Labor, Loans and Leisure: The Impact of the Student Loan Payment Pause*

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Abstract

Beginning in March 2020 and ultimately continuing to September 2023, most student loan borrowers had their required payments on federal student loans paused. For student loan borrowers with limited access to credit, the payment pause provided additional cash-on-hand that may have allowed them to reduce their work hours. Using survey data capturing individual finances, monthly work characteristics and educational attainment, we find that suspended student debt payments reduced average weekly hours worked by 1.34 (-4%) over a 10-month period with declines concentrated among workers who had not completed a college degree. For borrowers who had completed a college degree or graduate degree, there is no evidence that the payment pause changed employment or hours worked. These findings are consistent with consumer finance data showing that borrower households without a college degree are approximately twice as likely to report liquidity constraints relative to more educated households with federal student debt.

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1 Introduction

From March 2020 to September 2023, most student loan borrowers had their required payments on federal student loans paused with zero interest accrual. Nearly 90% of federal student loans, approximately $1.34 trillion in debt, were eligible for the benefit. The practical effect of the pause was an increase in borrowers' monthly cash-on-hand by an average of $280 per month (Cooper and Haddix, 2025), along with an improvement in credit access in some cases. How this shift impacted individual labor supply decisions is the subject of this analysis.

As total student loan balances and the share of adults with student debt have grown in the last two decades, there has been increased attention to whether repayment obligations impact outcomes outside of the channel of educational attainment such as home ownership and family formation (Bleemer et al., 2021; Gicheva, 2016; Mezza et al., 2020). For some borrowers, student debt payments and limited access to credit may reduce the capacity for borrowers to smooth consumption across periods. As a result, borrowers may be induced to work additional hours or take on additional jobs in order to meet household expenditure obligations and student loan payments. The pause in required payments provides an opportunity to assess the magnitude of this potential effect and whether the impact differs with borrower characteristics such as level of education.

To estimate the labor supply response due to the pause in student loan payments, we use a matched difference-in-differences research design, together with detailed, individual-level data on finances and work characteristics from the Survey of Income and Program Participation (SIPP). Using the monthly observation of hours worked along with other detailed employment and job characteristics in the SIPP, we estimate that suspended student debt payments led to a decline of 1.34 weekly average hours worked (-4%) overall. These declines are driven by borrowers without a bachelor's degree ("some college"), for whom the estimated decline is 3.3 weekly hours worked (-11%). Both a decline in borrowers employed at the extensive margin and declines in hours worked at the intensive margin explain this decline. Overtime work (greater than 40 hours) and moonlighting (working multiple jobs) fall markedly for borrowers impacted by the pause. Labor supply outcomes for borrowers with BA and graduate degrees are largely unaffected by the policy.

Canonical models of consumption smoothing and the permanent income hypothesis would predict no labor supply responses from pause-induced increases in liquidity. In this context, our findings are consistent with the presence of binding liquidity constraints for some borrowers without a college degree. Utilizing data from the Survey of Consumer Finances (SCF), we show corroborating evidence for this hypothesis; among households with federal student debt, those with less than a bachelor's degree are approximately twice as likely to report liquidity constraints relative to more educated households.

For student loan borrowers facing difficulty balancing high payments relative to income levels, income-driven repayment (IDR) repayment options might be expected to provide relief. In practice, administrative burdens may have limited access to these programs in the pre-pandemic period. The results in this paper point to the presence of frictions that may create deviations from consumption smoothing for student loan borrowers which may be related to difficulties in accessing alternative repayment programs.[1] Related survey evidence highlights this issue, with over half of lower income households with student debt reporting being "unaware they could change plans or needing help selecting an alternative repayment plan" (Caldwell et al., 2024).

We begin by documenting the institutional details of the payment pause, reviewing other analytic papers examining the impact of the payment pause on consumption and savings, and outlining evidence and theory informing predictions about how the payment pause might impact labor supply. We then measure the impact of the initial pause and its August 2020 extension on labor market outcomes. Finally, we consider how these estimates align with the potential liquidity constraints faced by individual borrowers and the policy options, such as income-driven repayment, intended to assist struggling borrowers.

2 Background

2.1 The Student Loan Payment Pause

From March 2020 to September 2023, most federal student loan borrowers were not required to make monthly payments and did not have interest accrual on their balances. President Trump used executive authority to introduce the student loan payment pause (also known as forbearance or debt moratorium), which coincided with the COVID-19 national emergency declaration on March 13, 2020. Then, on March 27, under the Coronavirus Aid, Relief, and Economic Security (CARES) Act, the Department of Education suspended payments until September 20, 2020. The Act also stopped collections on eligible defaulted loans.

When the CARES Act provisions were not renewed in summer 2020, President Trump used executive action to direct Secretary of Education Betsy DeVos to continue the payment pause through December 31, 2020, with a subsequent extension to January 31, 2021. The

[1] Cornaggia and Xia (2024) use data on a representative sample of students who begin postsecondary education to describe and identify borrowers that select suboptimal repayment plans. Evidence in their analysis suggests that, conditional on other characteristics, students who complete their degrees are more likely to switch to favorable repayment plans. This is consistent with our findings that point to frictions for borrowers in the "some college" group.

policy environment became more complex in later periods. Under the Biden administration, additional interventions were introduced using authority from the Higher Education Relief Opportunities for Students (HEROES) Act of 2003, which amended the Higher Education Act of 1965 to provide executive authority for "waivers or relief" to federal aid recipients during national emergencies. By August 2022, the payment pause extension coincided with other significant policy changes, including a broad-based student loan cancellation plan that was ultimately struck down by the Supreme Court.

Our analysis focuses on the first 10-month period of the pause (March-December 2020) which offers some advantages. First, it isolates the labor supply effects of the payment pause from confounding policies that were introduced later, such as the Public Service Loan Forgiveness Waiver (October 2021 - October 2022), the broad-based forgiveness plan (announced August 2022), the Fresh Start program (December 2022 - September 2024), and expanded eligibility for defaulted borrowers (March 2021). Second, this window plausibly captures borrowers' immediate responses to an unexpected policy change, before they could adjust their behavior in anticipation of additional relief measures. In addition, our analysis period allows us to maximize the sample size in the available panel data. During our sample period, borrowers experienced only two extensions: the August 8 announcement (extending relief through December 31, 2020) and the December 4 announcement (extending relief through January 31, 2021).

Most student borrowers were eligible for relief provided by the federal pause. Ineligible debt primarily constituted of loans that were initiated prior to 2010 and were part of the legacy Federal Family Education Loan (FFEL) Program under which loans were originated (and often held) by private entities. Some of these outstanding FFEL loans had already been transferred to federal ownership either through buyback provisions or because individuals had defaulted; these loans became eligible for the payment pause in March 2021 (Congressional Research Service, 2023). Second, student loans originated by private firms (approximately 7% of all student loan balances) were not eligible for the pause. Our data allow us to identify individuals with student loan debt, but we cannot directly observe which borrowers were affected by the payment pause. We impose age restrictions on our sample to isolate eligible borrowers, although we likely still tag a relatively small proportion of ineligible borrowers as treated. We believe this likely attenuates our estimates toward zero, and provide further details in Section 3.

For those with loans eligible for forbearance, there are two major mechanical impacts. First, individuals experienced an increase in liquidity – the equivalent of an interest-free loan. The size of this effect varies with the level of expected payments prior to March 2020. Those with low payments or those who had already signed up for income-based repayment programs benefited far less than those with large conventional payments. Second, with the forbearance period, the Department of Education stopped reporting delinquency status to credit bureaus, resulting in increases in credit scores. The increases in scores were concentrated among those borrowers with poor credit histories, with increases particularly large among borrowers with a prior delinquency. Chava et al. (2023) find a nearly 70-point increase in credit scores among distressed federal student loan borrowers during the first year of the payment pause. Beyond these effects, there is a nominal wealth effect equal to the market return on saving the added liquidity, these effects were likely close to inconsequential in 2020 (our period of study) as the annual return on a money market account fell below 1% in late 2020.[2]

2.2 Analysis of the Effects of the Payment Pause

Analysis of the impacts of the student loan payment pause commenced years before the policy concluded. A common set of initial questions concerned how much the policy would cost, who would benefit, and how the policy would impact macroeconomic variables, including consumption and inflation. Using data from the Department of Education, a Government Accountability Office (GAO) analysis found that costs associated with the emergency relief between March 13, 2020 and April 30, 2022 totaled $102 billion. Analysts in the private sector reached broadly similar conclusions. In August 2022, the Committee for a Responsible Federal Budget (CRFB) estimated the total cost of the pause through the end of 2022 to be $155 billion. Broadly, these analyses paint a picture of a cost of about $5 billion per month. Adding to the direct cost identified are the inflationary implications of the pause, with the CRFB estimating an effect of about 20 basis points per year.

Using data from the Survey of Consumer Finances, Briones et al. (2023a) show that the pause provided the greatest monetary benefits to those in the highest income groups. More than 65 percent of the benefit flowed to families with incomes greater than $75,000. Broadly, the incidence of borrowing is concentrated in the middle of the distribution (about 71% in the middle 60% of the household income distribution), even as payments and balances are concentrated in the top part of the income distribution. By education level, borrowers with graduate degrees tended to benefit disproportionately, accounting for about 29% of borrowers but 55% of balances. At the other end of the education distribution, those with some college but no BA degree accounted for about 30% of borrowers and 15% of balances.

Our study is most closely related to several papers investigating behavioral responses to the pause using rich credit bureau data, which are particularly suited to the examination of liabilities and utilization of other credit instruments. A strength of the credit bureau data

[2]By September 2023, when the payment pause came to a close, such effects were meaningful as the money market return on assets grew to 5%.

is the timely availability of large samples with very detailed records on different types of transactions, even as these data lack detailed data on education, race, and labor market outcomes that are typically available in surveys. In general, these papers focus their research designs on the comparisons of student loan borrowers who would appear to have been eligible for the pause to those with FFEL or private loans who would not be eligible.[3] Dinerstein et al. (2024) use a national 10% sample of credit records and find substantial impacts of the pause on utilization of other types of credit, including home mortgages, auto loans, and credit cards. This research posits that much of this consumption (and investment) impact operates through the channel of increased liquidity, as effects are concentrated among those without a prior delinquency on their student loans. The analysis by Chava et al. (2023) focuses on the subpopulation with a prior student loan delinquency, who are the students with the largest increase in credit access. Chava et al. (2023) find increases of 12.3% in credit card borrowing and 4.3% in auto loans, which are also accompanied by meaningful increases in delinquencies on these debt instruments. Using a data set that combines credit and bank account transactions, Lourie et al. (2023) also find increased credit card utilization and add evidence of increased transfers to investment accounts and reductions in overdrafts. Using of employer direct deposits as an indicator of labor market activity, Lourie et al. (2023) find a modest negative link between forbearance eligibility and employer payments, which is consistent with a reduction in labor supply.

2.3 Expected Effects of the Pause on Labor Supply

Even as questions of the behavioral impacts of student loan finance have received increased attention in recent years, analysis of the impact of loan payments on labor supply has been limited. Our study addresses this gap, focusing on the labor market effects of existing debt rather than how the need for student loans influences educational choices and post-collegiate outcomes – an important but separate question explored elsewhere.[4] With several notable

[3]There are several challenges for the use of FFEL borrowers as a control group. First, by construction, nearly all of the borrowers with outstanding FFEL loans in 2020 faced some type of delay or forbearance that produced an extension of payment beyond the conventional 10-year horizon, as all FFEL loans were initiated before 2010. Add to this, the stock of borrowers with FFEL loans outstanding (nearly all in their late 30s or older) is not representative of the contemporary pool of those holding student debt, thus limiting external validity. Finally, the payment pause likely caused many of those with FFEL loans to consolidate to gain eligibility for the payment pause and other relief measures. Between the first quarter of 2020 and the fourth quarter of 2023, privately-held FFEL loans declined from 6.2 million loans to 3.5 million loans ($169 million to $105 million). Over this interval, privately-held FFEL loans fell from 11% to about 7% of the federal student loan portfolio.

