Parental Love in Debt: Intergenerational Effects of Student Loans*

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Abstract

We document that the costs of student debt extend beyond borrowers to their parents. Exploiting the student loan federal payment moratorium which paused payments for a subset of borrowers, we estimate the effect of children's debt relief on parental finances. Parents of treated borrowers experience a 5.7% reduction in installment delinquencies, a 6.4% decline in bankruptcy, and increased mortgage, installment, and home equity borrowing. Effects concentrate within families with stronger financial ties and where children are least able to manage their obligations, and imply that borrower-only evaluations of debt relief understate its credit response by roughly a third.

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1 Introduction

Student loan debt in the United States has grown dramatically over the past two decades, reaching approximately $1.8 trillion by 2025 and surpassing all other forms of consumer debt except mortgages.[1] Whether and how to intervene in this market has been a persistent economic policy debate of the past decade, prompting executive actions, Supreme Court litigation, and extensive analysis of the costs and benefits of various relief proposals.[2] A large body of research informs this debate, documenting effects of student debt on borrowers' income and borrowing, homeownership, entrepreneurship, retirement savings, and other dimensions of their economic lives.[3] While economic theory and this literature model the effects of student debt on borrowers' own financial decisions, borrowers may not bear these burdens in isolation. Families are often financially interconnected, and the consequences of student debt on one member can propagate to others—particularly to parents, who often serve as the financial backstop for their children.

In this paper, we examine the intergenerational effects of student debt by studying how children's student loan forbearance affects their parents' financial outcomes. The question of whether and how children's debt burdens spill over to parents is both economically important and empirically ambiguous. On one hand, parents may bear part of the financial burden when their children struggle with student loan payments, and relief from that burden could improve parental finances. On the other hand, by the time children enter repayment they are typically working adults with independent finances, and the financial link to parents may be weak or nonexistent. Moreover, moral hazard models suggest that debt relief can reduce borrowers' incentive to work or maintain financial discipline (Lourie et al., 2023; Catherine and Yannelis,

[1] See Federal Reserve Bank of New York, *Quarterly Report on Household Debt and Credit* (2025); Looney and Yannelis (2022).

[2] See, e.g., Dynarski (2014) for an overview of the policy landscape; Catherine and Yannelis (2023) and Goss et al. (2024) for analyses of the distributional effects of forgiveness proposals.

[3] For a comprehensive review, see Yannelis and Tracey (2022). See also, e.g., Mezza et al. (2020), Di Maggio et al. (2025), Krishnan and Wang (2019), and Dinerstein et al. (2024).

2025), potentially increasing their dependence on parental support and worsening parental financial outcomes. Whether and in which direction intergenerational spillovers from student debt operate is thus an open empirical question—but one with significant policy implications, as their presence would imply that conventional estimates of the welfare effects of student debt relief are incomplete.

Addressing this question poses two significant challenges. First, it requires data that links children's student loan information to their parents' financial outcomes, which is rare. Second, student loan borrowing is endogenous to family characteristics that also determine parental financial health. Families in which children borrow more for education may differ systematically from families in which children borrow less, making naive comparisons across these groups difficult to interpret.

We address these challenges by combining administrative data with a natural experiment. Our data come from Equifax Inc. and include anonymized consumer credit histories at the credit-line level for all individuals with a credit history in the United States. These data include account types, balances, payment performance, financial distress events such as bankruptcies and foreclosures, anonymized historical names and addresses with age. They also allow us to identify household members and link student loan borrowers to their parents, enabling us to simultaneously observe both children's student loan obligations and their parents' financial outcomes.

In a difference-in-differences setting, we exploit the 2020 federal student loan moratorium that paused payments for a sample of borrowers with federally held student loans. Following Dinerstein et al. (2024) and Hamdi et al. (2025), we compare parents of borrowers whose loans were frozen under the moratorium to parents of borrowers whose loans were not frozen. Until 2010, the federal government offered two loan programs that differed only in their funding source: Direct Loans (DL) were funded by the U.S. Treasury, while Federal Family Education Loans (FFEL) were funded by private banks with government guarantees. The

2020 moratorium applied only to federally held student loans—primarily Direct Loans—while FFEL loans were excluded. This generates plausibly exogenous variation in forbearance across otherwise similar borrowers, determined by the administrative classification of their loans rather than borrower or family characteristics.

Our estimates show significant intergenerational spillovers. The moratorium delivered a substantial cash-flow shock to treated children, reducing their scheduled student loan payments by $163 per month—roughly 70% of the amount they owed before the moratorium, or approximately $1,950 per year. This relief transmits to parents across both margins of their balance sheets. We find that children's forbearance reduces parents' installment delinquencies by 5.7% and the likelihood of bankruptcy by 6.4% relative to the control mean. At the same time, parents' installment loan balances increase by approximately $754, mortgage balances by $760, and HELOC balances by $150 following the onset of their children's forbearance.

The magnitude of these responses implies that conventional evaluations of debt relief substantially understate its effects. We estimate that parents increase their borrowing by roughly $0.33 for every dollar of relief their children receive. Applying the same construction to the borrower-level estimates in Dinerstein et al. (2024) implies that borrowers themselves borrow approximately $0.60 per dollar of relief. The response measured across both generations is thus roughly one and a half times the figure obtained from borrowers' balance sheets alone. Policies evaluated on borrower outcomes alone therefore miss a substantial share of the credit demand they generate.

To evaluate whether these spillovers arise because parents bear part of their children's student loan burden, we first ask whether they require that forbearance free actual resources. Among children who were making lower payments before the moratorium—who received the same improvement in credit standing but little cash-flow relief—we find no effect on parental outcomes. We then examine whether the effects are stronger where financial ties between

generations are stronger. They are: the reduction in installment delinquencies is over six times larger among parents whose children still co-reside with them, and among parents who borrowed directly for their children's education through the Parent PLUS program, installment delinquencies fall by 11% relative to that group's mean.

The spillovers also concentrate where children are least able to manage their obligations on their own. Parents of children with subprime credit scores, lower incomes, and pre-existing delinquencies on their non-student debts experience larger reductions in distress and larger increases in borrowing than parents of children in stronger financial positions. This pattern speaks to the direction of support within the family. Transfers flowing from children to parents would require children with the capacity to make them, and would therefore concentrate among parents of financially stronger children. That the effects instead concentrate where children are least able to support anyone is consistent with relief reaching parents through a reduction in the support they extend.

We further examine how parental financial conditions shape the nature of these spillovers. Financially constrained parents are closer to distress because of the partial burden of children's student debt and therefore likely to experience larger effects through reduced delinquencies and bankruptcies. Less constrained parents, by contrast, can use the relaxation of their budget constraints to increase consumption and borrowing. Consistent with these predictions, we find that delinquency reductions concentrate among parents with lower credit scores, while increases in mortgage and HELOC borrowing concentrate among homeowning parents with higher credit scores. These parents appear to use home equity to finance increased consumption when their children's debt burden is relieved.

We provide several pieces of evidence supporting our empirical design. Our balance tests show that treated and control parents are similar on observable characteristics in the pre-treatment period, and dynamic treatment effect plots confirm parallel pre-trends across all outcome variables. To address any residual pre-treatment differences between treated and

control parents, we further augment our specification with fixed effects for pre-treatment bankruptcy status, auto delinquencies, and credit card balances, and find stable estimates. We find similar results both qualitatively and quantitatively when we exclude parents whose own student loans were subject to forbearance, confirming that the effects reflect intergenerational spillovers rather than direct treatment of parents. Our estimates are also robust to absorbing local economic conditions through region-by-quarter fixed effects, to measuring distress by whether parents newly enter delinquency rather than whether they are delinquent, and to alternative approaches to clustering.

This paper contributes to a large and growing literature on the consequences of student debt. This literature has documented wide-ranging effects of student loans on borrowers' own outcomes, including educational attainment and financial wellbeing (Black et al., 2023), human capital investment (Chakrabarti et al., 2023; Hampole, 2024), homeownership and household formation (Mezza et al., 2020; Goodman et al., 2021), career choice and labor supply (Field, 2009; Rothstein and Rouse, 2011; Hamdi et al., 2025)[4], consumption and labor income (Di Maggio et al., 2025), entrepreneurship (Krishnan and Wang, 2019), retirement savings (Batkeyev et al., 2024), financial distress and default (Dinerstein et al., 2024; Looney and Yannelis, 2022; Chava et al., 2023), and the design and distributional implications of repayment and forgiveness policies (Boutros et al., 2024; Catherine and Yannelis, 2023; Herbst, 2023; Mueller and Yannelis, 2022). A common feature of these studies is that they treat the borrower as an isolated economic agent. We contribute to this literature by showing that the effects of student debt relief extend beyond borrowers to their families, highlighting a new intergenerational dimension of these effects. Because the affected parents are predominantly approaching retirement age, these spillovers have particularly significant welfare implications — financial distress later in the life cycle has lasting implications for

[4]While Hamdi et al. (2025) find null and zero effects of student loan forbearance on labor market outcomes, other work including Di Maggio et al. (2025) shows that other forms of debt relief have significant impacts on labor market outcomes.

retirement security. Our results imply that evaluations of student debt relief programs that focus solely on borrower outcomes understate their total effects by roughly a third of the credit response they generate.

We also contribute to a literature on intergenerational financial linkages. A large body of work documents how parental wealth and resources shape children's economic outcomes (e.g., Becker and Tomes, 1986; Solon, 1992; Sacerdote, 2002; Charles and Hurst, 2003; Black et al., 2005; Engelhardt and Mayer, 2006; Lovenheim, 2011; Dahl and Lochner, 2012; Chetty et al., 2014; Bleakley and Ferrie, 2016; Hilger, 2016; Chetty and Hendren, 2018). More recently, Hamdi et al. (2024) show that parental bankruptcy protection improves children's long-run earnings, providing causal evidence that parental debt relief propagates across generations. We provide evidence on the complementary but previously undocumented channel: the financial costs that children's debt obligations impose on their parents. Our results indicate that these linkages are bilateral—parents who implicitly subsidize children's student loan burdens bear measurable financial costs, and the removal of those burdens generates significant improvements in parents' own financial outcomes.

2 Institutional Background

2.1 Federal Student Loan Programs

Student loans represent the second-largest category of household debt in the United States. The vast majority of these loans are federal student loans, whose interest rates and borrowing limits are set by Congress and do not vary with borrower-specific risk characteristics such as credit scores. Prior to 2010, the federal government administered two separate programs for disbursing these loans: the Direct Loan (DL) program and the Federal Family Education Loan (FFEL) program. Although both programs offered identical loan terms to borrowers, they differed in how the loans were funded and who ultimately held them.

Under the DL program, the federal government made loans directly and the Department of Education retained ownership. Under the FFEL program, private lenders originated the loans using their own capital, with the federal government serving as guarantor. Lenders could then either keep FFEL loans on their balance sheets or sell them to secondary market investors, including, in some cases, the Department of Education itself.

Which program funded a given borrower's loans was determined not by the borrower but by their college's financial aid office, which was responsible for choosing how to obtain the requested funds. In practice, financial aid offices typically selected one of the two programs without consulting the student, and most borrowers were likely unaware of whether their loans were being originated through DL or FFEL at the time of disbursement (Dinerstein et al. (2024);Hamdi et al. (2025)). This feature of the institutional environment is important for our identification strategy, as it implies that assignment to one program versus the other was largely orthogonal to borrower characteristics. In 2010, the Health Care and Education Reconciliation Act discontinued new lending under the FFEL program, directing all subsequent federal student loan originations through the DL program. However, FFEL loans originated before 2010 remained outstanding and continued to be serviced by their original holders.

2.2 The Student Loan Moratorium

As part of the CARES Act enacted in March 2020, the federal government placed all DL borrowers into forbearance, suspending their required payments and halting the accumulation of interest on their loans. The goal of this program was to provide economic relief to student loan borrowers during the COVID-19 pandemic. Every loan originated under the DL program was eligible for this forbearance. However, the government's legal authority over FFEL loans extended only to those that were federally owned—that is, FFEL loans that had been purchased by or transferred to the Department of Education. Privately held

FFEL loans, which constituted the majority of outstanding FFEL balances, were not covered by the moratorium. As of the first quarter of 2020, approximately 75% ($250 billion) of FFEL loans were privately owned, while the remaining 25% ($88 billion) were held by the Department of Education. The moratorium, initially set to expire in September 2020, was extended several times through executive action and ultimately remained in effect until September 2023.

The differential treatment of DL and FFEL borrowers under the moratorium provides our source of identifying variation. DL borrowers saw their payments drop to zero beginning in March 2020, while most FFEL borrowers—those whose loans remained in private hands—continued to owe their regular monthly payments. Because the distinction between DL and FFEL borrowers traces back to decisions made by college financial aid offices years or even decades earlier, rather than to any choice or characteristic of the borrowers themselves, this variation is plausibly not correlated to the outcomes we study. We exploit this setting as a natural experiment to compare the financial outcomes of parents whose children received forbearance (DL borrowers) to parents whose children did not (FFEL borrowers).

3 Data and Sample

3.1 Data Sources

We conduct our analysis using consumer credit histories from Equifax Inc. The data contain the credit histories of the entire United States population conditional on having a credit history. At the individual level, the data include information on credit scores, credit inquiries, and derogatory public records such as foreclosures and bankruptcies. At the credit account level, the data include information on account types, account balances, credit limits, original loan amounts, required and actual payments, and any missed or late payments. The data cover formal sources of credit such as credit cards, auto loans, mortgages, and student

loans.[5] Critically for our purposes, the student loan data allow us to distinguish between Direct Loan and FFEL borrowers based on the behavior of their required payments around the onset of the moratorium, following the classification approach in Dinerstein et al. (2024) and Hamdi et al. (2025).

The data also contain anonymized information on names, current and past addresses, and ages. We use these fields to link student loan borrowers to their parents within the same household, as described in detail below.

3.2 Identifying Households and Linking Generations

We identify parent-child relationships using the household matching procedure developed in Hamdi et al. (2024). Using last names and addresses—both current and historical—we classify individuals with the same last name who shared at least one mailing address over their address histories as belonging to the same household. The matching tracks up to five last names, allowing us to follow household members who may have changed their names (e.g., due to marriage), and up to ten historical addresses. The addresses are granular and include postal address information at the level of specific apartment or house numbers. Within each household, we classify individuals who are at least 20 years older than the student loan borrower as their parents.[6] When a child matches to two parents, both enter our sample as separate observations.[7] We drop households with more than five children, as these likely reflect nontraditional living arrangements such as group housing.

Because individuals enter our data only if they obtain formal credit, our sample is limited to children who report or use their parents' address on at least one credit account (e.g., a credit card, student loan, or auto loan) with the lender. This can occur in several ways. First,

[5] For more details about the consumer credit data, see Kalda (2020) and Gibbs et al. (2024).

[6] Our results are robust to choosing alternate age-difference cutoffs.

[7] We account for the resulting within-family correlation and weighting in robustness tests reported in Section 7

children residing with parents at the time of any credit application will use their residential address. Second, several lenders collect historical addresses at the time of application, so children who apply for credit within a certain duration after moving out of their parents' home (e.g., two or three years) may report the parental address as part of their historical addresses. Third, children can use their parents' address even if not residing with them—for example, students living in university dormitories who list their parents' home as their permanent address for credit accounts.

3.3 Sample Construction

We construct our sample in several steps. We begin by extracting the population of DL and FFEL borrowers in repayment from the consumer credit data. Following Hamdi et al. (2025), we require that borrowers originated their student loans between 1992 (the first year of the DL program) and 2010 (the last year of the FFEL program), and that they were actively repaying their loans during the pre-treatment period of Q1 2018 to Q4 2019.[8] We classify borrowers as treated (DL) if their average required student loan payments declined by more than 50% between the pre- and post-treatment periods, and classify the remaining borrowers as control (FFEL), following the approach in Dinerstein et al. (2024).

We restrict our attention to borrowers who were between the ages of 18 and 22 when the first student loan appeared on their credit profile, focusing on student debt borrowed to finance undergraduate education rather than graduate degrees. We then apply the household matching procedure described above to link these borrowers to their parents.

