinquencies. The effects are concentrated among borrowers without prior delinquencies, who saw no change in credit scores, and we see little effects following student loan forgiveness announcements. The results highlight an important complementarity between liquidity and credit, as liquidity increases the demand for credit even as the supply of credit is fixed.

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1 Introduction

During economic disruptions, governments may seek to stimulate the economy and smooth consumption by making transfers to households. Economists have had a longstanding debate regarding whether and how to make such transfers. If the disruption is transitory, then households may primarily require liquidity to raise short-term consumption or insure against adverse shocks.[1] Policymakers may then turn to debt moratoria, which inject immediate liquidity without large long-term fiscal costs for the government. If the disruption has permanent effects, however, then households may require compensation for permanent income losses in addition to liquidity. Policymakers may then turn to direct cash transfers, which address both liquidity and permanent shocks, but at higher fiscal cost.

In this paper, we study the effects debt moratoria, the lower cost option, on borrowing, consumption, and loan repayment during times of economic distress. Our empirical context is the 2020 student loan payment freeze in the U.S. that led to a complete stoppage of student loan payments for most borrowers. Student loans were the second largest source of household debt in the United States in 2020, with an approximate $1.7 trillion outstanding. As part of relief during the 2020 coronavirus pandemic, the federal government ordered a temporary pause in student loan payments, aimed at relieving households from debt burdens. This pause was subsequently extended until the end of 2022. We study the effects of the payment pause by using historical features of the federal student loan program: due to the ownership of loans, a subset of borrowers saw no change in payments. We use administrative credit panel data and compare borrowers who saw payment freezes to those who had to continue paying down their loans. The payment freeze led to a sharp drop in student loan payments and delinquencies for borrowers subject to the debt moratorium, as well as an increase in credit scores.

Our paper makes three central contributions. First, we evaluate whether debt moratoria increase consumption during a period of economic distress. We estimate that the student debt payment pause immediately increased consumption, as borrowers used the new liquidity to increase borrowing on credit cards, mortgages, and auto loans rather than avoid delinquencies. Second, we study the mechanisms explaining this effect and argue that the effects are driven

[1] Liquidity may prevent large aggregate losses in the presence of externalities arising from household balance sheets (Mian, Rao and Sufi, 2013) or debt overhang (Donaldson, Piacentino and Thakor, 2019).

by an interaction between liquidity and credit demand, rather than credit supply or balance changes. The estimated effects are concentrated among borrowers without prior delinquencies who likely saw no change in their credit supply. Further, we find no evidence that borrowers reacted to expected changes in balances from a student loan forgiveness announcement. The results thus highlight an important complementarity between liquidity and credit, as liquidity increases the demand for credit. Third, we compare the relative policy effectiveness of debt moratoria versus the promise of direct transfers. We estimate the student debt payment pause has a larger effect on immediate consumption than the proposed balance discharge, which suggests that less costly liquidity-targeting policies may be more effective than direct transfers.

Our empirical strategy uses variation in the payment stoppage driven by historical details affecting legal loan ownership. Prior to 2010, the federal government operated two nearly identical loan programs, the Direct Loan (DL) program and the Federal Family Education Loan (FFEL) program. The programs were identical in all loan terms; the only difference was the source of the funds and ultimate ownership of loans. Under the DL program, the source of funds was the US Treasury, while for the FFEL program the source of funds was private banks, which carried guarantees from the government. In 2020, the federal government was only able to legally pause payments for federally owned loans, under the DL program, and could not pause payments for FFEL loans under the guarantee program. This generated essentially random variation for borrowers from cohorts which received loans under both the FFEL and DL programs, with most borrowers likely being unaware of the source of funds for their federal loans.

To examine the effects of the payment pause, we compare DL and FFEL borrowers in similar cohorts who received different exposure to the payment pause. For DL borrowers their payments drop, on average, by $138 relative to FFEL borrowers following the policy announcement. We use a standard difference-in-difference approach, and study borrowing and loan delinquency behavior following the policy announcement. We use national and comprehensive administrative data from a large credit bureau, TransUnion. The data comprise a panel sample consisting of one in ten individuals with a credit history in the United States.

Consistent with the policy, we find a sharp reduction in payments for borrowers subject to the payment pause, which leads to a sharp rise in balances. By the end of 2021, borrowers

subject to the payment freeze had an additional $1,500 in outstanding student loan balances relative to those that did not see a payment stoppage. Borrowers subject to the pause are also 0.8 percentage points less likely to be delinquent on their student loans, and have significantly higher credit scores. We see little effect, however, on delinquencies for non-student loans.

Despite having higher cash on hand, borrowers do not use their additional liquidity to pay down other debt. In fact, household leverage rises as borrowers make higher payments on other loans, and mortgage, auto, and credit card debt rise. Overall, excluding student loans, household leverage increases by $1,200 (3%) for households subject to the pause. Student loan balances increase by a similar amount. The results suggest that the payment pause led to higher durable and non-durable consumption in the short term, but higher overall leverage, consistent with binding liquidity constraints.

We find that the effects – except for the increased credit scores – are concentrated among borrowers who have not previously been delinquent on a loan. These borrowers likely had no change in their supply of credit. Instead, their increased use of credit implies that their demand for credit increases as they have more liquidity. For instance, these borrowers may have required liquidity to make down payments or initial monthly payments after taking out new loans. This results suggests a complementarity between liquidity and use of credit.

We then estimate the effects of a second policy change that targets loan balances. In August, 2022 Biden Administration issued executive an executive order to cancel between $10,000 and $20,000 in student loan debt.[2] Like the payment pause, this policy only applied to DL borrowers, not FFEL borrowers. We estimate whether the policy announcement affected borrowers' use of credit and find no impact.

Our results provide new evidence on debt moratoria, a commonly used policy during economic crises.[3] First, we estimate large short-term effects. That households increase their consumption in response to the liquidity shock indicates that policies targeting liquidity may be particularly effective in helping households maintain consumption levels and stimulating the

[2]As of December 2022, this proposal is undergoing legal challenges and is blocked by courts.

[3]While we focus on a particular application, temporary forbearance programs pausing payments during crises have existed since at the least the Great Depression (Rose, 2011), and have continued during recent downturns. Along with the national student loan payment pause we study, the CARES act included provisions to pause mortgage payments. Several states such as California also paused mortgage payments in 2020. Cherry et al. (2021) provide an overview of pandemic relief policies. The 2008 financial crisis also saw several state and federal programs pausing loan payments, such as HAMP.

economy during periods of economic distress. Second, we find that debt moratoria have long-run effects through households' increase credit use. This effect may be positive if households enjoy higher future consumption from making productive investments or durable purchases; the effect may be negative if households face effects from future debt overhang.

The results also relate to the ongoing policy debate over how to provide relief to student loan borrowers. The effects of student loan burdens have received considerable policy attention, with policy proposals ranging from full forgiveness to more modest capped proposals. Much of the ongoing policy debate centers around the effects of student loan payments on other credit outcomes, and whether these effects are driven by the need for liquidity, or due to the effects of high debt burdens hindering borrowing through damaging credit scores or otherwise lessening the ability to borrow. For example the August 2022 executive order cancelling student debt balances noted *high monthly payments and ballooning balances that make it harder for them to build wealth, like buying homes, putting away money for retirement, and starting small businesses.* The announcement further noted that high student debt burdens and delinquencies were lowering credit scores, with adverse impacts. On the one hand, if students have short-term liquidity needs, policies effectively extending maturity terms, such as Income-Driven Repayment, can alleviate burdens. On the other hand, if students have a longer term structural inability to pay, then discharging debt, a costlier option from a fiscal perspective, may be important. Our results are consistent with decreased liquidity as the key constraint on student loan borrowers, such that the less costly student debt policies may still be effective.

This paper primarily joins a finance literature on household leverage that studies the impacts of liquidity and related government policies on debt and consumption outcomes. A large body of work focuses on times of crisis and studies the response of consumption and debt to government crisis policies, such as loan relief (Agarwal et al., 2017), household leverage (Mian and Sufi, 2011), monetary policy (Di Maggio et al., 2017), and quantitative easing (Di Maggio, Kermani and Palmer, 2020). Several papers have also studied how debt and consumption respond to income and wealth shocks (Agarwal, Liu and Souleles, 2007; Mian and Sufi, 2009; Agarwal and Qian, 2014; Baker, 2018; Aydin, 2022). Aydin (2021) studies the effect of loan forbearance on delinquency in a debt relief experiment,and Ganong and Noel (2020) study how liquidity versus balance relief affect loan repayment. Indarte (forthcoming) also finds

that liquidity plays an important role in household bankruptcy decisions. Other work studies consumption, debt, and employment responses to wealth shocks (Chodorow-Reich, Nenov and Simsek, 2021) and firm responses (Giroud and Mueller, 2017). This paper contributes by providing new evidence on the effects of temporary loan payment pauses, a common policy pursued during recessions. The fact that we find large increases in borrowing suggests that liquidity constraints are important as opposed to wealth effects. The absence of results regarding loan delinquency in other credit markets also suggests that liquidity is not an important driven of loan default, at least in the period studied. Finally, our results caution that temporary debt relief may lead to greater household leverage in the future and associated concerns with financial stability and debt overhang (Donaldson, Piacentino and Thakor, 2019; Mian, Rao and Sufi, 2013).