[4]For example, Hampole (2022) presents convincing evidence that the elimination of loans at a set of highly selective colleges and universities impacts within-college choices, leading students to shift from majors with high initial earnings (but somewhat flatter trajectories) to those with higher lifetime earnings. This evidence is also consistent with Akers (2012) who finds evidence of decreased graduate school attendance

exceptions, the available evidence on liquidity changes and labor supply is not specifically tied to student lending. At the same time, our question concerning the labor supply effects of changes in liquidity for workers with different levels of education has implications for more general analyses of liquidity changes over the life cycle.

Parallels to the student loan payment pause appear in other circumstances where individuals receive (plausibly) unexpected increases in liquidity and, to a lesser extent, debt relief. Modifications to home mortgages for distressed borrowers that occurred as part of the federal government's Home Affordable Modification Program included different combinations of reductions in principal owed (a "wealth" effect) and changes in short-term payment requirements (a "liquidity" effect). Ganong and Noel (2020) find that the latter has a much larger impact on default and consumption outcomes. For a group of borrowers in default who benefit from an unanticipated student loan discharge, DiMaggio et al. (2020) find that there is a plausible "debt overhang," with the reduction in liability leading to increased access to credit along with an increased probability of job change and growth in income.

In the context of canonical models of consumption smoothing and the permanent income hypothesis, liquidity from the payment pause would only be expected to have an impact on labor market outcomes of those borrowers facing credit constraints which inhibit consumption smoothing. For these borrowers, increased liquidity would be expected to have a negative impact on hours worked under conditions in which leisure is a normal good.[5] Empirically, such effects should be distinguished by a borrower's level of constraint prior to the payment pause: borrowers who were net savers of financial assets in advance of the pause and those without credit card debt are included in the pool of borrowers who are not likely to be constrained (and for whom there would be little or no labor supply effects).

Expectations about the duration of the pause may have impacted any labor supply response. Although relief was eventually extended over three years, borrowers may have been relatively uncertain of a long benefit period during its initial implementation in March 2020 and first extension in August 2020. Under these more pessimistic expectations, borrowers might have been less likely to adjust their labor supply, anticipating a quick resumption of payment obligations that would not allow sufficient time for such adjustments (e.g., job search). While the potential cancellation of student loan obligations was an issue raised in the 2020 presidential election, no executive action was taken until August of 2022 when the Biden administration announced a plan which would have cancelled up to $20,000 in federal balances for qualifying borrowers; the Supreme Court struck down the plan in June 2023

among those with undergraduate student debt.

[5]Outside of the student loan context, Kumar and Liang (2023) find that credit-constrained individuals with below-permanent income reduce labor supply when given increased liquidity access.

before it was implemented. With increased attention to student loans leading up to the 2020 presidential election, borrowers may have reasonably anticipated that the payment pause would continue at least to the end of 2020 even as loan forgiveness remained uncertain during this period.[6]

3 Data

The Survey of Income and Program Participation (SIPP), a nationally representative longitudinal survey administered by the U.S. Census Bureau, provides our analytic sample. For our purposes, the SIPP's main strengths include a larger sample of student loan borrowers than in other surveys, a rich set of individual characteristics, and labor market information measured at a monthly frequency. Together, these features enable us to proxy eligibility for the pause and learn about the dynamics of labor supply over many months for borrowers.

The SIPP interviews survey respondents annually for up to four years about prior-year activities. Each year, the survey introduces a new set of respondents, with these cases forming wave 1 of a sample panel. For our analytic work, we focus on the set of respondents who are observed consecutively from January 2019 to December 2020. These are individuals recorded in the third and fourth waves of the 2018 SIPP panel, as well as the first and second waves of the 2020 panel. The two-year panel focus reduces our overall sample size but allows us to improve our research design by observing a longer history of pre-pause labor market trajectories.[7]

Analysis of most labor market outcomes is at the individual-month level. The SIPP reports up to six jobs within the reference year, and we construct labor supply measures based on the individual's primary and, if applicable, secondary job characteristics. We classify primary work as the job with the highest earnings in that month. Our measure of working hours is the average number of hours worked per week at the job during the reference month. Naturally, the data on earnings and hours include zeroes generated from individuals who did not work. As we are interested in capturing both extensive and intensive margins of borrower responses, we use levels of hours and earnings in our main analysis, following recommendations from Chen and Roth (2024).

A key financial characteristic we observe is the individual's student loan balance, which

[6] Average borrower expectations of expanded student loan forgiveness (in the next 12 months) increased from 21% to 32% between December 2019 and April 2020, and further to 45% by December 2020 after the November election. Still, the combined probability of no change or reduction in forgiveness remained higher throughout 2019-2022 (see Figure A1).

[7] COVID-19 complications reduced SIPP response rates, resulting in a smaller two-year panel. To mitigate potential nonresponse bias, we use SIPP survey weights, which have been shown to be effective in reducing bias for key estimates and demographic groups (U.S. Census Bureau, 2023).

we use to proxy for payment pause eligibility. This variable, along with all debt measures in the SIPP, is measured as of the last day in the reference year (e.g., January 31, 2019). We also observe information on an individual's household resources beyond labor income such as liquid assets (checking account balances, value of stocks, etc.) and spousal income which allow us to construct a more complete approximation of an individual's income and liabilities. A limitation of the SIPP is that we cannot observe student loan payment nor origination details; thus, eligibility is not observed directly. In practice, nearly 90% of federal loan balances were eligible for relief and so any borrowers we incorrectly identify as treated are likely a very small share of all student loan holders.[8]

Throughout our analysis, we limit attention to college-educated respondents who were between the ages of 25 and 45 in 2019 because younger borrowers are less likely to be making payments and to limit chances that we tag ineligible FFEL borrowers, who are older, as treated.[9] This sample restriction is distinguished from strategies comparing within a relatively older group of borrowers that either hold FFEL or Direct loans such as in Dinerstein et al. (2024) and Chava et al. (2023). Because we observe a richer set of covariates tied to demographics and education than are available in credit bureau data, we pursue a conditional on observables definition of control cases.

Summary statistics for borrowers and non-borrowers are presented in columns (1) and (2) of Table 1. Baseline differences between college-educated individuals with and without student debt, in part, reflect the likelihoods of certain demographic groups to borrow and their repayment trajectories. Women and Black Americans are more likely to rely on student debt to finance postsecondary investments and tend to have slower repayment rates (Addo and Zhang, 2022; Scott-Clayton and Li, 2016). That a higher share of borrowers have graduate degrees reflects the disproportionate share of debt held by this group (Meyer, 2022) and the mean student debt balance observed in our sample, $40,663, is comparable to the average balance of borrowers ages 25 to 49 as reported by Federal Student Aid, $37,217. Additional details on the SIPP, our sample construction, and our key variables of interest are provided

[8]Borrowers with only private and/or FFEL loans would be ineligible for the pause. Available evidence on private student loans suggests that the likelihood we are capturing student borrowers who only have private loans is small. See Briones et al. (2024) for a discussion on the SIPP and potential bias from the private student loan market. FFEL-only borrowers had the opportunity to consolidate into Direct loans throughout policy period and so could become eligible at any point throughout the sample period. In our setup, any borrowers who have payments across a mixture of eligible Direct loans and ineligible loans would still be correctly identified as treated.

[9]Many of the borrowers under 25 are likely still in school or in grace periods, and thus not making payments prior to the pause. Moreover, SIPP coverage for this group is weaker when compared to Federal Student Aid data. Tables from the Department of Education show that 82% of those with any federal student debt were younger than age 50, and 51% were younger than 35. While there are no borrowers under age 25 with FFEL debt, only 7.4% of borrowers between ages 25 and 34 and 27.6% of borrowers between ages 35 and 49 held FFEL loans in 2020.

in Section C.

4 Empirical Strategy

Our goal is to estimate the potential impact of additional liquidity generated from the pause on individuals' labor supply. The panel structure of our data and the discrete timing of the program motivate a difference-in-differences research design. We mitigate the concern that student loan borrowers may differ from those who are not holding student debt even in the absence of the payment pause by matching on a rich set of covariates, including demographic indicators, level of education and financial circumstances before the pandemic. Even as the period of the payment pause aligns with a dramatic overall change in hours and employment associated with the COVID-19 pandemic, we measure the effect of the payment pause as the difference in outcomes between student loan borrowers and the matched control group.

With monthly data producing a two-year (24-month panel), we place this design in the somewhat richer event-study specification. Our approach corresponds to the well-established matching difference-in-differences framework (Heckman et al. (1997) and Andersson et al. (2022)) and the more recent two-way fixed effects matching approaches, which use matching on observables to establish a control group.[10]

The event-study specification that follows is of the following form:

$$Y_{it} = \sum_{\tau \neq Dec2019} \beta_\tau (Borrower_i \times \mathbf{I}_{\tau t}) + \mu_{m(i)} + \delta_t + \alpha Borrower_i + \epsilon_{it}, \tag{1}$$

where Y_{it} is hours (earnings) for individual i in month t. $Borrower_i$ is an indicator equal to one for individuals that report having a positive student loan balance in December 2019. $\mu_{m(i)}$ and δ_t are matched pair and month-year fixed effects. The coefficients of interest β_τ correspond to the interaction between a set of month-year indicators, $\mathbf{I}_{\tau t}$, and our treatment group dummy for student loan borrowers with $\beta_{Dec2019}$ normalized to zero. We weight observations using two-year SIPP longitudinal weights of student loan borrowers, with matched non-borrowers weighted using the longitudinal weights of their corresponding matched borrower. As demonstrated in Table A1, results without weights are qualitatively similar. ϵ_{it} captures the error term.

We complement equation (1) with the estimation of a difference-in-differences framework that captures the cumulative "post" effect from March to December 2020, with the estimating specification as

[10] Recent examples include Adams-Prassl et al. (2024), Colmer et al. (2024), DeFusco et al. (2024), and Fenizia and Saggio (2024).

$$Y_{it} = \beta Borrower_i \times Post_t + \mu_{m(i)} + \delta_t + \alpha Borrower_i + \epsilon_{it}. \tag{2}$$

where $Post$ is an indicator equal to one when t is after February 2020.

The matching part of our specification requires an exact match on education (some college, BA only, graduate degree) and SIPP survey panel (third wave of 2018 panel or first wave of 2020 panel) to account for potential time-in-survey effects (Warren and Halpern-Manners, 2012). Within education-SIPP panel cells, we then do a probabilistic match based on a logit specification with marital status, sex, age, race, household income, and total debt as observable characteristics. One-to-one matching without replacement produces 1,381 matched pairs. Following recommendations from Abadie and Spiess (2022), we cluster our standard errors at the level of the matched pair. Our results are robust to alternative matching specifications. These additional results, robustness checks, and details on the matching procedure can be found in Section B.

Revisiting Table 1, column 3 shows mean difference estimates between borrowers and non-borrowers in December 2019. There are clear differences between college-educated borrowers and non-borrowers which reflect differences in the likelihood of student loan borrowing and repayment trajectories across demographic groups. Our matching procedure appears to perform well in identifying observationally similar non-borrowers, as shown in the relatively small and statistically insignificant differences in the matched panel (column 4). Note that we do not match on pre-pause values of our primary outcomes of interest, earnings from work and hours.