Our final sample consists of approximately 296,000 parents, of whom roughly 131,000 are in the treated group (parents of DL borrowers who received forbearance) and 165,000 are in the control group (parents of FFEL borrowers who did not). The panel spans the first

[8] Actively repaying borrowers are defined as those whose student loans had positive required payments and were not in deferral or forbearance between Q1 2018 and Q4 2019.

quarter of 2018 through the fourth quarter of 2022 at a quarterly frequency, covering eight quarters before and twelve quarters after the onset of the moratorium in March 2020.

3.4 Summary Statistics

Table 1 presents summary statistics for treated and control parents as of December 2019, one quarter before the onset of the moratorium. The average parent in our sample is approximately 66 years old, with an average credit score of 757. Treated parents are slightly younger (64 versus 67) and have slightly lower credit scores (750 versus 762). Mortgage balances are the largest component of household debt in our sample, averaging roughly $76,000. Installment delinquencies—the number of past-due occurrences on a parent's installment accounts—are low, averaging 0.036 across the sample, with treated parents modestly higher (0.044) than control parents (0.030). The bankruptcy flag rate is 3.1% overall. These differences, while modest, motivate our inclusion of fixed effects for parent age, children's age, credit score bins, and parental student loan indicator in all specifications.

Table A1 reports the corresponding summary statistics for children. The average child in our sample holds a student loan balance of approximately $20,800, with scheduled payments averaging $245 per month—the obligation whose suspension we study—and a credit score of 722. Treated children have lower credit scores, credit limits, and mortgage balances than control children, differences that echo those documented in Dinerstein et al. (2024). Our design does not require that treated and control children be comparable in levels; identification comes from within-parent variation over time, and we assess the identifying assumptions and further evaluate the comparability of treated and control parents directly in Section 4.2.

3.5 Sample Selection

As discussed earlier in this section, our sample construction requires that children report or use their parents' address on at least one credit account, which raises a potential sample selection concern. Children who use their parents' address—either because they share an address or use their parental address on application—may differ from other student loan borrowers, particularly in the strength of their financial relationships with their parents. If the address-based match disproportionately captures children who depend on their parents financially, our estimates would reflect intergenerational spillovers within a selected set of closely tied families rather than the broader population of student loan borrowers and their parents.

In this section, we assess this concern in two ways. First, the match does not require that children live with their parents. As described in Section 3.2, children enter our sample through historical addresses collected at the time of credit applications and through permanent or previous addresses reported while living elsewhere, in addition to current co-residence. Consistent with this, we find that only about one-third of matched children co-reside with their parents at the onset of the moratorium. Second, we directly compare the credit profiles of student loan borrowers who match to parents in our data against those who do not. Figure A1 plots the distributions of credit outcomes for matched and unmatched borrowers as of December 2019, one quarter before the moratorium, including the number of and balances on home loans, auto loans, credit cards, and student loans, as well as credit scores. If the address-based match disproportionately captured financially dependent children, matched borrowers should exhibit weaker credit profiles—lower credit scores, thinner credit histories, and less collateralized borrowing. Instead, the distributions largely overlap across all outcomes. Matched borrowers are slightly less likely to hold a mortgage than unmatched borrowers, but the differences are small, and their credit scores, card balances, and student loan positions are nearly indistinguishable.

We note that this selection concern applies to the external validity of our estimates rather than their internal validity. Because assignment to the DL and FFEL programs traces to decisions made by college financial aid offices years before the moratorium, treatment status is plausibly orthogonal to whether a borrower matches to a parent in our data. Our estimates therefore identify the causal effect of children's forbearance on parental outcomes for the matched population, and the evidence in Figure A1 indicates that this population resembles the broader population of student loan borrowers on observable credit characteristics.

4 Empirical Framework

4.1 Empirical Strategy

Our identification strategy exploits the differential treatment of Direct Loan and FFEL borrowers under the 2020 federal student loan moratorium. Because the moratorium paused payments only on federally held loans, borrowers with Direct Loans received automatic forbearance while otherwise similar borrowers with commercially held FFEL loans continued to make payments. We extend the identification strategy developed by Dinerstein et al. (2024) and subsequently used by Hamdi et al. (2025) — which compares outcomes across these two groups of borrowers — to study the parents of these borrowers, comparing parental outcomes before and after the moratorium's onset in March 2020.

We estimate the following difference-in-differences specification:

$$y_{i,t} = \beta \cdot Post_t \times Treated_i + \alpha_i + \delta_{s(i),t} + \gamma_{a^p(i),t} + \phi_{a^c(i),t} + \psi_{d(i),t} + f(SLBalance_{i,t}) + \varepsilon_{i,t} \quad (1)$$

where $y_{i,t}$ denotes the outcome variable for parent i in year-quarter t. $Treated_i$ is an indicator equal to one if the parent's child held Direct Loans subject to the moratorium, and $Post_t$ is an indicator equal to one for periods on or after March 2020. α_i denotes par-

ent fixed effects, which absorb all time-invariant differences across parents. $\delta_{s(i),t}$ denotes credit score decile-by-quarter fixed effects, which account for the possibility that parents at different points in the credit distribution may be differentially affected by time-varying macroeconomic conditions, including the COVID-19 recession. $\gamma_{a^p(i),t}$ and $\phi_{a^c(i),t}$ denote parent age-by-quarter and children's age-by-quarter fixed effects, respectively, which control for differential trends across cohorts—for example, older parents and parents of younger children may respond differently to aggregate economic shocks. $\psi_{d(i),t}$ denotes an indicator for whether the parent holds their own student loans, interacted with quarter fixed effects; this accounts for the possibility that parents whose own student loans were subject to the moratorium may have experienced direct effects on their financial outcomes that evolved differently over time. $f(SLBalance_{i,t})$ is a cubic polynomial in the parent's own student loan balance, which controls flexibly for differences in the size of the student debt burden across families.

The coefficient of interest is β, which captures the differential change in parental outcomes following the moratorium for treated parents (whose children received forbearance) relative to control parents (whose children did not). Under the parallel trends assumption—that outcomes for treated and control parents would have evolved similarly absent the moratorium—β identifies the causal effect of children's student loan forbearance on parental outcomes.

We double cluster standard errors at the parent level and by year-quarter to account for serial correlation within parents over time and common shocks across parents within the same period. In robustness checks, we also present results with alternative approaches to clustering and weighting.

4.2 Identifying Assumption and Validation

The key identifying assumption is that, absent the moratorium, outcomes for parents of DL borrowers and parents of FFEL borrowers would have followed parallel trends. While

this assumption is inherently untestable, we assess its plausibility in two ways.

First, we evaluate whether treated and control parents are similar in observables after conditioning for our baseline fixed effects and specification. We do so by regressing parental outcomes on the treatment indicator in the quarter immediately preceding the moratorium (October 2019 to December 2019). Table 2 presents these results. In the baseline specification (Panel A), the treatment indicator is statistically insignificant for the majority of outcomes, including installment balances, auto balances, mortgage balances, mortgage delinquencies, credit card delinquencies, mortgage trades, and mortgage payments, thereby suggesting that treated and control parents are similar across these observables once we condition on our baseline specification. However, we observe significant differences for auto delinquencies, credit card balances, and bankruptcy flag. To address these remaining differences, we augment the specification with pre-treatment auto delinquency, bankruptcy flag, and credit card balance bin fixed effects. Panel B shows that with these additional controls, the treatment indicator is statistically insignificant across all outcomes, confirming balance between the groups. We verify in Section 7 that our main results are robust to the inclusion of these additional fixed effects and hence not driven by any observable differences between treated and control parents.

Second, we estimate a dynamic version of our specification that replaces the single $Post_t \times Treated_i$ indicator with a series of interactions between the treatment indicator and quarterly dummies:

$$y_{i,t} = \sum_{q \neq -1} \beta_q \cdot \mathbf{1}[Quarter = q] \times Treated_i + \alpha_i + \delta_{s(i),t} + \gamma_{a^p(i),t} + \phi_{a^c(i),t} + \psi_{d(i),t} + f(SLBalance_{i,t}) + \varepsilon_{i,t} \tag{2}$$

where q indexes quarters relative to the onset of the moratorium in March 2020, and the quarter immediately preceding the moratorium ($q = -1$) is omitted as the reference period. The coefficients β_q trace out the differential trajectory of parental outcomes for treated

relative to control parents over time. This specification serves two purposes. First, the pre-treatment coefficients provide a test of whether treated and control parents were on similar trajectories prior to the moratorium. Second, the post-treatment coefficients show the dynamics of the effects—whether they emerge immediately or build gradually and whether these effects persist in short- or long-term.

5 Main Results

This section presents our main results. Section 5.1 estimates the first stage of our design, quantifying the payment relief that the moratorium delivered to treated children. Section 5.2 examines the effects of children's forbearance on parents' financial distress, and Section 5.3 examines the effects on parental borrowing and consumption.

5.1 First Stage: Children's Payment Relief

We begin by quantifying the first stage underlying our analysis: the payment relief that the moratorium delivered to children in our sample. Our intergenerational interpretation rests on the premise that treated children experienced a reduction in their required student loan payments, and the size of this reduction defines the cash-flow shock whose transmission to parents we study in subsequent sections. To estimate it, we re-estimate our difference-in-differences design at the child level, replacing parent fixed effects with child fixed effects and retaining quarter interactions with children's pre-treatment credit score deciles and age. We examine children's scheduled student loan payments and their student loan delinquencies as outcomes.

Table A2 presents the results. The moratorium generated substantial payment relief. Scheduled student loan payments for treated children decline by $163 per month relative to

control children (Column 1), against a pre-treatment mean of $225[9]—a reduction of roughly 70% of scheduled payments.[10] These magnitudes align closely with first-stage estimates for the borrower population, which range from $138 to $183 per month (Dinerstein et al., 2024; Hamdi et al., 2025), confirming that our payment-based classification identifies the same treatment in our sample of parent-linked borrowers. In annualized terms, forbearance freed approximately $1,950 per year in required payments per child—the cash-flow shock whose transmission to parents we trace in the remainder of the paper. Figure A2 traces the dynamics of this relief where we find that scheduled payments for treated children drop sharply at the onset of the moratorium and remain suppressed through the end of the sample period.

The payment relief also improved children's own repayment performance. Their student loan delinquencies decline by 0.010 occurrences following the moratorium—a 38% reduction relative to the control mean of 0.026 (Column 4)—consistent with the delinquency effects documented for the borrower population (Dinerstein et al., 2024). Because the policy paused payments for treated children, the effect operates through a reduction in new past-due occurrences among treated children relative to the control group. The remaining columns show a systematic asymmetry in treatment intensity across children's age. Older children, who face larger scheduled payments, experience greater payment relief than younger children ($179 versus $143 per month; Columns 2 and 3) and correspondingly larger reductions in delinquencies (Columns 5 and 6).

[9] $224.8 is the pooled average of all pre-shock periods, while $245 (reported in Table A1) is only for December 2019.

[10] The reduction falls short of 100% because some borrowers hold loans from both programs: treated borrowers with residual FFEL loans continued to owe partial payments, while some control borrowers with federally held loans received partial relief (Hamdi et al., 2025).

5.2 Student Loans and Parental Financial Distress

The central question is whether children's student loan forbearance meaningfully affects parental financial outcomes, and if so, in which direction. If parents implicitly subsidize their children's student loan burdens then forbearance should relax parents' budget constraints, reducing financial distress. However, if children in repayment are financially independent from their parents, the effects may be negligible regardless of the size of the relief. A third possibility, motivated by moral hazard models of debt relief (Lourie et al. (2023);Catherine and Yannelis (2025)), is that forbearance reduces children's incentive to maintain financial discipline or supply labor, increasing their dependence on parental support and potentially worsening parental outcomes. We begin by examining parental financial distress to evaluate these different plausibilities.

Table 3 presents these results. Column 1 reports the effect of children's student loan forbearance on parents' installment delinquencies. The coefficient on $Treated \times Post$ is -0.0017, statistically significant at the 5% level, indicating that parents of treated borrowers experience 0.0017 fewer past-due occurrences on their installment loans following the moratorium. Relative to the control group mean of 0.030, this represents a 5.7% reduction—an economically meaningful improvement in payment performance. Column 2 reports estimates for non-installment delinquencies but we find a small and insignificant coefficient. Column 3 turns to bankruptcy, a more severe measure of financial distress. We find that forbearance reduces the likelihood of parental bankruptcy by 0.18 percentage points, significant at the 1% level. Against a control mean of 0.028, this represents a 6.4% decline. That the effect extends to bankruptcy suggests that the burden children's student debt imposes on parents is not merely marginal but can push financially vulnerable parents toward severe distress.

Figure 1 presents dynamic treatment effect estimates from Equation (2), plotting the quarterly coefficients and 95% confidence intervals for each of our main outcome variables. The figure serves two purposes: validating the parallel trends in the pre-period and illus-

trating the dynamics of the treatment effects. Panel (a) plots the coefficients for installment delinquencies. We find that the pre-treatment coefficients are small and statistically indistinguishable from zero, confirming that treated and control parents were on similar trajectories prior to the moratorium. Installment delinquencies begin to decline shortly after March 2020, with the effect growing over time and remaining persistently negative through the end of the sample period. The gradual onset of the effect is consistent with the nature of the outcome—delinquencies accumulate over time, so the full reduction in financial distress materializes only as parents who would have otherwise missed payments avoid doing so over successive quarters.

At first blush, the effects on parents' installment delinquencies may appear surprising, especially given that Dinerstein et al. (2024) find that forbearance reduced borrowers' own student loan delinquencies but left their non-student delinquencies unchanged. This suggests that borrowers place student loans at the bottom of their payment hierarchy, perhaps because of the low costs of default. If a financially constrained borrower would not sacrifice other obligations to remain current on a student loan, it may seem unlikely that a parent would. However, the vast majority of children in our sample remained current on their student loans (i.e., lowest-priority obligation) and were therefore those whose budgets cleared, in many cases with parental help. Hence, the burden of servicing the debt, and the relief from its suspension, falls partly on parents rather than on borrowers alone. Further, while this hierarchy preference likely holds within the same individual, it may not necessarily translate across generations. Parents' exposure to children's student debt likely operates through the support they extend to their children. Altruistic parents internalize their children's wellbeing (Becker, 1974), and a large literature documents that this support is substantial in practice. For example, parents direct transfers toward children in need (McGarry and Schoeni, 1995), provide in-kind support through the family home (Kaplan, 2012), and adjust their own savings in response to children's economic risk (Boar, 2021). For an altruistic parent, a

child's needs may rank near the top of the payment hierarchy, even as the student loan ranks at the bottom of the child's.

Even when children's debt strains parents' budgets, ceteris paribus parents should be more likely to miss payments on unsecured debt before installment debt since the costs of default are lower on such credit. This may seem at odds with the results in Table 3. However, ceteris paribus here includes the amount required to pay different obligations. A choice among obligations only arises when forgoing one obligation frees enough cash to cover another. Remaining current on installment debt requires the full fixed payment—scheduled mortgage payments average $678 per month in our sample (Table 1)—whereas remaining current on a credit card requires only a small minimum payment. A parent facing a moderate cash shortfall therefore cannot cover the installment payment but covers the card minimum trivially: installment delinquency occurs without any choice among obligations ever arising, because forgoing the card would free too little cash to close the shortfall. Only when shortfalls grow large enough that even minimum payments compete for cash does a choice among obligations become possible.

The cost-of-default ordering provides a sharp prediction for parents already delinquent on their credit cards. Because unsecured credit carries the lowest cost of default, the card is among the first obligations forgone as shortfalls deepen, and existing card delinquency marks parents who have exhausted that margin—for whom any further deterioration must surface on installment debt. The cash flow freed by children's forbearance may allow these parents to avoid installment delinquencies. Table A3 splits the sample by whether parents were delinquent on their credit cards prior to the moratorium. Among parents with pre-treatment card delinquencies, installment delinquencies decline by 0.0197 occurrences following their children's forbearance, roughly fifteen times the decline of 0.0013 among the remaining parents—for whom the effect is smaller but not absent, consistent with the payment-size logic operating throughout the distribution of shortfalls.