This paper also joins a rapidly growing literature on student loans, the second largest source of household debt in the United States.[4] Specifically, this study analyzes a major policy intervention in the student loan market. Previous work has focused on the effects of loan forgiveness (Catherine and Yannelis, 2022; Di Maggio, Kalda and Yao, 2019), loan limits (Black et al., 2020; Goodman et al., 2021), alternative repayment plans (Mueller and Yannelis, 2019, 2022; Herbst and Hendren, 2021), or maturity extension (Boutros, Clara and Gomes, 2022). Amromin and Eberly (2016), Lochner and Monge-Naranjo (2016), and Yannelis and Tracey (2022) provide reviews of the literature. Briones, Powell and Turner (2023) also look at the payment pause, with a focus on the distributional effects. This paper contributes to this area in two ways. First, we provide real evidence on the effects of student loan payments on consumption and debt outcomes.[5] Second, our results speak to the ongoing debate on student debt forgiveness. Liquidity from extra payments has large effects on outcomes even in the absence of debt forgiveness, and lower payments may be offset by further borrowing.

The remainder of this paper is organized as follows. Section 2 discusses institutional details regarding student loans in the United States, and our empirical strategy to capture the effects of the payment pause. Section 3 introduces the data used in the paper. Section 4 presents the

[4] In a narrow sense, this paper also joins a literature on guaranteed versus direct lending, which largely focuses on smaller business rather than household lending (Erel and Liebersohn, 2022; Granja, Leuz and Rajan, 2022a; Granja, Makridis, Yannelis and Zwick, 2022b).

[5] Chakrabarti, Gorton and Lovenheim (2020) study the effects of state appropriations for higher education on similar outcomes.

main results and discusses their implications. Section 5 concludes.

2 Institutional Details and Empirical Strategy

2.1 Student Loans

In January 2020, there were approximately 45 million student loan borrowers, with an outstanding balance of over $1.7 trillion. This was the second largest source of household debt in the United States, after mortgages, and unlike other consumer loans, the federal government directly disburses or guarantees the vast majority of student loans. The average student loan borrower holds $37,693, conditional on borrowing, and repayment typically begins six months after separating from school. Prior to 2009, the vast majority of federal student loans were in a ten-year fixed payment plan, called the Standard Plan. Since 2009, students have increasingly enrolled in income-driven repayment (IDR) plans. By 2017, one quarter of students and half of balances were in IDR (Karamcheva, Perry and Yannelis, 2020).

Modern federal student loan programs were established with the Higher Education Act in 1965. Congress sets interest rates and limits, and students take loans through financial aid offices at their respective institutions. Initially, private banks provided capital for students loans, with parameters set by the government under the FFEL Program. These loans were owned by lenders and guaranteed by the government; in the case of default, lenders were reimbursed. In 1992, the DL program was created under which loans were directly made by the US Treasury. For many years, both the FFEL and DL programs existed side by side, and colleges could choose the source of funds. Despite the different source of funds and ownership, FFEL and DL loans were otherwise identical in terms of limits, interest rates, and repayment plans available, and many students likely were unaware of the different source of funds when taking loans through their schools' financial aid offices. In 2010, following analysis and a bribery scandal of college administrators, the Health Care and Education Reconciliation Act of 2010 eliminated new loans under the FFEL program.[6] Since 2010, all federal student loans have been made under the DL program.

[6] See the New American Foundation for a discussion of the elimination of the FFEL program.

2.2 2020 Payment Pause

The CARES act enacted a payment pause on March 27, 2020, as part of relief during the 2020 COVID-19 pandemic. The payment pause included a suspension of loan payments, the forgiveness of interest, and the ending of collections on defaulted loans. Initially set to last three months, the payment pause was subsequently extended via executive action several times through June 30, 2023.

The pause applied to the majority of borrowers, but many borrowers were ineligible as a result of the ownership status of loans through the history of the DL and FFEL programs. Due to legal authority, only loans owned by the Department of Education were eligible for the payment pause. All loans made under the DL program were owned by the Department of Education, and thus eligible, along with a smaller number of FFEL program loans which were bought by the Department of Education.[7] FFEL program loans, being owned by banks, were ineligible for the payment pause despite the fact that these loans were made under federal programs and had identical terms to loans made under the DL program. Approximately 12 million FFEL loan borrowers in repayment saw no pause in payments, while 25 million DL borrowers in repayment saw a pause in payments due to the student loan forbearance relief.

2.3 2022 Loan Forgiveness Announcement

While our focus is on the payment pause, a second policy targeted DL borrowers. In August 2022, the White House announced substantial loan forgiveness for many borrowers through an executive order. Most student loan borrowers where scheduled to receive $10,000 in forgiveness, while borrowers who received Pell grants were promised $20,000 in forgiveness. Like the earlier forbearance, this action only applied to DL borrowers, as White House lawyers deemed that the President did not have the legal authority to cancel FFEL loans due to their ownership structure. The announcement was widely publicized and reported in the media. By the end of 2022, the policy was not yet implemented following successful legal challenges.

[7] In the first quarter of 2020, there was approximately $250 billion outstanding of lender held FFEL loans, while $88 billion FFEL loans were held by the Department of Education. The remaining volume of federal loans are predominantly in the DL program. Federal Student Aid provides more information on loan status, and The Department of Education provides further information on eligibility for the payment pause.

2.4 Empirical Strategy

The payment pause's eligibility rules suggest a difference-in-difference empirical strategy. We compare how outcomes differentially evolve for borrowers eligible for the payment pause (DL borrowers) versus those ineligible (FFEL). Let i index borrowers and t index month-year. We model outcome y_{it} as:

$$y_{it} = \beta Eligible_i Post_t + \mu_i + \tau_t + \epsilon_{it}, \tag{1}$$

where $Eligible_i$ is an indicator for whether i is a DL borrower, $Post_t$ is an indicator for whether the month is March, 2020 or later, and μ_i and τ_t are individual and time fixed effects, respectively. In all specifications, we cluster our standard errors at the level of the borrowing cohort – the year when the borrower took out her last student loan – as average eligibility varies across borrowing cohorts. When clustering at the individual level, standard errors are predictably smaller.

Our identification assumption is that in the absence of the payment pause, outcomes for the DL and FFEL borrowers would have followed parallel trends. To assess the existence of pre-trends and to trace out how the treatment effects vary over time, we also estimate a dynamic difference-in-difference specification. Our estimating equation is:

$$y_{it} = \sum_t \beta_t Eligible_i + \mu_i + \tau_t + \epsilon_{it}, \tag{2}$$

where we include 12 months prior to the policy change and 18 months after the policy change. We normalize the coefficient for December, 2019 to zero such that all treatment effects are relative to levels a few months before the COVID pandemic reached high levels in the US.[8]

Our specifications' additive structure may be a poor fit for our context for two reasons. First, because the FFEL program was eliminated in 2010 while the DL program has continued, eligible borrowers are likely to be at different points in the lifecycle from ineligible borrowers. If lifecycle effects are nonlinear, then a parallel trends assumption is unlikely to hold. Further, many FFEL borrowers are old enough that any current student loan balance implies

[8] A recent literature has found that two-way fixed effect estimators of difference-in-difference specifications can lead to negative weights on treatment effects (Roth et al., 2022). Our analysis is robust to these considerations, as the treatment occurs at the same point for all treated units and random assignment is not conditional on further covariates.

non-standard repayment plans or past delinquency or forbearance. We address these concerns by restricting our sample of eligible (DL) borrowers to those whose most recent student loan was opened in 2010 or earlier. This restriction places our eligible and ineligible groups at similar parts of the lifecycle and with similar past events related to student loan repayment.

Second, the structure of loans means additivity for one outcome is unlikely to mean additivity for another. As an example, if our eligible and ineligible groups have pre-treatment differences in loan balance, then the nonlinearity of repayment plans (because of compounding interest) would imply differential trends in minimum payment due, even in the absence of treatment. We address this second concern by investigating the robustness of our results to varying fixed effects. Specifically, instead of month fixed effects, we use fixed effects for each payment group-month. Payment group is the borrower's decile in the distribution of minimum payments as of December, 2019. These fixed effects soak up initial variation in balances. We interact these with time to capture nonlinearities.[9]

In a final analysis, we examine whether the loan forgiveness announcement changed borrowers' outcomes. This analysis serves two purposes. First, it provides direct evidence on the effects of the loan forgiveness announcement. Second, it allows us to test whether outcomes are more responsive to a liquidity shock or a balance reduction. The challenge is that the loan forgiveness announcement occurs after the payment pause such that changes over time could reflect responses to the new announcement or dynamic treatment effects from the payment pause. We thus parameterize the effect of the payment pause to be linear and test for deviations in outcomes from this linear trend:

$$y_{it} = \beta Eligible_i Post_t + \gamma Eligible_i MonthsPost_t + \pi Eligible_i PostForgiveness_t + \mu_i + \tau_t + \epsilon_{it}, \tag{3}$$

where $MonthsPost_t$ is the number of months since the start of the payment pause, and $PostForgiveness_t$ is an indicator for being after the student loan forgiveness announcement.