Our primary identification assumption is that, in the absence of the pause, labor outcomes for student borrowers and college-educated non-borrowers would have evolved in parallel. We argue that this parallel trends assumption is plausible when evaluating the effects of the liquidity shock using pairs of similar individuals matched within defined educational attainment bins. The intuition of our matching approach is to construct a set of counterfactual individuals with similar resources and debt levels. That is, our control group contains individuals who have similar levels of debt and household income as student borrowers, it is just that their debt obligations are not directly impacted by the pause. Although the parallel trends assumption is not directly testable, we do not find clear evidence for differential labor supply trends in 2019 leading up to the policy's implementation. Contemporaneous shocks that differentially affect the trajectories of these two groups would also be a violation of our identification assumption. In our setting, pandemic-induced economic shocks might confound treatment effects if the shock had a differential effect on individuals that have characteristics associated with student loan borrowing. For example, differences in 2019 household income and overall debt balance observed in the unmatched sample could lead us to understate/overstate labor adjustments if non-borrower individuals with relatively lower debt-to-income ratios were more/less affected by the pandemic than higher leveraged student debt holders. Our matching procedure reduces the potential for this issue, minimizing differences on these margins and a broad set of demographic characteristics. In Section 5, we further address these concerns and test for alternative explanations that could be driving our results.

5 Results

5.1 Main Results

Our two primary outcome measures are weekly hours and monthly earnings, which provide indications of changing quantity of work and take-home earnings. We present results using the matching framework in event-history plots (Figure 1), with difference-in-differences regression estimates providing a summary measure (Table 2). We present results for the combined levels of post-secondary education and the subgroups of no BA ("some college, no degree"), BA only, and graduate degree recipients. Figure 1 shows a vertical line in the period (February 2020) before the pause takes effect. Points below the baseline (0) indicate a negative effect for the borrowers relative to the non-borrowers.

Overall, the payment pause produced a modest, negative effect on hours worked per week, while the impact on monthly earnings is indistinguishable from zero. The hours impact is consistently negative for all months in the treatment period (Figure 1), with an average impact of -1.34 hours (a decline of about 4.1%), as measured in the difference-in-differences specification (Table 2).

When we focus on the differentiated education categories, it becomes evident that the behavioral adjustments are largely concentrated among those with "some college, no degree." For these borrowers, the average hours impact is -3.31 (11.4%), which is appreciably larger than that observed for the other education groups (Table 2). Corresponding to this negative impact is a negative earnings effect of about $-290.90 or a -10.4% impact relative to the control mean. We note, however, that our estimates on earnings are only statistically significant in our primary matching specification. In contrast, for those with BA degrees and graduate degrees, the effects on both hours worked and earnings are consistently indistinguishable from zero.

To interpret the observed behavioral changes as labor supply responses requires that the labor demand changes occurring concurrently with the payment pause did not differentially impact those with student loan debt. Such demand-side factors might include differential employment in sectors that had large employment changes (e.g., hospitality) or sectors such as health care in which workers were differentially at risk for becoming infected with COVID, forcing absence from work. We are able to use responses to a question about reasons not at work last week to test the plausibility of these alternative explanations. Figure 2 presents difference-in-differences estimates for the less than BA group, where various explanations for work absence (illness, layoff, etc.) serve as outcomes in separate linear probability specifications. If the decline in borrower work hours stems from a labor supply response, we should not observe differential reporting of involuntary work separations. Indeed, for all available options, including "on layoff" and "unable to find work," point estimates are near zero and statistically insignificant, with the exception of a small, weakly significant decline in reporting "injury" (0.837 percentage points, significant at the 10% level).

Further, reductions in hours do not appear to be driven by differential employment in industries most affected by service restrictions. We do not find statistically significant differences between borrowers and non-borrowers in December 2019 employment in accommodation and food services, or arts, entertainment, and recreation – sectors where 48 percent and 36 percent of establishments, respectively, experienced a government-mandated closure (U.S. Bureau of Labor Statistics, 2021).

A related concern is that macroeconomic shocks (e.g., COVID) might have unequal level effects on the outcomes. To address this, we follow recommendations from Chen and Roth (2024) and make an alternative identifying assumption that the percentage changes in the mean would have been the same for borrowers and matched non-borrowers in the absence of the pause. Table A2 and Figure A2 show estimates under this identifying assumption using Poisson QMLE. Proportional effects are very similar to our implied percentage effects using levels, and the event study plots do not suggest that the identifying assumption under levels or percentage changes dominates.

5.2 Robustness and Specification Tests

A necessary condition for causal interpretation is the parallel trends assumption. The very consistent point estimates near zero for all months in 2019 for each of the specifications (earnings, hours, and education subgroups) are consistent with this assumption. Still, there is a visibly evident negative effect for January and February of 2020 for the full sample in the hours specification and the "some college" subgroup for both hours and earnings.

At play is the long-established phenomenon of panel survey data known as a "seam effect," which reflects "too many" changes between interviews and "too few" changes within reference periods from the same interview. These issues in the SIPP (and other panel surveys) have been well-documented by researchers going back to at least Pischke (1995), along with studies of more recent SIPP panels (Bennett et al., 2022). Practically, the "seams" tie to the retrospective recording – answering questions about January 2019 in early 2020 while responses about January 2020 are recorded in early 2021. To the extent that respondents allow months to "blend together" (effectively telescoping responses), the January-February responses are anchored to subsequent monthly responses. Our results are robust to dropping these seam months.

Unsurprisingly, the January and February 2020 data are influential in standard sensitivity analysis following Rambachan and Roth (2023). For example, we find that estimates of $\beta_{March2020}$ and $\beta_{April2020}$ for the less than BA group have a "breakdown value" in $[0.5, 1)$. In other words, a significant result in these periods is robust to allowing for violations of parallel trends greater than half of the size of the maximum violation in 2019 through February 2020. If we drop January and February 2020, the range for these estimates becomes $[1, 1.5)$, thus allowing for violations greater than the observed deviation in 2019. While we cannot dismiss potential violations of parallel trends, these results make the assumptions to infer causality clear. We present the full set of these sensitivity estimates in Section B.4.

A different type of specification check is to consider an earlier (untreated) panel. Specifically, we match student borrowers and non-borrowers in 2017-18. Table 3 shows that impacts for the "placebo" interval are consistently indistinguishable from zero. Finally, we explore different matching specifications, along with a simple difference-in-differences without matching, presenting these specifications in the appendix (Section B.2 and Section B.3). The negative impact on average hours worked for all workers and the subgroup with less than a BA is persistently negative, with results broadly similar in magnitude across specifications (see Table B3).

6 Mechanisms of Adjustment

The robust estimates of a decline in labor supply for borrowers concentrated in the some college category lead to questions about how these borrowers adjust their work effort. We are particularly interested in understanding whether those adjusting their behavior move out of the labor force (extensive margin) or if they reduce hours, potentially from levels above the standard 40-hour work week.

Our analysis of the different margins of adjustments does not show a unicausal response, but rather adjustments at multiple margins. Table 4 shows the matched difference-in-differences results for outcomes at the margin of participation, hours conditional on working and other measures of intensity of work. First, there is a decrease in workers with non-zero hours, with this impact concentrated in the some college category. This decline is 5 percentage points relative to a pre-pause mean of 80% or a relative decline of about 6.25%. On the intensive margin, there is a relative decline of 1.85 hours for the treated group, about 4.6%.[11]

While there is little evidence of workers shifting across the broad boundary of full- or part-time work, the borrowers in the some college group show a shift in behavior among those who work multiple jobs ("moonlighting") and those with overtime hours, based on employment characteristics pre-pandemic. Among these "high intensity" workers, "moonlighting" drops by 5.3 percentage points, and overtime work drops by 6.2 percentage points; these are large relative changes in light of pre-pause means of 11% and 22%.

A fundamental economic question concerns the potential presence of liquidity constraints, which may limit the capacity of workers to access credit markets in order to smooth consumption. To test this, we turn to the 2019 Survey of Consumer Finances, which allows us to identify households with federal student loans and leverage a long-standing question asking households, "Has a particular lender or creditor turned down any request made for credit, or not given you as much credit as you applied for?" Following Calem and Mester (1995), we classify households being turned down as "credit-constrained."Among families with student debt, 33% of those where the highest educational attainment of either the household head or spouse was below a bachelor's degree (subbaccalaureate) were credit constrained prior to the pause (Figure A4). This rate was substantially higher than among families where the highest degree earned by either the head or spouse was a bachelor's degree (15%) or a graduate degree (17%). Moonlighting and overtime work are plausibly associated with liquidity constraints: individuals working more hours than they would if they could borrow to finance current consumption.

7 Conclusion

How the student loan payment pause originating with the COVID-19 pandemic impacted labor supply varies markedly with the level of education of borrowers. For those borrowers who completed a BA degree or a graduate degree, the payment pause does not appear to have impacted labor supply or earnings, even as there may well have been impacts on other margins, including the use of other sources of credit (Dinerstein et al., 2024). Because those borrowers with the most education also have the highest debt levels, the direct financial benefits of the payment pause also favored these borrowers (Briones et al., 2023b).

Where there is a significant impact of the payment pause on labor supply is among those with the least post-secondary education ("some college"). Labor force participation declined

[11] It is important to note that the extensive and intensive margin estimates are generally not separately point identified without imposing additional assumptions. Assumptions required under typical bounding approaches (e.g., Lee (2009)) are unlikely to hold in our setting, so we present our intensive margin estimates as they are.

for some of these borrowers. At the intensive margin, we find evidence of reductions in hours for borrowers, including declines in "moonlighting" and "overtime" in response to the payment pause. If those workers who changed labor supply behavior in response to the pause were effectively liquidity constrained, an open question is whether they had access to policy relief such as income-driven repayment or graduated payment schedules.

A growing body of research evidence suggests that many of the borrowers who struggled with repayment could have found relief with income-driven repayment programs but did not avail themselves of these options (Abraham et al., 2020; Herbst, 2023; Mueller and Yannelis, 2022). While we do not observe payment status directly, the observed labor supply responses suggest that a group of borrowers concentrated in the some college category were likely working more hours than would be consistent with consumption smoothing owing to the liquidity burden of repaying student debt. If this inference is correct, there are likely welfare gains to policies that help borrowers understand and exercise repayment options that reduce near-term loan payment burdens.[12]

[12]A contemporary post-script follows from the "legal limbo" generated by the legal injunction of the Saving for a Valuable Education (SAVE) plan, an income-driven repayment which began enrolling borrowers in 2023 before. By July 2024, when the Eighth Circuit Court issued an injunction blocking the program, enrollment had grown to nearly 8 million borrowers. Since July 19, 2024, these borrowers have been placed in interest-free forbearance, neither making payments nor accruing interest. The February 18, 2025 appellate court decision upholding the injunction means these borrowers will likely need to transition to other income driven repayment plans with less generous terms. Under SAVE, many borrowers qualified for zero or very low payments due to the higher income protection threshold (225% vs 150% of poverty) and anticipated lower payment rates for undergraduate loans (5% vs 10%). The transition to alternative plans will create new liquidity constraints, potentially leading to increased labor supply as borrowers seek additional income to meet higher required payments along with other consumption. This effect may be particularly pronounced for undergraduate borrowers and those with incomes between 150% and 225% of the poverty line who had qualified for zero payments under SAVE.