Pre-existing distress predicts larger responses beyond this specific margin. Table A4 splits the sample by a broader marker of pre-existing distress—whether parents were delinquent on any of their non-student-loan debts prior to the moratorium. Among these ex-ante distressed parents, the responses to children's forbearance are uniformly larger: installment delinquencies decline by 0.0149 occurrences, compared with an economically small and insignificant estimate for the remaining parents, and installment and mortgage balances rise by $1,979 and $2,397, roughly three times the corresponding estimates among non-distressed parents—and larger still in proportional terms, given this group's smaller baseline balances. The concentration extends to the most severe outcome: bankruptcy declines by 0.61 percentage points among ex-ante distressed parents—a 7% reduction relative to this group's mean of 8.7%—compared with 0.16 percentage points among the remaining parents. This pattern suggests that the bankruptcy effect does not require that parents rank their children's student loans above their own solvency. For parents near the bankruptcy margin, children's debt was one strain among several, and the removal of that strain moved some parents back from the margin. The effect reflects a shift at the margin of severe distress among already-vulnerable parents, not a choice by any parent to preserve a child's loan standing at the cost of their own.

5.3 Student Loans and Parental Borrowing

The reduction in financial distress suggests that parents bear part of their children's student loan burden. A natural follow-up question is how parents use the additional financial slack created by forbearance. If the moratorium relaxes a binding budget constraint, parents may not only avoid distress but also increase consumption, which in our data may manifest as higher borrowing. Alternatively, parents may use the freed resources to deleverage—paying down existing debt rather than taking on new obligations. The direction of the balance response thus provides additional evidence on how tightly constrained parents are by their

children's debt burdens.

Table 4 reports the effects of children's forbearance on parents' outstanding balances across three major debt categories. Column 1 shows that parents' installment loan balances increase by approximately $754 following the onset of the moratorium, significant at the 1% level. Columns 2 and 3 decompose borrowing further: mortgage balances increase by $760 and HELOC balances by $150, both statistically significant.[11] The concentration of balance increases in secured, housing-related debt—mortgages and home equity lines—suggests that parents with available home equity use it to finance increased consumption when their children's debt burden is relieved. Table A5 examines parents' remaining balance categories. The responses are modest: credit card balances rise by $42.65, significant at the 5% level, auto balances decline marginally, and revolving and bankcard balances show no significant change—indicating that the borrowing response concentrates in secured and housing-related credit rather than reflecting a broad expansion across all margins.

To better understand the nature of these balance increases, Table 5 asks what kind of borrowing lies behind them. For instance, parents could be drawing on existing credit lines, opening new credit, or simply refinancing existing debt. For home equity lines, the response operates on both margins: the number of HELOC trades increases significantly (Column 2), indicating that parents open new lines, and the dollar amounts of new HELOC originations rise by $224 (Column 4). For mortgages, the response is concentrated on the intensive margin: the number of mortgage trades is unchanged (Column 1), while the dollar amounts of new mortgage originations rise by $896 (Column 3). Scheduled payments on both mortgages and HELOCs rise as well (Columns 5 and 6)—the opposite of what refinancing into lower payments would produce—indicating that parents take on new debt obligations rather than restructure existing ones.

[11] Installment balances comprise mortgage, auto, student loan, and other closed-end products. HELOCs are revolving credit lines and are not included in installment balances. Because each column reports a separate regression, component estimates need not sum exactly to the estimate for installment balances.

Panels (b) through (d) of Figure 1 plot the dynamic treatment effect estimates from Equation (2) for installment, mortgage, and HELOC balances. As before, we find no differential pre-trends followed by a steady divergence between treated and control parents after the moratorium. The balance effects grow progressively over the post-treatment period, consistent with parents gradually increasing their borrowing as the forbearance persists. Appendix Figure A3 shows the same pattern for HELOC trades and for mortgage and HELOC scheduled payments with no differential pre-trends, and effects that emerge after the moratorium and grow as forbearance persists. The sustained and increasing trajectory of these effects—rather than a sharp one-time jump—suggests that parents' consumption and borrowing responses compound over time as the continued absence of children's loan payments provides ongoing budget relief.

An alternative explanation for the increases in mortgage and HELOC balances is that parents are relocating—purchasing new homes or refinancing as part of a move—rather than borrowing against existing housing wealth to finance consumption. A related possibility is that parents' incomes rose, so that the balance increases reflect greater borrowing capacity rather than a response to the cash-flow relief. Table 6 tests both possibilities. Columns 1 and 2 measure mobility as whether parents relocate to a different ZIP code or ZIP3 area relative to their pre-treatment residence. Both coefficients are small, negative, and statistically insignificant, ruling out geographic mobility as a driver of the borrowing results. Columns 3 and 4 examine parents' annual income for the subsample of parents with available income data. The estimates are small relative to the mean and statistically insignificant, indicating that treated parents' incomes did not rise following their children's forbearance. The balance increases documented above thus reflect parents borrowing against their current homes out of the additional financial slack—consistent with a consumption response rather than a housing transition or an income effect.

A related question is why parents bear the fixed costs of originating HELOCs rather

than drawing on existing credit cards. One potential explanation can be that the parents who respond on this margin are those constrained by high credit card utilization. Table A6 evaluates this plausibility by splitting the sample based on parents' pre-treatment credit card utilization, defined as card balances over credit limits. Among parents with utilization above 75%, HELOC balances increase by $365 following their children's forbearance, roughly three times the $123 increase among the remaining parents. The contrast is nearly identical at an 80% threshold. Parents turn to home equity precisely where the unutilized capacity on cards is limited.

The magnitude of these borrowing responses can be summarized as a marginal propensity to borrow (MPB) out of forbearance, i.e., the additional net borrowing undertaken per dollar of payment relief. Because our balance estimates are pooled averages over the post-treatment period, we scale them by the corresponding average relief: cumulative suspended payments accrue at $163 per month, so their time average over our 34-month post-treatment period is approximately $2,850 per treated child. Against this relief, parents' installment, HELOC, and credit card balances rise by a combined $946 (Tables 4 and A5)—a parental MPB of approximately 0.33. Applying the same construction to the borrower-level estimates in Dinerstein et al. (2024), whose non-student debt balances rose by $1,408 against the average cumulative relief implied by their first stage, yields a borrower MPB of approximately 0.6. The household-level marginal propensity to borrow out of student loan forbearance is thus approximately 0.95—roughly one and a half times the borrower-only figure. Evaluations of debt moratoria that measure credit demand and outcomes on borrowers' balance sheets alone therefore substantially understate the total stimulus these policies generate.

Taken together, the results in this section establish that children's student loan forbearance generates significant intergenerational spillovers. The moratorium delivered a cash-flow shock of roughly $163 per month to treated children, and this relief transmitted to parents across both margins of their balance sheets: financial distress fell, with installment delin-

quencies and bankruptcy declining, while borrowing rose through mortgages, home equity lines, and credit cards. These effects emerged gradually after the onset of the moratorium, grew as forbearance persisted, and are not explained by geographic mobility or changes in parental income. At the household level, the combined borrowing response implies a marginal propensity to borrow out of forbearance well above borrower-only estimates. In the next section, we examine why these spillovers arise—which families transmit them, and through which channels.

6 Mechanism

The results so far show that children's student loan forbearance reduces parental financial distress and increases parental borrowing. In this section, we examine the mechanism underlying these spillovers—whether parents bear part of their children's student loan burden through the support they extend to their children. We conduct four sets of tests. First, we examine whether the effects are present for parents whose children experienced no change in cash flows under the moratorium. Second, we test for heterogeneity in our findings based on the strength of financial ties across generations. Third, we examine heterogeneity based on children's own financial conditions. Fourth, we examine how the response varies with parents' financial conditions, and in particular whether distress reductions and borrowing increases arise among the same group of parents.

6.1 The Cash-Flow Channel

Forbearance provided more than a suspension of payments: treated borrowers also had their student loan delinquencies cured and their credit standing improved, so the parental effects we document could in principle reflect more than the cash-flow relief. To help isolate the two components we evaluate the heterogeneity in the effects based on whether children

received any cash-flow benefits. We identify children in the bottom decile of actual student loan payments prior to the moratorium, whose average payment of $64 per month is roughly one-quarter of the sample average. Our heterogeneity analysis compares the effects on parents of children in the bottom decile of actual payments to parents of all remaining children.

Table A7 presents the results. Among parents of children in the bottom decile, the estimates are small and statistically insignificant across all outcomes. Among the remaining sample, the effects mirror the baseline with installment delinquencies declining by 0.002 and installment and mortgage balances rising by $703 and $688, respectively. The contrast indicates that the spillovers operate through the cash-flow relief itself rather than through the other benefits forbearance conferred. Where the moratorium cured children's delinquencies and improved their credit standing but freed little cash flows, parental outcomes are unchanged—in presence of low cash-flow relief, there is no spillover to transmit.

6.2 Intergenerational Financial Ties

If the spillovers reflect support flowing between generations, they should be stronger in families where financial ties between parents and children are more active. Families vary in the degree to which their finances are integrated: at one extreme, parents and children maintain fully separate budgets; at the other, they effectively pool resources through transfers, shared housing, and covered expenses. The support parents extend is the conduit through which children's debt burdens reach the parental balance sheet, and the degree of integration governs how much of the child's obligation—or of the relief from its suspension—passes through to the parent's budget. We estimate the heterogeneity in our findings based on the strength of these financial ties using three proxies: whether the child co-resides with the parent, whether the parent borrowed directly to finance the child's education, and the child's age at the time of the moratorium.

We begin by estimating heterogeneity in our findings based on whether parents and children co-reside one quarter before treatment. Panel A of Table 7 reports the results. The contrast is striking. Among co-residing families (Columns 1, 3, and 5), the effects are large and significant across all outcomes: installment delinquencies decline by 0.004, and installment and mortgage balances increase by approximately $1,229 each. Among non-co-residing families (Columns 2, 4, and 6), the effects are substantially attenuated—the delinquency reduction is 0.0006 and statistically insignificant, while balance increases are roughly half the size and marginally significant. The installment delinquency effect is over six times larger for co-residing families, consistent with the prediction that financial spillovers are strongest where parents and children are more likely to share financial ties.

Next, we evaluate heterogeneity based on whether the parent borrowed directly to finance the child's education. Under the Parent PLUS program, parents take out federal loans in their own names to finance their children's undergraduate education, creating a contractual obligation rather than an inferred tie. Panel B of Table 7 splits the sample by whether the parent held a PLUS loan. Among PLUS-borrowing parents, installment delinquencies decline by 0.0084 following their children's forbearance—an 11% reduction relative to this group's mean—compared to a 0.0006 decline among the remaining parents. Balance increases appear in both groups, with somewhat larger point estimates among PLUS-borrowing parents.

Finally, we evaluate heterogeneity by children's age, splitting the sample at the median. Younger children are more likely to be financially dependent on their parents—they are earlier in their careers, have had less time to accumulate savings, and are more likely to rely on parental support for housing and other expenses. Panel C of Table 7 reports the results. The delinquency reduction is concentrated among parents of younger children: the coefficient is -0.0035, significant at the 5% level (Column 1), compared to -0.0010 for parents of older children (Column 2)—the pattern consistent with dependence declining in age. The balance effects run in the opposite direction, with point estimates somewhat larger for

parents of older children (Columns 4 and 6), though the differences are modest. The contrast in balance effects may partly reflect treatment intensity: older children face larger scheduled payments and received correspondingly larger relief (Table A2), so borrowing responses may track the size of the cash-flow shock even as distress responses track dependence. Appendix Figures A4 and A5 show no differential pre-trends within either subsample, and Table A8 confirms that neither mobility nor income responses differ across the groups. We interpret the heterogeneity in our baseline results by age with caution. Based on our sample construction, older children in our sample have carried their student debt for up to two decades, and borrowers who remain in repayment this long are negatively selected on repayment capacity. Children's age thus confounds life-cycle dependence with selection into prolonged repayment, making it a coarser proxy for financial ties than co-residence or direct parental borrowing.

Across these proxies, the evidence suggests that the effects of children's forbearance concentrate where financial ties between generations are stronger, consistent with the support parents extend serving as the conduit through which children's debt burdens, and relief from them, reach the parental balance sheet.

6.3 Children's Financial Conditions

If parents extend support in response to their children's needs, the burden that children's student debt imposes on parents should be greatest where children are least able to manage their obligations on their own—and the relief from forbearance should follow the same gradient. We therefore evaluate heterogeneity in our findings based on children's own financial conditions. We use three measures of children's financial constraints: their credit scores, their income, and whether they were delinquent on their non-student-loan debts prior to the moratorium.

Panels A and B of Table 8 present the results for children's credit scores and income, respectively. The pattern is similar across both measures. Among parents of children with

subprime credit scores (below 660), installment delinquencies decline by 0.0026, compared to a 0.0009 decline among parents of higher-score children. The income split, available for the subsample of children with income data, shows the same concentration: delinquency reductions of 0.0045 among parents of lower-income children versus a 0.0027 decline for the remainder. The balance responses follow the same gradient—installment and mortgage balances rise significantly among parents of lower-score children, by $985 and $972 respectively, while the corresponding estimates for parents of higher-score children are roughly half the size and insignificant. Where children are less able to carry their obligations, both margins of the parental response are larger.

Panel C examines heterogeneity based on whether children were delinquent on their non-student-loan debts prior to the moratorium. These children were making their student loan payments—the cash-flow shock from forbearance was therefore present—while their delinquencies on other obligations captures financial strain, identifying children more likely to be leaning on parental support. Consistent with this, the effects are largest in these families: installment and mortgage balances rise by $1,327 and $1,502, respectively, among parents of delinquent children, roughly twice the corresponding estimates among the remaining families. The delinquency reduction shows a similar contrast in point estimates—0.0044 versus 0.0014—though the estimate for the smaller delinquent-child subsample is imprecise.

Across all three measures, the spillovers concentrate where children's financial need is greatest. This pattern also speaks to the direction of support within the family: transfers from children to parents would require children with the financial capacity to make them, and would therefore concentrate among parents of financially stronger children. That the effects instead concentrate where children are least able to support anyone—themselves included—is consistent with relief flowing to parents through a reduction in the support they extend, rather than through transfers they receive.

6.4 Parental Financial Conditions

The results above show that spillovers are strongest in families with tighter intergenerational financial ties. We now examine how the nature of the parental response varies with parents' own financial conditions. Parents who are financially constrained are closer to the margin of distress because of the implicit burden of their children's debt, and forbearance should primarily reduce their delinquencies and bankruptcy risk. Less constrained parents, by contrast, are unlikely to be pushed into distress by their children's obligations but may nonetheless be subsidizing them at the expense of their own consumption. For these parents, forbearance should primarily free resources for increased borrowing and spending. We test these predictions using two proxies for parental financial conditions: homeownership and credit scores.

Panel A of Table 9 splits the sample by whether the parent is a homeowner. Homeowning parents are, on average, wealthier and have access to housing equity as a borrowing channel. Consistent with this, the borrowing response is concentrated entirely among homeowners: installment and mortgage balances increase by approximately $957 and $966, respectively (Columns 3 and 5), both significant at the 5% level. Among non-homeowners, the corresponding estimates are $68 and $80, economically small and statistically insignificant. The delinquency reduction, by contrast, is similar in magnitude across both groups (Columns 1 and 2), though only the homeowner coefficient is marginally significant—likely reflecting the smaller sample size and higher variance among non-homeowners rather than a true difference in the distress channel.

Panel B provides a sharper test by splitting parents into those with subprime credit scores (below 660) and those with non-subprime credit scores (above 660). The distress and borrowing channels separate cleanly along this dimension. Among parents with subprime credit scores, the delinquency reduction is large—0.011 occurrences, significant at the 5% level (Column 1)—consistent with these parents being closest to the margin of financial dis-

tress. Among parents with non-subprime credit scores, the delinquency effect is economically small and statistically insignificant (Column 2). The borrowing response runs in the opposite direction. Installment and mortgage balance increases are statistically significant for parents with non-subprime credit scores (Columns 4 and 6), while the corresponding estimates for subprime-credit parents are similar in magnitude but imprecisely estimated.