[9] We also show estimates from a specification with state-month fixed effects, where state is the borrower's state of residence as of December, 2019. These fixed effects control for geographic-time variation that may reflect borrowers of different types having sorted to different labor markets since their studies.

3 Data

We use data from the Booth TransUnion Consumer Credit Panel. The data are an anonymized 10% panel sample of all TransUnion credit records from 2000 to 2022. All individuals who were initially in the sample in 2000 have their data continuously updated, and each year 10% of new first time individuals in TransUnion data are added.[10] We further restrict to accounts that have been updated after 2018 and have been making positive payments prior to March 2020 and drop duplicates, joint accounts, cosigned loans, and individuals missing birthdates, as some of these may be parent borrowers. Scaling to match aggregates, there are 48 million such individuals, which is slightly higher than the 45 million federal student loan borrowers in the United States.

To compare relatively similar cohorts that had both FFEL and DL options available, we include only borrowers whose most recent loan was opened in 2010 or earlier. We classify DL borrowers as those who see more than a 50% decline in scheduled payments after March 2020, and classify the rest as FFEL borrowers.[11] The fact that we use scheduled rather than actual payments to construct the groups avoids concerns that we may be misclassifying delinquent FFEL borrowers as DL borrowers. We have 299,637 DL borrowers and 354,680 FFEL borrowers in our final sample.

Table 1 presents summary statistics. The table shows balances, payments, and delinquencies for various loan types, as well as credit scores and the share of DL borrowers. Table 2 further breaks down summary statistics by each group. The FFEL borrowers tend to have opened loans slightly earlier, as the program existed for a longer time and was larger prior to its end in 2010. Generally, the two groups are similar across credit outcomes, although FFEL borrowers have higher mortgage balances.

[10] All tables and figures that list TransUnion as a source have statistics calculated (or derived) based on credit data provided by TransUnion, a global information solutions company, through a relationship with the Kilts Center for Marketing at the University of Chicago Booth School of Business.

[11] 70.3% of DL borrowers see their scheduled payments drop to zero. The remainder have scheduled payments drop by less than the full amount, likely due to having both FFEL and DL program loans. The fact that we drop cosigned loans makes it unlikely that many of loans classified as FFEL are non-government private loans, as the vast majority of private student loans in the United States require a parent or other cosigner.

4 Main Results

4.1 Results

Table 3 shows estimates of the coefficient β from equation (1), varying fixed effects. The first column includes time period (month) and individual fixed effects, the second column adds state by month fixed effects, and the final column adds pre-treatment payment group (in deciles) by time period fixed effects. The top panel shows student loan outcomes, the second panel shows mortgage loan outcomes, the third panel shows auto loans, the fourth panel shows revolving credit, and the final panel shows total loan balances excluding student loans.

We first examine the drop in student loan balances. Figure 1 shows the drop in payments and evolution of loan balances. Specifically, the figure shows estimates of the coefficients β_t from equation (2), along with a pointwise 95% confidence interval. Consistent with the policy, we see a $138 drop in payments following the payment moratorium for DL borrowers, relative to FFEL borrowers who did not see a drop in payments. This leads to an approximate $1,500 in additional student loan balances in the following year, as the payment pause continued. The top panel of table 3 presents regression estimates from the corresponding difference-in-difference model (equation 1).

There is a sharp drop in student loan delinquencies following the payment moratorium, shown in figure 2.[12] The drop in student loan delinquencies is in part mechanical, as existing defaults were cured and individuals with no payments due are unable to become delinquent. For other loan types, however, which did not see payment pauses (mortgage, auto, and revolving debt), we see no change in delinquencies. Mortgage delinquencies show a slight increase of 0.0002, but this is economically tiny. This suggests that the additional liquidity from not making student loan payments did not have effects on the ability to pay other pre-existing loans during the two years of payment pauses. Thus, additional liquidity was not important in preserving financial stability arising from loan defaults.

The drop in delinquency is associated with a sharp rise in credit scores, shown in the bottom panel of figure 2. By the end of the sample period, credit scores increase by approximately 8

[12] We measure delinquencies as a flow. That is, our primary outcome is an indicator of whether a borrower is at least 90-days delinquent on a particular type of loan, for the first time in a given month.

points.

Figure 3 shows auto, mortgage, credit card, and total loan payments and balances following the student loan payment pause. For all types of credit, payments and balances increase. By the end of the sample period individuals with a student loan payment pause owe an additional $1,800 in other debt, and pay an additional $20 monthly. Most of the increase in debt is driven by mortgage payments, which tend to be longer maturity, and revolving debt.[13] The third through fifth panels of table 3 present regression estimates corresponding to figures 2 and 3, presenting the coefficient β in order to quantify effects over the full sample period.

The estimates show that the payment pause had effects on use of credit (through increased payments and balances) and creditworthiness (through credit score). To understand whether the creditworthiness may have led to more use of credit, we split the sample of borrowers based on an important factor in baseline creditworthiness – whether the borrower had ever been delinquent on a loan prior to the payment pause. We repeat our difference-in-difference analysis separately for these two borrower groups and report the results in Table 4.

We start with the student loan outcomes, in the top panel. We see similar decreases across these two groups in student loan payments from the payment pause. Because the policy applies to both groups' loans, this is unsurprising. We see differences, though, when we look at effects on student loan delinquency rates. The payment pause does not affect the probability of becoming newly delinquent, as not-yet-delinquent borrowers see no treatment effect. But borrowers who have been delinquent at least once enjoy a 5 percentage point decrease in the probability of being delinquent. These heterogeneous effects suggest that the effect of the pause was curing existing defaults rather than avoiding new ones.

We next turn to the effects on credit scores and other borrowing on non-student loans. We estimate that the effect on credit scores is concentrated among borrowers who have previously been delinquent on a loan. The payment pause, by curing existing defaults, led to a particularly large change in (perceived) creditworthiness: an increase in credit score of 28 points. Borrowers who had not previously been delinquent see only a modest increase in credit score

[13] Note that taking an additional $1,000 in mortgage debt at a 7% interest rates would lead to an approximately $7 increase in monthly payments under a standard 30-year fixed mortgage. Similarly, taking an additional $100 in auto loan debt at a 7% interest rate would lead to a payment of $2 under a 5-year payment plan, which is the typical maturity of auto loan contracts.

of just 2 points. Interestingly, we see that the effects on balances and payments for non-student debt are concentrated among borrowers who have not previously been delinquent. For example, we estimate increased mortgage balances of $917 for never delinquent borrowers and a decrease of $120 for ever delinquent borrowers. In terms of non-student total debt, the effect on balances is large and positive for never delinquent borrowers while small and negative for ever delinquent borrowers. The effect on payments is 2.2 times as large for never delinquent borrowers.

4.2 Student Loan Forgiveness

Finally, we study effects of the loan forgiveness announcement. Figures 2 and 3 show little relative change in outcomes for DL borrowers following the August, 2022 loan forgiveness announcement. Payments, balances, and delinquencies evolve smoothly around the date of announcement. This negligible change in borrowing or payments occurs despite the promise of a significant boost in wealth of $10-20,000, which is 10-20% of median household wealth in the United States. The fact that we do not see any changes in borrowing for DL borrowers, who were affected by the forgiveness announcement, relative to FFEL borrowers, who were unaffected by the announcement, suggest very small wealth effects arising from the debt forgiveness announcement. The lack of response to the loan forgiveness announcement is also reflected in the regression results. In Table 5, we present the estimates of Equation 3. We see the estimated deviations from trend are small and statistically insignificant. The estimates are stable across fixed effect specifications.

The lack of changes in outcomes following the announcement of significant debt forgiveness is again consistent with our finding that liquidity plays a larger role in debt and consumption outcomes. The announcement led to no actual change in cash on hand; the announcement, however, promised future payment cancellation. An alternative hypothesis is that student loan borrowers did not actually believe that any forgiveness would occur, and hence treated the announcement with skepticism. Our data do not allow us to rule out the possibility that the lack of reaction is simply due to borrowers anticipating that courts would strike down potential forgiveness.[14]

[14] It is also possible FFEL borrowers misunderstood the announcement, and thought that they would be eligible.

4.3 Discussion

Our results speak to several policy comparisons. First, we can compare the effects of a payment pause relative to no policy change. We find that the payment pause had a large effect on immediate consumption. Policymakers focused on boosting short-term consumption, especially for stimulus effects, may then find debt moratoria to be effective policy tools at relatively low long-term fiscal cost. At the same time, we find that the student debt payment moratorium led to higher levels of overall leverage, not only through borrowers not paying down student debt balances, but also through the accumulation of other types of household debt. By the end of the sample period, student debt borrowers have about 5% more household debt, driven roughly half by student and non-student debt. Perhaps paradoxically, temporary student debt relief leads to higher overall household debt levels and larger future debt burdens. On the one hand, this increased debt could be financing productive investments or durable purchases that smooth consumption. On the other hand, this could be a potential concern to policymakers, as both theoretical and empirical work has shown that higher levels of leverage can affect aggregate consumption and the transmission of costs through household balance sheet or debt overhang channels.