Tables and Figures

Table 1. Descriptive Statistics for Borrowers and Non-Borrowers, December 2019

	(1) Borrowers	(2) Non-Borrowers	(3) Difference	(4) Matched Difference
Married	0.440	0.587	-0.147***	0.006
			(0.018)	(0.022)
Male	0.419	0.478	-0.059***	0.024
			(0.018)	(0.022)
Age	33.630	35.027	-1.397***	-0.101
			(0.215)	(0.257)
White	0.730	0.728	0.002	-0.004
			(0.017)	(0.021)
Black	0.183	0.114	0.069***	0.008
			(0.016)	(0.020)
Asian	0.047	0.119	-0.072***	0.006
			(0.009)	(0.009)
Other Race	0.041	0.039	0.002	-0.010
			(0.007)	(0.009)
<BA	0.313	0.411	-0.097***	0.000
			(0.017)	(0.021)
BA	0.398	0.377	0.021	-0.012
			(0.018)	(0.022)
>BA	0.288	0.212	0.076***	0.012
			(0.016)	(0.020)
Monthly Earnings from Work	4,734	5,318	-584***	117
			(213.384)	(232.771)
Average Weekly Hours	35.320	33.324	1.996***	1.816**
			(0.654)	(0.778)
Household Income	115,603	140,557	-24,954***	1,338
			(4414.840)	(4322.702)
Student Debt	40,663	0	40,663***	40,154***
			(1722.483)	(1710.284)
Total Debt	97,778	78,533	19,245***	10,195
			(6051.846)	(6536.040)
Observations	1,387	3,631	5,018	2,762

Notes: This table presents summary statistics for college-educated individuals ages 25-45 measured in December 2019. Columns 1 and 2 report the mean of each variable for borrowers and non-borrowers, respectively. Column 3 reports the difference between borrowers and non-borrowers in the full sample, while Column 4 reports the difference in the matched sample. All estimates are weighted using SIPP two-year longitudinal weights. Robust standard errors clustered at the person level are reported in parentheses. Significance levels are indicated as * 0.10 ** 0.05 ***0.01.

Table 2. Difference-in-Differences Estimates of Effects on Earnings and Hours, 2019-2020 Panel

Panel A: Real Monthly Earnings

Post X Borrower	-14.99	-290.9**	51.53	195.3
	(108.3)	(138.0)	(161.7)	(263.2)
Education	All	<BA	BA	>BA
Time FE	Yes	Yes	Yes	Yes
Match FE	Yes	Yes	Yes	Yes
Control Mean	4,458.87	2,793.97	4,846.82	5,746.37
Observations	66,096	19,968	27,168	18,960

Panel B: Average Weekly Hours Worked

Post X Borrower	-1.337**	-3.311***	-0.484	-0.359
	(0.601)	(1.234)	(0.903)	(0.989)
Education	All	<BA	BA	>BA
Time FE	Yes	Yes	Yes	Yes
Match FE	Yes	Yes	Yes	Yes
Control Mean	32.94	28.96	35.37	33.91
Observations	66,096	19,968	27,168	18,960

Notes: This table presents estimates from OLS regressions, estimated on a matched sample of college-educated individuals ages 25-45. Standard errors are clustered at the matched group level. Each estimate reflects the difference between student borrower and non-borrower outcomes relative to the pre-pause period (January 2019 - February 2020). All regressions include matched pair and month fixed effects (FE) and an indicator variable equal to one if the observation is a student borrower. Means of the dependent variable are reported for non-borrowers in the pre-period. Earnings are in 2019 dollars and are measured as the sum of an individual's monthly work earnings from up to two primary jobs. Work hours are the sum of the individual's average weekly hours in a month from up to two primary jobs. Outcomes are winsorized at the 1% level. Significance levels are indicated as * 0.10 ** 0.05 ***0.01.

Table 3. Difference-in-Differences Estimates of Effects on Earnings and Hours, 2017-2018 Placebo Panel

Panel A: Real Monthly Earnings

Post X Borrower	-54.18	-90.35	75.69	-207.5
	(104.8)	(145.9)	(159.6)	(250.8)
Education	All	<BA	BA	>BA
Time FE	Yes	Yes	Yes	Yes
Match FE	Yes	Yes	Yes	Yes
Control Mean	4,111.79	2,754.30	3,948.76	5,985.22
Observations	73,344	23,520	29,952	19,872

Panel B: Average Weekly Hours Worked

Post X Borrower	0.0375	0.742	0.118	-0.928
	(0.618)	(1.150)	(1.023)	(0.988)
Education	All	<BA	BA	>BA
Time FE	Yes	Yes	Yes	Yes
Match FE	Yes	Yes	Yes	Yes
Control Mean	32.05	29.44	32.33	34.76
Observations	73,344	23,520	29,952	19,872

Notes: This table presents estimates from OLS regressions, estimated on a matched sample of college-educated individuals ages 25-45 in the placebo 2017-2018 panel. Standard errors are clustered at the matched group level. Each estimate reflects the difference between student borrower and non-borrower outcomes relative to the placebo pre-pause period (January 2017 - February 2018). All regressions include matched pair and month fixed effects (FE) and an indicator variable equal to one if the observation is a student borrower. Means of the dependent variable are reported for non-borrowers in the pre-period. Earnings are in 2019 dollars and are measured as the sum of an individual's monthly work earnings from up to two primary jobs. Work hours are the sum of the individual's average weekly hours in a month from up to two primary jobs. Outcomes are winsorized at the 1% level. Significance levels are indicated as * 0.10 ** 0.05 ***0.01.

Table 4. Difference-in-Differences Estimates: Additional Work Characteristics, 2019-2020 Panel

Panel A: All Education Levels

	(1) Hourly wage	(2) Non-zero hours	(3) Hours | Hours > 0	(4) Moonlight	(5) Overtime	(6) Full-time	(7) Part-time
Post X Borrower	1.789	-0.029**	-0.310	-0.011	-0.025*	0.002	-0.006
	(2.703)	(0.012)	(0.444)	(0.009)	(0.013)	(0.017)	(0.012)
Control Mean	26.49	0.83	39.87	0.06	0.21	0.48	0.13
Borrower Mean	26.55	0.86	40.67	0.09	0.26	0.47	0.13
Observations	66,096	66,096	55,207	66,096	66,096	66,096	66,096

Panel B: <BA

	(1) Hourly wage	(2) Non-zero hours	(3) Hours | Hours > 0	(4) Moonlight	(5) Overtime	(6) Full-time	(7) Part-time
Post X Borrower	-3.184	-0.050**	-1.850**	-0.053***	-0.062**	-0.000	0.012
	(3.150)	(0.025)	(0.921)	(0.017)	(0.024)	(0.032)	(0.021)
Control Mean	17.39	0.76	38.24	0.04	0.15	0.45	0.15
Borrower Mean	16.20	0.80	40.27	0.11	0.22	0.42	0.16
Observations	19,968	19,968	15,320	19,968	19,968	19,968	19,968

Panel C: BA

	(1) Hourly wage	(2) Non-zero hours	(3) Hours | Hours > 0	(4) Moonlight	(5) Overtime	(6) Full-time	(7) Part-time
Post X Borrower	6.504	-0.019	0.141	-0.001	-0.017	0.017	-0.019
	(5.849)	(0.018)	(0.648)	(0.014)	(0.021)	(0.026)	(0.018)
Control Mean	27.90	0.86	40.96	0.07	0.24	0.52	0.11
Borrower Mean	28.31	0.87	40.40	0.09	0.25	0.50	0.11
Observations	27,168	27,168	23,144	27,168	27,168	27,168	27,168

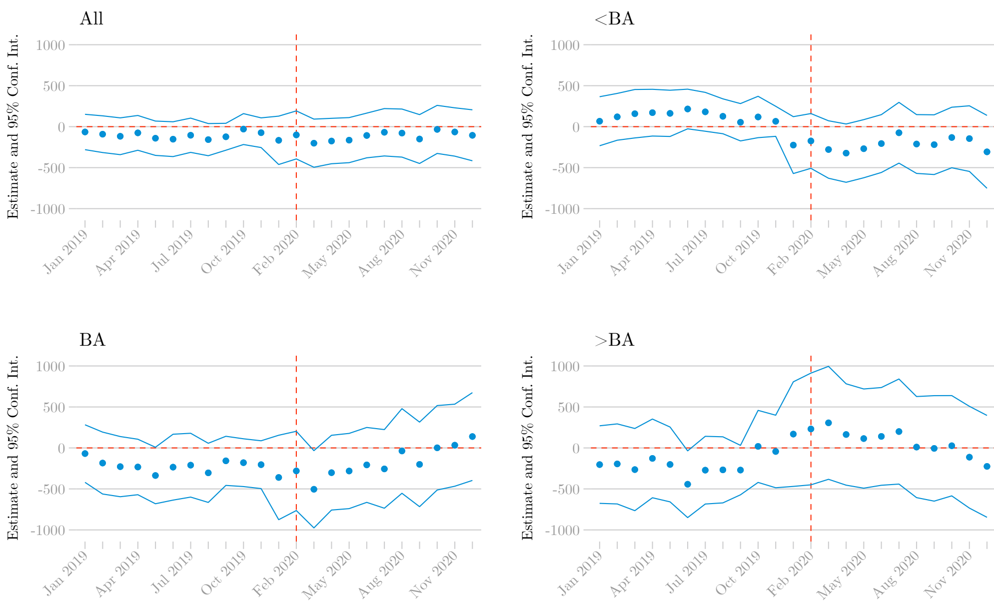
Panel D: >BA

	(1) Hourly wage	(2) Non-zero hours	(3) Hours | Hours > 0	(4) Moonlight	(5) Overtime	(6) Full-time	(7) Part-time
Post X Borrower	0.664	-0.019	0.447	0.019	0.004	-0.016	-0.008
	(3.217)	(0.017)	(0.781)	(0.018)	(0.024)	(0.028)	(0.026)
Control Mean	34.51	0.85	39.91	0.06	0.23	0.47	0.15
Borrower Mean	35.45	0.91	41.43	0.08	0.30	0.49	0.12
Observations	18,960	18,960	16,743	18,960	18,960	18,960	18,960

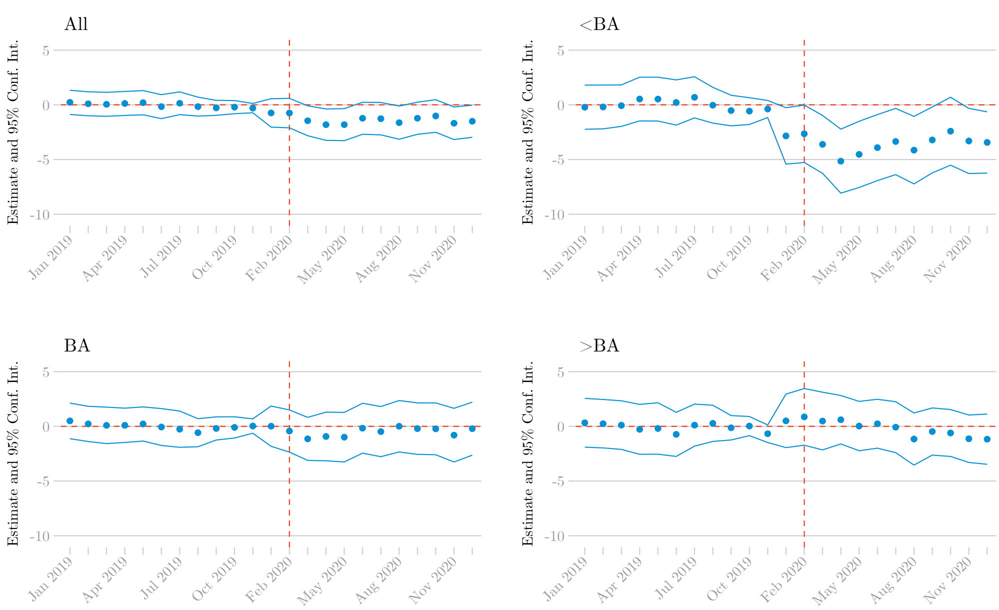
Notes: This table reports estimates from difference-in-differences regressions on the cumulative post estimate from March to December 2020. Dependent variables represent different labor characteristics. All regressions include matched pair and month fixed effects (FE) and an indicator variable equal to one if the observation is a student borrower. Outcomes in columns 2, 4, 5, 6, and 7 are dummy variables. Hourly wage is approximated by $\frac{earnings_{it}}{hours_{it} \times 4.3}$. An individual is moonlighting if they hold two jobs simultaneously for at least three weeks or more within a month. Overtime (>40 hours), full-time (35-40 hours), part-time (>0 and <35 hours) follows definitions from the Bureau of Labor Statistics. Significance levels are indicated as * 0.10 ** 0.05 ***0.01.