Taken together, the results in this section support the interpretation that parents bear part of their children's student loan burden through the support they extend. The spillovers arise only where forbearance freed actual resources; they are strongest where financial ties between generations are most active—in co-residing families, among parents of financially dependent children, and among parents who borrowed directly for their children's education; and they concentrate among parents of children least able to manage their obligations on their own, consistent with support flowing from parents to children rather than the reverse. Parents' own financial conditions then determine the form the relief takes: parents near the margin of distress experience fewer delinquencies and bankruptcies, while parents with stronger balance sheets channel the freed resources into borrowing and consumption. The costs of children's student debt are thus borne within the family in proportion to how tightly its finances are linked—and relief from that debt propagates along the same lines.

7 Robustness

In this section, we assess the robustness of our main results to the choices underlying our baseline specification and to potential threats to our interpretation.

7.1 Parents' Own Student Loans

Parents' own forbearance: Parents are themselves student loan borrowers in roughly 22% of our sample. A natural concern is that the moratorium that paused children's pay-

ments may have paused their parents' payments as well. This could contribute to the effects we document rather than intergenerational spillovers from children's forbearance. Our baseline specification accounts for this through an indicator for whether the parent holds student loans interacted with quarter. In addition, we include cubic controls for parents' student loans themselves. However, these controls distinguish parents by whether they hold student loans or balances on their loans rather than by whether those loans were forborne. Hence, they do not separate parents who received relief on their own loans from those who did not.

However, only 5.6% of parents in our sample had their own student loans subject to forbearance, which limits the scope for direct effects on parents to account for our results. We nonetheless evaluate this concern in two different ways. First, we re-estimate our main specifications on the subsample of parents whose own loans were not forborne, i.e., we drop the observations where parents received forbearance. We report these results in Panel A of Table 10. Installment delinquencies decline by 0.0015 and bankruptcy by 0.18 percentage points, while installment and mortgage balances increase by $910 each. The delinquency and bankruptcy estimates are close to their baseline counterparts, while the balance estimates are somewhat larger. Figure 2 plots the corresponding dynamic estimates and shows no differential pre-trends, with post-treatment effects that emerge and grow as in the baseline.

Second, we augment our specification with pre-treatment student loan balance decile-by-quarter fixed effects interacted with an indicator for whether the parents' own loans were originated under the DL program, thereby absorbing differential trends by both the size of parents' own student debt and its treatment status under the moratorium. We report these results in Panel B of Table 10. Installment delinquencies decline by 0.0013 and bankruptcy by 0.18 percentage points, while installment and mortgage balances increase by $868 and $739, respectively, all retaining statistical significance. Overall, our results are not driven by relief that parents received on their own student loans.

Parents' student loans in outcomes: A related concern is that parents' own student loans enter our outcome measures directly. Student loans are an installment product, so a parent whose own loans were forborne would have had any existing delinquencies on those loans cured, mechanically reducing our measure of installment delinquencies without any support flowing between generations. We evaluate this concern by excluding observations in which parents were delinquent on their own student loans and report these results in Panel C of Table 10. Installment delinquencies decline by 0.0014 and bankruptcy by 0.18 percentage points, while installment and mortgage balances increase by \$759 and \$762, respectively. These estimates are close to their baseline counterparts across all outcomes. The restriction removes only 0.17% of our observations, indicating that parental student loan delinquencies are too rare in our sample to account for the effects we document.

Parents' student loans in controls: Our baseline specification accounts for parents' own student loan exposure through an indicator for holding student loans interacted with quarter, along with a cubic control in their student loan balances. A potential concern is that these controls may themselves get affected by the moratorium. The relief we document relaxes parents' budget constraints, and parents may respond along their own student loan margin—repaying their balances more aggressively as resources free up, or taking on new educational debt. Conditioning on a variable that treatment itself moves can bias our estimates. We evaluate this concern by replacing both controls with pre-treatment student loan balance decile-by-quarter fixed effects, which compare parents holding similar student loan positions at the onset of the moratorium and allow their outcomes to trend differentially over time. We report these results in Panel D of Table 10. Installment delinquencies decline by 0.0018 and bankruptcy by 0.18 percentage points, while installment and mortgage balances increase by \$904 and \$769, respectively. These estimates are close to their baseline counterparts across all outcomes, with the installment balance effect somewhat larger. Overall, our results do

not depend on how we condition on parents' own student loan balances, and the effects we document are not an artifact of controls that the moratorium may itself have influenced.

7.2 Pre-treatment Differences & Local Economic Conditions

Residual pre-treatment differences: Our balance tests in Section 4.2 document pre-treatment differences between treated and control parents in auto delinquencies, credit card balances, and bankruptcy status. A potential concern is that these differences, while modest, may drive the results we observe. We evaluate this concern by augmenting our baseline specification with pre-treatment auto delinquency-by-quarter, bankruptcy flag-by-quarter, and credit card balance bin-by-quarter fixed effects—resulting in the same controls that eliminate the differences in Panel B of Table 2. We report these results in Panel A of Table 11. Installment delinquencies decline by 0.0015 and bankruptcy by 0.12 percentage points, while installment and mortgage balances increase by \$707 and \$713, respectively. The delinquency and balance estimates are close to their baseline counterparts, and the bankruptcy estimate attenuates by roughly a third while remaining significant at the 5% level. Figure 3 plots the corresponding dynamic estimates and shows no differential pre-trends, with post-treatment effects that emerge and grow as in the baseline. Overall, our results are robust to including these additional fixed effects and differences in pre-treatment characteristics between treated and control parents thus do not account for the effects we document.

Local economic conditions & house prices: Yet another potential concern may be that local economic conditions can differentially affect treated and control parents in ways correlated to the moratorium. This can happen because our sample period coincides with the COVID-19 pandemic, which affected different regions differently along a number of dimensions including public health conditions, labor market outcomes, and house price growth.

We evaluate this concern by augmenting our baseline specification with 3-digit zip code-by-quarter fixed effects, which restrict comparisons to parents residing in the same region in the same period. A 3-digit zip code is usually more granular than an MSA though their geographic boundaries do not overlap perfectly.[12] We report these results in Panel B of Table 11. Installment delinquencies decline by 0.002 and bankruptcy by 0.09 percentage points, while installment and mortgage balances increase by $735 and $745, respectively. The delinquency and installment and mortgage balance estimates are close to their baseline counterparts, while the bankruptcy estimate halves.

Differential exposure to house prices and growth among treated and control borrowers can allow for differences in borrowing capacity. If treated parents reside in areas with faster house price appreciation, the increases in mortgage and HELOC borrowing we document could reflect greater home equity extraction driven by local price growth rather than a response to children's debt relief. We evaluate this concern by augmenting our baseline specification with the lagged home value index of the parents' residential 5-digit ZIP code, measured using the Zillow Home Value Index. We report these results in Table A9. Our estimates essentially remain unchanged. Installment delinquencies decline by 0.0018, and installment and mortgage balances increase by $717 and $722, respectively, all retaining statistical significance. Overall, our results are robust to controlling flexibly for local economic conditions and house prices.

7.3 Alternate Clustering & Weights

Alternate clustering: Our baseline specification clusters standard errors at the parent and quarter level, accounting for serial correlation within parents over time and for common shocks across parents within a period. A potential concern is that errors are correlated across parents along dimensions that neither of these levels captures. We evaluate this

[12] There are 875 3-digit zip codes in the USA compared to 393 MSAs.

concern using two alternative clustering choices and report the results in panels A and B of Table A10. The first clusters on parents' pre-treatment credit score deciles, allowing for correlated shocks among parents at similar points in the credit distribution. The second clusters on the year in which children originated their student loans, which is the level that affects the assignment to the DL and FFEL programs.

We then consider two additional alternatives that address structural dependence across observations and report the results in panels C and D of Table A10. The first clusters on parents' state of residence, allowing errors to be correlated among parents exposed to the same local economic conditions. The second clusters on families, defined by the unique combination of children linked to a set of parents. Because both parents of the same child share that child's treatment status, their outcomes are mechanically correlated, and clustering at the student level (i.e., everyone associated with the same student) accounts for the presence of multiple parents per family in our sample. Because the level of clustering affects only the estimated standard errors, the point estimates are identical to those in our baseline. Standard errors change in both directions across outcomes, and all of our main results retain statistical significance under all four alternatives. Overall, our inference does not depend on the level at which we cluster.

Alternate weights: A related consequence of our sample construction is that students who match with two parents receive twice the weight of families that match to a single-parent in our baseline estimates. As discussed earlier, both parents enter our sample as separate observations when matched to a child. To evaluate whether this implicit weighting affects our results, we re-estimate our baseline specification assigning each parent a weight equal to the inverse of the number of parents in their family, so that every family contributes equally to the estimates. We report these results in Table A11. Installment delinquencies decline by 0.0022 and bankruptcy by 0.19 percentage points, while installment and mortgage

balances increase by $612 and $620, respectively, all retaining statistical significance. The delinquency estimates are close to their baseline counterparts, while the balance estimates are somewhat smaller. Overall, our results are not an artifact of the implicit weighting of multi-parent families.

7.4 Alternate Delinquency Measures

Our baseline delinquency outcomes are stock measures that record the number of delinquency occurrences on a given obligation in a given quarter. A reduction in these measures can arise in two ways. Fewer parents may enter delinquency, or parents who were already delinquent may resolve their existing delinquencies. Stock measures also persist: a single episode of delinquency contributes to the outcome for as long as the account remains past due, so a decline in the stock may reflect the resolution of a small number of long-standing delinquencies rather than a broad reduction in new distress. Our baseline estimates therefore do not distinguish between parents avoiding delinquency and parents curing delinquencies they had already incurred.

We evaluate this concern using measures of new delinquencies, which record the number of new delinquency occurrences on debts for which parents were current in the preceding quarter. We report these results in Table A12, alongside our baseline stock measures for comparison. New installment delinquencies decline by 0.00031, significant at the 5% level. Relative to the control mean, this represents an 8.6% reduction, larger in proportional terms than the 5.7% reduction we estimate for the stock measure. New non-installment and new credit card delinquencies are both economically small and statistically insignificant, mirroring the pattern in our baseline estimates. Overall, the reduction in parental financial distress reflects fewer parents entering delinquency rather than the resolution of delinquencies parents had already incurred.

8 Conclusion

Student loan debt has grown to become the second-largest category of household debt in the United States, and the question of how to address this burden has been a persistent policy debate. A large literature documents the effects of student debt on borrowers' own financial outcomes, but borrowers do not exist in financial isolation — families are interconnected, and the consequences of one member's debt can propagate to others. In this paper, we examine whether and how children's student loan burdens spill over to their parents. Using consumer credit histories that link student loan borrowers to their parents, we exploit the 2020 federal student loan moratorium—which paused payments for Direct Loan borrowers while leaving otherwise similar FFEL borrowers unaffected—as a natural experiment to estimate the effect of children's debt relief on parental financial outcomes.

We find significant intergenerational spillovers. The moratorium reduced treated children's scheduled student loan payments by \$163 per month, roughly 70% of what they owed beforehand, and this relief reached their parents across both margins of the balance sheet. Parents of treated borrowers experience a 5.7% reduction in installment delinquencies and a 6.4% decline in bankruptcy relative to control parents, while increasing their mortgage, installment, and home equity balances by \$760, \$754, and \$150, respectively. The increases in borrowing reflect both new originations and draws on existing credit lines, and are driven neither by geographic mobility nor by changes in parental income. Dynamic treatment effect estimates confirm no differential pre-trends and show effects that emerge after the moratorium and grow steadily over the post-treatment period. Scaled against the relief that generated them, these responses imply that parents borrow roughly \$0.33 for every dollar their children receive—so that the response measured across both generations is roughly one and a half times what borrowers' balance sheets alone would suggest.

Our heterogeneity analyses support an intergenerational mechanism in which parents

bear part of their children's student loan burden. The spillovers arise only where forbearance freed actual resources. Among children who were making lower payments before the moratorium, and who therefore received the same improvement in credit standing with little cash-flow relief, parental outcomes are unchanged. They are strongest where financial ties between generations are stronger—the reduction in installment delinquencies is over six times larger among co-residing families, and larger still among parents who borrowed directly for their children's education through the Parent PLUS program. They also concentrate among parents of children least able to manage their obligations on their own, whether measured by credit scores, income, or pre-existing delinquencies on non-student debt. That the effects are largest where children have the least capacity to support anyone is consistent with relief reaching parents through a reduction in the support they extend, rather than through transfers they receive. Parents' own financial conditions then determine the form the relief takes. Constrained parents with lower credit scores benefit primarily through reduced distress, while less constrained homeowning parents with higher credit scores channel the freed resources into borrowing and consumption.

Our results have important implications for evaluating student debt relief policies. Conventional analyses focus exclusively on borrower outcomes, but our findings demonstrate that the costs of student debt—and correspondingly, the benefits of relief—extend beyond borrowers to their families. Because the affected parents are older and near or past retirement in several cases, the financial distress imposed by children's debt carries particularly significant welfare consequences. Missed payments and bankruptcy late in the life cycle have lasting implications for retirement security, and parents at this stage have limited capacity to recover from distress through future earnings. More broadly, our results contribute to the understanding of intergenerational financial linkages as bilateral. While a large body of work documents how parental resources shape children's outcomes, we show that children's financial obligations imposing costs on parents is economically important and policy-relevant.

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Table 1:
Summary Statistics for Parents

This table reports summary statistics for parents in the sample as of December 2019, one quarter prior to treatment, separately for the treated and control groups.

	All	Treated	Control	Difference
Variable	Mean	Mean	Mean	Treated − Control
Children's Age	37.48	35.38	39.14	-3.75
Parents' Age	65.98	64.13	67.44	-3.30
Installment Delinquencies	0.036	0.044	0.030	0.014
Non-Installment Delinquencies	0.089	0.100	0.081	0.019
Installment Balance	90,287.51	96,366.15	85,468.59	10,897.56
Mortgage Balance	75,867.29	80,362.80	72,303.41	8,059.39
HELOC Balance	5,589.13	5,543.62	5,625.23	-81.61
Auto Balance	7,412.41	8,042.46	6,912.93	1,129.53
Credit Card Balance	5,782.54	6,028.59	5,587.42	441.16
Student Loan Balance	3,792.52	4,433.93	3,284.03	1,149.89
Bankcard Balance	5,884.18	6,163.93	5,665.42	498.51
Revolving Balance	13,193.99	13,399.25	13,032.64	366.61
Mortgage Trades	0.49	0.52	0.47	0.05
HELOC Trades	0.15	0.15	0.16	-0.01
Auto Trades	0.54	0.58	0.51	0.07
Credit Card Trades	4.31	4.29	4.32	-0.04
Student Loan Trades	0.27	0.32	0.23	0.09
Mortgage Scheduled Payments	678.05	719.75	644.99	74.77
Mortgage Actual Payments	695.60	736.32	663.31	73.01
HELOC Scheduled Payments	40.84	40.30	41.28	-0.98
HELOC Actual Payments	67.83	67.06	68.45	-1.39
Student Loan Scheduled Payments	30.44	34.86	26.94	7.92
Student Loan Actual Payments	28.19	31.84	25.29	6.55
Credit Score	756.81	750.35	761.93	-11.59
D(Bankruptcy)	0.031	0.036	0.028	0.008
D(Foreclosure)	0.003	0.004	0.003	0.001

Table 2:
Balance Test

This table shows regression results between an indicator of treated parents and their credit outcomes in the quarter prior to treatment, i.e., December 2019. Panel A shows the test with baseline fixed effects. Panel B includes additional fixed effects. Standard errors, corrected for heteroskedasticity, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Baseline Fixed Effects

Dependent Variables:	Installment Delinquencies	Mortgage Delinquencies	Auto Delinquencies	Card Delinquencies	Installment Balances	Auto Balances	Mortgage Balances	Card Balances	Mortgage Trades	Mortgage Actual Payments	Bankruptcy Flag	Foreclosure Flag
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	0.0019**	1.01×10^{-6}	0.0015***	0.0003	-521.9	-16.88	-182.1	-246.8***	0.0013	-8.237*	0.0020***	9.48×10^{-5}
	(0.0009)	(0.0002)	(0.0004)	(0.0006)	(522.5)	(49.39)	(500.3)	(32.99)	(0.0024)	(4.475)	(0.0007)	(0.0002)
Fixed Effects:												
Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parents Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Children Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	295,951	294,179	294,179	294,179	295,951	294,179	294,179	294,179	295,951	294,116	295,951	295,951
R^2	0.111	0.014	0.041	0.163	0.058	0.054	0.036	0.197	0.053	0.034	0.057	0.007