Second, we can compare the effects of a payment pause to an announced direct transfer. As the former targets liquidity while the latter targets debt reduction, the optimal policy depends crucially on the nature of constraints households face.[15] The larger effects from the payment pause indicate that it may be more effective in providing stimulus and consumption smoothing.

Our combination of results also highlights the potential mechanisms at play. That outcomes only respond to the payment pause and not the loan forgiveness announcement suggests that borrowers are more constrained by limited liquidity than high balances.[16] Further, the finding that the increased consumption (through borrowing) is concentrated among never delinquent

This appears to be less likely, however, as we see no obvious changes in raw means following the announcement, which is shown in appendix figure A.1. For various outcomes, we do not see changes in either group following the announcement.

[15] Because the loan forgiveness has only been announced, it has not yet affected liquidity. While an implemented forgiveness policy would also provide liquidity – and thus may have similar effects to a payment pause – it would come at a higher fiscal cost.

[16] A small set of borrowers subject to the payment pause may see a change in expected future payments if they are enrolled in income-based repayment plans that offer forgiveness after a certain number of periods. If changes in expected total future payments were the mechanism driving our results, we likely would see responses to the loan forgiveness announcement.

borrowers, whose creditworthiness hardly changes, highlights an important interaction between liquidity and credit.[17] The policy serves as a liquidity shock that induces increased use of credit. The effects do not appear to be driven by the subset of borrowers who saw an improvement in their credit scores. This raises the question of why such credit was not previously being used. We speculate that using credit requires a certain level of liquidity for making down payments or making the first due monthly payments. This interaction between liquidity and credit is important for policy design and highlights that policymakers should consider them jointly.

5 Concluding Remarks

We study the role of debt moratoria, a commonly used form of relief to distressed borrowers, through the lens of student loan forbearance. We find that, relative to borrowers who had to continue paying their loans, borrowers who had a pause in their payments sharply increased mortgage, auto, and credit card borrowing. We find little effect on loan delinquencies. The effects appear driven by increased credit demand and ability to service loans, as opposed to a credit supply effect. The results indicate that debt payment pauses can increase consumption in the short term, but that overall debt increases, as borrowers use increased liquidity to service new debt.

[17] Individuals may have increased their consumption without borrowing, as well. These actions are not captured in our credit report data, so we simply note that some of the consumption increase came through borrowing.

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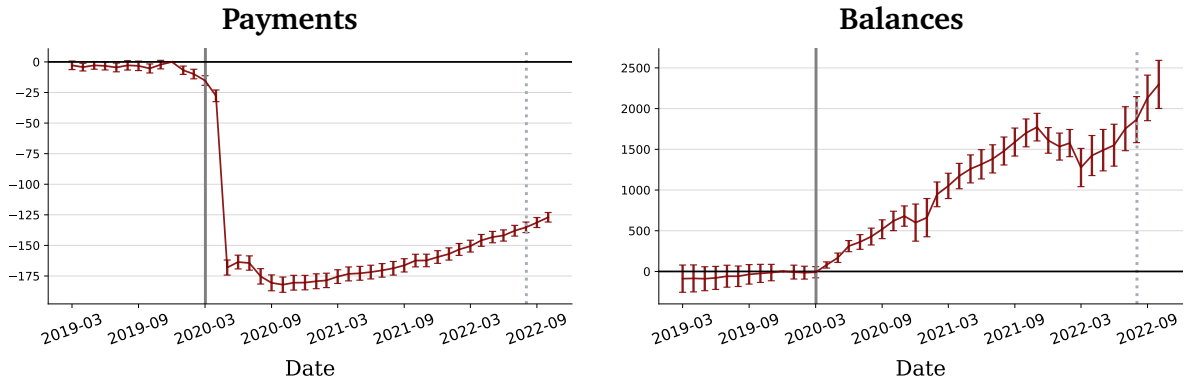
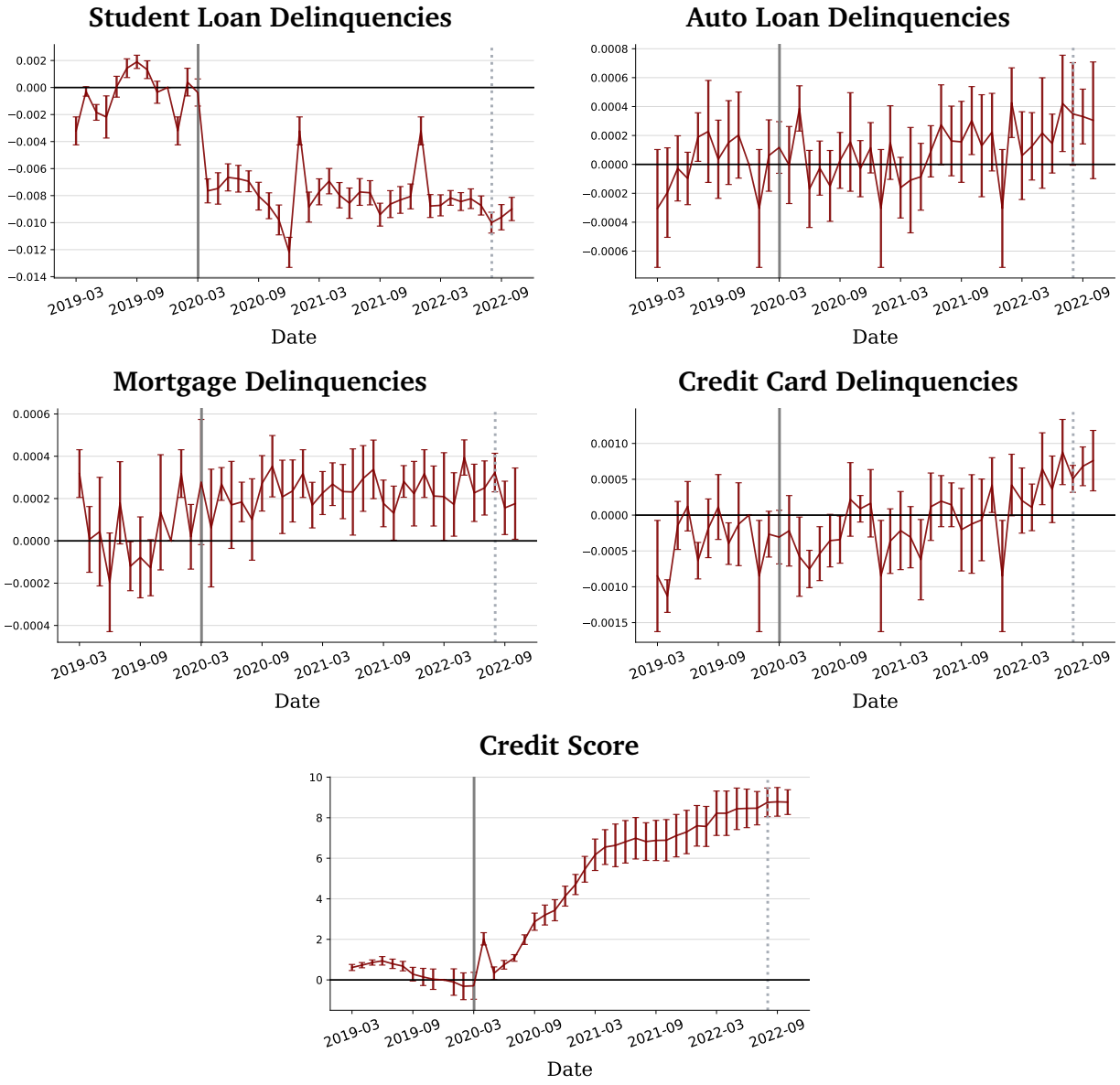
Figure 1: Student Loan Balances and Payments