Figure 1. Effect of the Student Loan Payment Pause on Work Hours and Earnings: Event Study Estimates

A. Real Monthly Earnings

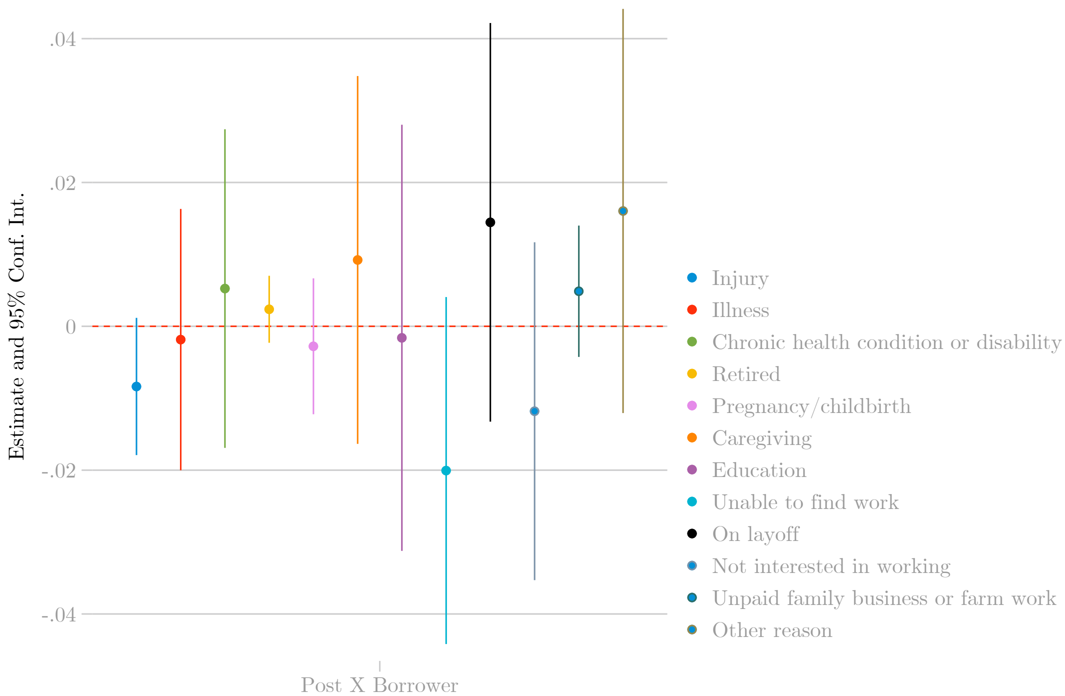


B. Average Weekly Hours Worked



Notes: These figures present estimates from OLS regressions, estimated on a matched sample of college-educated individuals ages 25-45. Standard errors are clustered at the matched group level. All variables are in levels. Earnings are in 2019 dollars and are measured as the sum of an individual's monthly work earnings from up to two primary jobs. Work hours are the sum of the individual's average weekly hours in a month from up to two primary jobs. The reference period is December 2019. The red vertical dashed line marks the period before the pause takes effect. 95% confidence intervals for estimates are displayed.

Figure 2. Respondents' Reasons for No Job Spell: Difference-in-Differences Estimates for <BA Group



This figure reports estimates from difference-in-differences regressions in our main analysis sample. The outcomes are dummy variables equal to one if the respondent reports the indicated reason for non-employment within the month. All variables and stated reasons come directly from the SIPP.

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A Appendix Tables and Figures

Appendix Table A1. Difference-in-Differences Estimates: Weighted versus Unweighted, 2019-2020 Panel

Panel A: Real Monthly Earnings

Post X Borrower	-80.08	-14.99	-291.0**	-290.9**	-129.0	51.53	212.2	195.3
	(101.0)	(108.3)	(146.9)	(138.0)	(140.6)	(161.7)	(243.7)	(263.2)
Weight Least Squares	No	Yes	No	Yes	No	Yes	No	Yes
Education	All	All	<BA	<BA	BA	BA	>BA	>BA
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	4,565.82	4,458.87	2,845.14	2,793.97	4,922.68	4,846.82	5,865.87	5,746.37
Observations	66,288	66,096	20,016	19,968	27,264	27,168	19,008	18,960

Panel B: Average Weekly Hours Worked

Post X Borrower	-1.028**	-1.337**	-2.965***	-3.311***	-0.760	-0.484	0.628	-0.359
	(0.501)	(0.601)	(1.024)	(1.234)	(0.770)	(0.903)	(0.812)	(0.989)
Weight Least Squares	No	Yes	No	Yes	No	Yes	No	Yes
Education	All	All	<BA	<BA	BA	BA	>BA	>BA
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	33.37	32.94	29.15	28.96	35.79	35.37	34.36	33.91
Observations	66,288	66,096	20,016	19,968	27,264	27,168	19,008	18,960

Notes: This table presents regression estimates, from a matched sample of college-educated individuals ages 25-45. In regressions that use weighted least squares, we utilize student borrowers' two-year longitudinal survey weights provided by the Census Bureau. All other matching and analysis procedures follow our main results. Significance levels are indicated as * 0.10 ** 0.05 ***0.01.

Appendix Table A2. Poisson Regression Estimates of the Average Proportional Treatment Effect on the Treated, 2019-2020 Panel

Panel A: Real Monthly Earnings

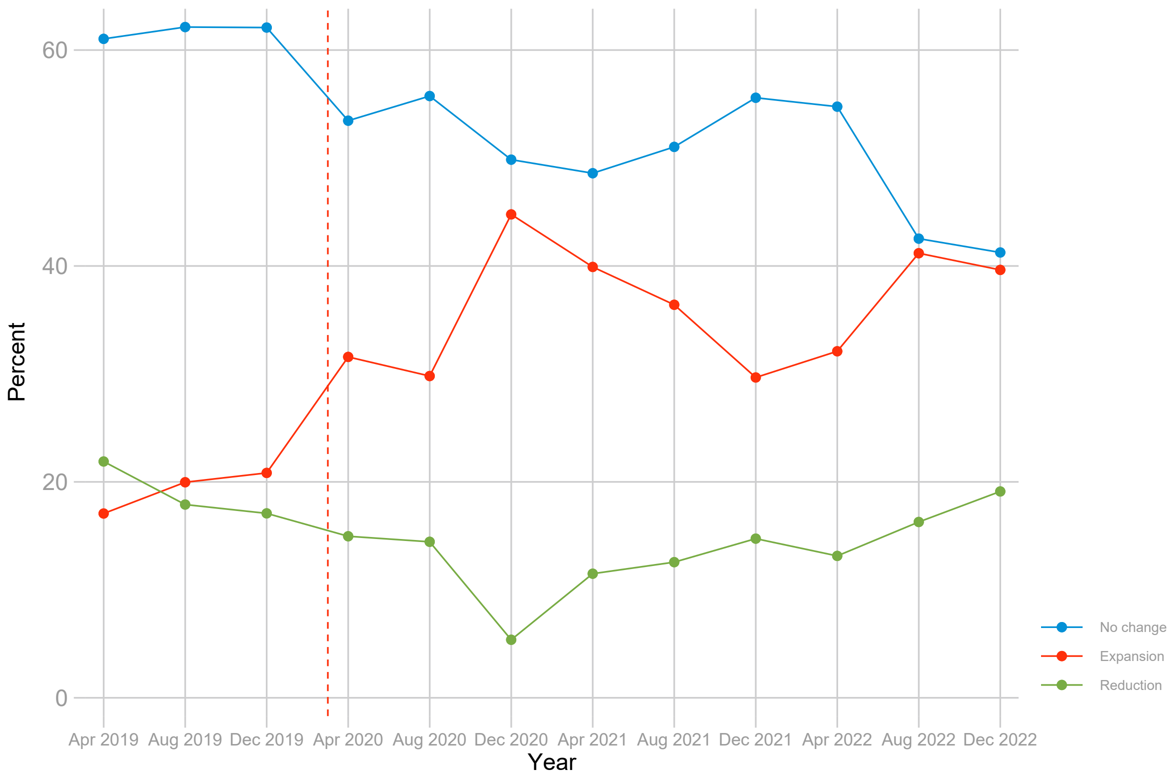
Post X Borrower	-0.00233	-0.106**	0.0136	0.0270
	(0.0240)	(0.0495)	(0.0349)	(0.0418)
Prop. Effect	-0.00232	-0.100	0.0136	0.0274
Prop. Effect (SE)	0.0240	0.0446	0.0354	0.0430
Education	All	<BA	BA	>BA
Time FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	65664	19680	27072	18912

Panel B: Average Weekly Hours Worked

Post X Borrower	-0.0390**	-0.109***	-0.0145	-0.0119
	(0.0180)	(0.0420)	(0.0262)	(0.0273)
Prop. Effect	-0.0382	-0.103	-0.0144	-0.0118
Prop. Effect (SE)	0.0173	0.0377	0.0258	0.0269
Education	All	<BA	BA	>BA
Time FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	65664	19680	27072	18912

Notes: This table presents Poisson regression estimates using a matched sample of college-educated individuals ages 25-45. Standard errors are clustered at the matched group level. Prop. Effect rows are the implied estimate of the proportional treatment effect showing exp(β)-1 where β is the estimate from Post X Borrower. Prop. Effect (SE) are the associated standard errors of the proportional effect. Poisson regressions are run using ppmldhfe in Stata. Significance levels for the Post X Borrower estimates are indicated as * 0.10 ** 0.05 ***0.01.

Appendix Figure A1. Federal Loan Forgiveness Expectations: Average Percent Chance of Policy Change in the Next 12 Months, 2019-2022

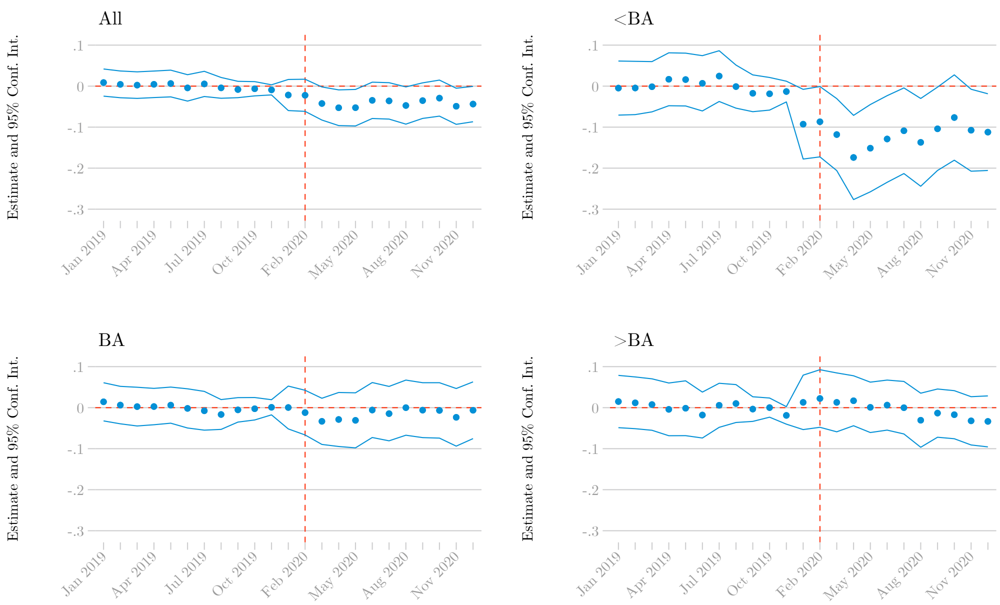


Notes: This figure reports the average percent chance of a policy change in federal student debt forgiveness in the next 12 months for a sample of student loan borrowers in the Survey of Consumer Expectations (SCE). Respondents are asked to provide probabilities for "no change", "expansion", and "reduction" (must sum to 100). The sample is restricted to those who indicated they possess student loans in the SCE Credit Access Survey. The vertical line marks the start of the payment pause.

