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SECOND CHANCE: LIFE WITHOUT STUDENT DEBT

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ABSTRACT

We exploit an episode of plausibly-random debt discharge, due to the inability of National Collegiate to prove chain of title, to examine the effects of student debt relief on individual credit and labor market outcomes. We find that borrowers experiencing this debt relief shock reduce their indebtedness by 11%, and number of other delinquent accounts by 24%. After the discharge, we see increases in the borrowers' geographical mobility, probability of changing jobs, and ultimately their income, which increases by about $3000 over a three year period. Although we cannot quantify its costs, these findings speak to the benefits of loan forgiveness in reducing the consequences of debt overhang.

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1. Introduction

Student debt has experienced staggering growth in the last decade, reaching $1.5 trillion in 2018 (NYFED, 2018). Since the Great Recession, student debt levels have surpassed auto loans, credit card debt and home-equity lines of credit and currently only trail mortgage liabilities as the largest consumer debt in the United States. Since 11 percent of borrowers are 90 days or more delinquent on their student debts, rising student debt is considered one of the more insidious threats of our time. This situation has ignited a heated debate about potentially bringing relief to borrowers crippled by student debt, and policymakers have considered ways to keep the student loan problem from swelling out of control. The newly appointed Chairman of the Federal Reserve stated that "As this goes on and as student loans continue to grow and become larger and larger, then it absolutely could hold back growth."

Federal student loans are directly funded by the government and offer numerous consumer protections (i.e., income-based repayment options) that help borrowers in need, while private student loans do not offer the same protections, although both federal and private student loans are not dischargeable in bankruptcy. Many people with private student loans, due to lower earnings potential, end up shouldering debt that are not able to repay. These trends might have aggregate effects because about 44 million graduates hold student debt with amounts averaging about $28,000, and such burden might adversely affect borrowers' financial and labor outcomes. What exacerbates the situation is also a general lack of consensus on the policy objectives. For instance, policy might be designed to target the liquidity constraints that have pushed the borrowers into distress, *i.e.*, by relating the monthly repayments to borrowers' income. Alternatively, policymakers could implement interventions targeting the debt overhang problems associated with facing a significant debt burden, *i.e.*, forgiving student loan principals altogether.[1] The main empirical challenge in examining how borrowers' behavior would respond to similar policy changes is to finding plausibly exogenous variation in the borrowers' exposure to student debt and collecting detailed information about the borrowers' decisions over time.

This paper overcomes these challenges in two ways. First, we exploit a plausibly exogenous debt

[1] See, for instance, the policy proposals recently discussed by various presidential candidates (https://www.forbes.com/sites /robertfarrington/2019/04/24/the-2020-presidential-candidates-proposals-for-student-loan-debt/1c74e147520e).

relief shock experienced by thousands of borrowers due to the inability of the creditor to prove chain of title. Specifically, the largest holder of private student loan debt, National Collegiate, with 800,000 private student loans totaling $12 billion, and its collector agency, Transworld Systems, lost a series of collection lawsuits against the borrowers they were collecting from. National Collegiate bought the student loans from a series of banks and other financial institutions over the years, but judges throughout the country tossed out collection lawsuits by National Collegiate, ruling that it failed to establish for a subset of borrowers the chain of title because it was not able to prove it owned the debt on which it was trying to collect. This provides an ideal setting to explore the effects of relieving borrowers from student debt, as the lack of documentation by National Collegiate is likely to be random and exogenous to borrowers' choices. Second, we have credit bureau data on borrowers' balance sheets, which provides information such as monthly payments and loan amounts on all type of accounts. In contrast to the existing household finance literature using credit bureau data, for a substantial sample of borrowers, we are able to observe income from a proprietary database used for income and employment verification services.

We hand-collect a unique dataset with information about these lawsuits, which provides us with details on the borrowers' identities, when these lawsuits were filed and in which court. This allows us to then match this information to borrower level data from the credit bureau in order to obtain a rich set of outcome variables for these borrowers. In order to isolate the effect of the student debt relief on these borrowers, we compare the borrowers that experience the discharge with those who were subject to collection by the same trust, National Collegiate, but did not experience debt relief either because they lost the case as the trust did not lose paperwork for their loans or because their case was not adjudicated by the end of our sample period. The main advantage of this strategy is that both groups of borrowers are likely to be similar as they both had defaulted on their student loans owned by the same trust and were the subject of a collection lawsuit by the same agency. This helps us addressing any potential selection issues related to the decision of National Collegiate to purchase certain loans. We also control for individual and filing year by calendar year-month fixed effects to ensure that the variation we exploit comes from comparing borrowers who were sued at the same time. Consistent with the idea that these clerical errors are indeed random, we verify that the treated and control groups are very similar along a vast array of observable characteristics before treatment and show the absence of pre-trends. To address concerns related to the borrowers' ability to show up in court to defend their case against National Collegiate, we also provide consistent evidence when we restrict attention to the treatment group by only exploiting the timing of the discharge.

Before evaluating our main outcomes, we first investigate the effect of the student debt relief, *i.e.*, the size of our initial shock. We find that, on average, debt relief leads to a decline in student loan balance by $7,901. This decline is substantial, as the borrowers in our sample had an average monthly income of about $2,000. In addition, there is ample variation in this decline in student loan balance, as reflected by the standard deviation of $11,100, which is an interesting source of heterogeneity we use in the analysis. At this point, it is important to notice that there is no increase in the treated borrowers' disposable income post relief, because they were not paying the loan before the discharge and are not paying it afterwards. So this initial shock of $7,901 affects their net wealth, without having an immediate cash-flow effect. When we estimate our baseline specification, which allows us to compare the effects to our control group, we also find that treated borrowers' credit scores increase, consistent with their financial health improving.

Before moving to the core of the analysis, it is worth discussing the type of effects one may expect on consumption and credit outcomes for these borrowers. Since the treated individuals in our sample have been in default for some time, it is plausible that they are facing liquidity constraints. As a result, even if these borrowers don't receive any additional liquidity due to relief, they might still experience a relaxation from such constraints in expectation as the relief ensures that they will not be subject to potential wage garnishment in the future. If this is the case, we should expect their consumption to increase, as their access to credit is likely to improve. On the other hand, as suggested by recent evidence on mortgage modification by Ganong and Noel (2018), this increase in spending might be moot, if the relief only affects their wealth without significantly relaxing their liquidity constraints. Alternatively, borrowers experiencing the debt relief might want to improve their financial health to avoid experiencing debt collection in the future and so might be more reluctant to use credit.

We test these hypotheses by analyzing three main sets of outcome variables. We begin by exploring whether borrowers' leverage changes in the aftermath of the debt relief. In this way, we can test whether the treated borrowers increase their consumption either using credit cards or in general by demanding more credit. In contrast to what hypothesized, we find that borrowers reduce their total liabilities (excluding the student loans), by more than $4,500. The results are consistent across accounts as borrowers delever across all types of loans, from credit cards to auto loans to home loans. We also find that such adjustment happens along both extensive and intensive margins. In particular, both the number of accounts and balance on the existing accounts decrease, mainly driven by higher repayments and lower utilization ratios. To the extent that credit card balance and auto loans reflect consumption, we find that the treated borrowers seem to increase their savings (*i.e.*, lower their liabilities) by lowering their consumption.

One important critique to most types of policy proposals about student debt is that it constitutes a transfer from lenders, and in most cases from the taxpayers, to borrowers. This is especially troublesome if these borrowers are likely to end up in financial disarray again. In fact, the returns from helping borrowers that are experiencing difficulties in repaying their liabilities are lower if we expect those same individuals to be somewhat more prone to delinquency and not to be responsible with their finances. To evaluate if this is indeed the case, in the second set of results we test whether borrowers whose student debt is relieved experience lower or higher delinquency on *other* accounts. We find that the treated borrowers have significantly fewer delinquent accounts, an average decrease of about 24% relative to the control group, and this decline occurs across different accounts, namely credit cards and mortgages. Also, conditional on being delinquent, their past-due balance decreases significantly by about $400. We also show that borrowers experiencing debt discharge are less likely to file for bankruptcy, be subject to foreclosures or default on their medical bills. These findings speak to the potential spillover effects across liabilities, as other lenders benefit from these borrowers being financially more resilient. Although in principle these effects might just be temporary, we also show that the effects on the borrowers' financial health last for at least three years. In fact, their delinquency decreases steadily over time without ever catching up to pre-relief levels.

Probably the biggest concern regarding the student loan crisis, and a big part of the narrative around the need for policy interventions, is the possibility that the need to repay this debt could hold back an entire generation by affecting their decisions in the labor market. In fact, borrowers might find it too costly to move or to take risk which might lead them to accept jobs that are less than a perfect match for their skills. Our setting provides a unique opportunity to test whether this is likely to be the case. We are able to trace the residence of these borrowers before and after the debt relief shock. Consistent with a debt overhang problem affecting these borrowers, we find that the treated individuals are significantly more likely to move their residence following student debt discharge. To complement this result, we find that treated borrowers are significantly more likely to change both employer and industry with respect to the control group after the debt relief shock. This strongly suggests that these borrowers are more able to pursue opportunities elsewhere when relieved from the burden of their student loan obligations.

We further explore this dimension by analyzing whether the borrowers' income increases in the aftermath of the debt discharge. Consistent with the hypothesis that once their debt is discharged borrowers are able to pursue better opportunities, we find that these borrowers experience higher income growth and earn higher income by about $3,000 over a three-year period following discharge, which is roughly equivalent to 1.5 months' salary. In light of the previous evidence, this increase in income seems to be driven by the borrowers' ability to accept better jobs.

Before exploring the mechanisms behind our results, we test the robustness of our findings in a number of ways. One concern with comparing borrowers whose debt is discharged with those facing repayment is that those who get discharged are individuals who chose to show up in court and fight the collection lawsuit, while those who didn't get discharged may not have shown up to court. And the propensity to be present in court might be correlated with some unobservable characteristics of the borrower, *i.e.*, financial sophistication. Although the absence of pre-trends is reassuring (if sophistication is confounding our results, it is likely to affect outcomes even before the discharge), we perform a couple of different tests to assuage this concern. First, we exploit the timing of the discharges by repeating our main analyses only on the sample of treated individuals. This allows us to compare individuals who get discharged at time t with those that get discharged at time $t+n$ and show that the results hold even in this restricted specification. This suggests that our findings are less likely to be biased by differences between borrowers in the treatment and the control group.

Second, we also replicate our analyses with a larger sample based on matching our treatment group to a broader control group. Specifically, for each borrower in our treated sample, we match borrowers whose loan is not held by National Collegiate, but who reside in the same ZIP Code, are of the same age, have a similar student loan amount to pay off, and most importantly, were also in default on their student loans. In addition to helping us rule out that differences across treated and control groups in our baseline specification are not driving our results, this matched sample also allows us to enrich our specification by including county by event-time fixed effects that absorb any time-varying regional shock. Even using this matched sample, we find results similar to our baseline estimates.

We believe there are several mechanisms at play driving our results. First, the relaxation of liquidity constraints is theoretically an important channel. This channel might work both through the treatment group or the control group. The fact that borrowers have stopped paying their loans means that debt relief does not impact their immediate disposable income. Thus, the evidence relating to deleveraging is surprising as it is less likely to be driven by cash flow effects.[2] These results may also be driven by control group's constraints becoming tighter due to the wage garnishment they face.[3] To test this hypothesis, we examine the heterogeneity in effects across borrowers that reside in states with different levels of restrictions on wage garnishment. Intuitively, we should find a stronger effect in more creditor-friendly states (*i.e.*, higher cap on wage garnishment) if the control group's liquidity constraints were playing a significant role, however we do not find significant differences across states.

In principle, debt overhang is another channel that could potentially drive our results. For every additional dollar that these borrowers earn, they might expect to pay a significant fraction towards their loans, especially because student loans are not dischargeable in bankruptcy. This may reduce their incentives to pursue better opportunities or provide labor supply. To evaluate the importance of this channel in our setting, we exploit the heterogeneity in the amount of debt relief experienced and find that the results are stronger among borrowers whose discharge amount is above the median. This is consistent with the debt overhang channel, particularly because our labor market results are concentrated among borrowers experiencing higher levels of debt relief. Our data allows us to also examine wage composition to further investigate this channel. We find that the fraction of variable pay, *i.e.*, bonus, commissions and overtime pay, significantly increases for treated borrowers following debt discharge. We find that treated borrowers are both exerting more effort in their jobs as well

[2]Note that the relief does affect borrowers' expectations, because treated borrowers know that they will not to be forced to repay these loans in the future, which might relax their liquidity constraints.

[3]The amount that can be garnished is limited to 25% of the borrower's disposable earnings by federal law, but each state set their own caps within this limit.

as moving to riskier jobs following discharge, both of which are consistent with the debt overhang channel.

One question at this point is whether our results are informative of the potential benefits of debt forgiveness to the wider student borrower population, including those with federal student loans. Although our sample of borrowers is likely to be more constrained than the average borrower, we find that debt forgiveness leads to a significant improvement in their financial conditions and better labor market outcomes. It is worth noting that for borrowers that are current on their student loan liabilities, forgiving part of their debts, as it has been proposed, would also immediately affect their disposable income thus relaxing their liquidity constraints. However, our setting is not able to help us quantify the likely important moral hazard costs associated with debt forgiveness on existing and future borrowers.[4]

Overall, our results shed novel light on the adverse effects of the increase in student debt on individuals' outcomes. Although we cannot use our experiment to infer the costs of intervening in the student loan market, our findings suggest that the rising student debt burden can indeed have important effects on the new generations: student debt limits the borrowers' access to better opportunities and also has significant spillover effects to other debt classes. Putting all the findings together, it seems that the debt relief results in a multiplier slightly above one, because a debt relief shock of \$7,400 translates into \$4,600 lower indebtedness, a \$3,000 increase in income and a \$1,000 decline in delinquency amount.[5] Our findings also highlight dimensions that are not currently discussed in the policy debate, such as, the positive externalities that other creditors would experience if student debt was forgiven due to deleveraging, and the persistence of the beneficial effects resulting from these borrowers being in better financial health.

The rest of the paper is organized as follows. We discuss the related literature in Section 2 while Section 3 describes the data employed, the construction of the sample and the empirical strategy. Section 4 presents the main results of the paper. In an effort to better understand the borrowers

[4]In contrast to Mayer et al (2014), which shows homeowners' strategic response to changes of mortgage modification programs induced by settlement of U.S. state government lawsuits against Countrywide Financial Corporation, we do not find evidence for strategic behavior. This is likely to be driven by the fact that National Collegiate is not a lender, and it might be very cumbersome for the borrowers to figure out who is the ultimate owner of their debt and act accordingly.

[5]The average amount in default is \$3,947, so a decline of 24% results in approximately \$1,000 lower amount in delinquency.

mostly affected by debt relief, Section 5 explores whether our effects are heterogeneous depending on borrowers' characteristics and presents the different mechanisms behind our results. Section 6 discusses the policy implications of our paper, Section 7 describes a series of robustness checks, while Section 8 concludes. Additional results and robustness checks are included in the appendix.

2. Related Literature

Our paper is related to the recent evidence showing the effects of bankruptcy protection (*i.e.*, Dobbie, and Goldsmith-Pinkham, 2014, Dobbie and Song, 2015, and Dobbie, Goldsmith-Pinkham, and Yang 2017), mortgage debt overhang (*i.e.*, Melzer 2017, and Bernstein 2017) and credit constraints (Herkenhoff, Phillips and Cohen-Cole 2018, 2019). Similar conclusions about the importance of debt overhang have recently been drawn in the context of credit card modification programs by Dobbie and Song (2019). Also related is a recent paper by Cheng, Severino and Townsand (2017), which explores how consumers fare outside of the court system when they negotiate directly with debt collectors. Our paper provides insights into how debt relief, not associated with the negative consequences of bankruptcy (e.g. exclusion from credit markets), affects student loan borrowers. In addition, student loans are the only liability that cannot be discharged in bankruptcy, and the debt burden affects individuals that are likely to face decision that will shape the rest of their economic life, e.g. which job to take and where to live, which makes student loans different from mortgages and credit cards. Furthermore, the granularity of our data allows us to shed lights on the mechanisms behind the benefits of debt forgiveness for these borrowers and complement the existing literature.