Panel B: Additional Fixed Effects

Dependent Variables:	Installment Delinquencies	Mortgage Delinquencies	Card Delinquencies	Installment Balances	Auto Balances	Mortgage Balances	Card Balances	Mortgage Trades	Mortgage Actual Payments
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated	0.0012	-1.46×10^{-5}	0.0003	-119.6	-10.41	173.6	-33.05	0.0032	-4.936
	(0.0009)	(0.0002)	(0.0006)	(518.5)	(49.11)	(497.1)	(20.10)	(0.0024)	(4.441)
Fixed Effects:									
Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parents Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Children Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
D(Auto Delinquency)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
D(Bankruptcy)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Card Balances Bins	Yes	Yes	Yes	Yes	Yes	Yes		Yes	Yes
Observations	295,951	294,179	294,179	295,951	294,179	294,179	294,179	295,951	294,116
R^2	0.128	0.014	0.165	0.072	0.065	0.048	0.616	0.069	0.048

Table 3:
Installment Delinquencies

This table reports regression results on the effects of children's student loan forbearance on parents' installment delinquency outcomes. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Installment Delinquencies (1)	Non-installment Delinquencies (2)	D(Bankruptcy) (3)
Treated × Post	-0.0017**	0.0020	-0.0018***
	(0.0008)	(0.0016)	(0.0005)
Fixed Effects:			
Parents	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes
Controls:			
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes
Observations	5,783,388	5,783,388	5,783,388
R^2	0.518	0.487	0.771

Table 4:
Balances

This table reports regression results on the effects of children's student loan forbearance on parents' balance outcomes. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Installment Balances (1)	Mortgage Balances (2)	HELOC Balances (3)
Treated × Post	753.7***	760.4***	149.5**
	(262.5)	(261.7)	(56.50)
Fixed Effects:			
Parents	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes
Controls:			
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes
Observations	5,783,388	5,783,388	5,748,743
R^2	0.854	0.845	0.739

Table 5:
Additional Home Loan Outcomes

This table reports regression results on the effects of children's student loan forbearance on additional parents' home loan outcomes. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Mortgage Trades	HELOC Trades	Mortgage Amounts	HELOC Amounts	Mortgage Scheduled Payments	HELOC Scheduled Payments	Mortgage Actual Payments	HELOC Actual Payments
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post	0.0020	0.0026***	895.6***	223.7***	6.112***	2.176***	5.079**	1.620**
	(0.0013)	(0.0009)	(300.8)	(74.28)	(2.093)	(0.4253)	(2.394)	(0.6850)
Fixed Effects:								
Parents	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Pre-Treatment D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls:								
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,783,388	5,748,743	5,748,723	5,748,742	5,748,605	5,744,599	5,747,612	5,743,582
R^2	0.838	0.813	0.852	0.797	0.841	0.731	0.733	0.588

Table 6:
Geographical Mobility and Income

This table reports regression results on the effects of children's student loan forbearance on parents' mobility and income. Dependent variables indicate relocation to a different ZIP code or ZIP3 relative to pre-treatment residence in the first two columns, and annual income and its log transformation in last two columns. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	D(Move out of ZIP code) (1)	D(Move out of ZIP3) (2)	Annual Income (3)	Log Annual Income (4)
Treated × Post	-0.0003	-0.0003	-410.50	-0.00497
	(0.0003)	(0.0003)	(368.65)	(0.00301)
Fixed Effects:				
Parents	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes
Controls:				
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes
Observations	5,740,518	5,740,518	598,255	598,255
R^2	0.134	0.126	0.920	0.935

Table 7:
Heterogeneity by Family Ties

This table reports the heterogeneity in the baseline findings based on differences in financial ties using three proxies: co-residence (panel A), parent PLUS loans (panel B), and children's age (panel C). All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Co-residence

Dependent Variables:	Installment Delinquencies		Installment Balances		Mortgage Balances	
Co-reside:	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.0040**	-0.0006	1,228.9**	572.3*	1,229.3**	577.6*
	(0.0015)	(0.0009)	(470.5)	(319.3)	(461.9)	(320.0)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,977,469	3,805,919	1,977,469	3,805,919	1,977,469	3,805,919
R^2	0.522	0.515	0.860	0.849	0.852	0.840

Panel B: Parent PLUS Loans

Dependent Variables:	Installment Delinquencies		Installment Balances		Mortgage Balances	
Taken PLUS Loan:	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.0084***	-0.0006	820.5	717.5**	795.0	745.4**
	(0.0025)	(0.0007)	(598.0)	(296.3)	(573.4)	(292.2)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,307,879	4,475,468	1,307,879	4,475,468	1,307,879	4,475,468
R^2	0.548	0.494	0.857	0.851	0.843	0.846

Panel C: Children's Age

Dependent Variables:	Installment Delinquencies		Installment Balances		Mortgage Balances	
Children's Age:	Young	Old	Young	Old	Young	Old
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.0035**	-0.0010	691.4	929.3**	710.1*	908.9**
	(0.0014)	(0.0009)	(401.2)	(326.8)	(394.3)	(324.6)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,718,508	3,064,880	2,718,508	3,064,880	2,718,508	3,064,880
R^2	0.524	0.505	0.846	0.860	0.835	0.854

Table 8:
Heterogeneity by Children's Financial Constraints

This table reports the heterogeneity in the baseline findings based on children's financial constraints using three proxies: credit score (panel A), income (panel B), and ex-ante non-student loan delinquencies (panel C). All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Children's Credit Score

Dependent Variables:	Installment Delinquencies		Installment Balances		Mortgage Balances	
Children Credit Score:	Subprime	Non-Subprime	Subprime	Non-Subprime	Subprime	Non-Subprime
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.0026*	-0.0009	985.2**	414.0	972.3**	415.4
	(0.0013)	(0.0009)	(348.4)	(379.9)	(343.9)	(375.4)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,820,017	2,811,722	2,820,017	2,811,722	2,820,017	2,811,722
R^2	0.520	0.511	0.853	0.856	0.843	0.848

Panel B: Children's Income

Dependent Variables:	Installment Delinquencies		Installment Balances		Mortgage Balances	
Children Income:	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.0045**	-0.0027	760.9	218.3	857.3	222.3
	(0.0021)	(0.0020)	(560.7)	(701.2)	(562.5)	(687.1)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	925,307	924,729	925,307	924,729	925,307	924,729
R^2	0.522	0.526	0.852	0.855	0.842	0.846

Panel C: Children's Non-Student Loan Delinquencies

Dependent Variables:	Installment Delinquencies		Installment Balances		Mortgage Balances	
Children Non-Student Loan Delinquencies:	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.0044	-0.0014*	1,326.7*	661.7**	1,502.4**	634.2**
	(0.0033)	(0.0008)	(726.9)	(280.3)	(706.5)	(279.2)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	551,018	5,080,812	551,018	5,080,812	551,018	5,080,812
R^2	0.520	0.518	0.857	0.854	0.848	0.845

Table 9:
Heterogeneity by Parental Financial Condition

This table reports the heterogeneity in the baseline findings based on parental financial conditions using two proxies: home ownership (panel A) and credit score (panel B). All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Homeownership

Dependent Variables:	Installment Delinquencies		Installment Balances		Mortgage Balances	
Parents' Homeowner:	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.0020*	-0.0016	957.1**	68.40	966.4**	79.82
	(0.0010)	(0.0012)	(382.4)	(238.6)	(377.7)	(231.3)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,562,277	2,221,111	3,562,277	2,221,111	3,562,277	2,221,111
R^2	0.518	0.520	0.836	0.534	0.828	0.399

Panel B: Parents' Credit Score

Dependent Variables:	Installment Delinquencies		Installment Balances		Mortgage Balances	
	(1)	(2)	(3)	(4)	(5)	(6)
Parents' Credit Score:	Subprime	Non-Subprime	Subprime	Non-Subprime	Subprime	Non-Subprime
Treated × Post	-0.0111**	-0.0007	741.5	781.5**	856.1	766.0**
	(0.0049)	(0.0005)	(656.5)	(286.1)	(637.4)	(284.6)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	755,988	5,027,326	755,988	5,027,326	755,988	5,027,326
R^2	0.514	0.426	0.848	0.855	0.831	0.847

Table 10:
Parents' Own Student Loans

This table reports the baseline results based on different criteria imposed across panels. Panel A excludes parents subject to student loan forbearance on their own student loans from the sample. Panel B includes fixed effects for parents' own treatment status. Panel C excludes observations in student loan delinquencies. Panel D includes fixed effects for pre-shock student loan balances. Regressions otherwise follow the baseline specification, except where the parental student-loan controls are replaced as indicated. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Remove Parents Under Student Loan Forbearance

Dependent Variables:	Installment Delinquencies (1)	Non-Installment Delinquencies (2)	Bankruptcy Flag (3)	Installment Balances (4)	Mortgage Balances (5)	HELOC Balances (6)
Treated × Post	-0.0015*	0.0017	-0.0018***	910.4***	910.2***	144.9**
	(0.0007)	(0.0017)	(0.0005)	(272.3)	(270.9)	(55.94)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.0278	0.0838	0.0302	87,630	74,565	5,541
Observations	5,460,907	5,460,907	5,460,907	5,460,907	5,460,907	5,449,135
R^2	0.510	0.486	0.769	0.853	0.846	0.741

Panel B: Parents' Own Treatment Status FEs

Dependent Variables:	Installment Delinquencies (1)	Non-Installment Delinquencies (2)	Bankruptcy (3)	Installment Balances (4)	Mortgage Balances (5)	HELOC Balances (6)
Treated × Post	-0.0013*	0.0020	-0.0018***	868.3***	738.6**	145.9**
	(0.0008)	(0.0016)	(0.0005)	(264.8)	(263.9)	(56.70)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Student Loan Balance Deciles × D(Parents' DL/FFEL)	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	0.0360	0.0891	0.0315	90,288	75,867	5,589
Observations	5,783,363	5,783,363	5,783,363	5,783,363	5,783,363	5,748,616
R^2	0.521	0.486	0.771	0.854	0.846	0.740

Panel C: Remove Student Loan Delinquencies

Dependent Variables:	Installment Delinquencies (1)	Non-Installment Delinquencies (2)	Bankruptcy Flag (3)	Installment Balances (4)	Mortgage Balances (5)	HELOC Balances (6)
Treated × Post	-0.0014*	0.0021	-0.0018***	759.4***	762.0***	150.0**
	(0.0008)	(0.0016)	(0.0005)	(263.1)	(262.2)	(56.59)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.0326	0.0863	0.0314	90,202	75,865	5,593
Observations	5,773,448	5,773,448	5,773,448	5,773,448	5,773,448	5,738,729
R^2	0.497	0.484	0.772	0.854	0.846	0.740

Panel D: Pre-shock Student Loan Balance FEs

Dependent Variables:	Installment Delinquencies	Non-Installment Delinquencies	Bankruptcy Flag	Installment Balances	Mortgage Balances	HELOC Balances
Treated × Post	-0.0018**	0.0018	-0.0018***	904.2***	769.2***	151.2**
	(0.0008)	(0.0016)	(0.0005)	(269.0)	(263.2)	(56.73)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Student Loan Balance Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,783,388	5,783,388	5,783,388	5,783,388	5,783,388	5,748,743
R^2	0.519	0.488	0.772	0.854	0.846	0.740

Table 11:
Alternate Fixed Effects

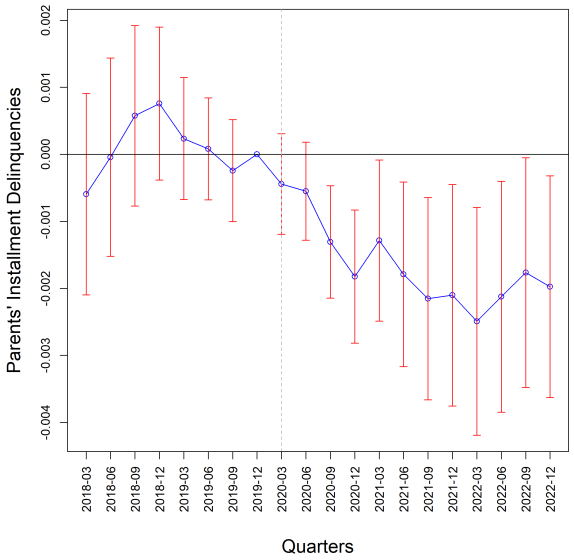
This table reports the baseline results using additional fixed effects as indicated in different panels. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Fixed Effects Matched to Balance Tests

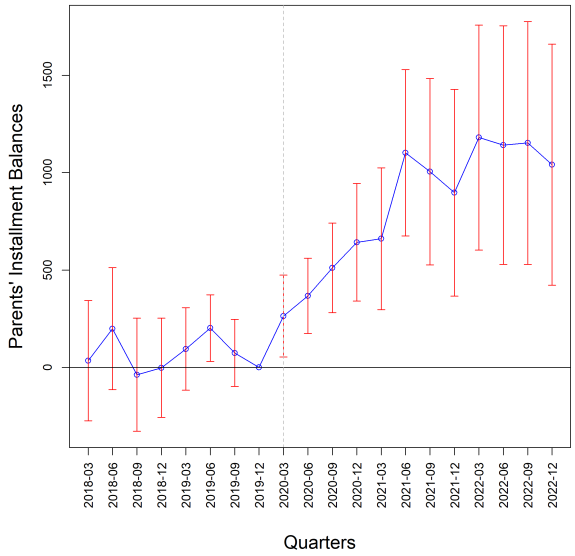
Dependent Variables:	Installment Delinquencies (1)	Non-Installment Delinquencies (2)	Bankruptcy Flag (3)	Installment Balances (4)	Mortgage Balances (5)	HELOC Balances (6)
Treated × Post	-0.0015*	0.0025	-0.0012**	706.9**	713.4**	142.5**
	(0.0008)	(0.0016)	(0.0004)	(260.9)	(260.2)	(56.24)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Bankruptcy)	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Auto Delinquency)	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Card Balance Bins)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.0360	0.0891	0.0315	90,288	75,867	5,589
Observations	5,783,367	5,783,367	5,783,367	5,783,367	5,783,367	5,748,636
R^2	0.520	0.488	0.816	0.855	0.846	0.740

Panel B: Local Economic Conditions

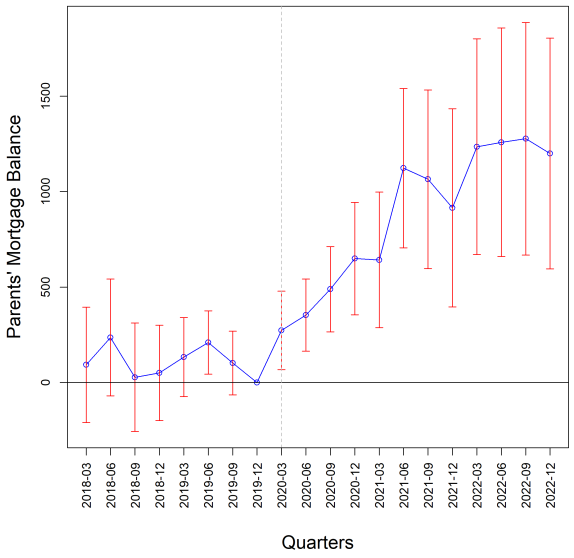
Dependent Variables:	Installment Delinquencies (1)	Non-Installment Delinquencies (2)	Bankruptcy Flag (3)	Installment Balances (4)	Mortgage Balances (5)	HELOC Balances (6)
Treated × Post	-0.0020**	0.0019	-0.0009*	734.8**	744.7***	93.31*
	(0.0008)	(0.0016)	(0.0005)	(262.0)	(260.1)	(53.87)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × ZIP3	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.0360	0.0891	0.0315	90,288	75,867	5,589
Observations	5,783,347	5,783,347	5,783,347	5,783,347	5,783,347	5,748,616
R^2	0.520	0.489	0.773	0.855	0.846	0.742