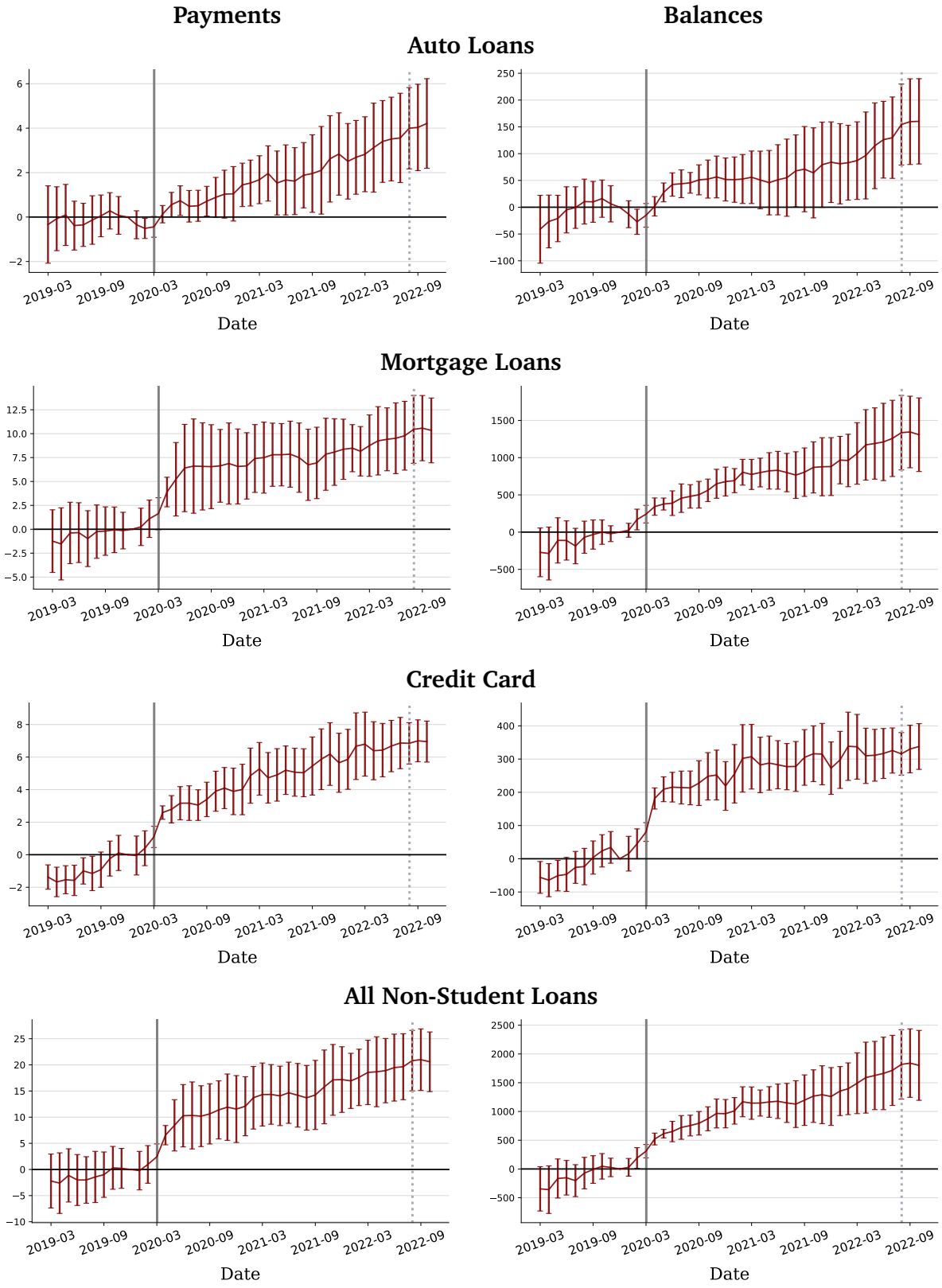
Notes: This figure shows the coefficients β_t from the OLS regression $y_{it} = \sum_t \beta_t Eligible_i + \mu_i + \tau_t + \epsilon_{it}$, where y_{it} is student loan balances or payments, along with a 95% confidence interval. We include fixed effects for individual and initial loan payment decile. The outcome is labelled above each panel. The solid line shows the month before the payment moratorium announcement, while the dashed line shows the month before the forgiveness announcement. Standard errors are doubled clustered at the repayment cohort level. Source: TransUnion

Figure 2: Loan Delinquency



Notes: This figure shows the coefficients β_t from the OLS regression $y_{it} = \sum_t \beta_t Eligible_i + \mu_i + \tau_t + \epsilon_{it}$, where y_{it} is loan delinquency or credit scores, with a 95% confidence interval. We include fixed effects for individual and initial loan payment decile. The outcome is labelled above each panel. The solid line shows the month before the payment moratorium announcement, while the dashed line shows the month before the forgiveness announcement. Standard errors are doubled clustered at the repayment cohort level. Source: TransUnion

Figure 3: Student Loan Balances and Payments



Notes: This figure shows the coefficients β_t from the OLS regression $y_{it} = \sum_t \beta_t Eligible_i + \mu_i + \tau_t + \epsilon_{it}$, where y_{it} is loan balances or payments, along with a 95% confidence interval. We include fixed effects for individual and initial loan payment decile. The outcome is labelled above each panel. The solid line shows the month before the payment moratorium announcement, while the dashed line shows the month before the forgiveness announcement. Standard errors are doubled clustered at the repayment cohort level. Source: TransUnion

Table 1: Full Sample Summary Statistics

	Mean	St. Dev.
Panel A: Student Loans		
Current Balance Amount	21332.29	30707.40
Payments	139.05	189.39
Delinquency	0.01	0.08
Credit Score	702.19	91.82
Direct Loan	0.46	0.50
Panel B: Mortgages		
Current Balance Amount	37989.60	98938.03
Payments	281.10	706.27
Delinquency	0.00	0.04
Panel C: Auto Loans		
Current Balance Amount	6692.26	12073.25
Payments	183.82	292.46
Delinquency	0.00	0.06
Panel D: Credit Cards		
Current Balance Amount	6571.84	10019.11
Payments	157.18	231.29
Delinquency	0.01	0.09
Panel E: Non-Student Debt		
Current Balance Amount	51650.16	104167.74
Payments	631.08	901.52
Delinquency	0.01	0.11
Observations	28,789,948	

Notes: This table shows summary statistics for the main analysis variables. The data are averaged on all observations from March, 2019 to December, 2021. Source: TransUnion

Table 2: Direct Loan vs FFEL Loan Summary Statistics

	Direct Loans		FFEL	
	Mean	St. Dev.	Mean	St. Dev.
Panel A: Student Loans				
Current Balance Amount	23462.71	31285.62	24210.63	31790.39
Payments	206.84	183.79	226.29	186.11
Delinquency	0.01	0.04	0.01	0.03
Credit Score	678.06	94.10	700.07	90.29
Open Year	2007	3.57	2005	2.90
Panel B: Mortgages				
Current Balance Amount	27547.93	76776.47	39430.98	97164.26
Payments	215.95	576.44	311.09	728.99
Delinquency	0.00	0.02	0.00	0.02
Panel C: Auto Loans				
Current Balance Amount	6166.54	10331.30	6589.07	11243.08
Payments	170.02	254.21	187.30	285.01
Delinquency	0.01	0.03	0.00	0.03
Panel D: Credit Cards				
Current Balance Amount	6075.87	9057.24	7496.17	10440.39
Payments	148.93	211.84	174.55	238.43
Delinquency	0.01	0.04	0.01	0.04
Panel E: Non-Student Debt				
Current Balance Amount	40031.24	81612.10	53923.32	102623.31
Payments	540.73	756.71	683.16	929.82
Delinquency	0.02	0.05	0.02	0.05
Number of Subjects	299,637		354,680	

Notes: This table shows summary statistics for the Direct Loan and FFEL groups. The data are averaged on observations prior to the policy (March, 2019 to March, 2020). Open years are rounded to the nearest whole year. Source: TransUnion

Table 3: Main Results by Credit Line

	(1)	(2)	(3)
Panel A: Student Loans			
Balance	1501.564***	1509.361***	1197.205***
	(101.104)	(99.947)	(121.535)
Payments	-138.390***	-138.402***	-149.067***
	(5.748)	(5.811)	(3.567)
Delinquent	-0.008***	-0.008***	-0.007***
	(0.001)	(0.001)	(0.001)
Credit Score	5.793***	5.730***	5.193***
	(0.4827)	(0.466)	(0.373)
Panel B: Mortgages			
Balance	773.149**	712.731**	837.799**
	(227.770)	(251.106)	(237.115)
Payments	7.255**	7.046*	7.658**
	(2.628)	(2.735)	(2.719)
Delinquent	0.000	0.000	0.000**
	(0.000)	(0.000)	(0.000)
Panel C: Auto Loans			
Balance	95.275*	101.409**	85.385*
	(36.215)	(35.009)	(39.198)
Payments	2.282*	2.429*	2.214*
	(0.994)	(0.968)	(1.013)
Delinquent	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Panel D: Credit Cards			
Balance	327.736***	331.501***	290.710***
	(43.122)	(41.651)	(45.847)
Payments	6.617***	6.734***	5.886***
	(0.833)	(0.805)	(0.928)
Delinquent	0.000	0.000	0.000*
	(0.000)	(0.000)	(0.000)
Panel E: Non-Student Debt			
Balance	1192.812***	1142.312***	1214.139***
	(269.279)	(292.501)	(293.234)
Payments	15.671***	15.730***	15.401**
	(4.110)	(4.157)	(4.383)
Delinquent	0.000	0.000	0.001*
	(0.000)	(0.000)	(0.000)
Individual FE	✓	✓	✓
Time FE	✓	✓	✓
State × Time FE		✓	✓
Payment Group × Time FE			✓
Observations	28,789,948	28,789,948	28,789,948

Notes: This table shows the coefficients β_t from the OLS regression $y_{it} = \beta Eligible_i Post_t + \mu_i + \tau_t + \epsilon_{it}$. The outcome is labelled above each panel, and the inclusion of fixed effects is noted at the bottom of the table. Standard errors are in parentheses, and are clustered at the repayment cohort level. Source: TransUnion. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