Our evidence also contributed to a recent strand of the literature that shows that alleviating short-run liquidity constraints have beneficial effects on individuals' behavior, by highlighting instead the role of long-run constraints. In fact, given the existing evidence on mortgage modifications in the aftermath of the crisis, we would expect null to small effects from reducing the student debt principal. For instance, Ganong and Noel (2018) show that, in the context of the Home Affordable Modification Program (HAMP), principal write-downs had no impact on underwater borrowers, while lower monthly payments benefited borrowers.[6] In contrast, our findings show that forgiving

[6]This is consistent with the evidence on the effects of lower monthly mortgage payments shown by Di Maggio et al. (2017) and Fuster and Willen (2017) and the literature on marginal propensity to consume out of

student debt has significant effects on borrowers' credit and labor market outcomes. The difference might be due to the fact that student loan cannot be discharged in bankruptcy, while the other studies have focused on other types of debts that can be more easily discharged. More importantly, unlike other types of debt, because student loans are used by younger generation primarily as a way of social mobility, debt overhang induced by these loans could potentially have more lasting effect on borrower's economic outcomes.

Given its staggering growth and potential consequences on generation of young individuals, the student loan market has attracted increasing attention from academics.[7] For instance, Fos, Liberman and Yannelis (2018) analyze federal student loan borrowers in the US, and document a negative relationship between the level of undergraduate student debt and graduate school enrollment. Similarly, Scott-Clayton and Zafar (2016) investigate the effect of merit-based aid on future earnings and debt. Further, Mueller and Yannelis (2019) study the effects that the federal government income-driven repayment (IDR) plans, in which monthly student loan payments depend on the borrowers discretionary income, have on new delinquencies, monthly payments, and consumer spending. Also related are some recent studies on mobility. Bleemer, Brown, Lee and van der Klaauw (2017) provide evidence that in regions where many students are exposed to college costs, increased tuition is associated with more co-residence with parents and less living with roommates. Meanwhile, Goodman, Isen and Yannelis (2018) show that an increase in federal government lending has a significant effect on household formation early in the lifecycle, leading to a persistent increase in homeownership, with larger effects among those most financially constrained. Our paper builds on this literature by exploiting quasi-exogenous variation to causally assess the effects of debt relief on financial and labor outcomes.[8]

Overall, we believe our paper offers a unique opportunity to investigate how student loan burden

transitory income shocks (*i.e.*, Gross and Souleles, 2002, Johnson, Parker and Souleles, 2006, and Agarwal, Liu and Souleles, 2007).

[7]See Avery and Turner (2012) for an early discussion of which students are more likely to borrow too much and those more likely to underinvest in college education.

[8]There are also a few papers trying to understand the reasons behind the recent increase in the stock of student loans. It has been related to an increase in tuition across country and to the financial crisis. Specifically, Lucca, Nadauld, Shen (2018) establish a causal link between student loan availability and college tuition which has been the subject of policy discussion and debate for at least three decades (Bennett, 1987, for example), whereas Amromin, Eberly and Mondragon (2018) analyze the relationship between student loans and the housing market and estimate that, for every lost dollar of home equity credit that would have been used to nance college enrollment, households increase student loan debt by forty to sixty cents.

affects the individuals' consumption and borrowing decisions, as well as their income and employment prospects. In doing so, this paper also quantifies how valuable it is for distressed individuals to lift the constraints attached to student debt burden.

3. Empirical Framework

This section first describes the source of our exogenous variation, then discusses the data sources and empirical methodology we adopt to measure the impact of debt discharge on borrowers' outcomes.

3.1. Court verdicts

National Collegiate is the largest private holder of student debt in the US with 800,000 private student loans, totaling $12 billion.[9] According to an investigation conducted by the Consumer Financial Protection Bureau investigation, more than $5 billion of the debt held by National Collegiate was in default as of 2018. National Collegiate with its collection agency, Transworld System, have brought tens of thousands of lawsuits in the past five years across the country to aggressively pursue borrowers who fell behind on their bills. However, judges throughout the country have tossed out some of these lawsuits by National Collegiate, ruling that it failed to establish the chain of title, because it was not able to prove it owned the debt on which it was trying to collect.

The issue arises from the fact that National Collegiate is not a lender, but instead purchased loans made to college students a decade ago by dozens of different banks, which were bundled together by a financing company and sold to investors through securitization.As the debt passed through many hands before landing in National Collegiate's trusts, critical paperwork documenting the loans' ownership disappeared for a subset of loans. In other words, National Collegiate's legal problems have hinged on its inability to prove it owns the student loans.

While valid affidavits must be signed by a witness with personal knowledge of the consumers' account records, the CFPB found that such affidavits didn't exist in many of the lawsuits. In fact, similarly to the robo-signing phenomenon in the subprime mortgage market, Transworld employees completed and notarized sworn legal documents for lawsuits brought on behalf of the trusts, but these

[9] National Collegiate is not a lender but an umbrella name for 15 trusts.

were ruled insufficient to prove ownership of the debt because the collector did not have personal knowledge of these records.[10] In 2017, the CFPB fined the National Collegiate Student Loan Trusts, and its debt collector nearly $22 million, charging them for aggressively suing students for debts that they allegedly couldn't prove were legitimate. These lawsuits rulings provide an ideal setting to identify the effects of debt relief on borrowers' outcomes, as they are arguably orthogonal with respect to the borrowers' characteristics and led to wiping of student debt from their balance sheets. Importantly, borrowers are not able to choose whether their loans are bought by National Collegiate, and it is likely that many did not even know about who the owner of their debt was, similarly, to how it is difficult for a mortgagor to know whether her mortgage has been sold in a securitized deal. Furthermore, since the paperwork is lost only for a subset of loans, it would be impossible for the borrower to know in advance that he would be discharged during collection.

3.2. Data

Our analysis relies on two unique data sources. First, to take advantage of the settlements as source of variation, we hand-collected information about all collection lawsuits initiated by National Collegiate or its collection agency, Transworld Systems, using a new platform provided by LexisNexis. Lawsuits against borrowers who have fallen behind on their consumer loans are typically filed in state or local courts, where records are often hard to search. This means that there is no national tally of just how often National Collegiate's trusts have gone to court. This required us to go through all filings related to the trusts and then select the ones related to the collection of student loan debt county by county. Following this process, we gather information about the identity of the defendants, the court in which the case was filed, and the date of filing. The data covers all civil courts reported by LexisNexis in the US starting in 2010 and ending in 2017 and includes both cases that are adjudicated to National Collegiate and those won by the borrowers.

The second unique data is provided by Equifax Inc., one of the main credit bureaus, which allows us to construct the key outcome variables. The credit bureau provides information on households balance sheets, specifically, monthly history of all the borrowers' loans, including auto loans,

[10] In one frequently cited ruling, Lovett v. National Collegiate Student Loan Trust 2004-1, a Florida appeals court held that the creditor, a securitized investment trust, had not submitted sufficient evidence to prove that it owned the note on a loan originated by Bank One in Chicago.

mortgages, home equity lines of credit, student loans and credit cards (revolving). The data has granular information about the main features of these loans, such as date opened, account type, credit limits, monthly scheduled payment, balance, and performance history. It contains more than 260 million consumer credit files and over a billion credit trades, *i.e.*, information about single loans, and is updated monthly. Limited versions of this data have been employed in other papers studying households' financial decisions. However, our proprietary version is unique in a few respects.

First and foremost, to carry out our analysis, we need to be able to match the borrowers' information from the lawsuits to the credit bureau's information. The bureau matched the names and location of the borrowers with credit records by using both the names of the borrowers as well as the location and the existence of a defaulted student loan account on file, and provided us with the matched anonymized sample. To select the treated individuals, we verified the match by also making sure that the identified borrowers had student debt discharged after the lawsuit. This resulted in about ten thousand borrowers for which we could match the legal information to the credit files showing the discharge.

Second, our data are not confined to households balance sheet information, but include several other information about the borrowers. For a significant sample of borrowers including millions of individuals from more than 5,000 employers in the U.S., we observe their masked employer identity, as well as the industry of the firm in and their main occupation, through Equifax's proprietary employment data used in employment and income verification. This data is self-reported by the employers subscribing to verification services on a payroll-to-payroll basis and includes information on each employee's wages, and whether the employee remains employed at the firm at a given point in time. We discuss the similarities between the borrowers included in the employment data and those who are not in later sections.[11] Overall, we believe our data provide us with a unique opportunity to study the value of student debt relief on borrowers' credit outcomes and mobility.

[11] See Kalda (2019) for a more detailed discussion on the representativeness of the employment and income data.

3.3. Empirical methodology

Our empirical strategy consists of exploiting the individual court decisions as source of exogenous debt relief plausibly uncorrelated with borrowers' characteristics. Then, the individuals involved in the failed collection lawsuits constitute our treatment group and we can compare their outcomes before and after the debt discharge.[12] Since this is likely to be a population of severely-constrained borrowers, we do not want to compare their behavior with borrowers that were current on their debts. Instead, we want to exploit the cross sectional variation provided by the fact that only a subsample of borrowers won against National Collegiate.

Then, a natural control group is composed of individuals whose debt was held by National Collegiate, and that were also subject to a collection attempt by National Collegiate trusts, but that did not experience any discharge during our sample period either because the trust did not lose paperwork for their loans or their case was not adjudicated by the end of our sample period. Data limitations do not allow us to observe the adjudication in the court filings, but we can back it out from the credit report by observing whether the student loan disappears from the report at any point after the collection lawsuit is filed. Using this control group allows us to estimate our results controlling for any unobserved heterogeneity that could have let National Collegiate to purchase some loans and not others, *i.e.*, riskier loans or those of less sophisticated borrowers. Furthermore, by comparing our treated individuals to borrowers who have been subject to collection as well, makes sure that our baseline results are not confounded by the potential effects of dealing with a collection agency.

Having defined our treatment and control groups, our main specification takes the following form:

$$Outcome_{i,j,t} = \alpha + \beta \times (DebtRelief_i \times Post_t) + \mu_i + \gamma_{j \times t} + \varepsilon_{i,j,t} \tag{1}$$

where the outcome variables range from defaults to leverage, to mobility and income; $DebtRelief_i$ is a dummy that identifies the treated individuals who received the debt discharge during our sample period; $Post_t$ is an indicator variable that takes a value of one following debt discharge and zero otherwise, while μ_i and $\gamma_{j \times t}$ are individual and filing year (j) by calendar year-month (t) fixed

[12]Note that National Collegiate lost documents for only a fraction of loans, so treatment group comprises a sub-sample (and not all) of borrowers whose loans were owned by the company.

effects. These fixed effects ensure that we compare treated borrowers to the control borrowers that were sued during by National Collegiate during the same year. Since borrowers residing in the same neighborhood may be subject to similar economic shocks, we cluster the standard errors at the ZIP Code level thus allowing the errors to be correlated for all borrowers within the same neighborhood.[13]

To study how long it takes for the borrowers to react to the discharge, and to explicitly show that the trends in treatment and the control group are indistinguishable before the discharge, we also estimate the following dynamic specification:

$$Outcome_{i,j,t} = \alpha + \sum_{\tau=-25}^{25} \beta_\tau \times (DebtRelief_i \times Post_\tau) + \mu_i + \gamma_{j\times t} + \varepsilon_{i,j,t} \tag{2}$$

so that we can plot the estimated coefficients β_τ with the corresponding confidence intervals. Since our sample consists of greater than 24 months on either side of treatment, the dummy variable at both ends captures all months before or after that particular month, *i.e.*, $\tau = 25$ ($\tau = -25$) captures all months after (before) 24 months from treatment.

3.4. Summary statistics

We begin our analysis by comparing the treated and control group of borrowers in our sample in Table 1. There are 9,878 individuals in the treatment group and 6,388 in the control group. Our borrower $\times$ month panel data contains over 1.2 million observations but we compare these borrowers during the month when National Collegiate filed the case against them.

Table 1 reports the mean values for main variables used in the analysis for the treated and control borrowers in Columns (1) and (2) respectively, while Column (3) reports differences in means across these sub-samples. We find that the treated borrowers on average hold 6.55 accounts (excluding student loans), compared to 6.28 accounts held by the control group. The difference between the two remains statistically indistinguishable from zero. We also find that both groups of borrowers hold similar number of student loan, credit card, auto and mortgage accounts.[14] Both groups of borrowers hold similar balances on their accounts. For instance, the total debt balance (excluding

[13]In Table A1, we examine whether our results are sensitive to this choice of clustering variable and find that our results are robust to double clustering by ZIP code and calendar-month level, by individual and double clustering by individual and calendar month.

[14]Mortgage accounts include home equity line of credit, second mortgages etc.

student loans) held by the treated and control groups is \$39,385.82 and \$39,441.42 respectively. The treated group has \$719 higher balance relative to the control group which is marginally significantly different from zero. Both groups of borrowers have similar levels of credit card utilization and number of delinquent accounts while the treated group is 0.36 years younger than the control group. Overall, both the treated and control borrowers are very similar across these different dimensions.

To complement the previous statistics, we also investigate the geographical distribution of these borrowers across the US. Panel A of Figure 1 plots a heat map of the US showing the geographical distribution of delinquent student loan borrowers based on a random sample of the credit bureau data. It shows that the delinquency is quite spread out across the US but with a higher incidence in California, Texas and Florida. Panel B of Figure 1, instead, shows the geographical concentration of our treated individuals which are similarly present across several states in the US. Figure 2 complements the previous one by plotting the number of lawsuits settlements matched to the credit file for the treated borrowers over our sample period. We find that these settlements are staggered throughout the sample, with an increase during the 2014-2016, period thus lending naturally to the difference-in-differences setting we employ.

3.5. Student debt relief validation

Before evaluating our main outcomes, we first verify the effect of student debt relief on student loan balance and credit score for treated borrowers in our sample. In other words, we assess the size of the shock for our treated individuals. By just analyzing simple average statistics, we find that, on average, debt relief leads to a decline in student loan balance by \$7,900. This decline is substantial for borrowers in our sample whose average monthly income is about \$2,000. However, there is ample variation in this decline in student loan balance experienced by different borrowers as reflected by the standard deviation of 11,100. We utilize this variation to further validate our main results in Section 4.

Formally, we examine these changes relative to similar changes for the control group and estimate our baseline specification for the number of accounts, balance and credit score. Table 2 reports estimates for this analysis which includes both treated and control borrowers. All columns control for individual fixed effects and filing year by calendar year-month fixed effects, which allows us to compare treated and control borrowers who have been sued in the same year. We begin this analysis by examining the effect of debt relief on the number of student loan accounts in Column (1). We find that on average borrowers who experience debt relief have 0.73 fewer student loan accounts relative to borrowers who were sued by National collegiate because they were delinquent but did not experience debt relief. This is consistent with student loan account getting closed following court judgments. We examine the effect on student loan balance in Column (2) which shows a decline of about $7,404 for the treated borrowers relative to the control group following debt relief. Taken together, these results verify a decline in student debt following court judgments in our sample. In Column (3), we analyze the effect of debt relief on credit scores, because when debt relief gets reflected on credit reports, it is likely to positively affect credit scores. Consistently, we find that credit scores increase for borrowers experiencing relief from their student loans relative to the control group by about 7 points.

4. Main Results

This section describes the main results of the paper, distinguishing between the effects of the discharge on different credit and labor market outcomes including leverage, delinquencies, bankruptcy, medical defaults, mobility, and income.

4.1. (De)Leveraging

The first hypothesis we analyze is whether the sudden student debt discharge affects the borrowers' behavior with their other credit accounts, as an indication of their financial health post-discharge. On the one hand, although the discharge has a wealth effect but does not increase the disposable income of these borrowers, the debt discharge might represent for the treated borrowers a relaxation of credit constraints, which would result in an increase in their demand for credit and ultimately an increase in consumption. Even more so, if borrowers are in default because of their inability to manage their finances, they are likely to rack up again their debts. On the other hand, debt collection might be such a negative experience, that borrowers might be trying to avoid falling behind again it at all costs, which would incentivize them to exploit this lucky break to improve their financial situation.