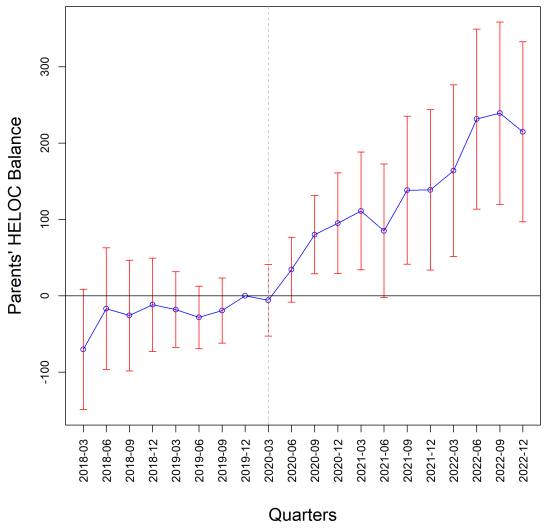
(a) Installment Delinquencies



(b) Installment Balances



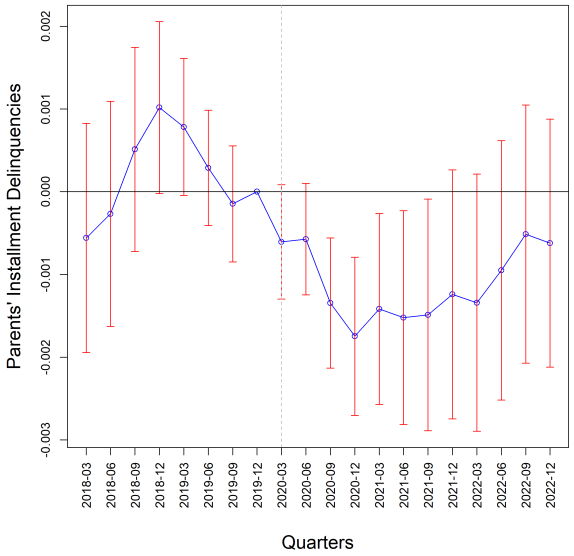
(c) Mortgage Balances



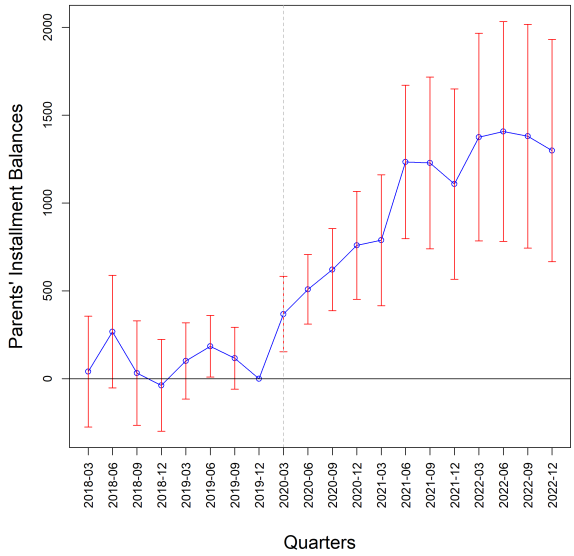
(d) Heloc Balances

Figure 1:
Dynamic Effects

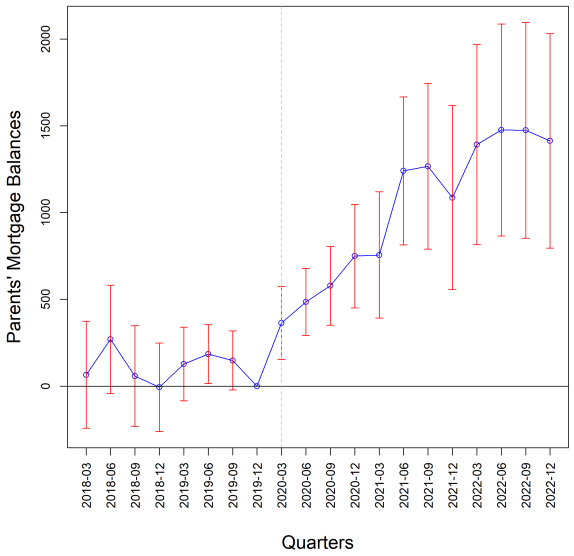
This figure shows the timing of the effect of children's student loan forbearance on parents' outcomes. The figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-differences estimation. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Standard errors are double-clustered at the individual and quarter level.



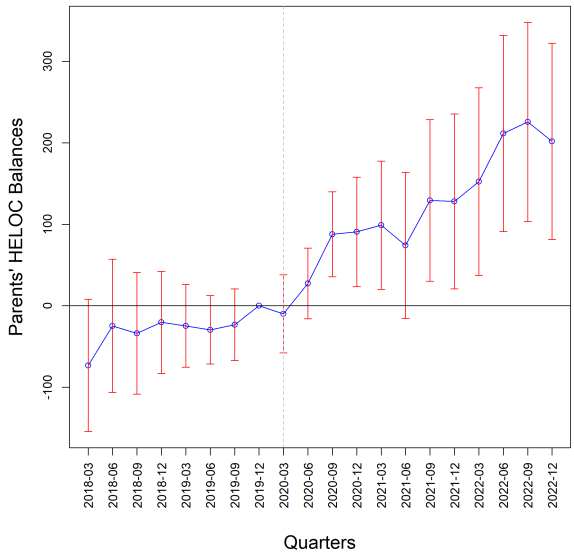
(a) Installment Delinquencies



(b) Installment Balances



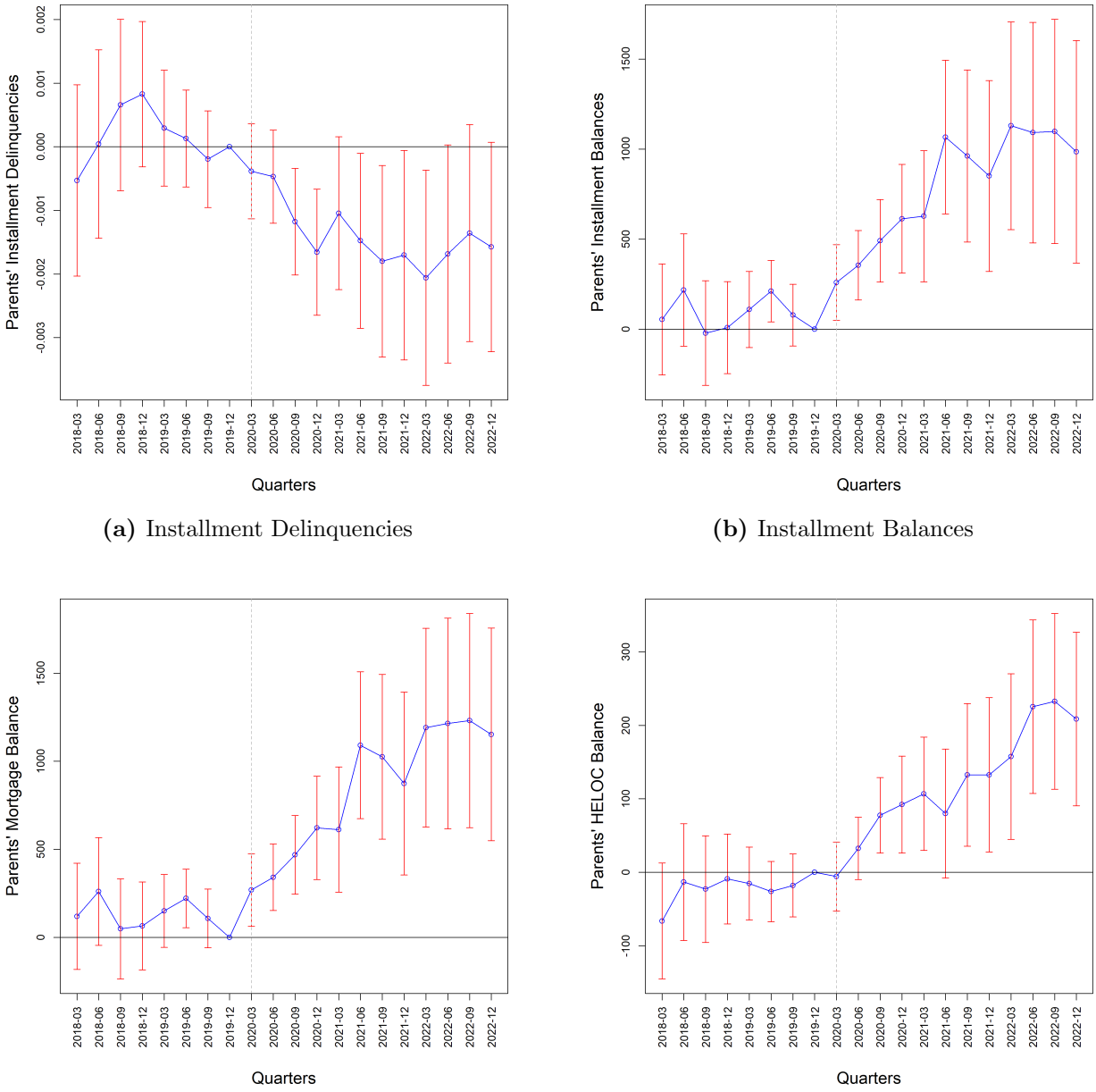
(c) Mortgage Balances



(d) Heloc Balances

Figure 2:
Dynamic Effects: Exclude Parents with Student Loan forbearance

This figure shows the dynamic effects of the baseline results, excluding parents subject to student loan forbearance on their own student loans from the sample. The figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-differences estimation. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Standard errors are double-clustered at the individual and quarter level.



(a) Installment Delinquencies

(b) Installment Balances

(c) Mortgage Balances

(d) Heloc Balances

Figure 3:
Dynamic Effects: Additional Fixed Effects

This figure shows the dynamic effects of the baseline results with additional fixed effects matched to the balance test. The figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-differences estimation. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, an indicator for a positive student loan balance, an indicator for auto loan delinquency, credit card balance bins, and bankruptcy status, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Standard errors are double-clustered at the individual and quarter level.

Appendix for Online Publication

Table A1:
Summary Statistics for Children

This table reports summary statistics for children in the sample as of December 2019, one quarter prior to treatment, separately for the treated and control groups.

	All	Treated	Control	Difference
Variable	Mean	Mean	Mean	Treated − Control
Credit Score	722.10	714.44	728.81	-14.38
Credit Limit	33,155.04	27,085.33	38,470.58	-11,385.25
Mortgage Balance	137,504.80	105,269.90	165,734.60	-60,464.63
Credit Card Balance	7,907.14	7,017.34	8,686.39	-1,669.05
Auto Balance	11,617.42	10,976.86	12,178.39	-1,201.53
HELOC Balance	2,982.94	1,825.89	3,996.22	-2,170.32
Student Loan Balance	20,817.18	20,316.01	21,256.08	-940.07
Mortgage Delinquency	0.002	0.002	0.002	0.000
Credit Card Delinquency	0.037	0.041	0.034	0.006
Auto Delinquency	0.010	0.012	0.009	0.004
HELOC Delinquency	0.000	0.000	0.000	0.000
Student Loan Delinquency	0.022	0.026	0.018	0.008
Mortgage Trades	0.58	0.49	0.66	-0.18
Credit Card Trades	3.71	3.53	3.87	-0.34
Auto Trades	0.73	0.72	0.74	-0.02
HELOC Trades	0.06	0.04	0.08	-0.04
Student Loan Trades	2.40	2.65	2.17	0.48
Mortgage Scheduled Payments	1,037.83	790.09	1,254.79	-464.71
Credit Card Scheduled Payments	203.14	185.40	218.68	-33.28
Auto Scheduled Payments	320.01	297.56	339.67	-42.12
HELOC Scheduled Payments	21.87	12.17	30.37	-18.20
Student Loan Scheduled Payments	245.39	237.95	251.90	-13.95
Mortgage Actual Payments	1,093.99	833.79	1,321.85	-488.06
Credit Card Actual Payments	434.78	370.30	491.25	-120.94
Auto Actual Payments	324.33	297.08	348.20	-51.12
HELOC Actual Payments	64.85	34.44	91.48	-57.05
Student Loan Actual Payments	262.61	248.09	275.33	-27.24

Table A2:
First-Stage: Children's Student Loan Payments

This table reports the effects of the student loan forbearance on children's student loan payments. All regressions include children fixed effects and quarter interactions with children's credit score deciles and age, all measured one quarter before treatment. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Student Loan Scheduled Payments			Student Loan Delinquencies		
Children Age	Pooled	Younger	Older	Pooled	Younger	Older
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-163.1***	-143.0***	-179.2***	-0.0100***	-0.0086***	-0.0119***
	(17.73)	(17.18)	(17.86)	(0.0019)	(0.0020)	(0.0020)
Fixed Effects:						
Children	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	224.8	208.6	241	0.0260	0.0295	0.0238
Observations	6,218,091	3,092,413	3,125,638	6,218,091	3,092,413	3,125,638
R^2	0.824	0.801	0.830	0.460	0.485	0.431

Table A3:
Heterogeneity on Parents' Pre-shock Card Delinquencies

This table reports the heterogeneity effects on whether parents have credit card delinquencies in the pre-shock period. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Installment Delinquencies		Installment Balances		Mortgage Balances	
Parent Card Delinq:	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.0197*	-0.0013*	-219.4	785.5***	465.5	769.5***
	(0.0106)	(0.0007)	(1,140.6)	(268.4)	(1,098.1)	(266.9)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	0.2524	0.0296	69,077	93,587	50,508	78,931
Observations	177,438	5,605,884	177,438	5,605,884	177,438	5,605,884
R^2	0.496	0.511	0.851	0.855	0.833	0.846

Table A4:
Heterogeneity on Parents' Pre-shock Non-Student Loan Delinquencies

This table reports the heterogeneity effects based on whether parents have non-student loan delinquencies in the pre-shock period. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Installment Delinquencies		Installment Balances		Mortgage Balances		D(Bankruptcy)	
Parent Non-Student Delinq:	Yes	No	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post	-0.0149*	-0.0009	1,978.6*	703.7**	2,396.5**	684.1**	-0.0061*	-0.0016***
	(0.0083)	(0.0007)	(988.8)	(270.2)	(952.5)	(268.8)	(0.0035)	(0.0005)
Fixed Effects:								
Parents	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls:								
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	0.3025	0.0211	77,425	93,708	58,249	79,185	0.0866	0.0323
Observations	311,125	5,472,207	311,125	5,472,207	311,125	5,472,207	311,160	5,472,207
R^2	0.506	0.492	0.832	0.855	0.816	0.847	0.75652	0.77305

Table A5:
Other Balance Outcomes

This table reports the baseline regression results on parents' other balance outcomes. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Auto Balances (1)	Card Balances (2)	Revolving Balances (3)	Bankcard Balances (4)
Treated × Post	-63.99*	42.65**	81.65	25.04
	(35.79)	(19.71)	(84.27)	(23.13)
Fixed Effects:				
Parents	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes
Controls:				
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes
Outcome Mean	7,412	5,782	13,193	5,884
Observations	5,783,388	5,748,743	5,607,071	5,141,159
R^2	0.671	0.800	0.749	0.790

Table A6:
Heterogeneity on Parents' Credit Card Utilization

This table examines heterogeneity in the treatment effect on parents' HELOC balances by their pre-shock credit card utilization rate, defined as credit card balance divided by credit limit. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	HELOC Balances		HELOC Balances	
Credit Card Utilization (%)	> 75	< 75	> 80	< 80
	(1)	(2)	(3)	(4)
Treated × Post	365.0**	123.2*	386.3**	125.1**
	(158.6)	(59.03)	(172.4)	(58.62)
Fixed Effects:				
Parents	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes
Controls:				
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes
Outcome Mean	5,080	5,647	4,957	5,650
Observations	581,661	5,166,924	506,254	5,242,328
R^2	0.784	0.735	0.785	0.736