Table 4: Results Split by Delinquency Status

	Never Delinquent			Ever Delinquent		
	(1)	(2)	(3)	(1)	(2)	(3)
Panel A: Student Loans						
Balance	1633.790***	1640.227***	1303.036***	904.610***	913.541***	801.676***
	(88.045)	(87.335)	(105.607)	(138.349)	(138.737)	(162.136)
Payments	-140.795***	-140.642***	-152.624***	-126.021***	-125.708***	-133.197***
	(5.725)	(5.797)	(3.634)	(5.019)	(5.026)	(3.795)
Delinquent	0.000	0.000	0.000	-0.048***	-0.048***	-0.048***
	(.)	(.)	(.)	(0.000)	(0.000)	(0.000)
Credit Score	1.610***	1.554***	0.981**	27.840***	27.677***	27.655***
	(0.380)	(0.369)	(0.298)	(0.377)	(0.377)	(0.373)
Panel B: Mortgages						
Balance	917.130***	860.045***	958.247***	-120.076	-142.825	-139.124
	(209.264)	(234.348)	(227.464)	(237.693)	(238.173)	(243.960)
Payments	7.749**	7.572**	8.143**	3.817	3.761	3.473
	(2.571)	(2.703)	(2.702)	(1.955)	(1.936)	(2.058)
Delinquent	0.000	0.000	0.000**	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel C: Auto Loans						
Balance	103.056**	110.097**	86.740*	49.174	54.617	46.673
	(36.405)	(35.073)	(39.993)	(57.021)	(56.473)	(58.620)
Payments	2.581**	2.735**	2.389*	0.518	0.685	0.536
	(0.901)	(0.889)	(0.956)	(1.657)	(1.602)	(1.650)
Delinquent	0.000**	0.000*	0.000***	-0.001*	-0.001*	-0.001*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel D: Credit Cards						
Balance	381.447***	385.715***	339.698***	58.175*	58.878*	45.725
	(47.048)	(45.395)	(49.319)	(23.575)	(22.493)	(23.690)
Payments	7.127***	7.276***	6.355***	3.978***	3.963***	3.473***
	(0.926)	(0.895)	(1.024)	(0.724)	(0.681)	(0.711)
Delinquent	0.000	0.000	0.000***	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel E: Non-Student Debt						
Balance	1397.255***	1352.039***	1383.846***	-13.474	-31.040	-47.776
	(242.463)	(268.406)	(277.569)	(273.878)	(273.545)	(281.277)
Payments	16.987***	17.124***	16.568***	7.689*	7.780*	6.878
	(3.917)	(3.986)	(4.278)	(3.420)	(3.291)	(3.524)
Delinquent	0.000	0.000	0.001***	-0.001	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Individual FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
State × Time FE		✓	✓		✓	✓
Payment Group × Time FE			✓			✓
Observations	24,114,244	24,114,244	24,114,244	4,675,704	4,675,704	4,675,704

Notes: This table shows the coefficients β_t from the OLS regression $y_{it} = \beta Eligible_i Post_t + \mu_i + \tau_t + \epsilon_{it}$ for never delinquent and ever delinquent groups. An individual is classified as ever delinquent if she has one or more delinquencies reported on her student loans between 2019 and 2021 and never delinquent if she has zero reported delinquencies. The outcome is labelled above each panel, and the inclusion of fixed effects is noted at the bottom of the table.Standard errors are in parentheses, and are clustered at the repayment cohort level. Source: TransUnion. $^{*}p < .1$, ** $p < .05$, *** $p < .01$.

Table 5: Results on Loan Forgiveness Announcement

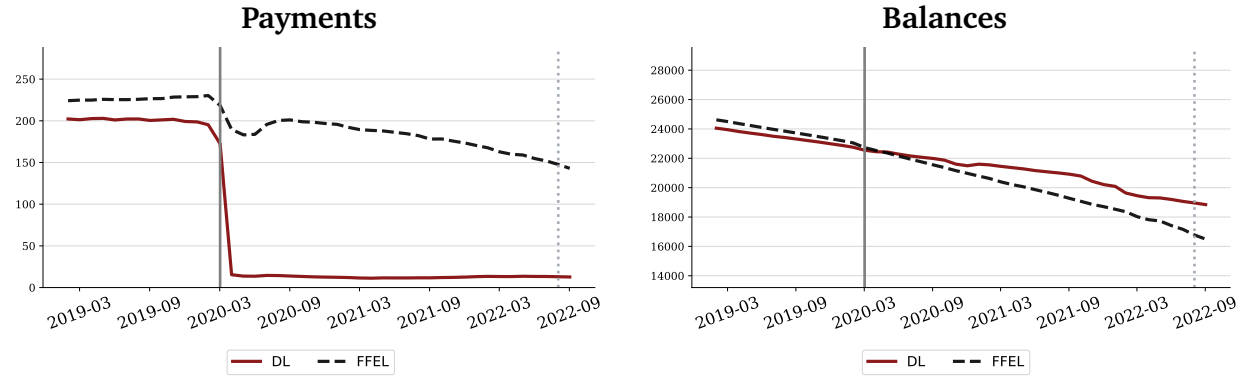
	(1)	(2)	(3)
Panel A: Mortgages			
Balance	67.296	82.167	49.815
	(115.353)	(118.103)	(123.685)
Payments	0.387	0.413	0.316
	(0.923)	(0.962)	(0.994)
Delinquent	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Panel B: Auto Loans			
Balance	27.525*	24.073	33.066*
	(12.144)	(12.369)	(13.346)
Payments	0.143	0.118	0.290
	(0.281)	(0.296)	(0.299)
Delinquent	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Panel C: Credit Cards			
Balance	-33.825*	-36.026*	-24.542
	(15.309)	(14.184)	(16.342)
Payments	-0.802	-0.883*	-0.614
	(0.417)	(0.380)	(0.432)
Delinquent	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Panel D: Non-Student Debt			
Balance	45.658	53.999	43.774
	(126.051)	(130.493)	(135.634)
Payments	-0.964	-1.064	-0.633
	(1.087)	(1.119)	(1.185)
Delinquent	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Individual FE	✓	✓	✓
Time FE	✓	✓	✓
State × Time FE		✓	✓
Payment Group × Time FE			✓
Observations	28,789,948	28,789,948	28,789,948

Notes: This table shows the coefficients β_1 from the OLS regression $y_{it} = \beta Eligible_i Post_t + \gamma Eligible_i MonthsPost_t + \pi Eligible_i PostForgiveness_t + \mu_i + \tau_t + \epsilon_{it}$, where $Post_t$ is an indicator for being after the payment pause, $MonthsPost_t$ is the number of months since the start of the payment pause, and $PostForgiveness_t$ is an indicator for being after the student loan forgiveness announcement. The outcome, y_{it}, is labelled above each panel, and the inclusion of fixed effects is noted at the bottom of the table. Standard errors are in parentheses, and are clustered at the repayment cohort level. Source: TransUnion. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