To test these hypotheses, Table 3 examines the effect of the debt discharge on leverage. The first step towards a better understanding of how the affected borrowers change their leverage is to examine the extensive margin on their total debt and components of debt. That is, whether they tend to change their number of accounts in total and across different credit types. When we consider total number of accounts other than the student loans in Column (1) of Panel A, we find that it significantly decreases relative to the control group. Columns (2) through (4) examine the effects on the different types of accounts. We find that consistently across all debt categories, the treated borrowers are significantly more likely to reduce the number of accounts.

On the intensive margin in Panels B, we also find that the total debt balance of the borrowers that experienced the discharge decrease significantly with respect to that of the control group. Column (1) shows that borrowers reduce their balance by over four thousand dollars. Relative to an average total balance of $39,385 for the treated borrowers during the month of filing, this corresponds to a 11.6 percent reduction. Columns (2)-(4) explore the intensive margin across different credit types and find that credit card balance and mortgage balance are the ones that are reduced the most by $618 and $1,500. Overall, these findings suggest that treated individuals are significantly more likely to reduce their debt balances after the debt is discharged.

Although the result of the legal disputes is likely orthogonal to borrowers' behavior, and borrowers are similar on observables, an important assumption of our analysis is that the treatment and the control group were on parallel trends in the pre-period. Figure 3 shows that this is indeed the case. It plots the dynamic coefficients of our baseline regression and shows that, while the trends in treatment and the control groups are indistinguishable from each other in the pre-period, the treated borrowers tend to dramatically reduce their total debt balance (excluding the student loans) right after it gets discharged, and they continue doing so for several months after the event. The finding that deleveraging persists is also evidence that the benefits are not transitory.

Next, we examine in Table 4 how this deleveraging occurs. While Panel A focuses on credit cards, Panels B and C focus on auto and home loans respectively. Column (1) of Panel A finds that treated borrowers tend to use the existing accounts less as their utilization decreases by about 2 percentage points, which is equivalent to a 5-percent reduction with respect to the average of 39%.

This reduction may occur either due to a reduction in the use of credit or an increase in credit limit for borrowers. Since, we observe an increase in credit score following debt discharge, it is plausible that banks increase credit limit for treated borrowers. However, we find a significant decline in the credit balance in Column (2) of $971 which suggests that the decline in utilization is driven by an active reduction in the use of credit. Column (3) shows that the borrowers are significantly less likely to open a new credit card account. Column (4) shows that deleveraging is also partially driven by an increase in repayment above the minimum payment.

We complement these results by examining the dynamics of this behavior in Figure 4, which focuses on credit utilization and shows that while there is no significant difference in the utilization ratio between borrowers that get their loans discharged and those who do not in the pre-period, we find that there is significant wedge right after the legal decision.

Panel B examines whether the borrowers' behavior for auto loans is any different. Similar to Panel A, we look at the account opening and payments, but rather than utilization, we examine the response in the origination amount. We find that in the case of auto loans, most of the effects are driven by smaller auto loan originations compared to the control group, with a reduction of about $600, and higher payment amounts. Panel C shows a similar pattern for mortgages after the student debt is discharged: treated borrowers exhibit significantly smaller mortgage originations, with an average effect between $7,340, and higher payment on their accounts.

Overall, we find a propensity to repay of about 60% if we consider the discharge amount of $7,900 and the debt repayment of $4,600. However, as we will show in the next sections, these borrowers also experience an increase in income following the debt discharge, which could potentially be used to repay their debts. Further, this propensity to repay is similar to what has been recently shown in the literature (*i.e.*, Cookson, Gilje, and Heimer, 2019). These findings provide evidence that one of the effects of relieving borrowers from their student loans is to allow them to better manage their finances and start significantly deleveraging which is likely to make them more resilient to future negative shocks.

4.2. Delinquency and Bankruptcy

A natural question at this point is whether the treated individuals are likely to end up in default again after the discharge. Intuitively, the borrowers that ended up in default the first time around might be more likely to be subject to similar negative shocks in the future or might be less responsible with their finances and, since they are likely credit-constrained, they might find themselves unable to meet their obligations again. However, the findings discussed above would suggest that the lower leverage relative to the control group would reduce the likelihood to being delinquent on their accounts.

We test this hypothesis in Table 5. Panel A investigates the extensive margin, *i.e.*, whether the borrowers are likely to default, by differentiating between total delinquency (which excludes the student loans) and being delinquent on credit cards, auto loans or mortgages. By comparing the results across accounts, we find that treated individuals are delinquent on 0.26 fewer number of accounts in the post period. This is economically significant as it corresponds to 24% decline when compared to the average number of 1.08 delinquent accounts for the treated borrowers. Most of this effect comes from a significant reduction, up to 0.23 accounts, on credit cards. In other words, the debt discharge leads to positive externalities to other creditors, an effect that is not currently discussed in the policy debates about student debt relief.

Figure 5 reports the dynamic coefficients for the probability of being delinquent on any account (except the student loans subject of the collection attempt by National Collegiate). We find that, although treatment and control group exhibit a very similar delinquency behavior for a long period of time before the discharge, about three months after it, the treated borrowers are significantly less likely to be delinquent on any account. This reassures us about our identification strategy and shows that the effects we find are quite consistent and economically significant for the treated individuals.

Panel B of Table 5 quantifies these effects by looking at the delinquency amounts. We find that on average the treated borrowers exhibit about $400 lower delinquency amount. We find that credit card balance declines by $100, while the effect for mortgage and auto loan delinquencies is smaller.

In addition to an effect on delinquency, we explore other measures of distress. Borrowers might be less prone to file for bankruptcy if their student loans are discharged, and can be less likely to fall behind on their mortgages or medical bills. Table 6 investigate these hypothesis by estimating our baseline specification where the outcome variables include an indicator variable for bankruptcy in Column (1), an indicator variable for foreclosure in Column (2), and an indicator variable for defaulting on medical bills in Column (3). Consistent with our delinquency results, we find that treated individuals are 0.04 percentage point less likely to file for bankruptcy. This is economically large when compared to the average bankruptcy filing rate of 0.08 percentage point. In a similar vein, these borrowers are 0.03 percentage point less likely to experience a foreclosure and also less likely to default on their medical bills.

Taken together, we find further evidence that the borrowers significantly improve their financial conditions in the aftermath of their student loan being discharged, as they have lower debt balances and are significantly less likely to experience default.

4.3. Mobility and Income

Having established that borrowers with discharged student debt are able to improve their credit outcomes, we now investigate whether the discharge also improves other real outcomes. One of the key channels through which student debt relief might improve borrowers' condition is by reducing the extent to which these borrowers face debt overhang problems. Specifically, after the debt is discharged, borrowers might have higher incentives to potentially move and pursue better opportunities. This hypothesis has been at the forefront of the policy debates about the costs of rising tuition costs and of student debts being out of control: increased student debt liabilities might have ripple effects also in the labor market. Student debts might impact labor choices both before and after defaults. Intuitively, borrowers might settle for jobs providing a more certain stream of income allowing them to repay the debt at cost of being a less good fit. After defaulting, borrowers might also not have access to jobs performing background and credit checks.

We test this hypothesis by exploiting the breath of our data which includes data on employment and income for a significant sample of borrowers. Table 7 presents estimated coefficients from our baseline regressions using different forms of mobility, income growth and dollar value of income as dependent variables. In column (1), we first measure geographical mobility as changes to the borrowers' ZIP code of residence. Similar to the previous tables, our specification includes individual fixed effects and filing year by calendar year-month fixed effects. We find that borrowers that see their student loan discharged are significantly more likely to move. The effects are both statistically and economically significant; in fact, the treated borrowers are about 0.3 percentage point more likely to move to a different ZIP code in the post period than similar borrowers that still suffer from the student loan burden.

A complementary way of investigating whether treated borrowers are able to improve their economic conditions involves exploiting the granularity of our data, for a restricted sample of borrowers, to test if borrowers' job mobility increases by examining employment changes. Although the test is low-powered due to the lower number of observations, column (2) of Table 7 provides evidence that this is indeed the case: borrowers whose student debt gets discharged are more likely to change jobs relative to the control group of similar borrowers. Column (3) examines the characteristics of this increased mobility and complement these results by showing that borrowers experiencing debt discharge are more likely to move to a new industry.

Finally, columns (4) and (5) complement the previous findings by quantifying the increased income that borrowers, who are not constrained by student debt anymore, are able to achieve in the aftermath of the discharge. We find that treated borrowers do exhibit higher income growth of 1 percentage point and higher income compared to the control group by about \$80 per month. We can use this estimate to quantify the cumulative income gained over the three years after discharge to be \$79.98 $\times$ 37 = \$2,956. This is a substantial gain, as it is equal to about 1.5 months' salary for the average individual in our sample.

Figure 6 plots the dynamic coefficients for income that compare changes in the outcome variable between treated and control groups. We find that, although both groups exhibit very similar income trends for a long period of time before the discharge, income for treated borrowers gradually increases after the discharge.

Overall, we find that treated borrowers are more likely to change home, change jobs and earn more. These findings strongly suggest that the increase in student loans burden for young borrowers might be an important drag on their economic outcomes by limiting their ability to pursue better opportunities. By quantifying the costs of the looming student debt crisis, these findings can inform the debate about the potential benefits of intervening in this market.

5. Plausible Mechanisms

This section discusses plausible mechanisms and heterogeneity in the effects we document.

Discharging debt for defaulted borrowers can affect their credit and labor market outcomes for a number of reasons. For instance, borrowers may have defaulted due to liquidity constraints in the first place, which in turn might have reduced their ability to move or change jobs. Relieving these borrowers from outstanding debt would reduce their constraints and allow them greater flexibility to look for better opportunities. In our setting, for the sub-sample of borrowers on which we have payment data we find that they had stopped making payments on their student loans. Hence, relieving them from debt on which they were delinquent is less likely to give them access to higher disposable income. This makes it less likely that liquidity constraints drive our results. Liquidity constraints may also potentially operate through the control group of borrowers whose wages may be garnished as part of collections. If higher levels of liquidity constraints are imposed on the control group during the post period relative to the treated group, for instance, because the control group is forced to repay the debt through wage garnishment, it may explain some of our results.

To evaluate this potential channel, we examine the heterogeneity in effects across borrowers that reside in states with different levels of restrictions on wage garnishment. Following Lefgren and McIntyre (2009) and Kalda (2019), we split the sample into borrowers that reside in states with severe, medium and no restrictions on wage garnishment, that is, states that impose different caps to wage garnishment, potentially lower than the 25% of disposable income that is the federal maximum. Intuitively, if liquidity constraints are playing a significant role, we expect the results to be stronger in states with more creditor-friendly wage garnishment rules, as in those states our control groups would be more distressed compared to the borrowers who experience the discharge. Table 8 reports results for our main variable of interest: total debt balance excluding student loans in Column (1); mortgage balance in Column (2); credit utilization in Column (3); indicator of any delinquent account in Column (4); indicator of moving to different ZIP code in Column (5); and income growth in Column (6). Across different levels of restrictions on wage garnishment, we find similar effects of debt relief on borrower outcomes further suggesting that liquidity constraints do not seem to play an important role through the control group. However, one can imagine borrowers to save as they expect to be collected upon at some point in the future, with their wages potentially garnished by creditors, so the discharge might potentially relieve them from future liquidity constraints.

Debt discharge may also lead to changes in credit scores for borrowers which may directly or indirectly affect their opportunity set. We evaluate the importance of this channel in our setting by estimating our baseline effects after controlling for credit score changes in a non-parametric manner. Comparing the estimates for this analysis with our baseline estimates would shed light on the importance of this channel in our setting. Table A2 reports results for this analysis where in addition to individual and filing year $\times$ calendar year-month fixed effects, we also control for credit score decile $\times$ month fixed effects. The estimates become stronger than our baseline coefficients when we control for credit score changes. This suggests that changes in credit score is likely not an important mechanism in our setting otherwise one would expect to find smaller magnitudes by controlling for that channel.

Discharging debt may also relieve borrowers from associated debt overhang problems ultimately changing their incentives to provide labor supply and look for better opportunities. If debt overhang is important in our setting, one would expect to find stronger effects for borrowers that experience larger amounts of debt relief. Table 9 evaluates this heterogeneity where we estimate triple interaction coefficients which interacts our baseline difference-in-differences coefficient to dummy variables Above and Below that take a value of one when the debt relief amount for the treated individual is above and below the medial level respectively. As before, the dependent variables include total debt balance excluding student loans in Column (1); mortgage balance in Column (2); credit utilization in Column (3); indicator of any delinquent account in Column (4); indicator of moving to different ZIP code in Column (5); and income growth in Column (6). We find stronger results for borrowers who experience above median levels of debt relief. In fact, our expansionary results in terms of higher mobility and income are concentrated within the sub-sample experiencing above median levels of discharge for which the average debt relief is \$12,259. These results are consistent with debt overhang being a key mechanism at play in our setting, as we find the distortions in the labor market to be present only for borrowers whose debt discharge is large.

There is one more test we can perform to examine the merits of debt overhang by exploiting the granularity of our data and examine the borrowers' wage composition. Table 10 reports results for this estimation where the outcome variable in Column (1) reflects the fraction of total income coming from variable pay defined as the sum of bonus, commissions and overtime pay. We find that borrowers earn higher fraction of their income from variable pay post debt discharge. This is consistent with either borrowers working harder or moving to riskier jobs, both of which would be consistent with debt overhang channel. However, to further distinguish between these two alternatives, we focus on only those borrowers that did not change employer post debt relief in Column (2) and find a similar increase in the fraction of variable pay suggesting that borrowers exert higher effort in their jobs post relief. [15]

6. Discussion

We can now discuss the implications of our results for policymakers by contrasting them with the existing literature.

One key policy question highlighted by the millions of borrowers who were delinquent on their mortgages during the recent financial crisis is how to better support them to get back on their feet, *i.e.*, by targeting monthly payment reduction or principal write-offs. Similarly, the staggering increase in student loan defaults has policymakers debating these issues. Although some theoretical work has suggested the benefits of debt write-downs in the context of the mortgage crisis (see, for instance, Eberly and Krishnamurthy 2014, and Haughwout, Okah, and Tracy 2016), the evidence has suggested that addressing short-term liquidity constraints might be significantly more successful.

In particular, Ganong and Noel (2018) exploit the fact that, through the Home Affordable Modification Program (HAMP), some underwater borrowers received payment reductions for the first five years, due to a maturity extension of their obligations, while others also received an average of $67,000 in mortgage principal forgiveness. The key finding is that, while lower payments lead to lower likelihood of defaulting and higher consumption, mortgage principal reduction has no positive impact on either outcomes.

Our results suggest that the student debt market might require different policy interventions.

[15] Finally, it is worth mentioning that debt relief might also lead to higher income through its effect on labor productivity. If being in default adversely affects productivity, similar to the effect of negative home equity (Bernstein, McQuade and Townsend 2019), relieving debt can potentially alleviate these adverse effects and may lead to an increase in income. Debt relief may also lead to an increase in income if increased potential to move increase employees' bargaining power (Gopalan et al 2018).

Specifically, by analyzing a setting where monthly payments stays at zero, because the borrowers have stopped paying, but the debt is charged off, and by showing that this discharge does have significant effects on these borrowers' outcomes, we draw different conclusions than existing papers.

There might be several potential reasons for these differences. First and foremost, student loans are not dischargeable in bankruptcy, which might make these borrowers significantly more sensitive to debt write-offs than mortgagors. For instance, consider underwater borrowers in non-recourse states, where defaulting on their mortgages might lead to the foreclosure of their homes but not to income garnishment. In contrast, defaulting on student loans would lead to income garnishments and, because there is no statute of limitations, collections will continue to occur till the loan is paid off. Second, it is possible that liquidity constraints may be less important in the student loan market where delinquent borrowers might postpone their payments with deferment or forbearance. Third, underwater borrowers' behavior might be motivated by their desire to keep their homes which would make them sensitive to any immediate intervention to avoid foreclosure. In contrast, the borrowers in our sample have been in default for some time and might not expect to be ever current on their student loans again.