Table A7:
Heterogeneity on Children's Student Loan Payments

This table reports the heterogeneity effects based on children's student loan payments. The average pre-treatment actual student loan payment is $64 among children in the bottom payment decile and $263 among the remaining children. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Installment Delinquencies	Installment Delinquencies	Installment Balances	Installment Balances	Mortgage Balances	Mortgage Balances
Treated × Post	0.0005	-0.0020**	452.3	702.5**	488.5	687.8**
	(0.0024)	(0.0008)	(564.8)	(276.4)	(557.9)	(274.1)
Sample:						
Children Actual Payment	Bottom 10%	Else	Bottom 10%	Else	Bottom 10%	Else
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	0.0394	0.0277	89,123	89,457	74,848	75,610
Observations	567,740	5,010,174	567,740	5,010,174	567,740	5,010,174
R^2	0.548	0.521	0.870	0.857	0.862	0.849

Table A8:
Heterogeneity by Children's Age: Parents' Mobility and Income

This table examines heterogeneity in the effects of treatment by children's age. The sample is divided into younger and older children. Panel A reports effects on parents' residential mobility, while Panel B reports effects on parents' annual income. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Parents' Residential Mobility

Dependent Variables:	Move ZIP Code	Move ZIP3	Move ZIP Code	Move ZIP3
Children Age:	Younger	Younger	Older	Older
	(1)	(2)	(3)	(4)
Treated × Post	-0.000546	-0.000364	-3.02×10^{-5}	-0.000324
	(0.000440)	(0.000388)	(0.000449)	(0.000301)
Fixed Effects:				
Parents	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes
Controls:				
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes
Outcome Mean	0.024	0.014	0.021	0.013
Observations	3,099,899	3,099,899	2,640,619	2,640,619
R^2	0.136	0.128	0.133	0.125

Panel B: Parents' Annual Income

Dependent Variables:	Annual Income	Log Annual Income	Annual Income	Log Annual Income
Children Age:	Younger	Younger	Older	Older
	(1)	(2)	(3)	(4)
Treated × Post	-190.80	-0.00368	-968.43	-0.00834
	(439.99)	(0.00343)	(620.98)	(0.00588)
Fixed Effects:				
Parents	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes
Controls:				
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes
Outcome Mean	90,216	90,216	80,179	80,179
Observations	422,229	422,229	176,003	176,003
R^2	0.917	0.933	0.928	0.935

Table A9:
Controlling for Local House Prices

This table reports the baseline regression results controlling for lagged local house prices, measured using the Zillow Home Value Index (ZHVI) for the ZIP code of the parents' residence. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Installment Delinquencies (1)	Non-Installment Delinquencies (2)	Bankruptcy (3)	Installment Balances (4)	Mortgage Balances (5)
Log House Price (Lagged)	0.0038**	0.0178***	-0.0030***	10,624.5***	10,933.8***
	(0.0016)	(0.0036)	(0.0009)	(811.2)	(806.1)
Treated × Post	-0.0018**	0.0019	-0.0018***	716.7**	722.4**
	(0.0008)	(0.0016)	(0.0005)	(260.3)	(259.1)
Fixed Effects:					
Parents	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes
Controls:					
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes
Outcome Mean	0.0360	0.0891	0.0315	90,288	75,867
Observations	5,783,367	5,783,367	5,783,367	5,783,367	5,783,367
R^2	0.518	0.488	0.772	0.855	0.846

Table A10:
Alternate Standard Error Clusters

This table reports the baseline results with standard errors clustered by parents' credit score deciles, measured one quarter before treatment, in Panel A; children's student loan origination year in Panel B; parents' state of residence in Panel C; and family in Panel D. In Panel D, families are defined based on the unique combination of children linked to the parents. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Clustering on Parent Credit Score Deciles

Dependent Variables:	Installment Delinquencies	Non-Installment Delinquencies	Bankruptcy Flag	Installment Balances	Mortgage Balances
Treated × Post	-0.0017**	0.0020	-0.0018**	753.7***	760.4***
	(0.0007)	(0.0017)	(0.0007)	(177.4)	(150.3)
Fixed Effects:					
Parents	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes
Controls:					
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes
Outcome Mean	0.0360	0.0891	0.0315	90,288	75,867
Observations	5,783,367	5,783,367	5,783,367	5,783,367	5,783,367
R^2	0.518	0.488	0.772	0.855	0.846

Panel B: Clustering on Children's Student Loan Open Year

Dependent Variables:	Installment Delinquencies	Non-Installment Delinquencies	Bankruptcy Flag	Installment Balances	Mortgage Balances
Treated × Post	-0.0017**	0.0020	-0.0018***	753.7***	760.4***
	(0.0008)	(0.0019)	(0.0005)	(182.1)	(195.8)
Fixed Effects:					
Parents	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes
Controls:					
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes
Outcome Mean	0.0360	0.0891	0.0315	90,288	75,867
Observations	5,783,367	5,783,367	5,783,367	5,783,367	5,783,367
R^2	0.518	0.488	0.772	0.855	0.846

Panel C: Clustering on State of Parents' Residence

Dependent Variables:	Installment Delinquencies (1)	Non-Installment Delinquencies (2)	Bankruptcy (3)	Installment Balances (4)	Mortgage Balances (5)
Treated × Post	-0.0017**	0.0019	-0.0018***	753.8***	759.5***
	(0.0007)	(0.0016)	(0.0004)	(222.2)	(212.0)
Fixed Effects:					
Parents	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes
Controls:					
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes
Outcome Mean	0.0360	0.0891	0.0315	90,288	75,867
Observations	5,748,636	5,748,636	5,748,636	5,748,636	5,748,636
R^2	0.519	0.488	0.771	0.855	0.846

Panel D: Clustering on Students/Families

Dependent Variables:	Installment Delinquencies (1)	Non-Installment Delinquencies (2)	Bankruptcy (3)	Installment Balances (4)	Mortgage Balances (5)
Treated × Post	-0.0017**	0.0020	-0.0018***	753.7**	760.4**
	(0.0008)	(0.0016)	(0.0005)	(267.3)	(266.3)
Fixed Effects:					
Parents	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes
Controls:					
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes
Outcome Mean	0.0360	0.0891	0.0315	90,288	75,867
Observations	5,783,367	5,783,367	5,783,367	5,783,367	5,783,367
R^2	0.518	0.488	0.772	0.855	0.846

Table A11:
Baseline Results with Family Weights

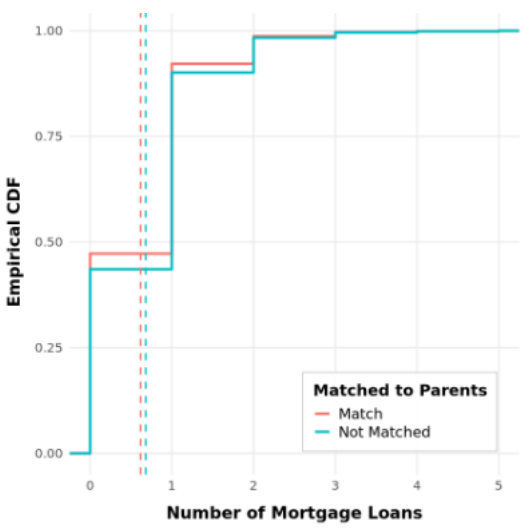
This table reports the baseline regression results using family weights to account for differences in the number of parents associated with the same family. A family is defined by the unique combination of children linked to a set of parents. Each parent is assigned a weight equal to the inverse of the number of parents associated with the same family. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variables:	Installment Delinquencies (1)	Non-Installment Delinquencies (2)	Bankruptcy (3)	Installment Balances (4)	Mortgage Balances (5)
Treated × Post	-0.0022**	0.0010	-0.0019***	611.9**	619.9**
	(0.0009)	(0.0018)	(0.0006)	(274.9)	(273.0)
Fixed Effects:					
Parents	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes
Controls:					
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes
Outcome Mean	0.0360	0.0891	0.0315	90,288	75,867
Observations	5,783,367	5,783,367	5,783,367	5,783,367	5,783,367
R^2	0.519	0.489	0.772	0.854	0.845

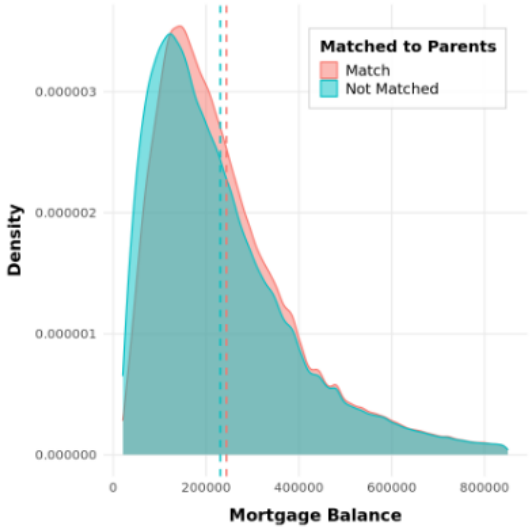
Table A12:
Alternate Delinquency Measures

This table reports robustness results using alternative measures of delinquency. The new delinquency measure captures the number of new delinquency occurrences on debts for which parents were current in the preceding quarter. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Robust standard errors, double clustered at the individual and quarter, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

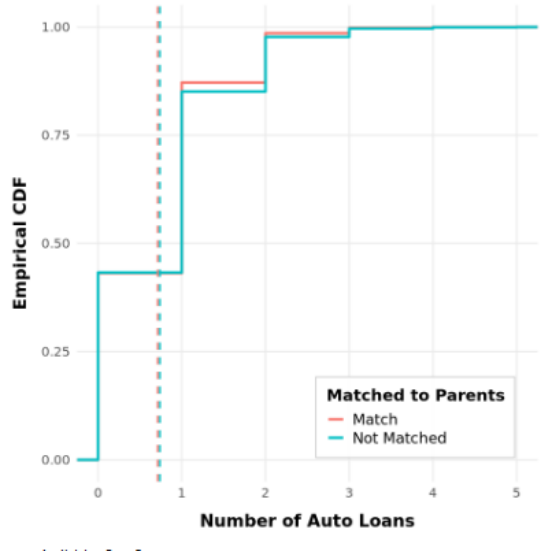
Dependent Variables:	Installment Delinquencies	Non-Installment Delinquencies	Card Delinquencies	New Installment Delinquencies	New Non-Installment Delinquencies	New Card Delinquencies
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.0017**	0.0020	0.0007	-0.0003**	0.0000	0.0000
	(0.0008)	(0.0016)	(0.0005)	(0.0001)	(0.0002)	(0.0001)
Fixed Effects:						
Parents	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Credit Score Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Parents Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × Children Age	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × D(Student Loan)	Yes	Yes	Yes	Yes	Yes	Yes
Controls:						
Parents' Student Loan Balance (Cubic)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,783,388	5,783,388	5,748,636	5,777,287	5,777,287	5,740,414
R^2	0.518	0.487	0.609	0.082	0.105	0.078



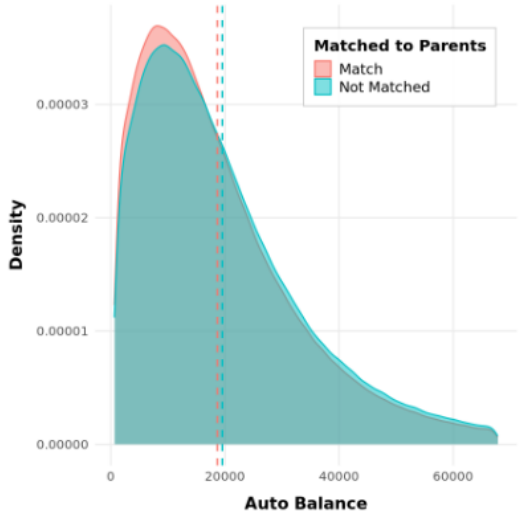
(a) Number of Home Loans



(b) Home Loan Balance



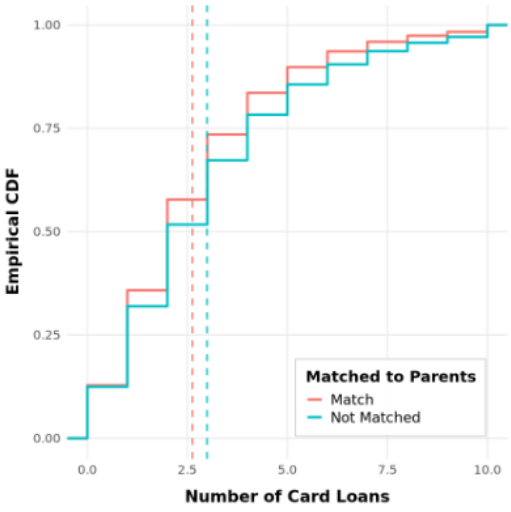
(c) Number of Auto Loans



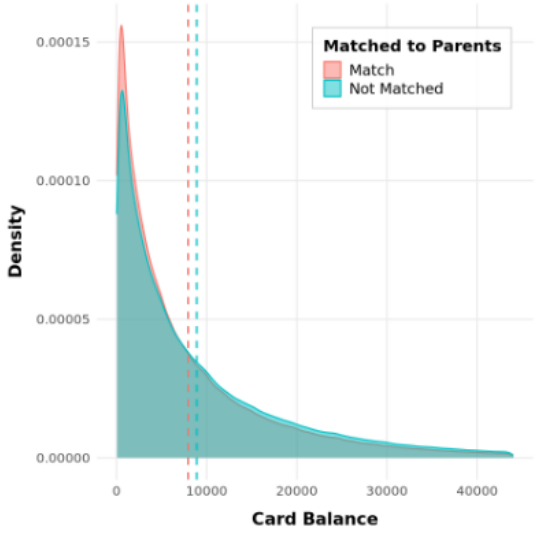
(d) Auto Loan Balance

Figure A1: Sample Selection into Children Matched with Families

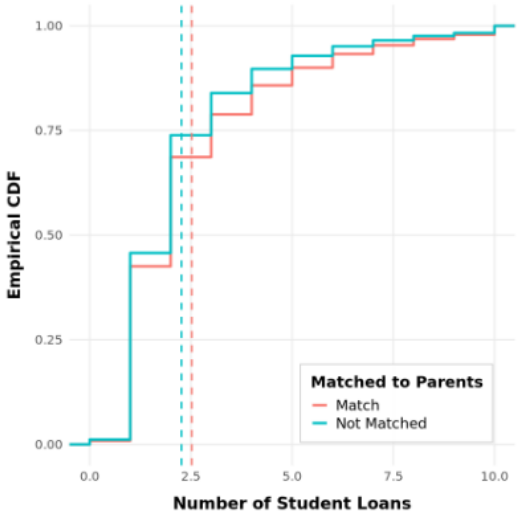
This figure compares children's credit outcomes in December 2019 (one quarter before the policy shock) for the subset of children matched to parents and not matched to parents.