Appendix

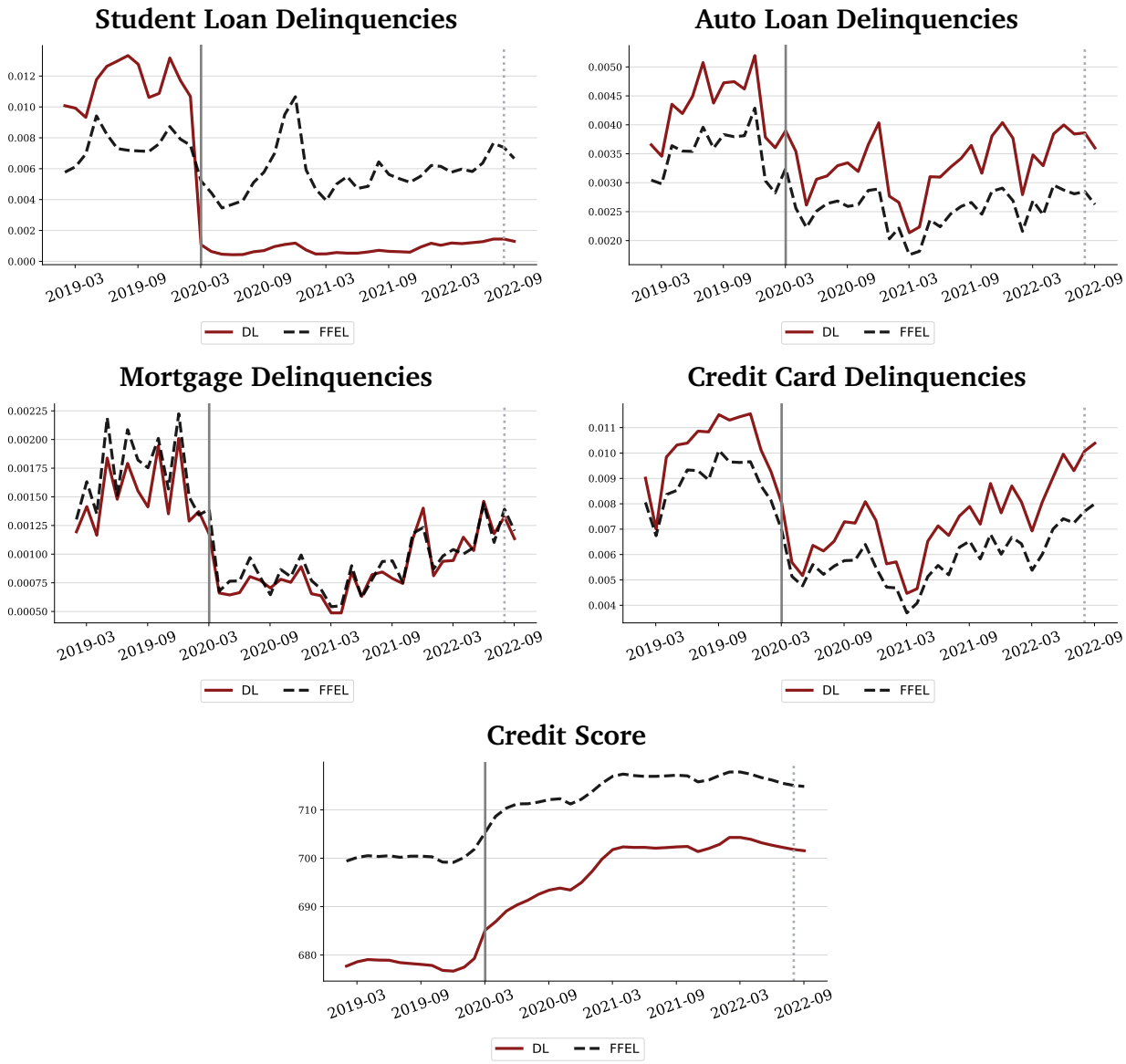
A Additional Tables and Figures

Figure A.1: Student Loan Balances and Payment



Notes: This figure shows mean student loan payments and balances for student loans, broken down by DL and FFEL borrowers. The solid line shows the month before the payment moratorium announcement, while the dashed line shows the month before the forgiveness announcement. Source: TransUnion

Figure A.2: Loan New Delinquencies



Notes: This figure shows mean delinquencies and credit scores, broken down by DL and FFEL borrowers. The solid line shows the month before the payment moratorium announcement, while the dashed line shows the month before the forgiveness announcement. Source: TransUnion

Figure A.3: Student Loan Balances and Payments

Notes: This figure shows mean payments and balances for non-student loans, broken down by DL and FFEL borrowers. The solid line shows the month before the payment moratorium announcement, while the dashed line shows the month before the forgiveness announcement. Source: TransUnion

Table A.1: Delinquency Status Summary Statistics

	Ever Delinquent		Never Delinquent	
	Mean	St. Dev.	Mean	St. Dev.
Panel A: Student Loans				
Current Balance Amount	26514.39	34048.98	20327.49	28984.16
Payments	127.88	146.14	141.22	149.53
Delinquency	0.04	0.02	0.00	0.00
Credit Score	591.49	66.08	723.65	70.24
Panel B: Mortgages				
Current Balance Amount	19509.26	60374.71	41572.90	93035.50
Payments	147.26	438.20	307.06	663.28
Delinquency	0.00	0.01	0.00	0.01
Panel C: Auto Loans				
Current Balance Amount	4141.66	6824.66	7043.05	9332.94
Payments	100.98	150.71	168.07	211.85
Delinquency	0.01	0.03	0.00	0.01
Panel D: Credit Cards				
Current Balance Amount	4141.66	6824.66	7043.05	9332.94
Payments	100.98	150.71	168.07	211.85
Delinquency	0.02	0.03	0.01	0.02
Panel E: Non-Student Debt				
Current Balance Amount	29775.12	64526.76	55891.69	98305.82
Payments	409.26	593.98	674.09	850.78
Delinquency	0.03	0.04	0.01	0.02
Number of Subjects	106,266		548,051	

Notes: This table shows summary statistics for the delinquency groups. The data is averaged over all observations (March of 2019 to October of 2022). Source: TransUnion

B Sample Construction

B.1 Main Sample

The main data set is constructed from the Booth TransUnion Consumer Credit Panel, which is a 10% panel sample of the full US population with credit reports. We begin by considering all individuals with open student loans in the year 2018. There are approximately 5.2 million such individuals. We first remove individuals whose information has not been updated beyond 2018, duplicate accounts, and borrowers with loans originating outside the continental United States. Of the remaining borrowers we filter out subjects with missing birthdays and borrowers possessing student loans that have co-signers attached as both conditions indicate possible parent borrowers, or private student loans which typically require a co-signer.

Because FFEL was discontinued in 2010 and we wish to compare relatively similar cohorts, we do not include borrowers with student loans originating after 2010. We additionally restrict to accounts that have made at least 3 positive payments between June, 2018 and March, 2020. Lastly, we remove borrowers that have more than one month of $0 payments concurrent with a positive loan balance. This eliminates any borrowers who may have been in forbearance prior to the policy announcement. This results in a set of 654,317 borrowers.

To classify DL and FFEL we consider changes in scheduled student loan payments before and after the forbearance policy for loans aggregated at the borrower-month level. We define the pre-period to be June, 2018 to March, 2020 and the post-period to be March, 2020 to January, 2022. Individuals with a sum total of $0 scheduled student loan payments in the post-period are classified as direct loan borrowers. Additionally, individuals who experienced greater than a 50% decrease in average monthly pre-period payments to average monthly post-period payments are classified as direct loan borrowers. Those with less than a 50% decline in pre-period to post-period payments are classified as FFEL borrowers.

For each individual in the sample of DL and FFEL borrowers we aggregate the balances, payments, and delinquencies of their mortgages, credit cards, and auto loans at the month level. We assume a balance, payment, and delinquency of zero for those with no reported debt in a given credit line. Finally, we calculate total debt as the sum of mortgage, credit card, and auto loan balances, payments, and delinquencies. We winsorize all balance and payment outcomes at the 1% level.

B.2 Alternative Sample Construction

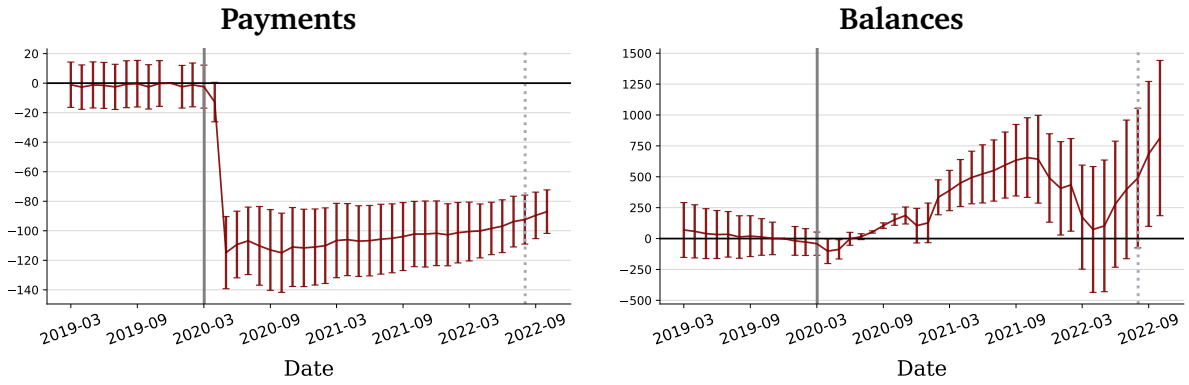
A potential concern with our construction is that it may induce measurement error, if payments drop to zero due to discretionary forbearance or other mechanisms to defer payments. To

confirm our results and classification method we employ an alternative method for identifying DL and FFEL borrowers using lender IDs. The Booth TransUnion Consumer Credit Panel masks the specific identifies of lenders but does provide a unique ID for each lender. Additionally, the Booth TransUnion Consumer Credit Panel provides industry codes for each lender. We can determine whether loans originated prior to 2010 were under the DL program, as these all used one particular large servicer. This information enables us to estimate lenders as either FFEL, DL, or private loan lenders.

To identify FFEL lenders we begin by considering lenders with industry codes in banking, personal finance, education, and government. Because FFEL loans are distributed by private banks but backed by the federal government it is plausible for credit bureaus to report FFEL lenders as either private or government entities. Among this subset of lenders we consider only lenders who stopped lending after 2010 since this was the year the FFEL program was discontinued and no FFEL loans should exist post 2010. All lenders with the previously mentioned industry codes and no record of lending post 2010 are considered FFEL lenders. Loans from lenders who have industry codes in banking and personal finance but are not classified as FFEL loans are considered private loans as long as more than 70% of the lender's student loans have a co-signer attached. Any lender that is not classified as a FFEL or private loan lender is considered a DL lender.