Overall, our conclusions about the importance of debt overhang problems are consistent with recent evidence in the case of credit card modification programs provided by Dobbie and Song (2019), which shows that, despite taking effect after several years, interest write-downs significantly improved the borrowers' financial and labor market outcomes while payment reductions had no positive effects. While we cannot examine how student loan borrowers would react to changes in monthly payments within our setting, our results strongly suggests that, for severely distressed borrowers, debt discharge might significantly improve the borrowers' outcomes.

7. Robustness

In this section, we briefly describe a number of tests we performed to show the robustness of our main findings.

7.1. Alternate Specifications

A potential concern with our setting may be that treated borrowers may be different from control borrowers on some unobserved dimensions. For example, borrowers whose debt is discharged are individuals who chose to show up in court and fight the collection lawsuit while those who lost the case might have not shown up in court. And the propensity to be present in court might be correlated with some unobservable characteristics of the borrower, *i.e.*, financial sophistication. Although the absence of pre-trends is reassuring, we can explore variation in the timing of the discharge to appease this concern. Specifically, rather than comparing borrowers whose loan get discharged because National Collegiate lost their paperwork with those whose loan is not discharged, we can take advantage of the fact that not all loans are discharged at the same time. Then, we can compare borrowers who are discharged to those who are not discharged yet. This is helpful in mitigating any concern that somehow the discharge is correlated with unobservable characteristics of the borrowers because all borrowers in this restrictive sample experience discharge.

Table 11 reports results from similar difference-in-differences regressions to the previous ones but focusing only on the treated group of individuals. Column (1) reports results for total borrower's debt balance, Column (2) focuses on mortgage balance, Column (3) on credit utilization, Column (4) delinquent accounts, while Columns (5) and (6) examine the effects on mobility and income growth respectively. We find very consistent results with the baseline specifications as the borrowers that see their student loan discharged tend to reduce their liabilities, are less likely to be delinquent, but more likely to move and experience higher income growth.

In addition, we use a second (matched) control group and re-estimate our outcomes. Specifically, this control group comprises borrowers that were similarly situated in default but whose loan was not held by National Collegiate. We build this control group by including all other individuals who reside in the same ZIP Code as the treated borrowers, are of the same age (less than one year apart), carry similar student loan amounts, and crucially, who defaulted on their student loans as well. In other words, this control group comprises borrowers exposed to the same local economic conditions as the treated group, with similar demographic characteristics, who also defaulted on their student debts, but whose loan was not held by National Collegiate, which resulted in their debt not being discharged. This control group has the advantage of being significantly larger which allows us to include additional controls. In addition, this group allows us to plot the dynamics of the effects separately for the treated and control groups and examine which group drives the effects we observe.[16] We also include county $\times$ event-time fixed effects in our analysis that ensure the effects are estimated by comparing treated and control borrowers who reside in the same county and are treated in the same month.

Table A3 reports estimates for this analysis. As before, Column (1) reports results for total borrower's debt balance, Column (2) focuses on mortgage balance, Column (3) on credit utilization, Column (4) delinquent accounts, while Columns (5) and (6) examine the effects on mobility and income growth respectively. We find results that are very consistent with the baseline specifications, as the borrowers that see their student loan discharged tend to de-lever, are less likely to be delinquent, but more likely to move and experience higher income growth. This suggests that unobservable differences across treated and control groups in our baseline specification are not likely to drive our results.

To examine if our baselines effects are driven by changes in treated or control borrowers, we plot our main outcome variables separately for both groups in Figures 1 through 4 of the Online Appendix. Across all figures, the blue color represents the treated individuals while the red color represents the matched control group. Figure 1 plots the estimates for total debt (excluding student loans) and, similar to Figure 3, shows that both treated and control group experienced similar changes in the outcome variable in the pre-period. However, they diverge following debt discharge as total balance declines significantly more for the treated group relative to the control group. Figure 2 plots the dynamics separately for credit card utilization and corroborates the finding that changes in credit card utilization were similar across both groups in the pre-period but diverged after debt discharge as utilization reduced for the treated individuals but remained at similar levels for the control group. In Figures 3 and 4, we plot delinquencies and income respectively. These plots show that in pre-period, the changes in the outcome variable were similar across both groups, however, they diverge in the post period mainly driven by changes in the treated group.

The previous specification purposely examines outcome for borrowers 36 months around treat-

[16]Since we don't observe the date of adjudication for borrowers in our baseline control group, we are not able to assign an event time for them and hence not able to plot the dynamics separately. However, with the matched sample, we are able to use the matched treated borrower's event time for the control group to estimate these plots.

ment because one may expect some of the effects to manifest over a few months following court judgments. However, our results are also robust to confining our analysis to a balanced panel one year around treatment as reported in Table A4. As suspected, some results are smaller in magnitudes as the effect of debt relief becomes stronger in due course following treatment. Finally, Table.A5 shows that including calendar-month fixed effects in our estimation using the matched sample yields similar results.

7.2. Sample Selection with Employment Data

Since our employment data is only available for a subset of borrowers and is skewed towards large employers within the US, it may raise concerns regarding sample selection. For instance, a potential concern maybe that borrowers included in the employment database maybe different from those not included and hence, the effects estimated using the employment data may not apply to other borrowers within the sample. More importantly, the debt discharge may impact both the probability of being employed by any firm and the probability of being employed by a firm in the earnings data. For example, if larger employers are more likely to do credit checks and not hire individuals with delinquencies, debt relief and removal of delinquency flag may lead to a higher likelihood of individuals being employed by employers within the dataset. Alternatively, delinquent individuals may avoid large employers if they perceive them to be more likely to comply with wage garnishment requests, and hence debt relief would be correlated to the likelihood of being employed by larger employers included in our database.

To evaluate the merits of these concerns in our setting, we begin by noting that, as detailed in Kalda (2019), the employment data is both geographically and industrially representative of the U.S. population with a similar income distribution. Further, the distribution of individual borrowing and credit scores for individuals in the employment database is statistically indistinguishable to that of the population. We confirm this latter comparison for borrowers within our sample by comparing borrowing behavior for those included in the employment database to those not included.

Figure 5 plots this comparison. The first three rows plot different categories of borrowing (i.e. total borrowing, credit card and home loans) while the fourth row plots credit score. The column on the left plots the empirical cumulative density function (CDF) for the number of accounts in different categories while the one on the right plots the kernel density function for balance amounts. The solid line represents individuals covered in the employment database while the dashed line represents those who are not. The first row shows that individuals included in the employment database have slightly higher number of open accounts but hold similar credit balances. Both groups of borrowers hold similar number of credit card accounts and mortgage accounts with similar total balances across both categories. Finally, borrowers included in the employment database also have similar credit scores as those not included in the database.

Next, we examine whether or not individuals are more likely to be included in the employment database following student debt relief. Column (1) of Table A6 reports estimate of this analysis which suggests no significant relation between student debt relief and the likelihood of being included in the employment data. In column (2), we further test this by examining whether borrowers are more likely to enter the employment database following debt relief and find that this is not the case.[17] Finally, we examine whether debt relief leads to a higher likelihood of individuals leaving the employment data in column (3) and find no statistical relation between the two.

Overall, similar to Kalda (2019), these findings suggest that borrowers included in the employment database are similar to those not included across different observable dimensions. And that student debt relief does not affect the likelihood of being included in the employment data.

7.3. Variation in Treatment Timing

Our setting utilizes staggered rulings of lawsuits filed against defaulted borrowers thus yielding a variation in treatment timing. A potential concern with this setting is that borrowers whose debt was relieved in the latter years may be inherently different from those whose debt was relieved in earlier years and hence our estimates may not necessarily apply to them. For example, those treated in the second half of the sample may have defaulted strategically owing to the information about the first half of the sample. We examine the importance of this concern by first plotting the distribution of lawsuit filing years for treated and control borrowers across different years in Figure 6. If there's a strategic element involved, one may expect higher proportion of treated borrowers' cases being

[17] Note that we allow individuals to enter and drop out of the sample based on whether or not they are included in the employment database.

filed in the latter years relative to the control borrowers. However, we find similar patters in the number of filings across both groups over the years. In addition, we estimate the heterogeneity of our main results for treated borrowers whose debt was relieved before 2015 and those whose debt was relieved later. Table A7 reports results for this estimation where we find similar results for both groups suggesting that both groups of borrowers are likely similar.

Another potential concern is that coefficients in settings with variation in treatment timing may be biased particularly if treatment effect varies with time as shown by recent academic works (e.g. Goodman-Bacon 2018). However, the similarity of results for borrowers whose debt was relieved during latter versus earlier part of the sample suggests that treatment effects don't vary substantially through time in our setting. Notwithstanding this result, we employ Goodman-Bacon decomposition theorem in order to examine the sources of variation in our setting and further explore whether staggered nature of our treatment potentially biases our coefficients. Table A8 reports estimates for this decomposition. Since the theorem requires a balanced panel, we confine our sample to a balanced panel with two years around treatment.[18]

As before, Column (1) reports results for total borrower's debt balance, Column (2) focuses on mortgage balance, Column (3) on credit utilization, Column (4) delinquent accounts, while Columns (5) and (6) examine the effects on mobility and income growth respectively. We find that a substantial portion of the variation that we capture in our treatment effects comes from variation in timing of treatment. Specifically, for the first five columns, 44.8% (i.e 22.4%+22.4%) and for the last column 37.4% (i.e. 18.7%+18.7%) variation comes from timing. However, for all outcomes except delinquencies the direction of the effect affiliated with variation in timing (i.e. earlier vs later treated and later vs earlier treated) is the same as the effect coming from comparing treated and never treated borrowers as well as our baseline estimates. Further, the magnitudes associated with earlier vs late treated and later vs earlier treated groups are similar. This suggests that differences in treatment timing likely don't bias our estimates. For delinquencies, the estimate from the decomposition calculated as the weighted average of the three components is negative (similar to our baseline estimates) but suggests that our baseline estimates may be biased towards zero owing to the bias induced by variation in timing. However, the magnitude of the bias seems small as the weighted

[18]Because we don't observe treatment date for the control group, we confine the control borrowers to two years around the case filing month.

average including all three components suggests a decline in delinquent accounts of 0.152 and the weighted average that subtracts the part related to treatment timing shows a decline of 0.172.

8. Conclusion

A crisis in the student loan market has said to be looming over the economy, due to an explosion in recent graduates' indebtedness since the Great Recession and a worrisome increase in delinquency. Several policies have been advocated to help borrowers unable to meet their financial obligations, especially in the private student loan market, which is usually tapped by more fragile borrowers attending for-profit institutions and experiencing lower returns to education. Although these issues have led to increased interest, we still know very little about what would be the benefits of offering some type of debt relief to borrowers in need. Furthermore, policy makers need guidance on the type of policies that are likely to be effective in this market, from those addressing the immediate liquidity constraints of some of these borrowers to more ambitious policies aimed to forgive a portion of their debts. The main challenge faced by the existing literature has been the inability to observe detailed information about borrowers' balance sheets and decisions over time coupled with the difficulty to infer the causal link between debt and behavior due to the lack of plausibly-exogenous variation in the data.

This paper overcomes these challenges by taking advantage of the debt discharge that affected thousands of borrowers across the US due to the inability of National Collegiate to prove chain of title of the debts and by matching hand-collected lawsuits filings with individual credit bureau information. This allows us to build a unique panel dataset enabling us to estimate the effects of debt relief on borrowers.

In contrast to what is believed the effect of debt forgiveness is, we find that the borrowers experiencing the debt relief shock are significantly more likely to engage in deleveraging, by both reducing their demand for credit and limiting the use of the existing accounts. That is, borrowers benefiting from a debt relief seem to quickly try to improve their financial conditions. These efforts are successful in that they are also significantly less likely to default on their accounts, above and beyond their student loan accounts. Furthermore, these effects are not transitory. These findings speak to the potential spillover effects across borrowers' liabilities and to an indirect benefit of intervening in the student loan market by helping borrowers unable to afford their student loan debts. Finally, debt relief helps these borrowers to overcome debt overhang constraints as they are significantly more likely to move, change job and experience a significant increase in income. Although we are not able to estimate the moral hazard costs associated with debt relief in the student loan market, our findings speak to the forceful impact that it could have on distressed borrowers and can inform targeted policy interventions in this market.

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Additional Details about Debt Collection

This section provides few more legal details about the debt collection process in general and the specific instances that led National Collegiate to lose thousands of lawsuits across the country. The buying and selling of debt, in particular of past due debt, has become so commonplace that has spurred a whole new industry around debt collection. According to data from industry researcher IBISWorld, there are more than 7,000 debt collectors, who made more than $11 billion combined in 2018.[19] Even more telling is the fact that 14 percent of consumers have recently experienced debt collection, according to the Federal Reserve of New York.

In case of borrowers default, a law firm acting on behalf of the original lender or, more likely, of a debt buyer can sue debtors at any point before the states statute of limitations expires on the debt, typically between 3 and 10 years. After the lawsuit filing, a borrower is served with a summons requiring the debtor to appear in court on a certain date. If the borrower fails to appear, the debt holder typically wins a default judgment against the borrower. This has resulted in the spray and pray approach used by debt holders who try to win default judgements by filing lawsuits on a large number of cases.

If the borrower does appear in court, then one of the key roles played by the proceeding is to determine whether there is sufficient evidence showing that the creditor has the proper legal status and documentation to collect on the debt. Most state and local procedural rules put heavy documentation requirements on both the debt collector and creditor. In many states, the debt collector must attach to the complaint a copy of the account or written contract or agreement. This is often referred to as the attachment rule. Furthermore, the debt collector must also produce a copy of the original written agreement between the parties, such as the loan note or credit card agreement, preferably signed by the debtor.

If the account has been sold to another creditor, then that creditor must prove that it has the right to sue to collect the debt. This usually means producing proof that the debt was assigned to it. Often such proof will be a bill of sale, an assignment, or a receipt between the last creditor holding the debt and the entity suing the debtor. Also, the account needs to be verified, which usually translates into the creditor providing affidavits signed by a witness who "must have personal

[19] See, for instance, https://www.washingtonpost.com/business/2019/08/07/zombie-debt-how-collectors-trick-consumers-into-reviving-dead-debts/.

knowledge of the transactions and/or be familiar with the books and records."

If the creditor provides this information, which is what happens in the vast majority of cases, the court enters a Consent Judgment against the borrower, where the borrower admits he owes the debt, for an amount equal to the outstanding balance plus interest and court fees. With this judgement, the debt collector is allowed to contact the borrowers employer in order to garnish the borrowers wages up to certain statutory limits.

If, instead, the creditor or collector suing fails to produce proof of the assignment, the judge can dismiss the case. And this is what happened to our treated individuals because, as said by the CFPB Director Richard Cordray, The National Collegiate Student Loan Trusts and their debt collector sued consumers for student loans they could not prove were owed and filed false and misleading affidavits in courts across the country.

Specifically, between 2001 and 2007, National collegiate purchased and securitized the loans, and then sold notes secured by the loans to investors.[20] The trusts have no employees but instead use service providers, such as Transworld Systems, Inc. a nationwide debt collector, to interact with consumers about their loans. Transworld Systems employees complete, sign, and notarize sworn legal documents for collections lawsuits brought on behalf of the trusts. The CFPBs investigation launched at the end of 2017 found that the companies violated the Dodd-Frank Wall Street Reform and Consumer Financial Protection Act by filing false affidavits and for pursuing collections lawsuits they could not have won.

Specifically, the companies sued consumers for debts the trusts could not prove were owed, because the companies did not possess the necessary documentation required to sue. In these lawsuits, the trusts did not have or could not find the documentation necessary to prove either that they own the loans or that the consumer owed the debt. In some of these cases, the document the consumer signed promising to pay back the loan was missing. In numerous instances, the affiants lacked personal knowledge of the chain of assignment records necessary to prove that National Collegiate owned the debts. Nonetheless, the trusts filed suit against consumers to collect the debts. The Bureau also alleged that the National Collegiate Student Loan Trusts filed several collections lawsuits after the applicable statute of limitations on the debt collection had expired. Additionally, the complaint

[20]The information contained here has been provided by the consent order submitted by the CFPB against Transworld Systems, Inc. and National collegiate Trusts.