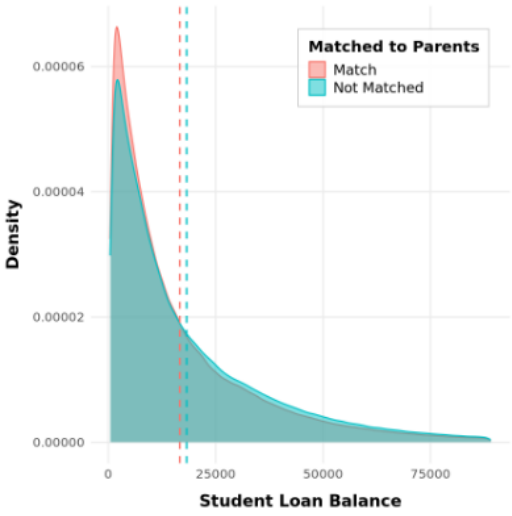
(a) Number of Credit Card Loans



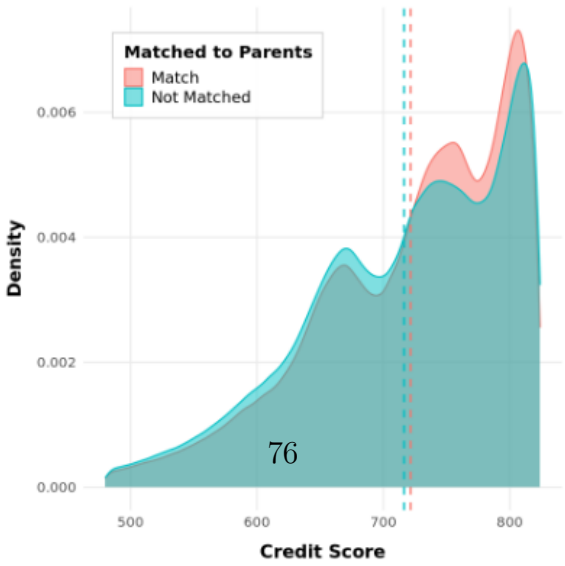
(b) Credit Card Balance



(c) Number of Student Loans



(d) Student Loan Balance



(e) Credit Score

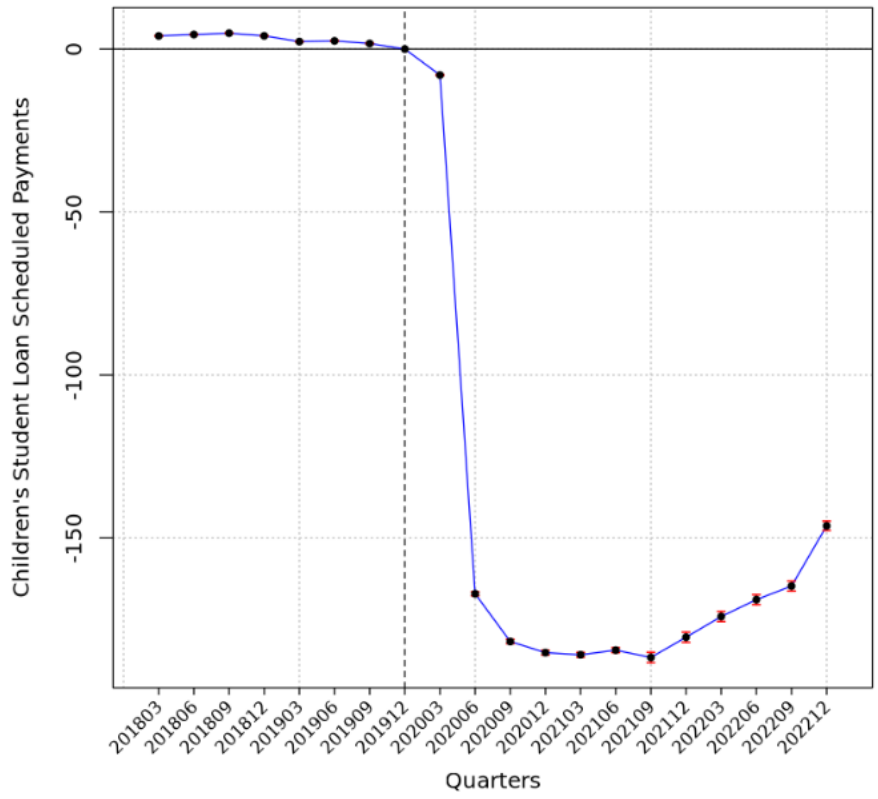
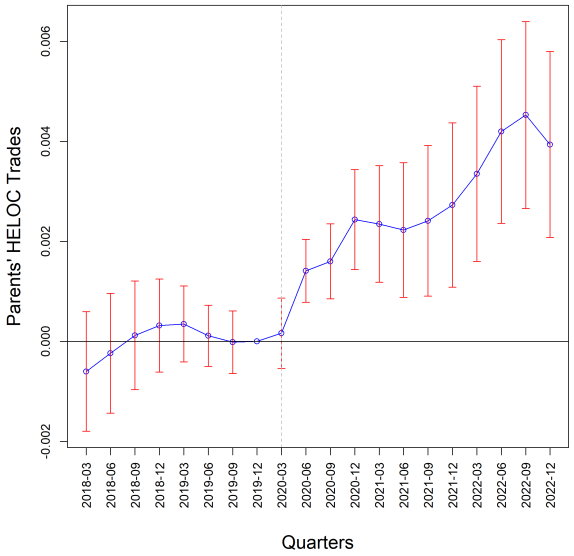
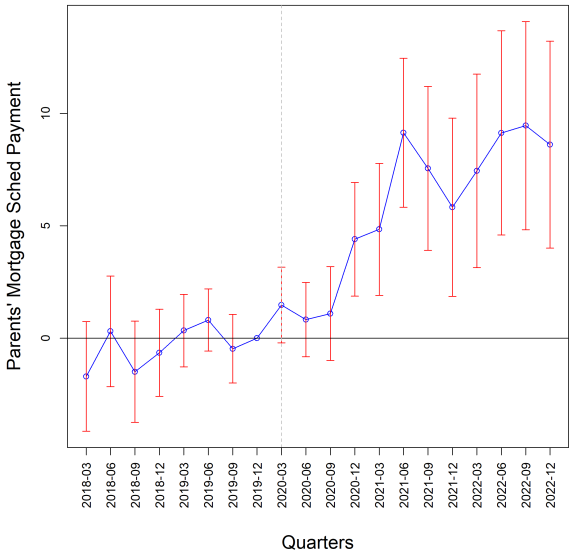


Figure A2:
Dynamic Effects: First Stage

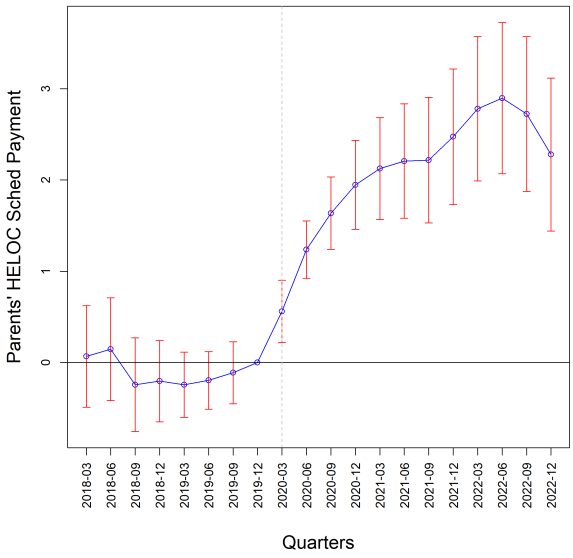
This figure shows the dynamic effects of the first stage, i.e., the effect of the moratorium on children's scheduled student loan payments. The figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-differences estimation. The regression includes children fixed effects and quarter interactions with children's credit score deciles and age, all measured one quarter before treatment. Standard errors are double-clustered at the individual and quarter level.



(a) HELOC Trades



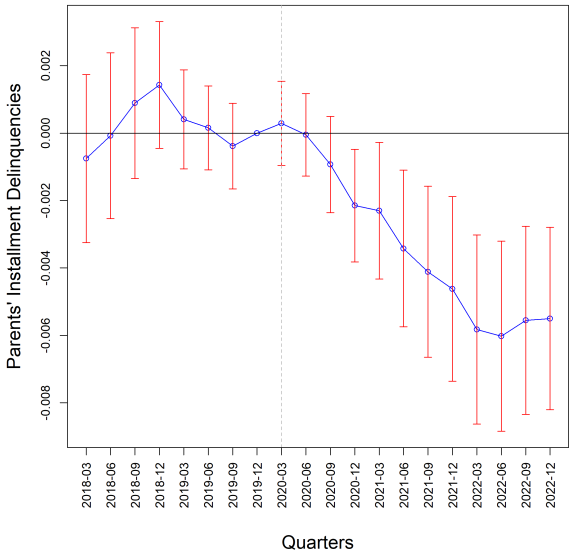
(b) Mortgage Scheduled Payments



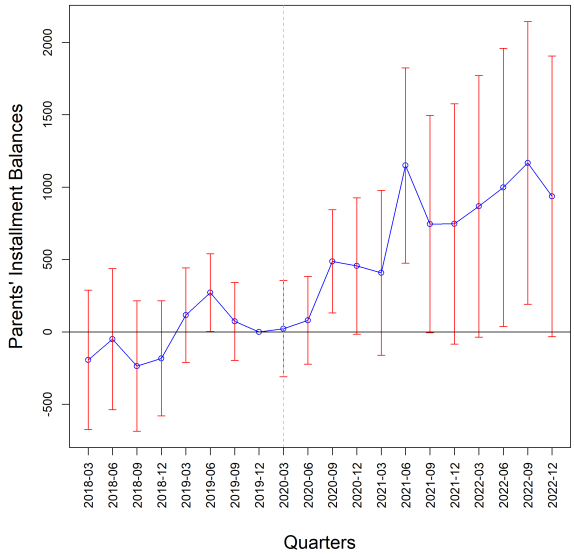
(c) HELOC Scheduled Payments

Figure A3:
Dynamic Effects: Additional Home Loan Outcomes

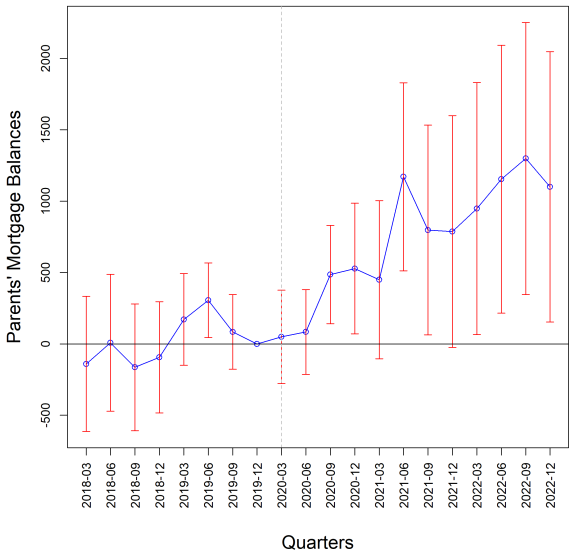
This figure shows the dynamics of the effects of children's student loan forbearance on parents' HELOC trades, mortgage payments, and HELOC payments. The figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-differences estimation. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, along with a cubic in parents' student loan balances, all measured one quarter before treatment. Standard errors are double-clustered at the individual and quarter level.



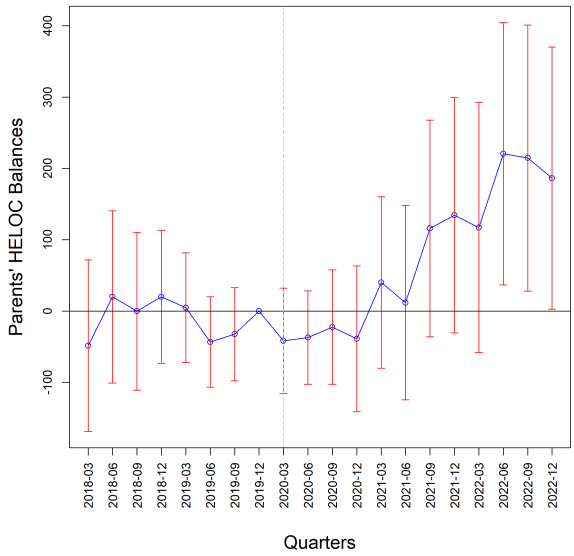
(a) Installment Delinquencies



(b) Installment Balances



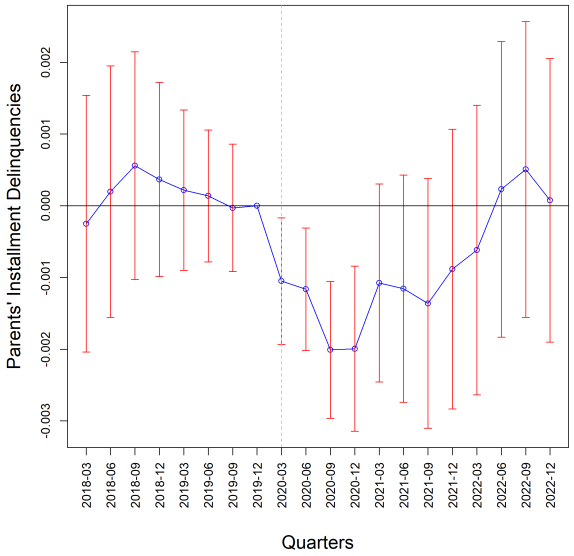
(c) Mortgage Balances



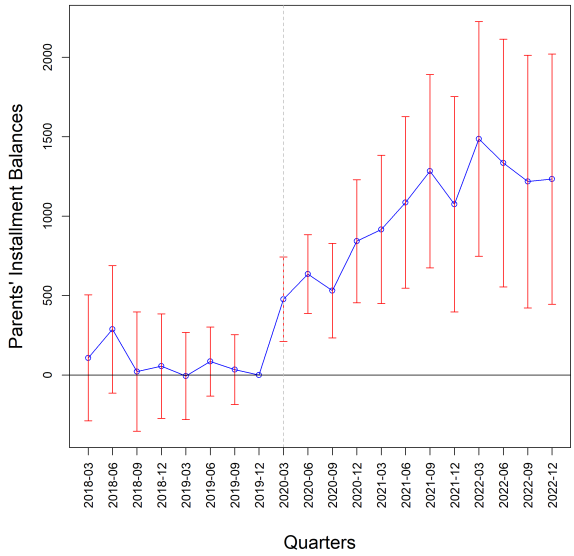
(d) Heloc Balances

Figure A4:
Dynamic Effects: Younger Children

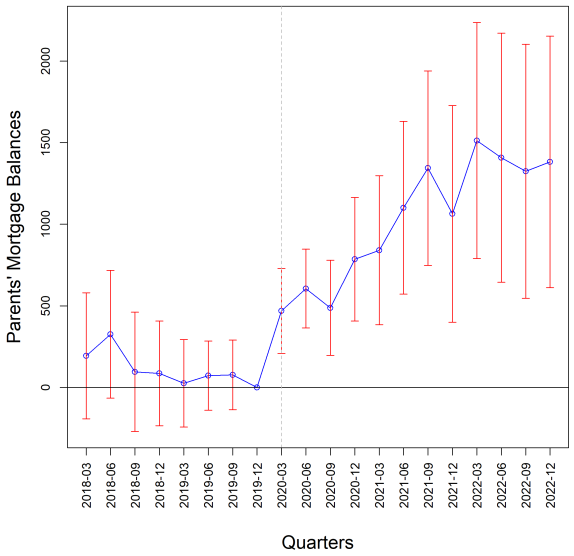
This figure shows the dynamic effects of the baseline results for parents of below-median-aged children. The figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-differences estimation. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Standard errors are double-clustered at the individual and quarter level.



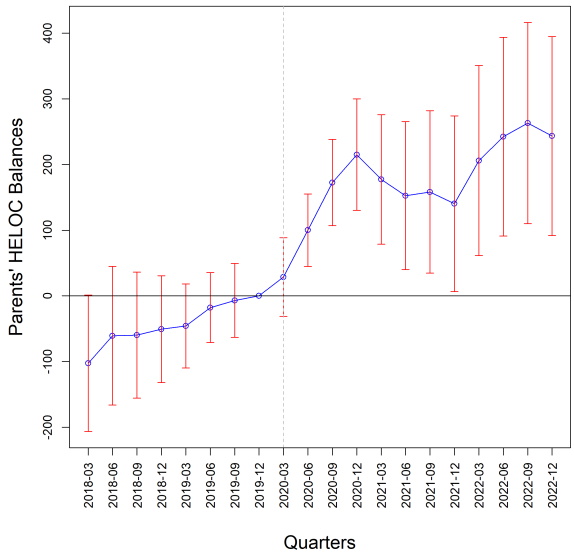
(a) Installment Delinquencies



(b) Installment Balances



(c) Mortgage Balances



(d) Heloc Balances

Figure A5:
Dynamic Effects: Older Children

This figure shows the dynamic effects of the baseline results for parents of above-median-aged children. The figure plots coefficient estimates and the 95% confidence intervals from dynamic difference-differences estimation. All regressions include parent fixed effects and quarter interactions with parents' credit score deciles, parents' age, children's age, and a positive student loan balance indicator, all measured one quarter before treatment, along with a cubic in parents' student loan balances. Standard errors are double-clustered at the individual and quarter level.