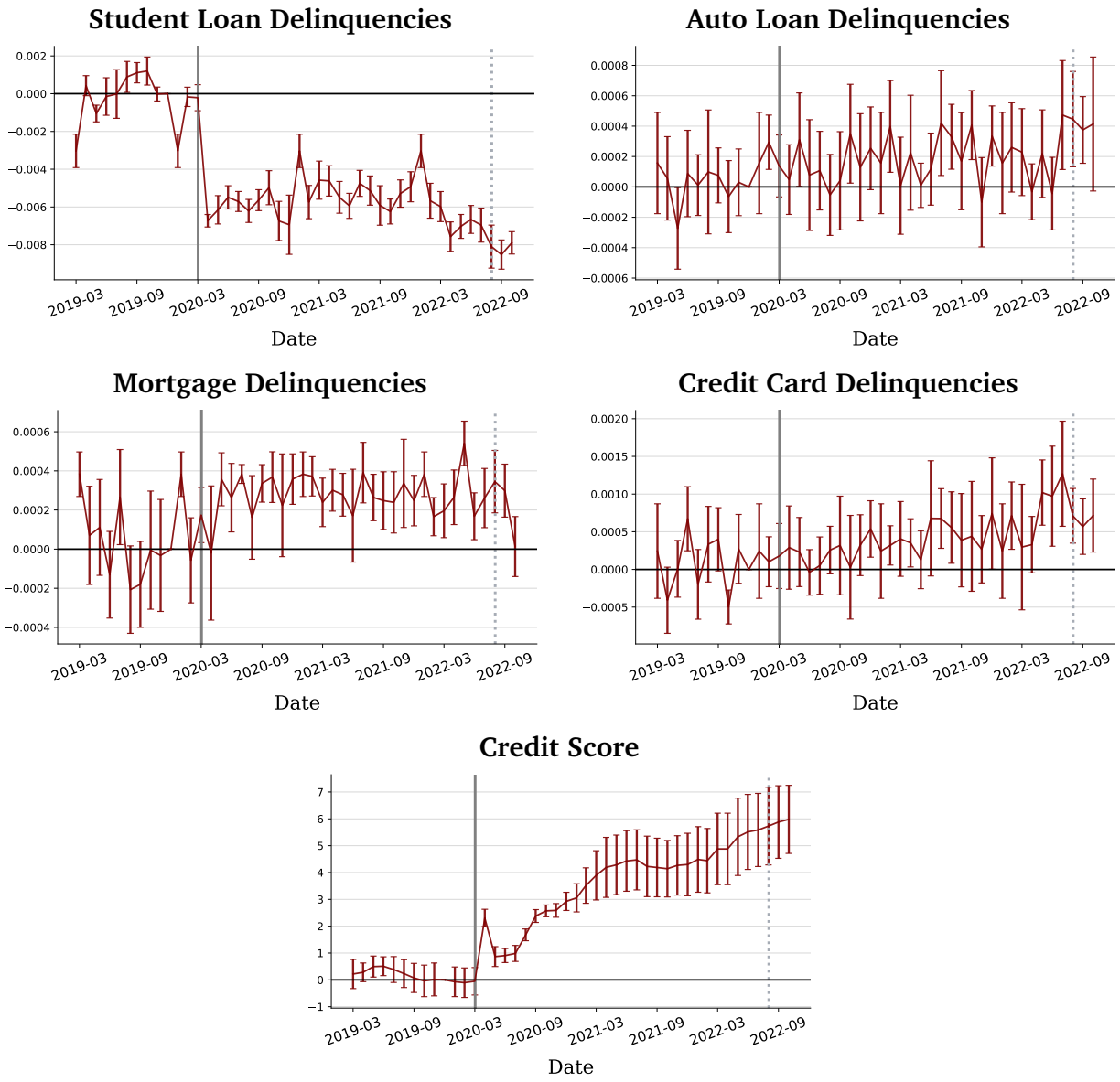
Comparing this classification method to our main classification method we find that approximately 72% of loans are identified as the same type with both methods. Of the loans whose main classification does not match the alternative classification, 90% are classified as FFEL in the main classification and DL in the alternative classification. This is likely because our alternative classification method assumes that all FFEL lenders stopped lending in any form after 2010. In reality some lenders may have shifted over to private loans or, in the case of government lenders, direct loans. Thus the alternative method runs the risk of over classifying direct loans and under classifying FFEL loans. That said, the results using the alternative classification method are found in Figures B.1 through B.3, and all align closely with the results using the main classification method and thus strengthen the robustness of our results.

Figure B.1: Student Loan Balances and Payments: Alternative Classification



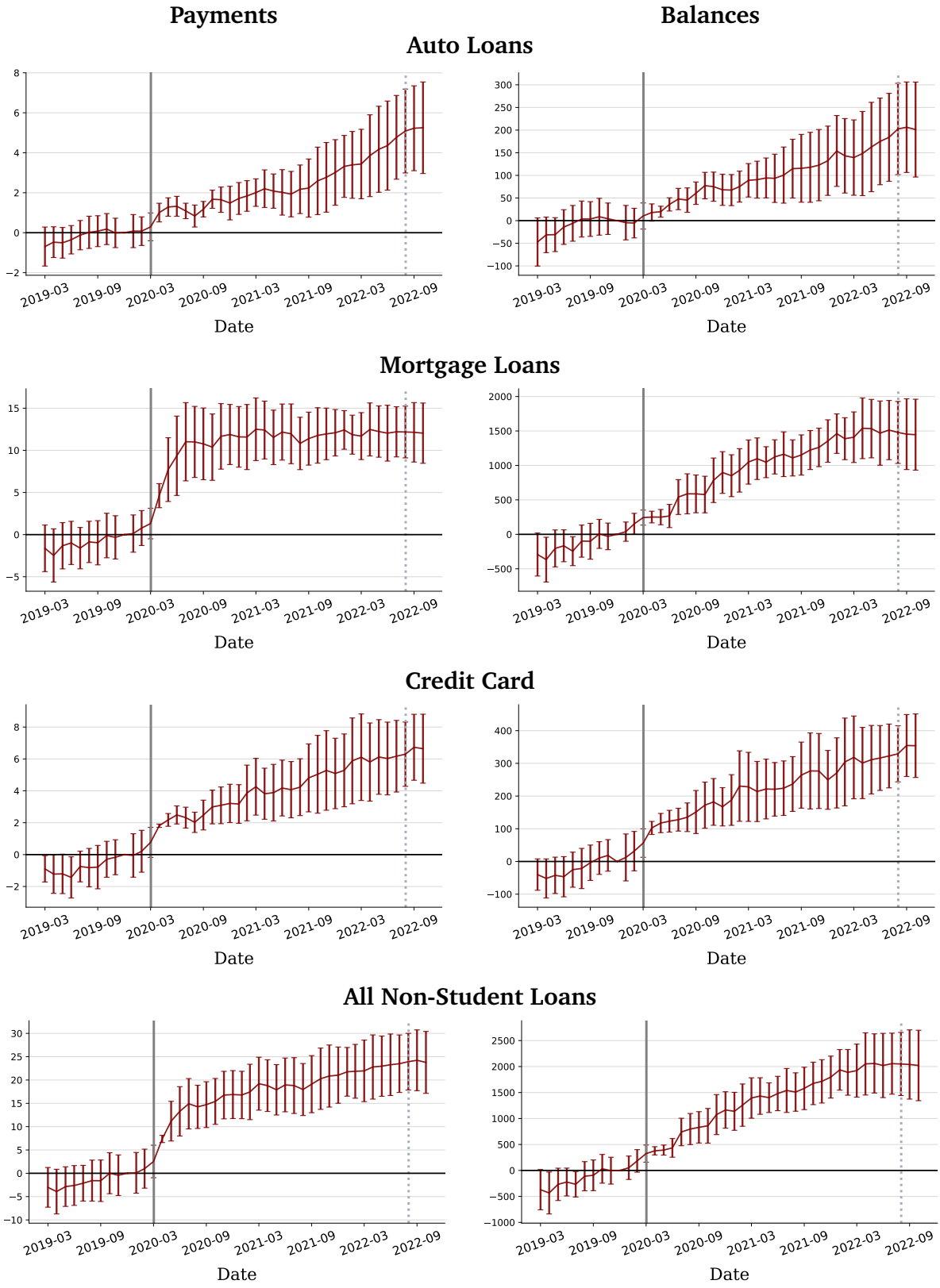
Notes: This figure shows the coefficients β_t from the OLS regression $y_{it} = \sum_t \beta_t Eligible_i + \mu_i + \tau_t + \epsilon_{it}$, where y_{it} is student loan balances or payments, along with a 95% confidence interval. We include fixed effects for individual and initial loan payment decile. The outcome is labelled above each panel. The solid line shows the month before the payment moratorium announcement, while the dashed line shows the month before the forgiveness announcement. Standard errors are doubled clustered at the repayment cohort level. Source: TransUnion

Figure B.2: Loan Delinquency: Alternative Classification



Notes: This figure shows the coefficients β_t from the OLS regression $y_{it} = \sum_t \beta_t Eligible_i + \mu_i + \tau_t + \epsilon_{it}$, where y_{it} is loan delinquency or credit scores, with a 95% confidence interval. We include fixed effects for individual and initial loan payment decile. The outcome is labelled above each panel. The solid line shows the month before the payment moratorium announcement, while the dashed line shows the month before the forgiveness announcement. Standard errors are doubled clustered at the repayment cohort level. Source: TransUnion

Figure B.3: Student Loan Balances and Payments: Alternative Classification



Notes: This figure shows the coefficients β_t from the OLS regression $y_{it} = \sum_t \beta_t Eligible_i + \mu_i + \tau_t + \epsilon_{it}$, where y_{it} is loan balances or payments, along with a 95% confidence interval. We include fixed effects for individual and initial loan payment decile. The outcome is labelled above each panel. The solid line shows the month before the payment moratorium announcement, while the dashed line shows the month before the forgiveness announcement. Standard errors are doubled clustered at the repayment cohort level. Source: TransUnion