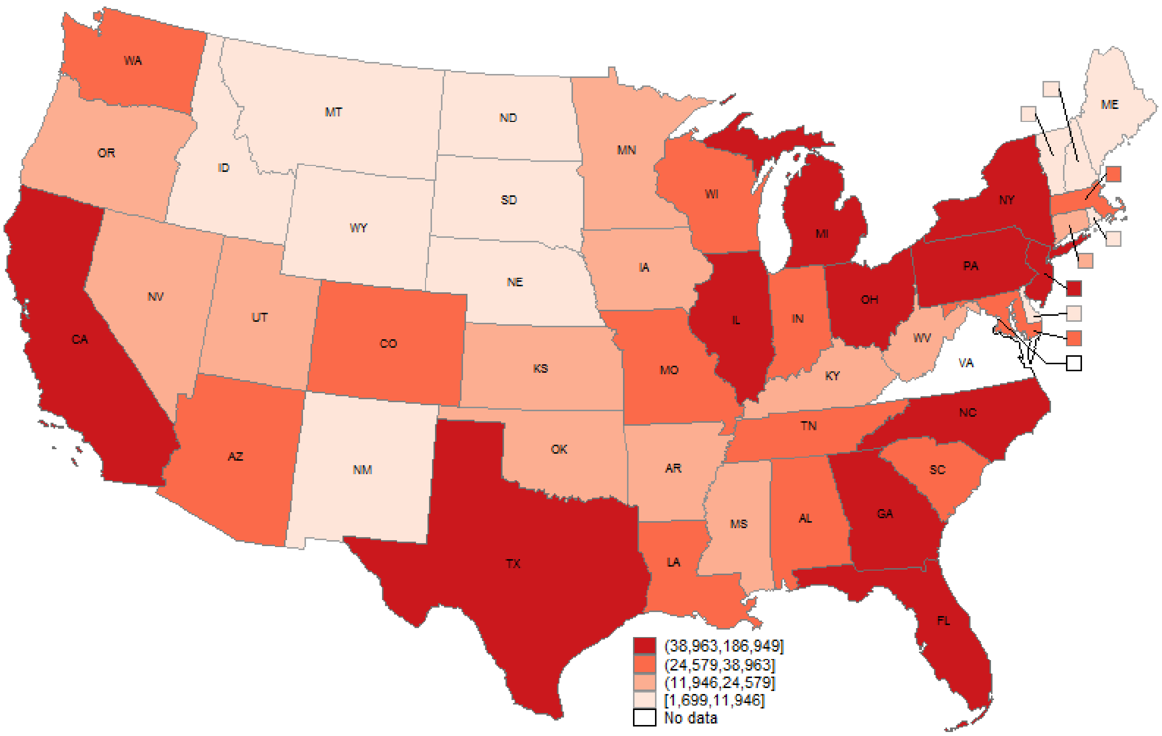
alleged that, in numerous instances, many of the affidavits filed were improperly notarized because they were not sworn or signed in the presence of the notary.

The case of National Collegiate is likely not the only one. Debt buyers buy thousands of accounts in bulk and have no way to readily verify the accuracy or the veracity of every account they collect on. Yet, they are likely to submit affidavits to courts swearing to the accuracy of these records. Additional evidence suggesting that this is likely to be a widespread phenomenon, is the staggering number of complaints, more than 81,000 in 2018, about debt collectors that have been submitted to the CFPB. It is no secret that the debt collection process is routinely abused by creditors, to the extent that the CFPB has proposed a first major update to the Fair Debt Collection Practices Act in more than 40 years. Our findings also show that limiting such abuses might be extremely beneficial for the borrowers and, ultimately, the economy.

Figure 1. Geographical Distribution of the Delinquent Student Loan Borrowers

The figures plot geographic distribution, at state level, of student loan borrowers. In Panel A, we plot total number of delinquent student loan borrowers based on complete credit bureau data. In Panel B, we plot number of treated individuals in our sample, who had delinquent student loans, but received debt relief due to favorable court rulings.

Panel A: All Delinquent Student Loan Borrowers



Panel B: Treated Individuals in Our Sample

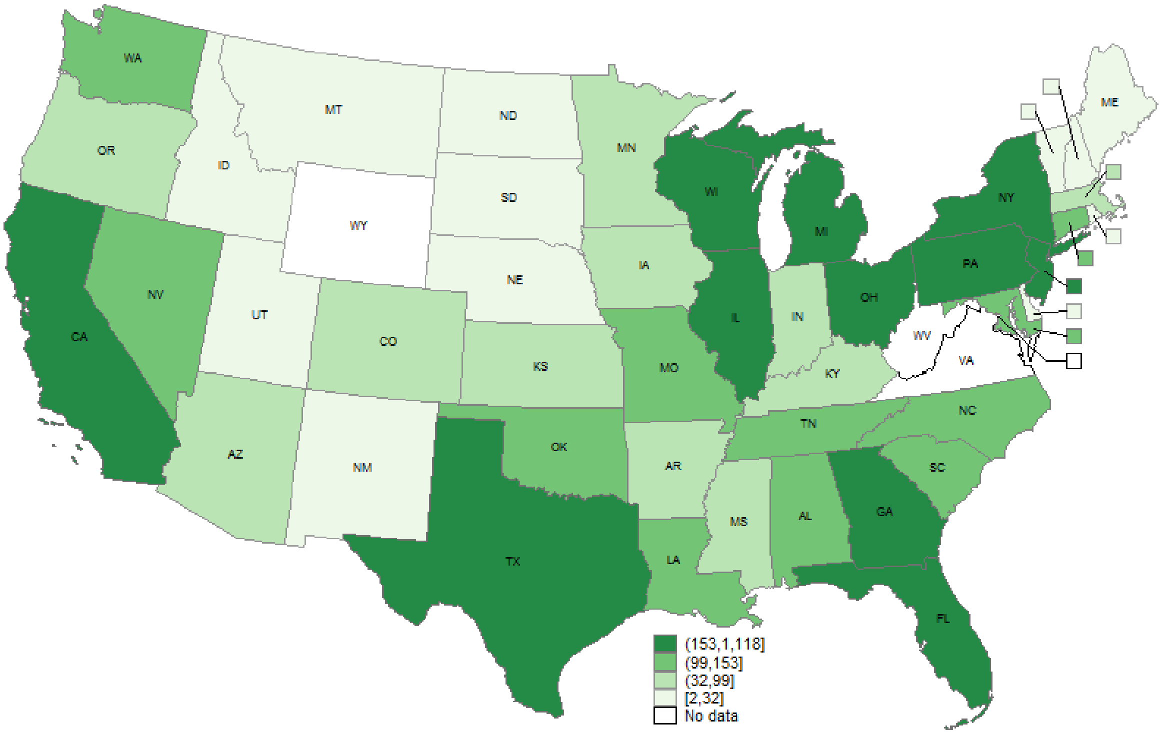


Figure 2. Number of Legal Settlements

The figure plots number of legal settlements over time. Y axis is the number of legal settlements we hand-collected from court cases. X axis is the court ruling month.

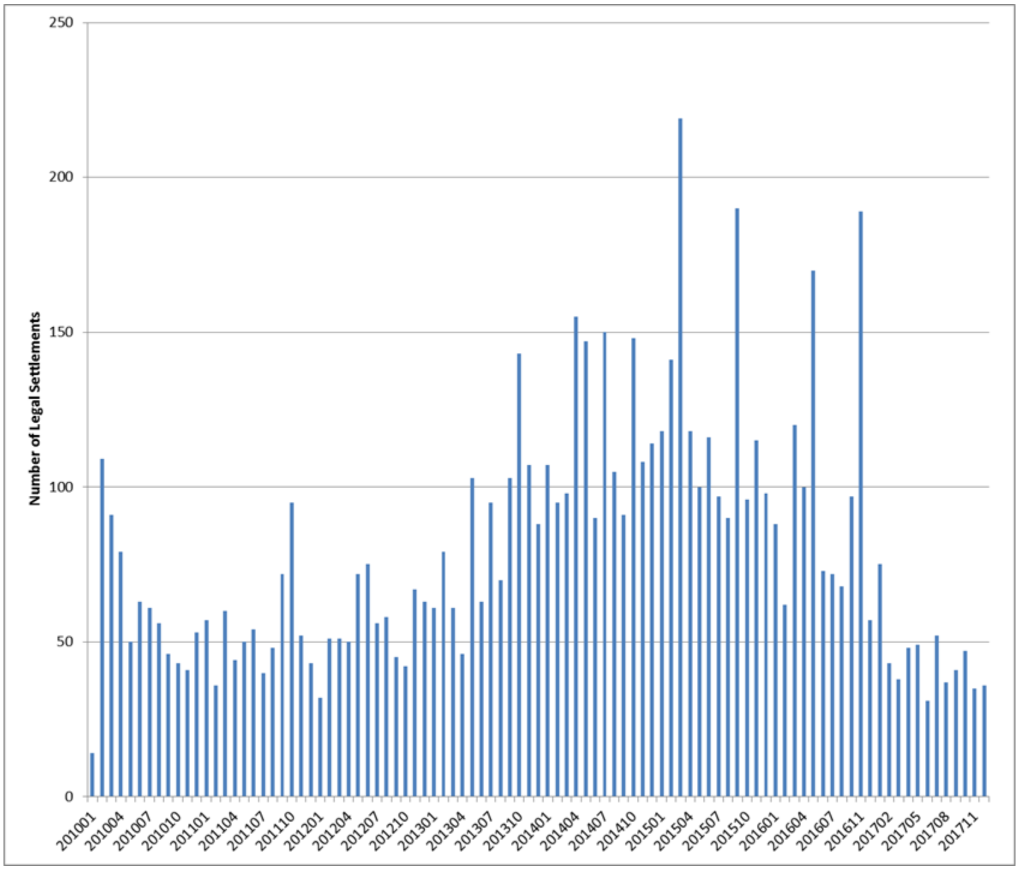


Figure 3. Dynamics of the Total Debt Balance

The figure plots the coefficients on the interaction term of treated borrower indicator and relative monthly dummies from regressions specified in Equation (2). Relative monthly dummies are defined as the interval, in months, from the debt discharge date to credit report date. Dependent variable is total debt balance (excluding student loans). On the right hand side, we control for individual fixed effects and filing year × calendar year-month fixed effects. Standard errors are clustered at ZIP Code level.

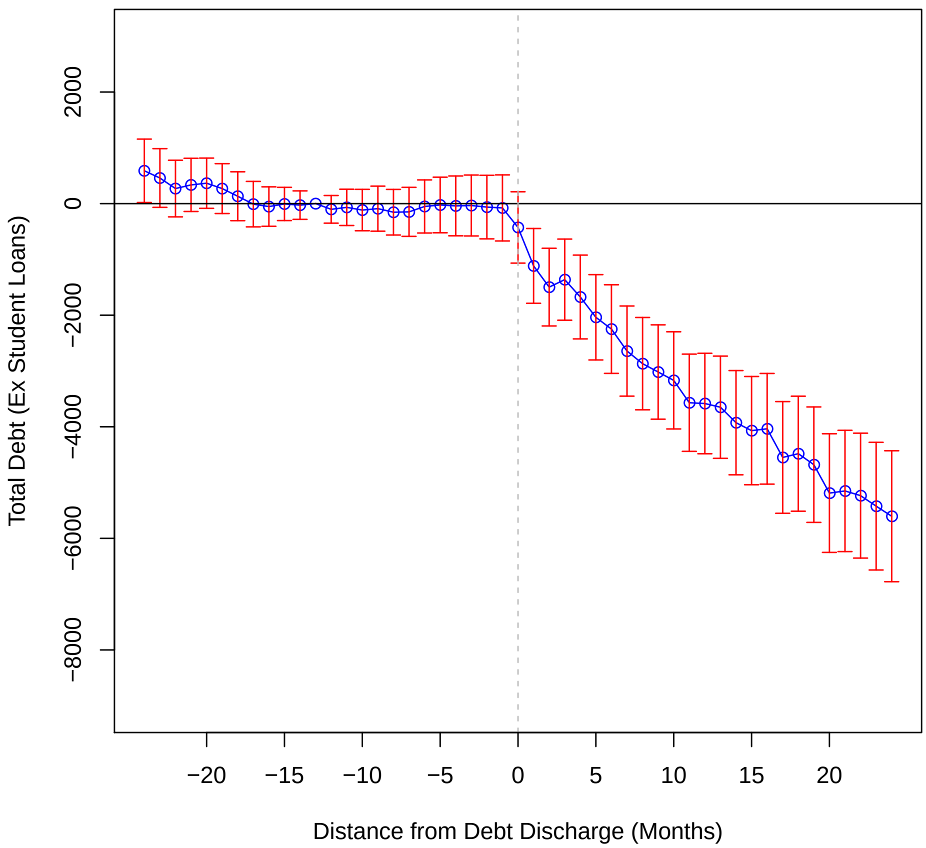


Figure 4. Dynamics of the Credit Card Utilization

The figure plots the coefficients on the interaction term of treated borrower indicator and relative monthly dummies from regressions specified in Equation (2). Relative monthly dummies are defined as the interval, in months, from the debt discharge date to credit report date. Dependent variable is credit utilization, calculated as ratio of revolving balance to revolving credit limit. It varies between 0 and 1. On the right hand side, we control for individual fixed effects and filing year $\times$ calendar year-month fixed effects. Standard errors are clustered at ZIP Code level.

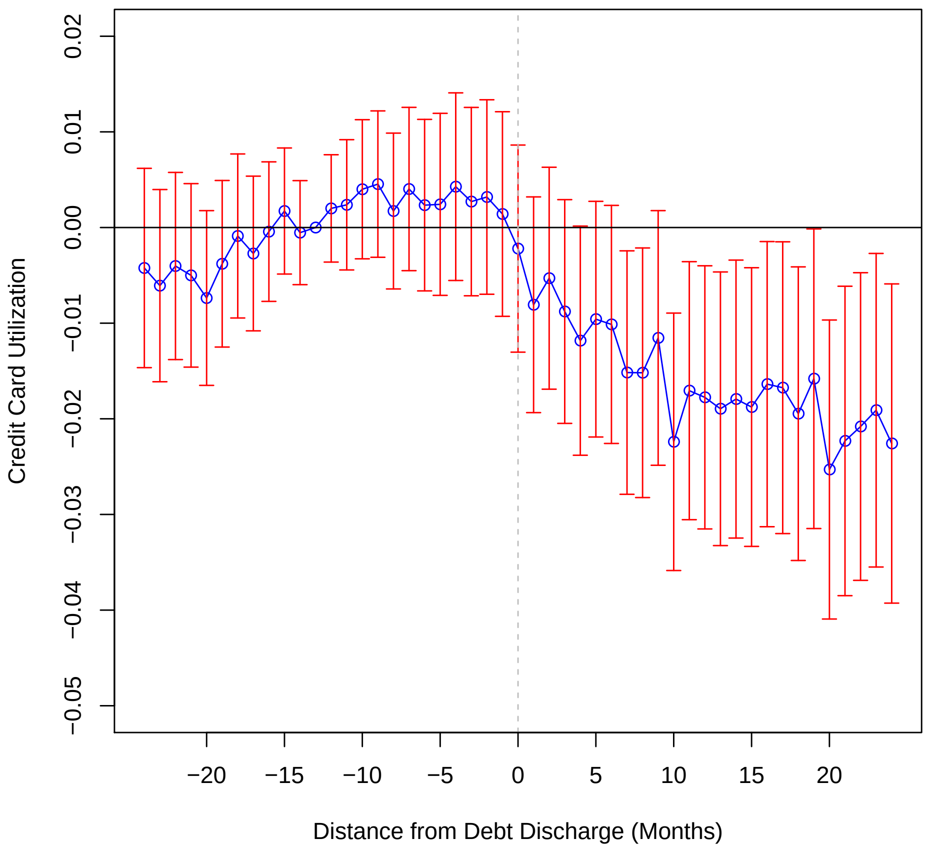


Figure 5. Dynamics of Delinquency Rate

The figure plots the coefficients on the interaction term of treated borrower indicator and relative monthly dummies from regressions specified in Equation (2). Relative monthly dummies are defined as the interval, in months, from the debt discharge date to credit report date. Dependent variable is the indicator of borrower having any delinquent account. On the right hand side, we control for individual fixed effects and filing year × calendar year-month fixed effects. Standard errors are clustered at ZIP Code level.