Appendix Figure A2. Poisson Regression Event Study Estimates

A. Real Monthly Earnings

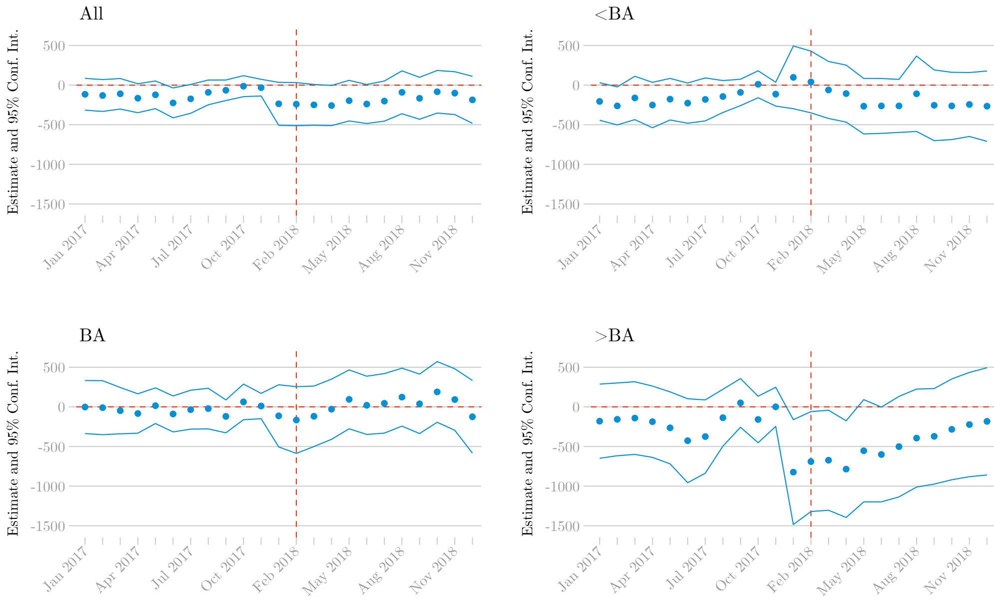
B. Average Weekly Hours Worked



Notes: These figures present Poisson regression event study estimates using a matched sample of college-educated individuals ages 25-45. Standard errors are clustered at the matched group level. Poisson regressions are run using ppmldhfe in Stata. The reference period is December 2019. The red vertical dashed line marks the period before the pause takes effect.

Appendix Figure A3. Event Study Estimates in the 2017-2018 Placebo Sample

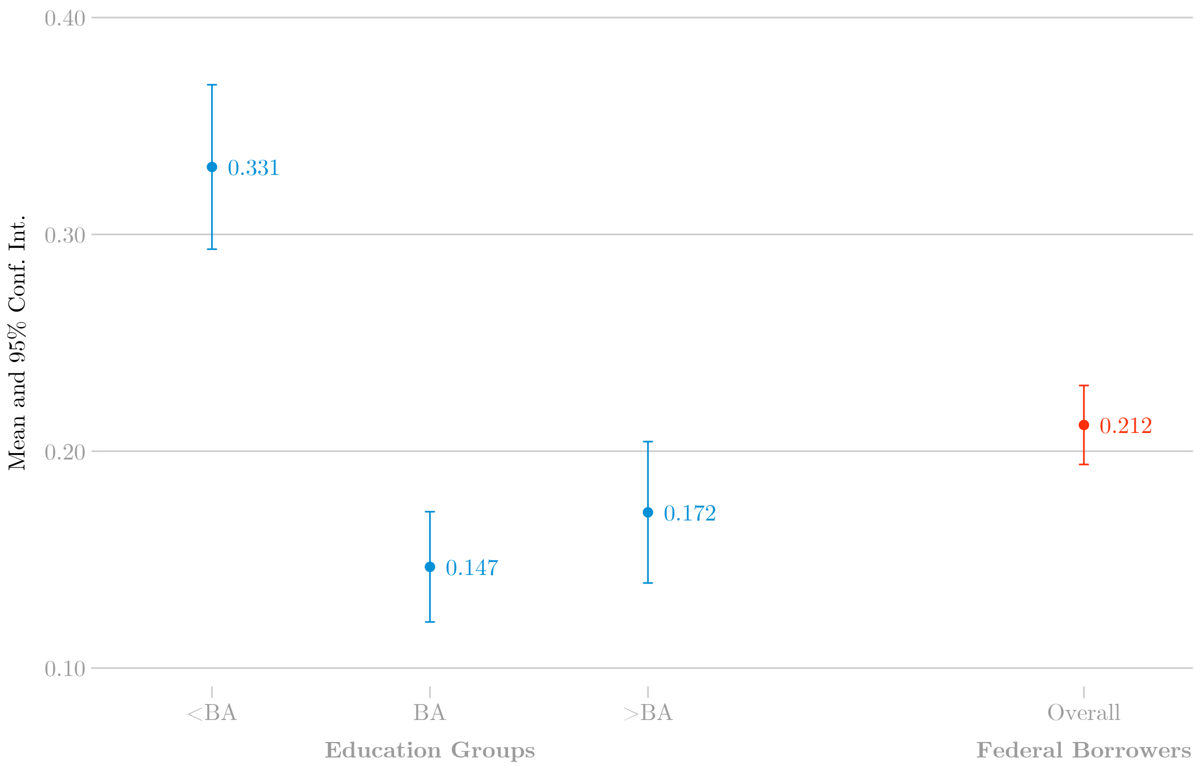
A. Real Monthly Earnings



A. Real Monthly Earnings

Notes: These figures present estimates from OLS regressions, estimated on a matched sample of college-educated individuals ages 25-45 in the 2017-2018 placebo sample. Standard errors are clustered at the matched group level. All variables are in levels. Earnings are in 2019 dollars and are measured as the sum of an individual's monthly work earnings from up to two primary jobs. Work hours are the sum of the individual's average weekly hours in a month from up to two primary jobs. The reference period is December 2017. The red vertical dashed line marks the period before the placebo "pause" (February 2018) takes effect.

Appendix Figure A4. Share of Federal Student Loan Borrower Households Ages 25-45 Reporting Credit Constraints, 2019 Survey of Consumer Finances



Notes: This figure reports the share of households with federal student debt that exhibit credit constraints in the 2019 Survey of Consumer Finances. The sample is restricted to households with federal student loan debt where the survey's reference person is between the ages of 25 and 45. In cases where a spouse is present, education categories are assigned based on the couple's highest level of educational attainment. An observation is credit constrained if they report they were turned down by a lender or received not as much credit as they requested in the past 12 months. We estimate shares and 95% confidence intervals by running OLS regressions on education group dummy variables using SCF sample weights.

B Additional Results, Robustness Checks, and Details on Matching Procedure

B.1 Baseline Matching Procedure

Our baseline specification identifies non-student borrower matches using propensity score matching. These matches are constructed using the psmatch2 command in Stata and follow the recommendations therein on constructing matches in survey data with sampling weights. Specifically, we estimate propensity scores using a logit model and match on the logarithm of the odds ratio, ignoring sampling weights in estimating the propensity scores. Sampling weights are reintroduced in our baseline difference-in-differences estimates. We use the longitudinal sampling weights of the student borrowers (the treated) such that matched pairs share identical weights. We exactly match on an individual's broad education level and SIPP panel and then perform a probabilistic match on additional characteristics. Propensity score distributions can be provided upon request.

Below are descriptive statistics for the matched sample, including earnings and hours observed over the sample period. All variables displayed except earnings and hours are included in the baseline matching specification. Student borrowers are those who hold a positive student loan balance in December 2019. Our main matching specification does not require non-student loan borrowers to report a positive total debt balance. Our results are robust to making this restriction.

Appendix Table B1. Descriptive Statistics for Matched Sample

Variable	Mean	Std. Dev.	Min	Median	Max
Married	0.437	0.496	0.000	0.000	1.000
Male	0.407	0.491	0.000	0.000	1.000
Age	33.679	5.823	25.000	33.000	45.000
White	0.733	0.442	0.000	1.000	1.000
Black	0.179	0.384	0.000	0.000	1.000
Asian	0.044	0.205	0.000	0.000	1.000
Other Race	0.044	0.204	0.000	0.000	1.000
<BA	0.314	0.464	0.000	0.000	1.000
BA	0.406	0.491	0.000	0.000	1.000
>BA	0.280	0.449	0.000	0.000	1.000
Monthly Earnings from Work	4,557	5,695	0	3,637	370,000
Average Weekly Hours	33.804	18.893	0.000	40.000	132.000
Household Income	115,000	104,000	-27,200	92,016	2,150,000
Student Debt	38,998	52,961	0	20,000	257,000
Total Debt	88,462	150,000	0	35,000	3,100,000

Notes: This table presents descriptive statistics for the matched sample of college-educated individuals ages 25-45. All estimates are weighted using SIPP two-year longitudinal weights. Monetary values are in 2019 dollars. Observations are at the individual by month level. Debt values, household income, and demographic characteristics are measured as of December 2019. Individuals may have negative household income if they experienced a business loss or net loss of income from assets. Monthly earnings and average weekly hours estimates shown here are not winsorized.

B.2 Full-Sample Difference-in-Differences Results

Below, we present results from the SIPP using the full sample of observations (without matching) of college-educated individuals ages 25-45 in the 2019-2020 panel.

Appendix Table B2. Difference-in-Differences Estimates of Effects on Earnings and Hours in the Full Sample, 2019-2020 Panel

Panel A: Real Monthly Earnings

Post X Borrower	170.9	-134.6	231.0	388.2
	(105.6)	(125.4)	(166.0)	(259.9)
Education	All	<BA	BA	>BA
Time FE	Yes	Yes	Yes	Yes
Match FE	Yes	Yes	Yes	Yes
Control Mean	4,909.07	3,263.41	5,486.90	7,068.54
Month x Unique Individuals	120,144	44,040	47,568	28,536

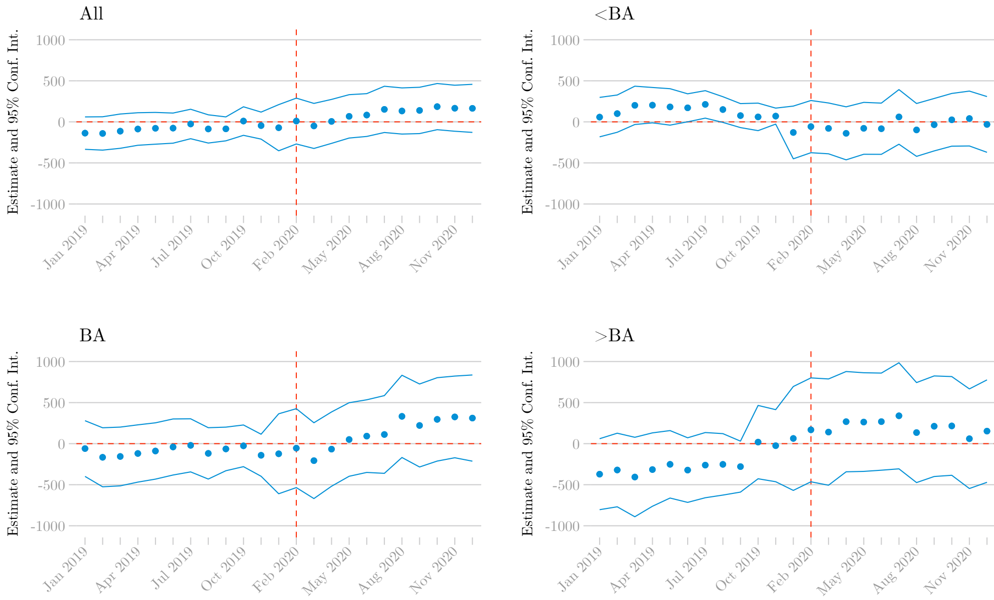
Panel B: Average Weekly Hours Worked

Post X Borrower	-0.434	-2.443**	-0.0727	1.082
	(0.532)	(1.060)	(0.809)	(0.862)
Education	All	<BA	BA	>BA
Time FE	Yes	Yes	Yes	Yes
Match FE	Yes	Yes	Yes	Yes
Control Mean	33.07	30.78	34.37	35.17
Month x Unique Individuals	120,144	44,040	47,568	28,536

Notes: This table presents estimates from OLS regressions, estimated on the full sample of college-educated individuals ages 25-45. Standard errors are clustered at the individual level. Each estimate reflects the difference between student borrower and non-borrower outcomes relative to the pre-pause period (January 2019 - February 2020). All regressions include individual and month fixed effects (FE) and an indicator variable equal to one if the observation is a student borrower. Means of the dependent variable are reported for non-borrowers in the pre-period. Earnings are in 2019 dollars and are measured as the sum of an individual's monthly work earnings from up to two primary jobs. Work hours are the sum of the individual's average weekly hours in a month from up to two primary jobs. Outcomes are winsorized at the 1% level. Significance levels are indicated as * 0.10 ** 0.05 ***0.01.