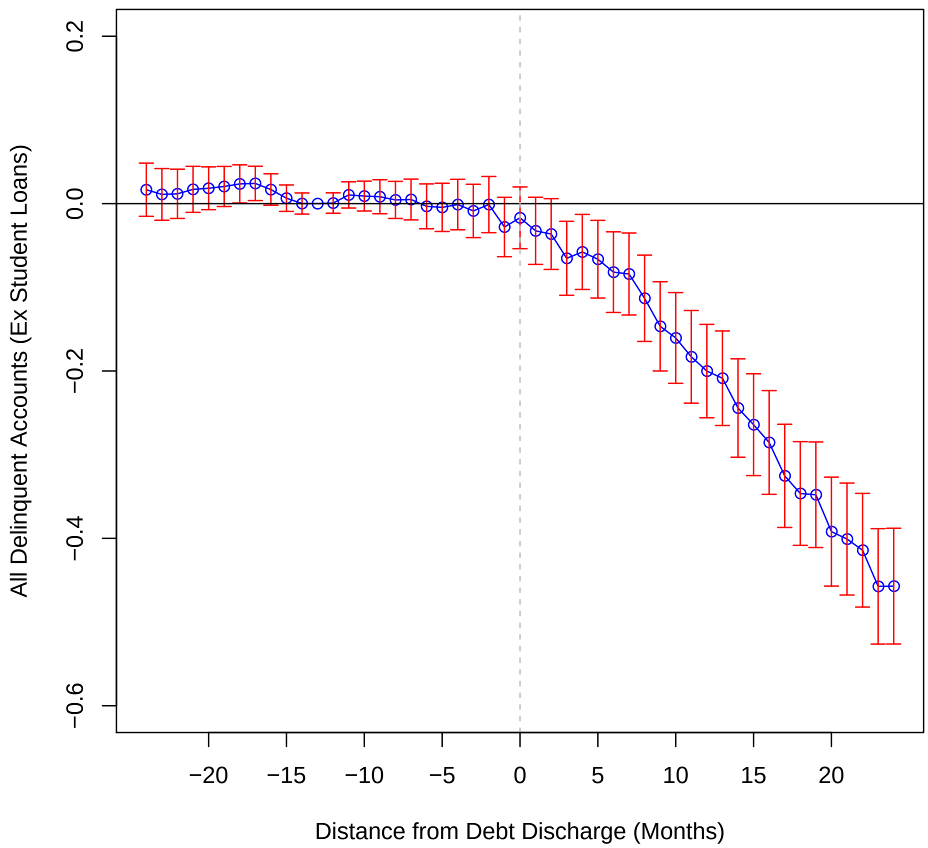


Figure 6. Dynamics of Income

The figure plots the coefficients on the interaction term of treated borrower indicator and relative monthly dummies from regressions specified in Equation (2). Relative monthly dummies are defined as the interval, in months, from the debt discharge date to credit report date. Dependent variable is dollar value of income. On the right hand side, we control for individual fixed effects and filing year $\times$ calendar year-month fixed effects. Standard errors are clustered at ZIP Code level.

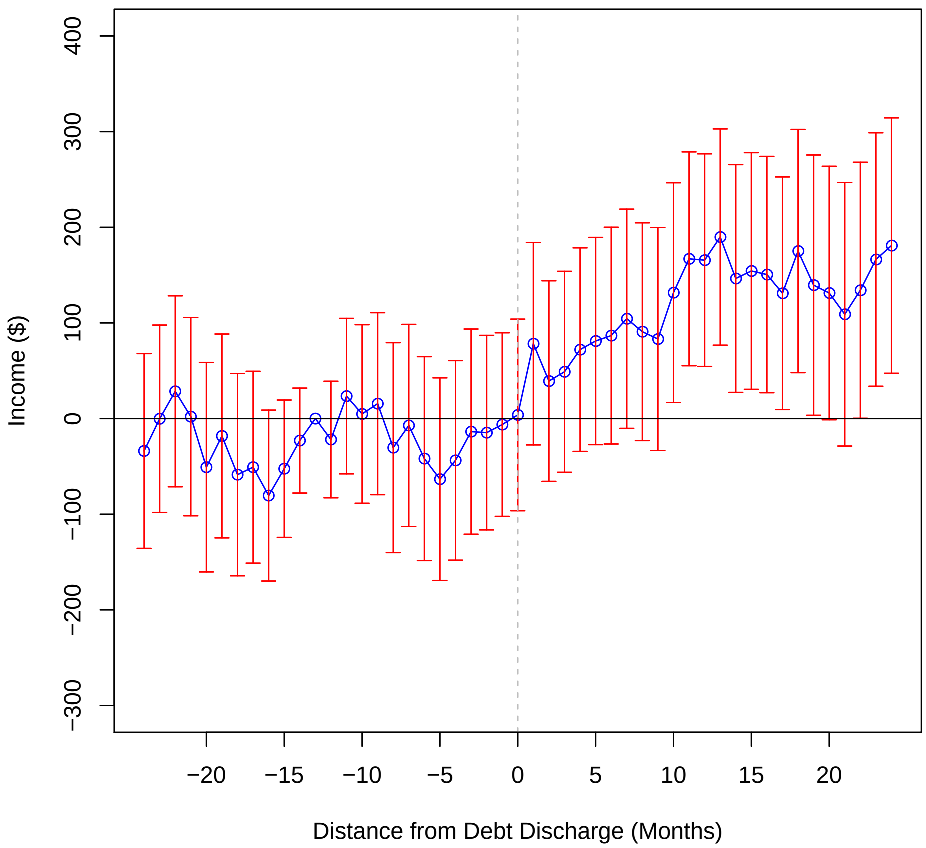


Table 1: Summary Statistics

This table reports mean differences for treated and control groups of borrowers. We hand-collected a set of borrowers who were sued by National Collegiate as they were delinquent on their student loans. A sub-set of these individuals experienced debt relief (treated group) as the trust lost a series of law-suits while others did not experience relief (control group). We compare attributes across these two groups during the month of filing in the last column. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	Treated Borrowers	Control Borrowers	Treated-Control
Number of Accounts (Ex Student Loans)	6.55	6.27	0.28
Student Loan Accounts	3.57	3.41	0.16
Credit Card Accounts	3.12	3.22	-0.10
Auto Accounts	0.624	0.612	0.012
Mortgage Accounts	0.211	0.194	.017
Total Debt Balance (Ex Student Loans, $)	39,385.82	39,441.42	-55.6
Student Loan Balance ($)	13,605.80	12,886.21	719.51*
Credit Card Balance ($)	2,060.39	2,191.93	-131.54
Auto Balance($)	4,843.04	5,091.91	-248.87
Mortgage Balance ($)	16,906.33	17,321.38	-415.05
Credit Card Utilization	0.395	0.382	0.013
Delinquent Accounts (Ex Student Loans)	1.08	1.01	0.07
Age	34.75	35.11	-0.36*

Table 2: Student Loan and Credit Score

This table reports results from difference-in-differences regressions of student loan accounts and balance, and credit score based on borrower-month panel data. The dependent variable is the number of student loan accounts in Column (1); student loan balance in Column (2); and credit score in Column (3). $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual and filing year × calendar year-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Dependent Var	Student Loan Accounts	Student Loan Balance	Credit Score
	(1)	(2)	(3)
$DebtRelief \times Post$	-0.73***	-7404.56***	6.81***
	(0.05)	(340.5)	(1.23)
Individual FE	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes
Observations	1,283,639	1,283,639	1,283,639
R^2	0.74	0.78	0.81

Table 3: Debt Usage

This table reports results from difference-in-differences regressions of consumer debt based on borrower-month panel data. In Panel A, the dependent variables are number of different types of accounts: total number of accounts excluding student loans in Column (1); number of credit cards in Column (2); number of auto accounts in Column (3); and number of mortgage accounts in Column (4). In Panel B, the dependent variables are total balances on different accounts: total debt balance excluding student loans in Column (1); balance of credit cards in Column (2); balance of auto accounts in Column (3); and balance of mortgage accounts in Column (4). $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual and filing year × calendar year-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A: Number of Accounts

Dependent Var	No of Accounts (Ex. Stud)	Credit Card Accounts	Auto Accounts	Mortgage Accounts
	(1)	(2)	(3)	(4)
$DebtRelief \times Post$	-0.47***	-0.36***	-0.01	-0.04***
	(0.04)	(0.02)	(0.01)	(0.005)
Individual FE	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes
Observations	1,283,639	1,283,639	1,283,639	1,283,639
R^2	0.81	0.81	0.72	0.85

Panel B: Debt Balances

Dependent Var	Total Debt Balance (Ex. Stud)	Credit Card Balance	Auto Balance	Mortgage Balance
	(1)	(2)	(3)	(4)
$DebtRelief \times Post$	-4,600.88***	-618.76***	-77.37	-1564.60***
	(387.55)	(36.22)	(88.43)	(181.82)
Individual FE	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes
Observations	1,283,639	1,283,639	1,283,639	1,283,639
R^2	0.77	0.65	0.51	0.75

Table 4: How Do Individuals Reduce Debt?

This table reports results from difference-in-differences regressions of consumer debt strategies based on borrower-month panel data. In Panel A, the dependent variables are changes in credit card accounts: credit utilization in Column (1); credit limit in Column (2); number of accounts opening in Column (3); monthly payment in Column (3). In Panel B (C), the dependent variables are changes in auto (home) accounts: origination amount in Column (1); number of accounts opening in Column (2); monthly payment in Column (3). *DebtRelief* is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. *Post* is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual and filing year × calendar year-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A: Credit Cards				
Dependent Var	Utilization	Credit Limit	Account Opening	Payment
	(1)	(2)	(3)	(4)
DebtRelief × *Post*	-0.023***	-971.82***	-0.002**	13.57***
	(0.007)	(67.72)	(0.001)	(2.47)
Individual FE	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes
Observations	1,283,639	1,283,639	1,283,639	513,455
R^2	0.608	0.803	0.042	0.477
Panel B: Auto Loans				
Dependent Var	Origination Amount	Account Opening	Payment	
	(1)	(2)	(3)	
DebtRelief × *Post*	-585.27**	0.0003	9.18***	
	(234.72)	(0.001)	(2.66)	
Individual FE	Yes	Yes	Yes	
Filing Year × YM FE	Yes	Yes	Yes	
Observations	1,283,639	1,283,639	269,788	
R^2	0.67	0.02	0.52	
Panel C: Home Loans				
Dependent Var	Origination Amount	Account Opening	Payment	
	(1)	(2)	(3)	
DebtRelief × *Post*	-7,340.27***	0.000	57.77***	
	(662.21)	(0.000)	(17.43)	
Individual FE	Yes	Yes	Yes	
Filing Year × YM FE	Yes	Yes	Yes	
Observations	1,283,639	1,283,639	109,888	
R^2	0.84	0.01	0.63	

Table 5: Credit Delinquency

This table reports results from difference-in-differences regressions of consumer delinquency outcomes based on borrower-month panel data. In Panel A, the dependent variables are number of delinquent accounts: number of all delinquent accounts excluding student loans in Column (1); number of delinquent credit card accounts in Column (2); number of delinquent auto accounts in Column (3); number of delinquent mortgage accounts in Column (4). In Panel B, the dependent variables are balance of delinquent accounts: balance of all delinquent accounts excluding student loans in Column (1); balance of delinquent credit card accounts in Column (2); balance of delinquent auto accounts in Column (3); balance of delinquent mortgage accounts in Column (4). *DebtRelief* is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. *Post* is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual and filing year × calendar year-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A: Extensive Margin

Dependent Var	All DLQ (Ex. Stud)	Credit Card DLQ Accounts	Auto DLQ Accounts	Mortgage DLQ Accounts
	(1)	(2)	(3)	(4)
DebtRelief × *Post*	-0.26***	-0.23***	0.01*	-0.01***
	(0.021)	(0.020)	(0.006)	(0.003)
Individual FE	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes
Observations	1,283,639	1,283,639	1,283,639	1,283,639
R^2	0.65	0.66	0.62	0.70

Panel B: Intensive Margin

Dependent Var	All (Ex. Stud) DLQ Amount	Credit Card DLQ Amount	Auto DLQ Amount	Mortgage DLQ Amount
	(1)	(2)	(3)	(4)
DebtRelief × *Post*	-436.24***	-108.03***	16.22	-33.79***
	(135.63)	(-16.32)	(13.23)	(11.26)
Individual FE	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes
Observations	1,283,639	1,283,639	1,283,639	1,283,639
R^2	0.52	0.52	0.51	0.39

Table 6: Bankruptcy and Medical Defaults

This table reports results from difference-in-differences regressions of consumer bankruptcy and medical default outcomes based on borrower-month panel data. The dependent variable is an indicator for bankruptcy in Column (1); an indicator for foreclosure in Column (2); and an indicator for medical defaults in Column (3). $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual and filing year $\times$ calendar year-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%). All coefficients are multiplied by 100 for ease of interpretation.

Dependent Var	Bankruptcy	Foreclosure	Medical Defaults
	(1)	(2)	(3)
$DebtRelief \times Post$	-0.04***	-0.03***	-0.1***
	(0.01)	(0.01)	(0.02)
Individual FE	Yes	Yes	Yes
Filing Year $\times$ YM FE	Yes	Yes	Yes

Table 7: Mobility and Income

This table reports results from difference-in-differences regressions of consumer mobility and income outcomes based on borrower-month panel data. The dependent variable are indicators of moving: mobility based on moving to a different ZIP code in Column (1); job mobility based on moving to a different job in Column (2); job mobility based on moving to a job in different industry (NAICS two-digit) in Column (3); income growth in Column (4); and dollar value of income in Column (5). $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual and filing year × calendar year-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Dependent Var	Mobility	Job Change	Moving to Different Industry	Change in Income (%)	Income ($)
	(1)	(2)	(3)	(4)	(5)
$DebtRelief \times Post$	0.003***	0.003**	0.003*	0.01**	79.98***
	(0.001)	(0.001)	(0.002)	(0.004)	(31.99)
Individual FE	Yes	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes	Yes
Observations	1,283,639	211,716	197,874	91,230	106,580
R^2	0.19	0.12	0.13	0.12	0.54

Table 8: Heterogeneity by Wage Garnishment Laws

This table reports results from difference-in-differences regressions of consumer debt, delinquency, mobility and income outcomes based on borrower-month panel data for sub-samples with different levels of wage garnishment restrictions at the states of residences. Panel A represents states with highest level of restrictions while Panel B and C represent medium and lowest levels of restrictions. The dependent variables are total debt balance excluding student loans in Column (1); balance of mortgage accounts in Column (2); credit utilization in Column (3); number of all delinquent accounts excluding student loans in Column (4); mobility based on moving to a different ZIP code in Column (5); and income growth in Column (6). $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual and filing year × calendar year-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A: Severe Restrictions

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
$DebtRelief \times Post$	-5,292.76***	-1,950.56***	-0.02**	-0.17***	0.004**	-0.001
	(581.19)	(571.33)	(0.01)	(0.03)	(0.002)	(0.005)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	529,330	529,330	529,330	529,330	529,330	37,318
R^2	0.76	0.81	0.62	0.75	0.29	0.54

Panel B: Medium Restrictions

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
$DebtRelief \times Post$	-5,202.27***	-1,425.88**	-0.03***	-0.14***	-0.001	0.01
	(808.47)	(697.92)	(0.01)	(0.04)	(0.002)	(0.01)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	345,498	345,498	345,498	345,498	345,498	23,868
R^2	0.79	0.78	0.60	0.65	0.20	0.65

Panel C: No Restrictions

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
$DebtRelief \times Post$	-5,034.17***	-1,359.79***	-0.003	-0.16***	0.004**	0.01**
	(678.36)	(665.04)	(0.01)	(0.04)	(0.002)	(0.005)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	372,844	372,844	372,844	372,844	372,844	30,044
R^2	0.78	0.76	0.58	0.68	0.20	0.62

Table 9: Heterogeneity by Debt Relief Amount

This table reports results from triple interaction regressions of consumer debt, delinquency, mobility and income outcomes based on borrower-month panel data that examines heterogeneity of the effect by debt relief amount. The dependent variables are total debt balance excluding student loans in Column (1); balance of mortgage accounts in Column (2); credit utilization in Column (3); number of all delinquent accounts excluding student loans in Column (4); mobility based on moving to a different ZIP code in Column (5); and income growth in Column (6). $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 after the debt relief and 0 before the debt relief. $Above$ ($Below$) is a dummy variable that takes a value of one if the debt relief amount is above (below) median level. On the right hand side, we control for individual and filing year × calendar year-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
$DebtRelief \times Post \times Above$	-5,998.42***	-2,508.78***	-0.04***	-0.05	0.004**	0.004**
	(751.22)	(689.31)	(0.01)	(0.05)	(0.002)	(0.001)
$DebtRelief \times Post \times Below$	-1,337.24*	-1,579.04**	-0.001	-0.05	0.0002	-0.001
	(707.97)	(702.11)	(0.01)	(0.05)	(0.002)	(0.01)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,283,639	1,283,639	1,283,639	1,283,639	1,283,639	91,230
R^2	0.76	0.74	0.57	0.63	0.13	0.59

Table 10: Wage Composition

This table reports results from difference-in-differences regressions of wage composition based on borrower-month panel data. The dependent variable includes the fraction of total income coming from variable pay defined as the sum of bonus, commissions and overtime pay. $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual and filing year × calendar year-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Dependent Var	Fraction of Variable Pay	Fraction of Variable Pay
	(1)	(2)
$DebtRelief \times Post$	0.031**	0.042**
	(0.015)	(0.018)
Sample	Entire Sample	No Job Change
Individual FE	Yes	Yes
Filing Year × YM FE	Yes	Yes
Observations	39,459	28,653
R^2	0.67	0.68

Table 11: Robustness: Exploiting Variation in Timing of Treatment

This table reports results from difference-in-differences regressions of consumer debt, delinquency, mobility and income outcomes based on borrower-month panel data for sub-sample of treated individuals that exploit variation in timing of treatment. The dependent variables are total debt balance excluding student loans in Column (1); balance of mortgage accounts in Column (2); credit utilization in Column (3); number of all delinquent accounts excluding student loans in Column (4); mobility based on moving to a different ZIP code in Column (5); and income growth in Column (6). $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual and filing year × month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
$DebtRelief \times Post$	-2,238.72***	-361.26*	-0.01**	-0.15***	0.004**	0.005***
	(613.290)	(188.200)	(0.005)	(0.020)	(0.002)	(0.001)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	438,117	438,117	438,117	438,117	438,117	38,443
R^2	0.84	0.86	0.72	0.78	0.46	0.46

Second Chance: Life without Student Debt

Appendix for Online Publication

Figure A1. Dynamics of the Total Debt Balances: Matched Sample

The figure plots the dynamic coefficients corresponding to relative monthly dummies estimated separately for the treated and control groups for the matched sample which allows us to assign event time for the control group. Relative monthly dummies are defined as the interval, in months, from the debt discharge date to credit report date for the treated borrowers while matched control borrowers are assigned relative monthly dummies based on discharge of their matched treated borrowers. Blue (Red) color represents the treated (control) group. Dependent variable is total debt balance. On the right hand side, we control for individual fixed effects and county × event-month fixed effects. Standard errors are clustered at ZIP Code level.

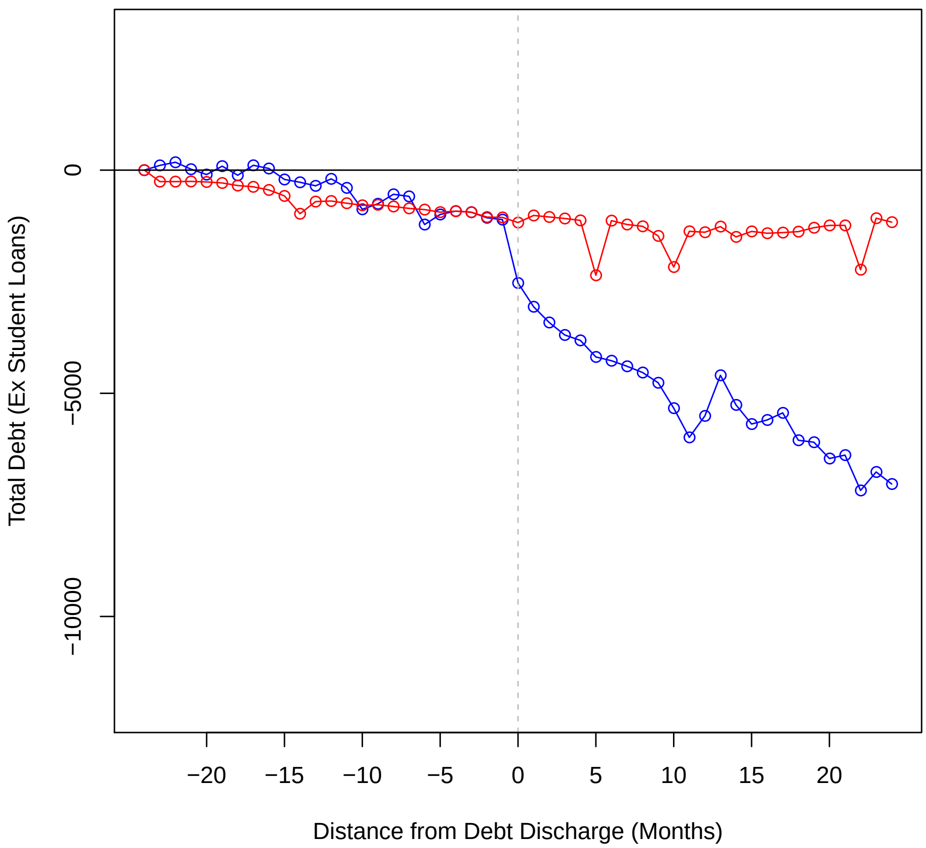


Figure A2. Dynamics of the credit utilization: Matched Sample

The figure plots the dynamic coefficients corresponding to relative monthly dummies estimated separately for the treated and control groups for the matched sample which allows us to assign event time for the control group. Relative monthly dummies are defined as the interval, in months, from the debt discharge date to credit report date for the treated borrowers while matched control borrowers are assigned relative monthly dummies based on discharge of their matched treated borrowers. Blue (Red) color represents the treated (control) group. Dependent variable is credit utilization, calculated as ratio of revolving balance to revolving credit limit. On the right hand side, we control for individual fixed effects and county × event-month fixed effects. Standard errors are clustered at ZIP Code level.

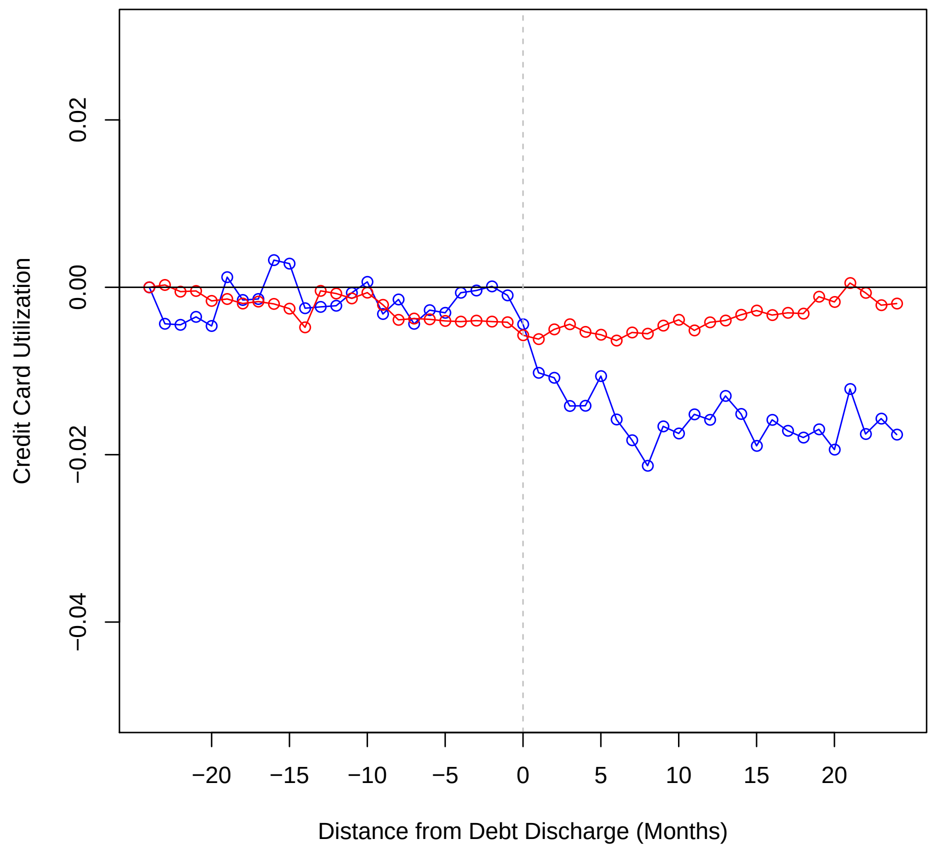


Figure A3. Dynamics of Delinquency Rate: Matched Sample

The figure plots the dynamic coefficients corresponding to relative monthly dummies estimated separately for the treated and control groups for the matched sample which allows us to assign event time for the control group. Relative monthly dummies are defined as the interval, in months, from the debt discharge date to credit report date for the treated borrowers while matched control borrowers are assigned relative monthly dummies based on discharge of their matched treated borrowers. Blue (Red) color represents the treated (control) group. Dependent variable is the indicator of borrower having any delinquent account. On the right hand side, we control for individual fixed effects and county × event-month fixed effects. Standard errors are clustered at ZIP Code level.

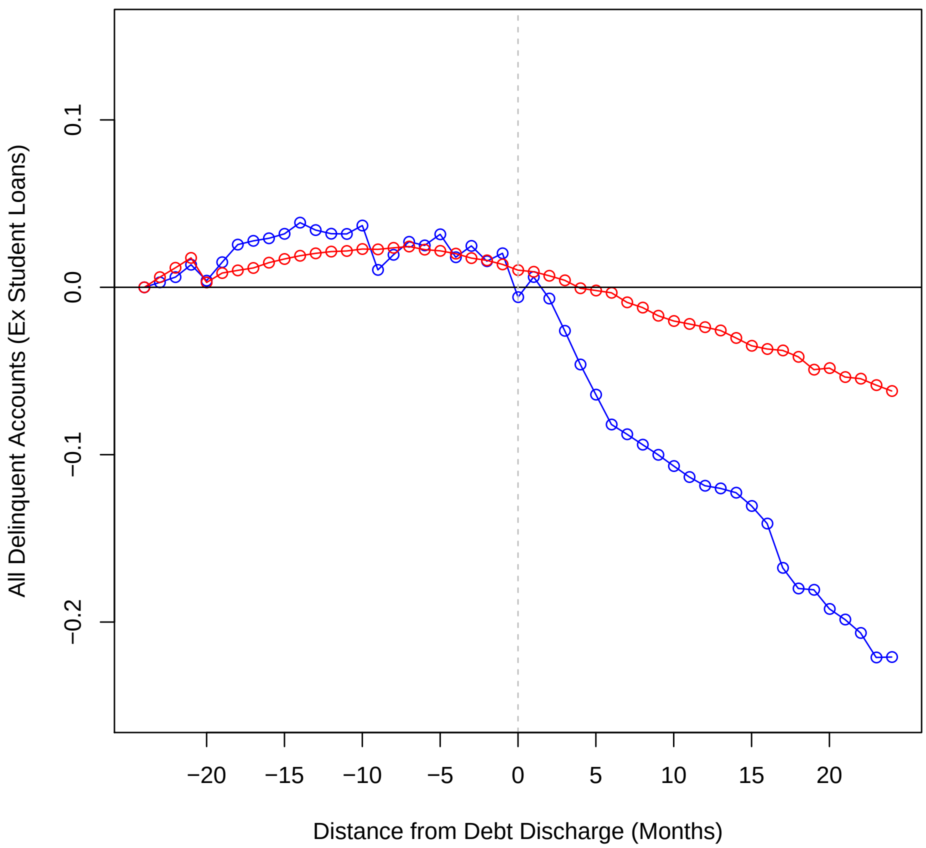


Figure A4. Dynamics of Income: Matched Sample

The figure plots the dynamic coefficients corresponding to relative monthly dummies estimated separately for the treated and control groups for the matched sample which allows us to assign event time for the control group. Relative monthly dummies are defined as the interval, in months, from the debt discharge date to credit report date for the treated borrowers while matched control borrowers are assigned relative monthly dummies based on discharge of their matched treated borrowers. Blue (Red) color represents the treated (control) group. Dependent variable is the indicator of borrower moving from one address to another month to month. On the right hand side, we control for individual fixed effects and county × event-month fixed effects. Standard errors are clustered at ZIP Code level.

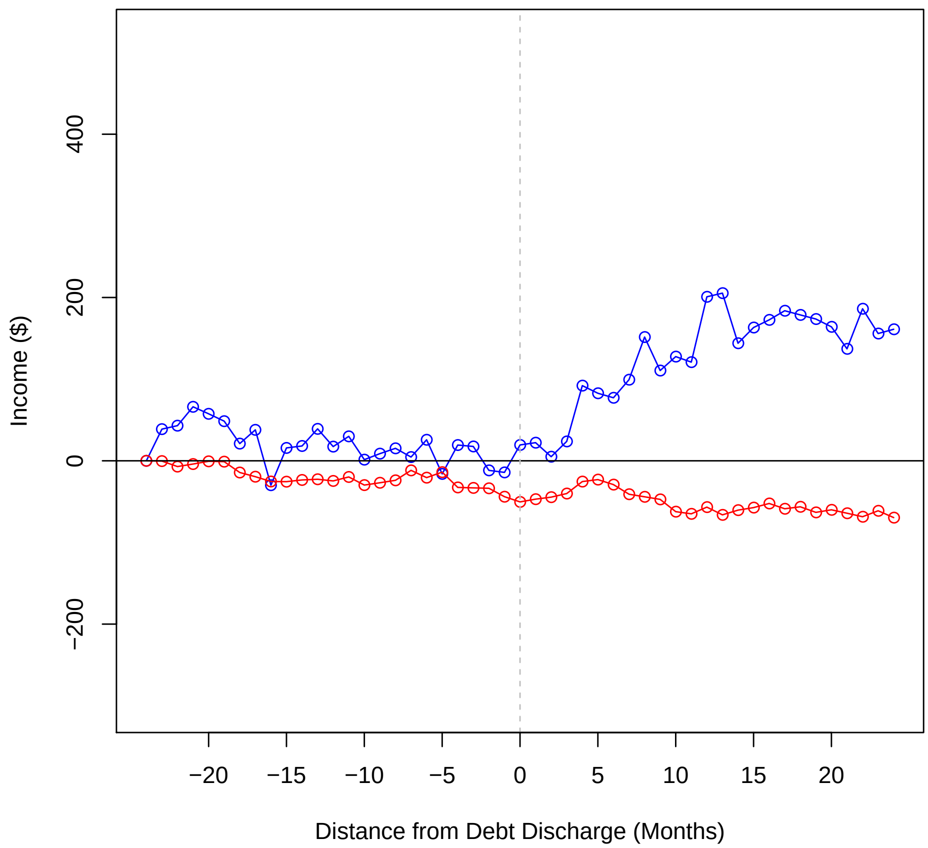


Figure A5. Sample Selection with Employment Data

The figure compares the distribution of individual borrowing and credit scores for individuals in the sample who are included in the employment database to those who are not. The first three rows plot borrowing behavior across different types of debt (i.e. total excluding student loans, credit card and home loans) while the fourth row plots credit score. The column on the left plots the empirical cumulative density function (CDF) for the number of accounts in different categories while that on the right plots the kernel density function for the amount of balances. The solid line represents individuals covered in the employment database while the dashed line represents those who are not.