Appendix Figure B1. Event Study Estimates in the Full Sample, 2019-2020 Panel

A. Real Monthly Earnings



B. Average Weekly Hours Worked

Notes: These figures present estimates from OLS regressions, estimated on the full sample of college-educated individuals ages 25-45. Standard errors are clustered at the individual level. All variables are in levels. Earnings are in 2019 dollars and are measured as the sum of an individual's monthly work earnings from up to two primary jobs. Work hours are the sum of the individual's average weekly hours in a month from up to two primary jobs. The reference period is December 2019. The red vertical dashed line marks the period before the pause takes effect. 95% confidence intervals for estimates are displayed.

B.3 Alternative Matching Specifications

Below, we present results from alternative matching specifications. Labels for each of the specifications are specified in bold and correspond to the "Matching Method" label in Appendix Table B3.

1. One-to-one propensity score matching without replacement:

(a) **Baseline** (Presented in the main text, repeated for comparison)

- Exact match on SIPP panel and broad education group (some college, bachelor's degree, graduate-level degree)
- Demographics: Indicators for marriage, sex, race, and age
- Total debt
- Household income

(b) **Total Debt > 0**

- All variables in baseline
- Non-student loan borrowers (control) must have a positive total debt balance

(c) **Liquid Wealth**

- All variables in baseline
- December 2019 liquid wealth: The sum of bank assets, bonds and stocks values. A reminder that the SIPP measures asset and liabilitie values as of the last day of the reference year (December 31).

2. One-to-one exact matching with replacement: In this specification we require that student borrowers and their matched controls to match exactly on the characteristics below.

(d) **Exact Match**

- Education group, SIPP panel, and sex
- Quantile of age, total debt, household income, and liquid wealth

Within matched groups, we randomly select one control non-borrower, but do not restrict a non-student borrower from being a control for multiple student borrowers. The exact matching requirement reduces overall sample size, though our main results are robust to this specification.

Appendix Table B3. Difference-in-Differences Estimates: Alternative Matching Specifications, 2019-2020 Panel

Panel A: All Education Levels

	Real Monthly Earnings				Average Weekly Hours			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post X Borrower	-14.978	64.154	65.484	-86.254	-1.337**	-1.156**	-1.466**	-2.112**
	(108.335)	(129.046)	(124.085)	(172.961)	(0.601)	(0.579)	(0.607)	(0.884)
Matching Method	Baseline	Total Debt > 0	Liquid Wealth	Exact Match	Baseline	Total Debt > 0	Liquid Wealth	Exact Match
Control Mean	4,458.88	4,734.98	4,369.81	4,701.09	32.94	34.75	31.91	34.05
Observations	66,096	65,520	66,096	29,328	66,096	65,520	66,096	29,328

Panel B: <BA

	Real Monthly Earnings				Average Weekly Hours			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post X Borrower	-290.941**	-220.278	-270.092	-190.550	-3.311***	-3.949***	-3.379***	-4.065***
	(138.016)	(150.656)	(170.320)	(157.806)	(1.234)	(1.113)	(1.236)	(1.472)
Matching Method	Baseline	Total Debt > 0	Liquid Wealth	Exact Match	Baseline	Total Debt > 0	Liquid Wealth	Exact Match
Control Mean	2,793.97	3,055.87	2,750.97	2,769.01	28.96	30.33	28.48	30.52
Observations	19,968	19,920	19,968	11,856	19,968	19,920	19,968	11,856

Panel C: BA

	Real Monthly Earnings				Average Weekly Hours			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post X Borrower	51.558	181.766	295.446	192.020	-0.483	0.175	-0.811	-0.645
	(161.672)	(179.262)	(212.434)	(385.563)	(0.903)	(0.938)	(0.932)	(1.570)
Matching Method	Baseline	Total Debt > 0	Liquid Wealth	Exact Match	Baseline	Total Debt > 0	Liquid Wealth	Exact Match
Control Mean	4,846.84	4,882.39	4,488.61	5,309.61	35.37	37.24	33.59	35.73
Observations	27,168	26,880	27,120	10,752	27,168	26,880	27,120	10,752

Panel D: >BA

	Real Monthly Earnings				Average Weekly Hours			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post X Borrower	195.312	218.367	113.428	-345.756	-0.359	0.111	-0.280	-0.716
	(263.227)	(337.634)	(253.293)	(360.241)	(0.989)	(0.895)	(0.974)	(1.094)
Matching Method	Baseline	Total Debt > 0	Liquid Wealth	Exact Match	Baseline	Total Debt > 0	Liquid Wealth	Exact Match
Control Mean	5,746.37	6,409.16	5,980.52	7,492.44	33.91	36.24	33.32	38.19
Observations	18,960	18,720	19,008	6,720	18,960	18,720	19,008	6,720

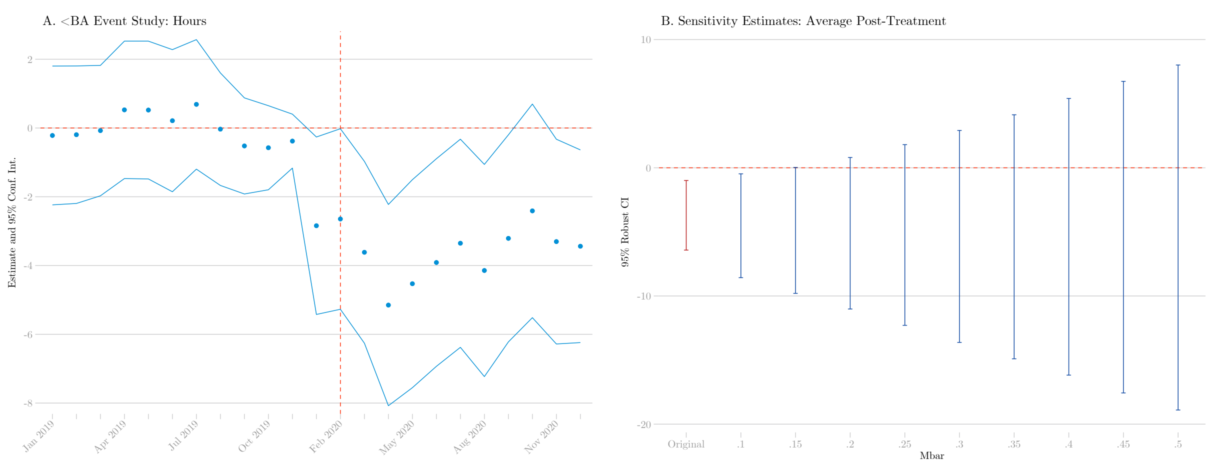
Notes: This table presents estimates from OLS regressions, estimated on matched samples of college-educated individuals ages 25-45. Columns 1-4 (5-8) show difference-in-differences estimates on earnings (hours) across matching specifications. Standard errors are clustered at the matched group level. Details on each matching procedure is provided in the appendix. Significance levels are indicated as * 0.10 ** 0.05 ***0.01.

B.4 Parallel Trends Sensitivity Analysis

Following Rambachan and Roth (2023), we use estimated differences between matched borrower and non-borrower pairs in the pre-payment pause period to inform how sensitive our results are to violations of parallel trends. The figures below present estimates of $\bar{M}$, the "breakdown value" for which an estimate is still significant. In other words we show whether particular results are robust to allowing for violations of parallel trends up to $\bar{M}$ as big as the maximum violation in the pre-payment pause period.

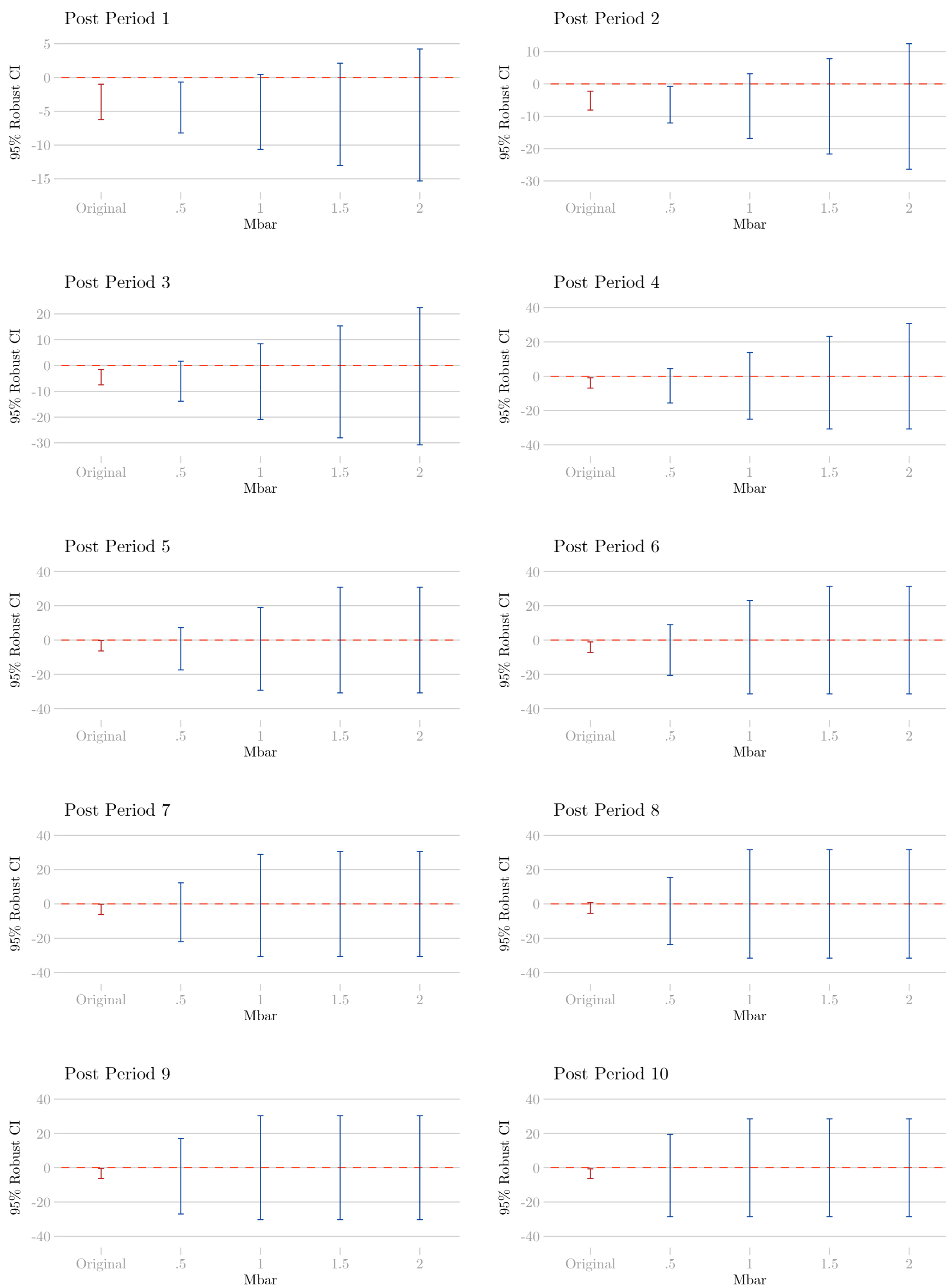
We focus on the set of estimates on average weekly hours worked for the $< BA$ group. The estimated hours decrease in the survey seam period (January-February 2020) naturally plays a critical role in estimates of $\bar{M}$. As we suspect this drop in hours is at least in part a data artifact (a documented feature of the SIPP), we also present estimates when dropping January and February 2020 from our sample. Sensitivity analyses are run using the HonestDiD package in Stata.

Appendix Figure B2. Sensitivity Analysis: Average Post-Treatment Period



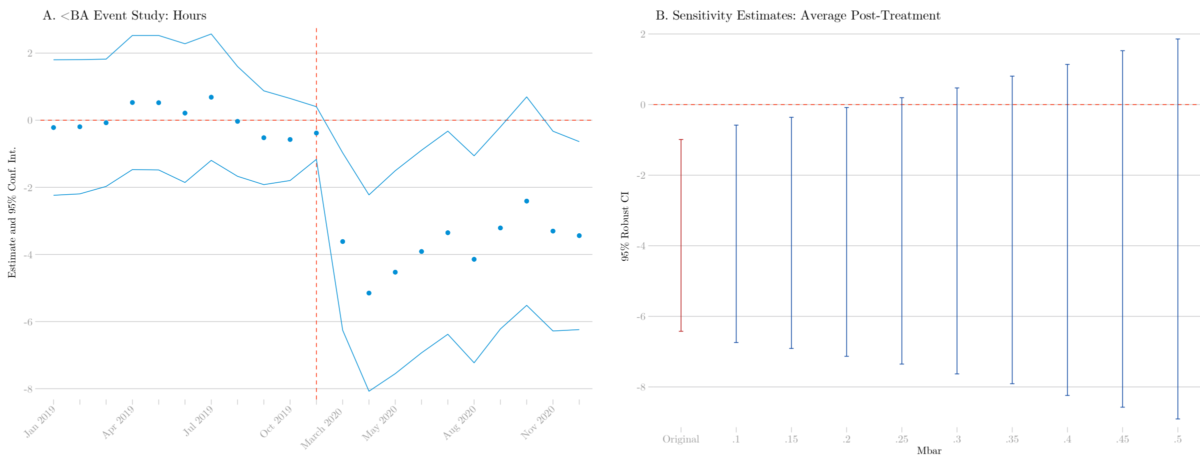
Notes: This figure presents our primary hours worked event study estimates for the $< BA$ group (panel A) and alternative confidence intervals for the average of the post-period coefficients (panel B). Estimates in panel B are calculated using the relative magnitude method described in Rambachan and Roth (2023) and implemented using the HonestDiD package in Stata. Estimates assume that that there is a violation in parallel trends in the post-period that is as large as the maximum deviation in the pre-preiod multiplied by a constant $\bar{M}$.

Appendix Figure B3. Sensitivity Analysis: Individual Month Estimates



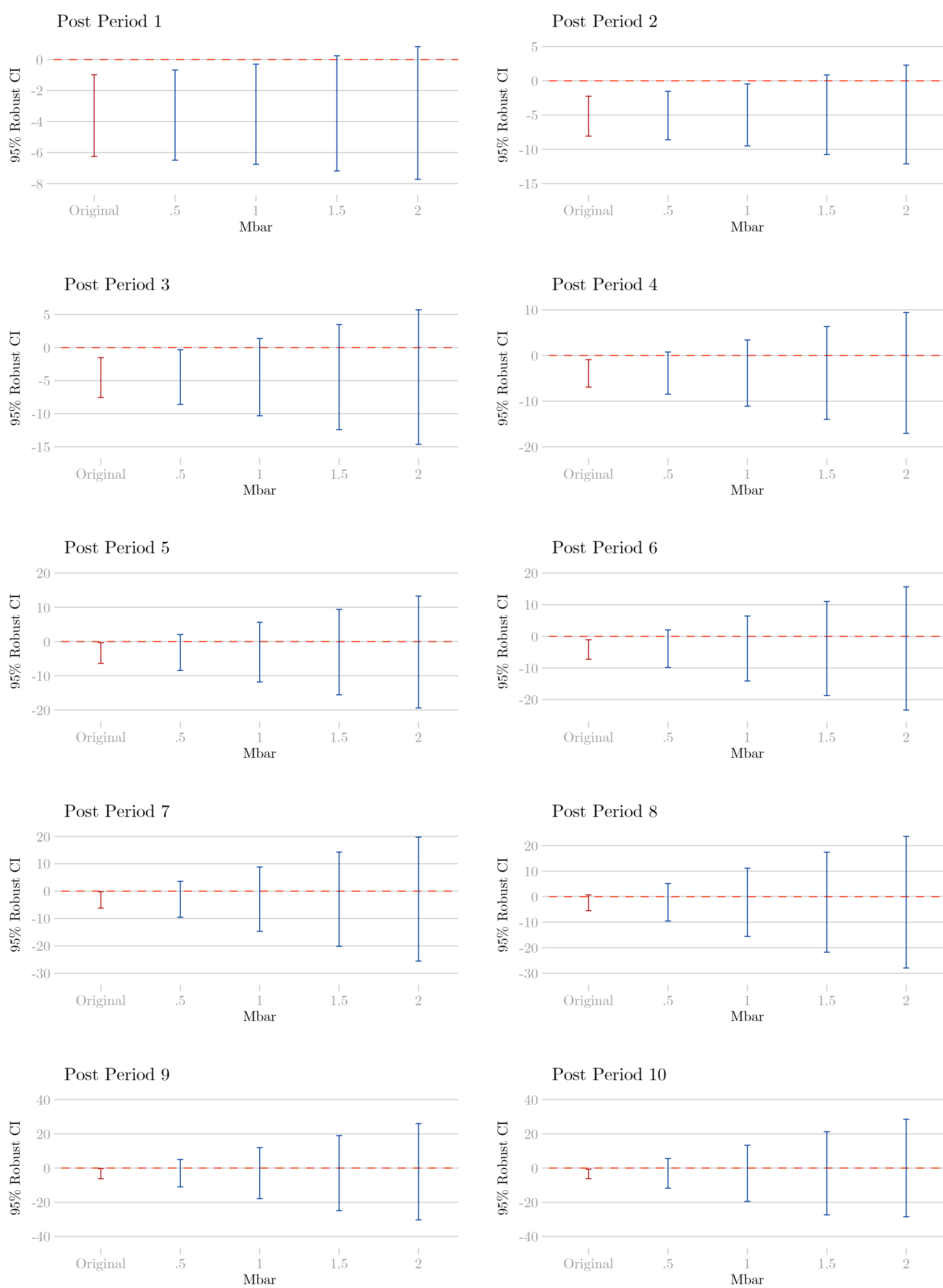
Notes: This figure presents alternative confidence intervals on each post-period coefficient where post period 1 (10) is March (December) 2020. Estimates are calculated using the relative magnitude method described in Rambachan and Roth (2023) and implemented using the HonestDiD package in Stata. Estimates assume that there is a violation in parallel trends in the post-period that is as large as the maximum deviation in the pre-period multiplied by a constant $\bar{M}$.

Appendix Figure B4. Sensitivity Analysis: Average Post-Treatment Period Without January and February 2020



Notes: This figure presents our primary hours worked event study estimates for the $< BA$ group (panel A) and alternative confidence intervals for the average of the post-period coefficients (panel B) when we drop January and February 2020 from our sample. Estimates in panel B are calculated using the relative magnitude method described in Rambachan and Roth (2023) and implemented using the HonestDiD package in Stata. Estimates assume that that there is a violation in parallel trends in the post-period that is as large as the maximum deviation in the pre-period multiplied by a constant $\bar{M}$.

Appendix Figure B5. Sensitivity Analysis: Individual Month Estimates Without January and February 2020



Notes: This figure presents alternative confidence intervals on each post-period coefficient where post period 1 (10) is March (December) 2020 when we drop January and February 2020 from our sample. Estimates are calculated using the relative magnitude method described in Rambachan and Roth (2023) and implemented using the HonestDiD package in Stata. Estimates assume that that there is a violation in parallel trends in the post-period that is as large as the maximum deviation in the pre-period multiplied by a constant $\bar{M}$.

C Data Appendix

C.1 Additional SIPP Details

The Survey of Income and Program Participation (SIPP) has an overlapping panel structure where respondents are interviewed about prior-year activities for up to four years. For example, a respondent who entered the SIPP in 2018 (the 2018 panel) can be observed consecutively about activities between 2017 (wave 1) through 2020 (wave 4). In order to maximize sample size while also having a longer pre-period to test our identification assumptions, we focus on individuals that we observe consecutively from 2019 through 2020, which corresponds to respondents in waves 3 and 4 of the 2018 panel and waves 1 and 2 of the 2020 panel. Due to the pandemic, the SIPP abandoned the 2019 panel. The public SIPP data files, data documentation, and user guides are available on the Census Bureau's website.

C.2 Sample Construction and Primary Variables

Our main analysis sample restricts to respondents ages 25 to 45 and had at least some college education as of December 2019. We then limit our sample to those who are observed for all 24 months from 2019 through 2020.

Prior-year activities and characteristics can be observed at different levels of frequency. Information on liabilities and assets are typically measured as of the last day of the reference period (i.e., December 31 of each year), while labor market information can be measured at the monthly level. Below, we outline the key variables to our analysis.

Monthly labor market information

The SIPP collects information for up to 6 jobs within a reference year. At a monthly level, we characterize an individual's work characteristics based on their primary, and if applicable, secondary job. Primary jobs are defined as the job with the highest earnings in that month.

- Real total earnings: The sum of earnings from up to two jobs in the month. All earnings are adjusted for inflation to 2019 levels.
- Total hours: The sum of average weekly hours worked for up to two jobs in the month.
- Moonlighting: Holds two jobs simultaneously for at least three weeks or more within a month. This approach follows Tazhitdinova (2022).
- Overtime (>40 hours), full-time (35-40 hours), and part-time (>0 and <35 hours) follow definitions from the Bureau of Labor Statistics as of the time of writing.

- Hourly wage is approximated by $\frac{earnings_{it}}{hours_{it} \times 4.3}$

- Industry: The SIPP panels we use follow the 2017 Census 4-digit codes to classify industries. We describe an individual's primary industry at the 2-digit level using the Census crosswalk to the 2-digit North American Industry Classification System.[13]

Individual characteristics

Unless otherwise noted, we use individual demographics, debt, and annual household income measured as of December 2019. Debt variables are only measured at the end of each reference period.

- Student borrowers: Positive student loan balance in December 2019.

- Total debt: The sum of all unsecured (e.g., credit card, student, medical) and secured (e.g., real estate, vehicles) liabilities in December 2019.

- Household annual income: The sum of monthly earnings and income received by household members ages 15 and older, as well as SSI payments received by children under age 15.

[13]Census industry and occupation code lists are available here: https://www.census.gov/topics/employment/industry-occupation/guidance/code-lists.html. See Appendix 1 of the following Census resource for SIPP datasets and their associated industry and occupation code lists: https://www2.census.gov/programs-surveys/demo/guidance/industry-occupation/IOindexesoverview2024.pdf.