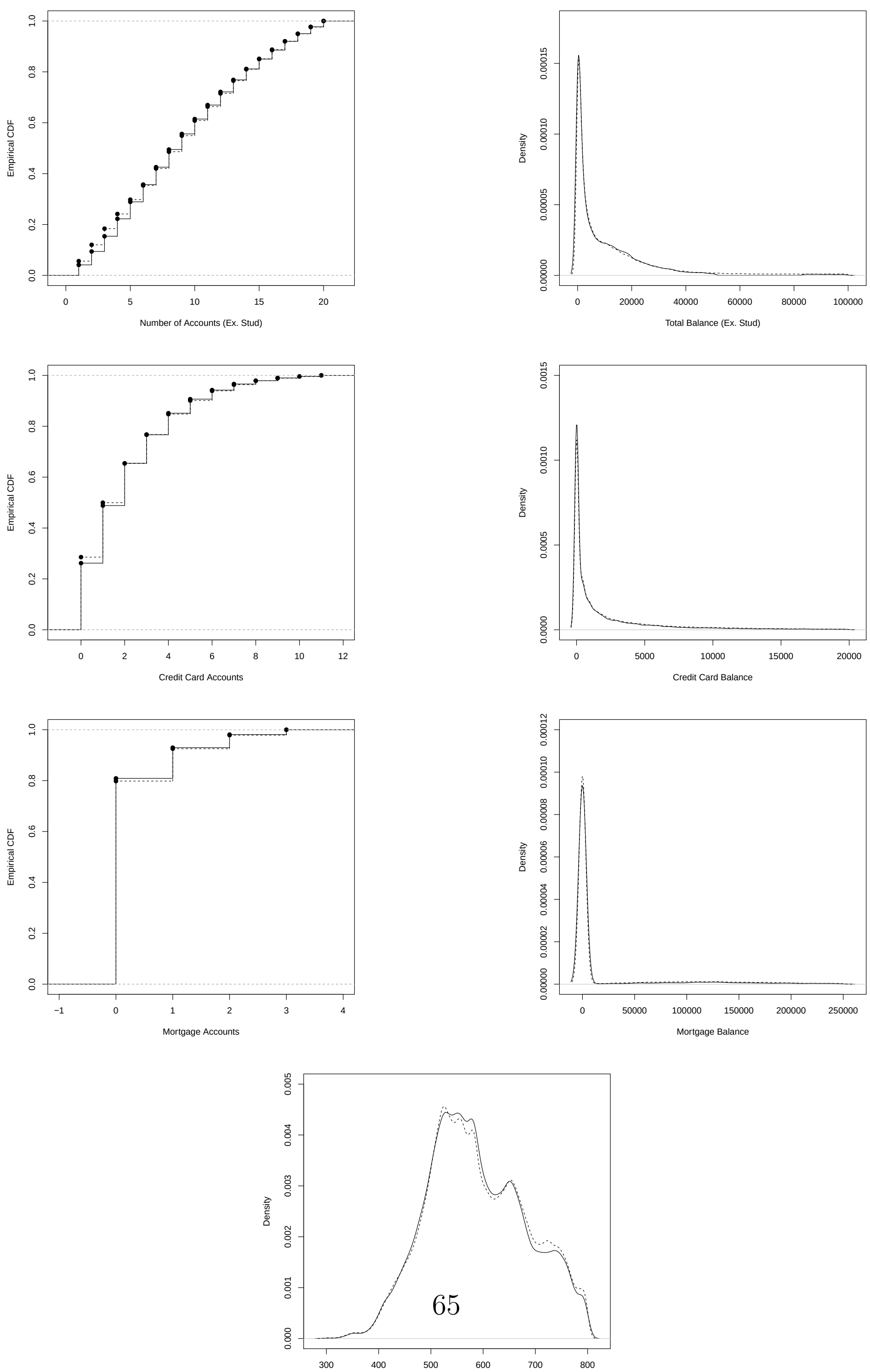


Figure A6. Number of Legal Filings in the Sample

The figure plots the number of legal filings by year for treated and control borrowers depicted by blue and orange colors respectively. Y axis is the number of filings and X axis is the filing year.

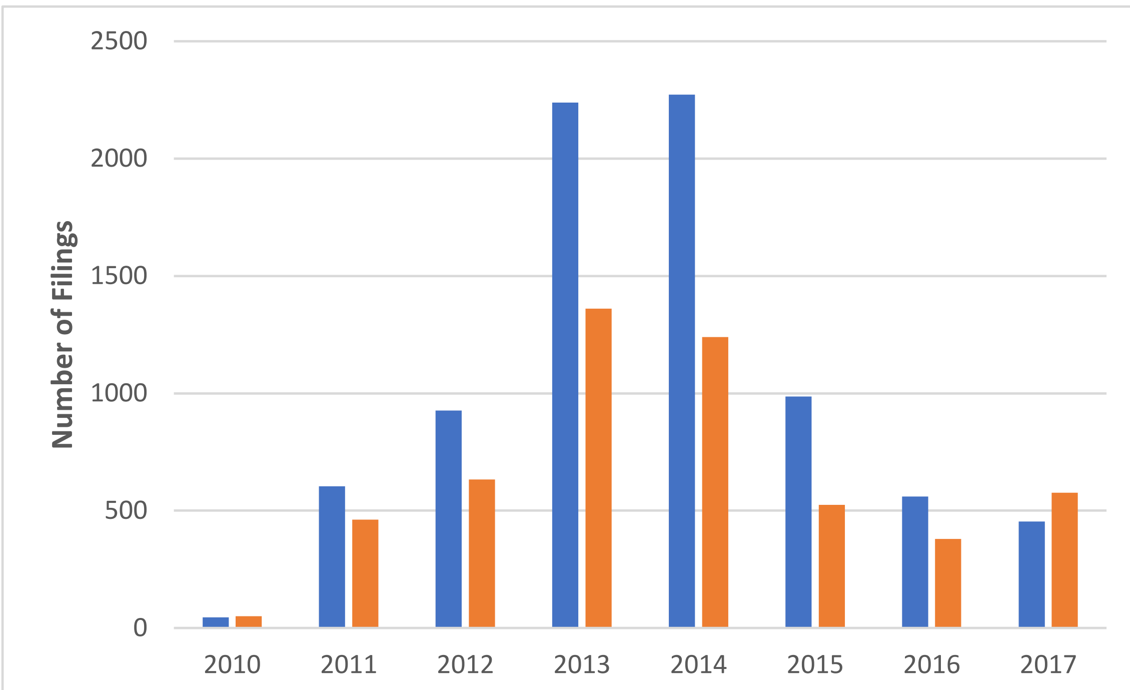


Table A1: Robustness: Clustering at Different Levels

This table reports results from difference-in-differences regressions of consumer debt, delinquency, mobility and income outcomes based on borrower-month panel data. The dependent variables are total debt balance excluding student loans in Column (1); balance of mortgage accounts in Column (2); credit utilization in Column (3); number of all delinquent accounts excluding student loans in Column (4); mobility based on moving to a different ZIP code in Column (5); and income growth in Column (6). $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual and filing year × calendar year-month fixed effects. Standard errors are clustered at different levels across panels. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A: Double Clustered at ZIP Code and Month Levels

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
$DebtRelief \times Post$	-4,600.88***	-1,564.60***	-0.023***	-0.15***	0.003***	0.01***
	(436.75)	(216.94)	(0.007)	(0.03)	(0.001)	(0.003)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,283,639	1,283,639	1,283,639	1,283,639	1,283,639	91,230
R^2	0.77	0.75	0.58	0.65	0.19	0.58

Panel B: Clustered at Individual Level

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
$DebtRelief \times Post$	-4,600.88***	-1,564.60***	-0.023***	-0.15***	0.003***	0.01***
	(463.94)	(159.04)	(0.005)	(0.026)	(0.001)	(0.003)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,283,639	1,283,639	1,283,639	1,283,639	1,283,639	91,230
R^2	0.77	0.75	0.58	0.65	0.19	0.58

Panel C: Double Clustered at Individual and Month Levels

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
$DebtRelief \times Post$	-4,600.88***	-1,564.60***	-0.023***	-0.15***	0.003***	0.01***
	(505.73)	(188.56)	(0.006)	(0.031)	(0.001)	(0.003)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,283,639	1,283,639	1,283,639	1,283,639	1,283,639	91,230
R^2	0.77	0.75	0.58	0.65	0.19	0.58

Table A2: Robustness: Controlling for Credit Score Changes

This table reports results from difference-in-differences regressions of consumer debt, delinquency, mobility and income outcomes based on borrower-month panel data controlling for credit score changes. The dependent variables are total debt balance excluding student loans in Column (1); balance of mortgage accounts in Column (2); credit utilization in Column (3); number of all delinquent accounts excluding student loans in Column (4); mobility based on moving to a different ZIP code in Column (5); and income growth in Column (6). $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual, filing year × calendar year-month fixed effects and credit score decile × month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
$DebtRelief \times Post$	-5,759.49***	-1,624.77***	-0.01**	-0.17***	0.003***	0.005*
	(388.31)	(383.34)	(0.005)	(0.020)	(0.001)	(0.002)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score Decile × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,283,639	1,283,639	1,283,639	1,283,639	1,283,639	91,230
R^2	0.77	0.75	0.60	0.67	0.19	0.59

Table A3: Robustness: Matched Sample

This table reports results from difference-in-differences regressions of consumer debt, delinquency, mobility and income outcomes based on borrower-month matched sample. The dependent variables are total debt balance excluding student loans in Column (1); balance of mortgage accounts in Column (2); credit utilization in Column (3); number of all delinquent accounts excluding student loans in Column (4); mobility based on moving to a different ZIP code in Column (5); and income growth in Column (6). *DebtRelief* is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. *Post* is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual fixed effects and county × event-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
DebtRelief × *Post*	-4,303.21***	-888.24***	-0.018***	-0.11***	0.003***	0.01**
	(652.21)	(163.55)	(0.004)	(0.020)	(0.001)	(0.004)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
County × Event YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,010,381	6,010,381	6,010,381	6,010,381	6,010,381	164,957
R^2	0.80	0.86	0.624	0.89	0.36	0.59

Table A4: Robustness: Balanced Panel with Matched Sample

This table reports results from difference-in-differences regressions of consumer debt, delinquency, mobility and income outcomes based on borrower-month matched sample for a balanced panel with one year around treatment. The dependent variables are total debt balance excluding student loans in Column (1); balance of mortgage accounts in Column (2); credit utilization in Column (3); number of all delinquent accounts excluding student loans in Column (4); mobility based on moving to a different ZIP code in Column (5); and income growth in Column (6). $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 for 12 months after the debt relief and 0 for 12 months before the debt relief. On the right hand side, we control for individual fixed effects and county $\times$ event-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
$DebtRelief \times Post$	-2,711.16***	-549.53***	-0.01***	-0.11***	0.005***	0.004
	(465.790)	(134.320)	(0.004)	(0.020)	(0.001)	(0.01)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
County $\times$ Event YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,066,903	2,066,903	2,066,903	2,066,903	2,066,903	164,957
R^2	0.89	0.9	0.78	0.89	0.36	0.59

Table A5: Robustness: Including Calendar Year-Month with Matched Sample

This table reports results from difference-in-differences regressions of consumer debt, delinquency, mobility and income outcomes based on borrower-month matched sample. The dependent variables are total debt balance excluding student loans in Column (1); balance of mortgage accounts in Column (2); credit utilization in Column (3); number of all delinquent accounts excluding student loans in Column (4); mobility based on moving to a different ZIP code in Column (5); and income growth in Column (6). $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual fixed effects, calendar month and county $\times$ event-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
$DebtRelief \times Post$	-4,962.88***	-898.06***	-0.02***	-0.11***	0.004***	0.01
	(651.970)	(163.510)	(0.004)	(0.020)	(0.001)	(0.01)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar YM FE	Yes	Yes	Yes	Yes	Yes	Yes
County $\times$ Event YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,010,381	6,010,381	6,010,381	6,010,381	6,010,381	445,114
R^2	0.16	0.8	0.63	0.74	0.31	0.77

Table A6: The Likelihood of Being Covered in the Employment Data

This table reports results from difference-in-differences regressions of selection based outcomes for being covered in the employment dataset. The dependent variables are the likelihood of being included in the employment dataset in Column (1); leaving the dataset in Column (2); and entering the employment dataset in Column (3). $DebtRelief$ is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. $Post$ is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual fixed effects, calendar month and county $\times$ event-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Dependent Var	Included in the Employment Data	Entering the Data	Leaving the Data
	(1)	(2)	(3)
$DebtRelief \times Post$	0.001	0.0001	-0.0001
	(0.003)	(0.0003)	(0.0003)
Individual FE	Yes	Yes	Yes
Filing Year $\times$ YM FE	Yes	Yes	Yes
Observations	1,283,639	1,283,639	1,283,639
R^2	0.45	0.03	0.03

Table A7: Heterogeneity by Treatment Timing

This table reports results from difference-in-differences regressions of consumer debt, delinquency, mobility and income outcomes based on borrower-month panel data for sub-samples where the treatment occurred during early versus later stages. Panel A (B) represents treatment before (after) January 2015. The dependent variables are total debt balance excluding student loans in Column (1); balance of mortgage accounts in Column (2); credit utilization in Column (3); number of all delinquent accounts excluding student loans in Column (4); mobility based on moving to a different ZIP code in Column (5); and income growth in Column (6). *DebtRelief* is defined as 1 for the delinquent student loans who received debt relief and 0 otherwise. *Post* is defined as 1 after the debt relief and 0 before the debt relief. On the right hand side, we control for individual and filing year × calendar year-month fixed effects. Standard errors are clustered at ZIP Code level. Asterisks denote significance levels (***=1%, **=5%, *=10%).

Panel A: Treatment Before 2015

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
DebtRelief × *Post*	-5,272.51***	-2,326.46***	-0.017**	-0.29***	0.002	-0.006*
	(694.19)	(700.04)	(0.007)	(0.04)	(0.002)	(0.003)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	621,890	621,890	621,890	621,890	621,890	48,142
R^2	0.77	0.75	0.59	0.68	0.21	0.59

Panel B: Treatment 2015 onwards

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
DebtRelief × *Post*	-5,110.63***	-1,751.84***	-0.011*	-0.447***	0.005***	0.007*
	(463.99)	(444.04)	(0.006)	(0.03)	(0.001)	(0.004)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Filing Year × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	940,222	940,222	940,222	940,222	940,222	72,754
R^2	0.77	0.75	0.58	0.66	0.19	0.58

Table A8: Goodman-Bacon Decomposition

This table reports the Goodman-Bacon decomposition for our main difference-in-differences results. The dependent variables are total debt balance excluding student loans in Column (1); balance of mortgage accounts in Column (2); credit utilization in Column (3); number of all delinquent accounts excluding student loans in Column (4); mobility based on moving to a different ZIP code in Column (5); and income growth in Column (6). Weights associated with different components of the decomposition are reported in parantheses.

Dependent Var	All Balance (Ex. Stud)	Mortgage Balance	Credit Card Utilization	All DLQ (Ex. Stud)	Mobility	Change in Income (%)
	(1)	(2)	(3)	(4)	(5)	(6)
Earlier vs Later Treated	-4,834.05	-1,351.37	-0.024	0.047	0.005	0.015
	(0.224)	(0.224)	(0.224)	(0.224)	(0.224)	(0.187)
Later vs Earlier Treated	-5,468.99	-1,509.85	-0.022	0.043	0.003	0.009
	(0.224)	(0.224)	(0.224)	(0.224)	(0.224)	(0.187)
Treated vs Untreated	-7,549.30	-2,889.82	-0.012	-0.312	0.002	0.018
	(0.552)	(0.552)	(0.552)	(0.552)	(0.552)	(0.626)